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Preface

Labor market and demographic developments are highly interrelated. One reason is that important events in the private domain, such as the formation of a family, can cause large adjustments in individuals' labor supply. There is, for example, an extensive empirical literature showing that female labor supply declines sharply with the presence of children (Lundborg et al., 2017; Kleven et al., 2018). Moreover, married individuals rely on spousal labor supply to smooth consumption when facing shocks (Blundell et al., 2016). Reversely, labor market outcomes can have important consequences for individuals' health and mortality. For example, a job loss is related to an increase in mortality and a worsening of mental health (Sullivan and von Wachter, 2009; Kuhn et al., 2009). This dissertation analyzes important intersections between labor and demographic economics. The first chapter looks directly at the importance of a change in household demographics, namely in fertility, on labor supply decisions. The second chapter studies interdependencies in spousal labor supply and the effectiveness of intra-household insurance in married couples. The third chapter focuses on the impact of a large immigration-related demographic shift on local labor markets, while the last chapter examines the development of mortality rates in Germany after reunification, which entailed substantial changes in the labor market.

In the first chapter of this dissertation, I investigate empirically how maternal labor supply responds to an increase in fertility in the context of Mexico. Existing empirical research mainly focuses on high-income countries and points to a negative causal relationship between family size and maternal labor supply, suggesting that mothers shift time from the labor market to child care.[1] Parents can alternatively focus on monetary rather than time inputs in raising their children. In the context of Mexico, where informal child care and flexible employment opportunities are easily available, working in the labor market to obtain additional resources for the growing family appears relatively attractive. In addition, children can carry high fixed costs relative to household income, which makes increases in labor supply more valuable. Hence, parents in middle- and low-income countries may react differently in terms of labor supply.
To address the endogeneity of fertility choices, I use an instrumental variable (IV) approach that exploits parents' preference for mixed-sex siblings, as proposed by Angrist and Evans (1998). The rationale is that, compared to parents who have children of different sexes, parents

[1] Lundborg et al. (2017) provides a useful review of the existing empirical literature.

with children of the same sex are more likely to have an additional child. At the same time, children's sex mix is assumed to be virtually randomly assigned. Using data on a sample of about 500,000 Mexican mothers, I show that parents with two children of the same sex are significantly more likely to have another child relative to parents with mixed-sex children. Relying on this instrument-induced variation, I find that an exogenous increase in family size beyond two children leads to a substantial and significant *increase* in women's probability to be employed. This extensive margin response is driven by mothers who start working in the informal sector. I further show that fertility increases maternal labor supply in the formal sector at the intensive margin for a small fraction of women, while mainly having an impact at the extensive margin in the informal sector.

I contribute to the literature by providing empirical evidence on a positive effect of fertility at both the intensive and extensive margin of maternal labor supply in a middle-income country. Further, I offer an explanation for this positive effect, which stands in contrast to results in high-income countries, by highlighting that mothers take up informal employment when their family size increases. This chapter investigates further mechanisms. First, paternal employment and working hours do not react to an exogenous change in family size, suggesting that mothers are responsible for providing additional resources for their family. Second, an increase in family size leads to a higher likelihood of co-residing with a maternal grandparent. Third, the positive effect on maternal labor supply is more pronounced in households with a particularly high value from labor income that is those with little household wealth.

The crucial obstacle for my research design is the internal and external validity of the instrument. I provide credible evidence for the internal validity of my results and test the identification assumptions formally. Regarding external validity, I first estimate the size of the group to which the estimated effects pertain, which are mothers who had more children than they otherwise would have only due to the sex composition of their first two children. The size of this group is very small, and its observable characteristics and potential labor market outcomes are, on average, different from other mothers in the sample. In further analysis, I therefore bound the effect of having more than two children on the propensity to be informally employed to be non-negative for all mothers in the sample using novel methods. Hereby, I complement the discussion about the local nature of average treatment effects in IV strategies.

The findings of this chapter suggest that policy measures that target the welfare of working mothers are important. Informal jobs often provide low pay and inadequate social protection but may offer the only possibility to work for mothers in Mexico. More flexible working conditions, for example, the provision of parental leave or part-time contracts, and more flexible child care schedules can help parents to combine employment in the formal sector and family responsibilities.

While private life events are important determinants of labor supply, individual labor market outcomes can as well change when firms adjust their labor demand. Mass layoffs and firm closures are instances that give rise to unexpected and large shocks to affected workers with persistent negative consequences in terms of employment and earnings (Jacobsen et al., 1999).

Marriage can provide the possibility to share these risks and to attenuate shocks: if one partner is affected by an unexpected shock, the other partner can increase her labor supply to prevent a drop in household consumption. The desire to raise a family and gender norms might, however, interfere with the risk-sharing potential by limiting the flexibility of spouses to respond.
In the second chapter, which is based on joint work with Martin Halla and Andrea Weber, we disentangle the roles of different channels in the responses to income shocks within married households. We study a sample of couples in Austria, where the husband loses his job due to a mass layoff or plant closure. These shocks hit the couples in our sample at crucial stages of family formation. Because of the complexity of the counterfactual life-cycle labor market patterns of wives, we propose three independent control groups to identify the causal effects of husbands' displacement on wives' labor supply. The first control group consists of couples with the husband working in a firm without mass layoff or plant closure. The second control group consists of couples where the husband works in a plant with a mass layoff but is not laid off himself. The third control group exploits the randomness in the *timing* of displacement.
Our main results are remarkably consistent across the three control groups. We find that husbands suffer large and persistent employment and earnings losses over the first 5 years after displacement. Wives' labor supply increases only moderately and they respond predominantly at the extensive margin. While the additional earnings generated by the wife only cover a very small fraction of the total income loss, public transfers and taxes are more important insurance mechanisms. In terms of non-labor market outcomes, we find a small positive effect on the probability of divorce, but no effect on fertility. The presence and age of children in the household are crucial determinants of the wife's labor supply response. The most responsive group are mothers who are planning to return to the labor market after a maternity break, while mothers of very young children or wives without children remain unresponsive.
With this chapter, we contribute to the large literature on family labor supply and the long-term effects of job displacement by providing clean quasi-experimental evidence on the effects of job loss on family labor supply. We also contribute to the emerging literature on the role of gender norms in shaping labor market outcomes (Bertrand et al., 2016; Kleven et al., 2018).
Based on our findings, we propose different types of policies that might strengthen the intra-household insurance channel. The first type of policies targets the re-entry of mothers into the labor market, by strengthening the job guarantee after parental leave (Lalive et al., 2014) and by expanding subsidized child care. The second type of policies targets fathers' involvement in child care at home, for example, by reserving part of parental leave benefits for fathers. Finally, policies targeting unemployed workers directly should take the household situation into account and also extend job search counseling to wives of unemployed married men.

The opening of local labor markets to foreigners is often assumed of having adverse effects for the labor market opportunities of natives. For example, when asked about whether they have fears related to the building of the European Union (EU), more than half of all Austrian respondents mentioned that they were afraid of losing their jobs.[2] These fears are mirrored

[2]This is based on data from the European Value Study from 2008. Respondents could respond on a scale

in the growing public and political opposition against the principle of free movement of labor within the EU in recent years. Concerns are particularly pronounced toward the mobility of individuals from Central, Eastern, and Southeastern European member states; among them the eight countries that joined the EU in 2004 (EU8). They are especially prominent in Austria, which shares a large part of its border with four of these eight countries.

In the third chapter, which is based on joint work with Andrea Weber, we investigate how local labor markets are affected when receiving a large number of immigrant workers. For this purpose, we examine the effects of increased immigration to the Austrian eastern border region resulting from the EU enlargement of 2004. We focus on the labor market outcomes of resident workers across municipalities and follow their employment and average earnings over time. In addition, we look at the labor demand side by investigating whether firms enter or exit regions that receive many immigrants.

In our empirical strategy, we combine the temporal variation in immigration resulting from the EU Eastern enlargement and the later implementation of free labor market access to EU8 workers with the spatial variation that comes from the tendency of immigrants to go to municipalities that are close to their home country, thus identifying the effect of immigration on the employment and earnings of different subgroups of workers and on firms. Our analysis is based on Austrian social security data, which provides us with detailed information on the universe of employees and employers in Austria over several years.

We find that the EU Eastern enlargement led to significant changes in Austrian labor markets close to the EU8 border. Over the period from 1997 to 2015, the employment share of EU8 nationals in the Austrian border region increased from 2.2% to 9.3%. The increase after the enlargement of 2004 is larger in municipalities that are located closer to the border to an EU8 country and is almost entirely driven by an increase in blue-collar employment. The responses of other workers differ substantially between Austrian and other immigrant workers: There is no decline in the employment of Austrian nationals in municipalities close to the border. For non-EU8 foreign workers, average employment decreases significantly and steadily in municipalities that are within 0 to 20 minutes to the border after 2004. Moreover, Austrians experience an increase of approximately 2% in their earnings, non-EU8 foreign workers face substantial decreases of almost 5% by 2015. Finally, we find that immigration leads to an increase in the number of firms.

This chapter extends our understanding of the relationship between immigration and labor market outcomes in a unique context. We highlight that the impact of immigration differs substantially between the group of Austrians and the group of other foreign workers and can even go in opposite directions. We further contribute to the literature on the role of the labor demand side in the effects of immigration by investigating whether firms enter markets that receive many immigrants and, hereby, potentially alleviate the pressure on residents' employment and wages. Last, but not least, understanding the labor market effects from the EU Eastern enlargement and the opening of a national labor market is important and interesting *per se*;

from 1 to 10, where 1 indicates "very much afraid" and 10 "not afraid at all". I count the responses from 1 to 3 as being afraid. *Source:* http://europeanvaluesstudy.eu, last access 05/2019.

in particular, in consideration of the aforementioned public and political debate. Our findings, for example, indicate that Austrian nationals do not experience a decline in their employment or earnings; a result that contradicts the perceptions expressed in the survey data.

The worsening of labor market opportunities can have important consequences for individuals' well-being and health. For example, Sullivan and von Wachter (2009) find that an involuntary job loss increases male mortality in the US immediately after the event as well as in the long-run. Kuhn et al. (2009) find that the mental health outcomes of Austrian men worsen and sickness benefits strongly increase due to job loss. At the same time, studies for the US show that, since 1998, mortality rates in midlife are increasing for white non-Hispanics (Case and Deaton, 2015, 2017). This trend is driven by deaths from drug overdoses, suicides, and alcohol-related diseases, termed as *deaths of despair*, and by the subgroup of low-educated individuals. Case and Deaton (2017) suspect that the mechanism behind this development is the deterioration of life circumstances for low-educated white non-Hispanics over time that is, for example, related to the worsening of employment opportunities.
In the fourth chapter, which is based on work with Peter Haan and Anna Hammerschmid, we extend the analyses of Case and Deaton (2015, 2017) for Germany for the period after 1990, analyzing how mortality in midlife changed for East and West German subpopulations with a focus on deaths of despair. Germany provides an interesting setting due to its reunification in 1990, which led to higher average incomes in East Germany, but also to substantial discontinuities in employment careers and to high unemployment.
Using official cause-of-death statistics, we descriptively analyze trends in mortality rates between 1990 and 2015. For men, we find a considerable gap in all-cause mortality between East and West Germany in the early 1990s. Up to the late 1990s, male mortality had fallen more rapidly in East Germany, closing part of the East-West mortality gap. However, the remaining gap has persisted, staying rather constant since the early 2000s. For women, the East German excess mortality has always been less pronounced and disappeared entirely during the early 1990s. The patterns are similar if we focus on the mortality rates from deaths of despair only, implying that also deaths of despair are more pronounced among East than among West German men. Further, we show that the largest share of these deaths of despair are alcohol-related diseases.
There are various potential explanations for the differences in the mortality patterns between Germany and the US. They might be, for example, related to the German welfare system that provides a broader and more generous safety net for individuals who experience economic or social shocks than the US. Moreover, the German health care system and pharmaceutical regulations might have been more successful at preventing large-scale drug addiction and its related mortality.

Chapter 1

Fertility as a Driver of Maternal Employment*

1.1 Introduction

An extensive and longstanding literature on the effects of children on household behavior emphasizes their costs in terms of maternal time inputs. Empirical research, stemming mainly from high-income countries, fuels this by almost exclusively finding a negative causal relationship between family size and maternal labor supply (see Lundborg et al. (2017) for a review). Hence, economists often hypothesize that having more children, especially young ones, clearly decreases the time mothers spend in the labor market. However, from a theoretical point of view, the sign of the relationship between family size and maternal labor supply is ambiguous. Under reasonable assumptions, a woman is predicted to decrease her leisure time after increasing her fertility. However, some women will increase labor supply and focus on monetary investments while others will reduce labor supply and focus on time investments in order to increase child quality (Heath, 2017). Besides the monetary inputs in child quality, there are some fixed costs of children that decrease household income, which makes increases in labor supply more valuable by increasing the marginal utility of wealth.

* Schmieder (2014) provides preliminary results of this chapter and was submitted as my master's thesis at the University College London. Schmieder (2014) deals with the effect of an increase in family size from two to more than two children on the weekly working hours of mothers in Mexico. The research design is similar as the one applied in this chapter. This chapter, however, contains several new key contributions. First, compared to Schmieder (2014), I also look at employment at the extensive margin and distinguish between employment and hours in the formal and the informal sector, which turns out to be crucial for answering the research question. In addition, I add a section in which I describe informal employment in Mexico and its prevalence among mothers. Second, I estimate and discuss the effects on fathers. Third, I introduce several further analyses to discuss the importance of informal child care and household wealth. Fourth, I apply recent econometric approaches to extrapolate from local treatment effects.

This paper provides new insights about the effect of fertility on maternal labor supply in a setting with a low level of average household wealth, informal child care provision within extended households, and a high prevalence of informal work. These are all conditions that are typically more prevalent in middle- and low-income than in high-income countries; all playing a potentially interesting role in the relationship between fertility and maternal employment. First, little wealth implies that households already have a high marginal utility of income. Budgetary consequences of children might be more severe in these households and they can have a larger impact on the mother's propensity to take up employment in order to provide for additional resources. Second, if there are other household members who inelastically provide some child care, this will lower the return to maternal time relative to monetary investments into the children. Hence, they increase the incentives to augment labor supply and focus on monetary inputs. Third, the informal labor market might also provide employment opportunities to mothers: informal employment might be easier to arrange with child care schedules, which are often half-day, it might provide the possibility to take care of the children while working, and/or fixed costs of work might be lower in informal compared to formal employment (Gong and van Soest, 2002; Heath, 2017).

In order to establish a causal link between the two variables, an instrumental variable approach exploiting parental preferences for mixed-sex siblings, as outlined in Angrist and Evans (1998), is used. The empirical analysis uses data on a sample of about 500,000 Mexican mothers aged 21 to 35. The setting provides two attractive features. First, in contrast to other low- and middle-income countries (Wolpin and Rosenzweig, 2000; Schultz, 2008), Mexican parents show a preference for a mixed-sex sibling composition of their children while at the same time there are no discriminatory practices toward one sex that might invalidate the identification strategy. Second, Mexican census data from 2010 allows for observing detailed labor market outcomes in both the informal and formal sectors as well as for identifying the complex household constellations and characteristics for a large sample of young mothers.

The first set of results confirms the presence of mixed-sex sibling preferences in my sample: Families with two children of the same sex are 3.3 percentage points more likely to have another child compared to those with children of different sexes. A considerable and significant negative correlation between having more than two children and female labor supply is reversed when the potential endogeneity of fertility is accounted for with the proposed instrumental strategy. I find that an exogenous increase in family size beyond two children leads to a significant 4 to 6 percentage points increase in women's probability to be employed. This extensive margin response is driven by mothers who start working in the informal sector. I further estimate the effect of an increasing family size at different quantiles of the maternal working hours distribution. My findings suggest that fertility increases female labor supply in the formal sector at the intensive margin for a small fraction of women, while mainly having an impact at the extensive margin in the informal sector.

What are the potential mechanisms behind this positive employment effect? Further analysis on households in which mothers live with their partners shows that they increase their labor supply, while paternal employment and hours do not react at all. This might indicate that

men had a harder time increasing their labor supply compared to women in 2010 in order to provide for additional resources for their growing family. This assumption is in line with previous findings that labor market opportunities were relatively better for women in Mexico during the Great Recession (World Bank, 2012). I further show that having more than two children increases the probability to live with the extended family. In particular, the probability of living with maternal grandparents rises by approximately 4 percentage points. Moreover, I find suggestive evidence that the positive employment effect is mainly driven by women who live in households with relatively low household wealth. This is in line with the idea that these households have a particularly high value from labor income as their family size increases.

The crucial obstacle for my research design is the internal and external validity of the instrument. I provide credible evidence for the internal validity of my results and I test the identification assumptions formally by a procedure recently proposed by Huber and Mellace (2015). The impact of fertility on female labor supply is likely heterogeneous and hence, the obtained IV estimates are interpreted as local average treatment effects (LATEs) that pertain to the group of females who had more children than they otherwise would have due to the sex composition of their first two children. The proportion of these compliers in my sample is estimated to be around 3 to 4% and I show that the average complier seems to be different from the average non-complier both in terms of her observable characteristics and potential labor market outcomes. Hence, the external validity of this local effect appears to be limited. I subsequently draw on recent approaches proposed by Kowalski (2016) and Brinch et al. (2017) in order to extrapolate to the treatment effect for non-compliers. My findings indicate that, for informal employment, the treatment effect is non-negative for all women in the sample and hence similar across compliers and non-compliers.

My paper is primarily related to the large literature on the relationship between fertility and female labor supply. Browning (1992) discusses methodological issues that arise in modeling the effects of children on household behavior. The emerging empirical literature, predominantly based on quasi-experimental approaches, finds a substantial negative effect of an exogenous increase in family size on maternal labor supply in high-income countries (see e.g. Rosenzweig and Wolpin (1980), Angrist and Evans (1998), Lundborg et al. (2017) and Kleven et al. (2018)). Empirical results on middle- and low-income countries are mixed and point to a zero or negative effect of an increase in family size on maternal employment at the extensive margin (Agüero and Marks, 2008, 2011; Cáceres-Delpiano, 2012; Aaronson et al., 2017).[3] Schmieder (2014) finds that weekly working hours of mothers increase in response to an exogenous increase in family size from two to more than two children but she does not investigate employment at the extensive margin. The study by Heath (2017) is closely related to this paper but uses data on Ghanaian households and a different empirical strategy. She finds a negative effect of fertility

[3]My results contrast with Cruces and Galiani (2007), who find an inverse relationship between fertility and mother's employment using the same identification strategy and Mexican data from the year 2000. There are different potential explanations for this. Around 2010, the labor market environment was more favorable for women compared to men, as such, women were increasingly responsible for the provision of financial resources to the household. Furthermore, two reforms reduced the price of child care substantially between 2000 and 2010: Preschool education was declared mandatory for all children aged 3 to 5 years and a federal daycare program for working mothers was launched (Staab and Gerhard, 2010; Mateo Díaz and Rodríguez Chamussy, 2013). Finally, it might be that the complier subpopulation affected by the instrument changed over time.

at the extensive margin of maternal employment and a positive hours response conditional on employment. In contrast to her, I can identify intensive margin responses with a local quantile treatment effect model that is more reasonable than conditional-on-positive effects, especially in a setting in which a positive hours choice is very selective. Hence, I contribute to the literature by providing empirical evidence on a positive effect of fertility both at the intensive and extensive margin of labor supply in a middle-income country. To the best of my knowledge, this is the first study that shows that mothers can be pushed into taking up employment when having a larger family.

I further contribute to the literature by identifying potential channels behind the positive effect of larger family size on employment. It appears that the effect is mainly driven by mothers moving into the informal sector. The results line up with Rodin et al. (2012) who find in an interview-based study in Mexico that the need for greater job flexibility to care for children and other family member forces some women to take up informal employment. A few other studies look at heterogeneities across the type of labor arrangement: Agüero and Marks (2011) show that, in low-income countries, an additional child does not impact maternal employment on average, but it reduces the probability of paid employment. Cáceres-Delpiano (2012) finds for a sample of several low-income countries that only jobs with a high degree of informality react to changes in family size, but the results go into the opposite direction as mine. In contrast, Heath (2017) finds a positive effect of having children on self-employment in Ghana. I add to the literature on child care and labor supply by showing a positive relationship between family size and co-residence with maternal grandparents.[4] Having a grandparent in the household might positively affect the availability of child care which makes time less attractive relative to monetary investments and, hence, increases the incentives to increase labor supply. This relates to Gong and van Soest (2002) who show that maternal labor force participation in Mexico is higher when other adults are present in the household. Moreover, my finding that the labor supply response differs by household wealth is interesting in the light of results by Aaronson et al. (2017), who estimate the effect of fertility on maternal labor supply across different income levels by comparing the effect across countries and time. They find that the effect of an increase in family size on maternal employment is not different from zero at low and negative at higher (per-capita) income levels. In contrast, by comparing households within the same setting, I can explore effects across households with different income levels that face the same institutions and labor market conditions.

Last, but not least, I contribute to the discussion about the validity of the *same sex*-instrument in the context of middle- and low-income countries. The internal validity is repeatedly criticized in the economic literature (see for example Wolpin and Rosenzweig (2000) and Schultz (2008)). In my paper, I directly address the concerns that were posed and substantiate my discussion with a formal test procedure. The external validity of the results from IV estimations is rarely discussed in applied work, despite its importance for interpretation and the derived policy implications. I show how proposed econometric procedures can be applied in order to discuss

[4] Posadas and Vidal-Fernandez (2013) provides an excellent literature review on the link between grandparental childcare and maternal labor force participation.

the representativeness of individuals who are affected by the instrument and to extrapolate from them. Important to mention here is a recent study by Bisbee et al. (2017). They apply the *same sex*-instrument across a huge number of countries, including Mexico, and provide an approach to extrapolate the local average treatment effect from a reference to a target context. Hence, their study revolves around the external validity of complier-specific LATEs across time and place. In contrast, my paper zooms in and investigates the potential to extrapolate local treatment effects within a context. Hence, the two studies are highly complementary in the discussion about the local nature of average treatment effects that are estimated using an instrument.

The remainder of the paper proceeds as follows: Section 1.2 provides background information on households and several aspects of the labor market in Mexico. This information is helpful to put the results into perspective. In Section 1.3, I describe the data and the construction of the sample. In particular, I provide details on informal employment in the Mexican setting. Section 1.4 explains the empirical strategy and assesses the underlying identification assumptions. Section 1.5 presents my findings on the relationship between family size and parental labor market outcomes. In Section 1.6, I provide some evidence on the potential mechanism behind my findings. The extrapolation of the effects for compliers to non-compliers within the sample is discussed in Section 1.7. The final section offers some concluding remarks.

1.2 Background

Female labor force participation in Mexico was with 43.2% among the lowest in Latin America in 2010.[5] One potential explanation for this is that Mexican women face difficulties in reconciling their family and work life. In 2009, women faced an exceptionally high burden of unpaid work: They shouldered 6.2 hours per day of housework and care, which amounts to 77% of all unpaid work in their households.[6] Moreover, there is a culture of long working hours in Mexico; the country has among the highest levels of average weekly working hours in the OECD. At the same time, working schedules in the formal sector tend to be very inflexible, and only a small share of workers are observed to work part-time (OECD, 2017). Both the unequally shared responsibilities at home and the inflexible formal labor market that expects long hours might force women to drop out of employment as they have more children. On the other hand, many women can rely on informal child care that is provided by family members who reside in the same household (Gong and van Soest, 2002). Besides, there were some policies implemented that have the potential to reduce the child care burden: Preschool education was declared mandatory for all children aged 3 to 5 years and a federal daycare program for working mothers was launched. The program targets mothers with children aged 1 to 4 from low-income households that are not covered by social security-based childcare services (Staab and Gerhard, 2010; Mateo Díaz and Rodríguez Chamussy, 2013).[7]

[5] Data retrieved from http://data.worldbank.org/country/mexico, last access 5/2019.

[6] Data retrieved from http://ilo.org/ilostat; last access 02/2018.

[7] By 2011, more than 250,00 children were enrolled, and their mothers exhibited very strong positive employment responses to the program (Ángeles et al., 2014).

As in many other middle- and low-income countries, Mexican women face a high likelihood of working in the informal sector. For example, in 2007, around 54% of all employed Mexican women, among them many with young children, worked informally (Cunningham, 2001; International Labour Organization, 2009). Moreover, there are large gender gaps: women are more likely to work informally and to have informal jobs of lower quality than men (OECD, 2017). In the literature, informal work is both interpreted as a result from segmentation in the labor market that prevents individuals from taking jobs in the formal sector as well as a free individual choice between formal and informal jobs resulting from a cost-benefit calculation (Perry et al., 2007). Perry et al. (2007) provide evidence that the Mexican labor market is well-integrated and that workers freely choose to be informal. So why would so many women favor informality? By working in the informal sector, individuals with a relatively small income avoid, on average, little taxes since the average tax rate in Mexico is close to zero for these households.[8] They do not pay social security contributions, which implies that they cannot claim social benefits, such as health or parental leave benefits, or retirement payments.[9] Still, it might be the case that women decide to enter the informal sector since it offers more flexibility than the formal sector to balance work and household activities (Cunningham, 2001; Perry et al., 2007; Rodin et al., 2012; Heath, 2017). Other hypotheses are that informal employment provides the possibility to take care of the children while working and that the fixed costs of work are lower in informal compared to formal employment (Heath, 2017). I discuss the relevance of these different hypotheses using my sample in Section 1.3.

The economic situation of many Mexican households with children is affected by the *Oportunidades* program.[10] In 2010, there were close to six million beneficiary families (about 20% of all households), each receiving an average 9,006 Mexican Pesos (715 US$) per year. The program provides basic health care for all members of the family, as well as means-tested educational grants and in-kind school supplies conditional on children attending school. The monetary transfer for school children increases in the grade and is slightly larger for girls than for boys starting from grade 7. In previous studies, it is shown that the time adult beneficiaries allocate to work or leisure is not affected by the program (Parker and Todd, 2017). Still, Attanasio and Lechene (2002) find that the program increased women's power in important expenditure decisions within the household.

1.3 Data

1.3.1 Data & sample selection

The analysis is based on Mexican Population and Housing Census ("Censo de Población y Vivienda") data from 2010. The original data is provided by the National Institute of Statistics

[8] I calculated the tax liability for a monthly income of 3,000 Pesos in the year 2010, which is the mean income of the women in my sample, using http://www.fiscalia.com/modules.php?name=Calculadoras&op=isr.

[9] However, there were several universal and noncontributory programs in place in Mexico in 2010, for example, universal health insurance (Conti and Ginja, 2017).

[10] The program was initiated in 1997 under the name *Progresa* and changed its name in 2002 (Parker and Todd, 2017).

and Geography (INEGI) in Mexico and was obtained via the IPUMS International database of the Minnesota Population Center (2014). The data set contains 11,938,402 observations in 2,903,640 households, which accounts for approximately 10% of the whole population.[11] It includes detailed information on each household member including his or her family relationship to the other individuals in the household, on labor market outcomes both in the formal and informal market, as well as on household characteristics such as the housing and economic situation.

The units of observation in this study are women who are between 21 and 35 years old and who have at least two children.[12] I am limited to look at this specific parity due to the empirical strategy that is introduced in Section 1.4. At the same time, this seems to be a reasonable margin to look at given that the reduction in the total fertility rate in Mexico is concentrated in this range, as described in Section 1.2.

The data set has only incomplete information about a woman's fertility history: It only includes the number of children ever born to each woman and the number of those who were still living at the time of the census. Child-specific characteristics, such as their age and sex, are only available for those who still live with their mothers at the time of the census. This information is necessary in order to implement the empirical strategy. Because of this, I match children and mothers within each household and drop mothers with children who died (6.5%)[13] or moved out of the household (8.3%).[14] I only include women whose eldest child was 18 or younger and whose second child was at least 12 months old at the time of the census (8.4%). Women who were 14 years or younger at first birth are dropped (1.5%).

The final sample consists of 505,569 women, of whom 90% are married or cohabiting at the time of the survey. Compared to the group of all women in the Mexican Census aged 21 to 50 (21 to 35), Table A.1 in the Appendix shows that the females in my sample are much more likely to be married or cohabiting and they tend to have lower education. By construction, the average woman in the sample has more children than the average Mexican woman. Around 58% (49%) of the group of women aged 21 to 50 (21 to 35) have at least two children, about 32% (25%) have more than two. In line with these differences in observable characteristics, women in the sample have lower employment levels and earnings, conditional on being employed, compared to the broader groups of Mexican women. Moreover, employed women in the sample are much more likely to have an informal job.

Table 1.1: Sample characteristics

	All (1)		Married | cohabiting (2)		Single (3)	
Demographic characteristics						
Age (yrs)	29.39	(3.83)	29.39	(3.82)	29.44	(3.89)
Age at first birth (yrs)	19.76	(3.08)	19.78	(3.09)	19.57	(3.00)
Literate	0.91	(0.28)	0.91	(0.29)	0.93	(0.26)
Primary completed	0.62	(0.48)	0.62	(0.48)	0.62	(0.48)
Secondary completed	0.12	(0.32)	0.12	(0.32)	0.14	(0.35)
University completed	0.03	(0.17)	0.03	(0.17)	0.03	(0.17)
Household characteristics						
Rural	0.49	(0.50)	0.50	(0.50)	0.41	(0.49)
Number of household members	5.41	(1.94)	5.38	(1.85)	5.72	(2.66)
Maternal parent present	0.10	(0.30)	0.06	(0.23)	0.49	(0.50)
Paternal parent present	0.08	(0.27)	0.08	(0.28)	0.00	(0.03)
Monthly non-mother income	3,913	(5,634)	4,039	(5,650)	2,712	(5,333)
Ownership dwelling	0.80	(0.40)	0.81	(0.39)	0.75	(0.43)
Fertility characteristics						
Children in household	2.84	(1.05)	2.87	(1.07)	2.60	(0.87)
More than 2 children	0.53	(0.50)	0.54	(0.50)	0.42	(0.49)
Same sex	0.50	(0.50)	0.50	(0.50)	0.51	(0.50)
Two boys	0.26	(0.44)	0.26	(0.44)	0.26	(0.44)
Two girls	0.24	(0.43)	0.24	(0.43)	0.25	(0.43)
Age second child (yrs)	6.44	(3.83)	6.43	(3.83)	6.55	(3.87)
Age youngest child (yrs)	3.74	(3.07)	3.65	(3.03)	4.59	(3.35)
Labor market outcomes						
Employed	0.25	(0.43)	0.21	(0.41)	0.60	(0.49)
Labor market outcomes for employed						
Informal employment	0.42	(0.49)	0.46	(0.50)	0.30	(0.46)
Hours worked per week	37.19	(20.78)	35.76	(20.88)	42.00	(19.68)
Monthly (Pesos)	2,959	(2,507)	2,946	(2,622)	3,001	(2,076)
Observations	505,569		457,546		48,023	

Note: 2010 census data from Mexico with restrictions as outlined in the text. The units of observation are women aged 21–35 with at least 2 children. Statistics depicted are means with standard deviations in parentheses. All variables are measured at the time of the survey. The labor market outcomes refer to the week before the survey.

1.3.2 Descriptive statistics

Descriptive statistics are presented in Table 1.1. The first part of the table shows some demographic characteristics. The women in my sample are, on average, 29 years old and gave birth to their first child at the age of 20. The majority of women (62%) completed at most primary

[11]The sample is clustered by municipality, with enumeration areas selected by simple random sampling within strata (https://international.ipums.org/international/sample_designs/sample_designs_mx.shtml; last access 08/2017).

[12]This age restriction is motivated in Angrist and Evans (1998). My results do not change if I apply a minimum age of 18 or a maximum age of 40.

[13]To be more precise, I exclude women who had children that were no longer living at the time of the census, considering all live births and, hence, excluding stillbirths.

[14]I also exclude females for whom at least one of the following variables is missing: Age and/or sex of the two firstborn children, employment status, working hours, and earnings. Moreover, females who had twins at the second birth (or triplets at the first birth) are excluded from the sample due to the difficulty of assigning a birth order to the second and third child.

school. Only 3% have a university degree. These characteristics are very similar across single and married or cohabiting women. About half of the women live in very small municipalities with less than 2,500 inhabitants. This proportion is lower for single women. Ten percent live with at least one of their parents in the same household, while 8% share a household with a parent of their partner. Single women have, at 49%, a very high likelihood to live with one of their parents. The mean monthly labor income of the household, excluding mother's income, averages 3,913 Mexican Pesos (307 US$).[15] For married and cohabiting women, the former is mainly earned by the spouse, while for single women by the maternal grandfather. Only 2% of the eldest children in my sample work.

The fertility characteristics in the third part of Table 1.1 demonstrate that mothers in the sample live with an average of 2.8 children in the same household. About 53% live with more than two children, constituting the group I refer to as the treatment group. The first two children of 50% of the observations in the sample are of the same sex, with two firstborn males being with a proportion of 26% a bit more likely than two females. This makes sense given that the natural sex ratio at birth is slightly biased toward boys (Hesketh and Xing, 2006).

Further differences between women with and without a partner arise when it comes to labor market variables presented in the last part of Table 1.1. At the time of the census, 60% of single women were working compared to only 21% of women in a partnership. Of these employed women, 42% work in an informal job, averaging 37 working hours per week with an income of 2,959 Pesos per month. Single women are more likely to be in formal employment, working more hours than married or cohabiting women and receiving slightly higher monthly earnings.[16]

1.3.3 Informal employment in Mexico

As noted earlier, the Mexican labor market is characterized by a large share of women who are employed in the informal sector. I use my sample to learn more about informal jobs in Mexico and to discuss the hypotheses stated in Section 1.2.

I define informal employment as working on one's own account (self-employment with no employees hired), day labor, or unpaid work, whereas formal employment refers to self-employment with employees or blue- and white-collar work.[17] In the Census data, I observe Mexican women in informal employment mainly working as store merchants (20.9%), workers in the cultivation

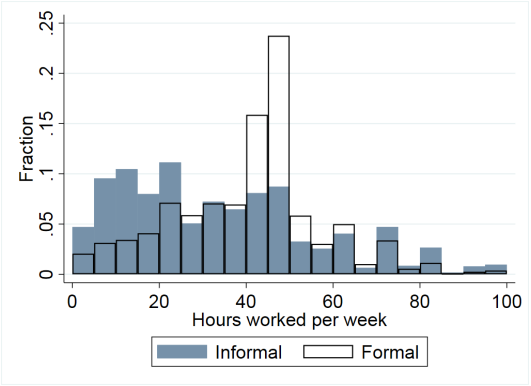
[15] Given an average exchange rate of 12.75 Mexican Pesos per US$ in 2010.

[16] Compared to the sample in Cruces and Galiani (2007), who use 2000 Mexican census data and similar sample restrictions, the proportion of women working for pay increased by around 7% in the overall sample and by around 5% in the sample of married women from 2000 to 2010. Moreover, the percentage of women in the sample who have more than two children is around 18% lower, and they have about 0.5 children less in 2010 than in 2000.

[17] According to the ILO, *Informal employment* is defined as comprising employers working in their informal sector enterprise, employees holding informal jobs (jobs without social protection and other job-related benefits) both in the formal and informal sector, and own-account workers (International Labour Organization, 2009). Unfortunately, I can neither observe whether employees are in jobs with social protection and job-related benefits nor whether employers are formally registered. Still, the job types I define as informal are those typically associated with informality (Cáceres-Delpiano, 2012). Moreover, I observe to what type of health insurance households have access. Among households with informally employed mothers, 80% are not covered by any social security institution in Mexico, i.e., they are neither insured nor are they part of the public insurance. This number is substantially lower (50%) for households with formally employed mothers.

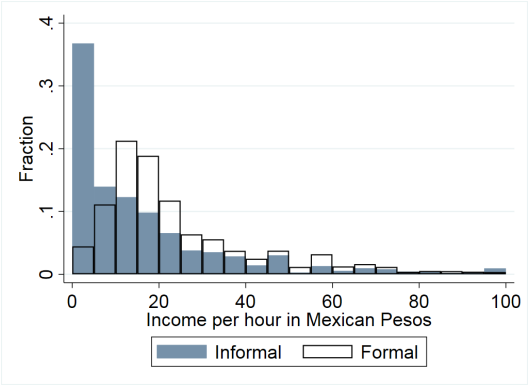
Figure 1.1: Distribution of working hours and wages in formal and informal employment

(a) Working hours



Formal: Mean: 39.7, Median: 40, SD: 17.8
Informal: Mean: 33.7, Median: 30, SD: 23.8

(b) Net hourly wages



Formal: Mean: 26.5, Median: 18.7, SD: 28.8
Informal: Mean: 20.3, Median: 10.4, SD: 37.8

Notes: 2010 census data from Mexico with restrictions as outlined in the text. The units of observation are women aged 21–35 with at least 2 children. Informal employment is defined as in the text. The graph to the left shows the distribution of working hours and the one to the right the distribution of net hourly wages by type of employment (informal/formal).

of maize/beans (6.0%), domestic workers (5.8%), servers of food and drinks (5.0%), and sales workers (4.5%). This observation is in line with findings in OECD (2017).

In Figure 1.1, I compare working hours and hourly wages between women in formal and informal employment. Of course, differences in the distribution of hours and wages over the type of employment must be interpreted with caution. It is likely that women observed in the formal versus the informal sector are very different from each other. Panel (a) shows that, on average, women work fewer hours in informal relative to formal employment. Furthermore, the working hours are much more dispersed for women in the informal sector. This observation suggests that informal employment is more flexible regarding working schedules. Moreover, women with an informal job are much more likely to be observed with working hours between zero and twenty, while around 40% of those in a formal job work full-time (40 to 50 hours). This supports the hypothesis that the fixed costs of working are higher in formal than in informal employment. These fixed costs drive women who want to work relatively few hours out of the formal labor force. Panel (b) shows that formal pays a higher average hourly net wage than informal employment. Interestingly, more than a third of all informal workers earn only 0 to 5 Mexican Pesos per hour; potentially they receive some payment in kind. To sum up, Figure 1.1 indicates that women face a trade-off between job quality, as measured by wages, and flexibility when choosing their employment.

1.4 Estimation strategy

1.4.1 Research design

The empirical analysis focuses on the effect of an increase in family size on household labor market outcomes. I define the treatment indicator D_i equal to 1 if a woman has more than 2 children and 0 otherwise. Let Y_{1i} denote the outcome if i has more than two children and Y_{0i} the outcome in the absence of treatment, i.e., with two children. For any individual, Y_{1i} and Y_{0i} cannot be observed simultaneously. The outcome observed for i is $Y_i = D_i \times Y_{1i} + (1 - D_i) \times Y_{0i}$. If fertility is randomly assigned, then the difference in the mean outcomes of treated and untreated individuals would identify the average treatment effect, since individuals of both groups were comparable and, thereby, had similar potential outcomes independent of actual treatment assignment.

However, the number of children is potentially endogenously determined. Women decide about their number of children, given earning potentials and career plans. Moreover, there probably exist other unobserved factors that influence both fertility and labor market outcomes, i.e., preferences for having children that are correlated with those for working in the labor market. Thus, the average potential outcome for females with two compared to those with three or more children would differ, even if they had the same number of children. Simply comparing the two groups confounds the effect of the treatment with other differences across these groups.

The idea of the estimation method is to use the variation from an instrument that indirectly shifts the number of children while holding other determinants of the outcome variables constant. I exploit parental preferences for a mixed-sex sibling composition to instrument for an increase in family size from two to three or more children. Introduced by Angrist and Evans (1998), this instrument is commonly used in the literature. The rationale behind this strategy is based on previous results showing that, compared to parents who have children of different sexes, parents with children of the same sex are more likely to have an additional child. At the same time, children's sex mix is assumed to be virtually randomly assigned. Z_i is hereafter defined as an indicator that is equal to 1 if the two firstborn children are of the same sex and 0 otherwise.

I use the quasi-random assignment of children's sex composition to estimate the causal effect from an increase in family size on household outcomes. In the main analysis, I estimate the following linear model:

$$D_i = \gamma Z_i + X_i'\theta + \epsilon_i \tag{1.1}$$

$$Y_i = \beta_i D_i + X_i'\delta + \eta_i, \tag{1.2}$$

where Y_i, D_i, and Z_i are defined as above. Vector X_i contains relevant control variables, including indicators of the sex of the first and second child plus additional variables such as education and age.[18] Since I allow the treatment effect β_i to be arbitrarily heterogeneous

[18]The set of additional control variables includes dummies for the age of the mother in yearly categories, dummies for her age at first birth in yearly categories, dummies for the age of the second child in yearly categories, municipality size in categories, dummies for schooling in yearly categories, indicator for literacy, indicator for

across individuals, I am only able to identify local effects that pertain to the subpopulation that responds to a change in the value of the instrument, the *compliers*. The target of the estimation is the average of β_i among compliers. The baseline specification uses 2SLS with first and second stage equations given by (1.1) and (1.2) to estimate this Local Average Treatment Effect (LATE). However, the outcomes of interest are limited: Employment status is binary and weekly hours worked are non-negative with a mass point at 0. The descriptive statistics in Section 1.3 show that a substantial fraction – 75% – of women in the sample work zero hours per week. In a model with a non-saturated set of covariates, the Conditional Expectation Function (CEF) for a limited dependent variable is typically non-linear, and it might be important to use more flexible modeling strategies (Angrist, 2001). Therefore, I additionally implement an estimator for the LATE that incorporates the covariates X_i in a fully nonparametric way (Frölich, 2007). For this nonparametric IV estimator (*NP-IV*), the conditional mean outcome is estimated via a local linear estimator and the conditional mean treatment via a local logit estimator. Thereby, I avoid the functional form restrictions on the conditional expectation functions of D_i and Y_i in the linear model.

The average treatment effect on working hours might hide important heterogeneity in the labor supply behavior of households. The overall effect comprises both the impact of family size on the decision to work and on the volume of work conditional on employment. The former refers to the extensive, the latter to the intensive margin of labor supply. In order to investigate the impact of fertility on the distribution of hours worked, I compare quantiles of the potential outcome distributions among compliers. For this purpose, I apply the local quantile treatment effect (LQTE) estimator developed by Frölich and Melly (2013). The identification of LQTE does not require any functional form restrictions and relies on the same assumptions necessary to identify LATE. Thus, the framework naturally accommodates discrete outcomes and outcomes with mass points (Melly and Wüthrich, 2017).[19] In contrast, alternative models that deal with limited outcome variables, such as the Tobit model, often rely on very restrictive underlying structural frameworks for identification (Angrist, 2001). Similar to the NP-IV estimator (Frölich, 2007), I can flexibly include covariates in the model in order to relax the identification assumptions.

1.4.2 Identification assumptions

In the following, I discuss the assumptions that are necessary for the validity of the above-described instrumental variable strategy. These assumptions are introduced by Imbens and Angrist (1994) and I refer to them as LATE assumptions.

I must assume that the sex composition of the two firstborn children is as good as randomly assigned. This assumption might be violated if parents can influence the sex mix of the two

indigeneity, and state fixed effects. I flexibly control for the age variables, since the outcome variables are potentially non-linear in these age variables and there may be discontinuities at particular ages; for instance at the school starting age or at the minimum age to enter early childhood education.

[19] However, the well-behaved asymptotic distributions for the LQTE estimator rely on the continuity of the dependent variable. This implies that the inference procedures are not valid in the neighborhood in which the dependent variable has a mass point, i.e., at 0 (Melly and Wüthrich, 2017).

firstborn children, for example by sex-selective abortions or discrimination in care practices for one sex. Sex ratios of children aged zero to four that are close to the biological ones speak against the former (Cruces and Galiani, 2007). Moreover, sex-selection technologies are very expensive (Dahl and Moretti, 2008) and abortions themselves carry high costs, given that access to abortion is prohibited or severely restricted in most of Mexico (OECD, 2017). It seems implausible that families have such extreme preferences for a particular sex composition of their children that they are willing to bear the burden of a sex-related abortion. In general, family institutions in Mexico do not exhibit extreme preferences for male children as observed in some Asian countries by Schultz (2008): For instance, there is no dowry custom and no systematic discrimination against girls (Cruces and Galiani, 2007).

Even in the absence of manipulation, the sex ratio at birth is around 105-107 male births for every 100 female births (Hesketh and Xing, 2006). This implies that having a male child among the two firstborn children makes it more likely that a couple has two firstborn children of the same sex, i.e., children's sex is systematically related to the *same sex* instrument. This is a problem if children's sex is related to potential labor market outcomes or potential fertility.[20] Since I cannot rule out that this is the case, I impose the weaker assumption that the instrument is randomly assigned conditional on the sex of the two firstborn children and account for the latter in the analysis.

Table 1.2 illustrates the correlation between predetermined characteristics of women and their instrument status. Columns (1) and (2) give the mean characteristics of the corresponding variable in households with two children of different sexes and the same sex, respectively. Columns (3) and (4) show the mean difference between the two groups and its standard error, respectively. Mothers with children of mixed sexes are remarkably similar in almost all observable characteristics to those with children of the same sex. Still, there is a small but statistically significant age difference between the two groups. As explained earlier, having a male child among the two firstborn children is positively associated with the instrument: parents with children of the same sex have, on average, a significant 1.3 percentage points (2.2 percentage points) higher likelihood of a male first (second) child. Given these differences between mixed- and same-sex children mothers, the main model will condition on the sex of the first- and second-born child. Additional controls include the variables listed in Footnote 18, among them maternal age, in a very flexible way. However, the results are not sensitive to the inclusion of these additional controls.

To interpret the IV estimates as identifying the causal effect of fertility on parents' labor market outcomes, I further have to rule out that the sex composition of the two firstborn children has a systematic effect on labor supply other than through its effect on having an additional birth. This exclusion assumption might be violated if the sex composition affects child costs (Wolpin and Rosenzweig, 2000). Lower levels of expenditures for children can result

[20]There is some evidence that this might be the case. Dahl and Moretti (2008) show that having a firstborn boy increases the likelihood to marry and decreases the probability to divorce. Moreover, there might be differences in the monetary or time cost of raising girls versus boys (Angrist and Evans, 1998; Dahl and Moretti, 2008). At the same time, marriage, divorce, and child costs are likely to be related to maternal labor market outcomes. Moreover, Dahl and Moretti (2008) show that having a male firstborn reduces the likelihood to have additional children.

Table 1.2: Testing for the quasi-random assignment of children's sex composition

	Mean		Difference in Means	
	Mixed $[Z_i = 0]$	Same $[Z_i = 1]$	Coefficient	SE
	(1)	(2)	(3)	(4)
Variable:				
Age (yrs)	29.408	29.381	0.027	(0.011)
Age at first birth (yrs)	19.765	19.760	0.005	(0.009)
Age of second child (yrs)	6.440	6.443	-0.003	(0.011)
Literate	0.911	0.912	-0.001	(0.001)
Primary completed	0.622	0.624	-0.002	(0.001)
Secondary completed	0.119	0.120	-0.001	(0.001)
University completed	0.029	0.029	0.000	(0.000)
Years of schooling	7.333	7.342	-0.009	(0.011)
Indigenous	0.343	0.342	0.001	(0.001)
Locality size [2,500-14,999]	0.242	0.241	0.001	(0.001)
Locality size [15,000-99,999]	0.125	0.127	-0.002	(0.001)
Locality size [100,000+]	0.140	0.140	0.000	(0.001)
First child boy	0.505	0.518	-0.013	(0.001)
Second child boy	0.495	0.518	-0.022	(0.001)
Observations	251,305	254,264		

Note: This table illustrates the balancing of predetermined characteristics by the value of the instrument. Columns (1) and (2) show the mean of the corresponding variable among households with a mixed and same sex composition of the two firstborn children, respectively. The coefficient and SE in column (3) and (4), respectively, are obtained by regressing the corresponding variable on the instrument dummy.

from hand-me-downs (i.e. for clothing and footwear) when children have the same sex. Wolpin and Rosenzweig (2000) demonstrate that these savings are existent for Indian households and that they account for a substantial fraction of the household income. Nevertheless, these results might vary significantly across countries. Bütikofer (2011), for instance, examines the Mexican Family Life Survey in 2000 and 2005. She finds no statistically significant differences in the economies of scale across households with different sibling sex compositions due to clothing- and room-sharing. Even if savings related to the sex composition were existent and affected female labor supply directly, their size would have to be meaningful enough to violate the exclusion restriction severely. In contrast to India, data on household expenditures in Mexico suggest that the fraction of household income spent on clothing and footwear for all members of the family in 2010 is, at 2.3%, very small.[21]

Another potential concern is that many poor households receive subsidies through the *Oportunidades* program as explained in Section 1.2. The amounts that can be received depend on the age and gender of the children in the household. Hence, there might be direct effects of children's sexes on maternal employment through differences in the subsidies. However, conditional on the sex of the first two children, the sex composition does not impact the amount of subsidies and hence, *Oportunidades* should not invalidate our empirical strategy. The exclusion

[21] Data is from OECD StatExtracs/Final consumption expenditure of households, http://stats.oecd.org/Index.aspx?DataSetCode=SNA_TABLE5; last access 09/2017.

assumption might also fail to hold if the sex composition of the two firstborn children directly affects the marginal utility of parents' leisure and, thus, labor supply. Conditional on the sex of the first and second-born children, assuming separability of sex-sameness and parental leisure does not seem to be too restrictive.

