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IZA DP No. 15407

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ABSTRACT

Are Shorter Cumulative Temporary Contracts Worse Stepping Stones? Evidence from a Quasi-Natural Experiment

Temporary employment contracts are often regarded as 'stepping stones' for workers' careers, because they can help inexperienced workers secure a permanent contract. Our study evaluates whether this stepping-stone function is moderated by the contract duration, exploiting a Dutch policy reform that shortened the maximum duration of sequences of temporary contracts with the same employers from 3 years to 2 years. Leveraging a sharp regression discontinuity design and administrative register data, we show that the reform accelerated the transitions of temporary workers to permanent contracts with the same employers, with the effect being strongest among those working for the same employers for 1-2 years. We conclude that the reform brought more job security to temporary workers without impeding the stepping-stone function of their contracts.

JEL Classification: J28, J41, J42

Keywords: temporary contracts, permanent contract, stepping stone, chain rule

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1. Introduction

Many European countries, including the UK, France, Italy, Portugal, Spain and the Netherlands, have dual labour markets in which temporary (or fixed-term) and permanent (or open-ended) contracts coexist. This duality is characteristic for countries that use stringent employment protection legislation (EPL) to disincentivize employers from firing workers with permanent contracts. In such labour markets, temporary contracts serve several important functions. One of these functions is that they facilitate employers' responses to transitory shocks, enabling them to quickly adjust their labour capacity while incurring minimal adjustment costs (see Bentolila and Bertola, 1990). Another important function is that they facilitate the screening of job candidates, acting as de-facto probationary periods for permanent positions. In this regard, temporary contracts can be thought of as 'stepping stones' that facilitate workers' transitions to permanent contracts (see Booth et al., 2002; Van den Berg et al., 2002; Gash, 2008; Ichino et al., 2008; de Graaf-Zijl et al., 2011; Faccini, 2014).

At the same time, temporary contracts can also prove damaging to the workers and the economy. Monopsonistic employers can be incentivized to rely on temporary contracts merely for the sake of avoiding the EPL (see Serrano, 1998; Blanchard and Landier, 2002; Knegt et al., 2007; Kahn, 2010; Aguirregabiria and Alonso-Borrego, 2014), leaving their workers exposed to economic fluctuations and job uncertainty. The rising share of temporary employment in many European countries also raises concerns about labour market segmentation, with more and more workers being trapped in precarious low-paid jobs that offer little to no prospect of upward mobility (see Nätti, 1993; Alba-Ramirez, 1998; Amuedo-Dorantes, 2000; Brown and Sessions, 2003; D'Addio and Rosholm, 2005; Güell and Petrongolo, 2007).

In light of these issues, commentators and policymakers have stressed the importance of government regulation, pointing out that the regulation can be used to set an appropriate balance between job flexibility and job security (see European Commission, 2003). One of the common regulatory instruments is to set a threshold (either in terms of the number of consecutive contracts or the total contract duration) after which a temporary contract has to be made permanent.[1] Advocates of these contract thresholds emphasize their benefits for workers' job security, whereas critics argue that they can impede vital economic functions of temporary contracts (such as the stepping-stone effect). In this paper, we evaluate the validity of such claims using a quasi-natural experiment arising from a reform of the temporary contract legislation in the Netherlands.

The reform lowered the threshold for maximum duration of a sequence of temporary contracts with the same employer (labeled as a "contract chain") from 36 months to 24 months, creating a sharp discontinuity in regulations applicable to contracts ending before and after July 1, 2015. Specifically, the contracts ending before the cutoff date could be followed by another temporary contract compli-

[1] Many countries have been using this instrument to tilt the regulatory balance one way or another (see OECD, 2020). For example, France increased the maximum number of successive temporary contracts from two to three in August 2015. On the contrary, Italy reduced the maximum duration of temporary contracts from 36 months to 24 months in July 2018.

ant with the old regulation (becoming permanent once the contract-chain duration reaches 36 months), whereas the contracts ending after the cutoff date could be only followed by temporary contracts compliant with the new regulation (becoming permanent once the contract-chain duration reaches 24 months). This reform extends naturally to the regression discontinuity (RD) design, with the calendar time being the running variable and July 1, 2015 being the discontinuity threshold. Using population-level register data combined with the labour force survey, we compare the labour market transitions of temporary contracts ending before and after the discontinuity threshold, investigating whether the reform affected the stepping-stone function of temporary contracts.

We show that the reform accelerated the stepping-stone function of temporary contracts, increasing workers' probabilities of transitioning to permanent contracts with the same employer. The effect is the strongest among workers with contract-chain durations between 13 and 24 months, increasing by 4.2 percentage points (10.9%). The reform did not have a statistically significant effect on transitions to temporary contracts or transitions to permanent contracts with different employers, but it did lead to a significant decrease in the probability of transitions to unemployment benefits. This suggests that the reform increased workers' job opportunities, with firms looking for a "buffer stock" of new temporary workers (who replaced those transitioning to permanent contracts).

Next, we investigate the possibility that the employers were incentivized to issue shorter temporary contracts following the discontinuity threshold, finding no empirical evidence of this practice. We also evaluate the effect heterogeneity, finding that the reform was particularly important for the outcomes of younger and less-educated workers. A battery of robustness checks reveals that our results are not meaningfully sensitive to the model specification. Finally, we explore anticipation effects, concluding that the employers were unlikely to manipulate the timing of temporary contracts in response to the reform, which bolsters the validity of our identification strategy.

The main contributions of our paper are twofold. First, our paper provides empirical evidence that aligns with the perspectives emphasizing the screening function of temporary contracts (see Faccini, 2014). We show that temporary contracts do not function only as an expendable buffer stock for economic fluctuations, but that they also provide workers a viable pathway towards permanent positions. Second, our paper contributes to the literature documenting the labour market effects of contract duration restrictions. Most of the scholarship on dual labour market policies has focused on policies that dealt with the objectives of temporary contracts, or the dismissal costs of permanent contracts (see Güell and Petrongolo, 2007; Cahuc et al., 2020). The literature looking into the effects of contract duration restrictions is much smaller, consisting of Martins (2016) and Silva et al. (2018), both of whom found a drop in the contract conversion rate after a reform that increased the maximum cumulative duration of temporary contracts in Portugal. To the best of our knowledge our paper is the first to investigate the effects of a reform that acted in the opposite direction, tightening the duration restrictions on temporary contracts. Our findings also provide additional insights because the Dutch institutional context differs from that of Portugal, being more comparable to the European countries that assume an intermediate position with regard to the stringency of EPL.[2].

The rest of the paper is organized as follows. The next section provides a brief summary of the existing literature. Section 3 introduces the background for the temporary contract in the Netherlands and the reform of the chain rule in 2015. Section 4 presents the data and descriptive statistics. Section 5 and 6 describe the empirical strategy and the main result.Section 7 explores the effect heterogeneity, Section 8 documents a battery of robustness checks, and Section 9 evaluates anticipation effects. Section 10 concludes.

2. Literature

There is an extensive literature studying the EPL and the duality of European labour markets. The justifications for the EPL include the need to protect employees from unfair treatment by their employers, the limited ability of employees to insure themselves against the risk of dismissal, and the long-term benefits of preserving firm-specific human capital investments (see Pissarides, 2010). At the same time, the EPL is recognized for imposing considerable costs on employers, since it limits their ability to adjust their production factors to short-run variations in demand and technology. The EPL reduces job destruction, but at the same time it discourages job creation, which can cause labour market deficiencies. Temporary contracts can compensate for these deficiencies, acting as a "buffer stock" which can be adjusted in response to economic shocks (see Bentolila and Bertola, 1990; Bentolila and Saint-Paul, 1994; Kugler and Pica, 2008; Skedinger, 2011; Martin and Scarpetta, 2012; Hijzen et al., 2017).

Temporary contracts are generally recognized as inferior to permanent contracts. Booth, Francesconi, and Frank (2002) use data from the British Household Panel Survey to document that temporary workers have lower levels of job satisfaction, receive less training and are less well-paid than permanent workers. At the same time, survey studies also indicate that temporary contracts can act as a stepping stone to permanent work. Gagliarducci (2005) finds that the probability of moving from a temporary to a permanent job increases with the duration of the temporary contract, but decreases with the frequency of renewed temporary contracts (especially among contracts with interruptions). Faccini (2014) shows that in most European countries temporary workers enjoy relatively high rates of transition into permanent employment, and temporary contracts significantly decrease the unemployment rate. The latter finding is attributed to temporary contracts acting as an important screening device for permanent positions.

Several studies examine the labour market consequences of EPL restrictions on temporary contracts. Blanchard and Landier (2002) study a partial reform of employment protection that allowed French firms to hire workers on temporary contracts, showing that the reform increased job turnover but did not lead to a substantial reduction in unemployment duration. Aguirregabiria and Alonso-Borrego

[2]Based on different dimensions of labour market institutions, Boeri et al. (2011) classify the Netherlands into the cluster of Scandinavian countries, which maintain much less stringent employment protection than other continental European countries, such as France, Spain and Portugal.

(2014) study a reform that lifted restrictions on the use of temporary contracts in Spain, finding a positive effect on total employment and job turnover, but little effect on labour productivity and the value of firms. Closer to our study, Martins (2016) investigated a Portuguese reform that increased the maximum duration of temporary contracts from three to four and a half years, finding a drop in the rate of transitions to permanent contracts. Silva et al. (2018) focused on another Portuguese reform that increased the maximum contract duration from three to six years, finding similar effects.

Other studies focus on different aspects of dual labour markets. Güell and Petrongolo (2007) study a reform that reduced the dismissal costs of permanent workers in Spain, showing that the reform expedited the conversion of temporary contracts to permanent contracts. Cahuc et al. (2020) analyze the consequences of changes to the taxation of temporary labour, showing that policies aiming to motivate firms to create more permanent contracts and increase the job duration did not reach their objectives: they reduced the mean duration of jobs and decreased job creation, employment and welfare of unemployed workers.

3. Background

3.1. Temporary Contracts in the Netherlands

Temporary contracts constitute an important part of the Dutch labor markets, being particularly common among young workers. Figure 1 plots the shares of temporary employment in the Netherlands and in the other OECD countries, showing that, in year 2016, temporary contracts were held by 55.56% of the Dutch workers aged 15-24, 15.18% of the Dutch workers aged 25-54, and 7.08% of the Dutch workers aged 55-64. In contrast to the other OCED countries, the Netherlands attains relatively high shares of temporary employment.

Furthermore, the use of temporary contracts in the Netherlands is on the rise. This is captured by Figure 2 which plots the age-specific shares of temporary employment observed in the Netherlands between years 2000 and 2016. Over this period, the temporary employment shares of men and women aged 15-24 rose by more than 50% (approximately 20 percentage points). Even stronger relative change was recorded among men aged 25-54, whose share more than doubled (from 6.65% to 14.32%). The only demographic group subject to the opposite trend were women aged 55-64, whose share dropped by 25% (from 9.12% to 6.93%).

Although the overall level of employment protection in the Netherlands takes only an intermediate position compared to other European countries (see Boeri et al., 2011), the Dutch system is fairly distinctive in its relative treatment of permanent and temporary contracts. Figure 3 shows that the Netherlands has one of the highest levels of legislative protection for permanent contracts (against individual dismissal), being surpassed only by Portugal and the Czech Republic.[3] At the same time, the

[3]This is because the Netherlands employs a rigorous system of dismissal grounds for workers with permanent contracts. The employer can choose to either ask permission from the labour office or to go to court, however the former option implies inconvenient and time-consuming bureaucratic procedure whereas the latter implies making a substantive severance payment.

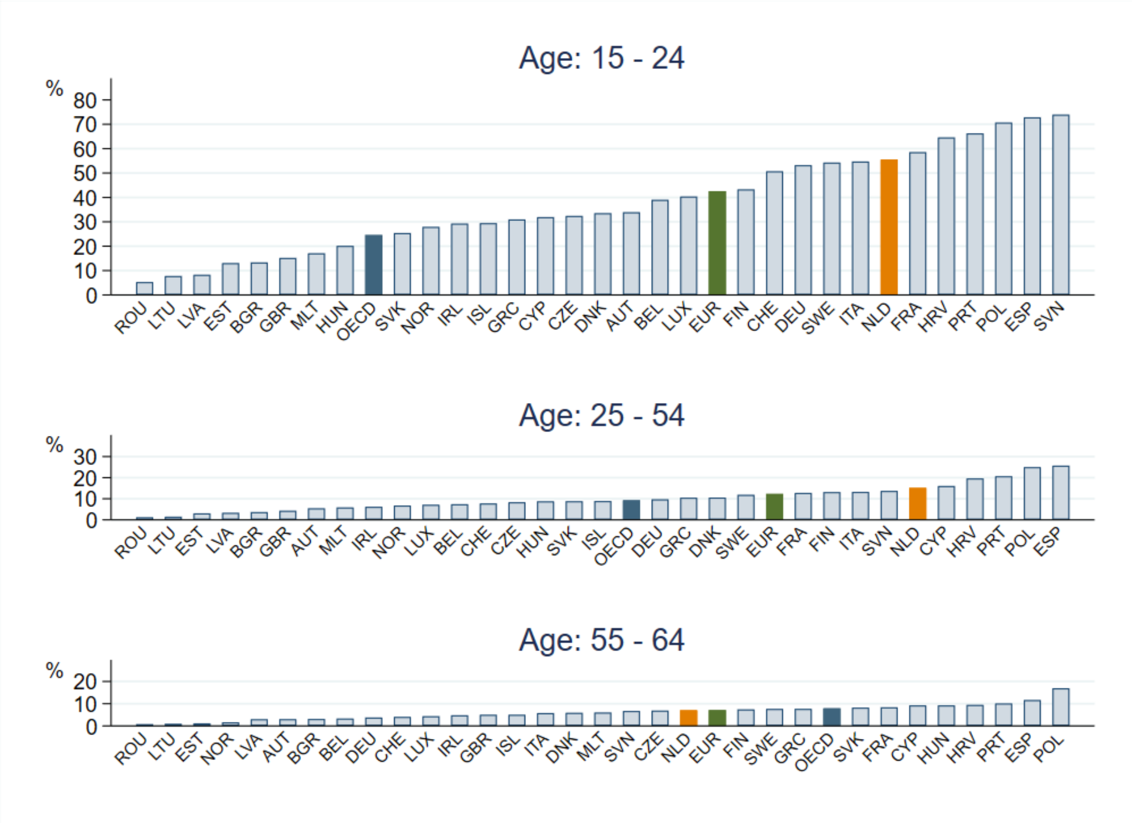

Figure 1: International Comparison of Temporary Employment Shares, Expressed as Percentages of Total Dependent Employment
Source: OECD Dataset: LFS - Employment by Permanency, 2016.

Netherlands has one of the lowest level of protection for temporary employment in Europe, with the lower level attained only by the United Kingdom.[4]

3.2. The Chain Rule and the 2015 Reform

To increase the flexibility of the labour market while maintaining an adequate level of protection for employees on temporary contracts, the Dutch government enacted the Flexibility and Security Act (*Wet Flexibiliteit en Zekerheid*) on 14 May 1998, in which the chain rule (*ketenregeling*) was first introduced. The chain rule targeted sequences of consecutive temporary contracts signed by the same employer and employee, with 'consecutive' being defined as not having interruptions longer than 3 months at a time. The rule stipulated that a temporary contract will be automatically turned into a permanent contract if 1) the total duration of the contract sequence (including its interruptions) has exceeded 36 months, or 2) if the contract has been renewed for the third time within the given sequence.[5] Note that the chain rule also applies to successive employment contracts between the same employee

[4]Prior to 2015, employers were able to terminate temporary contracts without giving workers any early notice. Since the Work and Security Act (Wwz) in 2015, there is a new requirement to give notice for termination of temporary contracts as well, with a minimum period of one month.

