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Precautionary Fertility: Conceptions, Births, and Abortions around Employment Shocks

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ABSTRACT
This paper studies the effects of employment shocks on births and induced abortions. We are the first to show that abortions play a role in fertility responses to job displacement. Furthermore, we document precautionary fertility behavior: the anticipatory response of women to expected labor market shocks. Using individual-level administrative data from Hungary, we look at firm closures and mass layoffs as conditionally exogenous employment shocks in an event study design. After establishing that both shocks have a similarly large and persistent negative effect on employment and wages, we show that women already react to the anticipation of these shocks, and their fertility responses differ substantially for firm closures and mass layoffs. We find that abortions increase by 88% in the year *before* firm closures, while the number of births is not affected. Mass layoffs have no significant effect on abortions in the preceding year but increase the number of births by 44%. Mass layoffs and firm closures differ in one crucial aspect: pregnant women cannot be laid off until the firm exists, but no such dismissal protection is available in the case of firm closures. Thus, when dismissal protection is available, anticipated employment shocks increase the number of live births, whereas when it is not, they increase the number of abortions. These results suggest that dismissal protection has the potential to support women to keep pregnancies at times of economic shocks.

JEL codes: I12, J13, J65
Keywords: Abortion, Birth, Pregnancy, Mass layoff, Firm closure

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Gyermekvállalás elővigyázatosságból: Fogantatás, élveszülés és abortusz foglalkoztatási sokkok idején

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ÖSSZEFOGLALÓ

Tanulmányunkban azt vizsgáljuk, hogy a foglalkoztatási sokkok milyen hatással vannak a születések és abortuszok számának alakulására. Ez a tanulmány az első a szakirodalomban, amely megmutatja, hogy az elbocsátásokra adott fertilitási válaszokban az abortusz döntések is szerepet játszanak. Ezen kívül dokumentáljuk az óvatossági gyermekvállalás jelenségét, vagyis, hogy a nők a várható munkaerőpiaci sokkokra megelőlegező fertilitási választ adnak. Hazai egyéni adminisztratív panel adatokon cégbezárásokat és tömeges létszámleépítéseket, mint feltételesen exogén sokkokat vizsgálunk eseménytanulmány (event study) módszerrel. Először megmutatjuk, hogy mindkét típusú sokk hasonló nagyságrendű negatív hatással van az érintett cégeknél dolgozó nők foglalkoztatottságára és átlagbérére. A várható sokkok már a megelőző évben hatással vannak a nők fertilitására, és ezek a hatások igen különbözőek cégbezárások és leépítések esetén. Becslésünk szerint a cégbezárások előtti évben az abortuszok száma 88%-kal nő, míg a születések száma nem változik számottevően. A létszámleépítések ugyanakkor nem hatnak az abortuszokra, viszont 44%-kal növelik a születések számát. Az eltérést a kétféle sokk közti különbség magyarázza: amíg egy cég működik, fő szabály szerint tilos terhes nőket elbocsátani, vagyis a létszámleépítések esetén a terhesség megvédi a nők állásait. A cég bezárásakor a felmondási védelem elvész, és a terhes nők is elveszítik az állásukat. Így, ha elérhető a felmondási védelem, akkor a várható sokkok megnövelik a születendő gyermekek számát, míg, ha nincs ilyen védelem, akkor a sokkok az abortuszok számát emelik meg. Az eredményeink alapján az anyák felmondási védelme hatékony eszköze lehet annak, hogy az állam támogassa a nőket a terhességeik megtartásában a gazdaságilag nehezebb időszakokban.

JEL: I12, J13, J65
Kulcsszavak: abortusz, szülés, terhesség, tömeges létszámleépítés, cégbezárás

Precautionary Fertility: Conceptions, Births, and Abortions around Employment Shocks

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Abstract

This paper studies the effects of employment shocks on births and induced abortions. We are the first to show that abortions play a role in fertility responses to job displacement. Furthermore, we document precautionary fertility behavior: the anticipatory response of women to expected labor market shocks. Using individual-level administrative data from Hungary, we look at firm closures and mass layoffs as conditionally exogenous employment shocks in an event study design. After establishing

Corresponding author: Agnes Szabo-Morvai (KRTK KTI and University of Debrecen, szabo-morvai.agnes@krtk.hu) *Acknowledgments*: We thank Balazs Reizer, Timea Laura Molnar, Kamila Cygan-Rehm, Peter Szobonya, Noemi Kreif, Adam Szeidl and the participants of the ESPE, EALE, WOLFE Conferences, KRTK and Health and Population Lendulet Seminars for their useful comments and discussions. We thank Norbert Kiss for excellent research assistance. The authors gratefully acknowledge financial support from the Hungarian National Scientific Research Program (OTKA), Grant no. OTKA FK138015 and FK131422 and the Lendület programme of the Hungarian Academy of Sciences (grant number: LP2018-2/2018). The administrative database used in this paper is a property of the National Health Insurance Fund Administration, the Central Administration of National Pension Insurance, the National Tax and Customs Administration, the National Employment Service, and the Educational Authority of Hungary. The data was processed by the Databank of the Centre for Economic and Regional Studies. The present study has been produced using the corporate financial statement and performance statistics data files of the Hungarian Central Statistical Office. The calculations and the conclusions within the document are the intellectual product of the authors.

that both shocks have a similarly large and persistent negative effect on employment and wages, we show that women already react to the anticipation of these shocks, and their fertility responses differ substantially for firm closures and mass layoffs. We find that abortions increase by 88% in the year *before* firm closures, while the number of births is not affected. Mass layoffs have no significant effect on abortions in the preceding year but increase the number of births by 44%. Mass layoffs and firm closures differ in one crucial aspect: pregnant women cannot be laid off until the firm exists, but no such dismissal protection is available in the case of firm closures. Thus, when dismissal protection is available, anticipated employment shocks increase the number of live births, whereas when it is not, they increase the number of abortions. These results suggest that dismissal protection has the potential to support women to keep pregnancies at times of economic shocks.

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1. Introduction

In modern labor markets with high female labor force participation rates, fertility decisions are increasingly determined by the compatibility of career and family goals (Doepke et al., 2022). Women no longer decide between a career or a family, but their aim is to have it all. Family policies support these goals by guaranteeing mothers' access to equal opportunity and equal treatment in the workplace. Besides providing maternity leave regulations, many countries also protect mothers from dismissal during pregnancy and maternity leave and guarantee them the right to return to their previous job (ILO, 2010).

Maternity policies might also play an important role in a situation where careers are especially vulnerable: around a job loss. It is well established that job displacement is related to large and persistent earnings and employment losses (Jacobson et al., 1993a; Bertheau et al., 2022). These losses are generally found to be larger for women who are more likely to end up in part-time employment or in unstable jobs than men (Illing et al., 2021). As a consequence, women reduce their fertility after a job loss with the aim of getting their career back on track (Del Bono et al., 2012; Huttunen and Kellokumpu, 2016). Less is known about how fertility responds in anticipation of a job loss, however. In this paper, we argue that in an environment with maternal dismissal protection, pregnancies can be used as a precautionary strategy to avoid job displacement. The idea is that a woman who is aware of economic problems and anticipates a potential mass layoff at her workplace chooses to become pregnant to protect herself against the layoff risk and wait out the crisis during the maternity leave period. This strategy will be successful as long as the firm survives the temporary crisis. In case of a firm closure, the precautionary mechanism breaks down and the woman might choose to terminate the pregnancy.

We study Hungary, a country that has adopted the latest ILO Maternity Protection Convention according to which pregnant women are protected from dismissal.[1] Hungarian family policy also offers generous leave benefits for employed mothers and

[1] Convention 183, Article 8

lower benefits if they are unemployed. We make use of unique and rich administrative matched employer-employee data which allows us to identify mass layoff events and plant closures, and which can be linked to health records with individual information on births and abortions. These data offer an ideal setting to study fertility responses around job loss.

We start by documenting that large layoff events are on average preceded by indicators of economic problems at the firm. While employment stays relatively stable, we show that orders decline significantly in the 6 months leading up to the layoff event. Second, we compare employment and earnings outcomes of women employed in firms with a mass layoff or a closure with a comparison group of similar women employed in firms with no layoff event. In line with the previous findings, we show that women affected by a layoff event at their workplace experience economically large losses after the event. The magnitude of the losses is similar in both types of layoff events. Third, we study the development of conceptions, births, and abortions around the layoff event. In the year preceding the layoff event, we find an increase in conceptions of women employed in firms with mass layoffs or closures relative to the comparison group. This result is in line with the precautionary motive as women who anticipate the layoff respond by becoming pregnant. Birth and abortion outcomes of pregnancies conceived in the year preceding the event differ by event type, however. While births increase in firms with a mass layoff, abortions increase in firms that are closing. Effect sizes are of the same magnitude in absolute terms: in case of a mass layoff event births increase by 8 out of 1000 women, and abortions increase by 7 out of 1000 women before a closure event. This finding is evidence of the riskiness of the precautionary strategy. A pregnancy cannot protect a woman's job or career if the firm ceases to exist. She must find a job with a new employer and loses the high maternity benefit if she becomes unemployed before giving birth.

We perform heterogeneity analysis to test the robustness of these findings. First, we identify groups with relatively high pregnancy rates who should be more flexible in timing their fertility in response to the threat of a layoff. We show that effects are indeed driven by young women and women with a high probability of getting pregnant. Second, we identify groups with high abortion rates conditional on getting pregnant. These women might be more likely to use abortions as a form of contraception. Our results show that women with high abortion probability are driving the increase in abortions in the closure sample. However, there is no difference in fertility responses between women with high and low probabilities of abortion in case of a mass layoff. These findings suggest that our results are due to strategic fertility decisions rather than responses to unplanned pregnancies.

Our research contributes to several strands of the literature on the effects of economic shocks on fertility and abortions. First, a large literature has studied the cyclicality of fertility in various settings (Dehejia and Lleras-Muney, 2004; Adsera, 2005). But relatively few studies address the effects of economic conditions on abortions. The primary objective of these studies is to test whether in times of economic hardship abortions are increasingly used to terminate unplanned pregnancies. Several studies confirm this hypothesis and document that lower unemployment or increased generosity of income support programs tend to reduce abortion rates (Blank et al., 1996; González and Trommlerová, 2021; Herbst, 2011). Abortion rates in Hungary are generally high compared to Western European countries, like Germany, and closer to rates in the UK and the U.S., which makes our findings relevant to this literature. Our results reveal an interesting time pattern. We show abortions only respond in anticipation of the initial employment shock, but in the years after the shock effects on abortion rates are smaller and insignificant. This suggests that abortions are less important in dealing with income losses in the longer run.

Second, studies investigating the effects of job displacements at the individual level have – due to the lack of data on abortions – focused on fertility responses *after* the loss of a job and studied total fertility effects by looking at medium to long run outcomes (Del Bono et al., 2012; Huttunen and Kellokumpu, 2016). Our medium-term results in the first three years after displacement show a slight decline in the number of births which confirms the previous literature. We contribute a new result on abortions and find no significant change in abortions relative to the comparison group in the years after displacement.

Third, we also contribute to the literature studying the anticipation of job loss. Survey evidence confirms that individuals have some prior knowledge about a future job loss (Hendren, 2017; Mueller and Spinnewijn, 2022). But it has been hard to deal with anticipation in a setup studying employment effects of mass layoffs and plant closures, as affected individuals are by construction required to remain employed until the shock occurs (Schwerdt, 2011). Halla et al. (2020) conclude that wives of displaced husbands adjust their job search intensity only after the shock has occurred. Our fertility results draw a more nuanced picture indicating that women anticipate their own job loss.

Lastly, our results also contribute to the large literature studying the effects of family policies. We show how maternity policy can affect fertility decisions when women face a high risk of job loss. Women who remain employed and thus eligible for high maternity benefits choose to bring forward their planned fertility to the period of uncertainty and thereby potentially rescue their careers. But women who lose their jobs and their access to high maternity benefits are more likely to terminate their pregnancies. This result implies that there is still scope to improve protection.

In the next section, we discuss the trends in births, abortions, and the relevant institutional background. We present a simple model of the anticipatory fertility decisions in Section 3. Section 4 describes the data. The empirical strategy is introduced in Section 5. We present our main results and the related robustness checks in Sections 6 and 7. We conclude in Section 8.

2. Fertility trends and institutional background

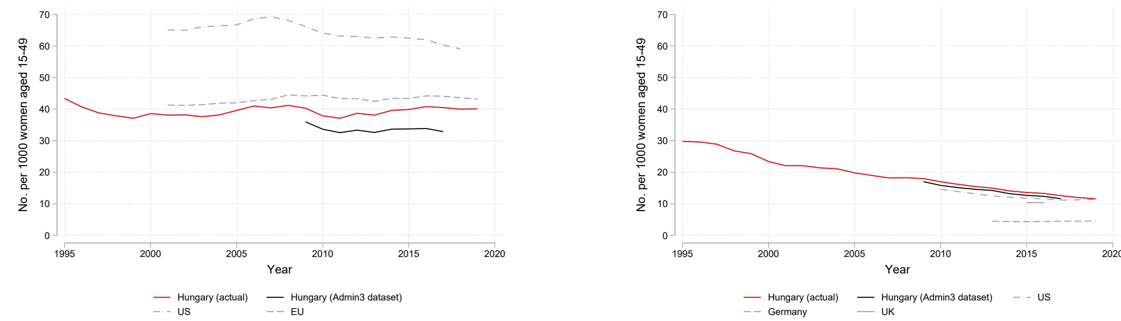
Births and Abortions. Hungary is a small developed country with low fertility, wide access to abortions, and a generous state-financed maternity benefits system. To put the Hungarian institutional and fertility landscape in context, we present it along with data on other developed countries.

The number of births per 1000 women of reproductive age (15 to 49 years) was around 40 in Hungary in our period of interest (2009-2017). This birth rate is close to the EU average of 43 to 44 and lower than the birth rate above 60 in the US in this period (Figure 1a).

