Essays in Applied Microeconomics

by

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Abstract

This dissertation evaluates two public policies using methodologies from applied microeconomics. The first chapter examines an immigration policy in the U.S and its impacts on local labor markets and by industry. The second chapter investigates how an expansionary maternity leave reform in Germany affected firm behaviors in terms of their employee structure, job flows, and survival. Despite their topics of importance, this dissertation is among the first to discover policy implications in the outcomes examined.

Chapter 1 begins by presenting an empirical study of labor market impacts of Secure Communities, a local immigration enforcement policy introduced in 2008 that is known to affect a wide range of immigrants. Exploiting its traits as a staggered roll-out program by county until 2013 when it became active in all U.S. counties, this study examines factors to policy adoption, and the economic impacts on local labor markets by using panel fixed effects regressions and event study analyses. It identifies the effects by pairing state border counties that are contiguous to each other and using their within-pair variations in activation. The study did not find discernible effects of the program on the labor market variables such as population, labor force, or earnings variables. Although the panel fixed regression show negative effects on per capita earnings or income variables, the event study analyses reveal that they may be stemming from trends that existed prior to the program implementation. In addition, there was no evidence of effects at the industry level, particularly in the immigrant-intensive industries, in counties that adopted the program. The

findings show that neither positive nor negative impacts of the program on the local labor markets were present.

Chapter 2 examines the effects of a maternity leave expansion in Germany that extended the job protection period of working mothers from 2 weeks to 6 months in 1979. The analyses use Establishment History Panel (BHP) from the Institute for Employment Research (IAB) in Germany for years 1975-1985 and explore differential effects of the reform by firm size in a difference-in-difference framework. While the results confirm previous empirical findings of higher female employment after an expansionary reform, this paper further pursues the possible reasons behind the phenomenon by looking at full-time and part-time female employment and female job flows. The results suggest that higher employment in female employees are driven by the increase in part-time female employment, and that this is more strongly observed in small firms. Additional findings suggest that small firms adjust to the reform by employing more female rehires who have previously worked in the firm, and are more likely to shut down following the reform relative to big-size firms. In addition, heterogeneity by location show that there were more pronounced reform effects in rural areas where labor markets are thin.

Chapter 1

Secure Communities: Impacts on Local Labor Markets

1.1. Introduction

According to the 2010 U.S. Census, the number of foreign-born in the U.S. was estimated to be around 40 million, which accounted for 12.9% of the total population.[1] This reflects a dramatic increase from 1970 when only 4.7% of the total population was foreign born. The trend since the 1850s is shown in Figure 1.1 using the Census data.[2] In the past few decade, with the rising level of legal immigrants came a surge of undocumented immigrants. The Pew Research Center estimates 11 million unauthorized immigrants to have been present in the year 2015, which accounted for 3.4% of the total U.S. population (Krogstad *et al.* 2017). This is a significant growth from their estimate of 3.5 million undocumented immigrants in 1990, which then accounted for 1.5% of the population. In 2014, 5% of the U.S. civilian labor force were unauthorized immigrants totaling approximately 8 million (Passel and Cohn 2016).

[1] Foreign-born population include naturalized citizens, legal immigrants and counted undocumented immigrants.
[2] One factor that contributed to the rise of immigrants in general since the 1970s is the Immigration and Naturalization Act in 1965 that repealed the previous immigration quota system based on nationality and allowed immigration of family members of U.S. citizens. Immigrant policies afterwards have focused on limiting unauthorized immigrants.

In an effort to control the number of unauthorized immigrants in the U.S., various approaches have been taken at both national and local levels.[3] This paper examines one immigration enforcement policy, named Secure Communities, a relatively recent immigration program that tailors federal level policies to regional levels. The program allows sharing with the Department of Homeland Security (DHS) the biometric information of arrestees collected during the booking procedures. DHS then checks the legal status of the individual against its immigration database and can issue a detainer should queries arise. Since its inception in 2008, all 3,181 U.S. counties have activated the program by January 2013. It is administered at the county level, and is unique in that an interior immigration program has never been expanded to such a national scale compared to other local immigration policies such as those enforced at the border or 287(g) of the Immigration and Nationality Act. Moreover, it requires no additional enforcement staff like other federal enforcement operations.[4]

Specifically, this paper aims to answer the following questions: (1) What are the factors that induce a county to activate the policy? (2) What are the economic impacts on the local labor markets at the county level?, and (3) What are the impacts on industries? Besides addressing the impacts of a hotly debated immigration policy, the paper contributes to the literature largely in two

[3] At the federal level, for instance, the Immigration and Control Act of 1986 penalized employers who employed unauthorized immigrants. In addition, there were policies aimed at limiting the access to public benefits program such as the Personal Responsibility and Work Opportunity Reconciliation Act of 1996 that prevented undocumented immigrants from receiving any kind of public benefits such as Medicaid, Temporary Assistance to Needy Families or Children's Health Insurance Program. The Illegal Immigration Reform and Immigration Responsibility Act (IIRIRA) of 1996 was also enacted to remove immigrants based on the previous record of non-violent offenses, expedite the process of deportation, and enhance border enforcement. These laws are known to have had devastating impacts on undocumented immigrants and on immigrants in general (Ewing 2012).

Policies targeting specifically the border region were enacted as well. From IIRIRA, heightened border control has been implemented to tackle the flow of undocumented immigrants, which now constitutes one of the major immigration enforcement policies. Further, various border patrol operations have been conducted to deter illegal migration, such as Operations Streamline in 2005 that ordered federal criminal charges for every person who crosses the border illegally. Numerous studies examine the link between U.S. border enforcement and the flow of illegal migration. (Espenshade 1994, Hanson *et al.* 1999, Angelucci 2012).

[4] For instance, there exists a task force for various border enforcement operations, and the 287(g) of the Immigration and Nationality Act requires training of officers in jurisdictions participating in the program.

ways. First, local or interior immigration enforcement policies are a relatively recent form of immigration reforms, thus they have not been extensively studied so far. Moreover, studies on their economic impacts are scant despite their importance for policy implications. For Secure Communities, in particular, past research has often focused on addressing the program's effects on crimes, remittances, health of immigrants, or centered on descriptive analyses on how counties utilize the program (Cox and Miles 2013, Miles and Cox 2014, Amuedo-Dorantes and Puttitanun 2014, Rhodes *et al.* 2015, Pedroza 2013). Thus, the current paper attempts to add empirical evidence to the literature in examining labor market impacts of an immigration policy enforced at the interior level. Second, the paper utilizes a local identification strategy by comparing contiguous county pairs along state borders to control for unobserved heterogeneous local shocks. The similarities in the economic trends within contiguous pairs allow one to better exploit the variations of staggered timing in Secure Communities activation and isolate its impacts on the local economies. Such selection of the regression sample with fixed effects can better capture spatial heterogeneity that may be present in traditional fixed effects specifications (Dube *et al.* 2010). Although using contiguous county pairs with pair-specific-period fixed effects had been recently suggested and debated in the minimum wage literature, its usage in the immigration literature has been relatively new.[5] Datasets from various source are used from years 2000-2015 to examine both pre-treatment and post-treatment impacts.

Although originally intended to deport immigrants with serious criminal offenses according to the DHS, Secure Communities is known to affect not only undocumented immigrants with minor offenses but also immigrants in general (Kohli *et al.* 2011, Jung 2015). Since immigration policies constitute an important determinant of location choice by immigrants, the

[5] To the author's knowledge, there is on paper, Bohn and Santillano (2017), that uses a similar sample selection method and the same identification strategy to examine industry impacts of 287(g) of the Immigration and Nationality Act.

internal composition of county residents with Secure Communities in place is expected to change since the program increases the risk of residing and working in such counties for undocumented immigrants, inducing them to migrate to other counties where immigration policies are more integrative or abroad.[6] Watson (2013) examines the effect of a local immigration enforcement policy named 287(g) agreements on migrant location choice, and finds that they encourage immigrants to relocate to another Census division or region within the U.S., but not abroad. The paper provides evidence that individuals do respond to local immigration policies by migrating elsewhere. Other papers show mixed results, however (Parrado 2012).

The program is expected to act initially as a labor supply shock, but the overall employment and wage outcomes may be ambiguous depending on the response of firms to the increased removals due to the program. The effects of migration on labor demand has long been discussed since Greenwood and Hunt (1984) that suggests several channels that can shift labor demand such as through the human and physical capital immigrants bring with them and their influence on the local good markets. Bodvarsson *et al.* (2008) use a general equilibrium model to show that the Mariel Boatlift augmented labor demand, suggesting it as the mechanism behind the benign effects of the immigration surge on wages. The authors note that immigrants have both labor supply and labor demand impacts as they are also consumers to the goods they help produce. Borjas (2013) also emphasizes the consumption behavior of immigrants and its resulting impacts on labor demand. In terms of examining a specific local immigration enforcement program, Bohn and Santillano (2017) also finds some evidence for adverse labor demand shocks from 287(g), a policy that facilitates deportation of unauthorized immigrants.

[6] There may also be legal immigrants with undocumented household members who may decide to migrate to other counties.

The current study finds no discernible impacts of Secure Communities on local labor markets. A fixed effects regression analyses show that there are no changes in the overall population or labor force in the counties that adopt the program. They also reveal negative impacts on earnings variables such as per capita personal income, per capita earnings, average wages and salaries, and average earnings per job. These findings alone may suggest the possibility of an adverse shock in the labor demand. However, event study analyses that allow treatment effects to vary across years show that these negative effects on many earnings and income variables may be actually be driven by trends that have existed prior to the program activation. Therefore, there is no strong support for either a positive or negative effects of the program on the local labor markets.

In addition, industry analyses were conducted on 12 high-level industries defined by the Bureau of Labor Statistics (BLS). The outcomes show that county-level employment or earnings in industries with high proportions of immigrants were not affected. And on affected industries, there appeared to be no specific pattern of the direction or magnitude of the program impacts. All in all, the industry-level findings also seem to suggest that Secure Communities did not have statistically significant economic consequences in the local economies.

The rest of the paper is organized as follows. Section 1.2 explains the background of Secure Communities. Section 1.3 describes the data. Section 1.4 examines determinants of policy activation. Section 1.5 shows the panel fixed effects model and empirical results, and Section 1.6 presents event study analyses. Section 1.7 shows the industry-level findings. Program impacts on arrests variables are examined in Section 1.8. Section 1.9 concludes.

1.2. Background of Secure Communities

Secure Communities, sometimes referred to as "S-Comm" is a DHS program introduced in 2008 that allows automatic electronic information sharing of individuals between local law enforcement agencies and the DHS. For decades, the fingerprint data of those arrested or booked into custody have been collected from local law enforcement agencies and shared with FBI where it is stored in their Integrated Automated Fingerprint Identification System (IAFIS). Under Secure Communities, these data are automatically forwarded to the DHS, which are then checked with their immigration database to see if the arrestees satisfy a criteria of deportation. The fingerprint repository that DHS refers to is called the Automated Biometric Identification System, also known as IDENT, containing massive information of travelers, immigrants, and etc. If arrestees are identified as potentially removable, or "matched," the Immigration and Customs Enforcement (ICE), the principal investigative branch of DHS, can issue a detainer to request the local law enforcement agencies to detain the individual in jail for 48 hours until ICE takes them into custody (Kohli *et al.* 2011). The request can be based on their status of residence in the U.S., their past criminal records, violations of immigration policies, severity of the crime, among other factors.

The goal of this program by ICE is to use the interoperability between FBI's IAFIS and DHS's IDENT as a cost-effective way to deport those who are of significant threat to the community, mainly referred to as criminal aliens.[7] Since it is costly to identify and remove all undocumented immigrants, Secure Communities allows prioritizing the removal of those who

[7] According to DHS IDENT/IAFIS Interoperability Statistics Report, interoperability is defined as "the sharing of alien immigration history, criminal history, and terrorist information based on positive identification and the interoperable capabilities of IDENT and IAFIS." See https://www.dhs.gov/xlibrary/assets/foia/US-VISIT_IDENT-IAFISReport.pdf.

have criminal records or those with repeated violations of immigration laws. ICE reports that it has identified a significant portion of convicted criminals through the program.[8]

The program categorizes individuals into one of the three groups based on the severity of the crimes: Level 1, 2 or 3. The most serious offenses that include national security violation, homicide, or drug uses are categorized into Level 1. Level 2 offenses include property crimes, burglary, traffic offenses, etc. Level 3 includes misdemeanors such as public order crime, gambling, or bribery. From the program's inception in October 2008 to February 2015, ICE removed over 406,441 aliens through fingerprint matching of which only 135,726 individuals (33.4%) were convicted of Level 1 crimes (DHS ICE 2015).

Like many other immigration policies, the program is highly debatable. Some policy reports reveal that the Secure Communities provides incentives to local law enforcement agencies for racial profiling or pre-textual arrests (Kohli *et al.* 2011). Other advocacy or community groups argue that the program goes beyond targeting individuals that is of great threat to public safety and is creating a hostile environment that encourages voluntary deportation of individuals. The same policy report suggests that 11% of the people who are charged with deportation choose to be removed voluntarily. There is also a strong perception among the community groups that there are spillover effects of the program on local policing as the local police act as ICE agents although only federal ICE/DHS officers are authorized to make immigration enforcement decisions (Treyger *et al*. 2014).

Amid the controversy, Secure Communities has been replaced by Priority Replacement Program (PEP) in July 2015, which will be the last year of data used for the current study. PEP provides narrower enforcement guidelines to seek and detain individuals in specific, limited

[8] See http://www.ice.gov/secure_communities/.

circumstances. Under Secure Communities, some ICE detainers were issued to foreign-born individuals who did not have a biometric match in the ICE database. The PEP, however, does not allow issuance of detainers unless there is a probable cause that the individual is of high enforcement priority, although this is also a topic of open debate.

Other programs that facilitate information sharing among local, state, and federal law enforcement agencies include programs such as 287(g) of the Immigration and Nationality Act and the Criminal Alien Program (CAP). The 287(g) program trains law enforcement agents to enforce federal immigration laws in a more effective way using a local task force model. The CAP works more like Secure Communities, but is a more expansive immigration enforcement program that leads to the initiation of removal proceedings in many cases. It has been present in the U.S. for decades in different forms, but very limited information has been disclosed to the public regarding how CAP works, its roles, and the relation to Secure Communities. It is currently active in all state and federal prisons, as well as more than 300 local jails throughout the country. The current paper does not address the effects of these programs, but only those of Secure Communities.

Secure Communities was activated in 14 pilot jurisdictions in October 2008. The program gradually expanded to other counties across the U.S., and was activated in all 3,181 jurisdictions by January 2013. Although voluntary at first, the DHS effectively announced the policy to be mandatory in October 2010 when some counties, such as in Illinois or Massachusetts, which already had Secure Communities in place attempted to opt out of the policy. The program was debatable in terms of its voluntary nature since its beginning, but on October 2010, Janet Napolitano, the then Secretary of Homeland Security, officially announced that it was not optional for municipalities to participate in the program. This date is used as the benchmark to divide the

counties into "voluntary" counties before the date and "involuntary" counties that adopted the program afterwards. The analysis that looks at the factors to program activation uses voluntary counties only.

Figure 1.2 shows the number of activated counties by year, and this covers all the counties in the U.S. by the year 2013. The 107 counties activated in January 2013 are mostly from counties in Illinois and Alabama. Figures 1.3 shows geographically the activation of Secure Communities by year for 3,143 counties. The analysis excludes Alaska or other territories of the U.S. The shades become darker for counties that activate the policy in later years. Counties close to the U.S.-Mexican border appear to be the earliest implementers of the program in 2008 and 2009. These are counties in states such as California, Arizona, New Mexico, Texas, and Florida, which adopted the policy statewide by 2010. These states are generally where most undocumented immigrants are known to reside. The majority of the counties in northern mid-west region in Montana, Wyoming, Colorado and Minnesota, for instance, adopted the program in 2012. Many northern ormid-west regions have activated the policy uniformly as a state. However, counties in the rest of the states appear to be have activated the policy in different quarters across different years.[9]

While Figure 1.3 includes all U.S. interior counties, Figures 1.4 shows the years of activation for counties that are selected for the main regressions.[10] The number of unique counties used in the regression totals 959 border counties in 49 states. Each of these counties is paired with contiguous county or counties across its state border resulting in 952 unique pairs. These will be referred to as Contiguous Border Pair Counties (CBCP) following the terminology from Dube *et*

[9] The exact dates of Secure Communities activation by each county can be seen here: http://www.ice.gov/doclib/secure-communities/pdf/sc-activated.pdf

[10] Figure 1.4 is taken directly from Dube *et al.* (2016) presented in Appendix Figure C1: Map of Contiguous Border Pairs. This paper uses counties that are dark shaded (red and blue), i.e. county pairs whose centroids are less than 75 km apart.

al. (2010, 2016). The motivation for selecting CBCP as the regression sample is to compare proximate counties and to take advantage of the variations within the contiguous county pairs that are different only in activation dates. The regressions account for time-varying local economic shocks as contiguous counties are known to be similar in many economic trends while responding similarly to macroeconomic and idiosyncratic shocks. This identification strategy is considered to better capture unobserved heterogeneous trends than those that use traditional fixed effects regressions using all samples (Dube *et al.* 2010, 2016).

1.3. Data Sources and Descriptive Statistics

1.3.1. Data Sources

This paper uses data from several sources: (1) the Bureau of Labor Statistics (BLS) Local Area Unemployment Statistics (LAUS), (2) Bureau of Economic Analysis (BEA) Local Area Personal Income and Employment (LAPIE), (3) Uniform Crime Reporting Program: County-level Detailed Arrest and Offense Data (UCR), and (4) the number of foreign born based on the 2000 decennial census and 2005-2009 American Community Survey 5-year estimates.

First, the BLS and BEA datasets provide annual, aggregated county-level data, from which years 2000 to 2015 will be used. The BLS LAUS provides variables for labor force, number of employed and unemployed, and unemployment rate. Second, employment variables and data related to earnings and income such as personal income, per capita income, per capita earnings, wages and salaries, plus population, are collected from the BEA. The BEA LAPIE also provides total earnings and employment data at the industry-level for years 2001-2015 although values for the majority of counties are not disclosed for confidentiality reasons. A total of 12 high-level industries are examined. Third, the number of crimes and arrests by county are obtained from the

UCR from the Inter-university Consortium for Political and Social Research (ICPSR) for years 2000 to 2014 (ICPSR 2017).[11] Crimes are defined as the total number of criminal activities reported that involved murder, rape, robbery, aggravated assault, burglary, larceny, motor vehicle theft, or arson. Violent crimes refer to the sum of the first four types of crimes.[12] The number of arrests are further divided into those of four crime categories: violent crimes, property crimes, other crimes, and minor crimes. Property crimes include burglary, larceny, motor vehicle theft, arson, and having stolen property. Other crimes consist of 15 types of crimes that are not categorized under either violent crimes or property crimes.[13] Of the crimes categorized under "other crimes," minor crimes consist of drug abuse violations, liquor law violations, vandalism, and prostitution and commercialized vice. The arrest levels of different crimes are used as outcomes to examine how the program has affected local policing patterns. Fourth, the foreign-born population data are obtained from the secondary source since this measure at the county level from the Census was not publicly available (GCIR 2017).[14]

1.3.2. Sample Construction

The empirical strategy of the current study is taken from Dube *et al.*(2010, 2016) that examines the impact of minimum wage laws on earnings and employment, among other things.

[11] To be precise, crimes variables are used for years 2008-2013 when examining the factors to Secure Communities activation in Section 1.4, and arrests variables are used for years 2000-2014 in Section 1.8.

[12] The current paper identifies the missing values of crimes and arrests by the coverage indicator variable provided by the ICPSR that refers to the proportion of county data that is not imputed for a given year. It is computed missing if this coverage indicator is zero and all other crime or arrests values are zero as noted in the codebooks of the data used.

[13] They are forgery/counterfeiting, fraud, embezzlement, vandalism, weapons violations, prostitution and commercialized vice, sex offenses, drug abuse violations, gamble, offenses against family and children, driving under influence, liquor law violations, drunkenness, disorder conduct, and vagrancy.

[14] The institution that provides the data is Grantmakers Concerned with Immigrants and Refugees (GCIR). See https://www.gcir.org/immigration/facts/demographics.

Largely phrased under border discontinuity designs, the methodology compares contiguous county pairs bordering different states relying on the fact that they differ in their policies but not in their economic trends. They argue that their local identification uses better control groups and well captures unobserved spatial heterogeneity compared to traditional fixed effects specifications with various time and regional effects. This has triggered a recent debate in the minimum wage literature over credible statistical methods used (Neumark *et al.* 2014, Neumark and Wascher 2017). Despite the debate, border difference approach in general has been employed in various contexts with different types of regressions, for instance, in firm entry, welfare, or school quality (Rohlin *et al.* 2014, McKinnish 2007, Dhar and Ross 2012).

