BDPEMS
Berlin Doctoral Program in Economics and Management Science

Working Paper Series
2015-12

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Employment consequences of changes in dismissal protection: Evidence from a 2004 German reform

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August 20, 2015

Abstract

This paper empirically analyzes the impact of a change in dismissal protection on employment dynamics and temporary employment patterns in small establishments. The identification strategy relies on a quasi-experimental change in the German Protection Against Dismissal Act (PADA) in 2004. Due to a raise of the minimum firm size threshold determining coverage by the PADA, dismissal protection was relaxed for some establishments. Using matched employer-employee administrative data linked to establishment survey data, we estimate the causal effect of the reform on worker and job flow rates and the use of temporary employment. We find evidence for a short-term increase in overall worker flow rates which does, however, not persist in the medium-term. Reestimation by gender suggests that the effect on the hiring rate is driven by women while the effect on the separation rate is driven by men. There is no robust evidence for an effect on the overall job flow rate and the share of employees on fixed-term contracts or temporary agency workers.

Keywords: Dismissal protection, worker flows, temporary employment

JEL classification: J21, J23, J38

*Humboldt Universität zu Berlin, Research Training Group 'Interdependencies in the Regulation of Markets', email: kai.priesack@hu-berlin.de. I thank Benjamin Bruns, Alexandra Spitz-Oener, Viktor Steiner and Hanna Wielandt for helpful comments and suggestions. I also thank participants of the SMYE in Ghent, the ESPE in Izmir, the Brown Bag seminar at HU Berlin and the Economic Policy seminar at FU Berlin for fruitful discussions. This study uses the Linked-Employer-Employee Data (LIAB) QM2 9310 from the IAB. 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) and subsequently remote data access. Financial support from the DFG is gratefully acknowledged.

1 Introduction

Employment protection legislation constitutes a key feature of labor markets. Its major aim is to protect employees from arbitrary dismissals and increase job security. At the same time, it may impose rigidities on the ability of firms to adapt to changing economic conditions. Too stringent dismissal protection may hinder job creation and worker reallocation (OECD 2013). Moreover, policy makers became increasingly aware of the interdependency between the regulation of open-ended and temporary contracts. A growing regulatory gap between these contract types may increase labor market segmentation by encouraging the excessive use of less-protected temporary employment (OECD 2014). Overall, the debate on the design of dismissal protection remains controversial both from a research and policy perspective. Hence, empirical evidence on this subject may provide valuable insights for theoretical work as well as for policy-making.

As the largest economy in the European Union and the fourth-largest in the world (by GDP), Germany is a worthwhile country to study the interrelation between dismissal protection legislation and employment. Moreover, the latest reform of the German Protection Against Dismissal Act (PADA - 'Kündigungsschutzgesetz' (KSchG)) in 2004 constitutes a suitable case for an empirical analysis of its causal effects on employment. Notably, the German government decided to change the minimum establishment size threshold determining coverage by PADA from five to ten employees as of 1 January 2004. The reform affected about 15 to 16% of all establishments (with at least one employee liable for social security payments) representing 7 to 8% of all employees (liable for social security payments) in Germany.[1]

In this paper, we exploit the temporal and cross-sectional variation in the PADA resulting from this 2004 reform as a quasi-experiment. Using a difference-in-differences (DiD) approach, we identify the causal effect of reduced dismissal protection on employment dynamics and temporary employment patterns. Notably, we compare average outcomes of establishments subject to the policy change (treatment group) with establishments that exhibit similar establishment characteristics, yet, are not exposed to the change (control group) before and after the reform. By drawing on detailed administrative employer-employee panel data (LIAB QM2 9310), we provide new estimates on the impact of the change in the PADA in 2004 on worker and job flows of establishments. Worker flows comprise yearly hirings and separations whereas the job flow is defined as the yearly difference between the two. In addition, we link the administrative data to establishment survey data (IAB Establish-

[1] Own calculations based on data from the IAB Establishment Panel and the LIAB QM2 9310 following the approach of Rudolph (1996).

ment Panel) and examine the impact of the reform on the use of temporary employment. We consider two types of temporary employment, fixed-term contract (FTC) employees and temporary work agency (TWA) employees. To capture both establishments' potential short- and medium-term adjustments, we take into consideration a three-and-a-half-year period after the reform.

Our analysis differs from previous studies on the 2004 PADA reform (e.g. Bauernschuster 2013, Bellmann et al. 2014) in a number of key dimensions. First, we use administrative data in addition to survey data which allows for a more accurate determination of the establishment size and thus coverage by the PADA. Second, we exploit the absence of any relevant changes to the PADA after 2004 and examine short- and medium term adjustments. Third, we provide a more comprehensive analysis by assessing potential effect heterogeneity on worker and job flows by gender. Fourth, we consider a potential impact of the reform on the use of different types of temporary employment relationships.

Our results provide evidence for a short-term increase in worker flow rates which does, however, not persist in the medium-term. Furthermore, estimates by gender suggest that the effect on the hiring rate is driven by women while the effect on the separation rate is rather driven by men. Moreover, there is no robust evidence for an effect on the overall job flow rate and the share of employees on fixed-term contracts or temporary agency workers. An array of placebo tests supports the success of our identification strategy.

The remainder of this paper is structured as follows: Section 2 outlines the institutional setting of the PADA. Section 3 reviews previous theoretical and empirical results. The identification strategy in the estimation is explained in Section 4. Section 5 presents the data, the sample selection rules and some descriptive statistics. Section 6 discusses the empirical results and robustness checks and Section 7 concludes. Appendix A contains more information about the data, Appendix B shows additional regression results and Appendix C presents robustness checks and F-statistics.

2 Institutional background

Since 1951, the German employment protection legislation is regulated by the Civil Code ('Bürgerliches Gesetzbuch' (BGB)) and the Protection Against Dismissal Act (PADA - 'Kündigungsschutzgesetz' (KSchG)). Its aim is to protect employees from arbitrary dismissals by their employer. In principle, the PADA applies to all employees of an establishment unless a person is employed on a fixed-term contract.[2] With regards to temporary agency workers,

[2]Although the Law on Part-Time Work and Temporary Employment Contracts ('Teilzeit- und Befristungsgesetz' (TzBfG)) does allow for individual or collective agreements which grant the right of regular

the PADA only applies in relation to the agency not the user establishment.

Given the employer is not exempted from the PADA and the duration of employment exceeds six months, a dismissal with notice is only effective if it is socially justified on either personal grounds, grounds of conduct, or operational grounds (KSchG §1 (2)). Prior to any dismissal, the employer has to give notice to its works council (if in place) under the terms of the Works Constitution Act ('Betriebsverfassungsgesetz' (BetrVG) §102). In case of a dismissal due to operational requirements, the employer has to carry out a social selection among his employees taking into consideration job tenure, age, maintenance obligation, and severe disability (KSchG §1 (3)). In addition, any dismissed employee may appeal to labor court and contest the termination of her contract (KSchG §4). If the court decides in favor of the employee, the employer commonly has to pay a severance payment as a reinstatement of the employee is infeasible in most cases.

Since its initiation, some employers are exempted from the PADA by legislation. In particular, the PADA comprises an exception provision for small establishments such that dismissals in establishments with less than a minimum number of employees do not have to comply with the PADA but only need to fulfill some general statutory rules. As part of the 'Agenda 2010' reform package, the minimum threshold for the applicability of the PADA was raised from five to ten full-time equivalent weighted (FTE) employees as of 1 January 2004 for any employee hired after 31 December 2003 (incumbent workers stayed protected as long as their initial number did not fall below the former threshold of five FTE employees). Prior to this reform, the threshold had been adjusted twice; first, it was raised from five to ten employees in 1996, and then reduced from ten to five employees in 1999. Since the latest reform in 2004, no further adjustments to the PADA have taken place.[3]

The PADA reform is a result of negotiations in the conciliation committee as of 16 December 2003 and was only approved on 19 December 2003, less than two weeks before it became effective. Prior to that, the government rather planned to facilitate the hiring of FTC employees in establishments with less than five workers (lastly announced in an information brochure published on 14 November 2003 by the German government).[4]

Besides the PADA reform, the 'Agenda 2010' entailed further major modifications of labor market policies that were gradually implemented between 2003 and 2005, subsumed under the four 'Hartz reforms'. Most importantly for this study, the active labor market policies of

termination to employees on fixed-term contracts (TzBfG §15 (3)), this is in practice the exception rather than the rule.

[3]Note that the PADA reform in 2004 comprised some further minor modifications, yet, unconditional on establishment size: The social selection process was simplified, the period for filing a suit was standardized to three weeks, and severance payment on the waiver taking legal action was introduced.

[4]The short-dated introduction of the reform is advantageous for our setting in that the analysis of the 2004 PADA reform is unlikely to be distorted by anticipation effects.

Hartz I deregulated temporary employment (i.e. fixed-term contract (FTC) and temporary work agency (TWA) employment) in 2003.[5] With respect to FTC employment, Hartz I implied a lowering of the age threshold for unlimited use of fixed-term contracts without valid reason from 58 to 52. For employees below this age, the maximum duration of fixed-term contracts without valid reason remained at two years. In terms of TWA employment, Hartz I entailed more extensive modifications. After the maximum period of assignment was already raised from 12 to 24 months in 2002, it was completely abolished in 2003. Moreover, the rehiring and synchronization ban was suspended such that TWA workers could be repeatedly hired by a particular agency and labor contracts could be synchronized with the duration of a specific assignment. Lastly, with an interim arrangement until 2004, the principal of 'equal pay' and 'equal treatment' was introduced. However, most agencies circumvented this rule by entering into collective agreements (Fertig and Kluve 2006). Except for the introduction of an upper limit of five years for fixed-term contracts without valid reason of workers above the age of 52 as of 1 May 2007, there have been no further changes to the regulation of FTC and TWA employment in the observation period of this study (until 30 June 2007).

3 Literature

Since the seminal work of Lazear (1990), the impact of dismissal costs on labor market flows and employment is analyzed in a number of theoretical studies. Drawing on Lazear (1990)'s result that severance payments between the employer and the employee can be offset by an efficient labor contract, early literature uses partial equilibrium models with third party transfers to show that a more stringent employment protection reduces layoffs in downturns, but also deters employers from hiring in upturns as firms take potential future dismissal costs into account.[6] Thus, increased dismissal costs reduce worker flows while its impact on the level of overall employment remains ambiguous. These findings also hold in a number of studies using general equilibrium models (e.g. Mortensen and Pissarides 1999, Ljungqvist 2002). Hence, in the case of the PADA reform studied in this paper, economic theory predicts an increase worker flows given an easing of dismissal protection. However, in terms of the overall effect on the employment level, theory does not provide a clear-cut prediction.

With respect to the effects of dismissal protection on temporary employment, theory also

[5] In addition, Hartz II comprised changes in the regulation of freelance work and marginal employment, Hartz III regulated the restructuring of the Federal Labour Office, and Hartz IV included revisions of the social and unemployment assistance. We refrain from a detailed discussion of these reforms as they are not the focus of this study.

