The Labor Market Returns to Delaying Pregnancy*

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Abstract

We study the extent to which delaying pregnancy mitigates the impact of children on women's labor market outcomes. We leverage quasi-random variation in the timing of pregnancy in a setting where women intend to delay having children by using long-acting reversible contraceptives. While most women successfully delay pregnancy, some have unplanned pregnancies. Analyzing linked health and labor market data from Sweden, we find that unplanned pregnancies halt women's career progression resulting in income losses of 20% by five years after the unplanned pregnancy. Using pregnancy as an instrument for birth in a dynamic treatment effect framework, the detrimental effects of unplanned children are larger for younger women and women enrolled in education. This indicates that unplanned first births are particularly disruptive early on when women are investing in their human capital. In contrast, we find smaller impacts of unplanned pregnancies for women who are already mothers. When we estimate the impact of first births identified from quasi-random success of fertilization procedures, we likewise find smaller impacts of children among women who intend to get pregnant. Taken together, our results suggest that the value of effective contraceptives is highest for women without children, and women can reduce the large labor market costs of having their first child by timing pregnancy.

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

JEL Codes: J13; J22; J24; J31

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1 Introduction

Over the past fifty years, the lives of women have transformed dramatically. Between 1970 and 2020, the average age at which US women were having their first child rose from 21 to 27 years old (Osterman et al., 2022). With access to better birth control, women delayed pregnancy and gained time to find a better partner and to increase their educational attainment and labor market attachment (Goldin and Katz, 2002; Bailey, 2006). Nonetheless, having a child is associated with large drops in earnings, hours, and employment for women in countries with and without generous maternity leave policies (Kleven et al., 2019b). In this paper, we study the extent to which delaying pregnancy mitigates the impact of children on women's careers, and how this varies with the circumstances in which children are born.

Learning about the trade-offs women face in deciding when to have children is challenging because women may avoid pregnancy at times in their lives when it would be disruptive. We use quasi-random variation in the timing of pregnancy in a setting where women would like to delay having children. We examine the labor market outcomes of Swedish women who become pregnant while using long-acting reversible contraceptives (LARC), in particular, intrauterine devices (IUDs) and birth control implants. These methods of birth control work passively and are effective, but not perfect. About 0.5-1.5% 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.[1] We refer to pregnancies shortly following a LARC prescription as unplanned pregnancies. 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. The women who experience an unplanned pregnancy may choose to have an abortion or to give birth to a child. The women who do not experience an unplanned pregnancy continue with their lives as planned and serve as a counterfactual. Before the LARC, the labor market outcomes of women who experience an unplanned pregnancy are virtually identical to those who do not.

We find that unplanned pregnancies have substantial, negative, and lasting consequences on the careers of previously childless women. Seven years after the LARC, earnings are 15% lower and the probability of working in an occupation requiring medium, high, or managerial skills is almost 20% lower than if the unplanned pregnancy had not occurred. We also find substantial heterogeneity in the impact of unplanned pregnancy: effects are larger for younger women and for women who are enrolled in education. This suggests that timing matters for the impact of an unplanned pregnancy.

Heterogeneity in the impact of an unplanned pregnancy may be driven by three primary factors: differ-

[1] Our baseline specification uses data on hormonal IUD and implant prescriptions to identify unplanned pregnancies, defining an unplanned pregnancy as a conception within nine months of receiving the LARC prescription. We provide several validations of this definition, as well as robustness to alternative definitions. This definition gives us unplanned pregnancy rates that are similar to what is found in the medical literature. See Section 3.3 for more details.

ences in the impact of children on women's careers, differences in the abortion rate for unplanned pregnancies, or differences in the behavior of the control group.[2] These three factors make it challenging to interpret the heterogeneity in the impact of unplanned pregnancies as arising from differences in the impact of children on women's careers.

In order to isolate the impact of children on women's careers, we use pregnancy as an instrument for birth. Using instrumental variables (IV) terminology, reduced form differences in the impact of pregnancy on outcomes may be driven by differences in the first-stage or differences in the impact of children. In our setting, differences may additionally arise due to *dynamic* non-compliance, as some women in the control group are later treated (*i.e.*, have children later). Our IV strategy allows us to account for differences in the rate of abortion and birth, imposing the assumption that abortion does not itself impact labor market trajectories of women. This assumption is consistent with narrative evidence from the Turnaway study (Foster, 2020), and the position of the American Psychological Association (APA) on the mental health effects of an abortion.[3] We corroborate this assumption in our sample. If abortion causes a discontinuity in labor market outcomes, then we would be concerned that the assumption is violated. However, in our data, labor market paths around an abortion are smooth.[4] The third factor, dynamic non-compliance, requires that we go beyond a simple Wald or IV estimate of the effect of children on labor market outcomes over time.

We develop a dynamic IV-GMM estimation strategy that accounts for dynamic non-compliance in estimating dynamic causal impacts of children on women's careers. In what follows we outline how we identify the impact of a child by the age of the child. The impact of a newborn is identified by the ratio of the reduced form in the year of treatment assignment to the fraction of compliers in that year (*i.e.*, the Wald estimate). In the year after treatment assignment, the reduced form difference in outcomes is a combination of the impact of a newborn for women who newly have children and the impact of a one-year old for women who had children in the previous year. Using the already identified impact of a newborn, the impact of a one-year old is identified. Continuing in this way, we identify the impacts of children in every year after birth. In line with the identification approach, the IV-GMM estimator defines a moment for each reduced-form estimate of the impact of a pregnancy. Our identification and estimation strategies can be extended to incorporate

[2] When we use the phrase "the impact of children," we refer specifically to the impact over time of first childbirth among nulliparous women who have not previously given birth.

[3] 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).

[4] It is worth noting that the fact that those who have abortions are on a different labor market path than those who do not is not evidence against the exclusion restriction, this just reflects the fact that the population of never takers differs from compliers.

different estimates for planned and unplanned children and impacts that can vary by age.

We find that unplanned births have large and lasting effects on earnings for new mothers. The trajectory of the impact is relatively flat, with year-to-year estimates between 20 and 30 percent of counterfactual earnings. The short-term impact seems to be driven by non-employment and a reduced probability of promotion, especially in the first two years after birth. By four years after birth, employment and the probability of a promotion recover but the occupational trajectories and earnings deviate substantially as a result of having an unplanned child. At five years after an unplanned birth, new mothers are 25 percentage points less likely to be in an occupation requiring medium, high, or managerial skills.

Heterogeneity in the estimates of the impact of children on women's careers, while less precise, parallels the heterogeneity in the impacts of pregnancy: the impacts of children on women's careers vary greatly with the circumstances in which they are born. For women age 22-27, the average decline in earnings 1-6 years after birth is 32% of counterfactual earnings. This is more than twice the size of the impact of unplanned birth for women age 28 and above. A back of the envelope calculation using these estimates implies that being one year older at the time of an unplanned birth is associated with a two and a half percentage points smaller earnings loss. These results do not seem to be driven by difference in the composition of the groups by age: we find similar results if we weigh the young women to have similar characteristics to the older women when they were of similar age, and only 15% of the women in the "younger" group would have children by the time they were in the "older" group, were it not for the unplanned pregnancy. Also striking is the difference in the impact of children for women who are making human capital investments in the form of enrolling in an education program, compared to those who are not. Women who are enrolled have earnings losses on average that are twice as high (relative to their counterfactual) compared with women who are no longer enrolled. These results imply that some moments in an individual's labor market path are particularly sensitive to the presence of children. Delaying pregnancy to avoid these critical periods may substantially reduce the career impacts of children.

We allow for the dynamic impact of an unplanned birth to differ from a planned birth, because women who intend to become pregnant may have made choices to minimize the career costs of having children or wait until they are lower. To do this, we estimate the impact of "planned" children by studying childless women who would like to become pregnant and are undergoing in vitro fertilization procedures (IVF) in order to do so. As first discussed by Lundborg et al. (2017), women who successfully become pregnant with the first IVF procedure are compared to similar women who do not get pregnant initially.[5] Planned births

[5] This strategy also requires the assumption that IVF failure does not directly impact women's careers. However, among women who never have an IVF success, labor market paths flatten. This is perhaps indicative that discovery of infertility has negative labor market consequences. We discuss this in detail in Section 5.

have about half the earnings impact of unplanned births and similar employment and promotion impacts.[6] In contrast to unplanned births, planned births have no impact on occupation progression.

Finally, we study the impact of unplanned second children by comparing career paths of women who already have one child, receive a LARC, and become pregnant soon after. We find limited career effects of an additional child. Interestingly, we find no impact on occupation trajectory and the earnings impact is one-third the size. However, we find some evidence of a persistently lower employment probability. 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). Our setting offers an alternative way of comparing the impact of an additional pregnancy for women who would like to delay having another child. Our estimates imply that the benefits of access to more effective contraceptives are higher for women who are not yet mothers.

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. For example, Kahn (2010) and Oreopoulos et al. (2012) document the lasting impact of graduating in a recession. Using Swedish data, Heckman and Walker (1990) find that rising female wages over the life-cycle delay time to conception, but barely affect childlessness. 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 ones' comparative advantage or accumulate skills. The large difference between planned and unplanned births is also consistent with models in which women choose careers considering the future impacts of children. Seminal work by Polachek (1981) and later work by Adda et al. (2017) and Bronson (2014) suggest that precisely because children have a large impact on careers, women may alter their career paths to minimize this impact. Our finding that planned births are associated with small impacts on careers may reflect the fact that women who intend to have children early in their life-cycle may adjust their careers so that children are not particularly costly.

Our paper also contributes to a large literature utilizing "natural experiments" in fertility to shift the timing of childbirth. Notable papers in this literature include Hotz et al. (2005) and later Bíró et al. (2019) and Miller (2011) 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 Lundborg et al. (2017) and Bensnes et al. (2023)

[6] Comparing the *same* women in a planned pregnancy and an unplanned pregnancy is impossible—their fertility intentions are presumably not random—but we implement a DiNardo et al. (1996) re-weighting of the samples in order to understand the extent to which observable characteristics can explain the differences between planned and unplanned births. If we do not re-weight, the impacts of planned births are negligible in all outcomes, and the earnings impact is less than 10 percent.

who use initial IVF success or failure to shift the timing of 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. Among these related empirical strategies, ours uniquely focuses on the subset of women who intended to *delay* pregnancy, rather than those who *planned* to have children but faced obstacles.

Our paper is related to the literature studying the impact of unplanned pregnancy through the lens of abortion access, to the extent that the effects of unplanned pregnancy are similar to the effects of unwanted pregnancy. Recent work in this area includes Miller et al. (2020), Miller et al. (2022), and Brooks and Zohar (2020).[7] The average woman exposed to laws that change abortion access differ substantially from the LARC users in our sample. Abortions are most common among women in their teens and early 20s. In contrast, the subset of 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 become pregnant, despite not intending the pregnancy.

Many women use LARCs or other forms of birth control to delay pregnancy. Rau et al. (2021) and Bailey et al. (2023) study the effects of an increase in the price of birth control on fertility outcomes. Bailey et al. (2023) find that LARC use substantially increases in a randomize controlled trial which made contraceptive access free. In work focused on the introduction of the birth control pill, Goldin and Katz (2002), Bailey (2006), and Ananat and Hungerman (2012) argue that the advent of reliable contraception raised women's age at first marriage, increased women's education, and increased female labor force participation.[8] In this paper, we ask whether LARC use for the purposes of delaying childbirth among women without children continues to improve the labor market opportunities available to women. This is an important parameter for assessing the value of effective birth control. A question we are able to ask in our setting is whether delaying and planning first birth mitigates the labor market costs of motherhood. Our setting is ideal for studying this question because the control group is successfully able to delay pregnancy, while the treatment group becomes pregnant sooner than intended. This is policy relevant because this type of delay is due to contraceptive use—a choice by the woman—as opposed to other settings in which delay in childbirth is caused by miscarriages or failed IVF treatments.

We also contribute to the methodological literature on identification and estimation of dynamic treatment

[7] A number of additional papers study the impact of abortion access on children's labor market outcomes, finding that children born when 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 dictator Nicolae Ceausescu and finds that children born in these cohorts had worse outcomes than would be predicted by their mothers' education on a variety of dimensions. Mølland (2016) studies abortion access among teenagers in Norway and finds that abortion access improves educational attainment.

[8] More recently, Stevenson et al. (2021) studies a program which made LARCs free in Colorado and finds that educational attainment of eligible women increased.

effects in settings with dynamic non-compliance. Cellini et al. (2010) and later Baron (2022) study the impact of school-improvement bond issues using a sharp regression discontinuity design assuming homogeneous treatment effects. van den Berg and Vikström (2022) investigate the effect of training on earnings and assume treatment assignment is random among the not-yet-treated, conditional on observables. 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. In our setting, treatment is irreversible and there is dynamic non-compliance with initial treatment assignment. Bensnes et al. (2023) use an event study framework to study the effect of childbirth using IVF successes as an instrument for the age of a child. Allowing for heterogeneous treatment effects, we show identification of local average dynamic treatment effects using an instrument in a setting with dynamic non-compliance. We match women based on age and year of treatment and develop a dynamic IV-GMM estimator to jointly estimate heterogeneous treatment effects for planned and unplanned births.

Finally, our paper contributes to a large 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 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, 2021; Andresen and Nix, 2022). These empirical strategies reveal that motherhood is associated with large and persistent earnings declines (about 30% in the long-term in Sweden) and adverse life outcomes, such as increased rates of homelessness among low-SES women in the US. Kuka and Shenhav (2023) emphasize the importance of hastening return to work to mitigate these impacts. 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—observational data may mostly include births that were strategically timed. We conclude that avoiding costly times to have a child may substantially reduce the impact of children on careers and may explain some of the large demographic shifts in recent decades.

2 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.[9] In each period, human capital h determines earnings, and investment in human capital i is costly because it takes

[9]We present a full life-cycle Ben-Porath model in Appendix D. A two-period model is sufficient to illustrate the main effects of children that we highlight.

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.[10] 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.[11]

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

[10]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 D.

[11]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: $h_1 - i^*h_1 + \frac{1}{1+r}(A(i^*h_1)^\alpha + (1-\delta)h_1)$

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).[12] 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.

3 Institutional Setting and Data

In this section, we describe relevant institutional details (Section 3.1) and the Swedish administrative data (Section 3.2) to put the empirical analysis into context. In Section 3.3, we describe our approach to identifying unplanned pregnancies and births among LARC users. In Section 3.4, we present summary statistics and, finally, balance in Section 3.5.

[12] 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).

3.1 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 C. It is important to keep this setting in mind when interpreting the empirical results—impacts of children on women's careers are not, in this setting, likely to be driven by the direct monetary costs of children. The sizes of effects may well differ in places like the US where direct costs of healthcare and childcare are higher and family support is lower.

3.2 Swedish Administrative Data

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 for health insurance and labour market studies (LISA) that contains yearly observations during the period 1990-2013 on earnings, social transfers, employment, sector, 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.[13] We also analyse employment, promotions, and occupation outcomes to study how income is affected. Our measure of employment is based on an employment indicator in the last week of November in a given year. To proxy promotions, we keep track of the maximum within-individual yearly earnings over time. We assume an individual got promoted if their yearly earnings increased by 15% compared to their highest past yearly earnings.[14] Finally, we construct measures of the types of jobs women do in terms of skill requirements and workplace flexibility. We use the first digit of the Swedish occupation code to construct an indicator for whether women are in jobs that require managerial responsibilities, "High" theoretical special competence or "Medium" (at least a short university degree) competence.[15] 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;

[13] See Appendix B.1 for more details on the income measures.

[14] We follow Bronson and Thoursie (2021) in classifying a 15% increase as a promotion. Unlike Bronson and Thoursie (2021), we allow these increases to come from cross-firm moves, while they focus on within-firm wage changes.

[15] The first 3-digits of the Swedish occupation code (SSYK96) have an almost one-to-one mapping to the international ISCO88 code that we use to merge with the O*Net data to construct measures of workplace flexibility. See Appendix B.2 for more details on the occupation measures.

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 includes all in-patient care (1987-2013) and outpatient doctor visits including day surgery and psychiatric care from both private and public caregivers (2001-2013), but *not* primary care.

Our sample includes all women who are born in 1965-83 and reside in Sweden. These cohorts of women are 22-47 years old when we observe their contraceptive prescriptions.

3.3 Defining Unplanned (and Planned) Pregnancies

In this section we describe in more detail how we identify unplanned pregnancies among LARC users and planned pregnancies from first IVF successes.

3.3.1 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 failure rates of less than one percent per year (Trussell, 2004; Sundaram et al., 2017). Unlike most Short-Acting Reversible Contraceptives (SARCs), LARCs work passively and do not require the women to take any action until they wish to have them removed. IUDs are typically given to older women who have completed their fertility. 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 (on average 28 years old) and about a quarter of them have not yet had a child. Appendix Figure A1 shows the age distribution of women who receive an IUD or implant.

We focus our analysis on women who purchase a LARC with a prescription. We observe the date at which a woman paid for and received her LARC device from a pharmacy in the prescription data.[16] 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.[17]

Our baseline definition of an unplanned pregnancies is one that occurs within 9 months of purchasing

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

[17]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.

an IUD or implant contraception. We consider conceptions that end in childbirth or in an abortion. The birth registry gives us information about pregnancies that end in childbirth. The birth registry 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. The outpatient data contains information about the initial meeting a woman has with a doctor in the process of having an abortion. We assume the woman had an abortion if we observe an abortion meeting and no record of a childbirth.[18] We calculate the conception date for an abortion using the average time from conception to the first abortion meeting for women who have an initial abortion meeting but go on to have the child. 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 assigned to the control group.[19] In the sample of LARC users who have no children, we observe 350 unplanned pregnancies using a nine-month window.[20] In the sample of LARC users who already have one child, we observe an additional 683 pregnancies.

One concern is that we mistakenly classify planned pregnancies as unplanned because women have the LARC removed early.[21] In our setting, pregnancies occur for three broad reasons: failures of the device as observed in clinical trails, failures due to improper insertions of the device, and pregnancies arising after removals or expulsions of the LARC. Estimates of failure rates in clinical trials range between 0.001 to just under 0.01 percentage point.[22] 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.[23] Our definition also includes pregnancies that occur after the device has been removed or expelled, when it is unlikely that intention has changed. Grunloh et al. (2013) study the reasons for discontinuation of LARCs and find that about 7% of women are no longer using the

[18] The abortion meeting is defined by diagnosis code Z640. This is similar to the definition used by Janys and Siflinger (2021) except that we do not take the intersection with the actual abortion procedure.

[19] 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%.

[20] 341 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.

[21] While we do observe some insertions and removals of LARCs in the outpatient data, we are missing most of these procedures as they typically take place at a primary care or midwife office. For example, only 6.2% of the LARC prescriptions to nulliparous women can be matched to an insertions. Because of this, we do not rely on outpatient procedure records to identify when women get a LARC inserted and removed. Since pregnancy rates among the subset of women for whom we observe insertions are similar to the whole sample (0.0080 vs. 0.0088), it is safe to assume that most women must be getting the LARC inserted after purchase.

[22] See Appendix Figure A2. In order to calculate a comparable pregnancy rate to the clinical-trail literature, we minimize the contributions of removals and expulsions by calculating the pregnancy rate within three months of the LARC prescription for all women who receive a LARC prescription. In this sample, the pregnancy rate is just under 0.005, similar to the rates found in clinical trials of LARCs.

[23] 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 mechanism that we did not see reported in the clinic-trial literature.

LARC after the first six months and 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. Based on this 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 measurement error. We estimate the effects for larger windows where planned pregnancies are more 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.[24]

Another concern is whether unplanned pregnancies among LARC users are as good as random. It is likely that pregnancies among LARC users 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 and do not find that matching on civil status and education (in addition to age and prescription date) 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 5, 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 birth control prescription and only diverge after treatment assignment.

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

Following Lundborg et al. (2017), we define planned pregnancies as pregnancies resulting from the first IVF treatment. This definition focuses on women who would like to have a child, but have trouble conceiving a child naturally. 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 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

[24] See Section 5.2.2.

that they are below 40 years old at the time of the first treatment.[25] Appendix Figure A1 also shows the distribution of women who receive at least one IVF treatment in a year.

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.[26] The procedure can take place at both public and private fertility clinics, but we only observe procedures from public clinics.

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. 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 in the public sector, since our data on procedures goes back farther (2001) than our prescription data (2005).

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.[27] 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.[28]

3.4 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 31.3 years old, LARC users are slightly older on average while IVF users are older still on average. There is also variation in age within LARC users: IUD users are on average 34.7 years old, while implant users are 28.5 years old on average. These age and cohort differences are also the primary driver of the differences we see in education between LARC users and the population of women, as well as within the two types of LARC users. 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 27% work in

[25] Other criteria includes 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).

[26] Specifically, we use the ATC codes G03GA01, G03GA02, G03GA05, G03GA06, G03GA08.

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

[28] 2,263 of these women are also observed in the labor market data the year before the first IVF treatment. A small fraction of women are not observed in the labor market data, because, for example, they are out of the country.

occupations requiring some college, high theoretical competencies, or entailing managerial responsibilities.[29]

We next describe differences in outcomes between these groups, conditional on birth. 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 details of this definition are provided in the next section), 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).[30] 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 substantially more likely to be smokers and to be snuffing, while women giving birth through IVF are older. In measures of gestational age, size for gestational age, APGAR, and days in the hospital after birth, children slightly less healthy in our population of interest than the overall population.[31]

Table A2 also presents the characteristics of fathers. 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. Nonetheless, it is interesting to note that among women who give birth as a result of unplanned pregnancy, 79% of women have a partner present at birth and one year later, either due to cohabiting with the child's father or because they are married. One reason why the impact of unplanned pregnancy may differ from the impact of a typical pregnancy is the potential for support from the child's father. While there is a 10 percentage point difference in this propensity to have a partner present relative to the population overall, even women who give birth to an unplanned child are very likely to have a partner present.

[29]When comparing births following LARC use to those resulting from IVF, we explore the role of differences in pre-birth characteristics in generating our results by using a propensity-based reweighting in order to adjust the sample of IVF recipients to have characteristics similar to women using LARCs, as described in Section 5.

[30]More specifically, 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 by this probability.

[31]Children born as a result of unplanned pregnancy while using a LARC are slightly more healthy than children born as a result of IVF success, even when the IVF population is re-weighted. Mothers giving birth to unplanned children also take less pregnancy and sickness leave than women giving birth through IVF.

3.5 Balance

In this subsection, we test balance on characteristics for LARC users who had an unplanned pregnancy vs. 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 somewhat in education. Married and divorced women are more likely to have an unplanned pregnancy than single women, possibly due to more sexual activity. We also check the balance matching additionally on civil status.[32] 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.

Table 2 also shows balance for women who already have one child and are LARC users experiencing an unplanned pregnancy and also women who undergo IVF and are successful. We see that the women who experience unplanned pregnancy look extremely similar to women who do not in this population, on all dimensions. 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 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 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 5.2.

4 Empirical Strategy

In this section, we present our empirical strategy. In Section 4.1, we describe how we estimate the impact of unplanned pregnancies on labor market trajectories. This strategy compares outcomes for women who experience an unplanned pregnancy to those who do not. The dynamic effect of unplanned pregnancy combines the effects of unplanned births among LARC users with the effects of future births among women who use a LARC successfully but eventually decide to have children. In order to isolate the effect of children born due to the initial unplanned pregnancy from the effects of later fertility decisions, Section 4.2 develops an IV-GMM strategy for estimating the dynamic impact of unplanned births.

