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

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Abstract

This paper studies how the shortened maximum cumulative duration for successive temporary contracts affects their transitions to permanent contracts, known as the stepping stone effect. By exploiting a natural experiment arising from a 2015 reform in the Netherlands and using a regression discontinuity design, we show that the policy reform accelerates the stepping stone effect, although it has heterogeneous effects on the employees who have accumulated the chains of temporary contracts with different lengths. The policy reform, shortening the total duration of temporary contracts, forces the employer to sign a permanent contract much sooner without interrupting temporary contracts' functioning as a screening device.

Keywords: Temporary Contracts, Permanent Contract, Stepping Stone, Chain Rule
JEL: J28, J41, J42

1. Introduction

Many European countries, including the UK, France, Italy, Portugal, Spain and Netherlands, have dual labour markets, in which temporary (or fixed-term) and permanent (or open-ended) contracts coexist. The duality arises because while stringent employment protection legislation (EPL) ensures that firms face substantial employment termination costs when hiring workers under permanent contracts, temporary contracts can be used for short-term working relationships with little separation costs. On the one hand, the increasing share of temporary employment in many European countries has raised concerns over the segmented labour markets, in which part of the workers are trapped in low-paid, low-productivity temporary jobs, with little prospect of upward mobility (see Nätti (1993), Alba-Ramirez (1998), Amuedo-Dorantes (2000), Brown and Sessions (2003), D'Addio and Rosholm (2005) and Güell and Petrongolo (2007)). On the other hand, there is some evidence that temporary contracts can function as a stepping stone to permanent work (see Booth et al. (2002), Van den Berg et al. (2002), Gash (2008), Ichino et al. (2008),

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de Graaf-Zijl et al. (2011) and Faccini (2014)). To reduce the gap between temporary contracts and permanent contracts, commentators and policymakers have stressed the importance of finding an appropriate balance between flexibility and security (European Commission (2003)). The regulation of temporary contracts aims at stabilizing employment and reducing the uncertainty for workers. However, its effectiveness is questionable: temporary jobs account for most job flows because employers may avoid permanent contracts (see Serrano (1998), Blanchard and Landier (2002), Knegt et al. (2007), Kahn (2010) and Aguirregabiria and Alonso-Borrego (2014)).

In recent years, as more and more countries began to relax their dismissal regulations for permanent contracts, countries reforming regulations on temporary contracts were quite split: some countries reduced restrictions on their usage, while others imposed additional ones[1] (see OECD (2020)). Which direction of the policy reform on temporary contracts can alleviate the duality gap is still a matter of debate, and the answer may depend on the specific institutional features of each country. In particular, it is not yet conclusively established whether a reduction in the cumulative maximum duration for successive temporary contracts can have a positive or negative impact on their transitions to permanent ones, known as the stepping stone effect. In this paper, we try to provide more evidence by exploiting the natural experiment that arises from a reform in the Netherlands.

In 1998, a so-called chain rule" was first introduced in the Netherlands to restrict the length of consecutive temporary contracts that the employer can renew with the same employee. According to this rule, when different temporary employment contracts, signed between the same pair of employee and employer, follow each other with intervals not exceeding three months, the last temporary contract automatically becomes permanent if their total duration has exceeded a period of 36 months[2]. As of July 1, 2015, a reform in the chain rule changed the restriction on the length from 36 months to 24 months, intending to improve flexible workers' legal position and discourage the abusive and prolonged use of flexible employment relationships.

The new chain rule only applies to the temporary contracts signed after July 1, 2015, while temporary contracts signed before that date are subject to the old rules. Apparently, one possible outcome of the policy is to speed up employees entering into permanent contracts(i.e., strengthen the stepping stone effect). As with most innovative regulations and policies, it is questionable whether the intended effect of this policy can be achieved. Boockmann and Hagen (2008) and Faccini (2014) show that firms use temporary contracts to screen workers for permanent positions, the chain rule decreases the learning period to 24 months in the temporary contract and that is probably not enough for employers to screen their employees' abilities and make the decision to retain them with permanent contracts. And that could force employers to stop hiring the employees at the end of the 24th month. Besides, if

[1] For example, France increased the maximum number of successive fixed-term contracts from two to three in August 2015. On the contrary, Italy reduced the maximum duration of fixed-term contracts from 36 months to 24 months in July 2018.

[2] If the last temporary contract is the fourth renewed one, it would also automatically becomes permanent.

employers foresaw that they could not screen workers within 24 months, they might not even hire them to begin with. Overall, the other possible effect of the policy reform would, therefore, be perverse, i.e., discourage hiring, since separations driven by the failure of learning about match quality would impair the stepping stone effect of temporary contracts.

To estimate the effect of the policy reform in the chain rule, we treat the date as the running variable and take July 1, 2015, as the discontinuity threshold in the regression discontinuity (RD) design. Using a linear probability model, we estimate the probabilities for the employees who have accumulated a chain of temporary contracts to obtain a permanent contract with the same or a different employer – the two main channels for the stepping stone effects. The data used to assess the effectiveness of policy reform is from the non-public micro-data sets of Statistics Netherlands (CBS). The primary data is SPOLISBUS, the monthly administrative data on jobs and wages of employees at Dutch companies from 2010 to 2018. We merge it with the monthly administrative data SECMBUS and Dutch Labour Force Survey (EBB) to obtain a data set of individuals who are 15 years and older and possess complete employment histories from 2010 to 2018 and recorded personal characteristics, which is a sub-sample of 15% of the total population in the Netherlands. We focus on all the temporary contracts ending between January 2013 and December 2016 and build up all chains of corresponding temporary contracts according to different chain rules before or after the policy reform.

The empirical results suggest that the policy reform in the chain rule accelerates the stepping stone effect, although it has heterogeneous effects on the employees who have accumulated the chains of temporary contracts with different lengths. The policy reform has no significant impact on the employee who has accumulated a chain in less than 12 months. However, for the employee who has accumulated a chain between 12 months and 23 months, the policy reform significantly strengthens the stepping stone effect, increasing the probability of signing a renewed permanent contract by 4.2%. For the employee who has accumulated a chain between 24 months and 35 months, the policy reform increases this probability by 3.17% but statistically insignificant. The results imply that the more stringent chain rule after the policy reform forces the employer to accelerate the process of offering a permanent contract without interrupting the functioning of the temporary contract as a screening procedure.

We then check the plausible hypothesis that employers may want to avoid the new chain rule by offering a much longer renewed temporary contract before the policy reform or a much shorter one after the policy reform. After grouping the chains of temporary contracts based on their length, we use the Kolmogorov-Smirnov test to check the equality of the distributions of the renewed temporary contracts' lengths before or after the policy reform. The results show that the hypothesis is not supported by the data for the most lengths of the chains.

The contributions in our paper are mainly twofold. First, the findings in this paper contribute to the literature by showing the evidence that temporary contracts are not only used as a buffer stock to adjust to economic fluctuations and avoid labour market inflexibilities but can be used as a screening device of employees' abilities as well, which gets aligned with the argument of Faccini (2014) that temporary contracts are used to screen workers for permanent positions. Under the new policy reform, employers have to make decision over permanent contracts one year sooner. If employers decide to continually hire the employees after screening their abilities, they prefer to offer a permanent contract directly rather to enjoy the benefit from using another temporary contract as a "buffer stock".

Second, our paper enriches the literature by showing that the policy, restricting the legal limits of the cumulative duration of temporary contracts, can be an alternative way to alleviate the gap between temporary and permanent contracts. Most of the previous empirical studies are devoted to the policy reforms on changes in the objectives of temporary contracts or the dismissal costs of permanent contracts (see, e.g. Güell and Petrongolo (2007) and Cahuc et al. (2020)) rather than changes in the legal limits of the cumulative duration of temporary contracts. The literature looking into the effects of the relaxed legal limits includes Martins (2016) and Silva et al. (2018), both of which find a drop in the conversion to permanent contracts after an increase in the cumulative duration of temporary contracts in Portugal [3]. To the best of our knowledge, our paper is the first to investigate how the tightened duration restrictions on the use of temporary contracts affect the labour market transitions. We believe that the policy implications in the Netherlands in the paper can also be applicable to other European countries that rely more on unemployment benefits rather than employment protections [4].

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 conducts the robustness check. Section 10 concludes.

2. Literature

These is an extensive literature that has thoroughly discussed duality in European labour markets. The justification for EPL includes the need to protect employees from their employers' unfair behaviour, the limited ability of employees to insure themselves against the risk of dismissal due to the imperfections in financial markets, and the need to preserve firm-specific human capital investment in the long-term (see e.g. Pissarides (2010)). However, the cost imposed from EPL on the employers is also considerable, since it limits their abilities to accommodate their workforce to the variation in demand and technology, and thus not only reduces job destruction but also discourages job creation, leaving the workforce with inefficient adjustment. Therefore, temporary contracts are used as a "buffer stock" to adjust

[3] Martins (2016) investigates the 2012 reform in Portugal, in which the maximum duration of temporary contracts was increased from three to four and a half years. Silva et al. (2018) focus on the 2004 reform in Portugal, in which the maximum duration was increased from three to six years.