[6] The theoretical literature commonly refers to firms rather than establishments. Yet, we view the theoretical predictions to be applicable to establishments as well.

provides some guidance. Extending the model of Mortensen and Pissarides (1999), Boeri (2011) examines the effects of dismissal protection in a two-tier economy where all entry jobs are initially temporary and must be either transformed into open-ended contracts or end at expiration. He finds that the share of temporary contracts on total employment increases in the stringency of dismissal protection of open-ended contracts. Similarly, Cahuc et al. (2012) study a search and matching model that allows for hires on temporary and open-ended contracts. Among other results, their model suggests that a larger gap in dismissal costs between open-ended and temporary contracts entails a large substitution of temporary for open-ended employees. Eventually, in terms of the PADA reform under study, the less stringent dismissal protection for some establishments reduces the protection gap and should decrease the share of temporary employment relative to the other establishments.

A number of empirical studies exploit the quasi-experimental setting of changes in dismissal protection to examine the effect on job and worker flows. Kugler and Pica (2008) analyze the impact of a labor market reform in Italy in 1990. They use an employer-employee panel and exploit the differential increase in dismissal protection for firms with fewer than 15 employees relative to firms with more than 15 employees. They find that the increase in dismissal costs reduces the individual probability for an accession by 13 to 14% and for a separation by 14 to 15%. At the same time, year-to-year employment declines by 5 to 6% in smaller relative to larger firms. Martins (2009) studies the effects of a law introduced in Portugal in 1989 under which dismissal constraints were softened for all firms. However, firms with 20 or fewer employees were partially exempted from the new law such that they experienced an even greater reduction in dismissal costs relative to larger firms. Using longitudinal data from an annual employment survey covering firms and workers based in Portugal, he finds evidence for a small relative increase in the small firms' job flow rate driven by a moderate increase in their hiring rate that corresponds to 5% of their average hiring rate.

Bauer et al. (2007) are the first to conduct a similar analysis for Germany by investigating the repeated changes in the threshold of the PADA in 1996 and 1999. They use an administrative dataset (on the basis of the German Employment Statistic Register) of West German establishments and study short-term changes in employment dynamics in a one-year observation period after each reform. Their results do not suggest a significant effect of changes in the stringency of dismissal protection on worker and job flows. The paper that is probably closest to mine in terms of approach and research question is Bauernschuster (2013). He assesses the effect of the latest adjustment of the threshold of the PADA in 2004 on the hiring behavior of firms. He uses survey data on establishments (IAB Establishment Panel) and estimates the impact of the change in the dismissal protection in the first one and a half years after the reform. He finds that the relaxed dismissal protection increased the hiring rate for small establishments relative to larger ones by 1.3 to 2.0 percentage points (in 2004) and 2.0 to 2.1 percentage points (in 2005).

Other studies examine the relationship between the stringency of dismissal protection and the use of temporary employment. Autor (2003) uses state-level variation in the employment at will legislation in the United States to assess a causal relation to the increased use of temporary help services. He finds that 20% of the growth of temporary services employment between 1973 and 1995 results from a stronger dismissal protection. Centeno and Novo (2012) study the effect of a labor market reform in Portugal in 2004 on the composition of employment. They exploit the differential increase in the dismissal protection of open-ended contracts for firms with 11 to 20 workers relative to all other firms and types of contracts. Their results suggest that the new legislation increased the use of fixed-term contracts in the treated firms relative to control firms by 1.6 percentage points.

As for Germany, empirical evidence on the effects of changes in dismissal protection on the use of temporary employment is limited and contradictory. Boockmann and Hagen (2001) finds some indication that the increased threshold of the PADA in 1996 lowered the probability of using fixed-term contracts in establishments subject to less stringent dismissal protection. However, using the same survey data (IAB Establishment Panel), Fritsch and Schank (2005) do not confirm the result. In both 1996 and 1999, they do not find a significant effect of threshold changes in the PADA on the use and share of fixed-term contracts. Yet, both studies do not conduct a rigorous analysis to identify a causal effect. Bellmann et al. (2014) are the first to provide some causal evidence for the impact of the 2004 PADA reform on the hirings of employees on fixed-term contracts. Based on data from the IAB Establishment Panel as well, their results suggest that the easing of the employment protection decreased the proportion of hirings on fixed-term contracts between 2004 and 2007 in small relative to large establishments.

4 Identification strategy

The aim of our paper is to identify the impact of the change in the PADA on employment dynamics (in terms of flow rates) and temporary employment patterns (in terms of the share of temporary employment). To this end, we apply a difference-in-differences (DiD) approach by comparing outcomes of establishments subject to a change in the PADA (treatment group) with establishments not exposed to this change (control group) before and after the policy reform (cf. Meyer 1995). More formally, this double difference can be expressed by the following equation:

$$\rho = \{E[Y_{it}|D_i = 1] - E[Y_{it}|D_i = 0]\} - \{E[Y_{it'}|D_i = 1] - E[Y_{it'}|D_i = 0]\},$$

where Y_{it} and $Y_{it'}$ denote the observable outcome of observation i in period t and t', t is a time period after and t' a time period before the policy reform. D_i is a binary variable indicating whether observation i belongs to the treatment group ($D_i = 1$) or the control group ($D_i = 0$). The key identifying assumption is a common time trend, meaning that in the absence of treatment the average outcomes of both treatment and control group would have evolved the same. Formally, this can be expressed as follows:

$$E[Y^0_{it} - Y^0_{it'}|D_i = 1] = E[Y^0_{it} - Y^0_{it'}|D_i = 0],$$

where Y^0_{it} and $Y^0_{it'}$ denote the potential outcome of observation i in period t and t' in the absence of the treatment. In other words, the common trend assumption justifies the replacement of the counterfactual (unobserved) non-treatment difference in average outcomes of the treated by the observed non-treatment difference of the non-treated. Assuming that the common time trend assumption holds, ρ identifies the average causal effect of the treatment on the treated. It should be noted, however, that this assumption is inherently not testable, and must thus be defended on grounds of economic reasoning.

Under the further assumption of an additive causal effect ρ, the following linear regression formulation can be obtained:

$$Y_{it} = \alpha_i + \lambda d_t + \rho(D_i d_t) + X^{'}_{it}\beta + \epsilon_{it}, \tag{1}$$

where $\alpha_i \equiv \alpha + A^{'}_i\eta$ denotes an individual fixed effect, α is a constant common to all individuals, η captures (un)observable time-invariant individual effects, λ captures time effects that are assumed to be common to all individuals (with a binary variable d_t equal to one in the post-treatment periods), β captures observable time-varying individual effects (with X_{it} containing the establishment level characteristics share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square), ϵ_{it} is an idiosyncratic error term, and ρ is the parameter of interest reflecting the differential effect on the outcome variables due to the policy reform.[7] Notably, empirical model (1) allows for time-varying confounding observables (X_{it}) as well as time-invariant confounding observables and unobservables (α_i). Since α_i cannot be consistently estimated and we are only interested in the estimation of ρ, we exploit the availability of the panel data and eliminate the individual

[7] In the present study, 'individual' refers to establishments as they constitute the unit of measurement.

fixed effect α_i by taking differences between the baseline year 2003 and post-treatment years for each observation i:[8]

$$\Delta Y_{it} = \lambda + \rho D_i + \Delta X_{it}^{'}\beta + \Delta\epsilon_{it}, \tag{2}$$

where $\Delta Y_{it} = Y_{it} - Y_{i,2003}$, $\Delta X_{it} = X_{it} - X_{i,2003}$, $\Delta\epsilon_{it} = \epsilon_{it} - \epsilon_{i,2003}$ and t represents either individual year effects ($t = 2004, 2005, 2006, 2007$), the short-term effect ($t \in \{2004, 2005\}$), the medium-term effect ($t \in \{2006, 2007\}$), or the total effect ($t \in \{2004, ..., 2007\}$). Finally, under the further assumptions $E[D_i \Delta\epsilon_{it}|\alpha_i] = 0$ and $E[\Delta X_{it}\Delta\epsilon_{it}|\alpha_i] = 0$, $\forall t$, OLS consistently estimates the causal effect ρ using regression formulation (2).

In Section 6, we present both estimates conditional and unconditional on X_{it}. On the upside, the inclusion of X_{it} allows for differences in time trends across observations based on observables X_{it} and can increase precision. On the downside, we have to additionally assume adjacent period exogeneity of the time-varying controls X_{it}, that is $E[\Delta X_{it}\Delta\epsilon_{it}|\alpha_i] = 0$, $\forall t$. Any control variable that is affected by the treatment itself may introduce a bias. Angrist and Pischke (2009) refer to this as a problem of 'bad controls', which are commonly controls that could themselves be outcome variables. For example, in the present case, we control for time-varying establishment-level characteristics such as the average share of part-time workers and apprentices. However, any such control may itself be influenced by the PADA reform. Consequently, results with additional time-varying covariates could potentially be biased. However, given the coefficients do not differ substantially when control variables are included, the scope for a bias seems to be limited.

Due to the panel data structure, serial correlation in the error term of an individual may be an issue when conducting statistical inference. Though, by taking differences and eliminating the individual fixed effect, we implicitly take into account any serial correlation that is captured by the unobserved time-invariant individual effect. Moreover, the differenced error term follows a moving average (MA) process by construction (unless the error term ϵ_{it} in the level equation (1) follows a random walk).[9] Since we do not want to make any explicit assumption on the error term structure, we estimate standard errors using the Huber/White/Sandwich estimator and thus allow for arbitrary correlation of error terms for each observation i.[10]

[8] In doing so, we implicitly allow for correlations between the individual fixed effect α_i and both the treatment status D_i and the control variables X_{it}

[9] The order of the MA process depends on the serial correlation structure of the error term in the level equation. Note, however, that even if the error term in the level equation is not serially correlated, the error term in the differenced equation would follow a MA(1) process by construction.

[10] In addition, we estimated standard errors by clustering at the establishment level. In the case of the individual year effects, the results do not differ since we essentially run OLS regressions on cross-sections. As for the short-term, medium-term and total effect, standard errors increase and significance levels decrease

5 Data

The analysis uses Cross-sectional Model 2 of the Linked-Employer-Employee Data 1993-2010 (LIAB QM2 9310) from the German Institute of Employment Research (IAB) (for details see Heining et al. 2013). The data set constitutes a representative sample of German establishments and contains both survey data on establishments and administrative data on individuals. For the years under consideration, the annual sample size amounts to roughly 16,000 establishments representing approximately 0.75% of the universe of German establishments. The data on individuals covers all employees liable for social security payments.[11] Given the administrative nature of the individual-level data, it is considered to be highly reliable. The data is particularly suitable for the analysis, as the individual-level data can be aggregated at the establishment-level using the unique identifier and allows computing worker flows. Moreover, the data provides sufficient information to calculate the establishment size in line with the legislation and thus to determine coverage by the PADA for each establishment. The Appendix A provides more details on the LIAB QM2 9310 data.