[32] Figures in Section 5.1 document the fact that pre-LARC differences 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.

4.1 Dynamic Effects of Pregnancy

We estimate the impact of a plausibly randomly-timed pregnancy on earnings and related labor market outcomes. 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 age and year. Our primary specification performs the matching using a fully-saturated regression model:[33]

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

where Y_{is} is the outcome of interest (for example, 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.*, $t = s - year_i$). $year_i$ is the calendar year in which individual i received the focal LARC.[34] The $UnplannedPregnancy_i$ indicator is equal to one if woman i had an unplanned pregnancy (see definition in Section 3.3). The second term ensures that we are specifying a fully-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 $(t, age_{i,year_i}, year_i)$. 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 effectively as intended. In some specifications we additionally interact the second term with observables measured the year before getting a LARC ($X_{i,year_i}$) to corroborate the robustness of our estimates. We estimate the impact of a "planned" pregnancy in a similar way.[35]

[33] Performing matching via a fully-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.

[34] 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.

[35] The model for planned pregnancies is

$$\begin{aligned} Y_{is} = & \sum_{t=-7}^{7} \alpha_t^{IVF} \mathbf{1}\left[t = s - year_i\right] PlannedPregnancy_i \\ & + \sum_{t=-7}^{7} \sum_{t,y,j,l} \delta_{t,y,j,l,k}^{IVF} \mathbf{1}\left[t = s - year_i\right] \mathbf{1}\left[y = year_i\right] \mathbf{1}\left[j = age_{i,year(i)}\right] \mathbf{1}\left[l = YearsSinceContra_{i,t=0}\right] \mathbf{1}\left[k = CollDegree_{i,t=-1}\right] \\ & + \varepsilon_{is}, \end{aligned}$$

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 $age_{i,t=0}$ at first IVF treatment, years since last contraception at time of first IVF treatment ($YearsSinceContra_{i,t=0}$), and

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 can implicitly test this identifying assumption as Y_{is} is observed for all women before getting a LARC ($s < year_i$ or $t < 0$). If early pregnancies are as-good-as randomly assigned conditional on age and year of LARC, then we would expect that $\alpha_{-7}^{LARC} = ... = \alpha_{-1}^{LARC} = 0$. The parameters α_t^{LARC}, $t \geq 0$ give the dynamic impact of an unplanned pregnancy on labor market outcomes.

Equation (1) measures the effect of an unplanned pregnancy compared to the counterfactual fertility path the women would have chosen. Women who have an unplanned pregnancy may have an abortion and possibly a subsequent birth, while women in the control group may have later pregnancies. Figure 1 shows a diagram of the dynamics of fertility decisions among LARC users.[36] Since there is dynamic non-compliance in both the "treatment" and "control" groups, α_t^{LARC} does not represent the effect of treatment (childbirth). Instead, 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. Hence, α_t^{LARC} is the parameter of interest for understanding the effect of unplanned pregnancies for women who get a LARC. It is informative for the value of having access to effective long-acting contraceptives. It is challenging, though, to compare these estimates to estimates for other groups of women whose compliance may be quite different. For example, we find that compliance is identical by age in the first few years after the LARC, but starts diverging after $t = 4$ and by $t = 7$ the rate of first childbirth in the control group is around 15 percentage points higher for younger women (age 22-27) compared to older women (age 28 or older) – the younger (older) women in the control group are around 45 (60) percentage points less likely to have a biological child seven years after the LARC. Since LARCs typically are intended to work for three years, and sometimes up to five years, this pattern validates that the divergence – potentially due to higher fertility among younger women – does not happen until after the focal LARC expired. Importantly, more children in the control group will lead to attenuated estimates in the long-run. On the one hand, if a younger group is the focal group of women, then this is the parameter of interest. On the other hand, it is difficult to compare estimates between different groups as differences may be due to differences in the impact of children on women's careers, differences in the abortion rate, or differences in the behavior or composition of the control group. In the next section, we describe our methodology for estimating the dynamic impact of *birth* on labor market outcomes.

pre-IVF college degree ($CollDegree_{i,t=-1}$). We match on additional observables as the pre-IVF estimates $\alpha_{-7}^{IVF} = ... = \alpha_{-1}^{IVF}$ are not balanced otherwise. For more details, see discussion about IVF setting in Section 3.3.

[36] Appendix Figure A4 shows the corresponding diagram of the dynamics of the compliers for the IVF treatment.

4.2 Dynamic Treatment Effect of Childbirth

In this section, we describe our instrumental variable estimation strategy to causally identify the impact of shifting the timing of first childbirth.

Let the labor market outcome of woman i in calendar year s be given by:

$$Y_{is} = \sum_{\tau=1}^{T} \rho_{i\tau} \mathbf{1}[\tau = s - s_i + 1] + f(\mathbf{X}_{is}) + \eta_{is}, \tag{2}$$

where $\rho_{i\tau}$ is the impact of having the first child of age $\tau \equiv s - s_i + 1$, s_i is the calendar year of first birth, and X_{is} is a vector of pre-determined characteristics that affect productivity and choices, for example age and year-of-prescription fixed effects. In this model, $\rho_{i\tau}$ is the impact of the first child on Y relative to having no children, τ years after the first child is born. This specification allows for dynamic treatment effects, since we flexibly allow $\rho_{i\tau}$ to vary with time since first birth.[37]

We start by adapting the standard potential outcomes notation for a setting with dynamic treatment effects and dynamic non-compliance (see Figure 1 for a diagram of the dynamic compliance and notation on treatment). We define treatment to be the birth of the first child. In our setting treatment is irreversible and, hence, individuals can only be treated once.[38] 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. 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. In other words, let $\tau = 0$ denote an individual who has not yet received treatment, $\tau = 1$ denotes treatment in the current period, and $\tau = 2$ denotes treatment in the previous period. Furthermore, let T_{it} equal to one if individual i had their first child (received treatment) in period t. Finally, assume that there is an instrument Z_i that affects treatment assignment in the first period $t = 1$.

Let an unplanned pregnancy after a LARC prescription be a valid instrument Z_i that shifts the timing of the first birth, such that women with $Z_i = 1$ are more likely have a child in period $t = 1$. We assume our instrument is valid conditional on age and year at the time of getting a LARC. In the following discussion, the matching of women by age and year is kept implicit.

Before showing identification, we adapt the standard instrumental variable assumptions to the dynamic setting. In what follows, we focus on compliance from the point of view of the first period, where we have compliers, never takers, and always takers with respect to choices in the first period. The main difference with respect to the static setting is that the exclusion restriction needs to be extended to include both labor

[37]Later in this section, we analyze heterogeneity in dynamic treatment effects by whether the first child is planned and by pre-determined characteristics like age, $\rho_{i\tau}(X_{i,t=-1})$.

[38]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.

market dynamics and future treatment trajectories.

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

The independence assumption is that an unplanned pregnancy among LARC users is as-good-as-randomly assigned in the first period ($t = 1$).

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

Relevance requires that unplanned pregnancies are correlated with unplanned births in the first period.

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

While the instrument changes the probability of pregnancy in the first period, 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 need to assume that abortion does not affect the labor market outcomes and fertility trajectories of women who have an abortion following an unplanned pregnancy (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 5.2.

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

The monotonicity assumption is that any woman with a unplanned pregnancy is more likely to have an unplanned birth in the first period.

A5 : **No anticipation:** $Y_{it}(\tau = 0, T_{i1} = 0) \perp \{T_{i,t+1}, T_{i,t+2}, \ldots\}$

Pre-treatment outcomes do not vary by timing of future treatment. This assumption rules out some forms of planning, namely those forms of planning in which outcomes depend on years from birth, such as switching to lower-paying and more flexible jobs in anticipation of future childbirth. For unplanned births, the no anticipation assumption is reasonable, but may appear strong for planned births. Some forms of planning do not have anticipatory effects, however, such as moving to jobs or positions where the cost of time away is lower, as long as these jobs do not have lower pay before children are born. In the IVF setting, no-anticipation is reasonable assuming that anticipatory changes have already been made by the time they start receiving their first IVF treatment. In the IVF setting, we also match on time since last contraception prescription to compare women who are at the same stage of planning.

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

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

where we use the no-anticipation 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) is the derivation of the Wald estimator for $t = 1$ which gives us the first year impact ($\tau = 1$) of having a child for compliers with notation that can account for multiple time periods and impact dynamics,

$$\rho_{\tau=1} = \text{Wald}_{t=1} \equiv \frac{\mathbb{E}[Y_{i1}|Z=1] - \mathbb{E}[Y_{i1}|Z=0]}{P\left[T_{i1}=1|Z=1\right] - P\left[T_{i1}=1|Z=0\right]} = \text{LATE}_{\tau=1}. \tag{4}$$

Next, consider the reduced form for $t = 2$ (the second year after getting a LARC), 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

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

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

If we re-arrange equation (5) to solve for the Wald estimator in period 2, we get the LATE for $\tau = 2$ plus an additional bias term representing the women who have first childbirth in period 2 (period 2 treated).

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

In order to estimate the $\text{LATE}_{\tau=2}$, we need to correct the $\text{Wald}_{t=2}$ estimator for the second term in equation (6). One simple and sufficient solution is to assume homogeneous treatment effects and homogeneous fertility rates after the first period. The necessary conditions are (1) ρ_1 is the same for compliers and for the subset of compliers who are treated in the second period ($E\left[Y_{i2}^{1}-Y_{i2}^{0}|T_{i2}=1,T_{i1}^{1}>T_{i1}^{0}\right]=E\left[Y_{i1}^{1}-Y_{i1}^{0}|T_{i1}^{1}>T_{i1}^{0}\right]$) and (2) the average rate of childbirth (treatment) in period two is the same for the compliers and never takers ($P[T_{i2}=1|T_{i1}=0,T_{i1}^{1}>T_{i1}^{0}]=P[T_{i2}=1|T_{i1}=0]$).[39] Under these two assumptions, we can correct equation (6) using our estimate of $\hat{\rho}_1$ from $t=1$ and the observed fraction of treated in the second period $\hat{P}[T_{i2}=1]$.

This is related to the recursive estimator of Cellini et al. (2010),

$$\hat{\rho}_2^{recursive}\equiv\widehat{\text{Wald}}_{t=2}+\hat{\rho}_1\hat{P}[T_{i2}=1]. \tag{7}$$

We can repeat the derivation for $t=3$, where we will have two terms that bias the Wald estimator: one for those that received treatment in period 2 and another for those that receive treatment in period 3. Under similar assumptions, the recursive estimator for $t=3$ is

$$\hat{\rho}_3^{recursive}\equiv\widehat{\text{Wald}}_{t=3}+\hat{\rho}_2^{recursive}\hat{P}[T_{i2}=1]+\hat{\rho}_1\hat{P}[T_{i3}=1]. \tag{8}$$

Notice that $\hat{\rho}_\tau^{recursive}$ estimated from earlier periods are used to correct the later period estimates. This recursive procedure suffers from accumulated sampling noise. Instead, we develop a dynamic IV-GMM estimator that estimates $\hat{\rho}_\tau$ jointly.

The baseline dynamic IV-GMM estimator is

$$g_{it}(\theta) \quad = \quad Z_i\left(Y_{it}-\sum_{\tau=0}^{T}\rho_\tau\mathbf{1}[\tau=t-t_i^b]\right). \tag{9}$$

In the traditional just-identified IV-GMM estimator there are as many equations as instruments. In our setting, we have one instrument, but we have multiple observations of Y_{it}. Due to the dynamic effects of

[39] Note that for IVF this assumption is fulfilled because there are no never-takers. In the LARC setting, we can test this assumption.

pregnancy on outcomes, the different ρ_τ are identified, as described above.[40]

In this framework, we can jointly estimate heterogeneous ρ_τ based on observables (*e.g.* age) or using different samples of compliers. For example, in most specifications we jointly estimate heterogeneous ρ_τ using both IVF and LARC settings, where a planned ρ_τ^p applies to IVF births and "planned" post-LARC births, and an unplanned ρ_τ^u only applies to unplanned pregnancies in the first period. The joint LARC-IVF IV-GMM estimator is

$$g_i(\theta) = \begin{cases} Z_i^{IVF}\left(Y_{it} - \sum_{\tau=0}^{T}\rho_\tau^p \mathbf{1}[\tau = t - t_i^b, b_i = p]\right) \\ Z_i^{LARC}\left(Y_{it} - \sum_{\tau=0}^{T}\rho_\tau^u \mathbf{1}[\tau = t - t_i^b, b_i = u] - \sum_{\tau=0}^{T}\rho_\tau^p \mathbf{1}[\tau = t - t_i^b, b_i = p]\right) \end{cases}, \quad (10)$$

where the first (second) set of moments use data from the IVF (LARC) setting, $b_i = u$ refers to unplanned births among LARC users, and $b_i = p$ refers to all other LARC births and IVF births.

Lastly, the data need to be prepared before using the GMM estimators described earlier. First, the LARC instrument is valid after matching women on age and year of treatment.[41] We perform the matching by demeaning the outcomes and indicators in each (age) X (year) X (time 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 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.[42] Second, we can jointly estimate effects of

[40]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.

[41]The data preparation for the IVF groups is similar, except that women are additionally matched on education and time since last contraception. When we estimate the effect of second birth, we additionally match on age of first child at the time of LARC.

[42]Blandhol et al. (2022) 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

planned and unplanned births by stacking the LARC and IVF data and using both instruments simultaneously.[43] Finally, we can easily estimate the model for different age sub-samples.

5 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 who receive IUDs or birth control implants in the seven years before and after they receive the LARC, comparing those who have a pregnancy soon after receiving this birth control to those who do not, and controlling for age and year of prescription. In order to meaningfully compare the impact of children by characteristics of the mother and the birth—for example, to compare the impact of unplanned birth by the age of the mother—we next implement an IV strategy to obtain estimates of the impact of childbirth on labor market outcomes. We focus our main results on five outcome variables: earnings including paid leave, employment, the propensity to work in skilled occupations, the propensity to receive a promotion, and the birth of a second child. We present additional results on labor market income, other job attributes such as flexibility in setting schedules, as well as hours and wages in order to understand the drivers of the patterns we see in the primary outcomes.

5.1 Effect of Unplanned Pregnancy on Earnings and Occupation

In this subsection, we restrict attention to women who had a LARC prescription. Figure 2b plots the first stage effect: women who have an unplanned pregnancy are about 70% more likely than women who do not have an unplanned pregnancy to have a child by two years after their LARC prescription. However, between years three and seven after the prescription, many women in the control group go on to have planned births. By year seven after LARC prescription, 40% of women who did not have an unplanned pregnancy (women in the "control" group) have become mothers.[44]

Unplanned pregnancy has a large impact on earnings and is not predicted by pre-earnings conditional on age and date of prescription. Figure 2c plots the evolution of income around the year of LARC prescription for women who had an unplanned pregnancy relative to women of the same age who did not have an unplanned pregnancy but purchased a LARC at the same time.[45] The left-hand side presents raw means, re-weighting

never-takers with negative weights.

[43] 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.

[44] 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.

[45] Our preferred definition of income includes both labor market income and paid leave due to sickness, pregnancy, maternity

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). We further break the treatment group into those who have an abortion and those who give birth following an unplanned pregnancy. As documented in Figure 2, about three-quarters of women who experience an unplanned pregnancy go on to give birth to the child. Those who have an abortion are selected relative to those who give birth. Those who have an abortion have slightly lower earnings, are in lower-skilled occupations, more likely to have had recent wage growth as measured by a promotion relative to women who have an unplanned birth. We also see that immediately following unplanned pregnancy, the labor market paths of women who have the child diverge from both the control group and from women who have an abortion. While the latter groups' income, employment, and occupation trajectory evolve smoothly around the unplanned pregnancy, there is an immediate drop in income and employment among women who give birth.[46]

The overall impacts of an unplanned pregnancy on income including paid leave, employment, occupation, and promotion are depicted on the right hand side of Figure 2 and 3, estimated using equation (1), along with 95% confidence intervals. Prior to the LARC prescription, women who experience an unplanned pregnancy are on the same income trajectory as those who do not. After an unplanned pregnancy, women experience large earnings declines. The difference shrinks over time but is economically meaningful throughout our window and is equal to about 25,000 SEK (US$ 2,700) six years after prescription, or a 20% impact of unplanned pregnancy income (see Appendix Figure A5b for percentage calculations). Employment falls only temporarily with unplanned pregnancy. Six years after prescription, 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.

Though employment rates are not affected by six years after an unplanned pregnancy, an unplanned pregnancy substantially reduces the likelihood that women advance in the career ladder, as measured by the skill requirements of their job. Women experiencing an unplanned pregnancy are less likely to be working in occupations requiring medium, high, or management skills relative to the control group. In Figure 3b we see that by six years after an unplanned pregnancy, women are around 20 percentage points less likely to be in occupations which are medium- or high-skilled, compared to the counterfactual. It is not the case that women who experience an unplanned pregnancy move into lower-skilled occupations. Instead, women who experience an unplanned pregnancy do not advance in the career ladder at the same rate as the control

leave, and parental leave. This is also closest to the definition used in Kleven et al. (2019a). In the Appendix, we also present the effect of unplanned pregnancy on raw labor market earnings.

[46] The abortion group overall is small, making the estimates for women getting abortions quite noisy. We observe 101 women having abortions in total. By seven years after the LARC, there are only 25 observations due to the panel structure of our data.

group (see Figure 3a).[47]

To give more context, 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.[48] Tables A3 and A4 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 A4 shows that 15% (13%) 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 around twice as many women 28% (26%) from age 28 to age 34. Persistence is around three times as high for the service, care, and security occupation. Second, most occupation switches are associated with occupation upgrading and enrollment in education acts as a mediator for occupation upgrades – especially for younger women.[49] 44% switch to an occupation with higher skill requirements from age 22 to age 28, 52% of those who had been enrolled in education during the period, and 98% of women who switched occupation following an education spell upgraded. The bottom panel of Table A4 shows that these numbers are even higher for the most common occupation with low skill requirements: 65% who switch occupation upgrade and this number is eight percentage points higher for women who were enrolled in education. Table A5 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). Finally, Table A3 corroborates that the women who have an unplanned pregnancy are more likely to stay in the same 3-digit occupation five years after LARC, and are thereby forgoing valuable occupation upgrades.[50] 32% (43%) of those who had (did not have) an unplanned pregnancy switched to occupations with higher skill requirements. While enrollment in education means much higher chance of occupation upgrade for the control group – nine percentage points higher likelihood of upgrading, compared to only three percentage points for those who had unplanned pregnancies.[51] 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.

[47] As displayed in Appendix Figure A6, women with unplanned pregnancies are more likely to work in occupations which offer flexibility as measured by the Bang (2022) extension of the Goldin (2014) flexibility index (though this contrast is only significant at the 10 percent level). More flexible jobs feature less time-pressure, less interaction with clients, and are more structured for the worker – thus there are presumably more coworker substitutes. Goldin (2014) provides a detailed description of these measures.

[48] 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.

[49] 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.

[50] These differences are not as pronounced for our IVF sample.

[51] In research studying career trajectories of women around motherhood, Hotz et al. (2017) also emphasize a lack of occupation upgrading in Sweden as a primary mechanism.

As another measure of progress in the career ladder, we study the rate of promotions around unplanned pregnancy. We measure promotion, following Bronson and Thoursie (2021), as the propensity to receive income in a given year which is fifteen percent or more larger than their previous maximum annual income.[52] We see a significant negative impact on the propensity to receive a promotion in the years following an unplanned pregnancy, but in the later years of our window we see (insignificant) positive effects on promotion. These dynamics suggest that eventually, it is possible that women who experience an unplanned pregnancy catch up relative to their counterfactual. However, these trends are difficult to interpret if the control group is having their first child—with the associated drop in promotion propensity—in the later years of our window. We present estimates removing these effects in the next subsection. We are limited in the ability to decompose earnings into changes in wages vs. changes in hours, because only a subset of women appear in the data which records wages, and women are much less likely to be in these data around motherhood. Appendix Figure A7 plots our measure of wages and hours, as well as an indicator for whether data on wages and hours are missing.[53] 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 are driving the impact of unplanned pregnancy on earnings.

Finally, we note that the effect of an unplanned pregnancy for women without children includes the possibility that having one child increases the probability of having more than one child. As depicted in Figure 3f, women who experience an unplanned pregnancy are almost 40 percentage points more likely to have a second child by five years after the initial LARC prescription. Table A2 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 very high–80 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.[54] These patterns suggest that women who experience an unplanned pregnancy do typically have a relationship with the father of the child and do typically go on to have more children soon after the birth of their first child.

One primary 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

[52] We use income, but Bronson and Thoursie (2021) use wages. We prefer income since wages are not observed in all years for all working women. See Appendix B.4 for details.

[53] Please see data appendix for details concerning the wage data.

[54] The data do not measure cohabitation unless the couple shares a child, so it is not possible for us to compare this rate to the control group we use for other outcome variables.

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 to those who are not. When we do this, we see that the results are extremely similar between the two groups—the difference between unmarried treatment and control is similar to the difference between married treatment and control, despite the fact that the unmarried treatment women are perhaps more likely to be in a relationship compared to unmarried control women (while there is no difference in relationship status between married treatment and married control women). This is displayed in Figure 4d. To assuage related concerns about the robustness of our results, Figure 4e sequentially adds controls for educational attainment, civil status, number of people in the household, indicators of mental health, and earnings measured in the year before the LARC prescription.[55] The controls do not meaningfully shift any of our estimates of the effects of unplanned pregnancy. Table 2 showed imbalance between women who have an unplanned pregnancy and women who do not on the probability of being married. Thus, Figure 4b and Appendix Figure A8 additionally match on civil status and education in order to compare the trajectories of more similar women. We find that these additional matching variables do not affect our estimates of the effect of unplanned pregnancy, and do not alter pre-LARC labor market differences.

How do the results differ when we consider women who have already had a child? Figure 5 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. Panel (a) suggests that the average earnings effect is a persistent earnings reduction of five to ten percent, and panel (b) suggests that employment may also be reduced by about five percent over our time horizon. In contrast to the main results, we see no effect on occupation for mo. We see a similar path for promotion probabilities relative to the main results. Overall, the data suggest substantially smaller effects of unplanned second children compared to unplanned first children.

5.2 Dynamic Impacts of Unplanned Births

How do the circumstances of an unplanned birth affect outcomes? To answer this question, we estimate the dynamic impact of childbirth (using pregnancy as an instrument for birth) relative to a counterfactual in which a woman does not have a child. First, we multiply the estimates of the impact of pregnancy by the inverse of the fraction of compliers to get the Wald estimate of the impact of unplanned childbirth.

[55]The indicators we use for health are whether or not a woman had a prescription for antidepressants, number of days on sick leave, and whether the woman had a diagnosis of depression or anxiety disorder in the year before the LARC prescription.

These estimates are interesting in and of themselves, but comparing the impact of children on women's labor market outcomes at different times in her life is difficult in the case that *counterfactual* behaviour differs. To get estimates of the dynamic impact of children relative to a counterfactual of not having a child, we need remove the impact of later children in the control group from the estimates.