[5]The Dutch Civil Code has no provision prohibiting fixed-term employment contracts in excess of three years. The Dutch word "*keten*" in the definition "*ketenregeling*" stands for "chain", which

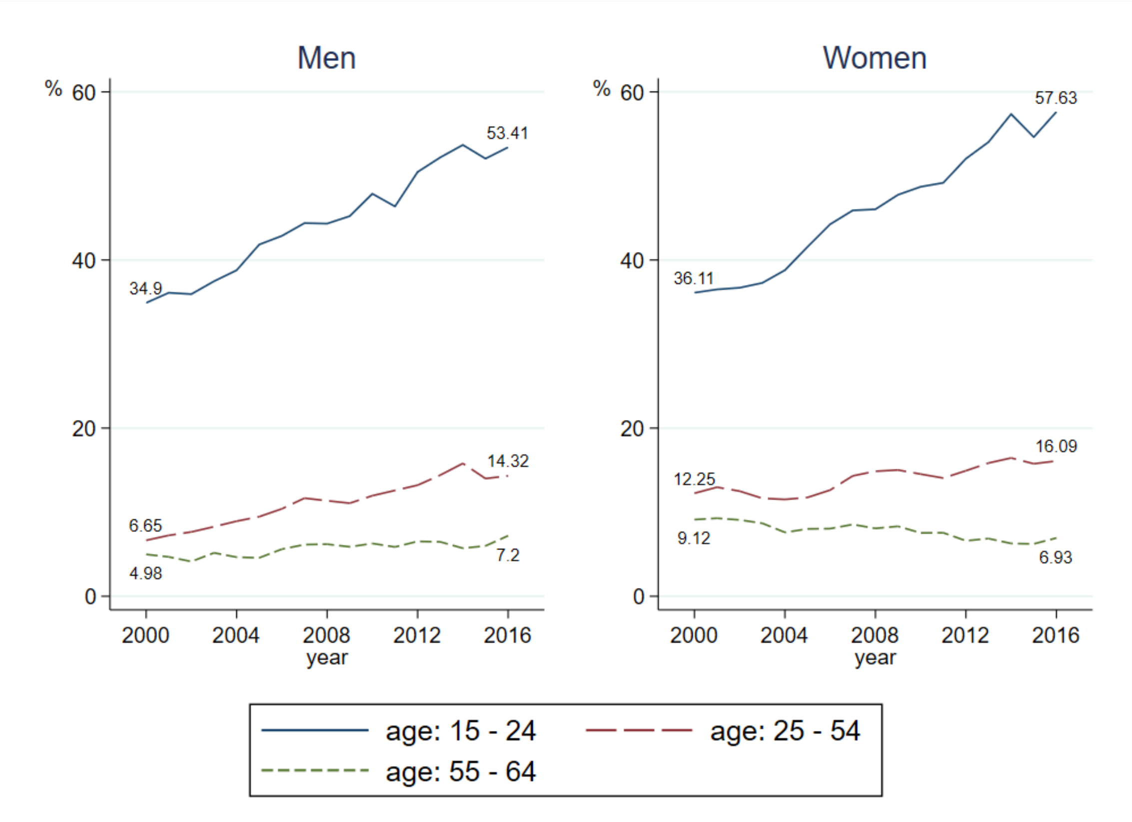

Figure 2: Temporary Employment in the Netherlands in 2000-2016, Expressed as Percentages of Total Dependent Employment
Source: OECD Dataset: LFS - Employment by Permanency, 2000 - 2016.

and multiple employers who (with regard to the work involved) should reasonably be deemed to succeed one another. This treatment is conditional on the worker using the same skills and having the same responsibilities across the contracts concerned, and on the existence of a link between the new employer and the former one (e.g., a merger, a relaunched company, or work for a consortium of employers).

The reform at the heart of this study was part of the Work and Security Act (*WWZ: Wet Werk en Zekerheid*), which was based on the social agreement concluded after the Dutch cabinet held consultations with central employers' and employees' organisations represented by the Labor Foundation (Stichting van de Arbeid) on 11 April, 2013 in the ROC Mondriaan in The Hague[6]. The proposal for WWZ was adopted by the House of Representatives on 18 February, 2014, and by the Senate on 10 June, 2014. WWZ was intended to strengthen the legal position of temporary workers, reduce the regulatory gap between temporary and permanent

means that in order to apply the "*ketenregeling*", there must be a chain of contracts following each other. Should the contracting parties agree on a singular temporary employment contract that exceeds three years, then the rule would not apply and the contract would not be automatically converted into a permanent contract.

[6]See Kamerbrief resultaten sociaal overleg, 11 April, 2013, Ministerie van Social Zaken en Werkgelegenheid. And see Perspectief voor een sociaal én ondernemend land: uit de crisis, met goed werk, op weg naar 2020, 11 April, 2013, Stichting van de Arbeid.

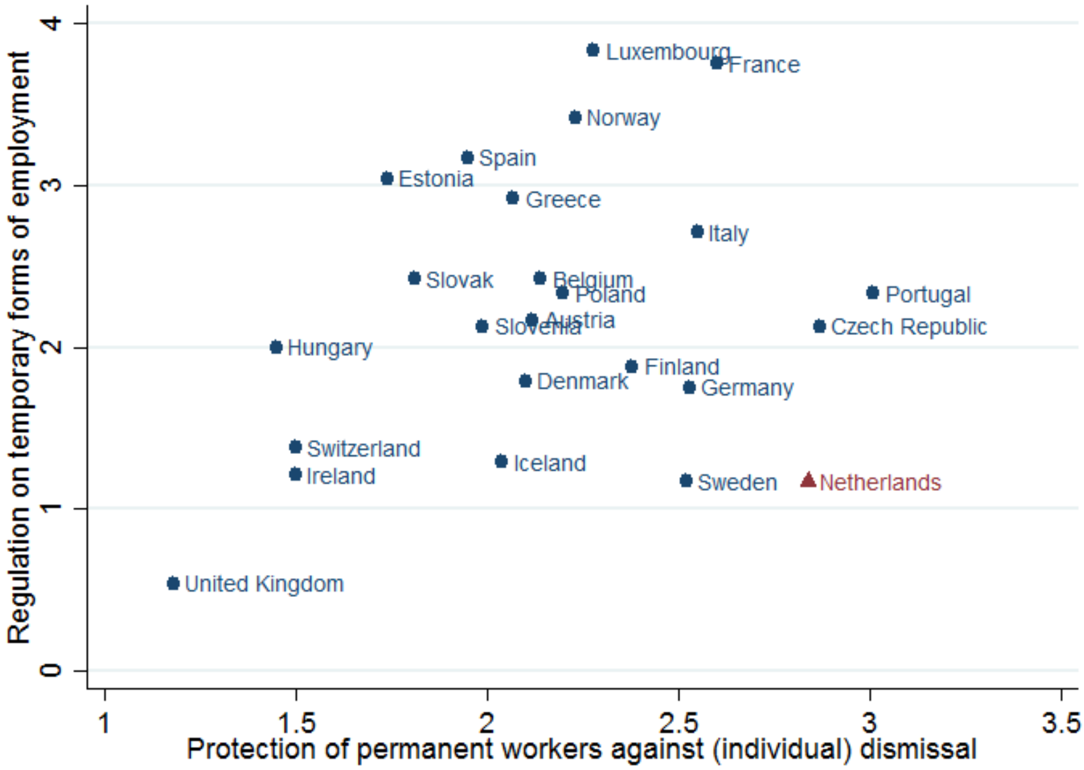

Figure 3: Employment Protection Indicators for European Countries
Source: OECD Employment Protection Database 2013 - 2014.

contracts, simplify the dismissal rules, and get more people out of the unemployment benefit scheme.

The new law altered the chain-rule regulations in the following ways: For contract chains starting on or after 1 July, 2015, the total duration threshold was reduced from 36 months to 24 months, and the maximum length of contract interruptions was increased from 3 months to 6 months. The threshold for the maximum number of temporary contracts within a sequence was kept at 3. To illustrate some practical considerations, if the same employer-employee pair engaged in two adjacent contracts of nine months, had a break for 6 months, and then engaged in a third contract of eight months, then the last contract would have immediately become a permanent contract because the total chain length at the start of the third contract is $9 + 9 + 6 + 1 = 25$, which is greater than the new threshold (further examples are provided in Figure 4).

For contract chains crossing 1 July, 2015, a transitional law (*Wet werk en zekerheid Artikel XXIIe*) was put into place. First, to qualify for the chain rule, the law stipulated that the contract interruptions preceding 1 July, 2015 should not be longer than 3 months, and the interruptions crossing or following 1 July, 2015 should not be longer than 6 months. Second, if a temporary contract within the chain commenced prior to 1 July, 2015, then this contract was to be subject to the 36-month duration threshold; if the contract commenced afterward, then it was to be subject to the 24-month threshold. From the perspective of our research design, the key

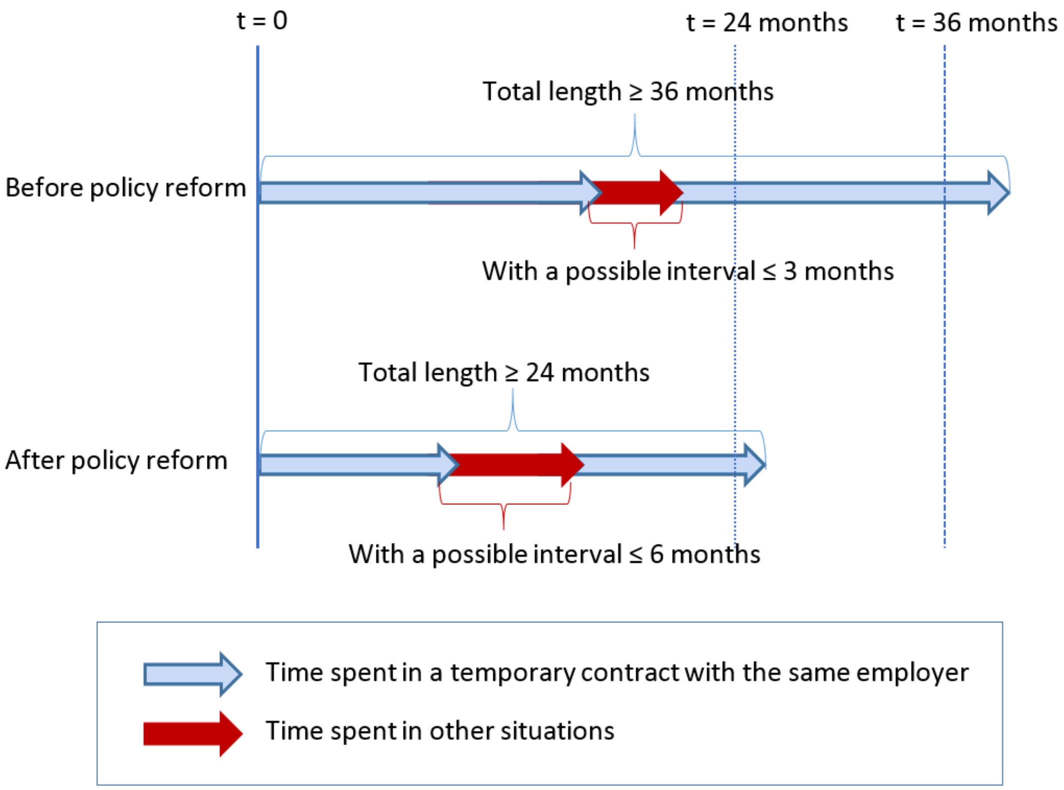

Figure 4: An Illustration of Chain Rule Criteria before and after the 2015 Reform

implication was that temporary contracts ending prior to the cutoff date could have been followed by another contract compliant with the old regulation, whereas those ending afterward could not.[7]

There are several exceptions to the chain rule (both before and after the 2015 reform). If an employer hires a new employee on a temporary contract longer than the maximum legislated duration and this contract is followed by another temporary contract for no more than three months, then the second contract is not subject to the chain rule[8]. The chain rule also does not apply to employees who are not yet 18 years old, to employees engaged in dual learning-work training, and to low-intensity part-time contracts (less than 12 hours of work per week).

Further exemptions can be operationalized through collective labour agreements

[7]To illustrate further practical considerations, if an employer and a new employee engaged in a one-year temporary contract ending in June 2015, then the same employer-employee pair could have followed this contract with another two-year temporary contract effective immediately. In this situation, the employee would not be entitled to a permanent contract, because the last contract was subject to the old regulation and the total chain length (12 + 24 = 36) was not greater than the old duration threshold. However, if the two-year temporary contract were to start six months later, then it would be subject to the new regulation and the employee would automatically become a permanent employee in July 2016 (12 + 6 + 7 = 25).

[8]See Artikel 668a van Boek 7 van het Burgerlijk Wetboek.

(CLA). In terms of the prevalence of collective labor agreements, the 2016 CLA report[9] indicates that approximately 76% of all employees in the Netherlands are covered by collective labor agreements. By examining 90 CLAs with an expiry date on or after December 31 2015 (covering about 85% of employees under CLAs), the report shows that 20 CLAs do not contain any agreements about the chain rule; 28 CLAs refer to the law (implicit agreements); 35 CLAs have explicit agreement in accordance with the new chain rule; 5 CLAs (7% of the employees involved in the sample) are in accordance with the old chain rule; and 2 CLAs (4% of the employees involved in the sample) contain agreements that deviate from the standard chain rule provision for all employees. Therefore, the percentage of employees who are not subject to the chain rule is roughly 10%[10]. The existence of CLAs that do not conform to the new chain rule could lead to a downward bias in the estimated impact of the reform. However, according to the CLA report, since the percentage is only about 10%, the downward bias is likely to be relatively minor.

Beyond reforming the chain rule, WWZ also introduced other changes effective on 1 July, 2015. First, the new policy simplified the dismissal rules, aiming to standardize the legal protections against different types of dismissal.[11] Second, the law mandated a transition allowance to be paid to employees subject to an involuntary dismissal after working for the same employer for two years or more. Third, the maximum duration of unemployment benefits was shortened from 38 to 24 months.

From the perspective of our analysis, it is important to discuss whether these concurrent policy changes could have affected the studied outcomes. The simplification of dismissal rules was unlikely to wield a major influence on employers and employees, because it was mainly aimed at shortening the processing times (without affecting the financial costs of involuntary dismissals). The transition allowance increased the cost of contracts longer than two years, which could have had a negative effect on transitions from temporary to permanent contracts. The shortening of unemployment benefit durations was largely irrelevant for workers on temporary contracts, because workers had to work for more than 36 years to qualify for more than 24 months of unemployment benefits.[12]

4. Data

The data used in our analyses come primarily from population-level administrative registers maintained by Statistics Netherlands (CBS). The backbone of our data is the SPOLISBUS register, which contains monthly records of all workers employed in the Netherlands between years 2010 and 2018. These records include earnings, hours of work, contract type (permanent or temporary, and part-time or full-time), sector of employment, and employer identifiers (BEID), which can be used

[9] See CAO-AFSPRAKEN 2016, Ministerie van Sociale Zaken en Werkgelegenheid

[10] ((7%+4%)/85%*76%)

[11] The new rules stipulated that a business dismissal or a dismissal due to a long-term disability are to be resolved by the Employee Insurance Agency; a dismissal for other reasons are to be resolved by a sub-district court.