The third assumption needed for a causal interpretation of the IV estimates is the monotonicity of the *same sex* instrument. Monotonicity requires that while having mixed-sex siblings compared to same-sex siblings may not affect the fertility of some women, all of those affected are assumed to be affected in the same direction. It fails, for instance, if there are mothers who have a preference for at least two children of the same sex and, thereby, choose to have a third child if the first children have different sexes. Lee (2008), for example, shows that parents with two sons are less likely to continue childbearing than parents with one son and one daughter in South Korea. Wolpin and Rosenzweig (2000) find a similar pattern in India. However, fertility preferences are likely to vary between settings, and both of the countries above are well known for their sex bias due to extreme son preferences. I show in Section 1.5 that, in my sample, households with two boys are more likely to increase their family size compared to those with one boy and girl. This implies that the preference for mixed-sex siblings is more prevalent than the preference for two boys. Nonetheless, this does not rule out that households with a preference for the latter exist in my sample. Similar to De Chaisemartin (2017), I estimate the proportion of "revealed" defiers in the Demographic and Health Survey data, which has information on the desired sex composition of children. As there is no data available on Mexico, I examine a sample of mothers from Colombia, which is geographically and culturally close.[22] Among women whose two firstborn children are a boy and a girl, 0.77% had three children or more and retrospectively declare that their ideal sex composition would have been two boys and no girl, or no boy and two girls. Thus, only a small fraction of women was induced to have a third child because their two children were of different sexes.[23]

Beyond the argumentation in favor of the validity of the instrument based on empirical and institutional facts, I also apply a testing procedure proposed by Huber and Mellace (2015) for a just identified (heterogeneous) treatment effect model with endogeneity. The intuition behind the test is that assuming independence, exclusion, and monotonicity, the mean potential outcomes of always-takers under treatment and of never-takers under non-treatment are both point-identified and bounded. The fact that the point-identified moments have to lie within the respective bounds provides testable restrictions. A rejection of the null hypothesis points to the violation of independence, exclusion, monotonicity, or any combination of them. However, asymptotic non-rejection does not imply the validity of the assumptions: the instrument might be invalid without violating the testable restrictions implied by the model. Hence, given the large data set, I have a high finite sample power to detect violations of the instrument validity if and only if the instrument is invalid in a way as to cause violations of these testable restrictions. Applying the test to the sample, I obtain a p-value above 0.7. This means that I cannot reject

[22] The data was retrieved from https://dhsprogram.com/Data/; last access 05/2018. I use the Standard DHS from Colombia in 2010, Individual Recode, and apply the same sample restrictions as for the main sample.

[23] Angrist and Evans (1996) show that even if non-monotonicity does not hold, it is still possible to capture the local treatment effect of compliers if defiers and compliers have the same treatment effects.

the hypothesis that the instrument satisfies the assumptions for validity.[24]
To sum up, the discussion based on empirical observations for the Mexican context and the formal test procedure provide no evidence that the *same sex*-instrument is invalid.

1.5 Empirical results

To estimate the effect of fertility on household labor market outcomes, I follow the instrumental strategy described in Section 1.4. I begin by examining the first stage relationship between fertility and the sex mix of the two firstborn children in Table 1.3. Column (1) suggests that there is a statistically significant relationship between the treatment and the *samesex* indicator: Women with two children of the same sex are 3.3 percentage points more likely to have an additional child relative to those with children of distinct sexes. This coefficient does not change if I add a large set of control variables including indicators whether the first and second born child is male (*boy1* and *boy2*, respectively) in column (2). The significantly negative effects of *boy1* and *boy2* indicate that women stop having more children in response to having a boy. The model in column (3) splits up the *same sex* instrument in two indicators that are equal to one if the first two children are boys or girls, labeled *twoboys* and *twogirls*, respectively. The estimated coefficients suggest that females increase childbearing both in response to the birth of two boys and two girls, but the response to two boys is significantly smaller than to two girls (2.6 compared to 4.1 percentage points, respectively). In column (4), the association between having first a male child and reduced childbearing at higher parities turns insignificant when the *twoboys* and *twogirls* indicators are entered separately.[25] This means that there is no relationship between *boy1* and fertility when the effect of sex composition is allowed to differ by sex. To sum up, the first stage results suggest that women in the sample have a preference for a balanced sex mix combined with a bias for boys. The latter should not be of concern *per se*. As outlined earlier, there is no evidence for strong discrimination against girls and the sex mix of children is unlikely to affect the outcome through a channel other than family size. Hence, it seems likely that the bias for boys does only reflect cultural preferences that do not challenge the identification assumptions. Furthermore, I condition on the sex of the first two children in the estimations.
The small partial R^2 of the first stage results indicates that the instruments can only explain a small amount of the variation in the treatment and, hence, the IV estimates are going to have relatively large standard errors. Still, the F-Statistics on the excluded instruments are well above the corresponding critical values, and I can reject the null that the instruments are weak in all specifications. The instrument is subsequently used in order to estimate the effect of an exogenous increase in family size from two to three and more children on mothers' labor market outcomes.

[24] The validity of the *same sex* instrument is formally tested for a sample of mothers in the 1980 US Census by Huber (2015). He uses the same testing procedure I outline here. Mourifié and Wan (2017) apply a different testing procedure to the 1990 US Census. Both studies find no evidence that the assumptions for the validity of the *same sex* strategy are violated.

[25] The second model allows the effect of having first two boys and having first two girls to differ. In order to avoid linear dependence of the included indicators in this model, either *boy*1 or *boy*2 must be dropped.

Table 1.3: First stage relationship: Fertility and children's sex composition

	Dependent variable: More than 2 children			
	(1)	(2)	(3)	(4)
samesex	0.033	0.033		
	(0.001)	(0.001)		
boy1		-0.009		-0.002
		(0.001)		(0.002)
boy2		-0.007		
		(0.001)		
twoboys			0.026	0.026
			(0.002)	(0.002)
twogirls			0.041	0.040
			(0.002)	(0.002)
Additional controls		✓		✓
Dependent mean			0.529	
Observations			505,569	
Partial R^2 (excluded Z_i)	0.0011	0.0016	0.0012	0.0016
F(excluded Z_i)	558.55	802.75	293.65	418.09

Note: This table reports the first stage coefficients based on equation 1.1. Columns (1) and (2) use as instrument an indicator whether the two firstborn children are of the same sex. Columns (3) and (4) use as instruments indicators whether the two firstborn children are boys or girls. F(.) reports the robust Kleibergen-Paap Wald rk F statistic on the excluded instruments. Additional control variables include dummies for the age of the mother in years, dummies for her age at first birth in years, dummies for the age of the second child, municipality size in categories, dummies for schooling in years, indicator for literacy, indicator for indigeneity, and state fixed effects. In column (4), *boy2* is excluded from the set of control variables because of multicollinearity. Robust standard errors are in parentheses.

1.5.1 Fertility and mothers' labor supply at the extensive margin

In Table 1.4, I present the results from OLS and IV estimations of the relationship between maternal employment and fertility for all women in the sample. The OLS estimates in column (2) illustrate that women with more than two children have a weaker labor market attachment than those with two children: They are an average of 8 percentage points less likely to be employed, which is mainly driven by lower employment in the formal sector. Including a set of control variables in column (3) indicates that only a small amount of this negative correlation is driven by differences in observable characteristics across these two fertility groups. In contrast, instrumented *samesex*-effects in columns (4) and (5) reveal a positive relationship between family size and the probability of employment: having more than two compared to two children induces an increase in employment by an average of about 4.8 percentage points. Given the relatively low employment rate in the sample, this corresponds to a 19% increase. This effect is statistically significant at the 10% level. The employment definition includes both employment in formal and informal jobs (own-account work, day labor, and unpaid work outside the home). In the following, I look at formal and informal employment separately. The rise in employment seems to be driven by an increase in the likelihood to be in informal employment when fertility increases beyond two children. However, the effect of fertility on informal is not statistically different from its effect on formal employment. In Table 1.3, I show that mothers in my sample tend to have more children when their two firstborn are of the same sex independent

of whether they are boys or girls. In column (6), I split up the *samesex* instrument into the two separate instruments *twoboys* and *twogirls*. The coefficients in these alternative 2SLS estimations increase slightly, and standard errors become a little bit smaller, but they are very similar to before. The coefficients in the nonparametric IV estimations in column (7) are slightly smaller than in the linear models.

Table 1.4: Relationship between fertility and mother's employment probability

	Mean	OLS		2SLS		2SLS^{+}	NP-IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:							
Employed	0.248	-0.082	-0.076	0.043	0.048	0.063	0.042
		(0.001)	(0.001)	(0.037)	(0.031)	(0.029)	(0.030)
Formally employed	0.143	-0.080	-0.058	0.004	0.005	0.011	0.001
		(0.001)	(0.001)	(0.029)	(0.027)	(0.026)	(0.028)
Informally employed	0.105	-0.002	-0.018	0.038	0.043	0.052	0.041
		(0.001)	(0.001)	(0.025)	(0.025)	(0.024)	(0.023)
Additional controls			✓		✓	✓	✓
Observations				505,569			

Note: This table illustrates the relationship between mothers' employment status and the treatment. Each row shows the results for the stated outcome variable. Column (1) shows the mean of the dependent variable. Columns (2) and (3) show the estimated coefficients on the treatment in an OLS regression without and with additional control variables. Columns (4) and (5) show the 2SLS regression results using *samesex* as instrument without and with additional control variables and (6) using *twoboys* and *twogirls* as instruments plus control variables. (7) shows the results from the nonparametric IV estimation with *samesex* as instrument. Columns (2)–(7) all include indicators for the sex of the first- and second-born child. *Employed* is an indicator equal to 1 if a woman is employed in the week prior to the survey and 0 otherwise. $Formally$ ($informally$) *employed* is an indicator equal to 1 if a woman is employed in a formal (informal) job in the week prior to the survey and 0 otherwise. Control variables are as in Table 1.3. Robust standard errors in parentheses.

The comparison of the OLS and IV estimates suggests that the preference for further childbearing are inversely related to working in the labor market, particularly in the formal labor market. An exogenous increase in childbearing beyond two does not have an impact on the probability to be formally employed, but it does increase the propensity of informal employment. As discussed earlier and in line with previous results, this might happen because informal jobs offer more flexibility to combine work and family responsibilities. In the following, I look at employment responses beyond the extensive margin.

1.5.2 Fertility and maternal working hours responses

Table 1.5 shows the estimated effects of *more than* 2 *children* on hours worked per week in total and both in formal and informal jobs. Similar to the probability of employment, women with more than two children are observed to work fewer hours compared to those with two in columns (2) and (3). The coefficients turn positive when instrumenting for the potentially endogenous fertility decision in columns (4)–(7), but they are not statistically distinguishable from 0 in almost all specifications. The coefficients in the regression with informal hours as outcome variable tend to be larger than those with formal hours. Subsequently, I go beyond the mean effects and look at the distributional impact of higher parity childbirth on hours to

allow for different responses at the extensive and the intensive margin of labor supply.

Table 1.5: Relationship between fertility and maternal working hours

	Mean	OLS		2SLS		2SLS^{+}	NP-IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:							
Working hours	9.228	-3.492	-3.391	1.977	2.134	2.674	1.922
		(0.054)	(0.064)	(1.618)	(1.570)	(1.547)	(1.756)
Working hours, formal	5.519	-3.252	-2.542	0.691	0.650	0.813	0.503
		(0.043)	(0.051)	(1.283)	(1.230)	(1.211)	(1.073)
Working hours, informal	3.708	-0.240	-0.849	1.286	1.483	1.861	1.420
		(0.037)	(0.046)	(1.116)	(1.123)	(1.105)	(1.314)
Additional controls			✓		✓	✓	✓
Observations				505,569			

Note: This table illustrates the relationship between maternal working hours and the treatment similar to Table 1.4. *Working hours* measure the number of hours worked in the week prior to the survey and are set to 0 for those not employed. *Working hours formal (informal)* are the working hours in a formal (informal) job. Control variables are as in Table 1.3. Robust standard errors in parentheses.

I estimate the quantiles of the potential outcome distributions for compliers based on Frölich and Melly (2013) using the *samesex* instrument. Figure 1.2 illustrates the estimated quantiles of maternal working hours separately for formal (a) and informal employment (b).[26] The blue, solid (red, dashed) line shows the estimated quantiles of working hours for compliers with more than two children (with only two children). The difference between the two lines corresponds to the local quantile treatment effect at the τ quantile. Appendix Table A.2 quantifies the results and provides the corresponding standard errors. First, if they had only two children, a large fraction of women, around 85% and 90%, would have zero working hours in formal and informal employment, respectively. Second, increasing fertility beyond two children does not have an impact at the extensive margin of formal work, but it seems to have a positive effect at the intensive margin by shifting a few quantiles from around 20 to 40 hours. The second panel in Appendix Table A.2 shows that this effect is statistically significantly different from zero. Third, having *more than* 2 *children* tends to increase some quantiles that would be equal to zero or to a small number of hours in informal employment if women had two children only. The quantiles are augmented by statistically significant 13 to 15 hours at this part of the outcome distribution (see the third panel of Appendix Table A.2). Summing up, fertility increases female labor supply in the formal sector at the intensive margin for a small fraction of women, while having mainly an impact at the extensive margin of labor supply in the informal sector.

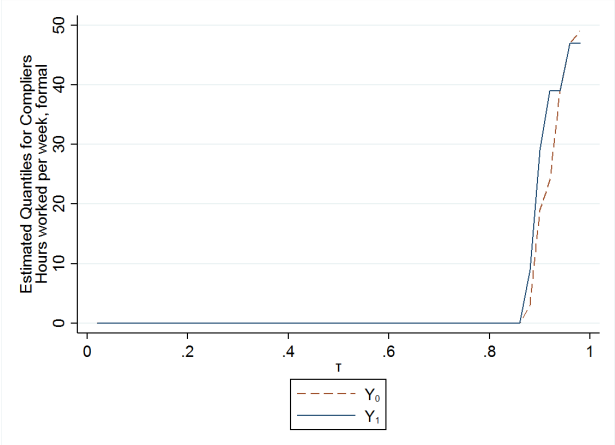
1.6 Mechanisms

In Section 1.5, I find that an increase in fertility beyond two children leads to a positive employment response of mothers. As outlined in Section 1.1, this finding contrasts with the negative

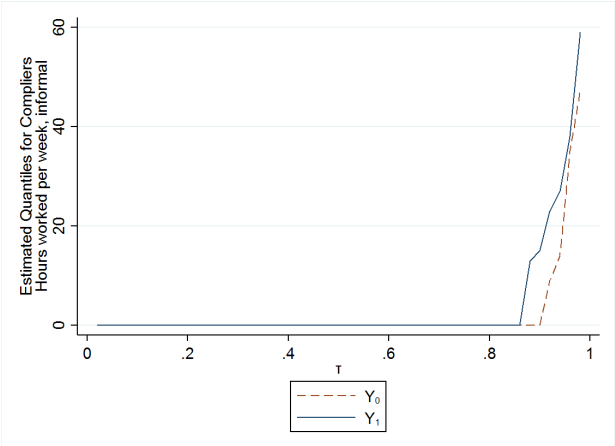
[26]The effect of the treatment on total hours in both formal and informal employment is presented in Appendix Figure A.1.

Figure 1.2: Distributional impact of fertility on mother's hours worked per week

(a) Working hours, formal



(b) Working hours, informal



Notes: This graph illustrates the estimated quantiles for the potential outcome distributions of maternal weekly working hours in formal (top) and informal jobs (bottom). The blue, solid and the red, dashed line show the estimated quantiles of working hours for compliers with more than two children, $\hat{Q}^{\tau}_{Y_1|c}$, and with only two children, $\hat{Q}^{\tau}_{Y_0|c}$, respectively. The model includes the control variables that are listed in Table 1.3.

effect found in most previous studies. There are different potential explanations for this deviation. I already show that the increase in employment is due to a rise in informal labor. Employment opportunities in the informal sector tend to be more prevalent in low- and middle-income compared to high-income countries. Most studies, focusing on the latter, find a negative relationship between fertility and female labor supply. In the following section, I discuss three more potential reasons for my empirical results. First, I show that husbands in couples do not increase their employment propensity or hours in response to an increase in their family size, which might indicate that they face difficult labor market conditions. If work opportunities were better for women around the time of the census, then this might provide a reason why they increase their labor supply. Second, I discuss the provision of informal child care within

Table 1.6: Relationship between fertility and mother's and father's employment

	Mean	OLS		2SLS		2SLS^{+}	NP-IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:							
Mother employed	0.211	-0.070	-0.056	0.033	0.043	0.052	0.035
		(0.001)	(0.001)	(0.037)	(0.034)	(0.034)	(0.035)
Mother employed, formal	0.114	-0.068	-0.041	-0.019	-0.011	-0.010	-0.019
		(0.001)	(0.001)	(0.028)	(0.026)	(0.026)	(0.027)
Mother employed, informal	0.097	-0.002	-0.015	0.052	0.055	0.062	0.054
		(0.001)	(0.001)	(0.026)	(0.026)	(0.026)	(0.027)
Mother hours worked	7.531	-2.793	-2.316	2.345	2.732	3.017	2.410
		(0.054)	(0.063)	(1.565)	(1.494)	(1.472)	(1.279)
Father employed	0.903	-0.017	0.000	-0.006	-0.004	-0.004	-0.004
		(0.001)	(0.001)	(0.026)	(0.026)	(0.026)	(0.023)
Father hours worked	43.319	-2.561	-0.117	0.152	-0.178	-0.520	-0.036
		(0.071)	(0.084)	(2.038)	(1.965)	(1.936)	(2.109)
Additional controls			✓		✓	✓	✓
Observations				412,452			

Note: This table illustrates the relationship between a set of employment outcomes for parents and the treatment similar to Table 1.4. The sample is restricted to households in which both parents are present in the household at the time of the survey and in which employment outcomes for both of them are observed. Control variables are as in Table 1.3. Robust standard errors in parentheses.

extended households that favors monetary relative to time investments by the mother. Third, I look at the response of women conditional on different proxies of household wealth.

1.6.1 Labor supply responses of married and cohabiting couples

In the following, I have a closer look at households in which mothers live together with their partners.[27] Table 1.6 shows the estimation results for the subsample of married and cohabiting couples in a similar way as for the overall sample before. The total employment response of women who live with their partners is similar to that of all women in the sample. Standard errors tend to be a bit larger in the subsample, and the estimated effects are not statistically significantly different from zero. The response of the probability to be informally employed and of total working hours is positive and significant for partnered women and tends to be slightly larger (but not statistically different) than in the overall sample.

Column (1) shows that average paternal is much stronger than the maternal labor market attachment in terms of participation and working hours: 90% of all fathers are employed, and they work an average of 43 hours per week. Column (2) illustrates that fathers with more than two children have worse labor market outcomes, on average, than those with two. In contrast to women, these differences in labor market outcomes are to a large extent explained by differences in observable characteristics, which I include in the regressions in column (3). The coefficients

[27] For 5.1% of the women who report that they are married or cohabiting, I do not observe their partner in the data. The cohabiting partner of a mother might not necessarily be the biological father of her children. In the following, "father" refers to the biological father, stepfather, or adoptive father of the children of the women in the sample.

in the IV models of paternal employment on *more than* 2 *children* are very close to zero and not statistically significant.

In a model with a unitary household, we would expect that the partner with the higher wage worked unambiguously more when having additional children because he or she had a comparative advantage in providing earnings for consumption and monetary inputs. The other partner would work more inside the home because his or her opportunity costs were lower (Heath, 2017). Surprisingly, the results here indicate that mothers increase their labor supply, while fathers do not react at all. Although wages are unobserved for most women and some men, one would probably expect that potential wages are higher for male partners.[28] One potential explanation for the contrary results are labor market frictions that impacted men differently than women in 2010, the year of the analysis. In Mexico, male labor market outcomes were the most vulnerable to the Great Recession with decreasing participation rates and income due to the economic contraction. At the same time, females labor market success was largely unaffected by the crisis (World Bank, 2012). Therefore it might have been easier for women to earn additional income that provided for the enlarged household in 2010 than it was for men. Another explanation is that the men's labor supply is less elastic.

1.6.2 Informal childcare through extended families

Another potential explanation for the positive effect of fertility on employment is that the availability of informal child care provided by the extended family makes time investments less attractive relative to monetary investments. Hence, they raise the incentives to increase labor supply and focus on monetary inputs. Several studies show that the availability of grandparent-provided childcare significantly increases maternal labor supply (see Posadas and Vidal-Fernandez (2013) for an excellent literature review). In particular, Compton and Pollak (2014) show that geographical proximity to mothers or mothers-in-law has a substantial positive effect on the labor supply of married women with young children.

In the data, I can observe whether the parents or parents-in-law are present in households. It is likely that women who live in the same household as their parents (or parents-in-law) are not comparable to women who live with their nuclear family. For instance, the descriptive analysis already revealed that co-residence is correlated with being a single mother. Moreover, co-residence with parents might be itself determined by fertility: Mothers (and their spouses) might decide to join their parents' household, or parents might choose to live with their children in order to offer them childcare when their family size increases. In order to examine the latter, I use the same IV estimation strategy as before, putting an indicator of whether grandparents are present in the household as the dependent variable. I assume that the sex composition of the two firstborn children does not affect co-residence with grandparents other than through its impact on fertility.

Table 1.7 illustrates the relationship between fertility and the presence of grandparents in the household. On average, mothers with two children are less likely to live with either their

[28] Although this is very selective, I observe that only 5% of women in the sample earn more than their partners in terms of their hourly wages among all couples in which both partners work.

Table 1.7: Relationship between fertility and household composition

	Mean	OLS		2SLS		2SLS^{+}	NP-IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Dependent variable:							
Maternal grandparent present	0.100	-0.064	-0.040	0.053	0.046	0.044	0.046
		(0.001)	(0.001)	(0.026)	(0.025)	(0.025)	(0.031)
Paternal grandparent present	0.076	-0.019	-0.000	0.011	0.005	0.004	0.005
		(0.001)	(0.001)	(0.022)	(0.022)	(0.022)	(0.021)
Additional controls			✓		✓	✓	✓
Observations				505,569			

Notes: This table illustrates the relationship between the household composition and the treatment similar to Table 1.4. *Maternal* (*paternal*) *grandparent present* is an indicator equal to 1 if at least one maternal (paternal) grandparent lives in the same household as the mother and her children. Control variables are as in Table 1.3. Robust standard errors in parentheses.

or their partners' parents. The negative correlation between fertility and the presence of a mother's parent is sustained even if I control for a broad set of variables. When I instrument the variable of interest, *more than* 2 *children*, there is a marginally significant positive relationship between family size and co-residence with maternal grandparents. The 4.4 to 4.6 percentage points increase is considerable given that 10 percent of the women in the sample live in the same household as their parents. Unfortunately, there is no information on child care in the data set, so it is not possible to test the hypothesis that the increased likelihood to live with grandparents allows women to work more when they have more children. Still, this pattern is consistent with the hypothesis.

1.6.3 Household wealth

As discussed earlier, negative budgetary consequences from fixed costs of additional children are likely to be more severe in households with low wealth. As a result, an increase in the family size of a "poor" households should have a larger impact on the mother's propensity to take up employment in order to guarantee a minimum income level for the household. I propose three different measures to proxy household wealth. The first one is the number of rooms in the house, the second is a mixed wealth measure generated using a principal component analysis using several characteristics of the households' dwelling (such as ownership of a dwelling, car, TV, or radio, as well as the type of electricity used and water supply), and the third is based on spousal earnings. Importantly, I do not find an effect of the instrumented treatment on any of these three variables. So it is reasonable to split the sample by these measures.
The corresponding results from the second stage using the *samesex*-instrument are presented in Table 1.8. The findings provide suggestive evidence that mothers in households with relatively low household wealth particularly increase their labor supply in response to an increase in their fertility beyond two children. There is no statistically significant effect of larger family size on formal employment or hours for any subgroup. In contrast, the positive effects on informal employment are larger and more likely to be statistically significant among women in low-wealth

Table 1.8: Effect of fertility on maternal employment by household wealth

	Employment		Hours		Obs.
	Formal	Informal	Formal	Informal	
	(1)	(2)	(3)	(4)	(5)
Household wealth measure:					
1. Number of rooms					
2 or less	-0.001	0.062	1.019	2.945	327,123
	(0.036)	(0.036)	(1.589)	(1.502)	
More than 2	0.021	0.029	0.151	0.238	174,833
	(0.044)	(0.038)	(1.968)	(1.728)	
2. Mixed wealth measure					
Low	0.009	0.032	1.436	1.803	255,249
	(0.040)	(0.047)	(1.782)	(1.864)	
High	0.001	0.050	0.100	1.265	250,320
	(0.037)	(0.030)	(1.652)	(1.399)	
3. Husband's earnings					
Low	-0.025	0.068	-1.638	3.876	197,536
	(0.031)	(0.042)	(1.260)	(1.757)	
High	0.004	0.048	1.716	1.457	218,110
	(0.039)	(0.033)	(1.636)	(1.476)	

Notes: This table illustrates the relationship between maternal employment and hours with the treatment by different household measures. Columns (1) to (4) show the estimated coefficients on the treatment in a 2SLS regression using *samesex* as instrument and including the control variables listed in Table 1.3. The mixed wealth measure is generated through a principal component analysis based on different dwelling characteristics. Standard errors in parentheses.

households.

These findings are in line with the idea that low-wealth households gain a particularly high value from labor income when their family size increases.

1.7 Beyond local treatment effects

So far, I estimated and discussed treatment effects for the group of compliers. The *internal validity* of these estimates hinges on the assumptions discussed in Section 1.4, namely conditional random assignment, exclusion, and monotonicity. In the following, I discuss the *external validity* of my results. To be more precise, I show whether the estimation strategy allows for drawing inferences about treatment effects for individuals in the sample other than those affected by the instrument.[29] This is important since a researcher might be interested in a policy that affects the fertility of women who are different from those that have another child due to a variation in their previous children's sex composition.

The following discussion is based, in large part, on the concept of *marginal treatment effects* introduced by Heckman and Vytlacil (1999, 2005).[30] Under the conditional exogeneity of the in-

[29] I am interested in the extrapolation of the effects for compliers to non-compliers *within the sample*. A paper by Bisbee et al. (2017) looks at the extrapolation of the relationship between fertility and maternal employment *across samples* from different countries and/or time periods.

[30] This paragraph additionally builds on a survey by Cornelissen et al. (2016).

strument Z_i, the monotonicity condition introduced in Section 1.4 is equivalent to the existence of a weakly separable treatment selection equation $D_i = \mathbb{1}[p(X_i, Z_i) - U_i \geq 0]$, where $p(X_i, Z_i)$ is defined as the probability of receiving treatment based on X_i and Z_i, $P[D_i = 1|X_i, Z_i]$, and where U_i is a measure of the unobserved distaste for treatment that is uniformly distributed over $[0, 1]$ conditional on X_i (Vytlacil, 2002). As $P(.)$ increases, individuals with successively higher unobserved distaste for treatment select into treatment. Treatment effects can vary across households with the same X_i in a way that depends on the unobservable component of the treatment choice U_i. The marginal treatment effect, $MTE(u, x) = E[Y_{1i} - Y_{0i}|U_i = u, X_i = x]$, can be interpreted as the treatment effect for individuals who are indifferent to treatment if they are exogenously assigned a value of Z_i such that their propensity score $P(.)$ is equal to u. For a U_i close to zero, the MTE measures the effect of treatment on individuals with unobservables that make them most likely to participate in treatment. If U_i is large, $P(.)$ would have to be large to induce people to participate. The $MTE(u, x)$ is equal to $MTO(u, x) - MUO(u, x)$, where MTO refers to the marginal treated outcome and MUO to the marginal untreated outcome.
Section 1.7.1 is carried out maintaining the LATE assumptions from Section 1.4 only, while Section 1.7.2 relies on further assumptions that are subsequently introduced and discussed.

1.7.1 External validity under the LATE assumptions

In the following, I group individuals according to their unobserved distaste for treatment U_i. Since Z_i and D_i are both binary, I can partition the population into *three types*, T_i (Angrist et al., 1996).[31] *Always-takers* have low resistance for having additional children: they choose to have three or more children irrespective of their previous children's sex composition. *Compliers* have an intermediate distaste for having more than two children: They are treated when the instrument is switched on and abstain from it when not, i.e., they have more than two children if the two firstborn children are of the same sex and otherwise not. *Never-takers* have a high resistance for having additional children. This means that always-takers select into treatment before compliers who themselves select into treatment before never-takers. More formally, define $u_1 \equiv P(D_i = 1|Z_i = 1)$ and $u_0 \equiv P(D_i = 1|Z_i = 0)$. Then, always-takers are individuals with an unobserved distaste for treatment $0 < U_i < u_0$, compliers with $u_0 < U_i < u_1$, and never-takers are individuals with $u_1 < U_i < 1$. The previously discussed $LATE$ parameter is a version of the MTE and can be expressed as $LATE = E[Y_{1i} - Y_{0i}|u_0 < U_i < u_1]$.
The compliance type of an individual cannot, in general, be identified. However, under independence (random assignment and exclusion) and monotonicity some always-takers, individuals observed with $Z_i = 0$ and $D_i = 1$, and some never-takers, individuals observed with $Z_i = 1$ and $D_i = 0$, can be detected. The percentage of never-takers, ϕ_n, is equal to $P(D_i = 0|Z_i = 1)$, that of always-takers, ϕ_a, is equal to $P(D_i = 1|Z_i = 0)$, and the share of compliers can be deduced by $\phi_c = 1 - \phi_n - \phi_a$ (Imbens and Rubin, 1997). Column (1) in Table 1.9 shows the

[31] In the following, I abstract from covariates X_i. This is under the more restrictive assumption of unconditional, rather than conditional, independence and exclusion. This is supported by the fact that my previous results are invariant to the inclusion of the control variables.

estimated proportion of the different compliance types among the households in the sample. The estimated share of compliers is, at 3%, very low. Never-takers are estimated to be 46% of the individuals; 51% are always-takers. The small number of compliers raises concerns about the external validity of the previously estimated local treatment effects since I estimate the effect over a very narrow range of the distribution of U_i.

Table 1.9: Compliance types: Estimated proportions & fraction employed

Compliance type (T)	Fraction ($\hat{\phi}$) (1)	$E[\widehat{Employed}_1 \mid T]$ (2)	$E[\widehat{Employed}_0 \mid T]$ (3)
Always-taker	0.512	0.208	
Complier	0.033	0.259	0.220
Never-taker	0.455		0.294

Note: This table shows the estimated proportion of the compliance types in the sample ($\hat{\phi}$) and their estimated average potential employment levels with and without treatment.

In order to investigate the possibility that the LATE is representative for the treatment effect of non-compliers, I exploit some more information that can be obtained using the LATE assumptions (Kowalski, 2016). Using independence, the average characteristics of always-takers, compliers, and never-takers can be identified. If they were the same across all types, then this would suggest that the LATE is indicative for the treatment effects of non-compliers. Table 1.10 shows the mean observable characteristics for the different groups in the sample. The average complier in my setting seems to be different from the average never-taker and always-taker in terms of her pre-determined characteristics. Never-takers are an average of about 1.5 years younger than always-takers and compliers. The mean age of mothers at first birth and of their second-born children is increasing and decreasing, respectively, when going from always-takers to compliers to never-takers. Education in terms of years of schooling is increasing in the unobserved distaste for more children. Furthermore, always-takers are more likely to live in rural areas and to be indigenous compared to compliers and never-takers. These differences in observable characteristics that are likely related to potential employment outcomes suggest that treatment effects might be heterogeneous across U_i.

Table 1.10: Mean of observable characteristics by types

	Always-taker	Complier	Never-taker
Age (yrs.)	30.02	30.21	28.63
Age at first birth (yrs.)	19.03	19.75	20.59
Age second child (yrs.)	8.26	7.02	4.35
Indigenous	0.40	0.24	0.28
Years of schooling	6.34	7.72	8.35
Literate	0.87	0.94	0.95
Rural	0.56	0.47	0.42

Notes: This table reports average characteristics of the different compliance types in the sample.

Even though it is not possible to identify treatment effects for never- and always-takers, I can identify the expectation of Y_{1i} for always-takers, of Y_{0i} for never-takers, and of both Y_{1i} and

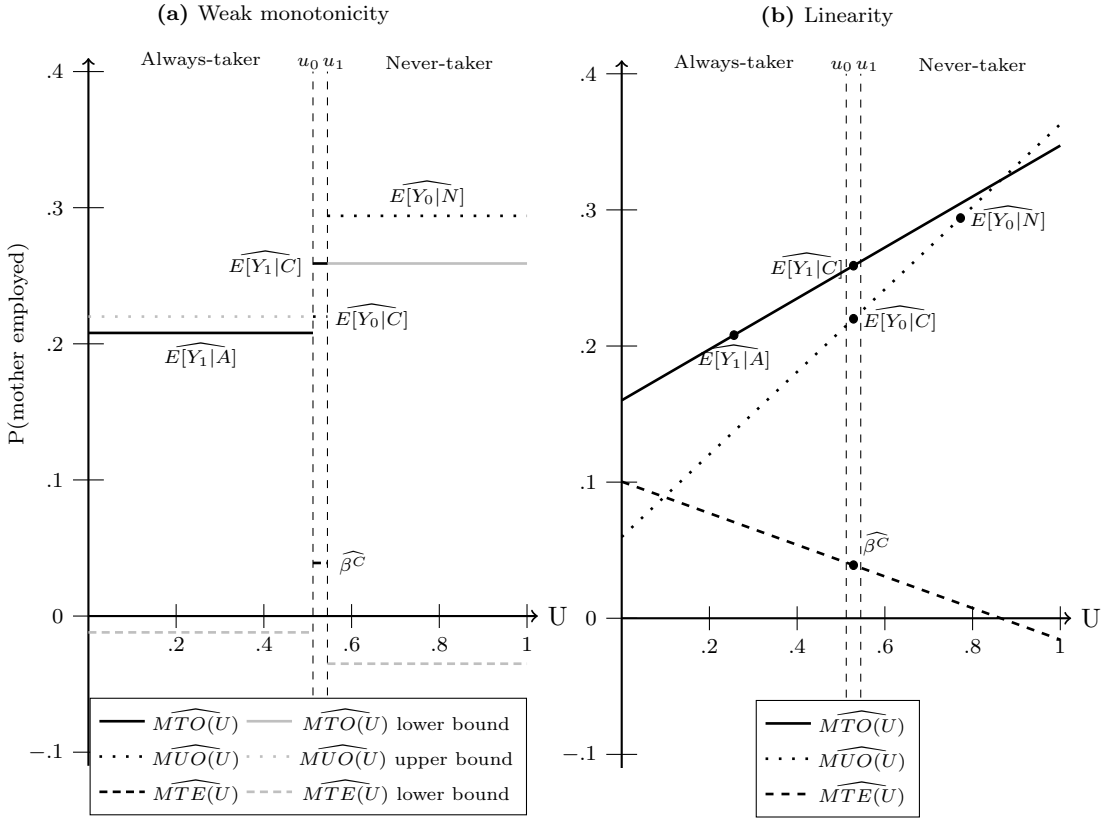
Y_{0i} for compliers. These outcomes can be informative: A difference in the average Y_{0i} of compliers and never-takers provides evidence of selection into a larger family size. A difference in the average Y_{1i} of compliers and always-takers provides evidence of selection, treatment effect heterogeneity, or both (Kowalski, 2016). Columns (2) and (3) in Table 1.9 show the estimated average potential employment with and without treatment, respectively. The proportion of employed women with only two children is estimated to be substantially larger for never-takers (29.4%) than for compliers (22.0%). This means that a low preference for having more than two children is related to a stronger labor force attachment.[32] Furthermore, the average employment levels with more than two children is around 5 percentage points larger for compliers compared to always-takers. Given the set of assumptions, I cannot disentangle to what extent this difference is due to selection or treatment effect heterogeneity.
To sum up, the size of the group of compliers is small, and it appears that they are different from the rest of the sample in terms of their observable characteristics and their potential outcomes. Hence, the effect of fertility on maternal employment is potentially different for women who do not comply with the samesex-instrument. Still, imposing further assumptions on the potential outcomes as functions of the unobserved distaste for treatment allows me to learn more about the effects of non-compliers.

1.7.2 External validity imposing further assumptions on marginal outcomes

Assuming *weak monotonicity* in the marginal untreated and treated outcome from always-takers to compliers to never-takers allows for estimating a bound on the average treatment effect for always- and never-takers, respectively (Kowalski, 2016; Brinch et al., 2017). This assumption means that the labor force attachment is either non-increasing or non-decreasing in the distaste for additional children regardless of the actual number of children. A stronger assumption is that both $MTO = E[Y_{1i}|U]$ and $MUO = E[Y_{0i}|U]$ are linear in U_i (Brinch et al., 2017). This means that a one percentage point change in the unobserved distaste for children U_i leads to an average increase in Y_d of λ_d. Linearity gives point identification of the MTO and MUO, and hence of the MTE function.
Figure 1.3 illustrates the estimated marginal outcomes and the $\widehat{MTE}$ under the assumption of weak monotonicity and linearity. The vertical axis measures the outcome of interest (here the estimated probability of maternal employment), whereas the horizontal axis measures the unobserved distaste for having more than two children. Tracing the potential outcomes and the effect of treatment over the unit interval shows how they vary with the unobserved component, where higher values of U_i correspond to lower propensities to have additional children. The black lines in both graphs illustrate estimates of moments that are point-identified under the respective assumption, while gray lines are the bounds. The estimated marginal outcome with and without treatment (or their bounds) are indicated with a solid and dotted line, respectively. The marginal treatment effect is drawn with a dashed line and is equal to the difference between

[32] The comparisons of formal and informal employment, and the working hours of never-takers and untreated compliers in Appendix Table A.3 go into the same direction.

Figure 1.3: Estimated marginal outcomes and MTE for employment



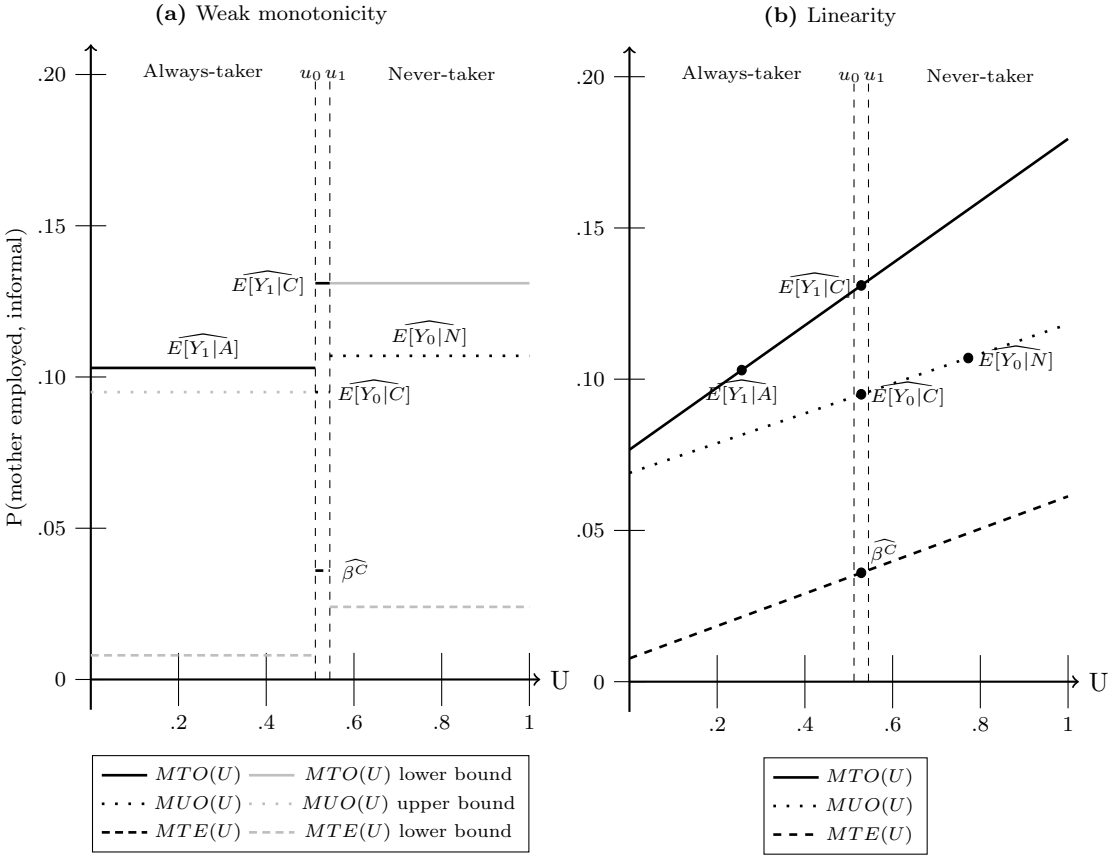
Notes: This figure illustrates point estimates and estimated bounds for the (marginal) treated outcomes ($\widehat{MTO}$), untreated outcomes ($\widehat{MUO}$), and treatment effects ($\widehat{MTE}$) as functions of the unobserved resistance toward having more than two children, U. Panel (a) assumes weak monotonicity, whereas (b) assumes linearity in the marginal treated and untreated outcome. The outcome variable is an indicator equal to 1 if a mother is employed.

the former two lines.

In Panel (a), I impose the weak monotonicity assumption on the potential untreated and treated outcome. Given that the estimated average outcome without treatment is increasing from compliers to never-takers, i.e., $\widehat{E[Y_0|N]} > \widehat{E[Y_0|C]}$, the weak monotonicity assumption implies that the $\widehat{MUO}$ is weakly upward-sloping in U_i. This suggests that the average outcome without treatment for compliers is an upper bound for always-takers. Similarly, the average outcome with treatment is increasing from always-takers to compliers implying that the $\widehat{MTO}$ is weakly upward-sloping as well. This means that the mean outcome with treatment for compliers is a lower bound for never-takers. From these bounds, I can infer a lower bound for the marginal treatment effect over the distribution of U_i: The former is -0.01 for always-takers and -0.03 for never-takers. So treatment effects are bounded to be close or slightly below zero also for non-compliers given the weak monotonicity assumption suggesting that there is no strong negative effect. Panel (b) shows that under linearity, the estimated marginal treated outcome, $\widehat{MTO}$, is larger than the estimated marginal untreated outcome, $\widehat{MUO}$, up to a value of U_i close to 0.8.

This means that an increase in family size from two to more children increases employment for all mothers with an unobserved distaste for treatment below the 80^{th} percentile of the distribution of U. At the same time, the $\widehat{MTE}$ is falling in U_i: low-resistance households have a higher treatment effect compared to those with a higher resistance toward having more than two children. Given the linearity assumption, I can formally test for unobserved treatment effect heterogeneity. If the slope in the linear MTE model is non-zero so that the MTEs are non-constant, I reject the external validity of the LATE (Brinch et al., 2017). In this setting, I estimate the slope to be within the 95% confidence interval of $[-0.39, 0.18]$. Therefore, I cannot reject the null hypothesis of constant MTE.

Figure 1.4: Estimated marginal outcomes and MTE for informal employment



Notes: This figure illustrates point estimates and estimated bounds for the (marginal) treated outcomes (MTO), untreated outcomes (MUO), and treatment effects (MTE) as functions of the unobserved resistance toward having more than two children, U. Panel (a) assumes weak monotonicity, whereas (b) assumes linearity in the marginal treated and untreated outcome. The outcome variable is an indicator equal to 1 if a mother is informally employed.

In parallel, Figure 1.4 illustrates the estimated marginal outcomes and the marginal treatment effect on informal maternal employment. The effect of an increase in family size on the probability that a mother is informally employed is bounded to be non-negative for all values of U_i under the weak monotonicity assumption in panel (a). This suggests that the non-negative

effect of an increase in the number of children on informal employment is not specific to the group of compliers, but it instead applies to all women in our sample. Under linearity, I find that the treatment effect is positive and increasing in the unobserved distaste for having more than two children, as illustrated in panel (b). The latter implies that mothers with a higher distaste for additional children are on average more likely to increase informal employment compared to those with a lower distaste when their family size increases.
Appendix Figure A.2 shows the estimated marginal propensity to be formally employed and the corresponding $\widehat{MTE}$ similar to Figure 1.3. Under the linearity assumption, the $\widehat{MTE}$ of an increase in fertility beyond two children on formal employment is decreasing in absolute terms in U_i, which is the same as for the $\widehat{MTE}$ on overall employment. It is positive for always-takers, close to zero for compliers, and negative for never-takers.
To sum up, I provide evidence that the small group of compliers in my setting is different from the group of always- and never-takers in terms of observable characteristics and potential outcomes. This challenges the one-to-one transferability of the effect for compliers to non-compliers. However, in further analysis, I can bound the effect of having more than two children on the propensity to be informally employed to be non-negative for all mothers in the sample under further assumptions.

1.8 Conclusions

Many low- and middle-income countries demonstrate a very dynamic evolution in both their fertility rates and their female employment levels over the past decades. However, the research on the relationship between these two variables is still surprisingly scarce. This research paper uses Mexican census data from 2010 in order to investigate the complex relationship between fertility and female labor supply. The endogeneity of fertility complicates disentangling causal effects of higher parity childbirth on labor market outcomes. The exogenous variation in fertility in my estimation strategy derives from parents' preference for mixed-sex siblings, as proposed by Angrist and Evans (1998). 2SLS estimates indicate that the compliant subpopulation of women significantly increases the probability of being employed due to an increase in the propensity to be in informal employment. I also find positive responses of working hours in formal employment at the intensive margin for a very small share of women. Further analyses provide suggestive evidence that an increase in the fertility beyond two children increases the propensity of living together with the maternal grandparents. Moreover, there is some indication that women in households with low wealth are the ones who especially increase their labor supply.
Declining fertility is often assumed to be an important shifter for female labor supply that is likely to account for part of the increase in women's labor force participation over the last decades. In contrast, the results of this study suggest that increased childbearing beyond the second child does not decrease female labor supply in a setting with low household wealth and high availability of informal child care and employment opportunities. Thus, policies targeting small family sizes are not a reasonable measure to increase the employment of women.
On the other hand, there might be ample scope for policies, if policy-makers are interested

in increasing the welfare of working mothers. It seems to be the case that women substantially decrease their leisure time to provide for an additional child by taking up employment in the informal sector. Informal jobs often provide low pay and inadequate social protection but may offer the only possibility to work due to inflexible working hours in the formal market. Public and corporate policies should encourage more flexible working contracts in the formal labor market, for example by providing the possibility of maternity leave,[33] access to workplace support, and part-time work to young mothers and fathers. Additionally, more flexible preschool and school schedules can help to combine employment in the formal sector and family responsibilities.

Talking about the interpretation and implications of the findings of this study, one has to keep in mind that the estimated effects in this research paper are specific to the parity of going from two to more than two children, which does not necessarily generalize. The compliant subpopulation of women for whom the effects are estimated is, moreover, very small and is likely not representative for the rest of the population. Still, under further assumptions, I can reject that there is a negative effect of an increase in fertility on overall employment for most women and on informal employment for all women in the sample.

[33]Mothers are currently obliged to take 12 weeks of maternity leave (six weeks before and six weeks after the birth). During this period, they receive benefits equal to their full wage if they work in the formal sector and meet some work requirements. Information retrieved from http://www.leavenetwork.org/introducing_the_network/; last access 05/2018.

Chapter 2

Job Displacement, Family Dynamics and Spousal Labor Supply

2.1 Introduction

An important economic motive for marriage is the opportunity to share risk within a couple. If one partner is affected by an unexpected shock, such as illness or job loss, the second partner can increase her labor supply as an insurance against a drop in household consumption. Other economic motives for marriage, such as the desire to have children and raise a family as well as the division of labor between home production and market work (Weiss, 1997), might, however, interfere with the risk sharing potential within marriage. For example, if preferences for spending time with children are unequally distributed in the couple, the spouses might not be willing to switch roles in response to an income shock. More generally, gender norms and role models might limit the flexibility of spouses to respond to changes in economic conditions.
From a policy perspective, the risk sharing potential of marriage is important, as strong intra-household insurance reduces the need for public insurance. Thus, the empirical literature has long sought to assess the importance of the risk sharing potential of marriage studying the so-called *added worker effect* (henceforth AWE). Early studies provide evidence of a negative correlation between the employment of married women and men across labor markets and over time (Mincer, 1962; Heckman and Macurdy, 1980), while later work focuses on the timing of spousal transitions between employment and unemployment within couples (Lundberg, 1985; Stephens, 2002; Juhn and Potter, 2007; Bredtmann et al., 2018). The findings from these studies are mixed, depending on the economic context and institutional framework. However, most studies indicate small employment responses by wives and little evidence for a substantial AWE.[34] In contrast to these empirical results, recent studies estimating structural life-cycle family labor supply models based on earnings and consumption data identify family labor

[34] See Appendix Table B.1 for an overview of cross-elasticity estimates in the literature.

supply as one of the major factors allowing married households to smooth consumption, even when they are facing persistent income shocks (Haan and Prowse, 2015; Blundell et al., 2016). The literature provides several arguments on why the risk sharing channel via family labor supply might be less relevant in practice. One is the generous availability of social insurance programs that crowd out self-insurance or family insurance (Cullen and Gruber, 2000; Autor et al., 2019). A second argument is correlated shocks at the household level, for example, due to economic recessions. Children and fixed gender roles within the household might also reduce the potential to share risk, but they receive comparably less attention in the literature. Blundell et al. (2018) address the importance of children in understanding family labor supply decisions over the life-cycle, within a unified model framework that captures the trade-offs between providing child care and insuring consumption against shocks within the household. Indeed, their findings confirm that families with children respond differently to income shocks than families without children.

In this paper, we try to disentangle the roles of different channels in the responses to income shocks within married households, paying special attention to the effects of children. Our evidence is based on a quasi-experimental setup of married couples in Austria, where the husband loses a job due to a plant closure or mass layoff. These layoff events provide credibly exogenous shocks to household income, allowing us to disregard problems with reverse causality. Also, the timing of the shock is precisely defined. A large literature documents persistent employment and earnings losses due to job displacement (Ruhm, 1991; Jacobson et al., 1993; Ichino et al., 2017; Lachowska et al., 2018). Thus, we have a setup in which couples face large, persistent, and unexpected shocks to household income, allowing us to explore the response of both partners around the time of the shock.

We show that, in the Austrian case, layoff events affect couples at different stages of the life-cycle. In particular, we observe many young couples with children, for whom we can study the trade-offs between insurance and child care. This is particularly interesting, as Austria is a very conservative society with strong gender identity norms (Akerlof and Kranton, 2000). The typical Austrian household follows the characterization of the male breadwinner model, where wives mostly enter the labor market as secondary earners and in part-time jobs (Bertrand et al., 2016). This social model is supported by Austrian welfare and family policies, which provide a generous parental leave system, but low levels of subsidized child care. As an illustration of the importance of gender norms and family values, Appendix Figure B.1 shows the share of individuals who agree with the assessment that "*a pre-school child is likely to suffer if his or her mother works*" for several countries. In this comparison, Austria stands out with more than a third of respondents who strongly agree. In Scandinavian and Anglo-Saxon countries, less than 10 percent of survey respondents agree with this statement. In terms of labor market institutions, Austria has a universal UI system and an individual based income tax system.

