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Does Tightening the Duration of Temporary Contracts Cost Jobs? Evidence from Germany

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Does Tightening the Duration of Temporary Contracts Cost Jobs?

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Abstract

Many countries limit how long a worker may be kept on a temporary contract, yet little is known about how firms and workers respond when this maximum duration is tightened. Does a shorter cap simply displace temporary jobs, or does it push firms towards permanent hiring? I provide causal evidence on this duration margin using a 2001 German reform that restricted temporary contracts longer than two years in previously exempt small firms. Using difference-in-differences combined with propensity-score matching, I find that new jobs became around five percentage points more likely to start with a permanent contract, that contract durations bunched at the cap, and that overall employment was not adversely affected. Once the first capped contracts matured, turnover of short-tenure workers rose by a third to a half, but conversions to permanent contracts dominated this churn margin, and post-reform labour-market entrants saw higher early-career earnings and job stability. The findings speak directly to the current debate over lengthening such caps, including Germany's plan to double its own from two to four years.

Keywords: Temporary Employment, Employment Protection, Labour Market Segmentation, Germany

JEL codes: J21, J41, J68

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1 Introduction

How long should firms be allowed to employ a worker on a temporary contract? Most industrialised economies restrict how long workers may be kept on temporary contracts, whether by capping their total duration, limiting renewals, or requiring an objective justification beyond a threshold, and governments repeatedly revisit where to set these limits, tightening them to steer firms towards permanent hiring or loosening them to give firms more flexibility. Yet there is remarkably little causal evidence on this duration margin: almost everything we know about regulating temporary contracts concerns whether firms may use them at all, or how short contracts are taxed, not how *long* they may run. This gap matters because the two margins need not behave alike. A restriction on duration bites precisely on the longer, more stable matches that are closest substitutes for permanent jobs, so tightening it may convert those matches into permanent employment rather than simply suppressing temporary work.

Germany offers a leading example of how live this question is. Like most European countries it restricts the duration of temporary contracts, and it is currently preparing a substantial liberalisation of this margin: the governing coalition plans to double the maximum duration from two to four years, making it considerably easier for firms to hire workers on long-term temporary contracts.[1] This is arguably one of the most consequential labour-market measures now before the German legislature, yet it would move exactly the margin on which evidence is scarcest.

This paper provides such evidence from the mirror image of the reform now under discussion. I analyse a 2001 German reform that harmonised the rules for temporary contracts exceeding two years. Prior to the reform, only larger firms had to provide a legally valid justification, such as project work or the replacement of a worker on leave, for employing someone on a temporary contract longer than two years. The reform extended this requirement to small firms with fewer than five employees, which were previously exempt, making it costly for them to keep workers on temporary contracts beyond two years, a tightening of exactly the duration margin that the current reform proposal would loosen.

The existing evidence on temporary-contract regulation comes largely from reforms at other margins (for a survey, see Boeri and Garibaldi, 2024). Liberalisations of temporary employment expanded its use without raising overall employment and depressed the earnings of young workers

[1] Temporary contracts in Germany are generally limited to a total duration of 24 months and three renewals; longer contracts require a specific legal justification (§14 of the Part-Time and Fixed-Term Employment Act, TzBfG; see Section 2.2). The labour-market reform package agreed by the governing coalition would extend these limits to 48 months and six renewals for workers hired through the end of 2030; the implementing bill is pending at the time of writing.

(Daruich et al., 2023; García-Pérez et al., 2019; Cappellari et al., 2012; Kahn, 2010).[2] At the opposite end of the duration distribution, France, Spain and Portugal introduced taxes on very short temporary contracts; Cahuc et al. (2020) show that the French tax backfired, reducing job duration and job creation. Closest to the margin studied here, Kabátek et al. (2023) find that shortening the maximum *cumulative* duration of temporary contracts in the Netherlands accelerated transitions to permanent employment, and Adrjan et al. (2026) show that restricting temporary contracts in Spain raised firm-provided training. The specific policy cell this paper occupies (a restriction on *long-term* temporary contracts) has, to my knowledge, not been studied before.[3]

The reform I study also differs in whom it affected. Much of the German flexibility debate has focused on workers with weak labour-market attachment in temporary agency work and mini-jobs, and the short-contract taxes analysed by Cahuc et al. (2020) primarily affected low-wage workers on brief contracts. The 2001 reform instead regulated a comparatively stable segment: long temporary contracts in small firms, held by workers with longer expected tenures.

To derive theoretical predictions, I extend the search-and-matching model of Cahuc et al. (2016), in which firms hire workers for production opportunities of heterogeneous expected length and choose contract types accordingly, by introducing an additional cost for temporary contracts exceeding a maximum duration $\overline{D}$. The model is deliberately illustrative: it yields signed predictions for contract composition, contract durations (including bunching at the cap), job creation and job destruction, which the empirical analysis confronts. Because long temporary contracts typically cover long productive phases, the model predicts that restricted matches are converted to permanent jobs rather than churned into shorter contracts. It also identifies the offsetting cost: matches at the cap must either convert or churn.

I estimate the effects using a difference-in-differences framework paired with propensity-score matching, comparing new hires in exempt small firms with new hires in larger firms before and after 2001. Combining Mikrozensus and social-security data, I examine the contract composition of new matches (separating permanent, short temporary and long temporary contracts), the duration distribution of temporary contracts, job creation and destruction, and unemployment flows. I also extend the analysis beyond the model's predictions by studying career impacts for labour-market entrants.

The reform substantially altered the contract composition of new matches. The dominant

[2] Conversely, Aguirregabiria and Alonso-Borrego (2014) report positive effects of the Spanish deregulation on total employment.

[3] Most evidence on temporary contracts also comes from Southern European labour markets, where temporary work is far more prevalent (about 22% of contracts in Spain against 12% in Germany in 2022, according to the OECD temporary-employment indicator) and dismissal protection is stricter (Boeri, 2011; OECD, 2013). An early exception for Germany is Hunt (2000), who studies the 1985 deregulation of temporary contracts using aggregate industry data and, like this paper, finds no significant employment effects.

response was a roughly 5-percentage-point increase in matches beginning on a permanent contract, a sizeable change equal to about a fifth of pre-reform temporary-contract use in new matches. Decomposing this shift shows that it came out of temporary contracts as a whole rather than shifting workers towards short temporary contracts, and the duration distribution of the remaining temporary contracts responded exactly as the model predicts: the share of contracts longer than two years fell by more than a quarter, and stated durations bunched at the two-year cap. In contrast, the reform had no adverse effect on overall employment: job creation in treated firms did not fall, job destruction barely moved, and net employment growth shows no sign of a cumulative decline at treated establishments. The offsetting cost that the model identifies is visible exactly where it should be: once the first contracts signed under the new rules reached the two-year cap, churning in treated firms rose by roughly a third to a half relative to its pre-reform level and short-tenure separations increased, with a small echo in unemployment inflows. The conversion margin dominates the churn margin, but both are at work.

Finally, for labour-market entrants hired into treated establishments, the reform raised cumulative early-career earnings and improved job stability. These positive long-term effects mirror the negative outcomes documented for liberalisations of temporary contracts (García-Pérez et al., 2019) and temporary-help programmes (Autor and Houseman, 2010).[4]

Together, the results show that restricting the long-duration margin of temporary employment shifted job matches towards permanent contracts and improved longer-term job security for labour-market entrants, without measurable employment costs. The theory clarifies why the same conclusion need not extend to tighter caps or other margins of temporary work.

2 Institutional Background

I will now briefly summarise the institutional background of temporary employment in Germany, and give an overview of the variation induced by the reform I analyse.

2.1 Employment protection law and temporary contracts in Germany

Most employment contracts in Germany are permanent. Dismissing a permanent employee can involve substantial firing costs in the form of notice periods, severance payments,[5] and administrative effort. Notice periods range from two weeks to seven months, depending on the worker's tenure.

[4] This analysis also relates to broader evidence on the lasting effects of labour-market conditions at entry (Altonji et al., 2016; Oreopoulos et al., 2012).

[5] Although not required by law, severance payments are often negotiated (typically about half a month's salary per year of tenure) to facilitate amicable separations and avoid legal disputes.

Employment Protection for Larger Firms: The Dismissal Protection Act (Kündigungsschutzgesetz, 1979) imposes additional restrictions on larger firms (in 2001, those with more than five employees). The relevant firm-size count is defined in the statute itself (§23 KSchG) and applied by the labour courts in full-time equivalents: employees working at most 20 hours per week count as 0.5, those working at most 30 hours as 0.75, while apprentices are excluded from the count altogether. Protection is triggered at the establishment level once the regular workforce *exceeds* five such full-time equivalents, so the hiring of a sixth employee extends dismissal protection to the entire existing workforce, a discrete cost jump at the threshold that I return to when defining treatment and control groups. Under this law, dismissals must be justified by misconduct, personal grounds such as long-term illness, or operational needs. For operational dismissals, which are the most common type, the employer must demonstrate that the position will permanently cease to exist and that no suitable vacant job is available anywhere in the firm. Because the burden of proof is high, employers frequently negotiate severance agreements to avoid lengthy legal disputes.

Temporary Employment Contracts: Alternatively, firms can also hire workers on temporary contracts, which are easy to end at expiry but difficult to terminate before the contract's end date. Temporary contracts longer than two years are permitted only if the employer provides an objective reason (Sachgrundbefristung), such as project-related work or replacing a worker on maternity or sick leave. Contracts of up to two years can be concluded without an objective reason (Sachgrundlose Befristung).[6] Within this two-year window, a contract without an objective reason may be renewed at most three times; it cannot be extended beyond two years in total unless an objective reason is stated. Until a federal employment court ruling (7 AZR 716/09) in 2011, which postdates my sample period by one year, any prior employment with the same employer also prevented the use of a contract without an objective reason. Two features of this regime matter for the analysis below. First, the binding legal object is the temporary employment *relationship* with a given employer, not the single contract: chains of short contracts with the same employer count towards the two-year limit, and the prior-employment rule forecloses restarting the clock. Second, the parameters of this window (24 months and three renewals) are exactly those the current German reform proposal would double to 48 months and six renewals, so the 2001 harmonisation studied here operates on the same statutory margin.

Table 1: Comparison of Temporary Contract Regulations Before and After the 2001 Reform

	Before 2001			After 2001		
	Temporary Contracts < 2 years	Temporary Contracts ≥ 2 years	Permanent Contracts	Temporary Contracts < 2 years	Temporary Contracts ≥ 2 years	Permanent Contracts
Treated (Less than 5 Employees)	No restriction	No restriction	No restriction	No restriction	**Justification needed** (Listed in law)	No restriction
Control (More than 10 Employees)	No restriction	Justification needed (Case Law)	No restriction	No restriction	Justification needed (Listed in law)	No restriction

NOTE.- This table summarises the relevant variation from the 2001 Part-Time and Fixed-Term Employment Act.

2.2 The 2001 Part-Time and Fixed-Term Employment Act

In January 2001, the Part-Time and Fixed-Term Employment Act (TzBfG; 2000) was enacted to implement EU Directive 1999/70/EC, which aimed to harmonise temporary employment regulations across member states. The law revised the rules for long temporary contracts in small establishments, and this change provides the key source of variation for my identification strategy.

The 2001 Reform: I summarise the variation introduced by the law in Table 1. Before the reform, firms with fewer than five employees (treated firms) were not required to justify temporary contracts lasting more than two years.[7] After the reform, these firms were subject to the same rules as firms with more than ten employees (control firms), including the obligation to provide an objective reason for extended temporary contracts. This change increased the relative cost of using temporary contracts longer than two years in treated firms. The reform did not affect permanent contracts or temporary contracts shorter than two years.

Changes in Regulations for Small Firms: Before the reform, only larger firms were required to justify long-term temporary contracts according to criteria developed through labour-court rulings. The reform made these criteria universal by writing them into law and applying them to firms of all sizes. Acceptable grounds for long temporary contracts now included job-role requirements, trial periods or periods following training or studies, substitution for another

[6] This rule for short temporary contracts was not part of the original legislation. It was introduced in 1985 as an interim measure to boost employment, renewed twice, and later made permanent. For an analysis of the 1985 reform, see Hunt (2000).

[7] These thresholds are typically interpreted in full-time equivalents.

employee, reasons related to the employee's person or the nature of the job, and situations involving the limited availability of public funds.

Firm Size and Legislative Impact: Although the reform's impact differed by firm size, this distinction did not appear directly in the statutory text. It arose through the interaction with employment-protection regulations. Before 2001, court rulings had restricted long temporary contracts in order to prevent firms from circumventing dismissal protection. Firms with fewer than five employees were exempt from dismissal protection and therefore also exempt from the case law limiting long temporary contracts. As a result, these smaller firms could use extended temporary contracts without providing justification, while larger firms could not.

Shifts in Employment Protection Over Time: Over time, the firm-size thresholds for dismissal protection have changed repeatedly. Before 1997, firms with more than five full-time equivalents were covered by employment protection. The threshold increased to ten in 1997–1998, fell back to five in 1999–2004, and then rose again to ten.[8] To account for these shifts, I define a treatment group that was consistently exempt from employment protection and a control group that was consistently above the threshold. This choice also matches the structure of the Mikrozensus, which records employee counts rather than full-time equivalents, ensuring that firms with up to five employees are always exempt from employment protection in my sample. Section 5.2 details the resulting treatment and control groups, the deliberately omitted band in between, and the robustness of the results to a stricter control-group definition.

Rules for Older Workers: Because the law also introduced new rules for extending temporary contracts for workers aged 58 and above, I exclude these workers from the analysis.[9]

3 Theoretical Framework

Intuitively, treated firms should opt for fewer long temporary contracts due to the increased legal requirements. However, the effects on other outcomes, such as overall employment or wages, are less clear a priori. In order to derive further predictions about the effects of the reform, I rely on a search and matching model by Cahuc et al. (2016), which explicitly examines firms' choices between permanent and temporary jobs. I extend this model to incorporate higher costs

[8]The 1996 and 1999 reforms are analysed by Bauer et al. (2007), and the 2004 reform by Bauernschuster (2013).

[9]Only 1.6% of observations are affected by this restriction.

for temporary contracts longer than a maximum duration $\overline{D}$ and analyse how an increase in this cost affects key outcomes.

The purpose of this section is deliberately modest: the model serves as an illustrative device that generates *testable directional predictions* for the contract composition of new matches, contract durations, job creation and job destruction, which the empirical analysis then confronts. It is not a calibrated quantitative model, and I do not use it to compute welfare effects or an optimal maximum duration. Its value lies in disciplining which signs to expect at each margin, and in making explicit the offsetting forces that render some reform effects theoretically ambiguous.

Main Idea of the Model: In the model, production possibilities have different anticipated durations. When firms and workers meet, they learn the expected productive duration of their job match and then decide whether they want to take up a job and what type of contract they will use. All production possibilities are ultimately limited in time in the model. Jobs differ in the arrival rate of shocks λ that render them unproductive. Firms and workers jointly maximise a match surplus that depends on this rate, and share it through Nash bargaining. Since temporary and permanent contracts have different termination rules, the match surplus for a given shock arrival rate varies between contract types. Unlike models in which jobs begin temporary by assumption or temporary workers can be dismissed free of charge at any time (e.g. Blanchard and Landier, 2002; Cahuc and Postel-Vinay, 2002; Faccini, 2014), the contract choice is explicit and reflects the actual termination rules: firms cannot dismiss workers during a temporary contract term but can part with them at no cost when it ends. In a recent alternative, Créchet (2024) derives the coexistence of contract types from risk sharing under dispersed match quality; there, too, easier access to temporary contracts crowds out permanent jobs rather than creating employment.

Screening and Employer Learning: An important alternative view treats temporary contracts as *screening* devices: extended probation periods during which employers learn about match quality before committing to costly dismissal protection (e.g. Faccini, 2014). The employer-learning literature shows that this motive cannot be dismissed: although Lange (2007) estimates that employers' initial expectation errors decline by half within three years, substantial uncertainty persists over the employment relationship, leaving scope for screening well beyond the first months of a match (Kahn and Lange, 2014). For Germany specifically, Boockmann and Hagen (2008) find that fixed-term contracts partly operate as prolonged probationary periods that accelerate the sorting of workers into stable jobs. For the 2001 reform, a screening model predicts the same sign as my main prediction: small firms that lose the option to extend probation beyond two years convert workers whose quality has been revealed to permanent contracts earlier. The two mechanisms differ at the hiring margin. Under screening, a binding cap on probation length makes hiring workers of uncertain quality riskier, so firms should become more selective and hire fewer hard-to-assess workers, such as labour-market entrants (for evidence on such hiring-standard responses, see Grasso and Tatsiramos, 2023; Butschek and Sauermann, 2024); under the search-and-matching mechanism, the reform reallocates matches with long expected productive durations to permanent contracts without making these hires less attractive overall. The hiring and entrant results speak directly to this distinction, and I return to it when discussing them in Section 6.3.

Modelling the Trade-off Between Contract Types: Firms have to pay a firing cost to dismiss an employee on a permanent contract if he or she becomes unproductive. However, employees on temporary contracts can be dismissed free of charge after the contract term, yet not before. If a temporary employee becomes unproductive before the contract has ended, the firm must keep paying the employee's salary until the contract term expires. Jobs that start temporary can be converted to permanent contracts when the original temporary contract expires. Alternatively, temporary contracts can be terminated free of charge after the expiration date. At the end of a temporary contract, employees and employers must decide whether to continue with a permanent contract afterwards or to terminate the job match.

Note that the model assumes that there is perfect information at the time of matching workers and firms. Therefore, the duration of employment is decided upon from the start. Explicit contract renewals are abstracted from. However, this simplification matches the legal context during my period of analysis, where long-term temporary employment required a justified cause from the very onset of a job match, especially if there was any prior employment with the same employer.

