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Clash of Career and Family: Fertility Decisions after Job Displacement *

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Abstract

In this paper we investigate how fertility decisions respond to unexpected career interruptions which occur as a consequence of job displacement. Using an event study approach we compare the birth rates of displaced women with those of women unaffected by job loss after establishing the pre-displacement comparability of these groups. Our results reveal that job displacement reduces average fertility by 5 to 10% in both the short and medium term (3 and 6 years, respectively), and that these effects are largely explained by the response of women who are more likely to invest in human capital. Using an instrumental variable approach we provide evidence that the reduction in fertility is not due to the income loss generated by unemployment but arises because displaced workers undergo a career interruption. These results are interpreted in the light of a model in which the rate of human capital accumulation slows down after the birth of a child and all specific human capital is destroyed upon job loss.

Keywords: fertility, unemployment, plant closings, human capital
JEL classification: J13, J64, J65, J24

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1 Introduction

A prominent argument in explaining the shortfall of women in top academic positions in fields like economics is that these careers require a large amount of front-loaded work effort, which is almost incompatible with the demands of a family (Ginther and Kahn, 2004). When the tenure and the biological clocks tick on the same schedule women have to sacrifice either career or family, but cannot have it all. The situation of women in top positions may, however, only present the tip of the iceberg. Over the last century women's role in the labor market has gradually changed from secondary workers with a limited planning horizon to equivalent partners or independent decision makers with a life-time planning perspective (Goldin, 2006). This means that jobs which provide opportunities for promotion and advancement have become more desirable for women, and labor market conditions that impede the establishment of stable careers early in their lives like unemployment, temporary contracts, or involuntary turnover, may be reasons for a delay or even a permanent reduction in fertility.

The specific relationship between career and fertility has received little attention in the formal analysis of fertility decisions. Life-cycle models acknowledge the interaction of human capital investment and fertility timing and show that women have an incentive to postpone childbearing if the opportunity cost of human capital accumulation early in the life-cycle exceeds the value of children (Blackburn et al., 1993; Cigno and Ermisch, 1989; Moffitt, 1984). In these models human capital is typically seen as being entirely transferable and accumulation occurs at a constant rate so that transitory labor market shocks do not have a significant impact on fertility. If, however, we distinguish between accumulation of general and specific human capital, fertility could be affected by short-lived as well as persistent labor market shocks.

In this paper we set out to investigate the effect of job displacement on the probability of having a child in the following period. Job displacement may

have temporary as well as long term consequences on an individual's economic circumstances. At a minimum, an exogenous job separation can be seen as a career interruption, in so far as it forces the individual to start anew with a different employer. Very often, however, job displacement causes significant reductions in future wages and earnings, so that its effects are felt in the long-term.[1]

In order to distinguish the different channels through which job loss affects the demand for children we set up a simple dynamic model of fertility which takes into account that the process of human capital accumulation may be predominantly firm or career specific. Within this framework we can identify four key effects of job loss on fertility outcomes. The first two are the income effect, which results from the inability to smooth consumption over time, and the opportunity cost effect, which results from the lower value of time during a period of unemployment. These are the effects traditionally emphasized by static models of fertility and empirically analyzed by Butz and Ward (1979) for the U.S., and by Ahn and Mira (2002) and Engelhardt and Prskawetz (2004) for a sample of OECD countries. The third effect is due to the loss of future income that is incurred if a woman is unable to invest in human capital at crucial stages of her career.[2] This may be a relevant aspect to consider if we think, for example, that a woman with a young child is not able to keep up with the intensive training and sorting that occurs in the first few years on a new job. The final effect operates through the job finding rate, as pregnant women or women with small children might be less attractive to potential employers.

We present empirical evidence using data on firm closures identified in the Austrian Social Security Database. The data contains several features which make it possible to empirically distinguish between the different effects identified in

[1] See Ruhm (1991), Jacobson et al. (1993), Stevens (1997), and Ichino et al. (2007) for empirical evidence on the economic effects of displacement.

[2] This is consistent with the existing empirical evidence showing that the timing of labor supply interruptions matters in order to explain future wages (Light and Ureta, 1995).

the theoretical model. First, Austria has a government-sponsored maternity leave system, which allows mothers to take time off work after giving birth and provides them with maternity benefits. This system applies equally to all women in our sample and grants the same amount of benefits independently of the mother's employment status or income. Second, the data covers all women in the labor market. This allows us to compare the displacement effects for different groups of women, and especially to contrast women in more and less career oriented occupations. Third, the amount of unemployment experienced after job displacement varies considerably in our sample. We exploit this variation to distinguish between the effects of displacement that come from a career interruption and those that work through unemployment.

In the empirical analysis we compare births to women affected by a firm closure and to a control group of non-displaced women. Our main analytical tool is an event study which we use to establish the pre-displacement comparability of both groups and to display the post-displacement effects. Threats to the causal interpretation of the displacement effects are differential survival probabilities across firms and self-selection of workers into different types of firms. The data provides a wide array of individual and firm characteristics which allow us to control for these potential selection problems. It turns out that the groups are remarkably similar before displacement and therefore selection issues do not play a major role.

Our empirical analysis leads to three main findings. First, our results reveal that job displacement reduces the number of children born by 5 to 10% in the short and medium term (after 3 and 6 years, respectively). This suggests that the negative effects of displacement clearly outweigh any opportunity cost effects. Second, by disaggregating the effects according to individual characteristics we can show that the reduction in fertility is largely due to the behavior of women in white collar occupations, with higher earnings, medium levels of experience, and steeper wage growth profiles. Clearly, those who suffer most

as a consequence of a firm closure are women more likely to invest in career or firm specific human capital and hence to be more affected by an involuntary separation. Third, we find that unemployment has no extra effect on fertility on top of the effect due to job loss. To establish this result we estimate a model that controls for displacement and unemployment separately and disentangle the two effects using variation in individual unemployment by industry, region, and year.

2 Theory

It is widely recognized that female labor supply and fertility should be seen as joint decisions, either because they are determined by the same economic variables (Mincer, 1963) or because preferences over children and work are strongly correlated (Tien, 1967). The earliest theoretical contributions date back to the work of Liebenstein (1957) and Becker (1960, 1965), who extended the neoclassical theory of consumer demand to model the household's demand for children. Their settings are static and assume that fertility decisions are taken only at one point in time. More realistic models incorporate the long term nature of the fertility process in a dynamic framework, so that features like birth timing, investment in human capital, capital market imperfections, or the stochastic nature of human reproduction can be incorporated.[3]

A standard assumption in all life-cycle fertility models to date, however, is that human capital is entirely general, which means perfectly transferable across jobs and simply accumulated with time spent in the labor market. The birth of a child affects the human capital accumulation process by either depreciating the existing stock of human capital or reducing the rate at which human capital grows in the future. Labor market shocks like a job loss do not have a major impact on fertility if they are transitory and have small effects on lifetime

[3]For an excellent survey of the theoretical fertility literature see Hotz et al. (1997)

income.

In considering the fertility response to job displacement we depart from this framework and introduce a second type of human capital, which is firm or career specific. The idea is that specific human capital is destroyed at the time of job loss and this creates a mechanism such that even transitory labor market shocks can affect fertility. Our model focuses on a woman's job search and fertility decisions after job loss. The job search aspect is relevant to our problem since one of the objectives of a displaced individual is finding a new job. Our framework extends the main setup of a dynamic job search model with endogenous savings (Card et al., 2007; Lentz and Tranaes, 2005) by allowing fertility to be another choice variable.

Within a discrete time setting we assume that the woman can influence the probability of birth p_t in period t via the choice of contraception. She derives utility from consumption and the number of children, and disutility from practicing contraception. It is assumed that the utility function is separable over time and in all arguments so that per period utility is given by $u(c_t) + v(k_t) - \psi(p_t)$, where c_t and k_t are consumption and the number of children, u and v are monotonically increasing and concave, while ψ is monotonically decreasing and convex. Income in each period depends on the employment and maternity status. An unemployed woman receives an unemployment insurance (UI) benefit level b, on a job she earns a wage w, and after giving birth she spends a period on maternity leave earning maternity benefit m. We assume that $m < b < w$.[4] Decisions are taken in three steps. Each period the woman first learns about her employment status, then she makes a contraceptive choice, and after the fertility outcome is realized she decides how much of her income to consume or save thereby adjusting her asset level A_t. We focus here on a discussion of the main predictions of the model while a full description can be found in

[4]The assumption that $m < b$ corresponds to the Austrian maternity regulations. It is not crucial in any way, but creates a simplifying similarity for modelling the fertility decisions of employed and unemployed women.

Appendix A.

In order to examine the different channels through which a job loss can affect fertility decisions we first focus on the effect of income and on the cost-opportunity of taking a maternity break. In Appendix A we show that the response of the optimal fertility choice p_t^* to a change in the asset level A_t is given by

$$\partial p_t^*/\partial A_t = \{u'(c_t^1) - u'(c_t^0)\}/\psi''(p_t^*) \geq 0, \tag{1}$$

and thus it is positively and proportionally related to the difference in the marginal utilities from consumption in the maternity leave state, c_t^1, and in the working or unemployment state, c_t^0, respectively. If the woman faces borrowing constraints and is unable to fully smooth consumption across states, then $u'(c_t^1) - u'(c_t^0) > 0$, which implies that an additional unit of wealth increases fertility. We call this the *income effect*.

Further, we show that the effect of an increase in w in period t on the optimal fertility choice is given by

$$\partial p_t^*/\partial w = -u'(c_t^0)/\psi''(p_t^*) \leq 0, \tag{2}$$

which is negatively and proportionally related to the marginal utility of consumption, because a higher wage decreases the marginal return to having a child to the extent that it raises the value of being employed. This is the *substitution effect*.

To examine the other channels through which job loss affects fertility timing we need to specify how wages evolve over time and how a birth affects the mother's employment and income status. We assume that human capital is entirely accumulated on the job, so that wages grow with job tenure, T, following a concave profile, $w = w(T)$. Displacement destroys all job specific human capital and a woman re-entering a new job starts with wage $w(0)$. We assume further that the birth of a child has two effects on the labor market situation of the mother:

it lowers her job finding rate, and it reduces the rate at which human capital is accumulated in the future. These are the two crucial assumptions which allow us to show that: (i) optimal fertility decisions are different for employed and unemployed women, and that (ii) the birth of a child has a different impact on future earnings at different stages in a woman's career because of the concavity of the wage tenure profile and the reduced human capital accumulation rate after motherhood.[5]

The growth of female wages after motherhood has been studied extensively in the literature and there is evidence that due to reduced working hours, job effort, or lower promotion opportunities, women face a slowdown in their wage growth after having children (Miller, 2007), and that this phenomenon may even be more pronounced for high skilled women (Ellwood et al., 2004). At the same time, women who are pregnant or have a small child might face more difficulties in finding a new job because of higher job search costs or because employers discriminate against them. The latter might occur if, as shown in recent studies (Cunningham and Macan, 2007), employers assume that pregnant women are more likely to be absent from work or have a lower job commitment than other women.[6]

The responses of fertility choice to job displacement in our model are sketched in Figure 1, which shows wage tenure profiles for various alternatives. In the upper panel we see the profile for a non-displaced woman, whose earnings evolve according to a concave schedule. If this woman decides to have a child in period t her earnings drop to the maternity benefit level m during the period on maternity leave. In the next period she returns to her former job and

[5]Increased opportunities to control the timing of motherhood (e.g. through the availability of oral contraceptives) have been shown to play an important role in human capital and labor supply decisions (Bailey, 2006; Goldin and Katz, 2002).

[6]In 2005 the Recruitment & Employment Confederation, which represents UK recruitment agencies, asked 122 of its members whether companies had ever told them to avoid pregnant women or those of child-bearing age. Of the 98 agencies that replied, 75% said they had. Some agencies also said that pressure from employers resulted in discrimination against pregnant women, or those of child-bearing age, when registering or putting applicants forward for a specific position (*The Guardian*, November 25th, 2005).

earns the same amount she was earning before, i.e. there is no depreciation. Her future wage profile, however, is characterized by a slower accumulation of human capital. As we can see, the cost of a child consists of two components: the earnings loss during maternity leave and the earnings loss due to lower human capital accumulation in the future. Shifting the birth of the child to later periods increases the former but reduces the latter, while the concavity of the wage profile implies that small changes in birth timing have a larger impact at low than at high tenure levels.

The bottom panel shows the earnings profile for a woman who is displaced from her job in period $t-1$. The job loss may lead to a spell of unemployment during which she receives UI benefits b. When she enters a new job all her specific human capital is lost so that she starts again at a wage level $w(0)$ and in the absence of a child her wages evolve according to the original profile. To see the full effects of job displacement on fertility decisions we consider the situation of a woman, who instead of re-entering employment in period t decides to have a child. This means her earnings drop to the maternity benefit level m during maternity leave. Since this woman finds it more difficult to get a new job, she may experience a further period of unemployment. The combination of the delay in job entry and the lower wage growth, which is particularly important while she is still on the steep part of the wage profile, result in a large long term earnings loss. This comparison makes clear that displaced women face an incentive to (1) try to find a new job before having a child, which is what we call the *employability effect*, and (2) delay childbirth until they reach a flatter part of the wage profile, which is what we call the *career effect*.[7]

In the remainder of the paper we investigate the empirical relevance of each of the four effects on fertility decisions after job displacement.[8]

[7] A similar argument for managerial labor supply was made by Holmström (1999): if talent is unknown, new managers are tempted to put in a lot of effort at the beginning of their career in order to signal a high talent, which will be rewarded by higher wages later on.

[8] We included the maternity leave in the model, because it is an important feature of the Austrian setting we are empirically investigating. Note, however, that all the effects on

3 Family Policy in Austria

Austria has an extensive system of family policies and transfers which is rather generous compared to what is available in other European countries (Lalive and Zweimüller, 2005). The major components of the system, the maternity protection and parental leave policies, are targeted at parents with young children. Other forms of support, such as child benefits, extend over the long-term.[9] Over the last 20 years the system has undergone several reforms, most of them with the aim to increase coverage in the population and to boost fertility.