In order to understand why these steps are helpful in comparing the impact of children in different circumstances, it is useful to consider a specific example. One source of heterogeneity in the circumstances of childbirth is whether the child was planned or unplanned. To study the impact of children on women's careers when children are planned, we can use plausibly random timing of the success of IVF procedures, as in Lundborg et al. (2017), to estimate the impact of "planned" childbirth. Appendix Figure A9a plots the first stage effect: women who have a successful first IVF treatment are about 70% more likely than women who do not have an unplanned pregnancy to have a child by the first year after their initial IVF treatment. However, within the next two years almost 40% of women who had an initial IVF failure go on to have their first child. Overall, we find that women who experience an initial IVF success have modest income and employment declines in the year following the IVF procedure, but these differences shrink to zero three years after the procedure (Appendix Figures A10 and A11).[56] We also note that women who have successful IVF procedures are likely to go on to have more children, though the longer term difference between treatment and control in the probability of having two children (or more) is completely explained by women who do not have any children.[57] This means that women in the control group who eventually have children have similar fertility to women in the treatment group. Given the high success rate of future IVF procedures for women who had an initial failure, the lack of a difference does not imply that the labor market impact of planned motherhood is zero, as these later births in the control group generate large earnings declines themselves.

In this section, we 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. To do this, we make several assumptions. First, we assume that unplanned pregnancy is a valid instrument for an unplanned birth. Second 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,

[56] Because the probability of IVF success is high and our data include almost ten thousand IVF procedures (for childless 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 earning (Figure A10a and A10b) and are about two percentage points more likely to be employed (Figures A10c and A10d). In addition, women with a successful first treatment are also more likely to be in occupations requiring high, medium, or management skills (Figures A11a and A11b). Half of this difference is explained by the employment difference. 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 also see that the groups are imbalanced on education in Table 2. We present estimates of the impact of IVF success on earnings both matching only on age, time since contraception, and year of IVF, (in black) and also matching additionally on education (in blue). Appendix Figure A12 plots the reduced form effects on wage outcomes.

[57] We note that a successful initial IVF procedure is *unlikely* to result in twins. Swedish procedure is to implant only one embryo during our data window (Bhalotra et al., 2022).

women may negotiate a different schedule with their boss before pregnancy. Third, we assume (as a baseline) that IVF success is a valid instrument for planned birth.

These assumptions are not trivial. 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. As discussed above, in the left hand panels of Figures 2 and 3, we generally 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. 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 (2021) 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.

There are some exclusion restriction concerns with the IVF instrument, so we use this as a baseline measure of the effect of planned birth, but also consider robustness to other estimates of the impact of planned childbirth from, for example, the event-study specification of Kleven et al. (2019a). In the case of IVF success and failure, the exclusion restriction means that IVF failure does not itself affect labor market outcomes of women.[58] In the conditional means in Appendix Figure A10a, we do see discontinuities at the time of IVF failure, but these arise because most women who have a failure in their first attempt go on to try again and many become pregnant in their second attempt. In Appendix Figure A13b, we plot the path of women who undergo a first IVF procedure at event time 0, but who do not yet have a successful pregnancy. We see that there seems to be a deviation from earnings trend even among women who do not have children. Some of this may reflect selection (endogenously choosing not to undergo additional fertility procedures), but this may also reflect a negative impact of infertility on earnings. Due to this, we do not definitively conclude that the IVF setting identifies the impact of children who are planned and show robustness to alternatives.

Figure 6 plots the TT impact of children on women's earnings over six years, estimated as in equation (10).[59] Each figure presents estimates of ρ_t^u, in red, alongside 95% confidence intervals. For earnings, we see substantial negative impacts of unplanned birth. The earnings impact of an unplanned birth is around 30% of counterfactual earnings compared to not having a child. Women who have an unplanned birth are almost 30% less likely to be in a medium/high skilled occupation by five years after birth, relative to a counterfactual in which that woman did not have a child. As in the reduced form, we see no lasting impact on employment

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

[59]We pool the last two years because we have few observations in the seventh period. Figure 4f shows robustness of the unplanned pregnancy effects to this right censoring. When only including the women we observe at $t = 7$ (red line) or $t = 6$ (magenta line) the labor market effects are larger than when including all women.

or promotion probabilities. We also see that women are more likely to have subsequent children following an unplanned birth, and this explains some of the earnings effect.

Next, we consider whether the effects of unplanned look different than planned births. In Figure 7, we plot the estimates of unplanned birth alongside the estimates for planned birth (in purple). For earnings, we see negative impacts of unplanned birth relative to planned birth: a 30% reduction relative to no children in the case of unplanned births, and only a 10% reduction relative to the counterfactual for planned births. We also see that there is no impact of planned birth on occupation. This is not explained by differential employment between the groups. In addition, we do not see substantial differences in subsequent fertility among women whose first child is planned vs. unplanned, though women who have a planned birth are somewhat more likely to have an additional child by four years following their first birth.

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, of course, are endogenous to planning, but we still may be interested in exploring whether the substantial age and occupation differences between the LARC and IVF groups drive the results. We use a DiNardo et al. (1996) propensity score re-weighting to compare estimates while holding characteristics fixed. In particular, we reweight women undergoing IVF procedures by the relative probability that a woman with her characteristics appears in the set of women receiving LARCs relative to the set of women undergoing IVF.[60] Figure A14 displays 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—we discuss below (under "Robustness") how assumptions concerning the impact of planned birth impact our estimates of the impact of unplanned birth, finding our assumptions about ρ^p do not much affect our estimates of ρ^u. 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.

One potential reason that planned births, as estimated from initial IVF success, have a small impact on earnings relative to unplanned births is that women who undergo IVF procedures already want children

[60] The characteristics we use to form this relative propensity are: age indicators, occupation indicators, earnings, and civil status indicators all as measured in period -1.

and the impacts of (anticipated) children on their careers occur *before* treatment assignment in both the treatment and control group. For example, women may move to the public sector, move closer to work, or move closer to their parents once they decide to start trying to have children. The impact of these decisions on women's careers would not be picked up in the comparison of women who have an initial IVF success or failure because women attempting IVF have already made changes in their life conducive to having children. Alternatively, all women who decide to undergo IVF may have experienced income shocks which led them to plan to have children. For this reason, it is difficult to directly compare estimates of planned and unplanned children.

5.2.1 When is Unplanned Birth Most Disruptive?

Central to understanding demographic shifts in the past fifty years is understanding the extent to which delaying pregnancy reduces the cumulative impact of children on women's careers. 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 mechanically younger at the time of their first pregnancy than the women in the control group. The first effect—the mitigating effect of planning—was discussed in the previous section. Next, we consider whether there are 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. In particular, when we study the impact of unplanned pregnancy among young women, we find that 15% of the control group go on to have children by seven years after the initial birth control prescription. These women mechanically cannot be in the set of older, childless LARC users. However, we consider this birth rate in the control group sufficiently small that our setting comes close to the ideal experiment.[61] One might also worry that what drives any differences across groups is the characteristics of compliers. If we had different rates of abortions across younger vs. older women, then we might infer that differences in the LATE estimates are driven by composition rather than treatment effects. In fact, abortion rates are similar across these groups (see Appendix Figure A15a). Given how similar the sample of young women is to older women, and how similar the compliance rate is between these groups, we focus on differences in treatment

[61] In Appendix Table A6 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. We discuss this further below.

effects below and interpret these as the effect of children at different times in a woman's life.

Appendix Figure A16 presents reduced form impacts of unplanned pregnancy by age (younger vs. older women) and human capital investments (whether the women are enrolled in education vs. not enrolled the year before prescription), and Table 3 presents our estimates of the average and cumulative effect of unplanned birth on labor market outcomes overall, as well as heterogeneity by age and by human capital investments. In the analysis for 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.[62] The odd columns of Table 3 come from our baseline IV-GMM estimation, while the even columns add controls for the average pre-treatment outcome in the three years before LARC prescription in order to further increase precision. Point estimates suggest that younger women have earnings losses which are 32-33% lower than counterfactual earnings, while older women have earnings losses only 13-16% lower than counterfactual earnings. These differences by age are marginally significant at the ten percent level with controls. We also find large differences in the levels of other outcomes, though in general the estimates are noisy (especially for older women). This age heterogeneity is unlikely to be driven by differences in who the compliers are. To reduce the role of heterogeneity in the composition of these groups defined by age, we re-weight towards those in the younger group who would eventually show up in the older group (those who would not have a child before they are 28 unless they have an unplanned pregnancy). Appendix Table A6 demonstrates that estimates of the impact of children are not significantly affected by this re-weighting, consistent with an empirically small fraction of the young LARC users having 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 which are 17-20% 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. Point estimates also suggest fewer years of employment and fewer promotions, but these differences are not statistically significant.[63]

[62]The average effect is an estimate of $\bar{\rho}$ (the impact of children after the parental leave period ends), restricting coefficients so that $\bar{\rho} = \rho_2 = \rho_3 = ... = \rho_7$. We allow ρ_1 (the impact of a newborn) to differ from impacts of children in later years.

[63]Within the subset of women undergoing IVF, Appendix Table A7 documents patterns by age and enrollment in education which are similar in sign but muted compared to the differences for women experiencing unplanned pregnancies. We note the challenge of interpreting comparisons of treatment effects across women deciding to undergo IVF at different times in their life: it is difficult to believe that they are similar to one another (and indeed they differ substantially on observable characteristics). Conditioning on the decision to become pregnant at different times is quite different than the ideal experiment we present above: the case of a LARC user who makes the decision to delay pregnancy for many years, but can have an unplanned pregnancy at any time.Nonetheless, we report the results in Appendix Table A7 for completeness. The impact of planned birth for women who are enrolled in education are larger than for women who are not enrolled, but not significantly so. In fact, the impact of planned birth among enrolled women is similar in magnitude to the impact of unplanned birth for women who are not enrolled

5.2.2 Robustness

Our measures of the impact of unplanned pregnancy (and birth) are valid under a number of assumptions. In our main estimates, we assume that all pregnancies are unplanned when conception occurs within nine months of receiving a LARC prescription.[64] Figure 4a plots the impact of unplanned pregnancy defined by a conception following a window of 3, 6, 9, 12, and 18 months of a LARC prescription.[65] For early windows, period 0 includes a non-trivial fraction of births. In the year before a LARC prescription, there are some (insignificant) differences between the estimates by the timing of pregnancy post-prescription. The three month window is close to the nine-month window, but there are larger differences in the six month window. Following nine months, there is a small monotonic increase in the pre-prescription difference between treatment and control. This is suggestive that planned births become an increasing fraction of conceptions as the window following LARC purchase gets larger, and that some planned births are selected. The differences are difficult to interpret however, in light of very large standard errors, especially for short windows. Incorporating the first stage, Figure 8a plots the effect of an unplanned birth across the windows. They are not completely monotonic—shorter windows suggest similar but (mostly) larger effects relative to the nine month baseline.

We can also directly model the possibility that at the nine-month horizon, some children may be planned. To do this, we extrapolate from the rate of pregnancy at the three-month window, what the expected rate of pregnancy would be at nine months, assuming a constant rate of pregnancy. This will include many of the pregnancies that occur due to removals for medical reasons. Our conservative calculation suggests that we have 30% more pregnancies in the nine month window than we would have expected based on the rate of pregnancy in the three month window. We model this by weighting each observation in the IV-GMM estimation by the probability that it was unplanned according to the timing of pregnancy, and using estimates from the weighted IVF sample for the impact of planned births.[66] This gives us the green line in Figure 8d, which implies the impact of unplanned pregnancy might be larger than our baseline. This is because there is already a large difference between the impact of IVF births and LARC births on earnings, and we wedge the two further apart by assuming the baseline estimate is a weighted average. To the extent that we are misclassifying some pregnancies as unplanned when they are in fact planned, we believe that our baseline estimates understate the impact of unplanned births on womens' careers.

Another assumption underlying our baseline estimates is that the timing of IVF success can be used to

in education.

[64]We restrict to conceptions that happen at least two weeks after LARC prescription in order to ensure that we do not mistakenly code LARC prescriptions which occur after an abortion as resulting in an unplanned pregnancy.

[65]Appendix A17 presents the same for other outcomes.

[66]By "weighted IVF sample" we refer to the estimates in Figure A14.

identify the impact of planned children on women's labor market outcomes. Figure 8c 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) measures 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. 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.

Finally, Figures 4b, 4e, and 8b demonstrate the robustness of our unplanned birth and pregnancy effects on our main outcome (earnings including paid leave) to matching on additional characteristics and to including a rich set of additional controls. Neither matching on education and civil status nor adding pre-determined controls alter any of our results. Similarly, Appendix Figures A8 and A18 present the impact of additional matching and controls for the other main outcomes.

6 Discussion

In this section, we discuss the implications of our estimates for the broader understanding of the impact of children on careers. First, we extrapolate from our estimates to the overall population of women. Second, we relate our estimates to past work in the area. Finally, we discuss how our estimates better help us understand the trade-offs women make when planning their fertility.

In our data, the raw gender earnings gap is 31.5% in 2005.[67] What portion of this overall gap is driven by the effects of children on women's careers? For women in the age range for which we observe first births (women age 22-43), we estimate an average impact of first birth of -18% in the first six years after birth. This estimate comes from taking the treatment effect of planned and unplanned children, by age as in Tables 3 and A7 columns (4) and (6), weighted by the probability of planned and unplanned first childbirths in Sweden by age in 2005.[68] Separating planned and unplanned pregnancies, we find the overall impact of unplanned births is -25% and the overall impact of planned births is -16%.

Our estimates of the impact of unplanned births are similar to estimates of the effect of motherhood on earnings using observational data on all births, but our estimates of the planned penalty are smaller

[67] 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%.

[68] For first births at age a, we assign a fraction x_a as unplanned and $(1 - x_a)$ as planned based on the rate that mothers report unwanted or mistimed births in the US National Family Growth Survey 2011-2019. We could not find a source for the fraction of first births which are unplanned by age in Sweden, but presume that, in our age range, the estimates are similar in Sweden and the US.

than descriptive estimates, even though most births are planned.[69] Using event study regressions around first birth, Angelov et al. (2016) estimate a long-term within-couple divergence in earnings attributable to children of 32 percentage points in Sweden, while Kleven et al. (2019b) find a motherhood penalty of 26% in Sweden.[70]

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.[71] If women strategically time childbirth, it becomes difficult to interpret estimates from event studies causally, as discussed in Bensnes et al. (2023).

Finally, we consider what the gender pay gap would look like were women to continue having children according to the 1970's age distribution at first birth. We assume that 38% of births are unplanned, which is based on the NSFG 1973 estimate for ever-married women – the only women surveyed (Williams and Pratt, 1990).[72] Shifting women's timing and planning to be as they were in Sweden in 1970, the impact of children on earnings would be 26.2%, instead of our estimate of 18% overall. Both improvements in birth control technology and in the labor market opportunities available to women are likely responsible for the substantial demographic changes in age at first birth. These demographic changes are associated with a meaningful reduction in the impact of children on women's careers.

7 Conclusion

In this paper we investigate a natural experiment in which women who were using long-acting reversible contraceptives (IUDs and implants) became 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) which make identification challenging. We document that empirically there are no labor market differences between women who become pregnant while taking a LARC and those who do not in the years before they purchased the LARC, conditional only on

[69]See Appendix Figure A19 for the fraction of births which are unplanned by age and the distribution of age at first birth.

[70]This estimate relies on the identifying assumption that the timing of birth is orthogonal to characteristics that affect earnings conditional on age and calendar year. This estimate excludes parental leave payments (unlike our estimate) so a better comparison may be their estimate from Danish data of 21%, which includes parental leave payments.

[71]Of course, this may be driven by other differences between women having planned and unplanned children, we only note that the pattern is consistent with avoiding giving birth at costly times in the life-cycle.

[72]Likely, this is an underestimate both due to sample selection and due to norms surrounding reporting of a child as mistimed or unwanted.

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 following birth, and women who have unplanned births are also less likely to advance to more skilled occupations.

When we use an IV strategy to study the impact of unplanned children on women's careers, we find long term earnings impacts of about 25%, as well as large reductions in the propensity to be in medium or high skilled occupations. The effects of children on women's careers are substantially larger when women are younger at the time of the unplanned pregnancy, as well as when they are enrolled in education at the time of the unplanned pregnancy.

Finally, these impacts are not similar to what we find in another setting with exogenous timing in births: success or failure in the first IVF treatment. Women taking LARCs and women undergoing IVF fertility treatments differ in their intentions concerning childbirth. In one case, a woman would like to avoid becoming pregnant, in the other case, a woman would like to have a child. These differences in intention, more so than the differences in characteristics of women with different intentions (such as age), lead to very different impacts of childbirth on careers. The impact of a unplanned birth is about twice as large as the impact of a planned birth.

One missing piece of our estimated impacts of children is any effect of children on women's careers which manifest before treatment assignment. In the case of planned births, this may exclude women moving to careers with more flexible schedules, so long as women do that before they undergo IVF. More broadly, women may choose majors in college with children in mind (Bronson, 2014) whether or not they later have unplanned or planned pregnancies. These patterns may begin at very young ages and this is a part of the cost of motherhood which neither our estimates nor more descriptive estimates such as Kleven et al. (2019a,b) capture. A rich structural literature following from Polachek (1981) and including work by Weiss and Gronau (1981), Heckman and Walker (1990), Gayle and Miller (2002), Francesconi (2002), Sheran (2007), and Keane and Wolpin (2010) can help researchers to get traction on the dynamic tradeoffs which children engender. Most recently, Adda et al. (2017) model how women's occupation choices depend on their future fertility plans, with future mothers choosing occupations that feature less human capital depreciation when taking time off work. Our estimates imply that labor market impacts of children depend on the mother's age and career progression, and also that planning mitigates these impacts. The central role of timing and planning lends strong empirical support to modeling the impact of motherhood dynamically through a structural lens in which women consider when to have children, and also potentially choose careers with the career costs of having a child in mind.

Our results have implications both for policy and for how we think about modeling life-cycle labor market decisions. First, the negative labor market impacts of unplanned pregnancy strengthen the case for ensuring access to high-quality contraceptives. In addition, heterogenity in the impact of children depending on the circumstances surrounding birth has important implications for how we model fertility decisions and estimate the effects of related policies.

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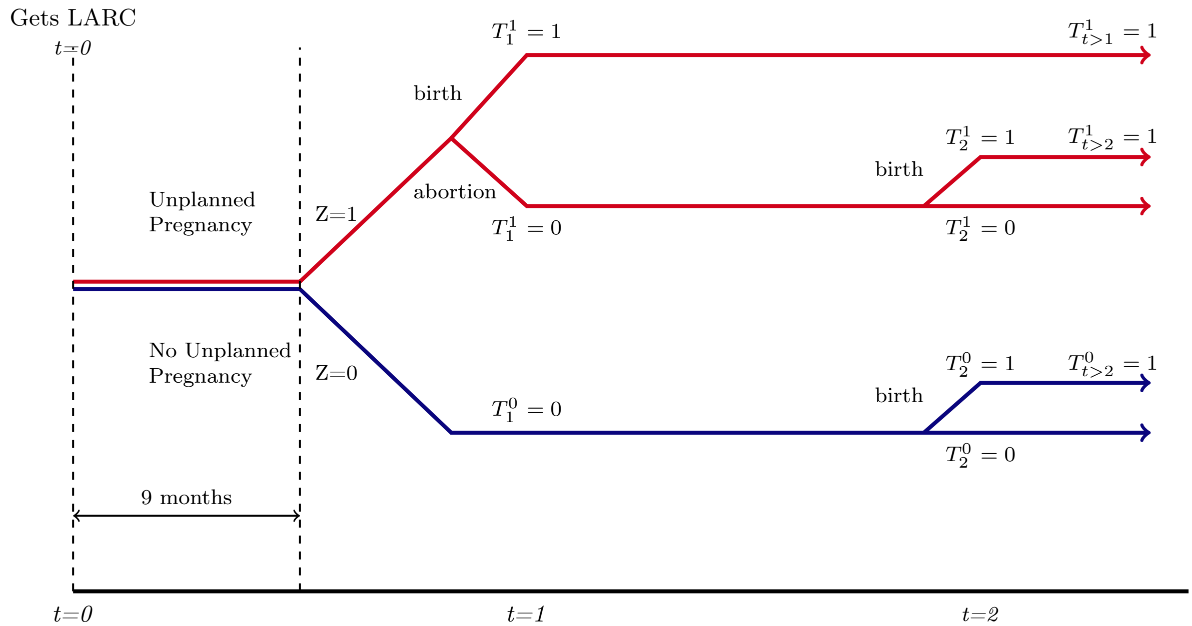
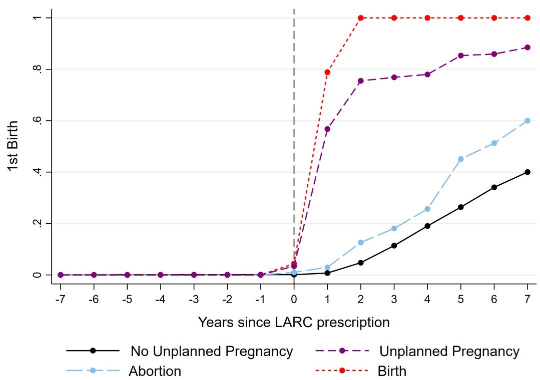
Figures

Figure 1: Dynamic Compliance: LARC Setting

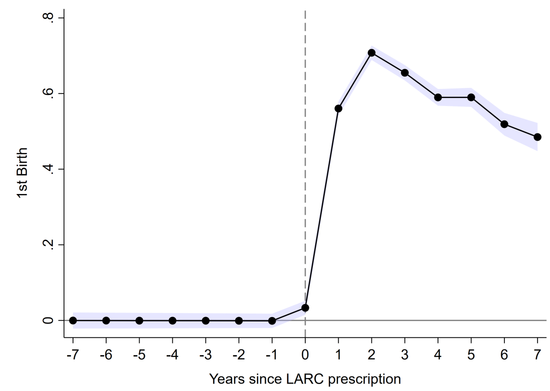


Note: This figure displays dynamic compliance for a group of women who get a LARC in the same year and at the same age, at $t = 0$. Some of these women experience an unplanned pregnancy within 9 months following LARC purchase ($Z = 1$, the "treatment group", red line) and some have no unplanned pregnancy ($Z = 0$, 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"). Over time, there are non-compliers in both groups. For example, two years after purchasing the LARC ($t = 2$) the non-compliers in the treatment group are the women who experience an unplanned pregnancy and then get an abortion and still do not have a child ($T_2^1 = 0$). The non-compliers 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 more patiently described in Section 4.2.