The threshold of the PADA applies to establishments (not companies with several establishments in several municipalities), which is also the unit of measurement in the data. Table A.1 in the appendix summarizes the procedure to determine the full-time equivalent weighted (FTE) establishment size defined by the PADA and the availability of the associated information in the data. In principle, all regular employees including marginal employees, employees with fixed-term contracts and employees hired within the last six months should be counted.[12] Part-time employees should be weighted depending on contractual weekly working hours. According to the legislation (KSchG §23), employees working up to 20 hours per week should be weighted by 0.5, employees working more than 20 and up to 30 hours per week should be weighted by 0.75, and employees working more than 30 hours should be weighted with 1.0. Although the data does provide information on part-time employees, it only records whether a person works up to 18 hours per week or more than 18 hours per week whereby we weight the former by 0.5 and the latter by 0.75. Accordingly, part-time employees working more than 18 and up to 20 hours per week as well as workers identified as part-time employees but working more than 30 hours per week are not weighted exactly according to the PADA. Owners and executive staff not subject to directives, family

slightly, yet, the overall qualitative interpretation of the results remains valid. Results are available on request.

[11]Employees liable for social security payments are all white- and blue-collar workers including apprentices and, since 1999, also marginal employees and unpaid family workers. Civil servants, self-employed and regular students are not recorded.

[12]Note that against a common misconception, the PADA applies to marginal employees without any restrictions.

members without a labor contract and vocational trainees should not be counted and are consequently excluded. Employees with an inactive work relationship (e.g. maternity leave) should be excluded in case of replacement. As the data does not record inactive employees and replacements cannot be identified, we assume that inactive employees are replaced in all cases.[13]

For the analysis, we compare average outcomes of establishments subject to the policy change (treatment group) with establishments that exhibit similar establishment characteristics, yet, are not exposed to the change (control group) before and after the reform. The binary treatment variable D_i (see Section 4) identifies each group. Since measurement error in D_i may cause an attenuation bias, we abstract from establishments that are clustered around the old and new threshold, that is, five and ten FTE employees (cf. Martins 2009). Consequently, the treatment group ($D_i = 1$) consists of establishments with six to nine FTE employees and the control group ($D_i = 0$) of establishments with 11 to 20 FTE employees prior to the reform. To address the problem of mean reversion, we follow previous analyses and restrict the sample to establishments that remain in the same size category during a three-year period preceding the reform (2001 to 2003) (cf. Martins 2009, Bauernschuster 2013).[14] Note that due to the described assignment procedure, we set the treatment status before the policy change and keep it unchanged in the post-treatment periods even if establishments change size. In doing so, we circumvent problems of self-selection into treatment in response to the policy change.

To examine changes in worker and job flows, we define dependent flow variables as point-

[13]Only recently, German Federal Labor Court (BAG) has decided that TWA employees should also be counted if they regularly work for the user establishment (BAG, judgment of 24 January 2013, 2 AZR 140/12). However, in the time under consideration, the common perception was that TWA employees should not be added to the number of employees of the user establishment. In line with this argument, we do not consider TWA employment in the determination of the establishment size.

[14]Following the argument of Davis et al. (1996) and Martins (2009), results may be distorted when establishments are assigned to the treatment and control group based on employment in a single year rather than on their long run size. For the case of two size categories (small and large firms), Davis et al. (1996) argue that firms assigned to the small size category based on a single year are more likely to have experienced a transitory decrease in employment. Correspondingly, these firms are more likely to return to their long run size revealing a positive job flow rate (caused by an increase in the hiring rate and/or decrease in the separation rate) which is attributed to the small size category. For firms in the large size category the argument holds vice versa. This so-called mean reversion effect makes small firms appear to outperform large firms in terms of job flow rates and to exhibit larger hiring rates and/or smaller separation rates. However, both the treatment group (six to nine FTE employees) and the control group (11 to 20 FTE employees) may suffer from the bias in either direction as the described phenomena may occur at the lower and/or upper limit of each group. Hence, the direction of the bias is a priori undetermined. With respect to the share of temporary employment, there is no obvious argument for a potential bias from mean reversion as it depends on the relationship between transitory movements in employment and the use of temporary employment. Nonetheless, we use the same sample of establishments to allow for a joint analysis of flow rates and temporary employment.

in-time comparisons. $Hirings_{i,t}$ denotes the number of employees working at establishment i in period t but not $t-1$ and $Separations_{i,t}$ denotes the number of employees working at establishment i in period $t-1$ but not t. Since t refers to 30 June in each year, short-term working relationships that begin and end within 12 months (or vice versa) and do not cover 30 June are not taken into account. Due to data limitations, the focus is on all separations irrespective of whether the contract is terminated by the employer or the employee. Hirings and separations are weighted according to the weighting scheme of the PADA. We use conventional flow rates by dividing the flows of establishment i in period t by its total number of FTE employees in $t-1$, denoted $E_{i,t-1}$.[15] Specifically, the hiring rate is defined as $HR_{i,t} = Hirings_{i,t}/E_{i,t-1}$ and the separation rate is defined as $SR_{i,t} = Separations_{i,t}/E_{i,t-1}$. Job flows are obtained as the difference between hirings and separations, with the job flow rate defined as $JFR_{i,t} = HR_{i,t} - SR_{i,t}$.[16] Since the legislation became effective on 1 January 2004, the flow rates for $t = 2004$ (covering hirings and separations between 30 June 2003 and 30 June 2004) are only subject to the policy change for the last six months. To examine this period in more detail, we conduct a supplementary analysis that distinguishes between hirings in the first (June to December in year $t-1$ denoted H1)) and the second (January to June in year t denoted H2) half of the yearly observation period. Due to data limitations, this additional analysis cannot be performed for separations. Lastly, we separate hirings and separations by gender to capture potential heterogeneity in the effects on men and women.

To study the impact on temporary employment, we additionally use information on FTC and TWA employment provided by the IAB Establishment Panel as the individual-level data does not provide information on the contract type. The individual-level data aggregated at the establishment-level is matched to the IAB Establishment Panel using the unique establishment identifier. To preserve consistency in the matched data, we follow Alda (2005) and drop establishments with substantial differences in the establishment size according to the two data sources.[17] The IAB Establishment Panel provides information on the number

[15] In contrast to the FTE establishment size determining coverage by the PADA, the total number of FTE employees, $E_{i,t-1}$, includes all employees in an establishment (e.g. owners, apprentices).

[16] In the literature of worker and job flows, flow rates are often obtained by using the average current and past employment as the denominator. This monotonic transformation facilitates an integrated treatment of establishment entries, exits and continuing establishments as rate measures lie in the closed interval [-2,2] (cf. Davis and Haltiwanger 1990). However, we use a balanced panel and refrain from flows on the extensive margins through establishment entry and exit. Thus, our conventional measure is more appropriate and simplifies the interpretation of the results. In addition, estimates using the transformed measure of flow rates are qualitatively the same and are available on request.

[17] To avoid mismatches, Alda (2005) defines limits for the difference in the number of employees according to the data sources contingent on establishment size categories. For establishments with up to five employees 40%, for establishments with five to 19 employees 30%, and for establishments with 20 to 100 employees 20%. If the limit is exceeded for one of the years under consideration, the establishment is dropped from the sample.

of FTC and TWA employees in an establishment as of 30 June each year.[18] As outcome variables, we define two variables which measure the share of either FTC employees or TWA employees on the total number of (unweighted) employees in the respective establishment.[19]

As in other studies, we restrict the sample by excluding establishments in the shipping and aircraft transport industry since other legislation applies to these sectors (KSchG §24). In addition, we drop establishments in the highly subsidized agricultural and mining sectors as well as non-profit firms and private households. For the analysis of worker and job flows, establishments with hiring rates larger than two are excluded to avoid that results are driven by extreme values. Likewise, for the analysis of the share of temporary employees, we drop establishments with a share of FTC or TWA employment larger or equal to one.[20] From the remaining observations, we abstract from establishment entries and exits and construct a balanced panel for all establishments with valid FTE establishment size information during the time periods 2001 to 2007.[21] As the reform became effective in 2004, the post-treatment periods 2004 to 2007 should capture short- and medium-term adjustments. In total, we obtain 439 different establishments for the analysis of flow rates in our main specification, of which 174 belong to the treatment group. Due to the matching procedure for the IAB Establishment Panel, the sample for the analysis of the use of temporary employment is reduced by approximately 20%.

Table 1 summarizes the descriptive statistics of the treatment and control group in the baseline year 2003. Note that besides the FTE establishment size, the differences in the means of the establishment characteristics in the two groups are not significantly different from zero. This constitutes a first indicator for a good control group choice as establishments in the treatment and control group exhibit similar average characteristics prior to the policy reform. Notably, although the Hartz I reforms on temporary employment are not contingent on establishment size, they may affect smaller establishments differently than larger ones which would confound our analysis. In this context, the comparable exposure to temporary employment prior to the reform makes us confident that the results are not biased due to

[18] In the case of TWA employment, the number of workers refers to the TWA employees in the user establishment not the agency.

[19] The data does not contain information on the part-time status of FTC and TWA employees. Therefore, we divide the total number of FTC and TWA employees by the total number of unweighted employees in an establishment.

[20] The share of FTC employment can exceed one only due to misreporting. However, the share of TWA employment may also exceed one if the number of TWA employees used in an establishment exceeds its total number of employees (which does not include TWA employees).

[21] Accordingly, flow rates are available for the time periods 2002 to 2007 as the denominator is employment in $t-1$. For the analysis of temporary employment, we further drop all establishments that are successfully matched to the IAB Establishment Panel but have missing values for the number of FTC or TWA employees during the time periods 2003 to 2007.

Table 1: Establishment descriptive statistics of treatment and control group in 2003

Variables	(1) Treatment group Mean	S.D.	(2) Control group Mean	S.D.	Mean (2) - (1)
Treatment identifier:					
FTE establishment size	7.297	0.875	15.091	2.371	7.794***
Outcome variables:					
Hiring rate	0.107	0.121	0.115	0.114	0.008
Separation rate	0.112	0.113	0.118	0.091	0.006
Job flow rate	−0.005	0.109	−0.003	0.112	0.002
Share of FTC workers[1]	0.021	0.060	0.018	0.056	−0.003
Share of TWA workers[2]	0.002	0.021	0.005	0.027	0.003
Control variables:					
Share of blue-collar worker	0.434	0.359	0.474	0.334	0.041
Share of part-time worker	0.199	0.219	0.176	0.203	−0.023
Share of apprentices	0.067	0.107	0.064	0.091	−0.003
Mean age	43.500	6.174	43.346	4.410	−0.154
Mean age squared	1930.156	544.880	1898.270	383.263	−31.890
Observations	174		265		

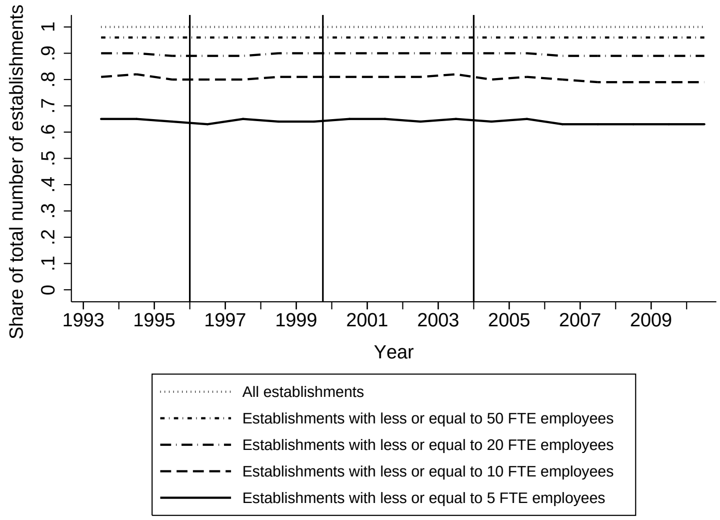
Notes: [1]N = 344, [2]N = 345. Treatment group: 6 to 9 full-time equivalent employees in 2001 to 2003. Control group: 11 to 20 full-time equivalent employees in 2001 to 2003. ***, ** and * denote statistical significance of the difference in the mean between the treatment and control group at the 1%, 5% and 10% levels. Data source: LIAB QM2 and IAB Establishment Panel.

a differential impact of the deregulation of temporary employment during the same time. Figures A.1 and A.2 in the appendix further show that the establishments in the treatment and control group are equally distributed over different regions and industries.