[12] As a basic rule, each year of work entitles the person to one month of unemployment benefits. After 10 years of work, the entitlements increase more slowly, by one month every two years.

to identify contract chains. Other relevant information is linked from the SECMBUS and EBB datasets. SECMBUS is a population-level register that contains monthly socio-economic categories (SECM) of Dutch residents, distinguishing those who are employed, self-employed, and receiving unemployment benefits, among other socio-economic categories. EBB is the Dutch Labor Force Survey, which is a rotating panel of approximately 53,000 households. EBB is used to recover various worker characteristics, such as their age, gender, education level, and immigration status. By merging EBB with SPOLISBUS and SECMBUS, we create a dataset of 2,650,758 working-age individuals with complete employment histories between years 2010 and 2018 and observable background characteristics. This sample corresponds to approximately 15% of the total population in the Netherlands.

4.1. Sample Selection Criteria

To construct our principal analytical dataset, we first identify all temporary contracts ending between January 2013 and December 2016. Then we work backwards, recovering the contract chains corresponding to each of these contracts. Restricting our attention to contracts ending in or after January 2013 plays a crucial role in this regard, because the contract chains can be up to three-years long, and the data starts in January 2010.

We reconstruct the contract chains by adhering to the interruption rules, linking retrospective employment contracts between the same employer-employee pairs that are not separated by more than 3 months before July 2015 and 6 months from July 2015 onwards. Note that the data do not contain a flag for contract renewal, which means that we are only able to observe renewals if there is a gap between two adjacent contracts. If there is no gap, then the two contracts are recorded as an uninterrupted employment spell and we cannot ascertain the corresponding date of renewal. The observed contract renewals are used in the following ways. First, we use them to split the chains into finer contract sequences. For example, if we observe a chain in which the contract is twice renewed, then we denote the three contracts forming this chain as C1, C2 and C3, and we treat C1, C1+C2, and C1+C2+C3 as three separate observations. By splitting the chain in this way, we can evaluate the transition decisions made at the three distinct lengths of the contract chain.

Second, we use the contract renewal data to discard a small number of chains with more than three temporary contracts. Since these chains do not conform with the chain-rule thresholds, we believe that they correspond to workers exempted from the legislation on the basis of special collective labour agreements. For similar reasons, we also exclude contract chains in public-sector organizations (which are more likely to be subject to special collective labour agreements), and temporary work agencies (the agencies are exempted from the chain rule). In addition, we exclude contract chains for which the average monthly salaries are below EUR 500 or above EUR 10,000, and we restrict our attention to individuals who were aged 18-60 in 2010 and who completed their highest education between 1960 and 2010. Finally, we restrict our principal analytical dataset to contract chains in which the last temporary contract is classified as full-time. This restriction is motivated by the substantial heterogeneity in the nature of the jobs and the individual characteristics of employees working part-time and full-time.[13] As a robustness check, we also estimate the model without the part-time restriction.

4.2. Descriptive Statistics

Table 1 reports the descriptive statistics corresponding to our principal analytical dataset. The first column lists the average values of characteristics corresponding to the full sample of contract sequences, whereas in the second and the third column the sample is split into two groups, depending on whether the last temporary contract within the sequence ended before or after July 1, 2015 (the date when the new chain rule came into effect).

As shown in Table 1, most contract sequences in the sample consist of only one temporary contract.[14] Temporary contracts are common in the Transport sector (24.2% of the contracts in our sample), and also in Finance & Economics (23.8% of the contracts in our sample). Most temporary workers in our sample are men, which is likely attributable to the exclusion of part-time contract chains.

Comparing the characteristics across Columns 2 and 3, we conclude that the characteristics of temporary contract sequences ending before and after the reform are largely comparable.

In Table 2 we explore what happens when the contract sequences come to an end. Specifically, we quantify the shares of contract sequences transitioning to different labor market destinations, conditional on their length and on the sequence ending before or after the reform. We classify the destinations into seven groups: signing a temporary contract with a different employer (TD) or with the same employer (TS), signing a permanent contract with a different employer (PD) or with the same employer (PS), becoming an entrepreneur (EN), receiving unemployment benefit (UB), and the rest.[15] Table 2 shows that the transitions to permanent contracts with the same employers (PS)–which we label as the stepping-stone transitions–increase with the length of the contract sequences. We can also clearly see that the sequences ending after the reform are significantly more likely to result in these stepping-stone transitions than the sequences ending before the reform. Notably, we do not see an increase in the post-reform transitions to temporary contracts with different employers or unemployment, which suggests that employers did not respond to the reform by becoming more selective in their screening process. Altogether, the evidence in Table 2 provides preliminary support to the claim that the reform strengthened the stepping-stone effect of temporary contracts.

[13] Farber (1999) shows that workers who previously lost a job and found new employment in temporary jobs are more likely to be working full-time, while people who have entered temporary work through other pathways are more likely to (choose to) work part-time.

[14] Note, however, that some chains may consist of multiple unobserved temporary contracts. Additionally, the chains for which we observe multiple temporary contracts are split into finer contract sequences, which is reflected in the presented statistics.

[15] The rest includes receiving assistance allowance, social allowance benefit, sickness benefit or pension payment, becoming a student, joining a board of trustees or engaging in other forms of self-employment. Note that we are unable to identify the chains that involve employers being succeeded by other employers, which means that the corresponding labor market transitions are coded as transitions to different employers.

5. Empirical Strategy

The goal of our study is to leverage the July 2015 discontinuity to study the effects of the chain-rule reform on the labor market transitions of temporary workers, focusing primarily on their *PS* transitions (that is, the transitions to permanent contracts with the same employers). Figure 5 presents descriptive evidence of these effects. We split our analytical dataset into six groups based on the lengths of the given contract sequences, and we evaluate whether the sequences ending around the discontinuity resulted in a *PS* transition. The vertical axis represents the shares of employees who transitioned to a permanent contract with the same employer (following the end of the given contract sequence), and the horizontal axis denotes the date when the given sequence ended. As the figure shows, for employees whose sequences were longer than one year but shorter than two years (Figures 5c and 5d), the share of *PS* transitions increased visibly after the reform. For other employees we do not observe a clear reform effect.[16]

According to the transitional law, if the temporary contract stops after July 1, 2015, the new regulation applies. If the temporary contract stops before July 1, 2015, the old regulation applies. Therefore, when analyzing the effect of the reform, we treat the first group of contracts as the treatment group and the second group as the control group. The assumption that is implicit to this RD design, is that the employers did not manipulate the ending dates of the temporary contracts in response to the reform. In this case, the implementation date is exogenous to the ending date of the temporary contract in the sample and then there is a sharp discontinuity in treatment at the cutoff date. In Section 9, we evaluate the validity of this assumption by checking the distribution of contract ending dates, and the smoothness of worker characteristics among contracts ending before and after the reform.

We implement an RD design with the calendar time as the running variable and July 1, 2015 as the discontinuity threshold (See a review of RD design in Lee and Lemieux (2010) and regression discontinuity in time (RDiT) in Hausman and Rapson (2018)). Note that since the new chain rule only applies to the temporary contracts signed after July 1, 2015, there is no effect of the reform on the temporary contracts that are signed before this date, even if they terminate after the threshold. Therefore, for the sequences in which the last temporary contracts were signed before July 1, 2015 and stopped just before or after the discontinuity date, their accumulated lengths should be exogenous to the reform. When two sequences have the same accumulated length, we can treat the one stopping just before the discontinuity date as the control group (because it can be followed by another contract subject to the old chain rule), and the one stopping just after the discontinuity date as the treatment group (because it can be followed only by a contract subject to the new chain rule). Thus, our RD design can be leveraged to estimate the reform effect on

[16] There is a January effect in the data: the share of workers signing permanent contracts with the same employer is much higher in January than it is in the other months of the year. We control for the January effect by including calendar month dummies in the empirical analysis, and we also check the sensitivity of our results by estmating a model with a sample that excludes the temporary contracts ending in January.

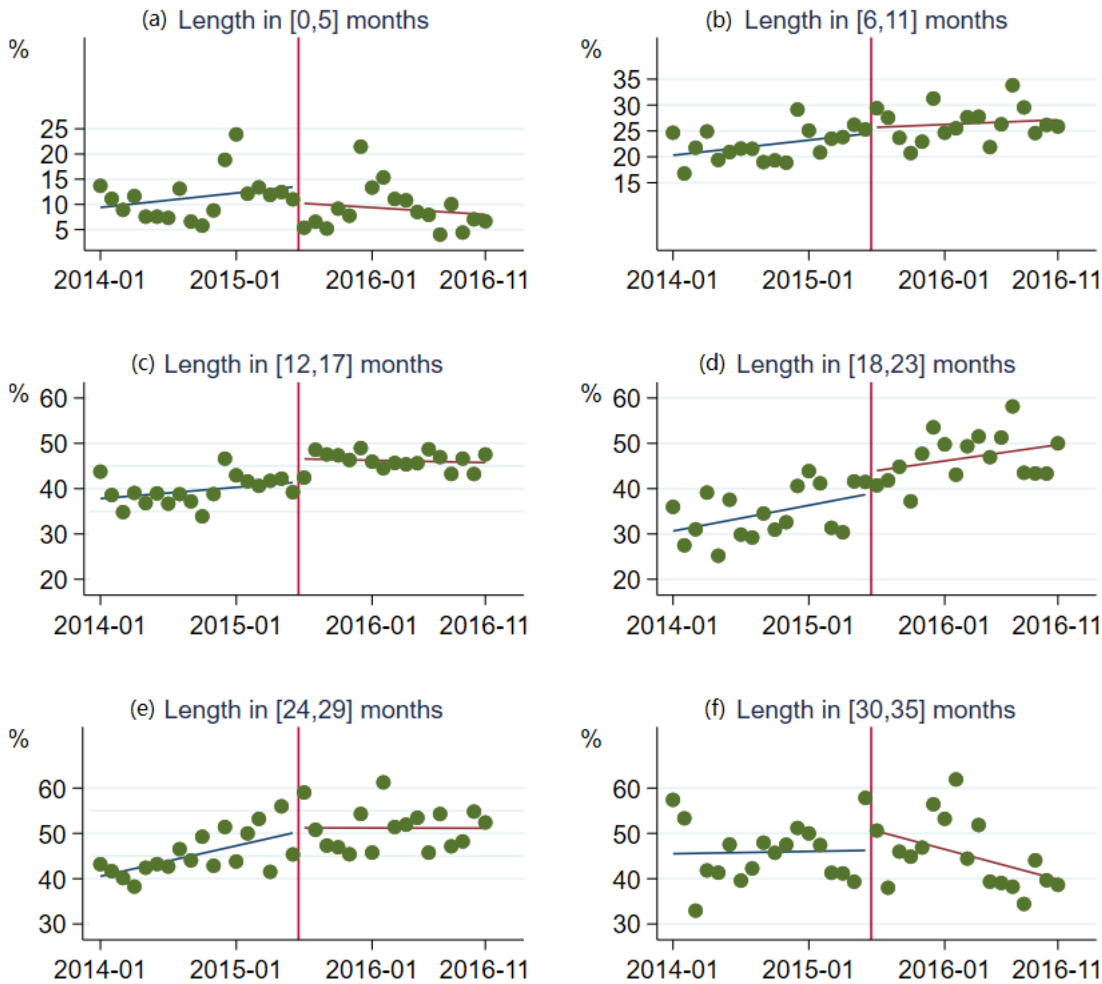

Figure 5: Discontinuities in Stepping-Stone Transitions Surrounding the Reform

Note: These figures show the percentages of employees who renewed permanent contracts with the same employers after their current temporary contracts terminated, conditional on the date of temporary contract termination. They are categorized into six groups based on the different lengths of the chains the employees have accumulated when their current temporary contracts terminated. The vertical red line represents July 2015, the month when the reform took effect.

the next labor market transition.

The RD design uses a uniform kernel with a bandwidth of 1.5 year on each side of the reform date: using the sequences within which the last temporary contract ends in between January 2014 and June 2015 as the data before the reform and the sequences within which the last temporary contract ends in between July 2015 and November 2016 as the data after the reform. We also conduct sensitivity analyses using alternative bandwidths.

The model is specified as follows. A sequence of temporary contracts is signed by the employee i with the company j. At time t, the last temporary contract in the chain terminates and employee moves into one of the following states S_{ijt}: *TD*, *TS*, *PD*, *PS*, *EN* and *UB*, as defined in Table 2. Let $p_{ijt}^{(PS)}$ and $p_{ijt}^{(PD)}$ denote the probability that the employee observed at time t moves into PS or PD, respectively, conditional on a set of control variables $\mathbf{X}_{ijt}$, i.e.,

$$p_{ijt}^{(PS)} = \Pr(S_{ijt} = PS|\mathbf{X}_{ijt}).$$

and

$$p_{jit}^{(PD)} = \Pr(S_{ijt} = PD|\mathbf{X}_{ijt}).$$

Let $y_{ijt}^{(PS)}$ and $y_{ijt}^{(PD)}$ denote the dummy variables that are equal to 1 if the employee observed at time t transitions into *PS* or *PD*, respectively. The effect of the reform on $p_{ijt}^{(*)}$ can be estimated through the following RD design:

$$\begin{aligned} y_{ijt}^{(*)} =& \beta_0 + \beta_1 D_{ijt}^{(12)} + \beta_2 D_{ijt}^{(24)} + \alpha_1 D_{ijt}^{(p)} + \alpha_2 D_{ijt}^{(12)} \times D_{ijt}^{(p)} + \alpha_3 D_{ijt}^{(24)} \times D_{ijt}^{(p)} \\ &+ f(T_{ijt} - c) + D_{ijt}^{(p)} \times g(T_{ijt} - c) + \gamma \mathbf{X}_{ijt} + \nu_j + \varepsilon_{ijt} \end{aligned} \tag{1}$$

where $D_{ijt}^{(12)}$ is the dummy variable that is equal to 1 if the length of the contract sequence is between 12 and 23 months and 0 otherwise, and $D_{ijt}^{(24)}$ is the dummy variable that is equal to 1 if the length of the contract sequence is between 24 and 35 months and 0 otherwise. The dummy variable for the reform, $D_{ijt}^{(p)}$, takes on the value 1 if the last temporary contract in the given sequence ended after the reform and 0 otherwise. Accordingly, the parameter α_1 estimates the baseline effect of the reform on the transition rates of contracts with sequence lengths ranging from 1 to 11 months, whereas $\alpha_1 + \alpha_2$, and $\alpha_1 + \alpha_3$ quantify the effects corresponding to contracts with sequence lengths ranging from 12 to 23 months, and 24 and 35 months, respectively. The threshold c denotes the month of July 2015, which is the date when the new chain rule took effect. T_{ijt} is the running variable in our RD design, denoting the calendar date that corresponds to the end of the last temporary contract in the given sequence. The functions $f(\cdot)$ and $g(\cdot)$ are polynomials of our decentralized running variable $(T_{ijt} - c)$. Our principal specification uses linear polynomials. Model specifications with higher-order polynomials are estimated as a robustness check.

