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Does Part-time Work Affect Firms' Employment Structure and Outcomes?

Lea Bergmann

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Ruhr-Universität Bochum (RUB), Department of Economics
Universitätsstr. 150, 44801 Bochum, Germany

Technische Universität Dortmund, Department of Economic and Social Sciences
Vogelpothsweg 87, 44227 Dortmund, Germany

Universität Duisburg-Essen, Department of Economics
Universitätsstr. 12, 45117 Essen, Germany

Bergische Universität Wuppertal, Schumpeter School of Business and Economics
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Series Coordination:
RWI Press Office, presse@rwi-essen.de, phone +49 (0) 201 8149-213
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Does Part-time Work Affect Firms' Employment Structure and Outcomes? *

Lea Bergmann
RWI
University Duisburg-Essen
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Abstract

Shorter working hours reflect a fundamental tension in modern labor markets: They may increase the labor force participation of women and improve work-life balance, but may impose organizational costs on firms through coordination frictions and reduced scheduling flexibility. I examine a 2001 German reform that granted employees in firms with 16 or more employees the legal right to reduce working hours permanently. Using firm-level administrative data and a difference-in-discontinuities design around the 16-employee threshold, I find no evidence that the mandate alters firms' employee structure. Moreover, it does not affect firms' business volume or hiring behavior. These null effects can be explained by firms already accommodating part-time requests before the reform. Overall, the results indicate that the mandate did not generate detectable distortions in employment or firm performance.

JEL Classification: *J16, J18, J22, J63*

Keywords: part-time, firms, labor regulation

Lea Bergmann: RWI – Leibniz-Institut für Wirtschaftsforschung, Hohenzollernstr. 1-3, 45128 Essen, Germany, Tel.: +49 201 / 8149-358, E-mail: lea.bergmann@rwi-essen.de.

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1 Introduction

Shorter working hours reflect a fundamental tension in modern labor markets: while they are intended to increase female labor force participation and improve work–life balance, they may impose rising unit labor costs from declining average hours per worker and organizational frictions on firms. Working hours are now a central topic in policy debates across advanced economies. Part-time work is common in modern labor markets, with nearly 20% of workers in OECD countries currently employed part-time (OECD, 2024) and over 40% in Germany (Institute for Employment Research, IAB, 2025).

This paper examines how a right to work part-time affects firms' labor demand and productivity. The answer is theoretically ambiguous. If splitting one full-time position into multiple part-time positions creates coordination costs, communication frictions, or other inefficiencies, firms may face higher labor costs and reduced productivity. In response, they may adjust hiring, reorganize production, or alter firm size to avoid regulatory thresholds (Oi, 1962; Kleven, 2016; Garnero, Kampelmann and Rycx, 2014; Devicienti, Grinza and Vannoni, 2018). Alternatively, if hours are flexible inputs in production, firms may reorganize with minimal disruption (Euwals and Hogerbrugge, 2006; Friesen, 1997).

While prior studies examine the relationship between part-time employment and firm outcomes, they do not identify causal effects on firms. Research analyzing part-time work legislation focuses exclusively on workers' labor market outcomes. To my knowledge, no study has investigated the firm-level consequences of a legal right to part-time work. Such a right was introduced in 2001 in German firms with more than 15 employees. This paper examines how the legal entitlement to part-time work for employees affects firms' outcomes, focusing on employment, hiring, and business volume. Additionally, it investigates which mechanisms drive firm reactions. In 2001, Germany introduced a mandate that granted employees in firms with 16 or more employees the right to reduce their working hours permanently. Employees must notify their employers three months in advance, making the policy a predictable and permanent change to working-time arrangements. Importantly, no other major labor market regulations change discretely at the 16-employee threshold during the analyzed period, allowing for

a clean comparison of firms just above and below the cutoff. Paul (2016) and Paule-Paludkiewicz (2024) study the same mandate and reveal that the mandate increased part-time work of mothers both during parental leave but also up to the child's sixth birthday. Yet whether firms incurred costs from accommodating these requests remains unexplored.

To shed light on the long-term implications of such mandates, I use high-quality administrative data, the IAB establishment history panel. To identify the reform's causal effects, I exploit the 16-employee threshold in a difference-in-discontinuities design (Grembi, Nannicini and Troiano, 2016). A simple comparison of firms just above and just below the cutoff would be confounded by the fact that firm size correlates with age, sector, and growth potential, even in the absence of any policy change. The difference-in-discontinuities design addresses this by comparing how the discontinuity at the threshold evolves before and after the reform, thereby removing any time-invariant differences between smaller and larger firms. I restrict my sample to firms with 11 to 20 employees, which account for 9% of all enterprises and 10% of the workforce in Germany. These firms may face disproportionate compliance costs due to limited scope for internal substitution.

First, I analyze whether firms do adjust to the cutoff at 16 employees by bunching at a workforce of 15 to avoid compliance. There is no evidence of such behavior. Second, firms affected by the reform neither experience a surge in their part-time employment share, nor do they adjust their hiring behavior. Third, there is no negative impact on firms' business volume or closures. The estimates are precisely centered around zero, and the confidence intervals rule out economically meaningful effects. Furthermore, several robustness checks confirm the null effects. Overall, the mandate did not materially alter small firms' labor demand or performance.

To investigate the reasons for these null effects, I complement the analysis with survey evidence from the IAB Establishment Panel. It suggests a central mechanism underlying these findings: firms already accommodated part-time requests prior to the reform. Although treated firms report more transitions from full-time to part-time work after 2001, most state that they would have granted such requests even without the legal entitlement. This pattern suggests that the mandate largely formalized existing practices rather than imposing constraints on firms.

This paper relates to three strands of literature. First, it adds to the evidence on how labor market regulations affect firm behavior. Standard economic theory predicts that employment protection reduces labor demand and distorts hiring (Oi, 1962; Lazear, 1990; Bertola, 1992). Specifically, working hour regulations can raise hourly labor costs, prompting firms to adjust wages, employment, or production (Bengtsson and Molinder, 2017). Nonetheless, empirical evidence does not completely confirm these theoretical predictions: while working-time reductions have been linked to employment losses in some settings (Crépon and Kramarz, 2002; Hunt, 1999), in others they do not affect employment (Chemin and Wasmer, 2009). Evidence from France also suggests broader effects, including lower firm sales following an increase in minimum working hours (Carry, 2026). Conversely, the reduction of standard working hours in Portugal in 1996 caused firms to use labor more efficiently (Asai, Lopes and Tondini, 2024). Importantly, most existing studies on intensive-margin regulations analyze uniform, collectively bargained reductions in standard working hours, and existing evidence on part-time work mainly concerns voluntary adoption by firms (e.g., Devicienti, Grinza and Vannoni, 2020; Chadwick and Flinchbaugh, 2016). Whether individually claimed, permanent part-time entitlements generate comparable or distinct firm-level responses remains unexplored. This paper provides the first evidence on this question.

A second strand studies how part-time entitlements shape labor supply, particularly for mothers. Evidence from Germany shows that the part-time mandate did increase the share of mothers working part-time during parental leave (Paul, 2016), but also up to the child's 6th birthday Paule-Paludkiewicz (2024). Thereby, it improved long-run employment outcomes for mothers. In contrast, a Dutch study analyzing a comparable reform finds no effect on hours worked (Nieuwenhuis, Postepska and Alessie, 2026). A comparable Spanish policy raised mothers' employment and earnings, while also increasing discriminatory hiring of women of childbearing age in small firms (De Quinto and González, 2025; Fernández-Kranz and Rodríguez-Planas, 2021). Finally, Bärtsch and Sandner (2025) and Zimmert and Zimmert (2024) show that the incentives to work part-time during parental leave increase mothers' employment. While this literature generally points to positive effects of part-time entitlements on mothers' labor market outcomes, the evidence is not uniform.

A third strand examines how firms respond to family-related employment regulations, particularly parental leave reforms. Recent studies show that firms adjust to temporary absences by hiring substitutes, reallocating hours, or increasing overtime, sometimes incurring short-run costs (Brenøe et al., 2024, 2025; Corekcioglu, Francesconi and Kunze, 2025; Ginja, Karimi and Xiao, 2023; Huebener et al., 2025). However, such absences are temporary and difficult to anticipate. By contrast, a legal entitlement to part-time work produces a permanent and predictable reduction in contractual hours. From a theoretical perspective, this predictability may facilitate organizational adjustment and mitigate disruption. Whether this is indeed the case remains an open empirical question.

This paper contributes to all three strands by providing the first causal evidence on how firms respond to a legal right to part-time work. Previous work either analyzed how part-time work affects firm performance, or how the right to part-time work affects individual labor market outcomes. The mandate to grant part-time work allows me to investigate a policy that governs how workers are employed once hired, rather than affecting hiring decisions at the extensive margin. The reform is permanent as reductions in hours are not accompanied by a right to return to full-time work, unlike temporary arrangements such as parental leave. Furthermore, claiming the right must be announced early, making part-time work highly predictable for both firms and workers. Exploiting the 16-employee regulatory threshold, I apply a difference-in-discontinuities design. I examine small firms, where compliance costs are highest, and find no distortions in employment, hiring, or business volume. Complementary survey evidence reveals why: firms overwhelmingly report that they would have granted part-time requests even without the legal entitlement. These results suggest that a part-time mandate can enhance worker flexibility without generating detectable costs for firms – at least in institutional settings where flexible working arrangements are already widespread.

The remainder of the paper is structured as follows. Section 2 provides institutional background and describes the 2001 reform. Section 3 sets the theoretical foundation for this paper. Section 4 discusses the data and presents descriptive statistics. Section 5 outlines the empirical strategy. Section 6 presents the main results as well as robustness and heterogeneity analyses. Section 7 concludes.

2 Institutional Background

Part-time work is traditionally more common in Germany than in many other European countries. In 2000 19% of employees in Germany worked part-time compared to 11.8% in the EU. A typical part-time employee worked 19 hours per week (Wirtschafts- und Sozialwissenschaftliches Institut, 2025). Among women, the part-time share was 38% (OECD, 2024). Motherhood is a key driver of part-time employment: in 2000, 61% of employed mothers worked part-time (Bundesministerium für Familie, Senioren, Frauen und Jugend, 2012). Reducing working hours after childbirth is one of the main drivers of the child penalty and gender pay gap (Kleven, Landais and Søgaard, 2019; Fitzenberger, Sommerfeld and Steffes, 2013; Blau and Kahn, 2017). Firms typically offer part-time arrangements either in response to employees' preferences or to adjust labor input to current business needs.

Since 1985, legislation mandated firms to treat part-time and full-time employees equally and prohibited discrimination against part-time employees. However, employees had no statutory entitlement to reduce their working hours, and access to part-time work largely depended on employer discretion. This changed with the introduction of the Part-Time and Fixed-Term Employment Act (*Teilzeit- und Befristungsgesetz*, §8 TzBfG), which came into force on January 1, 2001. The part-time mandate was introduced as part of broader labor market reforms intended to increase labor market flexibility and reduce unemployment. It established a general legal right for employees to request a reduction in working hours. However, it did not provide workers with the right to return to full-time employment.

Eligibility required at least six months of tenure, a three-month notice period, and employment in firms with usually more than 15 employees, excluding apprentices and trainees. The firm-size threshold embedded in the legislation is central to the empirical design. No other major labor market regulation changed discretely at this threshold. Nearby thresholds apply to different regulations: employment protection legislation is enforced in firms with more than five full-time equivalent (FTE) employees, while firms with more than 19 employees must employ at least one severely disabled worker.

Employers could reject requests only on business grounds, for example if the

reduction in working hours generated disproportionate costs, substantially impaired business operations or workplace safety. As discussed in Section 6.2, such refusals appear to have been relatively uncommon in practice.

Simultaneously, a reform of the parental leave legislation (§ 15 BErzGG) came into force. It allowed employees on parental leave to reduce working hours to between 15 and 30 hours per week for a minimum duration of three months. The same restrictions as for the part-time law apply. This paper studies the effect of both changes, the general part-time law and the parental leave part-time law, at once.

3 Firms' Adjustment Channels to a Part-Time Mandate

Most existing evidence on part-time work concerns voluntary adoption (e.g., Devicienti, Grinza and Vannoni, 2020; Chadwick and Flinchbaugh, 2016). In these settings, firms choose whether and how to implement part-time arrangements, and observed outcomes reflect this endogenous decision. A legal mandate changes this fundamentally, because firms must grant workers' requests regardless of their own cost assessment. This creates potential adjustment pressures, particularly for firms operating close to their organizational or administrative capacity. This section outlines the main channels through which firms may respond to such a mandate and derives empirical predictions for the analysis.

First, firms just above 15 employees may reduce their workforce below the threshold to avoid compliance. This form of *bunching behavior* is observed in other settings with size-dependent regulations (Kleven, 2016). For example, Garicano, Lelarge and Van Reenen (2016) show that regulations at the 50-employee threshold in France, including mandatory works councils, health and safety committees, employee reporting requirements, and union representation, generate bunching below the cutoff and reduce output.