The current paper uses the same sample of contiguous border county-pairs adopted from Dube *et al.* (2016) as shown in Figure 1.4. This totaled 1,137 unique counties and 1,177 county pairs to begin with. The final sample used for regression was 959 counties with 952 county pairs due to the following process. First, five counties that had inconsistent program activation dates across datasets were dropped along with the counties that were paired with them.[15] Second, counties whose centroids were more than 75 kilometers apart were dropped which was the cutoff used in Dube *et al.* (2016). In their paper, the authors show that contiguous counties that had a larger distance than this cutoff turned out to be less similar to each other based on mean-square error calculations of several variables by a randomization procedure.[16] Lastly, since BEA combines some independent cities in Virginia with adjacent or surrounding counties for statistical

[15] DHS reports Secure Communities activation date by county in http://www.ice.gov/doclib/secure-communities/pdf/sc-activated.pdf. However, some of the dates are different from those recorded in IDENT/IAFIS Interoperability Statistics reports. When using the CBCP sample, counties and their paired counties that had a difference of more than 16 days were dropped. This totaled 18 counties.

[16] Section 2 and online Appendix C of their paper explains this procedure in detail.

purposes, those cities in BLS and interoperability data were either combined or adjusted accordingly for unity.

For industry-level data, years 2001-2015 are used. Using the BEA LAPIE by industry, 12 high-level industries that are categorized by the BLS are used.[17] The BEA does not, however, show earnings and employment values for some counties for confidentiality reasons. The cells are marked as (D) for those not disclosed of confidentiality reasons, (L) for less than $50,000 in earnings or 10 jobs in employment, or (N) for data not available. For the current analyses, all those data points are treated as missing. The degrees of missing data vary widely by type of industry. For instance, for both earnings and employment variables, only 40% of the total CBCP sample are non-missing in the agriculture industry, and are used in the regressions. 85% of the CBCP sample was used in the regressions in the financial activities industry. Non-missing data proportions for other industries fall between these two ranges. The counties that were paired with the counties with missing data were further dropped since the methodology used in the paper requires within-pair comparisons. Despite its missing values, the analyses are still deemed useful in studying the varying impacts of the program on different industries, particularly in immigrant-intensive industries.

1.3.3. Descriptive Statistics

Table 1.1 shows the summary statistics of the BLS, BEA, UCRS, and foreign-born variables. The unit of observation is county-year, and the data contains 965 unique counties that are used for fixed effects regressions and event study analyses to follow. Counties that were combined or had their names changed throughout years 2000 to 2015 were dropped from the final

[17] See "Industries at a glance" on https://www.bls.gov/iag/.

dataset. Data of Alaska and counties or administrative divisions in other U.S. territories are also excluded.

1.4. Factors to Policy Activation

Before the main analyses to follow, it is informative to examine the factors that induce counties to activate Secure Communities since the results may shed some light on the decision rules for enacting the policy by county characteristics. I estimate a Cox proportional hazard model that is commonly used for duration analysis of policy adoption. In the current context, the hazard is defined as the "risk" of adopting Secure Communities. The counties are at risk from the year 2008, when the policy was first piloted in 14 counties. I use a sample of counties that "voluntarily" adopted the program, i.e. before October 1, 2010, which totals 626 counties.

Table 1.2 reports the estimates from a Cox proportional hazard model using time-varying characteristics of the number of crimes and labor market variables. The unit of observation is county-year, with years after the first year of policy adoption eliminated. The growth rates in the previous year (from year t-2 to t-1) of selected local labor market variables and the number of crimes at the county level are used as covariates in the regression. Using these covariates satisfies the proportional hazards assumption test using the Schoenfeld residuals. Different combinations of variables are shown for each column. Since there are multiple events with the same survival time, or tied events, the efron approximations are used to estimate the model (Borucka 2014).

The results show that some variables are statistically significant in affecting the speed of the program adoption. First, this program was initially advertised to remove criminal aliens, but the results show that higher growth rate in crimes of a county was not a significant factor in affecting the adoption of the policy. The result was the same when using violent crimes instead of

the number of total crimes although those results are not shown here. Second, changes in population growth by one percent makes a county more likely to adopt the policy no matter what other variables are used in the regression. Lastly, growth in unemployment rates or the number of unemployed are shown to make a county more likely to implement the policy when certain combinations of variables are used (column (2), (5), and (8)). This tends to be in line with some findings in previous literature that regions tend to welcome immigrants during economic booms but not during recessions (Monogan 2013, Berg 2015). However, its significance disappears when used with certain combinations of variables. Growth rates in other variables such as employment or earnings do not affect the program adoption. In summary, the only strong and consistent factor that induced counties to activate Secure Communities is the growth in the county population.

Note that the analyses only consider time-varying factors of the program activation decision. Political factors such as party leaning tendencies or geographical factors of counties are not used since they do not vary with time. Demographic factors such as the number of foreign born in the given county relative to the population are also informative, but they are not included here due to data limitations. Considering the nature of the program where it was publicized for its low-cost enforcement program, it may be interesting to see how county or state budget has also affected county's decision to activate the program. Due to possible omitted variables that can potentially affect policy activation, these findings should not be conclusive but suggestive of how economic factors and crime play a role in county participation of Secure Communities.

1.5. Program Impacts on Local Labor Markets

1.5.1. Panel Fixed Effects Regression

The following equation shows the main regression models for examining the impact of Secure Communities on local labor markets using the Contiguous Border County-Pair (CBCP) sample.

$$Y_{jpt} = \alpha\, TREATMENT_{jt} + \emptyset_j + \delta_{pt} + e_{jpt} \qquad (1)$$

The outcome variables Y_{jpt} to be examined are local labor market variables that vary by county *j*, county pair *p*, and year *t*=2000, 2001, … 2014, 2015. The following fourteen labor market outcomes at the county level will be examined: population, labor force, number of employed, number of unemployed, unemployment rate, number of total full-time and part-time jobs, number of wage and salary jobs, personal income, earnings per place of work, per capita income, per capita earnings, wage and salaries, average wage and salaries and average earnings. The $TREATMENT_{jt}$ variable is a dummy variable that is equal to 1 if county *j* has Secure Communities in place at year *t* and zero otherwise. Once a county activates the program, it will be treated throughout the rest of the sample period. $\emptyset_j$ is county fixed effects that is used to control for unobserved time-invariant characteristics across counties.

Another difference of these regressions compared to the traditional fixed effects regressions that employ all samples is that they include pair-specific time effects, δ_{pt}, to control for time-varying economic shocks by contiguous border county pairs to better exploit the variations in program activation dates and intensity within each pair. A coefficient exists for each pair-period dummy. The standard errors are clustered at the county level. In addition, a county can

be in many pairs since it can be contiguous to multiple counties across the border. Thus, to account for potential mechanical correlations across the border, Dube *et al.(*2010, 2016) also cluster the standard errors on the border segment, defined as the set of counties on both sides of a border between two states. The same is done here. The coefficient of interest is α. It represents the impact of Secure-Communities-activated counties on their labor markets compared to non-activated counties.

1.5.2. Main Findings

This section examines the regression results of equation (1) showing the impacts of Secure Communities on fourteen labor market outcomes. The motivation for these regressions is to investigate the effects of the program on the local labor markets, channeled through the response of migrants in terms of their location choice. All values are in natural logs except the unemployment rate. In addition, lead values are used for the outcomes considering the time lag the treatment may have had on the labor outcomes.

Table 1.3 shows the impact of the treatment. Due to the nature of the program, unauthorized immigrants and legal immigrants to some extent are expected to leave activated counties, but the results reveal that population or labor force is unchanged. This may be due to a few reasons: (1) even if the undocumented workers migrate out of the counties, natives may choose to enter instead due to a safer environment that the program is intended to create or due to better labor market opportunities perceived for the natives, (2) unauthorized immigrants are underrepresented in the data, or (3) the effect on migration of affected immigrants is indeed minimal. Due to the data limitations, I cannot examine how natives and immigrants are affected separately by the program in terms of their location choice, employment, or wage changes. Therefore, I only assume that

natives are least aware of the program and its nature, therefore are not likely to be affected. Thus the first possibility is excluded in the analysis.

There is a concern for the second possibility which is one of the difficulties in any empirical study covering unauthorized immigrants. There have been efforts to calculate the undercount rate of different types of data. For instance, the Census Bureau estimates an undercount rate of unauthorized immigrants to be 15% in the U.S. population censuses, which is the basis dataset for population and labor force variables uses in this paper (Hanson 2006). Nevertheless, researchers often do rely on the Census data to conduct analyses on unauthorized immigrants despite the undercounts (Hanson 2006, Passel and Cohn 2016). Moreover, Secure Communities is known to affect not only unauthorized immigrants but legal immigrants as well (Kohli *et al.* 2011). And evidence for foreign-born responses to local immigration policies like 287(g) also exists (Watson 2013).

The total number of employed and the number of unemployed also do not change due to the treatment. Wage and salary employment, however, increased by about 0.97% due to the policy adoption at the 10% significance level.[18] The bottom row of Table 1.3 shows the impact of Secure Communities on variables related to income or earnings. They show the per capita personal income decreased by 0.92%, per capita earnings by 1.24%, average wages and salaries by 0.78%, and average earnings per job by 1.64%. Personal income, earnings per worker, and wages and salaries were not affected.[19]

[18] The BEA employment series for states and local areas comprise of estimates of the number of jobs, full-time plus part-time, by place of work. Full-time and part-time jobs are counted at equal weight. Employees, sole proprietors, and active partners are included, but unpaid family workers and volunteers are not. Wage and salary jobs or employment measures the average annual number of full-time and part-time jobs in each area by place-of-work, and jobs for which wages and salaries are paid are counted.

[19] Average earnings per job is total earnings divided by total full-time and part-time employment. Average wages and salaries is wages and salaries divided by the number of wage and salary jobs (total wage and salary employment). Current-production income of sole proprietorships, partnerships, and tax-exempt cooperatives. Excludes dividends,

Given that the labor supply curve did not shift, the negative effect on earnings suggest possible adverse shifts in the labor demand. Examining these outcomes alone, it appears that the removals of unauthorized immigrants do not raise the earnings of the remaining workers as firms cannot readily substitute natives for those removed. This is possible if immigrants and natives are not perfect substitutes for each other in general, although most previous studies examine complementarities within an occupation, skill, or education group for a more in-depth analyses (Card 2009, D'Amuri *et al.* 2010, Ottaviano and Peri 2012).

There is a possibility that the labor force data may not capture unauthorized immigrants well compared to the earnings and employment data considering its sources. The BLS LAUS data is primarily based on the Current Population Survey, a collection of household surveys gathered by a combination of live telephone and in-person interviews with household respondents. The survey includes questions on where the individual was born and the date of arrival to the U.S., but it does not ask foreign individuals to reveal their legal immigration status to encourage full participation of respondents. Despite such confidentiality provided, unauthorized immigrants may be hesitant to respond to the survey if the survey method involves live calls or in-person interviews. On the other hand, the BEA data largely stems from surveys from employers who submit a large pool of information including those of their employees.[20] Since most unauthorized workers use fake social security number to be employed, their presence are likely to be better captured in the employer surveys. It is difficult to calculate the exact proportions of employers who unknowingly employ unauthorized workers. Hiring unauthorized workers has been illegal since the IRCA of

monetary interest received by nonfinancial business, and rental income received by persons not primarily engaged in the real estate business.

[20] Three widely used measures of annual county employment and wages by place of work from the BEA are (1) the Census Bureau's employment and payroll data in the County Business Patterns (CBP) series derived from Federal administrative records and survey information of business establishments, (2) the Bureau of Labor Statistics' (BLS) employment and wage tabulations from the unemployment insurance (UI) program, and (3) BEA's estimates of total wage and salary disbursements and employment derived from BLS data.

1986 has been enacted. However, enforcement is known to be lax and penalties low even when caught. Therefore, it is plausible that the employers may submit the information on their unauthorized workers even if they are aware of their status. In sum, the earnings variables may better capture the presence of undocumented workers compared to the labor force variable that reflects the labor supply.

All in all, the fixed effects models using CBCP sample show that the program has had negative effects on earnings variables. This points to the likely presence of an adverse shift in the labor demand. An adverse shift in labor supply may also be likely due to the reasons mentioned before, but using the current data shows no impacts. However, it will be discussed in the following section that the negative program impacts on the earnings variables may actually be driven by possible trends that existed prior to the program activation. Therefore, the findings should not be interpreted by these fixed effects regressiona alone, but together with the event study analyses in Section 1.6.

Note that this is an aggregate analysis conducted at the county level, thus the results are only indicative of how overall labor supply and demand respond. Thus, the analyses conducted by industry is further conducted in Section 1.7 in an attempt to examine the differential impacts of Secure Communities on the industries.

1.6. Dynamic Response to Secure Communities

1.6.1. Event Study Analyses

Event study analyses are conducted to examine the changes in labor market outcomes throughout the years around the Secure Communities activation. The year of activation is normalized to zero for each county. The conditional means comes from the following regression:

$$Y_{jpt} = \sum_{m=-14}^{m=-1} \pi_m D_{jt}(t - T^*_j = m) + \mu_0 D_{jt}(t - T^*_j = 0) + \sum_{m=1}^{m=7} \tau_m D_{jt}(t - T^*_j = m) + \emptyset_j + \delta_{pt} + e_{jpt} \quad (2)$$

where Y_{jpt}'s are the same outcome variables used in equation (1) in county *j* in year *t*=2000, … 2015 for each pair *p*. D_{jt}'s are variables that is equal to one if it satisfies the criterion in the indicator function, $D_{jt}(\ )$. That is, it is one when the year of observation is m = -14, -13, … 6, 7 years from the date T^*_j, the year that a county activated Secure Communities. For instance, counties that activated the policy in the earliest possible year, 2008, ranges from -9 (year 2000) to 7 (year 2015) in the data, while those that activated the policy last in 2013 ranges from -14 (year 2000) to 2 (year 2015) in the data. As before, the term $\emptyset_j$ is county fixed effects and δ_{pt} is pair-specific period effects.

Of interest are the coefficients of D_{jt}'s. The set of π's will describe the evolution of outcomes Y_{jkt} before the activation year, which allows us to examine any pre-existing trends before the activation. The set of τ's will inform us of outcomes after the Secure Communities was put in place. Together, these coefficients will shed light on the changes of the program 14 years before and 7 years after the implementation of the program, which is the purpose of this falsification test. It offers a more flexible specification compared to equation (1) in that the outcome variables are allowed to vary every year before and after the treatment.

1.6.2. Main Findings

Figure 1.5 shows the conditional mean values of labor market variables for the years of pre- and post- activation of Secure Communities. The coefficients of D_{jt}'s in equation (2) are

plotted in a solid line and the dashed lines depict the 95 percent confidence intervals for each coefficient estimate. Since the rollout of Secure Communities is a natural experiment, the treatment variable is not completely exogenous as displayed in Figure 1.3. Thus, using the CBCP sample again with contiguous counties as controls and variations of policy activation dates within pairs is considered to provide better estimates.

For population, the coefficients before the program activation year appears to be negative, while turning positive afterwards. However, looking at the confidence intervals, they are not much different from zero. Labor force or the number of employed also does not display statistically significant effects before or after the policy activation. The number of unemployed increases after the activation year, but there exists a statistically significant upper trend prior to the policy adoption. There is no impact on the unemployment rate, however, either before or after the program. The figures for total employment and wage and salary employment show a positive trend both before and after the activation year.

With regard to the earnings or incomes variables, it is important to compare the event study results of equation (2) to those of a more parsimonious specification of equation (1). The variables that were statistically significant in Table 1.3 (per capita personal income, per capita earnings, average wages and salaries, average earnings per job) are also shown to be significant after the program activation in Figure 1.5 when examining the treatment coefficients within the range of 95% confidence intervals. However, the patterns of the coefficients themselves do suggest a possible downward trend prior to the program adoption, and that they might be driving the negative results shown in Table 1.3. It may be arguable that the average earnings per job does not have a pre-existing trend and that a gradual decrease in the variable is observed after the program, but

altogether, there is no strong evidence that Secure Communities have had a causal impact on the local labor markets in general.

1.7. Industry-level Analysis

Using the BEA industry-level data, the local labor markets were divided into 12 high-level industries defined by the Census based on the North American Industry Classification System (NAICS) industry levels.[21] This allows us to examine which industries are affected by the policy. The same model from equation (1) in Section 1.5.1 is used for two outcome variables: total employment (the number of full-time and part-time jobs) and earnings. The counties that were consistently identified throughout the sample years 2000-2015 were used for the analyses. Table 1.4 shows the regression results.

Passel and Cohn (2016) provides estimates on how lawful and unauthorized immigrants are distributed across industries based on augmented 2014 American Community Survey data in their report.[22] The industries by estimated share of both lawful and unauthorized immigrants from highest to lowest are listed as follows: agriculture (30.6%), construction (24.4%), other services (22.7%), leisure/hospitality (20.7%), professional and business services (19.6%), manufacturing (19.3%), transportation/utilities (17.0%), wholesale/retail trade (14.9%), educational/health services (14.1%), financial activities (13.9%), information (13.6%), and mining (10.5%). These estimates can be used as a reference to examine whether the affected industries contain high proportions of immigrants.

[21] See BLS Industries at a glance https://www.bls.gov/iag/

[22] They use a multi-stage estimation process including the residual method to estimate the number of unauthorized immigrants in the survey. Details can be found in their Methodology section.

If the local labor market is affected at all due to Secure Communities, the effects are expected to be present in relatively more immigrant-intensive industries such as agriculture, construction. However, there is no such evidence shown in the results in Table 1.4. The industries that indicate significant program effects on either earnings or employment are trade, education and health, financial activities, and information by order of proportions of immigrants. They are actually industries that contain relatively small proportions of immigrants compared to others. There is also no unity found in the significance or the direction of the effects on earnings or employment. Therefore, there is also no strong evidence that imply the program has affected the labor market at the industry level.

One caveat of the industry analyses is that BEA does not disclose data for many counties that may reveal information of specific employers. Therefore, it is likely that the current available dataset does not capture earnings and employment of small firms in a county which are more likely to employ unauthorized immigrants. Nevertheless, the current results using the available data are suggestive of the absence of the program effects on the local labor markets.

1.8. Program Impacts on Arrests

One of the main criticisms of Secure Communities have been that it may lay grounds for pretextual arrests targeting immigrant groups of a certain race. The policing pattern may be expected to change as the local police utilize the program to check the immigration status of the arrestees. Thus, the levels of arrests divided by four categories of crimes as defined in Section 1.3.1 were used as outcomes to examine whether they were indeed affected by the program activation: violent crimes, property crimes, other crimes, and minor crimes. Equations (1) and (2) are employed again using the CBCP sample. Table 1.5 shows the regression results of equation

(1), and it appears that Secure Communities adoption has increased the number of arrests in total crimes by 40.5 percent in activated counties, and this increase is mainly coming from arrests in minor crimes which rose by 33.5 percent. However, the dynamic treatment effects shown in Figure 1.6 display the positive effects may be driven by increasing trends in the number of arrests prior to the program introduction. Therefore, it may seem that Secure Communities have altered policing patterns by increasing the number of arrests of crimes, particularly those of minor crimes, using the more parsimonious fixed effects regression of equation (1). However, the causal effects of the program is not well established when examining the results of the event study analysis using equation (2). Therefore, as with the labor market outcomes, there is no compelling support for the argument that the program has changed local policing practices.

With the motivation to study whether the program has improved public safety or encouraged racial profiling, Treyger *et al.* (2014) examines the levels of crimes and arrests using differences-in-differences at the city level. With regard to the level of arrests, they mention that if any program impact is to be found, it should be more apparent in the arrests of minor crimes since there is more room for police discretion compared to those of serious crimes. The authors find lack of effects on arrests for all crime categories. Thus, the outcomes of the current study that uses fixed effects regressions at the county level using the CBCP sample are more or less in line with their results that use traditional fixed effects regressions at the city level.

1.9. Conclusion

Secure Communities is an expansive immigration enforcement policy that has been administered at county levels and gradually expanded nationwide. It was initially introduced to prioritize the removal of criminal aliens in a cost-effective, electronic method that uses already

existing database to identify various types of immigrants. However, some studies and policy reports have provided evidence that the program has been used to sort out and remove undocumented immigrants without any criminal histories. Moreover, individuals who are legally residing or those who have been part of immigrant families are also known to be affected.

