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

Emilia Del Bono†, Andrea Weber‡, Rudolf Winter-Ebmer§

October 18, 2010

Abstract

In this paper we investigate how career considerations may affect fertility decisions in the presence of a temporary employment shock. We compare the birth rates of women displaced by a plant closure 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%, and that these effects are largely explained by the response of women in more skilled occupations. We offer an explanation of our results based on career interruptions of women.

forthcoming: Journal of the European Economic Association

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

*We are grateful to Jerome Adda, Martin Browning, David Card, Ken Chay, Raj Chetty, Uwe Dulleck, John Ermisch, Stephen Jenkins, Regina Riphahn, and seminar participants at UC Berkeley, Bocconi Milan, EALE 2007, ESPE 2008, Essex, ESSLE 2007, ISER (Essex), IZA (Bonn), RWI Essen, Munich, Nuremberg, Oxford, Paris, and SOLE 2008 for helpful comments. This project received funding from the Austrian Science Funds (NRN Labor Economics and the Welfare State)and the Austrian Marshallfonds Jubiläumsstifung. Andrea Weber also acknowledges financial support which was generously provided by the Center for Labor Economics at UC Berkeley. All errors and opinions are the authors' sole responsibility. We dedicate this paper to Alexander, who was born during the revision phase of this paper.

†ISER, University of Essex, and IZA, Bonn. E-mail: edelbono@essex.ac.uk
‡University of Mannheim, IZA, Bonn and CESifo. E-mail: webera@econ.berkeley.edu
§IHS Vienna, University of Linz, IZA, Bonn and CEPR. E-mail: rudolf.winterebmer@jku.at

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 may have to sacrifice either career or family, and they may not 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-shocks and fertility has received little attention in the formal analysis of fertility decisions. Life-cycle models acknowledge the interaction of wage growth 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.[1] In these models human capital accumulation typically occurs at a constant rate, which implies that only permanent changes in the labor market environment have a significant impact on fertility. Career advancement, on the other hand, could be very sensitive to temporary economic shocks, so that these could also have an impact on fertility.

In order to analyze the effect of career considerations on fertility, we investigate the effect of a temporary employment shock - here captured by job displacement - on the probability of having a child. Although job displacement may have significant long term

[1] See Hotz et al. (1997) for an excellent review.

effects on employment and earnings, its immediate impact is to interrupt a worker's career, forcing her to start a new job with a different employer. The comparison between displaced and non-displaced workers thus represents a straightforward way to investigate the trade-off between career concerns and family planning.

The empirical analysis is set in Austria where a government-sponsored parental leave and benefit system applies equally to all working women. The Austrian Social Security Database provides excellent micro-data with a longitudinal matched employer-employee structure, which allow us to identify firm closures and to observe long term labor market and fertility outcomes at the individual level. Moreover, the data covers all women in the private sector. We can therefore compare displacement effects for different groups of women, and in particular we can contrast women in more and less career-oriented occupations.

In the empirical analysis we compare births to women affected by a firm closure with 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. Although layoffs due to firm closure are not the result of the firm's discretion about whom to dismiss (Gibbons and Katz, 1991), threats to the causal interpretation of 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.

In order to guide the interpretation of our findings, we distinguish four different channels through which job loss affects the demand for children. The first two are the income effect, due to lost earnings, 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. The third effect is due to the loss of future income that is incurred if a woman has a child at a crucial stage of her career. This is

a relevant aspect to consider if we think that a woman with a young child might not be 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.

Our empirical analysis leads to three main findings. First, our results reveal that job displacement reduces the number of children born by about 5 to 10% in the short and medium term (after 3, 6 and 9 years), respectively. The magnitude of the effect is sustained even after 9 years, which indicates that the reduction in fertility is permanent. This suggests that the negative effects of displacement clearly outweigh any opportunity cost effects. Second, we demonstrate that the negative effect of displacement on fertility is largely due to the behavior of women in white collar occupations, with higher earnings, and steeper pre-displacement wage growth profiles. These are the jobs which generate more family income but also those where career considerations matter most. To establish the importance of the income vs. the career channel, we analyze fertility responses of male workers. Men are typically less engaged in the care of young children, and less likely to suffer in terms of career as a result of having children. Our third finding is that fertility is lower for all displaced male workers, that is men do not show a pattern of heterogeneity in their fertility responses. That leads us to conclude that while the income effect is clearly relevant to explain male fertility responses to displacement, the career channel might be an additional important factor in explaining the reduction of the number of births of females losing their jobs from firm closure. While this establishes no direct evidence on career or job finding effects with the data at hand, the existing evidence is certainly compatible with such a theory and clearly at odds with a traditional substitution effect.

Our work contributes to the theoretical and empirical debate in several areas of economics. The most immediate link is to the large literature on female labor supply and fertility, and the newer strand which explores the effect of fertility timing on human capital accumulation, wages and employment (Bailey, 2006; Goldin and Katz, 2002). While

Bertrand et al. (2010) highlight the effect of maternity related career interruptions on the divergence of career paths by gender, our analysis studies the effect of exogenous shocks to a woman's career on her fertility choice. Our results are also relevant in the area of population economics, where several studies have tried to explain changes in fertility rates emphasizing the role of unemployment and labor market institutions (Adsera, 2005; Ahn and Mira, 2002). Our study can also be seen as a new contribution on the effects of job displacement. There is a large literature which analyzes the effects of involuntary job separations on a range of economically relevant outcomes (Browning et al., 2006; Charles and Stephens, 2004; Jacobson et al., 1993); we supplement this with an analysis of women's job displacement on fertility.[2] Finally, we also contribute to the literature investigating long-term effects of early labor market experience (Kahn, 2010; Oreopoulos et al., 2006).

2 Institutional Background and Data

The *Austrian family policy* provides fairly generous government transfers for parents of young children. To protect the health of the mother and the child women are not allowed to work over 16 weeks around birth. During the maternity protection period women receive social security compensation equivalent to their previous monthly wage. Unemployed women receive a similar amount of compensation via an increase of the unemployment benefits. Parental leave sets in after the maternity protection period ends. Under the system in operation in the 1990s either parent can choose to go on parental leave until the child's second birthday and receives a flat rate benefit of 408 Euros per month (in 1997).

Eligibility for the parental leave benefits is tied to eligibility for unemployment insurance benefits. To establish a first-time claim the mother must have worked for 52

[2]Previous studies by Lindo (2010) and Amialchuk (2008) analyze the impact of husbands' job losses on fertility. Recent work by Huttunen and Kellokumpu (2010) investigates the impact of job displacement on both male and female fertility decisions in Finland using an empirical strategy very similar to the one we adopt here.

weeks out of the last 2 years. For mothers younger than 25 the requirement is reduced to 26 weeks during the last year. Unemployment extends the 2 year time frame by up to 3 years. This means that a woman who works for 12 months and then becomes unemployed has established eligibility for parental leave benefits for up to 4 years if she continuously stays on the unemployment register. After the first parental leave period the requirements to re-establish eligibility are lowered. Employed women are protected from dismissal when they first announce their pregnancy to the employer until the end of parental leave, plus one month if they return to the old job.

Our analysis is based on the *Austrian Social Security Database* (ASSD) which covers all private sector workers between 1972 and 2002. The data include daily information on employment and registered unemployment status, total annual earnings paid by each employer, and various characteristics of the workers and their jobs (Zweimüller et al., 2009). To derive a measure of fertility for every woman in the labor force, we merge the ASSD with child benefit records from the Ministry of Finance, which contain all births from 1975 to 2005.[3]

In the ASSD firms can be identified through an employer identifier that is reported with every employment spell. Our sample is based on firms that have at least one employed worker on the payroll, on any of four sampling dates (February 10, May 10, August 10, and November 10) over the years 1990 to 1998. Firm exit dates are defined as the last quarter date in which a firm employs at least one worker. For a detailed description of the ASSD Firm Panel see Fink et al. (2010b). This paper provides a comparison of the number of firms recorded in the ASSD with official statistics and a discussion of a worker-flow approach to identify true firm closures form spurious exits of employer identifiers form the database.