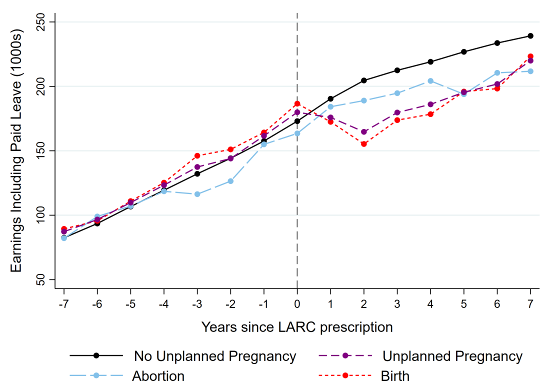
Figure 2: Matching Analysis: Dynamics Effects of Unplanned Pregnancy (LARC) on First Childbirth, Earnings, and Employment



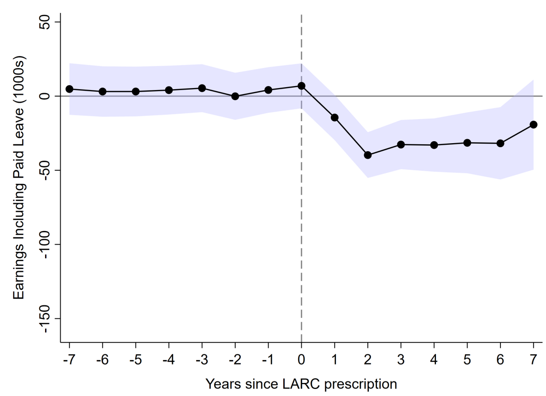
(a) Cond. Means: First Child



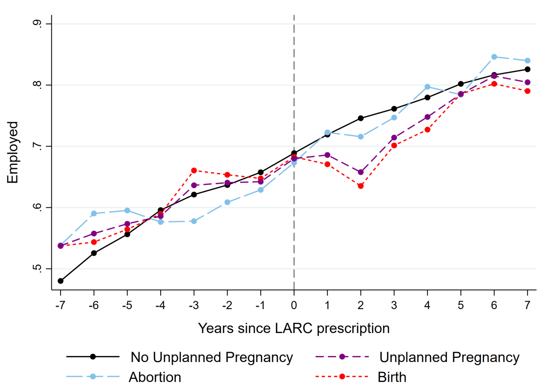
(b) ITT: First Child



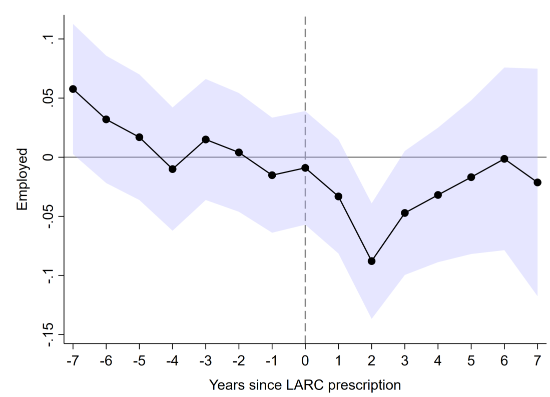
(c) Cond. Means: Earnings including paid leave



(d) ITT: Earnings including paid leave



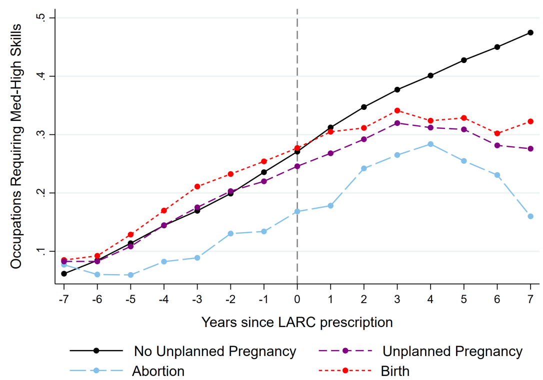
(e) Cond. Means: Employed last week of November



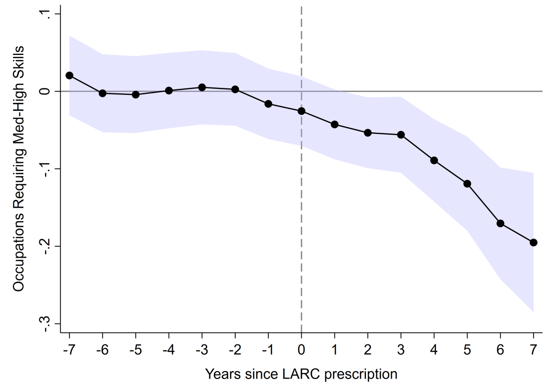
(f) ITT: Employed last week of November

Note: This figure displays the impact of unplanned pregnancy on first childbirth, earnings and employment (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 hundred thousands real 2010 SEK. Results for percent changes in earnings are shown in Appendix Figure A5. Employment is measured in the last week of November. 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). 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). Panels (b) and (d) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a fully saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription during 2005-12.

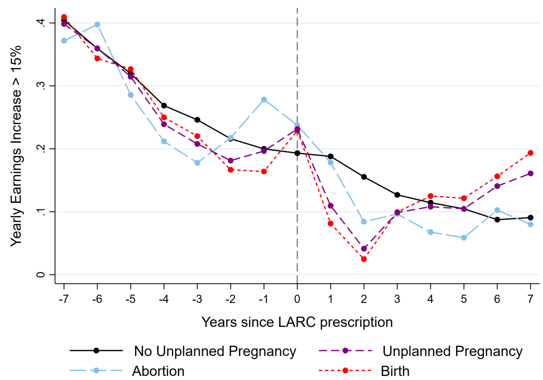
Figure 3: Matching Analysis: Dynamics Effects of Unplanned Pregnancy (LARC) on Occupation, Promotion, and Second Child.



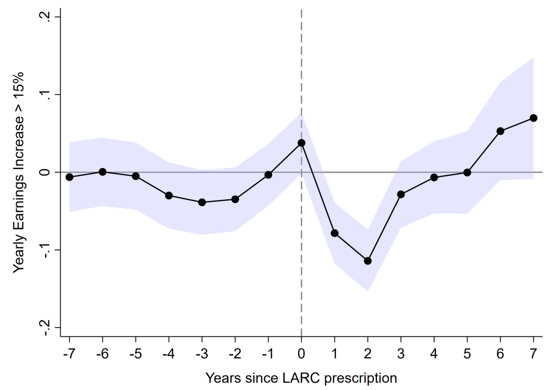
(a) Cond. Means: Occupation requires medium-high-managerial skills



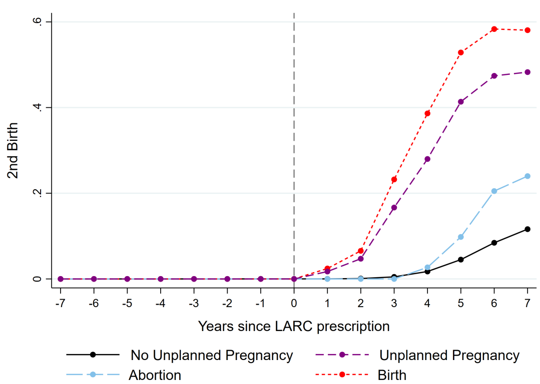
(b) ITT: Occupation requires medium-high-managerial skills



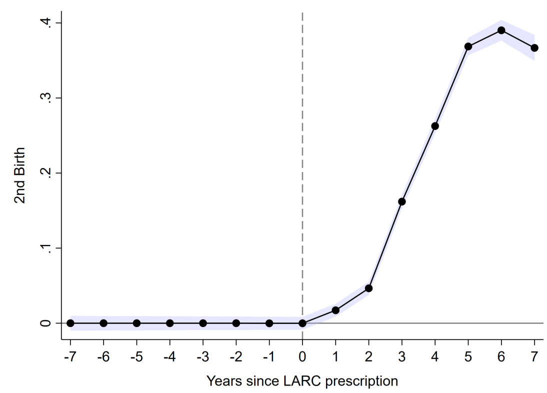
(c) Cond. Means: Promotion



(d) ITT: Promotion



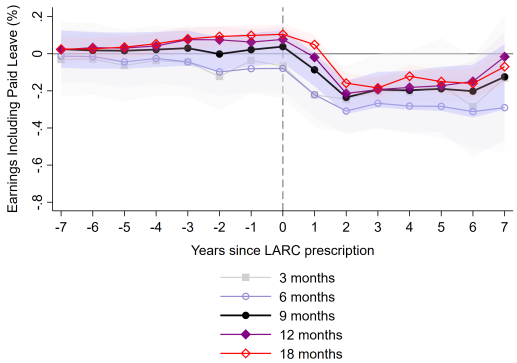
(e) Cond. Means: Second Child



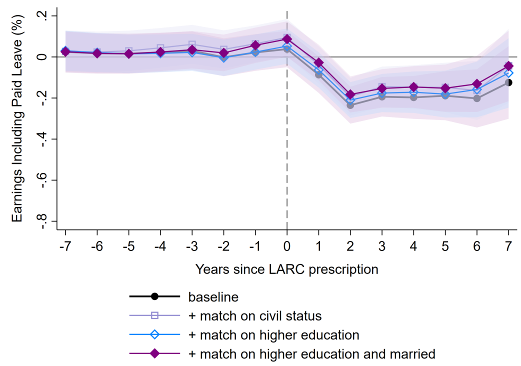
(f) ITT: Second Child

Note: This figure displays the impact of unplanned pregnancy on occupation, promotion, and the probability of having a second child (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Occupation is given by an indicator for being in an occupation requiring medium or high skills or being a manager. Promotion is measured by yearly maximum earnings to date increasing by more than 15%. 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). 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). Panels (b), (d), and (f) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Control variables include a fully saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription during 2005-12.

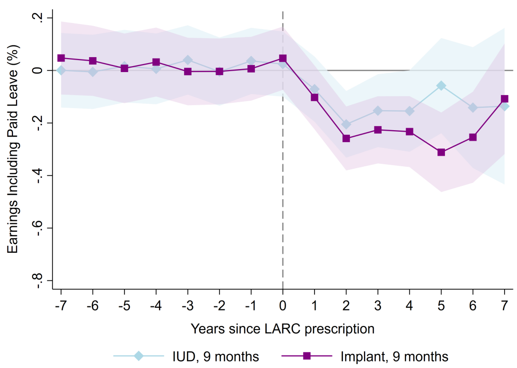
Figure 4: Dynamic Effects of Unplanned Pregnancy (LARC): Robustness



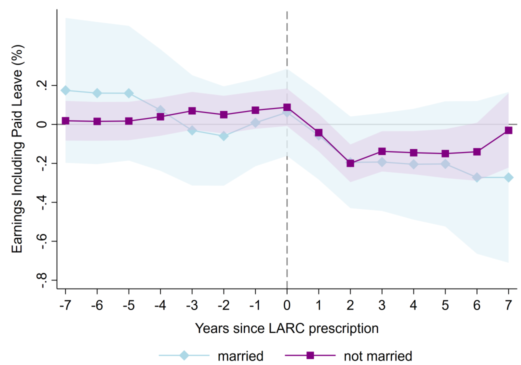
(a) Robustness to LARC window



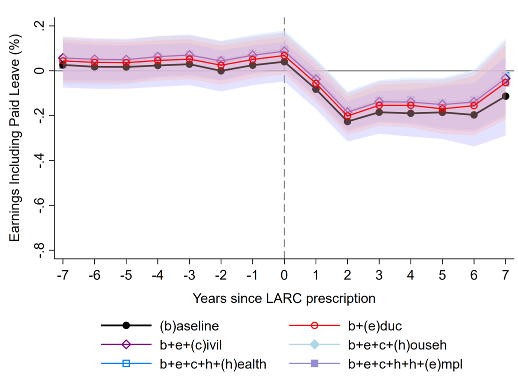
(b) Robustness to Additional Matching



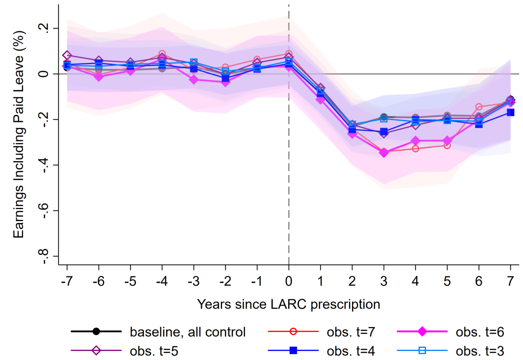
(c) Robustness to Type of Contraception



(d) Robustness to Marital Status



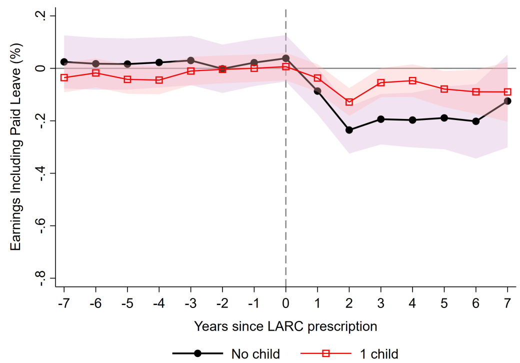
(e) Robustness to Additional Controls



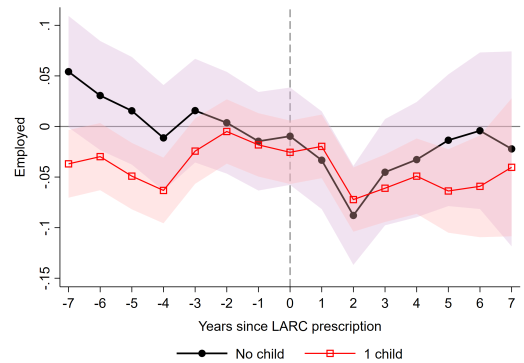
(f) Robustness to Panel Balance

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 ($t = 0$). Earnings including paid leave is measured in hundred thousands real 2010 SEK. These figures show how the dynamic effects and 95% confidence intervals vary by (a) definition of unplanned pregnancies (different windows to determine whether pregnancies among LARC users were unplanned), (b) when matching on additional pre-LARC characteristics, (c) when splitting by type of contraception: IUD and implants, (d) for married and unmarried woman in the year before contraception, (e) adding additional controls, and (f) the balance of the panel (whether we only include women observed at $t = \tau$ for $\tau = 3, ..., 7$). Baseline control variables include a fully saturated model with indicators for age and year of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription during 2005-12.

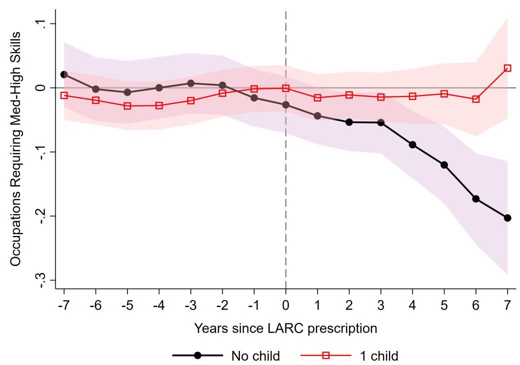
Figure 5: Dynamic Effects of Unplanned Pregnancies (LARC) having had 0 or 1 child



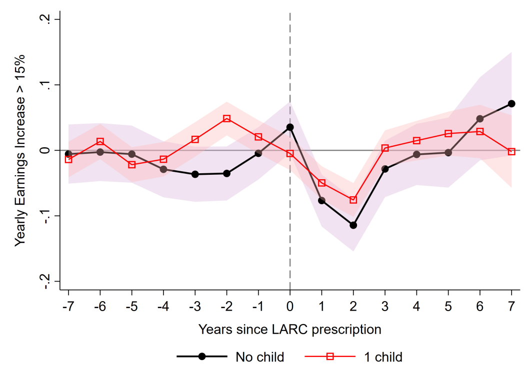
(a) Earnings including paid leave



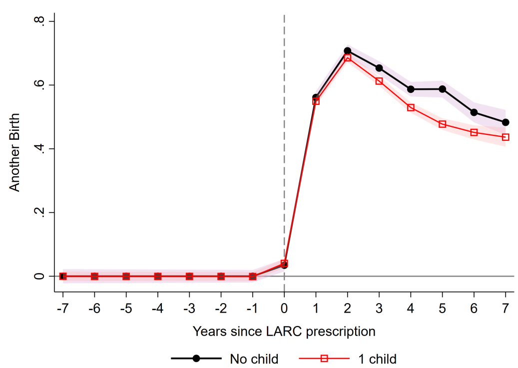
(b) Employed last week of November



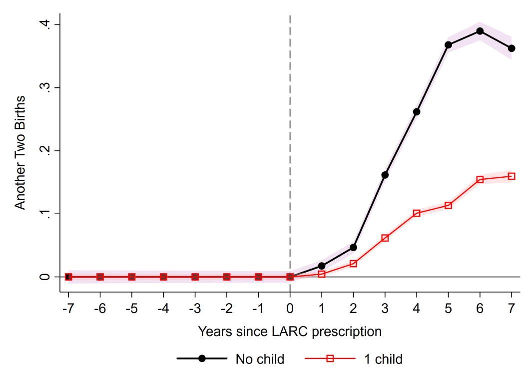
(c) Occupation requires medium-high-managerial skills



(d) Promotion



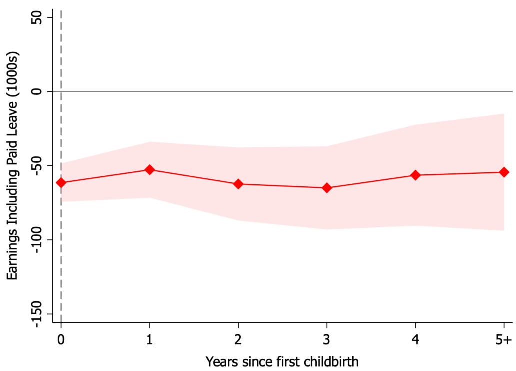
(e) First stage



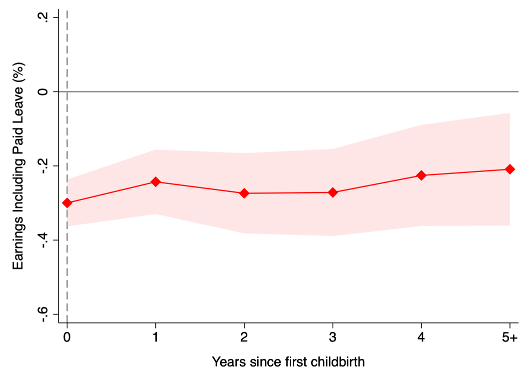
(f) Subsequent childbirth

Note: This figure displays the impact of unplanned pregnancy on labor market and birth outcomes (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). These figures show the dynamic effects and 95% confidence intervals for (a) earnings including paid leave is measured in hundred thousands real 2010 SEK, (b) employment in the last week of November, (c) being in an occupation requiring managerial, medium- or high-level skills, (d) receiving a promotion defined as a 15% increase in maximum lifetime yearly earnings, (e) giving birth to another child (*i.e.*, the "first stage"), and (f) giving birth to a subsequent child (*i.e.*, the second child for nulliparous women and the third child for women who had one child at $t = 0$). Baseline control variables include a fully saturated model with indicators for age and year of LARC prescription, and duration (years) since having the first child for women with one child at $t = 0$. Sample: Women born in 1965-83 with no or one prior child births at the time of LARC prescription during 2005-12.

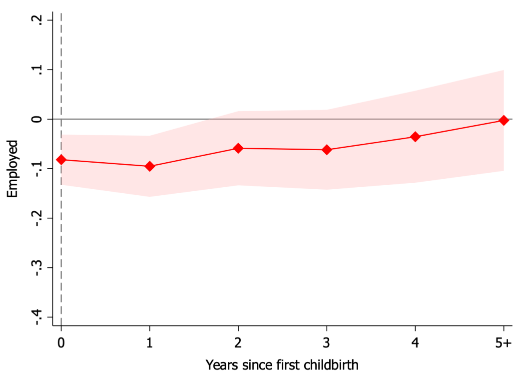
Figure 6: Dynamic Effects of Unplanned Births (LARC)



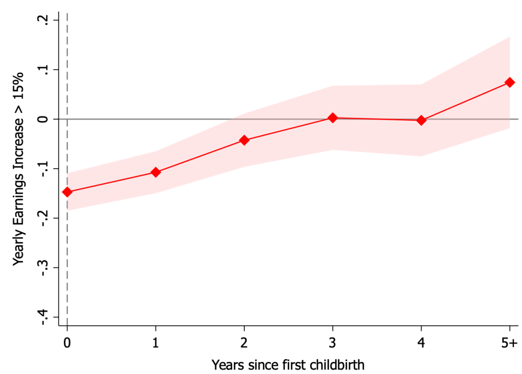
(a) Earnings including paid leave



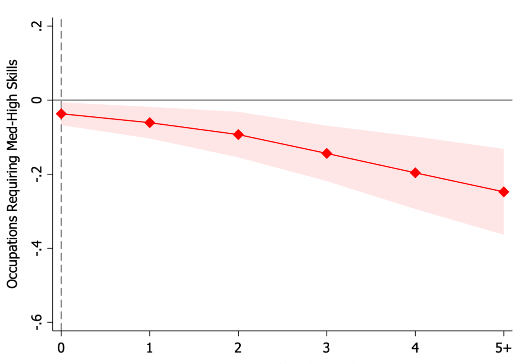
(b) Earnings including paid leave (%)



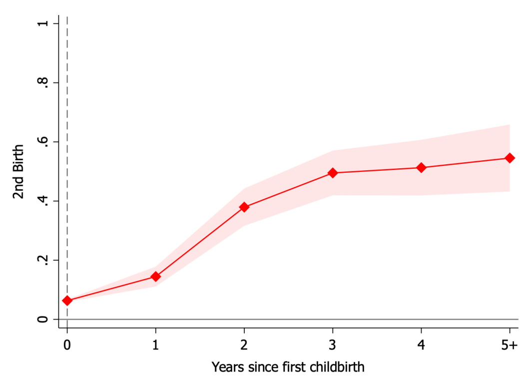
(c) Employed in the last week of November



(d) Promotion



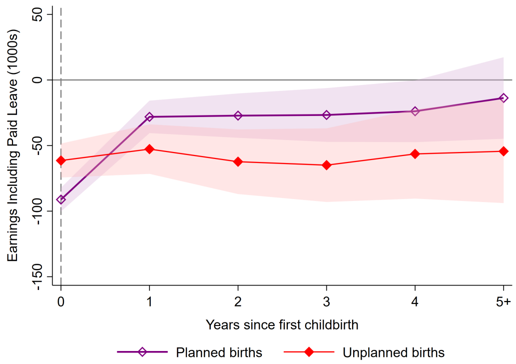
(e) Occupation requires medium-high-managerial skills



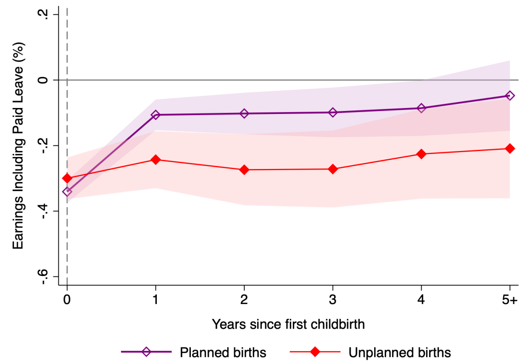
(f) 2nd childbirth

Note: This figure displays the impact of first child and 95% confidence interval (y-axis) by time since birth (x-axis). The effects estimated in a subsample of women who wanted to delay children (LARC users) are displayed in red, using IVF success as an instrument to identify the effect of planned births.