This paper is the first to document the local economic impacts of this program by comparing contiguous county pairs to better exploit the rollout nature of the program. Although a more parsimonious version of fixed effects regressions shows that the program had negative impacts on some earnings variables, a further investigation using the event study analysis suggests that the effects may be driven by pre-existing trends that have been present since a few years prior to the reform. This suggests the importance of examining dynamic treatment responses across all years and interpreting the outcomes together with a simpler fixed effects regression. In conclusion, no strong evidence of the causal effect of the program on local labor markets was found. Moreover, the program effects are not present in immigrant-intensive industries which imply that the labor market is neither impacted by the program at the industry level. Further examination on the program effects on the number of arrests by county was conducted, but no evidence was found that suggested the program has affected local policing patterns by increasing the level of arrests.

All in all, the current paper finds that Secure Communities has had negligible economic consequences in the economy as a whole and in industries. The findings are informative to both supporters and critics of the program of which the economic impacts of such immigration policies were not well known. An important supplement or extension to this analysis would be to isolate the migration patterns of immigrants and natives and by their skill or education levels. Examining the compositional changes in their populations separately may provide further insight into the mechanisms that drive the results found in this study.

1.10. References

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1.11. Figures

Figure 1.1. Foreign-Born Population (in millions and as percentage of total population)

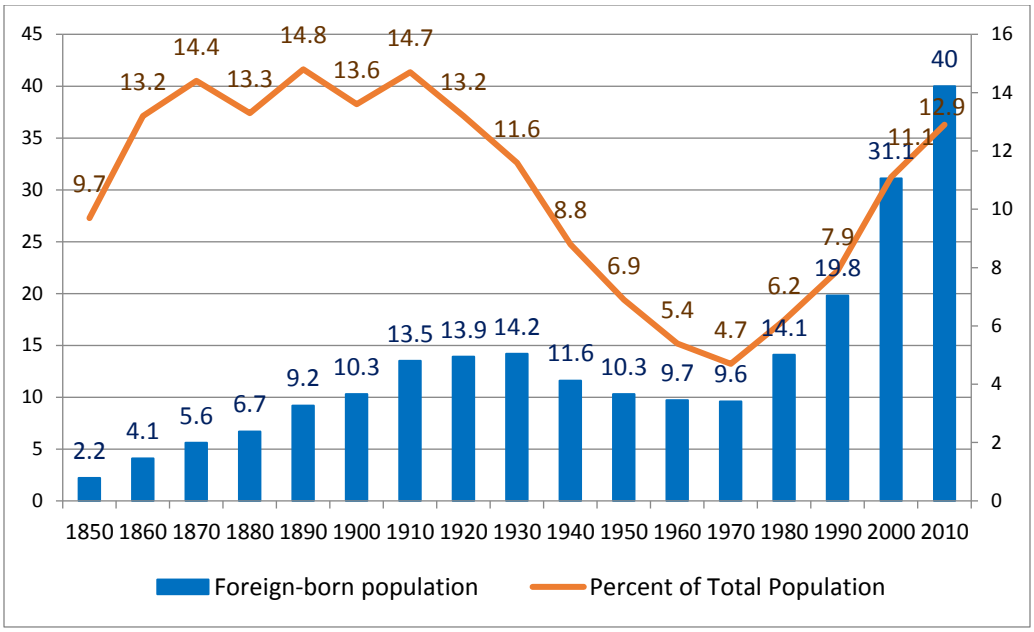


Figure 1.2. Secure Communities by Year

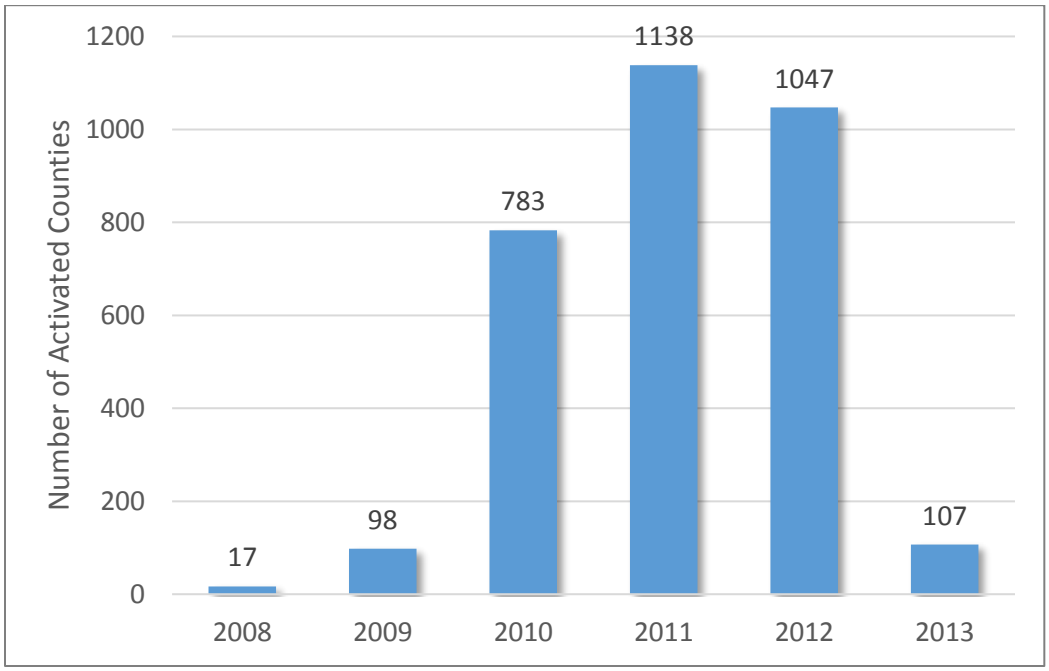
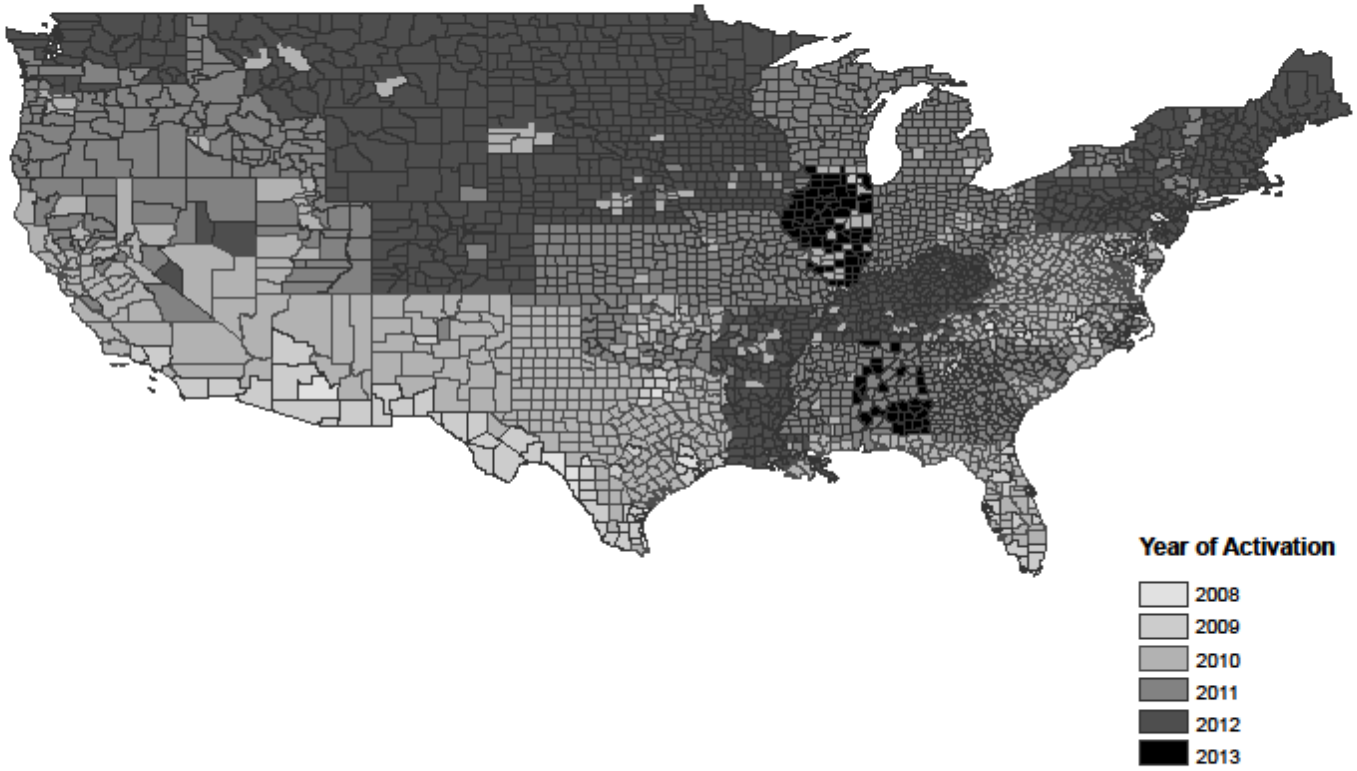
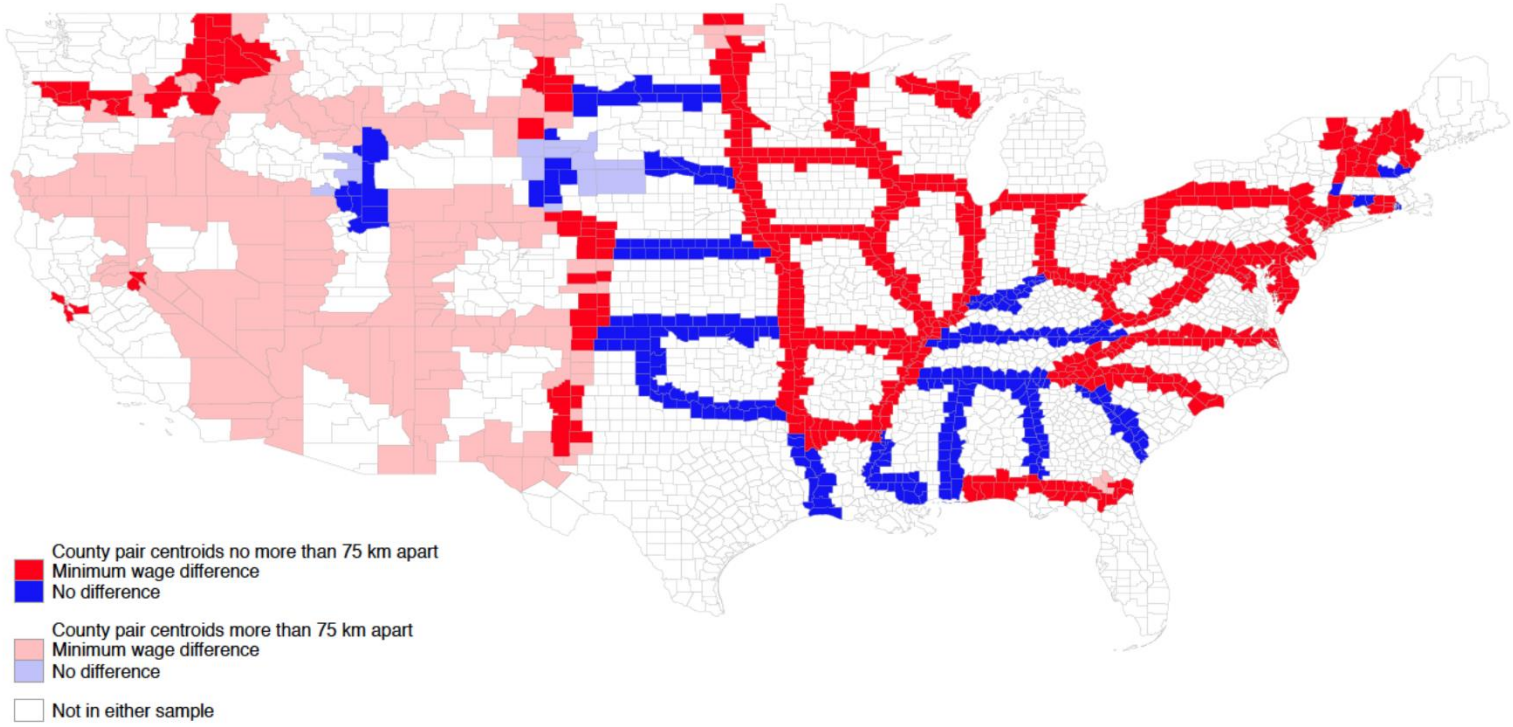


Figure 1.3. Activation of Secure Communities (all counties)



Note: A total ot 3,056 counties are shown. Alaska, American Samoa, Guam, N. Mariana, Puerto Rico and U.S. Virgin Islands are not included.

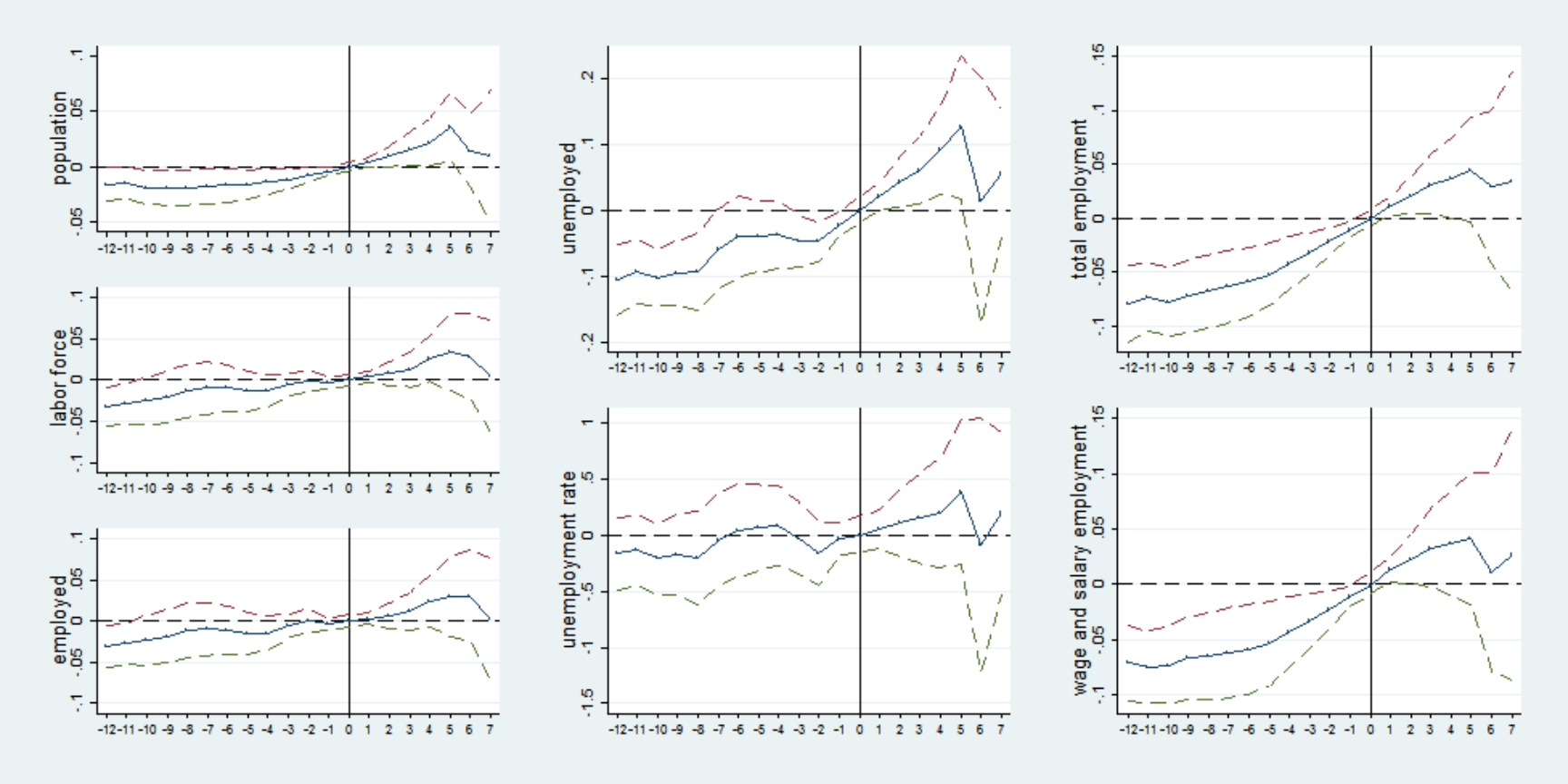
Figure 1.4. Activation of Secure Communities (contiguous border counties)



Source: Dube *et al.* (2016) Appendix Figure C1.

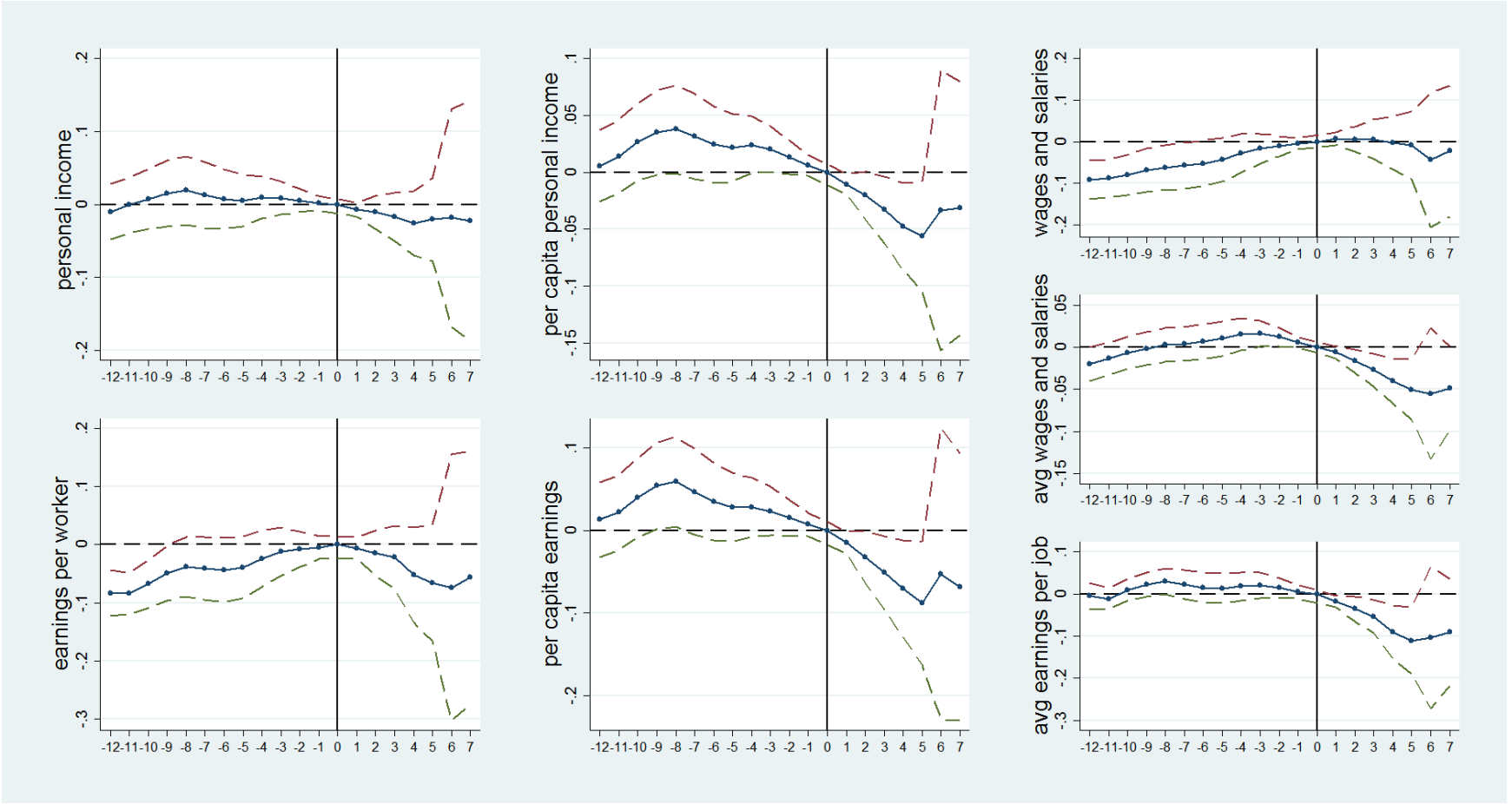
Note: The map shows contiguous border county pairs that are used for regressions and event study analyses in the paper. They amount to 1,137 counties.