One of the building blocks of the system is represented by *maternity protection*, which is aimed at protecting the health of the mother and the child during the period around the birth. This period extends over 16 weeks, usually divided into eight weeks before and eight weeks after the expected date of birth. During this period mothers are not allowed to work but are insured against dismissal.[10] While on maternity protection, women receive a compensation in the form of sick leave payments which are equivalent to their previous monthly wage. Unemployed women get an 80% increase of their regular unemployment benefits.

Parental leave sets in after the maternity protection period ends. The system was introduced in 1957 in order to enable mothers to stay at home with their young children while still having the opportunity to return to their previous job (later on this right was extended to fathers). Under the system in operation in the 1990s the mother or father could choose to go on parental leave until the child's second birthday. During this period the parent was protected from dismissal from her former job and not allowed to work in any other job.[11] If the parent returned to her former job after parental leave had expired she continued

fertility decisions that we have derived are independent of the assumptions about maternity leave, because they all depend on life-time income evolution.

[9] The interactions of Austrian family policies and active labor market policies are investigated by Lechner and Wiehler (2007).

[10] Employed women are protected from dismissal when they first announce their pregnancy to the employer.

[11] The formal requirement was that while on parental leave the parent must not earn an amount above the marginal threshold (about 20% of the median gross earnings).

to be protected from dismissal for one more month.[12]

During the parental leave period the parent received a flat rate benefit of 408 Euros per month (in 1997). Eligibility for the parental leave benefit was closely linked to eligibility for unemployment insurance benefits. To establish a first-time claim the mother must have worked for 52 weeks out of the last 2 years. For mothers younger than 25 the requirement was reduced to 26 weeks during the last year. Unemployment extended the 2 year time frame by up to 3 years. This meant that a woman who worked for 12 months and then became unemployed had established eligibility for parental leave benefits for up to 4 years if she continuously stayed on the unemployment register. After the first parental leave period the requirements to re-establish eligibility were lowered.

Child benefits are almost universal in Austria. This means that all parents with a sufficiently long period of residence in Austria can claim these benefits irrespective of their income. The period of eligibility extends to a child's 18th birthday, and can be as long as a child's 26th birthday if the child is in full-time education. The amount received varies with the age of the child, but it was about 100 Euros per month in the mid 1990s. Take-up of child benefits in Austria is extremely high.

4 Data

We analyze the effect of job loss on fertility decisions using data from the Austrian Social Security Database (ASSD) which covers all workers except civil servants and the self-employed between 1972 and 2002. The dataset includes daily information on employment and registered unemployment status, total annual earnings paid by each employer, and various characteristics of the workers and their jobs. The data also contains entries of births, mainly linked to

[12]For parents who were notified of a dismissal after the protected period the regular periods of notice applied. This was two weeks for blue collar workers, and up to three months for white collar workers.

maternity protection and parental leave spells.

Since eligibility for maternity protection and parental leave is based on previous employment, a measure of fertility derived from the ASSD might under-report births to women who do not have a strong attachment to the labor market or leave the labor force after having a child. For this reason we merge the ASSD with child benefit records, which contain all births from 1975 to 2005, to derive a measure of fertility for every woman in the ASSD.[13] Our data can therefore be described as a linked administrative records database.

The sample used in our analysis includes firms that were active, i.e. had at least one employed worker on the payroll, on any of four specific dates (February 10, May 10, August 10, and November 10) during the years between 1990 and 1998. Firms can be identified through an employer identifier that is reported with every employment spell.[14] The time at which a firm enters the data is defined by the first quarter in which we observe the firm identifier. Since we have the universe of workers and can track firms back to 1972, we can derive a pretty accurate measure of the age of the firm. Firm closure dates are defined as the last quarter in which a firm has at least one worker on the payroll.

We apply three selection criteria to arrive at the sample of firms considered in our analysis. First, we exclude firms in which more than 50% of the workforce move to the same new employer, in order to control for reassignments of firm identifiers or mergers. Second, we exclude firms operating in the agriculture, construction, and tourism industries. These firms are characterized by a high share of seasonal employment and this makes it difficult to identify firm entries and exits. Third, we consider only firms that have 5 or more employees at least once during the period 1972-2002, and take only firms with more than 3 employees but less than 200 employees in the last quarter before closure. The rationale behind this is that it is difficult to identify firm closures for very small

[13] See Appendix B for more details on this.

[14] Note that we use the term firm in a loose sense here, as the data does not allow us to distinguish between plants or establishments and firms.

firms and the event is extremely rare among larger firms.[15]

For the resulting sample of firms we consider all women employed between quarter 1/1990 and quarter 4/1998.[16] We restrict the sample to young women between 18 and 35 years with at least one year of tenure in the current firm. The tenure requirement is important as it ensures that all women are eligible for parental leave benefits and reduces to some extent the degree of heterogeneity in our sample.

We then split the sample of women as follows. First, we define as *displaced* women all women working in a closing firm the quarter before closure. Second, we look at all women who work in closing firms one year before closure (i.e. in the fourth quarter before the closing date). We use this group of women *employed one year before closure* to check for changes in the composition of the workforce of closing firms over the last year. Third, we define as *control* all women who are not affected by a firm closure, i.e. whose firms do not close within the next two years.

The control group is large, as it includes observations on every woman working in a non-closing firm in any quarter between 1990 and 1998 and hence multiple observations on most women. To reduce the computational burden, we take a 5% random sub-sample of the women in this group. The final sample therefore consists of 8,401 observations of women in the *displaced* group, 9,945 observations of women in the *employed one year before closure* group, and 212,738 observations of women in the *control* group. Note that the distinction between these groups is based on whether a woman holds a job in a closing or non-closing firm at a certain quarter. We will refer to this quarter as the reference date.[17]

[15] In the data there are only 31 firms with more than 200 employees going out of business between 1990 and 1998 in industries other than tourism and construction.

[16] Employed means holding either a blue collar or white collar contract. We do not consider apprentices as being employed.

[17] The condition of a minimum firm size of 4 employees at the reference date applies to all firms in the sample. Including smaller firms in the sample leads to large variation in some variables and especially to an over-representation of pregnant women in the closure sample. This is because, as we saw in section 3, pregnant women are protected from dismissal.

By construction, the sample may include multiple observations for the same individual.[18]

Firm characteristics

Table 1 compares the characteristics of closing and non-closing firms in the sample. In total we observe 3,050 closing firms and 41,240 non-closing firms. Closing firms are smaller, on average about two thirds the size of surviving firms. They reduce their workforce by 10% in the year before the reference date, and although they grow over the previous 3 years, they expand less than surviving firms do. Further, closing firms are characterized by a higher share of employment turnover, measured by the sum of new hires and layoffs over total employment, in the final quarter and also in the years before. Earnings in closing firms are only slightly lower than those in surviving firms. Younger firms are more likely to close down and we observe a relatively high share of firm closures in the sales sector and in the last quarter of each year.

Figure 2 shows the development of average firm size in the last five years before the reference date in more detail. Five years before the reference date both closing and surviving firms are approximately the same size, about 22 employees on average. Afterwards, closing firms show a sharp reduction in average firm size. But this downward trend in firm size hides important compositional changes. Some of the closing firms (in general small ones) were not in existence 5 years earlier, so that the trend shown in the figure is determined to a large extent by the entry of smaller firms into the sample. All firms in our sample have been in the data for at least one year, so the development over the last four quarters is not affected by compositional effects and resembles growth of individual firms. The graph indicates some downsizing preceding firm closure which may result in a non-random selection of workers who remain employed until the closure date.

[18]For about 32% of individuals we have more than one observation in the final sample.

Individual characteristics

Table 2 compares the characteristics of women in the displaced group and in the control group: see columns (1) and (3). Displaced and control women are remarkably similar in age, nationality, labor market experience, and the number of previous children. The differences between women affected by firm closure and those who are not seem to be mainly related to differences in firm characteristics. We observe a higher share of workers with apprenticeship education and a lower share of blue collar workers among displaced women, which probably reflects the industry composition of closing firms. In addition, we find that displaced women have on average 10% less tenure with their current firm, reflecting lower firm-age and higher turnover rates of closing firms.

Column 2 in Table 2 summarizes the characteristics of women employed one year before closure. The samples in columns (1) and (2) overlap only in part, because some women employed in closing firms leave before the closure date and because we apply the same age and tenure restrictions to all three groups of women. In particular, the displaced group is not a subset of the women employed in closing firms one year before closure.[19] Comparing columns (1) and (2) it seems that downsizing has no effect on the composition of the young female workforce in closing firms. The main difference is that women employed one year before closure are more likely to be blue collar workers.

Our outcome of interest is measured by the number of births per woman after the reference date. In the full sample we distinguish women who are pregnant at the reference date, that is who give birth within 6 months of the reference date. The main analysis will be conducted using a measure of fertility that excludes

[19]The observations of women employed one year before closure are also based on a slightly different firm sample, which includes all firms closing down after the reference date but does not exclude firms with no women employed at that point in time and those with three employees or less. 53% of the women employed one year before closure are not in the displaced group because they leave before the closure date or work in firms with three employees or less at the closure date. Also, 46% of the displaced group are not in the employed one year before closure sample because they did not fulfill the tenure and age requirement one year earlier or their firm had three employees or less at that time.

births to pregnant women because these fertility decisions have already been taken before firm closure. Since job displacement might affect the total number of children as well as the timing of fertility, we consider a short-term and a medium-term measure of fertility and look at the birth rate 3 and 6 years following the reference date. As we can see in the bottom rows of Table 2, the sample means indicate a lower birth rate in the displaced group than in the control group. In particular, birth rates for the displaced group are 7.9% lower after 3 years and 9.4% lower after 6 years. This suggests a lasting negative effect of displacement on fertility.[20]

5 Firm Closures as a Quasi-Experiment

Our method to analyse the effect of job displacement on fertility consists in comparing fertility outcomes after the reference date (which is the quarter of closure for the group of displaced women) for women in the displaced and control groups. In an experimental setting with random assignment, one could simply draw a comparison between the average number of births to displaced and control women in order to estimate the average effect of displacement due to firm closure on fertility. Firm closures, however, do not happen randomly and are typically preceded by a period in which the distressed firm downsizes rapidly. Therefore we potentially face issues of selection at two different levels.

First, we have already seen from Table 1 that closing firms differ from surviving firms in several ways. Typically, closing firms are smaller and younger than surviving firms, they have a higher turnover of workers, and they are more concentrated in the sales sector and in the Vienna region. So, women employed in closing firms may significantly differ from women employed in surviving firms if there is non-random selection of workers into firms with different propensity to

[20] As we only observe births up to 6 years after the reference date, we cannot draw any firm conclusion about completed fertility from our results. However, the 6-year interval should be long enough to show evidence of catching up effects if these were to occur.

close down in the future. Secondly, because of the downsizing and restructuring that we see in most firms in the last year before firm closure, a non-randomly selected pool of workers may be what is left at the closing date (see Figure 2).

In the literature there is a debate on whether the downsizing process leads employers to retain the most productive workers, whether it is the workers with the lowest unobserved ability who stay until the end because they have less outside options, or whether both effects cancel out each other. Some studies have tried to model the selection process theoretically and empirically, although the various possible sources of bias are difficult to disentangle (Lengermann and Vilhuber, 2002; Pfann and Hamermesh, 2001). Generally, the view is that workers who leave the closing firm during its downsizing period are all doing so involuntarily and are included in the displaced group (Dustmann and Meghir, 2005; Eliason and Storrie, 2006).[21]

Here we use a narrow definition of displacement, which considers as displaced only workers who are still employed in the closing firm the quarter before closure. This choice is motivated by the specific features of our data. We observe that the labor market careers of young women in Austria are characterized by frequent interruptions, and we show that our sample has a relatively low labor force attachment (see section 6.2). This implies that the longer we extend the window before the closing date the more likely we are to capture voluntary job interruptions. As voluntary separations of young women are mainly related to fertility reasons, by widening the definition of job displacement we would be likely to see relatively high birth rates in the *displaced* group.

We check for selection occurring during the downsizing period comparing the characteristics of our group of *displaced* women with that of women who were employed in the closing firms one year before the firm closure (i.e. 4 quarters

[21]The main assumption rationalizing the inclusion of "early leavers" in the sample of workers affected by the firm closure is that they all leave the firm involuntarily, i.e. because of a layoff. However, if workers are allowed to choose the time of departure from the firm, early separations cannot be considered totally exogenous, even if they would not have occurred in the absence of the firm closure.

before the last quarter). We do not only compare average characteristics of both groups as in Table 2, but also show detailed quarterly labor market and yearly fertility outcomes in several years before the reference date. As we will see below, we are able to demonstrate that these two groups do not differ significantly in terms of their labor market and fertility histories. We take this as evidence that the downsizing does not affect the composition of the group of women in childbearing age in these firms and consequently that our definition of displacement is not subject to selection bias.

To deal with the selection of women into more or less "risky" firms in terms of their likelihood to close down, we compare past labor market and fertility outcomes of women in the displaced and control group. We correct remaining differences between these two groups by a propensity score weighting procedure, which balances the distribution of observable characteristics in both samples. The method is described in the next section in more detail.

Our main analytical tool is a graphical analysis based on an event study. This means that we pool all observations at the reference date and plot the means of the variables of interest per each quarter before and after the reference date, separately for the displaced and comparison groups. This provides many insights into the labor market and fertility dynamics of our sample of young women, which is characterized by many spells in and out of employment.

The graphical analysis turns out to be particularly useful for two reasons. First, looking at the period before the reference date establishes the a priori comparability of different groups. Second, the comparison of several outcome variables after the reference date gives us an idea of the magnitude and the dynamics of the displacement effect. The variables we compare are days employed per quarter, days unemployed per quarter, earnings, and the number of births. To see how much sample selection issues affect differences in future outcomes we also apply the above mentioned propensity score weighting. The magnitude

and significance of the fertility effect are then estimated by means of a linear regression.