[4] 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.

to economic fluctuations and avoid labour market inflexibilities (see Bentolila and Bertola (1990), Bentolila and Saint-Paul (1994), Kugler and Pica (2008), Skedinger (2011), Martin and Scarpetta (2012) and Hijzen et al. (2017)).

However, the impact of introducing temporary contracts on the labour market is ambiguous. Blanchard and Landier (2002) argue that the effects of a partial reform of employment protection by allowing firms to hire workers on fixed-term contracts may be perverse. Using French data for young workers, they conclude that the reforms have substantially increased turnover, without a substantial reduction in unemployment duration. Aguirregabiria and Alonso-Borrego (2014) find that the reform of removing restrictions on the use of temporary contracts in Spain has positive effect on total employment and job turnover, but little effect on labor productivity and the value of firms.

Meanwhile, Booth, Francesconi, and Frank (2002) use data from the British Household Panel Survey to confirm that temporary workers have lower levels of job satisfaction, receive less training and are less well-paid, but there is some evidence that fixed-term contracts are 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 contract, but decreases with repeated temporary jobs and especially with interruptions. Faccini (2014) shows that in most European countries temporary workers enjoy high rates of transition into permanent employment and temporary contracts significantly decrease the unemployment rate. He offers a rationale for the finding that temporary contracts seem to act as an important screening device in European countries.

There are also other interesting findings related to the policy reform on the duality problem. Güell and Petrongolo (2007) find that the 1994 reform on reducing dismissal costs of permanent contracts in Spain induces the conversion of temporary contracts to occur much earlier than the 3-year legal limit. Cahuc et al. (2020) analyze the consequences of the taxation of temporary jobs recently introduced in several European countries to induce firms to create more open-ended contracts and to increase the duration of jobs, and show that the taxation of temporary jobs does not reach its objectives: It reduces the mean duration of jobs and decreases job creation, employment and welfare of unemployed workers.

3. Background

3.1. Temporary Contracts in the Netherlands

Temporary contracts are rather conventional in the Netherlands, although workers may have different types of contracts over the life cycle. As shown in Figure 1, for all the dependent employment (employment with either temporary contract or permanent contract) in the Netherlands, 55.56% of the workers at ages 15-24 have temporary contracts in 2016 (the rest have permanent contracts), and this percentage reduces to 15.18% for the workers at ages 25-54 and 7.08% for the workers at ages 55-64, which means that the Dutch labour market is more inflexible for the old workers than for the young workers. Moreover, with international comparison, the percentage of having temporary contracts at ages 15-54 in the Netherlands is among

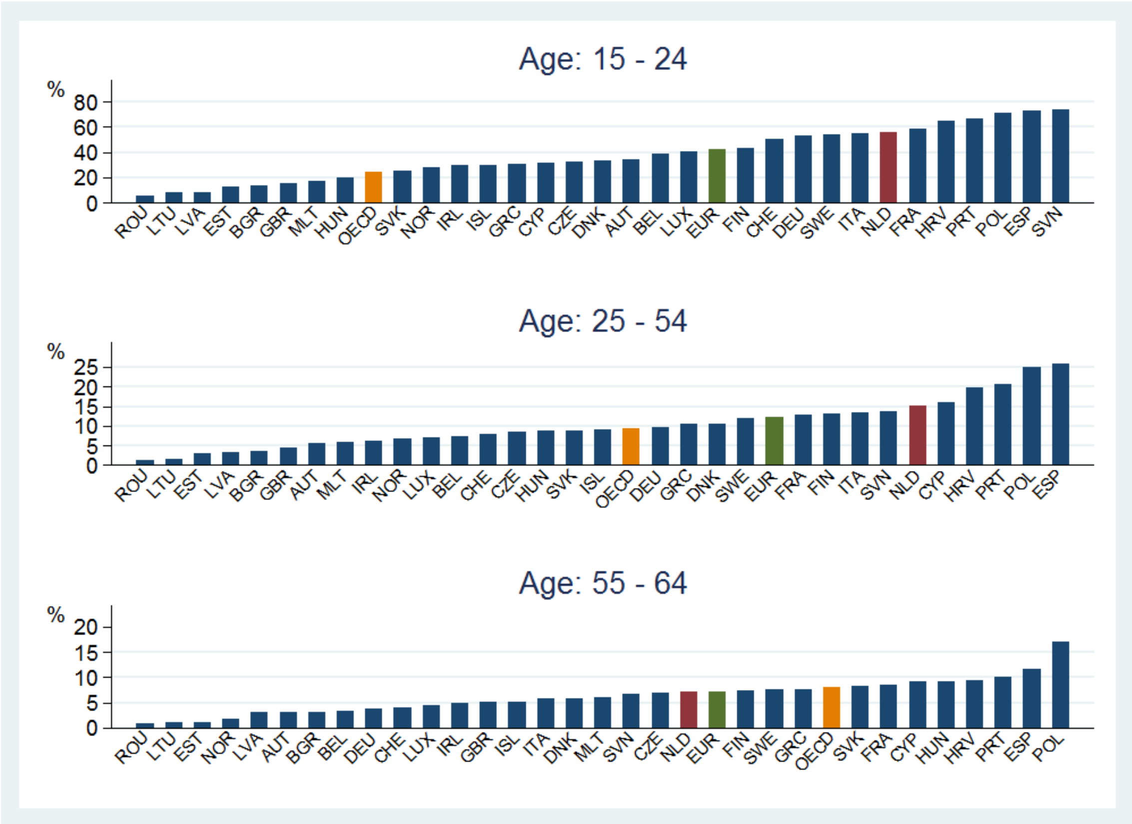

Figure 1: International Comparison of Temporary Employment, as Percentage of Total Dependent Employment
Source: OECD Dataset: LFS - Employment by Permanency, 2016.

the top levels, much higher than the average percentages of European countries and OECD countries.

Meanwhile, the use of temporary contract in the Netherlands is still on the rise. As shown in Figure 2, 34.9% of the men at ages 15-24 and 6.65% of the men at ages 25-54 in the dependent employment have temporary contracts in 2000, and they increase gradually to 53.41% and 14.32% in 2016. Similarly, the percentages of the women having temporary contracts in dependent employment also increase gradually, from 36.11% at ages 15-24 and 12.25% at ages 25-54 in 2000 to 57.63% and 16.09% in 2016, respectively.

Although the overall level of employment protection in the Netherlands only takes an intermediate position compared with the levels in other European countries, there is a substantial divergence in the protection for permanent and temporary contracts. As shown in Figure 3, the Netherlands has among the highest level of protection for the permanent contract against individual dismissal in Europe, only lower than Portugal and the Czech Republic. This is due to the fact that the Netherlands has a rigorous system for individual dismissal of workers with a permanent contract. The employer can choose to either ask permission from the labour office or go to court, but the first implies following inconvenient and time-consuming legal procedures and the second implies making a severance payment. Meanwhile, the Netherlands has among the lowest level of protection for temporary employment in Europe, only