Figure 1.5. Dynamic Response of Local Labor Market Variables



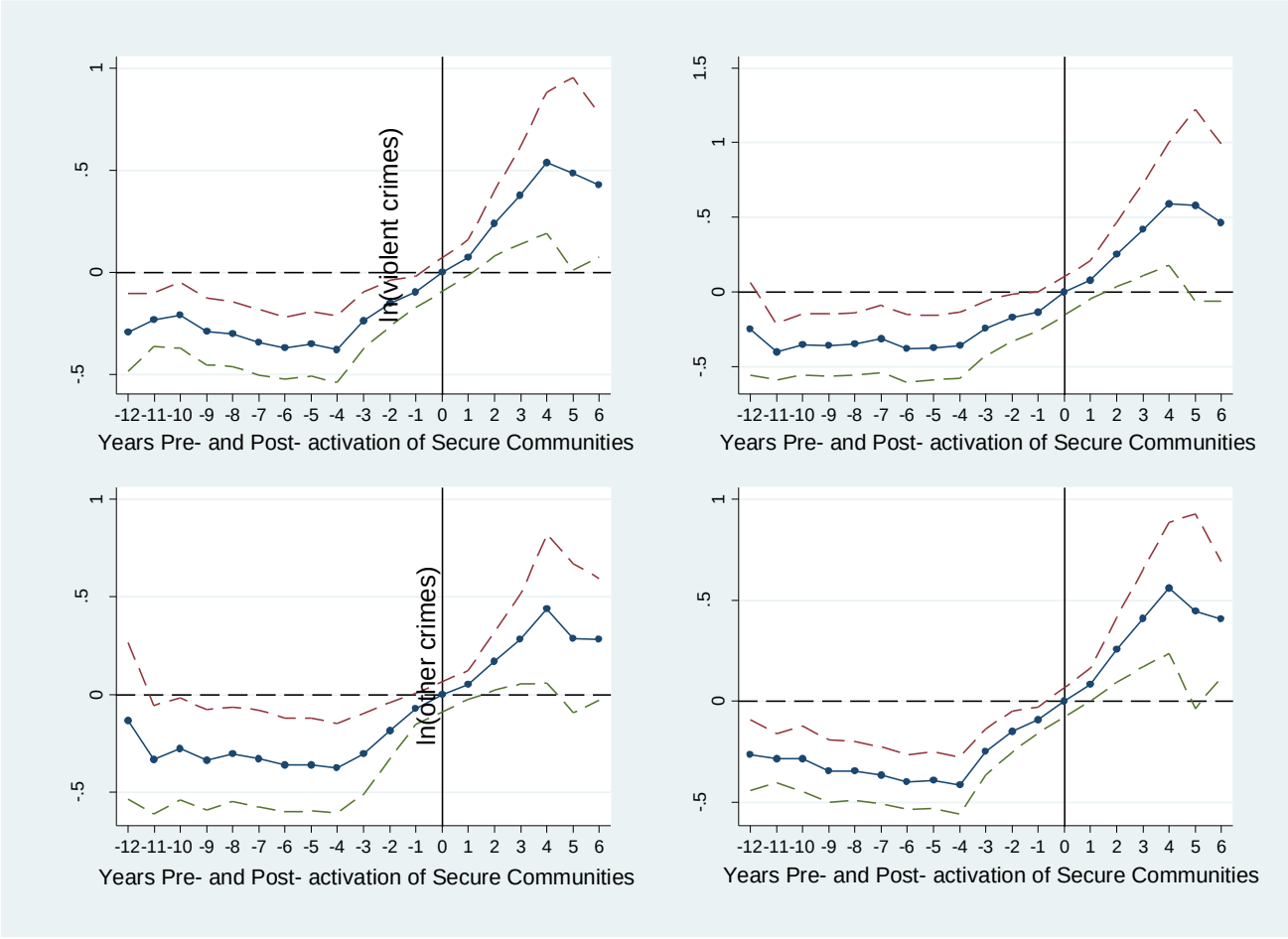
Note: The year counties activated the policy is normalized to zero on the x-axes. They show up to 12 years before and 7 years after the activation year. The coefficients plotted in the solid lines are π's and τ's from regression equation (3) using the CBCP sample with standard errors clustered by county and border segment. The dotted lines indicate 95% confidence levels. All variables except the unemployment rate are in natural logs.

(*Figure 1.5. continued*)



Note: The year counties activated the policy is normalized to zero on the x-axes. They show up to 12 years before and 7 years after the activation year. The coefficients plotted in the solid lines are π's and τ's from regression equation (3) using the CBCP sample with standard errors clustered by county and border segment. The dotted lines indicate 95% confidence levels. All variables except the unemployment rate are in natural logs.

Figure 1.6. Dynamic Response of Arrest Variables



Note: The year counties activated the policy is normalized to zero on the x-axes. They show up to 12 years before and 6 years after the activation year. The coefficients plotted in the solid lines are π's and τ's from regression equation (3) using the CBCP sample with standard errors clustered by county and border segment. The dotted lines indicate 95% confidence levels. The outcomes are the natural log of the number of arrests (all ages) in different crime categories. Violent crimes consist of murder, rape, robbery, and aggravated assault. Property crimes include burglary, larceny, motor vehicle theft, arson, and having stolen property. Other crimes consist of forgery/counterfeiting, fraud, embezzlement, vandalism, weapons violations, prostitution and commercialized vice, sex offenses, drug abuse violations, gamble, offenses against family and children, driving under influence, liquor law violations, drunkenness, disorder conduct, and vagrancy.

1.12. Tables

Table 1.1. Summary Statistics

	Mean	Std Dev	Obs
BLS/BEA Variables			
population	88,395	240,900	15,440
labor force	44,453	122,606	15,438
employed	41,660	114,280	15,438
unemployed	2,793	8,832	15,438
unemployment rate	6.38	2.69	15,438
total employment	52,101	172,615	15,440
wage salary employment	42,036	144,371	15,440
personal income	3,655,936	12,700,000	15,440
earnings by place of work	2,740,457	13,200,000	15,440
per capita personal income	32,140	10,941	15,440
per capita earnings	19,758	7,740	15,440
wages and salaries	1,955,259	9,747,424	15,440
average wages and salaries	32,335	8,404	15,440
average earnings	37,042	10,804	15,440
ln(population)	10.31	1.38	15,440
ln(labor force)	9.57	1.39	15,438
ln(employed)	9.51	1.39	15,438
ln(unemployed)	6.74	1.49	15,438
ln(total employment)	9.60	1.43	15,440
ln(wage salary employment)	9.27	1.53	15,440
ln(personal income)	13.73	1.48	15,440
ln(earnings per place of work)	13.17	1.58	15,440
ln(per capita personal income)	10.33	0.29	15,440
ln(per capita earnings)	9.83	0.34	15,440
ln(wages and salaries)	12.72	1.67	15,440
ln(average wages and salaries)	10.35	0.24	15,440
ln(average earnings)	10.48	0.28	15,440
Census/ACS Variables			
foreign born 2000	7,372	46,555	15,440
foreign born 2005-2009	8,647	48,927	15,440
Uniform Crime Report Statistics Variables			
total crimes	3,069	9,679	12,889
violent crimes	392	1,718	12,889
total arrests	3,784	10,050	13,641
arrests for violent crimes	161	595	13,641
arrests for property crimes	496	1,409	13,641
arrests for other crimes	1,636	4,561	13,641
arrests for minor crimes	729	2,690	13,641

Note: 965 unique counties for years 2000-2015 are used except for arrests variables that cover years 2000-2014. These counties do not include those whose distance with contiguous counties are more than 75 kilometers apart. The unit of personal income is in thousands of current dollars. Earnings or income variables are in current dollars.

Table 1.2. Factors to Secure Communities Activation

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
crime	0.9998	0.9999	0.9998	0.9998	0.9999	0.9998	0.9998	0.9999
	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)	(0.0006)
population	1.0200*	1.0202*	1.0192*	1.0220**	1.0203*	1.0200*	1.0192*	1.0203*
	(0.0116)	(0.0109)	(0.0109)	(0.0113)	(0.0109)	(0.0116)	(0.0109)	(0.0109)
labor force	1.0019	1.0017	1.0018	1.0038	0.9997	1.0002	1.0002	
	(0.0067)	(0.0067)	(0.0066)	(0.0056)	(0.0067)	(0.0068)	(0.0066)	
unemployment rate	1.0011	1.0013*	1.0010	1.0009				
	(0.0007)	(0.0007)	(0.0007)	(0.0007)				
number of unemployed					1.0013*	1.0011	1.0010	1.0013*
					(0.0007)	(0.0007)	(0.0007)	(0.0007)
total employment	1.0056	1.0054			1.0053	1.0056		1.0052
	(0.0088)	(0.0088)			(0.0088)	(0.0087)		(0.0072)
wage and salary employment			1.0067				1.0066	
			(0.0078)				(0.0078)	
personal income	0.9998					0.9997		
	(0.0033)					(0.0033)		
wages and salaries			0.9981				0.9981	
			(0.0054)				(0.0054)	
per capita earnings				1.0009				
				(0.0026)				
avg earnings per job		1.0026			1.0026			1.0026
		(0.0020)			(0.0020)			(0.0020)
Observations	1,719	1,719	1,719	1,719	1,719	1,719	1,719	1,719

Note: Hazard ratios are presented for each independent variable. Each column shows the separate regressions results for different combination of variables as factors to policy adoption. The regression employs 626 counties that "voluntarily" adopted Secure Communities, i.e. counties that adopted the program before October 1, 2010. Observations are used from the year 2008 when the policy is first implemented, and observations after the activation year for each county are dropped. All variables represent the growth rate from the previous year (t-1 and t-2). All regressions satisfy the proportionality assumption using the Schoenfeld residuals. *** p<0.01, ** p<0.05, * p<0.1

Table 1.3. Impact of Secure Communities on Local Labor Markets

	Population	Labor Force	Employed	Unemployed	Unemployment Rate	Total Employment	Wage & Salary Employment
treatment	0.0023	0.0007	0.0002	0.0159	0.0468	0.0067	0.0097*
	(0.0022)	(0.0045)	(0.0049)	(0.0135)	(0.1079)	(0.0043)	(0.0057)
Observations	27,990	27,978	27,978	27,978	27,978	27,990	27,990
R-squared	0.99	0.99	0.99	0.99	0.96	0.99	0.99

	Personal Income	Earnings by place of work	Per capita Personal Income	Per capita Earnings	Wages & Salaries	Average Wages & Salaries	Average Earnings per Job
treatment	-0.0069	-0.0098	-0.0092*	-0.0124*	0.0019	-0.0078**	-0.0164**
	(0.0047)	(0.0100)	(0.0050)	(0.0074)	(0.0078)	(0.0037)	(0.0077)
Observations	27,990	27,990	27,990	27,990	27,990	27,990	27,990
R-squared	0.99	0.99	0.97	0.97	0.99	0.98	0.93

Note: The table shows coefficient estimates for the treatment variable in equation (1). 959 counties are used in this regression. All outcome variables are in natural logs except the unemployment rate. Standard errors in parentheses are clustered by county and border segment. *** p<0.01, ** p<0.05, * p<0.1

Table 1.4. Impact of Secure Communities on Local Labor Markets by Industry

	Agriculture		Mining, quarrying, etc.		Construction		Manufacturing	
	earnings	employment	earnings	employment	earnings	employment	earnings	employment
treatment	-0.133	0.0054	0.1585	-0.0293	-0.031	0.0035	-0.035	-0.0107
	(0.182)	(0.011)	(0.110)	(0.043)	(0.025)	(0.016)	(0.023)	(0.018)
Observations	5,448	5,450	7,048	7,500	21,246	21,240	22,406	22,350
R-squared	0.88	0.99	0.96	0.98	0.99	0.99	0.98	0.99
	Trade		Transportation and utilities		Information		Financial activities	
	earnings	employment	earnings	employment	earnings	employment	earnings	employment
treatment	0.0105	0.0138**	0.0318	0.0031	0.0927**	0.0118	-0.0480**	-0.0047
	(0.012)	(0.006)	(0.025)	(0.016)	(0.042)	(0.016)	(0.023)	(0.007)
Observations	16,406	16,372	6,008	5,342	18,216	18,094	20,188	20,440
R-squared	0.99	0.99	0.99	0.99	0.98	0.99	0.98	0.99
	Business services		Education and Health		Leisure and Hospitality		Other services	
	earnings	employment	earnings	employment	earnings	employment	earnings	employment
treatment	-0.009	-0.0105	-0.0198*	-0.01	-0.01	0.0112	-0.011	-0.0031
	(0.021)	(0.012)	(0.010)	(0.007)	(0.016)	(0.008)	(0.009)	(0.005)
Observations	6,936	7,358	9,014	9,082	15,600	16,002	22,448	22,448
R-squared	0.99	0.99	0.99	0.99	0.99	0.99	0.99	0.99

Note: The table shows the effect of Secure Communities in equation (1) by industry. The number of jobs refers to full-time and part-time number of jobs. Trade refers to wholesale and retail trade. Other services except public administration include repair and maintenance, personal and laundry services, religious, grantmaking, civic, professional, and similar organization, and private households work. Standard errors in parentheses are clustered by county and border segment. *** p<0.01, ** p<0.05, * p<0.1

Table 1.5. Impact of Secure Communities on Arrests

	Total Crimes	Violent Crimes	Property Crimes	Other Crimes	Minor Crimes
treatment	0.405*	0.183	0.286	0.359*	0.335*
	(0.238)	(0.126)	(0.174)	(0.202)	(0.176)
Observations	23,198	23,198	23,198	23,198	23,198
R-squared	0.93	0.96	0.96	0.94	0.94

Note: The table shows the effect of Secure Communities in equation (1). The outcomes are the natural log of the number of arrests (all ages) in different crime categories. Violent crimes consist of murder, rape, robbery, and aggravated assault. Property crimes include burglary, larceny, motor vehicle theft, arson, and having stolen property. Other crimes consist of forgery/counterfeiting, fraud, embezzlement, vandalism, weapons violations, prostitution and commercialized vice, sex offenses, drug abuse violations, gamble, offenses against family and children, driving under influence, liquor law violations, drunkenness, disorder conduct, and vagrancy. Minor crimes refer to drug abuse violations, liquor law violations, vandalism, and prostitution and commercialized vice. Standard errors in parentheses are clustered by county and border segment. *** p<0.01, ** p<0.05, * p<0.1

Chapter 2

Effects of Maternity Leave Expansions on Firms by Size: Employee Structure, Job Flows and Survival*

2.1. Introduction

Promoting balance of work and family care for mothers who juggle with conflicting roles have long been an issue that has been addressed by policymakers. Expansions of maternity leave either in the form of higher maternal contributions after birth or prolonged job protection periods have been commonly adopted worldwide to help mothers care for their families while encouraging their participation in the labor market in their childbearing years and onwards. In OECD countries, as of year 2016, mothers were entitled to take 18 weeks of paid maternity leave on average around childbirth, with the U.S. being the only country that have not stipulated paid maternity leave on a nationwide level.[23] Most OECD countries replace over 50% of the previous earnings, while some cover 100% of the previous earnings during the leave.

* This study uses the weakly anonymous Establishment History Panel 1975-2016, DOI: 10.5164/IAB.BHP7516.de.en.v1. Data access was provided via on-site use at the Research Data Centre (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB) or remote data access.

[23] See OECD Family database: http://www.oecd.org/els/family/database.htm.

There exists a large body of literature that examines the effects of maternity leave policies with outcomes ranging from maternal and children's health to women's labor market outcomes of wage and employment. In terms of the economic impacts of maternity leave, previous research has particularly focused on mother's return to work and their long-term participation in the labor market using individual-level data. Relative to these topics that are centered on the labor supply, there has been only meager research looking at the impacts of reforms particularly from a firm-wide perspective, or the labor demand side.

Despite the importance of maternity leave reform effects on the firms from a policy standpoint, relatively less attention has been placed on the topic and more so on the differential firm effects that may exist. The current paper attempts to fill this gap in the literature by using establishment-level data in Germany to evaluate firm heterogeneity of responses to maternity leave expansions that took place in 1979. The reform extended the existing job protection period of 8 weeks to 6 months. All firms were affected by this reform, thus the firms are divided into different quartiles by size in order to explore differential effects by "treatment intensity." The main outcomes of interest are changes in firms' employee structure, job flows, and survival rate. To my knowledge, there is only one paper that explores the effect of maternity leave expansions specifically on firms and coworkers using the Danish data (Gallen 2016). In her paper, however, the author focuses primarily on the impacts on coworkers more than on firms, while the current paper examines a variety of firm-level outcomes by firm size.

The current study finds that small firms tend to bear higher burden of a maternity leave expansion. This can be seen by their adjustment behavior of hiring relatively more part-time employees and displaying higher turnover rates of female employees after the reform. Moreover,

their exit probability are found to be higher relative to large firms. Relative female-male wage are not affected by the reform, and heterogeneous effects on firms by location are shown to be present.

Competitive labor market theory suggests that from the labor supply side, female workers who value maternal benefits are willing to accept a lower wage for a given quantity of labor supplied. This value will depend on the degree of benefits provided in terms of the length of duration and maternity pay. The labor supply shift alone will put downward pressure on wages and lead to higher employment levels in the presence of a maternity leave expansion. Female workers will also choose their optimal period of maternity leave considering their reservation wage and the value of separating from the firm. Since some mothers take shorter leaves while others take longer ones, theory does not suggest a definitive answer on an optimal level of the leave (Klerman and Leibowitz 1999, Baker and Miligan 2008).

On the labor demand side, maternity leave expansions are costly to firms. In some countries, firms are responsible for all or part of maternity leave contributions including employee's health insurance coverage and income while female workers are on leave. These costs act as a tax to employing women. Even if firms do not incur direct monetary costs and the maternity contribution is financed primarily by the government, prolonged periods of absence increase nonwage costs to firms. These indirect costs involve loss of production if firms do not hire replacements for workers on leave. If they do decide to hire replacements, there are search and training costs associated with hiring temporary substitutes who have less firm-specific human capital than their previous employees. Higher costs may also be incurred in retraining the returning mothers since longer leaves can lead to higher human capital depreciation by the time they return to work. These expenses together can make hiring female employees especially in their prime childbearing years more costly to firms relative to hiring men. Thus, they lead employers to shift the labor demand

curve for female workers to the left to the degree that these wage and/or nonwage costs increase by the reforms, ultimately putting downward pressure their employment and wages.

The responses of employers and female employees in a competitive market show that the wages for female workers are to decrease, but the effect on employment is ambiguous. In what Zveglich and Rodgers (2003) note as the first-order effects, the employment effects depend on the value that employers and employees independently place on the maternity benefits. If the female workers assign higher value to the benefits by more than the costs incurred to employers, female employment increases. In the opposite case, employment would decrease.

A rightward shift in the labor demand in response to an expansionary reform is also possible as maternity leave expansions allow employers to retain female employees with firm-specific human capital whose labor productivity increases with continued labor market attachments after the leave. This effect would also be strong in firms where permanent separations are costly. Ruhm (1998) labels this as dynamic effects, and explains that this is highly likely in the presence of a market failure. The author writes, "A company voluntarily offering leave is likely to attract a disproportionate number of "high-risk" employees and be forced to pay lower wages. Persons with small probabilities of using the benefit will avoid these firms and so do without even socially optimal leave." In the presence of such asymmetric information, increases in both employment and wages of female employees can occur as a result of a government-led reform that removes such matching behavior.

While theory does not provide definitive predictions on labor market outcomes, evidence in recent literature point to small changes in or negative female employment with again small or negative wage changes after childbirth, and may suggest that an increase in labor demand is less likely. However, the majority of the studies uses individual employee level data and does not

consider firm heterogeneity when differential effects on labor demand may exist depending on characteristics of the firms, for instance, by its size, relative importance of firm-specific capital, or location. In fact, when devising policies, policymakers often assume that there may exist differential firm responses to the reforms, where some firms incur more costs than others. This has motivated some countries to grant exceptions to the compulsory pre- and post-natal leave to small- or medium-sized firms. In the U.S., for example, the Family and Medical Leave Act (FMLA) enacted in 1993 required employers with more than 50 employees to provide 12 weeks of unpaid leave to employees known to affect only about 60% of the private sector employees (Berger and Waldfogel 2004). The small firms arguably have less resources and flexibility in replacing female employees on leave, thereby are expected to incur higher costs of hiring female workers relative to medium- or big-size firms. There have not been many studies to support this, however.

Another possible source of firm differential effects depends on the skill-levels of female employees. Employers whose employees are mostly low-skilled with low levels of firm-specific capital may not incur high employee turnover costs in the first place, thus retaining female employees after a leave may not produce much benefits to the firms. In contrast, firms with high proportions of skilled workers may increase their labor demand for skilled female employees as the reforms are expected to bring more mothers back to work after the leave and that allow them to increase their firm-specific capital (Ruhm 1998, Berger and Waldfogel 2004, Baum 2003). Although examining the establishments split by average skill levels of its employees may be interesting, the current paper will not cover this topic in detail and will focus on the differential firm effects of the reforms based on its size. It does attempt to provide some insights on how employment and job flows are affected in firms with high levels of high-skilled workers.