We apply three selection criteria to arrive at the sample of firms considered in our analysis. First, we focus on closures and thus exclude firm exits where a large flow of workers, i.e. more than 50% of the workforce in the last year, jointly transits to the

[3]See the Web Appendix for more details.

same new employer. Second, we exclude firms operating in agriculture, construction, and tourism industries. These sectors are characterized by a high share of seasonal employment which makes it difficult to identify firm entries and exits. Third, we only consider firms with 5 or more employees on one quarter date during 1972-2002, and restrict the sample to firms with more than 3 workers but less than 200 in the closing quarter. Based on the worker-flow approach we cannot identify firm closures for very small firms and closure is extremely rare among larger firms. For the sample of control firms we apply the second and third restrictions to non-closing firms. Instead of the closing quarter, any quarter in 1990-1998 that fulfills the third restriction criterium and is more than 2 years away from the firm exit date is regarded as a reference quarter.

Based on this sample of firms we consider all women employed in any firm between quarter 1/1990 and quarter 4/1998. We restrict the attention to women between 18 and 35 years with at least one year of tenure in the current firm. The tenure requirement ensures that all women are eligible for parental leave benefits. We define as *displaced* all women working in a closing firm the quarter before closure and as *control* all women who are not affected by a firm closure, i.e. working in a control firm in any reference quarter. This leads to a large control sample as for each group of displaced women, say at the reference date 4/1992, all women working in a non-closing firm in the same quarter 4/1992 are potential controls. We therefore take a 5% random sub-sample of the observations in the control group. The final sample therefore consists of 8,651 observations of women in the *displaced* group and 218,548 observations of women in the *control* group.[4]

Firm characteristics

Table 1 compares the characteristics of closing and non-closing firms in the sample.

[4]We start with 19,348 women employed in closing firms and lose 2,911 because of the age restriction, 5,608 because of the tenure restriction and another 2,178 coming from very small ($>$ 3 workers in the last quarter) or large firms ($<$ 200 workers in the last quarter). The last selection is largely the result of excluding very small firms, while only a few observations are lost when cutting out very large firms. We think it is important to exclude very large firms because they create a strong imbalance between the treated and control groups, as we observe very few large closing firms but many large non-closing firms. Without imposing a restriction on firm size the propensity score weighting procedure is therefore very unstable. Allowing for large firms in our sample shows, however, that the (unweighted) results do not differ significantly from those we present here.

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 grew 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.

Individual characteristics

Table 2 compares the characteristics of women in the displaced group and in the control group: see columns (1) and (2). 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.

Our outcome of interest is the number of births per woman after the reference date. Since job displacement might affect the total number of children as well as the timing of fertility, we consider a short, medium and long-term measure of fertility and look at the birth rate 3, 6 and 9 years following the reference date.[5] Birth rates for the displaced group are 5.6% lower after 3 years, 8.0% lower after 6 years and 9.9% lower after 9 years - compared to the control group. This suggests a persistent negative effect of displacement on fertility.

[5]Information on births over 6 years after the reference date is available for all observations, while births over the next 9 years are only available for the sub-sample of women displaced in 1990-1995.

3 Empirical Analysis

3.1 Firm Closures as a Quasi-Experiment

We analyze the effect of job displacement on fertility by comparing fertility outcomes after the quarter of closure or the reference date for women in the displaced and control groups. Our empirical evidence consists of a graphical and a regression-based analysis. The main tool for the graphical analysis is an event study, where we pool all observations at the reference date and plot the means of the outcome variables each quarter before and after the reference date separately for the displaced and comparison groups. We focus on days employed per quarter, earnings, and the number of births. 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 outcome variables after the reference date gives us an idea of the magnitude and the dynamics of the displacement effect. Finally, we use regression analysis for the entire sample as well as for specific subgroups of women in order to get an idea of the channels through which fertility is affected by job displacement.

Firm closures do not happen perfectly randomly, hence we are concerned with two sources of selection that might bias the comparison of displaced and control women. First, closing firms differ from surviving firms and women might select into more or less "risky" firms in terms of their likelihood to close down. Second, because of the downsizing and restructuring in the period prior to firm closure, a non-randomly selected pool of workers may be left at the closing date. To address the first issue we start out with a detailed comparison of characteristics and labor market outcomes of the displaced and control women in Table 2 and in the graphical analysis. To correct for all remaining differences between these two groups we apply a propensity score weighting procedure, which balances the distribution of observable characteristics in both samples (Imbens, 2004).

To deal with selection over the firm closure process, the literature typically suggests to include worker separations from a longer period prior to the firm closure date (Dustmann and Meghir, 2005; Eliason and Storrie, 2006). This type of solution turns out to be infeasible in our application, however. All women who give birth are required by law to leave their jobs for at least four months, which means that we must avoid definitions of displacement that are likely to include voluntary quits. In addition, labor market careers of young women in Austria are characterized by frequent interruptions, making it difficult to distinguish between voluntary and involuntary separations the longer we extend the window before the closing date. Therefore we decided to apply a narrow definition of displacement, which considers as displaced only those who are still employed in the closing firm the quarter before closure.[6]

3.2 Effect of Job Loss on Employment and Earnings

Figure 1 Panel (a) shows the raw comparison of employment between displaced and control groups from 20 quarters before to 12 quarters after the reference date. Starting with the control group, we notice that - due to the one year tenure requirement - employment is at its maximum only during the four quarters before the reference date. Before and after that period, the average number of days employed per quarter is lower and the pattern is almost completely symmetric. The corresponding figure of unemployment - not presented here - confirms that labor force participation in the group of young women is low, as the numbers of days employed and unemployed do not add up to a full quarter.

The displaced group exhibits slightly lower employment than the control group in the pre-displacement period. This indicates that firm closure does not hit workers completely at random, although the difference is small. More importantly, we notice that displaced women face a substantial employment loss of more than three weeks in the first quarters after firm closure. The initial gap narrows over time and three years after the reference

[6]In the Web Appendix we provide evidence that the labor market and fertility histories of women employed in the firm one year before closure do not differ significantly from those of women in the displaced group.

date the employment difference between the control and displaced group, while still noticeable, amounts to less than 5 days per quarter. Monthly earnings, shown in Figure 2, Panel (a), mimic the pattern of employment. If we restrict the sample to employed women with positive earnings - not shown here - we find only very small differences in quarterly earnings after three years.

It is fair to conclude that the effects of job displacement on the employment and earnings of young women are concentrated in the first two to three years. This is in contrast to what is typically documented for men, where several studies find that displacement causes severe and persistent wage and employment losses (Ichino et al., 2007; Jacobson et al., 1993; Stevens, 1997). An explanation of this difference could be that we are focusing here on women aged 18-35, a relatively young group of workers who are also less attached to the labor market than prime age males.

In Panels (b) to (d) of Figures 1 and 2 we show results from a propensity score weighting approach to balance observable characteristics across displaced and control women. We use three different specifications of the propensity score. The first specification models firm closure using firm level information only. The idea is that firm closure is due to firm-level events and not based on actions at the individual level. The second specification is based purely on individual characteristics. The final specification comprises both worker and firm characteristics.[7]

Panels (c) and (d) in each figure use propensity scores based on individual characteristics, which include yearly employment and unemployment rates, and wages before the

[7]The estimated propensity score represents the probability that a firm with given characteristics will close down one year ahead. We use a one year time difference between the date of closure and the measurement of the firm and individual level variables to take into account the downsizing and restructuring process which affects the closing firms. Firm level variables: 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. Individual level variables: 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.

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-right panels (b) 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. The important thing to note is, however, that the patterns of post-displacement outcomes are essentially unchanged by all three 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.

3.3 The Main Effect of Job Loss on Fertility

Figure 3, Panel (a) plots the average yearly numbers of births in the 14 years before the reference date and 6 years afterwards. 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 tenure at the reference date. This means that all women must have been working during the year before closure and therefore, by construction, they can have no children between year -1 and year 0. 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.[8] 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.