Second, firms may adjust their *hiring strategies*. To avoid part-time requests, firms may not hire individuals who are more likely to demand reduced hours, such as women of childbearing age. Such selective hiring may reinforce existing gender

gaps in employment. Fernández-Kranz and Rodríguez-Planas (2021) find that especially small firms responded to a part-time mandate in Spain by discriminatory hiring.

Third, firms may comply with the mandate but adjust their *internal organization*. If an employee reduces working hours, firms can compensate by hiring additional staff (increasing total payroll) or redistributing tasks among existing workers (internal substitution). Small firms face this constraint most acutely because they have fewer internal substitutes and higher administrative costs per worker. Thus, they have less scope for internal substitution than larger firms (Huebener et al., 2025). Carry (2026) shows that firms cannot flexibly allocate hours between workers, questioning how far internal organization enables firms to adjust to working time regulations. Both adjustments may increase coordination and staffing costs, particularly if tasks are difficult to divide across workers.

Fourth, firms may reduce *hourly wages* if reduced hours are perceived as a non-wage employment benefit or employees' tasks change if they start working part-time (Hamermesh and Biddle, 2025; Goldin, 2014). Indeed, German research provides evidence that part-time work, especially when working only few hours, is associated with lower wages (Paul, 2016).

Finally, firms may experience little to *no change*. This may occur if firms already offered part-time work before the reform or if accommodating reduced hours does not affect production processes substantially. For example, the reduction of standard working hours in Portugal in 1996 made firms use labor more efficiently (Asai, Lopes and Tondini, 2024).

The adjustment channel that firms choose is theoretically ambiguous. Part-time arrangements can generate efficiency gains through greater staffing flexibility (Euwals and Hogerbrugge, 2006; Friesen, 1997) and improved employee retention among workers who would otherwise exit the labor force (Edwards and Robinson, 2004). At the same time, the relative importance of fixed employment costs increases. Additionally, coordination complexity increases when employees are absent for part of the day or week (Garnero, Kampelmann and Rycx, 2014; Devicienti, Grinza and Vannoni, 2018). Small firms may have higher adjustment costs relative to their workforce.

Whether the benefits or costs dominate depends critically on whether part-

time work is firm-initiated or mandated. With a mandate, firms must accommodate requests that they otherwise would decline. For example, industries with low coordination needs, such as the service sector, or with seasonal demand peaks, may benefit more from flexible part-time arrangements (Euwals and Hogerbrugge, 2006; Hagemann et al., 1994).

Adjustments to the part-time right would primarily be observable if the mandate substantially increased the number of part-time requests or if firms previously had strong incentives to deny such requests. Therefore, the empirical analysis tests whether firms near the policy threshold exhibit systematic changes in employment structures, hiring, or organizational outcomes following the introduction of the right to work part-time.

4 Data

I use administrative panel data from Germany, the establishment history panel (Betriebs-Historik-Panel, BHP v7523; Ganzer, Schmucker and Wolter (2024*a*)), constructed by the IAB. The BHP is a 50% random sample of German establishments with at least one employee subject to social security contributions. It is based on social security records and provides annual information. It covers the period 1975-2020. For the purposes of this study, I restrict the sample to 1995-2005 to exclude other reforms that might have influenced part-time work, parental employment and establishments. The annual information is always based on June 30 of the respective year.[1] For simplicity, I refer to establishments as firms throughout the paper, as they are identical in more than 90% of cases (Ganzer et al., 2025).

The BHP contains rich information on firm size, workforce composition, hires and separations, and firms' payrolls. In line with the mandate's legal definition, I measure firm size as the number of employees, excluding apprentices and trainees. This definition ensures that the treatment assignment corresponds precisely to the scope of the law.

To analyze the mandate's mechanisms, I additionally use the LIAB dataset (Ganzer et al., 2025). It combines the Establishment Panel (BP), a yearly representative survey of German establishments with administrative data from the IAB (incl. the

[1] Ganzer, Schmucker and Wolter (2024*b*) provide a detailed description of the data.

BHP). The LIAB allows me to investigate how firms assess their economic situation and whether they have staffing problems. However, only up to 15,000 firms are surveyed per year in the BP, decreasing my sample size. Therefore, I only use the LIAB for additional analyses.

The main outcomes include the share of employees working part-time, the share of female employees, the hiring rate, and business volume. They are measured at the firm-year level.

Employees are defined as all employees working at a firm on June 30 of a given year. It does not include paused employments (e.g., parental leave), but since 1999, it includes marginally employed individuals. *Part-time work* is identified using the occupation code firms report in the social security records. The classification defines part-time as any contractually agreed working time that is less than the standard working time specified in collective agreements or company regulations, typically fewer than 38 hours per week. Actual hours worked are not reported in the data. Part-time employment is underreported in the BHP because firms do not report all switches from full- to part-time work to social security.[2] Importantly, part-time work does not include marginal employment or partial retirement.

Based on these definitions, I define my outcome variables as follows: The *part-time share* is the number of part-time employees divided by total employment in the firm. The *female share* is the number of women employed relative to total employment. The *hiring rate* is the number of new hires, scaled by total employment. The *female hiring share* and *part-time hiring share* are defined by dividing the number of new female or new part-time hires by total new hires. The *separation rate* is measured as the number of employees leaving the firm (for any reason) relative to total employment. In addition, I observe each firm's *business volume*. For about 80% of firms this refers to revenues (in euro). Moreover, the daily *payroll* contains the sum of imputed gross average daily wages of all employees. In the analysis, I use the logarithm of these two outcomes. For firm closures I use a distinct sample. In the main sample, treatment is defined by having between 11 and 20 employees in 2000, implying that firms cannot exit before 2000. In this firm closure sample,

[2]To address this measurement concern, I re-estimate the main specification using the Establishment Panel. This allows me to examine both the administrative and self-reported part-time share for the same firms, confirming that the underreporting does not affect the results (see Appendix C2).

firms are allowed to exit before 2005. *Firm Closures* is an indicator for whether the firm exits the BHP panel in the subsequent year.

The main sample includes firms in the private sector with between 11 and 20 employees in 2000. Firms with 11–15 employees in 2000 form the control group, and firms with 16–20 employees form the treatment group. This sample restriction ensures comparability across firms just below and just above the 16-employee threshold, while excluding very small or substantially larger firms, whose dynamics may differ. I further exclude firms with missing or implausible values for the key outcome variables. I observe around 49,000 firms per year.

Table A1 provides descriptive statistics, including standardized mean differences (SMD) between the control and treatment group for my analysis sample. Firms are on average 17 years old, with treated firms being older. By construction, treated firms have on average five more employees. When only considering full-time equivalent employees (FTE) the difference reduces to four employees. Consequently, treated firms have a higher payroll than control firms. Treated firms are slightly less likely to be located in East Germany. Additionally, they have a lower part-time and female employment share before the reform than control firms. Figure A1 plots yearly means of the main outcomes. According to these figures, the treatment and control group develop in parallel both before and after the reform.[3] In sum, these statistics suggest that there are differences between the treatment and control group. Yet, considering economic relevance and SMD, these differences are rather small except for those, that stem from the construction of the two groups. For the main outcomes, the differences develop in parallel over time.

5 Empirical Strategy

Identifying the effects of a part-time mandate on firms involves several endogeneity issues. First, firms that do not expect negative consequences are more likely to offer part-time work than firms anticipating problems when hiring part-time workers. Second, workers who want to work part-time may select into firms and

[3]The drop in part-time share in 2003 for both groups can be explained by reform targeting marginal jobs (minijobs).

occupations that allow part-time work. The introduction of the part-time work mandate in 2001 allows me to overcome these identification issues because it exogenously mandated firms to allow part-time work. As long as a firm does not react to the mandate by reducing its workforce below the 16-employee cutoff and there are no unobserved characteristics of firms of different sizes, I can identify causal effects.

One approach would be to exploit the 16-employee threshold in a regression discontinuity design (RDD). The idea is to analyze the mandate's effects for firms with fewer than 16 employees (control group) and firms with 16 or more employees (treatment group). However, a cross-sectional RDD alone is insufficient here for two reasons. First, firm size is not randomly assigned: firms just above 16 employees tend to be older, are more likely to operate in certain sectors, and follow different growth trajectories than slightly smaller firms. These pre-existing differences may generate discontinuities in outcomes unrelated to the mandate. Second, firm size may itself respond to the mandate if employers reduce their workforce to fall below the threshold and avoid compliance, making the running variable endogenous. A Difference-in-Discontinuities design (Diff-in-Disc) addresses both concerns by using the discontinuity observed before the reform as a control. The identifying variation does not come from the level difference at the cutoff, but from the change in that difference after 2001. Any time-invariant characteristics of firms on either side of the threshold, including pre-existing differences in age, sector composition, or growth, are differenced out, leaving only the variation attributable to the mandate itself.

Estimating the Diff-in-Disc, I follow Grembi, Nannicini and Troiano (2016) and Picchetti, Pinto and Shinoki (2024). Treatment status is assigned based on firm size in the year 2000, measured as the number of employees excluding excluding apprentices and trainees. To estimate the effects among comparable firms, the treatment group is defined as firms with 16–20 employees in 2000, and the control group is defined as firms with 11–15 employees. Defining treatment status based on pre-reform size fixes treatment assignment over time and avoids endogenous reclassification of firms across the threshold in response to the mandate, even though firms and workers may still transition across the threshold over time.[4] I

[4]In a robustness check, I allow the treatment indicator to vary over time, including potential

track both groups over the period 1995 to 2005 to estimate the mandate's dynamic effects.

I estimate the following Diff-in-Disc regression:

$$Y_{it} = \beta_0 + \beta_1 D_i + \beta_2 Post_t + \beta_3 D_i Post_t + FirmSize_i(\beta_4 + \beta_5 D_i + \beta_6 Post_t + \beta_7 D_i Post_t) + \varepsilon_{it}, \quad (1)$$

where Y_{it} is the outcome of firm i in year t, D_i is an indicator of firms with 16–20 employees in 2000, and $Post_t$ indicates whether an observation occurred after the reform (i.e., after 2000). $FirmSize_i$ is the running variable measured as the number of employees in 2000 minus 16. The coefficient of interest is β_3. The interactions with $FirmSize_i$ allow the slopes to vary arbitrarily on each side of the firm size cutoff, as well as before and after the reform. Standard errors are clustered at the firm level.

Thus, this specification compares the evolution of outcomes in firms just above the threshold (16–20 employees) to those just below (11–15 employees), both before and after the reform. By applying a triangular kernel, firms close to the cutoff receive higher weights. I fix the treatment status in 2000, ignoring whether firms grow or shrink and manipulate their firm size due to the reform. Thereby, I identify an intention-to-treat (ITT) effect. As more firms grow, and I cannot observe whether a firm receives (or denies) part-time work requests, the ITT estimate will be smaller than the average treatment effect on the treated. Additionally, the Diff-in-Disc design estimates the local average treatment effect on the treated (LATT). This implies that the treatment effect is estimated locally at the cutoff and only reveals the treatment's effect on the treated group.

Several assumptions must be met to estimate causal effects with a Diff-in-Disc specification. First, firms should *not anticipate* the reform and adjust to it in advance. Since the mandate was first discussed in summer 2000, decided in December 2000, and the data for 2000 stem from June, this is unlikely to be the case. Second, the *stable unit treatment value assumption* requires no spillover effects. If untreated firms are more willing to offer part-time work to employees after the reform, this assumption would be violated. However, graphical (Figure A1) and survey evidence (presented in Section 6.2) reveals that untreated firms are less

endogenous transitions. This confirms my main results (see Appendix C8).

likely to grant part-time offers and also feel less affected by the part-time right. Therefore, spillovers are unlikely to drive my results. Third, the *parallel trends assumption* must be fulfilled. To assess this, I estimate an event-study specification. Specifically, I replace the post-reform indicator in Equation (1) with a full set of year dummies interacted with the treatment indicator. The resulting coefficients indicate how outcomes for treated firms changed compared to the control group in each year, relative to the base year of 2000. Additionally, the event study estimates allow me to capture the dynamic adjustment process after the reform, revealing whether effects occur immediately, grow gradually, or fade over time. The plots verify that the pre-trends of the control and treatment evolve in parallel before the reform. Moreover, the fact that no other policy changed in 2001, provides evidence for the parallel trends assumption.[5]

Finally, the running variable (number of employees) must be *continuous* around the cutoff. In this setting, firms might reduce their workforce to 15 employees or fewer, to avoid complying with the part-time law. I find no evidence of such bunching behavior (see Appendix B). Firm size distributions are smooth around the cutoff both before and after the reform, and McCrary density tests detect no discontinuity (Figure B1). As discussed in Section 2, there are no other firm-size dependent regulations affecting my main sample.