The article is organized as follows. In Section 2.2, previous literature and background of the changes in German maternity leave legislation is explained. Section 2.3 describes the data and provides basic summary statistics. The identification strategy is outlined in Section 2.4, and the results are presented in Section 2.5. Section 2.6 concludes.

2.2. Background

2.2.1. Related Literature

In previous research, major outcomes of interest to researchers of maternity leave reform effects have centered on mothers' labor supply and wages postbirth. There appears to be evidence that maternity leave legislations encourage mothers to return to prechildbirth work, but results on employment and wages of mothers who return are mixed. Using the National Longitudinal Survey of Youth (NLSY), Baum (2003) identifies mothers who gave birth between 1988 and 1994 and use their employment histories. He finds that the state legislations in the U.S. expanding maternity leave and FMLA enacted in 1993 raise the probability of return to previous employers for mothers who would not have returned to the labor market at all, although some of the mothers do choose to delay their return after the expansion. Baker and Milligan (2008) examines the effects of modest entitlements (17-18 weeks) and extended entitlements (29-70 weeks) separately by using variations of lengths of job protected leaves across provinces in Canada. They conclude that mothers stay longer at home after birth with longer entitlements compared to the shorter leaves, and maternity leave increases employer continuity regardless of the leave periods. Ondrich *et al.* (1996) explores the effects of the 1986 maternity leave reform in Germany, and find that the return probabilities of mothers to the labor market is small, and that the level of maternity benefits do not affect the length of delaying return. Schönberg and Ludsteck (2014) analyzes five reforms in

Germany that expanded either the job protection period or increased maternity leave pay and/or period. The effects on labor market outcomes on mothers vary by each reform, but overall, the authors find small impacts on mothers' employment both in the short run and the long run (3-6 years after childbirth). They also find some evidence that the reforms decreased the wages of mothers who returned to work, and suggest that mothers' firm-specific capital did not increase significantly to result in higher employment and wages.

Examining labor market outcomes of female workers in general, more evidence appear to point toward higher female employment, but suggest ambiguous results on wages. Zvegligh and Rodgers (2003) uses repeated cross-sections of household survey data in Taiwan to study the effects of a reform in Labor Standards Law in 1984 which required 8 weeks of maternity leave and full pay during the leave if they have been working for more than 6 months. The authors find that after the law enforcement of job-protected leave, the maternity benefits increase the hours worked and female employment, but have no significant impact on their wages. They note that the lack of wage decreases of female employees point to the presence of increasing productivity by stronger labor market attachments through job protection and increasing firm-specific capital that they are able to retain (with labor demand shifting to the right). Similarly, Winegarden and Bracy (1995) concludes that longer duration of paid leave raise labor force participation rates of females in the prime childbearing years using a sample of 17 OECD countries. Using macro-level data, Ruhm (1998) also finds evidence that the leave mandates increase the proportions of women employed relative to the population, but discovers that relative wages of females decrease the longer the leave.

In theory, the combination of behavior of female workers and firms produce uncertain predictions regarding female employment and wage adjustments. Therefore, it remains to be tested

empirically the effects of maternity leave expansions on those outcomes. While most previous research relies on individual level data or country comparisons that are limited to a small samples, relatively few attempts have been made by researchers to analyze the female labor market outcomes particularly at the firm level. This paper will tackle similar questions that have been addressed before, but with firm-level data, it will shed light on how firms behave by shifting their employee composition and wage levels. Besides adding empirical evidence to the existing empirical debate on female labor outcomes, it will provide new insights on firm heterogeneity that have not been examined before.

2.2.2. Maternity Leave Reforms in Germany

In line with the early movement sparked in Europe in the mid-19th century towards protecting working mothers after childbirth, Germany holds a long history of having maternity leave policies. In its earliest form, in 1878, Germany banned women from working within 3 weeks of childbirth unpaid, and the German Imperial Industrial Code of 1891 banned working within 4 weeks of childbirth (Wikander *et al.* 1995, Ruhm 1998). Closer to a more modern form of maternity leave, Germany have provided protection to mothers through the Maternity Protection Act (*Mutterschutzgesetz*) introduced in 1968. It granted female employees 14 weeks of mandatory maternity leave (*Mutterschutzgesetz*) for which they were paid their full salary from the social security and employers combined (Merz 2004). The Act has underwent several reforms and additions that expanded protection for mothers by preventing employment dismissal within a certain period after childbirth (job protected maternity leave) and/or providing maternity pay during parts of the leave. As of 2016, Germany still mandates a total of 14 weeks of paid maternity leave, granting workers to take the leave 6 weeks before their estimated due date and 8 weeks after

delivery, or the *Mutterschutz* period. This stands slightly below the OECD average in the length of paid maternity leave of 18 weeks. However, 100% of the previous earnings are paid for as maternity benefits during the paid maternity leave duration for those who earn national average earnings, and 162 weeks of employment protected parental leave is guaranteed, which puts Germany as one of the most generous countries among industrialized countries.[24]

The first reform of the Maternity Protection Act was passed in May 1979, which is the reform of interest in this paper. The full salary pay provided 6 weeks before and 8 weeks after childbirth remained the same as before, but the job protection period expanded from 8 weeks after childbirth to 6 months. During the 4 months of the extended job protection period, mothers were paid DM 750 per month by the federal government and health insurance. Other important features include prohibition of work for new mothers for a duration of 8 weeks after childbirth, and special protection from dismissal during pregnancy and 4 months after childbirth (Ondrich *et al.* 1996). These benefits were limited to female employees who have been employed prior to childbirth, and excluded self-employed mothers.

Subsequent set of reforms occurred in January 1986 where job protection period increased from 6 months to 10 months with a federal child-rearing benefit law (*Bundeserziehungsgeldgesetz*). Due to this reform, women who had not been working prior to childbirth were also eligible for the benefits. After the maternity pay of full salary (for previously employed) or DM 600 (for previously unemployed) for 6 weeks before and 2 months after childbirth, all women receive DM 600 from month 3 to 6, and pay for month 7 to 10 depends on annual net family income in the past 2 years. This reform also allowed fathers to share the parental leave granted to the mothers for the first time. In January 1988, July 1989 and July 1990, job protection and maternity benefit periods

[24] See Olivetti and Petrongolo (2017) for a recent comparisons of family policies among high-income countries.

were further extended from 10 months to 12 months, 15 months, ad 18 months, respectively. The next set of changes occurred in January 1992 when job protection period increased from 18 to 36 months, although maternity benefits were provided for a total of 18 months. The maternity pay period, however, extends to 24 months from January 1993 (Ejrnaes and Kunze 2013, Schönberg and Ludsteck 2014).[25] These changes continue until the end of the year 2000.

The current study will examine the effects of the first reform in 1979 since it provides a long range of pre-reform and post-reform years to capture possible trends of firm-level variables by firm size in the difference-in-difference methodology.

2.3. Data

2.3.1. Data Source

The dataset used in this study comes from the Establishment History Panel (BHP) provided by the Research Data Center (FDZ) of the German Federal Employment Agency (BA) at the Institute for Employment Research (IAB). BHP is an annual, aggregated data of employment notifications from the Employee History (BeH) of the IAB. It is a 50% random sample of all establishments in Germany which had at least one employee subject to social security as of the reference date of each year, June 30. Thus, establishments with employees not subject to social security, such as self-employed, students, and civil servants are not counted. It is estimated that 77.2% of all workers in Germany was covered by the social security as of 2001 (Bundesagentur für Arbeit 2004). Establishments refer to "a regionally and economically delimited unit in which employees work," and "may consist of one of more branch offices or workplaces belonging to one

[25] Ejrnaes and Kunze (2013) provides a nice summary of the reforms in Table 2.1 in their paper, and Schönberg and Ludsteck (2014) presents an organized overview in Figure 1.

company (Schmucker *et al.* 2018).” The establishment definition in the BHP is not necessarily equal to a firm or a plant. Nonetheless, this paper uses the terms establishments and firms interchangeably, although technically, the latter is a more comprehensive term that can consist of multiple establishments.

The most recent version of the BHP data for West Germany is available from 1975 to 2016 and 1992 to 2016 for East Germany. The number of observations ranges from approximately 640,000 to 1.5 million establishments for years 1975-2016. Each annual wave is cross-sectional data, but can be linked with an artificial establishment ID number provided by the FDZ to create a panel which is used for the current study. Along with the “core dataset” of the BHP which includes information of establishments on its characteristics such as its location and type of industry, employee structure by age, skills, and occupation, and wage structure as of the reference date each year, “extension files” with information on annual worker flows (inflows and outflows) and exit and entry are also utilized in the paper. While the core dataset records the stock of employees as of June 30 of each year, the outflows and inflows data of a particular year (t) record worker flows that occurred between June 30 of the previous year (t-1) and the respective year (t).

The advantage of the BHP primarily lies on its accuracy as an administrative data where measurement error is considered to be lower than other types of data such as survey-based data. Employers are required by law to annually report employment notifications of their employees who are covered by social security to the respective social security agencies. These notifications include employees’ demographic information, education background, occupation, earnings, and more. Such information aggregated at the establishment level of which 50% are disclosed for researchers produces a large sample size which further increases precision in analysis. It is

estimated that 80 percent of total employment in Germany is covered by BHP (Herberger and Becker 1983).

The dataset has its limitations, however. First, the data do not allow me to identify proportions of mothers on maternity leave or of mothers who had birth in an establishment, thus examining the reform effects specifically on the behavior of mothers who take the leave is not possible. Only the effects on female workers in general are analyzed. In addition, employees on maternity leave are considered as interruptions of employment, and are not counted as employees in the BHP during the leave. Thus, she would be counted as an outflow with respect to the year she took the leave, and an inflow once she returns. Since the dataset is annual data that measures the stock of employees and their characteristics as of the reference date of each year, mothers who took a leave between June 30 of one year and that of the following year are not captured as either inflows or outflows, and would be recorded as an employee for both years. Secondly, detailed employee characteristics are not divided by gender. For instance, BHP contains information on the number of employees by age group, skills, and occupation, but unfortunately, these variables are not divided by gender.[26] The main outcomes of interest can largely be summarized as follows: (1) stock and flows of female workers: full time and part time, (2) stock and flow of workers by age group, (3) median female and male wage and their wage inequality, and (4) exit probability of firms.

Previous papers have used a variety of datasets when examining maternity leave policies in Germany. The German Socio-Economic Panel Study was used by Ondrich *et al* (1996) to study post childbirth probability of return to work by identifying mothers and their characteristics.

[26] Reform effects on outcomes of share of employees by age were nevertheless examined. Although the data combine male and female employees for each age group, the regression results in Tables S.1 and S.2 in Section 2.10. show job flow patterns of "young" employees that include mothers in their prime childbearing years of 25-34.

Schönberg and Ludsteck (2014) and Dustmann and Schönberg (2012) use social security records from IAB Employment Samples (IABS) to construct work history of mothers to explore reform effects on labor market outcomes and childrens' outcomes. Guertzgen and Hank (2018) use German register data called BASiD to examine reform effects on mother's health. Jäger (2016) use matched employer-employee data from Integrated Employment Biographies of IAB to study effects of workers exits on coworkers within-firm levels, although the worker exits used here are worker deaths instead of exits by maternity leaves. Each dataset has its merits and drawbacks, but the BHP provides a rich set of firm-level variables that other datasets do not offer, and it is ideal for studying heterogeneity of reform effects across firms. The years covered for 1975-2016 also provide advantages in studying the long history of maternity leave reforms in Germany.

2.3.2. Summary Statistics

The focus of the German reform in maternity leave in the current paper is the first reform that took place in 1979, thus data for the years 1975 to 1978 are used for pre-reform observations while from 1979 to 1985 are used for post-reform observations, before the year of the second reform in 1986. Next, a subsample of establishments that first appeared in one of the pre-reform years, and have survived until at least the reform year 1979 is used. This offers a set of establishments that have existed before the reform and have been exposed to the reform in 1979. Firms that first appeared and last appeared on the same year regardless of when it occurred are thus excluded.

Since the reforms affected all firms in Germany, the firms are divided by intensity of exposure to the maternity leave expansion, or treatment intensity. The establishments were initially divided by four quantiles based on their size. However, since more than 50% of the data sample consisted of establishments with only one employee, there was not enough variation to divide the

establishments.[27] Therefore, each establishments were lined by the number of total workers, and the quartiles were divided accordingly with roughly by 25% cutoff for each quantile. This produced the first quantile consisting of establishments with number of employees between 1 and 24, second quantile between 25 and 144, third quantile between 145 and 766, and fourth quantile between 767 and 35,966 for the years prior to the reform (1975-1978), as shown in Table 2.1. The cutoffs produced are roughly similar to the ones used in Ondrich *et al.* (1996), where firms with less than 20 employees are denoted as small, and firms with employees between 20 and 200 are considered intermediate. Firms with more than 200 employees are considered big-sized firms. In the current setup, the minimum employees in firms in the last quartile is 766 so this can be considered as very large firms. They consist of only 0.22% of the data sample, but since the data set is large, there are on average 1,176 establishments used for each year across the sample years.

The summary statistics for all variables used in the study are reported in Table 2.2**.** These are based on pre-reform periods of 1975-1978. As mentioned earlier, the number of inflows recorded in the BHP denotes the inflows that occurred between June 30 of the previous year and June 30 of the current year. For the year 1975 when the BHP begins to be recorded, the number of inflows equals the current stock of employees. Therefore, when using the inflows variables, observations for the year 1975 are excluded in the analysis. For establishments that have exited the labor market, the number of outflows appear one year after closure and equals the stock of total employees of the year of exit.

[27] It was not possible to divide the sample into four quartiles when using the simple number of employees since 50% of the establishments consisted of only 1 employee. Even when excluding establishments with one employee, the first three quartiles ranged establishments with 2 to 16 employees, and the last quartile from 17 employees and beyond. The comparisons between small and big-size firms were therefore difficult.

2.4. Identification Strategy

2.4.1. Specification

I use a difference-in-difference design to examine differential effects of the 1979 maternity leave expansion in Germany on establishments divided by its size. Four quartiles are split by the number of total workers, which in effect produce the first quartile consisting of establishments with employees ranging from 1 to 24 employees. This represents the small-size firms in Germany. The last quartile consists of establishments with more than 767 employees, which represents big-size firms in the current study. The quartiles are to capture different degrees of the treatment intensity or reform effects. That is, small firms are expected to respond more strongly to the reform due to the higher costs associated with replacing workers on leave due to the limited resources or substitutability of female workers relative to big firms.

The following equations denotes the regressions used to examine the maternity leave expansion effects on establishments in the study:

$$Y_{jt} = \alpha POST_t + \sum_{k=2}^{4} \beta_k Q_j^k \times POST_t + \nu_{jt} \quad (1)$$

$$Y_{jt} = \delta POST_t + \sum_{k=2}^{4} \rho_k Q_j^k \times POST_t + \sum_{k=2}^{4} \gamma_k Q_j^k \times t + \mu_j + \varepsilon_{jt} \quad (2)$$

Y_{jt} denotes the establishment-level outcome variables for establishment *j* and year *t*. Q_j^k is an indicator variable denoting the *k*-th quartile of a treatment intensity variable for establishment *j*, $POST_t$ is an indicator variable equal to 0 for pre-reform years 1975-1978, and 1 for post-reform years 1979-1985. μ_j stands for establishment fixed effects. The standard errors, ν_{jt} and ε_{jt}, are clustered at the establishment level. The difference between equation (1) and equation (2) is the

inclusion of treatment-specific linear time trends in equation (2). This controls for the possible differential trends that may exist across quantiles. The coefficients of interest are $\sum_{k=1}^{4}\beta_k$ and $\sum_{k=1}^{4}\rho_k$ which denote the difference-in-difference estimates of each treatment intensity quartile.

2.4.2. Identification

The identifying assumption in the difference-in-difference framework is the parallel trends assumption that requires treated and control groups to follow similar trends in outcomes of interest prior to the reform. In the current setting, the trends of small firms that are likely to be most intensely treated (Q1) and those of big firms that are likely to be less intensely treated (Q4) are compared. This is examined by plotting the data across different quartiles, and the graphs displayed shows that this is less of a concern. Figure 2.1 plots three outcome variables by each quartile, with Q1 representing small-size establishments and Q4 representing big-size establishments. Panel A displays the changes in the shares for total female employees that include both full-time and part-time workers to total number of employees. The patterns for both Q1 and Q4 increase only slightly, if not almost stagnant throughout the years 1975-1985. This appears to be in line with previous studies of maternity leave reforms that did not affect mothers' return to the labor market much, although the current study combines all female workers (Schönberg and Ludsteck 2014). Panels B and C show the trends for full-time and part-time female employees separately. As the panels show, one can see that the share of full-time female employees slightly decreases after the 1979 reform. The share of part-time female employees, however, shows an increasing trend prior to the reform for both small and big firms, but after the reform, increases more for small firms.[28] This suggests that the firms, particularly small firms, may be adjusting to the maternity leave expansions

[28] There were no pronounced differential patterns across quantiles observed for total male employees, full-time male employees, and part-time male employees compared to female employees.

by substituting away from full-time female workers to part-time female workers. Houseman (2001) presents results from establishment surveys showing that the most common reason for using flexible staffing arrangements is to adjust for workload fluctuations and employee absences of regular workers. [29] The expansionary maternity leave reform may have increased such needs. Moreover, although firms are still required to pay maternity benefits to part-time employees, flexible work arrangements can be made relative to full-time employees between firms and workers, and firms may save costs on other fringe benefits (Montgomery 1988). From the labor supply side, female workers may be more willing to engage in part-time work after the reform has extended its maternity benefits. These forces together act to increase part-time employment and decrease full-time employment of female workers.

Figure 2.2 plots the raw data for inflows of female employees. Prior to the reform, Panel A displays no substantial differential trends across the quartiles on the share of inflows of total female employees. After 1979, all quartiles except small firms belonging to Q1 follow a similar trend. For Q1, the share of inflows is substantially reduced after the reform. The inflows of full-time female employees follow this trend as shown in Panel B. In Panel C that plots values for inflows of part-time female workers, the pre-treatment trends are also similar for Q1 and Q4, but it is difficult to conclude how their trends differ after 1979. Similarly, Figure 2.3 displays the share of outflows for female workers. All quantiles follow a similar trend in the share of total inflows prior to the reform, and after the reform, increase more for small firms. Such trends are followed by both full-time and part-time female workers.

In addition to examining pre-treatment trend with data, one can also control for possible differential trends in outcomes across quartiles. The dataset allows four years of pre-treatment

[29] In her paper, flexible staffing arrangements refer to temporary, on-call, part-time, and contract employment.

periods and seven years of post-treatment periods, so that treatment-specific time trends are included in the empirical specification for robustness checks. As shown in the next section, the difference-in-difference estimates were shown to be stable in the presence of the treatment-specific trends for most variables.

Another identification assumption requires the timing of the maternity leave reform to be exogenous. Dustman and Schönberg (2012) and Schönberg and Ludsteck (2014) examine the 1979 maternity leave reforms in Germany to examine their effects on mothers' employment and wages, among reform effects in other years. They confirm the unanticipated effect of the May 1979 reform by searching newspapers to examine possible discussions of the reforms in the public domain and finding that articles appeared no more than two months prior to the reform was implemented. Guertzgen and Hank (2018) also shows that for the 1979 reform, the German government proposed the bill in January 1979. Considering the current study uses annual observations and the first post-reform year is set to 1979, it is not likely that the reform would have affected behaviors of female workers and employers in 1978 or before. This reinforces the causal interpretation of the reform effects under study.

2.5. Results

2.5.1. Differential Reform Effects on Hiring Female Employees

The first set of outcomes to be examined is the percentage share of female employees relative to the total number of employees in an establishment. Column (1) of Panel A in Table 2.3 shows the results of the 1979 reform effects using equation (1). The coefficient of POST is equal to 0.0128, which is significant at 1% confidence level. This shows that the share of female employees for establishments in the bottom quartile, i.e. small establishments, increases by 1.28

percentage points after the reform. As the establishment size grows, the positive increase in the share of female employees is mitigated monotonically. For establishments categorized in the top quartile, i.e. big firms, share of female employees decrease by 1.73 percentage points compared to the bottom quartile. Column (2) adds to equation (1) a control variable of the stock of highly qualified workers in an establishment, which can affect the outcome. An important feature of the specification is that the inclusion of the control variable does not affect the main coefficients of interest. Columns (3) and (4) display the results for equation (2) with treatment-specific linear time trend with the latter column including an additional control variable. The last column would be the preferred specification for analyses. The results for the share of female employee share in column (4) are not much different from those in the previous specifications, which show the estimates are robust to the differential trends across quartiles and controls.