Incorporating Long vs. Short Temporary Contracts: To represent the reform, I integrate a secondary trade-off between long and short temporary contracts into the model. For this purpose, I introduce an additional cost c_{LONG} for concluding temporary contracts that exceed a duration of $\overline{D}$. In this new trade-off, employees and employers have to decide whether they want to end a temporary contract at exactly $\overline{D}$ to avoid paying c_{LONG} or whether to use a longer temporary contract. To derive predictions I study how an increase in this cost affects key outcomes. A very similar approach is also taken by Cahuc et al. (2020) who study how a tax on temporary contracts shorter than a duration $\bar{\Delta}$ (as implemented in France, Spain and Portugal) influence job creation and destruction.

The two trade-offs give rise to threshold values of the shock arrival rate λ that determine whether a job is created, which contract type it starts with, and which duration a temporary contract takes. The logic is that λ indexes how short-lived a match is expected to be: matches expected to stay productive for a long time (a low λ) are worth the firing cost of a permanent contract, matches of intermediate expected length start out temporary, and matches expected to end almost immediately are not created at all. The additional cost c_{LONG} inserts a further threshold inside the temporary region, separating matches short enough to stay under the cap $\overline{D}$ from those long enough to justify paying it. Comparative statics on these thresholds then deliver the model's predictions. I now make this premise precise and describe the equilibrium conditions for job creation, job destruction and contract durations; the predictions themselves are collected in Section 3.3.

3.1 Model Setup

The model economy consists of identical, infinitely-lived, risk neutral workers and firms, who face the same discount rate r. Since workers are identical, their total mass is normalised to 1. Labour is the only input used by perfectly competitive firms. All jobs produce the same quantity of output $y > 0$ per unit of time, but production opportunities differ in their expected duration. This difference between the expected durations is modelled as shocks, which reduce the output produced per time unit to $y = 0$ and arrive at the Poisson rate λ.[10] Job seekers and vacancies meet according to a standard constant returns to scale matching technology, and the job-type $\lambda \in [\underline{\lambda}; \bar{\lambda}]$ is randomly drawn from a distribution with $\lambda \sim G(\lambda)$ on match.

Firms and workers maximise a job-type dependent match surplus of $S(\lambda)$ and share it using Nash bargaining. Depending on the size of this match surplus, they choose between permanent and temporary contracts. Permanent contracts are open ended, but are terminated if the job becomes unproductive. At termination the employer pays a firing cost f to dissolve an unproductive permanent contract.[11] Temporary contracts have an endogenous duration $D(\lambda)$ until they expire and cannot be ended early. If a job becomes unproductive before the end of its entire term, the company must continue to pay the employee's wage until the contract expires.[12] If the contract stays productive for the whole duration $D(\lambda)$, workers and firms decide whether

[10]There is empirical evidence that temporary contract use depends strongly on the length of production opportunities. For example, Dräger and Marx (2017) find that workload fluctuations increase the likelihood of hiring temporary workers in countries with less flexible labour markets.

[11]f is assumed to be a red-tape cost and not a transfer from the firm to the worker (such as a severance pay) as such transfers can be neutralised by appropriately designed contracts (Lazear, 1990). Garibaldi and Violante (2005) discuss to what extent employment protection induces transfers from firms to employees or creates red tape firing costs.

[12]This represents the default in German temporary contract law, as a jointly determined dismissal provision between the employee and the firm is required for the premature termination of temporary contracts. Deviations from this basic rule are only possible in special cases like fraud or theft. Moreover, not all jointly determined termination provisions are legally justified. Similar rules also apply in other European countries like France, Belgium and Italy.

to dissolve the employment relationship free of any firing cost or whether to establish a permanent contract with a new wage. Agreeing upon another temporary contract after the term ends is not possible and the model abstracts from explicit contract renewals within the maximum productive duration. [13] Firms pay contract-writing costs c to establish a contract. In addition they have to pay a cost c_{LONG} if a temporary contract exceeds a duration $\overline{D}$[14] The reform is later modelled as an increase in the extra costs for writing long temporary contracts c_{LONG}.

The difference between the surplus of a temporary contract with optimal duration $S_T(\lambda, D^*(\lambda))$ and the surplus of a permanent contract $S_P(\lambda)$ determines the contract type choice in equilibrium. I provide a detailed definition of the surplus by contract type in appendix B.1.

3.2 Equilibrium Conditions

Cahuc et al. (2016) show that, given that both types of contract exist in an equilibrium, three endogenous thresholds of the shock arrival rate determine job creation, job destruction and the initial contract type: jobs start with a permanent contract below λ_S (where $S_P(\lambda_S) = S_T(\lambda_S)$), temporary jobs are converted to permanent contracts at expiry below λ_P (where $S_P(\lambda_P) = 0$), and no jobs are created above λ_T (where $S_T(\lambda_T) = 0$). The cost term c_{LONG} introduces two further thresholds that partition temporary contract durations: below λ_L the surplus from a duration longer than $\overline{D}$ justifies paying c_{LONG}, at $\underline{\lambda}$ the unconstrained optimal duration equals $\overline{D}$, and on the interval $[\lambda_L; \underline{\lambda}]$ workers and firms choose exactly the duration $\overline{D}$ to avoid the cost.[15] To simplify matters, I will only consider the case where $\lambda_L < \underline{\lambda} < \lambda_P$.

Figure 1 illustrates the resulting contract choice along the λ axis, and figure B1 in the appendix plots the corresponding duration choice.

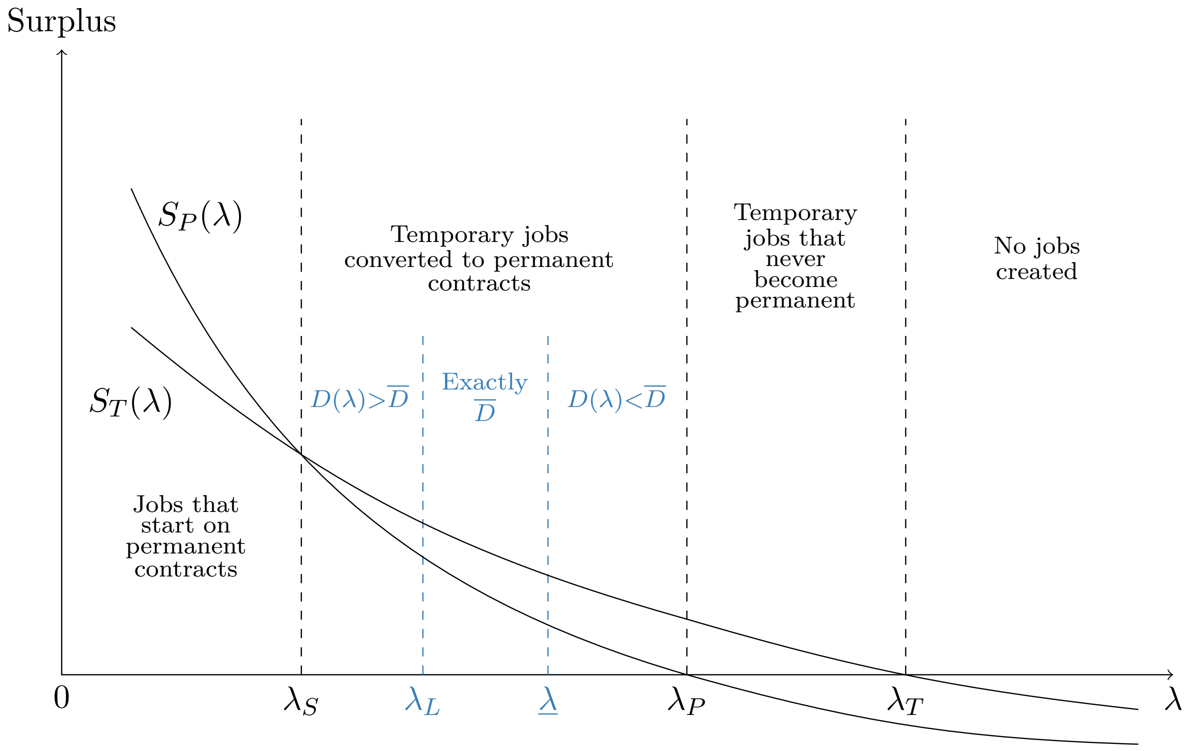
The labour market closes in the standard way. Unemployed workers u and vacancies v meet according to a constant returns to scale matching function (Pissarides, 1979), so the vacancy fill rate $q(\theta)$ and the job finding rate $\theta q(\theta)$ depend only on labour market tightness $\theta = \frac{v}{u}$; posting a vacancy costs $\kappa > 0$, and wages are set by Nash bargaining with a worker surplus share of γ. Free entry of firms drives the expected surplus of a vacancy down to its posting cost, which pins down equilibrium tightness, and the value of workers' outside option rises with tightness, since jobs are found more quickly. Because the surplus of temporary contracts longer than $\overline{D}$ enters the free-entry condition net of c_{LONG}, the cost of long temporary contracts feeds back

[13] While it is theoretically possible to establish consecutive temporary contracts with a valid justified cause in Germany, the law does not regard this as the norm. Renewing a temporary contract requires a special new justified cause for an extension. During my period of analysis any prior employment at an employer be it permanent or in a short temporary contract without an explicit justification made it impossible to defend a justified cause in litigation.

[14] Both terms represent legal costs of writing contracts. c_{LONG} represents the legal costs to agree upon a valid justified cause for long temporary contracts.

[15] A detailed overview of all equilibrium conditions can be found in appendix B.2.

Figure 1: Choice Between Temporary and Permanent Contracts



into tightness and hence into every threshold. The formal statements of the free-entry condition and the outside option, together with the derivations of all comparative statics used below, are collected in appendix B.2.

3.3 Model predictions

How does an increase in the cost c_{LONG} of establishing a temporary contract longer than $\overline{D}$ affect job creation, job destruction, the share of contracts that are started with a permanent contract and contract duration? I now discuss the reform effects on the main outcomes that I analyse. A more detailed description of the comparative statics for the equilibrium values of the shock arrival rate parameters and labour market tightness is discussed in the appendix (see the section on comparative statics in B.2)

Introducing contract writing costs for long temporary contracts c_{LONG} reduces labour market tightness, since the potential surplus of temporary jobs with contract durations longer than $\overline{D}$ decreases. This in turn also reduces the outside option of workers. Due to the lower outside option temporary jobs with very short-term expected production opportunities become more desirable ($\lambda_T \uparrow$) and more jobs are created after the reform.

At the same time, the reduction in the value of the outside option of workers, combined with the increase in the cost of long temporary contracts, reduces the relative cost of continuing

Table 2: Model predictions

	Prediction
Job creation	More job creation ($\lambda_T \uparrow$) as jobs with very short expected productive durations become more attractive as labour market thightness decreases overall
Job destruction	Less job destruction as more temporary jobs are converted to permanent contracts once their term expires ($\lambda_P \downarrow$)
Share of new permanent contracts	As the surplus of fixed-term contracts decreases on the interval between 0 and $\underline{\lambda}$ a direct effect increases the share of jobs that start with a permanent contract ($\lambda_S \uparrow$). However, there is a feedback effect as the lower labour market thightness decreases the surplus of permanent contracts indirectly due to a decreasesd outside option ($U(\theta) \downarrow \Rightarrow \lambda_S \downarrow$)
Contract duration	An decrease in the share of temporary contracts longer than $\overline{D}$, as the new cost introduces an interval between λ_S and $\underline{\lambda}$ where it is optimal to choose $\overline{D}$ instead (see figure B1 in the appendix).

NOTE.- This table contains a short overview of the main model predictions.

temporary jobs with a permanent contract after they have reached the end of their term. Hence, more temporary jobs are converted to open ended contracts (λ_P increases), which in turn reduces job destruction.

However, the impact of the reform on the share of contracts that directly begin with a permanent contract depends on two countervailing reform effects. First, for a given level of labour market tightness, creating a job directly with a permanent contract becomes more attractive as the cost c_{LONG} reduces the surplus of temporary contracts for all shock arrival rates between 0 and λ_L directly. Secondly, a decline in labour market tightness can offset this effect through a reduction in the outside option of workers. The overall effect is not clear from the onset and depends on different model parameters.

Lastly, the introduction of c_{LONG} into the model creates an interval $[\lambda_L; \underline{\lambda}]$ on which it is optimal for workers and firms to choose temporary contracts with exactly the duration $\overline{D}$, while durations on this range of λ are longer than $\overline{D}$ if there are no additional costs for long temporary contracts. Thus, the reform should decrease the share of temporary contracts that are longer than two years and produce *bunching* of contract durations at exactly $\overline{D}$, a distinctive prediction that I test directly in Section 6.3.

The Downside: Convert or Churn: The predictions above emphasise the benefits of restricting long temporary contracts: more conversions to permanent contracts and less job destruction. The same comparative statics, however, also identify the policy's cost. As c_{LONG} rises, every match whose unconstrained duration exceeds $\overline{D}$ faces a forced decision at the cap: *convert or churn*. On the conversion margin, matches on the interval $[\lambda_L; \underline{\lambda}]$ are converted to permanent contracts earlier than is privately optimal, so firms bear firing-cost risk on jobs that turn unproductive between $\overline{D}$ and the unconstrained duration $D^*(\lambda)$. On the churn margin, matches with high shock arrival rates whose surplus no longer justifies paying c_{LONG} are terminated at $\overline{D}$ even though they are still productive in expectation; the worker flows into unemployment and the firm must post a fresh vacancy to fill the remainder of the production opportunity. Both margins reduce the surplus of affected matches (λ_L falls with c_{LONG}), and the churn margin raises turnover and unemployment inflows for the affected matches. The net effect trades these costs off against the conversion gains, and the cost side grows as the cap $\overline{D}$ becomes shorter: a very tight cap would force churn on a large share of matches, so a restriction of this kind cannot be beneficial for arbitrarily short maximum durations. This trade-off is exactly what is at stake in the current German debate about doubling the maximum permitted duration of temporary contracts from two to four years, which I return to in the conclusion. Whether the conversion or the churn margin dominated for the 2001 reform is an empirical question; the job-creation, job-destruction and unemployment-flow results in Section 6.3 speak directly to it.

Comparison with Taxes on Short Contracts: It is instructive to contrast this restriction on *long* temporary contracts with the taxes on *short* temporary contracts studied by Cahuc et al. (2020). A tax on short contracts targets matches with brief expected production opportunities, for which a permanent contract is not a viable substitute: the alternative to a taxed short contract is often no job at all, which is why Cahuc et al. (2020) find that such taxes reduce job creation, shorten mean job duration and lower welfare. A restriction on long temporary contracts instead targets matches with long expected productive durations, precisely the matches for which a permanent contract is the closest substitute. The model therefore predicts substitution towards permanent contracts rather than towards non-employment, which is consistent with the contract-composition results below. The two policies sit at opposite ends of the duration distribution, and their contrasting effects underline that the design margin (which durations are made more expensive) matters more than the sign of the intervention.

4 Data Sources

To explore the impact of the 2001 restriction on long-term temporary contracts, I rely on data from two distinct sources. The Mikrozensus, a representative survey that includes 1% of the German population, is the central data set for this analysis, as it is the only employee-level data set that contains information on contract types and durations at the time of policy change in Germany. This allows me to assess the reform's influence on the proportion of new temporary contracts and their respective durations. However, due to its design as a repeated cross-section, the Mikrozensus cannot track individual employment paths over time, a key aspect in understanding the reform's effects on individual employment outcomes and long-term job stability. To compensate for this limitation, I use a 2% sample of German social security data.

4.1 Mikrozensus

I focus on the years from 1996 to 2010, starting with the year the Mikrozensus began asking about temporary contracts and their duration. In contrast, German Social Security data only include the employment contract type for years after 2011. Various features of the data provide a good starting point for analysing the reform.

Contract Type and Duration Variables: Most importantly, the survey contains two detailed questions on temporary contracts and their official duration. Unfortunately, an analysis of the contract duration variable is complicated due to several issues. The most significant issue is that the categorisation of contract durations changed seven times between 1996 and 2010. Although each version of the question aims to capture the number of months specified in a temporary contract, the available response options are given as categories and undergo regular changes. Prior to 1999, the category for contracts lasting 24 months (the maximum duration for short temporary contracts) is grouped together with durations of 20, 21, 22, or 23 months. From 1999 onwards, the variable is mostly continuous, although there are minor changes to the categories of durations exceeding three years. Yet, it is still possible across all waves of the Mikrozensus to distinguish contracts that are shorter or longer than two years, which allows me to estimate how the share of the newly restricted long temporary contracts was impacted.

Identifying Firms Affected by the Reform: Secondly, the data allows me to identify if a worker's firm is impacted by the reform. The law's restrictions on long temporary contracts apply only to firms exempt from employment protection. This exemption is solely determined by the firm's number of full-time equivalents (FTE). Notably, the exemption threshold fluctuated between 5 and 10 FTEs due to changes in 1999 and 2004. To ensure consistent and reliable comparisons, I focus on firms with 5 or fewer employees, consistently exempt throughout these changes. This treatment criterion also addresses the gap between the legal definition of the exemption and the Mikrozensus firm size question, which measures a firm's total number of employees instead of specific FTEs. Because firms with fewer than 5 employees will have fewer than 5 FTEs, it's straightforward to identify those impacted by the reform.

Control Group Considerations: While the treatment group is very clear cut, the Mikrozensus constrains the control-group choice: for firms with more than 10 employees the firm-size variable offers only three categories (11 to 19, 20 to 49, and more than 50 employees), and I exclude firms with over 50 employees throughout because their hiring patterns differ strongly from those of smaller firms. Section 5.2 discusses the resulting group definitions, the deliberately omitted 5–10 band and the contamination robustness check using only firms with 20 to 49 employees as controls.

Analysing New Job Matches: Lastly, I can identify roughly 3,000 new job matches per year, which allows for detailed insights into the effects of the reform on the hiring behaviour of firms and the contract types used.