The control variables $\mathbf{X}_{ijt}$ include a set of characteristics corresponding to the temporary contracts and the workers who hold them. These characteristics include average monthly salary (in four salary bands), the number of contract interruptions[17], sectoral dummies (classified into eight groups: culture, financial & economic, industrial, IT, government, transport, health care and construction), and worker characteristics (age, gender, immigration status and education level). In addition, we also control for the long-run trends and seasonality by including yearly dummies and monthly dummies, and for firm-specific fixed effects ν_j. The idiosyncratic effects ε_{ijt} are unobserved. Since some individuals may have multiple chains and there is also dependence across the renewed temporary contracts within a given chain (that is split into a series of sequences), we cluster standard errors at the worker level to account for unobserved individual effects.

6. Main Results

Table 3 lists the parametric coefficient estimates corresponding to our principal model of transitions to permanent contracts with the same or a different employer (equation 1). The models in Columns 1 and 4 operate with a parsimonious set of covariates, which includes the dummy variables for the chains' length and the reform, and also the first-order polynomials of the decentralized running variable. The models in Columns 2 and 5 expand the set of covariates by the control variables $\mathbf{X}_{ijt}$, and the models in Columns 3 and 6 also control for firm-level fixed effects.

As shown in the first three columns, the estimates of α_1 (i.e., the reform effect for the shortest contract chains) are all negative and only significant in the specification 2. The estimates of α_2 are all positive with similar magnitude and are all significant at the 1% level. The F-tests of $\alpha_1+\alpha_2=0$ are all rejected with 1% significance level, indicating that the reform had a significant effect on the converting probabilities of the contract chains whose lengths are between 12 and 23 months. The estimates of α_3 are all positive, and they attain statistical significance when we add control variables and the firm-fixed effect. However, the F-tests of $\alpha_1+\alpha_3=0$ cannot be rejected, meaning that the policy effect on the converting probabilities of the contract chains whose lengths are between 24 and 35 months could be ambiguous.

The coefficient estimates corresponding to our preferred specification in Column 3 show that, for employees with contract chains shorter than 12 months, the reform had no significant effect on the probability of *PS* transitions. For employees with contract chains between 12 months and 23 months, the reform significantly increased the transitioning probability by $\alpha_1+\alpha_2=4.2$ percentage points (10.9%). For employees with longer contract chains the reform increased the probability by $\alpha_1+\alpha_3$ $=3.17$ percentage points (6.86%), however the corresponding F-test indicates that this effect is borderline statistically insignificant.

The differences in the presented reform effects can be explained as follows. The employees whose contract chains are shorter than one year do not immediately benefit from the chain-rule reform, because their contracts can be followed by equivalent contracts without the corresponding chain crossing the new chain rule threshold. The employees whose chains are longer than one year but shorter than two years benefit more, because their renewal contracts are likely to cross the new threshold

[17] According to Gagliarducci (2005), the probability of moving from temporary contract to a permanent contract decreases if the temporary work is subject to interruptions.

(unless the employer decides to offer them very short renewal contracts, which are uncommon). The employees whose chains are longer than two years benefit less, because many of their renewal contracts would cross the threshold regardless of the reform. At the same time, some benefits remain because the reform eliminated the employers' option to follow these contracts by very short temporary contracts instead of permanent contracts.

The empirical evidence in Table 3 shows that employers respond to the reform by expediting the transitions to permanent contracts. This suggests that, apart from functioning as a "buffer stock" for economic fluctuations, the temporary contracts also function as a device for screening employees' abilities. In addition, the responses to the reform suggest that employers are generally able to screen their employees in less than two years. This is in line with Faccini (2014) who argues that, while it takes employers (on average) more than seven months to learn about the quality of the employer-employee match, the probability that the quality of a match is unknown after two years is only about 3.5%.

Moving to the other control variables, Column 3 shows that before the reform, for an employee who has accumulated a chain of temporary contracts with a length between 12 and 23 months, the probability of transitioning to a permanent contract with the same employer was 15.8 percentage points larger than the probability for an employee who has accumulated a chain with length less than 12 months. For an employee who has accumulated a chain of temporary contracts with a length between 24 and 35 months, the probability of transitioning to a permanent contract with the same employer is 26.0 percentage points larger than the probability for an employee who has accumulated a chain with length less than 12 months. Both effects are significant at 1% level.

Column 2 of Table 3 shows that the number of temporary contract renewals decreases the probability of transitioning to a permanent contract by 6.13 percentage points. However, this effect become insignificant in the column (3) when we account for firm-level fixed effects. Gagliarducci (2005) argues that the probability of transitioning from a temporary to a permanent contract increases with the duration of the contract but decreases with the number of contract renewals. One interpretation is that the employees can accumulate firm-specific human capital during the job, so the more time they spend in the firm, the more likely they will be offered a permanent contract. Similarly, contract renewals may signal that the firm was reluctant to sign a permanent contract with the given employee, resorting to a sequence of temporary contracts instead. Another interpretation of these findings is that firms that offer shorter temporary contracts with interruptions may be intrinsically different from firms that offer longer and continuous temporary contracts. The former group may be subject to a higher worker turnover and lower promotion prospects than the latter group, which would also explain these findings. Based on the findings presented in Columns 2 and 3, we conclude that the data is more aligned with the interpretation operating with firm-level differences in turnover and promotion rates.

In terms of worker characteristics, Column 2 shows that women are subject to lower transition probabilities than men, however this difference becomes statistically insignificant in Column 3. Being an immigrant decreases the transition probability by 2.90 percentage points at 1% significance level.

Besides investigating the transitions to permanent contracts with the same employer (*PS*) we are also interested in determining whether the reform influenced transitions to permanent contracts with different employers (*PD*). We are not expecting significant reform effects on the PD transitions, because the chain rule only applies to contract renewals with the same employer. However, there is a possibility that the effects on *PS* transitions are actually attributable to unrelated confounds, such as improvements of labor market conditions that are coinciding with the reform. In such case, we would observe that the transition rates to both *PS* and *PD* respond positively to the reform. As shown in Column 6, the reform effect estimates on the *PD* transitions are all insignificant, which supports the causal interpretation of reform effects on the *PS* transitions. Note that the probability of *PD* transitions decreases with the duration of the contract chain but increases with the number of contract renewal, which is a dynamic that is opposite to the one observed among *PS* transitions.

In addition to the models of *PD* and *PD* transitions, we also estimate the models quantifying the transition probabilities to the other states, including the temporary contracts renewals with the same employer (*TS*), temporary contracts with a different employer (*TD*), unemployment benefit receipt (*UB*), and becoming an entrepreneur (*EN*). The estimation results are shown in Table 4.

The reform effects corresponding to temporary contract renewals are negative and they are marginally statistically insignificant. The reform effects corresponding to transitions to temporary contracts with different employers and to entrepreneurship are also negative, although they are estimated with lesser precision. The reform effects corresponding to transitions to unemployment benefit receipt are negative and statistically significant at the 5% level. This implies that, while the chain rule restrictions may have reduced the probability of transitions to other active labor market states, the primary effect compensating the higher rates of *PS* transitions was the reduction in transitions to unemployment. One possible explanation of this finding is that the higher rates of transitions to permanent positions translated into more job opportunities for the unemployed, because companies needed to hire replacement temporary workers in order to maintain their "buffer stocks".

7. Heterogeneous Effects

The reform may have had heterogeneous impacts on the young and old workers. To test this, we add age, age squared, and their interactions with the reform dummy to our principal model of *PS* transitions. However, as shown in Column 1 of Table A1[18], the coefficients on these interaction terms do not prove statistically significant. In Column 2 we use a more refined set of age controls, adding dummies for age groups and their interactions with the reform dummy. Also in this case, the interaction terms do not prove statistically significant. In Column 3, we expand the set of age covariates further by adding interaction terms with the dummies for different contract lengths. This specification yields significant interaction terms, with the reform-effect estimates being more pronounced among younger workers with con-

[18] All tables and figures starting with A are included in the Online Appendix.

tracts longer than 11 months. One interpretation of this heterogeneity is that older temporary workers have relatively high labour costs and risks, which makes them unlikely to be offered a permanent job. This means that the stepping-stone function is more important for younger workers, which makes them the main benefactors of the chain-rule reform. Another possibility is that the screening of older job candidates is likely to be much faster and less dependent on their performance during the probationary period (older workers can support their cases by past experience and references), which means that the employers may be incentivized to make desirable employees permanent well ahead of them reaching the chain rule threshold.

The new chain rule may have also had heterogeneous impacts on employees with different levels of education and employees in different sectors. To explore these possibilities, we estimate the corresponding interaction models (Tables A2 and A3). Similar to workers' age, heterogeneous effects with respect to workers' education are only found when we add interaction terms with different contract lengths. The reform effects only prove heterogeneous among contract chains longer than 2 years. Among this group, the reform had a positive effect on employees with low and medium levels of education, but did not change the transition probabilities of highly-educated employees (see F-test in Table A2). One possible explanation is that the highly-educated employees who are well-matched with their employers would be offered permanent contracts earlier (with their education acting as an additional screening device), whereas those who remained on temporary contracts past their 2nd year are unlikely to be well-matched, and therefore are unlikely to benefit from the reform.

With regard to sector of employment, we also observe sizable heterogeneity. For contract chains of 12-23 months, we observe strong positive reform effects in IT and in Culture, whereas for longer contract chains we observe strong positive reform effects in healthcare. While these patterns are interesting, we remain cautious with their interpretation, because the numbers of observations identifying many of these sectoral effects are rather modest.

8. Robustness Checks

In this section, we present a number of validation exercises and robustness checks. To validate our RD design, we first test for discontinuities in the characteristics of workers whose contracts ended before and after the cutoff date. Figures A1, A2, and A3 (in the Online Appendix) present results of these exercises, showing that the characteristics are smooth across the cutoff date. This means that we find no evidence of selection into the control and treated groups, which supports the validity of our RD design.

Next, we evaluate the sensitivity of our results to the choice of the RD bandwidth and kernel. Table A4 presents the coefficient estimates corresponding to the models of PS-transitions with alternate kernels and bandwidths. Column 1 presents the results corresponding to a model using the triangular kernel with the same 18-month bandwidth as our principal specification. Columns 2-5 present the results corresponding to a model using the same kernel as our principal specification (uniform) with different bandwidths (12, 6, 5, 4, and 3 months). The resulting effect estimates lie in the 95% confidence interval of our principal effect estimates (Column 3 of Table 3), which supports the robustness of our results.

Table A5 explores the sensitivity of our results to the choice of running-variable polynomials, $f(\cdot)$ and $g(\cdot)$, presenting the results of models with linear, quadratic, and cubic polynomials. Again, the results are comparable to our principal model specification (which is listed in Column 1). The estimates of α_2 or α_3 are all significant and have similar magnitude across the three specifications. The F-test on $\alpha_1 + \alpha_2 = 0$ cannot be rejected when a second-order or a third-order polynomial is assumed, however this is likely attributable to lower precision of the models with higher-order polynomials (note that none of the coefficients on the second-order or the third-order terms in the polynomials proves statistically significant). For that reason, we prefer to rely on our principal specification with linear polynomials.

Table A6 presents two placebo tests in which we manipulate the timing of the reform. The models used for these tests mimic the model presented in Column 4 of Table A4, using a shorter bandwidth of 5 months. The placebo tests are important, because there is a strong seasonal effect influencing the probability of moving from temporary contracts to permanent contracts. Therefore, we have to make sure that the changes observed in July 2015 are attributable to the reform and not to the seasonality. In Columns 1 and 2, we present estimates corresponding to the model that evaluates a placebo reform taking place one year earlier, in July 2014. This model uses the data corresponding to the temporary contracts ending in between February and November of 2014. In Columns 3 and 4, we do the same for a placebo reform taking place in July 2016 . Both the point estimates and F-tests show that there is no significant effect of the placebo reform on the probabilities of renewing a permanent contract with the same employer. Therefore, it can be concluded that the reform effects are not attributable to seasonality.

Note that our sample is obtained by merging the administrative data with the labour force survey. To test whether our findings are representative of the broader population, we also estimated the model 1 with a larger dataset consisting solely of administrative data (excluding controls for worker characteristics). Table A7 presents the corresponding coefficient estimates. In Columns 1-3, the estimates of α_1, α_2 and α_3 show the same sign and similar magnitudes compared with the estimates in Table 3. In Columns 4-6, the estimates of α_2 and α_3 become statistically significant but are still not economically significant. Since we cannot screen on people's age and education level by the main administrative data, the estimates could be somewhat biased by differences in these observable characteristics.

As mentioned in the Section 3.2, the chain rule applies to temporary employees who work at least 12 hours per week, and so we also check the estimation results when part-time temporary contracts with at-least 12 working hours per week are included in the sample. As shown in the Table A8, the main estimation results are similar to their principal specification. For the employees who have accumulated contract chains of 12-23 months, the reform increased the probability of PS transitions by 3.7 p.p. (9.67%) with 1% significance level. For the employees who have accumulated contract chains of 24-35 months, the probability was raised by 3.6 p.p. (7.79%).

In the last robustness check, we explore the premise that worker's salary may be potentially endogenous to contract duration. To rule out confounding effects stemming from this endogeneity, we run the main regressions without controlling for salary. The coefficients of the key parameters of interests are not significantly impacted by this change (see Table A9).

9. Anticipation Effects

In this section we investigate potential anticipation effects which could be confounding our analysis. First, we assess whether the employers have manipulated contract dates in anticipation of the reform, looking for changes in the density of contracts starting and ending in the vicinity of July 1, 2015.

Figure A4 shows the total number of temporary contracts signed with a new employer in each month in the sample. The vertical red line distinguishes the contracts signed before and after the reform. The figure shows that the incidence of new contracts is seasonal, being usually highest in January and lowest in December. We can also see that the monthly incidence of temporary contracts starting between July and November of 2015 is similar to the those starting between February to June of 2015, and we do not observe clear evidence of the two patterns deviating from the patterns observed in 2014 and 2016. Similar conclusions can be made about Figure A5 which shows the total number of temporary contracts ending in each month in the sample. We can't observe any sharp changes around the policy cutoff, which bolsters the evidence in support of the claim that employers did not manipulate the contract ending and start dates prior to or past the policy date. Figure A6 presents the monthly percentage of temporary contracts signed by workers who were previously receiving unemployed benefits, which enables to see whether the reform discouraged hiring new workers (due to the higher labor costs imposed on firms).

The hiring effects are further explored in the following linear regression model of workers' transitions from unemployment to temporary contracts with new employers.

$$y_{ijt}^{(TC)} = \beta_0 + \alpha_1 D_{ijt}^{(p)} + f(T_{ijt} - c) + D_{ijt}^{(p)} \times g(T_{ijt} - c) + \gamma \mathbf{X}_{ijt} + \nu_j + \varepsilon_{ijt} \tag{2}$$

where $y_{ijt}^{(TC)}$ is the dummy variable that is equal to 1 when the employee signs the first temporary contract after receiving unemployed benefits, 0 otherwise.

The positive and significant estimates in Table A10 reject the hypothesis that the reform discouraged hiring. In fact, perhaps reflecting employers' need for additional temporary workers, the reform led to more workers being hired.