It is worth mentioning that the inclusion of this variable is motivated by the fact that establishments with high number of skilled workers may find it harder to replace these mothers on leave due to the large investments in firm-specific capital relative to those of low-skilled employees. Given that firms with high level of skilled workers in general also have high levels of skilled workers who are female, these firms may increase demand for female workers to retain firm-specific capital following the reform. Ruhm (1998) notes that this is likely to occur in a market failure setting due to asymmetric information of employees who will take the leave.[30] The coefficient of the number of skilled workers is small but negative, which does not support the firm-specific human capital story.

[30] To directly quote from Ruhm (1998), "Employers and workers can always voluntarily negotiate maternity leave, mitigating the joblessness and retaining the specific investments. Moreover, with competitive labor markets, the groups most likely to use parental leave will pay for it by receiving lower wages […] Entitlements that allow substantial time off work may cause employers to limit women to jobs where absences are least costly […]"

It appears at first glance that the increase in the share of female employees after the reform for small firms, and the decrease for big-size firms may appear counter-intuitive. Small firms are expected to respond more strongly to the reform by decreasing female employment more than big firms since it may be more costly for them to replace mothers on leave. Investigation in how the share of full-time and part-time female employees change may provide further insight behind the phenomenon as shown in Panels B and C in Table 2.3. Panel B shows that following the reform, the share of full-time female employees decreased by 0.35 percentage points for small size firms as shown in column (1). This decrease is reinforced when the firm size grows, but the effect is not monotonic. Big-size firms further reduces the full-time female workers by 0.79 percentage points, but this effect is gone when we add highly qualified workers to the specification in column (2). The effects are similar when treatment-specific linear time trends are added in columns (3) and (4).

There exist more pronounced effects with respect to part-time female employees in Panel C of Table 2.3. The number of part-time female workers in small firms increase by 1.87 percentage points on average after the reform in column (1). The increase is reduced monotonically as firm size increases. Compared to small firms that are expected to be more intensely affected, there is only a total of 0.5 percentage point increase in part-time female employees in big-size firms. The estimates are stable with treatment-specific linear time trends in column (3). The effect for the top quantile is only slightly reduced when the control variables are added in columns (2) and (4), with and without treatment-specific trends, respectively.[31]

These results are in line with those in Figure 2.1. They suggest that the bulk of the rise in the share of total employees for small firms after the reform is coming from the increasing share of part-time employees. That is, the small firms tend to adjust to the extensions of mandatory job

[31] Examining the total part-time employees as a share of total employees prior to the reform (1975-1978), about an average of 7.6% of employees in the firms in Q1 was part-time, 8.4% in Q2, 8.6% in Q3, and 6.8% in Q4.

protection periods by hiring more part-time female employees and reducing employment of full-time female workers. Small firms may be reluctant to hire full-time female workers considering the higher costs associated with employee absence but less so for part-time workers as noted earlier.

In the joint dynamic model of labor supply and fertility for women, Francesco (2002) models and simulates different labor market participation rates and fertility paths for full-time and part-time female workers. Maternity leave expansions can also differentially affect the labor supply decisions of mothers depending on their working hours. More women may be willing to supply part-time employment after a more generous form of maternity benefits is mandated. This may reinforce higher levels of employment of part-time workers in addition to the increase in their labor demand.

2.5.2. Differential Reform Effects on Job Flows

The BHP data allows one to examine the inflows and outflows of employees by gender, occupation and age. The outcomes to be examined in this section are the share of inflows and outflows of female employees divided by their working hours (full-time, part-time, and together). In addition, the inflows of the share of employees who are rehired are also analyzed.

Table 2.4 shows the reform effects on the share of inflows of female employees. In small size firms, there is a 3.75 percentage points decrease in female employee share that enter these firms post-reform as shown in column (1) of Panel A. The decrease is largest for big-size firms and the estimate appear to be sizeable. When controlling for the number of highly qualified employees, the monotonic effects by firm size disappears, however, and the effects decrease in magnitude. Including treatment-specific time trends in columns (3) and (4) does not change the estimates on POST much, but with the additional control, the share of inflows of female employees in larger firms (Q2 and Q4) turns positive and is statistically significant. Quantitatively, column

(4) shows that there are 4.77 less inflows of female workers in percentage points for Q1, but for Q4, increase by a 10.37 percentage points more than Q1. Column (4) in Panel B that examines the share of inflows of full-time female employees shows a similar trend. The outcome decreases by 3.52 percentage points following the reform for small firms, while it relatively increases by 7.72 percentage points for big firms. With respect to the share of inflows of part-time female workers, shown in Panel C, there is again a decrease by 0.48 percentage points in column (4), but the effect on larger firms are not statistically different.

Next, the share of outflows of female employees are examined from Table 2.5. Column (4) of Panel A reports the outcome increase by 3.11 percentage points for small firms after the 1979 expansionary reform. There is a monotonic discount of such positive effect as the firm size becomes larger with that of big firms decreasing by 3.22 percentage points compared to Q1. Similar trends are reported for outflows for both full-time and part-time female workers as shown in the preferred specification, column (4), in Panel B and Panel C, respectively. All the estimates for outcomes related to outflows are fairly stable across specifications.

In summary, with the addition of treatment-specific linear time trends and a control variable, the estimates of the differential effects of the reforms on *inflows* of female workers appear to be less robust than other outcomes. However, the common inference from the preferred specification is that the inflows of female employees, whether full-time or part time, decrease for small firms, but increase as the firm size grows. There is a higher share of outflows of female workers for small firms as well, while for big firms, there appears to be less changes after the maternity leave expansion.

An additional outcome of interest is the share of inflows of female employees who are rehired. By definition, an employee is regarded as a rehire or re-employed in the current year (t) if

he/she was working on at least one of the reference dates (June 30) in the preceding three years (t-2 and/or t-3), but not working one year earlier (t-1). The results are shown in Table 2.6. Examining column (1) without treatment-specific linear time trends, small firms in the bottom quantile tend to increase the share of rehires by 0.27 percentage points. The rehires in big firms in the top quantile, however, decrease by 3.69 percentage points more than Q1. Controlling for the number of highly qualified workers produce similar estimates in column (2). When including treatment-specific linear time trends, however, the overall magnitude is reduced as shown in column (3), but the pattern of small firms rehiring a larger share of female employees relative to larger firms remains the same. With an additional control, however, the significance is gone for the third and fourth quartiles in column (4).

The number of rehires of female workers may capture mothers who have returned to work after a maternity leave and also those who are not affected by the leave who have returned to their previous employers. Due to the limitations of the data, it is not possible to identify what percentage of female workers that are rehired are mothers that returned from a leave. However, considering that the definition of rehires in the BHP data requires returning employees to have had at least one full year of absence from work (and a maximum of almost three years), there are expected to be female workers who are not mothers who return to work. The 1979 reform also provided paid leave for 6 weeks before and 8 weeks after delivery while guaranteeing the job protection for 4 months after that. Thus, considering the maternal benefits are provided up to 6 months and that most mothers exhaust their protected leave duration, the female worker rehires after 1 year will consist of female employees who did not take a leave. Therefore, the increase in rehires after the reform more for the small firms suggests the that firms choose to rehire more female workers who have previously worked to save costs on finding ad training new replacements for mothers.

2.5.3. Exit probability

This section explores the effect of maternity leave expansions on exit probability of establishments based on their size. In previous papers, Gallen (2016) addresses essentially the same question by using a 2002 Danish reform that extended the duration of parental leave by 22 weeks. The author finds evidence that firms are more likely to shutdown by 2 percentage points overall five years after the reform. Dividing by size, firms with 16-30 employees had a positive shutdown probability that was statistically significant as opposed to no significant effects found for either smaller firms with less than 15 employees or larger firms with more than 30 employees. She uses firms with more than five employees due to data limitations. The data used in the current study do not suffer such restriction, and include all firms.

In case of firm closures in the BHP data, Hethey and Schmieder (2010) classifies the reasons behind an exit with a methodology utilizing clusters of worker inflows and outflows data. The exit types are categorized into one of the following: (1) establishment ID change, (2) take-over/restructuring, (3) spin-off/pushed, (4) small death, (5) atomized death, (6) chunky death, (7) reasons unclear.[32] I first combine categories (4), (5), (6) to identify these establishment exits as true exits, which I denote as "shutdown exits," then combine all categories to refer to exits in general, as "general exits." In equation (1) and (2), the outcome variable is equal to 1 for establishment *j* in year *t* if the establishment exits in that year. The results with shutdown exits as outcome are shown in Panel A of Table 2.7, and those with general exits are shown in Panel B.

[32] Detailed explanation of the methodology is found in Hethey and Schmieder (2010). Using clusters of worker flows information from individual level social security data, the authors classify small establishment deaths as establishments that exit with less than four employees. Atomized deaths refer to exit establishments whose ratio of maximum clustered outflows (MCO) to last employment is less than 30%. MCO is defined as the largest flow of all the clustered outflows in a year. Chunky deaths refer to those whose value is between 30-80%. Their classification of exiting establishments helps identify which exits result in complete job destruction as opposed to spin-offs, restructuring, or simple ID change. Some previous papers rely on the last appearance of establishments in the data or when the number of employees are reduced to zero as indicators of firm closures, but if cases of spin-offs or restructuring are sizeable, it can lead to misleading results.

Panel A of Table 2.7 shows that small firms are more likely to undergo exits resulting in shutdown after the 1979 reform by 7.23 percentage points using the most simple specification in column (1). As the firm size grows, the exit probability reduces by 5.78 percentage points for Q2, 7.19 percentage points for Q3, and 7.34 percentage points for Q4, the largest set of firms. Overall, the estimates are stable with treatment-specific linear time trends and additional control. In the preferred specification in column (4), the probability of exit decreases monotonically as firm size grows, with the reform effects for Q1 being the highest with 7.21 percentage point increase in exit probability and 6.79 percentage points less for Q4 following the reform. The results are qualitatively the same when using general exits as outcome which include establishment ID change, take-over/restructuring or spin-offs as additional reasons for establishment closures as shown in Panel B of Table 2.7.

The results shown are as expected. They suggest that the probability of closure increases after the 1979 extension of job protection period for all firms, but more for small firms relative to big firms. This is confirms the hypothesis that it is more costly for small size firms to adjust to the expansionary maternity leave reforms.

2.5.4. Differential Reform Effects on Wage

In this section, the set of outcomes of interest are wage changes of employees after the reform. Only the mean and median wages of full-time female and male employees are provided in the BHP for comparison. They are not divided by age and gender at the same time so the dataset does not provide information of wage for female employees specifically in their childbearing years. Nonetheless, the analyses are conducted to compare the wage variations of female and male

workers in general, and subsequently their level of wage inequality. Real wage is in euros with the year 1980 as the base year.

Panel A in Table 2.8 shows the changes in median full-time female employee wage. The preferred specification in column (4) shows that the female employee wage increased after the reform for all firms. For small firms, real wage increased by 6.92 percent, and the increase was higher for establishments in Q2 and Q3 by 1.71 percent and 0.95 percent, respectively. The effect on large firms in Q4 was not much different from effects on small firms in Q1. Panel B in Table 2.8 shows that median real wage for full-time male employees have also increased. Column (4) shows that the reform increased their wage in small firms by 6.6 percent, and increased further by 0.68 percent for firms in Q2. Additional increase relative to Q1 was shown to be only slightly less for Q3 at 0.59 percent, but again, the effect on large firms in Q4 was similar to the one on Q1.

A better indicator of wage effects of the reform is the relative wage of female and male workers. A female-male wage inequality variable was created by dividing the real median female employee wage by median male employee wage. The results are shown in Panel C. The changes in wage inequality are not statistically different from zero because both male and female employee wages increased by similar magnitudes.

2.5.5. Heterogeneity by Urban-Rural Districts

This section explores the differential effects of maternity leave reforms that may be present based on the location of an establishment. Jäger (2016) explores the substitutability of incumbent and outside workers by using worker deaths as exogenous worker exits in Germany. Among other things, the author examines whether workers are more replaceable in thicker external markets. His motivation comes from Lazear (2009) that suggests agglomeration of workers, particularly of the

same skill sets, near an establishment affect the level of hiring frictions and ability to replace workers. Jäger (2016) uses the share of employment relative to the nationwide employment of by occupation × commuting zone levels as a measure for labor market thickness, and find evidence that workers are indeed more substitutable in thicker labor markets.

In this paper, I use a coarser definition by simply dividing firms by locations into urban versus rural districts, where firms in urban districts are expected to located in larger clusters of workers and other firms, which allow relatively easier replacement of employees than in rural areas. Therefore, with a maternity leave reform that extended job protection period of mothers, firms in rural areas may be more negatively affected. To examine differential effects of firms by location, equation (1) is examined separately for a subsample of establishments in urban and rural areas separately. There are 410 districts, or *kreis*, in Germany of which 107 districts are classified as urban and the remaining 294 as rural. This is divided primarily by the region's population density and share of settlement area. The current data sample produces 89 urban districts and 236 rural districts.

Table 2.9 shows the results of equation (2) with treatment-specific linear time trends for outcomes of female employee share in columns (1) and (2), full-time female employee share in columns (3) and (4), and part-time female employee share in columns (5) and (6). The directions of the reform effects on the firms in both urban and rural areas are consistent with the results found in Table 2.3 when combining all firms. That is, small firms hire higher share of female workers relative to big firms following the reform, but the higher share primarily comes from hiring more part-time female workers. The absolute magnitude of the effect, however, appears to be slightly larger in rural areas than in urban areas. This suggests that in rural areas where there are less

agglomeration of workers and firms, firms may be hiring more part-time female workers to adjust to the mandatory expansions in maternity leave as shown in columns (5) and (6).

The same regressions separated by urban and rural districts were conducted for other outcomes as well, and are displayed in the subsequent tables**.** For female worker inflows (Table 2.10), outflows (Table 2.11), and exit probability (Table 2.12), the directions of reform effects are again consistent with those when using all sample firms shown in Tables 2.4, 2.5 and 2.7, respectively. The absolute magnitude for all these outcomes appear to be stronger in urban districts than in rural areas, however. In terms of job flows, this may be because establishments in urban districts undergo higher employee turnover rates in general, and this is true for female employees as well. In terms of probability of shutdown exits or general exits, there is a higher probability of firm closures in urban districts than in rural districts as shown in Table 2.12.

Lastly, the inflows of rehires of female employees for urban and rural districts are shown in Table 2.13. Using equation (2), the rehires of female employees in urban districts are not statistically different from zero for any of the quartiles as shown in column (1). However, in the rural districts, small firms tend to have larger share of inflows of female rehires by 0.16 percentage points and less so for firms in Q2. The effects on firms that are larger are not statistically different from the effects on small firms. The estimates show that the results in Table 2.6 when combining firms from all regions are driven by firm behaviors in rural districts. In rural areas where employees are not readily substitutable, firms may opt to choose to hire previous employees that have worked in the past. In addition, mothers themselves who take maternity leaves may find it difficult to search for new jobs in rural areas, thus may have more incentives to return to their previous employers.

2.6. Conclusion

Maternity leave expansions have long been a topic of interest for many economists. While their effects on labor market outcomes of mothers and female workers in general have been widely studied in the past, analyses from a firm-level perspective have been very rare. This paper used establishment-level data in a difference-in-difference framework to explore the heterogeneity of reform effects on firms by size using the 1979 maternity leave reform in Germany. While the reform did not increase monetary contributions paid to mothers by firms, it increased the job protection period from 8 weeks to 6 months. All firms were affected by the reform, but firms of different sizes are expected to be differentially impacted. The firms are therefore divided into 4 quartiles based on the size of firms, with the small firms belonging to the bottom quantile expected to have relatively more "intense" exposure to the reform, or treatment, due to the limited resources and flexibility in finding and replacing mothers on leave. In other words, small firms are considered to be more vulnerable to the reform due to the higher costs involved. This paper was motivated from this hypothesis.

In theory, the most plausible scenario resulting from an expansionary maternity leave reform is the labor supply curve shifting to the right as much as women value the maternity benefits. Labor demand is likely to shift to the left as much as the wage/non-wage costs are added due to the reform. Since in Germany, like in most European countries, monetary contributions are primarily paid for by the government, labor demand only shifts as much as non-wage costs increase. And this shift is expected to be less than the degree of the rightward shift in the labor supply, resulting in higher female employment and lower wages (Ruhm 1998). Previous literature that examine the effects on female workers in general have pointed to an increase in female

employment but no sizeable effects on wages. The results of this study generally confirms these previous findings with establishment-level data.

The contribution of this paper to the existing literature lies in the empirical findings that provide insight as to how the expansionary maternity leave reform differentially affects firms by size. The heterogeneous effects can be summarized as follows: (1) Total female employment share is increased more in small firms than big firms. This is shown to be driven by a positive increase in part-time female workers as opposed to a decrease in employment levels of full-time female employees; (2) Small firms undergo higher turnover rates of female employees measured by their annual inflows and outflows; and (3) Small firms have higher probability of shutdown after the reform and the probability decreases monotonically as firm size grows. These results provide evidence that small firms do bear higher costs of the reform.

In addition to the effects on small firms, other outcomes were examined. (1) There are no statistically significant changes in relative wage of female and male workers; (2) There exists additional heterogeneous effects of the reform by urban and rural areas. In particular, firms tend to hire more part-time female employees and rehire previous female employees in rural areas; and (3) The level of skilled employees of a firm decreases female employment suggesting that firms do not increase demand for labor to retain firm-specific human capital of their female workers following the reform.

The drawback of the current study is that the analyses are conducted on outcomes of total female workers. Further information on how the reform affects mothers on leave or female workers in prime childbearing years could not be provided. Nevertheless, the results in the current study provide evidence on how labor markets respond differentially by firm size, skill levels, and location. In addition, the 1979 expansionary reform in Germany used in this paper primarily

increased the job protection period, but future research can examine how the reform effects and magnitudes differ when there is an increase in monetary maternity benefits granted to mothers.