Socio-Demographic Controls and Sample Restrictions: In addition, the Mikrozensus offers a rich set of socio-demographic controls, such as age, gender and education. I distinguish three skill groups based on the highest educational qualification obtained. An individual is medium-skilled if he or she has completed an apprenticeship or graduated from high school (*Abitur*). A person is high-skilled if he or she graduated from college. Moreover, detailed information on industry of employment (at the 2-digit level) is also included.

For the analysis, I restrict the sample to West German individuals between the ages of 20 and 58, since the reform also introduced new rules for temporary contracts for older workers. I further exclude individuals that are either self-employed, in civilian or military service, or in vocational training, since the legal rules regarding employment protection and temporary work do not apply to these groups.

4.2 Social Security Data and Establishment History Panel

Although the Mikrozensus contains detailed information on contract types, I cannot use it to analyse employment effects because I cannot track individuals in my sample over time.

Job Creation and Job Destruction: However, administrative social security data allow for a detailed analysis of the theoretical predictions on job-creation and destruction, as it is possible to follow individuals over time. Although contract types are not directly observable in the social security data for the reform period, this allows for both analyses on the transition between unemployment and employment and an analysis on the long-term reform effects for labour market entrants. Consequently, I use a 2% random sample of the population of workers and firms covered by the social security system in Germany in order to study employment effects and long-term reform outcomes in greater detail. The data provide information on each individual's employment status in the social security system as of June 30th for each year.

For the establishment-level outcomes, I draw on the Establishment History Panel extension of these data, which aggregates the underlying notifications to yearly establishment records. Beyond employment counts and workforce composition, the extension provides establishment-level counts of worker inflows and outflows, including separations by tenure class, and a typology that distinguishes genuine establishment births and deaths from identifier changes, spin-offs and takeovers (Hethey-Maier and Schmieder, 2013); I use the typology to purge the entry and exit margins of job creation and destruction of such spurious events. One institutional break requires care: jobs with earnings below the marginal-employment threshold only became notifiable in April 1999, so recorded employment jumps around that date, most strongly in small establishments. I measure all employment stocks and flows net of jobs flagged as marginal, but the flag is itself incomplete during the phase-in of the new notifications, so netting alone does not fully remove the jump in measured growth. My preferred specifications therefore exclude the affected years 1999–2000, with full-window estimates reported in the appendix. The panel covers establishments that ever employed a worker from the 2% sample, so the very smallest establishments are underrepresented relative to their population share; the estimation sample nevertheless contains 68,880 treated establishments with fewer than five employees. The matching on establishment characteristics addresses this feature, but it should be kept in mind when comparing magnitudes with the Mikrozensus results.

I apply the same sample restrictions as for the Mikrozensus data to make the results comparable across data sets. Since the education variable in the social security data is missing for about 20% of the observations, and exhibits some inconsistencies over time, I use the panel structure of the data to impute education in the current year from past and future spells following Fitzenberger et al. (2006).

4.3 Measurement and Data Limitations

Both data sources have limitations that shape what my estimates can and cannot show, and I state them here explicitly.

Self-Reported Firm Size: In the Mikrozensus, firm size is reported by *workers*, not by firms, and refers to a headcount rather than to the full-time-equivalent measure that the law and the labour courts apply. Recall error and the headcount–FTE gap can therefore misclassify treatment status for some observations. Two features limit the resulting bias. First, my treatment definition (five or fewer employees) is conservative: a firm with at most five employees mechanically has at most five full-time equivalents, so treated observations are correctly classified whenever the reported headcount is accurate; misclassification through the FTE gap operates only in the control group, where it would attenuate the estimated effects. Second, classical misclassification of a binary treatment biases difference-in-differences estimates towards zero, so the reported contrasts are, if anything, lower bounds.

Worker-Level Sampling: Because the Mikrozensus samples individuals, my estimates are employment-weighted: they describe the contract composition of new *job matches*, not the behaviour of the average *firm*. Larger treated firms contribute more observations than the smallest ones. Statements about firms would require reweighting by the inverse of firm size; I deliberately phrase all results in terms of job matches and new hires, for which the worker-level design is the appropriate one.

No Contract Types in the Social Security Data: The social security data do not record contract types in the reform years. All analyses based on these data therefore identify reform effects through the firm-size assignment rule alone and should be read as reduced-form effects of the reform on employment flows and careers, without conditioning on contract type. The contract-composition channel is established with the Mikrozensus; the social security data show what the reform did, or did not do, to employment stocks, flows and entrant careers.

Income and Earnings Measures: The Mikrozensus income question captures net monthly income from all sources, not the gross hourly wage of the sampled job, and I therefore use it only descriptively. The career analyses for labour-market entrants instead rely on administrative daily earnings from the social security records. Even there, a caveat applies: if treated firms became more selective in hiring after the reform, part of any earnings gain for post-reform entrants could reflect changes in workforce composition rather than treatment effects on given workers. The matching strategy addresses selection on observables, and the absence of any decline in hiring (Section 6.3) limits the scope for tightened hiring standards, but selection on unobservables cannot be fully excluded.

5 Empirical Strategy

The 2001 German reform introduced tighter controls on long temporary contracts in small firms only. To take advantage of this variation, I adopt a difference-in-differences approach, comparing firms with fewer than 5 employees (treatment group) to those with 10 or more employees (control group), before and after the reform. The firm-size categories entering the regressions are firms with 1 to 5, 10 to 19 and 20 to 49 employees.

The main estimation equation is given by

$$\textsc{Outcome}_{ift} = \alpha\textsc{Treated}_{if} \times \textsc{Post 2001}_t + \beta\textsc{firm-size}_f + \gamma\textsc{year}_t + \varepsilon_{ift}, \tag{1}$$

where i indexes individual new job matches, f indexes firm-size categories, and t indexes years. I include year- and firm-size-category dummies to capture year specific shocks to all firms and time-constant level differences between different firm-size groups.

The variable $\textsc{Treated}_{if} \times \textsc{Post 2001}_t$ is an interaction effect between the treatment status of a worker's firm and an indicator variable for years after 2001.

Since I control for the firm-size categories that determine a firm's treatment status and year fixed effects, the effect of the reform α is identified by the change in the outcome variable in treatment firms relative to control firms, in 2001 or later relative to 2000 or earlier.

Strategic Firm Size Adjustments: One additional concern is that firms might have strategically adjusted their size in response to the reform: size-contingent labour regulations are known to distort the firm-size distribution around exemption thresholds (Schivardi and Torrini, 2008; Garicano et al., 2016). The direction of the incentive is worth spelling out. A treated firm could not escape the new restrictions by growing: after 2001 the objective-reason requirement applied to firms of all sizes, and crossing the five-employee threshold additionally triggers dismissal protection for the entire workforce. What the reform did change is the *value of staying small*: before 2001, remaining below five employees exempted a firm from both dismissal protection and the case law on long temporary contracts; afterwards, only the dismissal-protection exemption remained. If bunching below the threshold had been partly motivated by the temporary-contract exemption, marginal treated firms should have grown past five employees after the reform, selectively changing the composition of the treatment group. If the distribution of firm sizes shifted significantly after the reform, it would undermine the assumption that job-matches in the treatment and control groups are comparable over time. To investigate this concern, I analyse the firm-size distribution before and after the policy change. Figure A1 in the appendix illustrates the density of firm sizes for firms with 0-50 employees for the years around the reform. The graph shows a stable distribution over time, with no notable shifts in firm-size density and, in particular, no post-reform thinning of the mass just below five employees. The switching behaviour between firm-size groups, shown in Figure A2 in the appendix, also reveals no systematic switching between the treatment and control groups: the share of treatment firms growing beyond five employees is flat through the reform year. These findings indicate that firms did not engage in strategic size adjustments in response to the policy.

The Omitted 5–10 Employee Range: Firms with six to nine employees belong to neither group in my design, and this omission is deliberate. Their treatment status is ambiguous for two reasons. First, the employment-protection threshold moved within this range during the sample period (five employees before 1997 and from 1999 to 2004, ten in 1997–98 and after 2004), so firms in this band switched legal regimes repeatedly for reasons unrelated to the 2001 reform. Second, the legal threshold counts full-time equivalents while the Mikrozensus reports headcounts, and the two diverge most in exactly this range: a firm reporting seven employees may fall on either side of the five-FTE cutoff depending on part-time composition. Excluding the band therefore removes the firms whose assignment would be least reliable, at the cost of a gap between treatment and control that widens the scope for level differences between the groups, differences that the firm-size fixed effects absorb and that the matching further reduces. The robustness check using only firms with more than 20 employees as controls (appendix table A2) shows that the results do not depend on how close the control group sits to this ambiguous range.

Inference: Since the treatment status is determined by the firm-size categories, I cluster the standard errors of my estimate by these categories. Due to the limited number of such clusters, I've employed randomization inference (MacKinnon and Webb, 2020) for more robust p-value estimation. This entails permuting treatment allocation and re-estimating the respective model 2,000 times, with detailed distributions of the main coefficient presented in the appendix.

Other Events: Simultaneous external events that impact both the treatment and control groups pose a challenge to the assumptions for a difference-in-differences analysis. The early 2000s saw significant events that could have influenced firms and workers in various industries differently. These included China joining the WTO in 2001, the European Union's eastward expansion in 2004, and the series of Hartz reforms between 2003 (Hartz I to III) and 2005 (Hartz IV). Although these events did not specifically target firms based on size and were not directly related to temporary employment, they may have had disproportionate effects on firms in the treatment and control groups.

5.1 Propensity-Score Matching

To mitigate this, I have implemented a nearest-neighbour propensity score matching method, focusing on observable characteristics such as the industry of the firm (categorized by a two-digit code), as well as the age and educational background of employees for every job match in the Mikrozensus. This approach aims to enhance the comparability of the treatment and control groups, significantly diminishing the risk of misattributing effects to the reform that could actually be caused by external events. Furthermore, this method helps to account for differences within the groups, ensuring that any post-reform outcome disparities are more reliably attributed to the reform itself, rather than to pre-existing differences in firm and worker characteristics between the groups. I report results for a matched and an unmatched sample throughout the paper. [16]

Balance After Matching: The matching process effectively reduces the initial disparities between the groups. Figure A3 in the appendix displays the absolute mean differences in the matching covariates between the treatment and control groups before and after matching: the initially marked differences in industry and educational composition fall to subgroup-share differences that rarely exceed one percentage point in the matched sample.

Common Support: At the same time, there exists strong common support between the groups, as demonstrated in Figure A4 in the appendix. The propensity scores of both the treatment and control groups have a substantial overlap, validating the effectiveness of the matching procedure in identifying control units that closely match the treated units in terms of the observed covariates used to compute the propensity score.

Matching for Social Security Data: For outcomes based on social security data, such as job creation and destruction, worker flows and long-term outcomes for labour market entrants in affected firms, I use a similar matching strategy. However, since I can track pre-existing firms and their workers over time in the social security data, I can match on the firm level instead of the individual job-match level. Treated and control establishments are matched on their pre-reform (2000) characteristics: two-digit industry, the log median full-time wage, establishment age, the female, part-time, high-qualification and low-qualification shares of the workforce, and mean workforce age. I use nearest-neighbour propensity-score matching with replacement and a caliper, so that treated establishments without a sufficiently close control are dropped and control establishments can serve as matches for several treated units; the resulting frequency weights enter all matched estimates. In practice the caliper barely binds and the reuse of controls is moderate: of 68,880 treated establishments, a single one is dropped for lack of a close control, and the 29,769 control establishments serving as matches carry a median frequency weight of one and a mean of 2.3, with the 99th percentile at 18. Figure A5 in the appendix reports covariate balance as standardised mean differences. Matching removes the large raw imbalances, most notably in the log median wage (a standardised difference of 0.82 before matching, 0.00 after),

[16]For the implementation of the matching procedure and the difference-in-difference approach, the MatchIT, fixest, and Modelsummary R packages were used (see Ho et al. (2011), Bergé (2018), and Arel-Bundock (2022)).

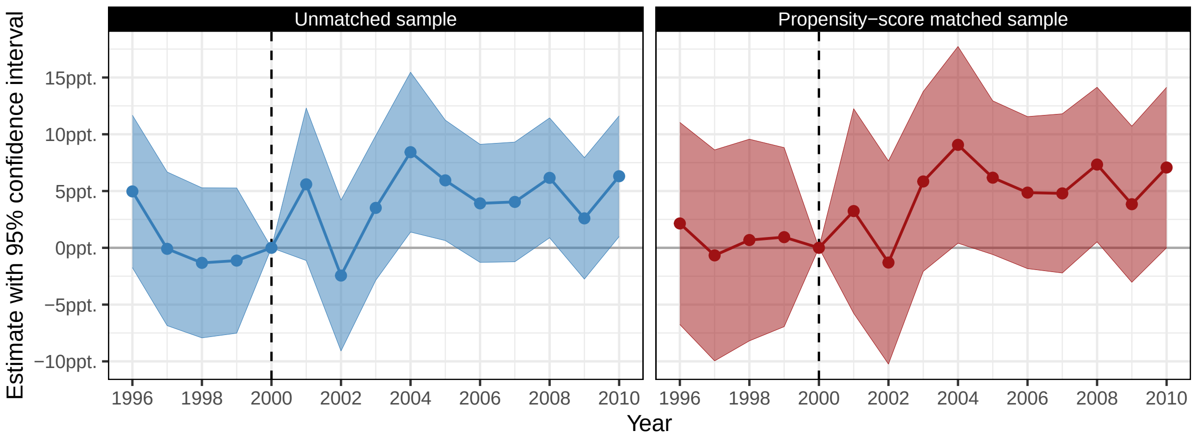
establishment age (0.55 to 0.08) and the female share (0.50 to 0.05), and aligns the industry composition almost completely. The remaining imbalances are small: mean workforce age sits exactly at the conventional threshold of 0.1 (0.07 to 0.10), and the low-qualified share, the one covariate whose imbalance the matching itself introduces (0.00 to 0.13), slightly exceeds it. The difference-in-differences design absorbs the associated level differences through the group indicator.

While the matching procedure improves the comparability of my treatment and control groups, the validity of my difference-in-differences identification strategy relies on the key premise that job-matches in the treatment and control groups would have followed the same trends if there was no reform.

5.2 Parallel Trends

To verify this, I examine the pre-trends of the control and treatment groups through a typical event study. Therefore, I replace the single difference-in-differences indicator from equation 1 with interactions between each calendar year and the treatment indicator, using 2000 as the reference year. Moreover, the event-study enables me to investigate the timing of the effects.

Figure 2: Event-study plot for the reform



Note.- The figure plots the coefficient of interactions between years and the treatment indicator in a regression of a permanent contract indicator for new job matches on firm-size category fixed effects and year-fixed effects. The left panel presents the estimates for an unmatched sample, whereas the right panel shows the estimates for a matched sample.
Source: Mikrozensus sample for West German employees aged 20 to 58

Figure 2 plots the event-study coefficients for the share of job-matches that start directly with a permanent contract both for the unmatched (left panel in blue) and matched (right panel in red) samples. For pre-reform years, the coefficients of the interactions of treatment status and the calendar year are close to zero and statistically insignificant. Consequently, the trends in the treatment and control group are parallel prior to the reform. In addition, the matching moves these coefficients even closer to zero.

There is an immediate reaction after the reform in 2001, however a slight decrease of the effect in 2002. Yet, after 2003 the increase in the share of job-matches that start with stabilises at about 5 percentage points. Thus, the reform is largely persistent.

6 Results

6.1 Impact on employment contracts of new job matches

I now explore how the reform has influenced the contract types used in new job matches. The reform increased the cost of temporary contracts exceeding two years for impacted firms, likely reducing their usage. Furthermore, for treated firms, my theoretical model predicts a higher rate of job matches starting as permanent contracts post-reform.

This is because the new costs of long contracts, introduced by the reform, lower the surplus for temporary contracts in job matches that have long anticipated production opportunities, while the surplus of permanent contracts at similar productive durations remains unaffected. Yet, this theoretical prediction depends on the employment outcomes of the reform, since a simultaneous reduction in labour market tightness might offset this effect by also reducing the surplus of permanent contracts.

Turning now to the empirical evidence, Table 3 presents difference-in-difference estimates for the reform's impact on the contract types used in new job matches using Mikrozensus data. Panel A decomposes the contract composition of new job matches with a common denominator (all new matches), for the unadjusted and the propensity-score matched sample; Panel B reports the duration margin among new temporary contracts.

The share of job matches that begin with a permanent contract increases by 4.199 percentage points in the unadjusted sample and by 4.810 percentage points in the propensity-score matched sample. Since 21.0% of new job matches in the matched sample started with a temporary contract before the reform, the effect represents an economically substantial reduction of more than a fifth in the use of temporary contracts.[17] These results are also highly statistically significant at the 1%-level. Additionally, randomization inference tests, based on 2,000 models with random treatment assignments, further confirm the significance of these findings, with a

[17] The raw share of temporary contracts among new matches rose over the sample period in both the treatment and the control group, reflecting a secular trend in German hiring practices. This common trend is absorbed by the year fixed effects; the difference-in-differences estimates measure the relative shift between the groups, that is, how much less the temporary share rose in treated firms than it would have without the reform.