5.1 Propensity Score Weighting

Define the treatment variable C_i as equal to one for a firm closure, equal to zero otherwise, and the potential outcomes for each treatment status by (Y_{0i}, Y_{1i}). The observed outcome is given by $Y_i = (1 - C_i) * Y_{0i} + C_i * Y_{1i}$. Because we want to know the effect of job displacement on the probability of having a child for the average woman in the population, the estimand of interest is the average treatment effect $ATE = E[Y_{1i} - Y_{0i}]$, where E is the expectation operator. Motivated by the availability of rich longitudinal information on both firms and individual labor market careers we make the assumption that job displacement from firm closure is randomly assigned conditional on a set of observable variables X, or $(Y_{0i}, Y_{1i}) \perp C_i | X$. Under this assumption, the average effect of displacement on fertility is non-parametrically identified (Rosenbaum and Rubin, 1984).

To estimate the ATE we use a propensity score weighting estimator suggested by Imbens (2004). The idea of this estimator is to predict the probability that an individual is displaced conditional on the observable characteristics X, and then use this probability to construct weights that equalize the distribution of X in the displaced group and the distribution of X in the control group to the distribution in the overall population.

More formally, we estimate a parsimonious logit model to predict the probability $\hat{p}_i$ of a firm closure event $C_i = 1$ for each observation $i = 1, \ldots, N$

$$\hat{p}_i = Pr[C_i = 1 | X_i]. \tag{3}$$

We then weight every observation in the displaced group by $1/\hat{p}_i$, and every observation in the control group by $1/(1-\hat{p}_i)$. Further we normalize the weights

to sum up to 1 in each group. The average treatment effect on the outcome Y_i is then given by the difference between the weighted average outcome in the displaced group and the weighted average outcome in the control group

$$ATE_{weight} = \frac{\sum_{i=1}^{N} \frac{C_i Y_i}{\hat{p}_i}}{\sum_{i=1}^{N} \frac{C_i}{\hat{p}_i}} - \frac{\sum_{i=1}^{N} \frac{(1-C_i) Y_i}{1-\hat{p}_i}}{\sum_{i=1}^{N} \frac{1-C_i}{1-\hat{p}_i}}. \quad (4)$$

This estimator can be rewritten in terms of the following regression function

$$Y_i = \alpha + \tau C_i + u_i, \quad (5)$$

which can be estimated by weighted least squares using the weights described above. In this equation the parameter τ corresponds to the average treatment effect. We can also add covariates to the regression function and improve the precision of the estimated parameters

$$Y_i = \alpha + \tau C_i + Z_i \beta + u_i. \quad (6)$$

A comparison of the point estimates of τ across specifications (5) and (6) provides an additional check for the balancing property of the propensity score.

Estimation of the propensity score

Our data provides a large amount of longitudinal information both at the firm level and at the individual level. We could therefore experiment with different propensity score specifications. We start by estimating the propensity of firm closure using firm level information exclusively. The idea is that firm closure is due to firm-level events and not based on actions at the individual level.

In order to do so, we estimate the probability that a firm with given characteristics will close down one year ahead. We use a one year time difference to take into account the downsizing and restructuring process which affects the closing firms. The independent variables are: industry, region, year, and season dummies, firm age (16 dummies), and for each of the last 3 years: firm size, employment growth, employment turnover, median monthly wage, median wage

growth, share of blue collar workers, and of women in the workforce, as well as various interactions between these variables.

Second, we estimate a propensity score based purely on individual characteristics. Here we predict the probability that a worker with given characteristics is affected by a firm closure. The independent variables are: age and age squared, age at entry in the labor market, tenure in the current job, labor market experience, number of children aged 0-3, 3-6, 6-9, 9-12, indicators for Austrian nationality, blue collar worker status, apprenticeship education, and for each of the last 4 years: monthly wages, percentage of period employed, percentage of period unemployed, number of job changes, season and year dummies, as well as various interactions between these variables.

Finally, we estimate two additional propensity score weights based on both worker and firm characteristics. In one version we use a more parsimonious set of firm characteristics, including only region and industry dummies, firm age, firm size, and industry interactions. In the other version we consider the full set of firm and individual characteristics.

6 Results

6.1 The Effect of Downsizing on the Composition of the Female Workforce

We start by comparing past labor market outcomes and fertility outcomes of displaced women and women who worked in closing firms one year before closure, i.e. the samples in columns (1) and (2) of Table 2. Figure 3 shows the development of average quarterly employment, unemployment, and earnings over the last 5 years before the reference date. Panel (d) shows the yearly number of births in the 14 years before the quarter of reference.

As becomes clear from the employment graph in panel (a), the condition that

every woman has one year of tenure at the reference date is a strong requirement. Employment rates in the quarters prior to the first year before the reference date are much lower. Unemployment in panel (b), consequently, is higher in quarters -4 and earlier, but does not make up for the full drop in employment. This highlights the relatively low rates of labor attachment of women in this age group, which is at least partly explained by the length of subsidized maternity breaks but is presumably also due to other family-related reasons.[22] The tenure requirement also determines the shape of the graph of yearly number of births in panel (d). Fertility drops prior to the reference year because women who give birth cannot be employed according to the Austrian regulations.

None of the graphs shows any systematic difference between the two groups of women in this period, which confirms what we could see in Table 2. The fact that we find no evidence of selection for displaced women is an important indication that the downsizing which occurs over the last year before closure down does not affect the composition of the young female workforce. This does not mean that there are no compositional shifts in the overall workforce of closing firms during the period before closure, as shown by Schwerdt (2008), but simply that compositional issues do not seem to affect our sample of young women.

When looking at the effects of firm closure on fertility we consider only women directly displaced by firm closure, i.e. those employed in the closing firm up to the quarter preceding closure, and compare them to the control group of non-displaced women. For these two groups future outcomes can be defined simply by measuring variables in the quarters after the reference date, because the event that should distinguish them takes place exactly at the same time. Furthermore, both samples are based on women with equal tenure and age requirements at the reference date. We choose not to analyse post-closure outcomes for the group of women employed one year before closure. We argued

[22] Care for older children and elderly family members.

above that voluntary separations will be more frequent in this group, and that the timing of separation might be a choice variable for these women. As the timing of fertility is very important in our results, an analysis of the outcomes of women employed in the closing firm one year before closure would not be informative.

6.2 Effect of Job Loss on Employment and Earnings

Figures 4 and 5 show raw comparisons between displaced and control groups for employment and unemployment from 20 quarters before to 12 quarters after the reference date. Once again, we notice the importance of the one year tenure requirement. For the control group employment rises steadily up to one year before the reference date, then stays at the maximum for four quarters before falling again. This development is almost symmetric, which means that low employment rates in the quarters where no condition is in place is due to weak labor market attachment rather than an age related profile. Also the drop in employment is not fully covered by unemployment. Displaced women face a substantial employment loss, and an increase in unemployment in the first quarters after firm closure, and are not able to fully close the gap even after 3 years.[23] If we compare displaced and control women prior to the reference date, we note that pre-displacement employment is slightly lower and unemployment is slightly higher for the displaced group, which indicates that firm closure does not hit workers completely at random.

Monthly earnings in Figure 6 resemble the pattern of days in employment, which basically means that earnings for those who are employed do not differ much for displaced and non-displaced women (see Figure 7). For this group of relatively young women we therefore see only minor wage effects after displacement. To put it differently, expected monthly earnings mainly differ because of the loss

[23] See Ichino et al. (2007) for a comprehensive study of employment and earnings costs for (older) workers hit by a firm closure in Austria.

of employment opportunities.

Figures 8, 9, and 10 apply the propensity score weighting approach to balance observable characteristics across displaced and control women. Panels (b) - (d) in each figure use propensity scores based on individual characteristics, which include yearly employment and unemployment rates and wages before the reference date. Not surprisingly, weights based on these propensity scores fully balance both samples in each quarter prior to the reference date. The graphs in the upper-left panels (a) use weights that are only based on firm characteristics, not on individual labor market outcomes. There we see that the weighting procedure does not eliminate all the differences in displaced and controls prior to the reference date. This means that the event of firm closure is not only related to the type of firm, differences in the composition of the workforce are also important. The important thing to note is, however, that the patterns of post-displacement outcomes are essentially unchanged by the weighting schemes. This indicates that the differences between displaced and control groups prior to the reference date are minor with respect to the differences in labor market outcomes after firm closure. Next, we turn to analyze the probability of having a birth, our outcome measure.

6.3 Effect of Job Loss on Fertility

Figure 11 plots the average yearly numbers of births in the 14 years before the reference date and up to 10 years afterwards.[24] The mean number of births per year is age adjusted, i.e. it is based on the residuals of a regression of the number of births on age of the mother and its square. The graph shows that the average number of births per woman decreases rapidly up to the reference date, when it becomes zero, and then shoots up dramatically. This pattern is a consequence of the fact that we select only women with at least one year of

[24]Note that we only have complete information on births over 6 years after the reference date. This means that the last 4 points in figure 11 are not based on the full sample.

tenure at the reference date. This means that all women in our sample must have been working during the year before closure and therefore they have no children between year -1 and year 0 by construction. As these women are also more likely to have been in employment in the period leading to the reference date, we observe a decreasing birth rate in the years preceding closure. The jump in the probability of a birth after the reference date is also a consequence of our tenure requirement. Conditional on not having had a birth in the last year, these women are more likely to have a child in the following period. What is important to notice, however, is that there is a clear gap between the displaced and control groups after the reference date. While we observe no significant difference in fertility behavior prior to the quarter of closure, displaced women clearly and consistently exhibit lower birth rates from year 2 onwards.

To see how differences in the sample composition affect the birth outcomes we plot weighted graphs in Figure 12. The different weights neither affect the comparability of the two groups before firm closure nor do they change the difference in the fertility response. In comparison to employment and earnings dynamics, the differences in fertility between these groups according to firm and individual characteristics are even less pronounced.

Figure 13 provides more detail on the quarterly dynamics in the average number of births after the reference date. We see that the birth rates of displaced and control women start diverging after the third quarter. Since it takes nine months from conception to birth, we interpret this as evidence that women do not have enough information to foresee the exact timing of their job loss.

In the regression analysis we therefore restrict the sample to women who do not give birth within 6 months of the reference date, i.e. those who were not pregnant when the firm closed down. As explained in the data section, we measure fertility outcomes using the number of births in the first 3 and 6 years after the reference date to compare short versus medium term birth outcomes.

Results from regressing the birth outcome variables on a firm closure dummy, equation (5), are shown in Table 3. As with our graphical analysis, we present unweighted results along with results weighted by our four propensity score estimates. In the lower panel of Table 3 we show results obtained when we include standard human capital covariates as additional controls using equation (6). This can be seen as a further test for the experimental design, which implies that the effects should be invariant to the presence of additional control variables. To correct the standard errors in the weighted regressions we use a bootstrapping procedure.

The results are robust and consistent across specifications. Births in the 3 years after firm closure drop by 1-2 percentage points. This corresponds to a fall in the mean birth rate over 3 years of 5 to 10%. In the medium run, the magnitude of the effect is in the same ballpark, about 2 to 4 percentage points, which is to say a reduction in the mean birth rate over 6 years of between 4.9 to 9.3%.[25] Although the point estimates are remarkably similar across specifications we see that bootstrapped standard errors are larger in the specifications using firm variable based weights. This confirms our observation that the firm type may be a poor proxy for the individual risk to lose a job.

If we were to interpret these findings in terms of income vs. substitution effects, we would think that the income loss due to job displacement largely dominates any opportunity-cost considerations. However, we saw in section 2 that a negative response of fertility could arise also because of the career effect or because of job search problems. Next, we investigate if the effect of an unexpected job loss is the same across different demographic groups. Differential patterns across women with different characteristics may help to disentangle competing

[25]To give some idea of the magnitude, we compare these findings with those of Lalive and Zweimüller (2005). They find that the extension of the maximum duration of parental leave from the child's first to the child's second birthday which took place in Austria in 1990 resulted in a 4.9% increase in birth rates of second children after three years and a 3.9% increase after 10 years. So the effects we find are at least as large as, and in some cases twice the size of the effect of a 12 months increase in parental leave.

theoretical explanations for the drop in fertility. So, in Table 4 we compare the results across women with and without previous children, in white and blue collar jobs, two age groups, and three experience and earnings groups.[26]

All the results point out a consistent pattern. Women without previous children are those who experience a drop in fertility, whereas women with previous children remain unaffected. This is possibly a consequence of the fact that women with previous children have most likely already realized their fertility plans. Age does not seem to matter much, as we see that both women older and younger than 25 reduce their fertility. Instead there is a clear difference according to the occupational position. Women in white collar occupations and those with higher pre-displacement earnings observe a significant and large reduction in the probability of having a child, while women in the lowest third of the earnings distribution and those in blue collar jobs do not show any change. White-collar jobs offer higher job protection and opportunities for career advancement, and thus higher expected returns for specific training. This suggests that women for whom career concerns and the destruction of firm specific human capital matter most show the largest response to a job loss. We also find that women with medium experience levels are affected much more than those with either very little or very much experience. This is also very interesting, as it hints at a big effect of displacement for women who are at a crucial stage of their career.

The theoretical model sketched in Figure1 predicts that women who face the highest incentives to adjust their fertility behavior should be those with long tenure or who have already gone through the initial phase of high wage growth. We therefore split the sample also by tenure and previous wage growth profile. We define long tenure as length of tenure above the median (3 years), and high wage growth as an average annual growth of price adjusted wages of more than 5% over the last three years. The results are shown at the bottom of Table 4.

[26] Due to space constraints we only present results from the unweighted regressions.

Long tenure does not have a higher impact on the reduction of fertility per se. This could be due to the fact that simply working longer for a specific employer does not automatically mean building a career there. It is clear, however, that women who lose their jobs after a period of high wage growth reduce their fertility more than women with flatter wage profiles. The effect is strongest for women who had high wage growth with the same employer over the last 3 years, which is exactly what the model would predict. Upon unexpectedly losing their jobs these women face the challenge of getting their career back on track and therefore delay their fertility plans. Over the next six years, these women experience a fertility rate 20 to 25% lower than similar non-displaced women.

6.4 Firm Closure Effect versus Unemployment Effect

So far we have looked at the overall effect of an unexpected job loss on the fertility behavior of young women. This is not to be mistaken with the effect of unemployment on fertility. On the one hand, a large share of women affected by a firm closure do not experience any unemployment; only 36% of our sample are unemployed for at least one day in the first year after firm closure (which is still considerably higher than the 7% of the control group). On the other hand, as pointed out in the theoretical model, the very fact that a firm closure occurs might have an impact on fertility over and above its effect through unemployment. This direct effect is what we call the *career* effect and is merely caused by the destruction of specific human capital and the need to start a new career somewhere else. Making use of the variation in individual unemployment experience we can try to disentangle the potential effects of a career interruption from the effects operating through unemployment.