In the previous section, we only focused on the effect of the reform on the probability of moving to the permanent contract after a temporary contract terminates. But the reform could have also affected the durations of temporary renewed contracts. For instance, employers anticipating the new chain rule could have offered their workers a longer temporary contract ending shortly before July 1, 2015 to utilize the old rule as much as possible. In addition, the employees crossing the July 1, 2015 cutoff date could have been offered much shorter renewal contracts in order to prevent them from reaching the lower duration threshold.

We test these hypotheses by checking the differences in the empirical cumulative distribution functions (CDFs) of the length of the renewed contract offered by the same employer before and after the cutoff date. Figure A7 plots the CDFs conditional on the lengths of the chains already accumulated by the workers. The blue lines denote the CDFs of contracts ending before the cutoff date, and orange lines denote the CDFs of contracts ending after the cutoff date. The dashed lines indicate that the sum of the renewed contract's length and the previous chain's length already exceed the length requirement for the chain rule to come into effect (36 months before the reform and 24 months after the reform). Note that if the renewed contract is a permanent one, its length is treated as infinity, so the line of the CDF converges to the fraction of all the renewed contracts as a temporary one. The flattened dashed lines indicate that only a small fraction of the renewed contracts are recorded as temporary ones even when their lengths actually trigger the effectiveness of the chain rule. The Kolmogorov-Smirnov test is used to check the equality of the length distributions of the renewed temporary contracts before and after the reform. As shown in Table A11, we find no significant difference in the length distributions of the renewed temporary contracts before and after the reform.

We further investigate the policy impact on the lengths of the renewed contracts offered by the employers via a RD setting. The effect of policy reform on L_{ijt} can be estimated through the following RD design:

$$\begin{aligned} L_{ijt} =& \beta_0 + \beta_1 D_{ijt}^{(12)} + \beta_2 D_{ijt}^{(24)} + \alpha_1 D_{ijt}^{(p)} + \alpha_2 D_{ijt}^{(12)} \times D_{ijt}^{(p)} + \alpha_3 D_{ijt}^{(24)} \times D_{ijt}^{(p)} \\ &+ f(T_{ijt} - c) + D_{ijt}^{(p)} \times g(T_{ijt} - c) + \gamma \mathbf{X}_{ijt} + \nu_j + \varepsilon_{ijt} \end{aligned} \tag{3}$$

where L_{ijt} is the length of the renewed contract.

The hypothesis that the employers are incentivized to offer longer renewed temporary contract ending before the reform and/or much shorter ones ending after the reform is not supported by the data (Table A12). However we acknowledge that the partial observability of contract renewals is a limitation of this exercise.

10. Conclusion

This paper studies the 2015 reform of temporary employment contract legislation in the Netherlands. The reform involved a change of the "chain rule", which is a regulation mandating that workers engaged in multiple temporary contracts with the same employers will be automatically recognized as permanent employees once their sequence of temporary contracts (known as the contract chain) exceeds a prespecified duration threshold. This threshold was lowered from 3 years to 2 years, effectively shortening the time during which the employers could dismiss these workers without incurring meaningful financial costs.

The reform enables us to explore the stepping-stone function of temporary contracts, wherein the temporary contracts act as de-facto probationary periods for permanent positions within the same firm. The effects of the chain-rule reform on this function are theoretically ambiguous. On one hand, the reform could weaken the stepping-stone function (if it hinders the employers' capacity to screen job candidates), but it could also accelerate the stepping stone function and thereby improve workers' conditions.

Using a regression discontinuity (RD) design with a sharp policy cut-off, we show that the reform accelerated the stepping-stone function of temporary contracts. The positive effects were concentrated among workers who have accumulated contract chains of one to two years, with the probability of transitioning to a permanent contract (with the same employer) increasing by 4.2 percentage points (10.9%) at the end of their contracts. We have also found tentative evidence of smaller, yet positive effects on workers with contract chains longer than two years, although this effect proved marginally statistically insignificant.

The reform effects prove to be heterogeneous with regard to worker characteristics. We show that the effects are more pronounced among younger workers, and workers with low and medium levels of education. We do not find evidence of employers strategically manipulating the contract starting and ending dates, which bolsters the credibility of our empirical design. We also do not find evidence of discontinuities in the worker characteristics associated with temporary contracts surrounding the reform. The positive effects on transitions to permanent contracts with the same employers are primarily compensated by negative effects of the reform on transitions to unemployment. This indicates that the firms increased their hiring in response to the reform, possibly in an effort to replenish their "buffer stocks" of temporary workers. This narrative is also supported by the results of a standalone RD analysis of transitions from unemployment to temporary contracts with new employers, which confirms that the reform stimulated hiring of new workers.

It is worth emphasizing that our analysis focuses on the short-run effects of the reform, leveraging policy discontinuities applicable to contracts that surrounded the reform implementation. The long-run effects of the reform on firms and temporary workers may deviate from our findings, reflecting the broader and more gradual workforce adjustments to the new regime, changes to the pool of temporary workers, and other hiring and firing decisions within firms. These effects, as well as the effects on other outcomes of workers directly affected by the reform, constitute a promising avenue for future research. From the modeling perspective, studies using discrete-time competing hazards model of worker transitions also hold great promise and can lead to further insights into the effects of the chain-rule reform.

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Declarations of Interest

None.

Appendix A. Method for Constructing the Chains

In our sample, suppose that for the individual employee i, we have the data on his/her history of non-overlapping temporary contracts, denoted by a series of TC_{ij}, where $j \in \{1, 2, \ldots, J_i\}$. For each TC_{ij}, we have the information on its ending month, $t(TC_{ij})$, total length, $L(TC_{ij})$, and monthly average income, $MI(TC_{ij})$. We also assume that $t(TC_{ij})$ is increasing in j. Meanwhile, in the middle of any two adjacent temporary contracts, TC_{ij} and $TC_{i,j+1}$, there could be a possible time interval G_{ij}, where $j \in \{1, 2, \ldots, J_i - 1\}$. For each G_{ij}, we also have the information on its total length, $L(G_{ij})$, monthly average income, $MI(G_{ij})$, and ending month $t(G_{ij})$, satisfying $t(TC_{ij}) \leq t(G_{ij}) < t(TC_{i,j+1})$. If there is no interval between TC_{ij} and $TC_{i,j+1}$, we assume $L(G_{ij}) = 0$, $MI(G_{ij}) = 0$ and $t(G_{ij}) = t(TC_{ij})$.

Our sample of the chains of temporary contracts are constructed as follows in 3 steps:

(1) For each month $\tilde{t}$ between January 2013 and December 2016, if the individual i has a TC_{ij} such that $t(TC_{ij}) = \tilde{t}$, then his/her first chain ending at month $\tilde{t}$ is defined as $C^{(1)}_{i\tilde{t}} = \{TC_{ij}\}$, with its total length $L(C^{(1)}_{i\tilde{t}}) = L(TC_{ij})$ and its monthly average income $MI(C^{(1)}_{i\tilde{t}}) = MI(TC_{ij})$.

(2) If $L(G_{i,j-1})$ is smaller than 3 months when $t(G_{i,j-1})$ is before July of 2015, or smaller than 6 months when $t(G_{i,j-1})$ is in or after July of 2015, and if $TC_{i,j-1}$ is signed with the same employer as TC_{ij}, then we can construct his/her second chain ending at month $\tilde{t}$ as $C^{(2)}_{i\tilde{t}} = \{TC_{i,j-1}, G_{i,j-1}, TC_{ij}\}$, with

$$L(C^{(2)}_{i\tilde{t}}) = L(TC_{i,j-1}) + L(G_{i,j-1}) + L(TC_{ij})$$

and

$$MI(C^{(2)}_{i\tilde{t}}) = \frac{MI(TC_{i,j-1}) \cdot L(TC_{i,j-1}) + MI(G_{i,j-1}) \cdot L(G_{i,j-1}) + MI(TC_{ij}) \cdot L(TC_{ij})}{L(C^{(2)}_{i\tilde{t}})}.$$

(3) If $C^{(2)}_{i\tilde{t}}$ can be constructed, we trace back her working history one step further. If $L(G_{i,j-2})$ is smaller than 3 months when $t(G_{i,j-2})$ is before July of 2015, or smaller than 6 months when $t(G_{i,j-2})$ is in or after July of 2015, and if $TC_{i,j-2}$ is signed with the same employer as TC_{ij}, then we can construct his/her third chain ending at month $\tilde{t}$ as $C^{(3)}_{i\tilde{t}} = \{TC_{i,j-2}, G_{i,j-2}, TC_{i,j-1}, G_{i,j-1}, TC_{ij}\}$, with

$$L(C^{(3)}_{i\tilde{t}}) = L(TC_{i,j-2}) + L(G_{i,j-2}) + L(TC_{i,j-1}) + L(G_{i,j-1}) + L(TC_{ij})$$

and

$$MI(C^{(3)}_{i\tilde{t}}) = \frac{\Sigma^{j}_{j'=j-2} MI(TC_{i,j'}) \cdot L(TC_{i,j'}) + \Sigma^{j-1}_{j'=j-2} MI(G_{i,j'}) \cdot L(G_{i,j'})}{L(C^{(3)}_{i\tilde{t}})}.$$

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Table 1: Descriptive Statistics for the Chains of Temporary Contracts and Individuals

	Whole Sample	Before Policy	After Policy
Chains of Temporary Contracts			
Total Number	59,638	26,650	32,988
Number of Temporary Contracts Consisted of (%)			
1	92.30	92.43	92.19
2	6.81	6.65	6.94
3	0.89	0.92	0.87
Length (%)			
1 - 11 months	44.76	44.87	44.66
12 - 23 months	35.28	34.30	36.08
24 - 35 months	16.32	17.88	15.05
Monthly Average Salary (%)			
500-1500	13.00	14.86	11.50
1500-2000	22.98	23.43	22.61
2000-2500	22.80	22.25	23.24
2500-3500	24.51	23.69	25.17
3500-10000	16.72	15.78	17.48
Sectors (%)			
Culture	1.28	1.39	1.20
Finanicial & Economic	23.80	23.82	23.78
Industrial	17.70	18.15	17.34
IT	0.63	0.80	0.50
Governement	0.50	0.49	0.51
Transport	24.18	24.93	23.57
Healthcare	2.48	2.53	2.43
Construction	0.22	0.23	0.22
Unknown	29.21	27.67	30.45
Individuals			
Total Number	47,706	24,167	29,345
Age (%)			
18-24	31.74	30.15	33.40
25-34	29.34	31.21	28.13
35-44	22.06	21.59	22.14
45-54	14.12	14.13	13.87
55+	2.74	2.92	2.46
Male (%)	75.08	75.11	75.90
Immigrant (%)	8.22	8.43	7.85
Education level (%)			
Low	32.09	31.44	32.79
Medium	44.85	44.80	45.10
High	23.05	23.77	22.12

Note: Education is categorized following the criteria of International Standard Classification of Education (ISCED): Low (up to lower secondary education), Medium (between upper secondary and post-secondary non-tertiary), High (short-cycle tertiary education, bachelor, master and doctoral).

Table 2: Average Percentages of Destinations after a Temporary Contract Terminates

	Length of the Accumulated Chains								
Destination (in %)	1 - 11 months			12 - 23 months			24 - 35 months		
	Before	After	p-value	Before	After	p-value	Before	After	p-value
Temporary Contract									
Different Employer (TD)	29.13	29.15	0.96	21.25	19.64	0.00	16.68	16.30	0.60
Same Employer (TS)	8.21	11.02	0.00	4.53	4.41	0.65	3.19	3.25	0.88
Permanent Contract									
Different Employer (PD)	8.90	8.10	0.02	8.70	8.38	0.40	8.83	10.44	0.01
Same Employer (PS)	17.70	21.75	0.00	38.58	48.27	0.00	46.24	51.74	0.00
Entrepreneur (EN)	2.47	2.22	0.19	1.69	1.17	0.00	1.74	1.33	0.09
Unemployed Benefit Receiving (UB)	20.12	15.43	0.00	17.73	11.75	0.00	16.49	10.36	0.00
Rest	13.47	12.33	0.01	7.51	6.38	0.00	6.82	6.59	0.64
Total Number	11,958	14,733		9,621	13,052		5,071	5,203	

Note: The sample before the policy reform contains the chains within which the last temporary contract terminates in the period from Jan 2014 to Jun 2015. The sample after the policy reform contains the chains within which the last temporary contract terminates in the period from Jul 2015 to Nov 2016. The p-values are derived from t-tests on the equality of means.

Table 3: Parametric Estimates in the RD Design for the Transitions to PS and PD

	(1) PS	(2) PS	(3) PS	(4) PD	(5) PD	(6) PD
$\text{Length}_{12\rightarrow 23}$	0.209***	0.170***	0.158***	-0.00196	-0.00784**	-0.00710
	(0.00608)	(0.00602)	(0.00982)	(0.00392)	(0.00399)	(0.00654)
$\text{Length}_{24\rightarrow 35}$	0.286***	0.246***	0.260***	-0.000700	-0.00590	-0.0145*
	(0.00785)	(0.00797)	(0.0132)	(0.00482)	(0.00493)	(0.00775)
Post Reform	-0.00709	-0.0182**	-0.0175	-0.00142	-0.000741	-3.33e-05
	(0.00843)	(0.00889)	(0.0145)	(0.00545)	(0.00597)	(0.00972)
$\text{Length}_{12\rightarrow 23}\times$Post Reform	0.0617***	0.0669***	0.0595***	0.00193	0.00156	-0.00112
	(0.00846)	(0.00819)	(0.0130)	(0.00529)	(0.00528)	(0.00855)
$\text{Length}_{24\rightarrow 35}\times$Post Reform	0.0167	0.0347***	0.0492***	0.0199***	0.0204***	0.0137
	(0.0114)	(0.0113)	(0.0178)	(0.00703)	(0.00705)	(0.0107)
Month	0.00350***	0.00235***	0.00277**	-0.000360	-0.000229	0.000901
	(0.000547)	(0.000790)	(0.00128)	(0.000356)	(0.000514)	(0.000818)
Month×Post Reform	-0.00413***	-0.00148	-0.00431**	0.000240	-0.000118	-0.000756
	(0.000785)	(0.00129)	(0.00207)	(0.000499)	(0.000815)	(0.00130)
# Interruptions		-0.0613***	-0.00900		0.00587	0.0160***
		(0.00593)	(0.00913)		(0.00415)	(0.00606)
Female		-0.0141***	-0.0114		-0.00728**	-0.00224
		(0.00451)	(0.00768)		(0.00292)	(0.00481)
Immigrant		-0.0243***	-0.0290***		-0.0117***	-0.0112
		(0.00667)	(0.0111)		(0.00418)	(0.00685)
Constant	0.209***	0.199***	0.145***	0.0857***	0.0548***	0.0274*
	(0.00626)	(0.0134)	(0.0221)	(0.00408)	(0.00911)	(0.0144)
Control for						
Education	No	Yes	Yes	No	Yes	Yes
Age	No	Yes	Yes	No	Yes	Yes
Sector	No	Yes	Yes	No	Yes	Yes
Monthly Average Salary	No	Yes	Yes	No	Yes	Yes
Monthly Dummy	No	Yes	Yes	No	Yes	Yes
Yearly Dummy	No	Yes	Yes	No	Yes	Yes
Firm Fixed Effect	No	No	Yes	No	No	Yes
N	55,021	55,021	55,021	55,021	55,021	55,021
F-test						
$\alpha_1+\alpha_2=0$	32	23.28	7.121	0.00763	0.0166	0.0138
p-value	1.55e-08	1.40e-06	0.00762	0.930	0.898	0.907
$\alpha_1+\alpha_3=0$	0.620	1.693	2.655	6.278	6.350	1.402
p-value	0.431	0.193	0.103	0.0122	0.0117	0.236