Our empirical analysis is based on detailed data from linked Austrian registers, which allow us to identify partners in marriages and divorces as well as plant closure and mass layoff events at the plant level. In total, we have a sample of about 48,000 married couples where the husband is laid off. The data indicate a strong specialization in market and household work within the

couples. Only 50% of wives are working before the husband loses the job and a large fraction of wives are working part-time.

We show that our setup of high volatility in female life-cycle labor supply profiles, with mothers dropping out from the labor force after childbirth for extended periods, requires a careful choice for a control group to measure responses to the displacement shock. In the empirical analysis, we use three different control groups to confirm the robustness of our results. Following the literature, the first control group consists of couples with the husband working in a firm without mass layoff or plant closure. The second control group consists of couples where the husband works in a plant with a mass layoff but is not laid off himself. The third control group exploits the randomness in the timing of displacement, following the strategy applied by Fadlon and Nielsen (2017). We compare outcomes in couples who marry in the same year, but in one case, the husband is displaced earlier than in the other, and we use the time between the two displacement events as counterfactual.

Our main results are remarkably consistent across the three control groups. We find that husbands lose on average 21 to 24% of earnings over five years after displacement and have a 16 to 17% lower employment rate relative to the control group. The labor supply responses of wives are positive and statistically significant, but small compared to the husbands' losses. On average, the female employment rate increases by 1% and earnings by about 2%. We find that wives mainly respond at the extensive margin and are more likely to enter the labor market if they were not employed before the husbands' job loss. The implied participation elasticity with respect to the husband's earnings shock is very small, roughly -0.04 in the full sample and -0.07 in the sample of wives not employed at displacement.

The intra-household insurance mechanism plays a negligible role compared to public insurance via government transfers and taxes, as the wives' labor supply recovers only a tiny fraction of the overall loss in household income. In particular, UI benefits cover the sizable initial drop in household income following job loss. However, due to time-limited benefit durations, the longer term losses in household income are not covered by government transfers.

Overall, these results indicate a small role of risk-sharing within married couples in Austria. To disentangle the importance of mechanisms that limit the risk-sharing potential, we consider several channels. First, we investigate the stability of the family structure with respect to the husband's job loss. If the shock leads to divorce or changes in fertility plans, this could explain the limited scope of the insurance mechanism. We find a small increase in the probability of divorce comparing displaced couples with couples where the husband works in a firm without mass layoff or plant closure. However, there is no increase in the divorce rate of displaced couples relative to those where the husband works in a plant with mass-layoff, indicating some spill-over effects. Furthermore, we do not see any effects of the husband's job loss on fertility, which indicates that couples are not willing to revise fertility plans.

Second, we investigate heterogeneity in responses by the age of the youngest child in the household. The wife's labor force participation before the husband's job displacement varies greatly in size by the age of children in the household. Women with very young kids below the age of 3 are mostly on parental leave, and only 18% of them are employed. In contrast, wives with

children above compulsory schooling age or without children have a much higher employment rate of 66%. We find that the most responsive group are mothers with children between ages 3 and 15, who increase their employment rates and earnings persistently after the husband's job loss. We find no response among mothers of very young children or women without children or with older children. This seems to imply that some trade-off in child care provided by the mother and by formal channels occurs, especially among women who bring forward their entry into the labor market after a maternity break. Notably, we find no evidence on substitution in child-care responsibilities between mothers and fathers of very young children for whom no formal child care is available.

Third, it could be the case that labor market shocks are correlated among wives and husbands. Assortative matching and the fact that they work in the same labor market could reduce employment opportunities for wives. Indeed, we do not find any female labor supply responses in couples where the husband loses the job in a market with a high unemployment rate. Even in markets with low unemployment, the additional earnings from the wife's employment covers just a tiny fraction of the total household income loss. We further find that wives with high earnings potential, i.e., those with high earnings before marriage, respond more strongly to the husband's job loss. In addition, the wife's labor supply response is stronger in couples, where the husband loses a well-paid job from a firm that pays above average wages to all their other workers. If labor market shocks within couples were strongly correlated, we would not expect to find heterogeneity along these two dimensions.

Our paper relates to the large literature on family labor supply and the long-term effects of job displacement, to which we contribute clean quasi-experimental evidence on the effects of job loss on family labor supply in married couples. We also contribute to the emerging literature on the role of social norms and gender identities in shaping labor market outcomes (Bertrand et al., 2016; Kleven et al., 2018). In our setup, we show that the traditional male breadwinner model of the family can severely limit the insurance potential of marriage. Further, we contribute to the literature on the motives of marriage and fertility (Weiss, 1997). In particular, we provide empirical evidence that in Austria, fertility decisions often precede marriage decisions.

The remainder of the paper is organized as follows: Section 2.2 discusses relevant aspects of the institutional setting. Section 2.3 introduces our data sources. We also discuss how we identify plant closures and mass-layoffs, subsequently providing descriptive statistics for the key outcome variables. Section 2.4 describes the life-cycle patterns of women of displaced husbands and motivates our three alternative quasi-experimental counterfactual scenarios. Section 2.5 outlines our estimation strategy. Section 2.6 presents our main estimation results along with a number of robustness checks and three extensions. First, we examine the consequences of the husband's displacement on households' disposable income by accounting for changes in taxes and benefits. Second, we explore the underlying mechanisms of the AWE that go beyond an income effect and affect the family structure. Third, we investigate heterogeneity in responses for different types of households. This last step helps us to understand the reasons for the limited responses by wives. The final Section 2.7 concludes the paper and discusses potential policy implications.

2.2 Institutional setting

In this section, we provide background information on several aspects of the institutional setting in Austria. This information helps to put our results into perspective.[35]

Trends in household formation Austria witnessed trends in marriage and fertility behavior that are quite comparable to other high-income countries. Both the age at first marriage and first birth have increased substantially over time, while other patterns have remained stable.

The vast majority of Austrian females will be married at some point in their lives and will give birth to at least one child. About 90 percent of females 45 years of age or older have been married at some point (see Census 1981, 1991, and 2001). An almost equal share of this age group gave birth to at least one child. The relative timing of marriage and first birth also remained constant. Most women give birth to their first child within the first two years following marriage. A sizeable (but declining) fraction of these women gives birth to a second child a couple of years later. The birth timing gives rise to drastic changes in women's labor market participation in the years following marriage, as we will see below.

Development of the female labor force participation In 1990, about 64 percent of all Austrian women between the ages of 25 and 54 were participating in the labor market. This rate has increased over time and, since the early 2000s, the female labor force participation has been consistently above 80 percent.[36]

However, even in 2018, the female participation rate is still well below the male rate of 92.5. Moreover, at any point in time, there is much more heterogeneity in the female than in the male participation rate. The most important dimensions predicting labor force participation are women's age, marital status, and the number and age of children. Married women with children, especially those with young children, are the group with the lowest participation rates (see Appendix Figure B.2).

Gender identity norms and beliefs about child care One potential explanation for the rather low participation rates of (married) women with children are prevailing gender identity norms and beliefs about the quality of child care. Using data from the *European and World Values Surveys*, Appendix Table B.2 shows that a large share of Austrians believe that "*a pre-school child is likely to suffer if his or her mother works*", while few agree with the statement that "*a working mother can establish just as warm and secure a relationship with her children as a mother who does not work*". A comparison with figures from other high-income countries reveals that Austrians hold a comparably high degree of conservatism toward gender roles and the labor force participation of mothers. In line with this, relatively few Austrians consider "*sharing household chores*", as "*important for [a] successful marriage*". This is supported by the evidence presented by Bertrand et al. (2016), who classify Austria, based on a series of measures of gender attitudes, as a high-sexism country. These patterns are not only very robust across subpopulations defined by sex and marital status, but also hardly change over

[35] Time-constrained readers may appreciate the five most important stylized facts at the end of this section and skip to Section 2.3.

[36] All figures are according to estimates of the *International Labour Office*, Source: *ILOSTAT Database*, last access 09/2016).

the available sample period from 1990 to 1999.

Maternity and parental leave policies Another explanation, for the rather low participation rates of (married) women with children, is the generous parental leave system. Austrian law mandates a compulsory maternity leave period of eight weeks before and after delivery for all working mothers (Lalive et al., 2014). Subsequently, eligible parents are entitled to paid and job-protected parental leave up to the child's third birthday. In the vast majority of the cases, it is the mother who takes the leave. Thus, almost all women leave the labor market at least temporary after the birth of a child, while a significant share also leaves the labor market permanently. The latter particularly applies to mothers with two or more children (see above).

Child care The Austrian system of formal child care distinguishes between facilities for children below the age of three (nurseries) and for those aged three to six (kindergarten). While the vast majority of communities have offered a kindergarten since the 1980s, the local availability of nurseries has been traditionally much lower. In 1995, only about 3 percent of communities had nurseries. These nurseries were predominantly located in more densely populated areas and covered about 35 percent of the total population. A widespread problem with both types of institutions are oversubscriptions, short opening hours (until noon) and long holidays.

Taxation of families The Austrian tax system follows the standard of individual income taxation, which means that partners in married couples are taxed separately. Thus, the entry tax rate for the second earner is lower, all other things equal, than in joint or family-based taxation systems. In addition, basic family allowances are rewarded universally and independent from the level or distribution of earnings (OECD Economic Surveys: Austria 2015). Both aspects of the tax system should promote dual-earner households. On the other hand, certain characteristics of the tax and benefit system work in favor of a single-earner household or a " 1.5 model". In particular, the quite high marginal tax wedge for medium incomes promotes part-time work.

Unemployment insurance In Austria, all private sector workers are automatically enrolled in the universal UI system. Eligibility for and duration of unemployment benefits depends on the individual's work history and age. UI payments replace around 55% of the previous net wage and are subject to a maximum and minimum. Job losers in our samples can receive UI benefits for 20 to 39 weeks. After exhausting regular unemployment benefits, job losers can obtain means-tested income support, unemployment assistance (UA), that pays a lower level of benefits indefinitely. Unemployment assistance is reduced euro for euro by the amount of any other family income (Card et al., 2007).

The five *most important stylized facts* are: First, within the typical Austrian couple, the man is still the primary earner. Second, women in the age range between 20 to 35 have complex employment patterns. This is particularly true in the initial years after marriage and first birth. Third, Austrians have on average very traditional views on gender roles and prefer mothers of (young) children not to participate in the labor market. Fourth, the supply of formal child care facilities for children below the age of three does not meet demand. Fifth, married couples are taxed individually.

2.3 Data sources, firm events, and descriptive statistics

Our empirical analysis is based on combined data from several administrative registers. Information on individual labor market careers is provided by the *Austrian Social Security Data (ASSD)*. This is a linked employer-employee database that covers the universe of Austrian workers in the private sector from 1972 onward (Zweimüller et al., 2009).[37] The data record individual employment spells on a daily basis along with an employer identifier, as well as individual earnings per calendar year and employer. In addition, the data include information on other social security relevant events such as unemployment, retirement, parental leave, and, in the case of women, births. Information on a worker's marital status and the identity of their partner is provided by the *Austrian Marriage Register* and the *Austrian Divorce Register*.

2.3.1 Plant closures and mass layoffs

We make use of the linked employer-employee structure of the ASSD to identify plant closures and mass layoffs. Our identification strategy relies on an approach investigating detailed flows of workers between employer identifiers and is described in Fink et al. (2010).[38] In particular, we organize plant level information from ASSD employment records in a quarterly panel measuring the number of blue- and white-collar employees at each employer identifier on February 10, May 10, August 10, and November 10 of each year.

Plant closures are observed in the quarter when an employer identifier vanishes from the ASSD. We analyze the flows of workers from the exiting identifier to subsequent employer identifiers to distinguish "true" closures from identifier reassignments or mergers with existing plants. We refer to the *closing quarter* as the last quarter in which the plant employs workers. To define our sample of closing plants, we consider all closures in the period from 1990 to 2007, restricting the sample to plants with at least five employees during the last four quarters of their existence.

Mass layoffs are defined by a similar approach. We identify large drops in plant size in the quarterly time series but exclude events in which a large group of employees moves to the same employer identifier. The exact thresholds to define a reduction in plant size between two quarters as a mass layoff is inspired by the Austrian system of advance layoff reporting. Employers planning to lay off an unusually large number of workers within the next month must provide advance notice to the employment office if the number of layoffs exceeds a threshold that depends on the size of the plant.[39] In analogy to the closing quarter, we define a *mass*

[37] The ASSD comprises only incomplete information on self-employed and civil servants (*Beamte*). Since we do not observe earnings for these two groups, we exclude them from our main analysis. Notably, the majority of persons employed with public authorities today are not civil servants, but so-called contractual civil servants (*Vertragsbedienstete*). Since we have precise information for this group, we include them in our main analysis.

[38] In the ASSD, we cannot distinguish between firms and establishments as there is no uniform rule for recording employer identifiers. As the vast majority of identifiers refers to small units, a plant in most cases will refer to an establishment (Fink et al., 2010).

[39] Our definition only considers plants with more than 10 employees in the quarter before the mass layoff and we apply the following rules for size reductions. In plants with 11 to 20 employees, the size must decline by at least three individuals; in plants with 21 to 100 employees, the size has to decline by a minimum of five individuals; in plants with 100-600 employees the size has to decrease by at least 5%. In firms with more than 600 employees, the number of employees between two quarters has to decline by at least 30 employees. In the robustness analysis in Appendix B.3, we present our main results with a more restrictive definition of mass

layoff quarter as the quarter immediately before the large drop in employment. In our sample, we consider all mass layoff events between 1990 and 2007. As the Austrian labor market is characterized by strong seasonality in employment, which makes it difficult to distinguish closures or mass layoffs from purely seasonal employment fluctuations, we exclude plants from sectors with a high share or seasonal employment (i. e. agriculture, construction, and tourism).

Restrictions on the sample of displaced workers At the individual level, we define workers as being affected by a plant closure if they are employed at a closing plant on the closure date or in the two preceding quarters. Workers affected by a mass layoff are employed on the mass layoff date, but leave the plant in the subsequent quarter. Our sample of displaced workers consists of men displaced by a plant closure or mass layoff, who have been married for at least two years, and who have at least one year of tenure at layoff. We further restrict the age at displacement to 25–55 for husbands and to 25–50 for wives, selecting the upper age limits to exclude transitions into early retirement.[40] Some individuals are displaced by firm events multiple times over their careers. We only consider the first displacement event for each husband, as subsequent outcomes might be influenced by the first displacement. We also drop couples who are displaced by the same firm event.[41] Our final sample comprises 18,466 couples, with the husband displaced by a plant closure and 30,027 couples with the husband displaced by mass layoff.[42]

2.3.2 Outcome variables and sample characteristics

The main outcome variables considered in our analysis are the employment and earnings of husbands and wives. We organize individual observations at the quarterly level and define employment by an indicator equal to one if the individual is employed at the quarter date (February 10, May 10, August 10, and November 10). Earnings refer to average monthly real earnings in Euro (2000 prices) over the quarter with the main employer. Note that the ASSD do not provide information on working hours. Thus, our earnings measure combines wages and hours. For each individual, we collect quarterly observations in the 5 years before and after the displacement. We define the individual *reference quarter* by the mass layoff quarter or closing quarter or by the quarter in which the individual is last employed in the case of workers, who leave before the closing quarter. In further analysis, we also analyze registered unemployment, receipt of UI benefits and unemployment assistance, household income, divorce, and fertility.

Table 2.1 presents the main descriptive characteristics measured in the reference quarter. Columns (1) and (2) list the plant closure and mass layoff samples, respectively. Both groups of displaced workers are quite similar in the personal characteristics of husbands and wives, but firm characteristics are different. Mass layoffs tend to happen in larger plants than closures

layoffs.

[40] Our data suggest that this age restriction is reasonable: Less than 1% of all husbands and wives in our sample receive pensions when they are last observed in our sample. On average, 0.7% and 0.5% of husbands and wives, respectively, receive pensions in any quarter in our sample period.

[41] 663 couples are affected by the same plant closure and 344 by the same mass layoff. Relative to all households that experience a plant closure (mass layoff) these are 3.47% and 1.13%, respectively.

[42] The highest numbers of displacements are observed in the late 1990s and early 2000s (see Figure B.3 in the Web Appendix). There is evidence of seasonality in the number of displacements with peaks in the fourth quarter of each year.

and plants with different industry and regional composition. Mass layoff plants also pay higher wages to their average workers. This is reflected in the difference in husbands' pre-displacement earnings of both groups.

Displaced couples in our sample are relatively young: husbands are on average aged about 39 years, and their wives are roughly 2.5 years younger. Note that the median age of husbands and wives is slightly younger than the mean. At displacement, the average couple has been married for 12 years (median is 11 years), and they have 1.4 children. Looking at the distribution of the age of the youngest child in the household, we can see that about 18% of couples have a child below the age of three, 57% have children between age 3 and 15, and roughly a quarter of households either have no child or children aged 16 or older.

Furthermore, the employment rate among wives before the husband's job displacement is low, with only 50% of wives working. If they are employed, their earnings are significantly lower than their husband's. On average, a working wife earns about 62% of her husband's earnings, which corresponds to 38% of the household's labor income. The large earning gap within couples can only be explained by a high share of part-time work among wives.

Table 2.1: Sample characteristics

	Displaced		Control	
	Closure (1)	Mass layoff (2)	Group 1 (3)	Group 2 (4)
I. Husband				
Age (yrs)	39.41	39.05	40.09	39.74
	[38.95]	[38.54]	[39.84]	[39.44]
	(6.75)	(6.79)	(6.63)	(6.67)
Experience in employment (yrs)	16.97	16.70	18.54	18.06
	[17.03]	[16.75]	[18.61]	[18.36]
	(6.77)	(6.72)	(6.61)	(6.46)
Tenure (yrs)	6.92	6.92	9.66	8.77
	[4.58]	[4.73]	[6.86]	[8.11]
	(6.24)	(6.06)	(6.91)	(6.70)
Number of previous jobs	4.44	4.11	2.90	3.14
	(4.34)	(4.17)	(3.29)	(3.49)
Number of previous mass layoffs	1.41	1.92	0.53	1.94
	(2.26)	(2.39)	(1.31)	(2.46)
Share blue collar	0.47	0.48	0.38	0.44
	(0.50)	(0.50)	(0.49)	(0.50)
Real Monthly Earnings (€)	2443.16	2500.61	2706.99	2672.92
	[2319.86]	[2455.63]	[2722.46]	[2649.97]
	(918.09)	(776.33)	(725.15)	(722.34)
Censored earnings	0.16	0.20	0.25	0.24
	(0.37)	(0.40)	(0.43)	(0.43)
II. Wife				
Age (yrs)	36.66	36.39	36.99	37.40
	[36.38]	[35.97]	[36.77]	[37.23]
	(6.14)	(6.20)	(6.14)	(6.13)
Experience in employment (yrs)	9.50	9.41	9.95	9.72
	[8.50]	[8.37]	[8.94]	[8.73]
	(6.15)	(6.06)	(6.28)	(6.19)
Number previous jobs	1.57	1.52	1.49	1.53
	(2.64)	(2.49)	(2.46)	(2.56)
Employed	0.49	0.50	0.50	0.50
	(0.50)	(0.50)	(0.50)	(0.50)
Blue collar | employed	0.31	0.31	0.28	0.31
	(0.46)	(0.46)	(0.45)	(0.46)
Real monthly earnings (€) | employed	1320.50	1343.11	1321.56	1320.63
	[1196.09]	[1232.67]	[1181.57]	[1207.13]
	(788.78)	(800.86)	(806.11)	(795.31)
Earnings rel. to husband | employed	0.63	0.61	0.52	0.53
	(0.67)	(0.66)	(0.39)	(0.44)
Censored earnings | employed	0.02	0.02	0.02	0.02
	(0.13)	(0.15)	(0.14)	(0.14)
III. Household composition				
Marriage duration (yrs)	12.20	12.00	13.06	12.84
	[11.20]	[10.93]	[12.40]	[12.13]
	(6.80)	(6.76)	(6.92)	(6.84)
Number of children	1.39	1.38	1.41	1.38
	(1.00)	(1.00)	(0.99)	(0.99)
Share with youngest child 0–2	0.18	0.19	0.16	0.16
	(0.38)	(0.39)	(0.37)	(0.37)
Share with youngest child 3–9	0.36	0.36	0.35	0.35
	(0.48)	(0.48)	(0.48)	(0.48)
Share with youngest child 10–16	0.20	0.20	0.22	0.22
	(0.40)	(0.40)	(0.41)	(0.41)

Continued on next page.

Table 2.1 — continued from previous page.

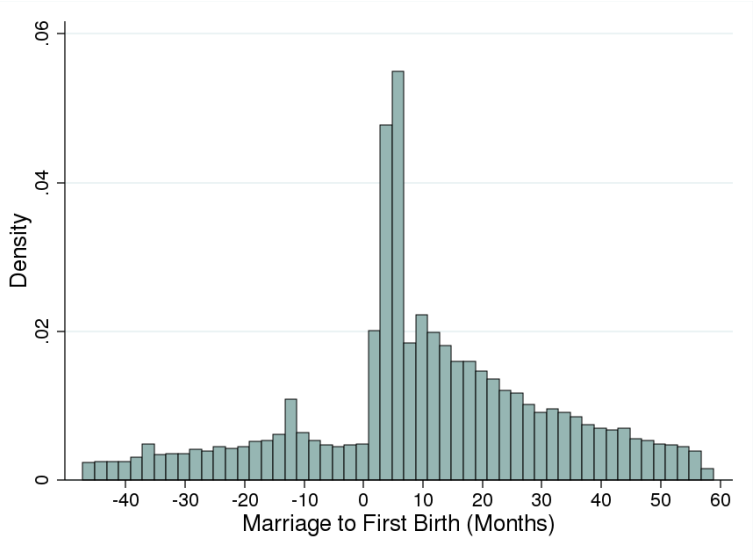
	Displaced		Control	
	Closure (1)	Mass layoff (2)	Group 1 (3)	Group 2 (4)
IV. Employer (husband)				
Firm size	51.94	244.39	397.15	326.87
	[20.00]	[138.00]	[135.00]	[239.00]
	(97.79)	(312.98)	(771.13)	(315.70)
Turnover	0.25	0.19	0.14	0.17
	[0.16]	[0.14]	[0.10]	[0.13]
	(0.34)	(0.24)	(0.22)	(0.19)
Mean monthly wage	1903.49	2072.28	2232.27	2160.57
	[1878.23]	[2025.60]	[2191.31]	[2106.37]
	(553.48)	(582.05)	(597.37)	(551.37)
Industry				
Manufacturing	0.41	0.46	0.47	0.59
	(0.49)	(0.50)	(0.50)	(0.49)
Sales	0.29	0.23	0.20	0.17
	(0.45)	(0.42)	(0.40)	(0.38)
Transport	0.10	0.06	0.06	0.05
	(0.30)	(0.24)	(0.23)	(0.22)
Services	0.19	0.25	0.28	0.19
	(0.40)	(0.43)	(0.45)	(0.39)
Region				
Vienna	0.22	0.24	0.15	0.20
	(0.41)	(0.43)	(0.36)	(0.40)
Eastern Austria w/o Vienna	0.22	0.20	0.20	0.22
	(0.41)	(0.40)	(0.40)	(0.41)
Southern Austria	0.21	0.19	0.20	0.22
	(0.40)	(0.39)	(0.40)	(0.41)
Western Austria	0.35	0.36	0.44	0.36
	(0.48)	(0.48)	(0.50)	(0.48)
Observations	18,466	30,027	58,516	61,360

Notes: Statistics depicted are means with standard deviations in parentheses. Medians are presented in brackets. Column (1) refers to households with a husband displaced through a plant closure, column (2) to those with a husband displaced through a mass layoff in the quarter after the reference date. Column (3) refers to a 10% random subsample of households with husbands without a firm event in the quarter after the reference date. Households in column (4) are a 40% random sample of non-displaced husbands employed at a firm where other workers are displaced from a mass layoff in the quarter following the reference date. There is one observation per household-event. All variables (except firm size, turnover, and mean monthly wage) are measured at the reference date (one year before the reference date, respectively). All households fulfill the following requirements: Husband and wife are aged 25–55 and 25–50, respectively, at the reference date. They are married for at least two years and husbands have at least one year of tenure at the reference date.

2.4 Family dynamics around displacement and definition of control groups

Fertility plans and the presence of young children typically affect household labor supply decisions. Therefore, we investigate marriage durations and the timing of first births in our sample of couples with displaced husbands. The mode of marriage durations in the sample is around 5 years, and the distribution has a long right tail. This implies that even though we only consider couples, who have been married for at least 2 years, the majority are relatively recently married

Figure 2.1: Distance between marriage and first birth



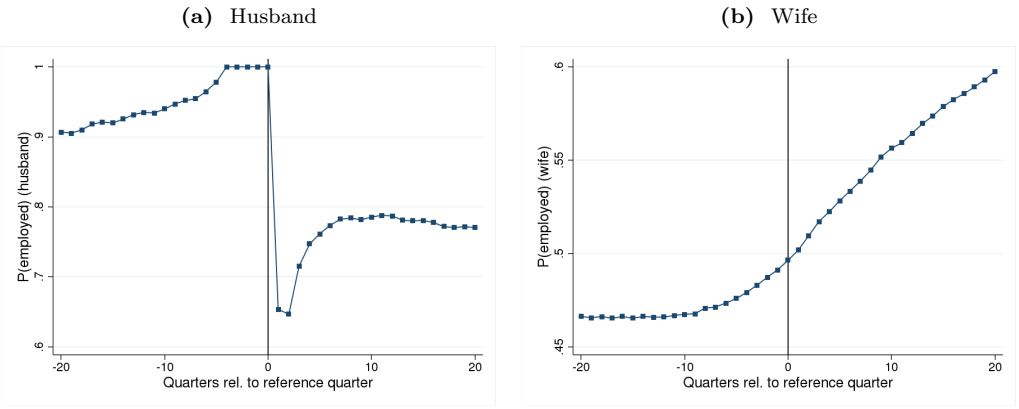
Notes: The figure displays the distribution of the distance from marriage to the birth of the first child in 2 months bins. The sample includes couples experiencing a displacement through a plant closure or a mass layoff. They are married for at least two years at the reference date. We include one observation per household event.

when the husband experiences the job displacement.[43] How quickly after marriage do couples have their first child? Figure 2.1 showing the distribution of the time between marriage and birth of the first child demonstrates that in Austria the marriage date is very strongly related to the birth of a child. While a few couples have their first child before marriage, we see a big spike in births 4 to 8 months after the marriage date and then a relatively long right tail. This pattern suggests that in many couples, marriage follows the fertility decision rather than the other way round. Overall, about 64 percent of first births occur within five years after marriage, and 30 percent occur in the first year. Due to the aforementioned generous Austrian parental leave system, fertility is also strongly related to female labor force participation. Together, the high prevalence of short marriage durations, the presence of young children in the household, and long parental leave periods imply that we observe the husband's job displacement shock during a period of high volatility in household labor supply. The next set of figures illustrates this argument by investigating the husband's and wife's employment around the displacement date.

Figure 2.2a plots the husband's employment probability around job displacement. We restrict displaced workers to be employed for at least one year at the plant closure or mass layoff event and, therefore, the graph shows full employment prior to the reference date and slightly lower employment rates in earlier years. After displacement, we see a sharp drop in employment of about 30 percentage points. This is followed by a quick recovery over the next 4 quarters. In the longer run, however, displaced workers cannot fully recover and their post-displacement

[43] The distribution of marriage durations at the reference date is shown in Appendix Figure B.4.

Figure 2.2: Employment of displaced husbands and their wives



Notes: Panel (a) and (b) show the mean employment probability around the reference date for all displaced men and their wives, respectively.

employment levels are about 20 to 25 percentage points below full employment.

Figure 2.3 examines the employment of the wives of displaced husbands. To point out the variation in female labor supply around childbirth and marriage, we plot employment rates for 5 groups with different marriage durations. The figure reveals substantial heterogeneity across groups. Starting with the group with the shortest marriage duration of 2 to 4 years, we can see that the average employment probability of women drops shortly after marriage — in line with the arrival of children — and then slowly recovers after maternity leave. This pattern is repeated in groups with longer marriage durations, by parallel shifts to the right of the wives' employment trajectories.[44] Thus, the life-cycle pattern creates a huge variation in female labor supply over time. Depending on the duration of the marriage, the wife's employment probability at the time of the husband's displacement varies between 40% and 50%, rising almost linearly for each group after the reference quarter. Prior to the husband's displacement, there is a lot of variation in the wife's employment across the different groups.

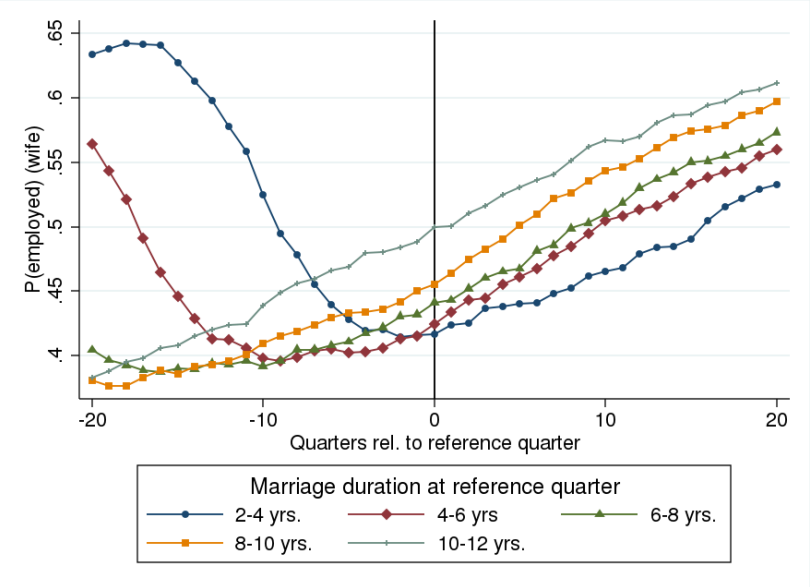
Figure 2.2b shows the average quarterly employment probability aggregated over all groups of wives. After having investigated different marriage cohorts, it is clear that the aggregate pattern of wives' employment rates is not at all informative about their response to husbands' job displacement.[45]

Because a simple event study design without a control group is highly sensitive to female life-cycle patterns, our empirical strategy relies on the choice of appropriate control groups of couples who did not suffer a job displacement. The idea is to compare labor market outcomes of

[44] Alternatively, we show in Appendix Figure B.5 the employment probability of wives around their husbands' displacement by the age of the youngest child in the household. Given the close relationship between marriage and fertility established above, the patterns look very similar.

[45] The latter interpretation is supported by Appendix Figure B.6. This graph shows quarterly means of the employment probability around displacement, while flexibly adjusting for marriage duration and the calendar quarter of observation. While husbands' employment results are hardly changed by the adjustment (see panel a), wives' results now show a very different pattern (see panel b). After the reference date the employment of wives still increases, but only by about 3 percentage points in the long-term. This indicates that the displacement effect on wives' employment is one order of magnitude smaller than that on husbands.

Figure 2.3: Wife's employment by different marriage durations



Notes: This figure shows the mean employment probability around the reference date for subsamples of wives of displaced husbands with different marriage durations at the reference quarter.

couples with and without displacement of the husband holding fixed the stage in the life-cycle. As we lack a design with full randomization of job displacements, we control for the complex counterfactual pattern in female employment using three different control groups: (i) households who are not affected by a firm event, (ii) households with husbands employed during a mass layoff, but not displaced themselves, and (iii) households who experience a displacement through a firm event in the near future.

Control group 1: Non-displaced husbands without a firm event. The first control group consists of couples fulfilling the same age, tenure, and marriage duration restrictions as our displaced sample. Husbands are employed at any reference quarter from 1990–2007 at firms that are not experiencing a closure or a mass layoff. Because this is a large group, where many couples are observed repeatedly, we draw a ten percent random sample. Workers in control group 1 are not affected by a displacement event, neither themselves nor in their plant. Table 2.1 column (3) reports descriptive statistics showing that their characteristics differ from those of displaced workers in terms of age, labor market experience, job stability, and earnings. Importantly, non-displaced workers are employed by larger firms that pay higher wages also to their average workers. Appendix Figure B.7 illustrates that firms that do not experience a mass layoff or closure are substantially larger and pay on average higher wages than event firms. Wives of non-displaced workers in control group 1 are slightly older than wives of displaced workers, but overall the difference in wives' characteristics are smaller than

among husbands.[46] The differences in observable characteristics between displaced couples and control group 1 couples gives rise to concerns that workers might be sorting into more and less risky firms and jobs also on the basis of unobservable characteristics.

Control group 2: Non-displaced husbands in mass layoff firms. To confront the concern of workers sorting into different firms, we define the second control group by husbands employed in mass layoff plants at the mass layoff date, who do not leave their employer in the subsequent quarter. As the number of non-layoff workers at the mass layoff plant typically exceeds the number of layoffs, we draw a 40% random sample of all observations. The reference date for this control group is defined by the mass layoff date.[47] Workers in control group 2 suffer a mass layoff at their plant but do not lose their jobs. As we see in column (4) of Table 2.1, these workers share average firm characteristics with workers displaced by mass layoffs. The mean firm size differs between columns (2) and (4) because larger firms tend to have more workers who survive a mass layoff event. With the definition of control group 2, we do not worry about selection into firms, but we might worry about selection into a layoff. Many firms apply " last-in first-out" or similar policies to determine mass layoffs (Sorensen, 2018). A further concern is that economic and psychological shocks related to a mass layoff can also affect non-displaced workers and their spouses, due to increased uncertainty or stress or because of a general deterioration of labor market conditions.[48]

Control group 3: Husbands displaced at a later date. For the third control group we do not sample individuals who were not displaced, rather, we exploit the timing of firm events and construct control groups of workers who are displaced themselves, but at a later date. Our approach is inspired by Fadlon and Nielsen (2017) and Hilger (2016), who exploit the timing of events to investigate the effects of spousal health shocks on employment and the effect of father's displacement on child outcomes, respectively. Under the assumption that the process determining involuntary job loss does not vary over time, workers who are displaced in later periods should not differ in unobserved characteristics from those who are displaced in the base period. Thus, the confounding effects of unobserved heterogeneity should be accounted for by a comparison of workers displaced at different times (Ruhm, 1991).

Our strategy to construct control group 3 is as follows. We start with a cohort of couples getting married in a fixed quarter and define households with husband displaced in a (reference) quarter h as the displaced group. The control group is given by the set of households in the same marriage cohort, who experience husband's displacement in the near future, in $h + \Delta$. We then assign a placebo shock at h to the households in the control group. It is important to hold the marriage date of the displaced and control group fixed to make sure that they are

[46] Family dynamics, i.e., the marriage duration at the reference date and the time between marriage and first birth, are similarly distributed for households that experience displacement and for those in the control groups. See Appendix Figures B.8 and B.9 for a comparison.

[47] We also exclude workers who are ever displaced from a plant closure or mass layoff over our observation period from control groups 1 and 2. However, individuals can be in the control group in more than one reference quarter. This happens for about 10% of the individuals in control group 1 and 26% of individuals in control group 2.

[48] Gathmann et al. (2018) show that mass layoffs worsen the local labor market situation in a causal way. They find that mass layoffs have sizeable negative effects on the regional economy, especially of firms in the same sector.

at the same stage of their life-cycle at date h. The choice of Δ is restricted by the trade-off between the length of the horizon over which we can observe post-displacement outcomes and the comparability of displaced and control couples. The two groups should be highly comparable if there is only a little time difference between displacements, i.e. if Δ is short. However, a short Δ also limits the period over which the counterfactual outcome can be observed. We experimented with values for Δ between 4 and 16 quarters, selecting only multiples of 4 because of the seasonality in mass layoffs and plant closures (see Appendix Figure B.3 and the robustness analysis summarized in Appendix B.3). As we do not find much evidence for reduced comparability, we present the main results for $\Delta = 16$.

We repeat the construction of the control group for every combination of the marriage quarter and reference quarter h and construct weights such that the displaced and control group size is balanced within each cell.

Due to the sample restrictions on marriage duration and tenure at displacement, we must put two additional restrictions on households in control group 3. This has implications for the comparability in the case of some of the outcome variables. First, the restriction on the husband's job tenure in control group 3 has to hold in quarter h and in quarter $h + \Delta$, which implies that there is full employment among husbands in control group 3 in the 4 quarters preceding $h+\Delta$. Therefore, we cannot directly compare the husband's employment and earnings outcomes in control group 3 with the displaced group. Second, due to the restriction on a marriage duration of at least 2 years prior to displacement, households in control group 3 are continuously married between h and $h+\Delta$. If job displacement has an effect on the probability of divorce, this cannot be measured by a comparison of couples with a displaced husband and couples in control group 3. We return to these arguments in the results section.

2.5 Estimation strategy

We measure the effects of the husband's job displacement by comparing outcome variables at the individual wife or husband level, as well as family outcomes for the displaced and control couples in the quarters before and after the reference date. In the results section, we present a set of graphical results that are quantified by regression estimates.

Our graphical results are based on the following regression model

$$Y_{ik} = \theta D_i + \sum_{l=-20}^{20} \gamma_l^q I\{k = l\} + \sum_{\substack{l=-20 \\ l\neq 0}}^{20} \delta_l^q D_i * I\{k = l\} + \upsilon_{ik}, \tag{2.1}$$

where Y_{ik} is the outcome of individual or household i in quarter $k \in [-20, 20]$[49], k measures the number of quarters relative to the reference quarter, D_i is an indicator equal to one if the husband is displaced at $k = 0$, $I\{.\}$ is the indicator function, and υ_{ik} is the error term. The parameter θ estimates the overall mean difference in the outcome between displaced and

[49] In the estimations with control group 3, we compare the displaced and control group only for four years around the reference date. Hence, l varies only from -16 to 16 in (2.1) and from -4 to 4 in (2.2).

controls, the parameters γ_l^q measure the quarterly time profile of the outcome in the control group and δ_l^q measure the difference in time profiles between the displaced and the control group relative to the reference quarter.

For the presentation of our quantitative estimation results, we average the difference between displaced and control individuals relative to the reference date over the 20 quarters after displacement. In addition, the model controls for the full set of industry and calendar quarter interactions, λ_{tj}. The model is given by

$$Y_{ik} = \theta D_i + \sum_{l=-20}^{20} \gamma_l^q I\{k = l\} + \sum_{l=-20}^{-1} \delta_l^q D_i * I\{k = l\} + \delta^{post} D_i * I\{k > 0\} + \lambda_{tj} + \upsilon_{ik}. \quad (2.2)$$

By construction, average household characteristics do not differ between control group 3 and the displaced households. However, we show in Table 2.1 that the other two control groups differ from displaced households in terms of their observed characteristics. To control for these differences, we apply a propensity score weighting strategy following Imbens (2004). In particular, we estimate flexible logit specification for the probability that the household is in the displaced group based on a large set of family and individual characteristics measured at the reference quarter and characteristics of the husband's employer one year prior to the reference date. A plant closure or mass layoff does not come as a complete surprise, and households might be able to foresee the event. To allow for responses of the wife in anticipation of the husband's displacement, we only control for the husband's time-invariant characteristics, his employment outcomes prior the reference date, his employer characteristics, and overall household characteristics at the reference date such as marriage duration and the number of children in yearly age categories. However, we do not condition on labor market outcomes of the wives before the reference date.[50]

Appendix Figure B.10 shows the distributions of the estimated propensity scores in the displaced group versus control group 1 and control group 2. The distributional overlap in pre-determined characteristics is closer between control group 2 and the displaced group than between control group 1 and the displaced. This is mainly due to the similarities in firm characteristics.

Based on predicted propensity scores from the logit models, we construct weights for members of the control groups such that the distribution of observable characteristics in each control

[50] We estimate the probability that the husband in a household is displaced by plant closure or mass layoff using a logit model separately for control groups 1 and 2 based on the following variables:

1. Husband characteristics: Interaction of year and season of displacement dummies, age (cubic), tenure in current job (dummies for deciles), employment experience (5 dummies), experience in unemployment (4 dummies), number of previous jobs (4 dummies), number of previous mass layoff events (7 dummies), indicator for blue-collar status in last job, and for the years -4, -3, -2 and -1 before the reference date: monthly wage, indicator for being employed and for being unemployed.
2. Wife characteristics: Labor market experience measured in last quarter of employment (5 dummies), age distance to husband (5 dummies).
3. Household characteristics: Marriage duration (30 dummies), number of children aged 0,1,2,...,12 (13 dummies) and total number of children under 18 at the reference date.
4. Husband's employer variables: Indicators for industry and region, firm age (16 dummies), firm age and industry interactions.

We impose common support. Based on the estimated propensity score $\hat{p}$, we assign control group households weights equal to $\frac{\hat{p}}{1-\hat{p}}$. The normalization ensures that the weights of the control group add up to 1.

group equals the distribution among displaced households. Using the weights, we estimate weighted regressions of equations 2.1 and 2.2. Hence, the estimated parameters reflect the treatment effect on the treated. In all weighted regressions, standard errors are bootstrapped (500 replications) with clustering at the household level.

2.6 Empirical results

To measure the shock of the husband's job loss on household income, we start by investigating the effect of job displacement on husband's employment and earnings outcomes up to five years after displacement. Next, we turn to labor supply responses of wives, reporting employment, earnings, and job search outcomes.

2.6.1 Husbands' employment and earnings responses

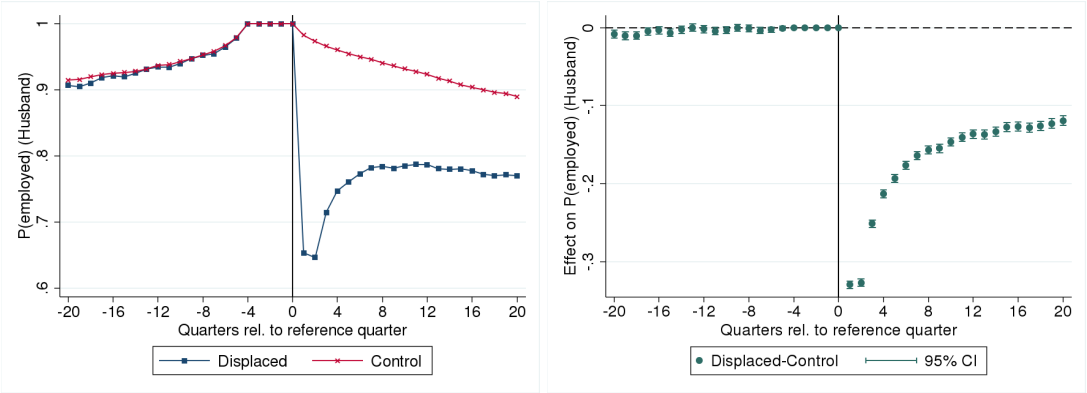
Figure 2.4 compares quarterly employment rates before and after job displacement for husbands in the displaced group and in control groups 1 and 2. The graphs on the left present employment profiles in the displaced group (blue line) and the control groups (red lines). The graphs on the right show the absolute difference between displaced and controls along with the corresponding 95% confidence intervals. A comparison across panels (a) and (b) confirms that the results do not differ by the choice of control group. Prior to job displacement, the weighted difference in employment rates is close to zero, but immediately after the event the employment rate in the displaced group drops by more than 30%. We see a rapid recovery in subsequent quarters, which stalls after about 3 to 4 years. Employment rates also decline in the control group after the reference date, but more gradually.

In Table 2.2, we summarize estimation results of equation (2.2) for the mean effects of job displacement on employment (in panel A) and earnings (in panel B). As we do not observe working hours in our data, we use monthly earnings in Euro (in 2000 prices) as the dependent variable and set the earnings of individuals who are not employed equal to zero. Columns (1) and (3) show the estimated effects for husbands referring to control group 1 and 2, respectively. The estimated coefficients of $Displaced \times Post$ report the difference between displaced and control individuals relative to the reference date averaged over the twenty quarters after displacement. Compared to the control group, displaced husbands suffer an average employment loss of about 16 to 17 percentage points over the first five years. The equivalent estimate for earnings amounts to, 22 to 25% of the pre-displacement mean earnings, depending on the control group. The relative magnitude of the earnings loss from job displacement mirror the husbands' employment losses, which indicates that lower employment rates are the main driver of earnings drops.[51] Appendix Tables B.3 and B.4 present the effects of the husband's job displacement on his labor market outcomes by year. This set of results confirms that employment and earnings of the

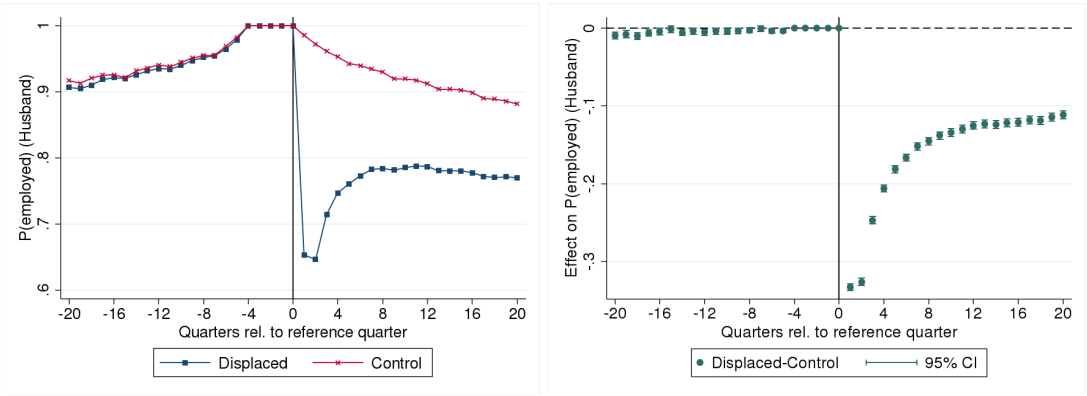
[51] The estimated employment effects are similar in magnitude to those reported for male Austrian workers displaced in the 1980s by Schwerdt et al. (2010). These estimated effects on male earnings are of comparable size to those reported in Jacobson et al. (1993) and slightly smaller than in Davis and von Wachter (2011) for the US. They are also a bit larger than those reported in Sullivan and von Wachter (2009) for Germany.

Figure 2.4: Employment of displaced husbands with control groups

(a) CG1: No firm event



(b) CG2: Non-displaced in mass layoff



Notes: Comparison of the probability to be employed of men that are displaced (blue, square) to men without firm event at the reference date (red, x) in panel (a) and to non-displaced men working in mass layoff firms at the reference date in (b) based on estimation equation (2.1). Control groups are reweighted to resemble the displaced group in time-invariant husband and wife characteristics, household composition, employment outcomes of the husband and characteristics of husband's employer (see Footnote 50 for details). The employment probability of the control group is adjusted by its mean difference relative to the displaced group. The graphs to the right plot the difference between the two lines with the corresponding 95% confidence interval.

displaced and control groups evolve similarly in the years prior to displacement. The largest employment and earnings losses occur in the first year after displacement, with a decreasing trend thereafter.

2.6.2 Wives' labor supply responses

2.6.2.1 Wives' employment and earnings

The graphs on the left-hand side in Figure 2.5 shows the employment rates of wives in the displaced group and each of the three control groups around the reference date. Irrespective of husbands' job loss, wives' employment rates in all groups follow the same upward sloping pattern, which confirms the importance of controlling for life-cycle profiles in female labor supply. Prior to the reference date, differences in employment rates between the displaced and

Table 2.2: Effects of husband's displacement on household labor market outcomes

	Control group 1		Control group 2		Control group 3
	Husband	Wife	Husband	Wife	Wife
	(1)	(2)	(3)	(4)	(5)
A. Employment					
Displaced×Post	−0.170	0.011	−0.162	0.008	0.006
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
$\eta^{\text{participation}}$		−0.043		−0.036	
		(0.011)		(0.010)	
Pre-event mean	1	0.490	1	0.486	0.468
B. Earnings					
Displaced×Post	−601.237	11.262	−542.034	9.245	13.064
	(6.473)	(3.789)	(5.819)	(3.402)	(4.402)
Pre-event mean	2458.082	658.549	2463.521	647.718	613.938
C. Job Search					
Displaced×Post		0.007		0.003	0.005
		(0.002)		(0.001)	(0.002)
Pre-event mean		0.041		0.041	0.039
Households	101,609		93,666		45,886
Observations	4,386,508		4,502,579		2,161,764

Notes: This table displays the impact of husband's displacement on household labor market outcomes based on equation (2.2), which includes displaced group, distance to event, and industry×quarter fixed effects. In panel A (C) the dependent variable is equal to one if the individual in household i is employed (unemployed) in a given quarter. In panel B it equals monthly earnings in Euro (2000 prices), with zeros for those not employed. Column (1) and (2) ((3) and (4)) compare individuals in households with a displacement to a reweighted control group with no firm event (with households in which husbands keep their jobs during a mass layoff). In column (5), we match to displaced households a control group of households from the same marriage cohort that experience displacement four years after the reference date. *Displaced×Post* measures the average difference in the outcome variable between displaced and control groups relative to the reference date in the twenty quarters after the reference quarter. $\eta^{\text{participation}}$ is the implied participation elasticity of wives with respect to the earnings of their husbands. *Pre-event mean* refers to the mean of the dependent variable in the year before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

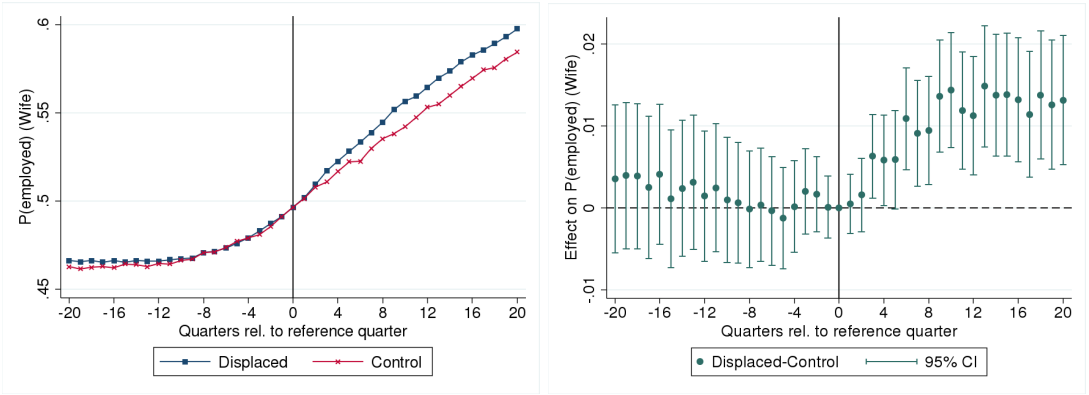
each of the control groups are close to zero (see the right hand side in Figure 2.5).[52] After the reference date a significant gap between the displaced and control groups opens and persists over the 5 year horizon. This gap is remarkably similar across all three control groups, which makes us confident that we can interpret it as the wife's labor supply response to the husband's job loss.