Table 3: Reform effects on the contract composition and durations of new job matches

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unadjusted				Propensity score matching			
Panel A: Contract composition, share of all new job matches								
	Permanent	Temporary	Short temp. ($\leq$ 2 yrs)	Long temp. (> 2 yrs)	Permanent	Temporary	Short temp. ($\leq$ 2 yrs)	Long temp. (> 2 yrs)
Treated × Post 2001	4.199***	-4.199***	-4.113***	-0.008	4.810***	-4.810***	-4.346***	-0.442***
	(0.021)	(0.021)	(0.017)	(0.009)	(0.001)	(0.001)	(0.002)	(0.001)
N	41710	41710	41710	41710	22091	22091	22091	22091
Adj. R^2	0.03	0.03	0.03	0.00	0.03	0.03	0.03	0.00
Panel B: Duration margin, among new temporary contracts								
	Long (> 2 yrs)	Exactly 24 months			Long (> 2 yrs)	Exactly 24 months		
Treated × Post 2001	-0.376*	0.549*			-2.501**	1.812**		
	(0.043)	(0.043)			(0.090)	(0.037)		
N	13241	12100			6184	5801		
Adj. R^2	0.02	0.00			0.01	0.00		
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Firm-size Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES

NOTE.- Panel A decomposes the reform effect on the contract composition of new job matches. All Panel A outcomes share the same denominator (all new job matches): the share starting with a permanent contract, with any temporary contract, with a short temporary contract ($\leq$ 24 months) and with a long temporary contract (> 24 months). Temporary contracts with missing stated duration are included in the temporary share but in neither duration category, so the two duration columns do not mechanically sum to the temporary column. Panel B reports effects on the duration distribution among new temporary contracts: the share longer than 24 months and the share with a stated duration of exactly 24 months, the bunching margin predicted by the model. Because the Mikrozensus groups contract durations of 20 to 24 months in a single category before 1999, the bunching outcome is estimated on the 1999–2010 waves only. Columns (1) to (4) present results for an unmatched sample; columns (5) to (8) for the sample matched via nearest-neighbour propensity-score matching. The results are expressed in percentage points. Because the treatment varies at the level of firm-size categories, standard errors are clustered by firm-size category; given the small number of clusters, randomization inference (figure A6 in the appendix) complements the cluster-robust inference.
Cluster-robust standard errors for firm-size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

p-value of 0.002 for the matched sample (see figure A6 in the appendix).[18] The uptick in the use of permanent contracts also aligns with the predictions from my theoretical model.

The remaining columns of Panel A show where this shift comes from: the increase in permanent contracts is mirrored, one for one, by a decline in temporary contracts as a whole, and splitting the temporary share by stated contract duration attributes −0.4 percentage points of the matched decline to measured *long* contracts (more than 24 months) and −4.3 percentage points to contracts with stated durations of up to 24 months.

At first sight it may appear puzzling that a restriction on long contracts should reduce the use of short ones; if anything, a naive substitution logic would predict the opposite. The resolution lies in the legal object of the reform and in what the Mikrozensus measures. As discussed in Section 2.2, the binding legal constraint applies to the temporary employment *relationship* with an employer, not to the individual contract: before the reform, a small firm could start a worker on a short contract and extend the relationship beyond two years without stating a reason; after the reform, this extension option was foreclosed, both directly and through the prior-employment rule. The Mikrozensus records the stated duration of the worker's *current* contract, so an intended long temporary relationship built from chained or extended short contracts appears in the data as a short contract. The reform therefore reduced the option value of *starting* any

[18] In the randomization inference test, the actual coefficient is markedly more extreme, positioned well to the right of the distribution of coefficients from the random models as highlighted by the histograms in figure A6.

temporary contract for matches with long expected horizons, and it is exactly these matches that the model predicts will shift to permanent contracts. The decline in measured short contracts is thus the footprint of the restricted long-duration margin, not evidence of an effect on genuinely short employment relationships.

Direct evidence for this duration-margin interpretation comes from the within-temporary duration distribution in Panel B of Table 3. Among new temporary contracts, the share longer than 24 months falls by 2.5 percentage points in the matched sample (more than a quarter of the treated pre-reform mean of 9.0%), while the share with a stated duration of *exactly* 24 months rises by 1.8 percentage points from a pre-reform base of only 0.9%. This bunching at the two-year threshold is precisely the prediction of the model, in which matches on the interval $[\lambda_L; \underline{\lambda}]$ optimally choose the duration $\overline{D}$ to avoid the cost of justifying a longer contract.[19] It also echoes the concentration of contract transitions at statutory duration limits that Güell and Petrongolo (2007) document for Spain. Two features of Panel B deserve comment. First, the gap between the unadjusted and matched estimates of the long-contract effect (-0.4 against -2.5 percentage points) is much larger than for the permanent-contract outcome. Panel B conditions on the roughly one quarter of matches that are temporary, a small sample whose industry and skill composition differs markedly between small treated firms and larger control firms; matching aligns this composition, and because long temporary contracts are concentrated in a few industries, the compositional correction moves the point estimate substantially. The long-contract result is correspondingly less robust than the permanent-contract result, with a randomization-inference p-value of 0.197 in the matched sample. Second, the effect may appear small for what was, in effect, a prohibition of unjustified long contracts, but the reform did not ban long temporary contracts: it required an objective reason for them, and contracts that satisfied such a reason (substitution during parental leave, project work, publicly funded positions) legally continued. The estimated effect captures only the unjustifiable portion.

In addition to these main findings, an analysis detailed in the appendix table A2 uses a control group of firms with more than 20 employees, as opposed to more than 10. This adjustment further minimises the likelihood of treatment-control contamination. The impacts on contract types in this comparison remain virtually unchanged, further validating my conclusions.

Timing and Confounding Reforms: Appendix table A3 probes the timing of these effects. Excluding the pre-1999 waves, during which the employment-protection threshold temporarily stood at ten employees, leaves both contract-composition results essentially unchanged. Restricting the sample to 1996–2002, before the Hartz reforms, shows that the directly restricted

[19] Because the Mikrozensus groups contract durations of 20 to 24 months in a single category before 1999, the bunching outcome is estimated on the 1999–2010 waves.

margin responded immediately and fully: the decline in long temporary contracts within this window (-2.3 percentage points) is indistinguishable from the full-sample estimate. The broader shift towards permanent contracts, by contrast, builds up over time, as the event study in figure 2 shows: it appears in 2001, dips in the 2002 downturn and stabilises at about five percentage points from 2003 onwards. A placebo difference-in-differences on the pre-reform years 1996–2000, with a counterfactual reform date of 1998, yields a near-zero estimate for the permanent-contract share. The corresponding placebo for the long-contract share is imprecisely estimated on a small sample and partly picks up the genuine 1997/98 threshold change itself, which is why the pre-1999 exclusion is the cleaner check for this outcome.

Taken together, my results indicate that the predominant effect of restricting long temporary contracts was a notable shift towards permanent contracts.

6.2 Employment

Did making long temporary contracts costlier reduce employment in the affected firms? The short answer is no. Job creation in treated establishments did not fall, net employment growth rose relative to matched controls, and the reform's cost surfaces on a different margin: more matches were dissolved around the two-year limit, and slightly more treated establishments closed. This subsection documents these results, which speak to the model's employment predictions ($\lambda_T \uparrow$, $\lambda_P \uparrow$; see Table 2) and to the convert-or-churn warning that unemployment inflows rise if the churn margin dominates. Answering both requires separating job flows from worker flows. Following Davis and Haltiwanger (1992), job creation (destruction) is establishment-level employment growth (contraction) relative to average size, summed over expanding and entering (contracting and exiting) establishments; worker flows count individual transitions, which can occur without any job being created or destroyed. Churning, worker turnover in excess of job turnover (Burgess et al., 2000), bridges the two and carries the convert-or-churn prediction.

One measurement issue shapes the specification choice. As described in Section 4.3, marginal employment became notifiable to the social security system only in April 1999, and the staggered arrival of these notifications mechanically inflates measured employment growth in 1999 and 2000, in both treatment groups and despite netting out all jobs flagged as marginal (appendix figure A7 shows the raw series). My preferred specifications therefore exclude 1999–2000, so that the pre-reform window is 1996–98; appendix table A4 reports the mechanically distorted full-window estimates alongside the unadjusted ones for completeness.

Table 4 reports the difference-in-differences estimates for the establishment-level flows in columns (1) to (5) and the individual worker flows in columns (6) to (8), in two specifications: the first row uses the full clean window, the plateau row compares the pre-reform years 1996–

Table 4: Reform effects on establishment-level job flows and individual worker flows

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Establishment level					Worker level		
	Job creation	Job destruction	Net growth	Churning	Hiring rate	Flow into unempl.	Hire out of non-empl.	Separation, tenure < 2y
Treated × Post 2001	3.90***	-1.17***	5.07***	1.08*	4.83***	0.63***	-1.50	6.07***
(1996–2010, excl. 1999–2000)	(0.26)	(0.35)	(0.43)	(0.58)	(0.33)	(0.18)	(1.59)	(1.03)
Treated × Post 2001, plateau	2.46***	-1.47***	3.93***	3.82***	4.62***	0.47**	-1.71	7.16***
(1996–98 vs. 2003–10)	(0.25)	(0.38)	(0.45)	(0.63)	(0.34)	(0.19)	(1.81)	(1.28)
Pre-reform mean, treated	10.9	13.4	-2.5	12.3	19.6	2.1	65.8	5.7
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Treatment Group Indicator	YES	YES	YES	YES	YES	YES	YES	YES
N	491,078	491,078	491,078	491,078	491,078	350,121	72,661	55,173
N, plateau	357,664	357,664	357,664	357,664	357,664	265,551	46,392	39,577

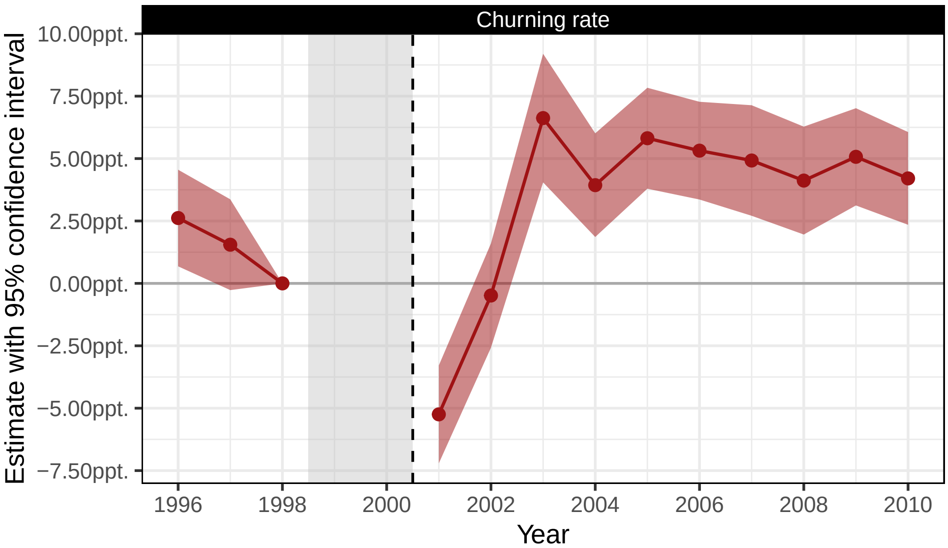
NOTE.- Difference-in-differences estimates on the propensity-score matched sample. The first row uses 1996–2010 and excludes 1999–2000, when the notification reform for marginal employment distorts measured employment growth (Section 4.3); the pre-reform window is 1996–98. The plateau row additionally drops the transition years 2001–02, so it compares 1996–98 with 2003–10 and measures the long-run effect once the first contracts signed under the new rules had reached the two-year cap. Columns (1) to (5) are establishment-level rates in percentage points of average establishment employment: Davis–Haltiwanger job creation and destruction, their difference (net growth), churning (hires plus separations minus job creation and destruction) and the hiring rate; regressions are weighted by average establishment employment times the matching frequency weight. Columns (6) to (8) are individual-level probabilities in percentage points: an employed worker transitioning into benefit receipt within a year, a new job match being filled out of non-employment, and a job with under two years of tenure ending within the following year; regressions are weighted by the matching frequency weight, and the separation outcome is right-censored in the final sample year, which the year fixed effects absorb. Pre-reform means are 1996–98 means in the treated group, weighted like the corresponding regressions. Appendix table A4 reports unadjusted and full-window variants.
Standard errors clustered at the establishment level in parentheses. $*** \, p < 0.01$, $** \, p < 0.05$, $* \, p < 0.1$

98 directly with 2003–10, after the first contracts signed under the new rules had reached the two-year cap. Job creation in treated establishments rises by 3.9 percentage points in the clean window; the plateau estimate of 2.5 percentage points is the better guide to the long-run magnitude, since the clean-window estimate also averages over the transition years 2001–02, in which the unwinding of the notification break still inflates measured job creation. The event studies in appendix figure A8, estimated excluding 1999–2000 exactly like the difference-in-differences, show the dynamics: pre-reform coefficients for both outcomes are near zero, and relative job creation settles from 2003 onwards at 2.0 to 2.8 percentage points. Net employment growth, in column (3), addresses a natural concern about yearly flow estimates: even small annual effects could in principle cumulate into sizeable employment losses. The estimates rule this out; net growth at treated establishments rises by 3.9 to 5.1 percentage points relative to controls. One scoping remark applies to job destruction: within the control group, smaller establishments show a size-specific reduction in job destruction of 1.7 percentage points after the Hartz IV reform (appendix table A6), so job-destruction contrasts after 2005 should be read jointly with the Hartz reforms. The years 2001–02, which precede this concern, show small and statistically insignificant job-destruction coefficients in the event study (-0.5 and -1.3 percentage points), confirming that the reform itself left job destruction essentially unchanged.

Two further results come from decomposing the flows into entry and incumbent margins (appendix table A5). First, genuine establishment entry did not respond to the reform: once entries that reflect identifier changes, spin-offs or takeovers are removed with the Hethey-Maier and Schmieder (2013) typology, the entry margin of job creation is a precisely estimated zero in both windows, confirming that the unpurged entry estimates merely picked up identifier events.

The aggregate job-creation response is carried by growing incumbents. Second, the one unambiguous cost of the reform at the establishment level is a higher exit rate: treated establishments are 1.3 to 1.7 percentage points more likely to close, robustly across all specifications. This is the cost side of the model operating at the establishment level: for a small firm whose marginal match is no longer viable without the long-temporary option, the alternative to conversion is not always a replacement hire but sometimes closure. The positive net-growth estimate shows that these additional closures are more than offset by incumbent growth.

Figure 3: Event-study plot for the establishment-level churning rate



Note.- Event-study coefficients for the establishment-level churning rate (hires plus separations minus job creation and destruction, in percentage points of average employment) on the matched sample, with 1998 as the base year and 95% confidence intervals from establishment-clustered standard errors. Regressions are weighted by average establishment employment times the matching frequency weight. The years 1999–2000 (shaded), in which the notification reform for marginal employment distorts measured flows, are excluded from estimation, exactly as in the preferred difference-in-differences specification; the asymmetric unwinding of that break still depresses the 2001 coefficient (see appendix figure A7).
Source: Establishment History Panel extension of the SIAB for West Germany.

The convert-or-churn trade-off concerns a different margin: what happens to temporary matches that reach the two-year limit. The central evidence is the timing of the churn response, and figure 3 shows it. Churning in treated establishments jumps in 2003, exactly when the first contracts signed under the new rules reach the two-year limit, and stays at 3.9 to 6.6 percentage points above its 1998 level through 2010. The plateau row of table 4 summarises this as a single estimate: 3.8 percentage points on a pre-reform churning rate of 12.3 percent, a rise of roughly a third, with the later event-study coefficients reaching half. Two features of the pre-2003 series require comment. The 1996 coefficient is positive at 2.6 percentage points, about half the size of the post-2003 average, and the 1997 coefficient is smaller still and only marginally significant; the worker-level short-tenure separation series (appendix figure A9), which is flat before the measurement break, indicates that these early coefficients are noise rather than a pre-trend. The negative 2001 coefficient is the unwinding of the notification break: measured churning was inflated in both groups in 1999–2000 and returned to its baseline a year earlier in treated establishments than in the larger control establishments (appendix figure A7). This transition also explains why the clean-window estimate in table 4, at 1.1 percentage points, is much smaller: it averages the negative transition years 2001–02 into the post period, which the plateau row drops.

The 2003 onset is also what separates the churn response from the Hartz reforms of 2003–05. No Hartz mechanism explains a jump timed to the first capped contract cohort and predating Hartz IV. The within-control placebo in appendix table A6 quantifies the remaining overlap: smaller control establishments show a churning increase of 1.6 percentage points after Hartz IV and none after Hartz I–III, so a Hartz-era size gradient can account for roughly a quarter to two-fifths of the treated increase from 2005 onwards and none of its onset.

The worker-level flows in columns (6) to (8) of table 4 complete the picture. Separations of workers with under two years of tenure rise by 6.1 percentage points, the individual-level counterpart of the churning result; the proportional rise exceeds the churning increase because pre-reform matches in treated small establishments were particularly stable, with a short-tenure separation probability of under six percent. Unemployment inflows rise by 0.6 percentage points on a base of 2.1 percent, an effect already present in the pre-Hartz years. Neither reflects a retreat from hiring: the establishment-level hiring rate rises by 4.8 percentage points, and the share of new hires recruited out of non-employment moves little, with a small and statistically insignificant decline. Treated establishments hire more, not fewer, workers after the reform, and the additional hires disproportionately replace churned workers, which is inconsistent with tightened hiring standards.

The churn margin from Section 3.3 therefore does materialise, and it is economically meaningful: churning rises by a third to a half relative to its pre-reform level. It nevertheless remains the smaller of the two margins. The reform shifted most affected matches into permanent contracts, while a smaller set of matches was dissolved at the two-year limit rather than converted, with a correspondingly small echo in unemployment inflows: 0.6 percentage points, against the five-point shift of new matches to permanent contracts. Both margins of the convert-or-churn trade-off are thus visible in the data, with the conversion margin quantitatively dominant. The entrant results below show that, for the average worker starting a career in a treated firm, the conversion margin also dominates in the long run.