Finally, figure 1 provides some evidence for the absence of a 'threshold effect' in terms of changes in the establishment size distribution. Based on the FTE weighting scheme applicable since 1 January 1999, the figure plots the share of establishments by FTE size categories for all establishments in the dataset between 1993 and 2010. The three vertical lines indicate the PADA reform in 1996 and 2004 (threshold raised from five to ten FTE employees) and 1999 (threshold reduced from ten to five FTE employees). Overall, the distribution of establishments has not changed significantly over the observation period. The solid and dashed line (the lowest two lines) are of particular interest since they display the share of establishments below the old and new threshold. Notably, the absence of any kink suggests that establishments did not (at least on a grand scale) circumvent coverage by the PADA by adjusting their size strategically in response to the threshold reforms.[22]

[22]As information on marginal employees is only available since 1999, we computed the FTE weighted average number of marginal employees in each year for all establishments based on the IAB Establishment Panel. Subsequently, we added this yearly figure to the size of each establishment for the years 1993 to 1998. For robustness, we additionally estimated FTE weighted establishment sizes by excluding marginal employees for 1999 to 2010. This results in a minor upward shift in the shares of the lower establishment size categories, yet, the distribution by size categories is comparable and does still not exhibit any kinks.

Figure 1: Establishment distribution by FTE size categories, 1993 to 2010



Notes: Establishment sizes are based on FTE weighting scheme according to legislation since 1 January 1999 (as described in the text). For the years 1993 to 1998, no individual-level data on marginal employees is available. Instead, the FTE weighted average yearly number of marginal employees for all establishments based on the IAB Establishment Panel is added to the size of each establishment for these years. The vertical lines signify the dates of the changes in the minimum firm size threshold determining coverage by the PADA. Source: LIAB QM2 1993-2010 and IAB Establishment Panel.

6 Results

6.1 Flow rates

Table 2 presents the main results for the worker and job flow rates. The difference-in-differences (DiD) coefficients indicate the causal effect of the reform on establishments subject to the relaxed dismissal protection. A coefficient of 0.01 corresponds to a one percentage point change of the flow rate in the treated relative to the control establishments. 'Short-term' refers to estimates using pooled data for the post-treatment periods 2004 and 2005, 'medium-term' to estimates using pooled data for the post-treatment periods 2006 and 2007, and the total effect to estimates using pooled data for all post-treatment periods 2004 to 2007.

Estimates in columns (1) and (2) show a relative increase in the hiring rate between treated and control establishments in all post-treatment periods. Though, only the individual year effect in 2004 (DiD 2004: 0.035 and 0.032), the short-term effect (DiD 2004-05: 0.027 and 0.025) and the total effect (DiD 2004-07: 0.021 and 0.020) are significant at the 5% level. In addition, section A in Table C.11 shows outcomes for F-tests of joint significance of the respective individual year effects for the short-term (DiD 2004 and DiD 2005), medium-term (DiD 2006 and DiD 2007) and total effect (DiD 2004 to DiD 2007). While the short-term

Table 2: Difference-in-differences results: Worker and job flow rates

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.035**	0.032**	0.027**	0.029**	0.008	0.003
	(0.017)	(0.016)	(0.013)	(0.013)	(0.020)	(0.019)
DiD 2005	0.018	0.017	0.022	0.026*	−0.004	−0.008
	(0.015)	(0.015)	(0.014)	(0.014)	(0.017)	(0.017)
DiD 2006	0.014	0.011	−0.006	−0.004	0.020	0.015
	(0.018)	(0.018)	(0.015)	(0.015)	(0.020)	(0.020)
DiD 2007	0.018	0.015	0.016	0.015	0.002	−0.001
	(0.017)	(0.017)	(0.015)	(0.015)	(0.020)	(0.019)
DiD 2004-05	0.027**	0.025**	0.025**	0.027***	0.002	−0.002
	(0.011)	(0.011)	(0.010)	(0.010)	(0.013)	(0.013)
DiD 2006-07	0.016	0.014	0.005	0.006	0.011	0.008
	(0.012)	(0.012)	(0.011)	(0.011)	(0.014)	(0.014)
DiD 2004-07	0.021**	0.020**	0.015**	0.017**	0.006	0.003
	(0.008)	(0.008)	(0.007)	(0.007)	(0.010)	(0.010)
Controls	No	Yes	No	Yes	No	Yes
N	439	439	439	439	439	439

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310. 9310.

effect is supported by jointly significant individual year effects at the 10% level, the F-test for the total effect remains slightly below this significance level. Overall, we interpret these results as evidence for a relative short-term increase in the hiring rate of 2.5 to 2.7 percentage points that does, however, not persist in the medium-term.

Since the reform became effective in the beginning of 2004 and the outcome flow variables are point-in-time comparisons of the 30 June of consecutive years, the first treatment period is only subject to the treatment in the last six month (January to June 2004). Hence, we would expect an effect on the hiring rate only in the second half of this observation period. Table B.1 reveals that the increase in the relative hiring rate in 2004 is indeed driven by the second half of the observation period with insignificant estimates for the first six months (June to December 2003 denoted H1) and significant estimates at least at the 10% level for the last six months (January to December 2004 denoted H2). Unfortunately, we do not observe an employee's date of separation and are thus not able to conduct the same analysis for the separation rate.

Next, Tables B.2 and B.3 show separate estimates for the flow rates of men and women and highlight significant gender heterogeneity in the effects of the reform. With insignificant effects for men and highly significant effects for women, the results in columns (1) and (2) clearly illustrate that women drive the dynamics in hirings. Notably, estimates for the women's hiring rate range between 1.5 and 3.3 percentage points and are significant over the entire post-treatment observation period. This suggests that for this subgroup, the causal effect of the reform is not only temporary, but also persists in the medium-term. Highly significant F-tests of joint significance of the respective individual year effects support this finding (see section C in Table C.11). By contrast, the effects for the men's hiring rate are close to zero and not significant for any time period.

Turning to the results for the separation rate, we find that they are similar to those of the hiring rate both in terms of the effect size as well as significance. Columns (3) and (4) in Table 2 show that except for the individual year effect in 2006, the estimates exhibit a relative increase in the separation rate between the treated and control establishments for all post-treatment periods. Again, the individual year effect in 2004 (DiD 2004: 0.027 and 0.029), the short-term effect (DiD 2004-05: 0.025 and 0.027) and the total effect (DiD 2004-07: 0.015 and 0.017) are significant at least at the 5% level. Moreover, the individual year effect in 2005 conditional on covariates (DiD 2005: 0.026) is significant at the 10% level. The F-tests of joint significance of the respective individual year effects further support the results for both the short-term and total effect (see section A in Table C.11). Given the results are clearly driven by the effects in 2004 and 2005, we again view the findings as support for a temporary effect, meaning a short-term increase in the separation rate of 2.5 to 2.7 percentage points that is not persistent in the medium-term.

Looking at the results by gender, we further find that, contrary to the hiring rate, the effect on the separation rate is rather driven by men than women. Estimates for men show significant short-term and total effects, yet, do not additionally indicate a significant medium-term effect (see columns (3) and (4) in Table B.2). Moreover, the effect on the men's separation rate is slightly smaller than the effect on the women's hiring rate with estimates ranging between 1.2 and 2.4 percentage points. Against this, the estimates for the women's separation rate are insignificant over the entire post-treatment observation period (see columns (3) and (4) in Table B.3).

Finally, the results in columns (5) and (6) of Table 2 indicate that the sign of estimates on the job flow rate is undetermined and there is no significant difference in the overall job flow rate for any time period. In addition, none of the F-tests for the job flow rate is significant (see section A in Table C.11). This does not come as a surprise given our estimates for the worker flows are of a comparable effect size and offset each other. Yet, the reestimation of the job flow rate by gender points to an opposing effect for men and women (see columns (5) and (6) in Tables B.2 and B.3). Besides some evidence for a negative short-term and total effect on the men's job flow rate, we find strong evidence for a positive short-term, medium-term and total effect on the women's job flow rate.

In summary, we consider our results as evidence for a short-term increase in the overall relative hiring and separation rate which is, however, not persistent in the medium-term.

Estimates by gender further reveal that the effect on the hiring rate is clearly driven by women while the effect on the separation rate is rather driven by men.[23] To better illustrate the economic impact of the causal change in the worker flow rates, we estimated the number of employees that correspond to the percentage point changes in the flow rates on the basis of total number of employees in German establishments subject to the policy reform (based on the IAB Establishment Panel and the LIAB QM2 9310). In doing so, we find that the relative short-term increase in worker flow rates of 2.5 to 2.7 percentage points corresponds to approximately 70,000 to 75,000 employees per year.

In relation to previous research, our estimates qualitatively and quantitatively confirm results for the short-term effect by Bauernschuster (2013). Using survey data on the total hirings in the first six months of each year, he finds significant effects for the hiring rate in 2004 (0.013 to 0.020) and 2005 (0.020 to 0.021). Using our comparable point-in-time measure for the second half of the year (see columns (3) and (4) in Table B.1), we obtain very similar estimates for the individual year effect in 2004 (DiD 2004: 0.025 and 0.023) and 2005 (DiD 2005: 0.019 and 0.018), although the estimate for the individual year effect in 2005 is not significant. Yet, in addition to Bauernschuster (2013)'s findings, our results suggest that the effect on the hiring rate is not persistent in the medium-term.[24] Furthermore, Bauer et al. (2007) do not find any significant effects for the previous analogous German PADA reforms in 1996 and 1999. Although their data is drawn from the same data source, the divergent results could stem from different sample selection criteria and, more importantly, from different labor market conditions at time of the previous reforms. Finally, our results only partially confirm the theoretical predictions in that we only observe a temporary effect on the worker flow rates.

6.2 Temporary employment

Table 3 presents the main results for the share of temporary employment. In terms of FTC employment, the estimates in columns (1) and (2) do not exhibit a consistent sign in the short-term, but all become negative in the medium-term. However, solely the individual year

[23]We tried to shed further light on the gender heterogeneity by also using worker flows by job type as dependent variables (i.e. hiring and separation rates of part-time and marginal employees). However, we do not find any evidence that our results for men and women are driven by significant changes in flows of a particular job type.