In Hungary, the number of abortions has been steadily declining since the '90s, but in 2016 it was still 33% of the number of births. The abortion rate, i.e abortions

Figure 1: Birth rates and abortion rates (1995-2020)

(a) Birth rates (b) Abortion rates



Data source: US: CDC and Guttmacher Institute; EU: Eurostat birth data and Eurostat abortion data; HU: KSH
Note 1: US figures refer to women of age 15 to 44. As fertility is lower at the ages of 45 to 49, this in itself could lead to higher birth rates in the US even if the age-specific fertility was the same. But the difference is not substantial. In Hungary, the birth rate in the 15-44 year age group is 38.2, compared to the 37.1 birth rate in the 15-49 years age group.
Note 2: The difference between the official Hungarian live birth statistics and our estimation data (Admin 3) stems from omitting births in private hospitals and births at home.
Note 3: The EU average of abortion rates is not available due to missing data for some countries. Instead, we report selected country-level data.

per 1000 women of age 15 to 49 (15 to 44 in the US). was 13.3 in 2016, slightly higher compared to the US (11.6) and the UK (10.4), and significantly higher compared to Germany (4.4). (Figure 1b)

Most births and abortions in Hungary take place in public healthcare institutions. Deliveries are financed by the National Health Insurance Fund which covers every citizen during the observation period. Abortion is not covered by this fund, but the price is low, about USD 90 to 100 in the period of our study (37 to 41 percent of the local minimum wage in 2010), and it can be further decreased if the woman proves financial difficulties. According to the categorization of the Guttmacher Institute, access to abortion is very easy in Hungary, similar to most developed countries (Singh et al., 2018). Abortions can be legally carried out on request before the 12th week of pregnancy, after having two consultations with the staff of the Family Protection Service[2]. All legal abortions are carried out surgically, as abortion pills are not

[2] Law 1992/79.

authorized.[3]

Family Policy. Hungary provides a generous system of maternity benefits, especially for employed women (OECD, 2022). Child-related benefits (Appendix Table A.5) are linked to previous employment and wages, and women are generally eligible for benefits until the 2nd birthday of the child. Specifically, women who have been employed for at least 12 months in the two years preceding childbirth and are employed until 42 days before childbirth, are eligible for a baby-care allowance until the child is 6 months old, and a childcare benefit from 7 to 24 months of age of the child.[4] Both the baby-care allowance and the childcare benefit pay 70 percent of the previous wage, but while the baby-care allowance is uncapped, the childcare benefit is maximized at a fairly high level (1.4 times the minimum wage). If a woman becomes unemployed during pregnancy, she will be entitled to a 50 to 70 percent lower amount.

Dismissal protection laws prohibit firms from laying off a pregnant employee, once she has informed the employer about the pregnancy, except if she seriously neglects her duties. Also, she has a guaranteed right to return to her previous job at the end of maternity leave. In our data, 41 percent of non-pregnant women get displaced in the mass layoff sample, while the same share for pregnant employees is only 20 percent, showing that pregnancy substantially decreases the layoff risk [5].Similarly strong dismissal protection policies are implemented in many European countries (e.g. Austria, Belgium, France, Germany, Italy, etc.). In other countries (e.g. USA,

[3]As a minor exception, abortion pills were used by a private medical institution in Hungary between 2010 and 2012. (Index, 2012)

[4]Women can be also eligible for a fraction of the benefit if they are not employed but pay social security contributions for some other reason. For example, if she has sufficient employment history, but is unemployed in the month of the delivery, she receives a child benefit of 70 percent of the minimum wage.

[5]Even if dismissal protection was perfect, it would be possible that some women are displaced in our data while they are pregnant, first because we include voluntary separations from the firm as well, second because pregnant women can be dismissed if they do not fulfill work requirements, and third because not every woman announces pregnancy to the employer, and dismissal protection can be only enforced in this case. In addition to these, anecdotal evidence shows that some employers try to trick the laws to be able to dismiss pregnant employees, e.g. pressuring the pregnant woman informally to leave the job ”voluntarily”.

UK, Canada), dismissal protection is weaker and is restricted to protection from discriminatory dismissal (ILO, 2022).

3. Fertility Decisions around Job Displacement: Theoretical Framework

The empirical literature shows that fertility decisions are shaped by the institutional framework with parental leave regulations and dismissal protection laws (see e.g. Lalive and Zweimüller (2009); Fitzenberger and Seidlitz (2023); De Paola et al. (2021)) as well as by economic conditions. In this section, we outline a short theoretical framework that explains how institutions and economic shocks might interact in determining fertility decisions. We follow the spirit of dynamic models of fertility (Hotz et al., 1997) where a woman decides on the optimal timing of birth. This framework will be useful to motivate our interpretation of fertility responses in anticipation of the two types of employment shocks considered in our setup, mass layoffs, and firm closures.

In the Hungarian context, the level of dismissal protection and leave benefits for pregnant women differs substantially between job displacements from firm closures and mass layoffs. This is due to two features of family policy. First, dismissal protection for pregnant women is only available as long as the firm exists but is lost when the firm closes. Second, high maternity benefits and the option to return to the previous job after the leave are only available for employed women. But a woman who loses her job from firm closure during pregnancy falls to the low benefit level and has no job to return to.

We consider an employed woman who decides whether or not to get pregnant. She derives income from employment while working and parental leave benefits after giving birth. We assume that her income increases with her job tenure. After giving birth the mother takes a period of parental leave, receives the benefit, and subsequently returns to her previous job. A layoff is associated with the loss of firm-specific capital and the need to restart the career with a new employer, which puts her at a lower position in the tenure profile. Figure 2 schematically summarizes income flows around the birth of a child in Panel (2a) and in case of a job loss in Panel (2b).

Next, we consider how these career interruptions interact to determine the timing of fertility decisions for women who anticipate a job loss. Panel (2c) shows how a precautionary pregnancy helps avoid income losses from job displacement in a mass layoff. If the woman starts her pregnancy right before the displacement, she is protected from layoff. Instead of having to restart her career at a new firm she collects maternity benefits and thereby waits out the crisis at her firm and then, re-enters the firm after the leave period. Compared to the dashed line which denotes the income profile without pregnancy, precautionary fertility timing can avoid large income losses.

In case of a firm closure, depicted in Panel (2d), things work out worse than that. As the firm stops existing, the woman loses her job. If the firm closes while she is pregnant, she receives low maternity leave benefits unless she manages to find a new job shortly after the firm closure. But finding a job while pregnant is difficult, evidence from the literature supports that displaced pregnant women suffer relatively high losses in employment and working hours (Meekes and Hassink, 2020). In any case, the woman has no option to return to the pre-displacement job with high earnings but she has to restart her career after the maternity leave period. Her income loss from giving birth around job displacement is thus larger than the income loss without birth, which can be seen from the comparison of the dashed and the solid lines.

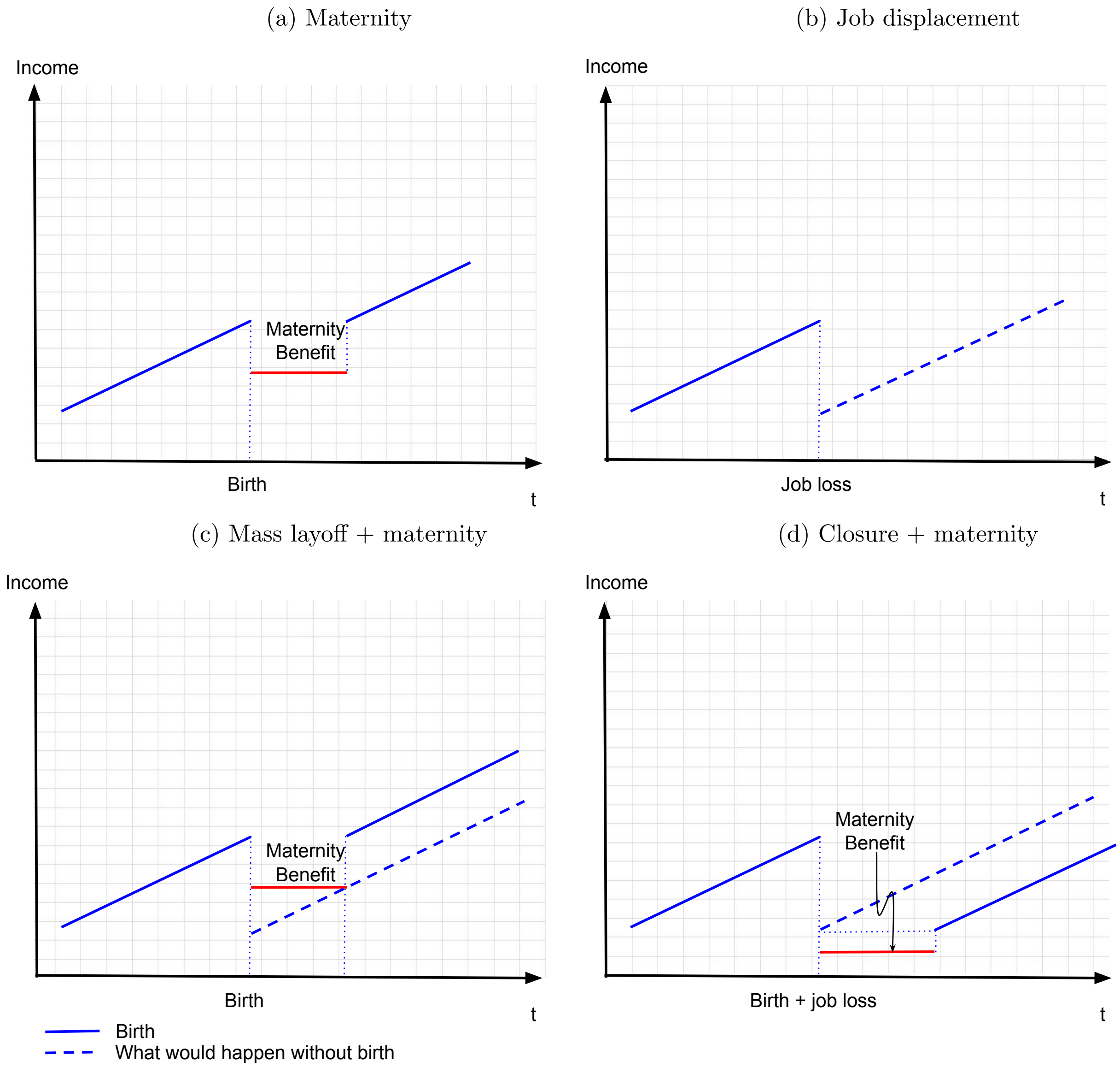
The figures illustrate the risk involved in a precautionary pregnancy for women who do not yet know the exact type of employment shock when they get pregnant. In case of a mass layoff, the precautionary pregnancy helps avoid any income loss from displacement. But if the firm closes the combined income loss from maternity and job displacement is the largest and can only be avoided by terminating the pregnancy. In Section A2 in the Appendix, we present the formal derivations of the model.

4. Data and Sample

4.1. Data

We use administrative individual-level monthly panel data. The data are hosted by the Databank of the Centre for Economic and Regional Studies and link adminis-

Figure 2: Income flows in case of four states of the world



trative records of the National Health Insurance Fund Administration, the Hungarian State Treasury, the National Tax and Customs Administration, the Ministry of Finance and the Educational Authority, based on anonymized social security numbers. For a more detailed description of data compilation and cleaning, see Sebők (2019). The data contain information about 5.17 million people, a random 50 percent sample of the Hungarian population drawn in 2003 and followed until 2017. We observe gender, age, county of residence, employment, occupation, wages, state transfers, registered unemployment, and employer identifiers each month. The employer identifiers are linked to a yearly database covering firm-level information on firm size, sector, foreign ownership, and revenues.

We use daily healthcare records to measure fertility outcomes. This part of the dataset contains the International Statistical Classification of Diseases (ICD) codes and dates of each person's public hospital visits. These data are only available for the years between 2009 and 2017. Based on these records, we can identify births (ICD codes O6, O7, and O8) and surgical abortions (ICD code O04) at public hospitals. These records cover the majority of the relevant events: we observe 88-93 percent of births and 95-98 percent of abortions reported in the official summary statistics (see Table A.4). Some of the childbirth records could be missing because of children born in private institutions, at home, or abroad, while the missing abortions are due to abortions in private institutions.

After aggregating birth and abortion data to the monthly level, we link them to individuals at the estimated date of conception. Throughout the analysis, we use the conception date instead of the date of the actual childbirth or abortion. This means that when we compare abortion and birth frequencies, we talk about pregnancies conceived at the same time. As we do not observe the date of conception, we pin down the conception dates 9 months before childbirth and 2 months before an abortion. Although these are crude approximations, they are very close to the actual conception date in the majority of the cases. To illustrate this, we use administrative birth records[6] which show that 90.9 percent of children were born about 9 months after conception (37^{th} to 41^{st} obstetric weeks), and 83.1 percent of abortions were carried out about 2 months after conception (7^{th} to 11^{th} obstetric weeks) in the period between 2003 and 2020 in Hungary. (See Appendix Figure A.17)

We also provide estimates on the number of pregnancies calculated as the sum of births and abortions, omitting miscarriages. Miscarriages amount to about 10% of

[6] Hungarian Central Statistical Office, Live birth database

all pregnancies according to the official records and their number is rather stable over time. The reason for not using miscarriage data in this study is its weaker reliability. Only less than 10% of miscarriages reported in the official summary statistics can be identified in this dataset, and the date of conception cannot be inferred. Appendix A1.1 discusses the potential impact of measurement error on the estimated effect of job loss on observed pregnancies.

4.2. Sample

4.2.1. Firm closures and mass layoffs

To form our treatment sample, we first identify closures and mass layoffs of private for-profit firms in the data and restrict our attention to those that happened between 2010 and 2014. This way, for each woman we observe at least 1 year of abortion and birth history (and 8 years of employment and earnings history) before the shock and at least 3 years after that.

We define the date of a firm closure as the month when the number of employees drops to 0 and stays 0 for two consecutive years. We take multiple cautionary steps to avoid including "false firm deaths" (Kuhn, 2002), when instead of real closure, a firm ID disappears for some other reason (e.g. ID change due to a new legal form, or a merger). First, we require firms to exist for at least 2 years preceding the closure. Second, similar to other papers in the literature using firm closures for identification (e.g. Eliason and Storrie (2006)), we only include firms where the number of employees is at least 10 at least once in our observed period, based on yearly firm records. We also require that the number of employees present in the data is at least 5 in the month before closure.[7] Third, we exclude firms if more than 30 percent of employees transferred to the same new firm after the month of closure, and if at one receiving firm, at least five people and 30 percent of the new entrants to the firm came from this same sending firm.

The date of a mass layoff is pinned down at the month when the number of

[7] As in our data 50 percent of the Hungarian population is included, requiring 5 employees in the individual-level data means that the firm's actual size before the month of closure/mass layoff is required to be at least 10 on average.

employees decreases by at least 20 percent and does not increase for 12 months following the decrease. If there are multiple mass layoffs at one firm, we include all of them. We drop those few firms which experience a mass layoff and a closure as well. We use the same criteria of firm size and age for downsizing firms and closures. Again, to avoid false layoffs, we exclude firms from the sample if more than 10 percent of previous employees move to the same new firm after the layoff.

4.2.2. Definition of the treatment and control groups

We define two treatment groups: women affected by closures, and women affected by mass layoffs.[8] We include everyone in the sample working at firms about to have a layoff event, even if they are not actually getting displaced. As a result, in the closure treatment sample every woman loses her job, whereas, in the mass layoff treatment sample, only a fraction is displaced (see Figure A.18).

Women in the treatment groups are required to satisfy the following selection criteria: they have to be of reproductive age (15-49 years), work at the firms in the quarter preceding the layoff event, and have at least 12 months of tenure at the time of the event.