There are, however, serious doubts about whether individual unemployment can be considered exogenous with respect to fertility. As there might be unob-

servable effects determining unemployment and fertility at the same time, an IV strategy seems appropriate. In our sample we observe considerable variation in unemployment after firm closure by industry, region, season, and calender year. Since there is no reason to assume that the direct impact of starting a new job on fertility would be different along these dimensions we use firm closure interacted with industry, region, season and year dummies as instruments for unemployment. We estimate the following model

$$Y_i = \alpha + \tau C_i + \gamma U_i + Z_i\beta + v_i \tag{7}$$

where C_i is an indicator for firm closure and U_i is a measure of unemployment. In our analysis we use three different variables to measure unemployment: the percentage of time the individual is unemployed during the first year after the reference date, an indicator variable equal to one if there is positive unemployment in the first year after the reference date, and an indicator variable equal to one if the women is employed less than 75% of the first year after the reference date.[27]

Table 5 shows the results from the IV estimations, with models 1 to 3 representing the three different measures of unemployment.[28] A remarkable pattern emerges; whatever measure we use, unemployment never has a negative effect on fertility. On the contrary, all the point estimates are even positive although not significantly different from zero. On the other hand, the direct impact of firm closure on fertility is negative in all cases and comparable in size to the effects in Table 3, although here the effects are less precisely estimated.

[27]We use different measures of unemployment to capture different aspects of the phenomenon. For displaced women the unemployment rate in the first year is 13%, compared to 2% for the controls; among those experiencing at least one day of unemployment the rate is 35% for the displaced group and 23% for the control group. Registered unemployment may not coincide with the time out of a job, so we also look at employment and consider individuals employed less than 75% of the year. 30% of the displaced women are employed less than 75% in the first year compared to 9% of the controls.

[28]For these IV estimates we have to use unweighed regressions because there is no clear correspondence between our chosen indicators of unemployment and the composition of the firm closure vs. non-firm closure sample. Our specification in Table 5 therefore has to be compared with the first result in the lower panel of Table 3, which is very close to the average effect across all specifications.

In the lower panel, we concentrate on white collar workers, because we are concerned that the differential unemployment experience across occupational groups might be what is driving our results. Remember, that we find the biggest fertility effects for white collar workers. If the latter experience lower unemployment rates than blue collar workers this might explain the lack of an unemployment effect for the whole sample. However, the same pattern emerges for white collar workers; the indirect effect of firm closure via unemployment is zero or even positive, while the direct effect of firm closure is larger, negative, and significant at least over the 6-year horizon.

This confirms that for women in white collar jobs, a firm closure has a detrimental *career* effect. Regardless of whether they become unemployed or not, women hit by a firm closure have to postpone their fertility plans in order not to jeopardize their transition to a new firm. This effect extends to a medium-term perspective, within 6 years after firm closure birth rates are reduced by approximately 4 percentage points, which corresponds to a 10% reduction in mean birth rates. Given that we cannot observe completed fertility we cannot conclusively determine whether fertility is only postponed or permanently lowered. Often though temporary postponement may lead to a permanent reduction in fertility not least because for biological reasons the probability of conception decreases with age.

6.5 Theoretical Interpretation of the Empirical Findings

The empirical analysis of employment and wage development after job loss shows that displacement mainly affects income through lower employment, while re-employment wages are hardly affected by the job loss. The employment effects are largest in the first year after job loss and rapidly decrease in subsequent years. We conclude therefore that for young women job displacement is a transitory shock; relative to their lifetime income, displacement effects are

likely to be small. Nevertheless we see substantial reductions in fertility which appear to be stable at least over a horizon of 6 years. What mechanisms within the theoretical context are responsible for this effect?

Our theoretical model identifies four main effects by which a job loss might affect fertility decisions. The substitution effect predicts that births are transferred to periods with lower opportunity costs, which would mean a higher birth rate after job loss. All other effects predict delays in fertility or lower fertility rates. The income effect accounts for the inability to smooth consumption during periods with lower income. The employability effect is due to the difficulties young mothers might face in finding a new job. The career effect results from long term income losses a mother might incur if she is unable to invest into human capital at a crucial stage of her career. How can we make use of the empirical evidence to distinguish between the different mechanisms and single out the most important ones?

Looking at the pattern of fertility reductions over time and comparing results across subgroups, it becomes clear that we do not find any evidence of a substitution effect. The negative effects of job displacement seem to outweigh any incentives to increase fertility. In particular, the penalty of a lower job finding rate for young mothers might be higher than the opportunity cost effect during unemployment, which would imply that the employability effect directly counteracts the substitution effect.

To distinguish between the income effect and other effects predicting a delay in fertility, we note that given our evidence on earnings, the only channel through which an income effect can influence fertility decisions is unemployment. However, when we compare the impact of unemployment on fertility with the direct effect of job loss using the instrumental variables strategy we find no evidence of a negative impact of unemployment on fertility. What we do find is a direct effect of unexpected job loss on fertility, which is present irrespective of whether

the woman experiences a spell of unemployment or not. This leads us to rule out explanations in terms of the income effect.

The empirical analysis of the fertility effects by subgroup also provides strong evidence of a career effect. Especially, we find that only women in white collar occupations reduce their fertility, but woman in blue collar jobs do not. Blue-collar jobs provide a low level of employment protection and limited opportunities for career advancement. Women in white collar occupations, on the other hand, face stronger incentives to invest in career specific human capital and are thus more likely to suffer from a job loss. Similar arguments apply to workers with previously high wages or high wage growth.

Our empirical set up does not allow us to distinguish clearly between career and employability effects. If we assumed that the reduction in job finding rates due to fertility is the same across all groups of mothers, we would think that there is no employability effect as we find no fertility reduction among blue collar women. However, if women in career-oriented occupations face more difficulties in finding a suitable job in the presence of a young child our results could be interpreted as a combination of the employability and career effects. As we have seen in the example in Figure 1 displaced women in career-oriented occupations might face large income losses having a baby because of the interaction of both effects. These women react by returning to a new job postponing maternity, and thereafter fully invest in career specific training before having children. Hence, all we can say is that both effects work in the same direction and contribute to reduce fertility among white collar women after job displacement.

7 Conclusions

The aim of this paper is to explore how women's fertility decisions are affected by career considerations. The literature has been aware of the incompatibility of career and family demands for women in top positions (Armenti, 2004; Cooney

and Uhlenberg, 1989; Ginther and Kahn, 2006), but there is a lack of formal discussion about how much career choices drive the fertility behavior of women in the overall population.

We investigate fertility responses to an unexpected career interruption in the form of a job loss after firm closure. We lay out a theoretical model which allows us to distinguish between different channels through which job loss can affect fertility decisions. A key feature of the model is that we consider the accumulation of firm or career specific human capital, which is destroyed as a consequence of the firm closure and needs to be re-built upon taking a new job. Our empirical results show that the main factor responsible for a reduction of fertility after job loss is the difficulty women face in re-establishing their careers. The experience of unemployment itself is not able to account for this. This effect is not confined to women in top positions, but we find a considerable reduction in fertility for the average white collar worker over a period of six years. On the other hand, fertility decisions of blue collar workers, who face little opportunities for career advancement in their jobs, do not seem to be affected by a job loss.

Our results highlight that the increasing importance of professional careers for female workers might go a long way in explaining recent fertility trends. In this context, even temporary labor market shocks, which hit young women at crucial stages of their careers may significantly contribute to delays or reductions in fertility. Our research stresses the importance of career progression and continuity and deemphasizes the role of unemployment, which has assumed particular relevance in explaining fertility trends in some European countries (Adsera, 2005; Ahn and Mira, 2002). Examples of constraints which impede the development of careers would be the high prevalence of fixed-term contracts which delay the entry into permanent positions and thus the establishment of stable careers for young workers (de la Rica, 2005). Our results also imply that family policies like the Austrian and German one, which rely mainly on

income support for young parents should be complemented by measures aimed at protecting young mothers' career prospects and labor market attachment, such as the provision of child care facilities, full-time schools, and more flexible working-time arrangements.

Appendix A

Model setup

Consider a discrete-time setting where individuals have a finite planning horizon and a subjective discount rate ρ. Let r denote the fixed interest rate in the economy. We assume that a woman can influence the probability of birth p_t via the choice of contraception. She derives utility from consumption and the number of children, and disutility from practicing contraception. It is assumed that the utility function is separable over time and in all its arguments. Per period utility is given by $u(c_t) + v(k_t) - \psi(p_t)$, where c_t and k_t are consumption and the number of children, u and v are monotonically increasing and concave, while ψ is monotonically decreasing and convex. All functions are assumed to be twice differentiable.

At the beginning of period t the state variables are assets A_t, employment status e_{t-1} in period t-1 ($e_{t-1} = 1$ if the woman was employed last period and 0 otherwise), k_{t-1} number of children by the end of period $t-1$, and T_t the accumulated job tenure. We assume that the woman enters period 1 after job displacement, when she is unemployed and tenure is reset to zero. For simplicity we also assume she is childless at the beginning of period 1, i.e. $e_0 = 0, k_0 = 0, T_1 = 0$. If the woman is unemployed she receives unemployment benefits b and finds a job at a rate which is exogenously given. Once found, a job is expected to last indefinitely. Wages on the job are paid according to a deterministic scheme and rise with tenure T. Specifically, $w'(T) > 0$, $w''(T) < 0$, and $w(0) > b$.

If a woman is employed at the beginning of period t she chooses p_t and with probability p_t she immediately has a child. After giving birth the woman spends the current period t entirely on maternity leave and earns maternity benefits m with $m < b < w(0)$. During maternity leave she does not accumulate tenure, but she enters the next period $t+1$ in employment and with $T = T_{t-1}$. With probability $1 - p_t$ the woman does not have a child in period t and earns wage $w(T_t)$. During employment she also accumulates tenure. A woman with no child accumulates one period of tenure per period worked, while a woman with a child accumulates tenure at a rate $\gamma < 1$ each period worked.

If the woman enters period t being unemployed, she is assumed to find a job at the exogenous rate s if childless and at a lower rate $s\delta$ ($\delta < 1$) if she has a child. If the woman finds a job, she immediately enters employment at the beginning of the period and her choices are the same as those of the employed woman. If she remains unemployed, she chooses p_t and if she has a child she goes on maternity leave and gets maternity benefits m in period t, but she enters period $t+1$ still unemployed and with a lower probability of finding a new job.

Summarizing, the woman is faced with the following problem

$$\begin{aligned} \max_{k_t, c_t, A_t} \quad & \sum_{t=0}^{t_{max}} (1+\rho)^{-t}(u(c_t) + v(k_t)) \\ \text{subject to} \quad & A_{t+1} = (1+r)(A_t + Y_t - c_t) \end{aligned} \tag{8}$$

and the tenure accumulation process is given by

$$T_{t+1} = T_t + e_t(1 - I(k_t > 0)) + \gamma e_t I(k_t > 0) \tag{9}$$

where period t income is given by $Y_t \in \{w, b, m\}$ and $I(k_t > 0)$ denotes the indicator function equal to 1 if a child is present and zero otherwise.

Optimal Fertility Choice

We focus on decisions about the first birth, because this turns out to be the

relevant decision for the women in our empirical setup. In each period decisions are taken at three stages. First, the labor market situation of the woman is observed. She is either employed or unemployed, and if unemployed the outcome of her job search effort is realized. Second, the fertility outcome is chosen conditional on her labor market status. Finally, optimal consumption and savings are decided upon and utility is maximized. We express each of these decision nodes in terms of Bellman equations.

Let us start from the situation of a woman who in period t is unemployed and looking for a job. Her value function at the stage of job search U_s is given by

$$U_s(0, k_{t-1}, A_t) = s(k_{t-1})V(0, k_{t-1}, A_t) + (1 - s(k_{t-1}))U(0, k_{t-1}, A_t), \quad (10)$$

with $s(k_t) = s(1 - I(k_t > 0)) + s\delta I(k_t > 0)$.

Now consider what happens when labor market status is known and the woman takes her fertility decision. The value functions for employed and unemployed women, V and U respectively, are given by

$$\begin{aligned} V(T_t, k_{t-1}, A_t) = \max_{p_t} \quad & p_t V_c(T_t, k_{t-1} + 1, A_t) + \\ & (1 - p_t)V_c(T_t, k_{t-1}, A_t) - \psi(p_t), \quad (11) \\ U(0, k_{t-1}, A_t) = \max_{p_t} \quad & p_t U_c(0, k_{t-1} + 1, A_t) + \\ & (1 - p_t)U_c(0, k_{t-1}, A_t) - \psi(p_t). \quad (12) \end{aligned}$$

After the realization of the job search and fertility outcomes all women know e_t, k_t, their income $I_t \in \{w, b, m\}$ and T_{t+1}, and choose consumption c_t and savings for period t.[29] At this stage the value functions for employed women with children and without children, V_c^1 and V_c^0 respectively, are

$$V_c^1(T_t, k_t, A_t) = \max_{A_{t+1}} \quad u(m + A_t - \frac{A_{t+1}}{1 + r}) +$$

[29]Other family income, like husband's income is assumed to be exogenous and does not enter our model in order to simplify the discussion.

$$v(k_t) + \frac{V(T_{t+1}, k_t, A_{t+1})}{1+\rho}, \tag{13}$$

$$V_c^0(T_t, k_t, A_t) = \max_{A_{t+1}} \quad u(w(T_t) + A_t - \frac{A_{t+1}}{1+r}) + v(k_t) + \frac{V(T_{t+1}, k_t, A_{t+1})}{1+\rho}. \tag{14}$$

The value functions for unemployed women U_c^1 and U_c^0 are determined analogously.

¿From equations (11) and (12) we derive the first order conditions for optimal fertility choice in the employed and unemployed states

$$\psi'(p_t^*) = V_c^1 - V_c^0, \tag{15}$$

$$\psi'(p_t^*) = U_c^1 - U_c^0. \tag{16}$$

These equations express the fact that the woman chooses p_t to equate the marginal cost and the marginal benefit of fertility, which is given by the difference between the optimized values of having an additional child or not.