[24]We also reestimated the individual year effects using the identical data and definition of the hiring rate based on the IAB Establishment Panel as in Bauernschuster (2013), but applying our assignment procedure describes in Section 5 and based on the administrative data (LIAB QM2 9310). Again, we obtain significant effects of the relaxed dismissal protection on the hiring rates for 2004 and 2005 at least at the 5% level, although the effects are slightly larger (DiD 2004: 0.026 and 0.027; DiD 2005: 0.035 and 0.032). In addition, we find positive but still insignificant effects for 2006 and 2007 in this setup. We thank Stefan Bauernschuster for making his statistical programs available for the comparison analysis.

Table 3: Difference-in-differences results: Share of temporary employment

Dep. variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
DiD 2004	0.000	−0.001	−0.001	−0.002
	(0.007)	(0.007)	(0.003)	(0.003)
DiD 2005	0.009	0.009	0.001	0.001
	(0.011)	(0.012)	(0.003)	(0.003)
DiD 2006	−0.009	−0.008	−0.005	−0.005
	(0.010)	(0.010)	(0.004)	(0.004)
DiD 2007	−0.015	−0.018*	−0.004	−0.004
	(0.009)	(0.010)	(0.006)	(0.006)
DiD 2004-05	0.005	0.004	0.000	−0.001
	(0.007)	(0.007)	(0.002)	(0.002)
DiD 2006-07	−0.012*	−0.013*	−0.004	−0.005
	(0.007)	(0.007)	(0.003)	(0.004)
DiD 2004-07	−0.004	−0.004	−0.002	−0.003
	(0.005)	(0.005)	(0.002)	(0.002)
Controls	No	Yes	No	Yes
N	341	341	336	336

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

effect in 2007 conditioned on covariates (DiD 2007: -0.018) and the medium-term effect (DiD 2006-07: -0.012 and -0.013) are significant and, yet, at the 10% level only.[25] Moreover, the F-tests of joint significance do not confirm the medium-term effect (see section A in Table C.12). Given this very weak statistical evidence, we refrain from a causal interpretation of the results on the share of FTC employment.

Likewise, there is no evidence for a causal effect of the policy reform on the share of TWA employment. Our main results show that none of the estimates is significant (see columns (3) and (4) in Table 3). Moreover, insignificant F-tests of joint significance further support this view (see section A in Table C.12).

Taken together, our estimates do not provide credible evidence for a causal effect of the policy reform on the share of FTC or TWA employment. We see our results in line with the previous study of Fritsch and Schank (2005) and partly with Bellmann et al. (2014) in that the effect of the three analogous German PADA reforms on the use of FTC employment is non-existent or at least very limited. In addition, the estimates suggest that this also holds for TWA employment. Eventually, the theoretical prediction that a less stringent dismissal protection should decrease the share of temporary employment is not confirmed

[25] A coefficient of 0.01 corresponds to a one percentage point change of the share of FTC or TWA employment in the treated relative to the control establishments.

by our empirical results.

6.3 Robustness checks

As stated in Section 4, our key identifying assumption is a common time trend in our dependent variables. Since the plausibility of this assumption most likely increases with a tighter establishment size range for the control group, we replicated the analysis using a more restrictive control group of establishment with up to 15 FTE employees only (cf. Bauernschuster 2013). However, the restrictive specification comes at a price, namely that it reduces the sample size to 258 observations for the analysis of the flow rates, and 202 (resp. 199) for the analysis of the use of FTC (resp. TWA) employment.

Columns (1) and (2) in Table C.1 show the estimates for the hiring rate under the restrictive specification and qualitatively confirm our main results. Despite the reduced sample size, the individual year effect in 2004, the short-term effect and the total effect remain significant. In addition, F-tests of joint significance of the respective individual year effects indicate that the short-term effect stays jointly significant at least at the 10% level (see section D in Table C.11). The picture for the separation rate looks similar. Estimates for the individual year effect in 2004, the short-term effect and the total effect under the restrictive specification remain significant (see columns (3) and (4) in Table C.1). Solely, the F-tests for the short-term and the total effect are no longer significant (see section D in Table C.11). Overall, we consider our main results for the worker flow rates to be robust to the restrictive specification.

Next, we reestimated the effects using 2002 instead of 2003 as an alternative baseline year. We therefore adjusted the assignment procedure accordingly, that is, we restricted the sample to establishments that remain in the same size category during the three-year period 2000 to 2002 (instead of 2001 to 2003). Again, the treatment group consists of establishments with 6 to 9 FTE employees and the control group of establishments with 11 to 20 FTE employees. As a result for this robustness check, we would expect to find insignificant effects for the untreated time period 2003 and comparable effects for the post-treatment periods 2004 to 2007.

Columns (1) and (2) in Table C.2 show the respective results for the hiring rate. The estimates strongly support the robustness of our main results to the alternative baseline year specification. The short-term and total effect are significant at the 1% level and are driven by significant individual year effects in 2004 and 2005. F-tests for the joint significance of the short-term and total effect further confirm this finding (see section E in Table C.11). In addition, the medium-term effect is significant at the 10% level. However, the respective individual year effects 2006 and 2007 are insignificant both individually and jointly. The respective estimates for the separation rate are also in line with our main results given that the individual year effect in 2004, the short-term and total effect are all significant at least at the 5% level (see columns (3) and (4) in Table C.2). Again, significant F-tests for the short-term and total effect further support this finding (see section E in Table C.11). Moreover, note that the individual year effect in 2003 for both the hiring and separation rate are insignificant which is comforting as it supports the argument that our main estimates are not distorted due to anticipation effects.

Finally, the results in columns (5) and (6) of Table C.1 indicate that the sign of the estimates on the job flow rate remains undetermined under the restrictive specification and there is no significant difference in the overall job flow rate for any time period. This result is also robust to the specification using 2002 the baseline year (see columns (5) and (6) in Table C.2).

To provide further support for the common time trend assumption and justify the identification of the causal effect in our econometric approach, we performed an array of placebo tests. We started with pre-treatment placebo tests that examine whether the treatment and control group exhibit significant differences in the time trend of the dependent variables during periods that should not have been affected by the reform. Accordingly, we implemented placebo tests using our DiD approach for time periods before the policy change (2001 to 2003). To be more precise, we used the identical assignment procedure based on the establishment size between 2001 and 2003 and constructed a balanced panel for the time periods 2000 to 2003 (again with flow rates only available for the time periods 2001 to 2003). We estimated placebo treatment effects under the assumption that the treatment took place after the time periods 2001 or 2002. The absence of significance indicates that there are no systematic differences in flow rates between the treated and control establishments during the placebo periods (see Table C.3 and Section H in Table C.11). Additionally, we repeated the same exercise for the restrictive specification and again none of the estimates is significant (results available on request).[26]

Next, we conducted placebo treatment tests using an artificial threshold to verify that the results are not driven by differences in the growth rate of the dependent variable that is correlated with the establishment size and unrelated to the policy change (cf. Martins

[26]Note that the sample size increased for the placebo tests as we only required establishments to have valid information on their employees during the time periods 2000 to 2003 instead of 2001 to 2007. We did this to increase precision in the placebo estimates and also capture potentially smaller deviations in the time trend. In addition, we estimated the pre-treatment placebo tests based on our main sample, although we lost some observations for the baseline year 2001 estimates since we had to extend the sample by the time period 2000. Again, we did not find any significant effects (results available on request).

2009). Eventually, we assumed an artificial threshold at 20 FTE employees and assigned establishments that employ 11 to 19 FTE employees from 2001 to 2003 to the treatment group and 21 to 35 FTE employees from 2001 to 2003 to the control group. The group sizes were chosen such that they exhibit a comparable sample size to the main specification while none of the establishments is subject to the change in the PADA. The estimates indicate that there are no significant differences in the flow rates contingent on the establishment size for establishments not subject to the policy change (see Table C.4 and Section I in Table C.11).

Lastly, we examined a potential distortion due to a mean reversion effect by using samples that are based on a decreasing time span for the assignment procedure. Therefore, we reestimated the regressions with a varying number of years for the assignment procedure. Tables C.5 and C.6 present the results for the worker and job flow rates using establishments that employ 6 to 9 FTE employees (treatment group) and 11 to 20 FTE employees (control group) during a two-year and one-year observation period prior to the policy change as opposed to the three-year period in our main specification. To the extent that the differences in the results are driven by a mean reversion effect, the estimates provide evidence for a downward bias of the worker flow rates. Almost all DiD estimates for the hiring and separation rate decrease with a reduction of the pre-treatment time span considered for the assignment procedure. Only the individual year effect in 2006 and the medium-term effect both for the hiring rate do not follow this trend. Moreover, the decrease in the separation rate outweighs the decrease in the hiring rate such that the job flow rate appears to be upward biased (again with the exception of the individual year effect in 2006 and the medium-term effect). To conclude, these results reveal that our main results depend on the assumption that the three-year assignment period eliminates a bias due to mean reversion while preserving a representative sample for an analysis of the causal effects under consideration.

In the case of FTC employment, the estimates under the restrictive specification (see columns (1) and (2) in Table C.7) seem, at first glance, to provide some evidence for a negative medium-term effect on the share of FTC employees. The individual year effect in 2007, the medium-term effect and the total effect become negative at least at the 10% level. F-tests of joint significance of the respective individual year effects seem to support the result at least for the medium-term effect (see section B in Table C.12). However, estimates using 2002 as the baseline year strongly contradict this finding. Columns (1) and (2) in Table C.8 show that the signs reverse and all estimates become positive. Moreover, there is strong support for a positive short-term and total effect with significant individual year effects already in 2003 as well as 2004 and 2005 (see also section C in Table C.12). Taken together, estimates for the baseline years 2002 and 2003 stand in great contrast to each other and do not allow for a causal interpretation. Unfortunately, we are not able to provide a coherent explanation for this odd result.[27]

As for the share of TWA employment, estimates under the restrictive specification remain insignificant, albeit a significant small negative total effect at the 10% level (see columns (3) and (4) in Table C.7). By further taking into consideration the insignificance of all F-tests of joint significance under the restrictive specification (see section B in Table C.12) as well as the insignificance of all DiD estimates (see columns (3) and (4) in TableC.8) and F-tests (see section C in Table C.12) using 2002 as the baseline year, we conclude that the results do not provide any evidence for a causal effect of the reform on the share of TWA employment.

Despite the inconsistent results for the effect on the share of FTC employment, we once more performed placebo tests for the time period 2001 to 2003.[28] Again, none of the estimates as well as F-tests is significant (see Table C.9 and section D in Table C.12). This also holds for the restrictive specification (results available on request). Finally, Table C.10 presents the results for placebo tests using the artificial threshold. Albeit the weak significance of the individual year effect in 2004 at the 10% level for the share of FTC employment, none of the DiD estimates and F-tests (see section E in Table C.12) is significant. Generally, we view these results as support for the common time trend assumption with respect to the share of FTC and TWA employment in our treated and control establishments.

7 Conclusion

In this paper, we provide new evidence for the impact of a change in the German PADA in 2004 on employment dynamics and temporary employment patterns in small establishments. We use detailed administrative employer-employee panel data (LIAB QM2 9310) linked to establishment survey data (IAB Establishment Panel) to estimate the causal effect of the change in the PADA on worker and job flow rates and the use of temporary employment. The identification strategy is based on a difference-in-differences approach exploiting a temporal and cross-sectional variation in the PADA.