These findings are confirmed by the estimation results summarized in Table 2.2. The estimated effects shown in columns (2), (4), and (5) show that wives of displaced husbands increase

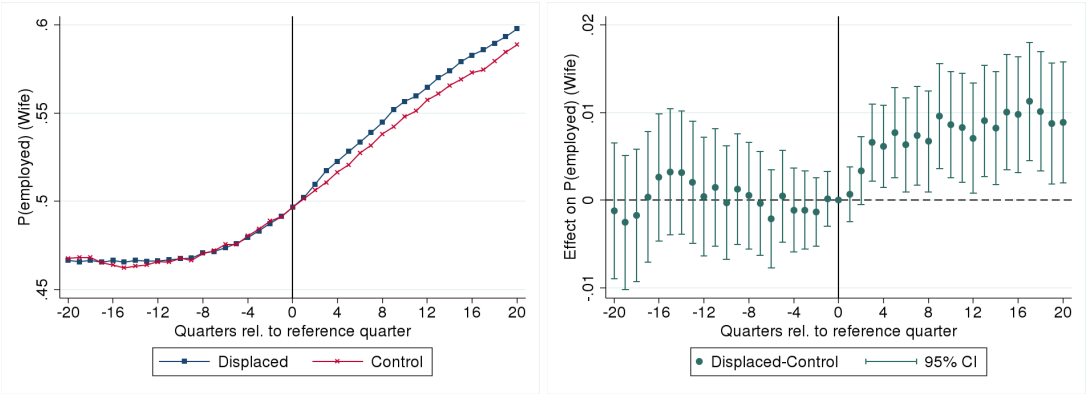
[52]Note that we adjust for differences in observed characteristics between the displaced group and control groups 1 and 2 by propensity score weighting on family, husband, and employer characteristics, which eliminates differences in wives' employment rates prior to the husbands' displacement. We do not correct for pre-displacement differences in observable characteristics between the displaced group and control group 3, as this control group is drawn from the same pool of couples and, thus, pre-displacement mean differences are zero by construction.

Figure 2.5: Employment of displaced husbands' wives with control groups

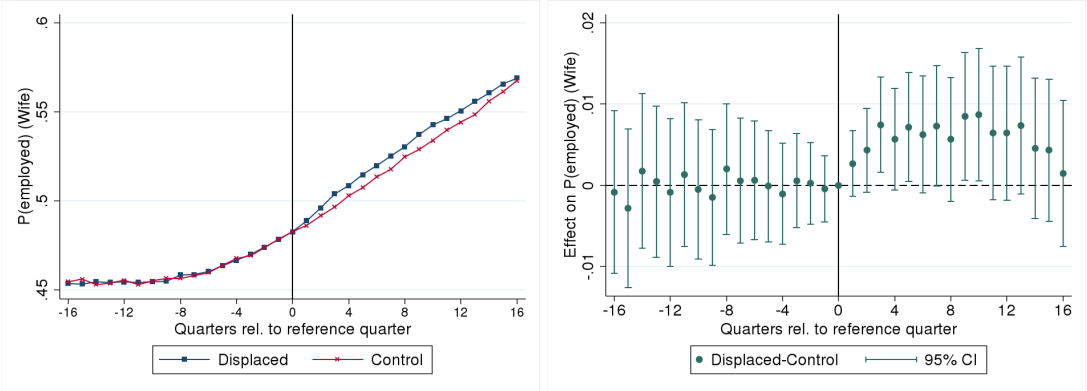
(a) CG1: No firm event



(b) CG2: Non-displaced in mass layoff



(c) CG3: Displaced in the future



Notes: Comparison of the probability to be employed of wives with displaced husbands (blue, square) to those with husbands without firm event at the reference date (red, x) in panel (a), with non-displaced husbands working in mass layoff firms at the reference date in (b), and with husbands displaced 16 quarters after the reference date in (c) based on estimation equation (2.1). CG1 and CG2 are reweighted to resemble the displaced group as explained in Figure 2.4. The employment probability of the control group is adjusted by its mean difference relative to the displaced group. The graphs to the right plot the difference between the two lines with the corresponding 95% confidence interval.

their employment on average by about one percentage point during the first twenty quarters after displacement (see panel A). While the employment effects are small, they are precisely estimated and highly robust to the choice of the control group. Compared to the displaced husbands' employment losses, the gains in wives' employment are small. Along with increases in employment, earnings increase by 1.5 and 2% (see panel B). The estimated effects are again similar across all three control groups. Comparing wives' earnings gains with husbands' earnings losses makes clear that the shift in labor supply within a household is hardly able to cover losses in household income.[53]

As explained in Section 2.3, the ASSD only records earnings consistently for employees in the private sector. To check the importance of self-employment as an alternative source of income after job displacement, we can examine the participation in self-employment. We find that self-employment increases among displaced husbands relatively rapidly after a job loss. However, the overall effect is rather small; five years after displacement, the self-employment rate is 5 percentage points higher among displaced husbands than in the control groups. The rate of self-employment is very low among wives in both the displaced and the control groups (see Appendix Figure B.11).

2.6.2.2 Anticipation of husbands' job displacement and job search

In the job displacement literature, which typically identifies job displacements from major firm events characterized by sudden drops in the employment level, it is difficult to deal with the anticipation of a worker's own job loss (Schwerdt et al., 2010). This is problematic in the light of Hendren (2017), who provides evidence from several sources that individuals have some knowledge about their future job loss. Evidence from married spouses offers an opportunity to assess the importance of anticipation at the household level, as the second spouse is not restricted to respond at a particular point in time and can start searching for a job before the first spouse is displaced. Here, we investigate job search and employment responses of wives prior to the husbands' displacement.

An important feature in Figures 2.5 is that the gap in wives' employment rates opens only *after* the husband's displacement. Thus, there is no evidence of wives' anticipation of the household shock, at least in terms of employment. This could be due to unawareness of the shock itself or its magnitude and persistence. However, job search takes time and wives' entry into employment could be delayed due to labor market frictions, even if they are aware of their husbands' job displacement in advance.

To confirm the lack of anticipation at the household level, we investigate responses in registered job search, as an alternative measure of the wife's labor supply that should be less affected by labor market frictions. In the ASSD, we observe job search by individuals who register as unemployed at the employment office. Registered individuals are not necessarily eligible for unemployment benefits, but can receive all job search counseling services. If the wife learns about her husband's planned job displacement, she can immediately register with the employ-

[53] Results for the effects of husbands' displacement on their wives' employment and earnings over time are provided in Appendix Tables B.3 and B.4. They confirm the patterns observed in Figure 2.5.

ment office. Thus, this measure should convey more direct information about the anticipation of the household shock.

In Figure 2.6, we plot the quarterly patterns of wife's registered unemployment. Let us first consider wives of displaced husbands, shown by the blue line in the graphs on the left. Job search rates among wives in the displaced group remain small and stable until one quarter prior to the husband's displacement. Job search rates start increasing in the final quarter before displacement and rise until the first quarter after displacement, thereafter they remain stable over the next five years. Thus, even in terms of job search, there is little evidence of anticipatory responses. Panels (a) to (c) consider the three different control groups. Among wives in control groups 1 and 3, we see no corresponding reactions. Their job search rates remain rather flat throughout. Wives in control group 2, whose husbands were not affected by the mass layoff in their plant, raise their job search rates with some delay after the reference date. This could indicate spillovers from the mass-layoff event to unaffected households, who react to rising uncertainty. The graphs on the right show the absolute difference between displaced and controls and provide a 95% confidence interval to assess statistical significance. Table 2.2 summarizes the mean effects for the twenty quarters after the reference date. Depending on the control group used, the estimated average difference in job search rates is between 0.3 and 0.7 percentage points. Given pre-treatment means of around 4 percent, these responses correspond to an increase in wives' job search by 7 to 17 percent.[54]

2.6.2.3 Intensive versus extensive margin labor supply responses

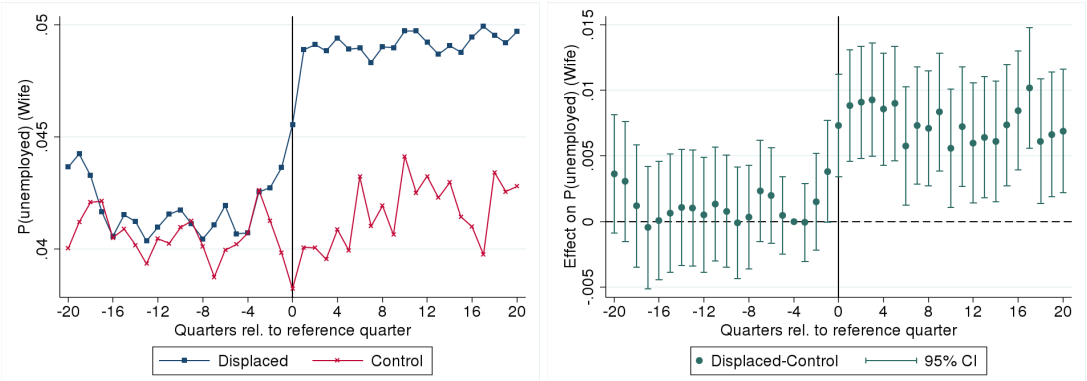
From the evidence in the previous section, we conclude that anticipation of the income shock due to the husband's displacement is moderate and does not affect the wife's labor supply prior to the displacement event. Given that, in the year when their husbands are displaced, only about 50% of wives in our sample participate in the labor force, this offers an opportunity to investigate whether wives' earnings respond at the intensive or the extensive margin. Put differently, we analyze to which extent already participating wives increase their working hours or switch to higher-paying jobs versus how many previously inactive wives join the labor force. In Table 2.1, we show that employed wives earn less than 40% of household labor income prior to the husband's displacement, probably due to part-time work. This means that in both groups of households, there should be room for labor supply responses, either on the intensive margin or at the extensive margin.

To identify the margin of response, we split the sample and distinguish between couples in which wives worked in the year before their husbands' job loss and those with inactive wives. Specifically, we define a woman as employed if she is employed in all four quarters before the reference date. As before, we weight each control group to resemble the observable characteristics of the displaced households and estimate equation (2.2) for each subgroup. Table 2.3 presents results by the wife's employment status before the reference quarter comparing women with displaced husbands with those in control groups 1 and 2. The estimated coefficients report the average

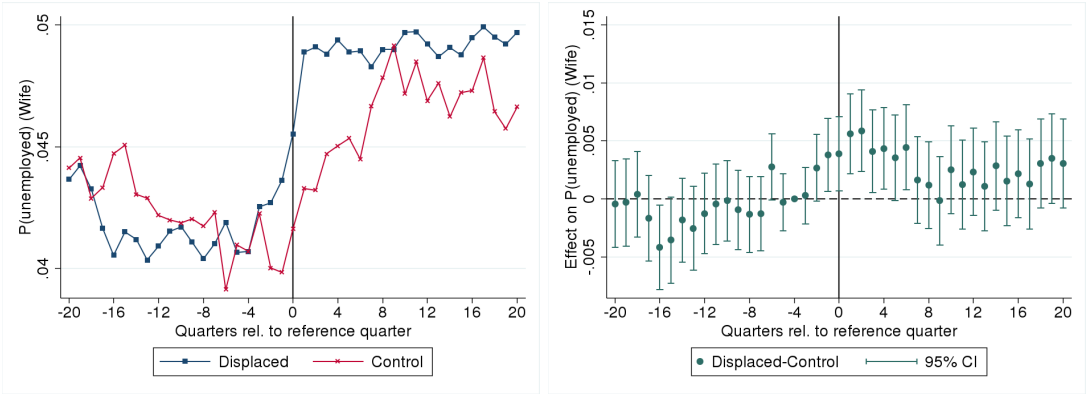
[54] Results for the effects of husbands' displacement on their wives' unemployment over time are provided in Appendix Table B.5.

Figure 2.6: Job search, probability of registered unemployment

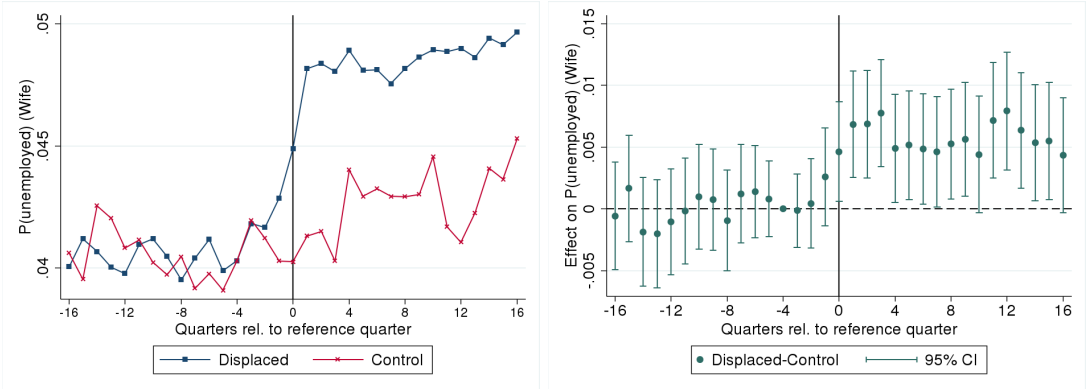
(a) CG1: No firm event



(b) CG2: Non-displaced in mass layoff



(c) CG3: Displaced in the future



Notes: Comparison of the probability to be unemployed of wives with displaced husbands (blue, square) to those with husbands without firm event at the reference date (red, x) in panel (a), with non-displaced husbands working in mass layoff firms at the reference date in (b), and with husbands displaced 16 quarters after the reference date in (c) based on an adapted version of estimation equation (2.1), in which we measure unemployment relative to its value in the quarter *one year before* the reference date. CG1 and CG2 are reweighted to resemble the displaced group as explained in Figure 2.4. The unemployment probability of the control group is adjusted by its mean difference relative to the displaced group. The graphs to the right plot the difference between the two lines with the corresponding 95% confidence interval.

Table 2.3: Displacement effects by wife's employment status prior reference date

Outcome	Wife employed			Wife not employed		
	Husband Earnings	Wife P(employed)	Earnings	Husband Earnings	Wife P(employed)	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
A. Control group 1						
Displaced×Post	−610.110	−0.008	−10.793	−595.601	0.019	22.378
	(9.853)	(0.003)	(6.060)	(8.120)	(0.004)	(5.002)
$\eta^{\text{participation}}$					−0.079	
					(0.016)	
Pre-event mean	2490.909	1	1376.356	2435.549	0.111	122.813
Households	43,366			59,165		
B. Control group 2						
Displaced×Post	−549.429	−0.005	−4.616	−536.020	0.015	16.652
	(8.979)	(0.003)	(5.207)	(7.539)	(0.003)	(4.479)
$\eta^{\text{participation}}$					−0.069	
					(0.015)	
Pre-event mean	2495.640	1	1365.551	2441.058	0.113	124.521
Households	40,492			55,237		

Notes: This table displays the impact of husband's displacement on own earnings, spousal employment and earnings by the employment status of the wife before the reference date. The left panel refers to the group of households in which the wife was employed in all four quarters before the reference date. The panel to the right refers to the group of households in which the wife was not employed in all four quarters before the reference date. Cluster-robust (at the household level) standard errors are bootstrapped (500 replications) and reported in parentheses.

difference between displaced and control groups relative to the reference date over the first five years after displacement.

Results in columns (1) and (4) of Table 2.3 show that earnings losses of husbands are similar in the two types of households. This indicates that the husband's labor supply after job displacement is independent of the wife's labor market status at displacement. Results for wives in columns (2), (3), (5), and (6) show that positive employment and earnings responses among wives are driven by couples, in which the wife was not working prior to the husband's job loss. Point estimates for the group of couples with wives employed in the year prior to husband's displacement are even negative, but small in magnitude and only marginally significant. Thus, we conclude that wives' labor supply responses are concentrated at the extensive margin, as wives who were not employed prior to husbands' displacement enter the labor market.

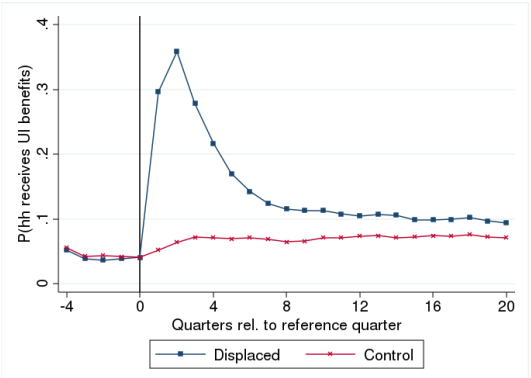
The interpretation of wives' labor supply responses to husbands' displacement as extensive margin responses allows us to compute a semi-elasticity of female labor force participation with respect to the husband's earnings. We relate the absolute change in the wife's employment rate to the husband's relative earnings loss averaging over the five years following job displacement for the group of couples with employed wives prior to the displacement shock. The estimated elasticity, $\eta^{\text{participation}}$, is reported in Table 2.3. Depending on the control group, the elasticity estimates range from -0.07 to -0.08. As about half of the total sample consists of couples with working wives, who are unresponsive to the husbands' job displacement, the corresponding participation elasticity for the full sample, reported in Table 2.2, is about half as big in absolute terms with -0.04, but still significantly different from zero.

2.6.3 Household income after displacement

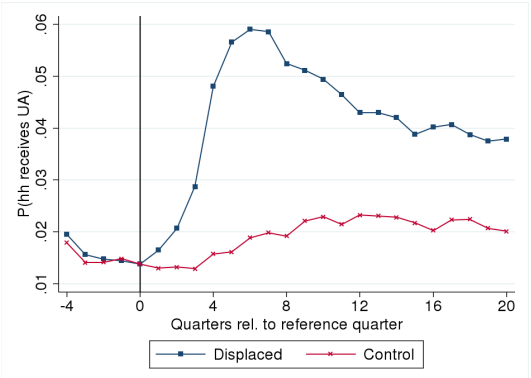
Next, we explore what fraction of the overall household earnings loss due to the husband's job displacement is covered by the tax and transfer system. If benefits are very generous and taxes progressive, intra-household insurance might be crowded out by public social insurance. In particular, we account for the role of income taxes and the receipt of unemployment benefits (UB) and unemployment assistance (UA) at the household level. In the data, net earnings and benefit income are only recorded from 2000 onward. As we want to observe outcomes for at least one year before the husband's job displacement, this part of the analysis focuses on households with a reference date of 2001 or later. As before, we weight couples in control groups 1 and 2 to have the same average predetermined characteristics as households in the displaced group.

Figure 2.7: Social benefits around displacement, CG1

(a) Probability that household receives unemployment insurance benefits (UB)



(b) Probability that household receives unemployment assistance (UA)



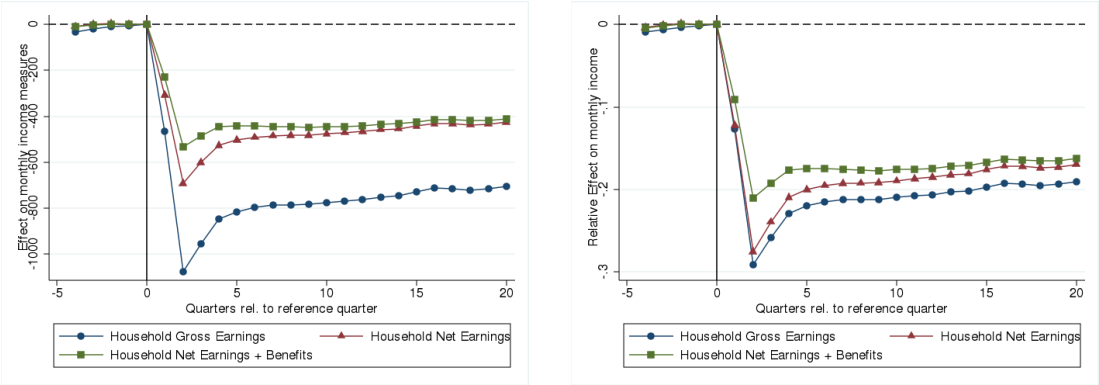
Notes: Comparison of the probability of receiving benefits of households with displaced husbands (blue, square) to those with husbands without firm event at the reference date (red, x). The control group is reweighted to resemble the displaced group within each subgroup as explained in Figure 2.4. The outcome variable of the control group is adjusted by its mean difference relative to the displaced group. With some exceptions, job losers can receive UB for up to 30 weeks. After exhausting UB, job losers can obtain means-tested income support, UA, that pays a lower level of benefits indefinitely.

Starting with benefit incomes, Figure 2.7 shows the quarterly probability that any household member receives unemployment benefits or unemployment assistance in graphs (a) and (b), respectively. The share of household receiving benefits is low prior to the displacement date, but in the displaced group UB receipt shoots up to more than 30% in the first few quarters following displacement. The potential duration of unemployment benefits is limited to 30 or 39 weeks for most unemployed workers in Austria, therefore we see a relatively sharp decline in the UI benefit rate after the initial quarters. In the long run, UI receipt is higher among the displaced households than in the control group, which can be explained with the lower stability of post-displacement jobs. Unemployment assistance benefits become available once UI expires, which is reflected in the delay with which UA receipt sets in after job displacement. However, note that the peak in the probability of receiving UA is at about 6%, which is much lower than the peak in UI. Only a relatively small fraction of households transit from UI to UA benefits after UI benefits exhaustion. The estimated effects summarized in Table 2.4 show that over

the first five years after job displacement, the average rate of UI benefit receipt is 8 percentage points higher in the displaced group and the average UA benefit receipt is 2 percentage points higher than in the control group. This already suggests that benefit income cannot fully cover the long-term earnings loss experienced by displaced households.[55]

Figure 2.8 shows the quarterly pattern of the estimated difference in household income between the displaced group and control group 1. The left panel plots the treatment effects in absolute terms, and the right panel provides a relative comparison to the corresponding pre-event level of household income. The blue line with the sharpest drop shows gross household labor earnings. This is the income measure we used, separately for husband and wife, in the analysis above.[56] Husband and wife's combined gross labor earnings drop sharply after the husband's displacement, recover in the next few quarters (see column 3 in Appendix Table B.6). The average difference over the five years after displacement is about 21 percent (see column 3 in Table 2.4).

Figure 2.8: Displacement effect on household income, CG1



Notes: This figure shows the effect of husband's displacement on monthly household income measures (in Euro, 2000 prices). The effect is given by the difference between households that experience a displacement and reweighted and mean-adjusted households that have husbands without any firm event at the reference date. *Household Gross Earnings* is the sum of husband's and wife's labor earnings in each quarter according to tax data. *Household Net Earnings* subtracts social security contributions and payroll taxes from the former. *Household Net Earnings + benefits* adds benefits from unemployment insurance and unemployment assistance.

The red line in Figure 2.8 shows net household labor income. After income taxes and social security contributions, the average absolute gap in household income between displaced and control groups is smaller than the gap in gross earnings. Due to progressive income taxation, the relative income gap is also smaller for net income and amounts to about 19% over the first five years (column 4 in Table 2.4). If we add UI and UA benefits received by the household to the net labor income, shown by the green line in Figure 2.8 and column 5 in Table 2.4),

[55] Appendix Figure B.12 replicates Figure 2.7 for control group 2. The Appendix Tables B.6 and B.7 report in the first two columns estimation results for the effects over time for control groups 1 and 2, respectively.

[56] Notice that the reported average household income measures and the effects of displacement on the former are larger than those for the sum of husband's and wife's gross earnings in Section 2.6. There are two reasons for that. First, we only look here at events in 2001–2007, whereas we previously considered events in 1990–2007. Appendix Figure B.7 shows that median real earnings were increasing over the relevant period. Hence, they are on average larger for later observations. Second, we use data from tax records for the income measures in this section, while we use earnings records from the ASSD in Section 2.6. The latter are top-coded at the maximum threshold for social security contributions; whereas the former are not.

Table 2.4: Effects of husband's displacement on household income

	Prob. of HH receiving		Monthly household income		
	UB	UA	Gross	Net	Net + benefits
	(1)	(2)	(3)	(4)	(5)
A. Control group 1					
Displaced×Post	0.077	0.023	−769.902	−474.298	−429.653
	(0.003)	(0.002)	(18.332)	(11.442)	(11.164)
Pre-event mean	0.040	0.015	3701.048	2515.338	2530.745
Households	40,771				
B. Control group 2					
Displaced×Post	0.066	0.021	−711.126	−441.015	−401.320
	(0.003)	(0.002)	(17.695)	(11.046)	(10.789)
Pre-event mean	0.040	0.015	3772.018	2553.295	2575.851
Households	34,031				

Notes: This table displays the impact of husband's displacement on household income measures based on (2.2) for the subsample of households with a reference date in 2001 or later. The dependent variable is equal to one if the household receives unemployment insurance benefits and unemployment assistance in column (1) and (2), respectively. In column (3), the outcome is household gross earnings (sum of the couple's labor earnings). Household net earnings in column (4) are gross earnings minus social security contributions and payroll taxes. In column (5), we add unemployment benefits and assistance to the former. All income variables are measured in Euro (2000 prices) on a monthly basis. In panel A (B) we compare individuals in households with a displacement to a reweighted control group of households with no firm event (with households in which husbands keep their jobs during a mass layoff). Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

we see that public social insurance primarily covers the large initial income shock suffered by displaced households, but it hardly affects household income in the long run. After five years, the red and green lines in Figure 2.8 almost overlap. See also columns (5) in Table 2.4 and in Appendix Table B.6.[57]

Overall, the Austrian tax and transfer system covers a larger fraction of the household income loss than the intra-household insurance mechanism, especially in the short run.

2.6.4 Effects of husband's job displacement on family structure

Husband's displacement may affect household outcomes other than his wife's labor supply. In particular, we consider fertility and divorce. These outcomes could be mediators that lie on the causal pathway between displacement, the associated negative income shock, and the wife's labor supply response. Alternatively, the female labor supply response could be a mediator in the causal effect of displacement on these other outcomes. Let us consider divorce, for example. Negative earnings shocks may cause divorce due to changes in the expected gains from marriage (Charles and Stephens, 2004; Rege et al., 2007; Eliason, 2012). This change in marital status could, in turn, affect women's labor supply behavior. Alternatively, the negative income shock due to displacement and the associated labor supply response of the wife might trigger a marital breakdown. In either case, the wife's labor supply adjustment and divorce are causally related to the husband's displacement, but the order in the causal chain differs. While a full mediation

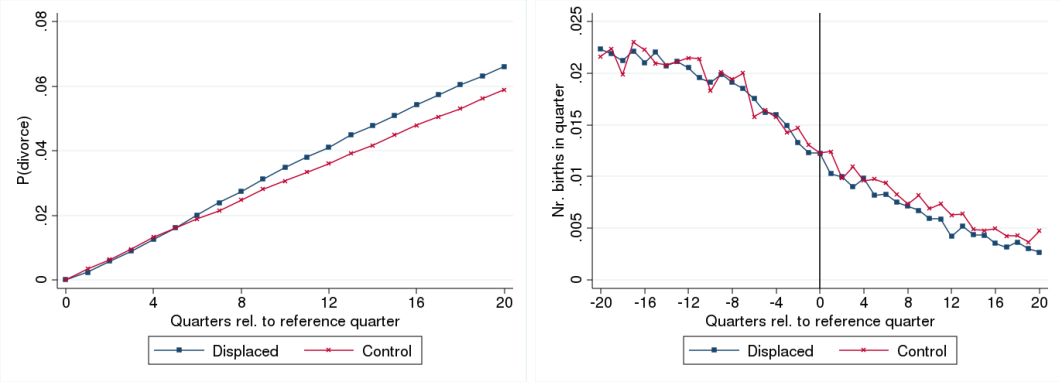
[57] Control group 2 provides very similar results, see also Appendix Figure B.13 and Appendix Table B.7.

analysis is beyond the scope of this paper, we investigate the effect of displacement on family stability and fertility to provide more context for the estimated effects in our main analysis.

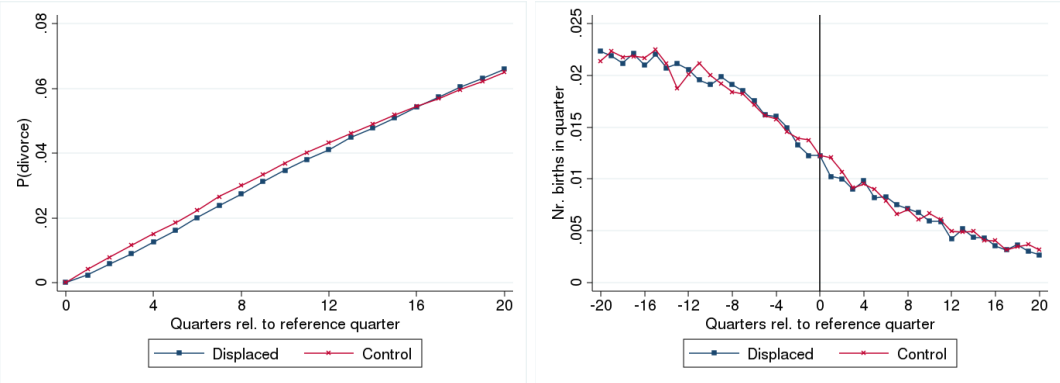
Divorce

Figure 2.9: Divorce and fertility around displacement

(a) CG1: No firm event



(b) CG2: Non-displaced in mass layoff



Notes: Comparison of the probability to live in divorce (left) and the number of births (right) for households with husbands experiencing a displacement (blue, square) to households with husbands without firm event (red, x) at the reference date in panel (a) and with non-displaced husbands working in mass layoff firms at the reference date in (b). CG1 and CG2 are reweighted to resemble the displaced group as explained in Figure 2.4. The number of births of the control group is adjusted by its mean difference relative to the displaced group. Divorce is only displayed after the reference date, since couples are required not to divorce until that date.

Our sample includes couples who have been married for at least 2 years at the reference date; thus, we investigate the probability of divorce in the subsequent years. The left panels of Figure 2.9 show the divorce rate over 20 quarters for the displaced group and for control groups 1 and 2.[58] In panel (a), we see a gradual increase in divorce probability among control group 1 couples, where husbands are employed in firms without mass-layoff or closure at the reference date. After five years, about 6% of these couples are divorced. Among couples with displaced husbands, the rise in the divorce probability is slightly steeper over the five-year

[58] In the case of divorce, control group 3 does by construction not provide a valid counterfactual. By assumption, control households remain married up to four years after the reference date.

horizon. However, the gap between both groups opens gradually, rather than immediately after the displacement shock. After five years, the divorce probability is about half a percentage point higher in the displaced group than in control group 1. This corresponds to an average difference in the probability of divorce of 0.04 percentage points, as shown in column (1) of Table 2.5. Interestingly, control group 2 couples, with husbands employed in mass layoff firms but not laid off themselves, face the same divorce rate patterns as the displaced group, which is shown in the left graph in panel (b) of Figure 2.9. These couples are potentially exposed to higher uncertainty and stress themselves, which may change their gains from marriage and affect their divorce decisions.

Overall, we do not find evidence of strong effects of husband's job displacement on divorce; thus, we conclude that husbands' job displacement is affecting relatively stable households whose partners share the income shock over a five-year period.[59] Marital stability after the displacement shock also implies the enforceability of intra-household insurance contracts.

Table 2.5: Effects of husband's displacement on divorce and fertility

	P(Divorce) (1)	No. of births (2)
A. Control group 1		
Displaced×Post	0.004	−0.001
	(0.001)	(0.001)
Pre-event mean	0.000	0.014
Households	101,609	
B. Control group 2		
Displaced×Post	−0.001	−0.000
	(0.001)	(0.001)
Pre-event mean	0.000	0.014
Households	93,666	

Notes: This table displays the impact of husband's displacement on the risk to be divorced in a given quarter in column (1) and the number of births per quarter in (2). The upper (lower) panel compare households with a displacement to a reweighted control group with no firm event (with households in which husbands keep their jobs during a mass layoff). Cluster-robust (at the household level) standard errors are bootstrapped (500 replications) and reported in parentheses.

Fertility

In Austria, fertility and women's labor supply decisions are strongly related, as we discuss above. Therefore, it is interesting to investigate whether the husband's displacement leads

[59] In the case of divorce, the Austrian divorce law may mandate some redistribution of income between the former spouses depending on the grounds of divorce. There are three main types of divorce: i. divorce by mutual consent; ii. divorce on the ground of fault; and iii. divorce on the grounds of irretrievable breakdown. Divorce by mutual consent is the simplest and cheapest way to obtain a divorce and is the most popular type of divorce. Since 1985, between 80 and 90% of all divorces were by mutual consent. In the case of this type of divorce, the law does not regulate alimony. However, an agreement on alimony is a condition to obtain such a divorce. In the case of the other types of divorce, typically the spouse whom the court found to be (solely or primarily) at fault must pay alimony to the other spouse if the latter does not have sufficient income or assets to live on. The amount of alimony depends on the spouses' financial circumstances. Spouses with no income of their own are entitled to 33% of the net income of the other spouse. Spouses who are employed are entitled to 40% of the common income, less their own income. Additional support obligations for children or another ex-spouse will reduce alimony payments by 3 to 4%.

to an adjustment of fertility decisions. The right-hand side panels of Figure 2.9 contrast the number of births per quarter in the displaced group versus control groups 1 and 2. Consistent with the evidence from Figure 2.1, fertility rates in our sample of married couples decline over time for all groups. At the reference quarter, about 1 in 100 women gives birth to a child. Given the low baseline fertility rate, it is perhaps not surprising that we find no indication of an impact of the husband's job loss on fertility. In Figure 2.9 fertility patterns in the displaced group follow the controls very closely. This is confirmed by the estimation results in column (2) of Table 2.5, which show a precise zero effect on fertility.[60] This result implies that households do not adjust fertility plans to cope with the income shock from the husband's job displacement.

2.6.5 Heterogeneity

Our results based on the full sample indicate that intra-household insurance against the husband's job displacement is almost negligible in Austria. To understand the reasons for the limited responses by wives and to identify impediments to the intra-household insurance mechanism, we investigate heterogeneity in responses for different types of households with the goal of identifying more and less responsive groups in the overall population. In particular, we seek to capture the impact of children on household labor supply decisions (Blundell et al., 2018), the role played by the earnings potential of the wife, by heterogeneity in the magnitude of the income shock (Lachowska et al., 2018), and by correlated shocks at the household level.

2.6.5.1 Heterogeneity by the age of youngest child

We document in Section 2.4 that labor supply patterns of young wives vary substantially over time and are largely determined by the timing of births. Thus, it is important to analyze how the wife's response to the husband's job displacement interacts with the presence of children in the household. To guide our analysis and the interpretation of results, we refer to the model of household labor supply with children introduced by Blundell et al. (2018). In this model, both partners in the household split their time between market work, child care provided at home, and leisure. Model estimates for the US indicate complementarity in husbands' and wives' leisure decisions, but substitutability in the spouses' time input in child care services. If the husband suffers a negative wage shock, this model predicts that the wife will increase her labor supply and, thus, partially insure the household against the income shock. If children are present in the household, two additional factors boost the wife's labor supply. First, as the husband's earnings drop and he works less, the husband takes over some of the wife's child care responsibilities at home. Second, the wife substitutes some of her time at home with the children with formal child care from outside of the household. Together these effects result in stronger predicted female labor supply responses in households with children.

[60] Existing evidence for Austria (Del Bono et al., 2012) points to small negative and not very robust effect of job displacement on the paternity of male workers in a sample that also includes non-married workers. In Finnish data, no effects are found (Huttunen and Kellokumpu, 2016). Notably, the focus of both studies is the effect of women's own displacement on subsequent fertility, which is found to be statistically significantly negative in both studies.

Now we translate the model predictions to the Austrian case, which is characterized by generous parental leave regulations, a scarce supply of formal child care for children below age 3, and by traditional gender roles within the household. According to the model, we expect the wife's labor supply responses to vary by the age of the child in the following way. First, a strong driver of labor supply responses among women with very young children should be the substitutability of home provided child care within the household. In this group, most mothers are on parental leave with the option of returning to their previous job; however with poor availability of formal child care. These households have the option to respond by spouses switching roles after the husband's job loss with the wife returning to her job and the husband taking over child care at home.

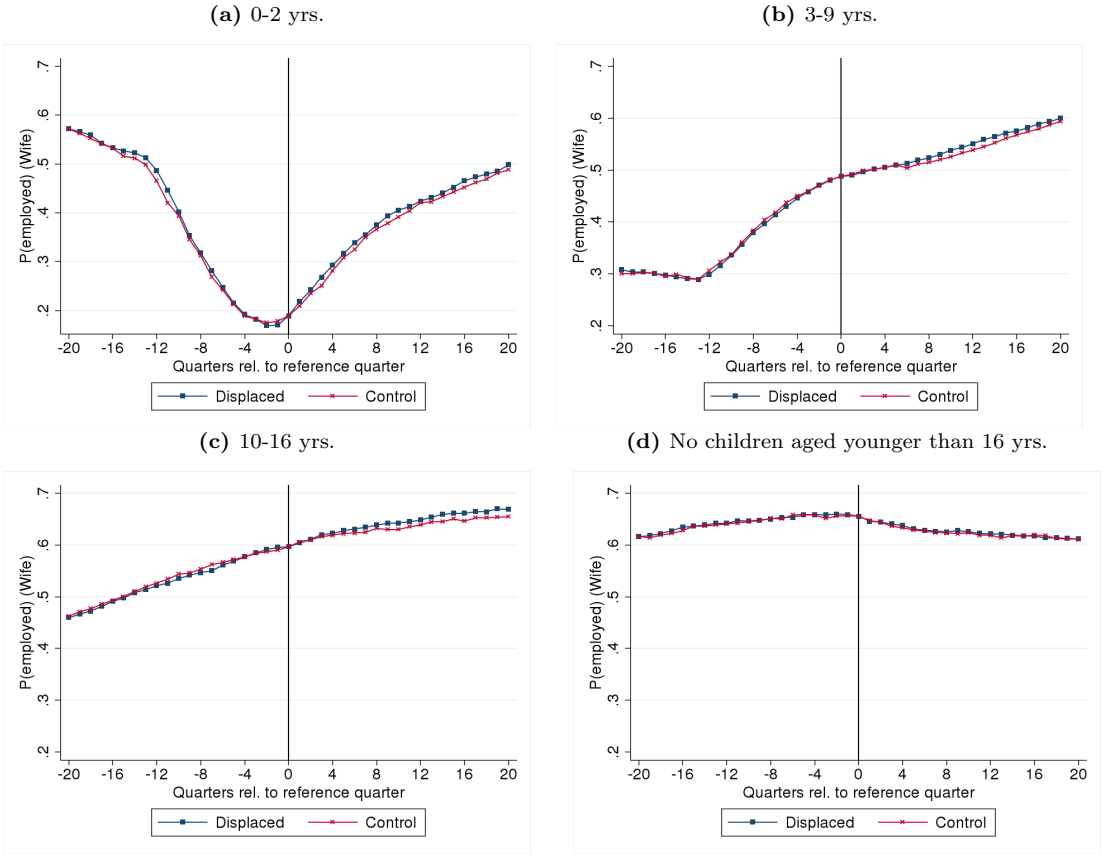
Second, in households with older children for whom formal child care is more widely available, mothers have the additional option of substituting their child-care time at home with child care outside the household, if they want to increase their labor supply. Third, among couples with children too old to require child care or without children, we should see wives' labor supply responses to the income loss after taking into account leisure complementarities with their husbands. A factor that might limit labor responses within all household is gender roles and differences in gender-specific preferences for spending time with children. This might be relevant in the Austrian case, where almost 40 percent of all Austrians agree that "*a pre-school child is likely to suffer if his or her mother works*" (see Appendix Figure B.1).

To test these predictions, we start by defining three categories of households with children below compulsory schooling age, where the youngest child is (a) 0–2 years old and parents are eligible for parental leave; (b) 3–9 years old; (c) 10-15 years old, and an extra fourth category (d) of households with no child or all children aged 16 years or older. As before, we weight the corresponding subsamples in control groups 1 and 2 to resemble the observable pre-determined characteristics of the displaced households for each category. Figure 2.10 plots employment rates of mothers in the displaced group and in control group 1 by the age of the youngest child.[61] Table 2.6, summarizes the corresponding estimation results of the average effects of husbands' job displacement on wives' employment probabilities for each of the three control groups in panels A to C.

A comparison of wives' average employment rates at the reference date across the four categories of households in Table 2.6 highlights the amount of heterogeneity in wives' labor supply over the life-cycle. Only 18% of the mothers of very young kids are employed at the reference date. If employed, they work few hours, as reflected in the wives' earnings, which are less than a third of the overall household labor earnings prior to the husband's job displacement. Wives' employment rates at the reference date rise with the age of the youngest child as mothers outgrow their maternity breaks. However, the wives' earnings are still low compared to their husbands', as wives on average contribute slightly more than a third of household labor income, if they are employed. We see the highest employment rates among wives, who have no children or all children above the compulsory schooling age; among those women the employment rate is 66% at the reference date and their share in household labor earnings is 41%, if they are

[61] Equivalent graphs for the other two control groups are provided in Appendix Figures B.14 and B.15.

Figure 2.10: Employment of displaced husbands' wives by age of the youngest child, CG1



Notes: Comparison of the probability to be employed of wives with displaced husbands (blue, square) to those with husbands without firm event at the reference date (red, x) for subgroups defined by the age of the youngest child at the reference date based on estimation equation (2.1). The control group is reweighted to resemble the displaced group within each subgroup as explained in Figure 2.4. The employment probability of the control group is adjusted by its mean difference relative to the displaced group.

employed.

The blue and red lines in Figure 2.10, show employment rates in the displaced group and control group, reflecting the wife's labor supply responses after the husband's job displacement. We can see small and positive employment gaps opening after the husband's displacement in panels (b) and (c) among mothers with a youngest child aged 3 and older. However, no gap appears for mothers with very young children in panel (a) or for wives without school-age children in panel (d). The graphical results are confirmed by estimates in Table 2.6. The response is close to zero and never statistically significant for the household category with very young children aged 0 to 2 in column (1). The wives' employment response increases in the groups with older children across all three control group comparisons in columns (2) and (3) where we see small positive and mostly statistically significant employment responses among couples with children aged 3 to 9 and 10 to 15. The corresponding participation elasticities, estimated for control groups 1 and 2 for which we can identify husbands' earnings losses, range between -0.03 and -0.07. In the fourth category of households without children of compulsory schooling age, column

Table 2.6: Wife's employment response by age of the youngest child

	0–2 years	3–9 years	10–15 years	None younger than 16 years
	(1)	(2)	(3)	(4)
A. Control group 1				
Displaced×Post	0.011	0.008	0.009	0.001
	(0.008)	(0.005)	(0.005)	(0.004)
$\eta^{\text{participation}}$	−0.054	−0.033	−0.034	−0.005
	(0.038)	(0.022)	(0.020)	(0.015)
Pre-event mean	0.182	0.466	0.584	0.659
Earnings rel. to husband | employed	0.491	0.514	0.539	0.709
Households	18,248	36,950	22,031	26,894
B. Control group 2				
Displaced×Post	0.005	0.007	0.015	0.005
	(0.007)	(0.004)	(0.004)	(0.004)
$\eta^{\text{participation}}$	−0.031	−0.035	−0.065	−0.020
	(0.034)	(0.019)	(0.019)	(0.016)
Pre-event mean	0.181	0.465	0.585	0.661
Earnings rel. to husband | employed	0.482	0.515	0.548	0.699
Households	17,623	34,883	20,560	25,153
C. Control group 3				
Displaced×Post	-0.001	0.013	0.011	0.006
	(0.008)	(0.005)	(0.007)	(0.007)
Pre-event mean	0.178	0.447	0.567	0.656
Households	11,927	20,619	10,844	11,536

Notes: This table displays the impact of husband's displacement on spousal employment for subgroups defined by the age of the youngest child at the reference date. The first (second) panel compare households with a displacement to a reweighted control group with no firm event (with households in which husbands keep their jobs during a mass layoff). The third panel compares the displaced group to a control group of households that experience displacement four years after that date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

(4), the wife's employment responses are precisely estimated zeros in all three control group comparisons. The corresponding participation elasticities are also close to zero.[62]

Overall, we find evidence for heterogeneity in the wife's labor supply response by the age of the youngest child. The only caveat is that the sample split reduces the number of observations and decreases statistical power; thus differences between columns are never statistically significant. If we interpret the estimates in the light of the predictions from the model by Blundell et al. (2018), we draw the following conclusions.

First, couples who are eligible for parental leave are unlikely to switch roles after the husband's job displacement, and the mother prefers to stay at home with the child in any case. Thus,

[62] Appendix Table B.8 reports detailed estimation results of the husband's earnings loss, wife's employment and earnings responses in each of the four categories of households using our three different comparison groups. These result document zero earnings responses among wives in the category with no children or all children aged 16 years or older, which confirms the absence of intensive margin labor supply responses even in the group of women with the highest employment rates.

there is no evidence for mothers and fathers substituting child care at home, at least among couples with children younger than three. For these children, the mother holds the main child-care responsibilities, even if the husband reduces his time in the labor market. Notably, in the sample of wives who are on parental leave at the time of the husband's displacement, we find no evidence for any employment response (these results are available on request).[63]
Second, the main respondents are mothers of children age 3 to 15, who still face child-care needs. These mothers respond to the trade-off between time spent on child care and time spent in the labor market after the husband's job displacement and substitute time at home with children and time in the labor market. Interestingly, this is also the group of wives on a strongly upward sloping profile in their life-cycle labor supply, as shown in Figure 2.3. These mothers are planning a return to the labor market after their maternity break, and their husbands' job loss might induce them to return sooner than otherwise, which is also in line with the evidence of extensive margin labor supply responses.
Third, we find smaller responses in the wife's labor supply to a permanent shock of the husband's wage for couples without children. This might not be surprising, given the relatively high employment rate of wives prior to the husband's job displacement in this category. The magnitude of effects in Austrian households is smaller than those reported by Blundell et al. (2018) for the US, as we discuss below.

2.6.5.2 Heterogeneity by wife's earnings potential

Next, we test whether the intra-household insurance mechanism is more important if the wife has a higher earnings potential or has a higher chance to cover the income loss. We use three different definitions of the wife's earnings potential: (i) relative earnings of wife and husband before marriage; (ii) years of wife's labor market experience before marriage; and (iii) wife's educational attainment. Information about education is, however, only available at the date of first birth and, thus, we can only measure education for mothers. Along each measure of earnings potential, we split the sample into two groups with high and low earnings potential and measure the responses in terms of the average husband's earnings, the wife's average probability of employment, and the wife's average earnings in the first 5 years after the husband's job displacement. Results comparing the displaced group with control group 1 are shown in Table 2.7.[64] For all three measures, the husbands' earning losses are slightly higher in the group of households with high wives' earnings potential, which might be due to assortative matching. However, there is also a clear difference in the wives' responses across both types of households. Wives with high earnings potential are more likely to be employed and have higher earnings after their husbands' job loss than wives with low earnings potential.

[63] In Austria, the labor supply of young mothers may not only be restricted by low substitutability of child-care time within the couple but also by the lack of formal child care. Therefore, we also check whether the mother's willingness to return to employment depends on the availability of formal child care for under three-year-olds. We split the sub-sample of mothers of young children by the availability of a nursery in the residential community. In neither subsample do we find a significant employment response among mothers (see Appendix Table B.9). However, it is hard to tell whether these results can be explained by selection into different types of communities or by the shortage of child-care slots in communities with existing facilities.

[64] Estimations results based on control group 2 provide similar results (see Appendix Table B.10).

The difference is strongest if we measure earnings potential by the wife's labor earnings relative to her husband's in the year prior to marriage. Wives who used to have well-paid jobs before marriage are twice as likely to be employed after their husbands' job loss than wives who had no job or low earnings. Their participation elasticity is -0.07. Further, their earnings increase significantly. However, even though wives with high earnings potential respond more strongly, their earnings gain is small relative to their husbands' earnings loss.

Table 2.7: Displacement effects by wife's earnings potential, CG1

		Low			High	
Outcome	Husband	Wife		Husband	Wife	
	Earnings	P(employed)	Earnings	Earnings	P(employed)	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
A. Measure 1: Earnings						
Displaced×Post	−558.957	0.008	7.241	−649.244	0.017	20.328
	(7.531)	(0.003)	(4.616)	(15.287)	(0.006)	(11.338)
$\eta^{\text{participation}}$		−0.035			−0.072	
		(0.014)			(0.025)	
Pre-event mean	2384.058	0.459	548.609	2711.692	0.580	1008.921
Households	68,925			20,959		
B. Measure 2: Experience						
Displaced×Post	−562.861	0.008	2.656	−598.032	0.012	16.496
	(9.725)	(0.004)	(6.365)	(9.077)	(0.004)	(6.238)
$\eta^{\text{participation}}$		−0.035			−0.049	
		(0.019)			(0.017)	
	2424.419	0.464	593.651	2491.314	0.510	714.223
Households	44,013			45,800		
C. Measure 3: Education						
Displaced×Post	−505.811	0.010	9.704	−659.886	0.015	16.063
	(8.900)	(0.004)	(5.478)	(12.267)	(0.005)	(9.059)
$\eta^{\text{participation}}$		−0.045			−0.063	
		(0.020)			(0.021)	
Pre-event mean	2306.676	0.405	468.030	2700.320	0.502	699.960
Households	43,822			29,762		

Notes: This table displays the impact of husband's displacement on own earnings, spousal employment and earnings by measures of wife's earnings potential. Measure 1: High indicates that the wife earned more than 33% of the wage of husbands in the year before marriage. Measure 2: High indicates above median experience compared to other wives in the year before marriage. Measure 3: High indicates that the completed education of the wife is beyond compulsory schooling and apprenticeship education. Pre-marriage wage and experience are only available for those married after 1974. Education is only available for women with children. Results based on control group 2 are in Table B.10. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

2.6.5.3 Heterogeneity by magnitude of the income shock

To investigate whether the wife's labor supply response varies by the magnitude of the income shock experienced by the household, we exploit variation in the average wage paid at the husband's pre-displacement firm. Card et al. (2013) document systematic differences in wage levels across employers that are unrelated to the worker's own productivity level. The idea is that an individual who loses a job in a firm that pays high wages to their average workers should

suffer a larger shock than an individual who loses a job in a firm that only pays moderate wages (Lachowska et al., 2018).