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table 4: Parametric Estimates in the RD Design for the Transitions to TS, TD, UB and EN

	(1) TS	(2) TD	(3) UB	(4) EN
$\text{Length}_{12\to 23}$	-0.0347***	-0.0747***	-0.00595	0.000277
	(0.00615)	(0.0101)	(0.00878)	(0.00294)
$\text{Length}_{24\to 35}$	-0.0520***	-0.124***	-0.0174	0.000822
	(0.00694)	(0.0120)	(0.0110)	(0.00380)
Post Reform	0.00282	0.00556	0.00354	0.00450
	(0.00898)	(0.0157)	(0.0133)	(0.00453)
$\text{Length}_{12\to 23}\times$Post Reform	-0.0142*	-0.0116	-0.0259**	-0.00625
	(0.00805)	(0.0135)	(0.0114)	(0.00389)
$\text{Length}_{24\to 35}\times$Post Reform	-0.0146	-0.0223	-0.0332**	-0.00835*
	(0.00942)	(0.0166)	(0.0145)	(0.00503)
Month	-0.000368	-0.000641	-0.00237**	2.00e-05
	(0.000726)	(0.00126)	(0.00114)	(0.000389)
Month$\times$Post Reform	-0.000990	0.00392*	0.00209	-0.000266
	(0.00127)	(0.00204)	(0.00181)	(0.000612)
# Interruptions	-0.0952***	0.0535***	0.0180**	0.00427
	(0.00851)	(0.00951)	(0.00814)	(0.00304)
Female	-0.00132	-0.00388	0.00677	-0.00324
	(0.00430)	(0.00772)	(0.00672)	(0.00221)
Immigrant	0.00127	-0.00763	0.0350***	0.00536
	(0.00626)	(0.0114)	(0.0105)	(0.00377)
Constant	0.353***	0.192***	0.0995***	0.00475
	(0.0162)	(0.0231)	(0.0199)	(0.00649)
Control for				
Education	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Sector	Yes	Yes	Yes	Yes
Monthly Average Salary	Yes	Yes	Yes	Yes
Monthly Dummy	Yes	Yes	Yes	Yes
Yearly Dummy	Yes	Yes	Yes	Yes
Firm Fixed Effect	Yes	Yes	Yes	Yes
N	55,132	55,132	55,132	55,132
F-test				
$\alpha_1 + \alpha_2 = 0$	1.781	0.157	2.927	0.159
p-value	0.182	0.692	0.0871	0.690
$\alpha_1 + \alpha_3 = 0$	1.632	0.914	3.673	0.562
p-value	0.201	0.339	0.0553	0.453

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Appendix A. Online Appendix (Kabátek, Liang and Zheng (2022))

Appendix A.1. Heterogeneous Effect

Table A1: Table for Age Effects

	(1) PS	(2) PS	(3) PS
Post Reform	0.00831	-0.0327	0.0132
	(0.0637)	(0.0328)	(0.0367)
$\text{Length}_{12\rightarrow 23}\times$Post Reform	0.0595***	0.0596***	-0.0298
	(0.0130)	(0.0130)	(0.0513)
$\text{Length}_{24\rightarrow 35}\times$Post Reform	0.0493***	0.0493***	-0.0532
	(0.0179)	(0.0179)	(0.0663)
Age	-0.00625**		
	(0.00292)		
Age×Age	3.45e-05		
	(3.98e-05)		
Age×Post Reform	-0.00155		
	(0.00378)		
Age×Age×Post Reform	2.04e-05		
	(5.23e-05)		
Age(25-34)		-0.0389***	-0.0391***
		(0.0112)	(0.0112)
Age(35-44)		-0.0744***	-0.0751***
		(0.0129)	(0.0129)
Age(45-54)		-0.104***	-0.104***
		(0.0144)	(0.0144)
Age(55+)		-0.126***	-0.127***
		(0.0245)	(0.0245)
Age(18-24)×Post Reform		0.0182	-0.0437
		(0.0323)	(0.0370)
Age(25-34)×Post Reform		0.0155	-0.0301
		(0.0325)	(0.0374)
Age(35-44)×Post Reform		0.00440	-0.0340
		(0.0330)	(0.0381)
Age(45-54)×Post Reform		0.0259	-0.00250
		(0.0339)	(0.0388)
Age(18-24)×Post Reform×$\text{Length}_{12\rightarrow 23}$			0.118**
			(0.0526)
Age(25-34)×Post Reform×$\text{Length}_{12\rightarrow 23}$			0.0823
			(0.0531)
Age(35-44)×Post Reform×$\text{Length}_{12\rightarrow 23}$			0.0740
			(0.0538)
Age(45-54)×Post Reform×$\text{Length}_{12\rightarrow 23}$			0.0692
			(0.0554)
Age(18-24)×Post Reform×$\text{Length}_{24\rightarrow 35}$			0.123*
			(0.0684)
Age(25-34)×Post Reform×$\text{Length}_{24\rightarrow 35}$			0.117*
			(0.0691)
Age(35-44)×Post Reform×$\text{Length}_{24\rightarrow 35}$			0.0970
			(0.0702)
Age(45-54)×Post Reform×$\text{Length}_{24\rightarrow 35}$			0.0477
			(0.0720)
Other Controls	Yes	Yes	Yes
N	55,021	55,021	55,021

Table A2: Table for Educational Effects

	(1) PS	(2) PS	(3) PS
Post Reform	-0.0177	-0.0129	-0.0232
	(0.0144)	(0.0163)	(0.0170)
$\text{Length}_{12\to23}\times$Post Reform	0.0592***	0.0598***	0.0755***
	(0.0130)	(0.0130)	(0.0182)
$\text{Length}_{24\to35}\times$Post Reform ($\gamma_1$)	0.0503***	0.0507***	0.0766***
	(0.0178)	(0.0178)	(0.0246)
Edu (Medium)	0.00204	0.00479	0.00469
	(0.00711)	(0.00983)	(0.00983)
Edu (High)	-0.0221**	-0.0163	-0.0165
	(0.00986)	(0.0126)	(0.0126)
Edu (Medium)$\times$Post Reform		-0.00529	0.00688
		(0.0132)	(0.0157)
Edu (High)$\times$Post Reform		-0.0116	0.0115
		(0.0159)	(0.0195)
Edu (Medium)$\times$Post Reform$\times\text{Length}_{12\to23}$			-0.0221
			(0.0202)
Edu (High)$\times$Post Reform$\times\text{Length}_{12\to23}$			-0.0267
			(0.0242)
Edu (Medium)$\times$Post Reform$\times\text{Length}_{24\to35}$			-0.0201
			(0.0273)
Edu (High)$\times$Post Reform$\times\text{Length}_{24\to35}$($\gamma_3$)			-0.0718**
			(0.0340)
Other Controls	Yes	Yes	Yes
N	55,132	55,132	55,132
F-test			
$\gamma_1 + \gamma_3 = 0$			3.557
p-value			0.0593

Table A3: Table for Heterogeneity in Sectors

	(1) PS	(2) PS	(3) PS
Post Reform	-0.0176	-0.0392	-0.0799**
	(0.0112)	(0.0315)	(0.0335)
$\text{Length}_{12\rightarrow23}\times$Post Reform	0.0781***	0.0775***	0.144***
	(0.0101)	(0.0102)	(0.0505)
$\text{Length}_{24\rightarrow35}\times$Post Reform	0.0261**	0.0260**	0.167**
	(0.0132)	(0.0132)	(0.0776)
S2 (Financial & Economic)	0.0616***	0.0474**	0.0474**
	(0.0164)	(0.0225)	(0.0225)
S3 (Industrial)	0.120***	0.104***	0.104***
	(0.0167)	(0.0229)	(0.0229)
S4 (IT)	0.202***	0.194***	0.194***
	(0.0299)	(0.0409)	(0.0409)
S6 (Government)	0.0489	0.0192	0.0191
	(0.0325)	(0.0439)	(0.0439)
S7 (Transport)	0.0667***	0.0648***	0.0647***
	(0.0164)	(0.0225)	(0.0225)
S8 (Healthcare)	0.00292	-0.0140	-0.0140
	(0.0197)	(0.0270)	(0.0270)
S9 (Construction)	0.0879*	0.0900	0.0901
	(0.0480)	(0.0654)	(0.0654)
S2$\times$Post Reform		0.0294	0.0682**
		(0.0312)	(0.0337)
S3$\times$Post Reform		0.0339	0.0836**
		(0.0316)	(0.0345)
S4$\times$Post Reform		0.0151	0.00686
		(0.0598)	(0.0822)
S5$\times$Post Reform		0.0600	0.196**
		(0.0651)	(0.0949)
S6$\times$Post Reform		0.00507	0.0398
		(0.0311)	(0.0335)
S7$\times$Post Reform		0.0346	0.112***
		(0.0382)	(0.0426)
S8$\times$Post Reform		-0.00388	0.106
		(0.0916)	(0.118)
S2$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.0599
			(0.0515)
S3$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.0785
			(0.0521)
S4$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			0.000754
			(0.0984)
S5$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.149
			(0.112)
S6$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.0587
			(0.0513)
S7$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.136**
			(0.0612)
S8$\times$Post Reform$\times\text{Length}_{12\rightarrow23}$			-0.354**
			(0.145)
S2$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.145*
			(0.0791)
S3$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.164**
			(0.0797)
S4$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.0593
			(0.139)
S5$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.356***
			(0.136)
S6$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.121
			(0.0790)
S7$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			-0.192**
			(0.0938)
S8$\times$Post Reform$\times\text{Length}_{24\rightarrow35}$			0.0641
			(0.162)
Other Controls	Yes	Yes	Yes
N	38,960	38,960	38,960

Note: the benchmark group is culture. All the sector dummy variables are: S2=Financial & Economic”; S3=Industrial; S4=IT; S5=Education; S6=Government; S7=Transport ; S8=Healthcare; S9=Construction.

Appendix A.2. Robustness Check

Figure A1: Testing the Smoothness of Control Variables - Worker's Education

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis shows the monthly percentages of different dummy variables .

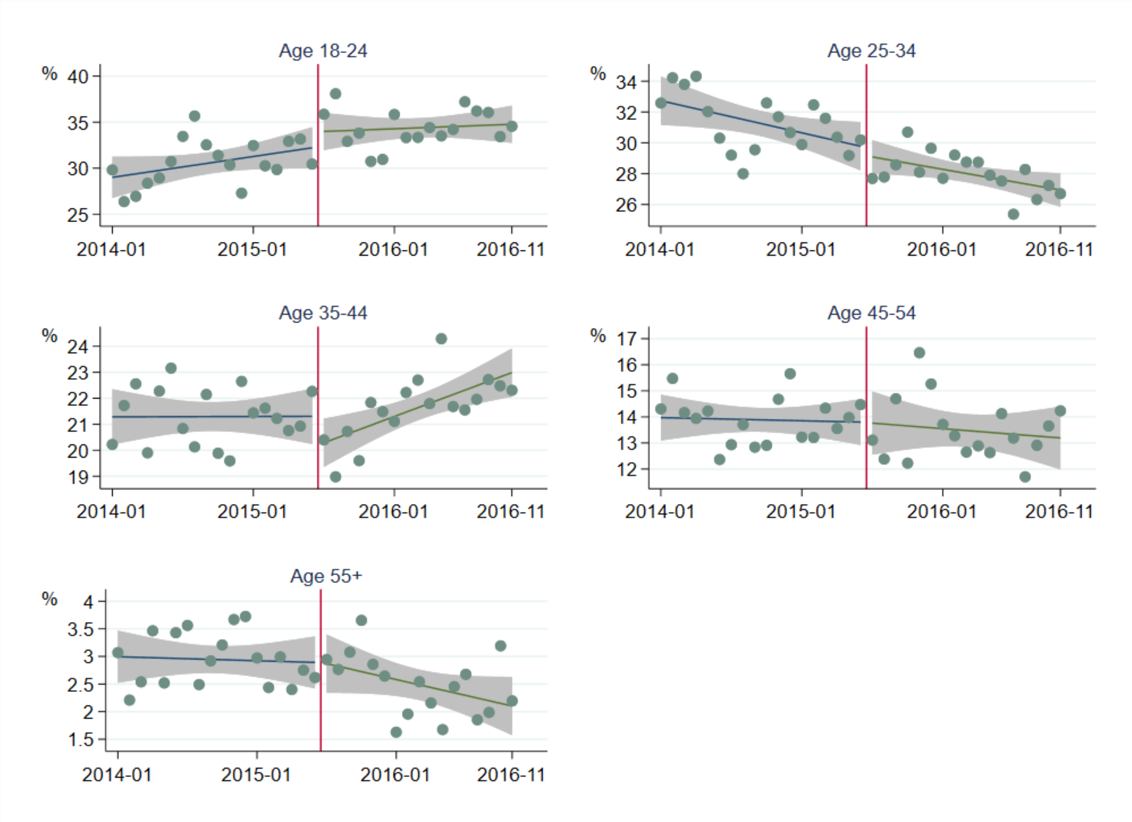

Figure A2: Testing the Smoothness of Control Variables - Worker's Age

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis shows the monthly percentages of different age groups.

Figure A3: Test the Smoothness of Control Variables - Worker's Salary
Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis shows the monthly percentages of employees with different levels of salary.