We follow the approach of Del Bono et al. (2012) and include not only women who stay at the firm until the last month before the layoff event, but also those who leave two or three months before that. The reason is that workers who stay until the very end are a selected sample. Including early leavers mitigates this selection, however, we exclude those leaving even earlier than three months. As employment of young fertile women is unstable, and we do not observe the reason for leaving a firm, it would be hard to argue that these very early separations are involuntary indeed.

Requiring 12 months of tenure ensures that, in case of giving birth, the woman would be eligible for the high child benefits linked to previous employment, had the firm not closed. It also makes our results comparable to previous studies, using the same tenure criterion (e.g., Del Bono et al., 2012; Huttunen and Kellokumpu, 2016).

Table 1 presents descriptive statistics of treated women in columns (2) and (5).

[8]Women affected by multiple closures or mass layoffs are excluded from the sample. 87 percent is affected by only 1 event.

Women working in closing firms are on average 36 years old, younger, and more likely to receive child benefits, than those working in firms with mass layoffs. Women in closing firms are more likely to work in white-collar occupations but they have on average lower wages than women in firms with mass layoffs. Closing firms tend to be smaller than firms with mass layoffs, they are less likely to be foreign-owned, and they have lower revenue in the year before the event. Note that while all women in the closing firms lose their jobs, only 41% of women working in the mass layoff group are displaced.

To form the control groups, we use a combination of exact matching and propensity score matching on individual and firm characteristics. The reference month, in which the matching is done, is set to the last month before the closure or mass layoff generally[9]. First, for every treatment woman, we find a pool of possible control women who work at non-closing and non-downsizing firms at the calendar time of the reference month and satisfy the other selection criteria used for treatment women (i.e are of reproductive age, and have at least 12 months of tenure at their firm). From this pool of control women, we match exactly on age group (15-19, 20-24, etc), county of residence, and yearly wage category history (0-50000 HUF; 50000-10000 HUF; etc.) from the 4$^{\text{th}}$ year to the 1$^{\text{st}}$ year before the reference month. Note that we do not use the wage in the year of the closure, as these wages might already be affected by the coming shock in the treatment group. The exact matching ensures that the treatment and control women are comparable in the aspects we find most important. They are the same age and from the same region with the same wage history at the time of matching. In addition, matching control women in a specific month automatically pins down the date of the pseudo-event for them.

Then, from the exact matches we select the (maximum) 10 nearest neighbors

[9] For those who leave the firm 2 or 3 months before the closure or mass layoff, the reference month is set to the last month when they still work at the firm

within a caliper based on the propensity score[10]. The propensity score is estimated using a probit model:

$$P(T_i = 1|X) = \Phi(X_i'\beta_i), \tag{1}$$

where T_i is a binary variable equal to 1 for treated women, and X_i denotes a large set of independent variables, including individual and firm characteristics[11]. The following variables in X are measured right before the event: the woman's age (in years), occupation (9 categories), an indicator of having a young child (based on previous child transfers received by the woman), tenure (in months), and experience (in months). We also include longer histories of wages, and months spent employed, from year -5 to year -1. In addition, X_i includes firm characteristics: size, revenue, foreign ownership, and sector measured one year before the reference month. Note that we do not use firm characteristics in the year right before the shock. Closing and downsizing firms already experience some distress before the actual shock happens, and we want to avoid matching on characteristics already affected by the coming events.

For the closure sample, the caliper is set to 0.09, and for the mass layoff sample to 0.001. In choosing the caliper there is a trade-off: with a small caliper we end up with very similar control women but lose both treatment and control observations if there are no close-enough matches, while a large caliper (or no caliper at all) allows for keeping many observations but at the expense of reducing similarity.

[10] In the matching we allow control women to be matched to multiple treatment women at different dates. Each control woman is included in the regressions as many times as she is matched, with the corresponding reference months. In the analysis, we use sample weights to account for the fact that for some treated women there are less than 10 controls matched, and that some controls are matched to more than one treated woman. The weight of a treated observation is always 1. The weight of a control observation depends on the number of treated observations she is matched to and reversely depends on the number of other controls in the same exact match set. However, entirely omitting the weighting would leave our results and figures mostly unchanged.

[11] The matching is implemented using the Stata package psmatch2 (Leuven and Sianesi, 2003).

Table 1: Means in the treatment and control groups

		Closure			Mass Layoff		
	Time of measurment	Control	Treated	Difference	Control	Treated	Difference
Age	Year 0	36.2	36.2	-0.014	38.2	38.2	0.005
Receives child benefits	Year 0	0.049	0.038	-0.011**	0.028	0.025	-0.003
Tenure (months)	Year 0	46.6	43.3	-3.326***	58.7	61.4	2.704***
Experience (months)	Year 0	81.6	82.2	0.665*	89.8	91.0	1.199***
White collar	Year 0	0.48	0.49	0.001	0.38	0.33	-0.054**
Wage (10000 HUF)	Year 0	13.53	13.38	-0.15*	14.17	14.38	0.22***
Percent losing job	Month 0	3.08	100.00	96.92***	2.89	40.71	37.82***
Firm characteristics							
Size: Small (max 49)	Year -1	0.48	0.64	0.156***	0.35	0.30	-0.044***
Size: Medium (50-249)	Year -1	0.31	0.21	-0.100***	0.29	0.30	0.008
Size: Large (min 250)	Year -1	0.21	0.15	-0.056***	0.36	0.39	0.036***
Log revenue (Million HUF)	Year -1	6.603	5.678	-0.925***	7.331	7.475	0.143**
Average wage (10000 HUF)	Year -1	14.89	13.80	-1.09***	15.44	14.79	-0.64***
Foreign owned	Year -1	0.20	0.14	-0.056***	0.33	0.35	0.024**
Firm age	Year -1	7.39	6.20	-1.185***	7.72	7.71	-0.009
Reproductive women employee share	Year -1	0.46	0.48	0.029***	0.46	0.46	-0.003
Fertilty variables							
Pregnancies	Year(-3)-(-1)	0.014	0.012	-0.003	0.010	0.010	0.001
	Year 0	0.034	0.041	0.008*	0.029	0.033	0.004
Births	Year(-3)-(-1)	0.002	0.003	0.001	0.001	0.001	0.000
	Year 0	0.024	0.026	0.003	0.019	0.027	0.008***
Abortions	Year(-3)-(-1)	0.012	0.009	-0.003**	0.009	0.010	0.001
	Year 0	0.010	0.015	0.005*	0.011	0.007	-0.004*
Number of observations		16860	2496		19736	4068	

*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Receiving child benefit includes all benefits available up to the 3^{rd} birthday of the child.

We choose calipers in a way to achieve a balanced sample in the sense that none of the independent variables of interest (i.e all variables used in propensity score matching) are different in magnitude in the treatment and the control group. Still, as Table 1 shows we allow for statistically significant differences for some variables (e.g.tenure, wage, firm characteristics), where the differences are not economically significant in our view. In a robustness check, we show that our results are not sensitive to the choice of the caliper, we end up with the same regression estimates using no caliper or a stricter one.

4.3. Firm outcomes around the layoff event

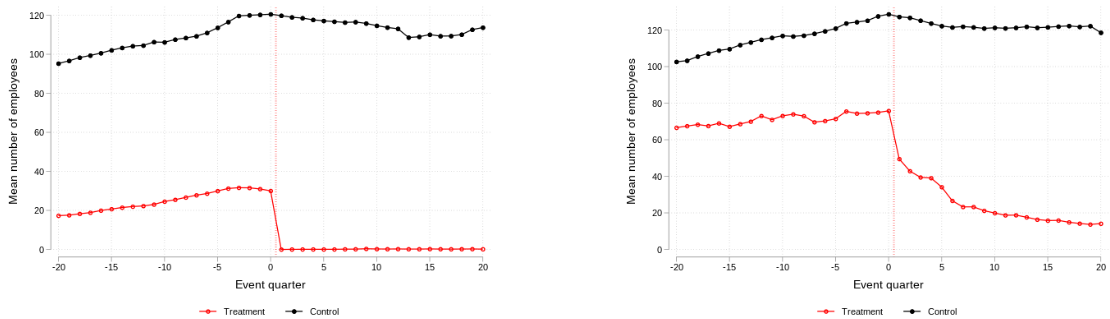
In this section, we discuss firm dynamics around the layoff event, looking at variables that might trigger the anticipation of layoff events among employees. First, Figure 3 shows that the evolution of the number of employees follows similar dynamics in treated and control firms. We do not see large numbers of employees exiting prior to the layoff events, but firm growth appears to be somewhat slower in treated than in control firms in the years leading up to the event. Annual firm revenues show a similar pattern. Log revenues grow a bit slower in closing firms in pre-treatment years, with a decrease in revenues in the year of the closure. Growth in firm revenues also stops in the year of the mass layoff, and there is a substantial drop in the next two years A.19

On the other hand, data on new orders in the manufacturing sector shown in Figure 4 demonstrates that orders start decreasing significantly on average 6 to 12 months before the layoff event. This pattern indicates that treated firms suffer negative shocks leading to the layoff event at the end of year 0. Our strategy is to compare firms that are similar in year -1 with treated firms that suffer shocks in the year leading up to the event. For this reason, we only include firm characteristics up to year -1 in the matching procedure. We assume that the negative shock can be observed either by the women themselves or by their colleagues who pass on the information. Survey evidence also confirms that individuals have some prior knowledge about a future job loss (Hendren, 2017; Mueller and Spinnewijn, 2022). Thus, it is plausible that employees anticipate problems at the firm already before

Figure 3: Firm size around the layoff event

(a) Closure

(b) Mass layoff



The last month of Quarter 0 is the month of the layoff event. For control firms, the date of the pseudo-event is set to the month when the most control women are matched

the layoffs happen.

Even though women can perceive economic problems at the firms, it might be hard to predict the actual outcomes. This idea is supported by the interview we conducted with a liquidation commissioner (who supervises liquidation procedures at firms). In general, when a firm starts to face problems, rumors start to spread around among the employees. After that, the firm can recover and go on with the business, there can be mass layoffs, or the firm can close altogether. But when the problems start, no one knows for sure how the troubles are going to end. Probably everyone assigns different probabilities for each outcome. The initial expectations are updated later when more information is revealed about the type of shock, and the behavior of the employees adjusts accordingly.

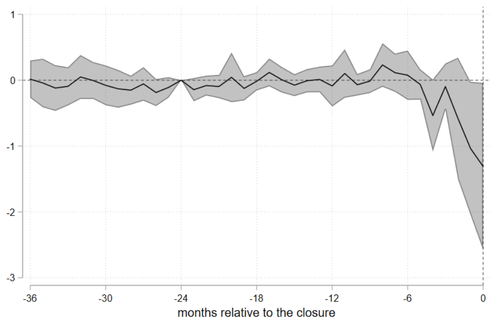
5. Empirical Strategy and Identification

5.1. Empirical Strategy

In our empirical strategy, we estimate event study and difference-in-differences models on the sample defined in Section 4. First, we run the following event study

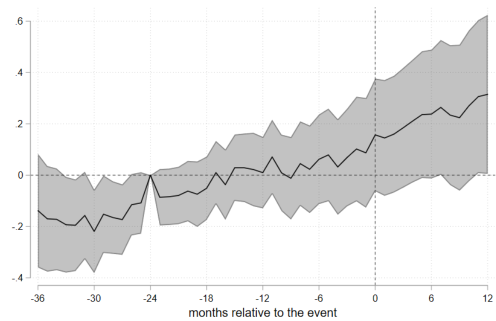
Figure 4: Firm orders

(a) Closure



(b) Mass layoff

(c) Controls



Sample: matched manufacturing firms with closure (a) or mass layoff (b) or with control women (c).
Regression: $logNewOrders_{it} = \sum_{k=-36}^{12} \beta_k EventMonth_{it}^k + \alpha_t + \alpha_i + \epsilon_{it}$ where i is firm, t is month, α_t is calendar month fixed effect and α_i is firm fixed effects and ϵ_{it} is the error term. Figures present the estimated β_k with the 95% confidence interval. We assign a single month of the event to control firms based on the highest number of women working there and being used as controls with that specific event time, considering closure and mass layoff events jointly.

regression:

$$Y_{it} = \alpha + \beta T_i + \lambda_t + \sum_{\substack{k=-5 \\ k \neq -3}}^{k=5} [\delta_k (T_i \times \mathbf{1}_{k=t})] + \gamma \hat{P}_i + \tau \mu_{g(i)} + u_{it} \tag{2}$$

where Y_{it} denotes the outcome variables: average wages, employment indicators, number of births, abortions, and pregnancies measured at the time of conception for woman i in event year t. The layoff events (or pseudo-events for control women) take place between the last month of event year 0 and the first month of event year 1[12]. T_i is the treatment assignment indicator, with value 1 if woman i worked at a firm with a layoff event in the three months preceding the event. Note that T_i is 1 for individuals working at downsizing firms even if they are not displaced. Event year fixed effects (λ_t) are also included. The coefficients δ_t are of main interest, showing the treatment-control difference in the outcome in event year t relative to the difference in the baseline event year.

To allow for anticipation effects, we set the baseline to year -3, long enough before the trouble at the firm should have started. According to Section 4.3, new orders decrease significantly 1 year before the layoff event, and insignificantly 2 years before it.

$\mu_{g(i)}$ denote exact match dummies in match set g. To control for remaining differences in the pre-treatment characteristics of women (see Table 1) we include the propensity score ($\hat{P}_i$) estimated in equation 1. Calendar year fixed effects are not included in the equation, because the matching is done in a given month, so including exact match dummies controls for calendar time.

To get robust standard errors accounting also for the fact that the regression is run after matching, we cluster the standard errors by exact match sets. Abadie and Spiess (2020) show that standard errors clustered like this are valid in regressions run after matching even if the regression equation is misspecified with regard to the population regression equation. Their results apply to non-parametric nearest neighbor matching without replacement, while we match on the propensity score within the exact match sets, and allow for replacement. As we are not aware of analytical results for the correctly specified standard errors with this extra detail in the matching, in addition to clustered standard errors, we also calculate standard errors by bootstrapping for the main coefficients of interest.

[12]For treated women, the last month of event year 0 denotes the last month when they still work at the closing or downsizing firm, or in case of those women who end up not leaving a downsizing firm, it denotes the last month before the mass layoff. For control women, the last month of event year 0 is the month when they are matched to treatment women.

After estimating yearly effects, we pool event years into three separate time periods, and run three-period DiD regressions for the same outcome variables, using the following equation:

$$\begin{aligned} Y_{it} = \alpha + \beta T_i + \gamma_1 Year_t^0 + \gamma_2 Year_t^{1,2,3} + \delta_1(T_i \times Year_t^0) + \delta_2(T_i \times Year_t^{1,2,3}) + \\ + \gamma \hat{P}_i + \tau \mu_{g(i)} + u_{it} \end{aligned}, \quad (3)$$

where $Year_t^0$ is a dummy equal to 1 in event year 0 (the year just before the event), and $Year_t^{1,2,3}$ is a dummy equal to 1 in event years 1 to 3. The reference time period is all event years available before year 0. Using these three stacked time periods is motivated by the theoretical results suggesting that women already react to the coming layoff event before it actually happens. We interpret δ_1 - the treatment-control difference in the outcomes in event year 0 relative to the difference in the reference time period - as the effect of anticipating the coming closure or mass layoff. The coefficient δ_2 shows the average yearly intent-to-treat effect of the shock in the following three years.