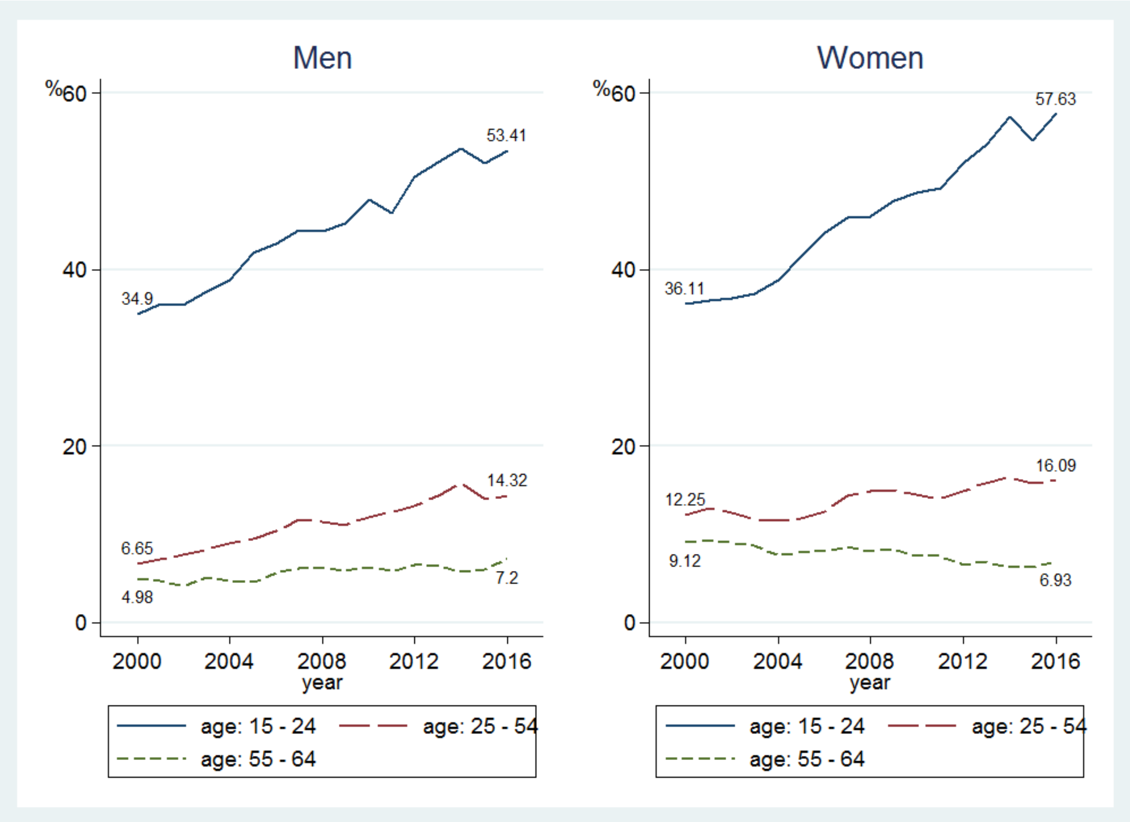

Figure 2: Temporary Employment of the Netherlands in 2000-2016, as Percentage of Total Dependent Employment
Source: OECD Dataset: LFS - Employment by Permanency, 2000 - 2016.

higher than the United Kingdom. The temporary contract ends on the expiry date without the requirement of the employer to give notice for termination[5]. In case of early termination of a temporary contract, the employer must give notice after having obtained permission from the Employee Insurance Agency (UWV WERKbedrijf) or dissolution by the cantonal court. In this case, the minimum notice period must be for one month.

3.2. The Chain Rule and 2015 Reform

To increase the flexibility in the labour system while maintaining an adequate level of protection for employees with temporary contracts, the Dutch government enacted the Flexibility and Security Act (Wet Flexibiliteit en Zekerheid in Dutch) on 14 May 1998, in which the chain rule (ketenregeling in Dutch) was first introduced. It stipulates that when different temporary employment contracts, signed between the same pair of employee and employer, follow each other with possible intervals not exceeding three months, the last temporary employment contract automatically becomes a permanent one, if the total duration of employment contracts including intervals has exceeded a period of 36 months or if the last temporary contract is the

[5]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.

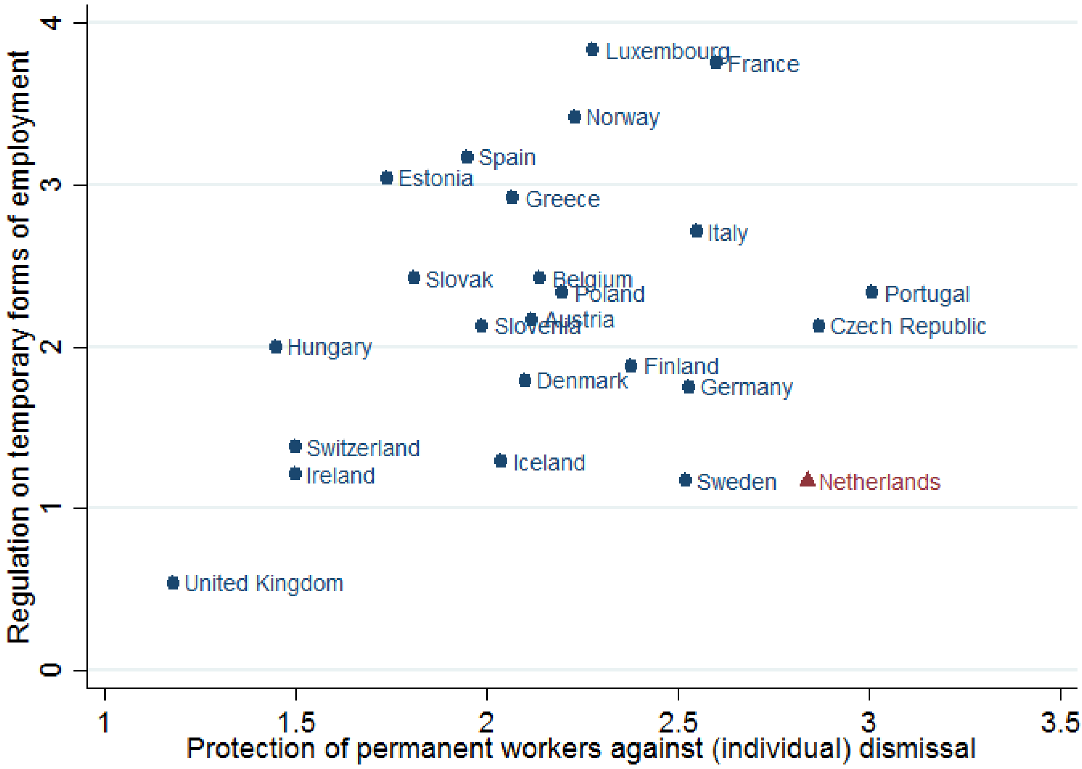

Figure 3: Protection Indicators for European Countries

Source: OECD Employment Protection Database 2013 - 2014.

fourth one [6]. An evaluation of the Flexibility and Security Act (see Knegt et al. (2007)) reveals that employers have made massive use of the opportunity to provide temporary contracts without violating the chain rule.

The Work and Security Act (Wet Werk en Zekerheid (WWZ) in Dutch), which was introduced in 2015[7], is the successor to this Act. It is intended to make dismissal faster and cheaper, to strengthen the legal position of flexible workers and limit their gap with permanent employment, and to get more people out of the unemployment benefit scheme. As of 1st July 2015, the chain rule for a temporary contract to automatically become a permanent contract has changed. From this date, one employer hiring one same employee through consecutive temporary contracts for more than two years is no longer permitted. After two years of temporary contracts signed with the same employer including possible intervals not exceeding six months, or when a fourth contract is offered, the presented new contract must be of a permanent

[6]The Dutch Civil Code has no provision prohibiting long fixed-term employment contracts. The Dutch word "keten" in the definition "ketenregeling" stands for "chain", which means that in order to apply the "ketenregeling", there must be multiple contracts following each other. So, when parties conclude one long temporary employment contract of more than three years in the beginning, this contract won't automatically be converted into a permanent contract.

[7]The proposal for WWZ was adopted by the House of Representatives (Tweede Kamer) on 18th February 2014, and by the Senate (Eerste Kamer) on 10 June 2014.

nature[8]. For example, if the same employee and employer complete two contracts of nine months, followed by an interval of up to six months, and then agree on the third contract of eight months, the last contract will automatically become a permanent contract (See the change in the eligible criteria for the chain rule to come into effect in Figure 4).

Both the old and the new chain rules also apply to successive employment contracts between an employee and various employers who, with regard to the work involved, should reasonably be deemed to succeed one another. This is, however, conditional upon the fact that under the new contract the skills and responsibilities required are the same as for the previous contract and there is a link between the new employer and the former one (e.g., a relaunched company or the employer within the group as a whole).

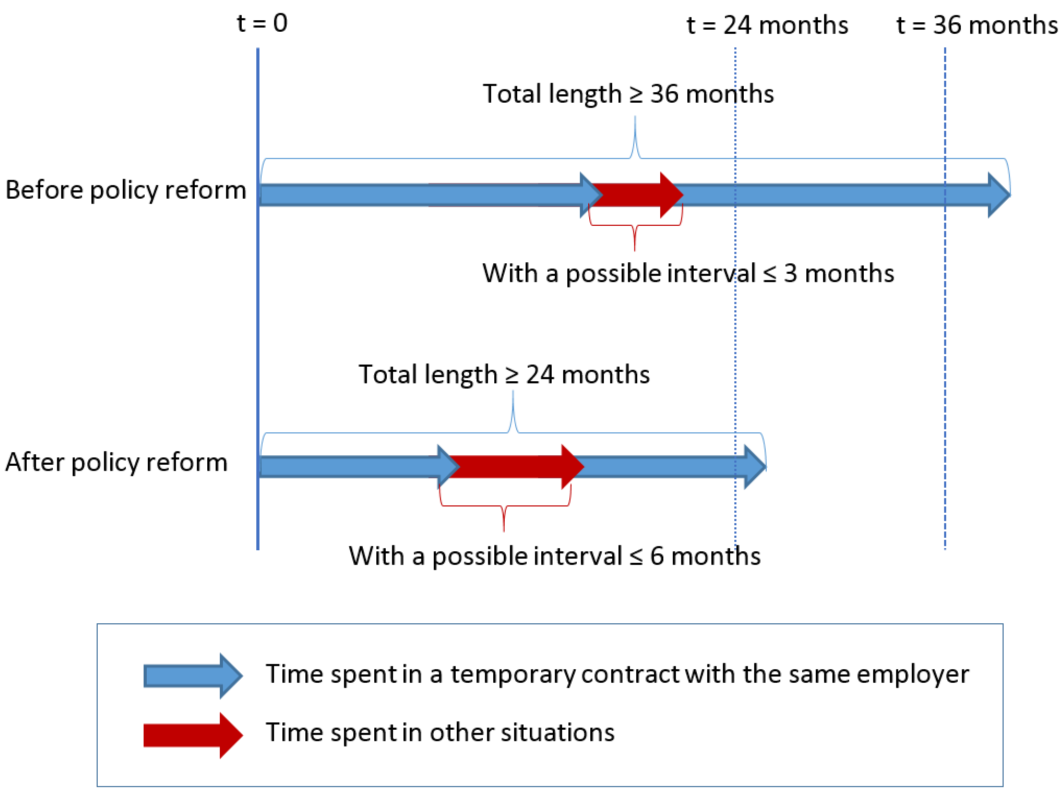

Figure 4: Eligible Criteria for the Chain Rule to Come into Effect

A transitional law[9] determines how a chain of temporary contracts can be accumulated if it just crosses 1st July 2015 and in which case the old or new chain rule