Given that this paper reports null effects, it is important to verify that the design is sufficiently powered to detect economically meaningful distortions. Having approximately 49,000 firms in the main sample and precisely estimated coefficients as documented in Section 6.1, speaks directly to this concern.

6 Results

6.1 Baseline Results

I first examine whether the obligation to grant part-time requests induces more part-time work, changes hiring margins or lowers downstream firm outcomes.

Panel (a) of Figure 1 plots event-study estimates for the firm-level part-time share. The coefficients are close to zero and statistically insignificant before the

[5] Please note that covariate balance is not a necessary condition for the Diff-in-Disc estimation.

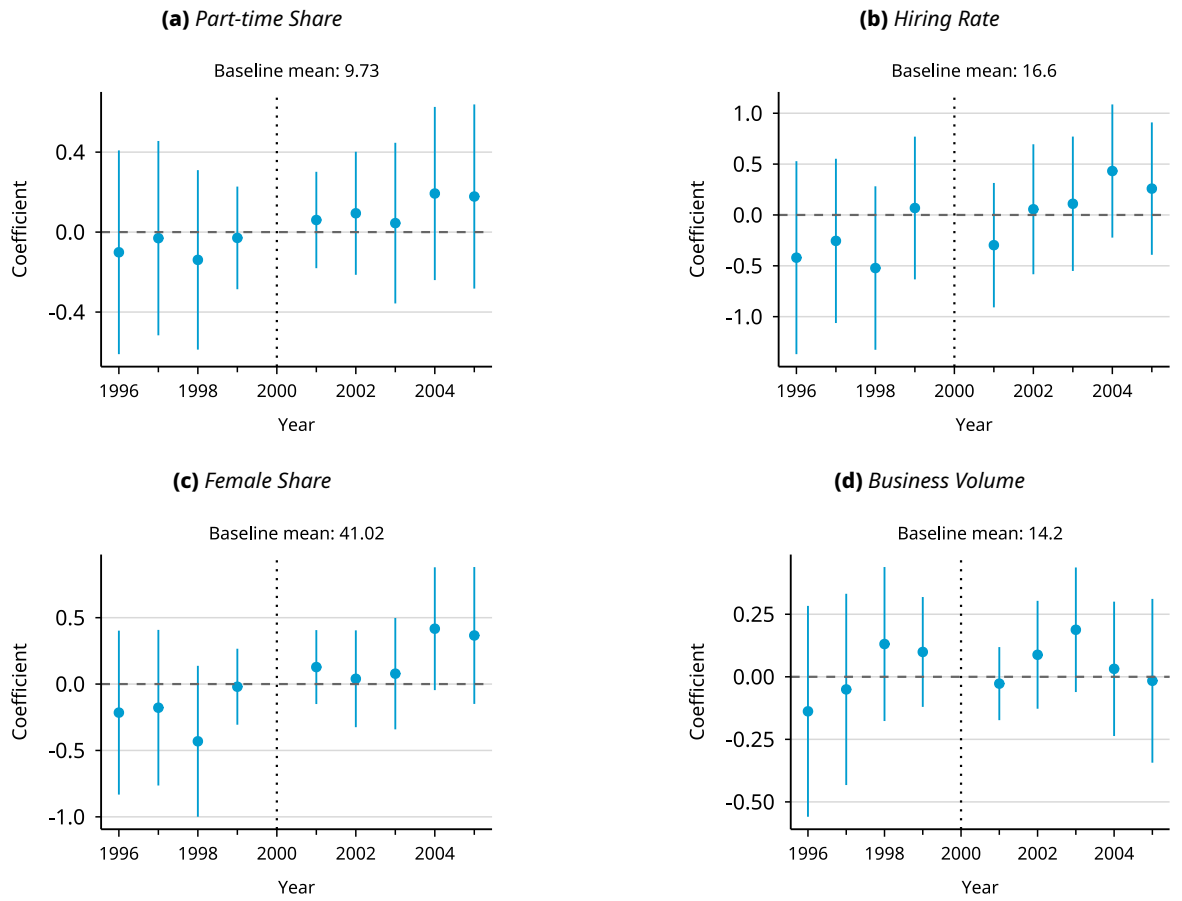
reform, consistent with parallel trends. Following the reform, the estimates are tightly centered around zero, ranging from -0.02 to 0.06 percentage points. The 95% confidence intervals, spanning not more than 0.4 percentage points, rule out any effect larger than 3.8% relative to the pre-reform mean. This is well below the magnitudes documented for comparable size-dependent regulations in other institutional settings. Overall, these results provide no evidence that the right to work part-time led to a systematic change in the part-time share at the firm level. This raises the question of whether part-time work did not increase due to the mandate or if firms adjust on different margins, such as by modifying their employee composition.

Panel (b) of Figure 1 shows that firms' general hiring behavior remains unchanged. The point estimates for the overall hiring rate range from 0.1 to 0.2 percentage points, ruling out changes larger than 6% relative to the pre-reform mean of 16.6%. For an average firm in the sample, the upper bound corresponds to fewer than one additional hire per year. Simultaneously, there is an expansion in part-time hiring by treated firms after the reform. However, this effect is not statistically significant (Figure C1, Panel (a)). Finally, firms may lay off workers who want to work part-time. However, there is no effect on employee exits (Figure C1, Panel (c)), which may be due to the high employment protection standards in Germany.

Still, there might be an effect on which individuals firms hire. Because mainly women work part-time in Germany, firms might employ fewer women to avoid additional part-time workers. I do not find evidence that the female share decreases in treated firms after the reform (Figure 1, Panel (c)). Coefficients range from 0.04 to 0.7 percentage points, ruling out changes larger than 3.7% relative to the pre-reform mean of 41%. Moreover, the female hiring share remains stable and statistically indistinguishable from zero effects throughout the post-reform period (Figure C1, Panel (b)). This provides direct evidence against discriminatory hiring responses.

Finally, I find no evidence that the mandate affects firm-level outcomes. Panel (d) of Figure 1 shows that the business volume did not significantly change after the reform. Moreover, the total payroll stays the same (Figure C1, Panel (d)). In 2002, the likelihood of firm closures marginally rises by 0.01 percentage points

Figure 1: *Event Study Plots – Difference-in-Discontinuities*



Notes: The figure reports the event-study coefficients of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level. See the corresponding Table C1.

(Figure C1, Panel (e)). However, given that the reform did not lead to changes in firms' part-time shares or hiring behavior, this is unlikely due to the part-time law. In the absence of adjustments in employment composition or labor input at the firm level, there is little reason to expect downstream effects on firm costs or performance.

Overall, the results imply that granting employees a legal right to part-time work did not alter firms' employment structure or behavior. On average, firms do not have more employees working part-time, and they do not change their general hiring behavior. Importantly, there is no indication of discriminatory responses against women. Consistent with these null effects, the mandate had no detectable impact on firms' business volume, payroll or firm survival. These results suggest that the part-time law did not materially change firms' labor demand or cost structures.

6.2 Mechanism: Pre-Existing Accommodation

The null effects documented in Section 6.1 raise a natural question: why did a legal mandate to grant part-time requests leave firm behavior unchanged? Survey evidence from the IAB Establishment Panel, collected three years after the reform, points to an answer. Firms were already accommodating part-time requests voluntarily before the reform took effect, meaning the mandate largely formalized existing practice rather than imposing genuinely new constraints.

To verify that the right to work part-time mainly affects treated firms, I examine transitions from full-time to part-time employment at the firm level using two different samples: The first is the main sample of the LIAB, as described in Section 4, consisting of approximately 700 firms. The second one relaxes the firm size restriction of the main sample and includes all firms with fewer than 100 employees to increase sample size (N $\sim$ 4,000). In the main sample, treated firms have a slightly higher share of full-time workers switching to part-time compared to untreated firms (7.7% versus 6.8%). This pattern becomes more pronounced when considering the broader sample. Again, treated firms report a higher incidence of full-time to part-time switches (8.1%) than untreated firms (3.6%). The widening gap is driven by a change in the control group. Untreated firms, not bound by the mandate, did not accommodate part-time requests at the same rate. This is consistent with the reform affecting the margin of worker demand rather than firm supply of part-time arrangements.

Furthermore, firms overwhelmingly state that they would have granted part-time requests even in the absence of the legal entitlement. Almost no firms denied part-time requests. Only 2.4% of treated firms and 1.7% of untreated firms expected staffing difficulties as a result of the reform. These shares are small in absolute terms and only modestly different across groups. This pattern suggests that, for most firms near the threshold, the mandate did not bind: the legal right to part-time work arrived in a context where granting part-time work to employees was already common. This result contrasts with the concern that part-time workers impose significant coordination or productivity burdens on firms. The lack of friction suggests that the mandate arrived in a context where informal accommodation was already common, rather than forcing firms to absorb new costs.

Taken together, this survey evidence supports the interpretation that the part-time mandate raised workers' formal security without requiring firms to fundamentally alter their employment practices. The mandate shifted the institutional basis for granting part-time work from employer discretion to employee entitlement. However, it left the actual incidence of part-time work at the firm level largely unchanged. This also explains why the mandate did not trigger the downstream firm-level adjustments that theory would predict if the mandate were genuinely binding, such as discriminatory hiring or business volume reductions.

6.3 Robustness Checks

I conduct a series of robustness checks to assess whether the main findings are sensitive to modeling choices, or bandwidth selection (Figure 2). I additionally estimate a post-reform RDD at the 16-employee cutoff. Consistent with the baseline Diff-in-Disc results, the RDD estimates confirm no discontinuous changes at the threshold in part-time employment, hiring rate, or female share (Figure C3).

First, I estimate a standard DID specification that compares treated and control firms before and after the reform.[6] Figure C4 shows that, unlike the Diff-in-Disc design, the DID approach shows pre-trend violations, suggesting that firms on either side of the threshold followed different trends prior to the reform. This pattern motivates the Diff-in-Disc approach, which removes such differential trends.

Second, the estimates are stable across alternative bandwidth choices. Coefficients remain close to zero and within the confidence interval of the baseline specification.

Third, I introduce a placebo reform at the 26-employee cutoff (Figure C6). This placebo reform does not yield discontinuities, confirming that the estimated treatment effects are not artifacts of unrelated firm-size dynamics.[7]

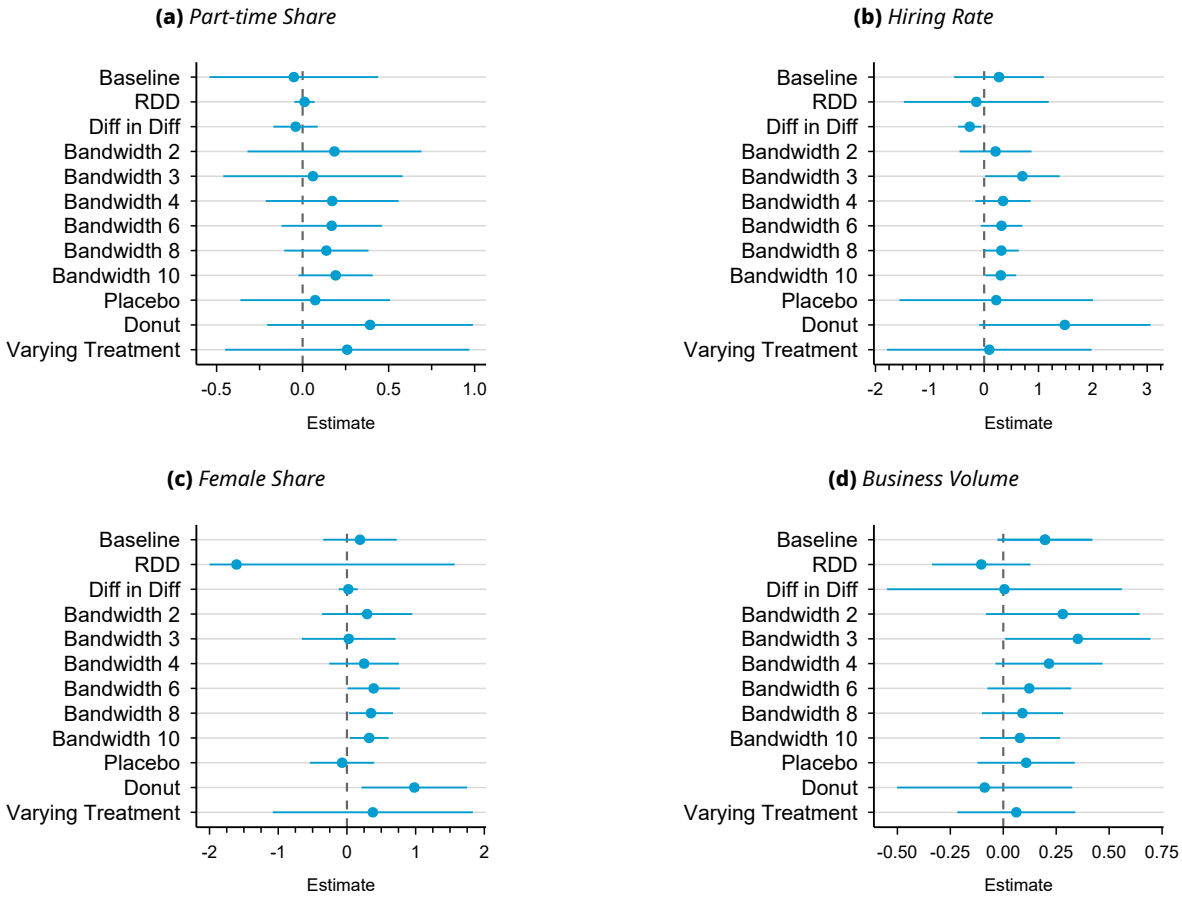
Fourth, I perform a donut estimation (Figure C7) to rule out potential measurement error in the running variable. Excluding firms with 15 and 16 employees does not alter the estimates for part-time share and hiring rate.