We define firm types by estimating employer-specific fixed effects from an AKM type wage decomposition (Abowd et al., 1999).[65] In panel A of Table 2.8, we distinguish between two groups of households where the husbands are displaced by firms with estimated fixed effects below (columns 1 to 3) versus above the median (columns 4 to 6) fixed effect. Results are shown for comparisons with control groups 1 and 2, respectively. As expected, husbands' average earnings losses in the first five years after displacement are larger, if they lose a job in a high-paying firm. Wives' labor supply responses are also significantly stronger in this group. A comparison of the wife's employment gain relative to the husband's earnings loss results in participation elasticities that are also larger for the group of households that suffer the larger income shock. The participation elasticity is -0.03 among households suffering a small shock and varies between -0.04 and -0.06 in the group with a large shock, depending on the control group.

2.6.5.4 Heterogeneity by local labor market conditions

The moderate female employment responses to the husband's job displacement could be due to correlated shocks affecting both partners. In a depressed labor market, every worker faces difficulties finding jobs. Even if secondary earners are willing to enter the labor market, there might be few job opportunities. To assess the potential impact of correlated shocks at the household level, we investigate the correlation between female and male labor market outcomes and present a heterogeneity analysis by predicted job opportunities for wives.

We start by investigating female and male local labor market conditions among the couples in our sample. Overall, we find that labor markets are strongly segregated by gender. Only 8% of couples where both partners are employed before the husband's displacement work in the same 4-digit industry. For control groups 1 and 2, we find similar rates of 10% and 8%, respectively. At the reference date, the correlation between occupation-specific male and female unemployment rates in the same district is positive, but not very large at 0.5. Again, this result is similar across displaced and control groups.

To evaluate the wife's response to the husband's displacement by local labor market conditions, we split our sample by male unemployment rates (measured in the district of the pre-displacement employer). Panel B of Table 2.8 summarizes estimation results of the effect of displacement on husband's earnings, and wife's employment and earnings. The first three columns refer to observations in districts with low unemployment, and the last three columns to those with high unemployment. The upper panel uses control group 1, while the lower panel focuses on control group 2. Husbands' average earnings losses are comparable across both types of local labor markets. However, we consistently find that in depressed labor markets, with male unemployment rates above the median, wives face indeed difficulties in entering the labor market. Their employment responses are small and insignificant. In contrast, in local labor

[65] See Haller (2017) for documentation of the wage decompositions in the ASSD.

Table 2.8: Displacement effects by plant wage level and unemployment rate at reference date

Outcome	Below median			Above median		
	Husband	Wife		Husband	Wife	
	Earnings	P(employed)	Earnings	Earnings	P(employed)	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
A. Subgroups by plant wage level at reference date						
Control group 1						
Displaced×Post	−485.974	0.007	1.737	−767.597	0.015	19.703
	(9.787)	(0.004)	(6.289)	(10.888)	(0.005)	(6.653)
$\eta^{\text{participation}}$		−0.032			−0.055	
		(0.018)			(0.017)	
Pre-event mean	2239.607	0.505	676.410	2785.463	0.515	711.476
Households	40,939			40,903		
Control group 2						
Displaced×Post	−466.395	0.006	2.484	−693.665	0.010	13.905
	(9.293)	(0.004)	(5.889)	(10.852)	(0.004)	(5.966)
$\eta^{\text{participation}}$		−0.028			−0.042	
		(0.018)			(0.015)	
Pre-event mean	2287.456	0.506	677.026	2796.831	0.507	692.800
Households	38,013			34,830		
B. Subgroups by male unemployment rate at reference date						
Control group 1						
Displaced×Post	−613.381	0.017	14.903	−587.012	0.006	9.024
	(9.652)	(0.004)	(5.747)	(8.486)	(0.004)	(5.245)
$\eta^{\text{participation}}$		−0.067			−0.025	
		(0.016)			(0.015)	
Pre-event mean	2463.174	0.466	607.385	2457.220	0.511	702.639
Households	50,906			51,311		
Control group 2						
Displaced×Post	−550.786	0.010	12.033	−540.268	0.006	6.975
	(8.944)	(0.003)	(4.662)	(8.183)	(0.003)	(5.086)
$\eta^{\text{participation}}$		−0.048			−0.026	
		(0.016)			(0.016)	
Pre-event mean	2478.804	0.465	605.749	2494.340	0.505	689.418
Households	46,973			46,544		

Notes: This table displays the impact of husband's displacement on own earnings, spousal employment and earnings for different subgroups. In panel A the wage level at plants are employer-specific fixed effects estimated based on the AKM approach (Abowd et al., 1999), and provided by Haller (2017). These estimates are available only after 1994. In panel B the male unemployment rate is measured at the husband's employment district in the year of the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

markets with male unemployment rates below the median, female employment and earnings respond positively.

2.6.6 Discussion and comparison to the literature

Our results for married couples hit by the husband's job loss indicate positive, but small, labor supply responses by wives predominantly at the extensive margin as wives enter the labor force after the husband's job loss. Among couples where the wife did not work when the husband

lost his job, we estimate a participation elasticity of -0.07, while among couples where the wife worked the response is zero. The heterogeneity analysis above identified certain groups of households with stronger responses. However, even among those groups, the participation elasticity of wives is around -0.07, and there is no group where the wife's labor supply response covers a significant share of the household's income loss.

How do the Austrian findings compare to the literature? In Appendix Table B.1, we collect elasticity estimates from three types of studies, categorized by the type of variation in the husband's earnings, which is used to identify the wife's labor supply response. They cover results from different countries, time periods, population groups, and they are based on both administrative as well as survey data. Most reported elasticities refer to the aggregate hours or earnings response, while some studies also distinguish between extensive and intensive margins. Most estimated elasticities are negative, but a few studies find elasticities with the opposite sign (Eliason, 2011; Hardoy and Schøne, 2014; Bredtmann et al., 2018). Interestingly, the studies reporting positive elasticities identify household labor supply responses from income variation due to a job displacement of the primary earner, taking an empirical approach similar to ours. A potential explanation for the overall negative labor supply response at the household could be correlated shocks or adverse labor market conditions for all household members, the so-called discouraged worker effect.

The average elasticity estimate across all studies that find evidence for an added worker effect is -0.4, which is an order of magnitude larger in absolute terms than our main estimates. Haan and Prowse (2015) is the only other study that finds a negative elasticity with an absolute value below -0.1. In a setup is similar to ours, Haan and Prowse (2015) estimate a structural model exploiting income variation from husbands' involuntary job loss based on data from Germany.[66] Blundell et al. (2018) report somewhat larger responses on the extensive than the intensive margin, especially among households with children. We can confirm this result, but what stands out in the Austrian case is the absence of evidence of intensive margin responses. Wives who already participated in the labor force when the husband was displaced, do not increase their labor earnings relative to the control groups. Given that most wives work part-time, this is a surprising finding. We also fail to find earnings responses in the group of women without children or children above the compulsory schooling age, who have the highest employment rates at the reference date. This seems to indicate that gender roles within the household are relatively fixed and even large shocks to husband's income are not able to reverse these patterns.

2.7 Conclusions

This paper investigates how different motives of marriage shape the labor market responses to an income shock within the family. If the insurance motive dominates, we would expect the second earner to increase her labor supply if the primary earner in the household loses his

[66] Unfortunately the paper does not report the earnings loss of the husband and we assume an earnings drop of 20% to calculate the elasticity.

job. If, however, other motives, such as child care or housework, are more important and the roles within the family are clearly defined, the responses to an income shock should be more moderate.

We test this hypothesis in a setup of married couples in Austria, where husbands lose their job from mass layoffs or plant closures. The setup allows for precise timing of the shock to the household and a clean quasi-experimental identification of the displacement effect. We document that the husband's job displacement leads to a large and persistent drop in his earnings and employment. The wife's employment responds positively, in line with the insurance motive, but the additional earnings generated by the wife only cover a very small fraction of the total income loss. Taxes and government transfers are far more important as insurance against income shocks, at least in the initial period following job displacement.

To find explanations for the low insurance value of female labor supply within the household, we investigate additional outcomes, such as job search, fertility, and divorce, and analyze the heterogeneity in responses by household characteristics. Our results indicate that gender roles, preferences for time spent with children, and availability of formal child care play a strong role in the wives' labor supply decisions. Wives and husbands are not willing to switch roles in the care of small children in response to a shift in relative wages when parental leave benefits are available, but child care outside the home is absent. Nor are wives without children, who are already participating in the labor market prior to the husband's income shock, willing to extend their hours and increase their earnings. The most responsive group are mothers of children aged 3 and older, who are in the process of reentering the labor market after a maternity break. These women are willing to bring the re-entry the labor market at higher rates.

In our heterogeneity analysis, we can identify certain groups of women who show stronger labor market responses to the husband's job loss. In particular, wives with higher earnings potential are able to cover a larger share of the household income loss, wives of husbands who lost well-paid jobs, and wives who face more favorable labor market conditions are more responsive. Overall, we find that the intra-household insurance mechanism is muted in Austria, compared to evidence from other countries. This may be explained by traditional gender norms that determine the role of women in the household in line with evidence by Bertrand et al. (2016), on the importance of the male breadwinner model, and by Kleven et al. (2018), on the impact of gender inequality in Denmark.

Based on these findings, we identify different types of policies that might strengthen the intra-household insurance channel. The first type of policies targets the re-entry of mothers into the labor market after a maternity period, by strengthening the job guarantee after parental leave (Lalive et al., 2014), expanding subsidized child care, and providing active labor market programs for mothers after a maternity break. The second type of policies targets fathers' involvement in child care at home, for example, by reserving part of parental leave benefits for fathers (daddy months). Finally, policies targeting unemployed workers directly should take the household situation into account and also extend job search counseling to wives of unemployed married men.

Chapter 3

Free Mobility of Labor - How are neighboring labor markets affected by the EU Eastern enlargement of 2004?

3.1 Introduction

In recent years, there is growing public and political opposition against the principle of free movement of labor within the European Union (EU). Concerns are particularly pronounced toward the mobility of individuals from Central, Eastern, and Southeastern European member states; among them the eight countries that joined the EU in 2004 (EU8).[67,68] The accession to the EU changed the migration perspectives of more than 70 million citizens of the EU8 countries permanently. Because of the large migration potential and substantially lower earnings levels in these countries, old EU member states feared a substantial increase in immigration with potentially adverse effects for the labor market opportunities of their residents. These worries were especially prominent in Austria due to its geographic proximity to the EU8 countries. As illustrated in Figure 3.1, Austria shares more than half of its border with four of the eight countries that joined the EU in 2004.

In this paper, we investigate how local labor markets are affected when receiving a large number of immigrant workers. For this purpose, we examine the effects of increased immigration to the Austrian eastern border region resulting from the EU enlargement of 2004.[69] We focus on the

[67] Some exemplary extracts from the Austrian press illustrate these discussions: http://derstandard.at/2000065798708/Lohndumping-Zuwanderung-aus-Osteuropa-massiv-gestiegen and http://wien.orf.at/news/stories/2757310/, last access 05/2019.

[68] The EU8 countries are Czechia, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia. Central, Eastern, and Southeastern European EU member countries further include Bulgaria and Romania (EU2) that followed in 2007 and Croatia that joined in 2013.

[69] According to our definition, the border region comprises the federal states that are neighbor at least one of the EU8 countries.

Figure 3.1: Austria and neighboring countries

Notes: Own illustration.

labor market outcomes of resident workers across municipalities and follow their employment and average earnings over time. In addition, we look at the labor demand side by investigating whether firms enter or exit regions that receive many immigrants.

By exploiting temporal and spatial variation across municipalities, we disentangle the effects of immigration from the general economic impacts of the EU enlargement. National governments of the old member states had the option of temporarily restricting access to their labor markets for citizens from the EU8 countries. As a result, the Austrian government opted for a seven-year transition period with limited labor market access. In our analysis, we take advantage of the fact that the accession of the EU8 countries and their entitlement to free access to the Austrian labor market happened at different points in time. We further exploit that within the Austrian border region, immigrants tend to enter the labor markets in municipalities that are close to their home country. Hereby, they can profit most from commuting, which is attractive given the substantial differences in earnings and the cost of living between the countries.

In our empirical design, we compare differences in outcomes in municipalities across three groups that differ in their commuting distance to the closest EU8 border over time and contrast them to the corresponding differences in the year before the EU Eastern enlargement. We distinguish between three time periods: the period before EU8 countries entered the EU (May 1997 to April 2004), the transition period with restricted labor market access to Austria (May 2004 to April 2011), and the period after the introduction of free entry to the Austrian labor market (May 2011 to April 2016).

Our analysis is based on Austrian social security data, which provides us with information on the universe of private sector employment in Austria. Based on this data, we create a balanced yearly panel of municipalities between May 1997 and April 2016. For each municipality and

year, we measure the labor market outcomes for different groups of workers. Based on their citizenship, we distinguish between Austrians, immigrants from EU8 countries, and immigrants from other foreign countries (non-EU8 foreigners) and within these groups by gender, occupation (blue- and white-collar), and age (below 30, between 30 and 45, above 45). On the employer side, we measure the number of firms with more than 5 employees. In addition, we measure the number of firms that predominantly employ workers with either Austrian, EU8, or other foreign citizenship.

First, we document that the employment of EU8 nationals in the Austrian border region grew moderately before accession and during the transition period. With free labor market access, the trend changed markedly, and growth in EU8 employment picked up. Over the period from 1997 to 2015, the employment share of EU8 nationals in the Austrian border region increased from 2.2% to 9.3%. Comparing workers across the three nationality groups, we show that EU8 workers tend to be similar to other non-Austrian workers in terms of their age, sex, and type of occupation. Notably, approximately 70% of EU8 workers are males, and almost all are employed in blue-collar occupations, which is well above the percentages among Austrian workers. Moreover, Austrian nationals receive the highest remuneration, which is well above that for non-EU8 foreign workers, which is in turn higher than for EU8 workers. With free access to the Austrian labor market, the composition of EU8 employees in Austria changed discontinuously toward younger, male, and lower-paid workers. EU8 employees are most represented in seasonal industries such as agriculture, tourism, and construction, which points to an important potential role for temporary and seasonal migration. The tourism and construction sectors experienced the largest increase in their EU8 worker share from 1997 to 2015.

Second, comparing municipalities within the Austrian border region, we show that the increase in EU8 employment after 2004 is larger in municipalities within 0 to 20 minutes distance to the closest EU8 border than within 20 to 60 minutes, which is again larger than in those with more than one hour distance. This increase is almost entirely driven by an increase in blue-collar employment in the age group 30 to 45. For Austrian nationals, we do not find that employment evolved differently, on average, in municipalities that are within 0 to 20 minutes travel distance to the border compared to those further away. In contrast, municipalities that are within 20 to 60 minutes travel distance to the border experience a significant increase in the employment of Austrians, which is driven by an increasing number of white-collar workers. For non-EU8 foreign workers, average employment decreases significantly and steadily in municipalities that are within 0 to 20 minutes to the border after 2004. These decreases are larger for blue-collar and male employees.

Further, we find that real monthly earnings of Austrian and non-EU8 foreign workers change in opposite directions in municipalities within a one hour distance to the EU8 border after 2004. While Austrians experience an increase of approximately 2% in their earnings, non-EU8 foreign workers face substantial decreases of almost 5% from 2003 to 2015 relative to the reference group. In contrast to employment, these effects are very similar across municipalities in 0 to 20 minutes and 20 to 60 minutes border distance.

Third, turning to the labor demand side in municipalities within 0 to 20 minutes distance to the border, we show that the number of firms increases before 2004, but not during the transition period. After 2011, there is an, albeit not significant, increase in the total number of firms. Regarding the number of firms with a mainly EU8 workforce, we observe a large and significant increase before 2004, during the transition period and, in particular, after 2011. By 2015, there is, on average, one more firm with a predominantly EU8 workforce in municipalities very close to the border relative to those further away. For municipalities within 20 to 60 minutes travel distance from the border, we observe a positive effect of the EU Eastern enlargement on the total number of firms. This increase is mainly due to a rise in firms with a predominantly blue-collar workforce and to a minor part due to firms with a mainly EU8 workforce.

A large literature on the effect of immigration on native labor market outcomes documents mixed results. These differences arise partly because of different empirical specifications (Dustmann et al., 2016) and distinct empirical identification approaches. Our study is related to an increasing number of studies that exploit the design of migration policies or policy changes in order to identify the labor market impacts of immigrants.[70] Most closely related to this study, two papers exploit the regional variation in immigration by distance to a border in combination with changes in migration policies in other European countries. Dustmann et al. (2017) analyze the temporary opening of parts of the German labor market to Czech commuters in the early 1990s. They find that the inflow of immigrant workers had a substantial negative effect on natives' employment through reductions in inflows into local labor markets and moderate negative wage effects. Beerli et al. (2018) find positive employment effects when Switzerland opened its labor market to European cross-border workers in the 2000s. The comparison of these two studies already points to the potential importance of context-specific factors. Our study contributes to this literature by furthering our understanding of the relationship between immigration and labor market outcomes in a unique context. The EU Eastern enlargement and the implied suspension of immigration restrictions between old and new member countries were anticipated and permanent. In the Austrian case, there was even a 7-year transition period that provided substantial time for workers and firms to adapt their behavior. We can investigate this unique setting with an extraordinary administrative data set, which allows us to observe the universe of private-sector employers and their employees in Austria.[71]

Our paper contributes to the understanding of the role of the labor demand side in the effects of immigration by investigating whether firms enter markets that receive many immigrants. Firms might find it attractive to enter the labor market by relocating or by creating new

[70] Among these studies are Glitz (2012) and Foged and Peri (2016), who exploit the quasi-experimental nature of dispersal policies for immigrants to Germany and Denmark, respectively. Prantl and Spitz-Oener (2014) study an unexpected rise in migration from East to West Germany resulting from the German reunification. Monras et al. (2018) analyze the impact of the unexpected legalization of 600,000 immigrants on the employment of low- and high-skilled workers in Spain.

[71] We further highlight that the impact of immigration differs substantially between the group of Austrians and the group of other foreign workers and can even go in opposite directions. The latter group is on average similar to the EU8 migrant workers in terms of their observable characteristics and therefore more vulnerable to substitution. In addition, our results rather point to a complementary relationship between the group of EU8 and Austrian employees. Heterogeneities in the effect of immigration on workers across education and skill groups are established, for example, by Ottaviano and Peri (2012), Dustmann et al. (2017), Beerli et al. (2018), and Monras et al. (2018)).

establishments when there is an increase in the supply of labor. As a result of this, they might alleviate the pressure on residents' employment and wages. Firms' responses to changes in immigration policies are relatively underexplored so far. Olney (2013) applies a shift-share approach and finds that low-skilled immigration to US cities leads to a significant increase in the number of establishments. Also relying on a shift-share approach, Dustmann and Glitz (2015) show that large immigrant inflows to West Germany cause within-firm changes in factor intensity and a net creation of new firms. Jahn and Steinhardt (2018) find that the inflow of ethnic Germans positively affects overall regional net firm formation by exploiting a placement policy in post-unification Germany. Finally, Beerli et al. (2018) show that the inflow of skilled workers stimulates firm and productivity growth of incumbents, increases innovations, and leads to the entry of new firms. In contrast to these studies, we analyze the labor market outcomes of workers and firms simultaneously using matched employer-employee data. This contrasts with Beerli et al. (2018), who rely on different surveys when analyzing workers' and firms' outcomes. Last, but not least, understanding the labor market effects from the EU Eastern enlargement and the opening of a national labor market is important and interesting *per se*; in particular, in consideration of the aforementioned public and political debate. There is some research on the economic effects of the EU Eastern enlargement. Dustmann et al. (2010) and Dustmann and Frattini (2014) find that EU8 immigrants who came to the UK after 2004 made a positive contribution to the public finances. Caliendo et al. (2017) quantify the economic effects of the trade and labor market integration of the EU Eastern enlargement of 2004 in a dynamic general equilibrium model and find that welfare for all EU countries, especially for the new member states, increased. Walterskirchen and Dietz (1998), Huber and Brücker (2003), and Prettner and Stiglbauer (2007) are *ex-ante* studies predicting the potential impact of the EU Eastern enlargement on the Austrian labor market. Huber and Böhs (2012) examine the inflows of EU8 immigrants to Austria immediately after full labor market liberalization. To the best of our knowledge, this is the first research paper that intends to establish the causal effect of immigration induced by the EU Eastern enlargement on natives' labor market outcomes and on firms.

The remainder of the paper is organized as follows: Section 3.2 discusses relevant aspects of the institutional setting. More precisely, we discuss how the EU enlargement affected the labor market access of individuals in the new EU member countries. Section 3.3 introduces our data source. Section 3.4 provides a descriptive analysis of the EU8 employment in Austrian border states with a focus on the period around EU entry and free labor market access. Section 3.5 outlines our empirical strategy. Section 3.6 shows how labor market outcomes of workers changed across regions that were exposed differentially to EU8 immigration. Section 3.7 presents firms' responses. The final Section 3.8 concludes the paper and discusses potential extensions for future research.

3.2 The EU Eastern enlargement of 2004

In this section, we describe the institutional background in the period around the EU Eastern enlargement of 2004 with a focus on changes in the legal rules that govern the access of workers from the joining countries to the Austrian labor market.

On May 1, 2004, Czechia, Estonia, Hungary, Latvia, Lithuania, Poland, Slovakia, and Slovenia (EU8) joined the European Union.[72] By entering the EU, these countries became members of the European Single Market that guarantees the free movement of goods, services, capital, and labor within the EU. However, the old EU Member States[73] had the option of temporarily restricting access to their labor markets for citizens from the EU8 countries. While some countries opened their labor market immediately (the UK, Ireland, and Sweden), the Austrian government opted for a maximum period with restricted access of 7 years.[74] The right to work as self-employed and the posting of workers fall within the scope of the freedom to provide services and, in general, were not restricted during that period.[75,76]

The EU Eastern enlargement had the following implications for the labor market access of EU8 citizens to Austria. Before May 2004, an EU8 national required a work permit in order to be employed in Austria for which the potential employer had to apply. To receive a permit, the Public Employment Service had to confirm that no equally qualified worker from Austria or another country within the European Economic Area was available for this job. There were simplified procedures for specific groups in place, namely for highly qualified workers, skilled workers in certain occupations, qualified health care personnel, individuals graduating from Austrian institutions of higher education, and seasonal workers in tourism and agriculture (Chaloupek and Peyrl, 2009).

During the period from May 2004 to April 2011, the Austrian government gradually opened its labor market for highly-educated individuals from EU8 countries. Work permits were still required for the low-educated, but they were now given priority over workers from non-EU countries (Chaloupek and Peyrl, 2009).

Since May 2011, EU8 nationals have unrestricted access to the Austrian labor market. Based on the fundamental principle of free movement of labor, as defined in Article 45 of the Treaty on the Functioning of the European Union, they are entitled to look for a job in Austria, work and reside there without a work permit, stay there after employment has finished, and enjoy equal treatment with nationals regarding employment, remuneration, and other conditions of work and employment. These rights do not apply to employment in the public service.[77]

[72] Cyprus and Malta also joined on May 1, 2004. However, this study focuses only on the EU8 countries, since citizens from Cyprus and Malta faced different employment rules in Austria.

[73] The old member states (EU15) in this context are Austria, Belgium, Denmark, Finland, France, Germany, Greece, Ireland, Italy, Luxembourg, the Netherlands, Portugal, Spain, Sweden and the United Kingdom.

[74] See Table C.1 for a timeline on the introduction of free movement from EU8 across EU15 countries.

[75] http://www.europarl.europa.eu/factsheets/en/sheet/40/freedom-of-establishment-and-freedom-to-provide-services, last access 05/2019.

[76] However, the posting of workers was restricted for a limited number of sectors that were considered to be vulnerable to disturbances. These sectors include gardening/horticulture, stone processing, manufacturing of metal structures, construction, security activities, industrial cleaning, home nursing, and social work (Chaloupek and Peyrl, 2009).

[77] http://data.europa.eu/eli/treaty/tfeu_2008/art_45/oj, last access 05/2019.

3.3 Data

Our empirical analysis is based on Austrian social security data, which covers the universe of private sector employment, that is workers who pay contributions to the social security system in Austria (Zweimüller et al., 2009). Self-employed persons and workers posted in Austria on a temporary basis by an employer from another EU Member State are not included in the data. The data record individual employment spells on a daily basis along with an employer identifier. We have information on employers (industry, location, and workforce composition) and on individual demographic characteristics such as date of birth, gender, and nationality.[78] From the raw data, we first create a data set of all blue- and white-collar employees aged between 18 and 55 in a given year.[79] Each year y refers to the period between May y and April $y+1$. For each employed individual, we have information on the municipality ("Gemeinde") of the employer. Since the municipalities were subject to substantial changes over time, we use the territorial boundaries as of 2017.[80] We exclude employees who work in public administration, defense, compulsory social security, or in activities of extraterritorial organizations and bodies since the right to free movement of labor within the EU does not apply to these sectors. We also exclude very large firms with more than 5,000 employees, since we cannot rule out that part of their workforce is employed at branches in different municipalities.

We then aggregate the individual data at the municipality level and create a balanced yearly panel of municipalities between May 1997 and April 2016. For each municipality and year, we measure the labor market outcomes of different subgroups of workers. We distinguish between EU8 nationals, Austrian nationals, and non-EU8 foreigners and within these groups by gender, occupation (blue- and white-collar), and age (below 30, between 30 and 45, above 45). Non-EU8 foreigners are individuals that have neither the Austrian nor the nationality of one of the EU8 countries. We measure the employment of each group by summing up the number of days employed by individuals in the respective group. This measure is preferred over counting workers who are employed on a certain reference date in the year, since temporary migration and seasonal work is prevalent among workers, especially among migrant workers in Austria (Schmieder and Weber, 2018). We standardize the measure by dividing by 365.25, the average number of days per calendar year. Hence, a person who works the full year counts as 1 and employees who work only part of the year count accordingly less. We are further interested in average monthly earnings (real, in 2000 prices). On the employer side, we measure the number of firms with more than 5 employees. Additionally, we count the number of firms that have a predominantly EU8 workforce, which we define as firms in which more than 70% of all employees have the EU8 nationality. The same cutoff is applied to define Austrian and non-EU8 foreign

[78] Austrian citizenship is based primarily on the principle of *jus sanguinis* meaning that citizenship is not determined by place of birth, but by having one or both parents who are citizens of the state (Source: https://www.migration.gv.at/en/living-and-working-in-austria/integration-and-citizenship/citizenship/, last access 05/2019).

[79] Blue- and white-collar employee refer to the terms "ArbeiterIn" and "Angestellte", respectively, which are defined by the Austrian Employment Law. According to the latter, a white-collar employee is a person who performs "kaufmännische oder höhere, nicht-kaufmännische Dienste oder Kanzleiarbeiten". We do not consider apprentices and marginally employed individuals.

[80] We retrieved the municipality register and the boundaries as of January 2017 from Statistics Austria, http://data.statistik.gv.at/web/meta.jsp?dataset=OGDEXT_GEM_1, last access 05/2019.

firms.

Table 3.1: Municipality characteristics in 2003

	All municipalities			Sample		
	Mean	Med	SD	Mean	Med	SD
I. Workforce						
Employment	959	189	4,730	310	178	358
Share Austrian	0.89	0.91	0.09	0.90	0.92	0.09
Share Non-EU8 foreign	0.08	0.06	0.07	0.07	0.05	0.05
Share EU8	0.03	0.01	0.06	0.04	0.01	0.07
Share blue-collar	0.62	0.64	0.14	0.63	0.65	0.12
Share male	0.58	0.59	0.14	0.59	0.60	0.13
Share aged below 30	0.29	0.29	0.08	0.28	0.28	0.07
Share aged 30-45	0.50	0.49	0.07	0.50	0.50	0.06
Mean monthly real earnings	1,653	1,647	276	1,631	1,617	255
Employment growth	0.13	0.07	0.55	0.13	0.07	0.49
Earnings growth	0.03	0.03	0.09	0.03	0.03	0.09
II. Firms						
Number of firms	34.8	11.0	130.7	14.1	9.0	13.8
Share in agriculture	0.04	0.00	0.10	0.05	0.00	0.11
Share in manufacturing	0.22	0.20	0.20	0.24	0.21	0.20
Share in construction	0.17	0.14	0.17	0.18	0.17	0.17
Share in trade	0.17	0.17	0.16	0.18	0.17	0.16
Share in hotels & restaurants	0.15	0.07	0.21	0.11	0.04	0.16
Share in transport	0.08	0.03	0.13	0.08	0.03	0.13
Share in services	0.17	0.14	0.16	0.16	0.14	0.16
Growth in number of firms	0.14	0.04	0.51	0.16	0.05	0.55
Municipalities		2,095			1,406	

Notes: The statistics in the panel to the left refer to all municipalities in Austria and in the panel to the right to all municipalities in the sample with restrictions as outlined in Section 3.3. All variables are measured in the reference year 2003, which is the year from May 2003 to April 2004. Employment is measured as described in Section 3.3. Growth rates refer to the 5-year growth rates from 1998 to 2003.

In our sample, we focus on municipalities in the Austrian border region that are within commuting distance to the EU8 countries; that is, municipalities that belong to one of the five states that share a border with at least one of the EU8 countries (Burgenland, Styria, Upper and Lower Austria, and Carinthia). We exclude very large and very small municipalities with total employment below the 5^{th} or above the 94^{th} percentile in the distribution of 2003. In Austria, municipalities are very heterogeneous with respect to the size of their workforce. In 2003, the smallest municipality had one worker, while Vienna, the largest municipality, had around 550,000 workers.[81] Since it is difficult to incorporate these municipalities in the later empirical analysis, we decide to drop them. We will come back to this point in Section 3.5.
Table 3.1 presents the main descriptive characteristics of the municipalities in all Austria and in our sample measured in the reference year 2003. In the upper panel, we describe the municipalities' workforce. The average employment in municipalities in all Austria is with 959 substantially above the one in our sample, which is 310. At the same time, the median employment is quite comparable between the two groups. As described earlier, we can see that the

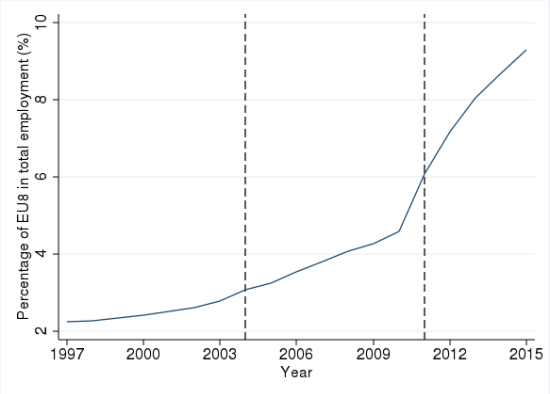
[81] These numbers refer to all workers according to the restrictions as outlined earlier, i.e. only blue- and white-collar workers aged 18-55 and excluding certain industries and large firms.

variation in employment is very substantial across all Austrian municipalities. By excluding the very large and very small municipalities, we move to a much more homogeneous group of municipalities. Except for the size, the two groups are very similar in terms of their other workforce characteristics. Roughly 90% of all employees have the Austrian and 3 to 4% have an EU8 country nationality. Approximately 60% are blue-collar workers. A similar share of workers is male. On average, the mean real wage in municipalities is around 1,650 Euro per month with a five-year increase by around 3% from 1998 to 2003. In line with the differences in mean employment, we observe that the mean number of firms is larger in overall Austria compared to our sample, while the median is very similar. The distribution of firms over industries is very similar, except for a slightly higher percentage of firms in the hotel and restaurant sectors in all of Austria compared to the sample municipalities.

3.4 EU8 workers in the Austrian border region

In this section, we first analyze how the employment of workers from EU8 countries in the Austrian border region developed between 1997 and 2015. Second, we examine how the composition of EU8 workers evolved, and we compare it to the average characteristics of workers with an Austrian or a non-EU8 nationality. Third, the distribution of EU8 workers over regions and industries is discussed. In all of the following figures, the vertical line to the left marks the date of the EU accession in 2004 and to the right the free labor market access to Austria from the EU8 countries in 2011.

Figure 3.2: Employment of EU8 workers over time



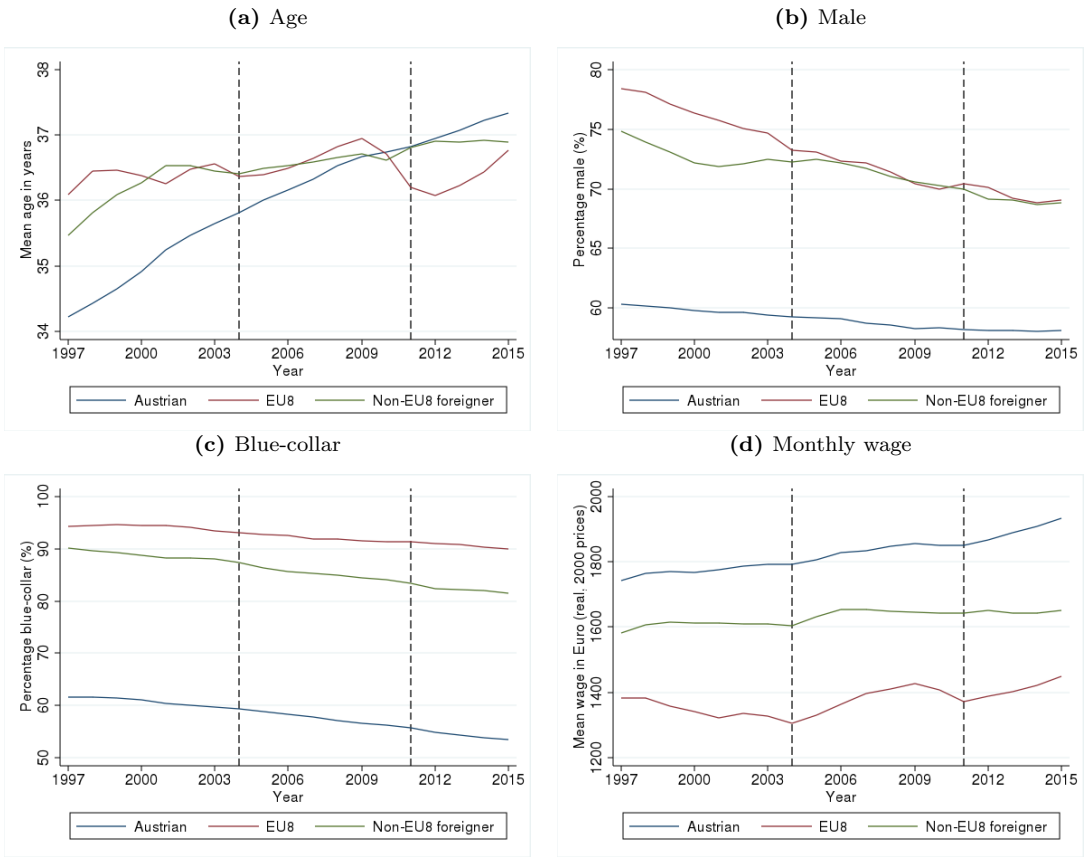
Notes: This figure shows the yearly employment shares of workers from EU8 countries in our sample. Year y refers to the period between May y and April $y + 1$.

Figure 3.2 shows the employment by EU8 nationals in Austrian border regions over time. Employment from the EU8 countries grew moderately before accession and during the transition period. With free labor market access, the trend changed markedly and growth in EU8 employment picked up persistently. Over the period from 1997 to 2015, the employment share of EU8 nationals increased from 2.2% to 9.3%. Appendix Figure C.1 indicates that among

EU8 workers in Austrian border regions after 2011, Hungarians were by far the largest group, followed by Slovaks, Slovenes, and Poles.

To provide a detailed picture of compositional shifts among EU8 migrant workers and to compare them with the rest of the workforce, Figure 3.3 shows the evolution of mean characteristics of employees from EU8, other foreign countries, and Austria over time. While the average age of Austrian employees in our sample is steadily increasing (Figure 3.3a), the mean age of workers from the EU8 and from other foreign countries remains relatively stable over time. An exception is the time around free labor market access for the EU8 countries during which we observe a substantial drop in the mean age of EU8 workers by about one year. Non-EU8 foreign and EU8 workers are, on average older, than Austrian workers until around 2011, then younger thereafter.

Figure 3.3: Average characteristics of EU8, Austrian, and non-EU8 foreign workers



Notes: These graphs show the average characteristics of individuals from different nationality groups employed in year y (period between May y and April $y+1$) in our sample.

Panel 3.3b shows that a very large share of workers with a non-Austrian background is male. These proportions are substantially above that of Austrians. The share of males is higher among those with an EU8 (78% in 1997) compared to those with foreign non-EU8 citizenship

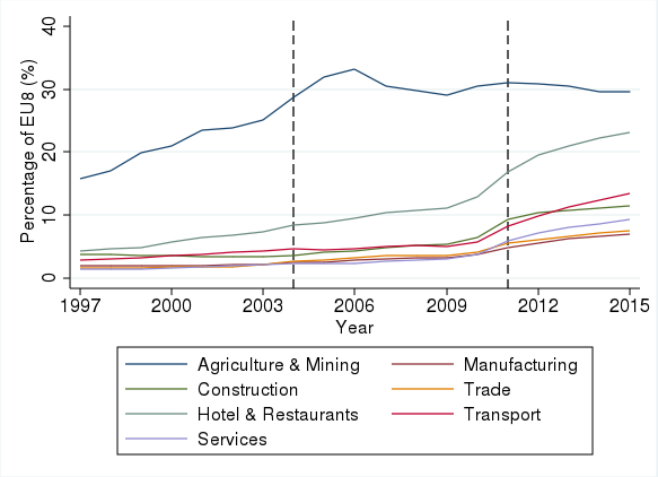
(75% in 1997) until the mid-2000s and very similar thereafter. For all three nationality groups, the male shares decline substantially over time. However, around 2011 there is a small increase in the relative number of males among EU8 workers.

We further see in Figure 3.3c that the vast majority of EU8 workers are in blue-collar jobs with a decreasing share from about 95% in 1997 to 90% in 2015. Foreign workers from non-EU8 countries are also mainly found in blue-collar jobs. However, this share is lower compared to that of EU8 workers, and it decreases faster over time. In contrast, around 62% of Austrian workers have a blue-collar job in 1997 and this number shrinks to 53% in 2015.

The mean of real monthly earnings of Austrian workers is well above that of foreigners and it increases from around 1,750 Euro in 1997 to more than 1,900 Euro in 2015. Non-EU8 foreigners earn an average of 1,600 Euro per month and experience little increases over the same period. The mean of EU8 workers' earnings is, at 1,400 Euro, even lower and it tends to decrease between 1997 and 2004. The subsequent increase is interrupted by a discontinuous drop in earnings around 2011. By 2015, EU8 workers have average mean monthly earnings of slightly more than 1,400 Euro.

To sum up, Figure 3.3 suggests that the composition of EU8 employees in Austria changed discontinuously with free access to the Austrian labor market. Moreover, it illustrates that EU8 workers are much more similar in terms of their observed characteristics to other foreigners than to Austrian workers.

Figure 3.4: Percentage of EU8 workers across industries



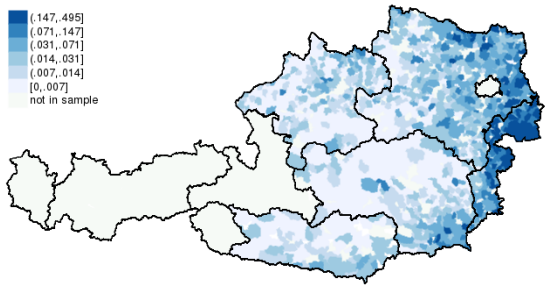
Notes: This figure shows the employment of EU8 nationals across industries relative to the employment of all workers in the corresponding year and industry. We only consider workers in our sample municipalities. Year y refers to the period between May y and April $y + 1$.

Figure 3.4 illustrates the employment shares of EU8 workers across industries relative to all workers in the corresponding year and industry. EU8 nationals are most represented in agriculture and mining. Their employment share in these two sectors increases from 16% in 1997 to around 32% in 2006. The decreasing and then stagnating share of EU8 workers in agriculture

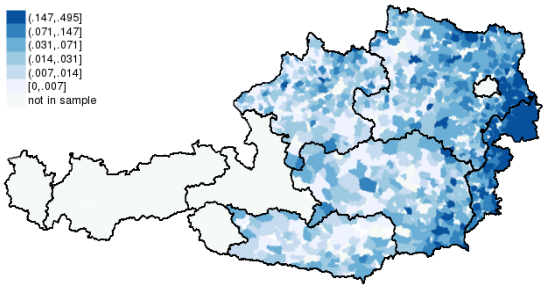
and mining after 2007 is related to the EU Eastern enlargement of 2007 and the resulting increased number of workers from Bulgaria and Romania in these sectors.[82] The representation of EU8 nationals in the tourism sector (hotels and restaurants) experienced the largest increase from 5% in 1997 to more than 20% in 2015. In the construction sector, the rise in the relative number of EU8 workers from 4% in 1997 to 10% in 2015 was also substantial. In all of the three aforementioned sectors employment varies substantially over the calendar year because of seasonal demand fluctuations (Schmieder and Weber, 2018). The high representation of EU8 workers in these industries suggests that temporary and seasonal migration is potentially important, which makes it less likely that EU8 workers in these industries resettle with their families in Austria. The transport sector experienced a similar increase in the relative EU8 employment to the construction sector. The remaining sectors, manufacturing, trade, and services also experienced an, albeit smaller, increase in the share of EU8 workers after 2011. Still, the proportion of EU8 workers in each of these sectors amounts to between 7% and 13% in 2015.

Figure 3.5: Spatial distribution of employment by EU8 nationals

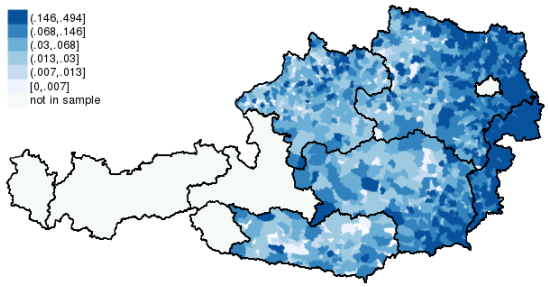
(a) May 2000 to April 2004



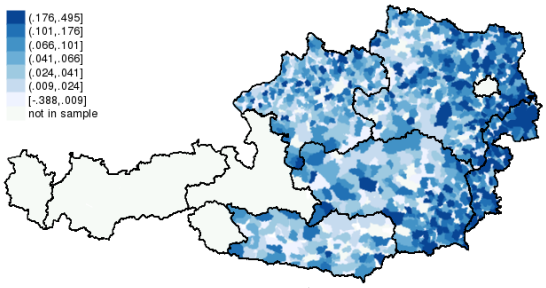
(b) May 2004 to April 2011 (transition period)



(c) May 2011 to April 2016 (free labor movement)



(d) Change between 2003 and 2015



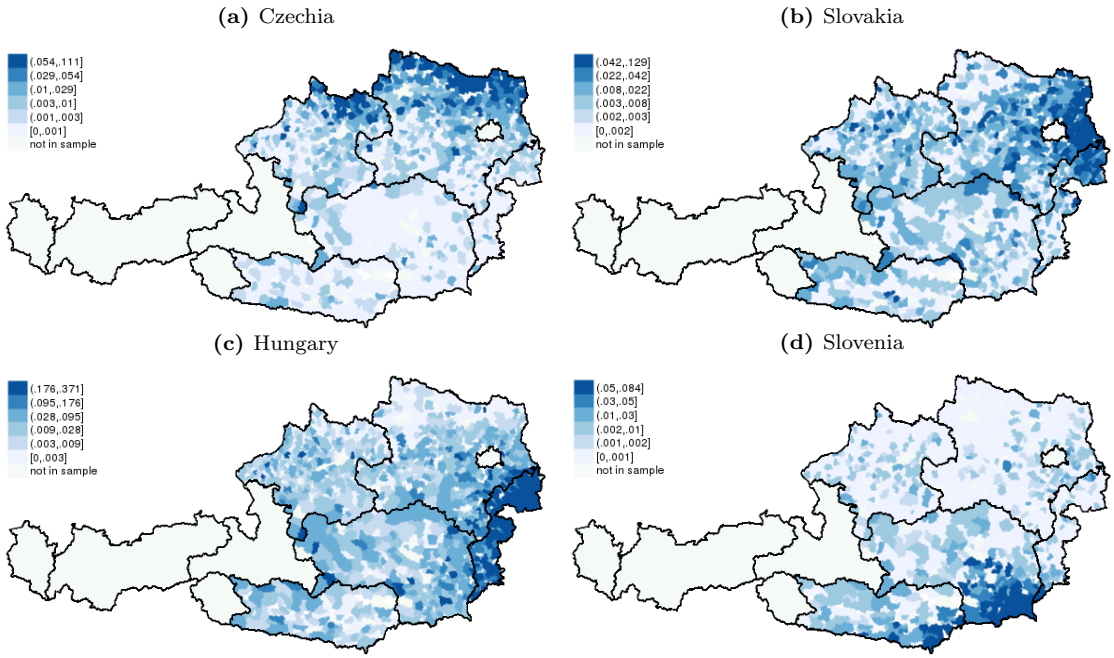
Notes: Panels (3.5a) to (3.5c) illustrate the employment shares of EU8 nationals across Austrian municipalities in the sample for three time periods. The upper bounds of the categories are the same for all periods and correspond to the 10^{th}, 25^{th}, 50^{th}, 75^{th}, 90^{th}, and top percentile of the distribution of the EU8 employment share over all periods. Panel (3.5d) shows the change in this variable between 2003 and 2015.

The panels 3.5a to 3.5c illustrate the regional distribution of migrant workers from EU8 countries in the Austrian border region for three different periods: before accession in May 2004, during the transition period from May 2004 to April 2011, and after May 2011. We plot the number of days employed by workers from EU8 countries relative to the days employed by all workers across Austrian municipalities. Darker areas on the map indicate groups with a higher concentration

[82] See Appendix Figure C.2 for the percentage of EU2 workers across industries.

of EU8 employment. Figure 3.5d plots the change in the EU8 employment shares between 2003 and 2015. Even before the EU Eastern enlargement of 2004, EU8 workers tended to cluster in the eastern border region of Austria. This is sustained in the transition period during which we observe a slight increase in the overall employment of EU8 workers. With the introduction of unrestricted labor market access in 2011, EU8 migrants' employment increases in most municipalities, especially in areas close to Austria's border with the neighboring EU8 countries.

Figure 3.6: Spatial distribution of employment by nationality after May 2011



Notes: Similar to Figure 3.5, these panels illustrate the relative employment shares of workers from Czechia, Hungary, Slovakia, and Slovenia across Austrian municipalities in the sample for the time period between May 2011 and April 2016. The upper bounds of the categories correspond to the 25^{th}, 50^{th}, 75^{th}, 90^{th}, 95^{th}, and the top percentile of the distribution of the nationality-specific non-zero employment shares from May 2011 to April 2016.

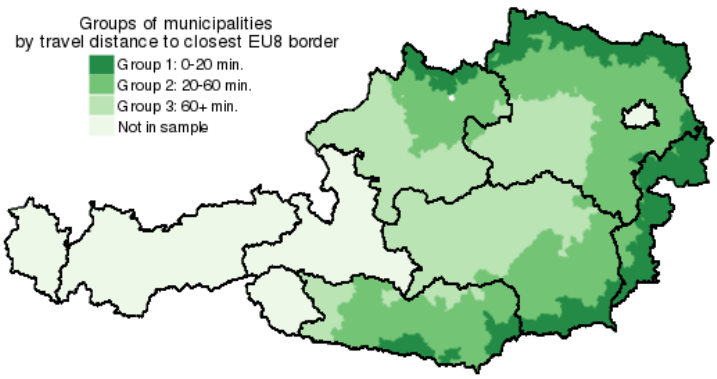
In Figure 3.6 we show the employment shares after May 2011 separately by workers' nationality. We distinguish between workers from the 4 neighboring EU8 countries of Czechia, Slovakia, Hungary, and Slovenia. The maps highlight that workers from the neighboring countries have a strong tendency to locate in municipalities in Austria that are closest to their home country. This again suggests that workers are likely to commute between their home country and their Austrian workplace.

3.5 Empirical strategy

We saw in the previous section that the number of workers in Austria from EU8 countries increased substantially between 1997 and 2015. In this section, we outline our empirical strategy to estimate the effect of an increase in the number of EU8 workers on the labor market outcomes

of Austrian and non-EU8 foreign nationals and on the number of firms in a municipality.
We exploit variation in the exposure to EU8 workers across municipalities over time and distance to the closest EU8 border.[83] The idea is that differences in wages and costs of living provide incentives for workers from EU8 countries to commute between their home country and Austria. Appendix Figure C.3 illustrates that earnings and prices in all of the EU8 countries were substantially below those in Austria from 2000 through 2015. In 2000, the mean annual net earnings of a single person without children in an EU8 country were only between 10% and 30% of those in Austria, while price levels (except Slovenian) were between a third and a half. These measures were converging towards the Austrian levels up to 2008 but were constant relative to them thereafter. Except for Slovenia, annual earnings were about 30% to 50%, and price levels were about 40% to 60% of the Austrian level in the period from 2008 to 2015.
Since commuting costs increase with distance to the border, municipalities closer to the border are expected to be more exposed to inflows of EU8 workers over time than those further away. Combining Austrian social security data with information on the place of residence, Huber and Böhs (2012) confirm that a substantial number of EU8 workers in Austrian border states, who first came to Austria between 2011 and 2012, were indeed commuters.[84] Moreover Figure 3.6 in the previous section shows that workers from the 4 neighboring EU8 countries are more likely to locate in municipalities close to their home country, which also supports the assumption that they tend to commute.