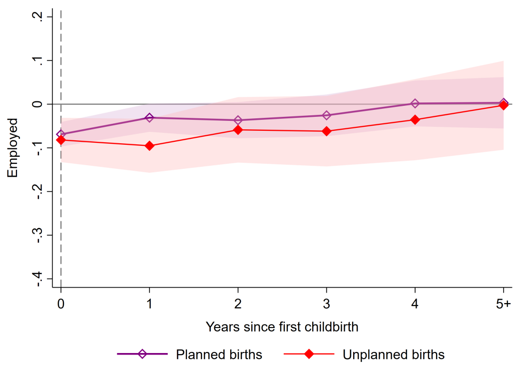
Figure 7: Dynamic Effects of Unplanned Births, Compared to Planned



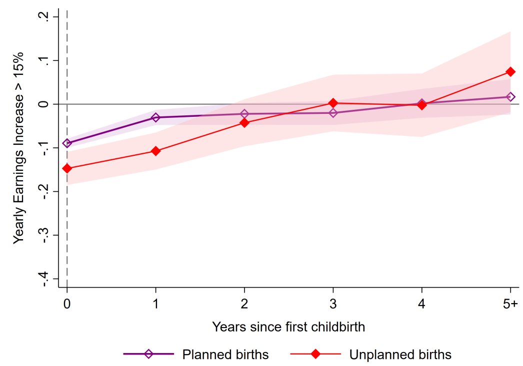
(a) Earnings including paid leave



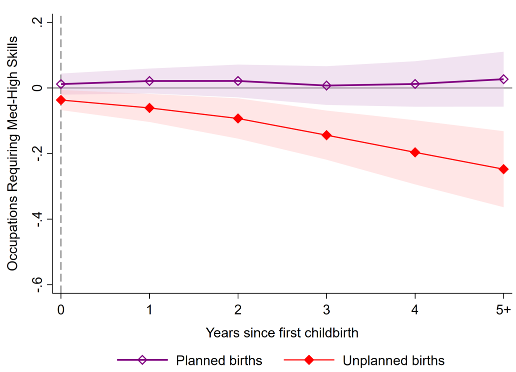
(b) Earnings including paid leave (%)



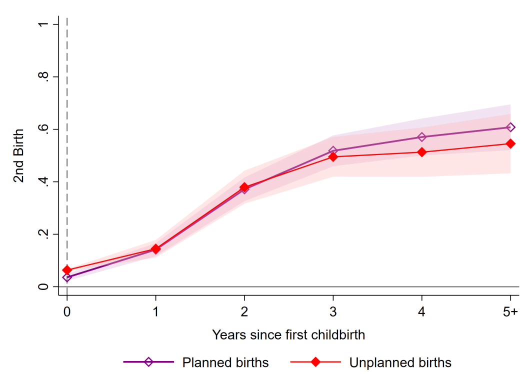
(c) Employed in the last week of November



(d) Promotion



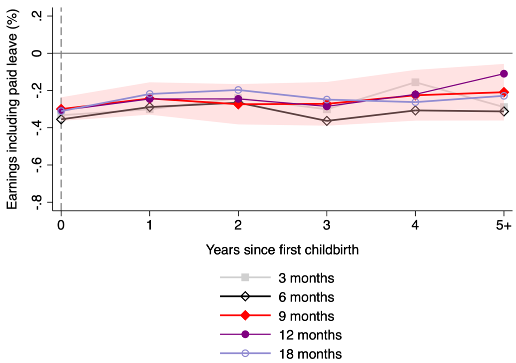
(e) Occupation requires medium-high-managerial skills



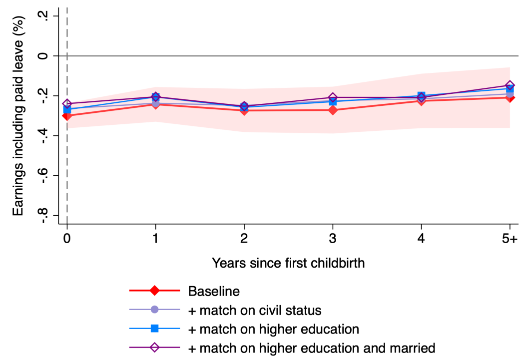
(f) 2nd childbirth

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 purple; the effects estimated in a subsample of women who wanted to delay children (LARC users), in red.

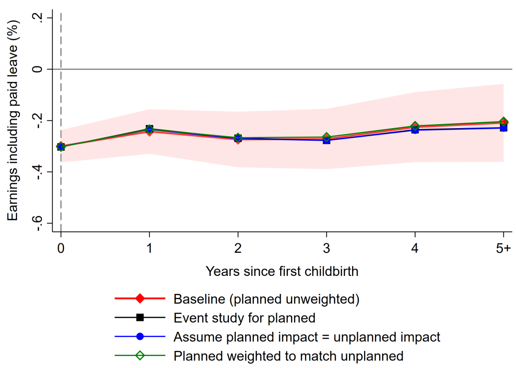
Figure 8: Dynamic Effects of Unplanned Birth: Robustness of Unplanned Estimates



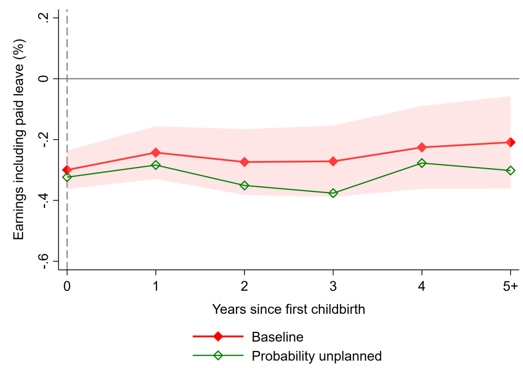
(a) Robustness to LARC window



(b) Robustness to Additional Matching



(c) Robustness to Assumptions about Planned Birth Impact



(d) Robustness to Probability Weights for Unplanned

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) when matching on additional pre-LARC characteristics, (c) when making different assumptions about the impact of planned births, (d) weighing the LARC sample for probability of being unplanned using conception time since LARC (conceptions nine months after a LARC have slightly higher probability of being planned compared to conceptions immediately after the LARC insertion.

Tables

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

		LARC		IVF
	All	Nulliparous	1 Child	Nulliparous
Age	31.262	30.272	33.298	32.251
Earnings, Yearly (1000s)	187.501	157.773	189.788	256.386
Earnings Including Paid Leave, Yearly (1000s)	191.696	164.827	214.518	260.124
Wage, Monthly FT-equivalent (1000s)	24.451	23.087	24.624	25.590
Fraction of FT employment (FT = 100)	88.704	83.704	87.216	92.082
Employed	0.702	0.661	0.789	0.860
Occupation Requiring Med-High Skills	0.359	0.246	0.378	0.508
Job Flexibility Index (Low = More Flexible)	0.107	-0.036	0.112	0.231
Enrolled in Education	0.195	0.262	0.126	0.161
High School	0.325	0.416	0.449	0.313
College Degree or Higher	0.477	0.347	0.374	0.573
Married	0.196	0.183	0.386	0.476
Single	0.756	0.754	0.522	0.489
Divorced	0.046	0.060	0.089	0.034
N	387,243	21,775	32,509	8,689

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, and nulliparous women who undergo IVF in the year before they start the treatment ($t = -1$). We focus on 2008 for the overall population because this is the year before midpoint of our prescription data. For LARC 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. 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 three columns have only 8136, 14209, 4908 observations, respectively, for these two variables. Cohabiting couples without common children are categorized in the "Single" categories as they cannot be distinguished in the data. We also note that a small number of women are not observed in the year before LARC purchase because, for example, they are out of the country and these women are not included in this tabulation.

Table 2: Balance: LARC and IVF Setting

	LARC: Nulliparous Women			LARC: Women with 1 child			IVF: Nulliparous Women		
	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff	Unplanned Pregnancy	No Unplanned Pregnancy	p-value diff	First IVF Successful	First IVF Unsuccessful	p-value diff
Earnings, Yearly (1000s)	151.123	151.680	0.956	142.211	146.090	0.563	259.159	255.409	0.002
Earnings including Paid Leave, Yearly (1000s)	161.609	158.068	0.624	176.951	177.645	0.992	263.012	259.107	0.002
Wage, Monthly FT-equivalent (1000s)	21.529	22.091	0.882	23.014	22.485	0.090	25.132	25.764	0.174
Fraction of FT employment (FT = 100)	82.843	82.508	0.718	82.272	82.220	0.568	91.926	92.084	0.766
Employed	0.642	0.657	0.558	0.707	0.724	0.259	0.880	0.853	0.001
Occupation Requiring Med-High Skills	0.220	0.236	0.490	0.289	0.292	0.929	0.526	0.501	0.000
Job Flexibility Index (Low = More Flexible)	-0.055	-0.057	0.811	0.013	0.025	0.567	0.232	0.228	0.088
Enrolled in Education	0.232	0.285	0.017	0.164	0.156	0.674	0.162	0.160	0.974
High school	0.434	0.408	0.329	0.435	0.454	0.317	0.306	0.315	0.011
College Degree or Higher	0.317	0.357	0.096	0.309	0.329	0.356	0.587	0.568	0.000
Married	0.223	0.132	0.000	0.327	0.337	0.713	0.487	0.472	0.625
Single	0.701	0.823	0.000	0.601	0.605	0.781	0.490	0.489	0.720
Divorced	0.070	0.043	0.028	0.070	0.057	0.265	0.023	0.038	0.040
Observations	341	21,434		682	31,827		2,263	6,426	

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 six 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 and do not include any leave payments. Employment status is measured in the last week of November of a given year. Wages and work hours are measured for the $\approx 50\%$ sub-sample (undersampling smaller firms) observed in the wage statistics. Cohabiting couples without common children are categorized in the "Single" categories as they cannot be distinguished in the data. 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.

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

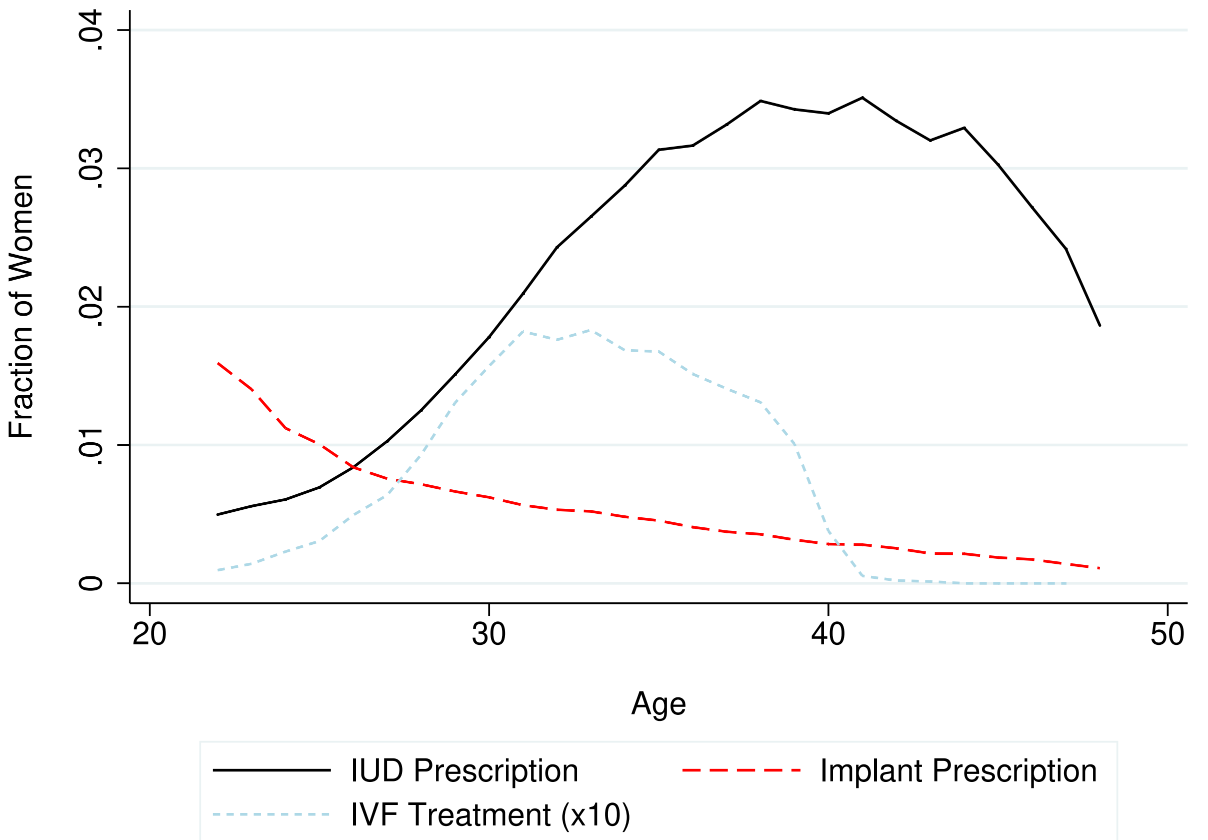
			Age				Enrolled in Education			
	All Unplanned		27 and Younger		28 and Older		Yes		No	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Average earnings loss (years 1-6)	-60.531	-60.028	-79.110	-76.544	-38.633	-31.258	-97.232	-92.791	-47.513	-40.206
	(12.006)	(9.561)	(14.320)	(11.717)	(20.318)	(15.741)	(17.725)	(16.863)	(15.077)	(10.977)
...As % of average counterfactual earnings	-0.255	-0.253	-0.332	-0.322	-0.163	-0.132	-0.385	-0.369	-0.207	-0.176
	(0.051)	(0.040)	(0.060)	(0.049)	(0.086)	(0.067)	(0.070)	(0.067)	(0.066)	(0.048)
Medium or high skill occ. by year 5	-0.248	-0.216	-0.213	-0.248	-0.290	-0.165	-0.332	-0.358	-0.167	-0.124
	(0.059)	(0.051)	(0.067)	(0.061)	(0.121)	(0.093)	(0.109)	(0.104)	(0.069)	(0.055)
Years in medium or high skill occ.	-0.778	-0.703	-0.835	-0.860	-0.615	-0.472	-1.253	-1.481	-0.458	-0.288
	(0.162)	(0.143)	(0.182)	(0.170)	(0.304)	(0.251)	(0.301)	(0.292)	(0.192)	(0.160)
Years employed	-0.336	-0.076	-0.609	-0.715	-0.096	0.512	-0.580	-0.381	-0.255	-0.105
	(0.156)	(0.150)	(0.192)	(0.183)	(0.270)	(0.289)	(0.257)	(0.244)	(0.190)	(0.173)
Number of promotions	-0.222	-0.171	-0.428	-0.429	-0.007	0.026	-0.461	-0.394	-0.082	-0.057
	(0.094)	(0.095)	(0.118)	(0.118)	(0.154)	(0.159)	(0.205)	(0.204)	(0.100)	(0.099)
Observations	36,578	35,993	9,744	9,642	26,834	26,351	7,886	7,886	27,749	27,749
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 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 are given by the predicted earnings in the control group when all child age indicators are set to zero. Years in medium or high skill occupation, years employed, and number of promotions 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.

Online Appendix for:
The Labor Market Returns to Delaying Pregnancy

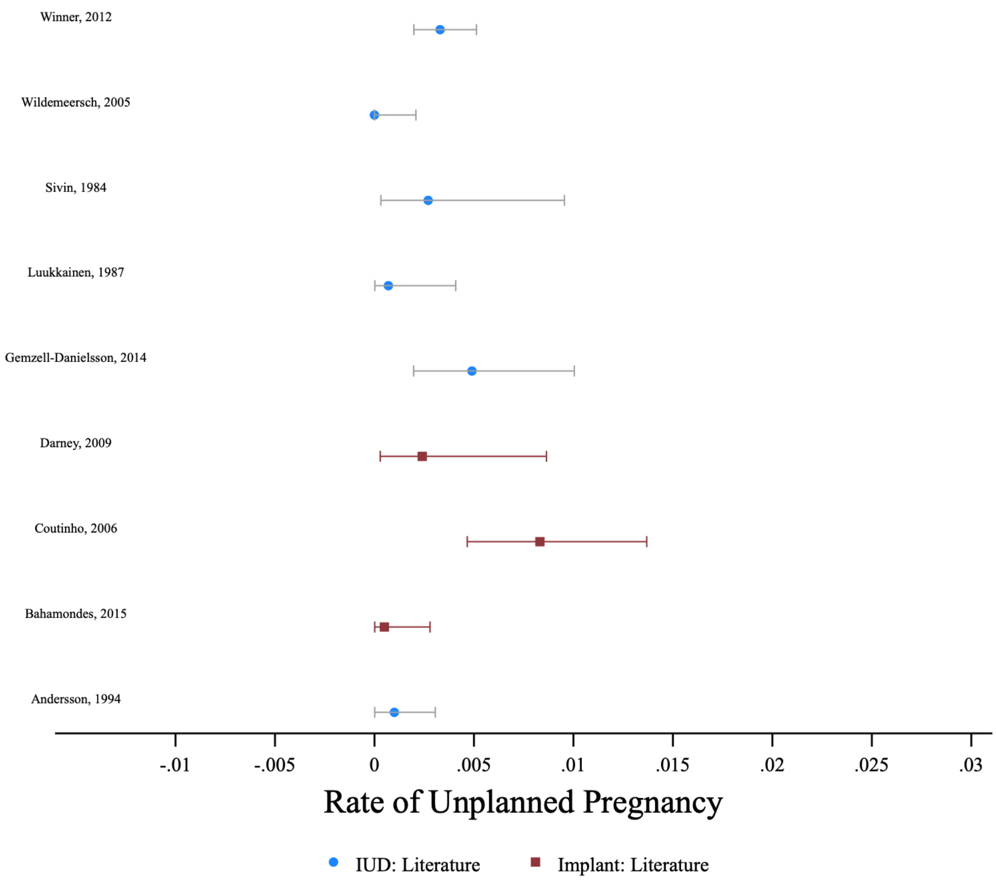
A Appendix Figures and Tables

Figure A1: Fraction of Women Receiving LARC Prescriptions and IVF Treatment by Age



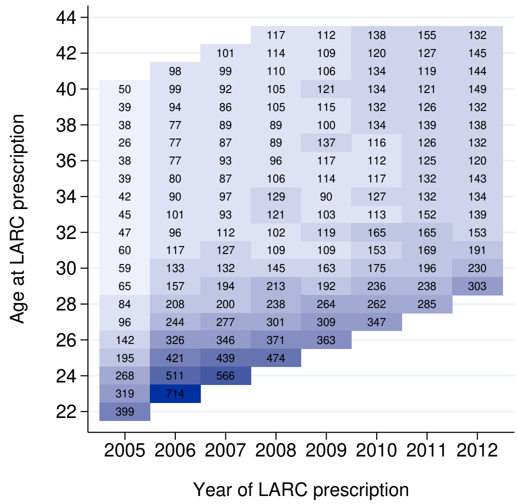
Note: This figure describes the fraction of women who receive a LARC prescription (an IUD or an implant) or receive IVF treatment at each age. Sample: Women born in 1965-83, prescriptions from July 2005 through 2012.

Figure A2: LARC Failure Rates

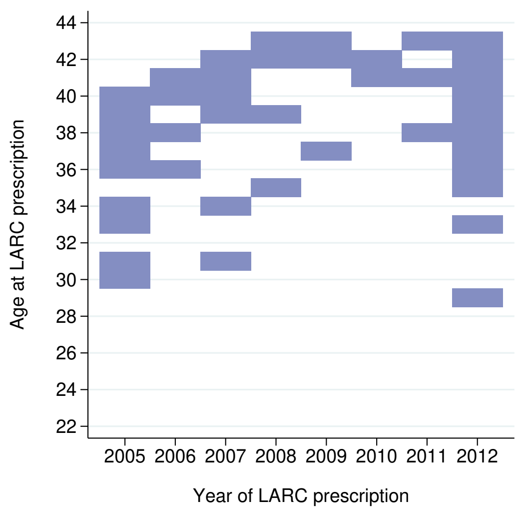


Note: This figure displays estimates of IUD and Implant failure rates from clinical trials, annualized to 1 year.

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



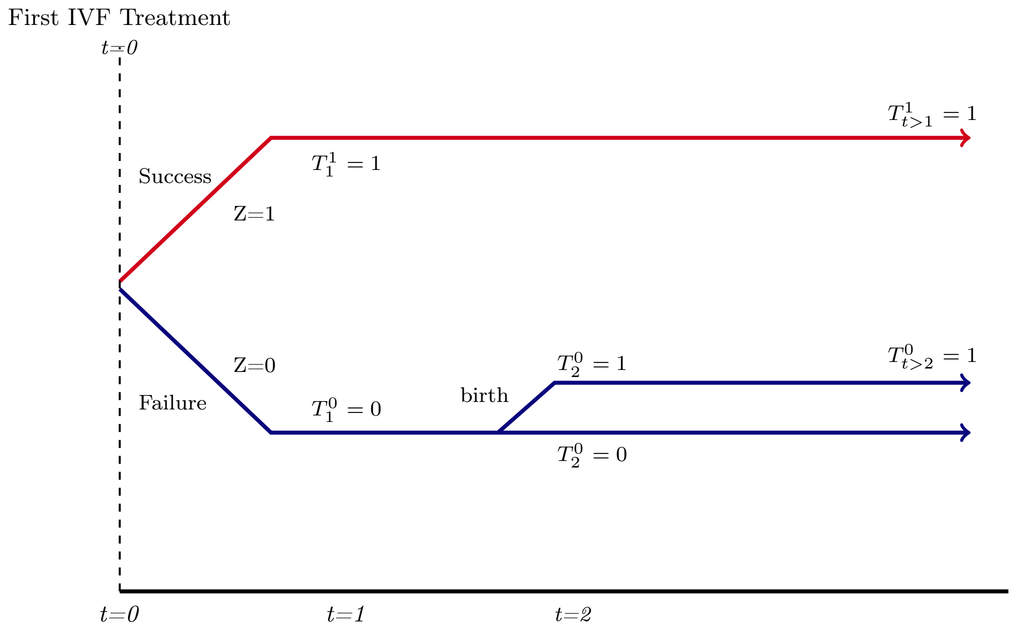
(a) N nulliparous LARC women



(b) Unmatched age×year cells

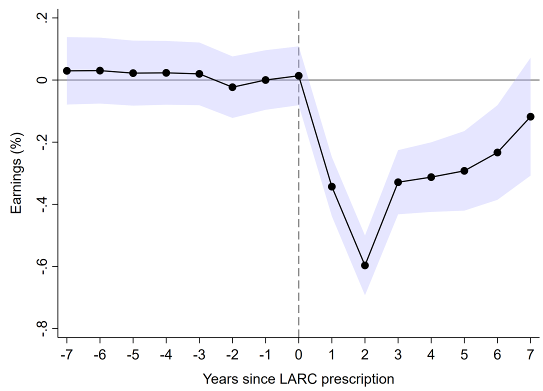
Note: The top panel (a) of this figure shows the number of women in each of the 142 age×year cells, while the bottom panel (b) marks the 42 cells in which we do not observe any unplanned pregnancy within 9-months of LARC purchase. That is, we only observe "control" women and no "treated" women in these cells. Note that most of the unmatched 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. Sample: Nulliparous women born in 1965-83 who at age 43 or younger fill a LARC prescription between July 2005 and 2012.