5.2. Identification

The identifying assumption of equations 2 and 3 is parallel trends conditional on observables. I.e. had the shock of the layoff event not affected the treatment group, their fertility would have changed the same way as that of the control group.

We took multiple steps to support this assumption. First, we ensured by the matching that controls are similar to treated women on many observables. Along with variables measured right before the shock, the matching also includes 4-year histories of wages and employment: this makes it more likely that women in the treatment and control group are not only similar right before the shock, but they are also on similar paths in their careers.

Second, we restricted our sample to women with at least 12 months of tenure and matched on firm characteristics one year before the shock. The average tenure in our treatment and control sample is almost 4 years in case of closures and around 5 years in case of mass layoffs. This increases the probability that the estimated fertility effects are not driven by some underlying variable correlated with firm and fertility choice. One can imagine, for example, that more risk-loving women are more likely to have unplanned pregnancies and abortions, and are also more likely to choose to get employed at more risky firms. By including women with long tenures, and by matching on firm characteristics, we minimize the probability that women know that they are getting employed at a risky firm, at the time when they are hired.

Third, we not only include women who stay until the last month of closure or mass layoff but include also those who leave the firm earlier to mitigate selection over the downsizing period.

6. Results

6.1. Event study estimates

In this subsection, we present raw yearly means of the outcome variables and the yearly event study estimates of Equation 2. These results provide a general picture of the yearly evolution of the outcome variables and the dynamics of the effects.

6.1.1. Labor market outcomes: employment, wages

First, we present evidence that women suffer large and persistent economic losses after closures and mass layoffs. Figures 5 and 6 show the raw means and the estimated yearly treatment effects (δ_t-s in Equation 2) for two outcomes: an indicator for being employed throughout the given year, and the mean yearly wage.

The career of treatment and control women evolves similarly before the shocks: employment and wages steadily increase for both.

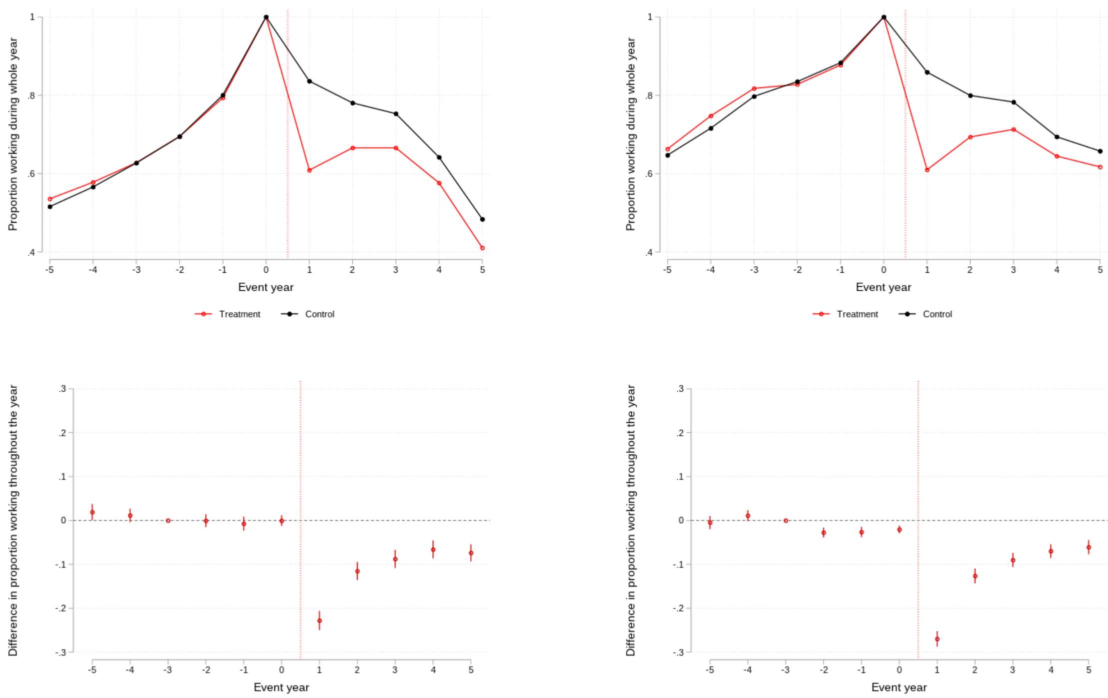
The share of women working throughout the year before the shock is 1 – a consequence of our criterion of 12 months of tenure. Closures and mass layoffs decrease the employment share by 23 and 27 percentage points in the first post-treatment year. The gap between treatment and control employment shrinks but persists in the following years (by 8 to 12 percentage points in years 2 to 5). The course of treated and control wages also diverges from event year 1, starting from a HUF 20,000 or a 10-14% difference, and persisting until event year 5 at a similar level.

The average effects of the two types of shocks on labor market outcomes are similar, which supports the idea that these are comparable shocks.

Other labor market variables show a similar pattern, such as the number of months spent working during the year (Figure A.21), registered unemployment (Figure A.22), and the wages of employed women (Figure A.23).

Figure 5: Employment in the treatment and control group before and after the shocks: raw means and regression estimates

(a) Closure (b) Mass layoff



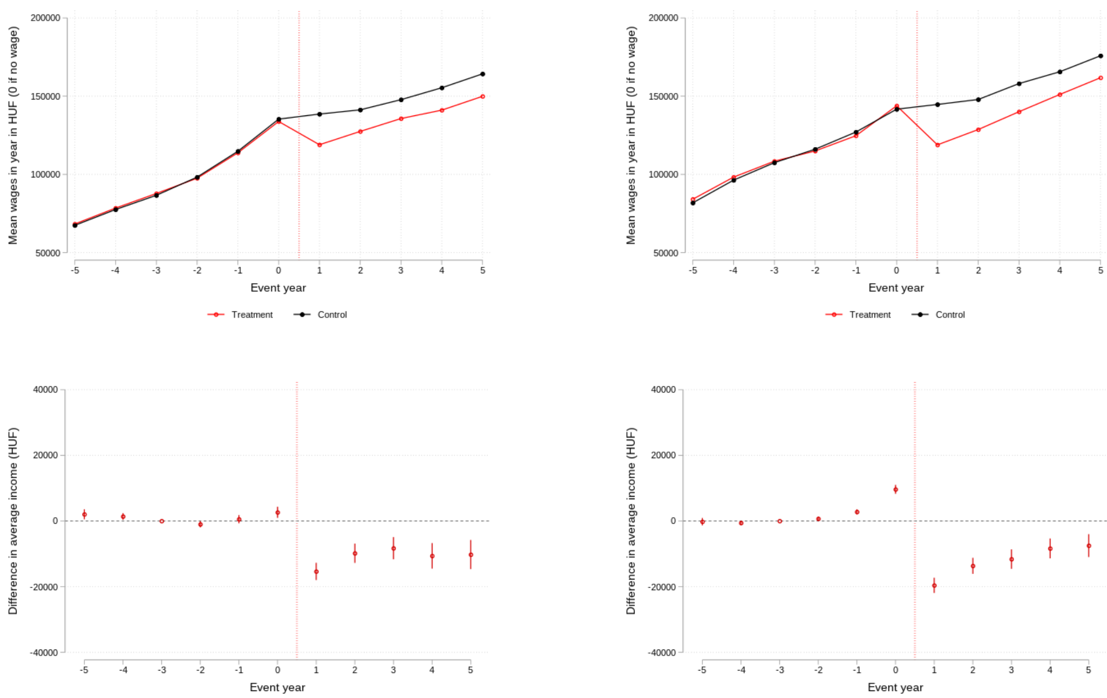
The last month of event year 0 is the time of matching. The number of observations by event years is shown in Figure A.20

6.1.2. Main outcomes: pregnancies, births, and abortions

After establishing the negative effect on labor market outcomes, we turn to the main variables of interest: pregnancies (Figure 7), births (Figure 8), and abortions (Figure 9), measured at the estimated time of conception.

Figure 6: Wages in the treatment and control group before and after the shocks: raw means and regression estimates

(a) Closure (b) Mass layoff



The red vertical line indicates the time of the layoff event. The last month of event year 0 is the time of matching. The number of observations by event years is shown in Figure A.20.

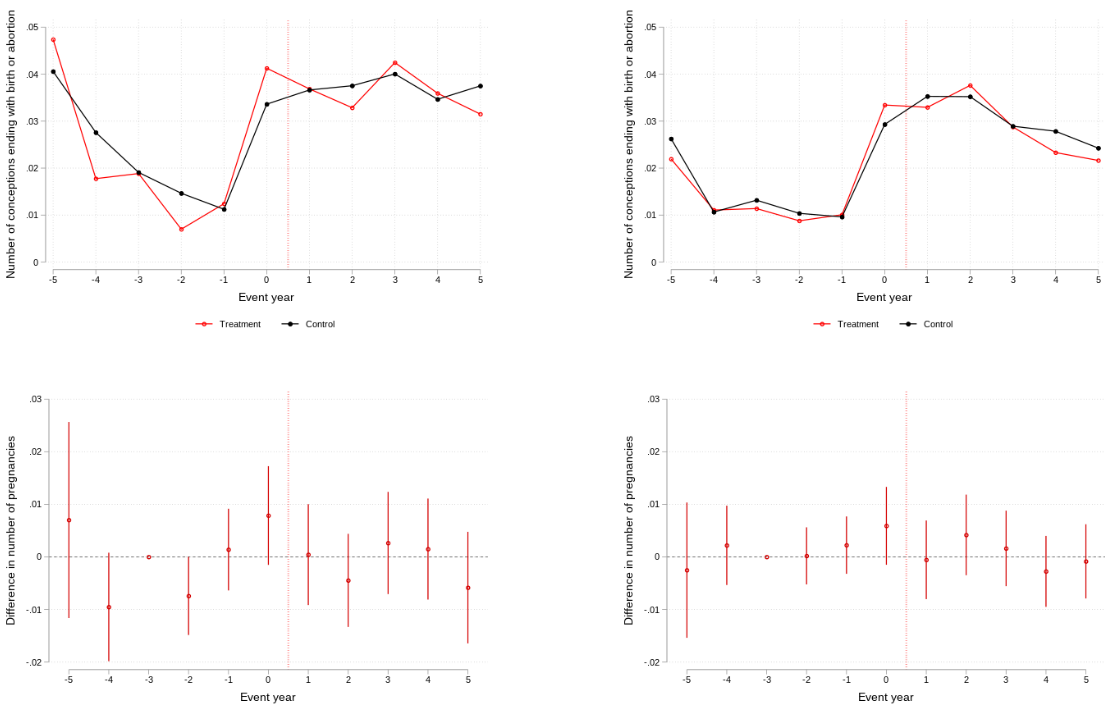
A defining feature of the fertility graphs is the appearance of treatment-control differences already in event year 0, the year before the shocks. Wages and employment are still the same this year, thus, these effects cannot be reactions to the current economic situation of women. Rather, we interpret these as women anticipating the coming shocks and the threat of job loss and reacting by strategically adjusting their fertility.

The graphs of fertility variables support the idea of precautionary pregnancies: pregnancies increase before closures and mass layoffs as well. In line with the strategy being successful only if the firm survives, the resolution of the pregnancies is markedly different in year 0 for the two types of layoff events. Births increase in case of mass layoffs, and abortions increase in case of closures. The effects in the post-treatment years appear to be more moderate than the initial responses.

But, as pregnancies, births, and abortions are rare events, yearly estimates for fertility outcomes are noisy, and even large yearly effects can be statistically insignificant in these specifications. To get more precise and robust estimates of the fertility effects, we turn to a difference-differences specification in the next section.

Figure 7: Pregnancies: raw means in the treatment and the control group and regression estimates

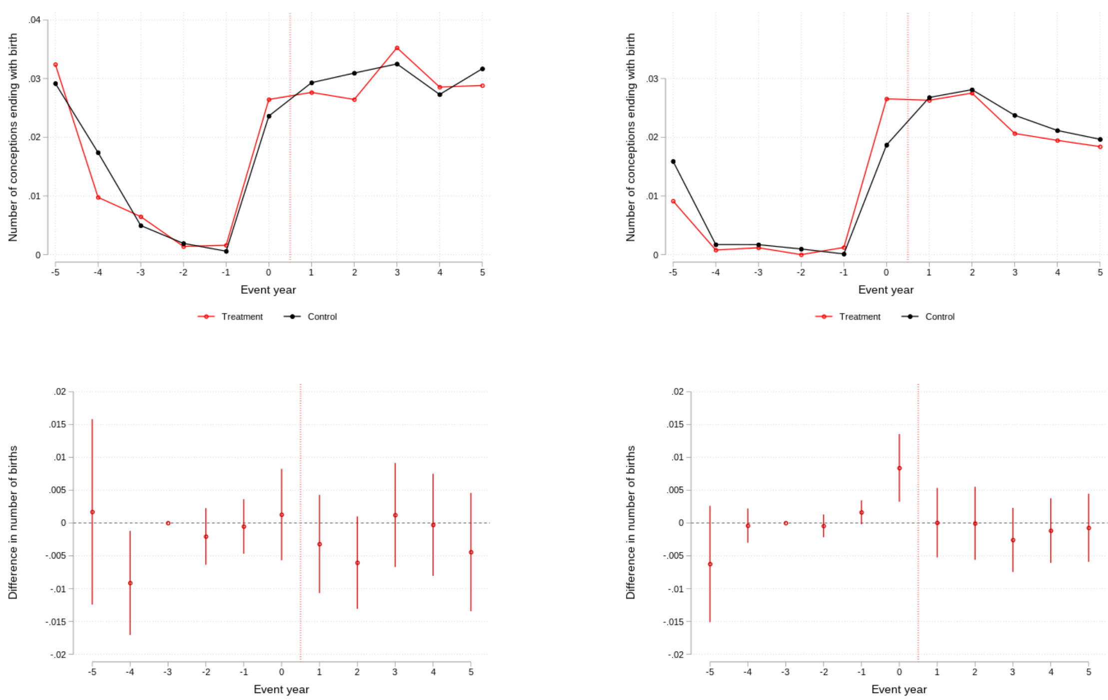
(a) Closure (b) Mass layoff



The red vertical line indicates the time of the layoff event. The last month of event year 0 is the time of matching. The number of observations by event years is shown in Figure A.20. The pregnancies, births, and abortions are counted in the year of conception.