Figure 3.7: Municipality groups by distance to the closest EU8 border



Notes: This figure shows the categorization of Austrian municipalities in our sample into three different groups that differ in their travel duration to the closest EU8 border. The black lines highlight the NUTS-II region borders. *Source:* Open Street Map, own illustrations.

We define the distance to the closest EU8 border as the travel duration by car from the municipality center to the closest EU8 border crossing. The center of each municipality is defined as the center of its largest town; EU8 border crossings are roads that cross the border to one of the

[83]The variation in exposure to migrants by distance to a border in combination with a change in migration policy is previously exploited in Dustmann et al. (2017) and Beerli et al. (2018).
[84]The exact numbers are 91% for Burgenland, 65% for Styria, 43% for Upper Austria, 12% for Lower Austria, and 9% for Carinthia.

EU8 neighboring countries of Czechia, Hungary, Slovenia, or Slovakia.[85] Based on their travel distance to the closest EU8 border, we group the municipalities in our sample into three groups, as illustrated in Figure 3.7. Municipalities in group 1 (group 2) are within a travel distance of 0 to 20 minutes (20 to 60 minutes) to the closest EU8 border crossing. For municipalities in group 3, the nearest EU8 border crossing can be reached by driving more than one hour.[86,87]

Table 3.2: Municipality characteristics across distance groups in 2003

	Group 1			Group 2			Group 3		
	Mean	Med	SD	Mean	Med	SD	Mean	Med	SD
I. Workforce									
Employment	273	132	344	323	189	370	314	189	349
Share Austrian	0.85	0.88	0.11	0.90	0.92	0.09	0.92	0.93	0.06
Share Non-EU8 Foreign	0.05	0.04	0.04	0.07	0.06	0.06	0.07	0.05	0.05
Share EU8	0.10	0.06	0.10	0.03	0.02	0.05	0.01	0.00	0.02
Share blue-collar	0.65	0.66	0.11	0.62	0.63	0.12	0.64	0.65	0.11
Share male	0.59	0.60	0.13	0.59	0.59	0.13	0.60	0.61	0.12
Share aged below 30	0.27	0.26	0.07	0.28	0.28	0.06	0.30	0.29	0.07
Share aged 30-45	0.50	0.50	0.06	0.50	0.50	0.05	0.49	0.49	0.05
Mean monthly real earnings	1,559	1,561	230	1,631	1,609	261	1,674	1,670	252
Employment growth	0.12	0.06	0.38	0.16	0.07	0.61	0.10	0.05	0.32
Earnings growth	0.03	0.03	0.09	0.03	0.03	0.10	0.03	0.03	0.08
II. Firms									
Number of firms	13.1	8.0	13.5	15.0	10.0	14.9	13.6	10.0	12.3
Share in agriculture	0.06	0.00	0.11	0.05	0.00	0.10	0.05	0.00	0.11
Share in manufacturing	0.24	0.21	0.22	0.22	0.20	0.19	0.27	0.25	0.19
Share in construction	0.21	0.19	0.18	0.19	0.17	0.18	0.16	0.14	0.16
Share in trade	0.19	0.17	0.18	0.18	0.17	0.16	0.18	0.17	0.15
Share in hotels & restaurants	0.09	0.00	0.15	0.10	0.05	0.16	0.12	0.04	0.18
Share in transport	0.07	0.00	0.11	0.08	0.03	0.14	0.08	0.03	0.13
Share in services	0.15	0.14	0.14	0.18	0.16	0.17	0.15	0.13	0.14
Growth in number of firms	0.22	0.09	0.63	0.16	0.05	0.54	0.12	0.00	0.49
Municipalities		277			648			481	

Notes: The statistics in the panel to the left refer to all municipalities in Austrian border states within a travel distance of 0 to 20 minutes to the next EU8 border (group 1). In the middle panel they refer to all municipalities in Austrian border states within a travel distance of 20 to 60 minutes to the next EU8 border (group 2) and in the right panel they refer to all municipalities in Austrian border states with a travel distance of more than 60 minutes to the next EU8 border (group 3). All variables are measured in the reference year 2003, which is the year from May 2003 to April 2004. Employment is measured as described in the text. The growth rates refer to the 5-year growth rates from 1998 to 2003. The sample restrictions are as outlined in Section 3.3.

[85] See Appendix Figure C.4 for a map of Austria with its main roads and the identified border crossing points.

[86] We already explained in Section 3.3 that we exclude very small and very large municipalities in the Austrian border region from our sample. Appendix Figure C.5 plots the workforce size of 2003 of all municipalities in the border region against their travel distance to the closest EU8 border. We immediately see that there are a few, exceptionally large municipalities that are mainly within 20 to 60 minutes travel distance to the closest border. For these municipalities, it is not possible to create a reasonable counterfactual scenario in our empirical strategy.

[87] Appendix Figure C.6 plots the employment shares of EU8 nationals against the travel duration to the closest EU8 border for the period before EU8 accession, the transition period, and the period after May 2011. It motivates the choice of our three groups by showing that municipalities within a 0 to 20 minutes travel duration experienced a large increase in EU8 employment shares between the three periods. Municipalities within 20 to 60 minutes received a smaller, but still substantial relative increase in EU8 employment. In contrast, the rise in EU8 employment was smaller and relatively homogeneous among municipalities that are more than one hour away from the border.

To get an impression on how municipalities in the three different distance groups compare to each other, we show descriptive statistics on workforce and firm characteristics in each group in Table 3.2. Turning to the upper panel, municipalities in group 1 have on average a lower employment level compared to those in group 3, which is in turn smaller than in group 2. This difference is more pronounced in the median employment levels. The employment share of Austrians (EU8 nationals) is 90% (3%) and 92% (1%) in group 3 and group 2, respectively. In contrast, the percentage of Austrians in group 1 is lower (85%), while it is larger for EU8 nationals (5%). The share of blue-collar and male workers as well as the distribution over age is similar across groups. Average monthly earnings are increasing from group 1 to 2 and from group 2 to 3. There is some variation in the average number of firms across groups that mirrors the differences in mean employment. While the industry composition is comparable across groups, the growth in the number of firms before 2004 tends to be larger the closer the municipalities are to the EU8 border. Having these differences across municipality groups in mind, we are now describing our empirical strategy.

Intuitively, we want to compare the differences in outcomes in municipalities in groups 1 and 2 to those in group 3 for each year from 1997 to 2015 and contrast them to the corresponding differences in the reference year 2003, which is the year before the EU Eastern enlargement. We denote the outcome of municipality m in year t as the variable Y_{mt}. We analyze the employment of EU8, Austrian, and foreign non-EU8 workers. We further look at the logarithm of mean monthly earnings in Euro in a municipality and the number of firms. We estimate the following model for our outcome variables:

$$Y_{mt} = \sum_{\substack{y=1997 \\ y\neq 2003}}^{2015} \left(\delta_y^{[0-20]} D_m^{[0-20]} * I\{t=y\} + \delta_y^{[20-60]} D_m^{[20-60]} * I\{t=y\} \right) + \theta_m + \lambda_{t,g} + \upsilon_{mt}. \tag{3.1}$$

The variables $D_m^{[0-20]}$ and $D_m^{[20-60]}$ are indicators equal to 1 if municipality m is within 0 and 20 and 20 and 60, respectively, minutes driving distance from the next EU8 border, and $I\{.\}$ is the indicator function. The variable θ_m captures time-constant municipality fixed effects. $\lambda_{t,g}$ are year-by-regions g fixed effects.[88] υ_{mt} is the error term.

Hence, $\delta_y^{[0-20]}$ captures the (conditional) difference in outcomes in year y between municipalities within 0 and 20 minutes and municipalities with more than 60 minutes travel distance from the next EU8 border relative to the difference in 2003. Similarly, $\delta_y^{[20-60]}$ measure the (conditional) difference between municipalities within 20 and 60 minutes and municipalities with more than 60 minutes travel distance from the next EU8 border relative to the difference in the reference year 2003. Standard errors are clustered at the municipality level.

For the presentation of our quantitative estimation results, we average the difference in the outcomes between the groups relative to the difference in 2003 over the years in the pre-accession, in the transition, and in the period with free access of EU8 nationals to the Austrian labor market. The model is given by

[88] For our region fixed effects, we mainly rely on the NUTS-II definition of the EU. However, since Burgenland, which is the region in southeast Austria, only contains municipalities in the distance groups 1 and 2, with none in 3, we define Burgenland and its neighbor region to the west, Styria, as one region.

$$
\begin{aligned}
Y_{mt} &= \delta_{pre}^{[0-20]} D_m^{[0-20]} * pre_t + \delta_{pre}^{[20-60]} D_m^{[20-60]} * pre_t \\
&+ \delta_{trans}^{[0-20]} D_m^{[0-20]} * trans_t + \delta_{trans}^{[20-60]} D_m^{[20-60]} * trans_t \\
&+ \delta_{free}^{[0-20]} D_m^{[0-20]} * free_t + \delta_{free}^{[20-60]} D_m^{[20-60]} * free_t \\
&+ \theta_m + \lambda_{t,g} + \upsilon_{mt},
\end{aligned} \tag{3.2}
$$

where pre is an indicator equal to 1 if t is between 1997 and 2002, $trans$ is an indicator equal to 1 if t is between 2004 and 2010, and $free$ is an indicator equal to 1 if t is between 2011 and 2015.

In order to interpret the coefficients δ as the effects of the EU Eastern enlargement on our outcome variables, we must impose the following assumptions. First, we have to assume that if there had not been the enlargement, then labor market outcomes in the 3 groups would have evolved in a parallel way. By including region-specific time trends $\lambda_{t,g}$, we take into account that municipalities might have different time trends across regions. We further need to rule out that there were simultaneous policies that affected the groups differently around 2004. One candidate is the subsequent EU enlargement in 2007. Bulgaria and Romania joined the union in that year and gained free access to the Austrian labor market in 2014. In Appendix Figure C.7, we show that the employment of Bulgarians and Romanians in a municipality is not related to its distance to the closest EU8 border. We further provide a robustness check in which we control for differential time trends due to differences in the pre-existing industry structure across municipalities as in Beerli et al. (2018).[89] Given the long time horizon of our data, we can later additionally check for differential trends across the groups before 2004 to support the plausibility of our assumptions.

Second, we must rule out that there are spillovers of the enlargement between the groups. One potential concern is that residents may avoid labor markets that are very exposed to immigrants by moving from municipalities closer to the border to those further away after the EU Eastern enlargement (see for example Borjas (2003)). Since we can follow individuals in our data over time, we can investigate changes in worker flows between labor markets and learn about their magnitude relative to the size of these markets.[90]

To relate the effects of the EU Eastern enlargement on labor market outcomes of Austrian and of other foreign workers and on firms to the differential exposure to EU8 workers, we further have to assume that the EU Eastern enlargement affected the distance groups differently only through the differential inflow of EU8 workers. For this assumption to hold, we have to rule out that Austrian municipalities closer to the border were differently affected by other freedoms granted to the new EU Member States by joining the EU. One concern is that the enlargement induced an increase in trade between Austria and the neighboring EU8 countries that affected

[89] For the construction of the Bartik (1991)-type control measure, which predicts the change in employment and wages based on national-and-industry-specific employment and wage changes, we apply the definition of Beerli et al. (2018). The variable is generated at the municipality level. The base year that determines the industry shares of municipalities is 1997.

[90] This is work in progress.

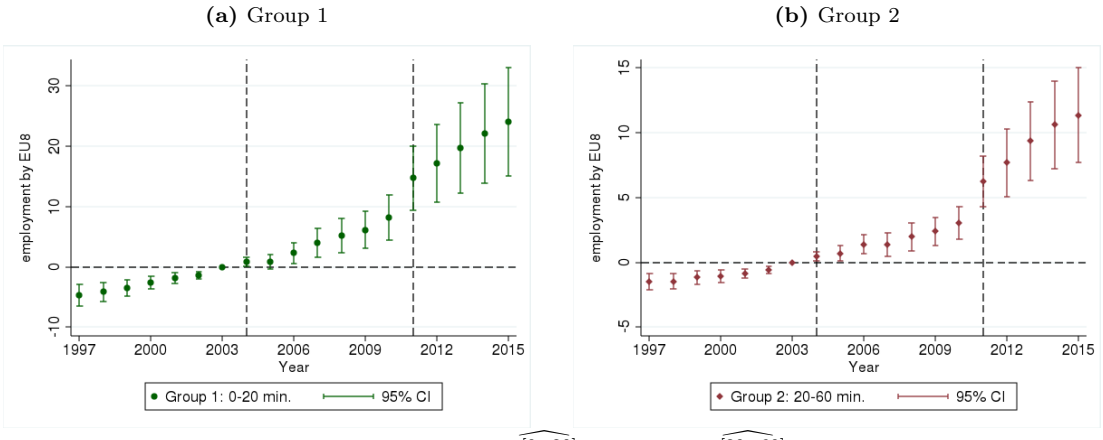
municipalities closer to the border differently than those further away (Redding and Sturm, 2008; Brülhart et al., 2012). Appendix Figure C.8 shows that import shares from and export shares to EU8 countries are relatively constant over the time period from 1997 to 2015. In particular, there are no changes around important dates related to the EU Eastern enlargement of 2004. Furthermore, we can exploit that changes related to the EU enlargement started earlier and proceeded more gradually because the accession of the EU8 countries and their entitlement to free access to the Austrian labor market happened at different points in time.

3.6 How did immigration impact workers in neighboring labor markets?

3.6.1 Employment

First, we are interested in whether the EU Eastern enlargement actually affects the trends in EU8 employment differently in municipalities closer to the border compared to those further away. Panel (a) in Figure 3.8 shows the estimated coefficients $\widehat{\delta_y^{0-20}}$ and their corresponding 95% confidence intervals from equation (3.1) annually from 1997 to 2015, with 2003 as the reference year. Similarly, panel (b) shows the estimated coefficients $\widehat{\delta_y^{[20-60]}}$ and their corresponding 95% confidence intervals. The dependent variable is the employment of EU8 nationals as defined in Section 3.3. The vertical line to the left marks the date of the EU accession in 2004 and to the right the free labor market access to Austria from the EU8 countries in 2011.

Figure 3.8: Effects of the EU Eastern enlargement on the employment of EU8 nationals



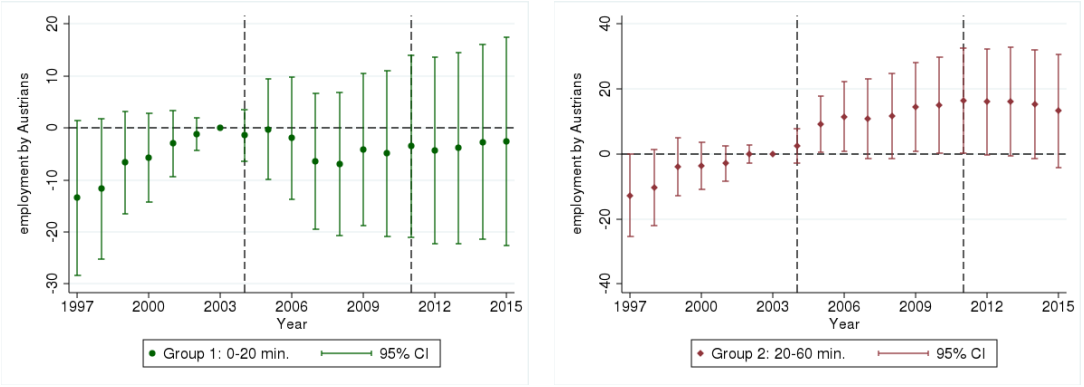
Notes: Figure 3.8 shows the estimated coefficients $\widehat{\delta_y^{[0-20]}}$ (green) and $\widehat{\delta_y^{[20-60]}}$ (red) over time with the corresponding 95% confidence intervals based on estimation equation (3.1), which includes municipality and year-by-regions fixed effects. Group 1 (group 2) refers to municipalities within 0 to 20 (20 to 60) minutes driving distance to the closest EU8 border. The outcome variable Y_{mt} is the employment of EU8 workers, which is the sum over the number of days employed by EU8 nationals, standardized by dividing by 365.25. Standard errors are clustered at the municipality level.

In panel (a) we observe that the employment of EU8 nationals increases significantly more in municipalities that are within 0 to 20 minutes compared to those that are more than one hour

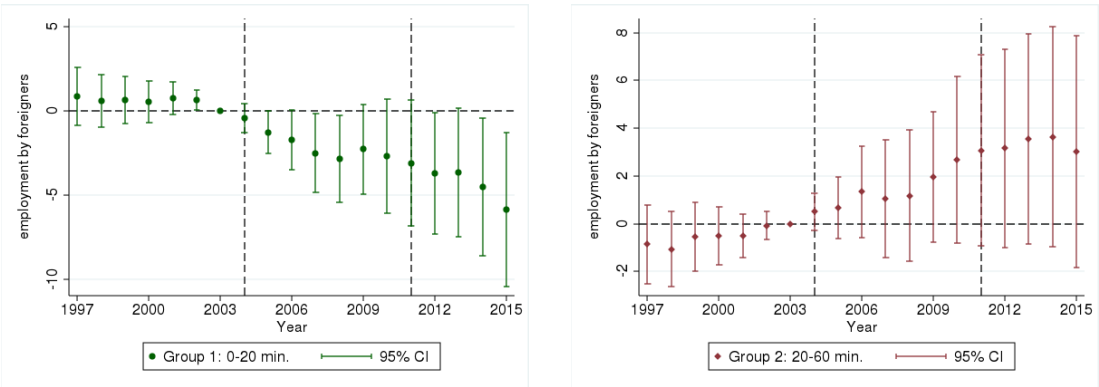
away from the closest EU8 border after 2004. The difference in EU8 employment between these two groups continually increases over the subsequent years. Following the introduction of free access to the Austrian labor market, the relative rise in the number of EU8 workers in group 1 accelerates. In 2015 the estimated relative increase in the number of EU8 workers amounts to approximately 20. The time pattern for municipalities in group 2 is very similar than in group 1; the effects are around half of the size. Hence, the increase in EU8 employment over time is larger in municipalities in distance group 1 than in group 2, which is again larger than in group 3. Thereby, Figure 3.8 confirms our assumption that municipalities closer to the border are more exposed to an increasing number of EU8 employees over time compared to those further away. We should note, however, that already before the EU enlargement of 2004, EU8 employment increases slightly faster, the closer the municipalities are to the EU8 border.

Figure 3.9: Effects of the EU Eastern enlargement on the employment of other workers

(a) Austrian - Group 1 **(b)** Austrian - Group 2



(c) Non-EU8 foreigner - Group 1 **(d)** Non-EU8 foreigner - Group 2



Notes: Figure 3.9 shows the estimated coefficients $\widehat{\delta_y^{[0-20]}}$ (green) and $\widehat{\delta_y^{[20-60]}}$ (red) over time with the corresponding 95% confidence intervals based on estimation equation (3.1), which includes municipality and year-by-regions fixed effects. The outcome variable Y_{mt} in the upper (bottom) graphs is the employment of Austrian (non-EU8 foreign) workers, which is the sum over the number of days employed by Austrian (non-EU8 foreign) nationals, standardized by dividing by 365.25. Standard errors are clustered at the municipality level.

In Figure 3.9, we turn to the employment of Austrians in panels (a) and (b) and of non-EU8 foreigners in panels (c) and (d). As before, the left panels refer to the effects of the EU Eastern enlargement on municipalities within 0 to 20 minutes and the right panels to the effects on those

within 20 to 60 minutes travel distance to the closest border. In panel (a), we have very wide confidence intervals and we do not find that the employment of Austrians evolves differently, on average, in municipalities that are within 0 to 20 minutes travel distance to the border compared to those further away. In contrast, panel (b) indicates a statistically significant relative increase in the employment of Austrians over time in municipalities that are within 20 to 60 minutes travel distance to the border. However, an increasing trend in the employment of Austrians for group 2 (and to a lesser extent for group 1) can already be observed before 2004.
For non-EU8 foreign workers, we observe in panel (c) that their average employment decreases significantly and steadily in municipalities in group 1 relative to the reference group after 2004. In contrast, panel (d) points to a relative, albeit not statistically significant, increase in the number of workers from non-EU8 countries over time for municipalities within 20 to 60 minutes driving distance to the border. There are no differences in the non-EU8 foreign employment trends between municipalities in group 1 and in group 2 relative to group 3 prior to the EU Eastern enlargement in 2004.

Table 3.3: Effects of the EU Eastern enlargement on employment

	All	EU8	Austrians	Non-EU8 foreigners
	(1)	(2)	(3)	(4)
$\delta_{pre}^{[0-20]}$	-9.32	-3.04	-6.97	0.67
	(4.70)	(0.57)	(4.26)	(0.56)
$\delta_{trans}^{[0-20]}$	-1.77	3.94	-3.74	-1.96
	(6.70)	(1.09)	(5.68)	(0.99)
$\delta_{free}^{[0-20]}$	11.95	19.55	-3.43	-4.17
	(12.07)	(3.66)	(9.25)	(1.97)
$\delta_{pre}^{[20-60]}$	-7.35	-1.10	-5.65	-0.60
	(4.09)	(0.22)	(3.73)	(0.57)
$\delta_{trans}^{[20-60]}$	13.68	1.62	10.72	1.34
	(6.11)	(0.38)	(5.33)	(1.04)
$\delta_{free}^{[20-60]}$	27.73	9.04	15.39	3.29
	(10.29)	(1.46)	(8.22)	(2.18)
Mean in 2003	310.27	8.65	279.93	21.71
Observations		26,695		
Municipalities		1,405		

Notes: This table displays the impact of the EU Eastern enlargement on employment based on equation (3.2), which includes municipality and year-by-regions fixed effects. The upper (lower) panel focuses on $\delta^{[0-20]}$ ($\delta^{[20-60]}$), which capture the difference in outcomes in the corresponding year between municipalities within 0 to 20 (20 to 60) minutes and municipalities with a driving distance $\geq$ 60 minutes from the next EU8 border. This difference is standardized to 0 in 2003. *Pre* refers to the period May 1997 to April 2003, *trans* to the period May 2004 to April 2011, and *free* to the period May 2011 to April 2016. The dependent variable is the employment of all workers in column (1), EU8 workers in (2), Austrian workers in (3), and non-EU8 workers in (4). Employment is measured as the sum over the number of days employed by the corresponding group, standardized by dividing by 365.25. Standard errors are clustered at the municipality level.

In Table 3.3, we show the employment effects averaged over the years in the period before 2004 (*pre*), in the transition period between 2004 and 2010 (*trans*), and in the period with free movement of labor (*free*) based on equation (3.2). The coefficients in the upper part of the

table refer to the effects in municipalities within 0 to 20 minutes travel distance to the closest EU8 border and in the lower part to municipalities within 20 to 60 minutes, respectively. The dependent variable is the employment of all workers in column (1), EU8 workers in (2), Austrian workers in (3), and non-EU8 foreign workers in (4). The estimated coefficients in column (1) suggest that the total employment in municipalities within 0 to 20 and 20 to 60 minutes driving distance to the border increases over time relative to municipalities that are more than 60 minutes away from the border; even before 2004. The coefficients are not statistically different from 0 for group 1. For group 2, there is a statistically significant increase by an average of 14 full-year employees in the transition period and by an average of 28 employees in the free movement period. Relative to the average employment level in 2003, this amounts to an increase of about 5% and 9%, respectively.

We then partition the total employment response into the effects on the three nationality groups. For municipalities that are within a 20 minutes driving distance to the EU8 border, we estimate a substantial and significant increase by, on average, 4 EU8 employees in the transition period and by 20 EU8 full-year employees in the free movement period relative to 2003. This increase compares with a significant decrease by about 2 non-EU8 foreign employees in the transition period and by 4 employees in the period after 2011. The coefficients on the employment response by Austrian nationals are negative, but not statistically significantly different from 0. For municipalities within 20 to 60 minutes driving distance to the border, we estimate that the increase in total employment is mainly due to an increase in the employment of Austrians and, in particular after 2011, also of EU8 nationals.

In Table 3.4, we zoom further into the employment effects on EU8 nationals by investigating different subgroups. Column (1) provides the effect on overall employment by EU8 nationals. The dependent variable in column (2) is the EU8 employment in blue-collar occupations. Contrasting the effects in columns (1) and (2) shows that the increase in EU8 employment during the transition and the free movement period is almost entirely driven by an increase in blue-collar employment. This is true for both distance groups 1 and 2. In group 1 approximately 60% and in group 2 roughly 70% of the relative increase in EU8 employment is via men as shown in column (3). Columns (4) to (6) further highlight that most of the increase in EU8 employment occurred in the age group 30 to 45.

In Table 3.5, we examine the effect of the EU Eastern enlargement for subgroups of Austrian employees. As explained before, we see in column (1) that we cannot detect a statistically significant effect on overall employment by Austrians in distance group 1, but we observe a positive effect in group 2. Focusing on the municipalities within 0 to 20 minutes travel distance to the border, the estimated coefficients point to negative effects for blue-collar employees in column (2) as well as for young and old Austrian employees in columns (4) and (6). However, these coefficients are not statistically significant at common significance levels. In distance group 2, the effect on blue-collar employment is small and not different from 0. Hence, the overall positive employment effect on Austrians appears to be rather driven by an increasing number of white-collar workers. Furthermore, this increase seems to occur across age groups, as illustrated in columns (4) to (6).

Table 3.4: Heterogeneous effects on the employment of EU8 nationals

				Age group		
	All	Blue	Male	≤ 30	$30-45$	$45<$
	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_{pre}^{[0-20]}$	-3.04	-2.76	-2.17	-0.56	-0.84	-1.64
	(0.57)	(0.55)	(0.47)	(0.20)	(0.29)	(0.22)
$\delta_{trans}^{[0-20]}$	3.94	3.54	2.39	0.63	2.47	0.84
	(1.09)	(0.99)	(0.84)	(0.29)	(0.69)	(0.22)
$\delta_{free}^{[0-20]}$	19.53	16.49	11.42	3.16	13.16	3.21
	(3.66)	(2.94)	(2.26)	(0.92)	(2.25)	(0.64)
$\delta_{pre}^{[20-60]}$	-1.09	-0.96	-0.73	-0.36	-0.29	-0.44
	(0.22)	(0.21)	(0.19)	(0.09)	(0.13)	(0.10)
$\delta_{trans}^{[20-60]}$	1.61	1.31	1.04	0.29	0.92	0.40
	(0.38)	(0.35)	(0.31)	(0.11)	(0.23)	(0.11)
$\delta_{free}^{[20-60]}$	9.04	7.79	6.60	1.60	5.82	1.62
	(1.46)	(1.34)	(1.07)	(0.40)	(0.85)	(0.29)
Mean in 2003	8.65	7.96	6.69	1.87	4.30	2.48
Observations	26,695					
Municipalities	1,405					

Notes: This table displays the impact of the EU Eastern enlargement on employment by EU8 workers in Austrian border regions based on equation (3.2), which includes municipality and year-by-regions fixed effects. The upper (lower) panel focuses on $\delta^{[0-20]}$ ($\delta^{[20-60]}$), which capture the difference in outcomes in the corresponding year between municipalities within 0 to 20 (20 to 60) minutes and municipalities with a driving distance of more than 60 minutes from the next EU8 border. This difference is standardized to 0 in 2003. *Pre* refers to the period May 1997 to April 2003, *trans* to the period May 2004 to April 2011, and *free* to the period May 2011 to April 2016. The dependent variable in column (1) is the employment of EU8 workers and in columns (2) to (6) the employment of EU8 workers in the corresponding subgroup. Employment is measured as the sum over the number of days employed by the corresponding group, standardized by dividing by 365.25. Standard errors are clustered at the municipality level.

Finally, the effects for subgroups of employees with neither an Austrian nor an EU8 nationality is presented in Table 3.6. Recapping our earlier findings, we observe an overall decrease in employment in municipalities in group 1 after 2004. Columns (2) and (3) show that it is the employment in blue-collar occupations and by males that fall most over time. Comparing the negative employment effects across age groups in columns (4) to (6), there is no group that stands out with a particularly large negative effect. Contrasting these effects to those on EU8 employment in Table 3.8, we can see that the increase in EU8 employment over time is larger for the blue-collar and male subgroup. At the same time, these are the subgroups that experience a larger decrease in the employment of non-EU8 foreign workers. The results are very similar when we additionally control for differential time trends in employment due to differences in the pre-existing industry structure across municipalities in columns (1) to (4) in Appendix Table C.4.

Taken together, the effects so far suggest that the large increase in EU8 employment in municipalities close to the border does not lead to a decline in the employment of Austrian nationals. If at all, they rather point to a complementary relationship between the group of (mainly blue-collar) EU8 and white-collar Austrian employees. At the same time, other foreign employees

Table 3.5: Heterogeneous effects on the employment of Austrians

				Age group		
	All	Blue	Male	≤ 30	$30-45$	$45<$
	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_{pre}^{[0-20]}$	-6.95	-4.95	-2.52	-4.19	-2.78	0.01
	(4.25)	(2.78)	(2.85)	(1.75)	(2.21)	(1.39)
$\delta_{trans}^{[0-20]}$	-3.74	-3.31	-1.28	-3.38	1.00	-1.36
	(5.68)	(3.10)	(3.71)	(1.75)	(2.95)	(1.90)
$\delta_{free}^{[0-20]}$	-3.42	-4.43	0.75	-3.36	4.73	-4.79
	(9.25)	(5.33)	(6.02)	(2.88)	(4.72)	(3.88)
$\delta_{pre}^{[20-60]}$	-5.64	-2.36	-1.22	-1.75	-4.81	0.92
	(3.72)	(2.26)	(2.53)	(1.73)	(1.81)	(0.97)
$\delta_{trans}^{[20-60]}$	10.71	2.56	6.20	2.01	5.30	3.40
	(5.32)	(2.54)	(3.51)	(1.70)	(2.76)	(1.53)
$\delta_{free}^{[20-60]}$	15.38	2.68	9.24	3.01	5.50	6.87
	(8.22)	(4.10)	(5.46)	(2.60)	(4.10)	(3.18)
Mean in 2003	279.93	160.24	166.34	80.74	138.49	60.70
Observations			26,695			
Municipalities			1,405			

Notes: Similar to Table 3.4, this table displays the impact of the EU Eastern enlargement on employment by Austrians based on equation (3.2). The dependent variable in column (1) is the employment of Austrian workers and in columns (2) to (6) the employment of Austrian workers in the corresponding subgroup. Standard errors are clustered at the municipality level.

appear to be substitutable with employees from EU8 countries. As shown in Section 3.4, this seems plausible given that EU8 and non-EU8 foreign workers in Austria have on average very similar observable characteristics that differentiate them from the average Austrian worker. Still, we have to be cautious in the interpretation of the effects given that we detect some pre-trends in the employment of Austrian nationals.

3.6.2 Monthly earnings

In this section, we investigate how the mean monthly earnings in municipalities evolve over the period around the EU Eastern enlargement depending on their distance to the closest EU8 border. In Figure 3.10 the panels to the left (right) show the difference in log mean earnings between municipalities within 0 to 20 (20 to 60) minutes and municipalities with a travel distance of more than 60 minutes from the next EU8 border. The difference is standardized to 0 in 2003.

Turning to the effects on Austrians in the upper panels, we observe that their average monthly earnings tend to increase after 2004 in group 1 and group 2 relative to the reference group. The difference is relatively constant over the years from 2005 to 2007 and then grows larger over time. By 2015, the mean earnings of Austrians in distance group 1 increases on average by more than 2% relative to the reference group. The increase is with 1 to 2% slightly smaller for group 2. In contrast, we estimate a relative decrease in the mean earnings of non-EU8 foreigners after approximately 2007. This effect is substantial and amounts to about 5% lower monthly

Table 3.6: Heterogeneous effects on the employment of non-EU8 foreigners

				Age group		
	All	Blue	Male	≤ 30	$30-45$	$45<$
	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_{pre}^{[0-20]}$	0.67	0.65	0.70	0.17	0.21	0.30
	(0.56)	(0.51)	(0.46)	(0.21)	(0.33)	(0.18)
$\delta_{trans}^{[0-20]}$	-1.96	-1.69	-1.70	-0.49	-1.10	-0.38
	(0.99)	(0.84)	(0.74)	(0.34)	(0.50)	(0.29)
$\delta_{free}^{[0-20]}$	-4.17	-3.73	-3.48	-0.87	-2.35	-0.95
	(1.97)	(1.59)	(1.39)	(0.67)	(0.92)	(0.56)
$\delta_{pre}^{[20-60]}$	-0.59	-0.38	-0.22	-0.24	-0.46	0.11
	(0.57)	(0.52)	(0.46)	(0.22)	(0.33)	(0.18)
$\delta_{trans}^{[20-60]}$	1.34	0.69	0.83	0.51	0.68	0.15
	(1.04)	(0.84)	(0.80)	(0.37)	(0.51)	(0.27)
$\delta_{free}^{[20-60]}$	3.29	1.71	2.11	0.93	1.50	0.86
	(2.18)	(1.59)	(1.55)	(0.71)	(1.04)	(0.58)
Mean in 2003	21.71	18.81	15.84	4.86	11.87	4.98
Observations			26,695			
Municipalities			1,405			

Notes: Similar to Table 3.4, this table displays the impact of the EU Eastern enlargement on employment by non-EU8 foreigners based on equation (3.2). The dependent variable in column (1) is the employment of non-EU8 foreigners and in columns (2) to (6) the employment of non-EU8 foreigners in the corresponding subgroup. Standard errors are clustered at the municipality level.

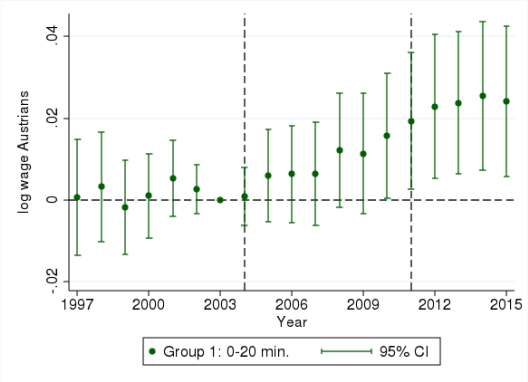
earnings in 2015. The estimated coefficients for the period before 2004 are not statistically different from 0 in all four panels in Figure 3.10. Hence, municipalities in all three groups were not experiencing diverging trends in their mean earnings before the EU Eastern enlargement in 2004. In columns (5) and (6) in Appendix Table C.4, we show that the effects on average earnings are robust when controlling for earnings trends over time related to the differential representation of industries across municipalities.

In Appendix Table C.2 and C.3, we decompose the effects on average earnings of Austrian and non-EU8 foreign workers, respectively, for different subgroups. For Austrian employees in group 1, we find the positive effects on average earnings to be larger for the age group 30 to 45. There is no effect on the earnings of male employees, suggesting that an increase in the earnings of women causes the estimated increase in average earnings. Notably, we find positive earnings effects for blue-collar workers. For Austrians in group 2, the patterns across subgroups are very similar to those described for group 1. For non-EU8 foreign workers in municipalities close to the border, we observe negative earnings effects across the subgroups except for workers older than 45 years. In municipalities in 20 to 60 minutes travel distance to the border, individuals experiencing no significant negative earnings impacts are those younger than 30 and older than 45 years.

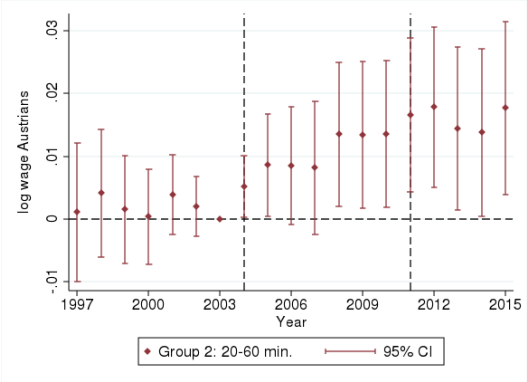
Taken together, we find that monthly earnings of Austrian and non-EU8 foreign workers in municipalities in the Austrian border region change in opposite directions after 2004. While Austrians experience an increase of approximately 2% in their earnings, non-EU8 foreign work-

Figure 3.10: Effects of the EU Eastern enlargement on earnings

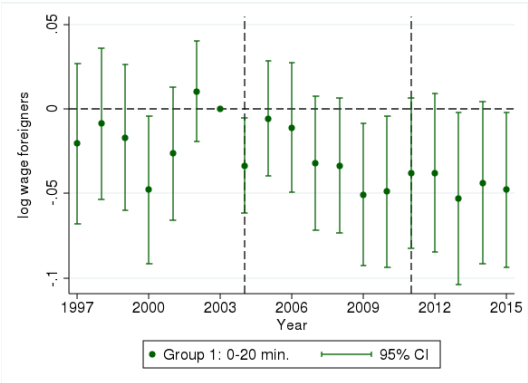
(a) Austrian - Group 1



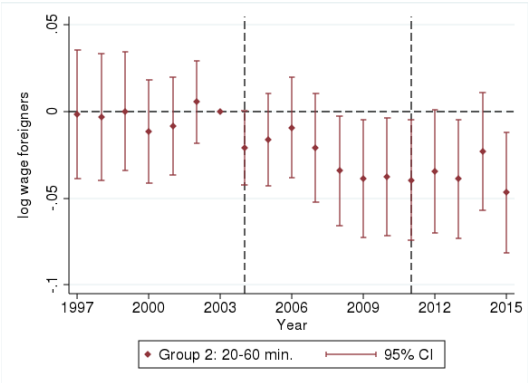
(b) Austrian - Group 2



(c) Non-EU8 foreigner - Group 1



(d) Non-EU8 foreigner - Group 2



Notes: Figure 3.9 shows the estimated coefficients $\widehat{\delta_y^{[0-20]}}$ (green) and $\widehat{\delta_y^{[20-60]}}$ (red) over time with the corresponding 95% confidence intervals based on estimation equation (3.1), which includes municipality and year-by-regions fixed effects. The outcome variable Y_{mt} in the upper (bottom) graphs is equal to the log average monthly earnings (real, 2000 prices) of Austrian (non-EU8 foreign) workers. Standard errors are clustered at the municipality level.

ers face substantial decreases of almost 5% in 2015. In contrast to the employment effects, these effects are very similar across municipalities in distance groups 1 and 2. These observed earnings changes can be driven both by effects for those who remain employed and by compositional effects. The latter might occur if individuals selectively leave or enter employment in municipalities that are closer to the border after the EU Eastern enlargement (see for example Borjas (2006) and Bratsberg and Raaum (2012)).

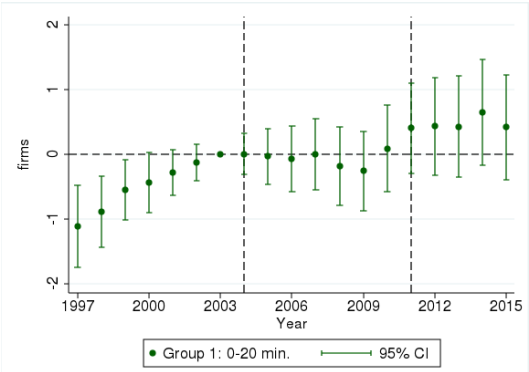
3.7 Effects on firms

The previous section illustrates that municipalities within one hour distance to the closest EU8 border experience large increases in the employment of EU8 nationals after the EU enlargement of 2004 compared to municipalities further away. We now want to examine whether and how the additional labor supply of EU8 migrant workers changes the number of firms in the

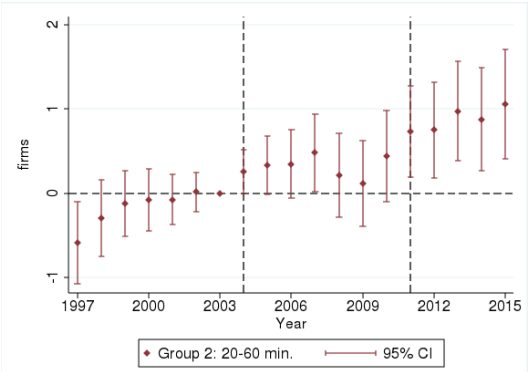
affected regions. Furthermore, we want to investigate which types of firms employ these EU8 workers. Do they rather enter established firms with mainly Austrian or other foreign workers? Alternatively, is there selection of EU8 workers into specific firms?

Figure 3.11: Effects of the EU Eastern enlargement on the number of firms

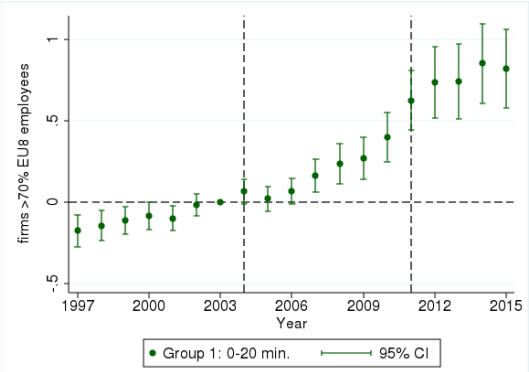
(a) Number of all firms - Group 1



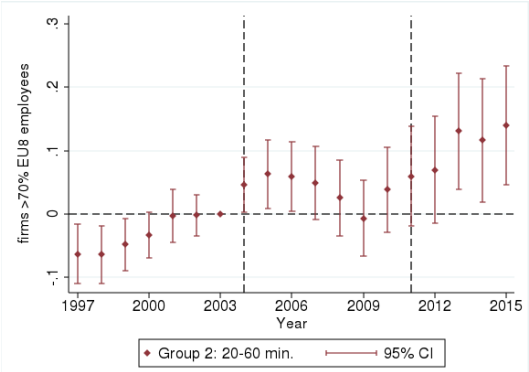
(b) Number of all firms - Group 2



(c) Number of firms with $> 70\%$ EU8 workers - Group 1



(d) Number of firms with $> 70\%$ EU8 workers - Group 2



Notes: Figure 3.11 shows the estimated coefficients $\widehat{\delta_y^{[0-20]}}$ (green) and $\widehat{\delta_y^{[20-60]}}$ (red) over time with the corresponding 95% confidence intervals based on estimation equation (3.1), which includes municipality and year-by-regions fixed effects. The outcome variable Y_{mt} in the upper panels is the number of all firms with more than 5 employees. In the bottom panels, Y_{mt} is the number of firms with more than 70% of their workforce being EU8 workers. Standard errors are clustered at the municipality level.

In Figure 3.11 we show the yearly effects of the EU Eastern enlargement on the number of firms in municipalities within 0 to 20 (20 to 60 minutes) minutes travel distance to the closest EU8 border in the left (right) panels. In the upper panels, we look at the number of all firms. Before 2004, the number of firms in group 1 increases on average faster than in the reference group. In contrast, the number of firms in the two groups changes similarly during the transition period. After 2011, there is a relative increase in the total number of firms in group 1, but it is not statistically significantly different from 0. For municipalities in group 2, we observe a relative increase in the number of firms after 2004. This increase gets larger after EU8 nationals gained access to the Austrian labor market in 2011. In the lower part of Figure 3.11 we focus on the number of firms that have a predominantly EU8 workforce. These are firms in which more

than 70% of all employees have an EU8 nationality. In municipalities within 0 to 20 minutes distance to the border, we observe a large and significant increase in the number of this type of firms before 2004, during the transition period and in particular after 2011. By 2015, there is, on average, one more firm with a predominantly EU8 workforce in municipalities very close to the border relative to those further away. This effect is large given that there are, on average, 14 firms per municipality in 2003. For municipalities within 20 to 60 minutes travel distance from the border, we observe a positive effect on the average number of firms with a mainly EU8 workforce immediately after 2004 and throughout the period after 2012.

Table 3.7: Effects of the EU Eastern enlargement on the number of firms

		> 70% employees			
	All	Blue collar	Austrian	Non-EU8 foreign	EU8
	(1)	(2)	(3)	(4)	(5)
$\delta_{pre}^{[0-20]}$	-0.56	-0.41	-0.32	0.03	-0.10
	(0.20)	(0.15)	(0.19)	(0.02)	(0.04)
$\delta_{trans}^{[0-20]}$	-0.06	-0.05	-0.24	-0.04	0.18
	(0.24)	(0.17)	(0.20)	(0.03)	(0.04)
$\delta_{free}^{[0-20]}$	0.47	0.46	-0.69	-0.03	0.76
	(0.37)	(0.25)	(0.29)	(0.04)	(0.11)
$\delta_{pre}^{[20-60]}$	-0.19	-0.12	-0.06	0.02	-0.04
	(0.16)	(0.12)	(0.16)	(0.02)	(0.02)
$\delta_{trans}^{[20-60]}$	0.31	0.13	0.12	0.03	0.04
	(0.19)	(0.13)	(0.18)	(0.02)	(0.02)
$\delta_{free}^{[20-60]}$	0.88	0.56	0.05	0.16	0.10
	(0.29)	(0.18)	(0.25)	(0.04)	(0.04)
Mean in 2003	14.14	8.19	12.26	0.11	0.16
Observations			26,695		
Municipalities			1,405		

Notes: Similar to Table 3.4, this table displays the impact of the EU Eastern enlargement on the number of firms based on equation (3.2). In column (1) the dependent variable is the number of all firms with more than five employees. In columns (2) to (5) the dependent variable is the number of all firms with more than 70% of all employees being blue-collar, Austrian, non-EU8 foreign and EU8 employees, respectively. Standard errors are clustered at the municipality level.

In Table 3.7 we show the average effects on the number of firms across the three periods. In column (1), we find a relative increase in the number of firms comparing municipalities within 0 to 20 minutes travel distance to the border to those in the reference group. However, this relative rise in the number of firms begins already before the EU Eastern enlargement. The same pattern occurs if we only look at the number of firms with a predominantly blue-collar or Austrian workforce in columns (2) and (3). Turning to the effects on firms with mainly EU8 workers, we again estimate that there is a significant difference between the two distance groups already in the pre-period. This difference expands, and by 2015, municipalities in group 1 experience an average increase of 0.8 firms with a predominantly EU8 workforce relative to the reference group. The average number of firms in municipalities in group 2 evolves similarly to the reference group before 2004. This is also the case if we focus on subgroups of firms in

columns (2) to (5). After the EU Eastern enlargement, we see a relative increase in the number of firms in group 2. This increase is mainly due to a rising number of firms with a predominantly blue-collar workforce. Roughly 10% of the extra firms have a predominantly EU8 workforce.
To sum up, we find that the EU Eastern enlargement tends to increase the total number of firms in labor markets close to the EU8 border. This is in line with the relatively few existing papers that examine the labor demand side effects of immigration. In particular, we find that the number of firms with a predominantly EU8 workforce increases substantially in municipalities within 0 to 20 minutes commuting distance to the border. This can be seen as suggestive evidence that EU8 workers do not mainly compete with other workers for positions in already existing firms. They rather provoke the entry of new firms that build up their workforce by hiring them.

3.8 Conclusions and outlook

In this paper, we study how local labor markets are affected when receiving a large number of workers by examining the increased immigration to the Austrian eastern border region after the EU enlargement of 2004. We combine the temporal variation in immigration resulting from this policy change with the spatial variation that comes from the tendency of immigrants to go to municipalities that are close to their home country, thus identifying the effect of immigration on the employment and earnings of different subgroups of workers and on firms. Our analysis is based on Austrian social security data, which provides us with detailed information on the universe of employees and employers in Austria over several years.
We find that the EU Eastern enlargement led to significant changes in Austrian local labor markets, with a pronounced immigration pattern of EU8 workers following the lifting of restrictions to their labor market access. Over the period from 1997 to 2015, the employment share of EU8 nationals in the Austrian border region increased from 2.2% to 9.3%. The increase after the enlargement of 2004 is larger in municipalities that are located closer to the border to an EU8 country and is almost entirely driven by an increase in blue-collar employment. The responses of other workers differ substantially between Austrian and other immigrant workers: There is no decline in the employment of Austrian nationals in municipalities close to the border. For non-EU8 foreign workers, average employment decreases significantly and steadily in municipalities that are within 0 to 20 minutes to the border after 2004. Moreover, Austrians experience an increase of approximately 2% in their earnings, non-EU8 foreign workers face substantial decreases of almost 5% by 2015. Finally, we find that immigration leads to an increase in the number of firms. This increase is mainly due to a rise in the number of firms with a predominantly blue-collar workforce and to a minor part due to firms with a mainly EU8 workforce.
We should note, however, that we observe differential trends in some outcome variables between municipalities that are within 0 to 20 or 20 to 60 minutes travel distance to the EU8 border compared to those further away already before the EU enlargement of 2004. This gives rise to further research, in which we can further develop our empirical strategy and also apply

alternative strategies to see whether the results are robust.
The results suggest further interesting research in various directions. We have very rich longitudinal data in which we can follow individuals over time. Hence, we can analyze individual mobility and responses across municipalities. With this, we can examine the plausibility of our identification assumptions that rule out significant spillovers between different regions. We can further decompose the employment responses to immigration into inflows and outflows from and into employment in other regions and nonemployment (Dustmann et al., 2016). Finally, we can take into account the selection into employment when analyzing the effects of immigration on average earnings across regions over time.