The results provide evidence that the policy reform caused a short-term increase in the overall hiring rate of treated relative to control establishments of 2.5 to 2.7 percentage points driven by a significant individual year effect in 2004. Moreover, the estimates suggest

[27]A closer look at the data solely shows that the difference in the effects is driven by a strikingly small average share of FTC employment in the treatment group in the sample for baseline year 2002 (0.007) as opposed to average in the sample for the baseline year 2003 (0.021). For comparison, the difference in the average share of FTC employment in the control group for the baseline year 2002 and 2003 is much lower (2002: 0.023, 2003: 0.018).

[28]Since information on TWA employment is not available in 2001, pre-treatment placebo tests that contain this year are only conducted for FTC employment.

a relative short-term increase in the overall separation rate of 2.5 to 2.7 percentage points again driven by a significant individual year effect in 2004. Estimates for the medium-term effect are, however, insignificant for both the hiring and the separation rate. This suggests that the effect on worker flows is only temporary. In addition, there is no significant effect on the overall job flow rate. This does not come as a surprise since the effect size on the hiring and separation rate is similar and the two effects offset each other such that the overall effect on employment is negligible.

Furthermore, separate estimates for the flow rates of men and women suggest that the effect on the hiring rate is driven by women while the effect on the separation rate is rather driven by men. Eventually, this adds a new aspect to the evaluation of threshold effects of the PADA which has not been discussed in the literature so far. Yet, further research would be necessary to shed more light on underlying causes of this gender heterogeneity.

As for the use of temporary employment, the results do not indicate a significant effect on the share of TWA employment. Moreover, the results initially provide some very weak evidence for a medium-term decrease in the share of FTC employment. However, this result is not robust to estimates based on an alternative baseline year. Therefore, we do not regard our findings as robust evidence for a causal effect of the PADA reform on the use of temporary employment.

With regard to the theoretical literature on dismissal protection, the results only partially confirm the propositions that a relaxed dismissal protection increases worker flows and decreases the use of temporary employment. In terms of the overall worker flows, the effect is only temporary and does not persist in the medium-term. Solely the effect on the women's hiring rate seems to be persistent. Regarding the use of temporary employment, the results are inconclusive and thus cannot be regarded as support for the theory.

Although our findings provide some evidence that establishments adjusted to the relaxed dismissal protection, the economic impact appears to be of limited scope. There are a number of potential reasons for this: First, the reduction in dismissal costs may not have been of a magnitude such that it had a significant and persistent effect on employment dynamics and temporary employment patterns. Besides, some establishments may not have been aware of the change in the threshold or generally misjudge coverage by the PADA. Finally, the effect on worker and job flows and the use of temporary employment may have been mitigated by other measures of adjustment not considered in this study (e.g. changes in wages or working hours). Future research may address these issues in more detail.

Appendix A: Data

The analysis uses the Cross-sectional Model 2 of the Linked-Employer-Employee Data 1993-2010 (LIAB QM2 9310) from the German Institute of Employment Research (IAB) (for details see Heining et al. 2013). The data set contains both survey data on establishments from the annual waves of the IAB Establishment Panel and administrative data on individuals drawn from the Integrated Employment Biographies (IEB). The data was accessed via on-site use at the Research Data Center (FDZ) and subsequently via remote data access.

The IAB Establishment Panel (for details see Fischer et al. 2008) is a stratified sample of German establishments with at least one employee liable for social security payments as of 30 June in the year prior to the survey. For the years under consideration, the annual sample size amounts to roughly 16,000 establishments representing approximately 0.75% of the universe of German establishments. The data on individuals are drawn from the IEB and entail administrative data from the Employment History (BeH) which covers all employees liable for social security payments. Since the data basis of the BeH is the integrated notification procedure for health, pension and unemployment insurance, it is considered to be highly reliable. The individual-level data is supplemented with basic establishment information (e.g. 3-digit industry code) from the Establishment History Panel (BHP) (for details see Gruhl et al. (2012)).

The linked-employer-employee panel merges the data from these various sources using a unique establishment identifier. It is constructed according to the following procedure: First, all establishments from the IAB Establishment Panel with a valid interview in the respective year are selected. Subsequent, information on all individuals that are employed at one of these establishments as of 30 June is drawn from the IEB. Not all surveyed establishments can be linked to individual-level data from the IEB. However with 89 to 98%, the yearly coverage rate is fairly high and maintains the representativity of the sample for the universe of German establishments.

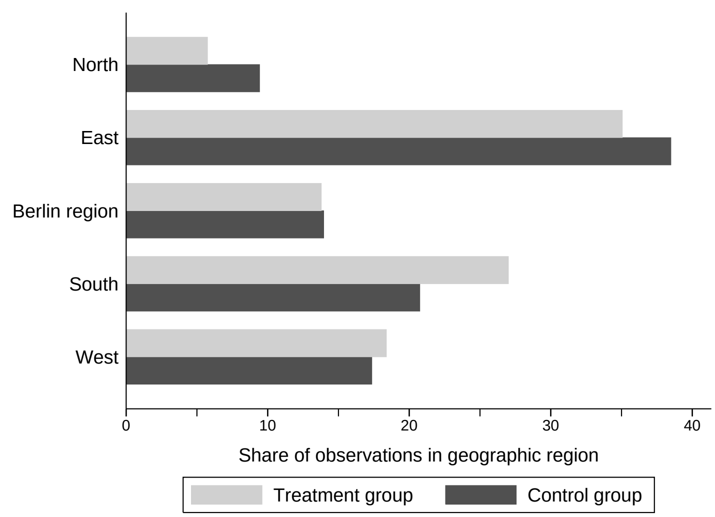
Table A.1: Determination of establishment size

Number of working hours per week	FTE Weight	PADA	LIAB QM2
Part-time	0.5	[0, 20]	[0, 18]
	0.75	(20, 30]	(18, Full-time)
Full-time	1.0	(30, ∞)	Full-time

Status of employment	PADA	LIAB QM2
Marginal employment	Included	Included
Fixed-term contract	Included	Included
Employed for less than 6 months	Included	Included
Owners and executive staff (not subject to directives)	Excluded	Excluded (if identified)
Family members without working contract	Excluded	Excluded
Vocational trainees (incl. apprentices)	Excluded	Excluded
Inactive work relationship	Excluded (if replaced)	Excluded (always)
Temporary agency worker	Excluded	Excluded

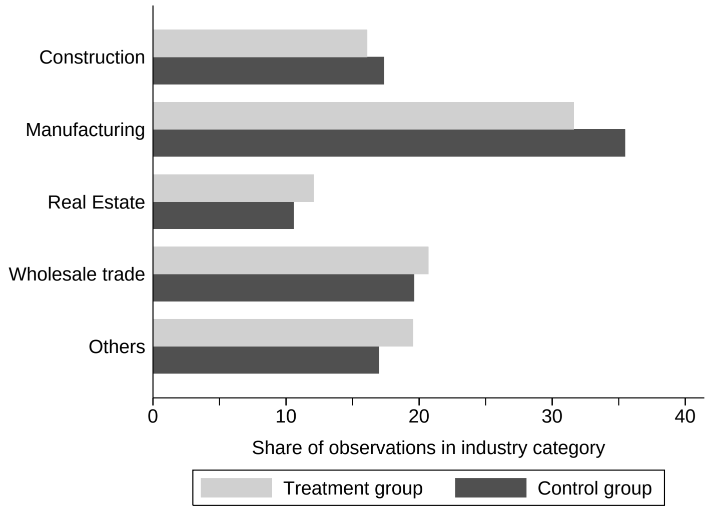
Notes: Column *FTE Weight* indicates the full-time equivalent (FTE) weighting scheme. Column *PADA* indicates the determination of the establishment size defined by the legislation. Column *LIAB QM2* indicates the determination of the establishment size based on the data.

Figure A.1: Distribution of establishment in sample by region



Notes: The region 'North' includes the federal states Bremen, Hamburg, Lower Saxony and Schleswig-Holstein. The region 'East' includes Mecklenburg Western Pomerania, Saxony, Saxony-Anhalt and Thuringia. The region 'Berlin region' includes Berlin and Brandenburg. The region 'South' includes Baden-Württemberg, Bavaria and Hesse. The region 'West' includes Northrhine-Westphalia, Rhineland Palatinate, Saarland. Due to reasons of data protection, a further regional breakdown is not possible. Source: LIAB QM2 1993-2010.

Figure A.2: Distribution of establishment in sample by industry categories



Notes: The category 'Others' includes the sectors 'electricity, gas and water supply', 'hotels and restaurants', 'transport, storage and communication', financial intermediation, 'education; health and social work', and 'other community, social and personal service activities'. Due to reasons of data protection, a further breakdown into industries is not possible. Source: LIAB QM2 1993-2010.

Appendix B: Additional results

Table B.1: Difference-in-differences results: Hiring rate by first (H1: Jun-Dec in $t-1$) and second (H2: Jan-Jun in t) half of each one-year observation period

Dep. variable	Hiring rate (H1)		Hiring rate (H2)	
	(1)	(2)	(3)	(4)
DiD 2004	0.010	0.009	0.025**	0.023*
	(0.011)	(0.011)	(0.013)	(0.012)
DiD 2005	−0.001	−0.001	0.019	0.018
	(0.009)	(0.009)	(0.012)	(0.012)
DiD 2006	0.009	0.009	0.005	0.002
	(0.011)	(0.011)	(0.013)	(0.013)
DiD 2007	0.008	0.007	0.010	0.007
	(0.011)	(0.012)	(0.012)	(0.012)
DiD 2004-05	0.005	0.004	0.022**	0.021**
	(0.007)	(0.007)	(0.009)	(0.008)
DiD 2006-07	0.009	0.009	0.007	0.005
	(0.008)	(0.008)	(0.009)	(0.009)
DiD 2004-07	0.007	0.007	0.015**	0.013**
	(0.005)	(0.005)	(0.006)	(0.006)
Controls	No	Yes	No	Yes
N	439	439	439	439

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310.