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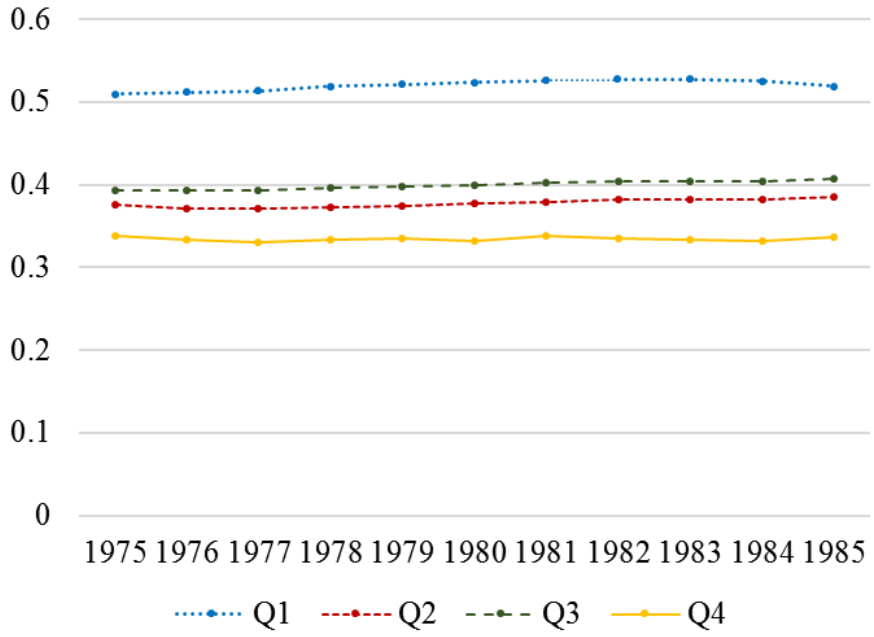
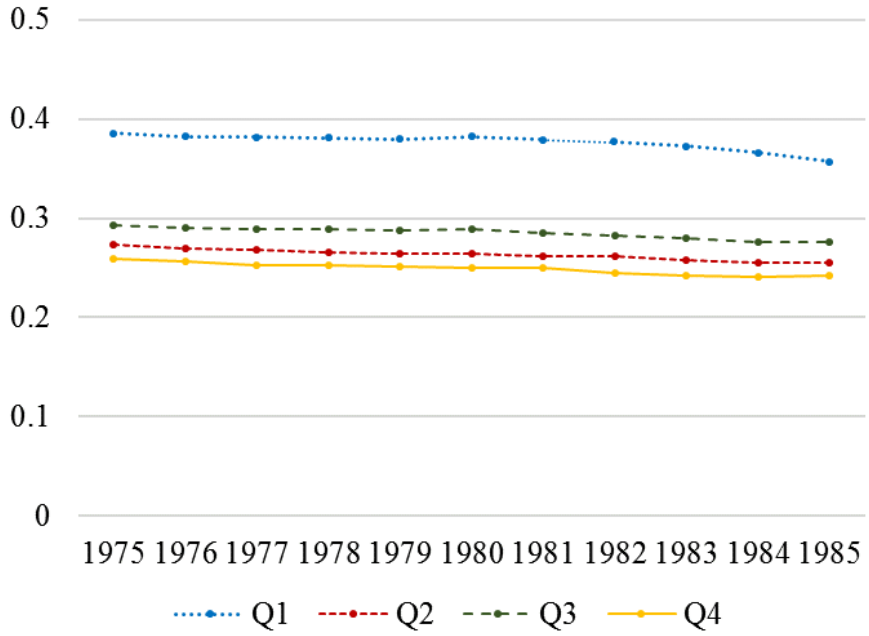
2.8. Figures

Figure 2.1. Annual Share of Female Employees

Panel A. Share of total female employees

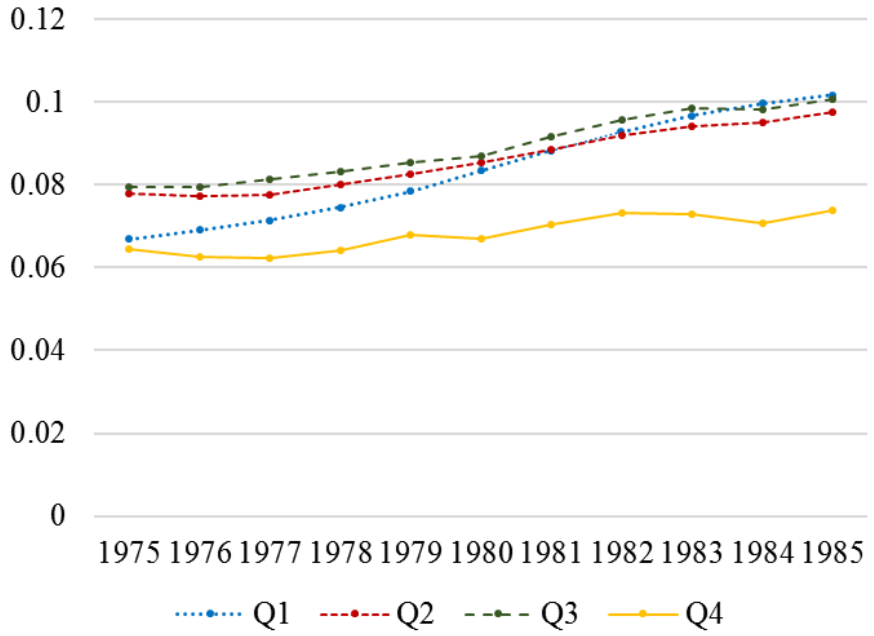


Panel B. Share of full-time female employees



(Figure 2.1. continued)

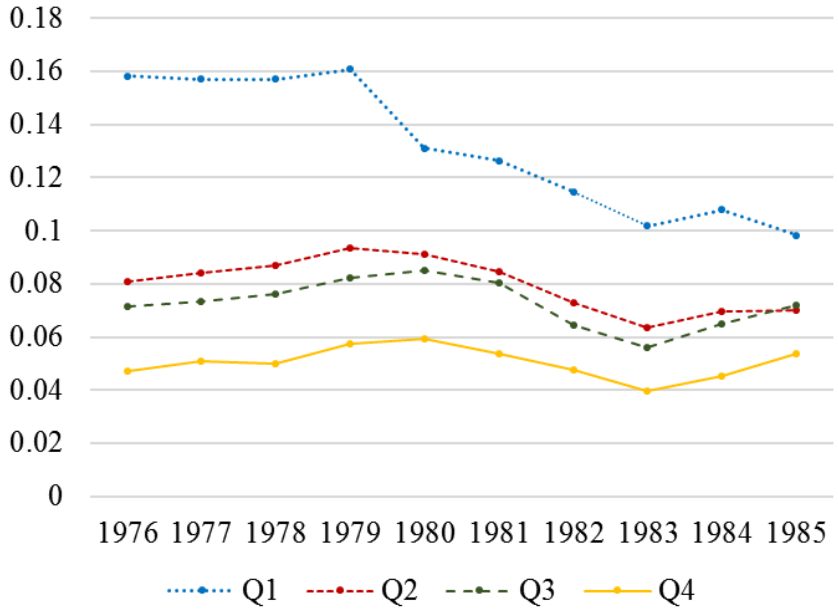
Panel C. Share of part-time female employees



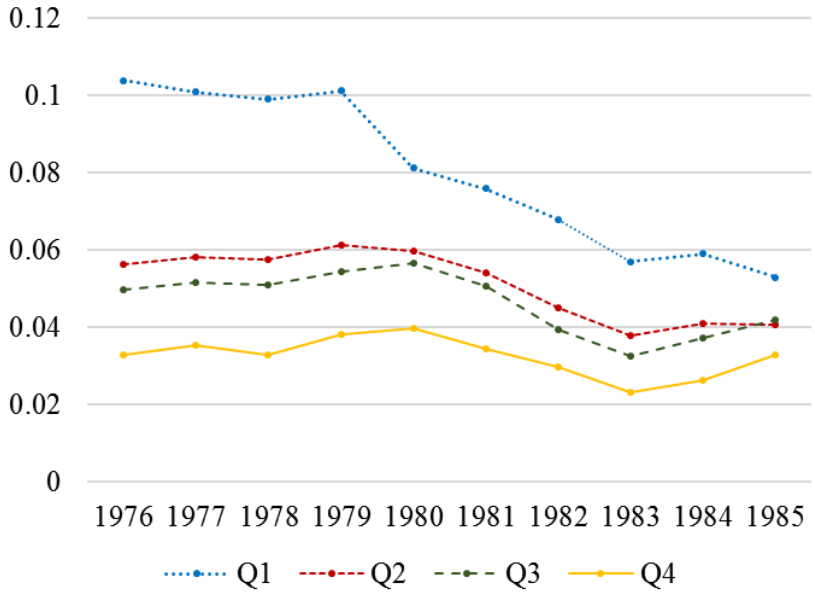
Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985, and total 5,561,101. The data plotted are female employees divided by total number of employees in an establishment. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees.

Figure 2.2. Annual Share of Inflows of Female Employees

Panel A. Share of Inflows of total female employees

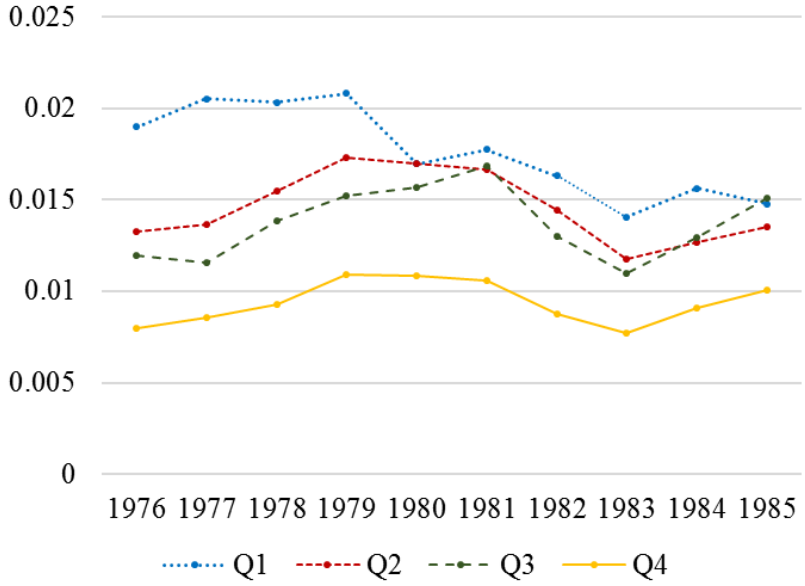


Panel B. Share of Inflows of full-time female employees



(Figure 2.2. continued)

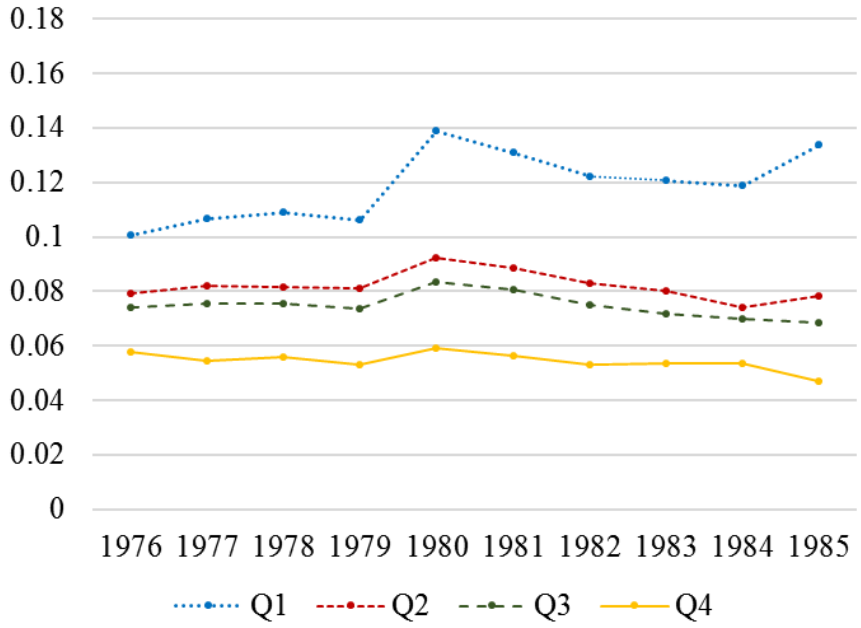
Panel C. Share of Inflows of part-time female employees



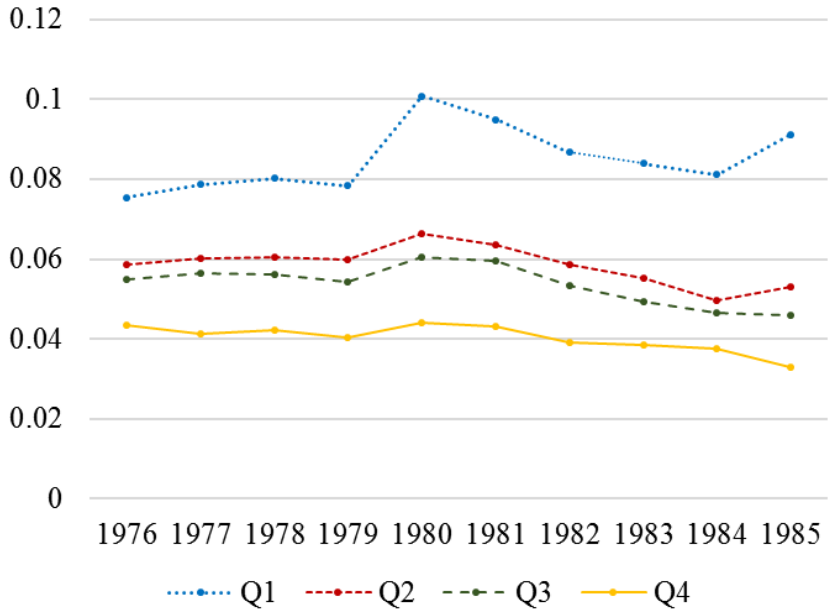
Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985, and total 5,095,896. Inflows of a year (t) refer to the number of employees who were working in an establishment on June 30 of that year (t) but were not working there on June 30 of the previous year (t-1). The outcomes are inflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). The data plotted are inflows of female employees divided by total number of employees in an establishment. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees.

Figure 2.3. Annual Share of Outflows of Female Employees

Panel A. Share of Outflows of total female employees

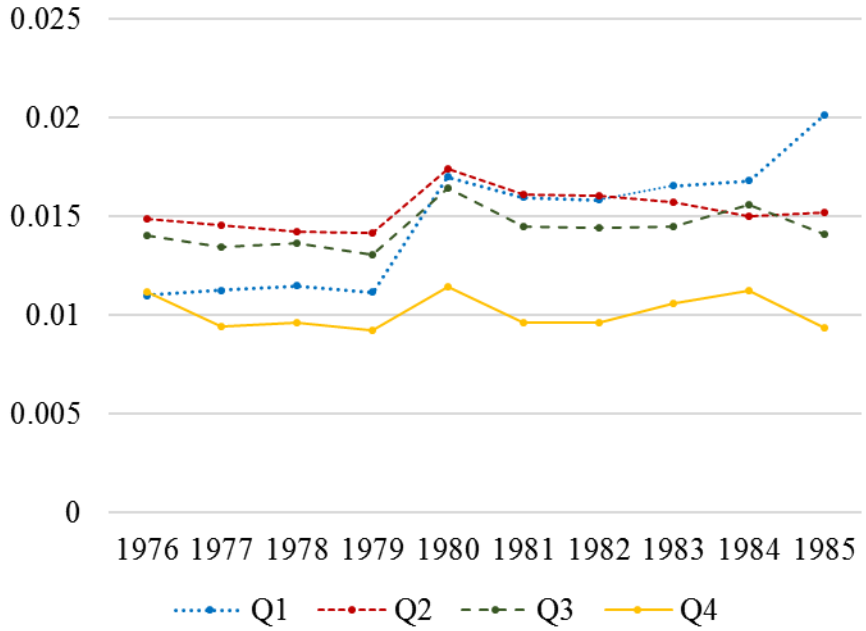


Panel B. Share of Outflows of full-time female employees



(Figure 2.3. continued)

Panel C. Share of Outflows of part-time female employees



Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985, and total 5,561,101. Outflows of a year (t) refer to the number of employees who were not working in an establishment on June 30 of that year (t) but were working there on June 30 of the previous year (t-1). The outcomes are outflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). The data plotted are outflows of female employees divided by total number of employees in an establishment. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees.

2.9. Tables

Table 2.1. Summary Statistics of the Number of Employees by Treatment Intensity Quartiles

	N	min	max	mean	median	SD	%
1	1,875,688	1	24	4.8	3	4.8	90.29
2	166,256	25	144	54.5	44	29.4	8.00
3	30,858	145	766	292.7	241	146.2	1.49
4	4,672	767	35,966	1930.4	1254	2315.1	0.22
Total	2,077,474	1	35,966	17.4	3	148.6	100

Note: The treatment intensity quartiles are divided based on the number of total workers of establishments. The table shows the statistics of the number of employees for each quartile for the pre-reform years, 1975-1978. The number of observations (N) and percentage of the sample in each quartile (%) with respect to the total sample are shown in the first and last columns, respectively.

Table 2.2. Summary Statistics by Quartiles

Variables	N	Mean	SD	Mean by quartile of total workers			
				Q1	Q2	Q3	Q4
A. Basic variables							
# of employees	2,077,474	17.4	148.6	4.8	54.5	292.7	1930.4
# of female employees	2,077,474	6.6	45.4	2.2	20.6	114.7	565.9
Inflows of female employees	2,077,474	2.7	24.9	1.0	8.6	44.9	211.3
Outflows of female employees	2,103,162	1.0	10.5	0.4	3.2	15.6	68.0
B. Wage variables [a)]							
Mean real wage of FT female employees	1,407,390	25.9	11.1	24.8	32.2	34.8	37.2
ln(mean real wage of FT female employees)	1,407,390	3.2	0.5	3.1	3.4	3.5	3.6
Median real wage of FT female employees	1,407,390	25.6	11.1	24.6	31.6	34.1	36.4
ln(median real wage of FT female employees)	1,407,390	3.1	0.5	3.1	3.4	3.5	3.6
Median real wage of FT male employees	1,379,629	39.0	13.4	38.3	43.3	45.0	46.4
ln(median real wage of FT male employees)	1,379,629	3.6	0.4	3.6	3.7	3.8	3.8
Wage Inequality by gender [b)]	839,098	0.74	21.0	0.74	0.74	0.76	0.79
Wage Inequality (p25/p50) [c)]	1,947,921	0.77	0.2	0.77	0.71	0.73	0.75
C. Worker Flows							
% of female employees	2,077,474	0.501	0.406	0.514	0.373	0.394	0.334
% of Inflows of female employees	2,042,537	0.154	1.024	0.161	0.086	0.076	0.051
% of Inflows of PT female employees	2,042,537	0.020	0.418	0.021	0.015	0.013	0.009
% of Inflows of FT female employees	2,042,537	0.099	0.734	0.103	0.058	0.052	0.035
% of Inflows of rehired employees	1,772,307	0.030	0.246	0.031	0.024	0.022	0.025
% of Inflows of rehired female employees	1,772,307	0.012	0.129	0.013	0.008	0.008	0.007
% of Outflows of female employees	2,077,474	0.103	0.209	0.106	0.081	0.075	0.055
% of Outflows of PT female employees	2,077,474	0.012	0.067	0.011	0.014	0.014	0.010
% of Outflows of FT female employees	2,077,474	0.076	0.181	0.078	0.060	0.055	0.042
D. Skill-levels and tasks							
% of highly qualified employees	2,077,474	0.670	0.339	0.673	0.650	0.614	0.615
% of qualified employees	2,077,474	0.648	0.342	0.651	0.622	0.578	0.560
% of unskilled employees	2,077,474	0.311	0.333	0.309	0.328	0.359	0.359

E. Employee age distribution							
% of employees aged 25-34	2,077,474	0.237	0.281	0.237	0.236	0.231	0.233
% of inflows of employees aged 25-34	2,042,537	0.075	0.492	0.077	0.055	0.045	0.036
% of outflows of employees aged 25-34	2,077,474	0.052	0.139	0.052	0.054	0.045	0.036
F. Region							
% of urban states	2,077,080	0.355	0.478	0.349	0.395	0.458	0.578

Note: The data comes from the Establishment History Panel (BHP) of Germany. The treatment intensity quartiles are divided based on the number of total workers of establishments. The table shows the statistics of listed variables for the pre-reform years, 1975-1978. The number of observations (N), mean and standard deviation (SD) are shown for the data sample used in the current study, while the last four columns show the mean of the variables divided by each quartile. % denotes the share of a respective variable divided by the number of total employees by each establishment, i.e. its size. [a)] The wage variables are real values deflated by CPI with the year 1980 as the base year. The unit is in euros. [b)] Wage inequality by gender is computed by median wage of FT female employees divided by median wage of FT male employees. [c)] Wage inequality is computed by the 25^{th} percentile wage divided by 75^{th} percentile wage of employees in an establishment.