Figure 8: Births: raw means in the treatment and the control group and regression estimates

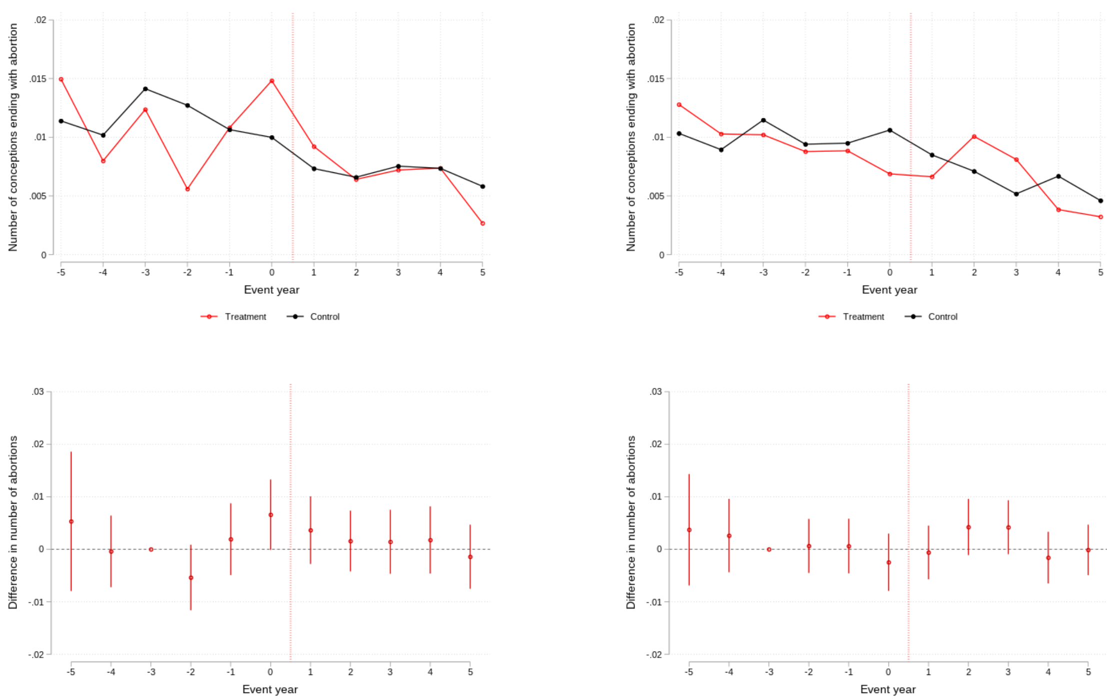
(a) Closure (b) Mass layoff



The red vertical line indicates the time of the layoff event. The last month of event year 0 is the time of matching. The number of observations by event years is shown in Figure A.20. The pregnancies, births, and abortions are counted in the year of conception.

Figure 9: Abortions: raw means in the treatment and the control group and regression estimates

(a) Closure (b) Mass layoff



The red vertical line indicates the time of the layoff event. The last month of event year 0 is the time of matching. The number of observations by event years is shown in Figure A.20. The pregnancies, births, and abortions are counted in the year of conception.

6.2. DiD estimates

In this subsection, we further study women's fertility responses using the difference-in-differences equation 3. Years -1 and before are pooled and serve as the baseline category, and we estimate the response separately in the anticipation period ($Year^0$) and in years 1 to 3 after the shock ($Year^{1,2,3}$). We use three post-treatment years because these years are observed for the whole sample. We also estimate the regressions for the labor market outcomes and present the results in Table A.6.

For the fertility outcomes, first, we study the effects in the year preceding the layoff events. The coefficient on $Treated \times Year^0$ in Table 2, Column (1) shows that for closures, pregnancies increase by 10 per 1000 women in the anticipation period. This is a large and statistically significant estimate[13]. The number of counterfactual pregnancies - number of pregnancies we would expect in absence of the treatment[14] - is 29 per 1000 women. Compared to this number the coefficient of 0.010 translates into a 35 percent increase. In the case of mass layoffs (Col. (4)), the point estimate is also large (0.005, or a 19% increase compared to the counterfactual) but insignificant.

The resolution of the extra pregnancies is different for the two types of layoff events. Women working at firms about to have a mass layoff, increase births by 8 per 1000 women (p=0.002) of reproductive age in anticipation of the coming events (Col. (5) Table 2). This is a large, 44% increase, compared to the counterfactual number of 18 births per 1000 women. We can put this effect size into a larger context by comparing it to the national level of 40 births per 1000 women in a year. On the other hand, the coefficient estimate on the number of births in the closure sample is not only insignificant but also smaller in magnitude.

Columns (3) and (6) in Table 2 report the estimates for abortions. Closures increase abortions by 7 per 1000 women (88% increase compared to the counterfactual) in year 0. This is a large effect and considering that there are 15 abortions per 1000 women of reproductive age per year in Hungary, it is even more stunning. For

[13] At the 1% level with clustered robust standard errors, and at the 5% with bootstrapped standard errors (see the p-values in the lower panel of the table)

[14] Calculated as pre-treatment mean in the control group (0.015) + coefficient on Treated (-0.002) + coefficient on Year 0(0.016)

mass layoffs, we estimate a relatively large reduction in abortions in year 0 that is insignificant (-0.003, -43%).

Next, we turn to the longer-term effects. The estimated yearly effects on births in the 3 post-treatment years are negative (-0.001, or -2.5%) but insignificant in both samples. For closures, the yearly post-layoff effects on abortions are smaller than the effects in the anticipation period. This suggests that abortions play a more important role in responding to immediate shocks rather than dealing with long-term economic hardship.

To calculate the net effect of the shocks, we estimate a difference-in-differences equation pooling year 0 and the 3 post-treatment years (Table A.7). The regression estimates reveal that neither closures nor mass layoffs change the overall number of births in the 4-year period surrounding the shocks statistically significantly. This suggests that the extra number of births in year 0 we observe in case of mass layoffs are mostly births brought forward from a few years later. Although we do not observe completed fertility in our data, this pattern suggests that mass layoffs do not increase the lifetime fertility of women. The result is different for abortions, however. Firm closures increase the number of abortions by 4 per 1000 women over the whole period.

Table 2: Three period DID regression results for the effect of closures and mass layoffs on fertility outcomes

Sample	Closure			Mass Layoff		
Outcome	Pregnancies	Births	Abortions	Pregnancies	Births	Abortions
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.002	-0.000	-0.002	-0.001	-0.000	-0.000
	(0.002)	(0.001)	(0.002)	(0.001)	(0.000)	(0.001)
Year 0	0.016***	0.018***	-0.002	0.018***	0.017***	0.001
	(0.002)	(0.002)	(0.001)	(0.002)	(0.002)	(0.002)
Year 1-3	0.020***	0.025***	-0.005***	0.022***	0.024***	-0.003***
	(0.002)	(0.002)	(0.001)	(0.002)	(0.001)	(0.001)
Treated X Year 0	0.010**	0.003	0.007**	0.005	0.008***	-0.003
	(0.005)	(0.004)	(0.003)	(0.004)	(0.003)	(0.002)
Treated X Year 1-3	0.002	-0.001	0.003	0.001	-0.001	0.002
	(0.003)	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
Exact matched set FE	YES	YES	YES	YES	YES	YES
Propensity score	YES	YES	YES	YES	YES	YES
Bootstrapped p-value of Treated X Year 0	0.027	0.378	0.016	0.181	0.002	0.151
Bootstrapped p-value ofTreated X Year 1-3	0.489	0.751	0.093	0.739	0.594	0.244
R-squared	0.074	0.073	0.057	0.083	0.086	0.061
Pre-treatment mean in control group	0.015	0.003	0.012	0.01	0.001	0.009
Observations		136,647			164,047	
N treated		2496			4068	
N control		16860			19763	

Notes: Standard errors clustered by exact match set in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Estimates from regression Eq. 3. Births and abortions are measured at the estimated times of conception. Pregnancies are the sum of births and abortions.

6.3. Discussion of the main results

While we are not aware of prior studies that have examined the precautionary birth and abortion effects of employment shocks, we can compare our results to studies that have examined post-displacement fertility responses in the short and medium term. Our estimates on the fertility effects after an employment shock are comparable in magnitude to the previous findings. Nevertheless, the estimates are insignificant and the most likely explanation is that our sample is much smaller compared to those in the previous studies. Our estimates of the effect on live births after the shock (2.5% insignificant) correspond to the estimates by Huttunen and Kellokumpu (2016) (about 3% significant effect) looking at job displacement events in Finland, but they are lower than estimates by Del Bono et al. (2012) (5-10% significant effect) who analyze Austrian data.

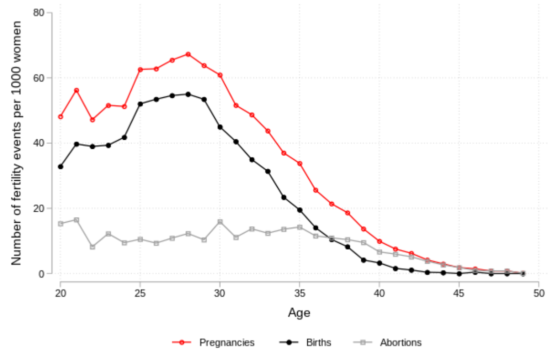
The abortion estimates after the shock (14-20% insignificant effect) are parallel to the estimates by González and Trommlerová (2021) who estimate the effect of a negative income shock in Spain on abortions and find a significant 13.5% increase.

The effect sizes on employment probabilities and wages after the shocks are also similar to the previous studies. Our results indicate that employment probabilities decrease by about 23% in the first year and by 8 to 12% in years 2 to 5. This comes near to the results of Ichino et al. (2017) who find that plant closures in Austria decrease employment probability by 27% in the first two years and 10 to 14% effects in years 3 to 10. On wages, we estimate a 10 to 15% effect lasting for at least 5 years. For comparison, two seminal papers find on US data that earnings losses of displaced workers are 25% per year (Jacobson et al., 1993b) and 9% per year (Stevens, 1997).

6.4. Heterogeneity analysis

In this section we provide additional evidence supporting our interpretation of the large fertility responses as precautionary pregnancies in anticipation of a firm closure or mass layoff event. In particular, we focus on women who are more flexible in timing their pregnancies or more willing to use abortions as a method of birth control.

Figure 10: Number of fertility events per 1000 women by age in the pooled control group



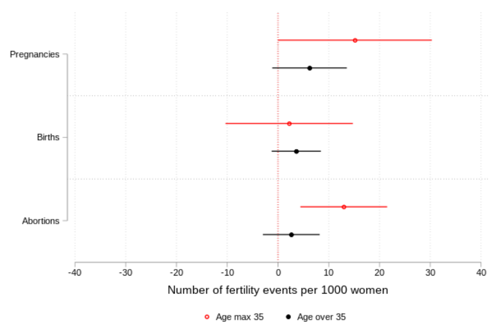
First, we check whether young women respond more in anticipation of the layoff events. We argue that women feeling threatened by job loss may respond by increasing pregnancies. This response is only possible if they can get pregnant relatively quickly: after starting to suspect troubles at the firm, but before the actual shock happens. In addition, they have to be willing to have a child. Women approaching the end of their reproductive age span are more likely to have already achieved their desired fertility and even if they decide to get pregnant, they are less likely to succeed in doing so: while the chance of natural conception each month is 25 percent for 25-year-olds, it drops to 5 percent by the age of 40 (ASRM, 2012; Dunson et al., 2002; van Noord-Zaadstra et al., 1991). Figure 10 showing the number of fertility events by age in our control group confirms that pregnancy probabilities are at their maximum for women between 25 and 30 years of age (more than 60 pregnancies per 1000 women), and they start to drop fast after this age (to under 10 pregnancies after age 40).

We split the sample at age 35, and estimate equation 3 separately for younger and older women. Figure 11 presents the anticipation effect (δ_1), for pregnancies, births, and abortions in case of closures and mass layoffs. The point estimates indicate that indeed women under age 35 drive the main results, while fertility effects are close to 0 for older women. Importantly, the magnitude of effects on pregnancies in the younger sub-sample is similarly large in the case of closures (14 pregnancies per

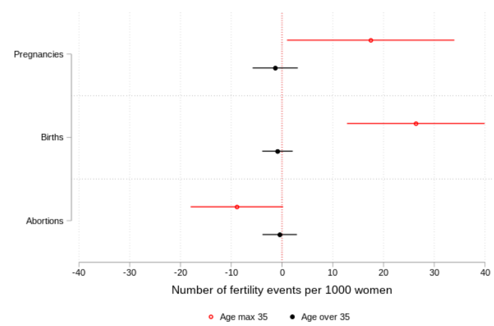
1000 women) and mass layoffs (11 pregnancies per 1000 women). But while in case of closures, a larger part of the conceived pregnancies gets aborted, young women affected by mass layoffs are more likely to give birth.

Figure 11: Anticipation effects by age, with 90 percent confidence intervals

(a) Closure



(b) Mass layoff

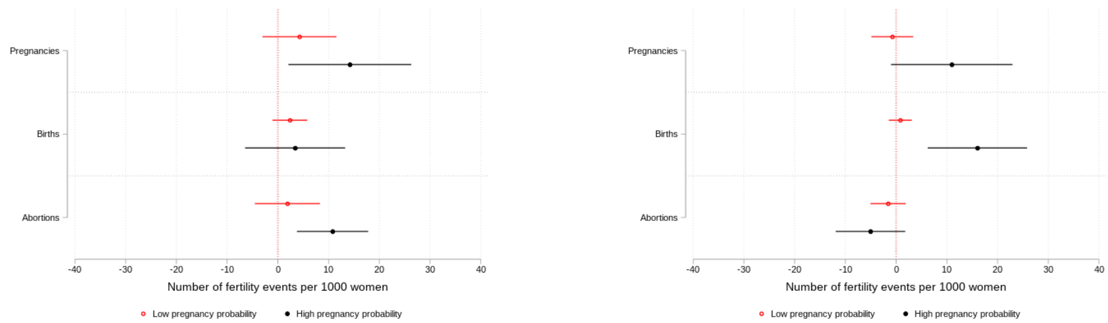


While age is an important determinant of fertility it is not the only one. For example, Figure A.24 shows that white-collar women tend to give birth at an older age than blue-collar women. In the following, we split the sample into low- and high-pregnancy probability groups, to investigate whether high-pregnancy probability women drive our results. To obtain the groups, we run a logit regression of an indicator for pregnancy using the pooled sample of the control groups. The predictors are age, occupation, their interaction, tenure, an indicator of having a young child, place of living, and wage- and employment history. Based on the estimated coefficients, we predict probabilities for treated and control women and split the sample at the median pregnancy probability of the control group. (The details of this analysis are available from the authors upon request.)

Figure 12 shows the estimates for the anticipation effect in these groups. This split produces very similar estimates to the split by age. The effects on all fertility variables are essentially zero for women with low predicted pregnancy probability. For women with high predicted pregnancy probability, the pregnancy effects are similarly large for mass layoffs and closures, but the effects on abortions and births markedly differ. This underlines that women who are more flexible in timing their pregnancies drive the anticipation effects and that the increase in precautionary pregnancies is similar before both types of shocks.