Table B.2: Difference-in-differences results: Worker and job flow rates, men

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.002	−0.001	0.015	0.016	−0.013	−0.017
	(0.012)	(0.012)	(0.011)	(0.011)	(0.016)	(0.015)
DiD 2005	−0.006	−0.007	0.020*	0.024**	−0.026*	−0.032**
	(0.012)	(0.012)	(0.012)	(0.012)	(0.014)	(0.014)
DiD 2006	−0.002	−0.003	0.002	0.004	−0.004	−0.008
	(0.015)	(0.016)	(0.012)	(0.012)	(0.017)	(0.017)
DiD 2007	−0.006	−0.007	0.012	0.012	−0.018	−0.019
	(0.013)	(0.013)	(0.012)	(0.011)	(0.016)	(0.016)
DiD 2004-05	−0.002	−0.004	0.018**	0.020**	−0.020*	−0.024**
	(0.009)	(0.008)	(0.008)	(0.008)	(0.011)	(0.011)
DiD 2006-07	−0.004	−0.005	0.007	0.008	−0.011	−0.013
	(0.010)	(0.010)	(0.008)	(0.008)	(0.012)	(0.012)
DiD 2004-07	−0.003	−0.004	0.012**	0.014**	−0.015*	−0.018**
	(0.007)	(0.007)	(0.006)	(0.006)	(0.008)	(0.008)
Controls	No	Yes	No	Yes	No	Yes
N	439	439	439	439	439	439

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

Table B.3: Difference-in-differences results: Worker and job flow rates, women

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.033***	0.033***	0.011	0.013	0.022	0.020
	(0.010)	(0.009)	(0.008)	(0.008)	(0.013)	(0.013)
DiD 2005	0.024***	0.025***	0.002	0.001	0.022**	0.023**
	(0.008)	(0.008)	(0.008)	(0.008)	(0.010)	(0.010)
DiD 2006	0.016*	0.015*	−0.008	−0.008	0.024**	0.023**
	(0.009)	(0.009)	(0.008)	(0.009)	(0.011)	(0.011)
DiD 2007	0.024**	0.022**	0.004	0.003	0.020*	0.019*
	(0.010)	(0.010)	(0.009)	(0.009)	(0.011)	(0.011)
DiD 2004-05	0.029***	0.029***	0.007	0.007	0.022***	0.021***
	(0.006)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)
DiD 2006-07	0.020***	0.019***	−0.002	−0.002	0.022***	0.021***
	(0.007)	(0.007)	(0.006)	(0.006)	(0.008)	(0.008)
DiD 2004-07	0.024***	0.024***	0.002	0.003	0.022***	0.021***
	(0.005)	(0.004)	(0.004)	(0.004)	(0.006)	(0.006)
Controls	No	Yes	No	Yes	No	Yes
N	439	439	439	439	439	439

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

Appendix C: Robustness checks

Flow rates

Table C.1: Difference-in-differences results: Worker and job flow rates, restricted specification

Dep. variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.047**	0.039**	0.028*	0.026*	0.019	0.013
	(0.020)	(0.019)	(0.016)	(0.016)	(0.024)	(0.024)
DiD 2005	0.024	0.021	0.022	0.021	0.003	0.000
	(0.019)	(0.019)	(0.018)	(0.018)	(0.023)	(0.025)
DiD 2006	0.015	0.010	0.005	0.007	0.010	0.003
	(0.023)	(0.023)	(0.018)	(0.018)	(0.025)	(0.025)
DiD 2007	0.029	0.023	0.025	0.022	0.004	0.001
	(0.020)	(0.020)	(0.019)	(0.018)	(0.024)	(0.023)
DiD 2004-05	0.035***	0.030**	0.025**	0.023**	0.011	0.007
	(0.014)	(0.013)	(0.012)	(0.012)	(0.017)	(0.017)
DiD 2006-07	0.022	0.017	0.015	0.014	0.007	0.004
	(0.015)	(0.015)	(0.013)	(0.013)	(0.017)	(0.018)
DiD 2004-07	0.029***	0.025**	0.020**	0.019**	0.009	0.005
	(0.010)	(0.010)	(0.009)	(0.009)	(0.012)	(0.012)
Controls	No	Yes	No	Yes	No	Yes
N	258	258	258	258	258	258

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 15 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310.

Table C.2: Difference-in-differences results: Worker and job flow rates, baseline year 2002

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2003	0.005	0.003	0.008	0.014	−0.003	−0.010
	(0.017)	(0.017)	(0.014)	(0.014)	(0.023)	(0.022)
DiD 2004	0.032*	0.030*	0.030**	0.031**	0.002	0.000
	(0.019)	(0.018)	(0.015)	(0.015)	(0.021)	(0.021)
DiD 2005	0.044**	0.043**	0.024	0.026*	0.020	0.017
	(0.018)	(0.018)	(0.016)	(0.016)	(0.022)	(0.023)
DiD 2006	0.019	0.017	0.003	0.003	0.016	0.014
	(0.019)	(0.019)	(0.016)	(0.016)	(0.021)	(0.022)
DiD 2007	0.030	0.022	0.033*	0.030*	−0.003	0.001
	(0.020)	(0.020)	(0.018)	(0.017)	(0.022)	(0.021)
DiD 2004-05	0.038***	0.037***	0.027**	0.028***	0.011	0.008
	(0.013)	(0.013)	(0.011)	(0.011)	(0.015)	(0.013)
DiD 2006-07	0.024*	0.023*	0.018	0.017	0.007	0.007
	(0.014)	(0.014)	(0.012)	(0.012)	(0.015)	(0.015)
DiD 2004-07	0.031***	0.030***	0.022***	0.023***	0.009	0.007
	(0.010)	(0.009)	(0.008)	(0.008)	(0.011)	(0.011)
Controls	No	Yes	No	Yes	No	Yes
N	344	344	344	344	344	344

Notes: Treatment group with 6 to 9 FTE employees in 2000 to 2002. Control group with 11 to 20 FTE employees in 2000 to 2002. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310. 9310.

Table C.3: Difference-in-differences results: Worker and job flows rates, pre-treatment placebo test

Baseline year 2001						
Dep. variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2002	0.009	0.010	−0.008	−0.010	0.017	0.021
	(0.014)	(0.013)	(0.012)	(0.012)	(0.016)	(0.016)
DiD 2003	−0.003	−0.005	−0.013	−0.015	0.010	0.010
	(0.013)	(0.013)	(0.012)	(0.012)	(0.016)	(0.016)
DiD 2002-03	0.003	0.003	−0.010	−0.013	0.013	0.015
	(0.009)	(0.009)	(0.008)	(0.008)	(0.011)	(0.011)
Controls	No	Yes	No	Yes	No	Yes
N	581	581	581	581	581	581
Baseline year 2002						
DiD 2003	−0.012	−0.015	−0.005	−0.006	−0.007	−0.009
	(0.011)	(0.011)	(0.011)	(0.011)	(0.015)	(0.014)
Controls	No	Yes	No	Yes	No	Yes
N	581	581	581	581	581	581

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Upper Panel: Baseline year 2001 with placebo treatment years 2002 and 2003. Lower Panel: Baseline year 2002 with placebo treatment year 2003. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310.

Table C.4: Difference-in-differences results: Worker and job flow rates, artificial threshold test

Dep. variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	−0.018	−0.013	−0.007	−0.007	−0.011	−0.006
	(0.014)	(0.013)	(0.010)	(0.010)	(0.018)	(0.017)
DiD 2005	−0.013	−0.010	0.004	0.001	−0.017	−0.012
	(0.014)	(0.013)	(0.010)	(0.010)	(0.017)	(0.016)
DiD 2006	−0.006	−0.003	0.006	0.002	−0.012	−0.005
	(0.015)	(0.013)	(0.012)	(0.011)	(0.019)	(0.018)
DiD 2007	0.016	0.016	0.009	0.006	0.007	0.010
	(0.014)	(0.014)	(0.011)	(0.011)	(0.017)	(0.017)
DiD 2004-05	−0.015	−0.012	−0.001	−0.003	−0.014	−0.008
	(0.010)	(0.009)	(0.007)	(0.007)	(0.012)	(0.0012
DiD 2006-07	0.005	0.007	0.007	0.005	−0.002	0.002
	(0.010)	(0.010)	(0.008)	(0.008)	(0.013)	(0.012)
DiD 2004-07	−0.005	−0.003	0.003	0.001	−0.008	−0.004
	(0.007)	(0.007)	(0.005)	(0.005)	(0.009)	(0.009)
Controls	No	Yes	No	Yes	No	Yes
N	450	450	450	450	450	450

Notes: Artificial treatment group: 11 to 19 FTE employees in 2001 to 2003. Control group: 21 to 35 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Column (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310.

Table C.5: Difference-in-differences results: Worker and job flow rates, two-year assignment period

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.027*	0.024*	0.013	0.017	0.014	0.007
	(0.015)	(0.014)	(0.013)	(0.012)	(0.019)	(0.018)
DiD 2005	0.013	0.012	0.007	0.012	0.006	0.001
	(0.013)	(0.013)	(0.013)	(0.013)	(0.016)	(0.016)
DiD 2006	0.002	−0.001	−0.006	−0.002	0.008	0.002
	(0.016)	(0.016)	(0.014)	(0.014)	(0.018)	(0.018)
DiD 2007	0.011	0.007	0.004	0.004	0.007	0.003
	(0.017)	(0.016)	(0.013)	(0.013)	(0.019)	(0.019)
DiD 2004-05	0.020*	0.018*	0.010	0.015	0.010	0.004
	(0.010)	(0.010)	(0.009)	(0.009)	(0.012)	(0.012)
DiD 2006-07	0.007	0.003	−0.001	0.001	0.008	0.002
	(0.012)	(0.012)	(0.010)	(0.009)	(0.013)	(0.013)
DiD 2004-07	0.014*	0.012	0.005	0.008	0.009	0.004
	(0.008)	(0.008)	(0.007)	(0.007)	(0.009)	(0.009)
Controls	No	Yes	No	Yes	No	Yes
N	539	539	539	539	539	539

Notes: Treatment group with 6 to 9 FTE employees in 2002 to 2003. Control group with 11 to 20 FTE employees in 2002 to 2003. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310. 9310.

Table C.6: Difference-in-differences results: Worker and job flow rates, one-year assignment period

Dep. Variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
DiD 2004	0.021	0.018	−0.002	−0.005	0.023	0.022
	(0.018)	(0.017)	(0.012)	(0.012)	(0.023)	(0.022)
DiD 2005	0.007	0.006	−0.005	−0.007	0.012	0.012
	(0.016)	(0.015)	(0.013)	(0.013)	(0.019)	(0.019)
DiD 2006	0.010	0.004	−0.017	−0.019	0.027	0.023
	(0.019)	(0.018)	(0.013)	(0.013)	(0.022)	(0.021)
DiD 2007	0.006	0.000	−0.001	−0.008	0.007	0.008
	(0.018)	(0.018)	(0.013)	(0.012)	(0.021)	(0.020)
DiD 2004-05	0.014	0.012	−0.004	−0.006	0.018	0.018
	(0.012)	(0.012)	(0.009)	(0.009)	(0.015)	(0.015)
DiD 2006-07	0.008	0.001	−0.009	−0.013	0.017	0.014
	(0.013)	(0.013)	(0.009)	(0.009)	(0.015)	(0.015)
DiD 2004-07	0.011	0.007	−0.006	−0.009	0.018*	0.016
	(0.009)	(0.009)	(0.006)	(0.006)	(0.011)	(0.010)
Controls	No	Yes	No	Yes	No	Yes
N	726	724	726	724	726	724

Notes: Treatment group with 6 to 9 FTE employees in 2003. Control group with 11 to 20 FTE employees in 2003. Results from OLS regression for empirical model (2). Columns (1), (3) and (5) do not control for any time-varying establishment-level characteristics. Columns (2), (4) and (6) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310. 9310.