To see how differences in the sample composition affect the birth outcomes we again compare weighted graphs in Figure 3, Panels (b) to (d). The different weights neither affect the comparability of the two groups before firm closure nor do they substantially change the difference in the fertility response.

In the regression analysis we measure fertility outcomes using the number of births in the first 3, 6 and 9 years after the reference date to compare short versus medium or long term birth outcomes (Table 3). As with our graphical analysis, we present unweighted results along with results weighted by our three propensity score estimates. In the middle panel of Table 3 we show results including standard human capital covariates as additional controls. 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.

The results are robust and consistent across specifications. All estimated coefficients are negative, fairly similar across weighting schemes, but not always statistically significant, in particular if weighting uses firm characteristics. Births in the 3 years after firm closure drop by 0.7 to 1.3 percentage points. If we compare these numbers to the average birth rate in the control group, which is 23 percent (see Table 2), the effect translates into a 3.1 to 5.7% fall over 3 years. In the medium run, the magnitude of the effect is in the same ballpark, about 1.6 to 3.6 percentage points, which implies a reduction in the mean birth rate over 6 years of 3.5 to 7.8%. In the long run, fertility declines by 2.0 to 5.9 percentage points which is a reduction by 3.2 to 9.5 percent.[9]

[8] Quarterly graphs confirm this pattern, although they are more noisy.

[9] To give some idea of the magnitude, we can compare these findings with those of Lalive and Zweimüller (2009). 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 5 more births per 100 women (or a 15% increase in birth rates of second children) after three years and 3.5 more births per 100 women (or a 5% increase in birth rates of second children) after 10 years. The effect we estimate is therefore comparable in size to that of a 12-month increase in parental leave,

The results are not compatible with a pattern of fertility postponement. In fact, for all eight specifications in Table 3 the numerical effect is increasing with time - from three to six and nine years. Although we cannot observe completed fertility for all women in our sample, the effect is so large and persistent that it seems unlikely it could be entirely reversed later on.

The lower panel of Table 3 presents the effects of plant closure on fertility at the extensive margin, that is on the probability of having a child in the next three to nine years. The effects are very similar at the extensive and at the intensive margin. Not surprisingly, there is no difference within the first three years after the reference date. Only over the longer horizons extensive margin effects are lower than the effects on the number of children. But even at the extensive margin, the effects on fertility after 6 or 9 years are still highly significant.

3.4 Heterogeneous fertility effects

After having established a negative average effect of job displacement on fertility, we compare fertility outcomes according to women's demographic and economic characteristics in Table 4. Each pair of coefficients shown in each column is derived from a separate regression where the displacement dummy is interacted with subgroup indicators. As we can see, women without previous children experience a drop in fertility, whereas women with previous children remain unaffected. It might be that women with previous children have already realized their fertility plans and this implies that the effect documented here can be interpreted as a response of first births to an employment shock. If we distinguish by age, we see that among women with no previous children, those aged 25 or above are most affected both in the short and in the medium term. Since older women are most affected, it is likely that the effects translate into a permanent reduction in fertility.

Considering occupational characteristics, we see that the effect of job displacement on

although in our case the magnitude is increasing rather than decreasing over time.

fertility is concentrated on women in white collar positions, while there is no impact on blue collar workers. The latter are mainly employed in manual occupations in low ranking positions, with modest salaries, faced with high job-turnover, and few prospects of career advancement. On the other hand, women in white collar jobs, for whom firm-specific human capital or ability are likely to be more relevant determinants of productivity, show a large fertility response to a job loss. Cutting the sample by earnings yields a very similar picture: women in the third tertile of the earnings distribution are affected most by job displacement, while for women in the first tertile the effect is basically zero.[10]

To investigate fertility responses of women in more or less career-oriented jobs we split the sample by tenure and pre-displacement wage growth profiles. We define long tenure as 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. While we do not see significant differences according to tenure alone, previous wage growth is a strong predictor of women's fertility reaction to a job loss. The effect is particularly strong for those women who had both high wage growth and stayed with the same employer over the last 3 years. These women reduce their fertility rate by a stunning 20 to 25% over the next 6 years, more than twice the size of the overall effect.

In Figure 4 we graphically present fertility patterns for white collar workers in the top panel and for blue collar workers in the lower panel. These graphs nicely show the strong similarity of treatment and control groups before firm closure for both groups of women, and clearly indicate that fertility effects are mainly felt by those in white collar jobs. Note that the graphs for blue collar women are more noisy because of the smaller number of women in this group.

Our results show convincingly that unintended employment interruptions in the form of job loss due to plant closure decrease fertility of women in Austria both in the short and the long run. We find negative fertility reactions mainly for women in white collar

[10]Huttunen and Kellokumpu (2010) also show negative fertility effects for higher educated women in their study of displaced workers in Finland.

jobs, with earnings in the upper part of the distribution, and with high wage growth in the past years. In the next section we seek to interpret the results and to reconcile them with theories of fertility decisions.

4 Theory and Interpretation

4.1 Theoretical framework

To motivate the interpretation of our results we introduce a theoretical framework based on a dynamic model of fertility timing that incorporates human capital accumulation and job search decisions. Here we outline the intuition behind the main model assumptions, mechanisms, and predictions. A fully specified model is presented in the Web Appendix to this paper.

Our setup provides two channels by which displacement affects the labor market situation of an individual. First, a displaced worker faces the need to search for a new job and may thus experience a spell of unemployment. Second, moving jobs typically determines the loss of some specific human capital, and we assume that this loss is greater for voluntary rather than involuntary separations. We further assume that human capital is accumulated at a high rate at the beginning of a career, but the process slows down over time, i.e. the wage profile is concave. This implies that the timing of the job loss matters. In other words, if the involuntary job loss occurs at the beginning of the career the worker faces greater losses than if the job loss occurs when her career is already established.[11]

With respect to fertility, we assume that the birth of a child has two effects on the labor market situation of the mother. First, it lowers her job finding rate, as women who are pregnant or have a small child might face more difficulties in finding a new job

[11] Our concept of human capital is very general, ranging from actual skills or knowledge to reputation or signals to the employer.

due to higher job search costs or if employers discriminate against them.[12] Second, in line with recent evidence on the effects of children on women's wages (Ellwood et al., 2004; Miller, 2009), we assume that a birth reduces the rate of wage growth.[13] These two assumptions 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.[14]

In our model we simplify this framework further by assuming that all human capital is lost upon displacement. This implies that the earnings profile rises over the career following a concave pattern but drops to the initial level after an involuntary separation. Within this model we can derive four effects of displacement on fertility. Analogous to a static model, job loss has a *substitution* effect - increasing fertility - as the opportunity cost of the woman's time is lower during a period of unemployment and an *income* effect - decreasing fertility - as the reduction of income during unemployment lowers the incentive to have a child. The dynamic process of career progression and human capital accumulation gives rise to two additional effects, which are illustrated as follows.

In Figure 5 we sketch the fertility response to job displacement, showing earnings profiles for various scenarios. In the upper panel we see the concave wage profile w for a non-displaced woman. 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 earns the same amount she was earning before, i.e. there is no depreciation. Her future wage profile, however, is flatter from this point onwards. As we can see, in this scenario the cost of a child consists of two components: the earnings loss during maternity leave and the earnings loss due to slower wage growth in the future. Shifting the birth of the child to later periods increases the former but

[12] This might be because employers assume that pregnant women are more likely to be absent from work or have a lower job commitment than other women as documented in Cunningham and Macan (2007); Manchester et al. (2009).

[13] This can be either because a woman with a child is slower in accumulating human capital, or because the employer interprets motherhood as a negative signal about the woman's ability or level of effort on the job and places her on a career with reduced opportunities for promotion.

[14] We implicitly assume that the direct utility of having a child stays constant over the life cycle.