[8]Note that there can be an interval (maximum 3 months before the new rule, and 6 months thereafter) between two adjacent temporary contracts during the accumulating process. In the interval, the employee can sign a temporary contract with another different employer. If the sequence also involves a contract with a different employee, we will treat this contract as the interval in the chain.

[9]See Wet werk en zekerheid Artikel XXIIe.

applies. Firstly, to accumulate a chain with a sequence of temporary contracts with the same employer, the possible intervals between the temporary contracts before 1st July 2015 should be no more than 3 months (old rule applies), and the interval just crossing or after 1st July 2015 can be no more than 6 months (new rule applies). Secondly, if the last temporary contract in a chain was signed before 1st July 2015, the total length of the chain should be no more than 36 months (old rule applies); if it was signed after 1st July 2015, the total length of the chain should be no more than 24 months (new rule applies). Otherwise, the last temporary contract in the chain should automatically become a permanent contract. According to this transitional law, for instance, if an employer first offers a new employee with a 1-year temporary contract which ends in June 2015, then it can immediately renew a temporary contract that could be as long as 2 years. However, if an employer first offers a new employee with a 1-year temporary contract that ends in July 2015, then it can only renew a temporary contract that could be as long as 1 year.

There are several exceptions to the chain rule (both before and after the policy change in 2015). Firstly, if the employee and employer first sign a temporary employment contract for more than two years (before the policy change it is three years), after which they sign a contract for a definite period of no more than three months, then the last contract will not automatically become a permanent contract[10]. Secondly, the chain rule does not apply to the employment contracts when employees are following dual learning-work training. Such deviation exists when employment contracts have been entered into predominantly because of the employee's education, insofar as this is necessary for the completion of the training. Thirdly, the chain rule does also not apply to the employment contracts with the employees who have not worked more than 12 hours per week on average or who are not yet 18 years old. Fourthly, deviation from the chain rule is also possible if the temporary contract follows from the collective labour agreement (CAO) for certain functions or the intrinsic nature of the business requires a deviation from the chain arrangement.

Through applying this new chain rule, the government intends to reduce the massive use of temporary employment contracts and thereby create job security. The question is whether the intended effect of the Work and Security Act (WWZ) is actually being achieved. After all, under the new regulation, employers might stop retaining the current employees with temporary contracts after 24 months. If this circumstance dominates, the positions of employees with temporary contracts are weakened rather than strengthened.

Beyond the policy reform in the chain rule, there are also other policy changes in the WWZ as of 1st July 2015. However, the other changes are mainly focused on the dismissal rule and the unemployment benefit. Firstly, the new policy makes the dismissal rule much simpler and clearer, aiming to standardize protections against different types of dismissal. For instance, a business dismissal or a dismissal due to a long-term disability will be resolved by the UWV; a dismissal for other reasons will be resolved by a sub-district court. Moreover, a transition allowance will be paid to employees, either temporary or permanent, who are involuntarily dismissed and have been in service for two years or more. Secondly, the duration of the

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

unemployment benefit changes from a maximum of 38 to 24 months, and people who become unemployed from 1st July 2015 and apply for a benefit must state their monthly income.

The first policy changes make the dismissal rule much more straightforward and precise. Even if both the employer and the employee can solve the dispute much sooner, it does not necessarily mean that the employer becomes less costly to dismiss its employees. One may argue that the employer would be more willing to offer a permanent contract to solve the dismissal dispute more quickly. However, compared to the substantial financial cost due to the dismissal, the savings in the time cost of resolving disputes is negligible.

A transition allowance would be paid to temporary or permanent employees who are involuntarily dismissed, so the cost of offering temporary contracts and permanent contracts increase simultaneously. This transition allowance increases the cost of hiring for employees (first order effect), which might produce a negative effect on the supply of short-term positions. These cuts on short-term positions could produce a second order effect on the transition probability to permanent contracts, where these cuts may decrease the competition for permanent position, and that could bias up the our main estimates. We have checked that the number of temporary contracts hasn't been cut (see Figure 9), which alleviates this possible biased-up concern.

The duration of the unemployment benefit is shortened, so it may reduce the employee's willingness to dismiss voluntarily or enhance his motivation to search for jobs when he is unemployed. This could increase the labour supply on the market, thus competition can be enhanced for the same short-term positions. It can motivate employees at the position work harder, which indirectly augments the probability of getting permanent contracts. This could possibly bias up the transition probabilities.

4. Data

The data used for evaluating the effect of policy reform is from the non-public microdata sets of Statistics Netherlands (CBS). The primary data set is SPOLISBUS, which provides monthly administrative data on the jobs and wages of the employees at the Dutch companies from 2010 to 2018. All the detailed information on the types of current employment contract, such as temporary or permanent, part-time or full-time, fixed or flexible, can be obtained from this dataset on a monthly basis. It also contains employers' sectors and identification numbers of business unit (BEID), which we can use to determine whether the employment contract is renewed with the same employer or a successive employer[11]. Since SPOLISBUS only has the record for the month when people have an employment contract, we supplement information for the other months by merging the secondary monthly administrative data set SECMBUS, which provides monthly socio-economic category (SECM), such

[11] For example, a supermarket chain with N stores spread across the country and the head office in municipality X shares the same BEID. However, by using the BEID, we do not account for the case of mergers and acquisitions or the case when an employee changes his responsibilities within the same business unit.

as self-employed, unemployment benefits receiving or being a student, derived from people's income sources.

The third data set used is Dutch Labor Force Survey (EBB), which is a rotating panel with five quarterly surveys under a sample of around 53,000 households, within which people of 15 years and older are interviewed. It provides information on people's age, gender, education level and immigration status, which are used for control variables in the following empirical analysis. After combining EBB data from 1996 to 2018, and merging with SPOLISBUS and SECMBUS, we obtain 2,650,758 individuals, who are of 15 years and older, with complete employment history from 2010 to 2018 and known personal characteristics. The data after merging is a subsample of 15% of the total population in the Netherlands.

4.1. Sample Selection Criteria

Given the data availability, our sample includes the temporary contracts that terminate between January 2013 and December 2016 and build up all the chains of corresponding temporary contracts. The available sample includes all temporary contracts that started after January 2010, which is fully exogenous. Then, we check for each temporary contract how long the chain the employee has accumulated and when the current temporary contract ends. For temporary contracts ending before January 2013, their lengths of the accumulated chain could be left-censored. In addition, the new chain rule is effective in July 2015 which is more than 2.5 years far from any dates before Jan 2013, thus, both employees and employers were less likely to endogenously adjust the termination date around Jan 2013 due to the policy reform. Therefore, our sample section is less likely endogenous.

For each month during this period, we first pick each temporary contract ending at this month and make it as the initial chain. Then we trace back this employee's working history to see how long this chain can be accumulated retrospectively [12]. For the current chain ending before July 2015, if we observe that there is another temporary contract signed by the same parties (employee and employer) before this chain, with a possible interval less than 3 months, we sum up the length of the initial chain, the other temporary contract and the interval as the length of the new chain. Then we trace back again until this chain cannot be accumulated further[13]. For the current chain ending from July 2015 onwards, the length of a possible interval can be relaxed as 6 months if the interval ends after July 2015[14]. According to the chain rule, both before and after the policy reform, if the number of temporary contracts in the chain is more than 3, the last temporary contract will automatically become a permanent contract. To identify the effect of the length restriction in the chain

[12]The SPOLISBUS starts from January 2010, so we have enough data to trace 3-year history before the ending date if the temporary contracts terminate from January 2013.