Figure 12: Anticipation effects by predicted pregnancy probability, with 90 percent confidence intervals

(a) Closure (b) Mass layoff

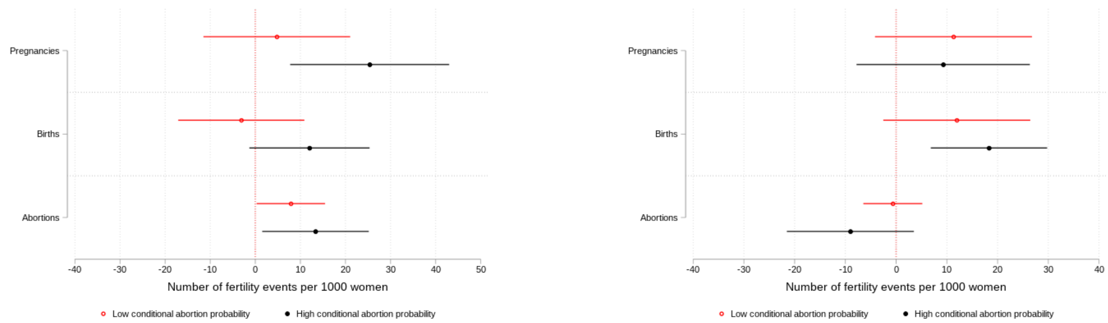


Estimates for δ_1 in equation 3.

Next, we compare fertility responses by the woman's willingness to use abortions. We focus on women with high pregnancy probability and split them into groups with a low and high predicted probability of abortion. To define the groups we run a logit of an indicator of having an abortion in event year 0 in the sample of women who get pregnant in the pooled control group, using the same right-hand side variables as before. Based on these estimates we predict the probability of having an abortion conditional on getting pregnant for the whole high pregnancy probability group. We again split the sample at the median of the control group to define a group with high and a group with low conditional abortion probability. Figure 13 shows the coefficient estimate of the diff-in-diff model for these groups. For closure events, women with a high predicted probability of abortions are the ones who drive the increase in pregnancies and abortions. For mass layoffs, we do not observe a clear difference between the groups with different abortion probabilities. This indicates that women who want to avoid abortions are less responsive in increasing pregnancies when the risk that the firm is closing - and thus the risk that the precautionary pregnancy strategy breaks down - is high. When the risk of firm closure is lower - in case of mass layoffs - women less willing to take the risk of abortion also respond to the threat of job loss.

Figure 13: Anticipation effects by predicted conditional abortion probability, with 90 percent confidence intervals

(a) Closure (b) Mass layoff



Estimates for δ_1 in equation 3.

Our heterogeneity results should be taken with a grain of salt because even when we see large differences in point estimates between the groups, we cannot differentiate them by statistical significance. Nevertheless, the differences in the point estimates are consistent with our main explanation of the treatment effects in the year before the events: women strategically increasing pregnancies in face of coming employment shocks.

7. Robustness checks

Our results suggest that the main reason the fertility responses to mass layoffs and closures differ is the difference in the availability of dismissal protection. An alternative explanation could be the different compositions of the two samples. The most important differences that are correlated with fertility decisions are that women in the closure sample are somewhat younger (mean age is 36, while it is 38 in the mass layoff sample), and a larger proportion of them has already at least one young child (26 percent vs 22 percent).

To check this explanation we run regressions similar to the one specified in Eq. 3, using the pooled sample of women affected by either shock. A modification compared to Eq. 3 is that we do not include exact matched set fixed effects in these specifications, because then we would not have sufficient overlap between the mass layoff and the closure samples. As without exact match set dummies calendar time is not controlled for automatically, we include calendar year fixed effects in these regressions. The results in Table 3 show that our estimates from the pooled samples are similar to our main results, indicating that it is not the different composition of the two samples that drive the differences in the fertility responses.

Table 3: Three-period DID regression results in the pooled sample

	(1) Pregnancies	(2) Births	(3) Abortions
Closure	-0.004**	-0.002**	-0.002
	(0.002)	(0.001)	(0.001)
Mass Layoff	-0.002	-0.001*	-0.001
	(0.001)	(0.000)	(0.001)
Year 0	0.020***	0.021***	-0.001
	(0.002)	(0.001)	(0.001)
Year 1-3	0.028***	0.030***	-0.003**
	(0.002)	(0.002)	(0.001)
Closure X Year 0	0.010**	0.004	0.006**
	(0.004)	(0.003)	(0.003)
Closure X Year 1-3	0.002	0.000	0.002
	(0.003)	(0.002)	(0.002)
Mass Layoff X Year 0	0.005	0.008***	-0.002
	(0.003)	(0.003)	(0.002)
Mass Layoff X Year 1-3	0.001	-0.001	0.002
	(0.002)	(0.002)	(0.001)
R-squared	0.006	0.010	0.001
Bootstrapped p-value if Closure X Year 0	0.025	0.237	0.034
Bootstrapped p-value if Closure X Year 1-3	0.441	0.966	0.22
Bootstrapped p-value if Mass layoff X Year 0	0.118	0.007	0.207
Bootstrapped p-value if Mass Layoff X Year 1-3	0.68	0.415	0.097
Exact matched set FE	NO	NO	NO
Propensity score	YES	YES	YES
Calendar year FE	YES	YES	YES
Observations		300,694	

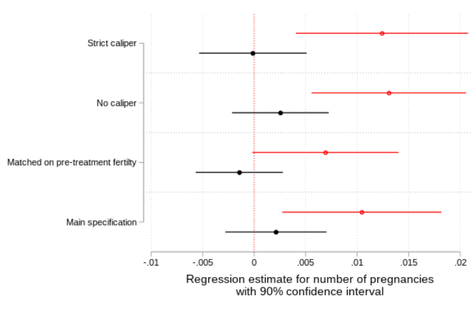
Standard errors clustered by exact match set in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Next, we show that the main results are not sensitive to our choices in the matching. First, we exactly match on a maximum of 4 years of birth and abortion history in event years -2 to -5[15]. This robustness check is important because our main identifying assumption is parallel trends of the outcomes, and by enforcing that parallel trends hold in the pre-treatment period, we make this assumption more plausible to be satisfied. Second, we use no caliper, and third, a stricter caliper of half of the size used in the main specification. Our choice of the caliper was subjective and was chosen in a way to minimize economically significant differences between the treatment and the control group while retaining a large enough sample size, and we want to make sure that the main results are not sensitive to this choice. Then we re-estimate Eq. 3.

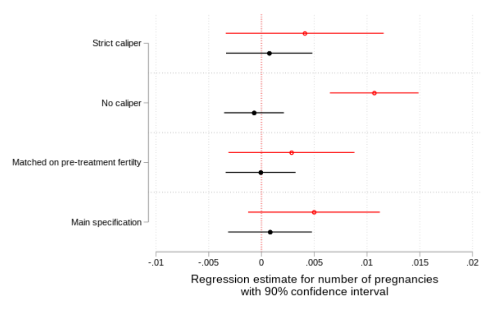
Figures 14, 15 and 16 summarise the regression estimates and reveal that our main results are robust to these modifications. In some cases, the statistical significance changes (e.g. the pregnancy increase for mass layoffs is significantly different from 0 when we use no caliper, and the abortion increase is insignificant in for closures if we match on pre-treatment fertility). Still, none of the estimates differ from the results in the original regressions in statistical terms, and they are of a similar magnitude.

Figure 14: Pregnancies: the effect of employment shocks - robustness checks

(a) Closure



(b) Mass layoff

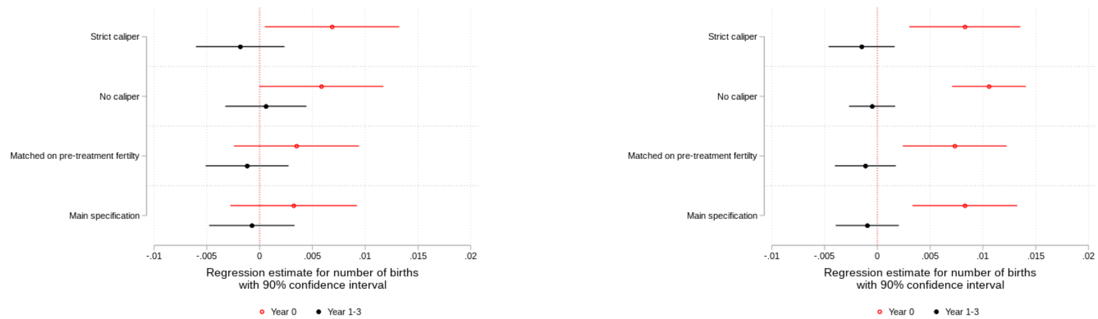


Estimates based on equation 3.

[15]For every woman we can only use the available pre-treatment years.

Figure 15: Births: the effect of employment shocks - robustness checks

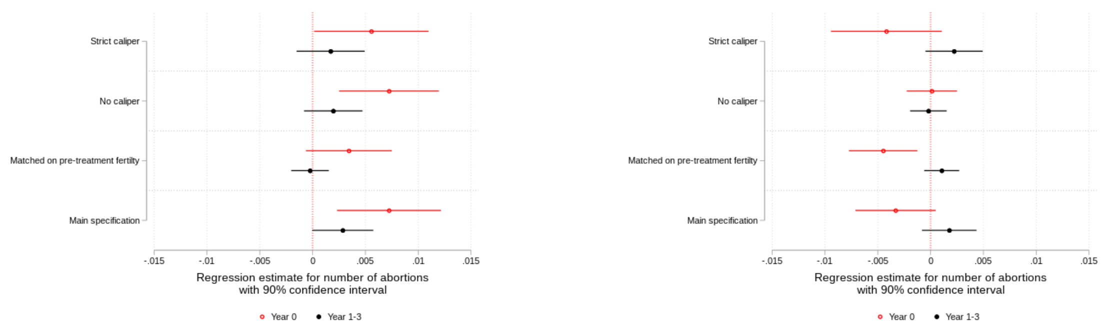
(a) Closure (b) Mass layoff



Estimates based on equation 3.

Figure 16: Abortions: the effect of employment shocks - robustness checks

(a) Closure (b) Mass layoff



Estimates based on equation 3.

As we noted earlier, miscarriage cases are not included among the pregnancies, and this could lead to a measurement bias of the main results. In Section A1.1 in the Appendix, we provide a calculation showing that this measurement error is too small to substantially influence our results.

8. Conclusion

In this article, we analyze women's fertility responses to two different types of employment shocks, firm closures, and mass layoffs. We argue that these shocks may have different impacts because of institutions that provide dismissal protection and financial benefits during pregnancy and after childbirth. We find strong evidence of precautionary fertility responses as women anticipate employment shocks and increase pregnancies. If they are covered by dismissal protection and high maternity benefits, women keep their pregnancies and use them as insurance against layoff. This happens by bringing births forward that were planned for later years. If dismissal protection is unavailable, however, the probability of abortion is increasing significantly for precautionary pregnancies. Even though the employment and earnings losses persist in the long run after the shock, we do not find longer-run effects on abortions. Thus, the role of abortions in controlling fertility appears to be the most important when women immediately react to unexpected shocks.

The novelty of our study is that we demonstrate the phenomenon of precautionary fertility behavior. Moreover, while previous studies already provided plausible causal micro evidence of the effect of employment shocks on the number of births, our research is the first to look at the number of abortions and pregnancies as well.

Our results are relevant for the increasing share of women who take into account career and employment conditions when planning their fertility. As we have shown, it is likely that workers can foresee the coming employment shocks, and a substantial fraction of young women are able to conceive in a few months. In terms of cross-country relevance, we think of firm closure and mass layoff shocks as two experimental scenarios resembling layoff conditions in countries with weak versus strong dismissal protection (ILO, 2022).

Our findings support the view that dismissal protection and maternity leave policies are powerful tools in incentivizing women to keep pregnancies in times of economic shocks. When protected, women can utilize the employment shock, by bringing forward their childbearing and smoothing their lifetime income flows. If there is no protection, they fully suffer the consequences of shocks. If they are not yet pregnant, they may postpone childbearing and decrease lifetime fertility (Currie and Schwandt, 2014). If they are pregnant, they may turn to abortion, or, if abortion is not possible, they suffer serious financial consequences as shown by Miller et al. (2023).

Our study aims to contribute to the social dialogue on abortions. We argue that dismissal protection can be an alternative to abortion bans in the sense that these policies help decrease the number of abortions. We believe that this new layer of the discussion would facilitate constructive, give-and-take solutions that are favorable for mothers and families.

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A1. Supplementary tables and figures

Table A.4: Number of Births and Abortions in Official Statistics and in Our Data

Year	Number of abortions, HSO	Number of births, HSO	Expected number of abortions in 50% admin data	Expected number of live births in 50% admin data	Observed number of abortions in 50% admin data	Observed number of live births in 50% admin data	Observed abortions (%)	Observed live births (%)
2009	43181	94707	21590.5	47353.5	20921	43464	97	92
2010	40449	88758	20224.5	44379	19406	41148	96	93
2011	38443	86632	19221.5	43316	18387	39388	96	91
2012	36118	88783	18059	44391.5	17592	40088	97	90
2013	34891	87189	17445.5	43594.5	17066	38928	98	89
2014	32663	90010	16331.5	45005	15709	39814	96	88
2015	31176	90190	15588	45095	14947	39649	96	88
2016	30439	91563	15219.5	45781.5	14453	39519	95	86
2017	28496	90077	14248	45038.5	13522	38615	95	86

Number of births is corrected by twin births

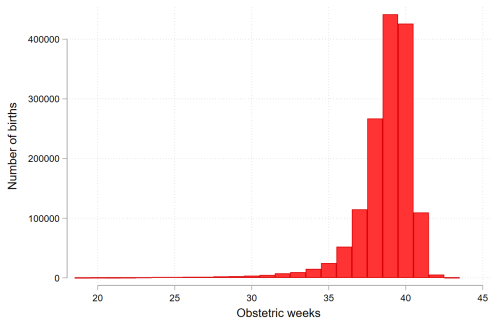
Table A.5: Child benefit rules

State child benefit	Availability at child age	Eligibility	Monthly sum	Monthly average in 2009[d]
Baby-care allowance[a]	0 to 0.5	employed at giving birth; worked at least 360 days in the past two years	70% of the previous wage	HUF 110,411 (USD 368)
Childcare benefit[b]	0.5 to 2	employed at giving birth; worked at least 360 days in the past two years	70% of the previous wage, maximum HUF 100,000 (about USD 334)	HUF 91,050 (USD 303)
Baby-care allowance[a]	0 to 0.5	on job search subsidy at giving birth; worked at least 360 days in the past two years	70% of the minimum wage	HUF 50,050 (USD 166)
Childcare allowance[c]	0 to 3	worked less than 360 days in the past two years	The amount of minimum pension	HUF 28,500 (USD 95)

(a) Csecsemőgondozási díj (CSED), Terhességi-gyermekágyi segély (TGYAS) before 2015
(b) Gyermekgondozási díj (GYED)
(c) Gyermekgondozást segítő ellátás (GYES)
(d) Based on data of the Hungarian Central Statistical Office