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 benefits b. When she enters a new job she starts 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, let us consider the situation of a woman who instead of re-entering employment in period t decides to have a child. Her earnings drop to the maternity benefit level m during maternity leave. Since she finds it more difficult to get a new job, she may experience a further period of unemployment between $t+1$ and $t+2$. The combination of the delay in job entry and the lower wage growth following re-employment results in a large long-term earnings loss. This comparison makes clear that a displaced woman faces an incentive to (1) find a new job before having a child - which is what we call the *employability* effect, and (2) postpone childbearing- which is what we call the *career* effect.

In our model job displacement affects fertility through its impact on economic factors, such as the opportunity cost of the woman's time, her income, her career progression, and job search. It is of course possible that displacement may affect fertility through its impact on other variables, such as health and partnership formation/dissolution, for example.[15] However, we have shown that the effect of displacement varies with the characteristics of the woman's job and her wage profile. In the following section we interpret these results in the light of the theory.

4.2 Interpretation of results

The theoretical framework identifies four specific channels through which displacement can affect fertility decisions. The *substitution* effect predicts that births are transferred

[15]See Browning et al. (2006); Charles and Stephens (2004); Eliason and Storrie (2006); Kuhn et al. (2009); Rege et al. (2009); Sullivan and von Wachter (2009).

to periods with lower opportunity costs, which would imply 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 unemployed mothers might face in finding a new job. The *career* effect results from long term income losses a mother might incur if she has a child at the initial stages of her career. Based on our empirical results we now discuss the relevance of each channel and establish thereby the most important mechanisms by which displacement affects fertility. [16]

Looking at the pattern of post-displacement fertility over time, it is clear that we do not find any evidence of a strong *substitution* effect. It is particularly striking, that in the first quarters after plant closure absolutely no increase in fertility is to be seen - which is a clear sign of a lack of a *substitution* effect. The negative effects of job displacement clearly 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.

After ruling out the substitution effect, we are left with the three remaining channels predicting a negative fertility response. The analysis by subgroups provides important clues that *career* effects may matter. Fertility drops mainly for women in occupations which lead to career advancement. This is particularly so among women who have just gone through a period of steep wage growth. Figure 5 suggests that women who face the highest incentives to adjust their fertility behavior should be those who have gone through the initial phase of steep wage growth. This is exactly what we see in Table 4 after splitting the sample by tenure and pre-displacement wage growth. Women who are hit by displacement right after having invested strongly in their career show by far the

[16]Note that we concentrate on plant closures, which are essential for the strict form of a career effect: the worker cannot come back to her initial job, because the firm does not exist any more. Other papers concentrate on broader definitions of layoff, like surveys (Chan and Stevens, 2001; Charles and Stephens, 2004) or focus on workers leaving firms experiencing some form of employment reduction (Couch and Placzek, 2010; Jacobson et al., 1993; Sullivan and von Wachter, 2009). The career effect might be less important in those other forms of layoff.

strongest reaction. By contrast, women in jobs which are low paid and characterized by flatter wage profiles do not seem to change their fertility behavior at all.[17]

Our empirical set up does not allow a clear separation between the *career* and *employability* effects. If the reduction in job finding rates around childbirth is the same for all women, one might say that the *employability* effect is small as we find no fertility drop for low skilled women. However, if women in career-oriented occupations face more difficulties in finding a suitable job in the presence of a young child, then our results are most correctly seen as a combination of *employability* and *career* effects, as shown in Figure 5.

While there is evidence in favor of the *career* and *employability* effects[18], we cannot rule out that some of the negative fertility response is due to an *income* effect. By definition, lost career opportunities go hand in hand with a loss in earnings potential. The analysis of employment and earnings paths after job displacement shows that earnings of young women are most affected in the short run. Earnings of displaced women are still systematically lower after three years, although the difference is relatively small.[19]

To shed light on the relevance of the income effect on fertility, we examine fertility responses of young males. Fathers typically participate less in the care of young children. Neither do they take time off their jobs to go on parental leave, nor does the birth of a child lower their job commitment. Therefore, neither the substitution effect, nor career or employability considerations should affect their households' fertility decisions after job-displacement. The major response among males is expected to come through the

[17]Evidence from the Austrian LFS (Mikrozensus) indicates that women with primary education contribute 35.1% to family income, while those with secondary education or more contribute 39.8%. This difference is not statistically significant, and therefore it is unlikely that the share of women's income is what lies behind the heterogeneity in fertility effects observed in Table 4.

[18]Career effects can also be indirect by providing better chances on the marriage market or allowing better fringe benefits, like work organization or more flexible schedules.

[19]Notice that we cannot provide direct evidence on the *career* effect by analyzing the heterogeneity in earnings losses due to displacement of high vs. low skilled women. This is because a large part of the female response to displacement is in terms of labor force participation, which is itself a function of the shape of the wage profile. Disentangling the effects on wages and employment of an involuntary separation is beyond the scope of this paper and requires data on a much longer time-horizon. For a related empirical analysis see Fink et al. (2010a).

income channels.

4.3 Displacement Effects for Men

We use a sample of males that is subject to the same restrictions as the female sample described in section 2. Although not shown here, we want to point out that selection into closing firms and also during the downsizing period before closure is more pronounced in the male sample.[20] Therefore balancing individual characteristics by propensity score weighting is more important in the assessment of displacement effects. In addition, employment and earnings losses due to displacement are larger and far more persistent for young males than for females.[21] This is promising for our analysis: if there is an income effect of displacement of fertility we should find it among males.

In Table 5 we replicate the results shown in Tables 3 and 4 using the male sample. Since *substitution* effects are likely to be small or non-existent for men, we are not surprised to find negative and significant fertility effects of displacement in this case.[22] Like before, the effects can be seen both in the short and the medium term. The point estimates are slightly smaller than those we find for women, but the magnitude of the effects is comparable. Notice, however, that here the effects are statistically significant only in the weighted regressions, to confirm the importance of selection into firms with a different propensity to close down.

In the bottom part of Table 5 we investigate heterogeneity in the displacement effect for more career-oriented and less career-oriented men.[23] Unlike what we found for women, we see hardly any difference in fertility effects due to a firm closure. This indicates that the main channel through which displacement affects male fertility is the *income* channel, and represents further evidence that the *employability* and/or *career* effects are

[20] For more details on the male sample see the Web Appendix.

[21] Male earnings effects are very much in line with the results found in the literature.

[22] Amialchuk (2008) and Huttunen and Kellokumpu (2010) also find negative effects of husband's displacement on fertility.

[23] For evidence on differences in earnings and employment effects for blue collar and white collar males see Schwerdt et al. (2010).

additional effects at work for women.

5 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, but there is a lack of formal discussion about how much career considerations 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. Our empirical results show that one of the main factors causing a reduction of fertility after job loss is the difficulty women face in re-establishing their careers. The effect is not only confined to women in top positions, but we find a considerable reduction in fertility for the average white collar worker over a period of nine years. Further analysis confirms that women in jobs with the highest opportunities to invest in firm-specific human capital or the highest returns to ability and effort are most strongly affected. We interpret these results in the light of a model where job displacement destroys a significant part of previously accumulated human capital, either because this investment is firm-specific or because a worker's ability or effort is not known to the employer and needs to be inferred through time. In this scenario not only the income loss from job displacement but also the increased cost of having a child lead to fertility postponement.

Our data from plant closures allow us to exclude substitution effects and provide some evidence for income effects. While our heterogeneous effects for career-oriented women are clearly compatible with our model of career and employment effects, we cannot exclude other possible effects. In particular, further analyses looking at family formation and household income might shed additional light on these issues.

Our results highlight that even temporary labor market shocks, which hit young women at a crucial stage of their careers may significantly contribute to delays or re-

ductions in fertility. This 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). These results also imply that constraints which impede the development of stable careers in the labor market, such as atypical jobs or temporary contracts, may have a detrimental effect on fertility (De la Rica, 2005). Finding that besides the income effect also career prospects matter a lot for young women, we suggest that family policies 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 policies may include the provision of more child-care facilities, extended school hours, as well as more flexible working-time arrangements.