Model Predictions

Our predictions about optimal fertility choices follow from the comparative statics derived from (15) and (16). This way we explore the different channels through which a job loss can affect fertility decisions. The first effects of job loss are to lower income and to lower the opportunity cost of taking a maternity break because of a period of unemployment. To consider the consequences of these effects, we examine the change in p_t^* due to a change in the asset level and a change in the wage rate. To simplify notation let $c_{t,e}^1$ denote optimal consumption of an employed woman who is on maternity leave in period t, $c_{t,e}^0$ the consumption of a woman who is working, and $c_{t,u}^0$ the consumption of a woman who is unemployed.

Differentiating equation (15) with respect to A_t and applying the envelope

theorem, we obtain

$$\partial p^*_{te}/\partial A_t = \{u'(c^1_{t,e}) - u'(c^0_{t,e})\}/\psi''(p^*_{te}) \geq 0, \quad (17)$$

$$\partial p^*_{tu}/\partial A_t = \{u'(c^1_{t,u}) - u'(c^0_{t,u})\}/\psi''(p^*_{tu}) \geq 0. \quad (18)$$

where p^*_{te} and p^*_{tu} are the optimal values for the employed and unemployed woman, respectively. The effect of additional wealth on fertility depends on the difference in marginal utilities between the maternity leave and working states. If women can smooth consumption perfectly, they set $u'(c^1_{t,e}) = u'(c^0_{t,e})$, and the effect on fertility is zero. If women face borrowing constraints, then $u'(c^1_{t,e}) - u'(c^0_{t,e}) > 0$, which implies that an additional unit of wealth increases fertility. We call this the *income effect*.

Next consider an increase in period t wage w

$$\partial p^*_{te}/\partial w = -u'(c^0_{t,e})/\psi''(p^*_{te}) \leq 0 \quad (19)$$

The effect of an increase in w in period t is negatively related to $u'(c^0_{t,e})$, because a higher wage decreases the marginal return to having a child to the extent that it raises the value of being employed. This is the *substitution effect*.

Apart from unemployment, a job loss can have significant effects on a woman's career because it destroys her firm or career specific human capital. This channel can be captured by investigating the effect of job tenure on the fertility choice. To simplify matters, assume that a woman's lifetime can be separated into periods in which she is fertile ($0 \leq t \leq R$) and periods in which she is too old to have children ($R + 1 \leq t \leq t_{max}$). A woman employed in the last fertile period R remains employed for the rest of her life and faces a deterministic income stream.

Suppose that in period R the woman is still childless.[30] In this case the effect

[30]Note that the fact that a woman is still childless in period R may simply be a consequence of a series of unlucky realizations of p_t. Also, we focus here on decisions about the first birth, as discussed above.

of an increase in tenure on fertility is given by

$$\begin{aligned} \partial p^*_{Re}/\partial T &= -u'(c^0_{R,e})w'(T)/\psi''(p^*_{Re}) + \\ &\frac{1}{\psi''(p^*_{Re})}\sum_{t=1}^{t_{max}-R}\frac{1}{(1+\rho)^t}u'(c^1_{t,e})w'(T+\gamma(t-1)) - \\ &\frac{1}{\psi''(p^*_{Re})}\sum_{t=1}^{t_{max}-R}\frac{1}{(1+\rho)^t}u'(c^0_{t,e})w'(T+t) \end{aligned} \tag{20}$$

The first term in this equation is negative and reflects the higher income loss during maternity leave for women with higher tenure. The difference between the second and third terms, however, is positive because it reflects the differences in marginal utility from a reduced income stream following childbirth which is due to the slowdown in tenure accumulation. Given that the time employed after the fertile life span is long enough, the discount rate is not too large, or the "maternity penalty" in terms of future tenure accumulation is substantial, the positive long term effect could easily outweigh the negative income loss from maternity leave, and $\partial p^*_{Re}/\partial T > 0$. By solving the model backwards we can generalize the same argument to earlier periods and show that fertility is delayed to periods with higher job tenure. This is what we call the *career effect*.

Finally, consider the difference in fertility decisions between unemployed and employed women. Using an argument similar to the one above we start by looking at employed versus unemployed women who are still childless at the beginning of their last fertile period. From equations (15) and (16) we know that differences in their optimal fertility choices are reflected in the gap between the value functions for having a child and for not having a child; $V^1_c - V^0_c$ and $U^1_c - U^0_c$, respectively.

In period R the immediate income loss from having a child is lower for unemployed women, because $u - m < w(T) - m$. In subsequent periods, how-

ever, the gap in income for unemployed women is determined by the lower job finding probability following the birth and the lower tenure accumulation once they find employment. Women employed in period R only suffer income losses in subsequent periods from the lower tenure accumulation. Therefore if the long term effect outweighs the income loss in the first period we will have $U_c^1 - U_c^0 > V_c^1 - V_c^0$ and hence $p_{tu}^* \leq p_{te}^*$. If we transfer this result to earlier periods this implies that women will try to delay childbirth until they have found a new job. We call this the *employability effect*.[31]

Appendix B

Birth records

The Austrian Social Security Database (ASSD) contains entries of births, spells of maternity protection, and parental leave for the period between 1972 and 2002. As discussed in section 3, maternity protection and parental leave is only available to women who worked a certain number of weeks prior to giving birth. Although the ASSD contains actual birth dates, which could potentially be unrelated to the mother's working history, close inspection of the data reveals that the vast majority of births recorded in the data are connected to a maternity protection and/or parental leave spell. This may raise concerns that any measure of fertility directly derived from the ASSD could under-report the number of births in the population.

As a first check on the data we show in Figure 14 the yearly number of births from the Austrian vital statistics and the same number as derived from the ASSD records. The two lines track each other nicely over time. We see them getting closer as female labor force participation rises. However, differences

[31] Note that in this model the job finding rate and the reduction in the job finding rate after childbirth are exogenous, i.e. they do not depend on individual decisions about search effort. An extension of the model allowing for endogenous search effort and with search costs rising in both search effort and the number of children would lead to a similar prediction.

remain and even in the last decade we notice about 18% less births recorded in the ASSD. An obvious reason for the difference in recorded births is the coverage of the ASSD, where self employed, housewives, and civil servants are not included. However, it is still possible that part of the difference between the ASSD births and those recorded in the vital statistics data is due to the fact that the ASSD under-reports births to women who have a more fragmented labor force history which implies extended gaps in their ASSD records.

Because of these concerns, we access information on births from a different data source. From 1993 onwards the Austrian finance ministry keeps electronic records of child benefits paid to either the mother or father of all children below the age of 18. These records contain the birth dates of children, and in most cases both parents' social security numbers. Since take-up of child benefits is almost universal, we should observe birth dates of all children born in Austria form 1975 onwards in these records.

Using the social security number of the mother we match the mothers in the ASSD data to those in the child benefits records. Figure 15 shows the number of births in both data sources for all women in the sample selected for analysis. The gap between the two data sources is particularly big in the period before 1990. This is most likely due to births not recorded in the ASSD because the mothers in our sample had not yet entered the labor market. Between 1990 and 1998 the gap is quite narrow, as our sample of women is selected on the basis of participation to the labor market, and opens-up in later years, when a high percentage of women in our sample drops out of the labor force.

We then compare the births to each mother in the ASSD records with the births collected via the child benefits records to cross-verify the information we obtain from these sources. We find that we can match about 98% of the birth records in the ASSD using the child benefit data.[32] This implies that the child benefit

[32]Missing matches occur for children born in the early 1970's who are not likely to be eligible for benefits any more, or from cases where the father claims child benefits and does not give

data provide a good coverage for our sample of women. At the same time we find 13% additional births in the child benefits data. This seems to justify our concern that the ASSD birth records could under-report the number of births. Our final measure of fertility is therefore derived by combining the ASSD and child benefits records. This gives us not only a very good measure of fertility, which is independent of the mother's labor force status and history, but also allows us to derive the full fertility history of the women in our sample since the child records go back to 1975.

information on the mother so that we cannot find a match the mother's social security number. From 1990 onwards there are less than 1% of births in the ASSD which do not have a match in the child benefit records.

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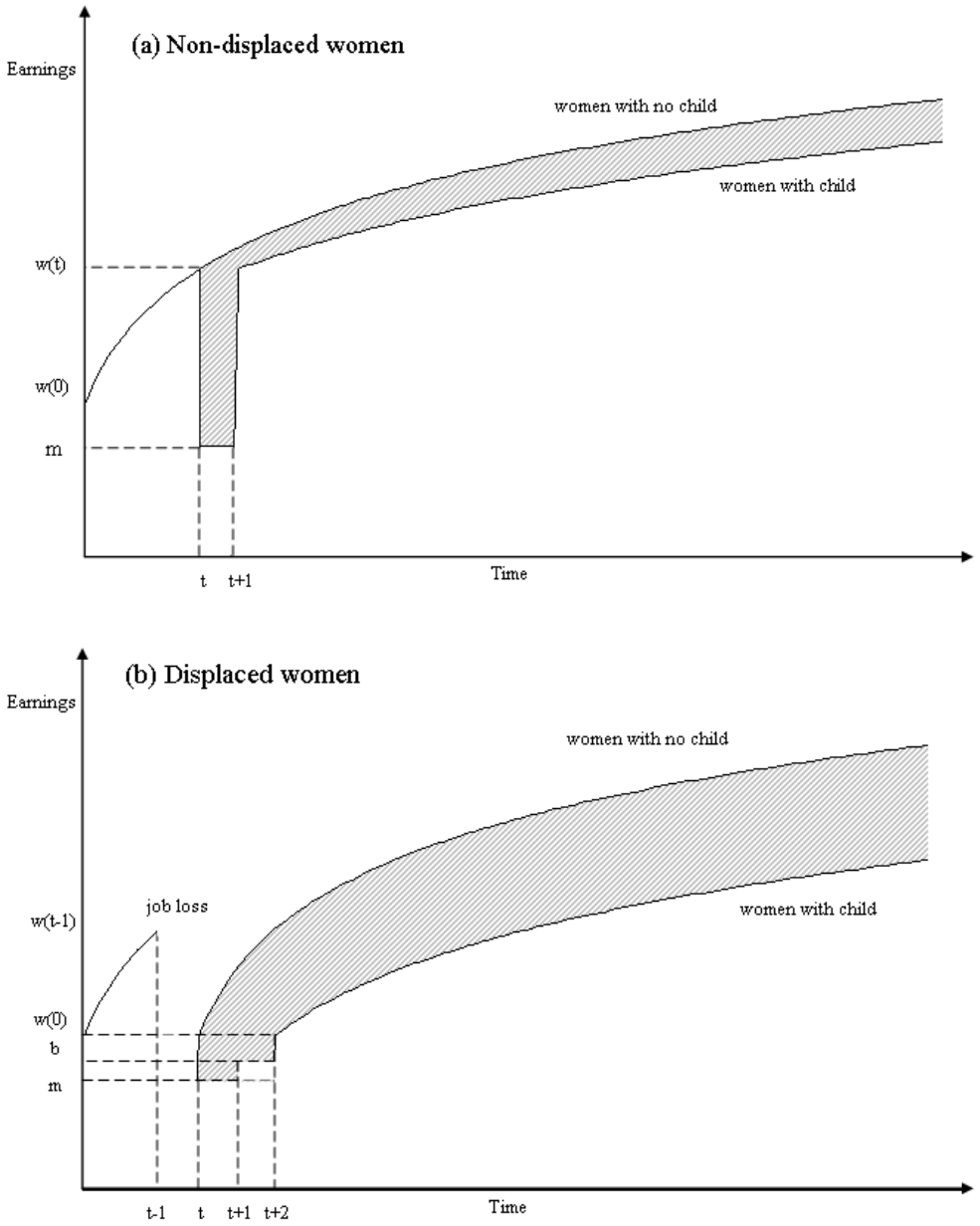
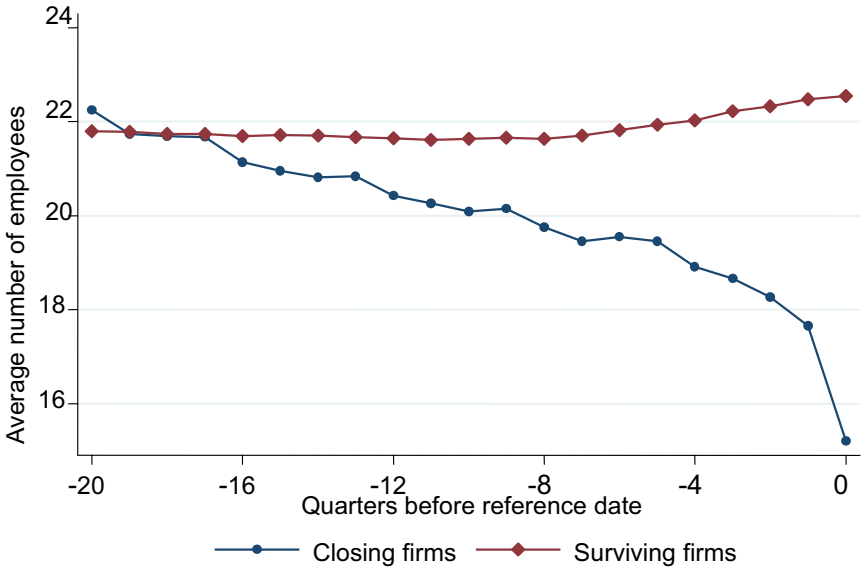
Figure 1: Fertility and job displacement