Figure A.17: Obstetric weeks of births and abortions

(a) Births



(b) Abortions

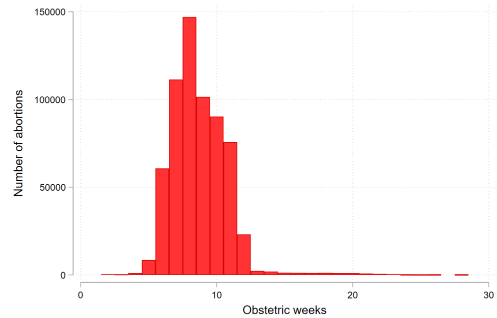
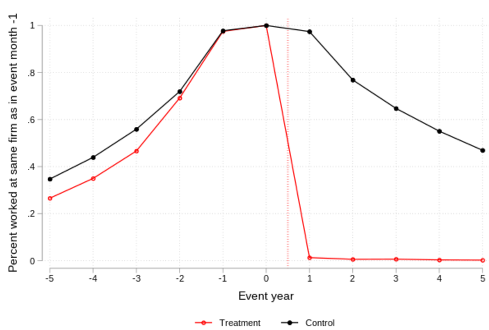


Figure A.18: Percent working at the same firm as in event year 0 in the treated and the control groups

(a) Closure



(b) Mass layoff

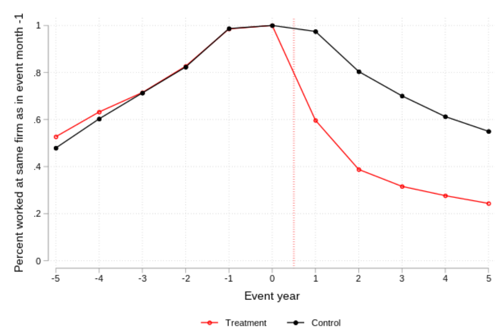
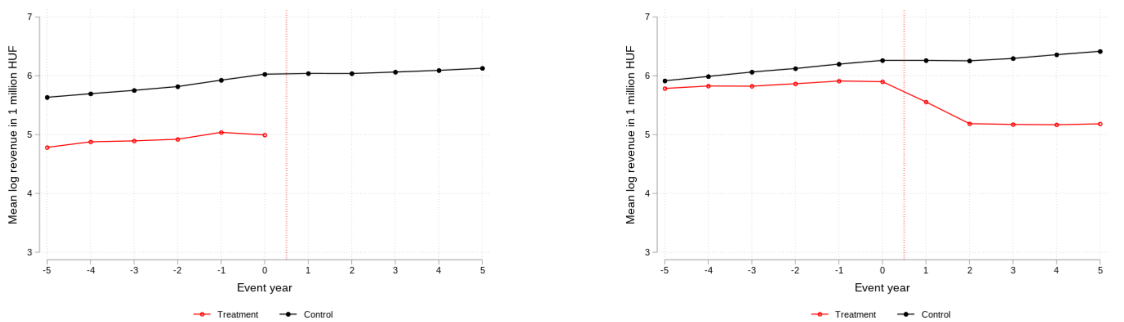


Figure A.19: Firm revenues around the layoff event

(a) Closure

(b) Mass layoff



Firm revenues are available at a yearly frequency. Event year 0 is the calendar year of the layoff event. For control firms, the date of the pseudo-event is set to the year when the most control women are matched

Figure A.20: Number of observations in the treated groups by event year

(a) Closure

(b) Mass layoff

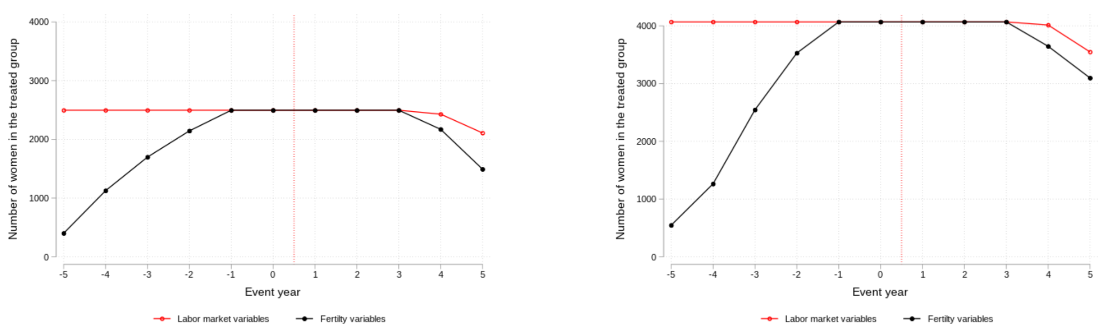
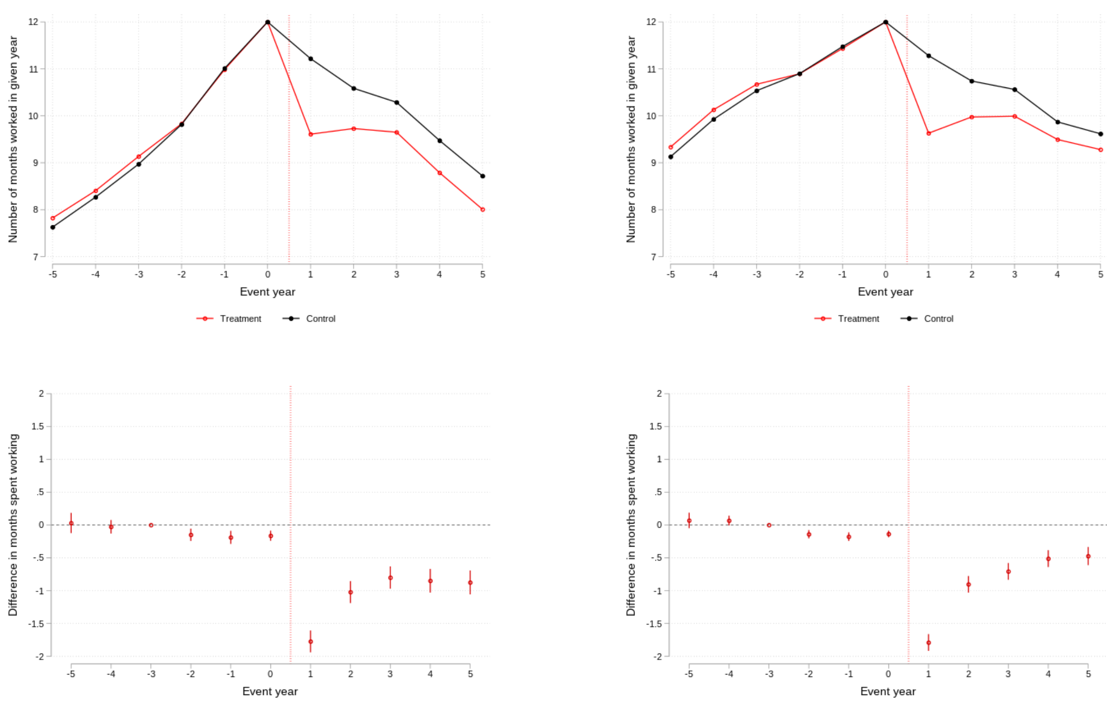


Figure A.21: Months spent employed in the treatment and control group before and after the shocks: raw means and regression estimates

(a) Closure

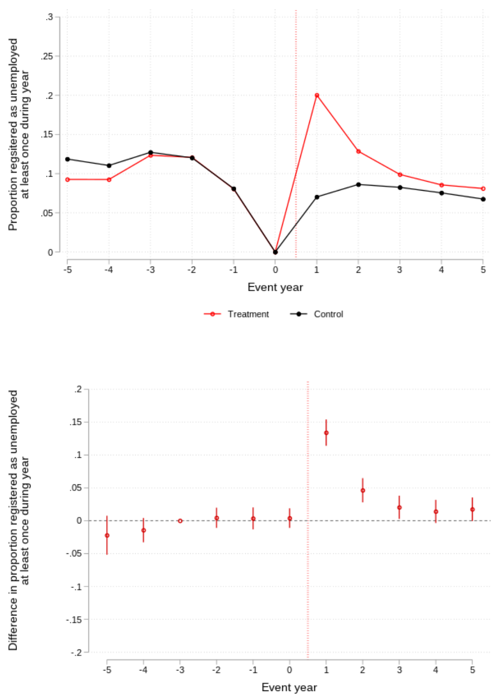
(b) Mass layoff



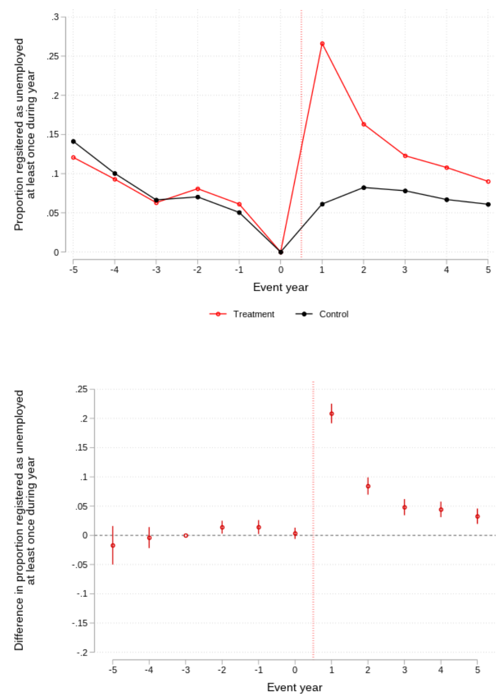
The last month of event year 0 is the time of matching. Number of observations by event years is shown on Figure A.20

Figure A.22: Unemployment in the treatment and control group before and after the shocks: raw means and regression estimates

(a) Closure



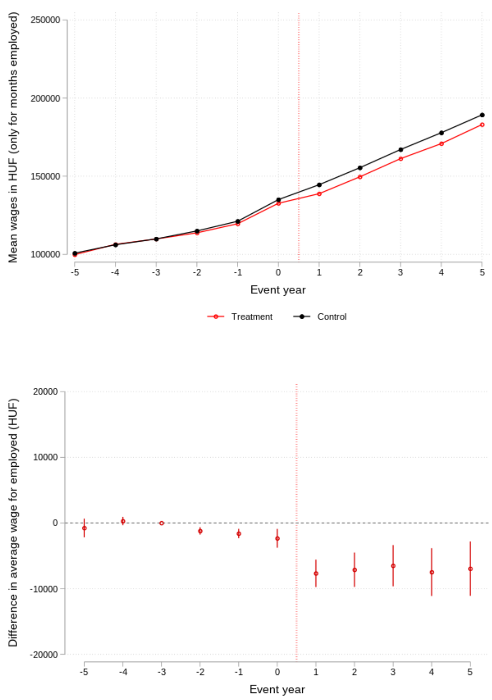
(b) Mass layoff



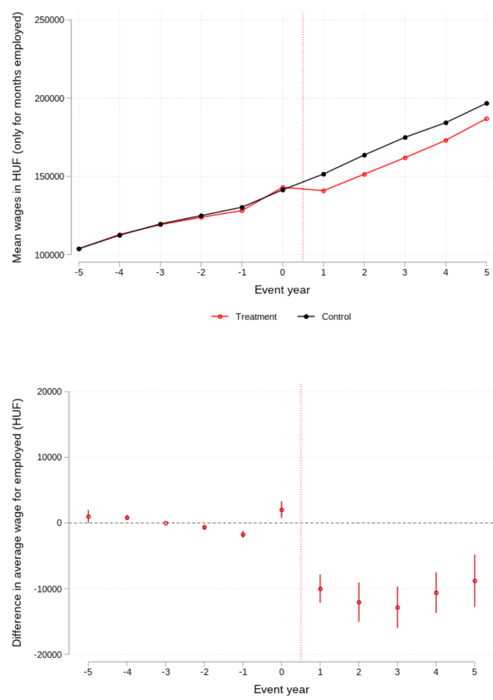
The last month of event year 0 is the time of matching. Number of observations by event years is shown in Figure A.20A.20

Figure A.23: Wages of working women in the treatment and control group before and after the shocks: raw means and regression estimates

(a) Closure



(b) Mass layoff



The last month of event year 0 is the time of matching. Number of observations by event years is shown on Figure A.20

Table A.6: Three-period DID estimates for the effects of employment shocks on labor market outcomes

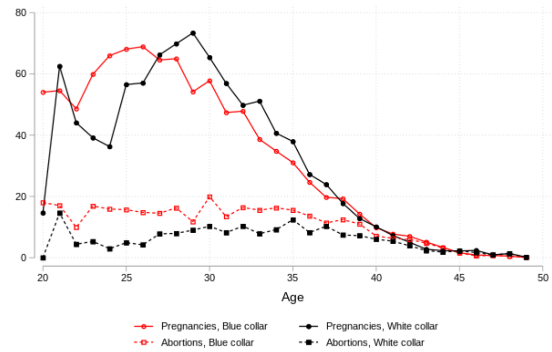
Sample	Closure		Mass layoff	
Outcome	Works throughout year	Wage (10000 HUF)	Works throughout year	Wage (10000 HUF)
	(1)	(2)	(3)	(4)
Treated	0.014***	0.062*	0.011***	0.034
	(0.005)	(0.037)	(0.003)	(0.027)
Year0	0.359***	4.639***	0.224***	3.592***
	(0.008)	(0.090)	(0.005)	(0.062)
Post1	0.149***	5.359***	0.038***	4.445***
	(0.007)	(0.110)	(0.005)	(0.101)
Treated X Year0	-0.005	-0.176**	-0.011***	0.182**
	(0.004)	(0.083)	(0.003)	(0.074)
Treated X Post1	-0.148***	-1.540***	-0.153***	-2.134***
	(0.009)	(0.163)	(0.007)	(0.145)
Exact matched set FE	YES	YES	YES	YES
Propensity score	YES	YES	YES	YES
Bootstrapped p value of Treated X Year0	0.255	0.028	0.002	0.016
Bootstrapped p value of Treated X Post1	0.000	0.000	0.000	0.000
R-squared	0.290	0.673	0.250	0.708
Pre-treatment mean in control group	0.672	8.728	0.851	10.569
Observations	174,204		214,479	
N treated	2496		4068	
N control	16860		19763	

Table A.7: DID regression results for the net effect of closures and mass layoffs on the number of births, abortions and pregnancies

	Closure			Mass Layoff		
	Births	Abortions	Pregnancies	Births	Abortions	Pregnancies
	(1)	(2)	(3)	(4)	(5)	(6)
Treated	-0.000	-0.002	-0.002	-0.000	-0.000	-0.001
	(0.001)	(0.002)	(0.002)	(0.000)	(0.001)	(0.001)
After	0.023***	-0.004***	0.019***	0.023***	-0.002**	0.021***
	(0.001)	(0.001)	(0.002)	(0.001)	(0.001)	(0.002)
Treated X After	0.000	0.004**	0.004	0.001	0.000	0.002
	(0.002)	(0.002)	(0.003)	(0.002)	(0.001)	(0.002)
R-squared	0.073	0.057	0.074	0.086	0.061	0.083
Exact matched set FE	YES	YES	YES	YES	YES	YES
Propensity score	YES	YES	YES	YES	YES	YES
Bootstraped p value of Treated x After	0.912	0.02	0.116	0.391	0.731	0.393
Pre-treatment mean in control group	0.003	0.012	0.015	0.001	0.009	0.01
Observations		136,647			164,047	
N treated		2496			4068	
N control		16860			19763	

Standard errors clustered by exact match set in parentheses; *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Figure A.24: Births and abortions by age for women in white collar and blue collar occupations



A1.1. Measurement error due to unobserved miscarriages

In the data we do not have accurate information on miscarriages so our measure of pregnancies defined as the number of births plus number of abortions is measured with error. Here we assess the potential bias of our results due to this measurement error. The main concern is that an increase in abortion will mechanically increase observed pregnancies if some of the aborted pregnancies would have been miscarriages.