6.3 Long-term outcomes for labour market entrants

In addition to analysing the theoretical predictions of my model, I also investigate the long-term impact of the reform on individuals affected by it. The debate surrounding temporary contracts often revolves around whether they serve as stepping stones to permanent employment or as dead ends for labour market entrants. For the long temporary contracts restricted by the reform, the expected outcome is not obvious: if such contracts build skills and experience that lead to permanent positions, capping their duration could hinder career progression, whereas if they trap workers in positions with little employer investment and few opportunities for advancement, capping them could improve career prospects. I therefore examine how entering the labour market under the new policy affected the long-term outcomes of post-reform entrants.

Once again, I use social security data since it allows me to track individuals over time who entered the labour market at the time of the reform. In a similar difference-in-differences setup as before, I compare the difference in the outcomes of post and pre-reform entrants, who entered the labour market in treatment and control firms. To compare similar cohorts of entrants I restrict their entry years to 1997 to 2003. This entrant design allows me to analyse outcomes such as the likelihood of still being employed by the same employer as in the entry year, the cumulative earnings, time out of employment (in weeks) and the number of jobs for entrants in the first five years after entry.

Table 5: Reform effects on long-term outcomes of entrants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Unadjusted				Propensity-score matched entry firms			
	Log cumulative earnings	Cumulative duration unemployed	Prob. stay in entry firm	Number of Jobs	Log cumulative earnings	Cumulative duration unemployed	Prob. stay in entry firm	Number of Jobs
Treated × Entry Post 2001	-0.0320^{***}	-0.3135^{***}	-0.0017	-0.0237^{***}	0.0753^{***}	-0.1441	0.02645^{*}	-0.0466^{*}
	(0.0011)	(0.0670)	(0.0059)	(0.0076)	(0.0246)	(0.2100)	(0.01528)	(0.0243)
Firm-size at entry FE	YES	YES	YES	YES	YES	YES	YES	YES
Entry-year FE	YES	YES	YES	YES	YES	YES	YES	YES
Calendar-year FE	YES	YES	YES	YES	YES	YES	YES	YES
N	1,274,972	1,274,972	1,274,972	1,274,972	260,252	260,252	260,252	260,252
R^2	0.6376	0.0071	0.0262	0.1834	0.6566	0.0048	0.021	0.2271

NOTE.- This table presents the reform effects on the long-term outcomes of labour market entrants. The outcomes analysed include the logarithm of cumulative earnings, the cumulative duration of unemployment in days, a dummy variable for staying with the entry firm, and the number of jobs held. Columns (1) to (4) show results for an unadjusted sample, while columns (5) to (8) display using propensity-score matched entry firms.
Cluster-robust standard errors for firm-size at entry clusters in parentheses.
$*** \, p < 0.01$, $** \, p < 0.05$, $* \, p < 0.1$

I report the results of the respective regressions in Table 5. All regressions in this table are based on difference-in-differences specifications that compare outcomes of labour market entrants based on the treatment status of the firm in the entry year. I again present results using an unadjusted sample and propensity-score matched entry firms.

Without adjusting for industry and firm-type composition through propensity-score matching, the results for treatment and control firms are ambiguous. There is a statistically significant minor reduction in unemployment (about one-third of a week) and a slight decrease in job turnover. However, the cumulative earnings of new entrants actually drop by 3%.

Applying propensity-score matching reveals a more positive impact of the reform on labour market entrants. There is a 2.6 percentage-point increase in their likelihood of staying with their initial firm, indicating enhanced job stability. This aligns with an approximately 7.53% increase in cumulative earnings, suggesting improvements in job quality and pay. Additionally, a slight decline in the number of jobs held hints at reduced job turnover and greater employment continuity.

Overall, the reform appears to benefit early career outcomes for labour market entrants. This is evident from increased cumulative earnings, a higher probability of staying with the entry firm, and fewer job changes, all pointing to better job quality and stability. This aligns with broader evidence from both Daruich et al. (2023) and Castellanos et al. (2024), where temporary contracts are shown to lead to slower wage growth and more precarious employment outcomes for labour market entrants. For example, Castellanos et al. (2024) find that workers in temporary contracts experience wage growth that is 22% lower than for those in permanent contracts, particularly for young and low-ability workers. They also find that temporary contracts rarely serve as stepping stones to permanent positions, instead often trapping workers in precarious employment with limited wage progression, thereby widening the gap between temporary and permanent workers.

A Screening Reading of the Entrant Results: The improvement in entrant outcomes is consistent with the search-and-matching mechanism, but it is important to acknowledge that an employer-learning model predicts the same sign: if long temporary contracts served as extended probation periods, the reform forced small firms to convert screened workers to permanent contracts earlier, which would likewise raise entrants' early-career stability and earnings (Lange, 2007; Kahn and Lange, 2014). My data cannot separate whether treated firms converted matches earlier because their production horizons were long or because match quality had been revealed; both mechanisms operate through the same observable margin, and both imply that entrants gained earlier access to permanent contracts. The entrant results do, however, speak against the distinctive *negative* prediction of a binding screening constraint: if two years were too short to assess new hires, treated firms should have responded by hiring more selectively, to the particular detriment of labour-market entrants, who have the shortest track records (cf. Grasso and Tatsiramos, 2023). Instead, total hiring rose (Section 6.2), its composition shifted little away from non-employment, and post-reform entrants fared better, not worse. I therefore read the evidence as indicating that, at the two-year margin in small firms, the duration-of-production-opportunity motive dominates the screening motive, while noting that the welfare-relevant conclusion for entrants is the same under either reading.

7 Conclusion

This article examines how a 2001 reform that raised the legal requirements for the use of long temporary contracts for small firms has affected the use of different contract types, employment and the careers of labour market entrants.

I find a sizeable increase in the share of new job matches that directly start with a permanent contract after the reform and the expected reduction in the share of long temporary contracts. However, I find no adverse employment effect of the reform: job creation in treated firms did not fall. The reform's cost surfaces instead at the churn margin: turnover of short-tenure workers rose by a third to a half relative to its pre-reform level once matches reached the two-year limit, and slightly more treated establishments closed. Furthermore, I present some evidence that the reform has reduced labour-market segmentation for new entrants: those who joined affected firms after the reform year saw higher cumulative wages in their first five years and greater job stability.

These findings should not be read as an unqualified endorsement of restricting temporary contracts. First, there is an efficiency concern: in the model, a cap that lowers both job creation and job destruction retains matches that should be dissolved and blocks the formation of promising new ones. My estimates suggest the 2001 reform operated mainly through contract composition rather than through job flows, so this cost appears to have been limited in the present case. The convert-or-churn margin is not merely theoretical, however: churning and short-tenure separations in treated firms rose once the first post-reform contracts reached the cap, with a small accompanying increase in unemployment inflows. For this reform these churn effects, though economically meaningful, were dominated by conversions, but a tighter cap, or the same cap in a labour market with more long-duration temporary work, could tip the balance towards costly churn. Second, there is a distributional concern: employment protection typically benefits insiders at the expense of outsiders. The gains I document accrue to workers who are hired by treated firms; if firms had responded by raising hiring standards or reducing hiring of hard-to-assess workers, outsiders would have borne the cost. I find little evidence of such a response: job creation did not fall and post-reform entrants fared better, not worse. Yet this reassurance is specific to a reform that restricted only the long-duration margin of temporary work in small firms; it does not license the conclusion that restricting temporary employment benefits workers in general.

A further word of caution concerns the time horizon. My analysis captures adjustments over roughly a decade. Longer-term responses of firms to permanently higher expected labour costs (substitution towards capital, outsourcing, or changes in firm growth around the employment-protection threshold) remain outside the scope of this study and are an interesting avenue for future research.

My results on a restriction on long temporary contracts also stand in contrast to the effects of reforms that exclusively restrict short temporary contracts (see Cahuc et al., 2020). Whereas taxing short contracts targets matches for which a permanent contract is no substitute, and therefore backfired, restricting the long-duration margin targets matches that can viably be made permanent. The design margin, not merely the direction of regulation, determines who gains and who loses.

The distinction matters beyond the historical episode studied here. Germany is currently preparing to move in the opposite direction, doubling the maximum permitted duration of temporary contracts from two to four years. My results suggest that such an extension would shift matches with long expected durations from permanent back to temporary contracts, with little gain in employment and a likely deterioration in early-career stability for entrants. The main beneficiaries would be firms retaining the option value of delayed commitment. The 2001 tightening thus offers a direct, if mirror-image, benchmark for the reform now under discussion.

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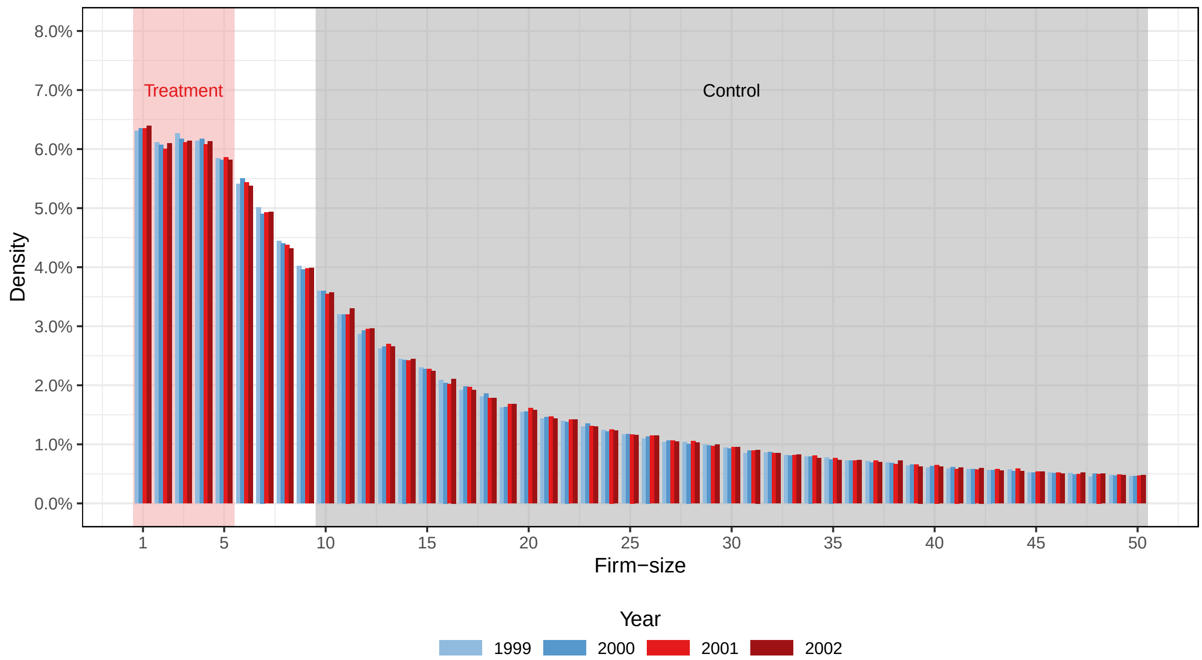
A Additional tables and figures

Figure A1: Firm-size distribution around the reform



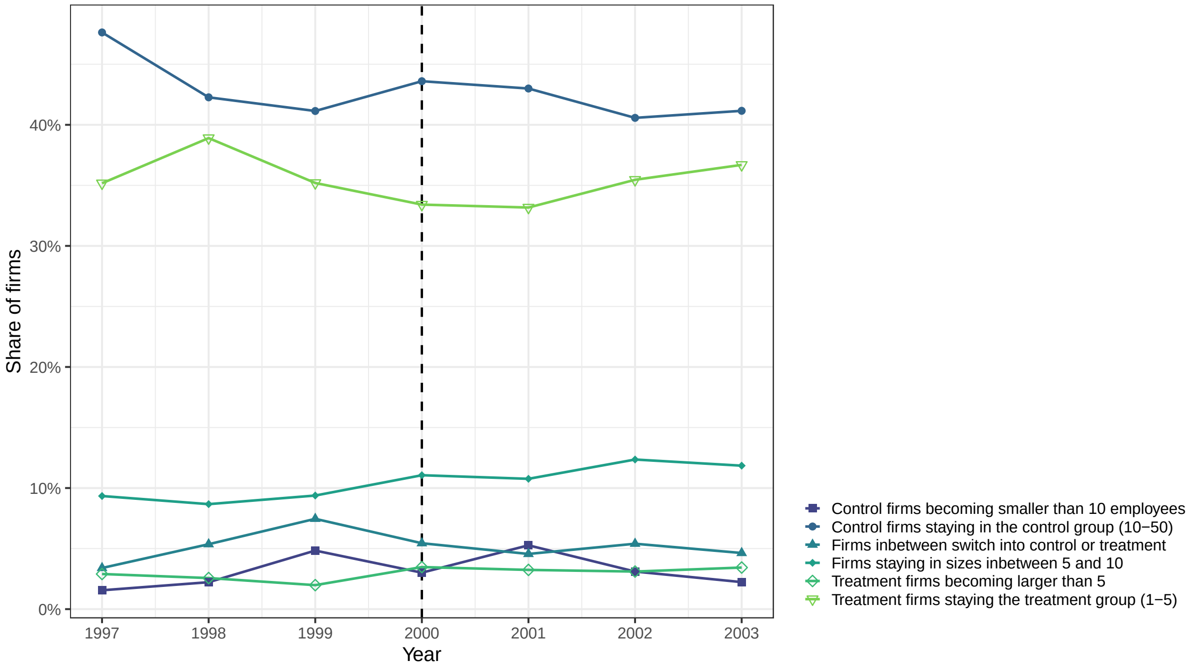
Note.- The figure plots the density of the firm-size distribution for firms with zero to 50 employess for the years from 1999 to 2002. The control and the treatment groups are highlighted with a red and a grey shaded area.
Source: Establishment History Panel of the IAB for West Germany

Table A1: Summary Statistics for New Job Matches in the Mikrozensus

	Unique Values	Mean	SD	Min	Median	Max
Temporary Contract	3	0.3	0.5	0.0	0.0	1.0
Hours worked per week	51	32.1	13.2	20.0	39.0	50.0
Female	2	0.5	0.5	0.0	1.0	1.0
Age	39	34.8	10.0	11.0	34.0	58.0
Service Sector	2	0.7	0.4	0.0	1.0	1.0
Medium Education	3	0.8	0.4	0.0	1.0	1.0
High Education	3	0.1	0.3	0.0	0.0	1.0

Note.- This table provides summary statistics for the Mikrozensus sample of new job matches.
Source: Mikrozensus sample for West German employees aged 20 to 58

Figure A2: Firm-size changes around the reform



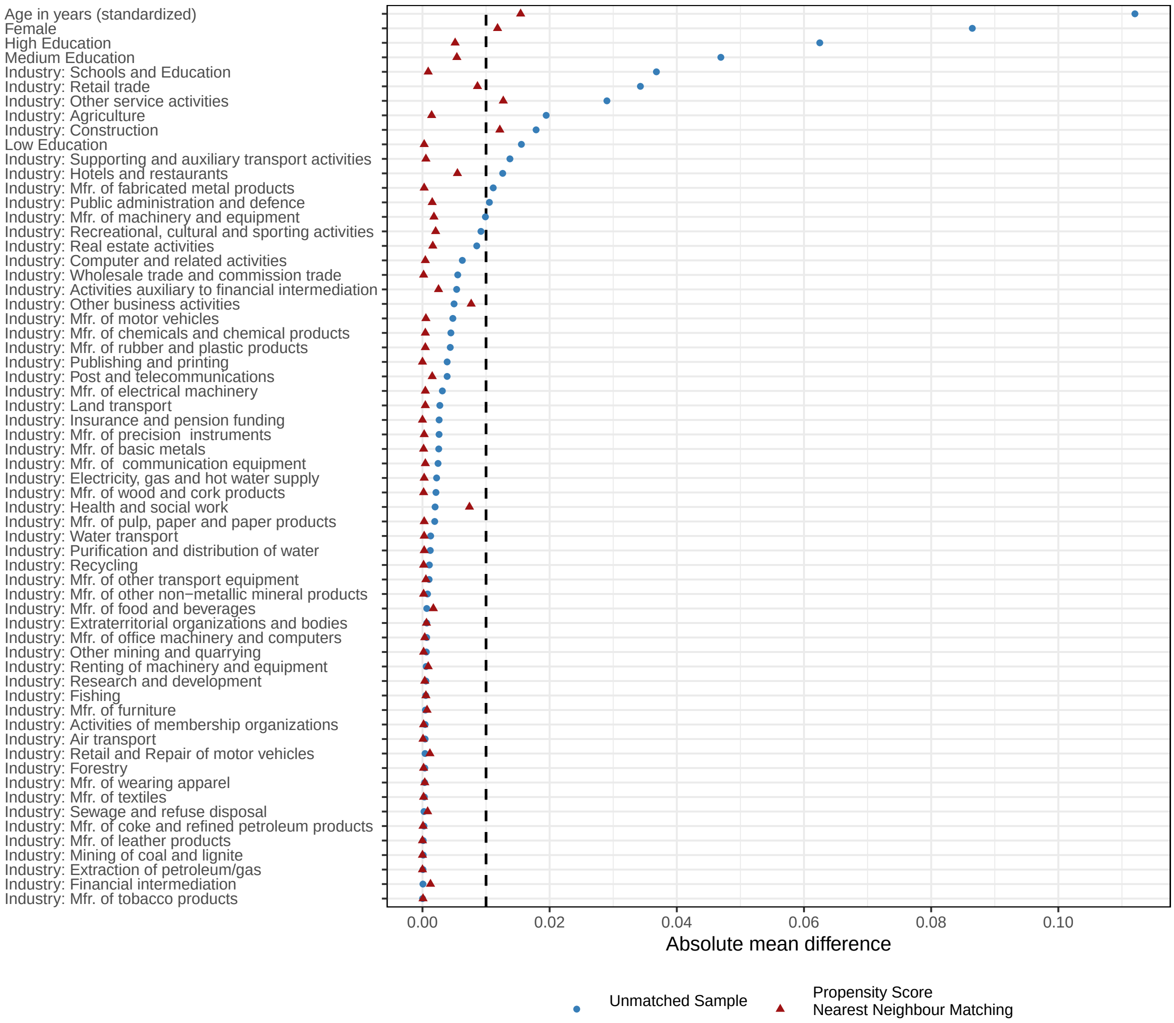
NOTE.- This figure illustrates the switching behaviour of firms between treatment and control groups, along with relevant subgroups, around the reform. The subgroups include control firms that became smaller than 10 employees, control firms remaining between 10 and 50 employees, firms staying between the 5 and 10 employee size range, and treatment firms that either grew larger than 5 employees or remained within the 1–5 employee size range. No systematic changes in firm size around the reform are observed.
Source: Establishment History Panel, Germany.