Chapter 4

Mortality in Midlife for Subgroups in Germany

Published as:

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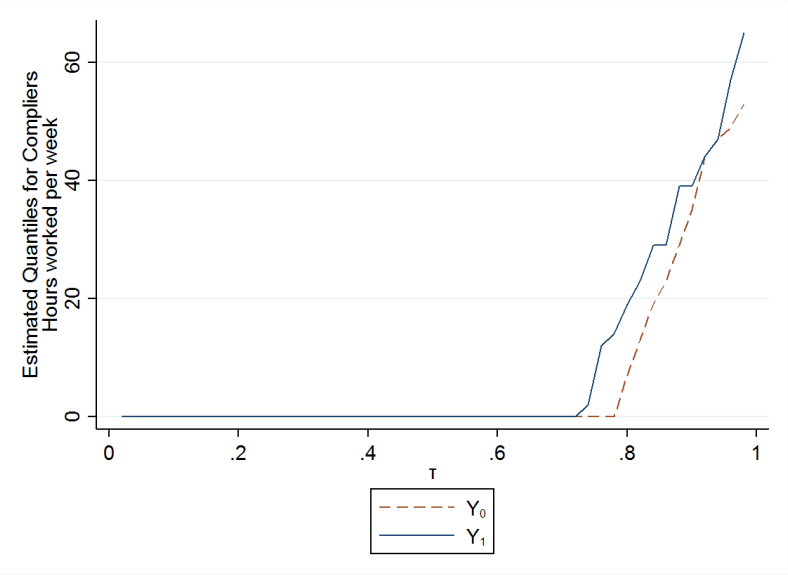
Appendix A

Appendix to Chapter 1

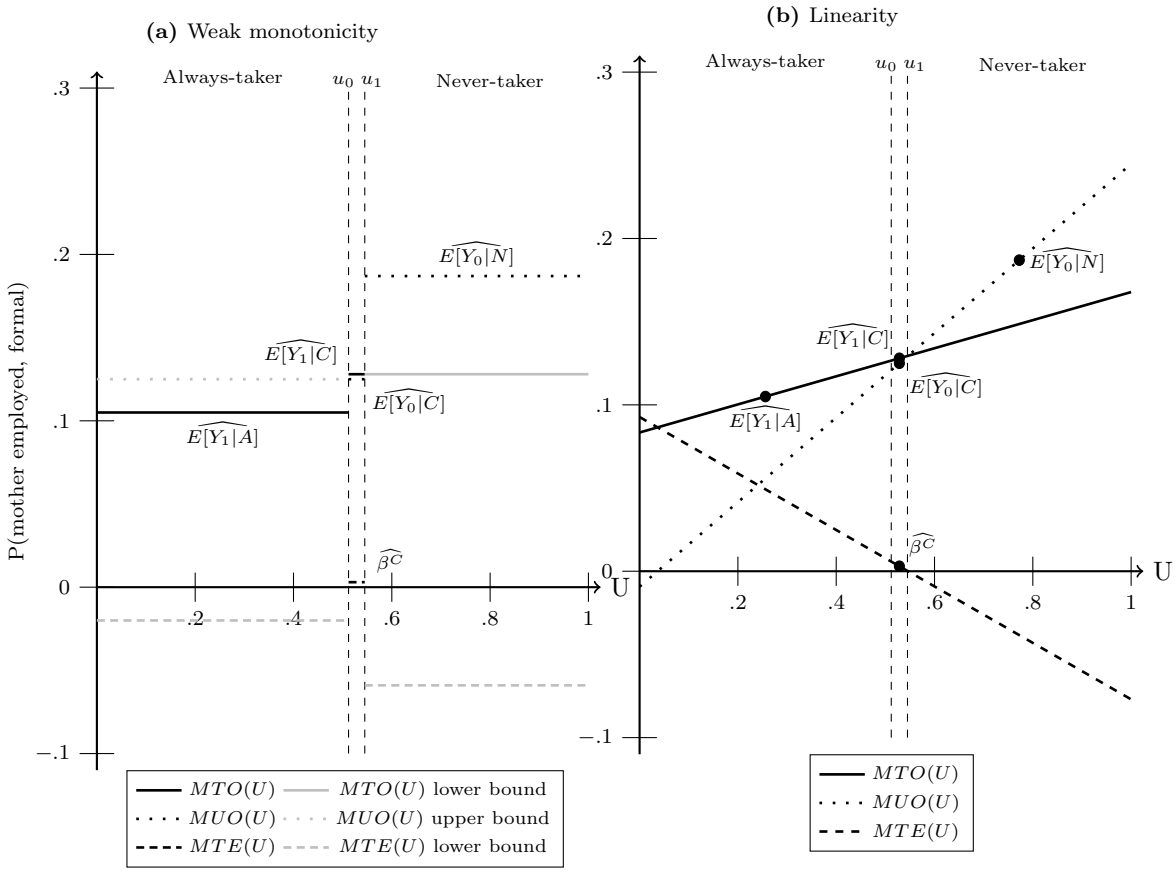
A.1 Additional figures

Figure A.1: Distributional impact of fertility on mother's hours worked per week



Notes: This graph illustrates the estimated quantiles for the potential outcome distributions of mother's total weekly working hours. The solid blue and the dashed red lines show the estimated quantiles of working hours for compliers with more than two children, $\hat{Q}^{\tau}_{Y_1|c}$, and with only two children, $\hat{Q}^{\tau}_{Y_0|c}$, respectively. The model includes the control variables that are listed in Table 1.3.

Figure A.2: Estimated marginal outcomes and MTE for formal employment



Notes: This figure illustrates point estimates and estimated bounds for the (marginal) treated outcomes (MTO), untreated outcomes (MUO), and treatment effects (MTE) as functions of the unobserved resistance toward having more than two children, U. The left panel assumes weak monotonicity, whereas the right panel assumes linearity in the marginal treated and untreated outcome.

A.2 Additional tables

Table A.1: Sample compared to a broader group of Mexican women

	Women aged [21–50] yrs.		Women aged [21–35] yrs.		Women in sample	
Age (yrs)	34.11	(8.50)	27.74	(4.33)	29.39	(3.83)
Age at first birth (yrs)	22.341	(5.100)	20.471	(4.015)	19.762	(3.080)
Married or cohabiting	0.72	(0.45)	0.68	(0.47)	0.91	(0.29)
Primary completed	0.25	(0.43)	0.18	(0.39)	0.62	(0.48)
Secondary completed	0.51	(0.50)	0.53	(0.50)	0.12	(0.32)
University completed	0.17	(0.37)	0.20	(0.40)	0.03	(0.17)
Children in household	1.94	(1.64)	1.62	(1.50)	2.84	(1.05)
At least 2 children	0.58	(0.49)	0.49	(0.50)	1.00	(0.00)
More than 2 children	0.32	(0.47)	0.25	(0.44)	0.53	(0.50)
Employed	0.36	(0.48)	0.34	(0.47)	0.25	(0.43)
Labor market outcomes for employed						
Informal employment	0.37	(0.48)	0.32	(0.47)	0.44	(0.50)
Hours worked per week	39.53	(19.69)	40.04	(19.30)	37.19	(20.78)
Monthly income (Pesos)	3778.94	(3434.82)	3655.94	(3174.08)	2958.68	(2507.40)
Observations	2,411,685		1,372,126		505,569	

Notes: Statistics depicted are means with standard deviations in parentheses. All variables are measured at the time of the survey. The labor market outcomes refer to the week before the survey.

Table A.2: Local quantile treatment effects of maternal working hours

τ	.02	...	0.72	0.74	0.76	0.78	.80	.82	.84	.86	.88	.90	.92	.94	.96	.98
Hours worked per week																
$\hat{\Delta}^{\tau}_{IV}$	0	...	0	1	12	14	12	10	10	6	10	4	0	0	8	12
	(0.30)	...	(0.13)	(15.2)	(37.8)	(1.26)	(6.40)	(5.72)	(5.26)	(5.84)	(13.12)	(4.91)	(6.80)	(2.03)	(9.93)	(6.82)
$\hat{Q}^{\tau}_{Y_0\|c}$	0	... 0	0	0	0	0	7	13	19	23	29	35	44	47	49	53
Hours worked per week, formal																
$\hat{\Delta}^{\tau}_{IV}$	0	...	0	0	0	0	0	0	0	0	6	10	15	0	0	-2
	(0.37)	...	(0.12)	(0.12)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(0.11)	(74.44)	(7.81)	(3.91)	(11.85)	(3.20)	(2.52)
$\hat{Q}^{\tau}_{Y_0\|c}$	0	...	0	0	0		0	0	0	0	3	19	24	39	47	49
Hours worked per week, informal																
τ	0	...	0	0	0	0	0	0	0	0	13	15	14	13	3	12
	(0.34)	...	(0.11)	(0.11)	(0.10)	(0.09)	(0.09)	(0.09)	(0.09)	(0.08)	(3.12)	(3.32)	(6.03)	(15.04)	(12.66)	(7.15)
$\hat{\Delta}^{\tau}_{IV}$	0	...	0	0	0	0	0	0	0	0	0	0	9	14	35	47

Notes: The table reports the results of the local quantile treatment effects estimation as proposed in Frölich and Melly (2013). $\hat{\Delta}^{\tau}_{IV}$ gives the quantile treatment effect estimate for compliers at quantile τ with estimated asymptotic standard errors reported in parenthesis below the corresponding estimates. $\hat{Q}^{\tau}_{Y_0|c}$ denotes the estimated τ quantile of $Y_0|c$. In the columns (quantiles 0.04 to 0.70) that are left out, $\hat{\Delta}^{\tau}_{IV}$ and $\hat{Q}^{\tau}_{Y_0|c}$ are (as in the neighboring quantiles) equal to 0 (see also Appendix Figure A.1 and Figure 1.2). Control variables are as in Table 1.3.

Table A.3: Outcomes by compliance type

	Formal Empl.		Informal Empl.		Weekly Hours	
$\mathbf{D_i}$	1	0	1	0	1	0
Type						
Always-taker	0.105	.	0.103	.	7.52	.
Complier	0.128	0.125	0.131	0.095	9.36	7.541
Never-taker	.	0.187	.	0.107	.	11.20

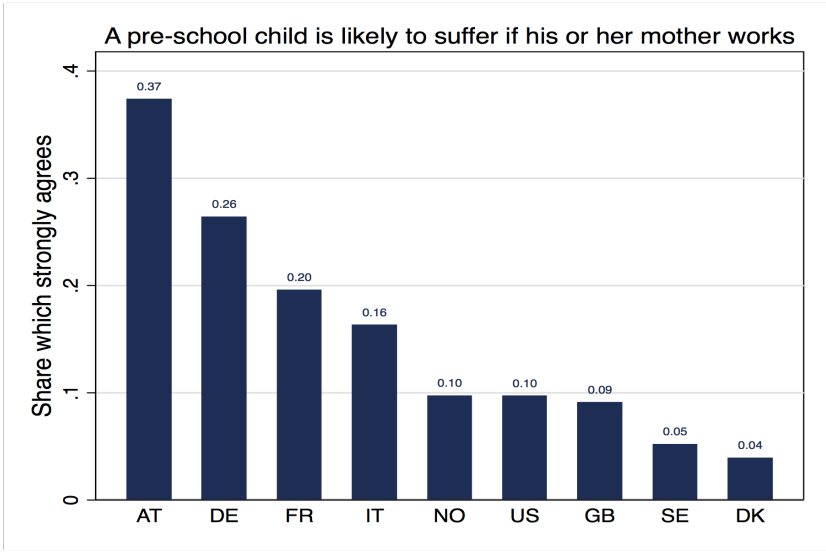
Note: This table shows the estimated average potential outcomes of the compliance types with and without treatment. The outcome variable in the left (central) panel is an indicator equal to 1 if a mother is formally (informally) employed. The outcome variable in the right panel is equal to the maternal working hours per week.

Appendix B

Appendix to Chapter 2

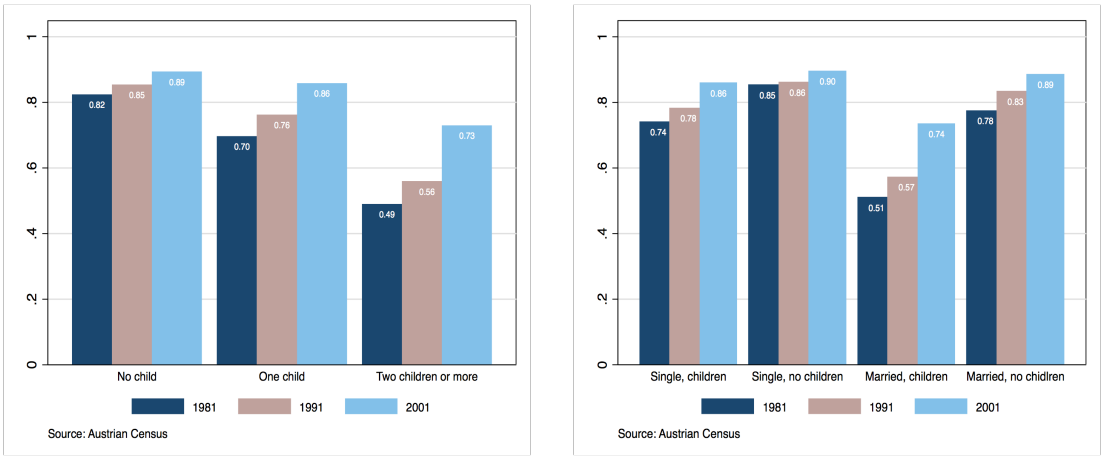
B.1 Additional figures

Figure B.1: Social norms regarding working mothers in selected countries



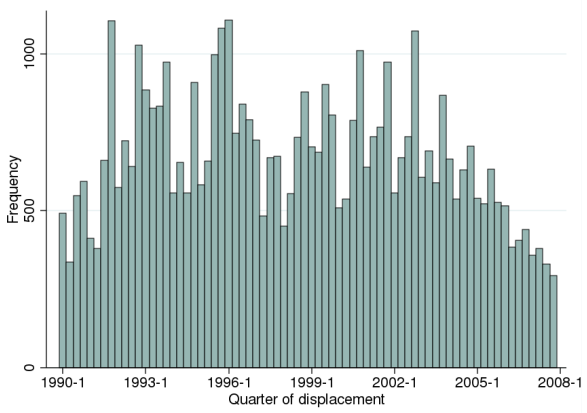
Notes: This figure is based on data from the *European and World Values Surveys* and include female (male) respondents between 25 and (55) years of age. The original survey questions is as follows '*A pre-school child is likely to suffer if his or her mother works*'. Respondents evaluate this statement on an ordered scale from '*Agree strongly*' (1), '*Agree*' (2), '*Neither agree nor disagree*' (3), '*Disagree*' (4), to '*Strongly disagree*' (5). In the case of some country-years the respondents where given a 4-point scale to answer, which does not include the answer possibility (3). The graph shows the share or respondents (by country), which strongly agrees with this statement. The data comprises for each country observations from at least two points in time (between 1990 and 1999). The total number of observations is 11, 574.

Figure B.2: Female labor force participation by family status, number of children, and year



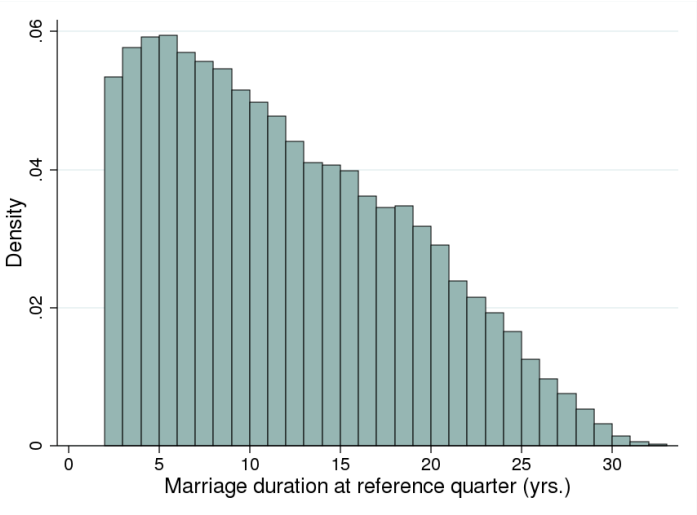
Notes: This figure shows the female labor force participation (for women between 25 and 54 years of age) by family status and year (left graph), and by the number of children and year (right graph). The figures are based on Austrian census data from the years 1981, 1991, and 2001.

Figure B.3: Number of displaced workers over time



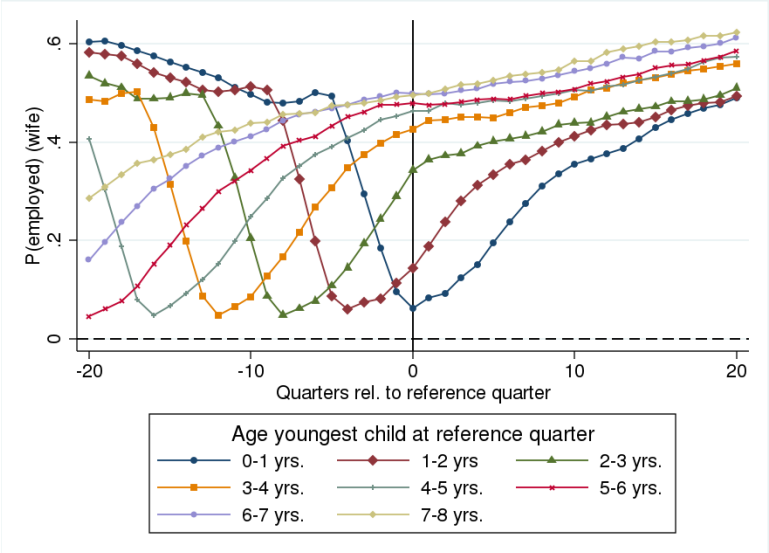
Notes: This figure shows the number of displaced workers for each quarter from 1990 Q1 to 2007 Q4. Workers are displaced through a firm closure or mass layoff event.

Figure B.4: Marriage duration at the reference quarter



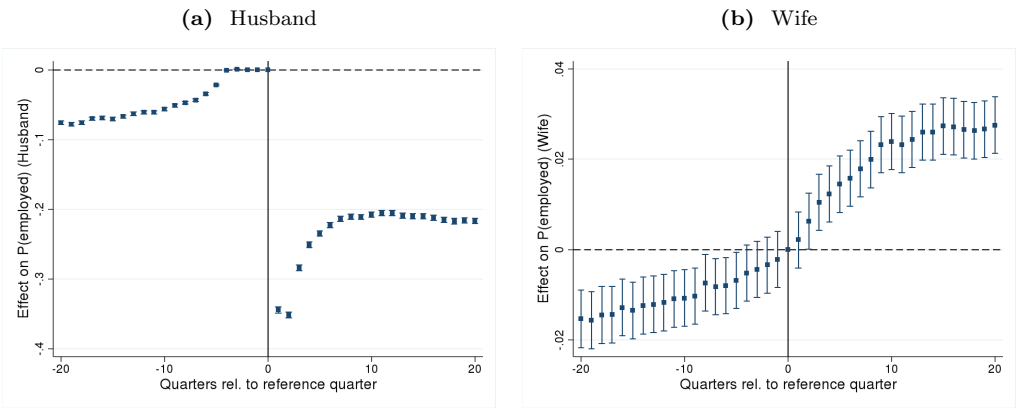
Notes: The figure shows the distribution of marriage durations at the reference date in years.

Figure B.5: Wives' employment by the age of the youngest child



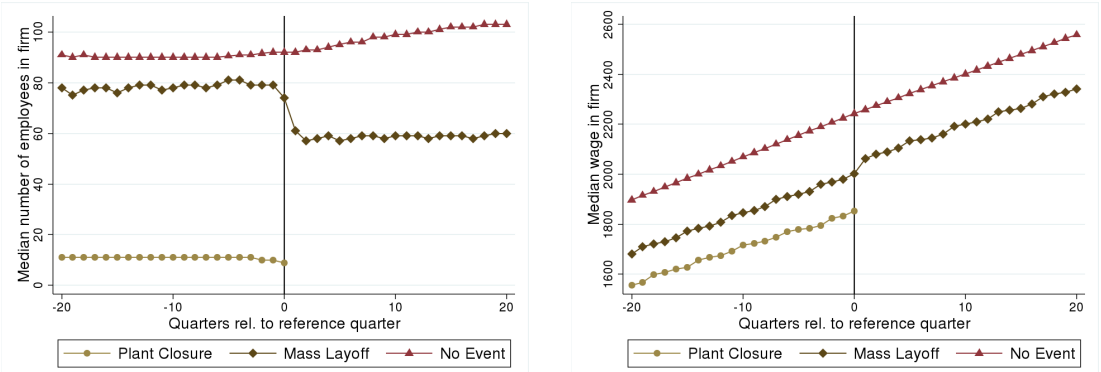
Notes: This figure shows the mean employment probability for subsamples of wives of displaced husbands with different ages of their youngest child at the reference date.

Figure B.6: Employment of displaced husbands and their wives controlling for marriage duration and quarter fixed effects



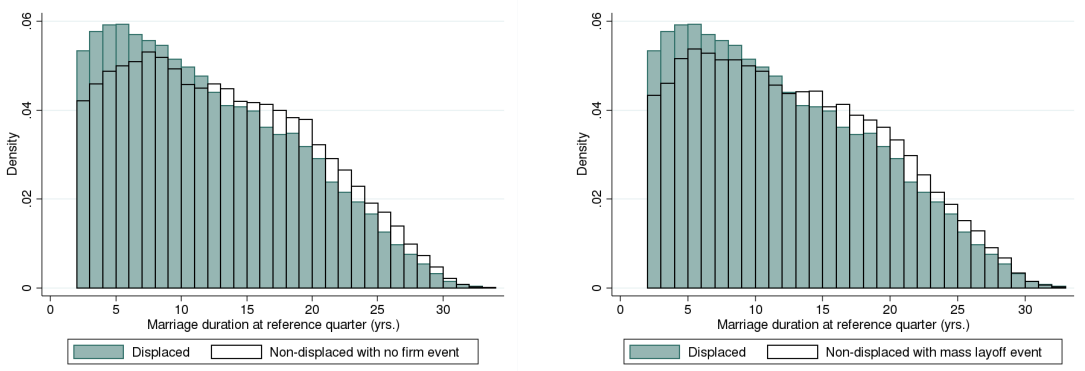
Notes: Panel (a) and (b) plot the probability that a displaced husband and his wife, respectively, is employed relative to the reference date holding constant the marriage duration and quarter of observation. We obtain the former by regressing an indicator of husband/wife being employed on indicators for the quarterly distance to the reference quarter, indicators for the marriage duration, and indicators for the quarter of observation.

Figure B.7: Employment and wages of firms around the reference date



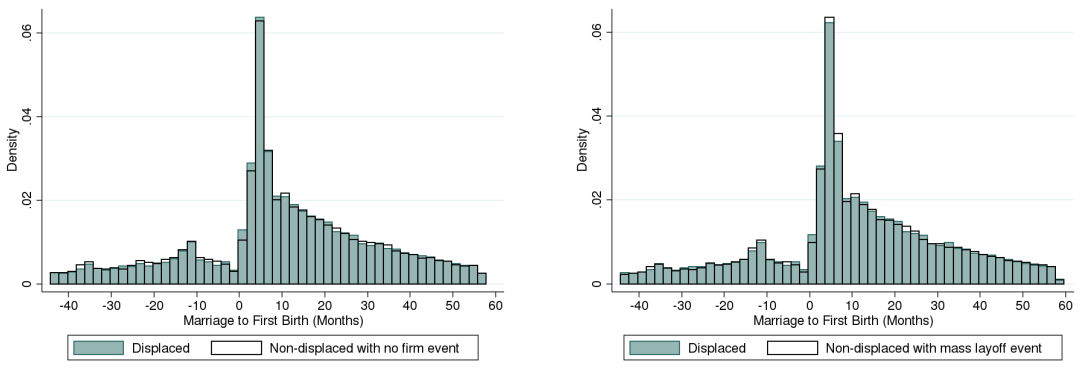
Notes: This figure plots the median number of employees and the average median monthly wage (in Euro, 2000 prices) over time for the employers in our sample. *Plant Closure* refers to firms that close down the quarter following the reference date. *Mass Layoff* refers to firms that reduce employment by at least 5% of their workforce the quarter after the reference date. *No Event* firms have neither a mass layoff nor closure the quarter following the reference date. For each quarter around the reference date, we include one observation per existing firm. We include any firm that employs at least one husband of our sample.

Figure B.8: Marriage duration at the reference date by treatment status



Notes: These graphs shows the distribution of marriage durations at the reference date. The graphs display the distribution for the sample of households experiencing a displacement at the reference date (green). The graph to the left adds the distribution of those with no firm event at the reference date (transparent); the graph to the right adds households with husbands working in mass layoff firms who keep their jobs (transparent). We include one observation per household event.

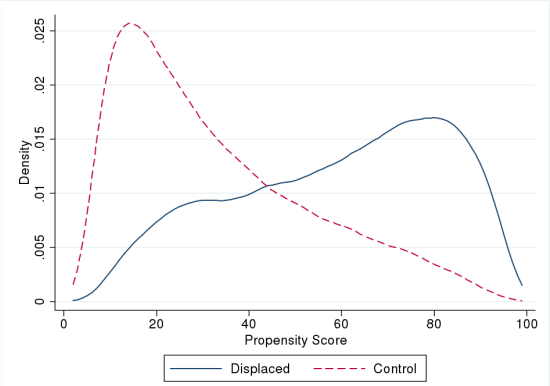
Figure B.9: Distance between marriage and first birth by treatment status



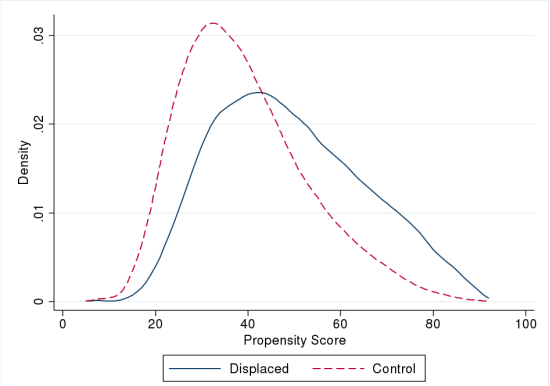
Notes: This figure shows the distribution of the distance from marriage to the birth of the first child in months. The graphs display the distribution for the sample of households experiencing a displacement at the reference date (green). The graph to the left adds the distribution of those with no firm event (transparent); the graph to the right adds households with husbands working in mass layoff firms who keep their jobs (transparent). We include one observation per household event.

Figure B.10: Propensity score distributions

(a) CG1: No firm event



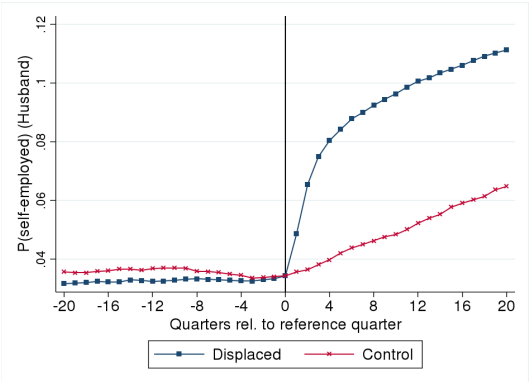
(b) CG2: Non-displaced mass layoff



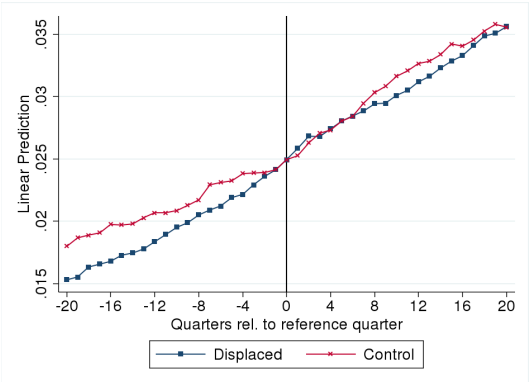
Notes: This figure shows the density distribution of the propensity score in the displaced and control groups. Panel (a) refers to the group of displaced and the group of households with husbands with no firm event. Panel (b) refers to the group of displaced and the group of households with husbands that have a mass layoff at the reference date, but are not displaced.

Figure B.11: Self-employment of displaced husbands and their wives, CG1

(a) Probability that husband is self-employed



(b) Probability that wife is self-employed

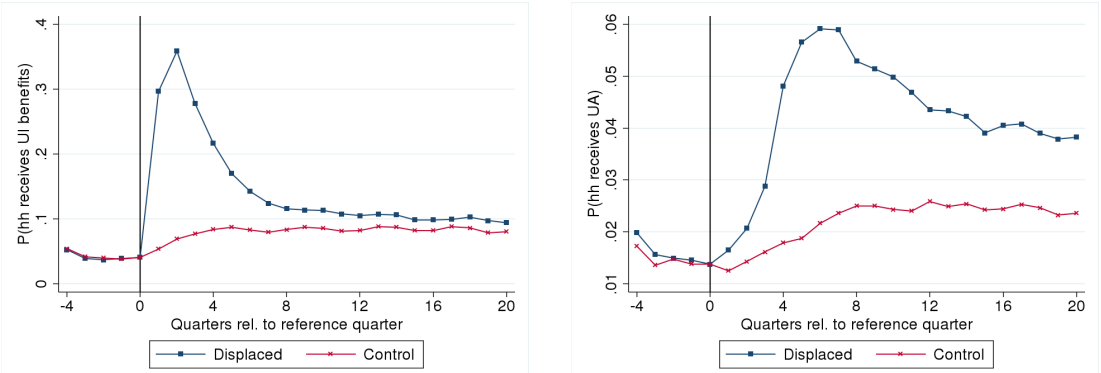


Notes: Figure (a) compares the probability of being self-employed of displaced husbands (blue, square) to husbands without firm event at the reference date (red, x). Figure (b) compares the probability of being self-employed of wives with displaced husbands (blue, square) to those with husbands without firm event at the reference date (red, x). This figure is constructed in the same way as Figure 2.4.

Figure B.12: Social benefits around displacement, CG2

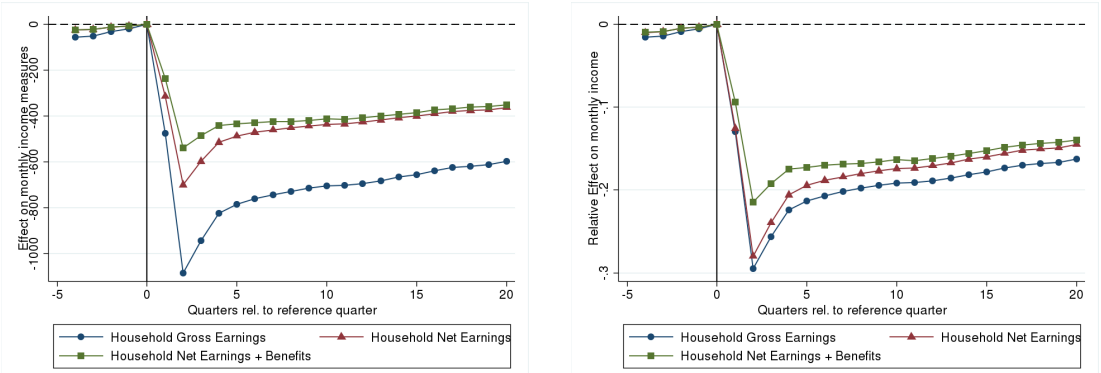
(a) Probability that household receives unemployment insurance benefits (UB)

(b) Probability that household receives unemployment assistance (UA)



Notes: Comparison of the probability of receiving benefits of households with displaced husbands (blue, square) to those with non-displaced husbands working at mass layoff employers at the reference date (red, x). The control group is reweighted to resemble the displaced group within each subgroup as explained in Figure 2.4. The employment probability of the control group is adjusted by its mean difference relative to the displaced group. With some exceptions, job losers can receive UB for up to 30 weeks. After exhausting UB, job losers can obtain means-tested income support, UA, that pays a lower level of benefits indefinitely.

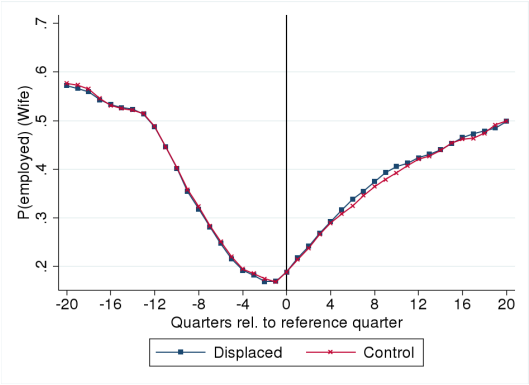
Figure B.13: Displacement effect on household income, CG2



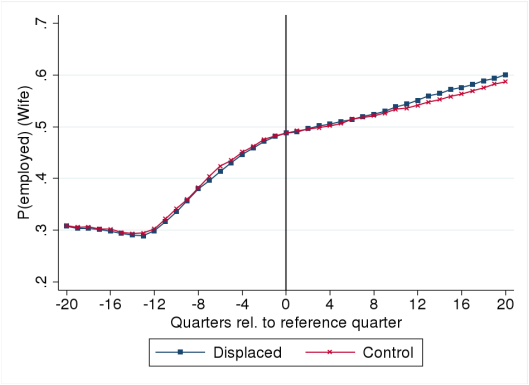
Notes: This figure shows the effect of husband's displacement on monthly household income measures (in Euro, 2000 prices). The effect is given by the difference between households that experience a displacement and reweighted and mean-adjusted households with non-displaced husbands who work at mass layoff employers at the reference date. *Household Gross Earnings* is the sum of husband's and wife's labor earnings in each quarter according to tax data. *Household Net Earnings* subtracts social security contributions and payroll taxes from the former. *Household Net Earnings + benefits* adds benefits from unemployment insurance and unemployment assistance.

Figure B.14: Employment of displaced husbands' wives by the age of the youngest child, CG2

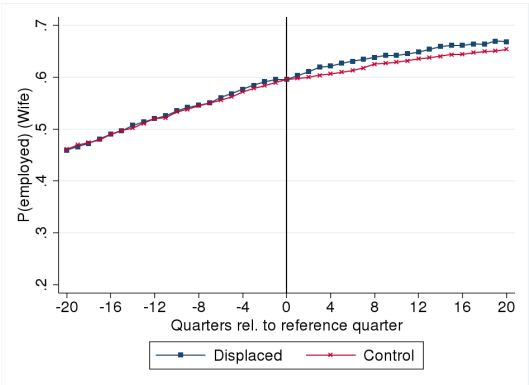
(a) 0-2 yrs.



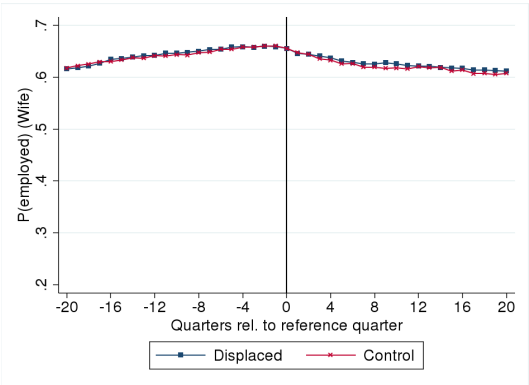
(b) 3-9 yrs.



(c) 10-15 yrs.



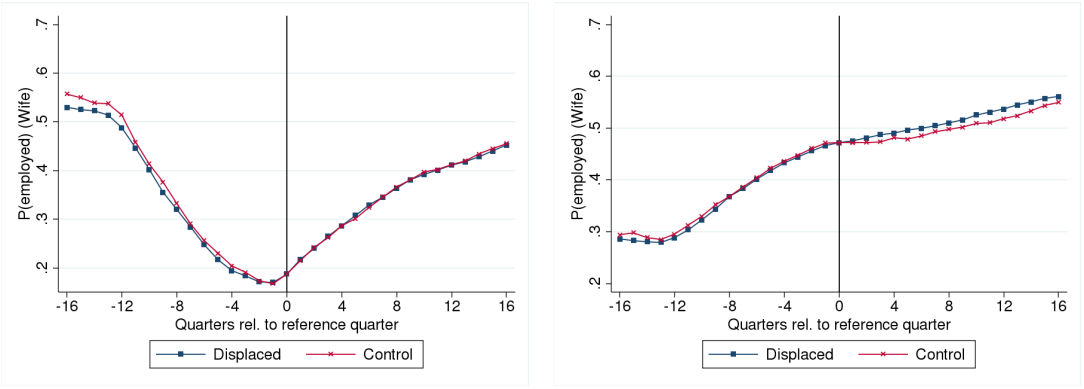
(d) No children aged 16 yrs. or younger



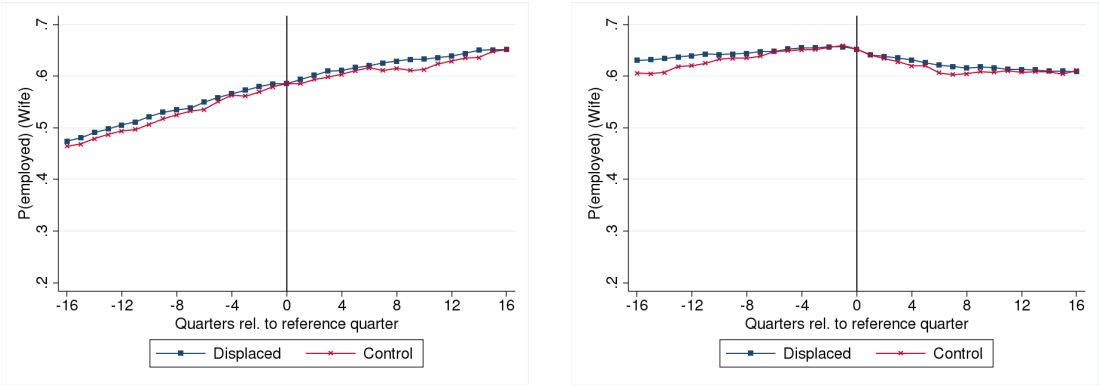
Notes: Comparison of the probability to be employed of wives with displaced husbands (blue, square) to those with non-displaced husbands working at a mass layoff firm at the reference date (red, x) for subgroups defined by the age of the youngest child at the reference date. The control group is reweighted to resemble the displaced group within each subgroup as explained in Figure 2.4. The employment probability of the control group is adjusted by its mean difference relative to the displaced group.

Figure B.15: Employment of displaced husbands' wives by the age of the youngest child, CG3

(a) 0-2 yrs. **(b)** 3-9 yrs.



(c) 10-15 yrs. **(d)** No children aged 16 yrs. or younger



Notes: Comparison of the probability to be employed of wives with displaced husbands (blue, square) to those with husbands displaced 16 quarters after the reference date (red, x) for subgroups defined by the age of the youngest child at the reference date. The employment probability of the control group is adjusted by its mean difference relative to the displaced group.

B.2 Additional tables

Table B.1: Wife's labor supply elasticities in added worker effect studies

	Country	Time	Data	Sample households	Wife's labor supply (semi-)elasticity		Comments
1.Variation in spousal income in a structural life-cycle model of household labor supply							
Haan and Prowse (2015)	DE	1991-2005	GSOEP	Married couples aged 16–65 with labor market experience	Participation without leisure complementarity	-0.025^{a} -0.056^{a}	Comparison of simulated optimal behavior when spouse is vs. is not subject to unanticipated job destruction
Blundell et al. (2016)	US	1999-2009	PSID	Households with participating and married male head aged 30–57	Hours (total response) Extensive margin	-0.75 -0.168	Permanent shock in spousal wage process identified in structural model
Blundell et al. (2018)	US	1999-2015	PSID	Married couples with wife aged 25–65			Permanent shock in spousal wage process identified in structural model
				with children aged ≤ 10	Hours (total response) Extensive margin Intensive margin	-0.296 -0.193 -0.170	
				no children aged ≤ 10	Hours (total response) Extensive margin Intensive margin	-0.14 -0.065 -0.088	
2.Quasi-experimental variation in spousal income through job displacements							
Stephens (2002)	US	1968-1992	PSID	Married couples aged 25–65	Hours	-0.50	Displacement through plant closure/moving, layoff, firing
Kohara 2010[b]	JP	1993-2004	Panel survey	Wife aged 24-35 in 1993	Hours	-0.893^{a}	Layoff, plant closure, and bankruptcy
Eliason (2011)	SE	1987	Admin panel	Married couples aged 25–51	Earnings	0.44	Plant closure
Hardoy and Schøne (2014)	NO	2002	Admin panel	Married couples aged 25–55	Employment Earnings	0.09 0.07	Closure, mass layoff; couple required to stay married in post-treatment period
				with wife not employed at displacement	Earnings	-0.5	
Bredtmann et al. (2018)	C-EU	2004-2013	EU-SILC	Married/cohabiting couples aged 16–65	Employment	0.12^{a}	Continental Europe (C-EU) refers to AT, BE, DE, FR, LU, and the NL
3.Quasi-experimental variation in spousal income through social insurance benefits							
Cullen and Gruber (2000)	US	1984-88, 1990-92	SIPP	Married couples aged 25–54	Hours	$[-0.49,-1.07]$	Lower and upper bound estimates, variation in spousal UI benefits
Autor et al. (2019)	NO	1989-2011	Admin panel	Married couples, one spouse (< 62) applying for DI benefits	Employment	-0.345	Simulated response to permanent change in spousal income in structural model, no separate elasticities by sex
Fadlon and Nielsen (2017)	DK	1980-2011	Admin panel	Married/cohabiting couples, widows (< 67) with spouse dying at age 45–80	Participation	-0.13	Variation in spousal survivor benefits

Notes: The (semi-)elasticity refers to the change in wife's labor supply to a 1% change in husband's income. For the elasticity of hours and earnings, the wife's response is relative to the baseline mean (in %). For the participation and employment elasticity, the response is in absolute terms (in percentage points). [a] Assuming a mean husband's income loss of 20%. [b] This study is published in the *Journal of Population Economics* Volume 23(4). The details for all other listed studies can be found in the List of References in the paper.

Table B.2: Gender identity norms and beliefs on child care in Austria and some selected high-income countries

	Share of survey respondents which strongly agrees with the respective statement across countries									
	AT	DE	DK	FR	IT	NO	SE	GB	US	Total
1.) A pre-school child is likely to suffer if [. . .] mother works	0.37	0.26	0.04	0.20	0.16	0.10	0.05	0.09	0.10	0.17
2.) A working mother [as good as] a mother who does not work	0.23	0.18	0.56	0.51	0.19	0.47	0.51	0.23	0.29	0.32
3.) Important for successful marriage: Sharing household chores	0.28	0.23	0.45	0.39	0.30	0.34	0.55	0.45	0.49	0.36
4.) Both husband and wife should contribute to household income	0.29	0.17	0.26	0.39	0.25	0.35	0.54	0.14	0.23	0.28

Notes: These figures are based on data from the *European and World Values Surveys* and include male respondents between 25 and 55 years of age, and female respondents between 25 and 50 years of age. The original survey questions on statement 1 is as follows '*A pre-school child is likely to suffer if his or her mother works*'. The original survey questions on statement 2 is as follows '*A working mother can establish just as warm and secure a relationship with her children as a mother who does not work*'. The original survey questions on statement 3 is as follows '*Important for successful marriage: Sharing household chores*'. Respondents are asked to evaluate this statement on an ordered scale from '*Very*' (1), '*Rather*' (2), to '*Not very*' (3). The table summarizes the share or respondents (by country), which strongly agrees with statements 1 to 3, and which answers statement 4 with very important. The original survey questions on statement 4 is as follows '*Both the husband and wife should contribute to household income*'. Respondents are asked to evaluate these three statements on an ordered scale from '*Agree strongly*' (1), '*Agree*' (2), '*Neither agree nor disagree*' (3), '*Disagree*' (4), to '*Strongly disagree*' (5). In the case of some country-years the respondents where given a 4-point scale to answer, which does not include the answer possibility '*Neither agree nor disagree*'. The data comprises for each country observations from at least two points in time. The first period is for each country the year 1990. The second (and third) period is AT: 1999, DE: 1997 and 1999, DK: 1999, FR: 1999, IT: 1999, NO: 1996, SE: 1996 and 1999, GB: 1998 and 1999, US: 1995 and 1996. The total number of observations varies across questions (Min: 11, 574, Max: 16, 729).

Table B.3: Effects of husband's displacement on household employment

	Control group 1		Control group 2		Control group 3
	Husband	Wife	Husband	Wife	Wife
	(1)	(2)	(3)	(4)	(5)
Prior event					
δ_{-5}	−0.008	0.002	−0.009	−0.002	
	(0.002)	(0.004)	(0.001)	(0.003)	
δ_{-4}	−0.003	0.002	−0.004	0.002	−0.004
	(0.001)	(0.004)	(0.001)	(0.003)	(0.005)
δ_{-3}	−0.002	0.001	−0.005	0.001	−0.002
	(0.001)	(0.004)	(0.001)	(0.003)	(0.004)
δ_{-2}	−0.002	−0.000	−0.003	−0.000	0.001
	(0.001)	(0.003)	(0.001)	(0.003)	(0.004)
δ_{-1}	−0.000	0.001	0.000	−0.001	−0.000
	(0.000)	(0.002)	(0.000)	(0.002)	(0.002)
Post event					
δ_1	−0.280	0.004	−0.278	0.004	0.005
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
δ_2	−0.173	0.009	−0.162	0.007	0.007
	(0.002)	(0.003)	(0.002)	(0.002)	(0.003)
δ_3	−0.144	0.013	−0.132	0.009	0.008
	(0.003)	(0.003)	(0.002)	(0.003)	(0.004)
δ_4	−0.131	0.014	−0.123	0.009	0.006
	(0.003)	(0.004)	(0.002)	(0.003)	(0.004)
δ_5	−0.123	0.013	−0.116	0.010	
	(0.003)	(0.004)	(0.003)	(0.003)	
Pre-event mean	1	0.490	1	0.486	0.468
Households	101,609		93,666		45,886
Observations	4,386,508		4,502,579		2,161,764

Notes: This table displays the impact of husband's displacement on own and spousal employment based on a specification similar to equation (2.2), which includes displaced group, distance to event, and industry×quarter fixed effects. In contrast to equation (2.2), we estimate the effect of the displacement separately for each year y. The dependent variable is equal to one if husband/wife in household i is employed in a given quarter. Column (1) and (2) ((3) and (4)) compare individuals in households with a displacement to a reweighted control group with no firm event (with households in which husbands keep their jobs during a mass layoff). In column (5), we match to displaced households a control group of households from the same marriage cohort that experience displacement four years after the reference date. The coefficient δ_y measures the average difference between employment in displaced and reweighted control groups y years to the reference quarter relative to the difference at the reference quarter. *Pre-event mean* refers to the mean employment in the year before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.4: Effects of husband's displacement on household earnings

	Control group 1		Control group 2		Control group 3
	Husband	Wife	Husband	Wife	Wife
	(1)	(2)	(3)	(4)	(5)
Prior event					
δ_{-5}	−6.642	5.572	−3.709	−4.608	
	(4.983)	(6.223)	(5.540)	(4.848)	
δ_{-4}	9.554	4.556	5.236	0.606	−3.661
	(3.833)	(5.853)	(5.202)	(4.679)	(6.814)
δ_{-3}	9.840	1.943	2.542	−1.637	−1.202
	(3.538)	(5.389)	(4.639)	(4.155)	(5.982)
δ_{-2}	9.687	−1.050	5.231	−1.866	4.143
	(3.011)	(4.360)	(3.686)	(3.671)	(4.908)
δ_{-1}	−2.522	−0.694	−1.990	−1.148	1.391
	(1.702)	(2.647)	(1.925)	(2.457)	(3.001)
Post event					
δ_1	−810.046	5.201	−783.445	5.618	11.390
	(6.049)	(2.696)	(5.564)	(2.283)	(3.153)
δ_2	−612.960	9.363	−554.224	7.261	11.581
	(6.611)	(4.071)	(6.382)	(3.552)	(4.768)
δ_3	−554.129	13.180	−482.088	9.510	15.370
	(7.308)	(4.547)	(6.944)	(4.120)	(5.643)
δ_4	−523.447	15.659	−454.925	10.312	14.027
	(8.092)	(5.122)	(7.372)	(4.600)	(6.212)
δ_5	−504.559	12.939	−434.683	13.541	
	(8.370)	(5.385)	(7.774)	(4.923)	
Pre-event mean	2458.082	658.549	2463.521	647.718	613.938
Households	101,609		93,666		45,886
Observations	4,386,508		4,502,579		2,161,764

Notes: This table displays the impact of husband's displacement on own and spousal monthly earnings in Euro (2000 prices) based on a specification similar to equation (2.2), which includes displaced group, distance to event, and industry×quarter fixed effects. In contrast to equation (2.2), we estimate the effect of the displacement separately for each year y. The dependent variable is set to zero if an individual is not employed. This table is constructed in the same way as Table B.3. *Pre-event mean* refers to the mean earnings in the year before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.5: Effects of husband's displacement on wife's job search

	Control group 1	Control group 2	Control group 3
	(1)	(2)	(3)
Prior event			
δ_{-5}	0.002	-0.001	
	(0.002)	(0.002)	
δ_{-4}	0.001	−0.003	-0.001
	(0.002)	(0.002)	(0.002)
δ_{-3}	0.001	-0.001	-0.000
	(0.002)	(0.002)	(0.002)
δ_{-2}	0.001	-0.000	0.001
	(0.001)	(0.001)	(0.002)
δ_0	0.003	0.003	0.002
	(0.001)	(0.001)	(0.002)
Post event			
δ_1	0.009	0.005	0.007
	(0.002)	(0.002)	(0.002)
δ_2	0.007	0.003	0.005
	(0.002)	(0.002)	(0.002)
δ_3	0.007	0.001	0.006
	(0.002)	(0.002)	(0.002)
δ_4	0.007	0.002	0.005
	(0.002)	(0.002)	(0.002)
δ_5	0.007	0.002	
	(0.002)	(0.002)	
Pre-event mean	0.041	0.041	0.039
Households	101,609	93,666	45,886
Observations	4,386,508	4,502,579	2,161,764

Notes: This table displays the impact of husband's displacement on spousal unemployment. The dependent variable is equal to one if the wife in household i is unemployed in a given quarter. The estimation is based on a specification similar to equation (2.2), in which the coefficients δ_y measure the average difference between displaced and reweighted control group for each year y relative to the quarter *one year before* the reference date. *Pre-event mean* refers to the mean unemployment in the year before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.6: Effects of husband's displacement on household income, control group 1

	Prob. of HH receiving		Monthly household income		
	UB	UA	Gross	Net	Net + benefits
	(1)	(2)	(3)	(4)	(5)
Prior event					
δ_{-1}	-0.004	0.001	−17.208	−0.588	−2.297
	(0.003)	(0.002)	(7.634)	(4.988)	(4.923)
Post event					
δ_1	0.223	0.015	−835.031	−530.986	−422.586
	(0.004)	(0.002)	(14.467)	(9.102)	(8.483)
δ_2	0.069	0.038	−794.45	−489.393	−442.212
	(0.004)	(0.002)	(19.488)	(12.243)	(11.867)
δ_3	0.037	0.024	−770.498	−472.311	−443.354
	(0.004)	(0.003)	(20.720)	(13.008)	(12.682)
δ_4	0.028	0.018	−734.077	−445.916	−425.226
	(0.004)	(0.003)	(22.537)	(14.106)	(13.780)
δ_5	0.025	0.017	−715.350	−432.797	−414.887
	(0.004)	(0.003)	(23.667)	(14.803)	(14.518)
Displaced×Post	0.077	0.023	-769.902	-474.298	-429.653
	(0.003)	(0.002)	(18.332)	(11.442)	(11.164)
Pre-event mean	0.040	0.015	3701.048	2515.338	2530.745
Households			40,771		
Observations			1,049,450		

Notes: This table displays the impact of husband's displacement on household income measures based on a specification similar to equation (2.2), which includes displaced group, distance to event, and industry×quarter fixed effects, for the subsample of households with a reference date in 2001 or later. In contrast to equation (2.2), we estimate the effect of the displacement separately for each year y. The dependent variable is equal to one if the household receives unemployment insurance benefits and unemployment assistance in column (1) and (2), respectively. In column (3), the outcome is household gross earnings (sum of the couple's labor earnings). Household net earnings in column (4) are gross earnings minus social security contributions and payroll taxes. In column (5), we add unemployment benefits and assistance to the former. All income variables are measured in Euro (2000 prices) on a monthly basis. We compare individuals in households with a displacement to a reweighted control group of households with no firm event. The coefficient δ_y measures the average difference between the outcome variable in the displaced and the reweighted control group y years to the reference date relative to the corresponding difference at the reference quarter. *Pre-event mean* refers to the mean outcome in the year before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.7: Effects of husband's displacement on household income, control group 2

	Prob. of HH receiving		Monthly household income		
	UB	UA	Gross	Net	Net + benefits
	(1)	(2)	(3)	(4)	(5)
Prior event					
δ_{-1}	-0.002	0.001	−42.231	−18.200	−17.807
	(0.002)	(0.001)	(7.563)	(4.935)	(4.850)
Post event					
δ_1	0.217	0.013	−829.348	−530.235	−423.316
	(0.003)	(0.001)	(13.795)	(8.667)	(8.138)
δ_2	0.055	0.035	−754.309	−467.076	−427.325
	(0.003)	(0.002)	(18.595)	(11.692)	(11.333)
δ_3	0.026	0.023	−703.547	−434.679	−412.512
	(0.003)	(0.002)	(20.218)	(12.712)	(12.363)
δ_4	0.018	0.017	−658.016	−401.865	−385.809
	(0.003)	(0.002)	(21.624)	(13.554)	(13.252)
δ_5	0.015	0.015	−610.017	−370.923	−357.572
	(0.003)	(0.002)	(22.906)	(14.353)	(14.074)
Displaced×Post	0.066	0.021	−711.126	−441.015	−401.320
	(0.003)	(0.002)	(17.695)	(11.046)	(10.789)
Pre-event mean	0.040	0.015	3772.018	2553.295	2575.851
Households			34,031		
Observations			947,225		

Notes: This table displays the impact of husband's displacement on household income measuresbased on a specification similar to equation (2.2), which includes displaced group, distance to event, and industry×quarter fixed effects, for the subsample of households with a reference date in 2001 or later. In contrast to equation (2.2), we estimate the effect of the displacement separately for each year y. The dependent variable is equal to one if the household receives unemployment insurance benefits and unemployment assistance in column (1) and (2), respectively. In column (3), the outcome is household gross earnings (sum of the couple's labor earnings). Household net earnings in column (5) are gross earnings minus social security contributions and payroll taxes. In column (6), we add unemployment benefits and assistance to the former. All income variables are measured in Euro (2000 prices) on a monthly basis. We compare individuals in households with a displacement to a reweighted control group of households with husbands who keep their job during during a mass layoff event at the reference date. The coefficient δ_y measures the average difference between outcomes in the displaced and the reweighted control group y years to the reference date relative to the corresponding difference at the reference quarter. *Pre-event mean* refers to the mean outcome in the year before the reference date. Standard errors are clustered at the household level and reported in parentheses.