Let the true number of pregnancies be P and assume it is not changed by a job displacement. We call $\tilde{P}$ the number of observed pregnancies, that is births plus abortions. The share of miscarriages among all pregnancies is m and a is the share of abortions. If there are no abortions $\tilde{P} = (1-m)P$. In case there are abortions

$$\tilde{P} = (a + (1-m)(1-a))P = (1 + am - m)P$$

This assumes all abortions happen before a miscarriage and only pregnancies that are not aborted are at risk of miscarriage.

We assume that the only difference between control and displaced women is the rate of abortions $a_0 \neq a_1$ and everything else is the same for both groups. In this case, we get

$$\begin{aligned} \Delta A &= P(a_1 - a_0) \\ \Delta\tilde{P} &= Pm(a_1 - a_0) \\ \frac{\Delta\tilde{P}}{\Delta A} &= m \end{aligned}$$

If $m = 0.1$, meaning that 10% of all pregnancies result in a miscarriage, an increase in the number of abortions by 10 would result in a mechanical increase in the number of observed pregnancies of 1. This calculation indicates that the implied mechanical increase of abortions from unobserved miscarriages is too small to explain the estimated effect of job displacement on observed pregnancies.

A2. Theoretical model derivations

I. General patterns

The timing of information and decisions within a period:

1. Women start as employed (E) or unemployed (U), depending on getting hired or fired at the end of the previous period
2. Women learn if the firm is in trouble and form their expectations on the probability of a within-period layoff: q_p for pregnant and q_n for non-pregnant
3. Women decide about pregnancy probability (p_0 or p_1 with $p_0 < p_1$)
4. Women get pregnant with probability p_0 or p_1 and learn about their pregnancy status
5. Women update their expectations about the probability of a within-period layoff (updated q_p and q_n)
6. Women decide on abortion if pregnant
7. Flow payoffs are realized: w for the employed, z for the unemployed (with $w > z$), and additional $B(\theta)$ if getting and saying pregnant or $-C(\theta)$ if getting pregnant but aborting (B is the discounted net value of having a child, including non-monetary costs and benefits, $C \geq 0$ is abortion cost, including non-monetary costs as well, both $B(\theta)$ and $C(\theta)$ increase in heterogeneity parameter θ)
8. For the employed within-period layoffs are realized with actual probability q_p^a or q_n^a if the firm is in trouble, these are zero in normal times
9. Women get hired with probability h_p (if stayed pregnant) or h_n (if not pregnant or aborted) if started as unemployed or were laid off within the period, and get laid off with probability f_p (if stayed pregnant) or f_n (if not regnant or aborted)

if started as employed and were not laid off within the period, assuming $h_p < h_n$, $f_p \leq f_n$ and $h_n + f_n \leq 1$

The value function for an employed woman with heterogeneity parameter θ in scenario s (where $E_s(\theta) = E(\theta)$ is the baseline scenario) with discount rate r is

$$rE_s(\theta) = w + (1-p)V_n + p\max\{B(\theta) + V_p, -C(\theta) + V_n\} + E(\theta) - E_s(\theta) \quad \text{(B.1)}$$

with

$$V_n = (q_n(1-h_n) + (1-q_n)f_n)(U(\theta) - E(\theta)) \quad \text{(B.2)}$$

$$V_p = (q_p(1-h_p) + (1-q_p)f_p)(U(\theta) - E(\theta)) \quad \text{(B.3)}$$

The value function for an unemployed woman with heterogeneity parameter θ and discount rate r is

$$rU(\theta) = z + (1-p)Y_n + p\max\{B(\theta) + Y_p, -C(\theta) + Y_n\} \quad \text{(B.4)}$$

with

$$Y_n = h_n(E(\theta) - U(\theta)) \quad \text{(B.5)}$$

$$Y_p = h_p(E(\theta) - U(\theta)) \quad \text{(B.6)}$$

Proposition 1. *A woman who chooses abortion when she starts the period as being employed will also choose abortion when she starts the period as being unemployed.*

Proof.

Women decide to abort when employed if

$$B(\theta) + C(\theta) < ((1-h_n-f_n)(q_n-q_p) + (1-q_p)(f_n-f_p) + q_p(h_p-h_n))(U(\theta) - E(\theta)) \quad \text{(B.7)}$$

Women decide to abort when unemployed if

$$B(\theta) + C(\theta) < (h_p - h_n)(U(\theta) - E(\theta)) \tag{B.8}$$

If θ satisfies inequality B.7, it also satisfies inequality B.8, as with $E(\theta) > U(\theta)$ the following inequality holds:

$$((1-h_n-f_n)(q_n-q_p)+(1-q_p)(f_n-f_p)+q_p(h_p-h_n))(U(\theta)-E(\theta)) < (h_p-h_n)(U(\theta)-E(\theta))$$

We will show that $E(\theta) > U(\theta)$ holds $\forall\theta$. ■

Proposition 2. *Intended pregnancies are never aborted if q_n and q_p do not change.*

Proof.

If a woman chooses to abort when becoming pregnant, then she is necessarily better off when she does not become pregnant, as $C(\theta) > 0$. Consequently, a woman who would want to abort upon becoming pregnant has no reason to increase the probability of becoming pregnant. ■

Proposition 3. *Some of the unintended pregnancies will not be aborted even if q_n and q_p do not change.*

Proof.

An employed woman is better off not getting pregnant, but she is also better off keeping the child upon becoming pregnant if two inequalities hold at the same time:

$$(q_n(1-h_n)+(1-q_n)f_n)(U(\theta)-E(\theta)) > B(\theta)+(q_p(1-h_p)+(1-q_p)f_p)(U(\theta)-E(\theta)) \tag{B.9}$$

$$B(\theta)+(q_p(1-h_p)+(1-q_p)f_p)(U(\theta)-E(\theta)) > -C(\theta)+(q_n(1-h_n)+(1-q_n)f_n)(U(\theta)-E(\theta)) \tag{B.10}$$

$\exists\theta$ satisfying both inequalities, as $C(\theta) > 0$, $\forall\theta$. Given inequality B.9, a woman with such θ does not increase her pregnancy probability, and her pregnancy will be unintended. Given inequality B.10 the woman will still be better off keeping the child upon becoming pregnant due to the high abortion cost. The same is true for an unemployed woman with θ for which the following holds:

$$h_n(E(\theta) - U(\theta)) > B(\theta) + h_p(E(\theta) - U(\theta)) \tag{B.11}$$

$$B(\theta) + h_p(E(\theta) - U(\theta)) > -C(\theta) + h_n(E(\theta) - U(\theta)) \tag{B.12}$$

■

Proposition 4. *A woman who starts the period as being employed and chooses not to increase her pregnancy probability would make the same decision if she started the period as being unemployed.*

Proof.

An employed woman chooses not to increase her pregnancy probability if inequality B.9 holds. An unemployed woman makes the same decision if inequality B.11 hods. Given our assumptions on the parameters, if a θ satisfies inequality B.9, with $U(\theta) < E(\theta)$ it also satisfies inequality B.11, as

$$(1 - h_p - f_p)(1 - q_p) - (1 - h_n - f_n)(1 - q_n) > 0$$

■

II. Baseline scenario

In the baseline scenario with $q_n = q_p = 0$, women with $\theta < \underline{\theta}$ will always abort, with $\underline{\theta} < \theta < \overline{\theta}$ only abort if being unemployed and with $\overline{\theta} < \theta$ will never abort. Similarly, women with $\theta < \hat{\theta}$ will never increase their pregnancy probability, with $\hat{\theta} < \theta < \ddot{\theta}$ only increase their pregnancy probability if being employed and with $\ddot{\theta} < \theta$ will always increase their pregnancy probability. With all parameter values satisfying the initial assumptions, we have $\underline{\theta} < \hat{\theta} < \ddot{\theta}$ and $\underline{\theta} < \overline{\theta} < \ddot{\theta}$.

III. General scenario

A specific scenario only affects employed women, but not the unemployed.

Proposition 5. *An employed woman with θ will keep the child in any scenario if $\theta > \overline{\theta}$, i.e. if she would keep it even when being unemployed.*

Proof.

$\theta > \overline{\theta}$ always satisfies the condition for keeping the baby in any scenario with $0 \leq q_p \leq q_n \leq 1$:

$$B(\theta)+C(\theta) > ((1-h_n-f_n)(q_n-q_p)+(1-q_p)(f_n-f_p)+q_p(h_p-h_n))(U(\theta)-E(\theta)) \tag{B.13}$$

The inequality holds for $\theta = \overline{\theta}$ and due to monotonicity, $B(\theta)+C(\theta) > B(\overline{\theta})+C(\overline{\theta})$ if $\theta > \overline{\theta}$.

■

Proposition 6. *An employed woman with θ will increase her pregnancy probability in any scenario if $\theta > \ddot{\theta}$, i.e. if she would increase her pregnancy probability even when being unemployed.*

Proof.

$\theta > \ddot{\theta}$ always satisfies the condition for increasing pregnancy probability in any scenario with $0 \leq q_p \leq q_n \leq 1$:

$$B(\theta) > (q_n(1-h_n)+(1-q_n)f_n-(q_p(1-h_p)+(1-q_p)f_p))(U(\theta)-E(\theta)) \tag{B.14}$$

The inequality holds for $\theta = \ddot{\theta}$ and due to monotonicity, $B(\theta) > B(\ddot{\theta})$ if $\theta > \ddot{\theta}$.

■

As for the baseline scenario, we can define cutoff values for the abortion and pregnancy probability increase decisions: women with $\theta < \widetilde{\theta}_s$ will always abort. Similarly, women with $\theta < \breve{\theta}_s$ will never increase their pregnancy probability.

Figures B.25 and B.26 summarize the potential order of the different abortion and planned pregnancy cutoffs in the various scenarios. The potential order is similar for the closure and mass layoff with no dismissal protection scenarios but it is different for the mass layoff with a full dismissal protection scenario. In the case of a mass layoff with partial dismissal protection, any of the presented cutoff orderings are possible.

Figure B.25: Cutoffs for abortion and planned pregnancies in the mass layoff with full or partial dismissal protection scenarios

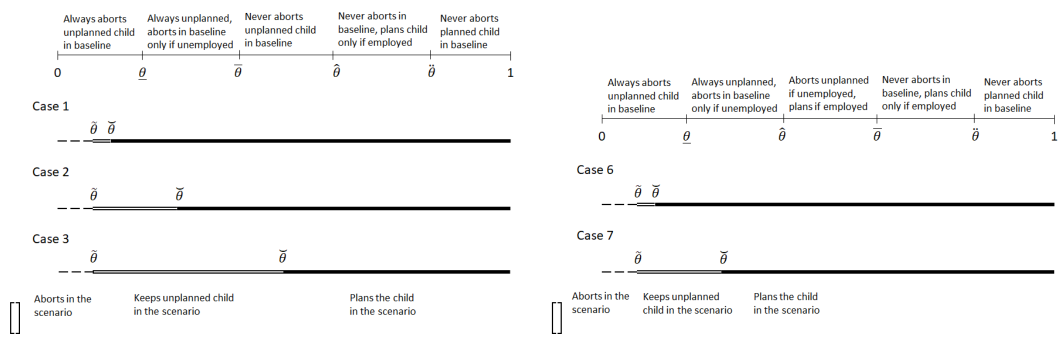
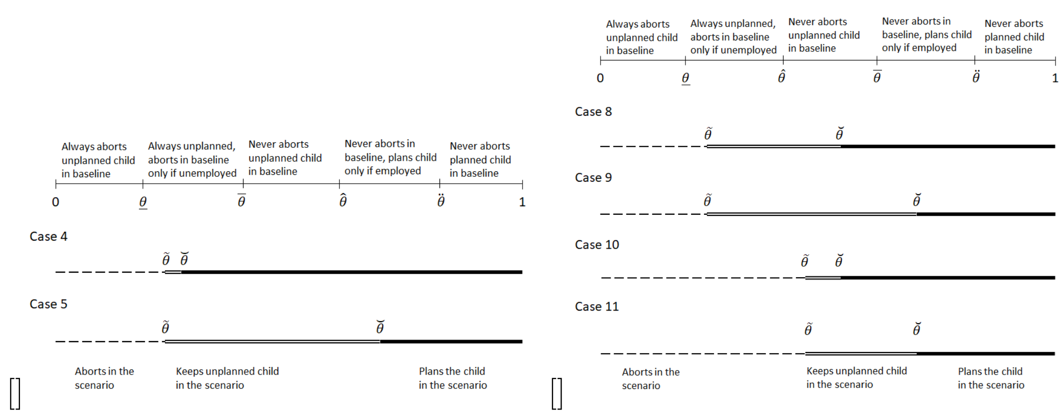


Figure B.26: Cutoffs for abortion and planned pregnancies in the closure and mass layoff with no or partial dismissal protection scenarios



IV. Number of abortions and births

The number of abortions per N women is given by $p_0\underline{\theta}N$ for the baseline scenario and $p_0\widetilde{\theta}_sN$ for a specific scenario s. We can show that $\widetilde{\theta}_s > \underline{\theta}$[16] in a closure scenario, while it is the other way around in a mass layoff scenario with full protection of the pregnant. Consequently, if women are fully rational, and their expectations about within-period layoff probabilities and available pregnancy protection are close to the actual probabilities, we expect to have more abortions in the closure scenario and fewer in the mass layoff scenario with full protection compared to the baseline case.

The number of births per N women is given by $p_0(\hat{\theta}-\underline{\theta})+p_1(1-\hat{\theta})$ for the baseline scenario and $p_0(\breve{\theta}-\widetilde{\theta})+p_1(1-\breve{\theta})$ for a specific scenario s. We can show that the number of births is lower in a closure scenario than in the baseline and it is higher in a mass layoff scenario with full protection of the pregnant. At the same time, the expectation of women about within-period layoff probabilities might be less precise by the time when they make the decision about increasing their pregnancy probability compared to the time of the abortion decision. This difference in expectation precision might increase the number of actual births in a closure scenario.

[16] We can provide the calculations upon request.