Fifth, by fixing the treatment status in 2000, firms who have 16 or more em-

[6] This is Equation 1 excluding β_4, β_5, β_6 and β_7.

[7] As the employment protection reforms in 1996 and 1999 might have affected my control firms, I do not introduce a placebo reform in the pre-treatment period.

Figure 2: *Robustness Checks*



Notes: The figure reports the estimates the 2001 part-time reform in Germany based on different robustness specifications as described in this chapter. The baseline uses a Diff-in-Disc design, where treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level. The confidence interval for the RDD specification of female share is truncated at $[-2, 2]$ *for readability.*

ployees in 2001 or afterward are always defined as treated. I relax this restriction by defining the treatment status as having 16 or more employees in a given year (Figure C8). This allows firms to move in and out of treatment based on current employment rather than fixing at 2000 levels. However, this approach introduces an endogeneity concern and should therefore be interpreted with caution. The estimates confirm the robustness of my findings.

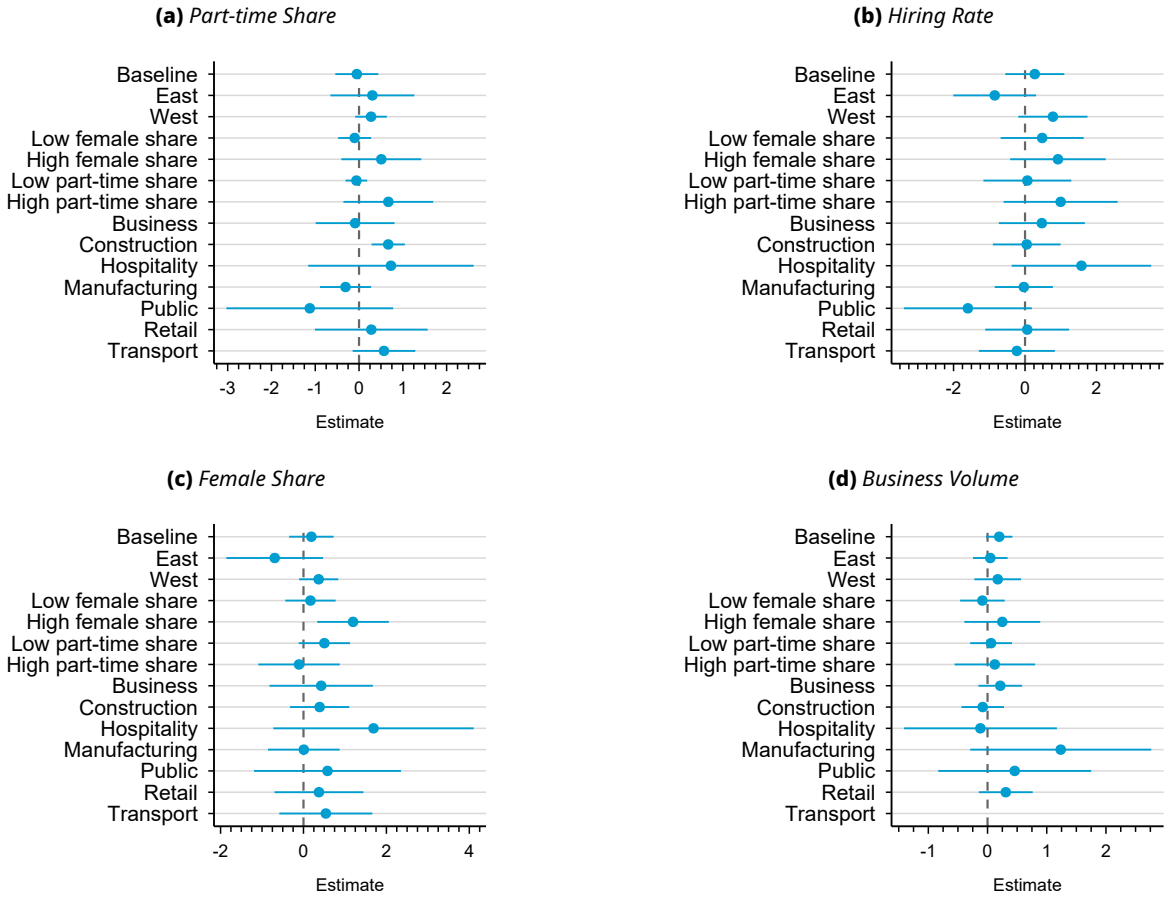
Overall, the robustness checks confirm that the estimated null effects are not driven by research design, bandwidth choice, or confounding policies. This consistent pattern of zeros for all outcomes strengthens the conclusion that the 2001 part-time mandate did not change firms' behavior or impose detectable costs on firms.

6.4 Heterogeneity Analysis

The effects of the right to work part-time may vary in several dimensions. Figure 3 presents an overview of the heterogeneities across those dimensions.
First, I analyze whether there are differences between *East and West Germany*. The economic situation, childcare availability, and gender norms vary widely between the two regions (Boelmann, Raute and Schönberg, 2025). Estimating the effects by region helps to assess whether the reform's impact on part-time take-up and employment varies in these contexts.

There are no substantial differences between East and West German firms. The hiring rate in West Germany slightly rises after the reform, while the female share slightly increases in East Germany. Overall, the coefficients for East German firms are comparable in size but have wider confidence intervals – driven by the smaller number of firms in East Germany.

Figure 3: *Heterogeneity Analysis*



Notes: Notes: The figure reports the estimates the 2001 part-time reform in Germany based on β_3 in Equation (1) for different subgroups. Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level.

Second, since mainly women work part-time, firms with a large female workforce may be more responsive to the mandate. Splitting the sample by the *pre-reform female share* helps determine whether the gender composition amplifies or mitigates the reform's effects. Firms in the lower quartile of the 2000 female share distribution (8% or below) are classified as low female-share firms, while those in the upper quartile (above 70%) are classified as high female-share firms. The part-time share slightly increases in firms with a low pre-reform female share, starting in 2003. However, these firms already exhibit pre-trends, therefore the results should be interpreted with caution. After the reform, the hiring rate rises by up to two percentage points for firms with a high pre-reform female share. In these firms, the female and part-time hiring shares slightly rise, suggesting that these hiring options drive the hiring rate. One might expect differences by the pre-reform part-time share. Firms with a high initial share may already have established part-time structures, implying limited scope for further adjustment (ceiling effects). Conversely, firms with a low share may face stronger constraints, making adjustments to the mandate more difficult. However, they also have more scope for an increase in part-time work. To assess this, I create subgroups with high and low part-time shares in 2000. The estimates show no differences between these groups, suggesting that pre-existing variation in part-time prevalence does not systematically mediate the reform effect.

Third, differences in production technology, shift requirements, and temporal flexibility across industries can influence the feasibility and cost of offering part-time work. By estimating *industry-specific* effects, I capture variation in adjustment channels that may otherwise be masked in aggregate analyses.
However, only one industry shows suggestive patterns. In the construction sector, the part-time share increases by around 0.6 percentage points after the reform. Considering the rather low part-time share in this sector before the reform, this increase is economically relevant. However, no other outcomes in the construction sector exhibit statistically significant effects.
Additional evidence stems from LIAB data: Treated firms that can predict production fluctuations are more likely to hire new part-time employees after the reform. Moreover, their female share increases, and they have a higher business volume (Table D1).

Finally, it might be that the mandate influenced larger firms more than smaller firms. Thus, I estimate size-specific treatment effects by interacting firm size in 2000 with a post indicator. Coefficients are expressed relative to firms with 16 employees, so that each estimate compares the before-after change for a given firm size to that of a 16-employee firm. This flexible specification allows the reform effect to vary across different firm sizes rather than imposing a binary treatment definition. The results show no systematic pattern across firm sizes, indicating that the reform's impact does not vary meaningfully along the firm size distribution.

Overall, the heterogeneity analysis underlines that the mandate's effects are concentrated in dimensions that align with underlying labor-force composition rather than institutional or structural characteristics of firms. Differences by firm size and between East and West Germany are limited and do not point to systematically different adjustment patterns. Firms with a high pre-reform female share exhibit stronger hiring responses, consistent with higher part-time employment following the reform. There is only modest sector heterogeneity, with some suggestive evidence of increased hiring in retail and higher part-time share in construction, but these effects are not widespread across industries. Taken together, these findings support the interpretation in which the part-time right primarily operates through firms' existing labor-force composition, rather than through sector-specific technologies or firm size.

7 Discussion and Conclusion

This paper is the first to provide firm-level evidence on Germany's 2001 part-time mandate. This mandate is not only permanent and predictable, but also allows me to investigate a policy that governs how workers are employed once hired. The setting is special as it analyzes a permanent and anticipated change in working hours. The main results indicate that the mandate did not alter small firms' employment structure or behavior. Furthermore, firm outcomes, such as business volume and firm closures, remain unaffected. These results are precisely estimated and surprising, given that small firms are expected to be most affected by part-time mandates, as they face higher costs per worker and have fewer internal substitutes.

Why did the mandate leave firm behavior largely unchanged? Survey evidence from the BP suggests that firms were already accommodating part-time requests voluntarily before 2001. Three years after the reform, treated firms report more transitions from full-time to part-time employment than untreated firms. However, most of them state that they would have granted part-time work even in the absence of the law and that they rarely denied employees' requests. Moreover, only a very small fraction of firms expected staffing issues due to the reform. Taken together, this suggests that the observed expansion in full-time to part-time transitions at treated firms is unlikely to be driven by changes in firm behavior. Rather, the mandate may have strengthened employees' willingness to request part-time work, without requiring firms to adjust their employment practices fundamentally.

Previous studies by Paul (2016) and Paule-Paludkiewicz (2024), analyzing the same mandate with individual level data, find that the reform indeed increased the share of mothers working part-time. The divergence from the zero effects of this paper might reflect differences in sample composition. Mothers with children below age six represent a small subset of the workforce; aggregate effects on firm-level part-time shares are necessarily smaller than effects on the subgroup of mothers. Additionally, neither restricts their sample to small firms with 11 to 20 employees. Finally, more mothers working part-time might shift the composition of part-time workers within firms without affecting the aggregate part-time share.

Other studies document that parental leave and part-time extensions affect firms. However, my results point to limited effects in Germany. This divergence highlights the role of institutional context and the breadth of the mandate. Analyzing the Spanish law for part-time work, Fernández-Kranz and Rodríguez-Planas (2021) find that it increased discriminatory hiring and female unemployment. Further studies such as Brenøe et al. (2024, 2025), Ginja, Karimi and Xiao (2023) and Corekcioglu, Francesconi and Kunze (2025) demonstrate that firms adjust employment when parental leave policies change.

Several factors may explain why firms do not react to the part-time law. First, firms could have already granted part-time work before the reform. Survey evidence confirms that firms do not refer to the law as a reason to grant part-time requests, supporting this hypothesis. Thus, employees' demand for part-time work was already satisfied before the reform, and it did not extend the amount of part-

time work in firms. This aligns with the findings of Nieuwenhuis, Postepska and Alessie (2026), who show that a comparable part-time mandate in the Netherlands did not affect hours worked. They attribute this finding to the fact that women who wished to reduce their working hours were already able to do so before the reform.

Second, there might be spillover effects to untreated firms. Although the mandate did not apply to firms with fewer than 16 employees, they sometimes offered part-time positions to avoid becoming less attractive employers. However, unemployment in Germany was high in the early 2000s, reducing the importance of employer attractiveness. Indeed, survey evidence shows that smaller firms were less likely to have workers switching from full- to part-time than larger firms.

Finally, the German labor market is rather rigid. In this context, employment protection, collective bargaining, and standardized working-time arrangements may have already limited firms' discretion prior to the reform. Thus, there was little room for additional adjustment once the right to part-time work was introduced.

A German study corroborates my finding that I do not detect any firm reactions. Considering a parental leave extension, Huebener et al. (2025) find small to null effects on firms. Their main result is the relevance of internal substitutes for firms' employment reactions. My data does not allow me to examine internal substitutes. It remains plausible that the right to work part-time mainly affected larger firms that have more internal substitutes available, but not analyzed in this paper.