References

Adsera, A., 2005. Vanishing children: From high unemployment to low fertility in developed countries. American Economic Review Papers and Proceedings 95, 189–193.

Ahn, N., Mira, P., 2002. A note on the changing relationship between fertility and female employment rates in developed countries. Journal of Population Economics 15, 667–682.

Amialchuk, A., 2008. The effect of husband's earning shocks on the timing of fertility, mimeo, University of Houston.

Bailey, M., 2006. More power to the pill: The impact of contraceptive freedom on women's lifecycle labor supply. Quarterly Journal of Economics 121, 289–320.

Bertrand, M., Goldin, C., Katz, L. F., 2010. Dynamics of the gender gap for young professionals in the financial and corporate sectors. American Economic Journal: Applied Economics, forthcoming.

Browning, M., Moller-Dano, A., Heinesen, E., 2006. Job displacement and stress-related health outcomes. Health Economics 15, 1061–1075.

Chan, S., Stevens, A., 2001. Job loss and employment patterns of older workers. Journal of Labor Economics 19, 484–521.

Charles, K. K., Stephens, M., 2004. Job displacement, disability, and divorce. Journal of Labor Economics 22, 489–522.

Couch, K., Placzek, D., 2010. Earnings losses of displaced workers revisited. American Economic Review 100(1), 572–589.

Cunningham, J., Macan, T., 2007. Effects of applicant pregnancy on hiring decisions and interview ratings. Sex Roles 57, 497–508.

De la Rica, S., 2005. Career planning in Spain: Do fixed-term contracts delay marriage and parenthood? Review of Economics of the Household 3, 49–73.

Dustmann, C., Meghir, C., 2005. Wages, experience and seniority. Review of Economic Studies 72, 77–108.

Eliason, M., Storrie, D., Oct 2006. Lasting or latent scars? Swedish evidence on the long-term effects of job displacement. Journal of Labor Economics 24, 831–857.

Ellwood, D., Wilde, T., Batchelder, L., 2004. The mommy track divides: The impact of childbearing on wages of women of differing skill levels. Russell Sage Foundation Working Paper.

Fink, M., Kalkbrenner, E., Weber, A., 2010a. The long term effects of job-loss in Austria on earnings and labor supply of men and women, *mimeo*, University of Mannheim.

Fink, M., Kalkbrenner, E., Weber, A., Zulehner, C., 2010b. Extracting firm information from administrative records: The ASSD Firm Panel, the Austrian Center for Labor Economics and the Analysis of the Welfare State, University of Linz, Working Paper 1004.

Gibbons, R., Katz, L., 1991. Layoffs and lemons. Journal of Labor Economics 9, 351–380.

Ginther, D. K., Kahn, S., 2004. Women in economics: Moving up or falling off the academic career ladder. Journal of Economic Perspectives 18, 193–214.

Goldin, C., 2006. The quiet revolution that transformed women's employment, education, and family. American Economic Review 96, 1–21.

Goldin, C., Katz, L. F., 2002. The power of the pill: Oral contraceptives and women's career and marriage decisions. Journal of Political Economy 110, 730–770.

Hotz, V. J., Klerman, J. A., Willis, R. J., 1997. The economics of fertility in developed countries. In: Rosenzweig, M. R., Stark, O. (Eds.), Handbook of Population and Familiy Economics. North Holland, Amsterdam, 275-347.

Huttunen, K., Kellokumpu, J., 2010. Job loss and fertility, mimeo, Helsinki Center for Economic Research.

Ichino, A., Schwerdt, G., Winter-Ebmer, R., Zweimüller, J., 2007. Too old to work, too young to retire? CESifo Working Paper 2118.

Imbens, G. W., 2004. Nonparametric estimation of average treatment effects under exogeneity: A review. Review of Economics and Statistics 15, 4–29.

Jacobson, L. S., LaLonde, R. J., Sullivan, D. G., 1993. Earning losses of displaced workers. American Economic Review 83, 685–709.

Kahn, L. B., 2010. The long-term labor market consequences of graduating from college in a bad economy. Labour Economics 17, 303–316.

Kuhn, A., Lalive, R., Zweimüller, J., 2009. The public health costs of job loss. Journal of Health Economics 28, 1099–1115.

Lalive, R., Zweimüller, J., 2009. Does parental leave affect fertility and return to work? Evidence from two natural experiments. Quarterly Journal of Economics 124 (3), 1363–1402.

Lindo, J. M., 2010. Are children really inferior goods? Evidence from displacement-driven income shocks. Journal of Human Resources 45(2), 301–327.

Manchester, C. F., Leslie, L. M., Park, T.-Y., 2009. Screening for commitment: The effect of maternity leave use on wages. Tech. rep., University of Minnesota.

Miller, A., May 2009. The effects of motherhood timing on career path. American Economic Review Papers and Proceedings 99, 154–158.

Oreopoulos, P., von Wachter, T., Heisz, A., 2006. The short- and long-term career effects of graduating in a recession: Hysteresis and heterogeneity in the market for college graduates. Working Paper 12159, NBER.

Rege, M., Telle, K., Votruba, M., 2009. Job Loss and Marital Dissolution, mimeo, University of Stavanger.

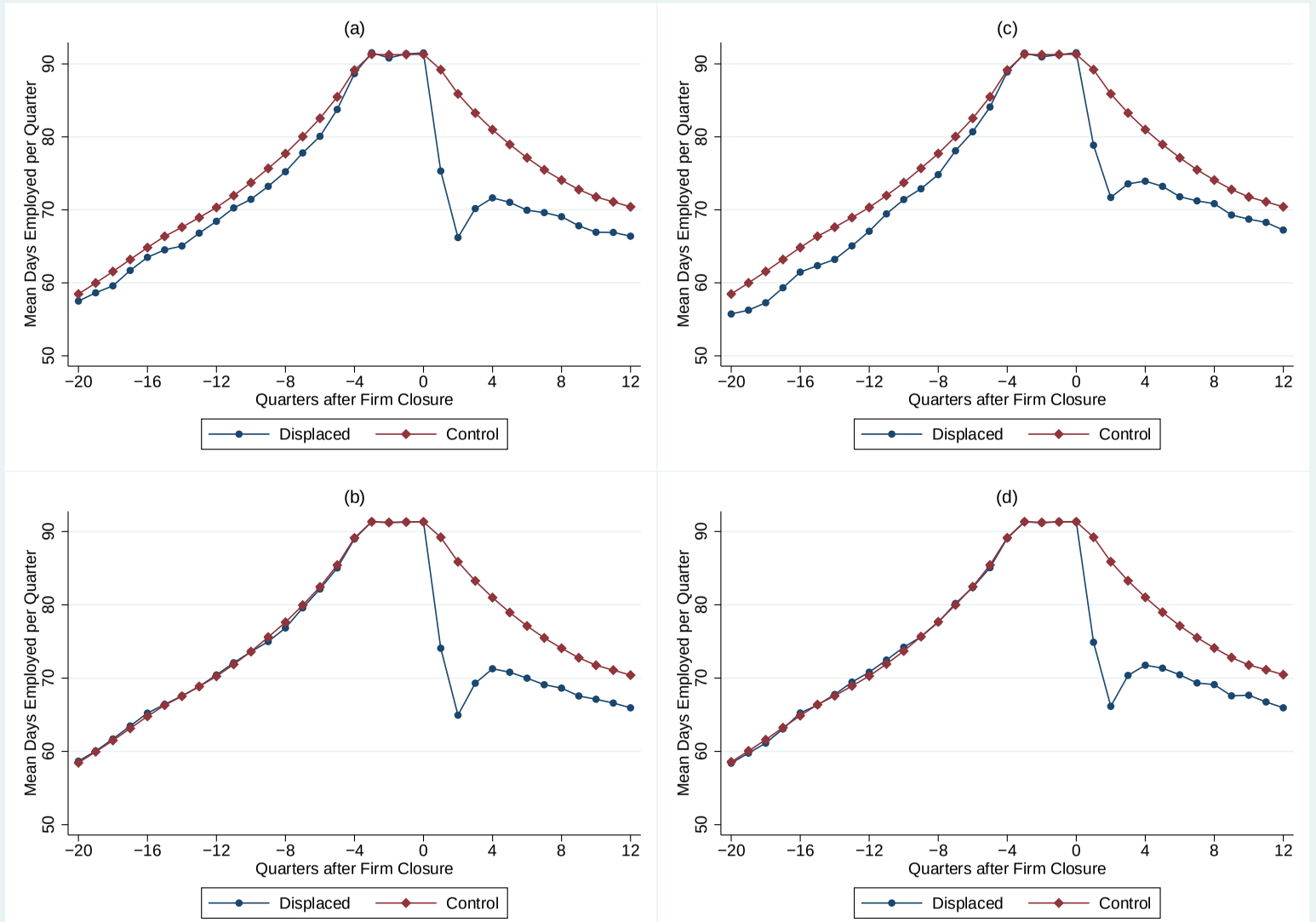
Schwerdt, G., Ichino, A., Ruf, O., Winter-Ebmer, R., Zweimüller, J., 2010. Does the color of the collar matter? Firm specific human capital and post-displacement outcomes. Economics Letters 108, 137–140.