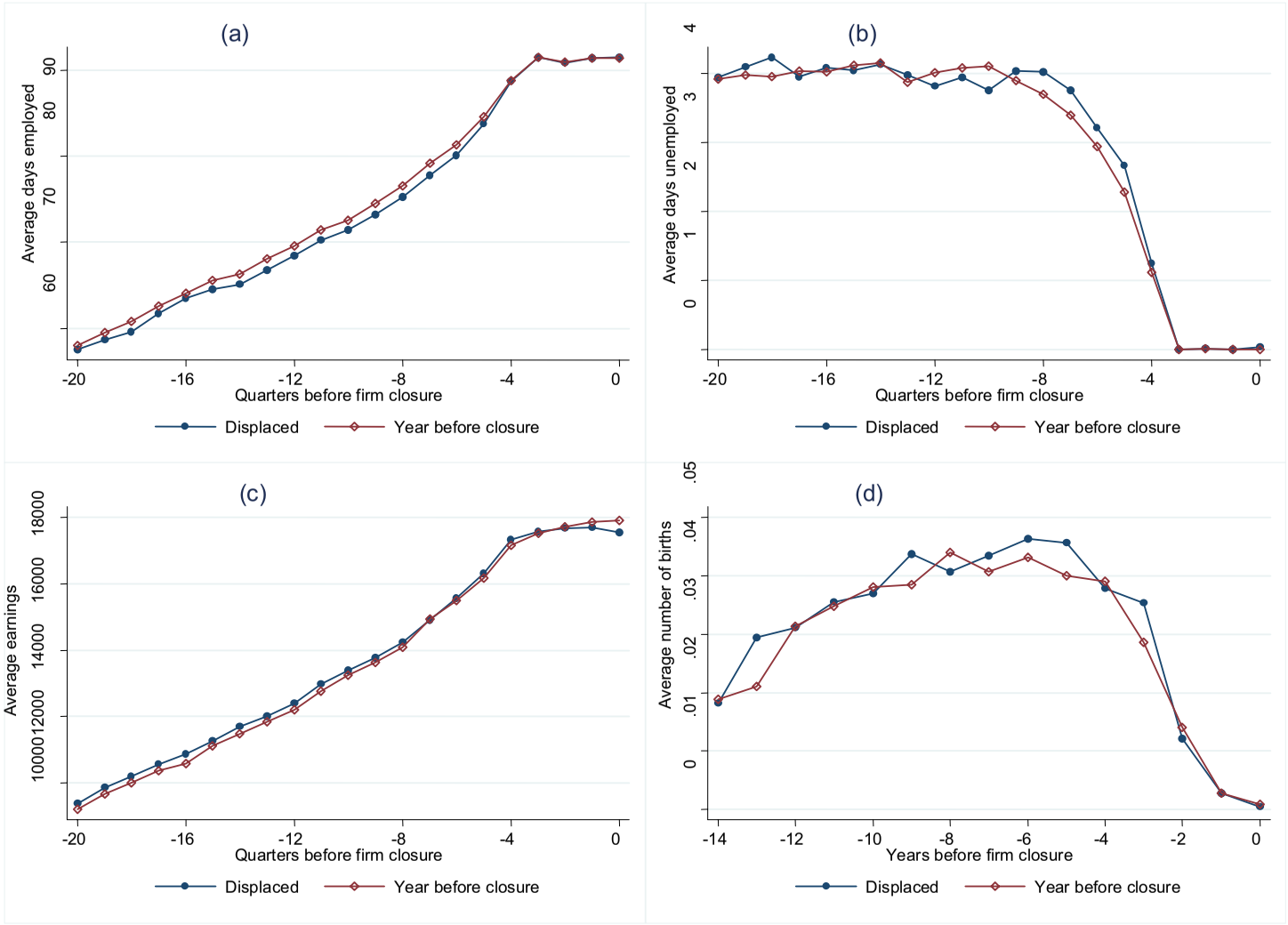
Notes: Panel (a) represents the wage-tenure profile of non-displaced women. It distinguishes between women without a child and women who have a child at time t and are on maternal leave between t and $t+1$. Panel (b) represents the wage-tenure profile of women displaced at time t-1, who are unemployed between t-1 and t. It distinguishes between women without a child and women who have a child at time t. The latter are shown to be on maternal leave between t and t+1 and to suffer a second unemployment spell between t+1 and t+2.

Figure 2: Average firm size in closing and surviving firms



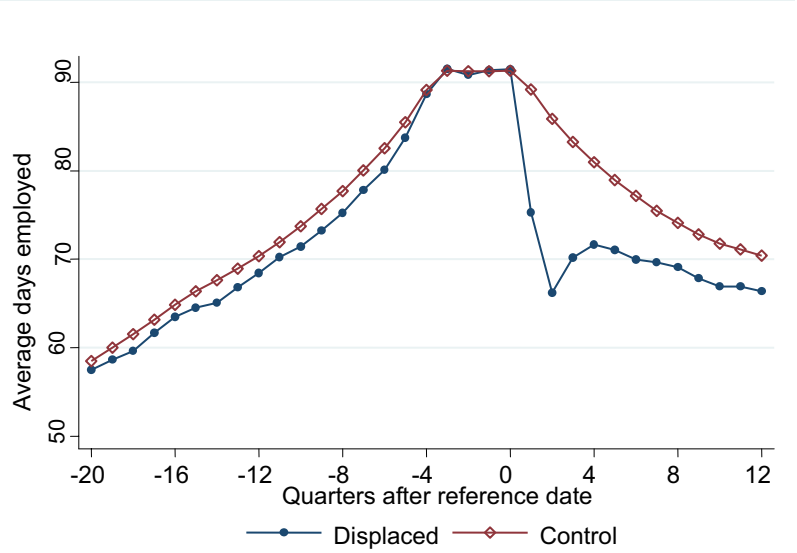
Notes: Average number of employees in closing firms and surviving firms by quarter.

Figure 3: Workforce composition of closing firms at the closure date and one year before closure



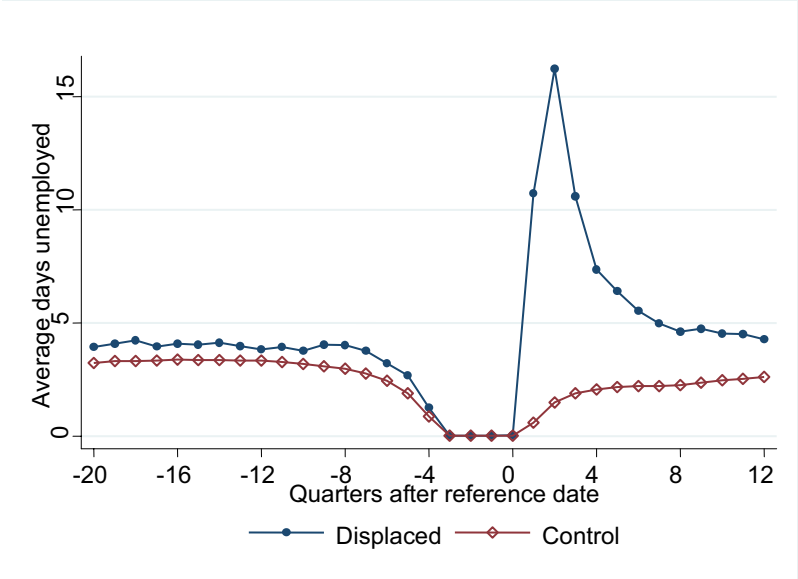
Notes: Comparison between displaced women and women working in closing firms one year before the closure. Panel (a) refers to average number of days in employment by quarter, panel (b) refers to average number of days in unemployment by quarter, panel (c) refers to average earnings by quarter, and panel (d) refers to average number of births per year.

Figure 4: Average days employed by quarter



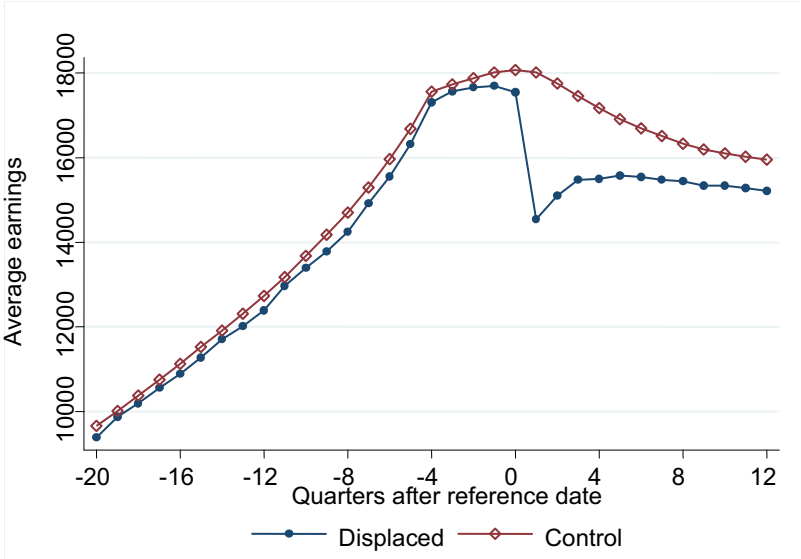
Notes: Comparison between the average number of days employed of displaced and control women by quarter.

Figure 5: Average days unemployed by quarter



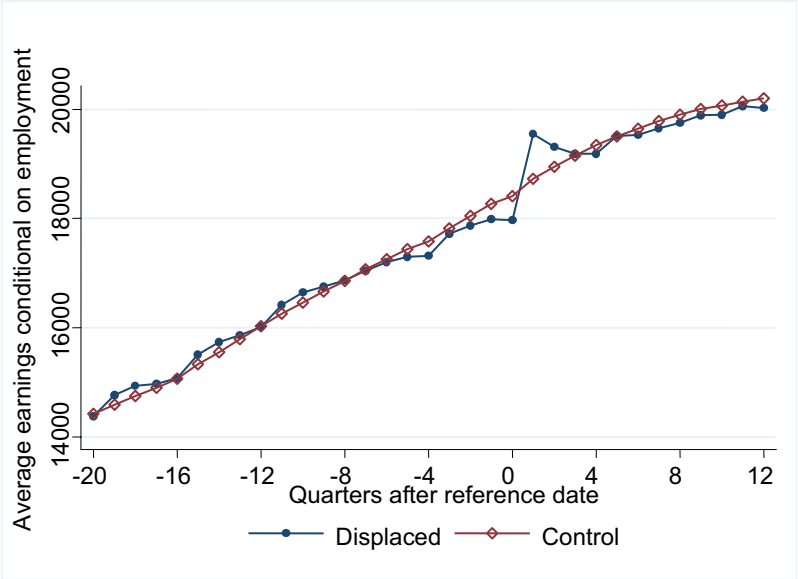
Notes: Comparison between the average number of days unemployed of displaced and control women by quarter.

Figure 6: Average earnings by quarter



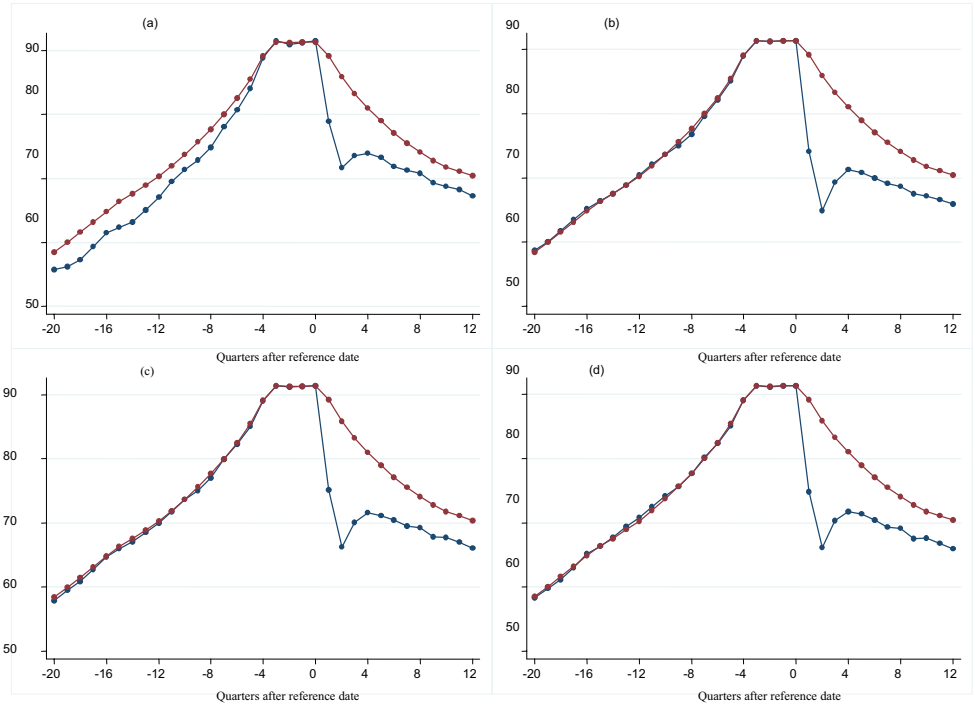
Notes: Comparison between the average earnings of displaced and control women by quarter.

Figure 7: Average earnings conditional on employment by quarter



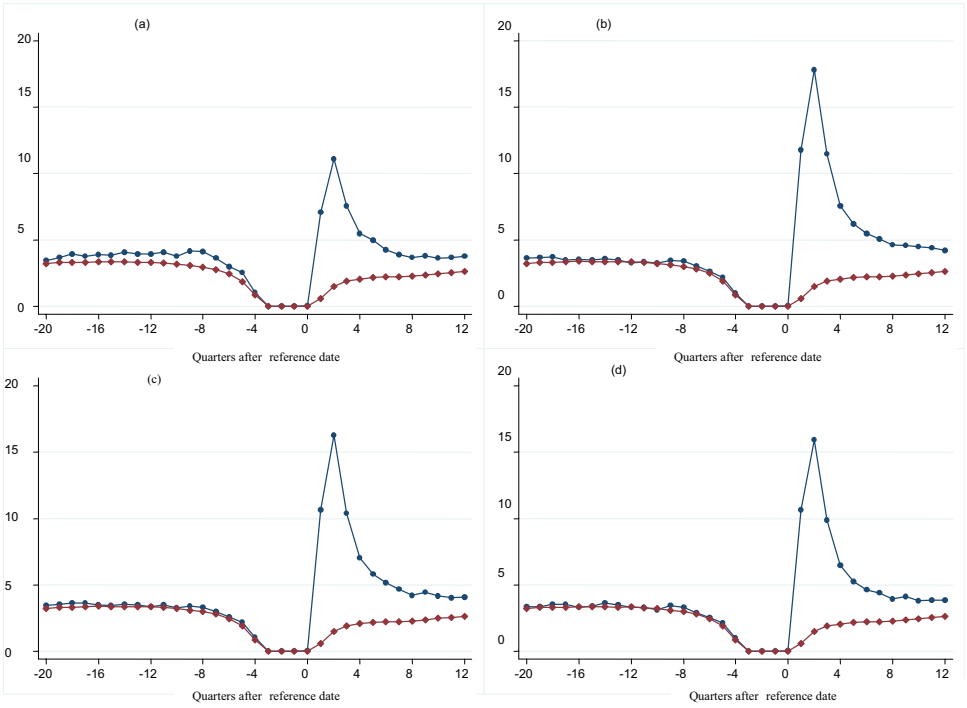
Notes: Comparison between the average earnings (conditional on employment) of displaced and control women by quarter.