[13]If a chain consists of three temporary contracts with interruptions in one employee's working history, we denote the three contracts as C1, C2 and C3. In our sample, we treat C1, C1+C2, and C1+C2+C3 as three observations (also three hypothetical chains) in the data and estimate their transition rates to a permanent contract. The last temporary contract in these three chains is C1, C2 and C3, respectively. So C1 and C2 can be the last temporary contract in the two hypothetical chains: C1 and C1+C2. They can also move to the state TS.

[14]See appendix A for the detailed description.

rule, we only keep the chain in which the number of temporary contracts is no more than 3[15].

We restrict our sample of temporary contracts to be signed with private companies, thus excluding all the temporary contracts in public sectors such as government, public education, academic hospitals, military and defence. The reason is that a special collective labour agreement (CAO), from which both the old and the new chain rules are exempted, is much more prevalent in public sectors than in private companies. Since we cannot identify which temporary contract in the data is subject to such a CAO, we exclude temporary contracts in public sectors to reduce the influence from a special CAO as much as possible. We also exclude temporary contracts that are signed with temporary agency companies, since they are not regulated by the chain rule (see a summarized selection criteria on SPOLISBUS in Table A1). Because there is substantial heterogeneity in the nature of jobs and the individual characteristics of employees for the jobs between part-time and full-time [16], We first restrict our attention to the chains whose last temporary contract is classified as full-time [17]. In the robustness check of Section 7, we also conduct a similar analysis on the sample including the chains whose last temporary contract can be either part-time or full-time. de Graaf-Zijl et al. (2011) also mention that some temporary jobs in the Netherlands have an explicit agreement to convert into an open-ended contract in case of good performance. Since the information of these agreements is not available in our data, we cannot exclude its effect in our later empirical analysis.

The CBS open data[18] shows that there were about 7.4 million employees in 2016 in the Netherlands. Meanwhile, the 2016 CAO report[19] indicates that about 5.6 million employees were covered by a collective labor agreement (CAO), which is about 75.8% of total employees. By examining 90 CAOs with an expiry date on or after December 31 2015, which covers about 85% of employees under CAOs, the report shows that 20 CAOs do not contain any agreements about the chain determination; 28 CAOs refer to the law (implicit agreements); 35 CAOs have explicit agreement in accordance with the new chain rule; 5 CAOs (7% of the employees involved in the sample) are in accordance with the old chain rule; 2 CAOs (4% of the employees involved in the sample) contain agreements that deviate from the chain provision for all employees. Therefore, the percentage of the employees to whom the new chain

[15]According to the chain rule, a temporary contract would automatically be converted to a permanent contract if the accumulated chain's length reaches the limit or the number of temporary contracts within the chain is more than 3. In our raw data, there are some rare cases in which the employees can form a chain of more than 3 temporary contracts, which do not conform to the chain rule. We believe these cases are due to a particular contract provision (e.g., CAO) and thus should be excluded from our sample.

[16]Farber (1999) shows that job losers who find employment in temporary jobs are more likely to be working full-time, while nonlosers who are employed in temporary jobs are more likely to be working voluntarily part-time.

[17]The part-time contract can also be counted in the chain, as long as its working hours are no less than 12 hours per week. Here, we distinguish whether a chain belongs to the type of part-time or full-time by the type of its last temporary contract.

[18]Data source: CBS open data - Employment; economic activity, quarterly, National Accounts

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

rule does not apply is roughly 9.8%[20]. The existence of CAOs 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 CAO report, since the percentage is only about 10%, the downward bias can be controlled within a small range.

By summing up the monthly basic salary for both temporary contracts and intervals in a chain and dividing it by the total length of the chain in month, we calculate the monthly average salary for each chain and classify it into five levels, while excluding the chains whose monthly average salaries are below 500 or above 10000. To have complete information on people's age and education level, we keep individuals whose ages were between 18 and 60 in 2010 and who completed their highest education between 1960 and 2010.

4.2. Descriptive Statistics

Table 1 reports the descriptive statistics for the sample used in the following empirical analysis. The first column refers to the whole sample of the chains within which the last temporary contract ends between January 2014 and November 2016. The second column refers to the sample of the chains within which the last temporary contract ends before July 1, 2015, the date of the policy reform in the chain rule, and the third column refers to the sample of the chains within which the last temporary contract ends after July 1, 2015. Note that if the chain consists of two temporary contracts, the first temporary contract is also counted as a chain and included in the sample. Similarly, if the chain consists of three temporary contracts, the first temporary contract is counted as a chain, and the first two temporary contracts constitute another chain[21]. As shown in Table 1, most of chains in the sample consist of only one temporary contract. Since women in the Netherlands take more part-time jobs than men, around three quarters of the employees having temporary contracts are male after part-time contracts are excluded from our sample. Comparing the percentages in the different columns, we can conclude that there is not much difference in the distributions of the chains of temporary contracts and the individuals before or after the policy reform.

Table 2 presents the average percentage of destinations after a temporary contracts terminates. 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[22]. For the different lengths of chains the employees have accumulated when their current temporary contracts terminate, we compare the percentages of destinations before or after the policy reform. As shown in Table 2, the percentages of conversion from chains of temporary contracts to permanent contracts with the same employers, also known as stepping stone effect, increase with the length of the

[20] ((7%+4%)/85%*75.8%)

[21] It means that there are some dependence among multiple chains for each employee. In Section 5, we account this dependence by using the clustered standard errors in the estimation.

[22] The rest includes head leader of big stockholders (DGA), receiver of assistance allowance, social allowance benefit, sickness benefit or pension payment, becoming a student, and other forms of self-employment.

chains. Moreover, these percentages also increase a lot after the policy reform in the chain rule, where the differences are all significant if we conduct t-tests on the equality of means. Meanwhile, after the policy reform, the percentages of leaving for temporary contracts with different employers do not increase, while the percentages of becoming unemployed benefit receivers decline significantly. All the evidence may indicate that the policy reform in the chain rule could strengthen the stepping stone effect.

5. Empirical Strategy

We estimate the effect of the policy reform on the chain rule using an RD design with a linear probability model. From July 1, 2015, the restriction on the length in the chain rule changes from 36 months to 24 months. We use this policy reform to estimate the effects of shortening the total length of the chains of temporary contracts on the probability of converting to a permanent contract. We only focus on the policy effect on the chains of temporary contracts whose lengths are between 1 and 35 months [23]. Figure 5 displays the descriptive evidences of the effect of the policy reform on the probabilities of converting from a chain of temporary contracts to a permanent contract with the same employer. They are categorized into six groups based on the different lengths of the chains the employees have accumulated when their current temporary contracts terminate. The vertical axis represents the percentages of employees who sign permanent contracts with the same employers after their current temporary contracts terminate. As the figure shows, for the employees who have accumulated the chains with the lengths between 12 and 17 months and between 18 and 23 months, the percentage increases significantly after the policy reform. For the employees who have accumulated the chains with other lengths, the percentage does not change significantly [24].

According to the transitional law, if the temporary contract stops after the July 1, 2015, the new chain rule applies. If the temporary contract stops before the July 1, 2015, the old chain rule applies. Therefore, when analyzing the reform's effect, we treat the first one as the treatment group and the second one as the control group. To valid this RD design, we are in need of the assumption that the employers do not deliberately choose the ending date of the temporary contract. 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 the robustness check, we try to justify the assumption by checking the distribution of the contracts ending dates and the distribution of the lengths of the renewal contracts before and after the policy

We implement an RD design with the date as the running variable and July 1,

[23] As shown by the robustness check in Section 7, only a small fraction of chains have lengths longer than 36 months. We believe that these observations could come from some imperfect compliance by employers or purely measurement errors.

[24] There is a January effect in the data: the percentage of signing permanent contracts with the same employer is much higher in January than it in the other months of the year. We control for the January effect by including month dummy in the empirical analysis and also check the sensitivity of our results when excluding the temporary contracts ending in January in section 7.

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 policy reform on the the temporary contracts that are signed before this date, even if they terminate after the threshold. Therefore, for the chains within which the last temporary contracts are signed before July 1, 2015 and stop just before or after the discontinuity date, their accumulated lengths should be exogenous to the policy reform. When two chains have the similar accumulated length, we can treat the one within which the last temporary contract stops just before the discontinuity date as the control group, and the one within which the last temporary contract stops just after the discontinuity date as the treatment group. Thus, our RD design will estimate the policy effect on their next move.

We specify a uniform kernel and use a bandwidth of 1.5 year on each side of the policy reform date: using the chains within which the last temporary contract ends in between January 2014 and June 2015 as the data before the policy reform and the chains within which the last temporary contract ends in between July 2015 and November 2016 as the data after the policy reform. The sensitivity check of bandwidth are conducted in Section 7.

Suppose that in the sample a chain 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 move into one of the following states S_{ijt}: TD, TS, PD, PS, EN and UB, defined in Table 2.