Stevens, A. H., 1997. Persistent effects of job displacement: The importance of multiple job losses. Journal of Labor Economics 15, 165–188.

Sullivan, D., von Wachter, T., 2009. Mortality, mass-layoffs, and career outcomes: An analysis using administrative data. Quarterly Journal of Economics 124(3), 1265–1306.

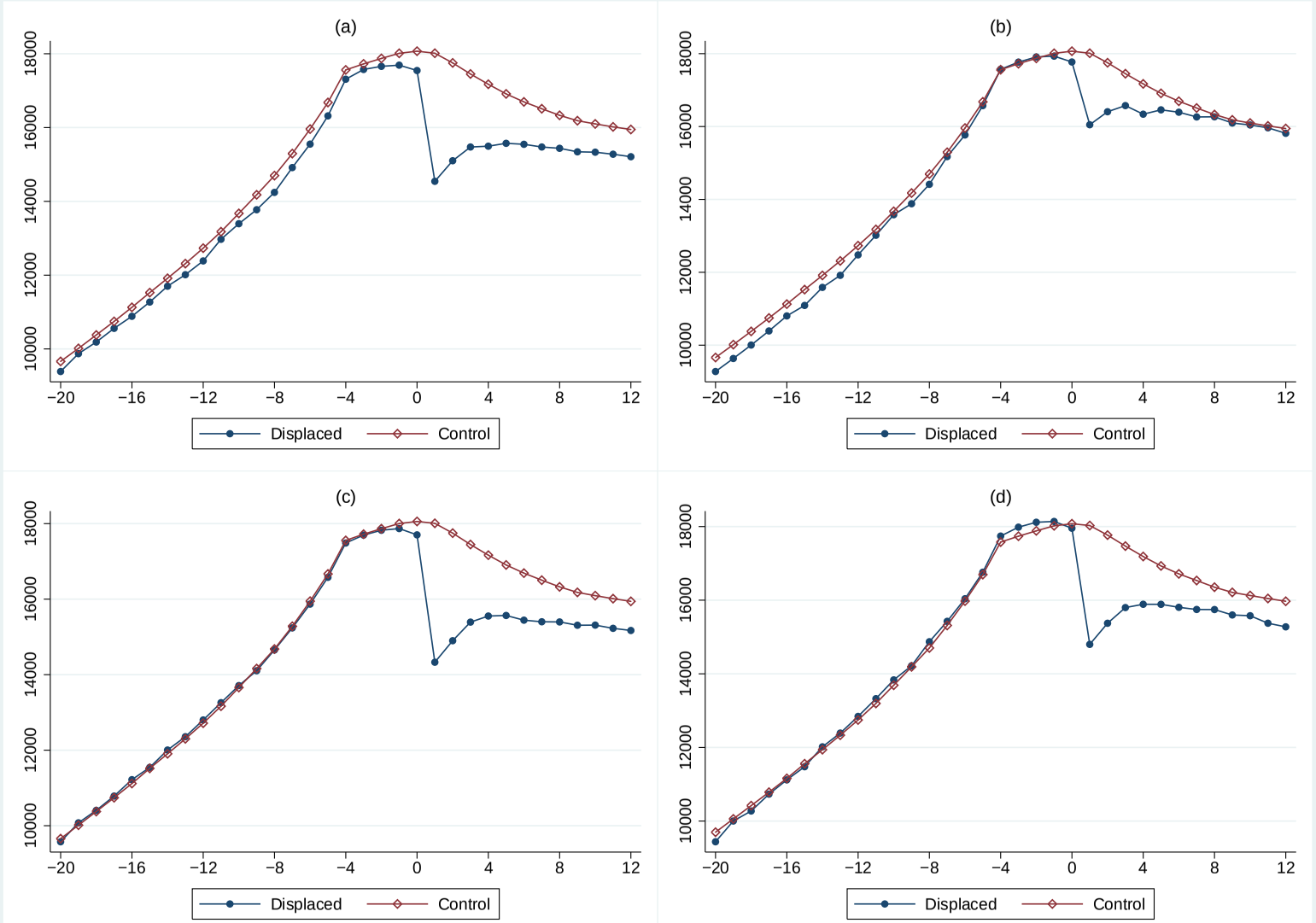
Zweimüller, J., Winter-Ebmer, R., Lalive, R., Kuhn, A., Wuellrich, J.-P., Ruf, O., Büchi, S., 2009. Austrian Social Security Database. Tech. Rep. Working Paper 0903, NRN: The Austrian Center for Labor Economics and the Analysis of the Welfare State.

Figure 1: Average days in employment by quarter weighted by propensity scores



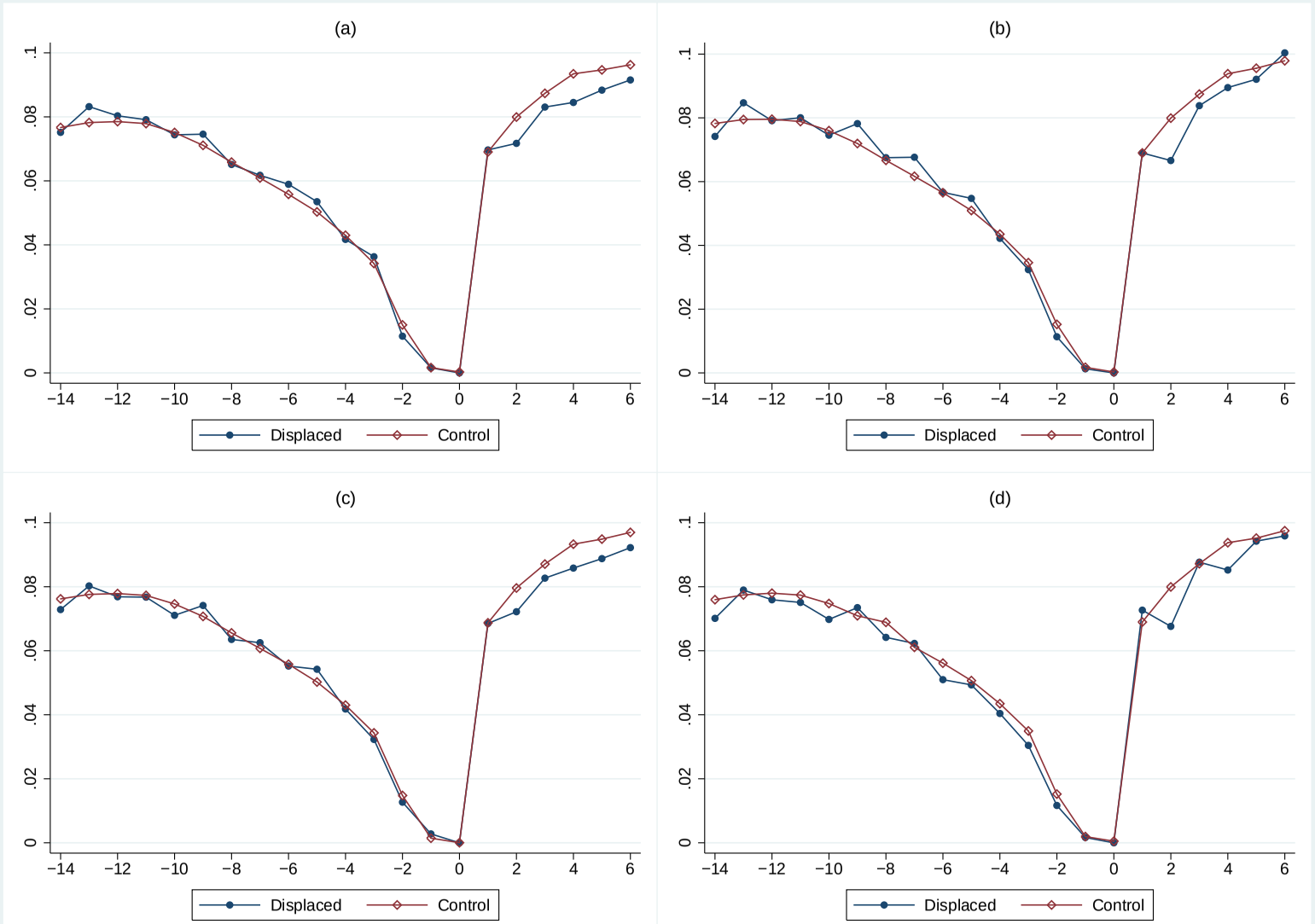
Notes: Comparison between the average number of days in employment of displaced (blue) and control (red) women by quarter. Average raw number of days in employment in panel (a), weighted by firm characteristics in panel (b), individual characteristics in panel (c), and firm and individual characteristics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