Figure 8: Average days in employment by quarter weighted by propensity scores



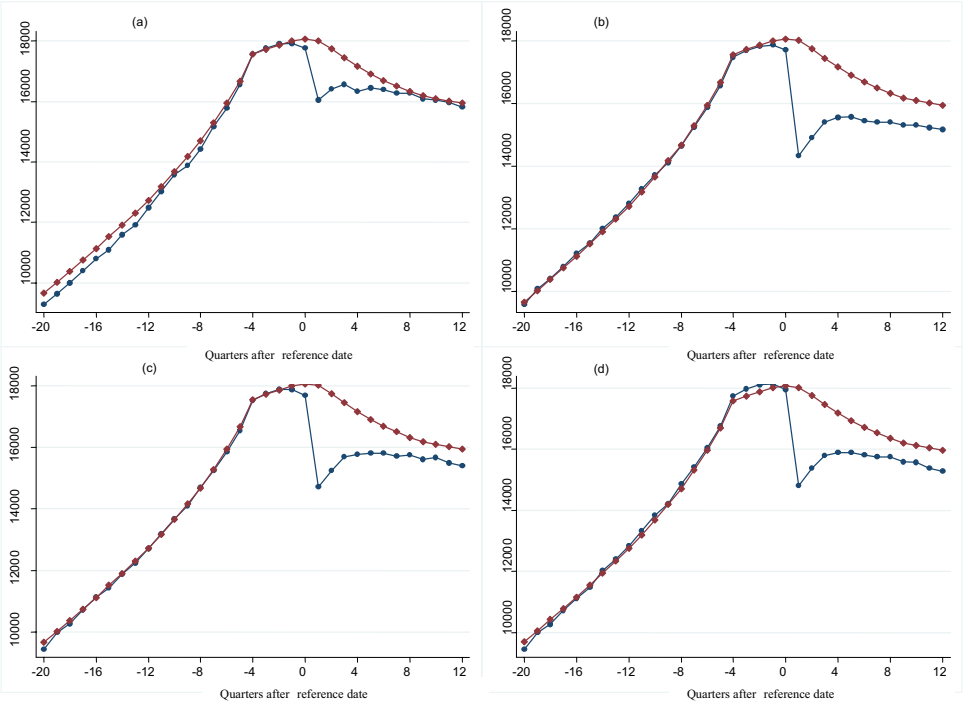
Notes: Comparison between the average number of days in employment of displaced (blue) and control (red) women by quarter. Average number of days in employment weighted by firm characteristics in panel (a), individual characteristics in panel (b), selected firm and all individual characteristics in panel (c), and all firm and all individual characteristics in panel (d).

Figure 9: Average days in unemployment by quarter weighted by propensity scores



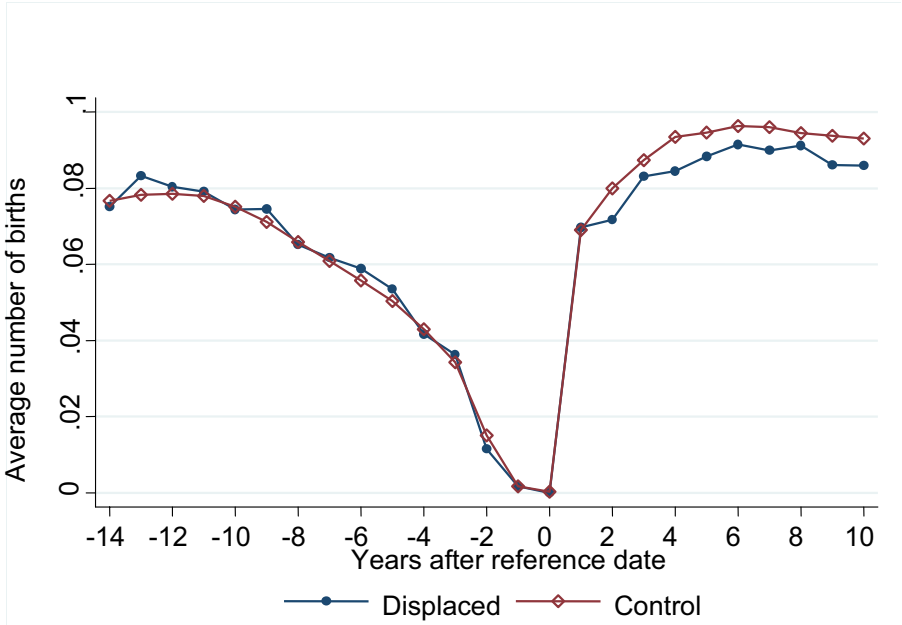
Notes: Comparison between the average number of days in unemployment of displaced (blue) and control (red) women by quarter. Average number of days in unemployment weighted by firm characteristics in panel (a), individual characteristics in panel (b), selected firm and all individual characteristics in panel (c), and all firm and all individual characteristics in panel (d).

Figure 10: Average earnings by quarter weighted by propensity scores



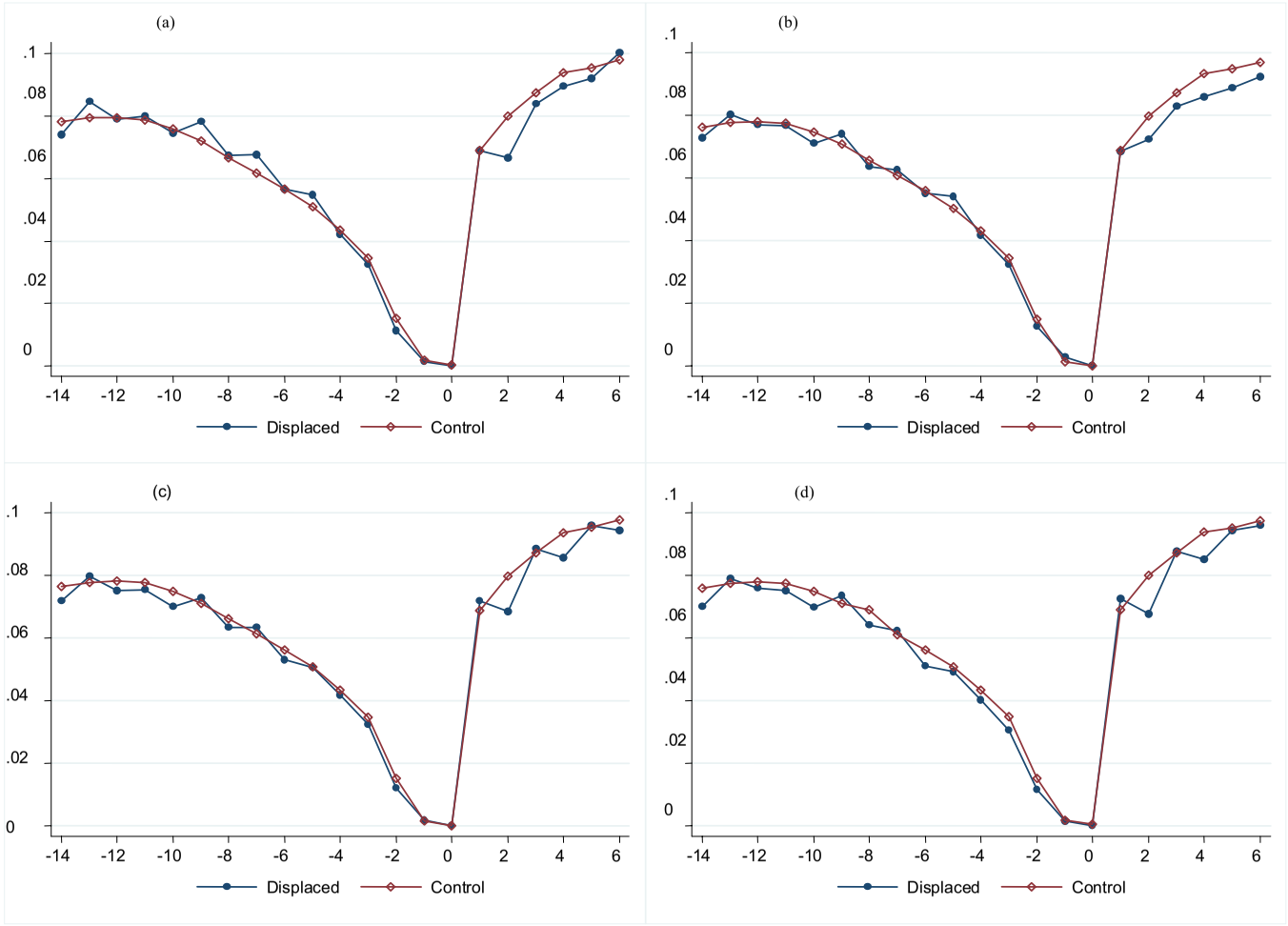
Notes: Comparison between the average earnings of displaced (blue) and control (red) women by quarter. Average earnings weighted by firm characteristics in panel (a), individual characteristics in panel (b), selected firm and all individual characteristics in panel (c), and all firm and all individual characteristics in panel (d).

Figure 11: Average number of births by year



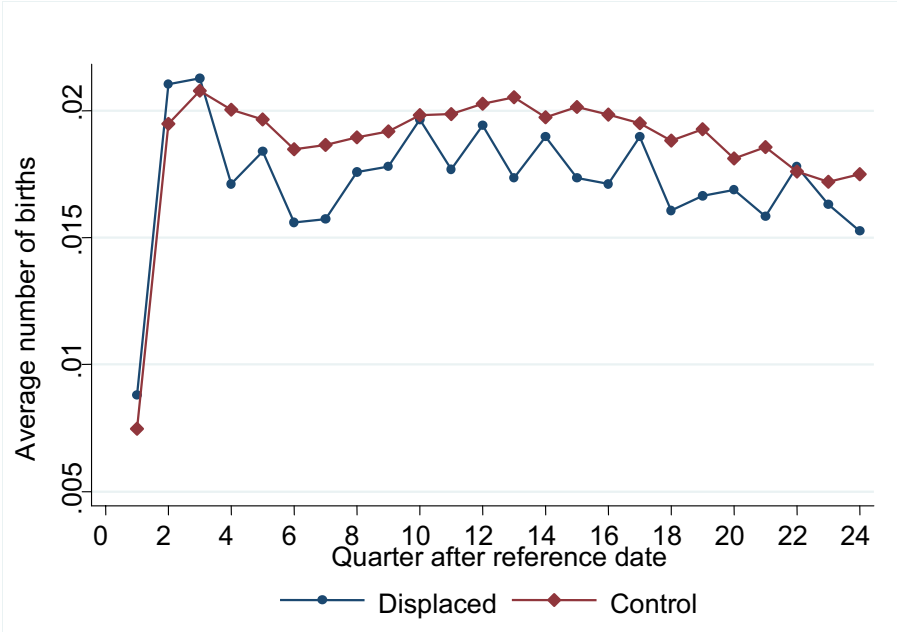
Notes: Comparison between the average number of births of displaced and control women by year after the reference date. Number of births is age adjusted.

Figure 12: Average number of births by year weighted by propensity scores



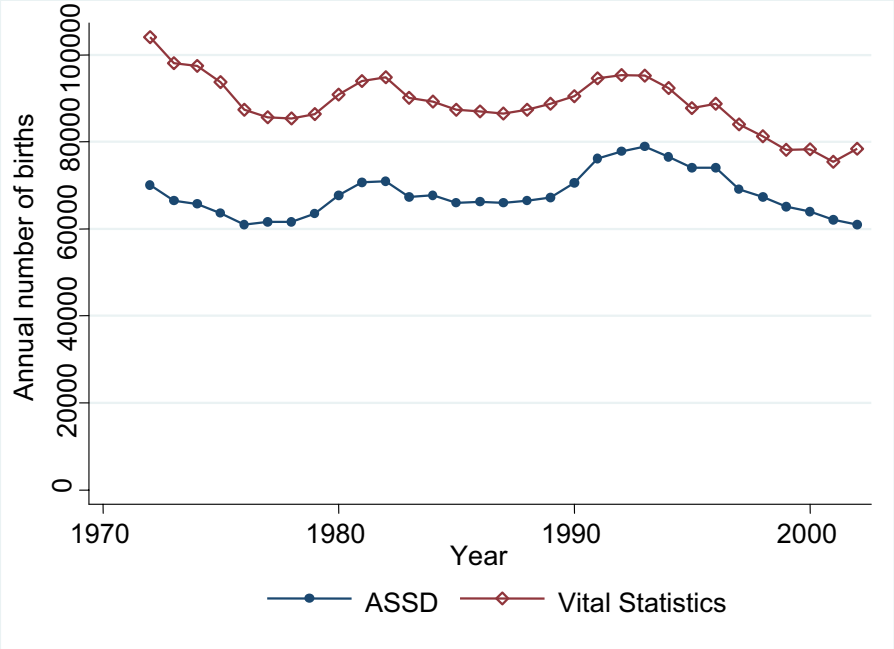
Notes: Comparison between the average number of births to displaced (blue) and control (red) women by year since reference date. Average number of births weighted by firm characteristics in panel (a), individual characteristics in panel (b), selected firm and all individual characteristics in panel (c), and all firm and all individual characteristics in panel (d).