Figure A4: Dynamic Compliance: IVF Setting

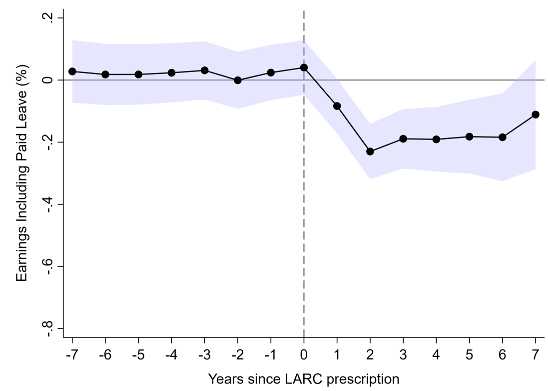


Note: This figure displays the dynamics of compliance for a group of women who start fertility treatment at the same age and in the same year, at $t = 0$. The "treatment group" are the women who have success in first IVF procedure ($Z = 1$, red line). The "control group" are the women who are not successful in first IVF procedure ($Z = 0$, blue line). In this setting, there is full compliance at $t = 1$, but in later time periods the "non-compliers" in the control group are the women who later give birth ($T^0_2 = 1$) and likely conceive through repeated IVF procedure.

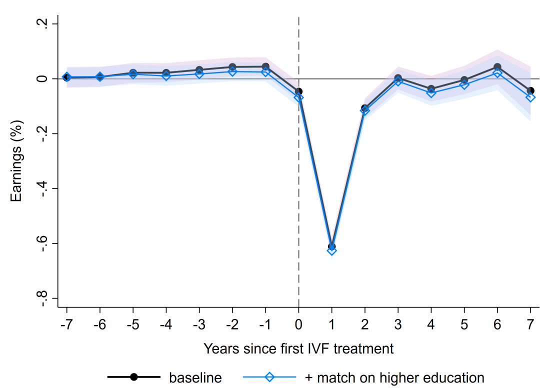
Figure A5: Matching Analysis: Dynamics Effects of Planned and Unplanned Pregnancy on Percent Changes in Earnings



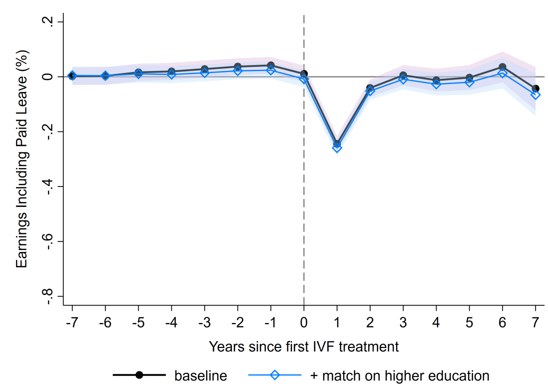
(a) LARC: Percent Changes in Earnings Without Paid Leave



(b) LARC: Percent Changes in Earnings Including Paid Leave



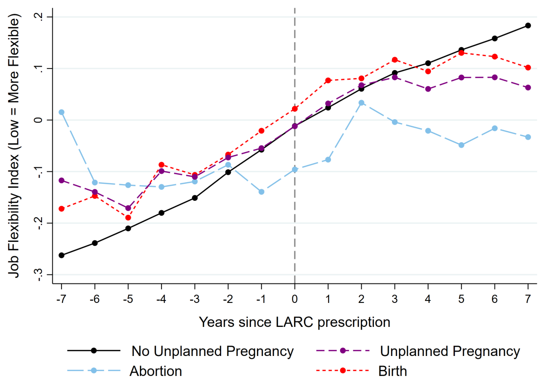
(c) IVF: Percent Changes in Earnings without paid leave



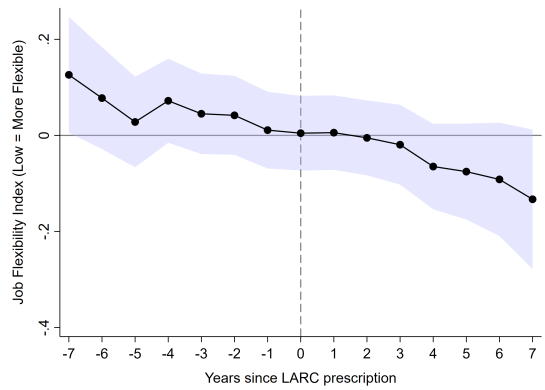
(d) IVF: Percent Changes in Earnings Including Paid Leave

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of LARC prescription and age. Control group: Women who do not conceive within one year of LARC prescription. Treatment group: Women who conceive the first child within nine months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription.

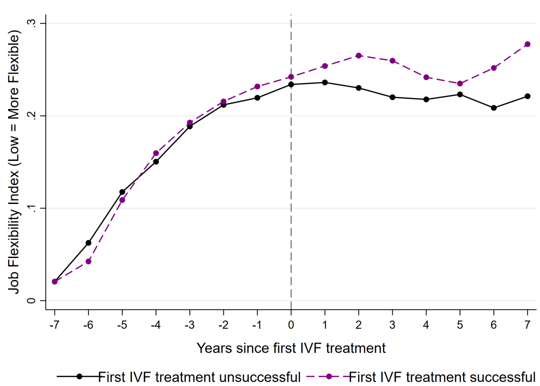
Figure A6: Matching Analysis: Dynamics Effects of Planned (LARC) and Unplanned Pregnancy (IVF) on Flexibility



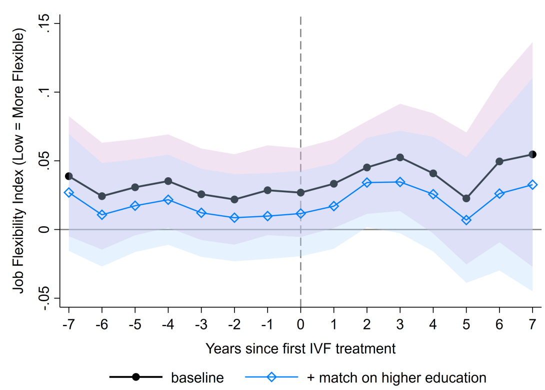
(a) Flexibility: Conditional Means



(b) Flexibility: ITT



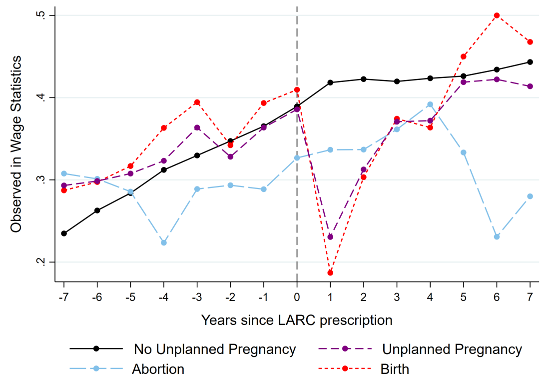
(c) Flexibility: Conditional Means



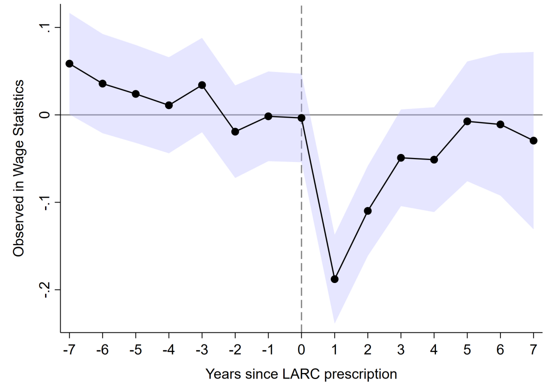
(d) Flexibility: ITT

Note: This figure displays the impact of unplanned (panels (a) and (b)) and planned (panels (c) and (d)) pregnancy on the flexibility of a job. Panel (b) 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 LARC prescription ($t = 0$). Controls include a fully saturated model with indicators for year of LARC prescription and age. Control group: Women who do not conceive within one year of LARC prescription. Treatment group: Women who conceive the first child within nine months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription. Panel (d) displays the impact of successful IVF procedure and 95% confidence interval (y-axis) by time since IVF procedure as measured by fertility drug prescriptions (x-axis). The vertical dashed line marks the year of fertility prescription ($t = 0$). Controls include a fully saturated model with indicators for year of IVF procedure and age, as well as higher education in blue. Control group: Women who do not conceive as a result of their first IVF procedure. Treatment group: Women who conceive as a result of their first IVF procedure. Sample: Women born in 1965-83 with no prior child births at the time of IVF procedure.

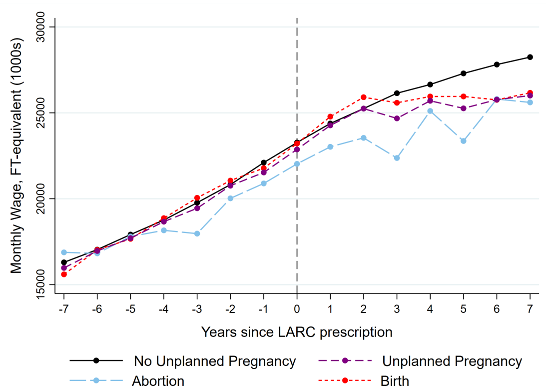
Figure A7: Matching Analysis: Dynamics Effects of Unplanned Pregnancy (LARC) on Wages, Hours, and Presence in Wage Data.



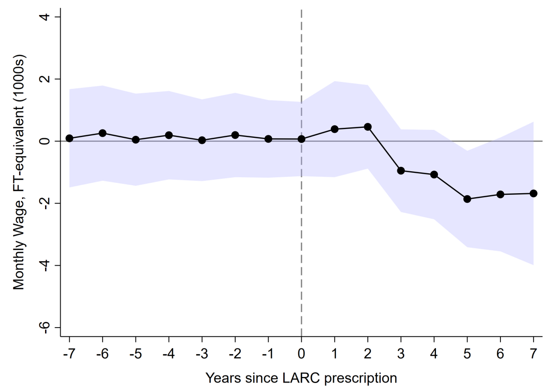
(a) In wage data



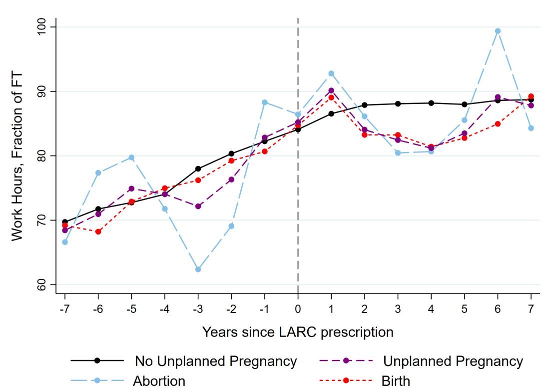
(b) In wage data



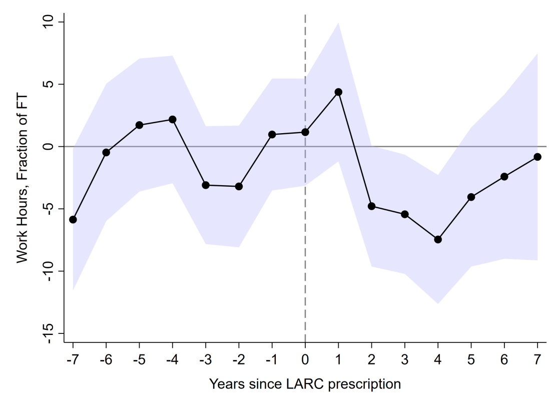
(c) Wages



(d) Wages



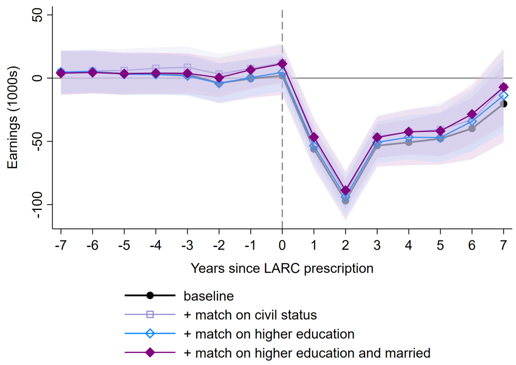
(e) Hours



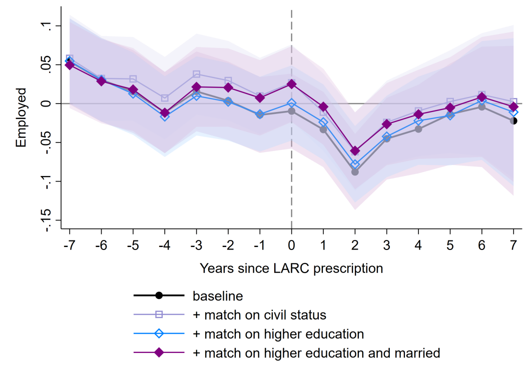
(f) Hours

Note: This figure displays the impact of unintended pregnancy and 95% confidence interval (y-axis) by time since LARC prescription (x-axis). The vertical dashed line marks the year of LARC prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of LARC prescription and age. Control group: Women who do not conceive within one year of LARC prescription. Treatment group: Women who conceive the first child within nine months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription.

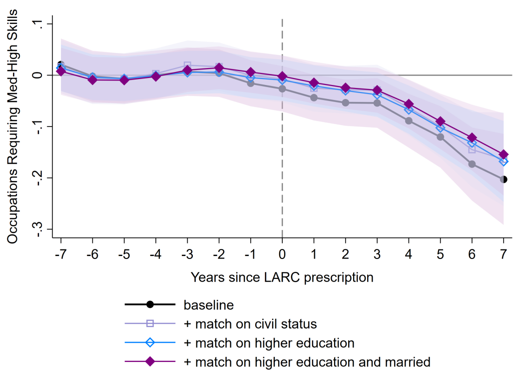
Figure A8: Matching Analysis: Robustness to Matching on Civil Status and Education



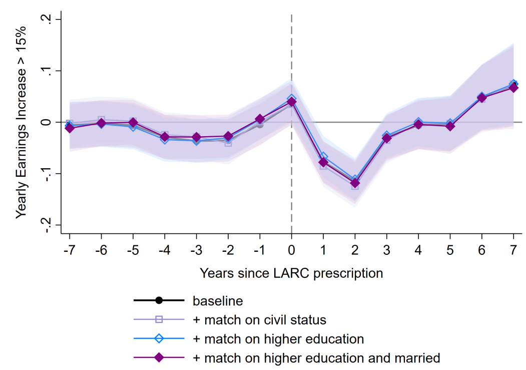
(a) Earnings



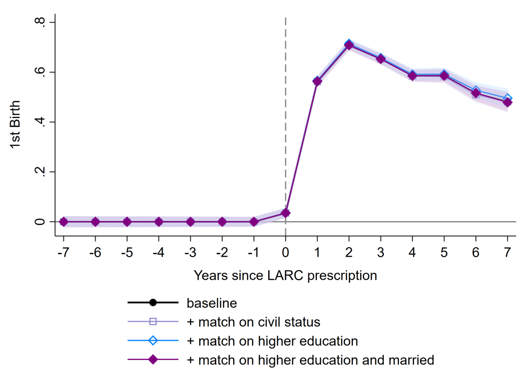
(b) Employed in the last week of November



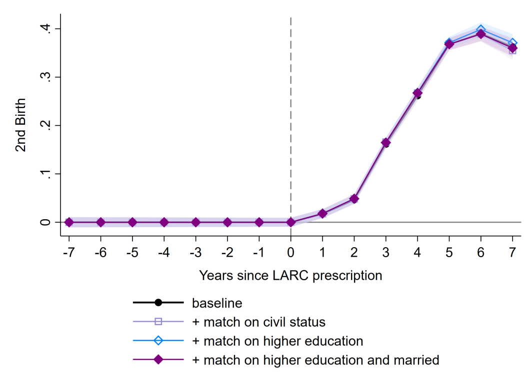
(c) Occupation requires medium-high-managerial skills



(d) Promotion



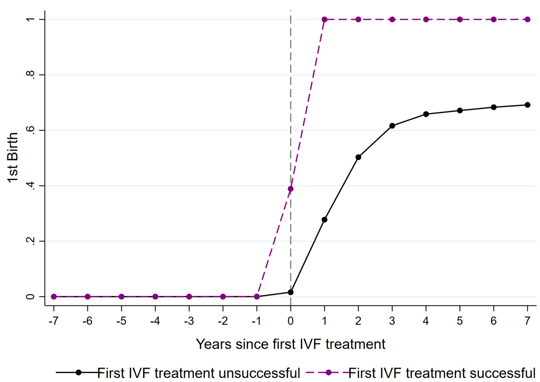
(e) 1st Birth



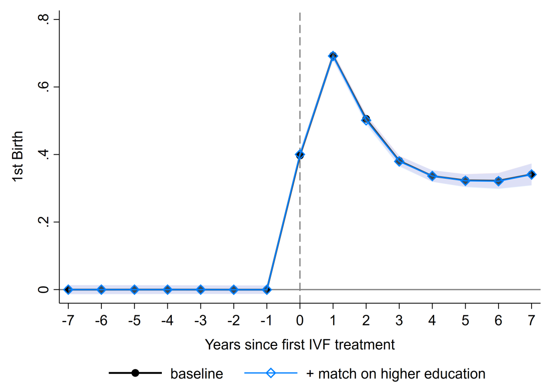
(f) 2nd Birth

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 LARC prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of LARC prescription and age. We compare to models that additionally match on an indicator for higher education, as well as marital status and whether the women were divorced the year before the LARC prescription, and their interaction (as indicated). Control group: Women who do not conceive within one year of LARC prescription. Treatment group: Women who conceive first child within nine months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription.

Figure A9: Matching Analysis: Effect of Planned Pregnancies on Childbirth



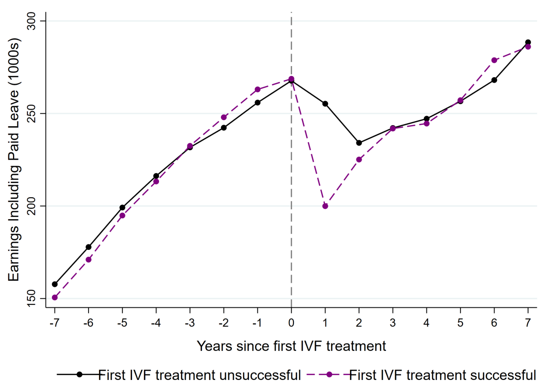
(a) Cond. Means: First Child



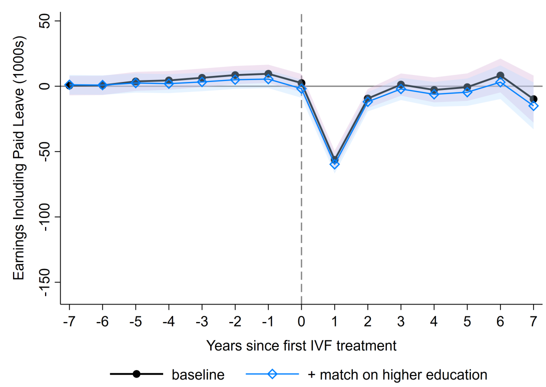
(b) ITT: First Child

Note: This figure displays the impact of planned pregnancy on the probability of first childbirth (y-axis) by time since IVF procedure as measured by fertility drug prescriptions (x-axis). The vertical dashed line marks the year of fertility prescription ($t = 0$). Panel (a) shows conditional means separately for the "control group" of women who do not conceive as a result of their first IVF procedure (black solid line) and the "treatment group" of women who conceive as a result of their first IVF procedure (purple dashed line). Panel (b) shows the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Baseline control variables include a fully saturated model with indicators for age and year of IVF procedure. Finally, we also show dynamic effects when we additionally match on an indicator for higher education attainment. Sample: Women born in 1965-83 with no prior child births at the time of IVF procedure during 2005-12.

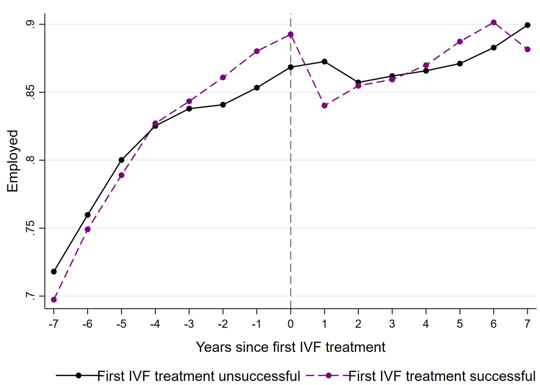
Figure A10: Matching Analysis: Dynamics Effects of Planned Pregnancy (IVF) on Earnings and Employment.



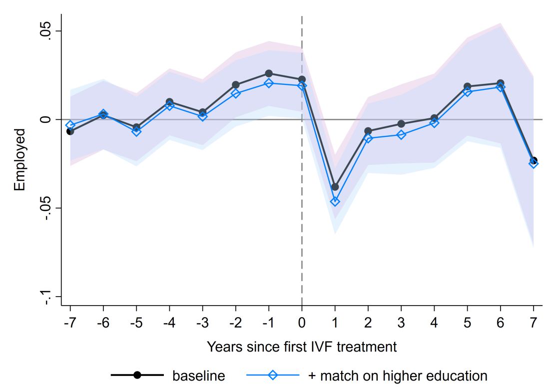
(a) Cond. Means: Earnings including paid leave



(b) ITT: Earnings including paid leave



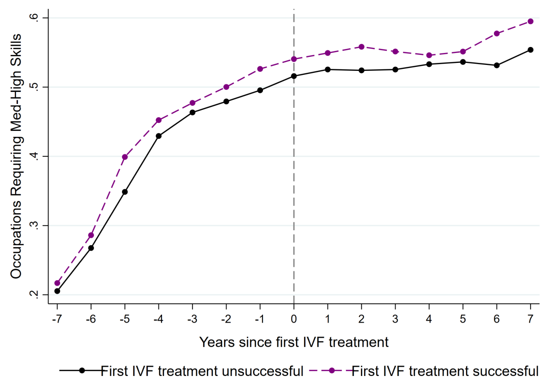
(c) Cond. Means: Employed last week of November



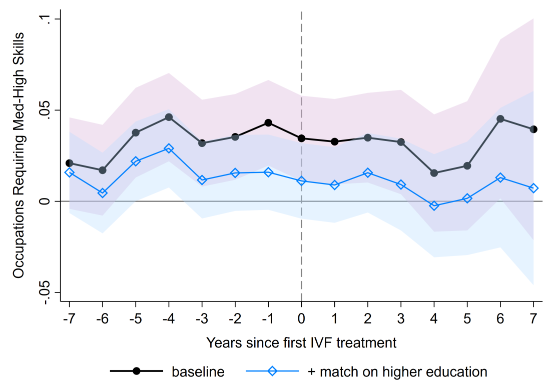
(d) ITT: Employed last week of November

Note: This figure displays the impact of planned pregnancy on earnings and employment (y-axis) by time since IVF procedure as measured by fertility drug prescriptions (x-axis). The vertical dashed line marks the year of fertility prescription ($t = 0$). Earnings including paid leave is measured in hundred thousands real 2010 SEK. Employment is measured in the last week of November. Panels (a) and (c) show conditional means separately for the "control group" of women who do not conceive as a result of their first IVF procedure (black solid line) and the "treatment group" of women who conceive as a result of their first IVF procedure (purple dashed line). Panels (b) and (d) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Baseline control variables include a fully saturated model with indicators for age and year of IVF procedure. Finally, we also show dynamic effects when we additionally match on an indicator for higher education attainment. Sample: Women born in 1965-83 with no prior child births at the time of IVF procedure during 2005-12.