Let $p_{ijt}^{(PS)}$ and $p_{ijt}^{(PD)}$ denote the probability that the last temporary contract in the chain 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 last temporary contract moves into PS or PD, respectively. The effect of policy 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 chain's length is between 12 and 23 months, and $D_{ijt}^{(24)}$ is the dummy variable that is equal to 1 if the chain's length is no less than 24 month [25]. The dummy variable for the policy reform, $D_{ijt}^{(p)}$, takes the value 1 if the last temporary contract in the chain ends after the policy

[25] Note that we restrict our attention to the chains whose length are less than 36 months, so there is no dummy variable for the length which is no less than 36 months.

reform and 0 otherwise. Therefore, the parameter α_1 estimates the baseline effect of the policy reform on the chain whose length is between 1 and 11 months, $\alpha_1 + \alpha_2$ estimates the effect of the policy reform on the chain whose length is between 12 and 23 months, and $\alpha_1 + \alpha_3$ estimates the effect of the policy reform on the chain whose length is between 24 and 35 months. The threshold c denotes the month of July 2015 when the new chain rule takes effect. The variable T_{ijt} denotes the month when the last temporary contract in the chain ends, which is our running variable in the RD design. The functions $f(\cdot)$ and $g(\cdot)$ are polynomials of our decentralized running variable $(T_{ijt} - c)$.

The control variables $\mathbf{X}_{ijt}$ include a set of characteristics of temporary contracts and individuals. Considering different sectors of employment may have different probabilities of converting to a permanent job, we control for the dummies of sectors, which are classified into eight groups: culture, financial & economic, industrial, IT, government, transport, health care and construction. For the individuals, we control for their age, gender, immigration status and education level. According to Gagliarducci (2005), the probability of moving to a permanent job while employed on a temporary basis decreases if there are interruptions, so the number of interruptions consisted of in a chain is also controlled in $\mathbf{X}_{ijt}$. The chain's monthly average salary is controlled after being classified into four groups, since it represents heterogeneous qualities of the temporary contracts and the employee's abilities. In addition, we also control for the seasonal effect by including yearly dummies and monthly dummies. The unobserved effects include the firm-specific fixed effect ν_j and the idiosyncratic effect ε_{ijt}. Since some individuals may have multiple chains and there is also dependence across the renewed temporary contracts in a chain, we use clustered standard errors [26] to correct standard errors for the correlated unobserved individual effects.

6. Main Results

Table 3 presents the parametric estimates in our model (1) for the probabilities of converting from a chain of temporary contracts to a permanent contract with the same employer (PS) or with a different employer (PD). At this stage, we first consider the RD design where the polynomial choices of $f(\cdot)$ and $g(\cdot)$ are of order one and the bandwidth choice is 18-month with uniform kernel. We will leave other specifications of polynomial choices and bandwidth choices in the following section for robustness check. The models in columns (1) and (4) only contain the dummy variables for the chains' length and the policy reform, and also the first-order polynomial of the decentralized running variable. Based on these two specifications, the columns (2) and (5) incorporate all the observed control variables, and the columns (3) and (6) additionally control for the unobserved firm-fixed effect.

As shown in the first three columns, the estimates of α_1 are all negative and only significant at the 5% level in the specification of column (2). The estimates of α_2 are all positive with similar magnitude and are all significant at the 1% level.

[26]For the method to estimate clustered standard errors in the maximum likelihood estimation, see e.g. [P]robust, particularly the section for maximum likelihood estimators, in the STATA manual.

The F-tests of $\alpha_1 + \alpha_2 = 0$ are all rejected with 1% significance level, indicating that the policy reform do have significant effect on the converting probabilities of the chains whose lengths are between 12 and 23 months. The estimates of α_3 are all positive and only significant 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 chains whose lengths are between 24 and 35 months could be ambiguous.

The column (3) of Table 3 shows that, for the employee who has accumulated a chain of temporary contracts with a length less than 12 months, the policy reform has no significant effect on the probability of converting to a permanent contract with the same employer. However, for the employee who has accumulated a chain of temporary contracts with a length between 12 and 23 months, the policy reform significantly increases the converting probability by 4.2% ((-0.0175)+0.0595). For the employee who has accumulated the chain of temporary contracts with the length between 24 and 35 months, the policy reform increases the probability by 3.17% ((-0.0175)+0.0492), but the F-test shows that this effect is not significant.

The column (3) also shows that before the policy reform, for the employee who has accumulated a chain of temporary contracts with a length between 12 and 23 months, the probability of converting to a permanent contract with the same employer is 15.8% larger than the probability for an employee who has accumulated a chain with length less than 12 months. For the employee who has accumulated a chain of temporary contracts with a length between 24 and 35 months, the probability of converting to a permanent contract with the same employer is 26.0% 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.

The column (2) of Table 3 shows that having a temporary contract renewed one more time decreases the converting probability by 6.13% with 1% significance level. However, such effect become insignificant in the column (3) when the firm-fixed effect is controlled. Gagliarducci (2005) argue that the probability of converting from a temporary to a permanent contract increases with the duration of the contract but decreases with repeated temporary jobs. One explanation is that the employees can accumulate firm-specific human capital during the job, so the more time they work in the temporary contract, the more likely they will stay in the company and be offered a permanent contract. However, the number of renewed temporary contracts may be a bad signal to the employee, since the more temporary contracts the company offers repeatedly, the more hesitated it will be to offer a permanent contract. Note that such effect could also be confounded with the fact that some types of jobs offer shorter temporary contracts much often and require a higher rate of renewal, and that their probabilities of moving to permanent contracts are generally low. Such confounding bias is confirmed by our results, if we compare the column (2) with the column (3). Moreover, being a female decreases the converting probability by 1.41% with 1% significance level in the column (2). However, this effect become insignificant in the column (3) if we also control for the firm-fixed effect. Being an immigrant decreases the converting probability by 2.90% with 1% significance level.

Although the new chain rule only applies when the employee wants to renew a contract with the same employer, we are interested in investigating whether signing a permanent contract with a different employer(PD) would provide a channel for the stepping stone effect or not. One possibility is that the increasing rate of renewing a permanent contract with the same employer (PS) may be due to the improved labor market environment or the employers' more optimistic attitude. In this case, we would observe that the transition rates to both PS and PD increase. Another possibility is that the policy plays its role and boosts only in the PS channel, and has no impact on the PD channel.

As shown in the column (6), the empirical results on PD is different from the one on PS . First, the estimates of α_1, α_2 and α_3 are all insignificant, which confirms our conjecture that the policy reform has no significant effect on this channel. Second, the probability of converting to a permanent contract with a different employer decreases with the duration of the chain but increases with the number of repeated temporary jobs within the chain, quite opposite to the findings for converting to a permanent contract with the same employer.

The different effects of the policy reform can be explained as follows. For the employee who has accumulated the chain of the temporary contract in less than one year, the initial employer can freely sign a renewed temporary contract, which will not make the new chain rule come into effect. In this case, the policy reform has little effect on these groups of employees and does not affect the probabilities for them to be offered permanent contracts. For the employee who has accumulated a chain of temporary contracts that is more than one year but less than two years long, the employer could freely offer another one-year temporary contract without activating the chain rule before the reform. However, after the reform, the employer needs to make decision on whether to offer a permanent contract, or to offer a much shorter temporary contract, or to stop hiring. The empirical evidence in Table 3 suggests that the employer is more likely to offer a permanent contract instead of another temporary contract. One explanation is that while temporary contracts are used by the employers as a "buffer stock" to adjust to economic fluctuations and avoid labour market inflexibilities, they also function as a device for screening employees' abilities. If screening can be completed within a series of temporary contract of no more than one year and employers decide to continually hire that person, they are more willing to forego the benefit from "buffer stocks" and offer a permanent contract directly when the policy reform asks them to make decision one year sooner. In fact, given the process of learning about match quality estimated by Pries (2004), Faccini (2014) argues that it takes more than seven months, on average, to discover the productivity of a worker, and that the probability that the quality of a match is still unknown after two years is only about 3.5%.

For the employees who have accumulated a chain of temporary contracts that is more than two years but less than three years long, the employers have two choices before the policy reform if they want to retain the employees thereafter: to offer a temporary contract of less than one year followed by a continued permanent contract or to directly offer a permanent contract. After the policy reform, however, the employers cannot choose the first option. The empirical results show that the policy reform only affects the employers' choices slightly.

It means that before the policy reform, for the employees who have accumulated a chain of temporary contracts with the length between two and three years, their abilities have been fully observed and their employers can already make decisions on whether to retain them in a long term. If the answer is yes, employers were mostly willing to offer permanent contracts directly instead of very short renewed temporary contract: retaining their productive employees with permanent contracts while forgoing the very small benefit from "buffer stocks" that are less than one year. For this type of employees, the policy reform has no effect, since their employers will offer permanent contracts anyway. However, if the answer is no, employers may still want to acquire some small benefit from a "buffer stock" by offering a short-term temporary contract. However, since employers' willingness to offer permanent contracts are very low, even after the policy reform that requires making decisions one-year sooner, these employees are less likely to obtain permanent contracts. It also means that for these groups of employees, renewing another short-term temporary contract does not enhance their opportunities to be hired with permanent contracts, since their abilities have been fully observed within two years of temporary contracts.