Table A2: Defining a broader control group

	(1)	(2)	(3)	(4)
	Unadjusted		Propensity score matching	
	Permanent	Long Temporary	Permanent	Long Temporary
Treated × Post 2001	3.697***	-0.457**	4.913***	-2.815**
	(0.028)	(0.029)	(0.000)	(0.069)
Year Fixed Effects	YES	YES	YES	YES
Firm-size Fixed Effects	YES	YES	YES	YES
N	26671	8325	16108	4379
Adj. R^2	0.04	0.02	0.04	0.01

NOTE.- The table shows effects for both the share of job matches that start directly with a permanent contract and the share of temporary contracts that are longer than two years. Column (1) and (2) present results for an unmatched sample. Columns (3) and (4) show the results for the sample matched via nearest-neighbour propensity-score matching. The control group was changed to only include firms with more than 20 employees. The results are expressed in percentage points. Cluster-robust standard errors for firm-size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

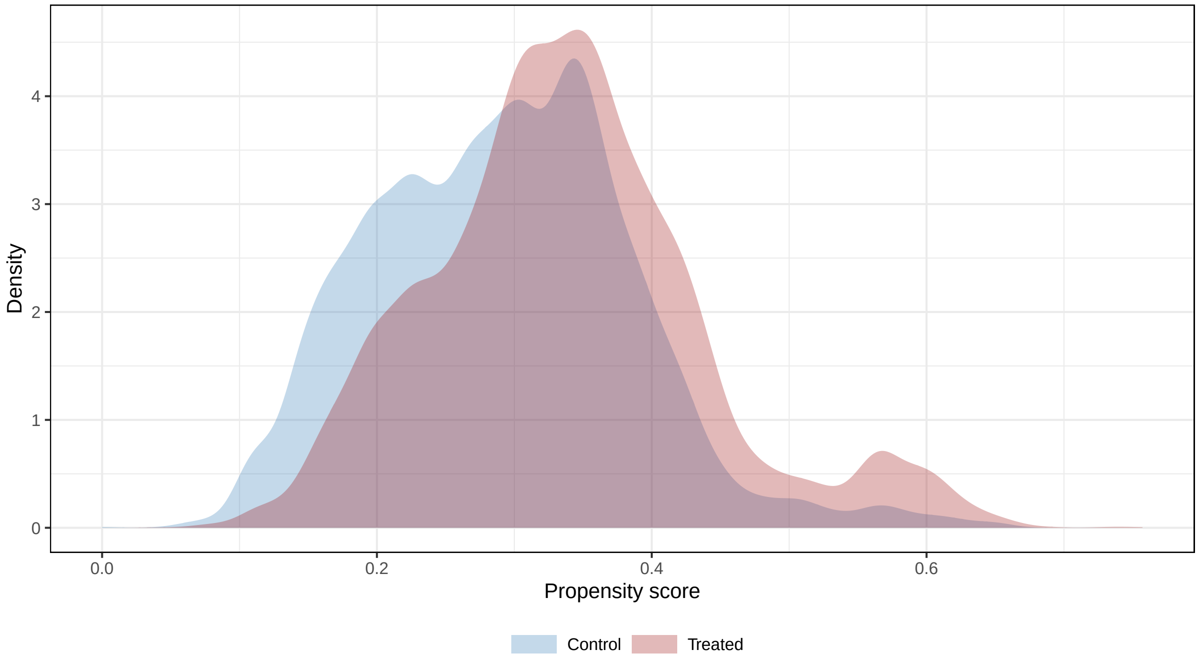
Figure A3: Covariate balance after matching in the Mikrozensus



NOTE.- The figure plots the absolute mean difference in the matching covariates between treatment and control group for a matched and an unmatched sample. All variables except for the age in years are binary indicators. Therefore, the absolute mean difference between treatment and control group can be interpreted as a difference in sub-group shares between these groups. A difference of one percentage point in a sub-group share is indicated by the dashed line. To fit on the same scale the only continuous covariate, age in years, was standardized.

Source: Mikrozensus new-job match sample for West German employees aged 20 to 58

Figure A4: Common support of matching procedure



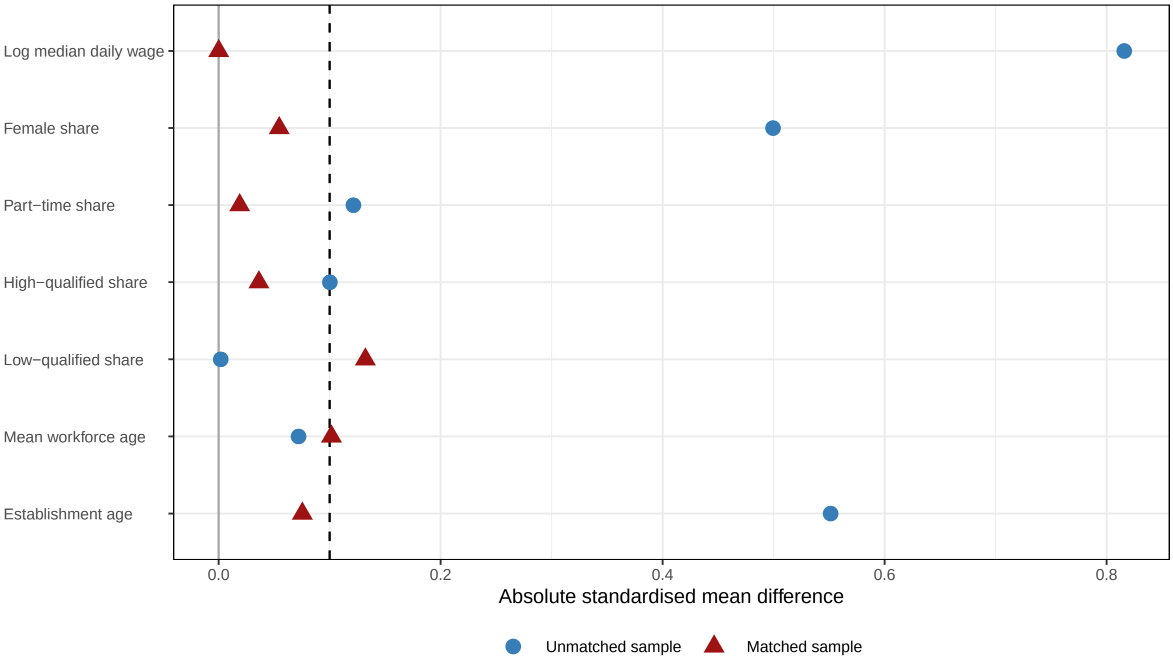
Note.- The figure presents the density distributions of propensity score for both the treatment and control groups, with the treatment group shown in red and the control group in blue. The horizontal axis represents the range of propensity scores, while the vertical axis indicates the density of observations within each score.
Source: Mikrozensus new-job match sample for West German employees aged 20 to 58

Table A3: Timing robustness: sample windows and pre-reform placebo

	(1)	(2)	(3)	(4)	(5)	(6)
	Permanent			Long Temporary		
	1999–2010	1996–2002	Placebo 1996–2000	1999–2010	1996–2002	Placebo 1996–2000
Treated × Post 2001	4.941***	0.286***		-2.292***	-2.328**	
	(0.001)	(0.004)		(0.002)	(0.146)	
Treated × Post 1998 (placebo)			-0.277***			-2.772*
			(0.001)			(0.256)
Year Fixed Effects	YES	YES	YES	YES	YES	YES
Firm-size Fixed Effects	YES	YES	YES	YES	YES	YES
N	20124	5288	3957	5801	1106	826
Adj. R^2	0.02	0.01	0.01	0.01	0.04	0.04

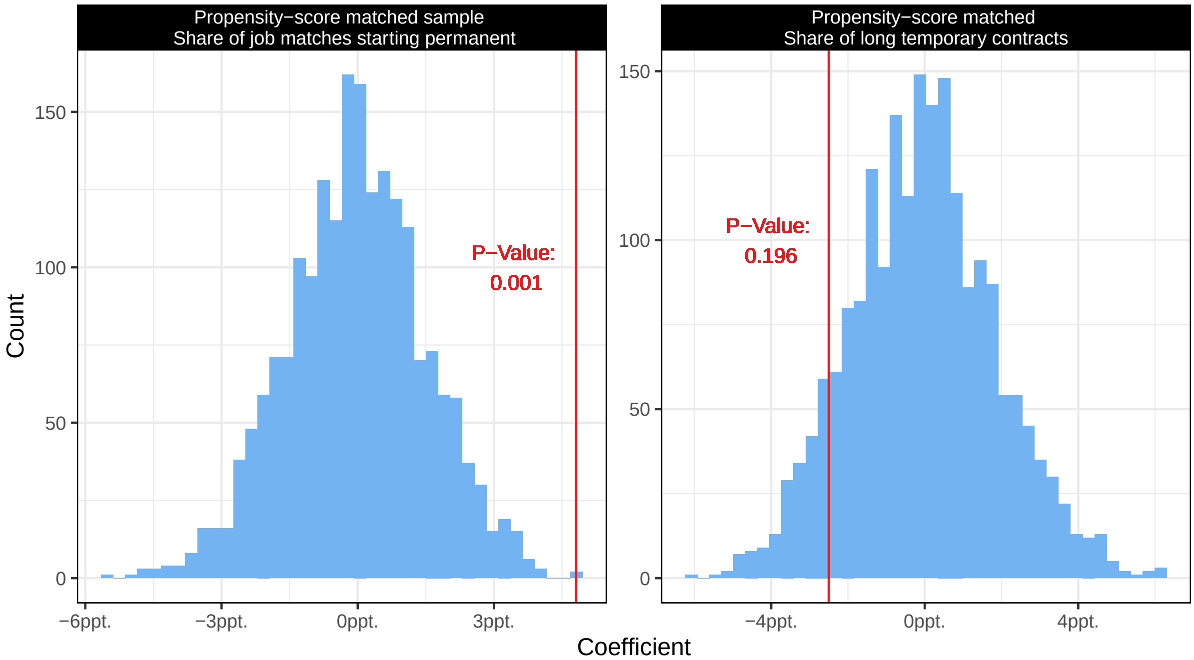
Note.- The table probes the timing of the main contract-composition results in the propensity-score matched sample. Columns (1) and (4) exclude the pre-1999 waves, so that the estimates are unaffected by the 1997/98 change in the employment-protection threshold. Columns (2) and (5) end the sample in 2002, before the Hartz reforms (2003–2005) came into force. Columns (3) and (6) report a placebo difference-in-differences on the pre-reform years 1996–2000 with a counterfactual reform date of 1998. The outcome in columns (1) to (3) is the share of new job matches starting with a permanent contract; in columns (4) to (6) it is the share of temporary contracts longer than 24 months. The results are expressed in percentage points.
Cluster-robust standard errors for firm-size clusters in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Figure A5: Covariate balance for the social security data



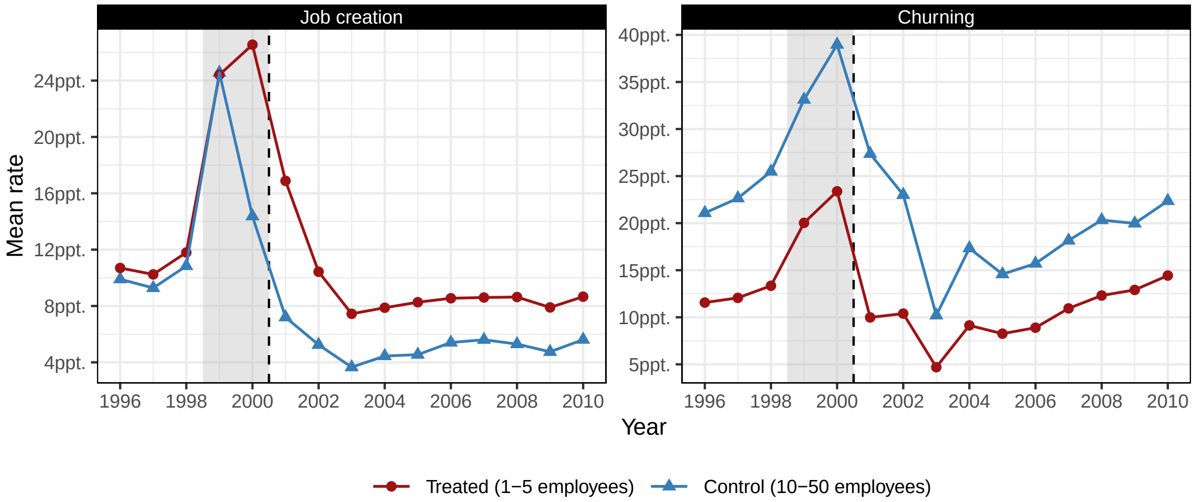
Note.- The figure plots the absolute standardised mean difference between treated and control establishments for each matching covariate in the pre-reform (2000) cross-section, before matching (blue circles) and after nearest-neighbour propensity-score matching with replacement and a caliper (red triangles), where the matched differences use the matching frequency weights. Differences are standardised with the pooled standard deviation of the unmatched estimation sample, so the denominator is the same before and after matching. The dashed line marks the conventional threshold of 0.1. Two-digit industry, also part of the propensity score, is balanced analogously: the Duncan dissimilarity index between the treated and control industry distributions falls from 0.25 before matching to 0.06 after.
Source: Establishment History Panel extension of the SIAB for West Germany, 2000 cross-section

Figure A6: Randomization inference



NOTE.- The figure plots the estimated difference-in-difference coefficients of the models presented in table 3 for 2,000 random permutations of the treatment status. The actual coefficient is indicated by a red vertical line for each model. Moreover, the randomization inference p-value is presented in the graph along the line.
Source: Mikrozensus new-job match sample for West German employees aged 20 to 58

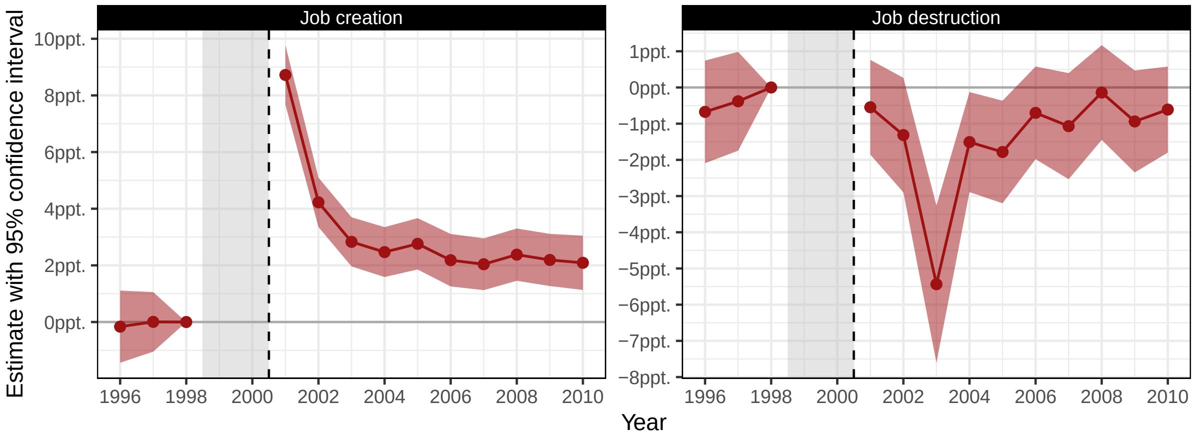
Figure A7: Raw flow series on the matched sample: the 1999–2000 notification break



Note.- Mean job-creation and churning rates by year and treatment group on the matched sample, weighted by average establishment employment times the matching frequency weight, in percentage points of average employment. The shaded band marks 1999–2000: marginal employment became notifiable to the social security system in April 1999, and the staggered arrival of these notifications mechanically inflates measured employment growth and worker flows in both groups, despite netting out all jobs flagged as marginal. The inflation unwinds asymmetrically: measured churning returns to its baseline in 2001 in treated establishments but only in 2002 in the larger control establishments, which produces the negative 2001 coefficient in figure 3.

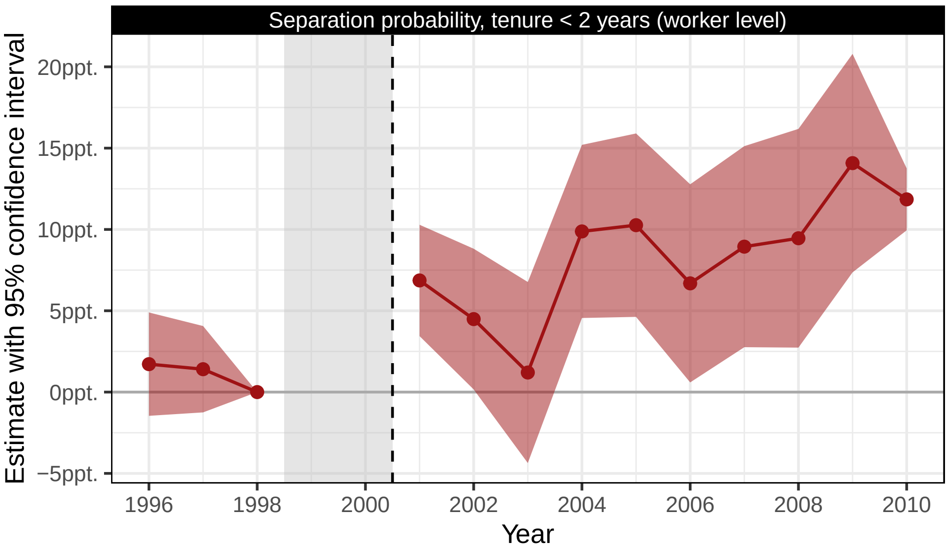
Source: Establishment History Panel extension of the SIAB for West Germany.