Figure 13: Average number of births by quarter



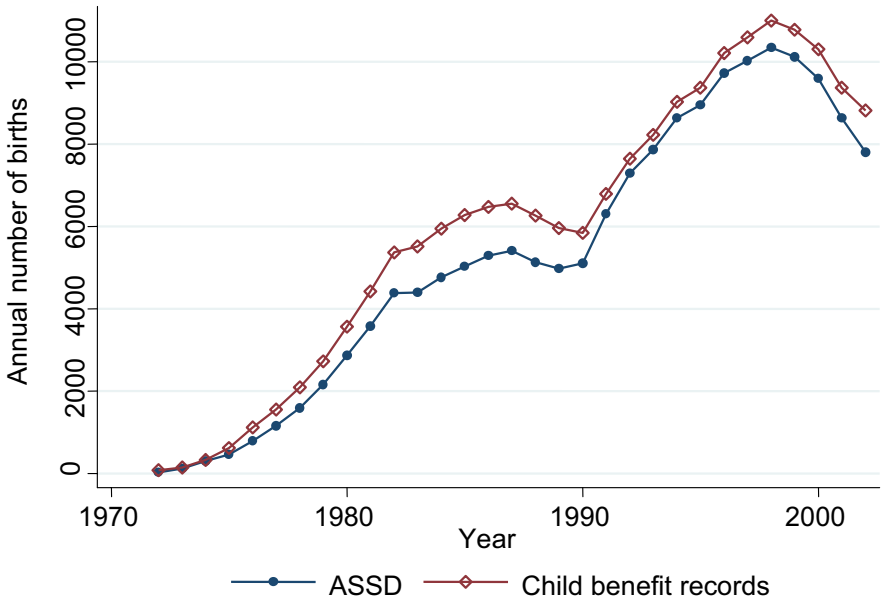
Notes: Comparison between the average number of births of displaced and control women by quarter after the reference date. Number of births is age adjusted.

Figure 14: Number of births per year



Notes: Annual number of births in the ASSD data and in the Austrian population.
Source: ASSD and Statistics Austria.

Figure 15: Number of births per year in the ASSD and Ministry of Finance child benefit records



Notes: Annual number of births in the sample selected for estimation in the ASSD and child benefit records. Source: ASSD and Austrian Ministry of Finance.

Table 1: Firm Characteristics

	Closing Firms			Surviving Firms		
	Mean	Median	Std.Dev.	Mean	Median	Std.Dev.
Employees quarter 0	15	7	24	23	10	33
Employees quarter -4	19	9	27	22	10	32
Female employees quarter 0	7	4	12	10	5	15
Female employees quarter -4	8	4	14	9	5	14
Employment growth year -1	-0.10	-0.1	0.34	0.05	0	0.22
Employment growth year -4 to -2	0.06	0.00	0.37	0.14	0.00	0.33
Turnover quarter 0	0.42	0.18	1.13	0.15	0.10	0.20
Turnover year -2	0.95	0.76	1.43	0.69	0.56	0.65
Turnover year -3	0.89	0.71	0.87	0.71	0.57	0.83
Median earnings quarter 0	18,143	16,720	7,330	18,152	17,156	6,559
Median earnings quarter -4	17,252	15,878	6,866	17,476	16,506	6,411
Firm age	11.52	10	8.19	13.53	15	7.91
Censored firm age	0.26	0	0.44	0.35	0	0.48
Manufacturing	0.26	0	0.44	0.23	0	0.42
Sales	0.41	0	0.49	0.34	0	0.47
Transport	0.05	0	0.22	0.04	0	0.20
Services	0.28	0	0.45	0.39	0	0.49
Vienna	0.28	0	0.45	0.23	0	0.42
Lower Austria	0.18	0	0.39	0.18	0	0.38
Upper Austria	0.17	0	0.37	0.17	0	0.38
South	0.18	0	0.39	0.20	0	0.40
Salzburg	0.07	0	0.26	0.08	0	0.27
West	0.12	0	0.32	0.14	0	0.35
Year						
1990	0.08		0.28	0.11		0.31
1991	0.10		0.30	0.11		0.31
1992	0.11		0.32	0.11		0.31
1993	0.11		0.32	0.10		0.30
1994	0.11		0.31	0.10		0.31
1995	0.11		0.32	0.11		0.31
1996	0.12		0.32	0.11		0.31
1997	0.12		0.33	0.12		0.33
1998	0.13		0.33	0.13		0.34
Quarter						
10-Feb	0.19		0.39	0.24		0.42
10-May	0.21		0.40	0.25		0.43
10-Aug	0.21		0.41	0.26		0.44
10-Nov	0.39		0.49	0.26		0.44
Observations	3,050			41,240		

Notes: Sample of firms with at least one female employee aged 18-35 with one year of tenure. For closing firms quarter 0 refers to the closing date, for surviving firms it represents the reference date. Quarter -4 refers to the quarter one year before the reference date. Year -1 is the last year before the reference date, year -2 the second but last year, and so on. Turnover rates are defined by the number of hires plus number of separations within the given year divided by number of employees at the end of the year. Median earnings refer to the median earnings in Euro (prices 2000).

Table 2: Individual Characteristics

	Displaced	Employed one year before closure	Control	Displaced-Control % difference
Age	27.23	27.01	27.10	0.46%
	(4.67)	(4.68)	(4.71)	
Austrian	0.94	0.95	0.96	-2.02%
	(0.24)	(0.22)	(0.21)	
Age at labor market entry	17.02	16.90	16.91	0.66%
	(3.03)	(2.84)	(2.72)	
Apprenticeship	0.40	0.40	0.37	9.44%
	(0.49)	(0.49)	(0.48)	
Blue collar	0.24	0.27	0.26	-8.97%
	(0.42)	(0.44)	(0.44)	
Number of children	0.46	0.44	0.44	5.21%
	(0.76)	(0.75)	(0.75)	
Experience (months)	104	105	104	0.37%
	(53.40)	(53.36)	(52.68)	
Tenure (months)	43	44	48	-11.98%
	(34.40)	(34.75)	(36.81)	
Earnings	17,547	17,902	18,070	-2.89%
	(7,765)	(7,027)	(7,373)	
Pregnant	0.029	0.028	0.026	9.88%
	(0.17)	(0.16)	(0.16)	
Births next 3 years	0.21	0.22	0.23	-5.57%
	(0.47)	(0.46)	(0.47)	
Births next 6 years	0.42	0.44	0.46	-7.97%
	(0.69)	(0.69)	(0.70)	
Observations	8,651	10,229	218,548	
Non-pregnant women				
Births next 3 years	0.18	0.18	0.20	-7.92%
	(0.43)	(0.42)	(0.44)	
Births next 6 years	0.39	0.40	0.43	-9.35%
	(0.66)	(0.66)	(0.68)	
Observations	8,401	9,945	212,738	

Notes: Variable means, standard deviations in parentheses. Displaced group includes women aged 18-35 with at least one year of tenure in closing firms at the closure date. Employed one year before closure group includes women aged 18-35 with at least one year of tenure and employed in the closing firms one year before the closure date. Control group is a 5% random subsample of women aged 18-35 with at least one year of tenure in firms that do not close within the next 2 years of the reference date. The outcome variables are given by the number of children born after 3 (or 6) years of the reference date. When considering non-pregnant women the outcome variables include the number of children born between 6 months and 3 (or 6) years of the the reference date.

Table 3: Effect of firm closure on fertility

	Births next 3 years	Births next 6 years
With no covariates		
Unweighted	-0.016**	-0.040**
	(0.005)	(0.008)
Weighted: firm char.	-0.010	-0.022
	(0.007)	(0.012)
Weighted: individual char.	-0.014**	-0.036**
	(0.005)	(0.008)
Weighted: individual and some firm char.	-0.010	-0.021*
	(0.007)	(0.010)
Weighted: individual and all firm char.	-0.010	-0.022
	(0.010)	(0.013)
With covariates		
Unweighted	-0.011*	-0.027**
	(0.005)	(0.007)
Weighted: firm char.	-0.011	-0.023*
	(0.007)	(0.012)
Weighted: individual char.	-0.014**	-0.034**
	(0.005)	(0.008)
Weighted: individual and some firm char.	-0.011	-0.024*
	(0.006)	(0.010)
Weighted: individual and all firm char.	-0.013	-0.027*
	(0.008)	(0.012)
Observations	221,139	221,139

Notes: Displaced group includes women aged 18-35 with at least one year of tenure in closing firms at the closure date. Control group is a 5% random subsample of women aged 18-35 with at least one year of tenure in firms that do not close within the next 2 years of the reference date. The outcome variable births next 3 years measures the number of children born between 6 and 36 months after the reference date. Covariates include: number of children born in the last 3, 6, and 9 years, age and its square, tenure, experience, indicator for apprenticeship education, blue collar status, earnings at the reference date, year and quarter dummies. Robust standard errors clustered at the individual level reported. For weighted regressions standard errors are bootstrapped (500 replications). Symbols: ** significant at 1%;* significant at 5%.

Table 4: Heterogeneous effects of firm closure on fertility

	With no covariates		With covariates	
	Births next 3 years	Births next 6 years	Births next 3 years	Births next 6 years
No previous children	-0.022**	-0.053**	-0.018**	-0.044**
	(0.006)	(0.009)	(0.006)	(0.009)
Previous children	0.002	0.005	0.003	0.005
	(0.009)	(0.013)	(0.008)	(0.013)
Bluecollar	0.012	-0.001	0.012	-0.001
	(0.010)	(0.016)	(0.010)	(0.015)
Whitecollar	-0.024**	-0.052**	-0.018**	-0.036**
	(0.006)	(0.009)	(0.005)	(0.008)
Age<25	-0.010	-0.039**	-0.009	-0.033*
	(0.009)	(0.013)	(0.008)	(0.013)
Age>=25	-0.016*	-0.033**	-0.013*	-0.025**
	(0.006)	(0.009)	(0.006)	(0.009)
Experience 1st tertile	-0.013	-0.034*	-0.010	-0.020
	(0.008)	(0.013)	(0.008)	(0.013)
Experience 2nd tertile	-0.025**	-0.057**	-0.023**	-0.051**
	(0.008)	(0.013)	(0.008)	(0.013)
Experience 3rd tertile	0.001	-0.010	0.001	-0.011
	(0.008)	(0.013)	(0.008)	(0.013)
Wage 1st tertile	0.001	-0.009	0.002	-0.004
	(0.008)	(0.012)	(0.008)	(0.012)
Wage 2nd tertile	-0.009	-0.048**	-0.003	-0.032*
	(0.009)	(0.014)	(0.009)	(0.013)
Wage 3rd tertile	-0.034**	-0.057**	-0.031**	-0.052**
	(0.009)	(0.013)	(0.008)	(0.013)
Tenure > 3 years	-0.012	-0.040**	-0.009	-0.031*
	(0.008)	(0.012)	(0.007)	(0.011)
Tenure < 3 years	-0.017**	-0.040**	-0.012*	-0.024*
	(0.006)	(0.010)	(0.006)	(0.010)
Wage growth > 5% p.a.	-0.029**	-0.069**	-0.021*	-0.047**
	(0.009)	(0.013)	(0.008)	(0.013)
Wage growth < 5% p.a.	-0.002	-0.013	0.000	-0.011
	(0.007)	(0.010)	(0.006)	(0.010)
Long tenure and high wage growth	-0.052**	-0.107**	-0.041**	-0.077**
	(0.015)	(0.023)	(0.015)	(0.022)
Short tenure or low wage growth	-0.006	-0.023**	-0.002	-0.016
	(0.006)	(0.009)	(0.005)	(0.008)
Observations	221,139	221,139	221,139	221,139

Notes: Displaced group includes women aged 18-35 with at least one year of tenure in closing firms at the closure date. Control group is a 5% random subsample of women aged 18-35 with at least one year of tenure in firms that do not close within the next 2 years of the reference date. The outcome variable births next 3 years measures the number of children born between 6 and 36 months after the reference date. Wage growth is wage growth over the last 3 years. Covariates include: number of children born in the last 3, 6, and 9 years, age and its square, tenure, experience, indicator for apprenticeship education, blue collar status, earnings at the reference date, year and quarter dummies. Models with wage growth are estimated on 201,962 observations with positive wages 3 years before the reference date. Robust standard errors clustered at the individual level reported. Symbols: ** significant at 1%;* significant at 5%.

Table 5: Pure displacement versus unemployment effect on fertility

	Model 1		Model 2		Model 3	
	Births next 3 years	Births next 6 years	Births next 3 years	Births next 6 years	Births next 3 years	Births next 6 years
All women						
Firm closure	-0.015	-0.023	-0.016	-0.024	-0.014	-0.025
	(0.010)	(0.015)	(0.012)	(0.017)	(0.012)	(0.019)
Unemployment	0.046	0.005	0.023	0.005	0.019	0.011
	(0.086)	(0.128)	(0.038)	(0.056)	(0.055)	(0.082)
F-stat (19 d.o.f.)	161.81		94.46		38.29	
Observations	221,139	221,139	221,139	221,139	221,139	221,139
Women in white collar jobs						
Firm closure	-0.018	-0.036*	-0.023	-0.042*	-0.017	-0.038*
	(0.012)	(0.018)	(0.012)	(0.019)	(0.013)	(0.020)
Unemployment	0.011	0.051	0.022	0.046	0.002	0.040
	(0.112)	(0.169)	(0.046)	(0.069)	(0.063)	(0.096)
F-stat (19 d.o.f.)	99.74		66.4		29.1	
Observations	164,215	164,215	164,215	164,215	164,215	164,215

Notes: Estimations from 2SLS regressions, where the unemployment variable is instrumented by firm closure interacted with 8 year, 3 season, 3 industry, and 5 region dummies. F-statistics refer to the joint significance of the excluded instruments in the first stage regression. Unemployment is measured by the percentage of time unemployed in the first year after the reference date (Model 1), an indicator for being unemployed for at least one day in the first year (Model 2), and an indicator for being employed less than 75% of the time in the first year (Model 3). Displaced group includes women aged 18-35 with at least one year of tenure in closing firms at the closure date. Control group is a 5% random subsample of women aged 18-35 with at least one year of tenure in firms that do not close within the next 2 years of the reference date. The outcome variable births next 3 years measures the number of children born between 6 and 36 months after the reference date. Covariates include: number of children born in the last 3, 6, and 9 years, age and its square, tenure, experience, indicator for apprenticeship education, blue collar status, earnings at the reference date, year and quarter dummies. Robust standard errors clustered at the individual level reported. Symbols: ** significant at 1%;* significant at 5%.