Figure 2: Average earnings by quarter weighted by propensity scores



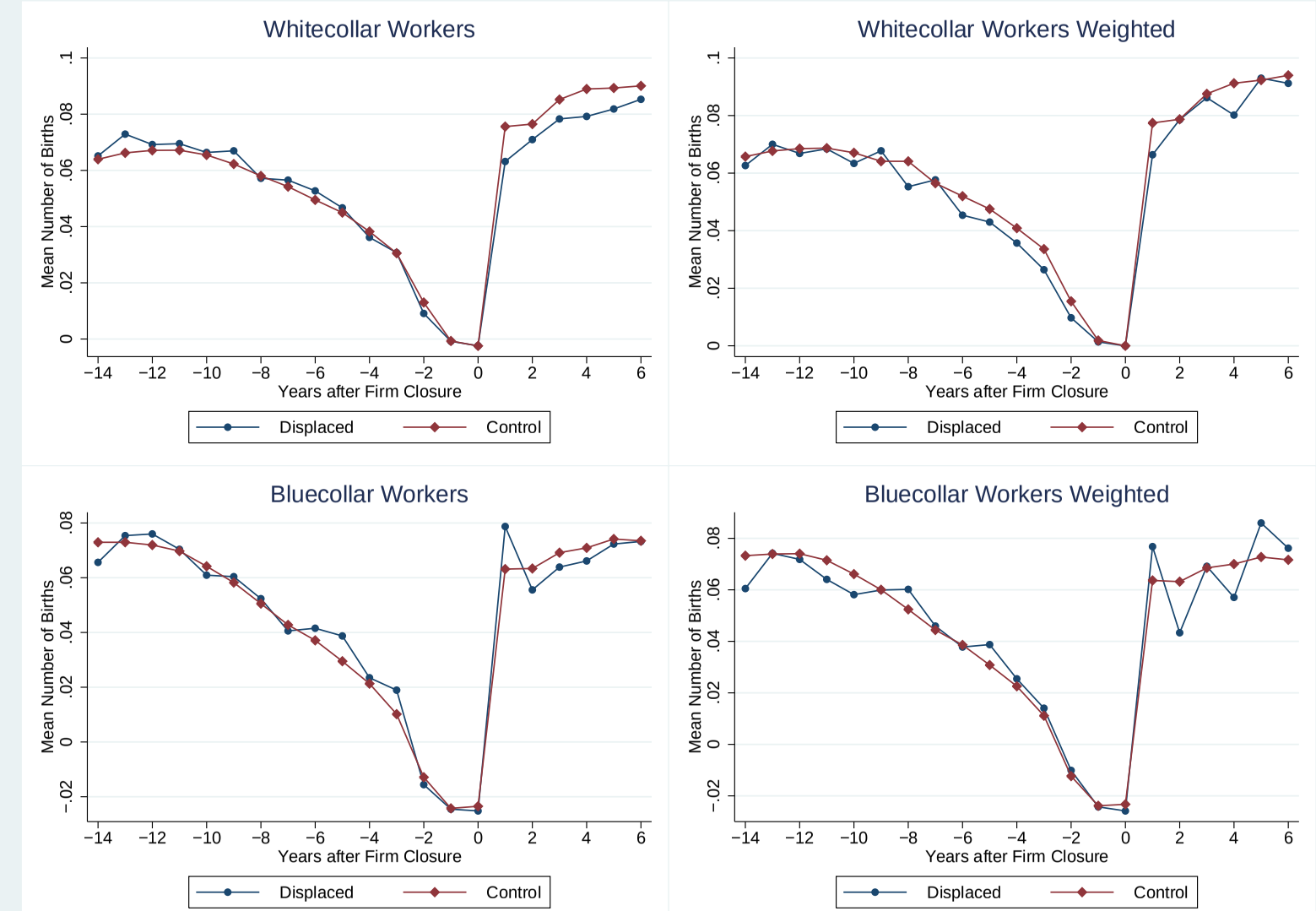
Notes: Comparison between the average earnings including non-earners of displaced (blue) and control (red) women by quarter. Average raw earnings in panel (a), weighted by firm characteristics in panel (b), individual characteristics in panel (c), and firm and individual characteristics in panel (d).

Figure 3: Average number of births by year weighted by propensity scores



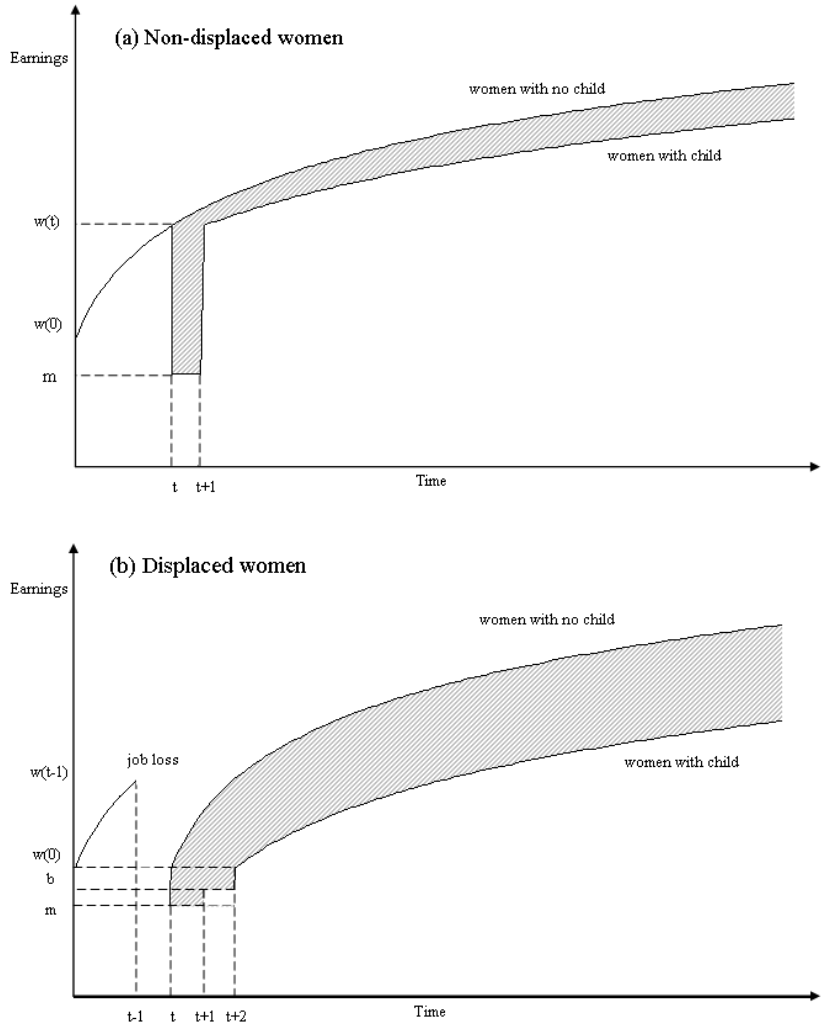
Notes: Comparison between the average number of births to displaced (blue) and control (red) women by year since reference date. Average raw number of births in panel (a), weighted by firm characteristics in panel (b), individual characteristics in panel (c), and firm and individual characteristics in panel (d). All women in the sample have at least one year of tenure at the reference date. This implies that they have no children between year -1 and year 0 by construction.