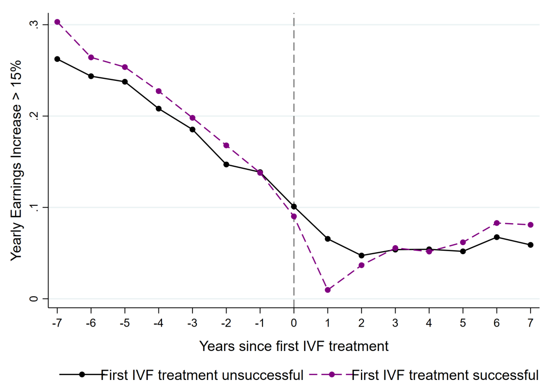
Figure A11: Matching Analysis: Dynamics Effects of Planned Pregnancy (IVF) on Promotion, Occupation, and Second Child.



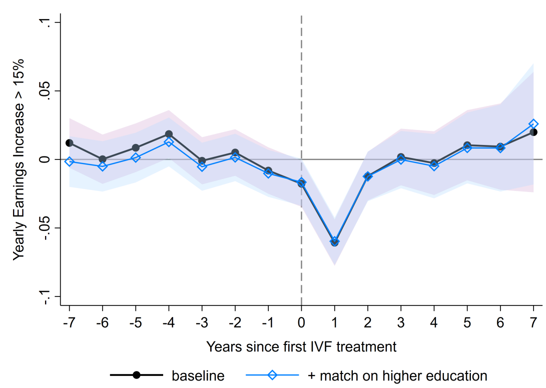
(a) Cond. Means: Occupation requires medium-high-managerial skills



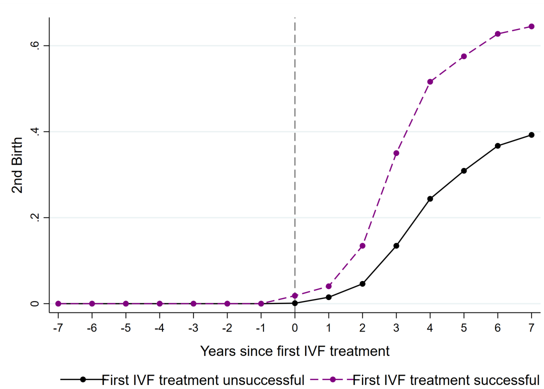
(b) ITT: Occupation requires medium-high-managerial skills



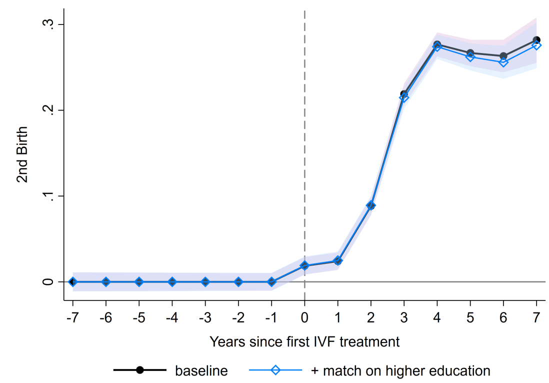
(c) Cond. Means: Promotion



(d) ITT: Promotion



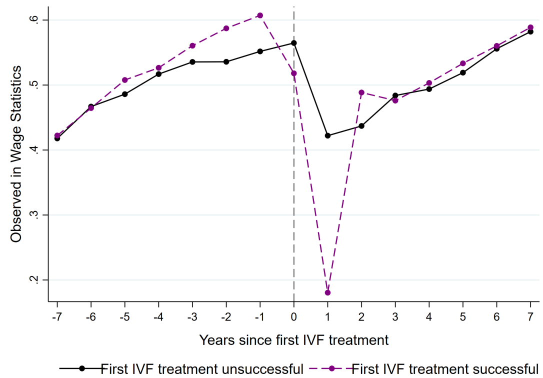
(e) Cond. Means: Second Child



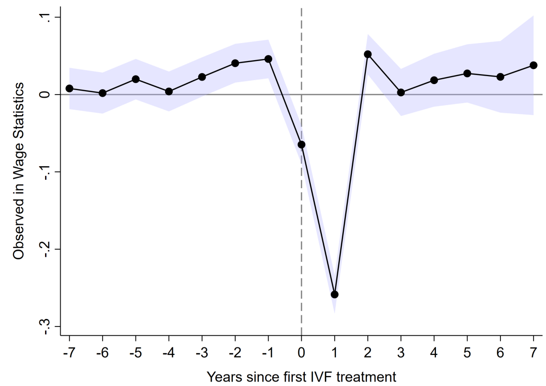
(f) ITT: Second Child

Note: This figure displays the impact of planned pregnancy on occupation, promotion, and the probability of having a second child (y-axis) by time since IVF procedure as measured by fertility drug prescriptions (x-axis). The vertical dashed line marks the year of fertility prescription ($t = 0$). Occupation is given by an indicator for being in an occupation requiring medium or high skills or being a manager. Promotion is measured by yearly maximum earnings to date increasing by more than 15%. Panels (a) and (c) show conditional means separately for the "control group" of women who do not conceive as a result of their first IVF procedure (black solid line) and the "treatment group" of women who conceive as a result of their first IVF procedure (purple dashed line). Panels (b) and (d) show the dynamic effects and 95% confidence interval of the impact of unplanned pregnancy. Baseline control variables include a fully saturated model with indicators for age and year of IVF procedure. Finally, we also show dynamic effects when we additionally match on an indicator for higher education attainment. Sample: Women born in 1965-83 with no prior child births at the time of IVF procedure during 2005-12.

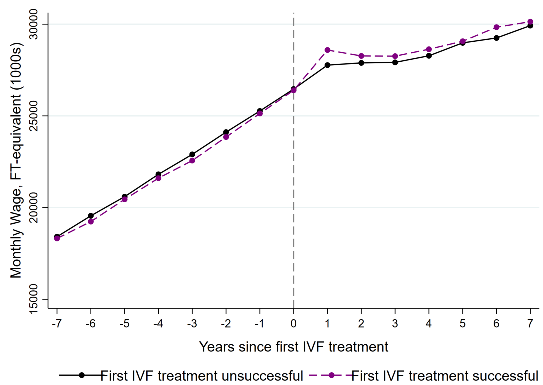
Figure A12: Matching Analysis: Dynamics Effects of Planned Pregnancy (IVF) on Wages, Hours, and Presence in Wage Data.



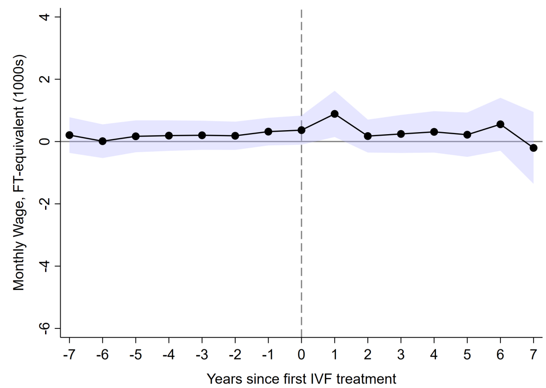
(a) In wage data



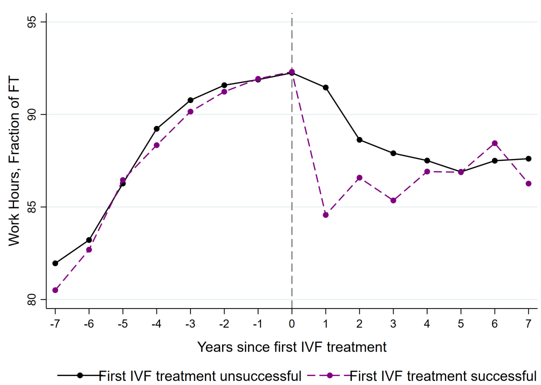
(b) In wage data



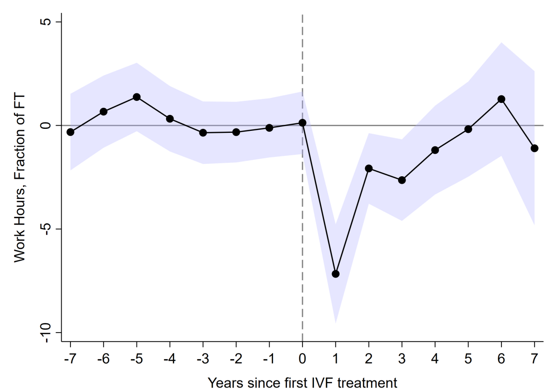
(c) Wages



(d) Wages



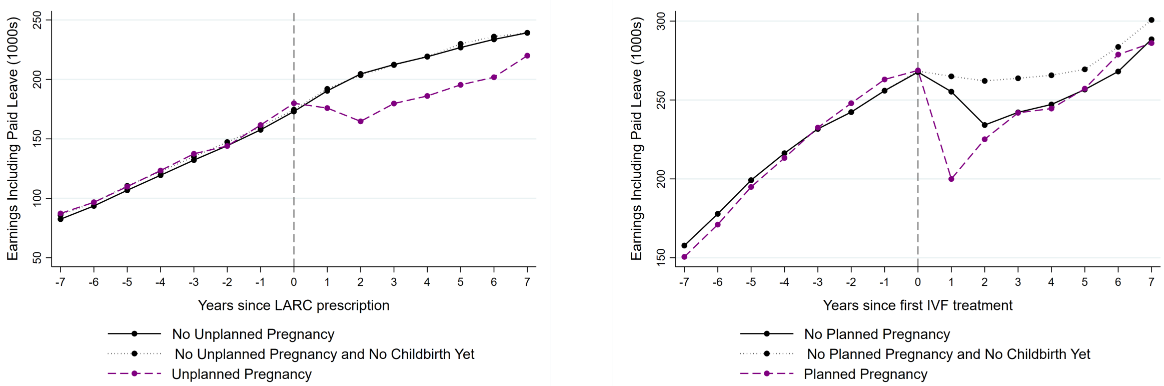
(e) Hours



(f) Hours

Note: This figure displays the impact of successful IVF procedure and 95% confidence interval (y-axis) by time since IVF procedure as measured by fertility drug prescriptions (x-axis). The vertical dashed line marks the year of fertility prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of IVF procedure and age. Control group: Women who do not conceive as a result of their first IVF procedure. Treatment group: Women who conceive as a result of their first IVF procedure. Sample: Women born in 1965-83 with no prior child births at the time of IVF procedure.

Figure A13: Conditional Means for Not-Yet-Treated

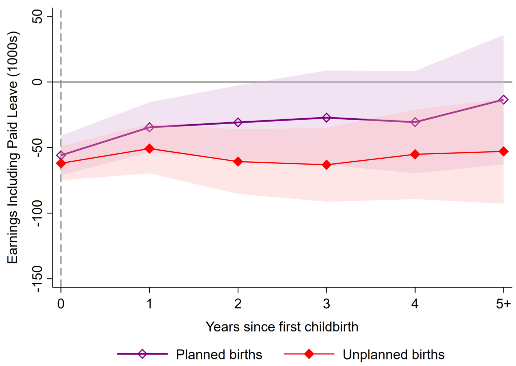


(a) Unplanned pregnancy (vs. no pregnancy)

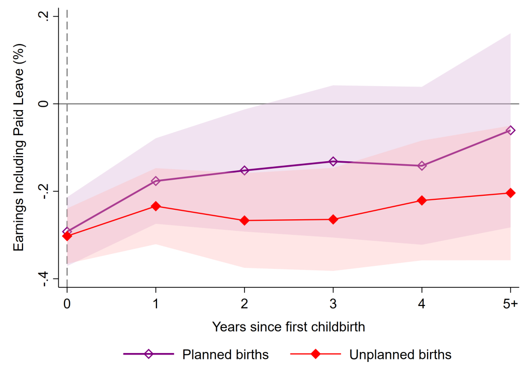
(b) Planned pregnancy (vs. no pregnancy)

Note: This figure displays the time path of earnings for women who experience an unplanned pregnancy (left hand side) and planned pregnancy (right hand side), compared to those women in the control group who do not yet have a child in a given year (dashed line).

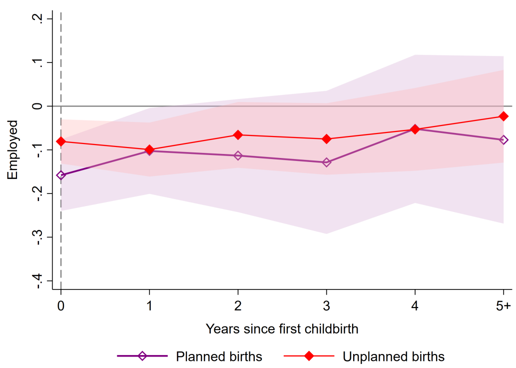
Figure A14: Dynamic Effects of Planned Births: Weighted



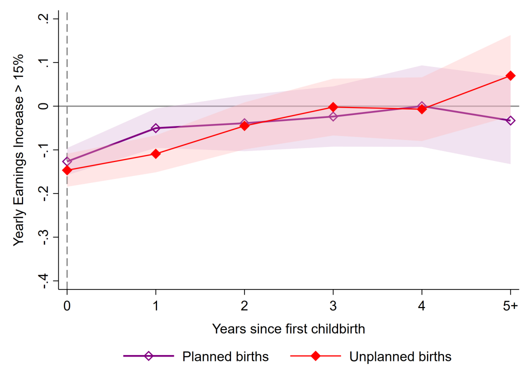
(a) Earnings including paid leave



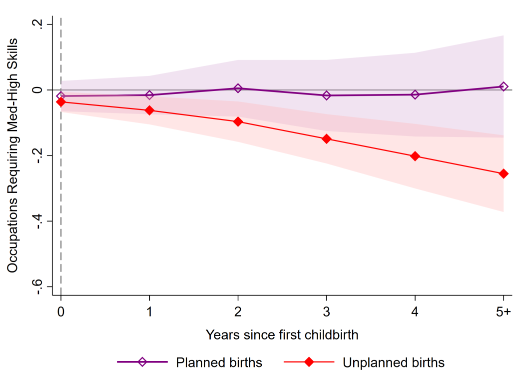
(b) Earnings including paid leave (%)



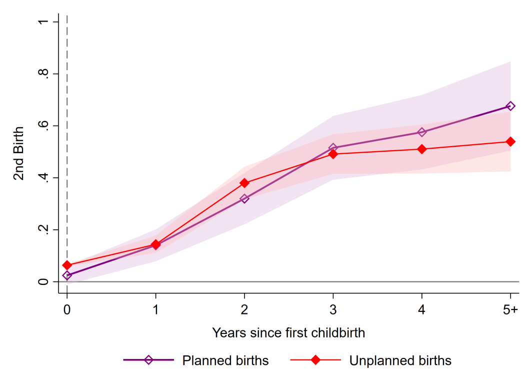
(c) Employed in the last week of November



(d) Promotion



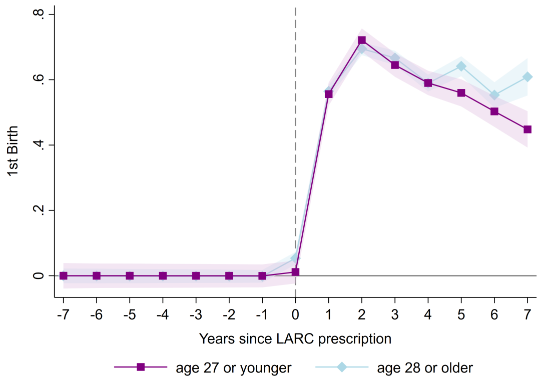
(e) Occupation requires medium-high-managerial skills



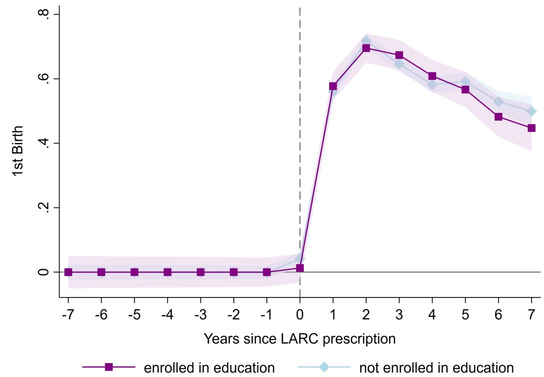
(f) 2nd childbirth

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 purple; the effects estimated in a subsample of women who wanted to delay children (LARC users), in red. The subsample of women undergoing IVF is weighted to have similar characteristics to the subsample of LARC users.

Figure A15: Matching Analysis: First Stage Effect of Unplanned Pregnancies on Having First Child



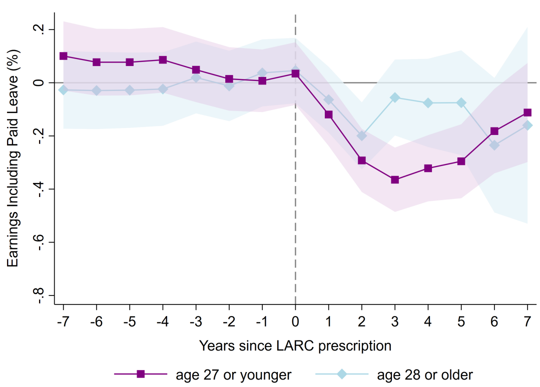
(a) Age



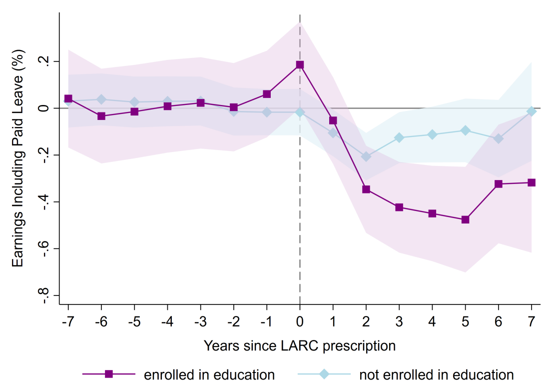
(b) Enrolled in education

Note: This figure displays the impact of an unplanned pregnancy on birth by time since LARC by age in panel (a) and by enrollment in education the year before prescription in panel (b). The vertical dashed line marks the year of LARC prescription ($t = 0$). Controls include a fully saturated model with indicators for year and age at LARC prescription.

Figure A16: Matching Analysis: Heterogeneous Effect of Unplanned Pregnancies on Earnings



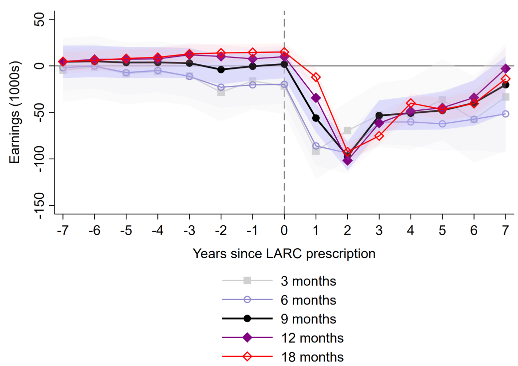
(a) Age



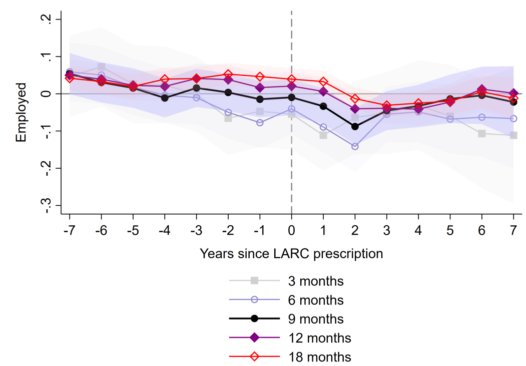
(b) Enrolled in education

Note: This figure displays the impact of an unplanned pregnancy by time since LARC by age in panel (a) and by enrollment in education the year before prescription in panel (b). The vertical dashed line marks the year of LARC prescription ($t = 0$). Controls include a fully saturated model with indicators for year and age at LARC prescription.

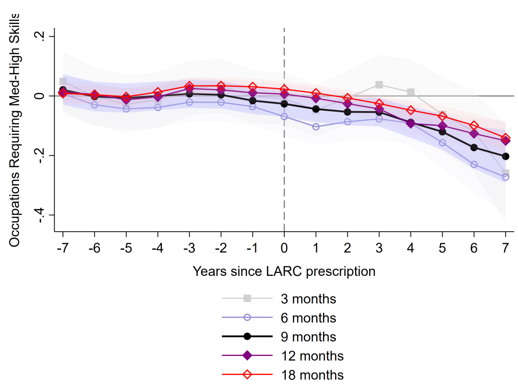
Figure A17: Matching Analysis: Robustness to Unplanned Pregnancy Window



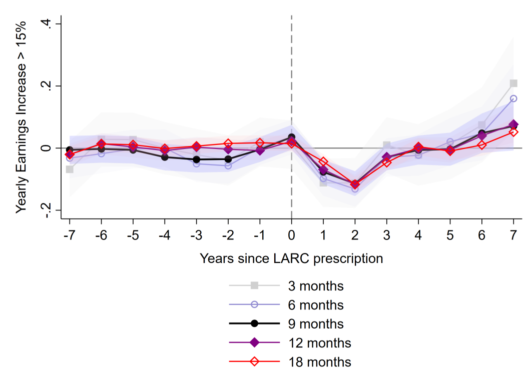
(a) Earnings



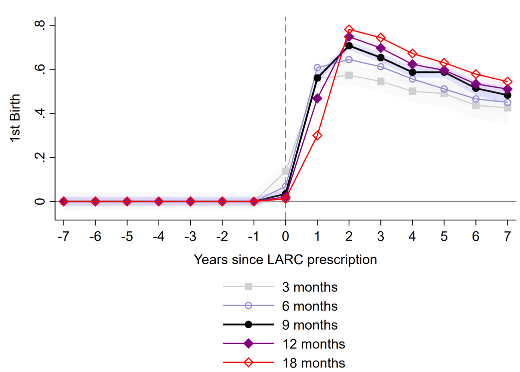
(b) Employed in the last week of November



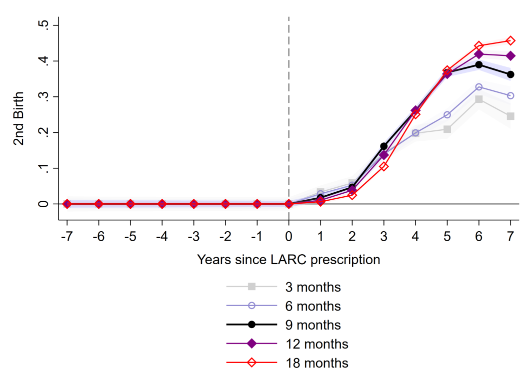
(c) Occupation requires medium-high-managerial skills



(d) Promotion



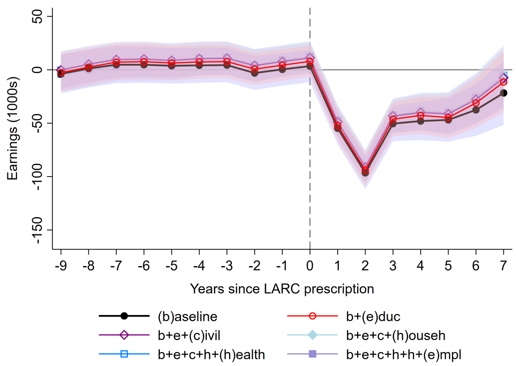
(e) 1st Birth



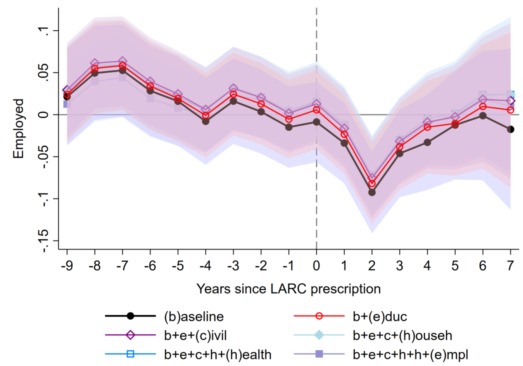
(f) 2nd Birth

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 LARC prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of LARC prescription and age. Control group: Women who do not conceive within 3, 6, 9, 12, or 18 months of LARC prescription. Treatment group: Women who conceive the first child within 3, 6, 9, 12, or 18 months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription.