Temporary employment

Table C.7: Difference-in-differences results: Share of temporary employment, restricted specification

Dep. variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
DiD 2004	−0.003	−0.005	−0.006	−0.007
	(0.007)	(0.007)	(0.006)	(0.006)
DiD 2005	−0.001	−0.002	−0.003	−0.003
	(0.014)	(0.014)	(0.005)	(0.005)
DiD 2006	−0.019	−0.020	−0.007	−0.007
	(0.012)	(0.013)	(0.005)	(0.005)
DiD 2007	−0.024*	−0.026**	−0.005	−0.005
	(0.014)	(0.013)	(0.008)	(0.009)
DiD 2004-05	−0.002	−0.003	−0.004	−0.005
	(0.008)	(0.008)	(0.004)	(0.004)
DiD 2006-07	−0.022**	−0.023**	−0.006	−0.006
	(0.009)	(0.009)	(0.005)	(0.005)
DiD 2004-07	−0.012**	−0.013**	−0.005*	−0.005*
	(0.006)	(0.006)	(0.003)	(0.003)
Controls	No	Yes	No	Yes
N	202	202	199	199

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 15 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

Table C.8: Difference-in-differences results: Share of temporary employment, baseline year 2002

Dep. variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
DiD 2003	0.019**	0.024**	0.003	0.003
	(0.009)	(0.010)	(0.005)	(0.005)
DiD 2004	0.023***	0.028***	−0.001	0.000
	(0.008)	(0.009)	(0.003)	(0.003)
DiD 2005	0.025*	0.025*	0.001	0.000
	(0.013)	(0.014)	(0.004)	(0.003)
DiD 2006	0.006	0.005	−0.004	−0.005
	(0.011)	(0.012)	(0.004)	(0.004)
DiD 2007	−0.004	−0.005	0.001	0.001
	(0.012)	(0.015)	(0.006)	(0.006)
DiD 2004-05	0.024***	0.026***	0.000	0.000
	(0.008)	(0.008)	(0.002)	(0.002)
DiD 2006-07	0.001	0.001	−0.002	−0.002
	(0.008)	(0.009)	(0.004)	(0.004)
DiD 2004-07	0.013**	0.013*	−0.001	−0.001
	(0.006)	(0.006)	(0.002)	(0.002)
Controls	No	Yes	No	Yes
N	261	261	261	261

Notes: Treatment group with 6 to 9 FTE employees in 2000 to 2002. Control group with 11 to 20 FTE employees in 2000 to 2002. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

Table C.9: Difference-in-differences results: Share of temporary employment, pre-treatment placebo test

Baseline year 2001				
Dep. variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
DiD 2002	0.003	0.004	-	-
	(0.004)	(0.004)		
DiD 2003	0.006	0.008	-	-
	(0.006)	(0.006)		
DiD 2002-03	0.005	0.006	-	-
	(0.004)	(0.004)		
Controls	No	Yes	-	-
N	642	642	-	-
Baseline year 2002				
DiD 2003	0.003	0.004	−0.001	0.000
	(0.006)	(0.006)	(0.004)	(0.004)
Controls	No	Yes	No	Yes
N	642	642	642	642

Notes: Treatment group with 6 to 9 FTE employees in 2001 to 2003. Control group with 11 to 20 FTE employees in 2001 to 2003. Upper Panel: Baseline year 2001 with placebo treatment years 2002 and 2003 (no data available for TWA employees in 2001). Lower Panel: Baseline year 2002 with placebo treatment year 2003. Results from OLS regression for empirical model (2). Columns (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

Table C.10: Difference-in-differences results: Share of temporary employment, artificial threshold test

Dep. variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
DiD 2004	−0.010*	−0.010*	−0.001	0.000
	(0.006)	(0.006)	(0.003)	(0.004)
DiD 2005	0.001	0.001	−0.003	−0.003
	(0.008)	(0.008)	(0.003)	(0.003)
DiD 2006	−0.003	−0.004	0.001	0.001
	(0.008)	(0.008)	(0.004)	(0.004)
DiD 2007	0.014	0.011	0.001	0.002
	(0.009)	(0.009)	(0.005)	(0.005)
DiD 2004-05	−0.005	−0.005	−0.002	−0.002
	(0.005)	(0.005)	(0.002)	(0.002)
DiD 2006-07	0.005	0.003	0.001	0.002
	(0.006)	(0.006)	(0.003)	(0.003)
DiD 2004-07	0.000	0.000	0.000	0.000
	(0.004)	(0.004)	(0.002)	(0.002)
Controls	No	Yes	No	Yes
N	340	340	336	336

Notes: Artificial treatment group with 11 to 19 FTE employees in 2001 to 2003. Control group with 21 to 35 FTE employees in 2001 to 2003. Results from OLS regression for empirical model (2). Column (1) and (3) do not control for any time-varying establishment-level characteristics. Columns (2) and (4) control for share of blue-collar worker, share of part-time worker, share of apprentices, average age and its square. Huber–White standard errors in parenthesis. Huber–White standard errors in parenthesis. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

F-statistics

Table C.11: F-statistics of joint significance tests: Worker and job flow rates

Dependent variable	Hiring rate		Separation rate		Job flow rate	
	(1)	(2)	(3)	(4)	(5)	(6)
A. Main specification (Table 2)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)
DiD 2004 and DiD 2005	2.96*	2.79*	3.30**	4.04**	0.12	0.13
DiD 2006 and DiD 2007	0.83	0.57	0.66	0.54	0.53	0.28
DiD 2004 to DiD 2007	1.89	1.68	1.98*	2.29*	0.32	0.20
B. Main specification, men (Table B.2)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)
DiD 2004 and DiD 2005	0.14	0.20	2.45*	3.22**	2.03	3.00**
DiD 2006 and DiD 2007	0.11	0.17	0.51	0.64	0.64	0.88
DiD 2004 to DiD 2007	0.12	0.18	1.48	1.93	1.33	1.94
C. Main specification, women (Table B.3)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)	F(q,1748)	F(q,1728)
DiD 2004 and DiD 2005	10.53***	11.62***	0.98	1.15	3.67**	3.85**
DiD 2006 and DiD 2007	4.21***	3.71**	0.58	0.47	3.91**	3.69**
DiD 2004 to DiD 2007	7.37***	7.67***	0.78	0.81	3.79***	3.77***
D. Restrictive specification (Table C.1)	F(q,1024)	F(q,1004)	F(q,1024)	F(q,1004)	F(q,1024)	F(q,1004)
DiD 2004 and DiD 2005	3.64**	2.86*	2.27	2.08	0.31	0.15
DiD 2006 and DiD 2007	1.28	0.75	0.92	0.80	0.09	0.01
DiD 2004 to DiD 2007	2.46**	1.80	1.60	1.44	0.20	0.08
E. Baseline year 2002 (Table C.2)	F(q,1710)	F(q,1685)	F(q,1710)	F(q,1685)	F(q,1710)	F(q,1685)
DiD 2004 and DiD 2005	4.31**	4.33**	3.13**	3.47**	0.40	0.27
DiD 2006 and DiD 2007	1.60	1.58	1.76	1.51	0.30	0.20
DiD 2004 to DiD 2007	2.96**	2.95**	2.45**	2.49**	0.35	0.24
F. Pre-treatment placebo test (Table C.3)	F(q,1158)	F(q,1148)	F(q,1158)	F(q,1148)	F(q,1158)	F(q,1148)
DiD 2002 and DiD 2003	0.23	0.40	0.79	1.16	0.72	1.01
G. Artificial threshold test (Table C.4)	F(q,1792)	F(q,1772)	F(q,1792)	F(q,1772)	F(q,1792)	F(q,1772)
DiD 2004 and DiD 2005	1.18	0.85	0.30	0.26	0.68	0.33
DiD 2006 and DiD 2007	0.66	0.68	0.42	0.18	0.29	0.20
DiD 2004 to DiD 2007	0.92	0.76	0.36	0.22	0.48	0.27
H. Mean Reversion: Two-year (Table C.5)	F(q,2148)	F(q,2128)	F(q,2148)	F(q,2128)	F(q,2148)	F(q,2128)
DiD 2004 and DiD 2005	2.20	1.80	0.71	1.34	0.36	0.07
DiD 2006 and DiD 2007	0.24	0.09	0.15	0.06	0.17	0.02
DiD 2004 to DiD 2007	1.22	0.95	0.43	0.70	0.27	0.04
I. Mean Reversion: One-year (Table C.6)	F(q,2896)	F(q,2868)	F(q,2296)	F(q,2868)	F(q,2296)	F(q,2868)
DiD 2004 and DiD 2005	0.78	0.59	0.10	0.21	0.72	0.74
DiD 2006 and DiD 2007	0.20	0.02	0.80	1.22	0.85	0.65
DiD 2004 to DiD 2007	0.49	0.31	0.45	0.72	0.79	0.69

Notes: The table reports F-statistics testing the null hypothesis that the difference-in-differences (DiD) coefficients are jointly zero. q is the number of coefficients being jointly tested (e.g. $q = 2$ for DiD 2004 and DiD 2005). Results in columns (1), (3) and (5) are based OLS estimates from empirical model (2) without time-varying establishment-level characteristics and columns (2), (4) and (6) with time-varying establishment-level characteristics. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310.

Table C.12: F-statistics of joint significance tests: Share of temporary employment

Dependent variable	Share of FTC employees		Share of TWA employees	
	(1)	(2)	(3)	(4)
A. Main specification (Table 3)	F(q,1356)	F(q,1336)	F(q,1336)	F(q,1316)
DiD 2004 and DiD 2005	0.33	0.29	0.06	0.15
DiD 2006 and DiD 2007	1.62	2.09	1.06	1.05
DiD 2004 to DiD 2007	0.98	1.19	0.56	0.60
B. Restrictive specification (Table C.7)	F(q,800)	F(q,780)	F(q,788)	F(q,768)
DiD 2004 and DiD 2005	0.12	0.24	0.62	0.68
DiD 2006 and DiD 2007	2.82*	3.29**	1.03	1.09
DiD 2004 to DiD 2007	1.47	1.76	0.82	0.89
C. Baseline year 2002 (Table C.8)	F(q,1295)	F(q,1270)	F(q,1295)	F(q,1270)
DiD 2004 and DiD 2005	5.60***	6.85***	0.04	0.01
DiD 2006 and DiD 2007	0.19	0.16	0.64	0.75
DiD 2004 to DiD 2007	2.90**	3.51***	0.34	0.38
D. Pre-treatment placebo test (Table C.9)	F(q,1280)	F(q,1270)		
DiD 2002 and DiD 2003	0.90	1.16	-	-
E. Artificial threshold test (Table C.10)	F(q,1352)	F(q,1332)	F(q,1336)	F(q,1316)
DiD 2004 and DiD 2005	1.42	1.41	0.42	0.35
DiD 2006 and DiD 2007	1.18	0.84	0.07	0.13
DiD 2004 to DiD 2007	1.30	1.13	0.25	0.24

Notes: The table reports F-statistics testing the null hypothesis that the difference-in-differences (DiD) coefficients are jointly zero. q is the number of coefficients being jointly tested (e.g. $q = 2$ for DiD 2004 and DiD 2005). Results in columns (1) and (3) are based OLS estimates from empirical model (2) without time-varying establishment-level characteristics and columns (2) and (4) with time-varying establishment-level characteristics. ***, ** and * denote statistical significance at the 1%, 5% and 10% level. Data source: LIAB QM2 9310 and IAB Establishment Panel.

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