Table B.8: Displacement effects by youngest child

Outcome	Husband	Wife	
	Earnings	P(employed)	Earnings
	(1)	(2)	(3)
Control group 1			
age $0-2$	−509.603	0.011	7.949
	(15.118)	(0.008)	(11.793)
age $3-9$	−552.944	0.008	12.988
	(10.531)	(0.005)	(6.477)
age $10-15$	−652.137	0.009	6.222
	(14.648)	(0.005)	(7.414)
No child	−707.224	0.001	-6.027
	(12.433)	(0.005)	(8.154)
Control group 2			
age $0-2$t	−473.784	0.005	6.828
	(13.840)	(0.007)	(9.986)
age $3-9$	−501.594	0.007	11.157
	(10.004)	(0.004)	(5.325)
age $10-15$	−585.793	0.015	14.937
	(13.394)	(0.004)	(6.393)
No child	−615.452	0.005	0.324
	(12.210)	(0.004)	(7.418)
Control group 3			
age $0-2$	−625.170	-0.001	25.420
	(13.635)	(0.008)	(11.193)
age $3-9$	−681.030	0.013	21.459
	(9.822)	(0.005)	(6.683)
age $10-15$	−778.636	0.011	14.136
	(13.567)	(0.007)	(8.661)
No child	−839.497	0.006	6.401
	(12.906)	(0.007)	(11.360)

Notes: This table displays the impact of husband's displacement for subgroups defined by the age of the youngest child at the reference date. It is similar to Table 2.6, but it additionally includes the effects on husband's earnings (1) and wife's earnings (3). The estimates measure the average difference in the corresponding outcome variable between displaced and reweighted control groups after the reference quarter rel. to the difference at the reference date.

Table B.9: Displacement effects by availability of nursery at reference date

Outcome	No nursery in district			Nursery in district		
	Husband	Wife		Husband	Wife	
	Earnings	P(employed)	Earnings	Earnings	P(employed)	Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Control group 1						
Displaced×Post	−503.182	0.005	20.075	−526.537	0.011	-7.443
	(18.987)	(0.010)	(12.455)	(25.127)	(0.014)	(21.868)
$\eta^{\text{participation}}$		−0.020			−0.053	
		(0.045)			(0.065)	
Pre-event mean	2333.331	0.164	166.678	2496.618	0.206	243.037
Households	11,058			7,170		
Control group 2						
Displaced×Post	−460.569	0.008	12.111	−482.589	0.003	4.978
	(17.073)	(0.008)	(10.093)	(22.442)	(0.011)	(17.933)
$\eta^{\text{participation}}$		−0.044			−0.021	
		(0.040)			(0.058)	
Pre-event mean	2339.078	0.162	162.721	2497.289	0.205	241.980
Households	10,754			6,892		

Notes: This table displays the impact of husband's displacement on own earnings, spousal employment and earnings by the availability of nurseries at the reference date. Nurseries provide child care for the under-three-year-old. The availability is measured at the community level. We only look at wives with children aged 0–3 at the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.10: Displacement effects by wife's earnings potential, control group 2

Outcome	Low			High		
	Husband Earnings	Wife P(employed)	Wife Earnings	Husband Earnings	Wife P(employed)	Wife Earnings
	(1)	(2)	(3)	(4)	(5)	(6)
Measure 1: Earnings before marriage						
Displaced×Post	−511.376	0.007	6.656	−592.745	0.010	13.671
	(6.587)	(0.003)	(3.848)	(15.434)	(0.005)	(9.707)
$\eta^{\text{participation}}$		−0.034			−0.045	
		(0.014)			(0.024)	
Pre-event mean	2390.720	0.456	544.524	2718.341	0.579	1002.762
Households	63,911			17,986		
Measure 2: Experience before marriage						
Displaced×Post	−518.182	0.006	6.614	−539.480	0.010	9.419
	(8.613)	(0.004)	(5.680)	(9.089)	(0.003)	(5.340)
$\eta^{\text{participation}}$		−0.024			−0.046	
		(0.018)			(0.016)	
Pre-event mean	2428.900	0.458	581.638	2496.457	0.507	706.263
Households	40,263			41,594		
Measure 3: Education						
Displaced×Post	−453.777	0.008	7.848	−619.957	0.010	13.339
	(8.453)	(0.004)	(4.808)	(12.191)	(0.005)	(8.780)
$\eta^{\text{participation}}$		−0.042			−0.043	
		(0.020)			(0.020)	
Pre-event mean	2315.542	0.399	457.583	2696.034	0.500	697.313
Households	40,168			26,208		

Notes: This table displays the impact of husband's displacement on own earnings, spousal employment and earnings by measures of wife's earnings potential. Measure 1: High indicates that the wife earned more than 33% of the wage of husbands in the year before marriage. Measure 2: High indicates above median experience compared to other wives in the year before marriage. Measure 3: High indicates that the completed education of the wife is beyond compulsory schooling and apprenticeship education. Pre-marriage wage and experience are only available for those married after 1974. Education is only available for women with children. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

B.3 Robustness analysis

In this section, we briefly summarize robustness checks using alternative definitions of displaced and control workers and variations in the weighting procedure.

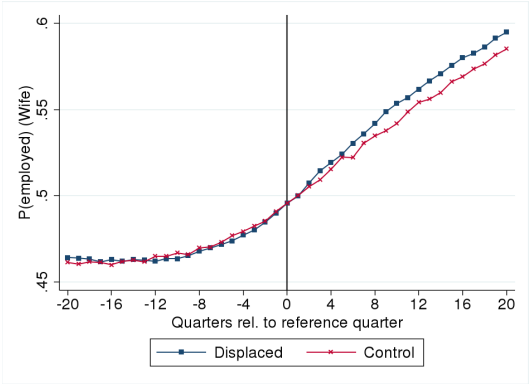
We start with sensitivity checks of our estimations using control group 1 and 2. First, we use an alternative, more restrictive measure to identify mass layoffs. Now firms experience a mass layoff, if at least ten and at most fifty percent of all workers are displaced from one quarter to the other.[101] The graphical evidence (see Figure B.16a) and the corresponding estimates (see column (1) of Table B.11) illustrate that our main estimation results are robust to a change in the definition of mass layoffs. Second, we match (in addition to the variables used in the main specification, see footnote 50) also on employment outcomes of wives up to one year before the reference date. The resulting estimates (see Figure B.16b) are similar and not statistically significantly different from the ones in the main specification. Third, we focus on displaced workers from plant closures. Workers who got displaced due to a mass layoff events are more prone to selection issues, since the underlying process determining leavers and stayers within struggling firms might be endogenous to workers' labor market outcomes. In contrast, there is no selection within a firm when it closes down, since all employees are eventually displaced. The resulting estimates (see Figure B.16c) are slightly smaller and not as precisely estimated as in the main specification, but they are indicating that results are robust. Fourth, we focus alternatively on displaced workers from mass layoffs and exclude those from plant closures. Cases from plant closure might be more selective at the firm level. For instance, we can observe that these firms are typically much smaller than other firms with a mass layoff event or no event at all (see Figure B.7). In addition, we also match on the firm size up to one year before the reference quarter. The resulting estimates (see Figure B.16d) are very comparable to those from our main results.

We now explore the robustness of our estimation result based on control group 3. This approach exploits the timing of displacement and requires the choice of a duration Δ between the events of the households in the treatment and the control group. Importantly, there is a trade-off in this choice: With a smaller Δ, the treatment and control group's displacement is closer in time and there are hence more likely to be comparable. A larger distance in the date of event allows us to compare outcomes of the two groups for more periods (Fadlon and Nielsen, 2017). In our baseline specification we choose a Δ of 16 quarters. Now we consider values of 4, 8, and 12. It turns out that the specific choice of Δ is not crucial (see Figure B.17 and Table B.12).

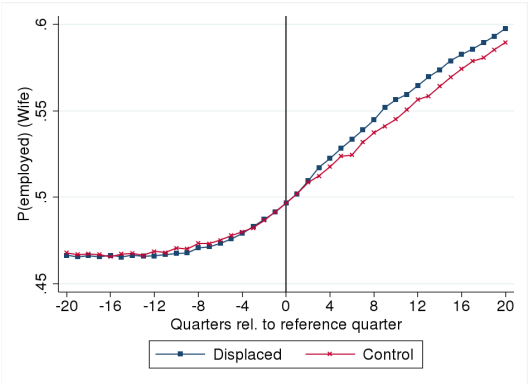
[101] Again, this relative cutoff applies to all establishments with 100 to 600 workers in the base quarter. For smaller firms, the cutoffs are more than 6 workers leaving in firms with less than 20 employees and more than 10 if the establishment has more than 20 and less than 100 workers. For establishments with more than 600 workers, at least 60 employees have to leave the firm in order to make it count as mass layoff.

Figure B.16: Displaced husband's wife employment, robustness checks control groups 1 and 2

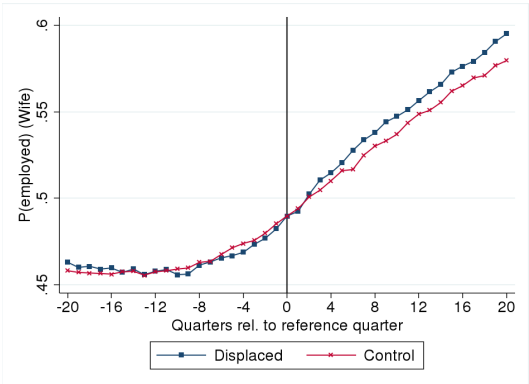
(a) Stricter mass layoff cutoffs



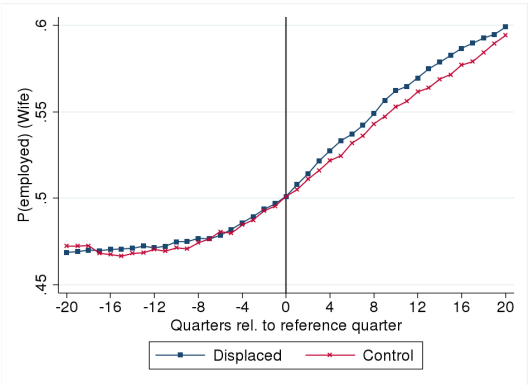
(b) Reweighting includes wives' pre-event employment



(c) Displaced from plant closures only



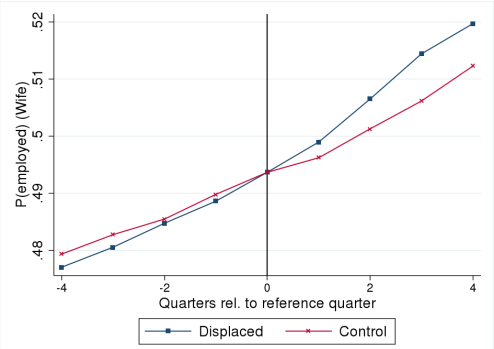
(d) Displaced and non-displaced from mass layoff firms only



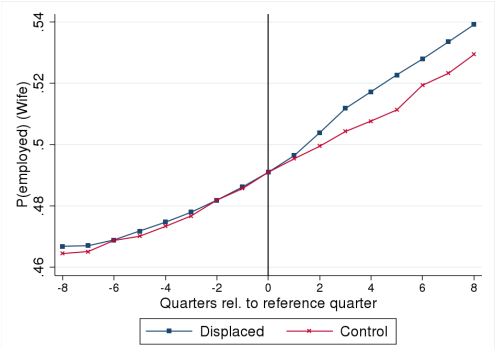
Notes: This figure provides robustness checks to Figure 2.5a and 2.5b. In Panel (a), we apply a stricter cutoff for mass layoffs. In Panel (b), we additionally include employment outcomes of wives (up to one year before the reference date) in the weighting procedure. In Panel (c), the group of displaced workers includes only those with a displacement due to a plant closure. In Panel (d), we only look at displaced and non-displaced workers at firms that have a mass layoff in the quarter after the reference date. We also match on the firm size up to one year before the reference quarter. The graphs are constructed in the same way as in Figure 2.5.

Figure B.17: Displaced husband's wife employment, robustness checks control group 3

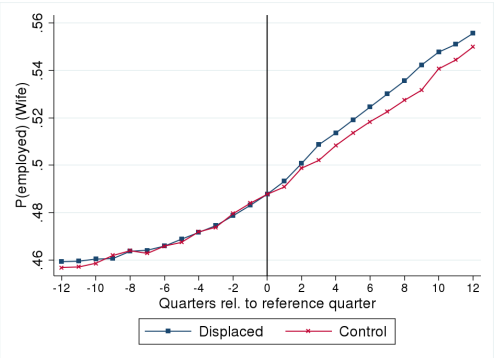
(a) Control group displaced in $\Delta = 4$



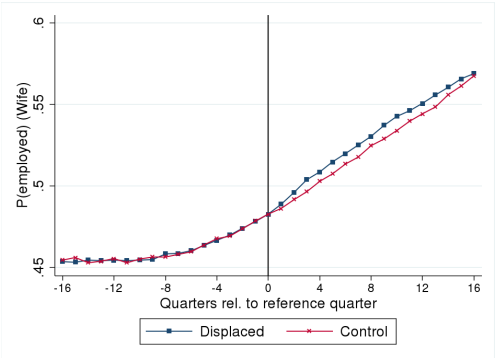
(b) Control group displaced in $\Delta = 8$



(c) Control group displaced in $\Delta = 12$



(d) Control group displaced in $\Delta = 16$



Notes: This figure provides robustness checks to Figure 2.5c by showing the effect of husband's displacement on wife's employment for different choices of Δ. We compare wives of men that are displaced at the reference date (blue, square) to that of men displaced Δ quarters after that date (red, x). The employment pattern of the control group is adjusted by its mean difference relative to the displaced group.

Table B.11: Robustness checks for control group 1 and 2

	(1)	(2)	(3)	(4)	(5)
Prior event					
δ_{-5}	0.001	−0.002	0.002	−0.002	0.003
	(0.005)	(0.004)	(0.005)	(0.004)	(0.003)
δ_{-4}	0.001	−0.001	0.001	0.003	0.003
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)
δ_{-3}	−0.002	−0.002	−0.002	0.003	0.002
	(0.004)	(0.004)	(0.005)	(0.004)	(0.003)
δ_{-2}	−0.002	−0.002	0.002	0.000	0.001
	(0.003)	(0.003)	(0.004)	(0.003)	(0.002)
δ_{-1}	−0.001	0.000	−0.003	0.001	0.000
	(0.002)	(0.002)	(0.003)	(0.002)	(0.002)
Post event					
δ_1	0.003	0.003	0.003	0.004	0.004
	(0.002)	(0.002)	(0.002)	(0.002)	(0.002)
δ_2	0.006	0.007	0.007	0.006	0.009
	(0.003)	(0.003)	(0.003)	(0.003)	(0.002)
δ_3	0.010	0.010	0.009	0.008	0.011
	(0.004)	(0.003)	(0.003)	(0.004)	(0.003)
δ_4	0.011	0.010	0.011	0.010	0.011
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
δ_5	0.010	0.008	0.007	0.007	0.010
	(0.004)	(0.004)	(0.004)	(0.004)	(0.003)
Pre-event mean	0.489	0.486	0.484	0.470	0.485
Households	87,876	101,609	70,942	75,212	100,036
Observations	3,823,455	4,387,451	3,123,503	3,745,965	4,320,949

Notes: This table reports different robustness checks to the results in Table B.3 that are based on control group 1 and 2. The dependent variable is equal to one if wife in household i is employed in a given quarter. The coefficient δ_l measures the average difference between employment in the displaced and the control group l years to the reference date relative to the corresponding difference at the reference date. *Pre-event mean* refers to the mean employment in the year before the reference date. In the robustness checks, we vary the approaches used in Table B.3 in the following ways: (1) We compare displaced and control group 2 with higher mass layoff cutoffs requirements, (2) We additionally balance displaced and control group 1 with respect to the pre-event employment outcomes of wives, (3) We only include individuals affected by a plant closure in the displaced group and compare them to controls with no firm event, (4) We only take displaced and non-displaced husbands from mass layoffs and additionally balance them with respect to husband's employer size, (5) Instead of matching, we control for the variables included the weighting procedure by including them in a simple regression model. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Table B.12: Robustness checks for control group 3

	$\Delta = 4$	$\Delta = 8$	$\Delta = 12$
	(1)	(2)	(3)
Prior event			
δ_{-3}			0.001
			(0.004)
δ_{-2}		0.002	−0.000
		(0.003)	(0.002)
δ_{-1}	−0.001	0.001	0.001
	(0.002)	(0.002)	(0.003)
Post event			
δ_1	0.006	0.006	0.004
	(0.002)	(0.002)	(0.002)
δ_2		0.010	0.007
		(0.003)	(0.003)
δ_3			0.009
			(0.004)
Pre-event mean	0.482	0.472	0.464
Households	46,730	46,263	45,476
Observations	766,593	1,324,436	1,779,150

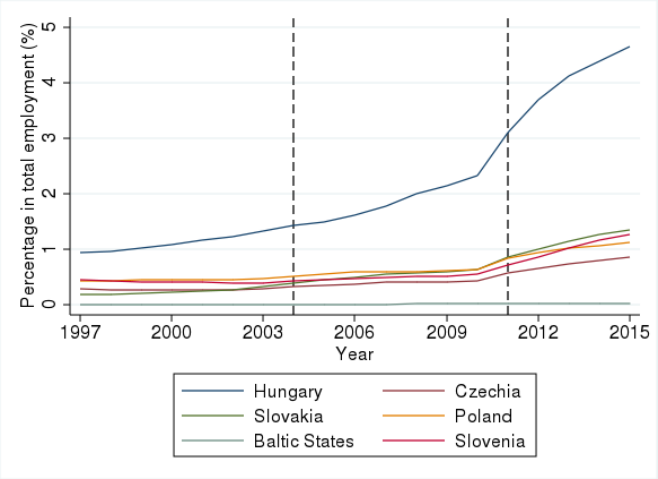
Notes: This table illustrates the robustness of results for control group 3 in Table B.3 to different choices of Δ. Column (1) shows the effect of husband's displacement on wife's employment comparing households that experience displacement at the reference date to those displaced 4 quarter in the future. Column (2) and (3) refer to estimations using as a control group those displaced 8 and 12 quarter in the future, respectively. The dependent variable is equal to one if wife in household i is employed in a given quarter. The coefficient δ_l measures the average difference between employment in the displaced and the control group l years to the reference date relative to the corresponding difference at the reference quarter. *Pre-event mean* refers to the mean employment before the reference date. Standard errors are bootstrapped (500 replications, with clustering at the household level) and reported in parentheses.

Appendix C

Appendix to Chapter 3

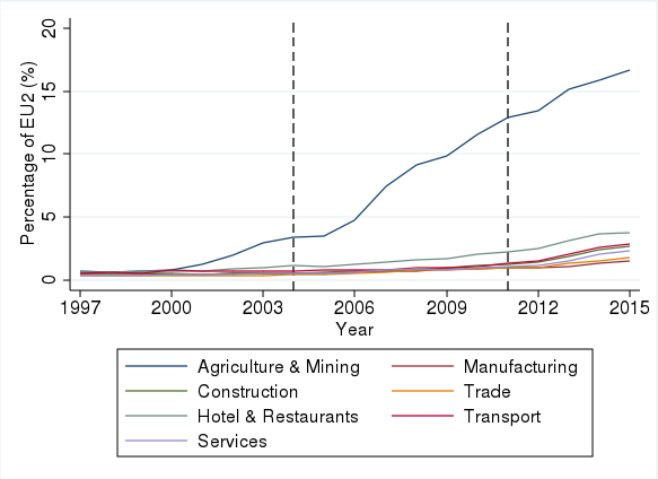
C.1 Additional figures

Figure C.1: Employment shares of EU8 workers by their nationality



Notes: This figure shows the employment shares of workers from EU8 countries by their nationality. We only focus on workers in our sample municipalities. Year y refers to the period between May y and April y + 1.

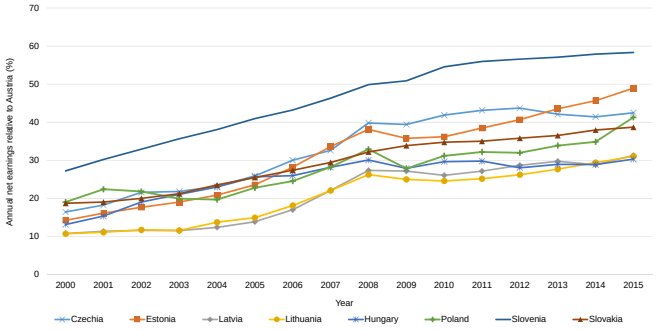
Figure C.2: Percentage of EU2 workers across industries



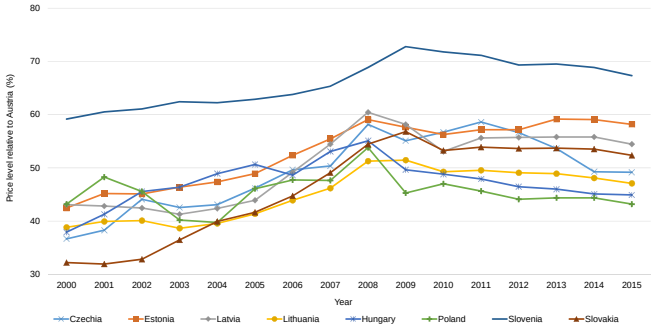
Notes: This figure shows the employment of EU2 nationals across industries relative to the employment of all workers in the corresponding year and industry. We only consider workers in our sample municipalities. Year y refers to the period between May y and April $y + 1$.

Figure C.3: Net earnings and price levels in EU8 countries relative to Austria

(a) Annual net earnings relative to Austria



(b) Price level relative to Austria

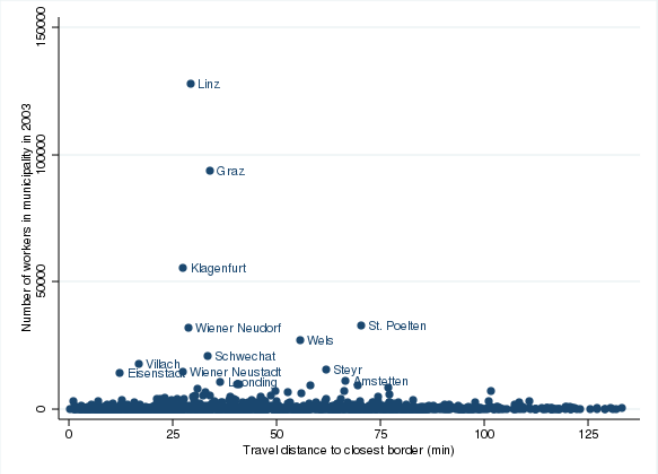


Notes: This figure shows the net earnings and price levels in EU8 countries relative to Austria from 2000 to 2015. Net earnings are annual net earnings in Euro of a single person without children who earns 67% of the average worker's earnings. Price level indices refer to actual individual consumptions. Source: *Eurostat*.

Figure C.4: Austrian border crossings to neighboring EU8 countries

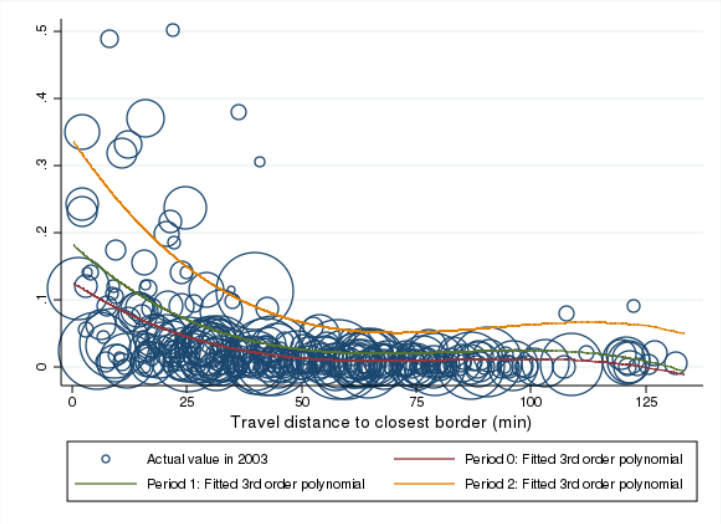
Notes: Border crossing points are indicated by brown dots. The gray lines within Austria show the main road network. Source: *Statistics Austria, open street map.*

Figure C.5: Municipalities' workforce size by the distance to the EU8 border in 2003



Notes: This figure plots the number of workers in 2003 of all municipalities in Austrian states that share a border with an EU8 country against their travel distance to the closest EU8 border.

Figure C.6: EU8 employment shares by the distance to the border



Notes: Figure C.6 plots the employment shares of EU8 nationals against the travel duration to the closest EU8 border. The blue circles show the EU8 employment share for a 20% random sample of municipalities in 2003; the size of each circle is proportional to employment in 2003. The fitted values are from a regression of EU8 employment shares by period on a third order polynomial in travel duration. Period 0 refers to May 1997 to April 2004 (prior EU8 accession), period 1 to the period from May 2004 to April 2011, and period 2 to the period after May 2011.

Figure C.7: EU2 employment shares by the distance to the border

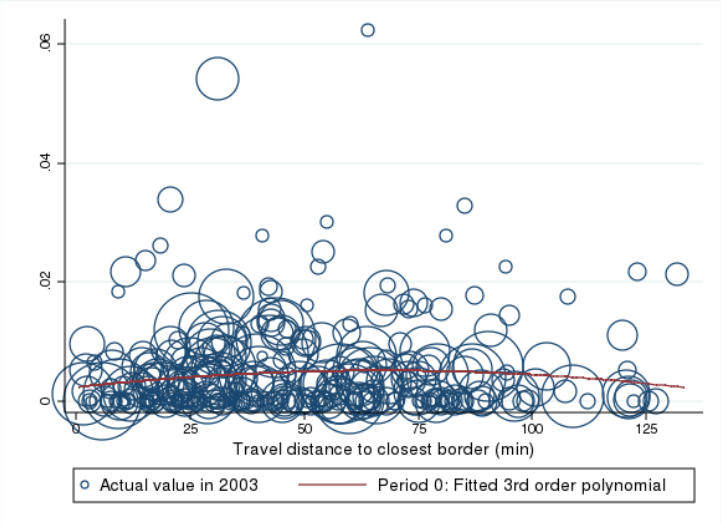
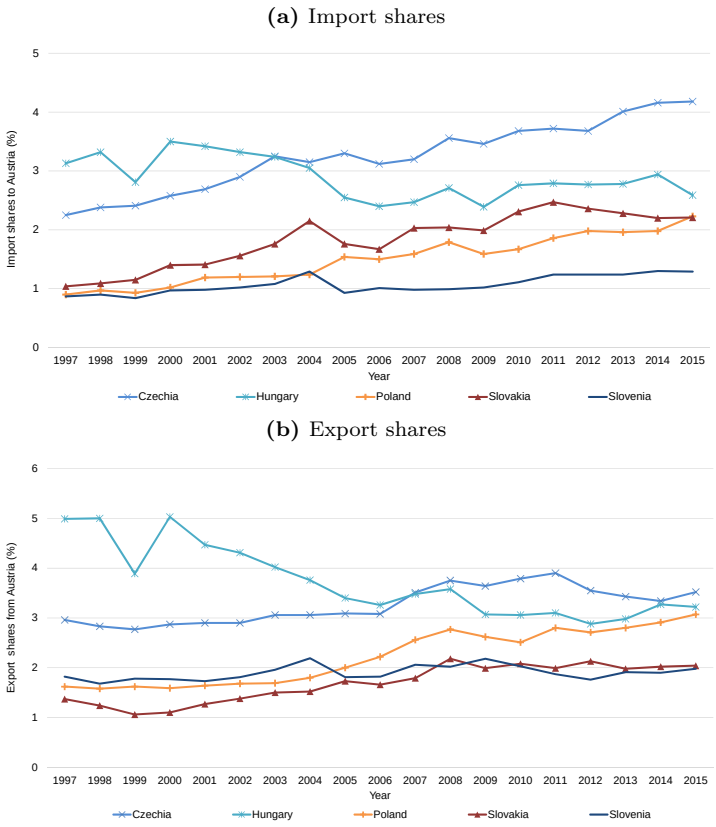


Figure C.7 plots the employment shares of EU2 nationals against the travel duration to the closest EU8 border. The blue circles show the EU2 employment share for a 20% random sample of municipalities in 2003; the size of each circle is proportional to employment in 2003. The fitted values are from a regression of the EU2 employment shares in the period before EU8 accession (May 1997 to April 2004) on a third order polynomial in travel duration.

Figure C.8: Austrian import and export shares with EU8 countries



Notes: Import and export shares refer to the total merchandise trade accounted for by the partner in a given year. *Source:* World Integrated Trade Solutions (WITS).

C.2 Additional tables

Table C.1: Free movement of labor from EU8 to EU15 countries

Date of introduction	Countries
May 2004	UK, Ireland, Sweden
May 2006	Greece, Portugal, Spain, Finland
May 2006 to May 2009	Italy, The Netherlands, Luxembourg, France
May 1 2009	Belgium, Denmark
May 1 2011	Austria, Germany

Source: Own collection of data from governmental websites.

Table C.2: Heterogeneous effects on monthly earnings of Austrians

				Age group		
	Average	Blue	Male	≤ 30	$30-45$	$45<$
	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_{pre}^{[0-20]}$	0.002	0.002	0.001	-0.004	0.004	0.009
	(0.005)	(0.005)	(0.004)	(0.006)	(0.006)	(0.009)
$\delta_{trans}^{[0-20]}$	0.008	0.009	0.001	-0.007	0.016	0.007
	(0.006)	(0.006)	(0.005)	(0.007)	(0.007)	(0.010)
$\delta_{free}^{[0-20]}$	0.023	0.028	0.004	0.000	0.036	0.019
	(0.009)	(0.009)	(0.009)	(0.010)	(0.011)	(0.014)
$\delta_{pre}^{[20-60]}$	0.002	0.006	0.002	0.003	-0.001	0.007
	(0.004)	(0.004)	(0.003)	(0.004)	(0.005)	(0.007)
$\delta_{trans}^{[20-60]}$	0.010	0.011	0.006	0.001	0.018	0.009
	(0.004)	(0.005)	(0.004)	(0.005)	(0.006)	(0.008)
$\delta_{free}^{[20-60]}$	0.016	0.018	0.008	-0.006	0.030	0.014
	(0.006)	(0.007)	(0.006)	(0.007)	(0.008)	(0.010)
Mean in 2003	1,663	1,579	1,955	1,565	1,683	1,752
Observations			26,695			
Municipalities			1,405			

Notes: Similar to Table 3.4, this table displays the impact of the EU Eastern enlargement on the earnings of Austrians based on equation (3.2). The dependent variable in column (1) is the log of average monthly earnings (real, 2000 prices) of Austrians in a municipality and in columns (2) to (6) the log of average monthly earnings (real, 2000 prices) of Austrians in a municipality in the corresponding subgroup. The mean in 2003 refers to the average of mean monthly earnings of Austrians over all municipalities in 2003. Standard errors are clustered at the municipality level.

Table C.3: Heterogeneous effects on monthly earnings of non-EU8 foreigners

				Age group		
	Average	Blue	Male	≤ 30	$30-45$	$45<$
	(1)	(2)	(3)	(4)	(5)	(6)
$\delta_{pre}^{[0-20]}$	-0.018	-0.016	-0.036	-0.019	-0.023	0.018
	(0.017)	(0.017)	(0.017)	(0.027)	(0.020)	(0.026)
$\delta_{trans}^{[0-20]}$	-0.031	-0.032	-0.028	-0.039	-0.028	0.000
	(0.016)	(0.016)	(0.016)	(0.027)	(0.020)	(0.025)
$\delta_{free}^{[0-20]}$	-0.044	-0.040	-0.043	-0.046	-0.051	0.008
	(0.022)	(0.022)	(0.022)	(0.032)	(0.027)	(0.033)
$\delta_{pre}^{[20-60]}$	-0.003	0.001	-0.011	0.025	-0.004	0.000
	(0.013)	(0.013)	(0.013)	(0.018)	(0.015)	(0.018)
$\delta_{trans}^{[20-60]}$	-0.025	-0.025	-0.024	0.002	-0.027	-0.018
	(0.012)	(0.012)	(0.012)	(0.019)	(0.015)	(0.019)
$\delta_{free}^{[20-60]}$	-0.037	-0.028	-0.035	0.007	-0.046	-0.025
	(0.016)	(0.016)	(0.016)	(0.022)	(0.019)	(0.024)
Mean in 2003	1,508	1,462	1,651	1,366	1,557	1,630
Observations			26,695			
Municipalities			1,405			

Notes: Similar to Table 3.4, this table displays the impact of the EU Eastern enlargement on the earnings of non-EU8 foreign nationals based on equation (3.2). The dependent variable in column (1) is the log of average monthly earnings (real, 2000 prices) of non-EU8 foreigners in a municipality and in columns (2) to (6) the log of average monthly earnings (real, 2000 prices) of non-EU8 foreigners in a municipality in the corresponding subgroup. The mean in 2003 refers to the average of mean monthly earnings of non-EU8 foreigners over all municipalities in 2003. Standard errors are clustered at the municipality level.

Table C.4: Effects of the EU Eastern enlargement: robustness check

	Employment				Earnings		Firms
	All	EU8	Austrians	Non-EU8 foreigners	Austrians	Non-EU8 foreigners	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\delta_{pre}^{[0-20]}$	-9.455	-3.167	-6.842	0.553	0.002	-0.018	-0.561
	(4.706)	(0.555)	(4.266)	(0.553)	(0.005)	(0.017)	(0.197)
$\delta_{trans}^{[0-20]}$	-1.408	4.265	-4.007	-1.666	0.008	-0.030	-0.060
	(6.746)	(1.070)	(5.737)	(0.952)	(0.006)	(0.016)	(0.238)
$\delta_{free}^{[0-20]}$	13.062	20.497	-4.154	-3.281	0.023	-0.043	0.473
	(12.229)	(3.547)	(9.421)	(1.905)	(0.009)	(0.022)	(0.380)
$\delta_{pre}^{[20-60]}$	-7.080	-0.867	-5.832	-0.381	0.002	-0.003	-0.190
	(4.042)	(0.235)	(3.662)	(0.558)	(0.004)	(0.013)	(0.160)
$\delta_{trans}^{[20-60]}$	13.065	1.018	11.272	0.775	0.010	-0.026	0.307
	(5.901)	(0.396)	(5.162)	(0.943)	(0.004)	(0.012)	(0.188)
$\delta_{free}^{[20-60]}$	26.476	7.649	16.832	1.995	0.016	-0.037	0.872
	(9.931)	(1.337)	(8.039)	(1.998)	(0.006)	(0.016)	(0.276)
Observations				26,695			
Municipalities				1,405			

Notes: This table displays the impact of the EU Eastern enlargement on different outcomes based on equation (3.2), which includes municipality and year-by-regions fixed effects. We additionally include a Bartik-type variable to control for differential time trends that are driven by the initial industry structure in a municipality. T he upper (lower) panel focuses on $\delta^{[0-20]}$ ($\delta^{[20-60]}$), which capture the difference in outcomes in the corresponding year between municipalities within 0 to 20 (20 to 60) minutes and municipalities with a driving distance $\geq$ 60 minutes from the next EU8 border. This difference is standardized to 0 in 2003. *pre* refers to the period May 1997 to April 2003, *trans* to the period May 2004 to April 2011, and $free$ to the period May 2011 to April 2016. The dependent variable is the employment by all workers in column (1), EU8 workers in (2), Austrian workers in (3), and non-EU8 workers in (4). In column (5) and (6) the dependent variable is the log of average monthly earnings (real, 2000 prices) of Austrians and non-EU8 foreigners, respectively. In column (7) the dependent variable is the number of all firms with more than five employees. Standard errors are clustered at the municipality level.

Appendix D

Appendix to Chapter 4

For copyright reasons, this appendix is not included in the online version of the dissertation. An electronic version of the appendix can be accessed at http://doi.org/10.1016/j.jeoa.2018.12.001.

Summary

Labor market and demographic developments are highly interrelated. This dissertation analyzes important intersections between the two. Its four chapters are based on large, mostly administrative micro data. The first three chapters apply innovative quasi-experimental methods, while the last chapter provides a descriptive analysis .

The first chapter investigates empirically how maternal labor supply responds to an increase in fertility in the context of Mexico, where the average household income is low and informal child care and flexible employment opportunities are easily available. To overcome the endogeneity of fertility decisions, an instrumental variable approach that exploits parental preferences for a mixed-sex children composition is used. The rationale is that, compared to parents who have children of different sexes, parents with children of the same sex are more likely to have an additional child. At the same time, children's sex mix is assumed to be virtually randomly assigned. Relying on this instrument-induced variation, an increase in family size beyond two children is found to raise women's probability to be employed. This positive effect is driven by an increase in informal work, which tends to be easier to combine with child care responsibilities, and by low-wealth households, for which additional income is particularly valuable. Moreover, the presence of grandparents who potentially provide informal child care seems to be important. Further analyses support the internal validity of these findings. The effect of having more than two children on the propensity to be informally employed is bounded to be non-negative for all mothers in the sample, which supports the external validity of these results.

The second chapter examines the labor supply responses of wives to a negative income shock experienced by their husbands for married couples in Austria. The empirical analysis is based on detailed administrative data and focuses on a sample where husbands lose their job due to mass layoffs or plant closures. Three different control groups are used to estimate the effect of husbands' job loss on spouses' subsequent labor supply. The main results are remarkably consistent across these groups. Husbands lose substantially both in terms of employment and earnings over a five-year period after displacement. The labor supply responses of wives are positive and statistically significant. However, the additional earnings generated by wives are small compared to their husbands' loss. Government transfers and taxes are more important providers of insurance, at least in the short run. Further results indicate that gender roles, preferences for time spent with children, and availability of formal child care play a strong role

in the wives' labor supply decisions.

The third chapter investigates how local labor markets are affected when receiving a large number of immigrant workers. The empirical strategy combines temporal variation in immigration resulting from the EU Eastern enlargement in 2004 and the later implementation of free labor market access to EU8 workers with the spatial variation that comes from the tendency of immigrants to go to municipalities that are close to their home country. The analysis based on social security data with information on both employees and employers finds significant changes in labor markets in the Austrian border region after 2004. First, the employment share of EU8 nationals in the region increases from 2.2% in 1997 to 9.3% in 2015. Second, this increase is larger in municipalities that are the closer to the border compared to those further away. Third, there is no decline in the employment of Austrian nationals, but average employment among non-EU8 foreign workers decreases significantly and steadily in municipalities with increased immigration after 2004. At the same time, Austrian workers experience an increase in their earnings, while non-EU8 foreign workers face substantial decreases. Finally, the number of firms is found to increase with immigration.

The fourth chapter analyzes how mortality for mid-aged individuals is changing for several important demographic subgroups in Germany over the period from 1990 to 2015, with a focus on deaths from drug overdoses, suicides, and alcohol-related diseases, termed as *deaths of despair*. The results show a very clear pattern: mortality rates declined between 1990 and 2015, with no increases in deaths of despair for any of the subgroups. These findings starkly contrast with those for the US, where midlife mortality rates are increasing for white non-Hispanics since 1998.

German Summary

Entwicklungen am Arbeitsmarkt und demographische Entwicklungen sind eng miteinander verknüpft. Diese Dissertation analysiert wichtige Schnittmengen zwischen diesen beiden Bereichen. In vier Kapiteln erfolgt eine Analyse großer, meist administrativer Mikrodatensätze. Dabei werden in den ersten drei Kapiteln innovative, quasi-experimentelle Methoden angewandt, während das letzte Kapitel eine deskriptive Analyse enthält.

Das erste Kapitel untersucht empirisch, wie sich eine Erhöhung der Fertilität auf das Arbeitsangebot von Müttern auswirkt. Die Untersuchung erfolgt am Beispiel Mexikos, wo das durchschnittliche Haushaltseinkommen niedrig ist und eine informelle Kinderbetreuung sowie flexible Beschäftigungsmöglichkeiten leicht verfügbar sind. Um einen Kausaleffekt der Fertilität auf das Arbeitsangebot zu bestimmen, nutzt die empirische Analyse im Rahmen eines Instrumentalvariablenansatzes aus, dass Eltern eine Präferenz für Kinder unterschiedlichen Geschlechts haben. Haben Eltern zwei Kinder des gleichen Geschlechts, ist die Wahrscheinlichkeit größer, dass sie noch ein drittes bekommen. Gleichzeitig ist die Geschlechterszusammensetzung der ersten beiden Kinder zufällig. Unter Ausnutzung dieser quasi zufälligen Variation in der Fertilität kommt die empirische Analyse zu dem Ergebnis, dass eine Zunahme der Familiengröße über zwei Kinder hinaus die Wahrscheinlichkeit erhöht, dass Frauen beschäftigt sind. Dieser positive Effekt ist darauf zurückzuführen, dass Mütter durch die höhere Fertilität bedingt eine informelle Beschäftigung aufnehmen, die in der Regel leichter mit Betreuung von Kindern zu vereinbaren ist. Außerdem wird ein höheres Arbeitsangebot von Müttern insbesondere für Haushalte mit geringem Vermögen beobachtet, die ganz besonders auf zusätzliches Einkommen angewiesen sind. Weiterhin nimmt mit steigender Fertilität die Wahrscheinlichkeit zu, dass die Großeltern im gleichen Haushalt wie die Familie leben und möglicherweise informelle Kinderbetreuung bereitstellen. Weitere Analysen belegen die interne Validität dieser Befunde. Ein zusätzliches ökonometrisches Verfahren zeigt, dass der Effekt eines Anstieges in der Fertilität auf die Wahrscheinlichkeit einer informellen Beschäftigung für alle Mütter in der Stichprobe größer oder gleich null ist.

Im zweiten Kapitel wird am Beispiel Österreichs betrachtet, wie das Arbeitsangebot von Ehefrauen auf einen negativen Einkommensschock ihrer Ehemänner reagiert. Die empirische Analyse basiert auf detaillierten administrativen Daten und konzentriert sich auf Paare, in denen Ehemänner aufgrund von Massenentlassungen oder Betriebsschließungen ihren Arbeitsplatz

verlieren. Es werden drei verschiedene Kontrollgruppen genutzt, um die Auswirkung des Arbeitsplatzverlusts der Ehemänner auf das Arbeitsangebot der Partnerinnen zu schätzen. Die Hauptergebnisse sind über diese Gruppen hinweg konstant. Ehemänner erfahren durch den Arbeitsplatzverlust über einen Zeitraum von fünf Jahren substanzielle Beschäftigungs- und Einkommenseinbußen. Gleichzeitig hat dieses Ereignis einen statistisch signifikanten, positiven Effekt auf das Arbeitsangebot der Ehefauen. Das zusätzliche Einkommen, das die Ehefrauen erwirtschaften, ist jedoch im Vergleich zum Einkommensverlust des jeweiligen Ehemannes gering. Das Steuer- und Transfersystem bietet zumindest auf kurze Sicht mehr Absicherung. Weitere Analysen legen nahe, dass Geschlechterrollen, Präferenzen hinsichtlich der Kinderbetreuung und die Verfügbarkeit von formaler Kinderbetreuung eine wichtige Rolle für die Arbeitsangebotsentscheidungen der Ehefrauen spielen.

Das dritte Kapitel analysiert, welche Folgen ein Anstieg der Einwanderung von ArbeitsmigrantInnen für lokale Arbeitsmärkte in österreichischen Grenzregionen hat. Die empirische Methode nutzt die Veränderungen in den Einwanderungszahlen, die durch die EU-Osterweiterung im Jahr 2004 und der später erfolgten Umsetzung des freien Arbeitsmarktzugangs für EU8-ArbeitnehmerInnen hervorgerufen wurde. Kombiniert wird dieser Umstand mit räumlichen Unterschieden in der Einwanderung. Diese ergeben sich dadurch, dass ZuwandererInnen eine starke Tendenz haben, in österreichische Gemeinden in der Nähe ihres Heimatlandes zu gehen. Die Analyse auf Basis von Sozialversicherungsdaten mit Informationen zu ArbeitnehmerInnen und ArbeitgeberInnen findet signifikante Veränderungen in Arbeitsmärkten österreichischer Grenzregionen zu EU8-Staaten. Erstens, der Beschäftigungsanteil von EU8-Staatsangehörigen in den Regionen steigt durchschnittlich von 2,2% im Jahr 1997 auf 9,3% im Jahr 2015. Zweitens, dieser Anstieg ist größer in Gemeinden, die näher zur Grenze liegen im Vergleich zu solchen, die weiter entfernt sind. Drittens, es ist bei österreichischen Staatsangehörigen kein Beschäftigungsrückgang in Gemeinden mit erhöhter Zuwanderung nach 2004 zu verzeichnen. Gleichzeitig sinkt in diesen Gemeinden die durchschnittliche Beschäftigung von ausländischen Arbeitskräften, die nicht aus einem der EU8-Länder stammen, jedoch stetig. Weiterhin erfahren österreichische Arbeitskräfte einen Anstieg ihrer Einkommen, während nicht-österreichische Arbeitskräfte einen Einkommensrückgang verzeichnen. Darüber hinaus nimmt die Zahl der Unternehmen mit ansteigender Einwanderung zu.

Im vierten Kapitel wird untersucht, wie sich die Mortalitätsraten von Männern und Frauen mittleren Alters in mehreren demografischen Gruppen in Deutschland im Zeitraum von 1990 bis 2015 entwickelt haben. Ein besonderer Fokus wird auf Suizide und Todesfälle, die mit Drogen und Alkohol zusammenhängen, sogenannte *deaths of despair* (deutsch: Tode aus Verzweiflung), gelegt. Die Ergebnisse zeigen ein sehr klares Muster: Die Mortalitätsraten sind zwischen 1990 und 2015 gesunken und für keine der demografischen Gruppen wird ein Anstieg in den *deaths of despair* verzeichnet. Diese Ergebnisse für Deutschland stehen im Gegensatz zu den USA, wo die Mortalitätsraten bei weißen, nichthispanischen Menschen mittleren Alters seit Ende der 90er Jahre steigen.

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Declaration

Erklärung gem. §4 Abs. 2 der Promotionsordnung

Hiermit erkläre ich, dass ich mich noch keinem Promotionsverfahren unterzogen oder um Zulassung zu einem solchen beworben habe, und die Dissertation in der gleichen oder einer anderen Fassung bzw. Überarbeitung einer anderen Fakultät, einem Prüfungsausschuss oder einem Fachvertreter an einer anderen Hochschule nicht bereits zur Überprüfung vorgelegen hat.

Berlin, May 2019

Julia Schmieder

Erklärung gem. §10 Abs. 3 der Promotionsordnung

Hiermit erkläre ich, dass ich für die Dissertation folgende Hilfsmittel und Hilfen verwendet habe: Stata, R, QGIS und Microsoft Excel.

Berlin, May 2019

Julia Schmieder