The heterogeneity analysis further clarifies these patterns. Firm size does not systematically influence responses, and East–West differences are limited. Gender composition is the clearest margin of heterogeneity: firms with a high pre-reform female share exhibit stronger hiring responses, consistent with more part-time work after the reform. Sector-level results mirror this pattern, with modest increases in female- and part-time-intensive industries such as hospitality and retail. These findings suggest that the mandate primarily facilitated internal adjustments where strong pre-existing preferences for part-time work existed, while broader structural or institutional constraints limited responses elsewhere.

Together, this reinforces the interpretation that the 2001 part-time mandate did not fundamentally alter small firms' behavior. The absence of bunching, lay-

offs, or business volume distortions indicates that the policy did not generate unintended adverse consequences. At the same time, the part-time share at the firm level did not significantly rise, raising questions about whether such mandates are suited to increase overall part-time work.

The analysis has several limitations. First, as noted, I cannot fully rule out any spillovers to untreated firms. Second, I observe realized part-time employment but not employees' requests or firms' denials, even though survey responses suggest that denials are rare. Third, the administrative data do not allow me to analyze whether firms' employees are on parental leave or parents and how many internal substitutes are available. Fourth, my results are limited to firms with 11 to 20 employees. As the paper by Paule-Paludkiewicz (2024) indicates, it might be that mainly larger firms reacted to the mandate. However, when examining firms with more than 20 employees, the comparability to my control group might not be fulfilled anymore. However, more than 70% of German firms have 20 or fewer employees, making it a relevant group to analyze. Fifth, external validity might be limited, as the German institutional setting regarding employment protection and parental leave is very specific and Germany has one of the highest part-time work shares of OECD countries.

Overall, the results suggest that Germany's 2001 part-time mandate had only limited consequences for small firms. While the mandate grants employees a new right, on average firm-level employment structures remain constant. This is driven by widespread existing part-time arrangements and rigid institutional frameworks. The absence of adverse firm reactions, such as layoffs, business volume reductions, or strategic bunching, underscores that permanent, predictable part-time mandates in institutional settings with high pre-existing part-time prevalence do not necessarily create distortions at the firm level. Thus, the mandate enhanced worker security without imposing firm-level distortions, because firms had already established informal part-time arrangements. However, the mandate's general effectiveness in increasing part-time employment remains unclear, as previous evidence only focused on mothers.

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Appendix

A Descriptive Statistics

Table A1: *Descriptive Statistics by Treatment Status for 2000 (BHP Sample)*

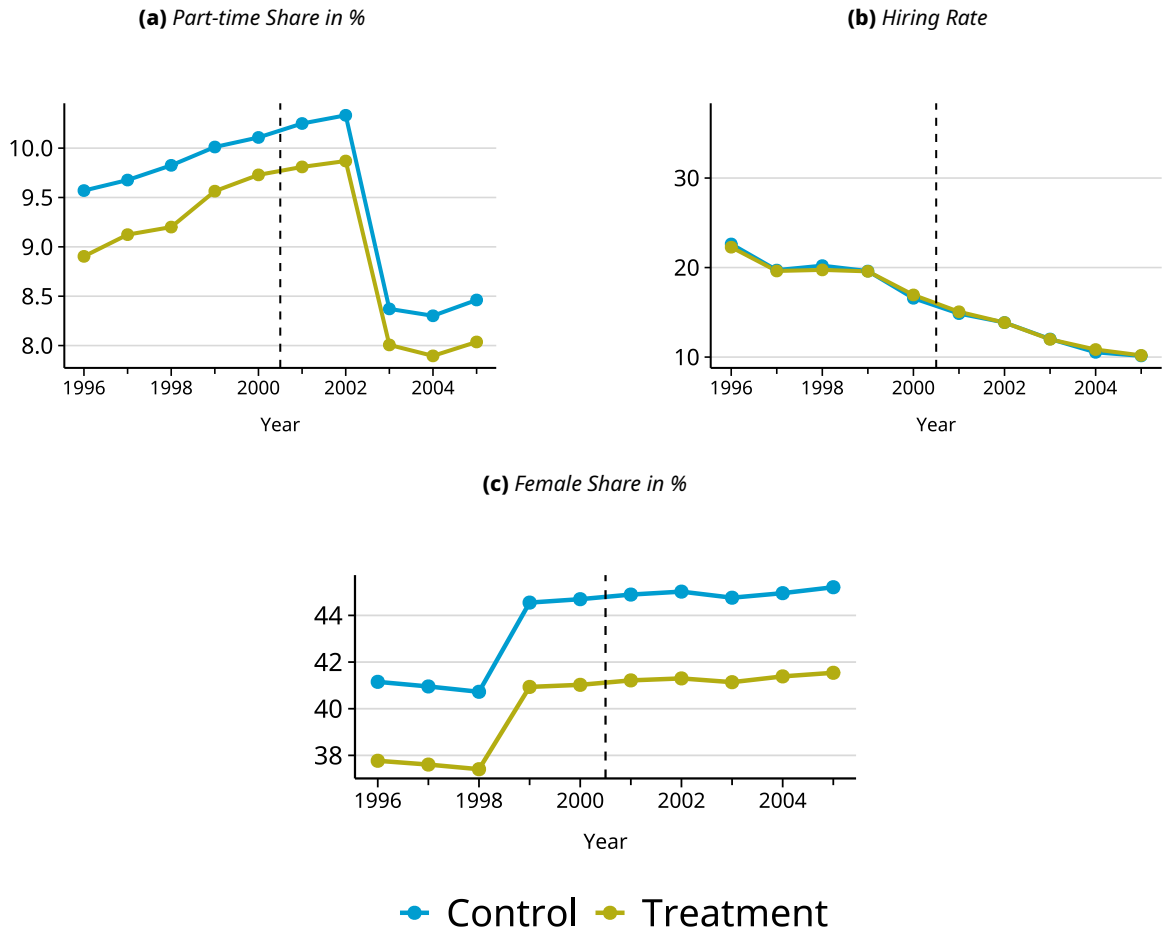
	Treatment Group		**Control Group**		
Variable	Mean	SD	Mean	SD	SMD
Firm Age	17.07	7.61	16.76	7.64	0.04
East Germany	0.14	0.35	0.15	0.36	-0.02
Employees	17.35	1.11	12.72	1.40	3.60
FTE	14.51	3.33	10.47	2.71	1.45
Part-time Share	9.79	14.13	10.11	14.46	-0.03
Hiring Rate	16.86	13.31	16.57	13.92	-0.02
Female Employment Share	41.47	28.79	44.70	29.95	-0.11
Female Hiring Share	39.69	36.60	40.61	39.48	-0.02
Part-time Hiring Share	8.22	18.50	8.20	20.07	0.00
Separation Rate	19.69	16.64	19.89	18.29	-0.01
Payroll (daily, in Euro)	1041.20	442.35	722.00	326.21	0.86
Observations	15,207		33,918		

Table A2: *Descriptive Statistics by Treatment Status for 2000 (LIAB Sample)*

	Treatment Group		**Control Group**		
Variable	Mean	SD	Mean	SD	SMD
Firm Age	13.40	8.43	12.18	7.94	0.15
East Germany	0.38	0.49	0.41	0.49	-0.05
Employees	17.89	1.40	12.95	1.42	3.50
FTE	16.05	3.49	11.46	3.59	1.29
Part-time Share	10.55	16.64	11.57	18.22	-0.06
Hiring Rate	17.37	15.12	18.06	16.53	-0.04
Separation Rate	23.53	28.73	23.76	33.03	-0.01
Payroll (monthly, in Euro)	36,532	21,799	26,200	16,871	0.60
Business Volume	5,314	45,100	2,134	5,745	0.09
Observations	700		808		

Notes: The value for business volume is reported in 1,000 €.

Figure A1: *Yearly Mean of Main Outcomes: Control vs. Treatment Group*



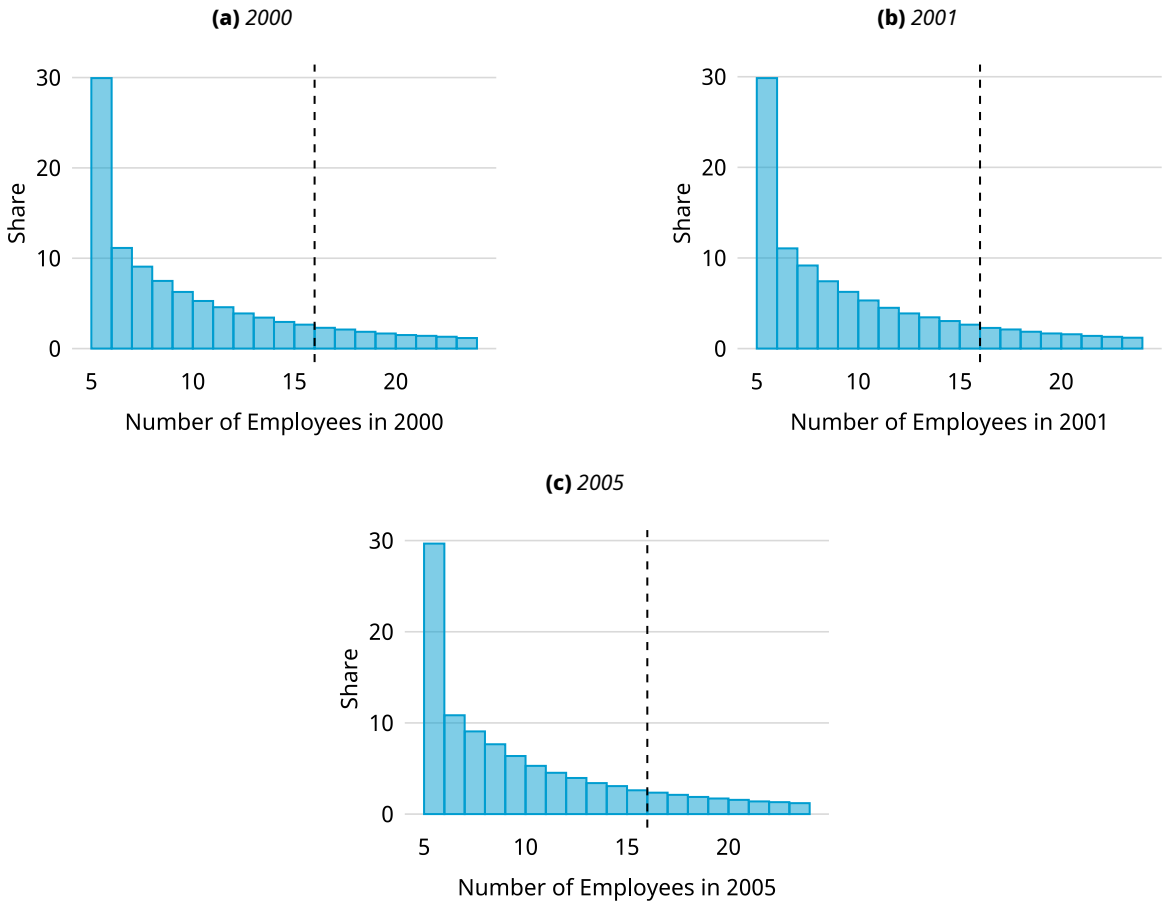
Notes: Each panel presents the yearly mean of the outcome by treatment status. The control group has 11-15, the treatment group 16-20 employees in 2000.

B Bunching

A central concern is that firms might react to the reform by reducing their workforce to 15 employees or fewer as discussed in Sections 3 and 5. This would create excess mass below the cutoff and threaten identification.

To assess this, I examine the workforce distribution around the cutoff. Figure B1 shows that it is smooth around 15 employees both before (2000) and after the reform (2001, 2005), with no excess mass below the cutoff. The McCrary density test also indicates no discontinuity at the cutoff.

Figure B1: *Histograms of the number of employees*

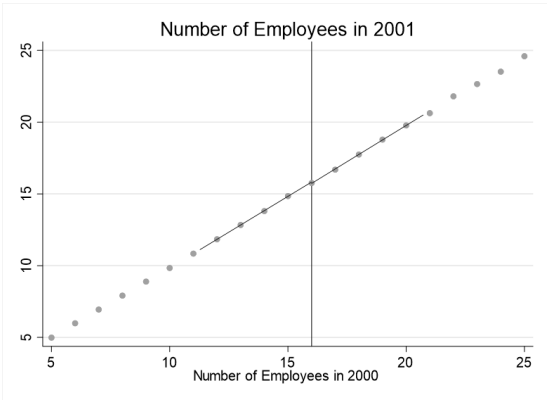


Additionally, I investigate the possibility of bunching using an RDD. Figure B2 further shows that firm size evolves smoothly around the threshold, with no drop just below 16 employees.