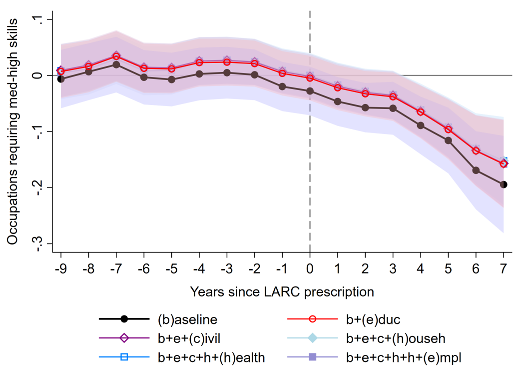
Figure A18: Matching Analysis: Robustness to Pre-Treatment Controls



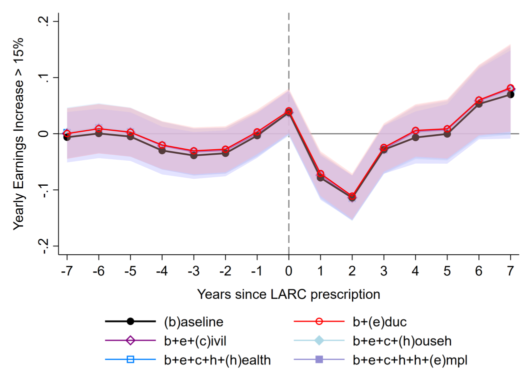
(a) Earnings



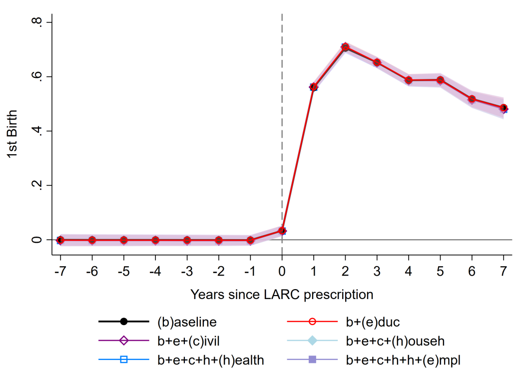
(b) Employed in the last week of November



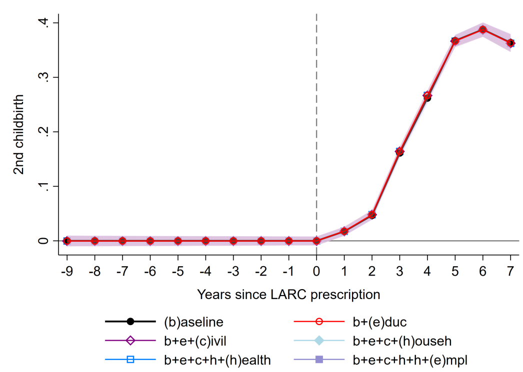
(c) Occupation requires medium-high-managerial skills



(d) Promotion



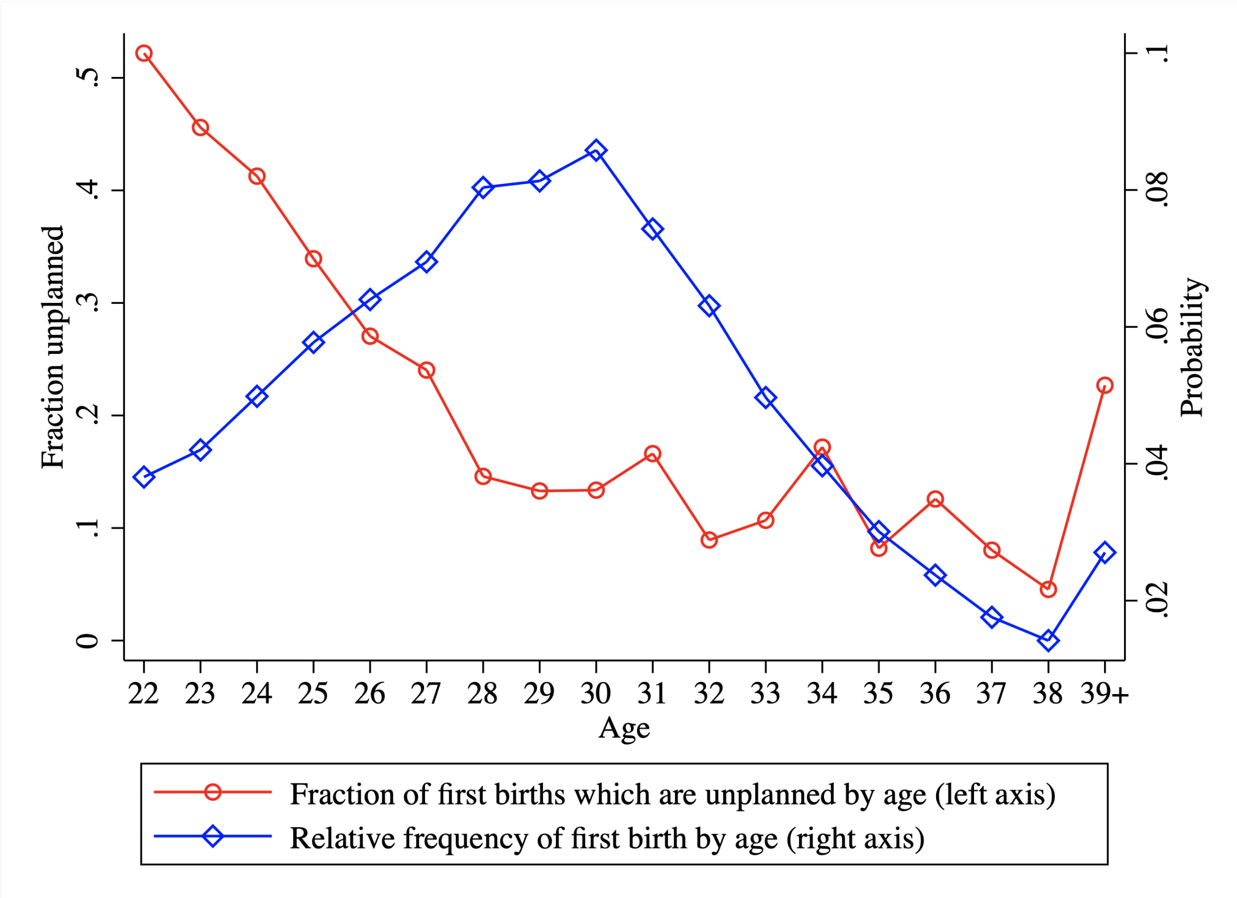
(e) 1st Birth



(f) 2nd Birth

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 LARC prescription ($t = 0$). Income is measured in hundred thousands real 2010 SEK. Controls include a fully saturated model with indicators for year of LARC prescription and age. We compare to models that additionally include controls for education (indicators for level and field of highest completed education, and enrollment in education and college education), civil status (married, divorced, and years since last change in civil status), household composition (number of children and number of prime-age men in the household), health measures (any sickness income), and employment status (employment and any unemployment) the year before the LARC prescription. Control group: Women who do not conceive within one year of LARC prescription. Treatment group: Women who conceive first child within nine months of LARC prescription. Sample: Women born in 1965-83 with no prior child births at the time of LARC prescription.

Figure A19: Distribution of Births by Age and Fraction Unplanned



Note: This figure presents (in red circles) the fraction of births by age, where we assign births as unplanned based on the rate that mothers report unwanted or mistimed live births in the US National Family Growth Survey 2011-2019. In blue diamonds we plot the fraction of all births within the age range 22-43 (the age of our sample) by the age at which they occur in Sweden in 2005.

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	30.84	30.54	33.80	33.13	29.87	29.67
Prenatal environment							
Child Gestational Age (GA) at 1st prenatal visit (weeks)	11.24	10.29	10.85	10.95	11.21	10.82	10.97
Mother's height (cm)	166.57	167.01	166.00	166.98	167.10	166.51	166.24
Mother's weight at 1st prenatal visit (kg)	67.27	69.81	69.57	68.15	67.73	68.14	67.16
Mother's BMI at 1st prenatal visit	24.23	25.02	25.29	24.43	24.24	24.57	24.33
Mother smoking...							
3 months prior to conception	0.15	0.18	0.26	0.09	0.09	0.15	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 psychological diagnosis	0.02	0.03	0.05	0.02	0.02	0.02	0.03
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	0.07	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	3.65	3.59
Child							
Any congenital anomalies	0.04	0.04	0.05	0.04	0.05	0.05	0.04
APGAR score 1 minute after birth (0-10)	8.61	8.61	8.43	8.56	8.62	8.58	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	3,446	3,407	3,355	3,318	3,360	3,294
Low Birth Weight (LBW) birthweight<=2500g	0.05	0.05	0.07	0.09	0.09	0.08	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	39.26	39.05	39.02	38.97	39.00	38.79
Premature (GA < 34 weeks)	0.02	0.03	0.06	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
Fullterm (GA >= 34 weeks)	0.93	0.92	0.92	0.90	0.90	0.90	0.88
Days in hospital	3.99	3.91	3.80	4.84	4.71	4.51	4.83
N	300,535	3,920	272	5,583	2,274	5,335	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 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 (our IVF treatment group). 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.

Table A2: 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	0.22	0.23	0.25	0.27
net days on pregnancy leave	6.78	5.46	7.77	6.35	6.67	7.98	9.75
any sickleave	0.43	0.49	0.50	0.52	0.52	0.52	0.52
net days on sickleave	16.67	13.77	23.48	18.34	18.18	22.45	23.11
total pregnancy- and sickleave income (SEK 1000s)	15.14	18.32	22.13	19.78	20.21	20.10	22.02
Mother postpartum leave							
any parental leave	0.97	0.98	0.98	0.98	0.98	0.97	0.97
days on parental leave	262.48	229.83	281.29	225.49	222.03	259.35	250.30
net days on parental leave	201.84	94.95	182.64	143.07	135.56	190.78	181.97
total parental leave compensation (SEK 1000s)	140.91	146.46	139.81	146.51	145.53	127.60	124.53
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	50.88	50.11	50.79	48.85	45.03	44.36
net days on parental leave	40.94	17.69	29.46	30.22	29.04	30.96	32.39
total parental leave compensation (SEK 1000s)	42.11	45.62	35.90	44.32	43.48	34.59	34.12
Conditional on father living with mother and child							
any parental leave	0.82	0.82	0.78	0.83	0.84	0.82	0.81
days on parental leave	61.22	56.32	60.29	53.02	51.38	48.13	47.78
net days on parental leave	44.04	19.41	35.19	31.48	30.58	32.98	34.93
total parental leave compensation (SEK 1000s)	45.43	50.82	43.35	46.27	45.84	36.90	36.85
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	0.92	0.93
3 years after birth	0.89	0.85	0.80	0.93	0.92	0.88	0.89
5 years after birth	0.86	0.72	0.67	0.90	0.88	0.80	0.84
Civil status: Mother married							
at birth	0.43	0.36	0.34	0.61	0.58	0.47	0.43
1 year after birth	0.48	0.41	0.36	0.64	0.62	0.52	0.48
3 years after birth	0.53	0.49	0.46	0.68	0.67	0.55	0.53
5 years after birth	0.57	0.45	0.42	0.70	0.70	0.59	0.61
Civil status: Mother divorced							
at birth	0.03	0.04	0.09	0.02	0.02	0.03	0.03
1 year after birth	0.03	0.04	0.09	0.02	0.02	0.03	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	0.06	0.06
N	300,535	3,920	272	5,583	2,274	5,335	2,183

Note: This table describes the postpartum environment, including parental leave, and parental family- and civil status. 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 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 (our IVF treatment group). 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.

Table A3: Occupation Transitions: LARC and IVF

	LARC		IVF	
Occupation transitions from t=-1 to t=5	**control**	**failure**	**control**	**success**
Same 2-digit occupation	0.43	0.47	0.63	0.64
Same 3-digit occupation	0.39	0.44	0.58	0.61
Switch to occupation with higher skill requirements	0.43	0.32	0.26	0.23
Switch to occupation with lower skill requirements	0.04	0.02	0.09	0.07
Conditional on enrollment in education:				
Switch to occupation with higher skill requirements	0.52	0.35	0.31	0.29
Switch to occupation with lower skill requirements	0.03	0.00	0.06	0.04
Conditional on enrollment in education and switching 3-digit occupation:				
Switch to occupation with higher skill requirements	0.95	1.00	0.85	0.88
Switch to occupation with lower skill requirements	0.05	0.00	0.15	0.12
Service, care, and security (51) at t=-1				
Same 2-digit occupation	0.56	0.64	0.76	0.74
Same 3-digit occupation	0.53	0.60	0.75	0.72
Switch to occupation with higher skill requirements	0.65	0.57	0.65	0.60
Conditional on enrollment in education:				
Switch to occupation with higher skill requirements	0.74	0.69	0.77	0.76

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 two columns (last two columns). The first column includes women born 1965-1983 who received a LARC prescription during 2005-2012 and did not have an unplanned pregnancy, the second column includes the women who had an unplanned pregnancy despite the LARC, the third column includes the women who did not have a successful first IVF treatment, while the last column includes those who who did have a planned pregnancy following successful first IVF treatment. The bottom panel zooms in on the most common 2-digit occupation: service, care, and security (51). Occupation is observed yearly during the period 2004-2013.

Table A4: Occupation Transitions

	Age	
	22-28	**28-34**
Same 2-digit occupation	0.15	0.28
Same 3-digit occupation	0.13	0.26
Switch to occupation with higher skill requirements	0.44	0.41
Switch to occupation with lower skill requirements	0.01	0.02
Conditional on enrollment in education:		
Switch to occupation with higher skill requirements	0.52	0.45
Switch to occupation with lower skill requirements	0.01	0.02
Conditional on enrollment in education and switching 3-digit occupation:		
Switch to occupation with higher skill requirements	0.98	0.96
Switch to occupation with lower skill requirements	0.02	0.04
Service, care, and security (51) initially		
Same 2-digit occupation	0.55	0.68
Same 3-digit occupation	0.52	0.66
Switch to occupation with higher skill requirements	0.65	0.62
Conditional on enrollment in education:		
Switch to occupation with higher skill requirements	0.73	0.74

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 bottom panel zooms in on the most common 2-digit occupation: service, care, and security (51). Sample: All women born 1965-1983 and occupation is observed yearly during the period 1990-2013.

Table A5: 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.

Table A6: Long-Term Effect Among Young LARC Users,
Weighted to Reflect Older LARC User Characteristics

	27 and Younger	
	Unweighted	Weighted
	(1)	(2)
Average earnings loss (years 1-6)	-79.110	-67.242
	(14.320)	(15.239)
...As % of average counterfactual earnings	-0.332	-0.303
	(0.060)	(0.069)
Medium or high skill occ. by year 5	-0.213	-0.254
	(0.067)	(0.071)
Years in medium or high skill occ.	-0.835	-0.961
	(0.182)	(0.177)
Years employed (years 0-6)	-0.609	-0.646
	(0.192)	(0.228)
Number of promotions (years 0-6)	-0.428	-0.566
	(0.118)	(0.123)
Observations	9,744	9,734
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.

Table A7: Long-Term Effect Heterogeneity: IVF/Planned

			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 loss (years 1-6)	-28.453	-39.669	-51.910	-66.303	-25.701	-36.836	-52.966	-68.202	-22.753	-35.080
	(6.237)	(4.286)	(12.933)	(9.553)	(6.853)	(4.685)	(15.249)	(11.861)	(6.894)	(4.700)
...As % of average counterfactual earnings	-0.103	-0.137	-0.234	-0.262	-0.089	-0.123	-0.221	-0.265	-0.080	-0.119
	(0.022)	(0.015)	(0.058)	(0.038)	(0.024)	(0.016)	(0.064)	(0.046)	(0.024)	(0.016)
Medium or high skill occ. by year 5	0.027	-0.133	-0.032	-0.267	0.038	-0.121	-0.055	-0.292	0.057	-0.077
	(0.043)	(0.033)	(0.111)	(0.090)	(0.046)	(0.035)	(0.135)	(0.115)	(0.046)	(0.034)
Years in medium or high skill occ.	0.101	-0.516	-0.231	-1.274	0.156	-0.429	-0.275	-1.299	0.212	-0.298
	(0.151)	(0.105)	(0.403)	(0.309)	(0.162)	(0.111)	(0.498)	(0.384)	(0.163)	(0.108)
Years employed (years 0-6)	-0.157	-0.631	-0.965	-2.253	-0.041	-0.400	-0.775	-1.864	-0.066	-0.363
	(0.108)	(0.086)	(0.401)	(0.314)	(0.106)	(0.088)	(0.414)	(0.348)	(0.107)	(0.086)
Number of promotions (years 0-6)	-0.143	-0.152	-0.181	-0.131	-0.142	-0.150	-0.682	-0.699	-0.080	-0.093
	(0.048)	(0.048)	(0.156)	(0.151)	(0.050)	(0.049)	(0.203)	(0.202)	(0.046)	(0.045)
Observations	36,578	35,993	9,744	9,642	26,834	26,351	7,886	7,886	27,749	27,749
Controls	N	Y	N	Y	N	Y	N	Y	N	Y

Note: This table presents IV-GMM estimates of the dynamic effects of the dynamic effects of planned 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 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 are given by the predicted earnings in the control group when all child age indicators are set to zero. Years in medium or high skill occupation, years employed, and number of promotions 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.

B Data Appendix

We merge several administrative registers via the unique Swedish 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*).

The Medical Birth Registry contains measures of the child's in-utero environment and health status at birth; incl. maternal diagnosis and complications during pregnancy and delivery, child birth weight, indicators for whether the child is heavy or light for gestational age, APGAR score (Apgar, 1952) at 1, 5, and 10 minutes after birth, and child diagnosis at birth for the cohorts born in 1973-83.

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, employment, sector, occupation, earnings, and social transfers.

B.1 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*). This is the income measure we use to proxy promotions. We calculate the running maximum within-individual yearly earnings over time. We define an indicator variable for receiving a promotion in a given year if individual maximum yearly earnings increased by 15% compared to the previous year.

Our main income measure also adds add all income sources related 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 a pregnancy leave benefits if they are unable to work during pregnancy.[73] 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 sickness income ("sjuklön") for the first 14 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*.[74]

B.2 Occupation Measures

Our primary measure of occupation skill requirements is based on the first digit of the Swedish occupation code (*SSYK3*) in LISA.

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

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

We construct an indicator for whether women are in jobs which require managerial responsibilities, "High" theoretical special competence or "Medium" (at least a short university degree) competence.

The 3-digit occupation code is organized hierarchically, with increasing levels of granularity. The first digit is defined as the "occupation area", which is the broadest category. Each "occupation area" is split into multiple "primary groups" (represented by the second digit); these, in turn, are split into multiple "occupation groups" (represented by the third digit). In total, there are 11 "occupation areas", 27 "primary groups", and 113 "occupation groups". The first 3-digits of the Swedish occupation code (SSYK96) have an almost one-to-one mapping to the international ISCO88 code that we use to merge with the O*Net data to construct measures of workplace flexibility, based on Bang (2022) applied to Swedish population distributions across occupations.[75]

B.3 Employment

In a given year, a person is classified as employed Register Based Labor Market Statistics (RAMS) based on their status in November of that year. The goal is to adhere as closely as possible to the ILO (International Labor Organization) definition of employment, namely carrying out at least one hour of paid work per week.

B.4 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 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 $TJOMF$).

B.5 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

[75] Bang (2022) follows Goldin (2014) in using five primary occupational characteristics in O*Net to characterize flexibility, but extends beyond college-degree holders. The five characteristics are variables from O*Net called Structured Work, Freedom to Make Decisions, Contact with Others, Time Pressure, and Establish Interpersonal Relationships with Others.

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.

B.6 Family

Each family is identified by a family ID (*FamId*). The family ID is based on the family definition in the Total Population Register (RTB). It equals the individual ID number of the oldest person of a maximum of two generations that have relationships with each other *and* have a registered address on the same property.[76] When more than two generations live together, the family ID is based on the youngest generation if unmarried. Only unmarried singles who have the same registered address as their parents relate to the same family. An individual can only be part of 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 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.

C 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.[77] 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.

C.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

[76] The relationships include spouse, registered partner, cohabitant who has children together (biological / adoptive), biological parent, adoptive parent, guardian (for children under 18 years of age) and parent other than guardian (foster parent).

[77] 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.

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 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 unemployment insurance fund membership and 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.[78]

C.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.[79]

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 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

[78]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.

[79]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.

the elderly, or certain aid for newly-arrived immigrants.[80]

C.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.[81] 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.

C.4 Health Care

Healthcare is universal, mostly public, and organized at the county level.[82] Co-payments and out-of-pocket expenses are generally low and capped.[83]

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). 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.[84]

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.

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

[81] 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.

[82] 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.

[83] 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.

[84] 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."

C.5 Abortion

Swedish abortion law has not changed during our sample period and abortion access is relatively unrestricted. Abortion is legal in Sweden until the 18th week of pregnancy. However, abortions after 18 weeks are allowed only if the fetus is deemed unable to survive Ministry of Social Affairs (1974).

We observe whether a woman discussed abortion due to an unwanted pregnancy, diagnosis code Z640. This is similar to the definition used by Janys and Siflinger (2021) except that we do not take the intersection with the actual abortion procedure. 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 later. We categorize all women who do not give birth and who had such a meeting as having an abortion, though it is possible that some of these abortions were spontaneous.

D More General Model

Extending the model in Section 2 to an infinitely lived agent, we can write

$$\dot{h} = A(i(t)h(t))^{\alpha} - \delta h(t)$$

where $\alpha \in (0, 1)$, $A > 0$, and $\delta \in [0, 1)$. Letting the flow death rate be given by ν, and setting the wage per unit of human capital to 1, the individual's problem is to

$$\max \int_0^{\infty} e^{-(r+\nu)}(1 - i(t))h(t) \quad s.t. \quad \dot{h} = A(i(t)h(t))^{\alpha} - \delta h(t)$$

One can show that individual's human capital choices converge to a steady state given by

$$h^* = \frac{A}{\delta}\left(\frac{r + \nu + \delta}{\alpha A}\right)^{\frac{\alpha}{\alpha-1}}$$

which is increasing in A.[85] This means that a permanent reduction in A caused by the presence of children would decrease steady state human capital.

If instead children represented a temporary shock to the productivity of human capital accumulation, then we would expect individuals to eventually recover their human capital, unless $\delta = 0$ so that there is no depreciation of human capital. If agents are finitely lived, however, even a temporary shock would translate to a reduction in human capital, similar to the model presented in Section 2.

[85] See Acemoglu and Autor (2009) for a complete derivation under more general functional form assumptions.