Table A4: Sensitivity Checks: Different Bandwidth Choices in the RD Design

Choice of Bandwidth	(1) 18 months (Tri)	(2) 12 months	(3) 6 months	(4) 5 months	(5) 4 months	(6) 3 months
$\text{Length}_{12\to 23}$	0.157***	0.169***	0.180***	0.179***	0.171***	0.175***
	(0.0109)	(0.00725)	(0.0110)	(0.0119)	(0.0131)	(0.0152)
$\text{Length}_{24\to 35}$	0.266***	0.250***	0.256***	0.266***	0.257***	0.249***
	(0.0146)	(0.00956)	(0.0147)	(0.0159)	(0.0176)	(0.0205)
Post Reform	-0.0158	-0.0234**	-0.0189	-0.0161	-0.0167	-0.0308
	(0.0150)	(0.0101)	(0.0145)	(0.0163)	(0.0183)	(0.0221)
$\text{Length}_{12\to 23}\times$Post Reform	0.0566***	0.0687***	0.0539***	0.0578***	0.0578***	0.0548***
	(0.0144)	(0.00980)	(0.0142)	(0.0160)	(0.0178)	(0.0206)
$\text{Length}_{24\to 35}\times$Post Reform	0.0505***	0.0376***	0.0215	0.0110	0.0211	0.0311
	(0.0193)	(0.0133)	(0.0190)	(0.0219)	(0.0244)	(0.0281)
Month	0.00259*	0.00344***	-0.00276	0.00495	0.00784	0.0148*
	(0.00146)	(0.000956)	(0.00288)	(0.00374)	(0.00517)	(0.00833)
Month$\times$Post Reform	-0.00424*	-0.00340**	0.0147***	-0.00757	-0.0168**	-0.0215*
	(0.00238)	(0.00135)	(0.00372)	(0.00522)	(0.00724)	(0.0116)
# Temp Contracts	-0.00220	-0.0589***	-0.0571***	-0.0464***	-0.0401***	-0.0326**
	(0.00990)	(0.00701)	(0.0102)	(0.0117)	(0.0132)	(0.0154)
Female	-0.00937	-0.0125**	-0.0233***	-0.0188**	-0.0149	-0.0220*
	(0.00840)	(0.00538)	(0.00766)	(0.00872)	(0.00968)	(0.0113)
Immigrant	-0.0271**	-0.0226***	-0.0244**	-0.0162	-0.0156	-0.00907
	(0.0122)	(0.00802)	(0.0114)	(0.0127)	(0.0144)	(0.0167)
Constant	0.140***	0.157***	0.140***	0.142***	0.137***	0.147***
	(0.0235)	(0.0120)	(0.0177)	(0.0199)	(0.0223)	(0.0271)
Control for						
Education	Yes	Yes	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes	Yes	Yes
Sector	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Average Salary	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Dummy	Yes	Yes	Yes	Yes	Yes	Yes
Yearly Dummy	Yes	No	No	No	No	No
N	55,021	39,381	19,615	14,378	11,648	8,711
F-test						
$\alpha_1 + \alpha_2 = 0$	5.854	15.84	4.481	5.066	3.890	0.941
p-value	0.0155	6.91e-05	0.0343	0.0244	0.0486	0.332
$\alpha_1 + \alpha_3 = 0$	2.759	0.956	0.0146	0.0472	0.0275	0.000103
p-value	0.0967	0.328	0.904	0.828	0.868	0.992

Note: Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A5: Sensitivity Checks: Different Polynomial Choices in the RD Design

	(1)	(2)	(3)
Order of Polynomials	1	2	3
$\text{Length}_{12\to23}$	0.158***	0.158***	0.158***
	(0.00982)	(0.00982)	(0.00982)
$\text{Length}_{24\to35}$	0.260***	0.260***	0.260***
	(0.0132)	(0.0132)	(0.0132)
Post Reform	-0.0175	-0.0173	-0.0337
	(0.0145)	(0.0271)	(0.0378)
$\text{Length}_{12\to23}\times$Post Reform	0.0595***	0.0596***	0.0595***
	(0.0130)	(0.0130)	(0.0130)
$\text{Length}_{24\to35}\times$Post Reform	0.0492***	0.0493***	0.0492***
	(0.0178)	(0.0178)	(0.0178)
# Interruptions	-0.00900	-0.00902	-0.00900
	(0.00913)	(0.00913)	(0.00913)
Female	-0.0114	-0.0114	-0.0114
	(0.00768)	(0.00768)	(0.00768)
Immigrant	-0.0290***	-0.0290***	-0.0291***
	(0.0111)	(0.0111)	(0.0111)
Constant	0.145***	0.150***	0.174***
	(0.0221)	(0.0309)	(0.0504)
Month	0.00277**	0.00339	0.0160
	(0.00128)	(0.00470)	(0.0202)
Month$\times$Post Reform	-0.00431**	-0.00600	-0.0243
	(0.00207)	(0.00718)	(0.0326)
Month^2		2.57e-05	0.00168
		(0.000228)	(0.00258)
$\text{Month}^2\times$Post Reform		3.34e-05	-0.000650
		(0.000416)	(0.00195)
Month^3			5.85e-05
			(9.07e-05)
$\text{Month}^3\times$Post Reform			-9.97e-05
			(0.000177)
Observed Controls	Yes	Yes	Yes
Firm-fixed Effect	Yes	Yes	Yes
N	55,021	55,021	55,021
F-test			
$\alpha_1 + \alpha_2 = 0$	7.121	2.343	0.456
p-value	0.00762	0.126	0.500
$\alpha_1 + \alpha_3 = 0$	2.655	1.127	0.147
p-value	0.103	0.288	0.701

Note: Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A6: Placebo Test for July 2014 and July 2016

	Jul 2014		Jul 2016	
	PS	PD	PS	PD
$\text{Length}_{12\to23}$	0.165***	-0.0257***	0.245***	-0.00705
	(0.0118)	(0.00826)	(0.0113)	(0.00727)
$\text{Length}_{24\to35}$	0.227***	-0.0236**	0.310***	0.00179
	(0.0159)	(0.0103)	(0.0158)	(0.00980)
Post Reform	-0.0373	-0.0399	0.0300	-0.000253
	(0.0657)	(0.0440)	(0.0611)	(0.0379)
$\text{Length}_{12\to23}\times$Post Reform	-0.0186	0.0174	-0.0133	-0.00316
	(0.0160)	(0.0110)	(0.0153)	(0.00969)
$\text{Length}_{24\to35}\times$Post Reform	0.0156	0.0128	-0.0468**	0.0225
	(0.0215)	(0.0135)	(0.0230)	(0.0145)
Month	0.00372	-0.00235	-0.00503	-0.00125
	(0.00365)	(0.00244)	(0.00364)	(0.00230)
Month$\times$Post Reform	-0.00306	-0.00344	0.000393	-4.87e-05
	(0.00512)	(0.00340)	(0.00512)	(0.00319)
# Interruptions	-0.0379***	0.0271***	-0.0757***	-0.000742
	(0.0113)	(0.00905)	(0.0112)	(0.00708)
Female	-0.0124	-0.00849	-0.00659	-0.00525
	(0.00853)	(0.00581)	(0.00864)	(0.00547)
Immigrant	-0.00840	-0.00979	-0.0384***	-0.00405
	(0.0122)	(0.00825)	(0.0128)	(0.00819)
Constant	0.136**	0.0299	0.175***	0.0802***
	(0.0566)	(0.0385)	(0.0369)	(0.0240)
Control for				
Education	Yes	Yes	Yes	Yes
Age	Yes	Yes	Yes	Yes
Sector	Yes	Yes	Yes	Yes
Monthly Average Salary	Yes	Yes	Yes	Yes
Monthly Dummy	Yes	Yes	Yes	Yes
N	13,457	13,457	15,446	15,446
F-test				
$\alpha_1 + \alpha_2 = 0$	0.696	0.261	0.0750	0.00804
p-value	0.404	0.610	0.784	0.929
$\alpha_1 + \alpha_3 = 0$	0.0998	0.366	0.0705	0.320
p-value	0.752	0.545	0.791	0.572

Note: In the placebo test for Jul 2014, the pre-reform period is Feb-Jun 2014 and post-reform period is Jul-Nov 2014. In the placebo test for Jul 2016, the pre-reform period is Feb-Jun 2016 and post-reform period is Jul-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A7: Parametric Estimates for Transitions to PS and PD in the Full Administrative Sample

	(1) PS	(2) PS	(3) PS	(4) PD	(5) PD	(6) PD
$\text{Length}_{12\to23}$	0.218***	0.175***	0.147***	-0.00131	-0.00570***	-0.00867***
	(0.00167)	(0.00165)	(0.00180)	(0.00105)	(0.00107)	(0.00120)
$\text{Length}_{24\to35}$	0.290***	0.244***	0.242***	0.00853***	0.00402***	-0.00922***
	(0.00219)	(0.00223)	(0.00247)	(0.00135)	(0.00138)	(0.00150)
Post Reform	-0.0178***	-0.0182***	-0.0251***	0.00164	0.00118	0.00379**
	(0.00218)	(0.00230)	(0.00251)	(0.00143)	(0.00158)	(0.00177)
$\text{Length}_{12\to23}\times$Post Reform	0.0605***	0.0641***	0.0615***	0.00396***	0.00451***	0.00493***
	(0.00227)	(0.00220)	(0.00235)	(0.00141)	(0.00141)	(0.00155)
$\text{Length}_{24\to35}\times$Post Reform	0.0432***	0.0564***	0.0743***	0.00855***	0.00989***	0.00852***
	(0.00317)	(0.00316)	(0.00335)	(0.00194)	(0.00195)	(0.00204)
Month	0.00303***	0.00166***	0.000912***	-0.000520***	-0.000309**	0.000568***
	(0.000148)	(0.000214)	(0.000233)	(9.58e-05)	(0.000140)	(0.000155)
Month$\times$Post Reform	-0.00300***	7.15e-05	0.000527	0.000355***	-0.000104	-0.000572**
	(0.000207)	(0.000346)	(0.000378)	(0.000132)	(0.000221)	(0.000247)
# Interruptions		-0.0514***	-0.0148***		0.000936	0.00957***
		(0.00158)	(0.00173)		(0.00106)	(0.00116)
Constant	0.184***	0.126***	0.124***	0.0785***	0.0578***	0.0460***
	(0.00166)	(0.00345)	(0.00379)	(0.00109)	(0.00228)	(0.00255)
Control for						
Education	No	No	No	No	No	No
Age	No	No	No	No	No	No
Sector	No	Yes	Yes	No	Yes	Yes
Monthly Average Salary	No	Yes	Yes	No	Yes	Yes
Monthly Dummy	No	Yes	Yes	No	Yes	Yes
Yearly Dummy	No	Yes	Yes	No	Yes	Yes
Firm Fixed Effect	No	No	Yes	No	No	Yes
N	743,030	743,030	743,030	743,030	743,030	743,030
F-test						
$\alpha_1+\alpha_2=0$	265.3	281.5	159.9	12.66	11.05	22.39
p-value	0.0000	0.0000	0.0000	0.000374	0.000885	2.22e-06
$\alpha_1+\alpha_3=0$	55.71	117.1	177.3	24.77	26.32	30.11
p-value	0.0000	0.0000	0.0000	6.46e-07	2.90e-07	4.07e-08

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A8: Parametric Estimates for Transitions to PS and PD Including Part-time Jobs

	(1) PS	(2) PS	(3) PS	(4) PD	(5) PD	(6) PD
$\text{Length}_{12\to 23}$	0.206***	0.165***	0.152***	-0.0138***	-0.0154***	-0.0181***
	(0.00441)	(0.00435)	(0.00659)	(0.00316)	(0.00319)	(0.00482)
$\text{Length}_{24\to 35}$	0.291***	0.248***	0.252***	-0.0130***	-0.0186***	-0.0293***
	(0.00592)	(0.00595)	(0.00919)	(0.00387)	(0.00396)	(0.00572)
Post Reform	-0.00749	-0.0158***	-0.0184**	-0.000611	-0.00284	-0.00473
	(0.00570)	(0.00597)	(0.00894)	(0.00420)	(0.00460)	(0.00681)
$\text{Length}_{12\to 23}\times$Post Reform	0.0602***	0.0631***	0.0557***	0.00233	0.00184	0.00886
	(0.00617)	(0.00594)	(0.00870)	(0.00423)	(0.00421)	(0.00621)
$\text{Length}_{24\to 35}\times$Post Reform	0.0259***	0.0361***	0.0544***	0.0188***	0.0219***	0.0256***
	(0.00864)	(0.00854)	(0.0124)	(0.00560)	(0.00560)	(0.00783)
Month	0.00298***	0.00200***	0.00235***	-0.000550**	0.000197	0.00117**
	(0.000381)	(0.000553)	(0.000825)	(0.000274)	(0.000398)	(0.000583)
Month×Post Reform	-0.00369***	-0.00110	-0.00267**	0.000481	-0.000950	-0.00169*
	(0.000549)	(0.000913)	(0.00135)	(0.000390)	(0.000642)	(0.000944)
# Interruptions		-0.0635***	-0.0175***		0.0222***	0.0205***
		(0.00344)	(0.00478)		(0.00331)	(0.00414)
Female		0.0138***	0.00428		0.00896***	0.00780**
		(0.00286)	(0.00466)		(0.00223)	(0.00349)
Immigrant		-0.0230***	-0.0175**		-0.0118***	-0.00417
		(0.00449)	(0.00708)		(0.00353)	(0.00518)
Constant	0.162***	0.189***	0.140***	0.0990***	0.0478***	0.0507***
	(0.00425)	(0.00913)	(0.0137)	(0.00325)	(0.00691)	(0.00987)
Control for						
Education	No	Yes	Yes	No	Yes	Yes
Age	No	Yes	Yes	No	Yes	Yes
Sector	No	Yes	Yes	No	Yes	Yes
Monthly Average Salary	No	Yes	Yes	No	Yes	Yes
Monthly Dummy	No	Yes	Yes	No	Yes	Yes
Yearly Dummy	No	Yes	Yes	No	Yes	Yes
Firm Fixed Effect	No	No	Yes	No	No	Yes
N	98,443	98,443	98,443	98,443	98,443	98,443
F-test						
$\alpha_1+\alpha_2=0$	54.59	41.23	12.30	0.141	0.0409	0.334
p-value	0.0000	1.36e-10	0.000452	0.707	0.840	0.563
$\alpha_1+\alpha_3=0$	3.945	4.567	7.007	9.824	9.678	6.079
p-value	0.0470	0.0326	0.00812	0.00172	0.00187	0.0137

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A9: Sensitivity Checks: Exclude Salary in the Control Variables

	(1) PS	(2) PS	(3) PS	(4) PS (Tri)
$\text{Length}_{12\to23}$	0.209***	0.200***	0.177***	0.176***
	(0.00607)	(0.00604)	(0.00980)	(0.0109)
$\text{Length}_{24\to35}$	0.286***	0.268***	0.275***	0.281***
	(0.00785)	(0.00797)	(0.0132)	(0.0146)
Post Reform	-0.00707	-0.0162*	-0.0162	-0.0154
	(0.00841)	(0.00903)	(0.0146)	(0.0151)
$\text{Length}_{12\to23}\times$Post Reform	0.0616***	0.0658***	0.0574***	0.0538***
	(0.00845)	(0.00832)	(0.0131)	(0.0144)
$\text{Length}_{24\to35}\times$Post Reform	0.0187	0.0409***	0.0522***	0.0537***
	(0.0114)	(0.0114)	(0.0179)	(0.0193)
Month	0.00346***	0.00305***	0.00338***	0.00335**
	(0.000546)	(0.000800)	(0.00128)	(0.00148)
Month×Post Reform	-0.00410***	-0.00169	-0.00439**	-0.00453*
	(0.000784)	(0.00131)	(0.00209)	(0.00240)
# Interruptions		-0.0900***	-0.0246***	-0.0151
		(0.00590)	(0.00915)	(0.00994)
Female		-0.0338***	-0.0239***	-0.0206**
		(0.00452)	(0.00766)	(0.00837)
Immigrant		-0.0458***	-0.0420***	-0.0388***
		(0.00674)	(0.0111)	(0.0122)
Constant	0.209***	0.310***	0.250***	0.245***
	(0.00625)	(0.0133)	(0.0211)	(0.0224)
Control for				
Education	No	Yes	Yes	Yes
Age	No	Yes	Yes	Yes
Sector	No	Yes	Yes	Yes
Monthly Average Salary	No	No	No	No
Monthly Dummy	No	Yes	Yes	Yes
Yearly Dummy	No	Yes	Yes	Yes
Firm Fixed Effect	No	No	Yes	Yes
N	55,132	55,132	55,132	55,132
F-test				
$\alpha_1 + \alpha_2 = 0$	32.04	23.46	6.719	5.094
p-value	1.52e-08	1.28e-06	0.00954	0.0240
$\alpha_1 + \alpha_3 = 0$	0.909	3.773	3.394	3.317
p-value	0.340	0.0521	0.0654	0.0686