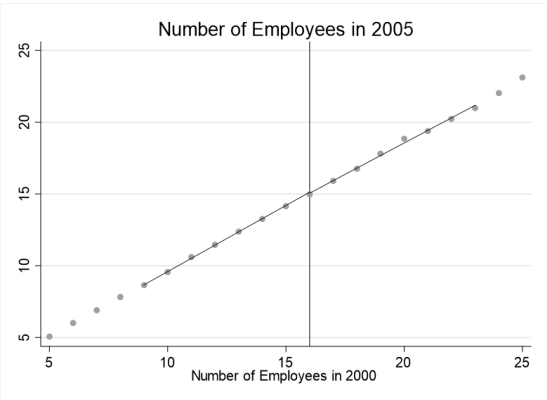
Overall, this indicates that firms did not systematically reduce their employment to avoid the part-time work regulation.

Figure B2: *RDD plots illustrating adjustments in the number of employees of treated firms*

(a) *2001*

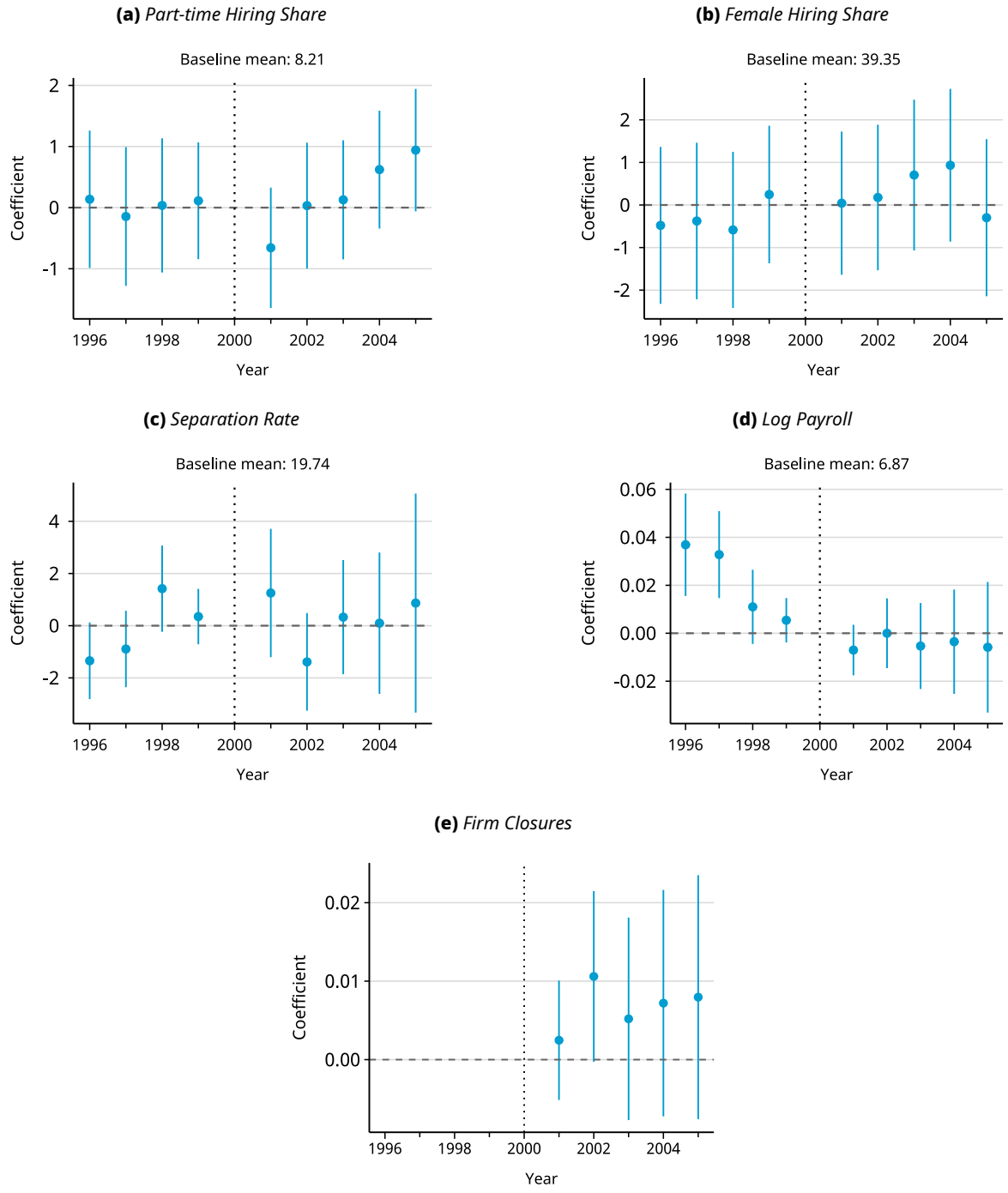


(b) *2005*



C Robustness Checks

Figure C1: *Event Study Plots – Secondary Outcomes*



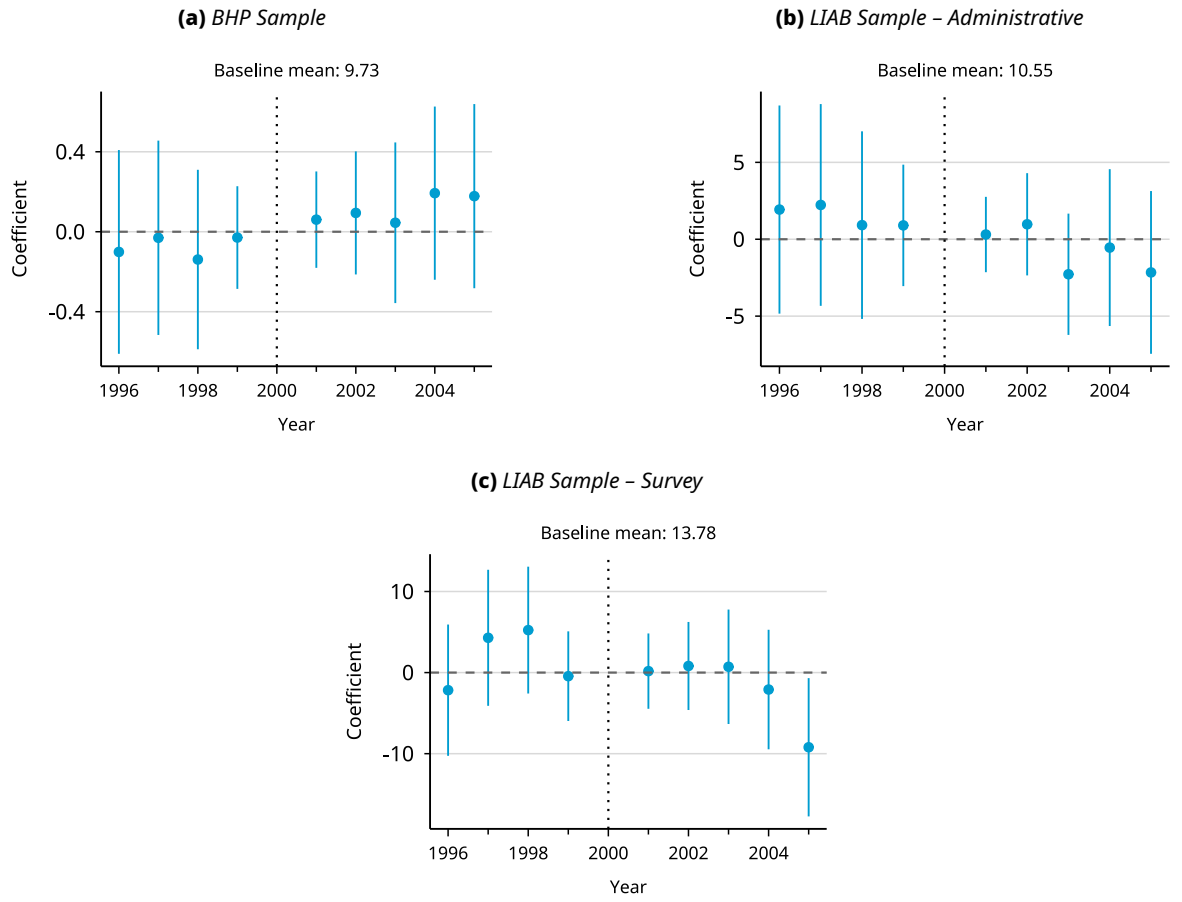
Notes: The figure reports the event-study coefficients of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level.

Table C1: *Difference-in-Discontinuities - Event Study Results*

Year	PT Share	Hiring Rate	Fem. Share	Bus. Vol.	Sep. Rate	PT HS	Fem. HS	Payroll	Firm Closure
1996	-0.317	0.255	-1.228	0.005	0.050	-0.484	0.053***	-0.138	
	(0.412)	(0.503)	(0.770)	(0.015)	(0.905)	(1.085)	(0.017)	(0.215)	
1997	-0.041	0.517	-0.766	0.010	0.181	-0.140	0.038***	-0.050	
	(0.392)	(0.477)	(0.653)	(0.015)	(0.912)	(1.196)	(0.015)	(0.195)	
1998	-0.169	0.007	-0.173	-0.008	-0.446	1.783	0.017	0.131	
	(0.364)	(0.462)	(0.652)	(0.015)	(0.887)	(1.349)	(0.013)	(0.157)	
1999	-0.029	0.044	0.017	0.005	0.193	0.894	0.013*	0.099	
	(0.209)	(0.232)	(0.565)	(0.013)	(0.768)	(0.834)	(0.008)	(0.112)	
2001	-0.036	0.181	0.079	0.001	-1.003	2.186	0.000	-0.027	0.002
	(0.195)	(0.226)	(0.494)	(0.013)	(0.788)	(1.995)	(0.009)	(0.074)	(0.004)
2002	-0.156	0.115	0.043	-0.002	-0.673	-1.700	0.006	0.088	0.011*
	(0.250)	(0.297)	(0.516)	(0.014)	(0.834)	(1.487)	(0.012)	(0.110)	(0.006)
2003	-0.163	0.236	0.677	0.010	0.028	0.591	0.010	0.188	0.005
	(0.327)	(0.341)	(0.533)	(0.014)	(0.788)	(1.829)	(0.015)	(0.127)	(0.007)
2004	0.040	0.191	0.077	0.005	0.343	1.923	-0.000	0.032	0.007
	(0.350)	(0.376)	(0.532)	(0.014)	(0.780)	(2.188)	(0.018)	(0.137)	(0.007)
2005	0.061	0.230	0.490	-0.005	1.127	1.538	-0.005	-0.016	0.008
	(0.373)	(0.420)	(0.525)	(0.015)	(0.808)	(3.393)	(0.022)	(0.167)	(0.008)

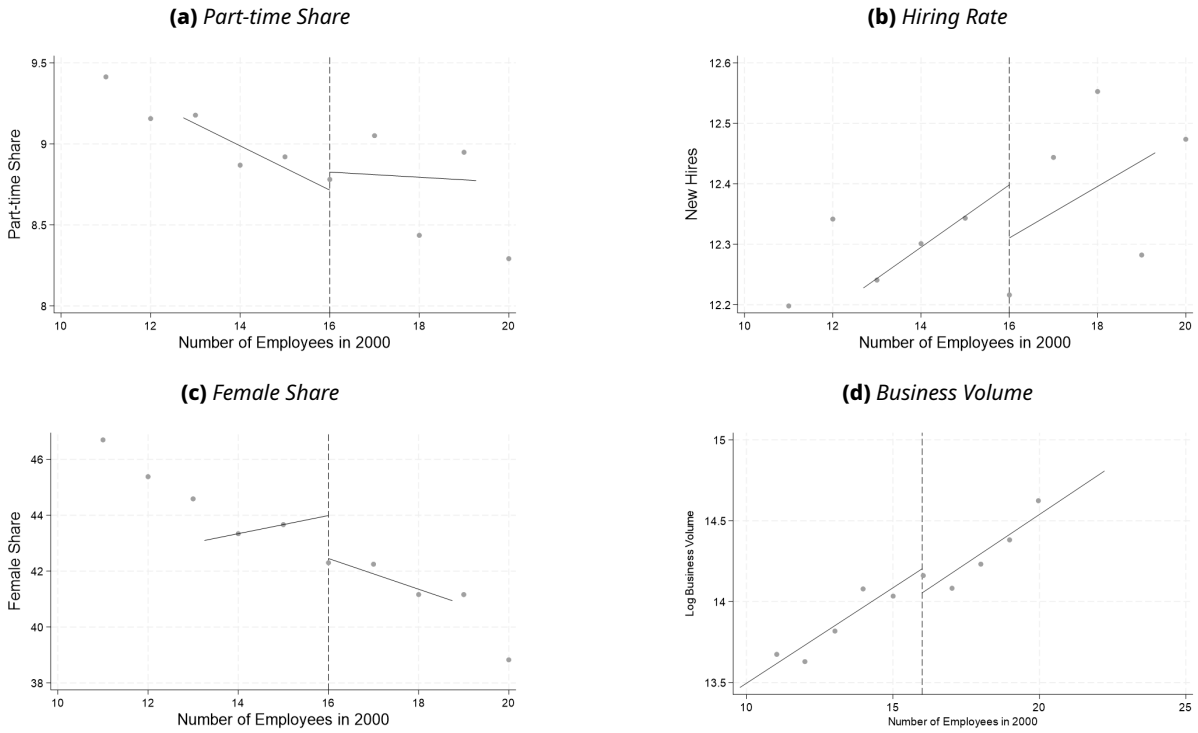
*Notes: The table reports the event-study coefficients of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. * p<0.1, ** p<0.05, *** p<0.01. Standard errors clustered at the firm level in parentheses.*