We also estimate the transition probabilities to other states, including the temporary contract with different employers (TD), temporary contracts with the same employer, unemployed benefit receiving (US), and entrepreneurs(EN) to fully understand the policy effect. The estimation results are shown in Table 4

The transition rates to other states (TD,TS, EN) are not significant after the treatment, while it appears a negative impact on the UB (significant at 5%). That implies that the new chain rule overall is not re-shaping the employment on other states, while decreasing the probability of becoming unemployed. One possible explanation is that the new chain rule pushes up the transition probability to permanent contracts, where at the same time vacates some short-term positions and that creates more opportunities to unemployed people.

7. Robustness Check

In this section, we run a number of robustness tests to justify the consistency of the main results and the validation of econometric models specified in the paper. To validate the RD setting, we first test the smoothness of control variables. We draw the evolution figures on control variables and find that they are smooth across the cutoffs, where we include the confidential intervals at 95% significant levels (see Figure 6, Figure 7, and Figure 8). These results confirm that our RD setting is valid.

Then we provide the robustness check for the choice of bandwidth in the RD design. Table 5 presents the estimation results when different numbers of months before or after the policy reform are used. They are comparable with the results in the columns (2) and (3) of Table 3. The column (1) presents the estimation results when a triangular kernel and a bandwidth of 18 months before or after the policy reform are used in the RD design. The columns (2)-(6) present the estimation results when a uniform kernel and a bandwidth of 12, 6, 5, 4, 3 or 2 months before or after the policy reform are used in the RD design. When more than two months are chosen as the bandwidth, the effects of the policy reform on the employees who have accumulated chains between 12 and 23 months have the similar magnitude and the F-tests indicate that they are all significant. It also suggests that there is no significant evidence that anticipation effect exists two months before the effectiveness of the policy reform. Otherwise, we would see a decreasing magnitude of the policy effect as we shrink the bandwidth of the data. When 2 months are chosen as the bandwidth, the F test indicates that this policy effect is less significant, but it still has the right sign and similar magnitude: increasing the converting probability by 5.19% (0.0179 + 0.0340). It may suggest that there is some anticipation effect for the temporary contracts which terminate just before the policy reform. During these two months ahead, instead of utilizing the old rule, some employers are inclined to apply the new rule and become more willing to offer a permanent contract directly if they want to retain the employee who has accumulated the chain between 12 and 23 months.

Table 6 explores different specifications regarding the polynomials of the decentralized running variable, $f(\cdot)$ and $g(\cdot)$. The results are comparable with the ones in the column (2) and (3) of Table 3 where a bandwidth of 18 months and a uniform kernel are assumed. In the columns (1)-(3) where firm-fixed effect are not controlled, the estimates of α_2 or α_3 are all significant and have similar magnitude, and the F-test on $\alpha_1 + \alpha_2 = 0$ are all rejected, whenever a second-order or a third-order polynomial is assumed. In the columns (4)-(6) where firm-fixed effect are controlled, we have the same conclusion excerpt that the F-test on $\alpha_1 + \alpha_2 = 0$ cannot be rejected when a second-order or a third-order polynomial is assumed. Note that none of the coefficients on the second-order or the third-order terms in the polynomials are significant. We believe that the specification with a first-order polynomial is the most appropriate one, and that higher-order polynomials will exacerbate the precision of estimates and thus make the F-test fail.

Table 7 conducts two placebo tests on the timing of the policy reform. The results are comparable with the ones in the column (4) of Table 5 where a bandwidth of 5 months is assumed. Since there is a strong seasonal effect on the probability of moving from temporary contracts to permanent contracts, it is necessary to make sure that the change in July 2015 is due to the policy reform instead of the monthly effect in July of each year. In the columns (1) and (2), we construct a dummy variable as if there is a policy reform in July 2014 and use the data of the temporary contracts ending in between February and November of 2014. In the columns (3) and (4), we construct a dummy variable as if there is a policy reform in July 2016 and use the data of the temporary contracts ending in between February and November of 2016. Both the point estimation and F-tests show that there is no significant effect of the pseudo policy reform on the probabilities of renewing a permanent contract with the same employer. Therefore, it can be concluded that the policy effect we found previously are not due to a particular monthly effect in July.

Note that our sample is obtained from merging the main administrative data with the labour force survey. To test whether it is still representative, we also estimate the model 1 by using the administrative data only. Table 8 shows the estimation results. In the columns (1)-(3), the estimates of α_1, α_2 and α_3 show the same sign and similar magnitude compared with the estimates in Table 3. The estimates of α_2 and α_3 in the columns (4)-(6) 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 of α_1 could be a little biased towards the young people whose age are below 18 or the elders whose age are over 60.

The salary is potentially endogenous to contract duration. To rule out the impact of this potential endogenous correlations on main results, we run the main regressions without controlling for salary and the coefficients of the key parameters of interests are not impacted significantly(Table 9).

8. Heterogeneous Effect

The policy reform may have heterogeneous impact on the young and old. We check this by adding age, age square, and their interactions with the dummy for policy reform in the regression (1). However, as shown in the column (1) of Table 10, the coefficients of the interaction terms are not significant. In the column (2) of this table, we add dummies for age groups and their interactions with the dummy for the policy reform. Whereas, the coefficients of the interaction terms are not significant, either. In the column (3) of this table, we further add interaction terms with the dummies for the different length groups. As the estimation results show, for the chains whose length is less than 12 months, we find no significantly heterogeneous policy effect on different age groups. However, for the chains whose length is between 12 and 23 months, we find a significantly larger policy effect on the younger workers whose age are between 18 and 24. For the chains whose length is between 24 and 35 months, we find a significantly larger policy effect on the younger workers whose age are between 18 and 34. These results also confirm the argument of Jovanovic (1979) that a worker-firm match is an "experience good" rather than an "inspection good". When screening employees' abilities through temporary contracts, employers cannot make quicker decisions simply by evaluating the employees' abilities from their past working experience, so the policy change does not have significantly larger effects on the elder employees.

The new chain rule may have heterogeneous effect both on employees with different levels of education and different sectors. To address these issues, we add dummies of education levels and sectors to check for heterogeneity (Tables 11 and 12). Indeed, we don't find any heterogeneous eduction effect overall when we don't distinguish employees according to the length of chains(see column (2) in Table 11). When dividing the groups according to chain lengths, we observe that there is also no significant heterogeneous eduction effect if chain lengths are less than 24 months. Whereas, when focusing on chain lengths of 24 to 35 months, the policy reform has a positive effect on low and medium education employees but doesn't change the transition probabilities significantly on high educated employees (see F-test in Table 11). One possible explanation is that it is more difficult to screen highly educated workers who may do complex tasks than low educated workers. Shortening the screening time will not help high-educated employees to get permanent contracts more quickly.

As mentioned in the Section 3.2, the chain rule also applies to the employment contracts with the employees who have worked no less than 12 hours per week, so we also check the estimation results when part-time temporary contracts with no less than 12 working hours per week are included in the sample. As shown in the Table 13, the main estimation results are similar to the ones when only full-time temporary contracts are considered (Table 3). For the employee who has accumulated a chain of temporary contracts with the length between 12 and 23 months, the policy reform increases the probability of converting to permanent contracts with the same employer by 3.73% ((-0.0184)+0.0557) with 1% significance level. However, for the employee who has accumulated a chain of temporary contracts with the length between 24 and 35 months, the policy reform significantly increases the probability of converting to permanent contracts by 3.60% ((-0.0184)+0.0544).

9. Anticipation Effect

In this section, we want to examine whether there are any anticipation effect of the policy, which might distort the estimates in the main results. To rule out this possibility, we first verify the possible change in the density of contract ending and origination dates near July 1 2015.

Figure 9 depicts the total number of temporary contracts signed with a new employer in each month in the sample. The left-hand side of the vertical red line presents the months before the policy reform, while the right-hand side of the red line presents the months after the policy reform. The figure shows that the total number of newly started temporary contracts is usually the highest in January and the lowest in December among a year. Moreover, the total numbers of temporary contracts starting from July to November of 2015 are similar to the total numbers of temporary contracts starting from February to June of 2015. Similar pattern can be found in 10 which draws the contract ending dates around the 1 Jul 2015. We can't observe any sharp changes around the policy date in Figures 9 and 10, implying that employers might not intendedly modify the contract ending and origination dates prior to or post the policy date.

Figure 12 presents the monthly percentage of signing a temporary working contract after receiving unemployed benefits. The vertical red line represents the month when the policy reform takes effect. As shown in the figure, we find no evidence that the chance for the unemployed person to obtain initial temporary contracts becomes much lower after the policy reform.