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. Uniform kernels are used in column (1)-(3) and a triangular kernel is used in column (4). *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Appendix A.3. Anticipation Effect

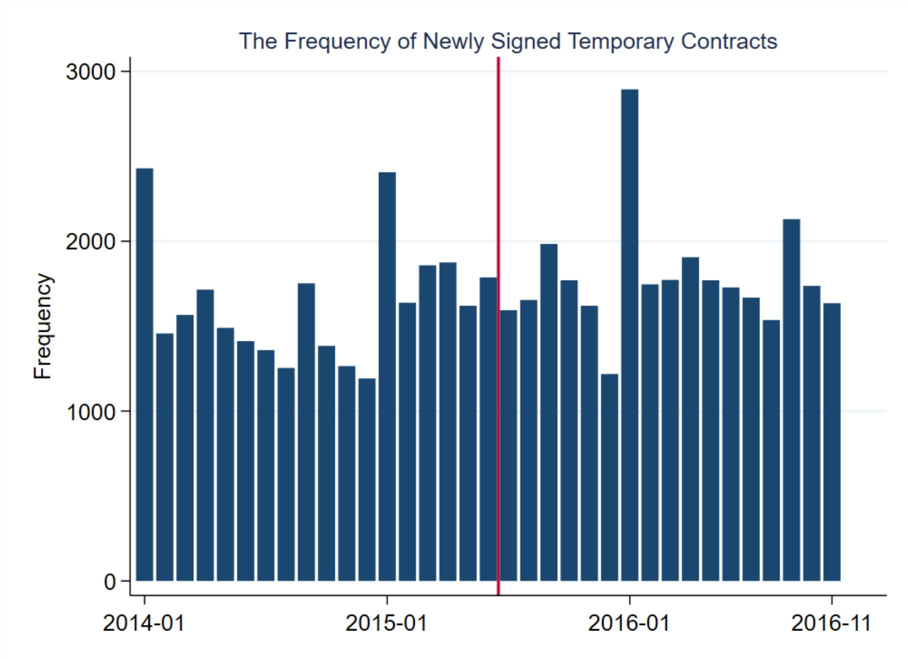

Figure A4: The Frequency of Newly Signed Temporary Contracts

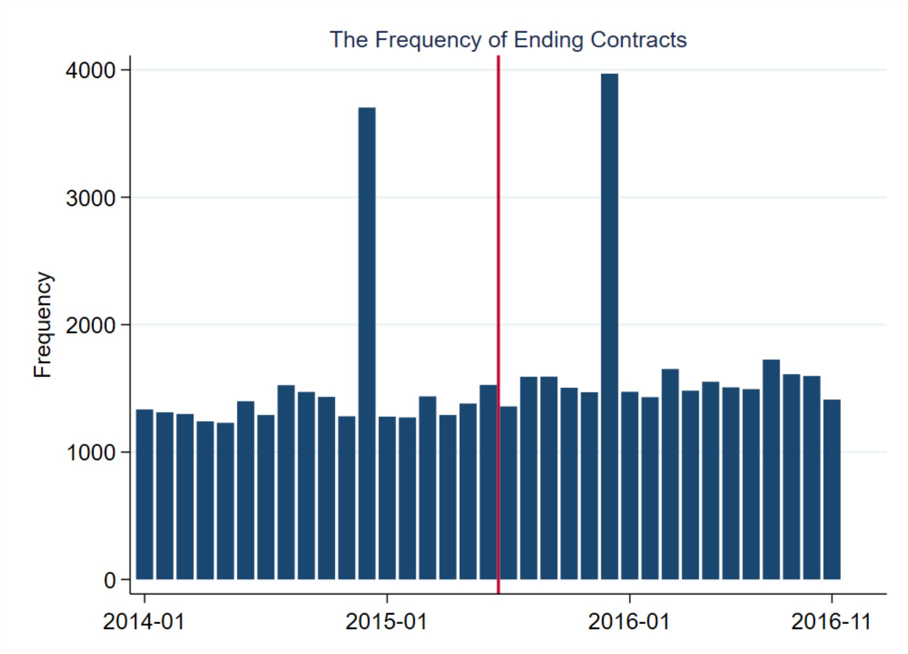

Figure A5: The Frequency of Ending Contracts
Note: The vertical red line represents the date when the policy reform takes effect.

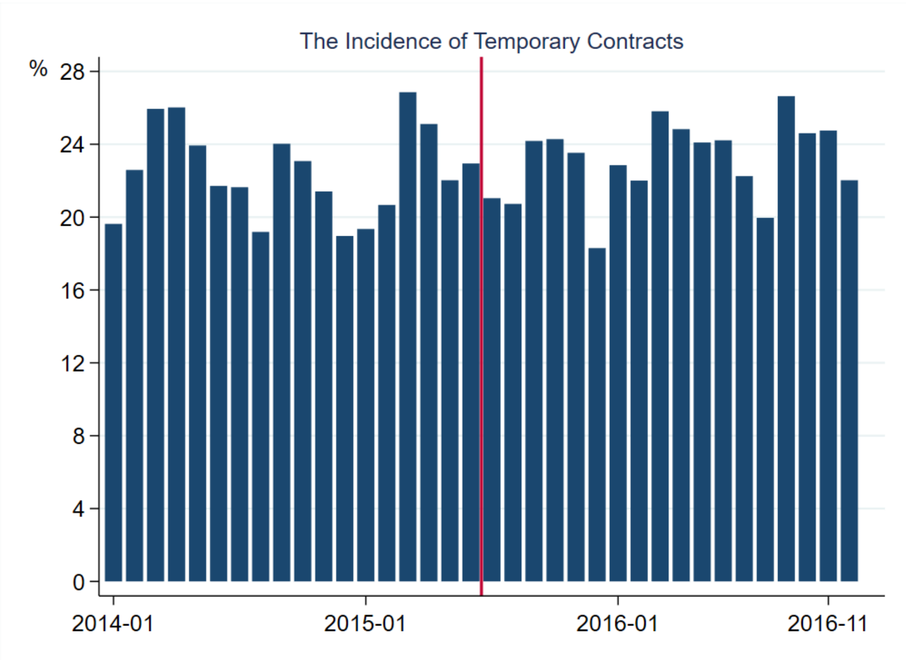

Figure A6: The Incidence of Temporary Contracts
Note: The vertical red line represents the date when the reform takes effect. The vertical axis in Figure A6 shows the monthly percentage of signing a temporary working contract after receiving unemployed benefits.

Table A10: Parametric Estimates for Causal Effects of the Reform on Initial Hiring

	(1)	(2)	(3)
Choice of Bandwidth	18 months	18 months	5 months
Post Reform	0.00698	0.00867	0.00772
	(0.00563)	(0.00614)	(0.0108)
Month	-0.00138***	-0.00107*	-0.00514**
	(0.000359)	(0.000574)	(0.00256)
Month×Post Reform	0.00246***	0.00181*	0.00977***
	(0.000580)	(0.00103)	(0.00378)
Female	0.000969	0.00118	0.00271
	(0.00340)	(0.00340)	(0.00586)
Immigrant	-0.0574***	-0.0569***	-0.0628***
	(0.00524)	(0.00523)	(0.00881)
Constant	0.341***	0.320***	0.329***
	(0.00630)	(0.0111)	(0.0121)
Control for			
Education	Yes	Yes	Yes
Age	Yes	Yes	Yes
Sector	Yes	Yes	Yes
Monthly Dummy	No	Yes	No
Yearly Dummy	No	Yes	No
N	97,000	97,000	28,022

Note: Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

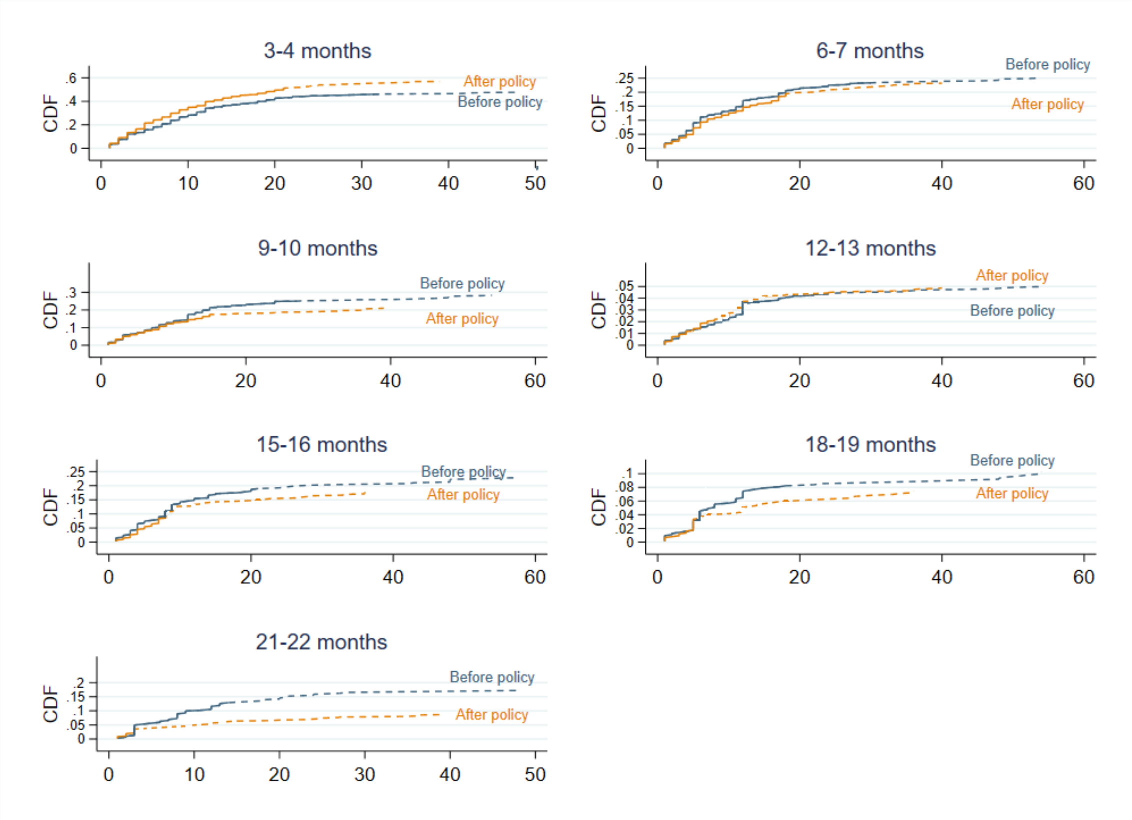

Figure A7: The Empirical CDF of the Length of the Renewed Contract
Note: the x-axis is the number of month and y-axis is the empirical CDF of the length of the renewed contract when a temporary contract ends between Jan 2014 and Jun 2015. Each sub-figure presents the CDF of different lengths of contracts. The blue line and orange line draw the CDFs before and after the reform, respectively. The dash blue line indicates the sum of the renewed contract's length and the previous chain's length already exceeds 36 months. The dash orange line indicates the sum of the renewed contract's length and the previous chain's length already exceeds 24 months. If the renewed contract is a permanent contract, its length is treated as infinity.

Table A11: Kolmogorov-Smirnov Equality-of-distributions Test

	Length of Chains (in Months)						
	3-4	6-7	9-10	12-13	15-16	18-19	21-22
Test for (1) $<$ (2)							
Largest difference	0.0048	0.0532	0.0184	0.0217	0.0381	0.0749	0.0810
p-value	0.9947	0.5809	0.9547	0.9469	0.8955	0.7181	0.7163
Test for (1) $>$ (2)							
Largest difference	-0.0603	-0.0510	-0.1234	-0.1274	-0.1393	-0.1538	-0.1736
p-value	0.4222	0.6065	0.1228	0.1529	0.2291	0.2475	0.2161
Combined test							
Largest difference	0.0603	0.0532	0.1234	0.1274	0.1393	0.1538	0.1736
p-value	0.7816	0.9488	0.2452	0.3048	0.4527	0.4875	0.4279
Corrected p-value	0.7475	0.9346	0.2033	0.2540	0.3832	0.4097	0.3486

Note: Group (1) refers to the chains ending between Jan 2014 and Jun 2015, and group (2) refers to the chains ending between Jul 2015 and Nov 2016. ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A12: Parametric Estimates for the Policy Effect on the Lengths of Renewed Contracts

	(1)	(2)	(3)	(4)
$\text{Length}_{12\to23}$	1.069	1.026	1.059	1.014
	(0.737)	(0.742)	(0.739)	(0.746)
$\text{Length}_{24\to35}$	2.498**	2.386**	2.478**	2.399*
	(1.202)	(1.214)	(1.225)	(1.235)
Post Reform	-0.986	-1.482	-0.732	-1.386
	(0.887)	(1.160)	(0.883)	(1.149)
$\text{Length}_{12\to23}\times$Post Reform	-0.990	-0.995	-0.928	-0.915
	(0.922)	(0.925)	(0.915)	(0.920)
$\text{Length}_{24\to35}\times$Post Reform	1.030	0.645	2.225	1.887
	(2.304)	(2.313)	(2.307)	(2.313)
Month	0.0272	0.0577	-0.0216	0.0387
	(0.0793)	(0.109)	(0.0794)	(0.108)
Month$\times$Post Reform	-0.126	-0.138	-0.0897	-0.154
	(0.0978)	(0.152)	(0.0970)	(0.150)
# Temp Contracts			-2.981***	-2.830***
			(0.605)	(0.615)
Female			-1.041**	-1.044**
			(0.427)	(0.427)
Immigrant			-0.234	-0.156
			(0.683)	(0.680)
Constant	12.59***	13.75***	13.68***	14.76***
	(0.746)	(1.287)	(1.160)	(1.550)
Control for				
Education	No	No	Yes	Yes
Age	No	No	Yes	Yes
Sector	No	No	Yes	Yes
Monthly Average Salary	No	No	Yes	Yes
Monthly Dummy	No	Yes	No	Yes
Yearly Dummy	No	Yes	No	Yes
N	3,119	3,119	3,119	3,119
F-test				
$\alpha_1 + \alpha_2 = 0$	3.474	3.696	2.419	3.216
p-value	0.0625	0.0546	0.120	0.0730
$\alpha_1 + \alpha_3 = 0$	0.000338	0.107	0.385	0.0385
p-value	0.985	0.743	0.535	0.844

Note: Pre-reform period: Jan 2014-Jun 2015. Post-reform period: Jul 2015-Nov 2016. Clustered standard errors by individuals are provided in parentheses. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table A13: Eligible Criteria on the dataset SPOLISBUS

Variable	Discription	Value	Lable
SCONTRACTSOORT	Code for the type of contract of the employee.	B	Certain period of time
SSOORTBAAN	Code for job type.	9	Rest
SCDAARD	Code for the nature of the employment relationship.	01	Labour contract
SARBEIDSRELATIE	Fixed or flexible employment relationship.	1	Fixed
SCAOSECTOR	Code that specifies the collective labor agreement of a company or institution.	1000	Private companies
SFSINDFZ	Code for phase classification in the context of the Flexibility and Security Act.	00 or --	Unknown

Note: This table shows which variables in the dataset SPOLISBUS are used to select the temporary contracts that construct the chains in the sample. The last two columns present which value I choose for each variable and its corresponding label in the dataset. For the full range of values in each variable, please refer to "Documentatierapport Banen en lonen op basis van de Polisadministratie (SPOLISBUS)" by CBS.