Figure C2: *Event Study Plots Using BP and BHP Data for Part-time Share*



Notes: The figure reports the event-study coefficients of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level. ***Panel (a)*** *shows the result for part-time share as presented in Figure 1. The part-time share is measured based on administrative data for the BHP sample (N = 258,310).* ***Panel (b)*** *shows the result for part-time share for the LIAB sample (N = 8,061). The part-time share is measured based on administrative data.* ***Panel (c)*** *shows the result for part-time share for the LIAB sample (N = 7,826). The part-time share is measured based on survey data.*

Figure C3: *RDD Plots*



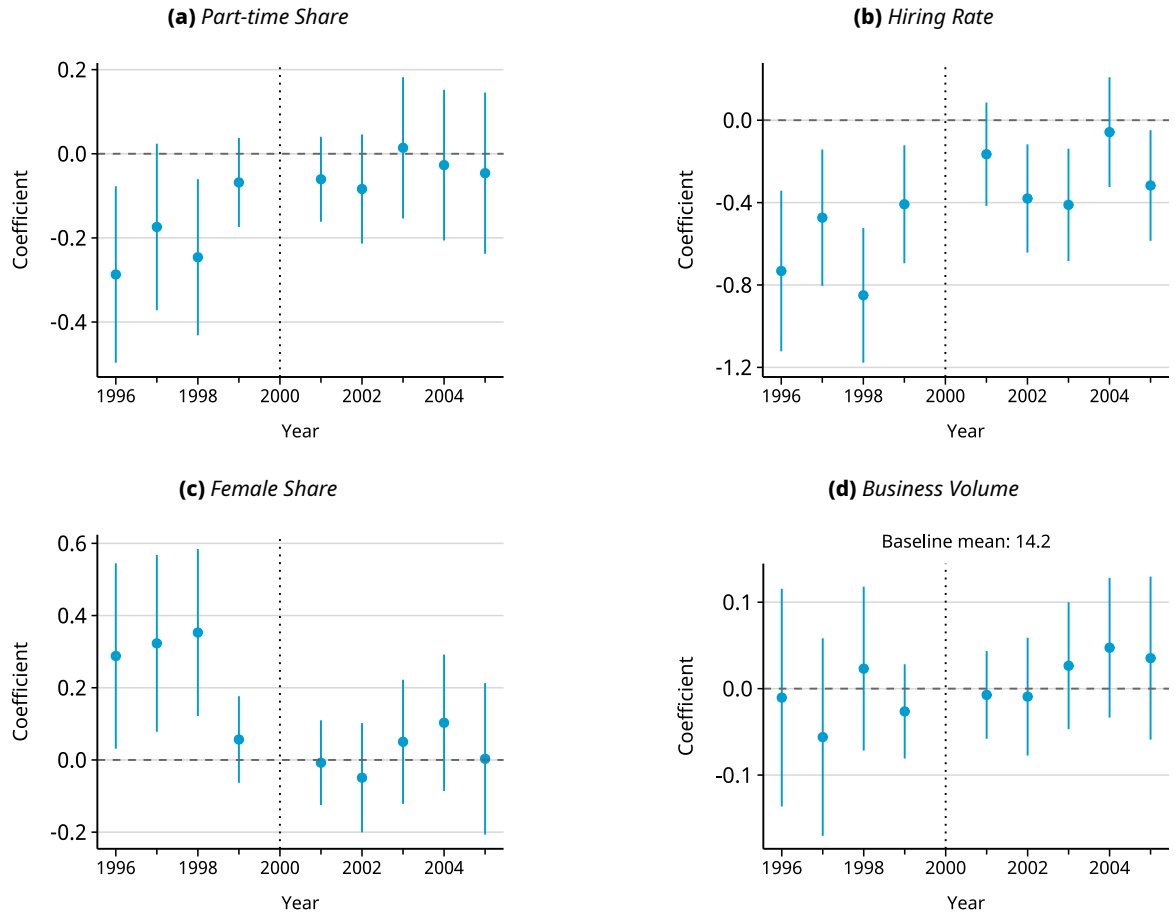
Notes: Each panel plots the RD of the 2001 part-time reform at the 16-employee cutoff. Each dot in the panels includes the percentage of firms with the corresponding outcome (y-axis) among firms with 11 to 20 employees. The x-axis shows the number of employees. Fit lines and RD estimate from a linear RD specification, using a triangular kernel and data-driven bandwidth selection, as implemented in the Stata package rdrobust (Calonico et al., 2026). See the corresponding Table C2.

Table C2: *RD Estimates*

	PT Share	Hiring Rate	Fem. Share	Bus. Vol.	PT HS	Fem. HS	Sep. Rate	Payroll
RD Estimate	0.0112	−0.145	−1.607	-0.10391	0.0684	−0.0182	−0.112	−0.0166
	(0.03)	(0.68)	(1.62)	(0.1185)	(0.25)	(1.85)	(0.15)	(1.02)
Obs. left	86,855	86,855	54,130	2,789	125,395	54,130	86,855	86,855
Obs. right	76,035	76,035	60,205	2,408	90,305	60,205	76,035	76,035
Bandwidth	3.265	3.303	2.745	6.218	4.064	2.846	3.451	3.259

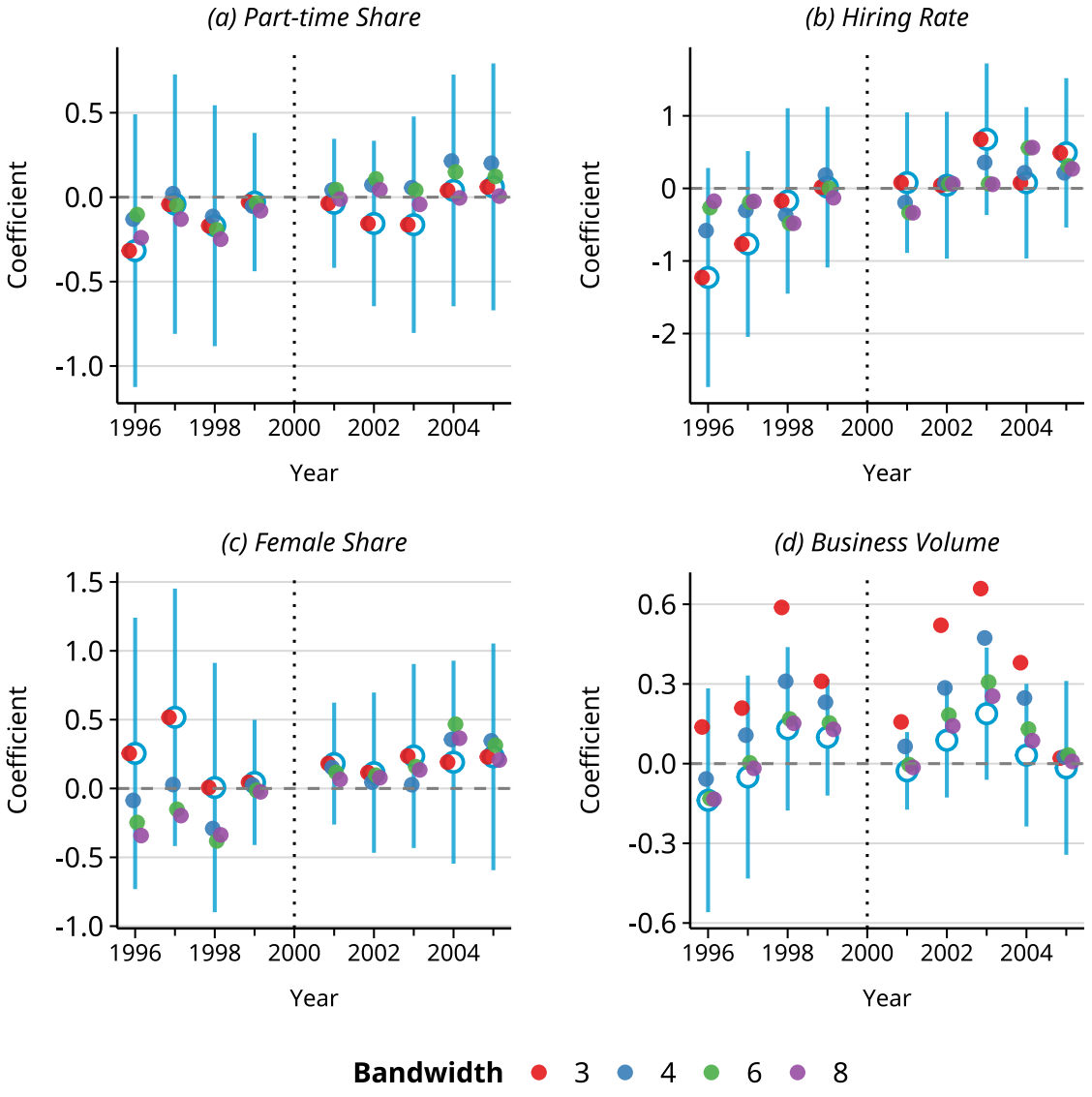
Notes: RD estimates of the 2001 part-time reform at the 16-employee cutoff. Linear RD specification using a triangular kernel and data-driven bandwidth selection, as implemented in the Stata package rdrobust. HS: Hiring Share. PT: Part-time.

Figure C4: *Event Study Plots – Difference-in-Differences*



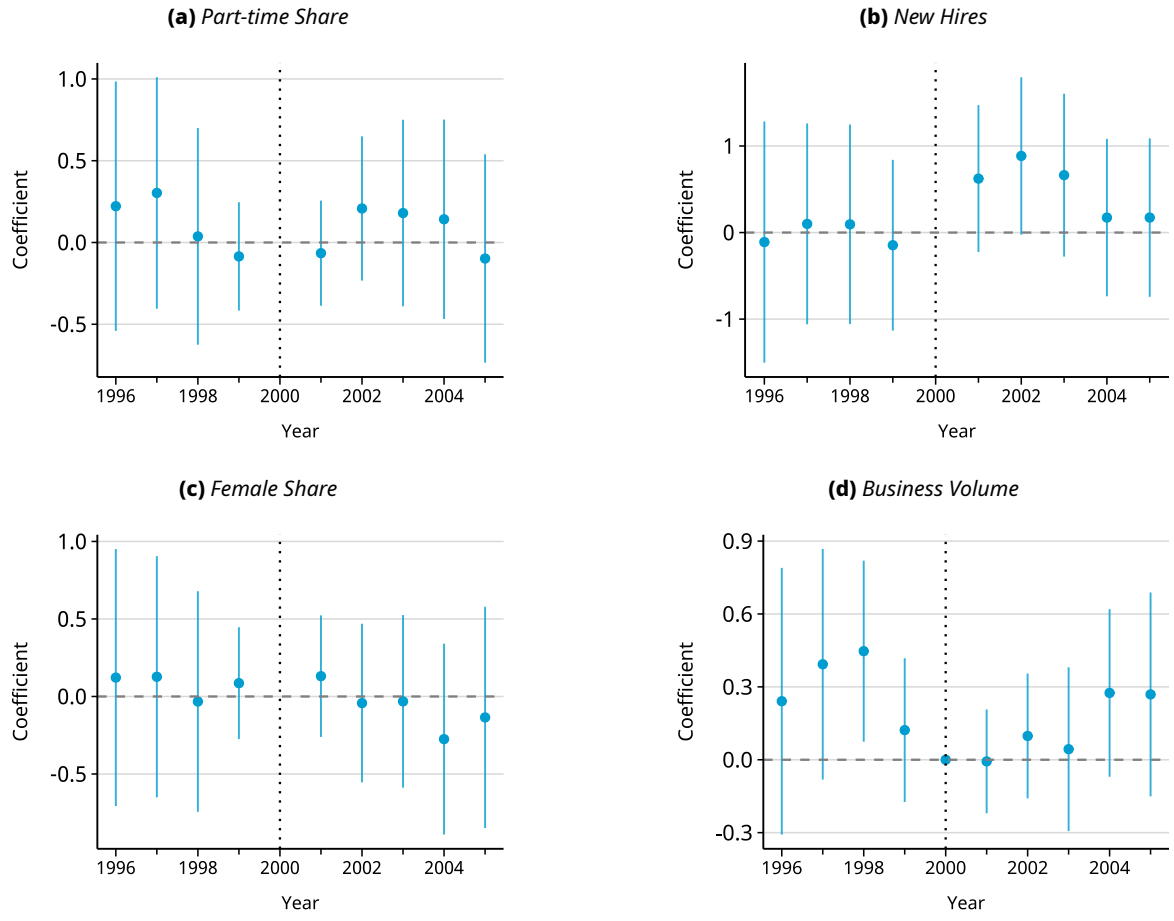
Notes: The figure reports the event-study coefficient of a difference-in-differences estimation of the 2001 part-time reform in Germany, interacting the treatment indicator with year dummies. Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals based on standard errors clustered at the firm level.