Figure A8: Event-study plots for job creation and job destruction



Note.- Event-study coefficients for establishment-level job-creation and job-destruction rates (Davis and Haltiwanger, 1992) on the matched sample, with 1998 as the base year and 95% confidence intervals from establishment-clustered standard errors. Regressions are weighted by average establishment employment times the matching frequency weight. The years 1999–2000 (shaded), in which the notification break shown in figure A7 distorts measured flows, are excluded from estimation, exactly as in the preferred difference-in-differences specification. The elevated job-creation coefficients in 2001–02 are the decaying footprint of that break, which leaves the lagged employment stocks only gradually; the pre-reform coefficients are close to zero, and relative job creation settles at 2.0 to 2.8 percentage points from 2003 onwards. The job-destruction dip in 2003 coincides with Hartz I and is not treated as a reform effect.
Source: Establishment History Panel extension of the SIAB for West Germany.

Figure A9: Event-study plot for worker-level short-tenure separations



NOTE.- Event-study coefficients for the worker-level probability that a job with under two years of tenure ends within the following year, in percentage points, on the matched sample with matching frequency weights, with 1998 as the base year and 95% confidence intervals from establishment-clustered standard errors. The years 1999–2000 (shaded) are excluded from estimation, exactly as in the preferred difference-in-differences specification. The series corroborates the establishment-level churning result in figure 3: flat pre-reform coefficients in 1996–97, elevated separations from 2001 onwards, and a plateau of about ten percentage points from 2004.
Source: 2% SIAB worker sample for West Germany.

Table A4: Job and worker flows: unadjusted and full-window variants

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Establishment level					Worker level		
	Job creation	Job destruction	Net growth	Churning	Hiring rate	Flow into unempl.	Hire out of non-empl.	Separation, tenure < 2y
Unadjusted (1996–2010)	-3.28***	1.25***	-4.53***	-1.70***	-4.42***	-0.59***	2.59***	-0.35
	(0.15)	(0.11)	(0.20)	(0.16)	(0.16)	(0.07)	(0.44)	(0.30)
Unadjusted, excl. 1999–2000	-1.01***	1.18***	-2.20***	-2.85***	-2.97***	-0.76***	1.15**	-1.54***
	(0.18)	(0.14)	(0.25)	(0.20)	(0.19)	(0.09)	(0.56)	(0.37)
Matched (1996–2010)	-0.69**	0.56*	-1.26***	3.52***	1.20***	0.25*	-2.96**	3.45***
	(0.29)	(0.31)	(0.47)	(0.40)	(0.31)	(0.13)	(1.20)	(1.02)
Year Fixed Effects	YES	YES	YES	YES	YES	YES	YES	YES
Treatment Group Indicator	YES	YES	YES	YES	YES	YES	YES	YES
N, unadjusted	2,518,316	2,518,316	2,518,316	2,518,316	2,518,316	1,658,707	495,081	489,994
N, unadjusted excl. 1999–2000	2,201,384	2,201,384	2,201,384	2,201,384	2,201,384	1,433,696	418,657	414,415
N, matched	642,472	642,472	642,472	642,472	642,472	456,879	112,345	94,831

Note.- Specification variants of the estimates in table 4; outcomes and weighting as described there. These variants are reported for transparency, not as alternative readings. The unadjusted rows compare establishments with 1–5 and 10–50 employees directly and are dominated by compositional differences (the raw treated-control gap in the female share alone is 19 percentage points). The full-window rows include 1999–2000, when the notification reform for marginal employment mechanically inflates measured employment growth in the pre-period baseline; the resulting bias is towards finding relative employment losses in treated establishments, and the contrast with the clean-window estimates in table 4 shows the size of that bias.
Standard errors clustered at the establishment level in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Table A5: Decomposition of job flows: entry and exit vs. incumbent margins

	(1)	(2)	(3)	(4)
	Job creation		Job destruction	
	Entrants	Incumbents	Exiters	Incumbents
Treated × Post 2001	0.64***	-1.34***	1.30***	-0.73***
(1996–2010)	(0.23)	(0.20)	(0.24)	(0.22)
Treated × Post 2001,	-0.04	-1.31***	1.42***	-0.71***
purged entries/exits (1996–2010)	(0.22)	(0.20)	(0.24)	(0.22)
Treated × Post 2001,	1.08***	2.82***	1.55***	-2.71***
excluding 1999–2000	(0.21)	(0.16)	(0.31)	(0.19)
Treated × Post 2001, purged	0.24	2.85***	1.68***	-2.70***
and excluding 1999–2000	(0.19)	(0.16)	(0.31)	(0.19)
Year Fixed Effects	YES	YES	YES	YES
Treatment Group Indicator	YES	YES	YES	YES
N	642,472	642,472	642,472	642,472
N, purged	639,821	639,821	639,821	639,821
N, excluding 1999–2000	491,078	491,078	491,078	491,078
N, purged and excl. 1999–2000	489,193	489,193	489,193	489,193

NOTE.- The table decomposes the job-creation rate into the contribution of entering establishments and growing incumbents, and the job-destruction rate into exiting establishments and shrinking incumbents, on the matched establishment panel (in percentage points). The first row uses the full 1996–2010 window. The second row additionally excludes establishment-years whose recorded entry or exit reflects an identifier change, spin-off or takeover according to the Hethey-Maier and Schmieder (2013) typology rather than a genuine birth or death. The third row instead excludes the years 1999–2000, in which the notification reform for marginal employment distorts measured employment growth, and the fourth row applies both restrictions. Regressions are weighted by average establishment employment times the matching frequency weight.

Standard errors clustered at the establishment level in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

Table A6: Hartz-by-size placebo: smaller vs. larger establishments within the control group

	(1) Job creation	(2) Job destruction	(3) Churning	(4) Hiring rate	(5) Separation rate
Smaller control × Post 2003	0.15	-0.21	0.22	0.22*	-0.07
(Hartz I–III)	(0.10)	(0.19)	(0.25)	(0.12)	(0.10)
Smaller control × Post 2005	-0.12	-1.72***	1.58***	-0.19	-0.07
(Hartz IV)	(0.10)	(0.18)	(0.24)	(0.12)	(0.10)
Year Fixed Effects	YES	YES	YES	YES	YES
Size Group Indicator	YES	YES	YES	YES	YES
N	1,395,396	1,395,396	1,395,396	1,395,396	1,395,396

NOTE.- The table restricts the sample to control establishments (10–50 employees) and compares the smaller (10–19 employees) with the larger (20–50 employees) among them around the Hartz reform dates: if the Hartz reforms had establishment-size-specific effects on flows, they should appear in this comparison. Outcomes in percentage points of average establishment employment; regressions include year fixed effects and a size-group indicator and are weighted by average establishment employment. The interactions are defined as post-2003 (Hartz I–III) and post-2005 (Hartz IV) indicators times the smaller size group.
Standard errors clustered at the establishment level in parentheses. $***\ p < 0.01$, $**\ p < 0.05$, $*\ p < 0.1$

B Model derivations

In this appendix I will provide further details of the theoretical framework set out in section 3. In particular, I provide detailed definitions of the surplus of creating jobs by contract type (B.1). Moreover, I discuss the comparative statics of an increase in contract writing costs for long fixed-term contracts (B.2).

B.1 Expected surplus for job creation

The expected surplus for creating a job of type λ is given by the sum of the associated expected profit of the firm and the worker's valuation of the match minus the worker's outside option. For a permanent contract, the expected profit of the firm is given by

$$\Pi_P(\lambda) = \int_0^\infty \left[\int_0^\tau [y - w(\lambda)]e^{-rt}dt - fe^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau - c = \frac{y - w(\lambda) - \lambda f}{r + \lambda} - c, \tag{2}$$

where the inner integral represents the discounted sum of expected profits until a random termination date τ, while the term $fe^{-r\tau}$ is the discounted value of the firing costs at this date. The whole expression in the brackets is then integrated over the Poisson process density $\lambda e^{-\lambda\tau}$ that determines at which date τ a job of type λ becomes unproductive.

Similarly, a worker's valuation of a job is given by

$$V_P(\lambda) = \int_0^\infty \left[\int_0^\tau w(\lambda)e^{-rt}dt + Ue^{-r\tau} \right] \lambda e^{-\lambda\tau} d\tau = \frac{w(\lambda) + \lambda U}{r + \lambda}, \tag{3}$$

where U is the worker's valuation of the outside option of the match. The surplus for a permanent contract is then given by

$$S_P(\lambda) = \frac{y - rU - \lambda f}{r + \lambda} - c \tag{4}$$

The expected profit of a firm for a temporary contract is the sum of the discounted profit flow up to an endogenous date $D(\lambda)$ and the discounted value of continuing in a permanent contract at time D less the cost of writing the contract. This cost is only c for contracts shorter than $\overline{D}$, but increases to $c + c_{\text{LONG}}$ for longer temporary contracts.

$$\Pi_T(\lambda, D) = \begin{cases} \int_0^D [ye^{-\lambda\tau} - w(\lambda, D)]e^{-r\tau}d\tau + CV(\lambda, D) - c - c_{\text{LONG}}, & \text{if } D(\lambda) > \overline{D} \\ \int_0^D [ye^{-\lambda\tau} - w(\lambda, D)]e^{-r\tau}d\tau + CV(\lambda, D) - c, & \text{if } D(\lambda) \leq \overline{D} \end{cases}, \tag{5}$$

with $CV(\lambda, D) = \max(\Pi_P(\lambda), 0) \cdot e^{-(r+\lambda)D}$. Note that in the discounted flow of profits up to D only the productivity y is evaluated at its survival probability, while wages are only discounted with r. This reflects that employers have to keep paying the wage until D if the productivity shock arrives before the expiration date of the contract. Moreover, the continuation value $CV(\lambda, D)$ is simply the discounted maximum of either the permanent contract profit for the same job-type λ or 0.

Similarly, a worker's valuation of a new temporary contract is given by

$$V_T(\lambda, D) = \int_0^D w(\lambda, D)e^{-r\tau}d\tau + \max(V_P(\lambda), U) \cdot e^{-\lambda D} \cdot e^{-rD} + U(1 - e^{-\lambda D})e^{-rD} \tag{6}$$

Lastly, just as for a permanent contract, the expected surplus for a temporary contract is defined as the sum of the firm's expected profit and the worker's valuation less his or her outside option.

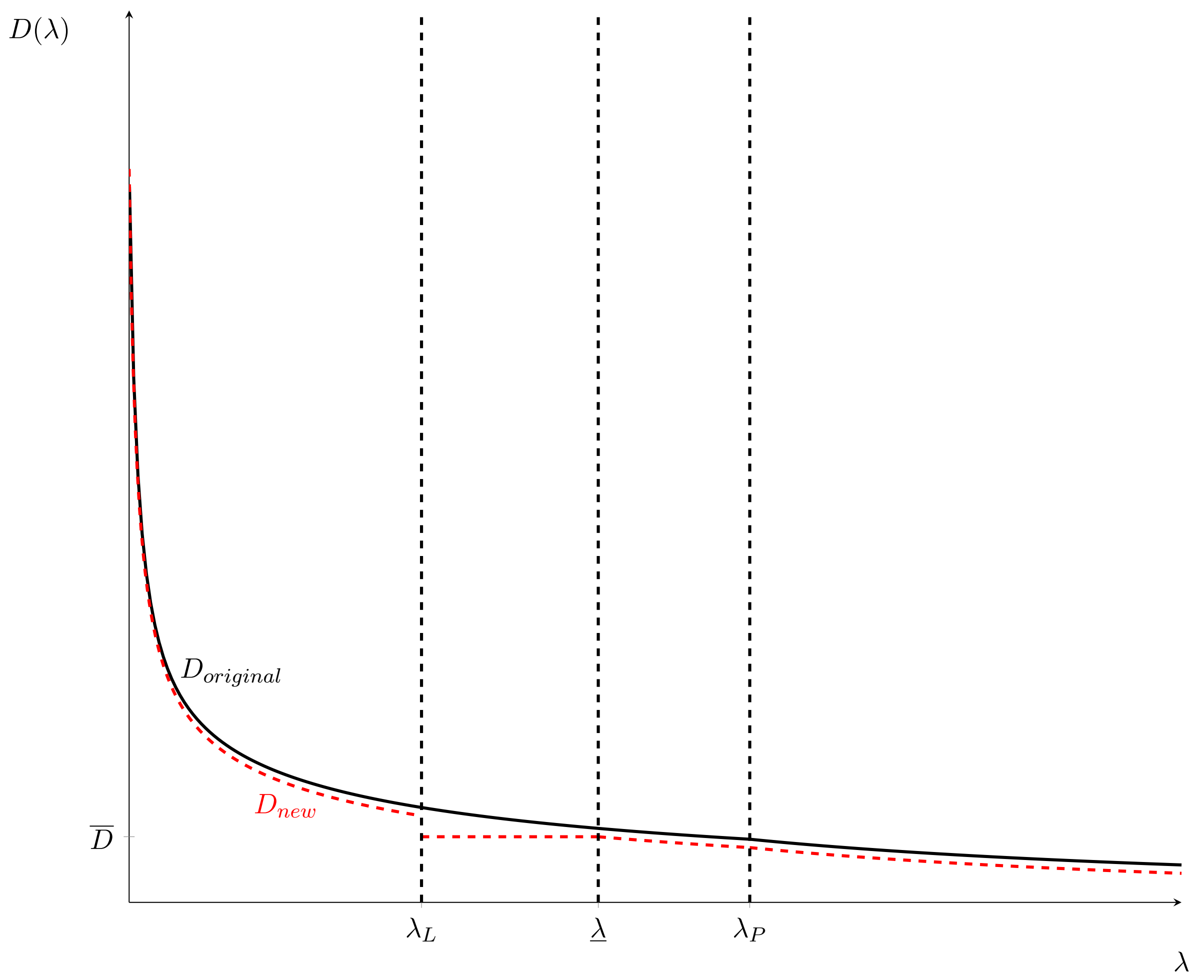
$$S_T(\lambda)\begin{cases}\int_0^{D(\lambda)}\left(ye^{-\lambda t}-ru\right)e^{-rt}\,dt+\max\left(S_P(\lambda),0\right)e^{-(r+\lambda)D(\lambda)}-c-c_{\text{LONG}}, & \text{if } D(\lambda)>\overline{D}\\ \int_0^{D(\lambda)}\left(ye^{-\lambda t}-ru\right)e^{-rt}\,dt+\max\left(S_P(\lambda),0\right)e^{-(r+\lambda)D(\lambda)}-c, & \text{if } D(\lambda)\leq\overline{D}\end{cases} \tag{7}$$

In equilibrium, employees and firms choose the optimal duration $D^*(\lambda)$ since it maximizes the expected surplus for a contract given λ. Hence, $D^*(\lambda)$ is obtained from the first order condition of equation 7 with regard to the duration D:

$$\frac{\partial S_T(\lambda,D)}{\partial D}=0\Leftrightarrow ye^{-\lambda D}=rU+(r+\lambda)e^{-\lambda D}\max\left(S_P(\lambda),0\right) \tag{8}$$

Since long temporary contracts incur the cost c_{LONG}, the duration profile is changed compared to the original model by Cahuc et al. (2016). This change is illustrated in figure B1. Note that the duration $\overline{D}$ implies an expected shock arrival rate $\underline{\lambda}$ such that $\overline{D}=D(\underline{\lambda})$. For a shock arrival rate λ_L smaller than $\underline{\lambda}$ it is reasonable for firms and workers to go permanent earlier at $\overline{D}$ instead of paying c_{LONG}. Thus, in all fixed-term contracts with shock arrival rates between λ_L and $\underline{\lambda}$ firms and workers choose the duration $\overline{D}$.