Figure 4: Average number of births by year by subgroups



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 5: 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.

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
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	Control	Displaced-Control % difference
Age	27.23	27.10	0.46%
	(4.67)	(4.71)	
Austrian	0.94	0.96	-2.02%
	(0.24)	(0.21)	
Age at labor market entry	17.02	16.91	0.66%
	(3.03)	(2.72)	
Apprenticeship	0.40	0.37	9.44%
	(0.49)	(0.48)	
Blue collar	0.24	0.26	-8.97%
	(0.42)	(0.44)	
Number of children	0.46	0.44	5.21%
	(0.76)	(0.75)	
Experience (months)	104	104	0.37%
	(53.40)	(52.68)	
Tenure (months)	43	48	-11.98%
	(34.40)	(36.81)	
Earnings	17,547	18,070	-2.89%
	(7,765)	(7,373)	
Earnings Growth	0.050	0.065	20.62%
	(0.16)	(0.29)	
Births next 3 years	0.21	0.23	-5.57%
	(0.47)	(0.47)	
Births next 6 years	0.42	0.46	-7.97%
	(0.69)	(0.70)	
Births next 9 years	0.56	0.62	-9.92%
	(0.79)	(0.82)	
Any birth next 3 years	0.19	0.20	-5.00%
	(0.39)	(0.40)	
Any birth next 6 years	0.31	0.34	-8.82%
	(0.46)	(0.47)	
Any birth next 9 years	0.40	0.43	-6.98%
	(0.49)	(0.50)	
Observations	8,651	218,548	

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. 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 (6 or 9) years of the reference date. Data on births up to 9 years after the reference date are only available for women with a reference date before 1996.

Table 3: Effect of firm closure on fertility

	Births next 3 years	Births next 6 years	Births next 9 years
Without covariates			
Unweighted	-0.013*	-0.036**	-0.059**
	(0.005)	(0.008)	(0.012)
Weighted: firm char.	-0.006	-0.016	-0.021
	(0.010)	(0.014)	(0 .019)
Weighted: individual char.	-0.013*	-0.034**	-0.047**
	(0.005)	(0.008)	(0.012)
Weighted: individual and firm char.	-0.007	-0.016	-0.026
	(0.009)	(0.013)	(0.017)
With covariates			
Unweighted	-0.007	-0.023*	-0.041**
	(0.005)	(0.008)	(0.011)
Weighted: firm char.	-0.008	-0.019	-0.020
	(0.008)	(0.012)	(0.018)
Weighted: individual char.	-0.013*	-0.032**	-0.044**
	(0.005)	(0.008)	(0.012)
Weighted: individual and firm char.	-0.010	-0.022*	-0.026
	(0.008)	(0.011)	(0.017)
Births (mean)	0.23	0.46	0.62
Extensive margin with covariates			
Unweighted	-0.008	-0.017**	-0.018*
	(0.005)	(0.005)	(0.007)
Weighted: firm char.	-0.008	-0.018*	-0.010
	(0.007)	(0.008)	(0.011)
Weighted: individual char.	-0.013*	-0.024**	-0.021*
	(0.005)	(0.006)	(0.007)
Weighted: individual and firm char.	-0.010	-0.021*	-0.014
	(0.007)	(0.008)	(0.010)
Any birth (mean)	0.20	0.34	0.43
Observations	221,139	221,139	145,448

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. 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

	Unweighted		Weighted	
	Births next 3 years	Births next 6 years	Births next 3 years	Births next 6 years
No previous children	-0.015*	-0.040**	-0.016	-0.039*
	(0.007)	(0.010)	(0.010)	(0.016)
Previous children	0.009	0.010	-0.002	0.013
	(0.008)	(0.010)	(0.013)	(0.016)
Age<25, no previous children	-0.001	-0.025*	-0.005	-0.037
	(0.011)	(0.016)	(0.013)	(0.022)
Age>=25, no previous children	-0.030**	-0.054**	-0.027*	-0.043*
	(0.008)	(0.013)	(0.013)	(0.020)
Blue collar	0.017	0.005	0.002	-0.013
	(0.011)	(0.017)	(0.016)	(0.023)
White collar	-0.015*	-0.032**	-0.013	-0.026*
	(0.006)	(0.009)	(0.009)	(0.013)
Wage 1st tertile	0.007	0.001	0.007	-0.008
	(0.009)	(0.012)	(0.014)	(0.020)
Wage 2nd tertile	0.006	-0.022	-0.005	-0.031
	(0.010)	(0.015)	(0.015)	(0.022)
Wage 3rd tertile	-0.034**	-0.053**	-0.030*	-0.047*
	(0.009)	(0.013)	(0.014)	(0.021)
Tenure > 3 years	-0.006	-0.027*	-0.006	-0.027*
	(0.008)	(0.008)	(0.012)	(0.018)
Tenure < 3 years	-0.006	-0.018	-0.014	-0.017
	(0.007)	(0.010)	(0.011)	(0.016)
Wage growth > 5% p.a.	-0.016	-0.043**	-0.027*	-0.052*
	(0.009)	(0.013)	(0.013)	(0.021)
Wage growth < 5% p.a.	0.003	-0.008	0.006	-0.008
	(0.007)	(0.010)	(0.011)	(0.015)
Long tenure and high wage growth	-0.037*	-0.072**	-0.035	-0.050
	(0.015)	(0.023)	(0.021)	(0.036)
Short tenure or low wage growth	-0.000	-0.013**	-0.001	-0.019
	(0.006)	(0.009)	(0.009)	(0.013)
Births (mean)	0.23	0.46	0.22	0.45
Observations	221,139	221,139	221,139	221,139

Notes: Sample description and covariates see Table 3. Coefficient estimates for separate regressions by subgroup are reported. Wage growth is wage growth over the last 3 years. 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: Effect of firm closure on fertility - Males

	Births next 3 years	Births next 6 years	Births next 9 years
Main effects			
Unweighted	-0.006	-0.014	-0.021
	(0.005)	(0.008)	(0.013)
Weighted: individual and firm char.	-0.011	-0.027*	-0.037*
	(0.010)	(0.013)	(0.015)
Heterogeneous effects			
Blue collar	-0.015	-0.032	
	(0.014)	(0.019)	
White collar	-0.007	-0.021	
	(0.011)	(0.016)	
Long tenure and high wage growth	-0.006	-0.027	
	(0.031)	(0.041)	
Short tenure or low wage growth	-0.011	-0.025	
	(0.011)	(0.015)	
Births (mean)	0.22	0.38	0.54
Observations	64,641	64,641	42,238

Notes: Displaced group includes men aged 18-35 with at least one year of tenure in closing firms at the closure date. Control group is a random subsample of men 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. All regressions control for: 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. Heterogenous effects are weighted. For weighted regressions standard errors are bootstrapped (500 replications). Symbols: ** significant at 1%;* significant at 5%.