Figure C5: *Event Study Plots – Different Bandwidths*



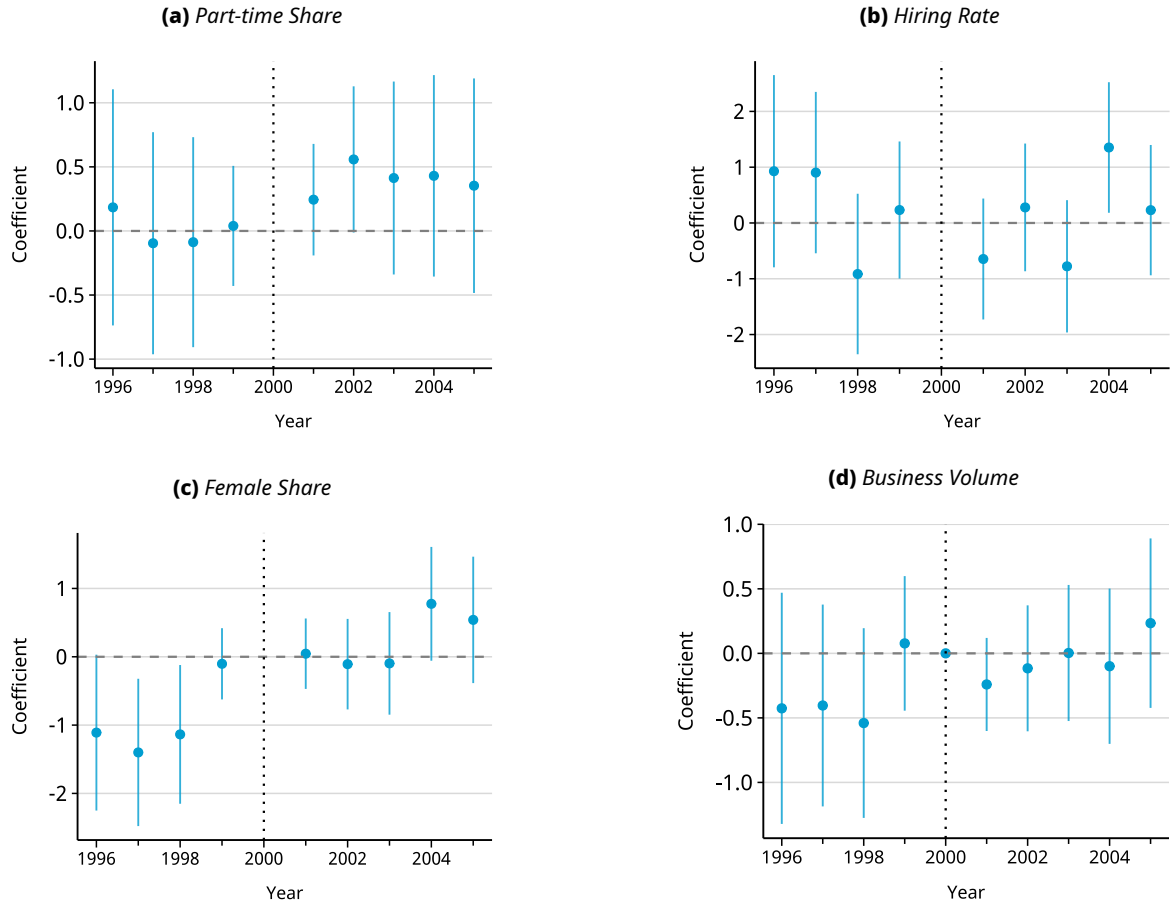
Notes: The figure reports the event-study coefficient of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees in 2000. The bars represent 95% confidence intervals for the bandwidth of 5 based on standard errors clustered at the firm level.

Figure C6: *Event Study Plots – Placebo at 26-Employee Cutoff*



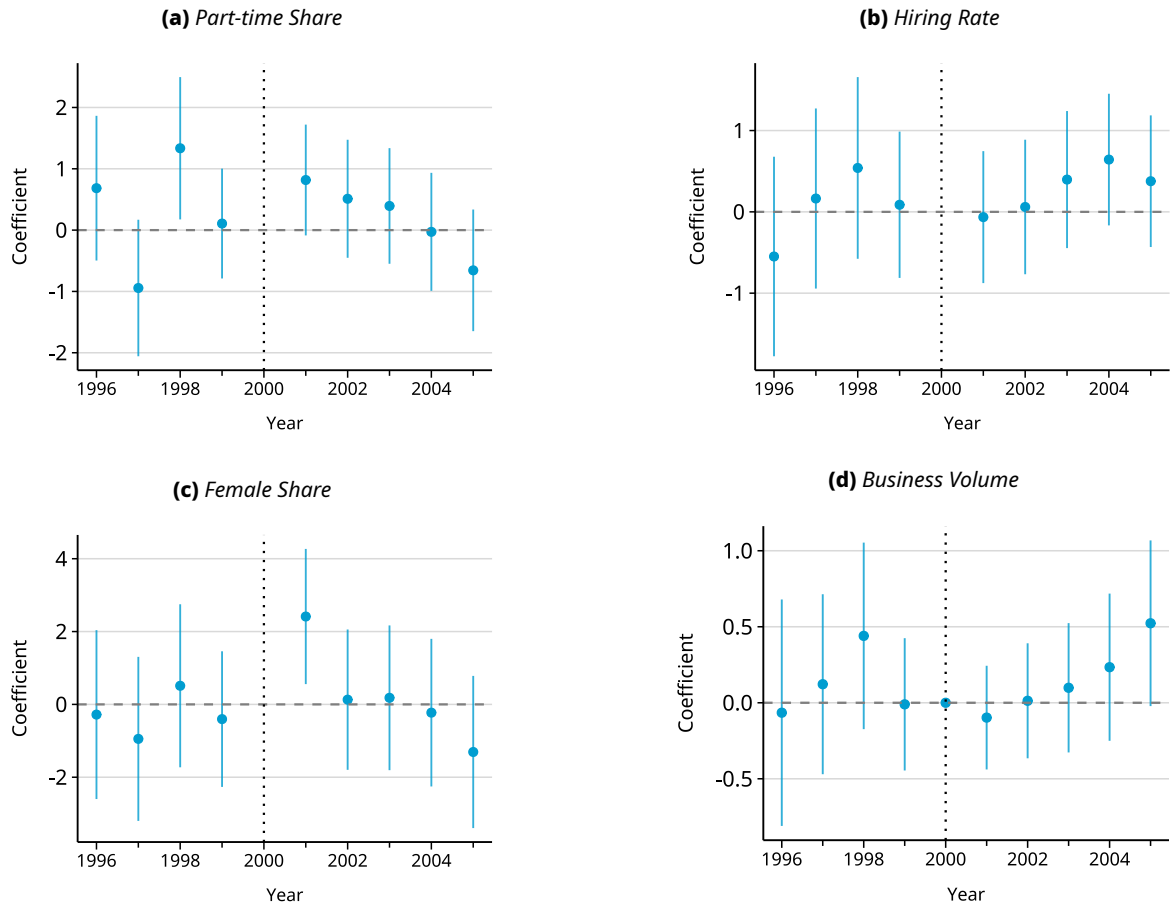
Notes: The figure reports the event-study coefficient of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 26-30 employees, while untreated firms have 21-25 employees in 2000. The bars represent 95% confidence intervals for the bandwidth of 5 based on standard errors clustered at the firm level.

Figure C7: *Event Study Plots – Donut*



Notes: The figure reports the event-study coefficient of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 17-20 employees, while untreated firms have 11-14 employees in 2000. The bars represent 95% confidence intervals for the bandwidth of 5 based on standard errors clustered at the firm level.

Figure C8: *Event Study Plots – Time Varying Treatment Status*



Notes: The figure reports the event-study coefficient of the 2001 part-time reform in Germany based on β_3 in Equation (1). Treated firms have 16-20 employees, while untreated firms have 11-15 employees. The bars represent 95% confidence intervals for the bandwidth of 5 based on standard errors clustered at the firm level.

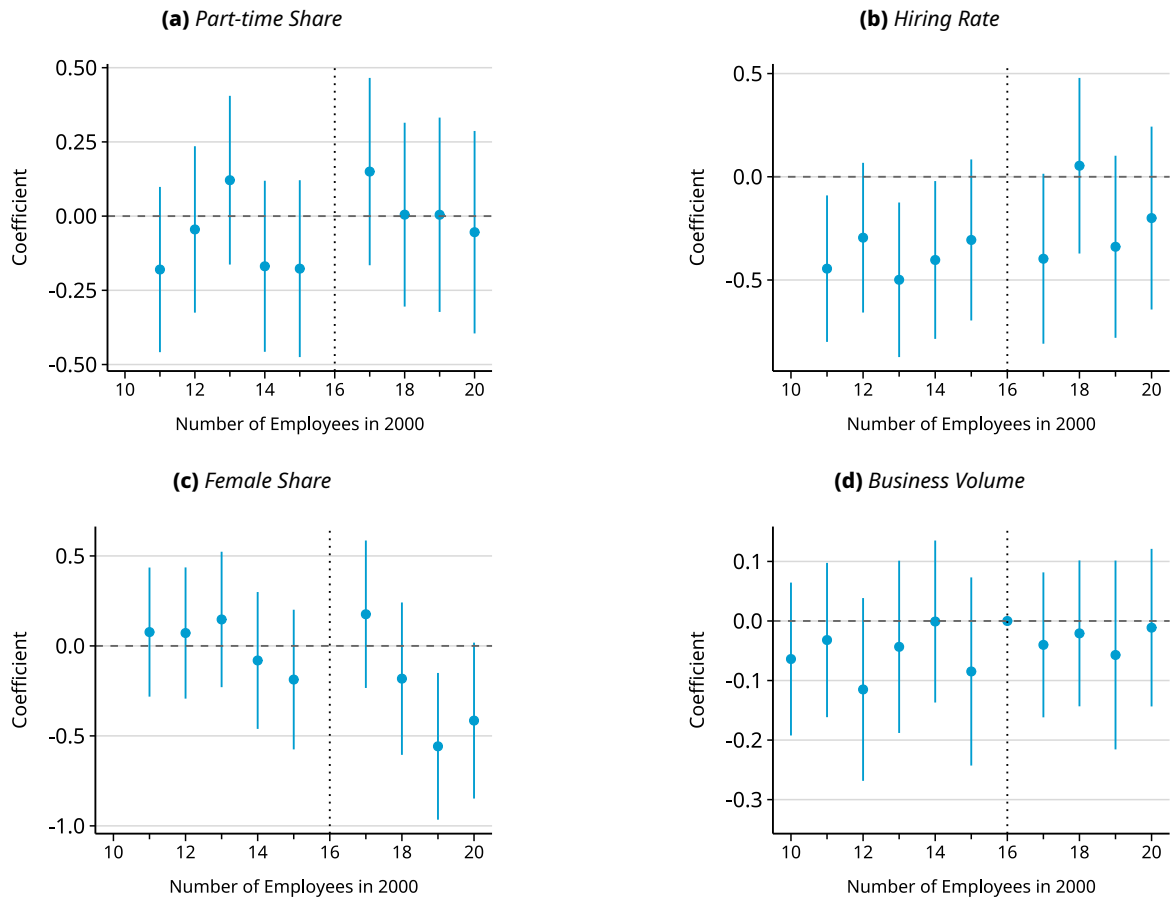
D Heterogeneity Analysis

Table D1: *Diff-in-Disc Results for Firms that can Predict Fluctuations in Production*

	PT Share	Hiring Rate	Fem. Share	Bus. Vol.	PT HS	Sep. Rate	Payroll
Post # Treat	1.717	2.180	13.30*	0.722**	18.43*	54.96	0.727***
	(3.502)	(5.565)	(7.606)	(0.345)	(9.367)	(68.60)	(0.219)
N	843	843	843	755	843	843	742

The table reports the coefficient β_3 in Equation (1), interacting the treatment indicator with a post dummy. $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. *Standard errors clustered at the firm-level in parentheses.*

Figure D1: *Event Study Plots by Firm Size*



Notes: The figure reports the event-study coefficient β_3 in Equation (1), interacting the treatment indicator with year dummies. Standard errors are clustered at the firm level. Bars represent 95% confidence intervals.