Figure B1: Duration of Fixed-Term Contracts



B.2 Equilibrium conditions and comparative statics

At an equilibrium with $\lambda_S < \lambda_L < \underline{\lambda} < \lambda_P < \lambda_T$ six conditions are satisfied:

1. **Temporary job creation rule:**
   Jobs are only created if the surplus of a temporary employment contract is greater than zero:
   $$S_T(\lambda_T) = \frac{y}{r+\lambda_T}\left(1 - e^{-(r+\lambda_T)D(\lambda_T)}\right) - U\left(1 - e^{-rD(\lambda_T)}\right) - c = 0$$
   The parameter λ_T is the maximum shock arrival rate at which jobs can be created, and is determined by the point at which the surplus of a temporary contract with optimal contract duration is zero. To obtain this expression I consider that the continuation value of the job match has to be 0 at λ_T so that the first order condition for the contract duration can be simplified to:
   $$ye^{-(r+\lambda_T)D(\lambda_T)} = rUe^{-rD(\lambda_T)}$$
   Using this condition and the original definition of the fixed-term job creation rule yields:
   $$h^{\text{TJCR}}(\lambda_T, \theta) = \frac{(1 - \frac{\lambda_T}{r})y}{r+\lambda_T} e^{-(r+\lambda_T)D(\lambda_T)} - U(\theta) - c = 0 \tag{TJCR}$$

2. **Temporary job destruction rule:**
   As jobs with a shock arrival rate above λ_P are not continued after the end of a temporary contract, job destruction is obtained from the point at which the surplus of a permanent contract is zero $S_P(\lambda_P) = 0$.
   $$h^{TJDR}(\lambda_P, \theta) = \lambda_P - \frac{y - rU(\theta) - rc_P}{c_P + f} = 0 \tag{TJDR}$$

3. **Starting as permanent job rule:**
   The parameter λ_S, which specifies whether jobs start with a temporary or permanent contract, is obtained by equating the surplus of a temporary contract at optimal duration with the surplus of an unlimited contract $S_T(\lambda_S, D^*(\lambda_S)) = S_P(\lambda_S)$.
   $$h^{\text{SPJR}}(\lambda_S, \theta, c) = \frac{\lambda_S f}{r+\lambda_S} - \lambda_S \frac{U\left(1 - e^{-rD(\lambda_S)}\right)}{r+\lambda_S} - c - c_{\text{LONG}} = 0 \tag{SPJR}$$

4. **Long duration temporary job rule:**
   The parameter λ_L is determined by the two cases of S_T. In particular it marks the point, where the difference between the continuation value at $D(\lambda_L)$ and the continuation value at duration $\overline{D}$ is equal to c_{LONG}
   $$h^{\text{LDTJR}}(\lambda_L, \theta, c_{\text{LONG}}) = \frac{\lambda_L U e^{-rD(\lambda_L)}}{r+\lambda_L} + \frac{rU + \lambda_L f + (r+\lambda_L)\, c}{r+\lambda_L} e^{-(r+\lambda_L)\overline{D}} - Ue^{-r\overline{D}} - c_{\text{LONG}} \tag{LDTJR}$$

5. **Level of λ associated with $\overline{D}$:**
   The shock arrival rate for fixed-term contracts associated with the duration $\overline{D}$ that does not incur additional costs c_{LONG} is determined by $\overline{D}$ and labour market tightness θ:
   $$h^{\text{DOPT}}(\underline{\lambda}, \theta, \overline{D}) = -\overline{D} + D(\underline{\lambda}, \theta) = 0 \tag{DOPT}$$

6. **Free entry condition:**
Lastly, there is a free entry condition for firms that equalises the expected surplus of a job with the costs of its creation κ.

$$\begin{aligned} h^{\mathrm{EC}}(\theta, c_{\mathrm{LONG}}) = \kappa - q(\theta)(1-\gamma)\Bigg[\int_0^{\lambda_S} S_P(\lambda)\, dG(\lambda) \\ + \int_{\lambda_S}^{\lambda_L} S_T(\lambda, c_{\mathrm{LONG}})\, dG(\lambda) + \int_{\lambda_L}^{\lambda_T} S_T(\lambda)\, dG(\lambda)\Bigg] \end{aligned} \tag{EC}$$

The valuation of unemployment $U(\theta)$ in these conditions is an increasing function of labour market tightness. The free-entry condition implies

$$rU = z + \theta q(\theta)\frac{\gamma\kappa}{1-\gamma}, \tag{9}$$

where z is the flow utility of unemployment; substituting this outside option into the threshold conditions closes the equilibrium. Next, I obtain the effect on an increase in c_{LONG} on the labour market tightness and the threshold values for the shock arrival rate by using total differentials of the equilibrium conditions

1. **Labour Market tightness** $[c_{\mathrm{LONG}} \uparrow \Rightarrow \theta \downarrow]$
I find an overall negative effect on Labour Market tightness because the increase of c_{LONG} reduces the surplus of fixed-term contracts with durations longer than $\overline{D}$. I obtain this result from the total differential of $h^{\mathrm{EC}}(\theta, c_{\mathrm{LONG}})$:

$$\frac{d\theta}{dc_{\mathrm{LONG}}} = -\frac{\frac{\partial h^{\mathrm{EC}}}{\partial c_{\mathrm{LONG}}}}{\frac{\partial h^{\mathrm{EC}}}{\partial \theta}} < 0$$

Here the derivative towards c_{LONG} is positive as $\lambda_S < \lambda_L$ and $G()$ is a well defined CDF:

$$\frac{\partial h^{\mathrm{EC}}}{\partial c_{\mathrm{LONG}}} = q(\theta)(1-\gamma)[G(\lambda_L) - G(\lambda_S)] > 0$$

Moreover, the derivative towards θ is positive since the terms in both rows of the following equation are positive:

$$\begin{aligned} \frac{\partial h^{\mathrm{EC}}}{\partial \theta} =& - q'(\theta)(1-\gamma)\left[\int_0^{\lambda_S} S_P(\lambda)\, dG(\lambda) + \int_{\lambda_S}^{\lambda_L} S_T(\lambda, c_{\mathrm{LONG}})\, dG(\lambda) + \int_{\lambda_L}^{\lambda_T} S_T(\lambda)\, dG(\lambda)\right] \\ & - q(\theta)(1-\gamma)\left[\int_0^{\lambda_S} \frac{\partial S_P}{\partial \theta}\, dG(\lambda) + \int_{\lambda_S}^{\lambda_L} \frac{\partial S_T}{\partial \theta}\, dG(\lambda) + \int_{\lambda_L}^{\lambda_T} \frac{\partial S_T}{\partial \theta}\, dG(\lambda)\right] > 0 \end{aligned}$$

In particular, the first term of the derivative towards θ is positive since the matching function $q(\theta)$ is decreasing in θ, while the second term is positive since surplus of both contract types is decreasing in labour market tightness: $\frac{\partial S_T}{\partial \theta} < 0$ and $\frac{\partial S_P}{\partial \theta} < 0$

2. **Job creation** $[c_{\mathrm{LONG}} \uparrow \Rightarrow \theta \downarrow \Rightarrow U(\theta) \downarrow \Rightarrow \lambda_T \uparrow]$
As the cost increase leads to a decrease in the value of the outside option more short-term jobs with a very low surplus are created.I plugin the result from the effect of c_{LONG} into

the total differential of $h^{\text{TJCR}}(\theta, \lambda_T)$ and find a positive effect on λ_T

$$\frac{d\lambda_T}{d\theta} = -\frac{\frac{\partial h^{\text{TJCR}}}{\partial \theta}}{\frac{\partial h^{\text{TJCR}}}{\partial \lambda_T}} < 0 \qquad \Rightarrow \qquad \frac{d\lambda_T}{dc_{\text{LONG}}} = \frac{d\lambda_T}{d\theta} \times \frac{d\theta}{dc_{\text{LONG}}} > 0$$

The derivatives in this result of job creation are:

- Derivative towards θ is negative since all three terms are positive:

$$\frac{\partial h^{\text{TJCR}}}{\partial \theta} = -\frac{\partial U(\theta)}{\partial \theta}\left[\frac{r}{r+\lambda_T} + \lambda_T \frac{1 - e^{-rD(\lambda_T)}}{r+\lambda_T}\right]$$

- Derivative towards λ_T:

$$\frac{\partial h^{\text{TJCR}}}{\partial \lambda_T} = y\frac{\left[1 + (r+\lambda_T)D(\lambda_T)\right] e^{-(r+\lambda_T)D(\lambda_T)} - 1}{(r+\lambda_T)^2}$$

- Since for $x > 0$ it holds that $e^{-x} < \frac{1}{1+x}$ the derivative is negative:

$$\frac{\partial h^{\text{TJCR}}}{\partial \lambda_T} < y\frac{\left[1 + (r+\lambda_T)D(\lambda_T)\right] \frac{1}{1+(r+\lambda_T)D(\lambda_T)} - 1}{(r+\lambda_T)^2} = 0$$

3. **Job destruction** $[c_{\text{LONG}} \uparrow \Rightarrow \theta \downarrow \Rightarrow U(\theta) \downarrow \Rightarrow \lambda_P \uparrow]$
After an increase of c_{LONG} there is less job destruction as a lower outside option shifts the surplus of permanent contracts upwards and leads to an increase in the number of temporary contracts that are converted to permanent contracts. This is derived from the total differential of $h^{\text{TJDR}}(\theta, \lambda_P)$:

$$\frac{d\lambda_P}{d\theta} = -\frac{\frac{\partial h^{\text{TJDR}}}{\partial \theta}}{\frac{\partial h^{\text{TJDR}}}{\partial \lambda_P}} < 0 \qquad \Rightarrow \qquad \frac{d\lambda_P}{dc_{\text{LONG}}} = \frac{d\lambda_P}{d\theta} \times \frac{d\theta}{dc_{\text{LONG}}} > 0$$

For the derivatives in this result of job destruction it holds that:

- The Derivative towards θ is positive since $\frac{\partial U(\theta)}{\partial \theta} > 0$:

$$\frac{\partial h^{\text{TJDR}}}{\partial \theta} = \frac{\partial U(\theta)}{\partial \theta}\left[\frac{r}{c+f}\right]$$

- The Derivative towards λ_P is also positive :

$$\frac{\partial h^{\text{TJDR}}}{\partial \lambda_P} = 1 > 0$$

4. **Starting as permanent job rule** $[c_{\text{LONG}} \uparrow \Rightarrow \lambda_S \uparrow$ and $c_{\text{LONG}} \uparrow \Rightarrow \theta \downarrow \Rightarrow U(\theta) \downarrow \Rightarrow \lambda_S \downarrow]$:
An increase in c_{LONG} leads to a strong positive direct increase on the share of jobs that are started with permanent contracts. The increase in c_{LONG} decreases the surplus of fixed-term contracts on the interval from 0 to λ_L directly (and indirectly on the interval from 0 to $\underline{\lambda}$), which leads to more jobs being started with permanent contracts. There is also a feedback effect to this as the surplus of permanent contracts also decreases. This can be

shown with the total differential of $h^{\text{SPJR}}(\theta, \lambda_S, c_{\text{LONG}})$:

$$\frac{d\lambda_S}{dc_{\text{LONG}}} = -\underbrace{\frac{\frac{\partial h^{\text{SPJR}}}{\partial c_{\text{LONG}}}}{\frac{\partial h^{\text{SPJR}}}{\partial \lambda_S}}}_{\text{Positive direct effect}} - \underbrace{\frac{\frac{\partial h^{\text{SPJR}}}{\partial \theta}}{\frac{\partial h^{\text{SPJR}}}{\partial \lambda_S}} \times \frac{d\theta}{dc_{\text{LONG}}}}_{\text{Negative Feedback effect}}$$

Here the derivatives are given by:

- Derivative towards c_{LONG} :
$$\frac{\partial h^{\text{SPJR}}}{\partial c_{\text{LONG}}} = -1 < 0$$
- Derivative towards λ_S :
$$\frac{\partial h^{\text{SPJR}}}{\partial \lambda_S} = \frac{r\left(f - U\left(1 - e^{-rD(\lambda_S)}\right)\right)}{(r+\lambda_S)^2} - \frac{\lambda_S r U e^{-rD(\lambda_S)} \times \frac{\partial D(\lambda)}{\partial \lambda}}{r+\lambda_S} > 0$$
The first term of the derivative is positive since $h^{\text{SPJR}}(\theta, \lambda_S, c_{\text{LONG}}) = 0$ implies that $f = \left(1 - e^{-rD(\lambda_S)}\right) + \frac{r+\lambda_S}{\lambda_S} c_{\text{LONG}}$. The second term is positive since $\frac{\partial D(\lambda)}{\partial \lambda} < 0$
- Derivative towards θ
$$\frac{\partial h^{\text{SPJR}}}{\partial \theta} = -\frac{\partial U(\theta)}{\partial \theta}\left[\lambda_S \frac{1 - e^{-rD(\lambda_S)}}{r+\lambda_S}\right] < 0$$

5. **Long duration temporary job rule** [$c_{\text{LONG}} \uparrow \Rightarrow \lambda_L \downarrow$ and $c_{\text{LONG}} \uparrow \Rightarrow \theta \downarrow \Rightarrow U(\theta) \downarrow \Rightarrow \lambda_L \uparrow$]
The shock arrival below which contracts are converted to permanent earlier λ_L decreases for an increase in c_{LONG}. For this first introduction of a c_{LONG} compared to zero the parameter λ_L enters the model and leads to an interval in the optimal contract duration between λ_L and $\underline{\lambda}$ where all contract lengths are $\overline{D}$. However, for further increases of c_{LONG} there is also a feedback effect: the decline in labour market tightness lowers the outside option $U(\theta)$, which raises the surplus of temporary contracts at all durations.
The total differential of $h^{\text{LDTJR}}(\theta, \lambda_L, c_{\text{LONG}})$ reveals both a negative direct and a positive feedback effect on λ_L:

$$\frac{d\lambda_L}{c_{\text{LONG}}} = -\underbrace{\frac{\frac{\partial h^{\text{LDTJR}}}{\partial c_{\text{LONG}}}}{\frac{\partial h^{\text{LDTJR}}}{\partial \lambda_L}}}_{\text{Negative direct effect}} - \underbrace{\frac{\frac{\partial h^{\text{LDTJR}}}{\partial \theta}}{\frac{\partial h^{\text{LDTJR}}}{\partial \lambda_L}} \times \frac{d\theta}{dc_{\text{LONG}}}}_{\text{Positive feedback effect}}$$

In this expressions the derivatives are given by:

- Derivative towards c_{LONG} :
$$\frac{\partial h^{\text{LDTJR}}}{\partial c_{\text{LONG}}} = -1 < 0$$
- Derivative towards λ_L:
  (a) Note that h^{LDTJR} can be rewritten as
$$h^{\text{LDTJR}} = S_{\text{Short}}(\lambda_L, D(\lambda_L)) - S_{\text{Short}}(\lambda_L, \overline{D}) - c_{\text{LONG}},$$
with $S_{\text{Short}}(\lambda, D) = \int_0^D \left(ye^{-\lambda t} - ru\right) e^{-rt}\, dt + \max\left(S_P(\lambda), 0\right) e^{-(r+\lambda)D} - c$

(b) For $h^{\text{LDTJR}} < 0$ it has to hold that:

$$\frac{\partial h^{\text{LDTJR}}}{\partial \lambda_L} = \frac{\partial S_{\text{Short}}(\lambda_L, D(\lambda_L))}{\partial \lambda_L} - \frac{\partial S_{\text{Short}}(\lambda_L, \overline{D})}{\partial \lambda_L} < 0$$
$$\Rightarrow \frac{\partial S_{\text{Short}}(\lambda_L, D(\lambda_L))}{\partial \lambda_L} < \frac{\partial S_{\text{Short}}(\lambda_L, \overline{D})}{\partial \lambda_L}$$

(c) Note that S_{Short} is continuous and monotonically decreasing in λ and that by definition it holds that for $\underline{\lambda} > \lambda_L$:

$$S_{\text{Short}}(\underline{\lambda}, \overline{D}) = S_{\text{Short}}(\underline{\lambda}, D(\underline{\lambda}))$$

(d) Moreover at λ_L it holds that:

$$S_{\text{Short}}(\lambda_L, D(\lambda_L)) - S_{\text{Short}}(\lambda_L, \overline{D}) = c_{\text{LONG}} > 0$$

(e) This implies that for $\lambda_L \leq \lambda < \underline{\lambda}$:

$$\frac{\partial S_{\text{Short}}(\lambda_L, D(\lambda_L))}{\partial \lambda_L} < \frac{\partial S_{\text{Short}}(\lambda_L, \overline{D})}{\partial \lambda_L} \qquad \text{and} \qquad \frac{\partial h^{\text{LDTJR}}}{\partial \lambda_L} < 0$$

- Derivative towards θ:

$$\frac{\partial h^{\text{LDTJR}}}{\partial \theta} = \frac{\partial U(\theta)}{\partial \theta}\left[\frac{\lambda_L e^{-rD(\lambda_L)}}{r+\lambda_L} + \frac{r}{r+\lambda_L}e^{-(r+\lambda_L)\overline{D}} - e^{-r\overline{D}}\right] < 0$$

- Note that $e^{-(r+\lambda_L)\overline{D}} < e^{-r\overline{D}}$ for any $\lambda > 0$ and that $e^{-rD(\lambda_L)} < e^{-r\overline{D}}$ for $\lambda < \underline{\lambda}$. Thus it holds that:

$$\frac{\partial h^{\text{LDTJR}}}{\partial \theta} < \frac{\partial U(\theta)}{\partial \theta}\left[\frac{\lambda_L e^{-r\overline{D}}}{r+\lambda_L} + \frac{r}{r+\lambda_L}e^{-\overline{D}} - e^{-r\overline{D}}\right] = 0$$

6. Shock arrival rate where $\overline{D}$ is optimal [$c_{\text{LONG}} \uparrow \Rightarrow \theta \downarrow \Rightarrow U(\theta) \downarrow \Rightarrow \underline{\lambda} \uparrow$]:
The shock arrival rate where choosing $\overline{D}$ is optimal increases as implied by the total differential of $h^{\text{DOPT}}(\theta, \underline{\lambda}, \overline{D})$:

$$\frac{d\underline{\lambda}}{d\theta} = -\frac{\frac{\partial h^{\text{DOPT}}}{\partial \theta}}{\frac{\partial h^{\text{DOPT}}}{\partial \underline{\lambda}}} < 0 \qquad \Rightarrow \qquad \frac{d\underline{\lambda}}{dc_{\text{LONG}}} = \frac{d\underline{\lambda}}{d\theta} \times \frac{d\theta}{dc_{\text{LONG}}} > 0$$

Here the necessary derivatives are:

- Derivative towards θ is positive since $\frac{\partial U(\theta)}{\partial \theta} > 0$:

$$\frac{\partial h^{\text{DOPT}}}{\partial \theta} = \frac{\partial U(\theta)}{\partial \theta}\left[\frac{-rc - \underline{\lambda}(f+c)}{\underline{\lambda}\,(rU + \underline{\lambda} f + (r+\underline{\lambda})c)}\right] < 0$$

- Derivative towards $\underline{\lambda}$:

$$\frac{\partial h^{\text{DOPT}}}{\partial \underline{\lambda}} = \frac{\partial D(\lambda)}{\partial \lambda} < 0$$

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