Table 2.3. Effects on the Share of Female Employees

Panel A:

	Share of female employees			
	(1)	(2)	(3)	(4)
POST	0.0128***	0.0128***	0.0127***	0.0127***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0077***	-0.0076***	-0.0070***	-0.0069***
	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Q3 × POST	-0.0130***	-0.0119***	-0.0138***	-0.0128***
	(0.0005)	(0.0006)	(0.0005)	(0.0006)
Q4 × POST	-0.0173***	-0.0093***	-0.0191***	-0.0118***
	(0.0010)	(0.0016)	(0.0010)	(0.0016)
highly qualified workers		-0.0002***		-0.0002***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.9068	0.9068	0.9068	0.9068

Panel B:

	Share of FT female employees			
	(1)	(2)	(3)	(4)
POST	-0.0035***	-0.0035***	-0.0036***	-0.0036***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q2 × POST	-0.0077***	-0.0076***	-0.0057***	-0.0055***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Q3 × POST	-0.0105***	-0.0095***	-0.0103***	-0.0094***
	(0.0006)	(0.0006)	(0.0006)	(0.0006)
Q4 × POST	-0.0079***	-0.0004	-0.0091***	-0.0024
	(0.0011)	(0.0015)	(0.0011)	(0.0016)
highly qualified workers		-0.0002***		-0.0002***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.8086	0.8086	0.8086	0.8086

(Table 2.3. continued)

Panel C:

	Share of PT female employees			
	(1)	(2)	(3)	(4)
POST	0.0187***	0.0187***	0.0188***	0.0188***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0056***	-0.0056***	-0.0065***	-0.0065***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q3 × POST	-0.0086***	-0.0086***	-0.0087***	-0.0087***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Q4 × POST	-0.0137***	-0.0140***	-0.0142***	-0.0144***
	(0.0008)	(0.0008)	(0.0008)	(0.0008)
highly qualified workers		0		0
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.7543	0.7543	0.7543	0.7543

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. The outcomes are female employees divided by total number of employees in an establishment. FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.4. Effects on the Share of Inflows of Female Employees

Panel A:

	Share of Inflows of female employees			
	(1)	(2)	(3)	(4)
POST	-0.0375***	-0.0374***	-0.0479***	-0.0477***
	(0.0005)	(0.0005)	(0.0008)	(0.0008)
Q2 × POST	-0.0732***	-0.0702***	0.0254***	0.0275***
	(0.0036)	(0.0038)	(0.0020)	(0.0026)
Q3 × POST	-0.2237***	-0.2015***	-0.0115	0.0048
	(0.0321)	(0.0322)	(0.0087)	(0.0130)
Q4 × POST	-0.2908**	-0.1297*	-0.0215	0.1037**
	(0.1316)	(0.0783)	(0.0237)	(0.0519)
highly qualified workers		-0.0052**		-0.0045**
		(0.0024)		(0.0023)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,095,896	5,095,896	5,095,896	5,095,896
R-squared	0.1544	0.1551	0.1590	0.1596

Panel B:

	Share of Inflows of FT female employees			
	(1)	(2)	(3)	(4)
POST	-0.2823***	-0.0282***	-0.0353***	-0.0352***
	(0.0004)	(0.0004)	(0.0006)	(0.0006)
Q2 × POST	-0.0507***	-0.0484***	0.0161***	-0.0178***
	(0.0025)	(0.0027)	(0.0014)	(0.0020)
Q3 × POST	-0.1530***	-0.1362***	-0.0107	0.0017
	(0.0220)	(0.0221)	(0.0067)	(0.0101)
Q4 × POST	-0.2199**	-0.0981	-0.0184	0.0772*
	(0.1025)	(0.0601)	(0.0180)	(0.0407)
highly qualified workers		-0.0039**		-0.0034*
		(0.0019)		(0.0018)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,095,896	5,095,896	5,095,896	5,095,896
R-squared	0.1600	0.1608	0.1642	0.1648

(Table 2.4. continued)

Panel C:

	Share of Inflows of PT female employees			
	(1)	(2)	(3)	(4)
POST	-0.0022***	-0.0021***	-0.0048***	-0.0048***
	(0.0002)	(0.0002)	(0.0003)	(0.0003)
Q2 × POST	-0.0235***	-0.0231***	0.0014*	0.0017**
	(0.0015)	(0.0015)	(0.0008)	(0.0008)
Q3 × POST	-0.0646***	-0.0613***	-0.0065***	-0.0043
	(0.0148)	(0.0148)	(0.0024)	(0.0027)
Q4 × POST	-0.0518***	-0.0281**	-0.0071	0.0097
	(0.0181)	(0.0142)	(0.0046)	(0.0060)
highly qualified workers		-0.0008***		-0.0006***
		(0.0002)		(0.0002)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,095,896	5,095,896	5,095,896	5,095,896
R-squared	0.1313	0.1314	0.1331	0.1332

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985. Inflows of a year (t) refer to the number of employees who were working in an establishment on June 30 of that year (t) but were not working there on June 30 of the previous year (t-1). The outcomes are inflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.5. Effects on the Share of Outflows of Female Employees

Panel A:

	Share of Outflows of female employees			
	(1)	(2)	(3)	(4)
POST	0.0309***	0.0309***	0.0311***	0.0311***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0234***	-0.0234***	-0.0262***	-0.0262***
	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Q3 × POST	-0.0304***	-0.0303***	-0.0297***	-0.0296***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Q4 × POST	-0.0332***	-0.0324***	-0.0330***	-0.0322***
	(0.0012)	(0.0012)	(0.0009)	(0.0010)
highly qualified workers		0		0
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.3415	0.3415	0.3415	0.3415

Panel B:

	Share of Outflows of FT female employees			
	(1)	(2)	(3)	(4)
POST	0.0189***	0.0189***	0.0189***	0.0190***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0172***	-0.0172***	-0.0187***	-0.0187***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q3 × POST	-0.0220***	-0.0218***	-0.0209***	-0.0207***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Q4 × POST	-0.0220***	-0.0205***	-0.0217***	-0.0204***
	(0.0010)	(0.0010)	(0.0008)	(0.0008)
highly qualified workers		-0.0000***		-0.0000***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.3047	0.3047	0.3047	0.3047

(Table 2.5. continued)

Panel C:

	Share of Outflows of PT female employees			
	(1)	(2)	(3)	(4)
POST	0.0072***	0.0072***	0.0072***	0.0072***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Q2 × POST	-0.0042***	-0.0042***	-0.0047***	-0.0047***
	(0.0001)	(0.0001)	(0.0001)	(0.0001)
Q3 × POST	-0.0055***	-0.0056***	-0.0055***	-0.0056***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q4 × POST	-0.0069***	-0.0080***	-0.0068***	-0.0078***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
highly qualified workers		0.0000***		0.0000***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.244	0.244	0.244	0.244

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. Outflows of a year (t) refer to the number of employees who were not working in an establishment on June 30 of that year (t) but were working there on June 30 of the previous year (t-1). The outcomes are outflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.6. Effects on the Share of Inflows of Female Employee Rehires

	Share of Inflows of female employee rehires			
	(1)	(2)	(3)	(4)
POST	0.0027***	0.0027***	0.0011***	0.0011***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0176***	-0.0175***	-0.0011***	-0.0010***
	(0.0010)	(0.0010)	(0.0003)	(0.0003)
Q3 × POST	-0.0198***	-0.0186***	-0.0003	0.0004
	(0.0018)	(0.0017)	(0.0010)	(0.0012)
Q4 × POST	-0.0369***	-0.0291**	-0.0082**	-0.0028
	(0.0135)	(0.0118)	(0.0041)	(0.0030)
highly qualified workers		-0.0003***		-0.0002***
		(0.0001)		(0.0001)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	4,597,238	4,597,238	4,597,238	4,597,238
R-squared	0.1310	0.1310	0.1320	0.1321

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1977-1985. An employee is a rehire in the current year (t) if he/she was working on at least one of the reference dates (June 30) in the preceding three years (t-2 and/or t-3), but not working one year earlier (t-1). The outcome is inflows of female rehires in a given year (t) divided by the number of total workers in the previous year (t-1). POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Qk is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.7. Effects on Establishment Exits

Panel A:

	Shutdown Exits			
	(1)	(2)	(3)	(4)
POST	0.0723***	0.0723***	0.0721***	0.0721***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0578***	-0.0577***	-0.0559***	-0.0559***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q3 × POST	-0.0719***	-0.0715***	-0.0652***	-0.0648***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Q4 × POST	-0.0734***	-0.0701***	-0.0704***	-0.0679***
	(0.0005)	(0.0007)	(0.0005)	(0.0007)
highly qualified workers		-0.0001***		-0.0001***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.1875	0.1875	0.1875	0.1875

(Table 2.7. continued)

Panel B:

	General Exits			
	(1)	(2)	(3)	(4)
POST	0.0766***	0.0766***	0.0762***	0.0762***
	(0.0002)	(0.0002)	(0.0002)	(0.0002)
Q2 × POST	-0.0562***	-0.0562***	-0.0527***	-0.0527***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Q3 × POST	-0.0730***	-0.0725***	-0.0651***	-0.0648***
	(0.0005)	(0.0005)	(0.0005)	(0.0005)
Q4 × POST	-0.0770***	-0.0734***	-0.0737***	-0.0711***
	(0.0006)	(0.0008)	(0.0006)	(0.0008)
highly qualified workers		-0.0001***		-0.0001***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.1859	0.1859	0.186	0.186

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. Reasons for exits in the data are categorized into the following: Shutdown exits refer to establishment closures that resulted in (1) establishment ID change, (2) take-over/restructuring, (3) spin-off/pushed, (4) small death, (5) atomized death, (6) chunky death, and (7) reasons unclear. Shutdown exits in Panel A refer to (4), (5), and (6), while general exits in Panel B combine all categories. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.8. Wage Effects and Female-Male Wage Inequality

Panel A:

	Median female employee wage			
	(1)	(2)	(3)	(4)
POST	0.0695***	0.0695***	0.0692***	0.0692***
	(0.0004)	(0.0004)	(0.0004)	(0.0004)
Q2 × POST	0.0148***	0.0147***	0.0172***	0.0171***
	(0.0008)	(0.0008)	(0.0009)	(0.0009)
Q3 × POST	0.0086***	0.0075***	0.0105***	0.0095***
	(0.0010)	(0.0010)	(0.0011)	(0.0011)
Q4 × POST	0.0062***	-0.002	0.0103***	0.0024
	(0.0016)	(0.0020)	(0.0017)	(0.0021)
highly qualified workers		0.0002***		0.0002***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	3,806,051	3,806,051	3,806,051	3,806,051
R-squared	0.8277	0.8277	0.8277	0.8277

Panel B:

	Median male employee wage			
	(1)	(2)	(3)	(4)
POST	0.0667***	0.0667***	0.0660***	0.0660***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q2 × POST	0.0018***	0.0016***	0.0069***	0.0068***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)
Q3 × POST	0.0021***	0.0008	0.0071***	0.0059***
	(0.0008)	(0.0008)	(0.0008)	(0.0009)
Q4 × POST	0.0054***	-0.0043**	0.0129***	0.0035
	(0.0015)	(0.0020)	(0.0016)	(0.0023)
highly qualified workers		0.0003***		0.0003***
		0.0000		0.0000
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	3,707,137	3,707,137	3,707,137	3,707,137
R-squared	0.8301	0.8301	0.8301	0.8301

(Table 2.8. continued)

Panel C:

	Female-male wage inequality			
	(1)	(2)	(3)	(4)
POST	0.0505	0.0505	0.052	0.052
	(0.0512)	(0.0512)	(0.0531)	(0.0531)
Q2 × POST	-0.0364	-0.0364	-0.0445	-0.0444
	(0.0445)	(0.0445)	(0.0547)	(0.0547)
Q3 × POST	-0.045	-0.0446	-0.0471	-0.0466
	(0.0502)	(0.0502)	(0.0533)	(0.0532)
Q4 × POST	-0.0486	-0.0454	-0.0512	-0.048
	(0.0509)	(0.0506)	(0.0533)	(0.0526)
highly qualified workers		-0.0001***		-0.0001***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	2,295,350	2,295,350	2,295,350	2,295,350
R-squared	0.3603	0.3603	0.3603	0.3603

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. Median female and male wage is real wage in euros with 1980 as the base year. They are in natural logs. Female-male wage inequality is computed as median female wage divided by median male wage. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.9. Effects on the Share of Female Employees by Urban-Rural Districts

	Female employees		FT female employees		PT female employees	
	Urban	Rural	Urban	Rural	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.0099***	0.0142***	-0.0037***	-0.0036***	0.0180***	0.0191***
	(0.0004)	(0.0003)	(0.0005)	(0.0003)	(0.0003)	(0.0002)
Q2 × POST	-0.0056***	-0.0076***	-0.0057***	-0.0056***	-0.0068***	-0.0063***
	(0.0006)	(0.0004)	(0.0007)	(0.0005)	(0.0006)	(0.0004)
Q3 × POST	-0.0104***	-0.0157***	-0.0094***	-0.0110***	-0.0084***	-0.0088***
	(0.0009)	(0.0007)	(0.0010)	(0.0008)	(0.0008)	(0.0006)
Q4 × POST	-0.0169***	-0.0196***	-0.0104***	-0.0073***	-0.0132***	-0.0149***
	(0.0014)	(0.0014)	(0.0015)	(0.0015)	(0.0011)	(0.0009)
Establishment FE	YES	YES	YES	YES	YES	YES
Treatment-specific trend	YES	YES	YES	YES	YES	YES
Observations	1,947,249	3,611,165	1,947,249	3,611,165	1,947,249	3,611,165
R-squared	0.9021	0.9087	0.7995	0.8127	0.7532	0.7555

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. 89 urban districts and 236 rural districts are used in the analyses. The outcomes are female employees divided by total number of employees in an establishment. FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.10. Effects on the Share of Inflows of Female Employees by Urban-Rural Districts

	Inflows of female employees		Inflows of FT female employees		Inflows of PT female employees	
	Urban	Rural	Urban	Rural	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
POST	-0.0600***	-0.0415***	-0.0442***	-0.0306***	-0.0067***	-0.0038***
	(0.0019)	(0.0008)	(0.0013)	(0.0006)	(0.0008)	(0.0003)
Q2 × POST	0.0318***	0.0210***	0.0206***	0.0135***	0.0024*	0.0004
	(0.0034)	(0.0022)	(0.0024)	(0.0016)	(0.0014)	(0.0007)
Q3 × POST	-0.0259	-0.0017	-0.0140	-0.0083	-0.0158***	-0.0004
	(0.0175)	(0.0081)	(0.0131)	(0.0063)	(0.0054)	(0.0025)
Q4 × POST	-0.0345	0.0053	-0.0310	0.0056	-0.0063	-0.0069
	(0.0393)	(0.0177)	(0.0305)	(0.0102)	(0.0062)	(0.0070)
Establishment FE	YES	YES	YES	YES	YES	YES
Treatment-specific trend	YES	YES	YES	YES	YES	YES
Observations	1,782,268	3,311,127	1,782,268	3,311,127	1,782,268	3,311,127
R-squared	0.1473	0.1772	0.1598	0.1722	0.1159	0.1627

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985. 89 urban districts and 236 rural districts are used in the analyses. Inflows of a year (t) refer to the number of employees who were working in an establishment on June 30 of that year (t) but were not working there on June 30 of the previous year (t-1). The outcomes are inflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.11. Effects on the Share of Outflows of Female Employees by Urban-Rural Districts

	Outflows of female employees		Outflows of FT female employees		Outflows of PT female employees	
	Urban	Rural	Urban	Rural	Urban	Rural
	(1)	(2)	(3)	(4)	(5)	(6)
POST	0.0333***	0.0298***	0.0206***	0.0180***	0.0085***	0.0065***
	(0.0004)	(0.0003)	(0.0003)	(0.0002)	(0.0002)	(0.0001)
Q2 × POST	-0.0311***	-0.0233***	-0.0224***	-0.0165***	-0.0066***	-0.0036***
	(0.0006)	(0.0004)	(0.0005)	(0.0004)	(0.0002)	(0.0002)
Q3 × POST	-0.0330***	-0.0275***	-0.0230***	-0.0195***	-0.0073***	-0.0043***
	(0.0008)	(0.0007)	(0.0007)	(0.0006)	(0.0004)	(0.0003)
Q4 × POST	-0.0359***	-0.0307***	-0.0243***	-0.0195***	-0.0081***	-0.0062***
	(0.0013)	(0.0014)	(0.0010)	(0.0012)	(0.0006)	(0.0003)
Establishment FE	YES	YES	YES	YES	YES	YES
Treatment-specific trend	YES	YES	YES	YES	YES	YES
Observations	1,947,249	3,611,165	1,947,249	3,611,165	1,947,249	3,611,165
R-squared	0.3296	0.3477	0.3006	0.3063	0.2385	0.2484

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985. 89 urban districts and 236 rural districts are used in the analyses. Outflows of a year (t) refer to the number of employees who were not working in an establishment on June 30 of that year (t) but were working there on June 30 of the previous year (t-1). The outcomes are outflows of female employees in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses. *** p<0.01, ** p<0.05, * p<0.1

Table 2.12. Effects on Firm Exits by Urban-Rural Districts

	Shutdown Exits		General Exits	
	Urban	Rural	Urban	Rural
	(1)	(2)	(3)	(4)
POST	0.0802***	0.0677***	0.0849***	0.0715***
	(0.0003)	(0.0002)	(0.0003)	(0.0002)
Q2 × POST	-0.0638***	-0.0519***	-0.0624***	-0.0476***
	(0.0006)	(0.0004)	(0.0006)	(0.0005)
Q3 × POST	-0.0731***	-0.0610***	-0.0743***	-0.0600***
	(0.0007)	(0.0006)	(0.0008)	(0.0007)
Q4 × POST	-0.0792***	-0.0651***	-0.0833***	-0.0677***
	(0.0006)	(0.0008)	(0.0007)	(0.0011)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	YES	YES	YES	YES
Observations	1,947,249	3,611,165	1,947,249	3,611,165
R-squared	0.1896	0.1876	0.1883	0.1859

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1975-1985. 89 urban districts and 236 rural districts are used in the analyses. Reasons for exits in the data are categorized into the following: Shutdown exits refer to establishment closures that resulted in (1) establishment ID change, (2) take-over/restructuring, (3) spin-off/pushed, (4) small death, (5) atomized death, (6) chunky death, and (7) reasons unclear. Shutdown exits in Panel A refer to (4), (5), and (6), while general exits in Panel B combine all categories. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Q*k* is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table 2.13. Effects on the Share of Inflows of Female-Employees Rehires by Urban-Rural Districts

	Rehires of female employees	
	Urban	Rural
	(1)	(2)
POST	0.0002	0.0016***
	(0.0003)	(0.0002)
Q2 × POST	-0.0003	-0.0014***
	(0.0004)	(0.0004)
Q3 × POST	0.0010	-0.0008
	(0.0015)	(0.0013)
Q4 × POST	-0.0059	-0.0105
	(0.0049)	(0.0071)
Establishment FE	YES	YES
Treatment-specific trend	YES	YES
Observations	1,605,412	2,989,347
R-squared	0.1326	0.1322

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1977-1985. 89 urban districts and 236 rural districts are used in the analyses. An employee is a rehire in the current year (t) if he/she was working on at least one of the reference dates (June 30) in the preceding three years (t-2 and/or t-3), but not working one year earlier (t-1). The outcome is inflows of female rehires in a given year (t) divided by the number of total workers in the previous year (t-1). POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Qk is equal to 1 if an establishment belongs to the *k*-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

2.10. Supplementary Tables

Table S.1. Effects on the Share of Inflows of Employees aged 25-34

	Share of Inflows of employees aged 25-34			
	(1)	(2)	(3)	(4)
POST	-0.0186***	-0.0186***	-0.0250***	-0.0249***
	(0.0003)	(0.0003)	(0.0004)	(0.0004)
Q2 × POST	-0.0516***	-0.0499***	0.0104***	0.0117***
	(0.0019)	(0.0019)	(0.0011)	(0.0014)
Q3 × POST	-0.1341***	-0.1211***	-0.0097**	0.0001
	(0.0162)	(0.0160)	(0.0047)	(0.0063)
Q4 × POST	-0.1461***	-0.0518	-0.013	0.0615**
	(0.0533)	(0.0343)	(0.0102)	(0.0243)
highly qualified workers		-0.0030***		-0.0027***
		(0.0011)		(0.0010)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	4,849,862	4,849,862	4,849,862	4,849,862
R-squared	0.1573	0.1583	0.1636	0.1644

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985. Inflows of a year (t) refer to the number of employees who were working in an establishment on June 30 of that year (t) but were not working there on June 30 of the previous year (t-1). The outcomes are inflows of female employees between ages 25-34 in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Qk is equal to 1 if an establishment belongs to the k-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1

Table S.2. Effects on the Share of Outflows of Employees aged 25-34

	Share of Outflows of employees			
	(1)	(2)	(3)	(4)
POST	0.0475***	0.0475***	0.0478***	0.0478***
	(0.0003)	(0.0003)	(0.0003)	(0.0003)
Q2 × POST	-0.0336***	-0.0337***	-0.0392***	-0.0392***
	(0.0005)	(0.0005)	(0.0006)	(0.0006)
Q3 × POST	-0.0492***	-0.0496***	-0.0460***	-0.0464***
	(0.0009)	(0.0009)	(0.0010)	(0.0010)
Q4 × POST	-0.0535***	-0.0570***	-0.0516***	-0.0548***
	(0.0022)	(0.0023)	(0.0021)	(0.0023)
highly qualified workers		0.0001***		0.0001***
		(0.0000)		(0.0000)
Establishment FE	YES	YES	YES	YES
Treatment-specific trend	NO	NO	YES	YES
Observations	5,561,101	5,561,101	5,561,101	5,561,101
R-squared	0.2753	0.2754	0.2754	0.2754

Note: The data comes from the Establishment History Panel (BHP) of Germany. Observations are establishment-year for years 1976-1985. Outflows of a year (t) refer to the number of employees who were not working in an establishment on June 30 of that year (t) but were working there on June 30 of the previous year (t-1). The outcomes are outflows of female employees between ages 25-34 in a given year (t) divided by the number of total workers in the previous year (t-1). FT stands for full-time, and PT for part-time. POST is an indicator variable equal to 0 for pre-reform years 1975-1978 and 1 for post-reform years 1979-1985. Qk is equal to 1 if an establishment belongs to the k-th quartile of treatment intensity divided by total number of workers. Establishment belonging to Q1 have 1-24 employees; Q2: 25-144 employees; Q3: 145-766 employees; and Q4: more than 767 employees. Treatment-specific trends are interactions of Q2, Q3 and Q4 with linear time trends. Highly-qualified workers refer to employees who have a degree from a university of applied sciences or a university. All specifications include establishment fixed effects. The standard errors are clustered at the establishment level, and are in parentheses.
*** p<0.01, ** p<0.05, * p<0.1