In the following, we run a linear regression that defines initial hiring as the dependent variable(mean 22.89%). The effect of policy reform on $p_{ijt}^{(TC)}$ can be estimated through the following RD design:

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

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 significant estimates in Table 14 do not justify the conjecture of reducing new hiring. Although the implementation of the new policy shortens the learning time for employers, it doesn't discourage new hires.

In the previous section, we only focus on the effect of the policy reform on the probability of moving to the permanent contract after a temporary contract terminates. It would be plausible that the policy reform also affects the lengths of the renewed contracts offered by the employers. Firstly, because the new chain rule could be foreseen from 2014 but only applies to the temporary contracts that are signed after 1st July 2015, the employers who only want to offer temporary contracts might offer a longer renewed temporary contract before 1st July 2015 to utilize the old rule as much as possible.Secondly, since the requirement on the chains' length in the chain rule is shortened by one year after the policy reform, the employers could be more willing to offer a shorter renewed temporary contract after 1st July 2015 if they want to avoid the new policy.

We test this hypothesis by checking the difference in the empirical cumulative distribution functions (CDFs) of the length of the renewed contract offered by the same employer after a temporary contract terminates before or after the policy reform. Figure 11 plots the CDFs based on different lengths of the chains they have accumulated. The blue lines and orange lines draw the CDFs before and after the policy reform, respectively. The dash lines indicate that the sum of the renewed contract's length and the previous chain's length already exceeds the length requirement for the chain rule to come into effect (36 months before the policy reform and 24 months after the policy 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 dash 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 or after the policy reform. As shown in Table 15, we find no significant difference in the length distributions of the renewed temporary contracts before or after the policy 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 more willing to offer a much longer renewed temporary contract before the policy reform or a much shorter one after the policy reform is not supported by the data (Table 16). Nevertheless, due to the limitation of our data, we cannot fully observe the renewal if there is no interruption between the two contracts.

The intention of the policy reform in the chain rule is to improve the legal position of flexible workers and discourage the improper and prolonged use of flexible employment relationships. However, under the more restricted rule, it would also be plausible that employers become more prudent to offer temporary contracts and that the chances for young people or unemployed workers to start career with temporary contracts become much lower.

10. Conclusion

This paper studies how the recent policy reform of tightening the length restrictions on the renewed temporary contracts affects the probability of moving to permanent contracts, known as the stepping stone effect. The empirical evidence shows that the policy reform can strengthen the stepping stone effect, although it may have heterogeneous effects on the employees who have accumulated the chain of temporary contracts with different lengths. The policy reform has no significant effect on the employee who has accumulated a chain in less than 12 months. Nevertheless, for the employee who has accumulated a chain between 12 months and 23 months, the policy reform significantly accelerates the stepping stone effect, increasing the probability of moving to permanent contracts by 4.2%. For the employee who has accumulated a chain between 24 months and 35 months, the policy reform increases the probability by 3.17%, which is not statistically significant. Given the existing data, there is no evidence that the employers are more willing to offer a shorter renewed temporary contract after the policy reform.

Besides, we find no evidence that the chances for the unemployed persons to obtain initial temporary contracts become lower due to the policy reform. These results imply that for the employees who have accumulated a chain of temporary contracts with a length between one and two years, the more restricted chain rule forces the employer to sign a permanent contract much faster without interrupting the functioning of temporary contracts as a screening procedure. Under the new policy, if employers decide to continually hire their employees, they are more willing to forego the benefit of using another temporary contract as a “buffer stock” and offer a permanent contract directly. The employees who have accumulated a chain of temporary contracts with a length between two and three years are more likely to be offered permanent contracts directly even before the policy reform if their employers want to retain them in the long run. Because their abilities have been fully observed within two years of temporary contracts and there is little benefit left from another very short “buffer stock. For the other employees whose employers do not want to retain in a long period, renewing another short-term temporary contract does not enhance their opportunities to be hired with permanent contracts. Therefore, the policy reform that restricts employers from renewing another less than one-year temporary contract after a chain of more than two-year temporary contracts has no significant effect on the stepping stone effect.

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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

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

Destination (in %)	Length of the Accumulated Chains: 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.

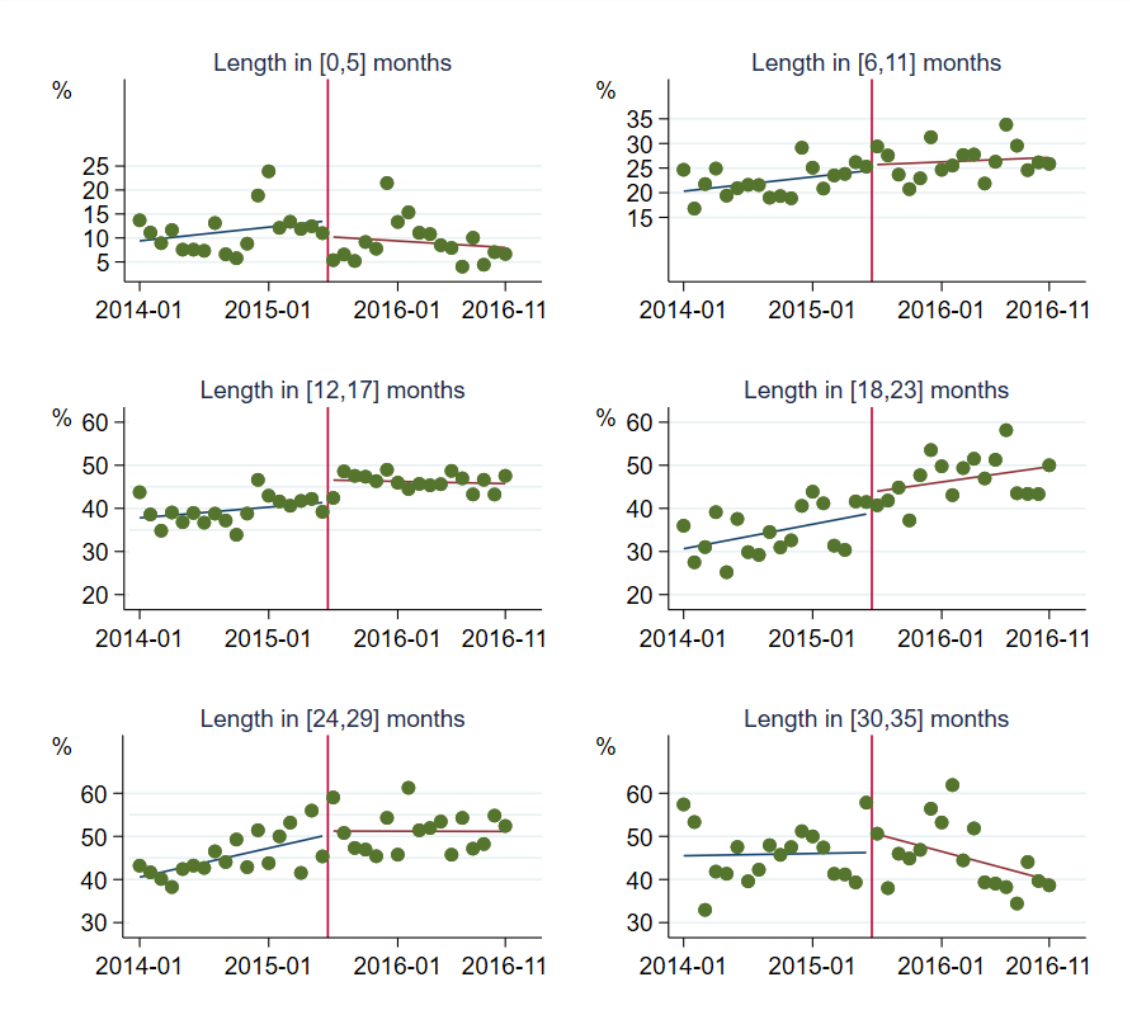

Figure 5: Discontinuity of the Stepping Stone Effects at the Policy Reform

Note: These figures draw the percentages of employees who renew permanent contracts with the same employers after their current temporary contracts terminates. They are categorized into six groups based on the different lengths of the chains the employees have accumulated when their current temporary contracts terminate. The vertical red line represents July 2015, the month when the policy reform takes effect.

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
$Length_{12\rightarrow 23}$	0.209***	0.170***	0.158***	-0.00196	-0.00784**	-0.00710
	(0.00608)	(0.00602)	(0.00982)	(0.00392)	(0.00399)	(0.00654)
$Length_{24\rightarrow 35}$	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)
$Length_{12\rightarrow 23}\times$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)
$Length_{24\rightarrow 35}\times$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
$Length_{12\to 23}$	-0.0347***	-0.0747***	-0.00595	0.000277
	(0.00615)	(0.0101)	(0.00878)	(0.00294)
$Length_{24\to 35}$	-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)
$Length_{12\to 23}\times$Post Reform	-0.0142*	-0.0116	-0.0259**	-0.00625
	(0.00805)	(0.0135)	(0.0114)	(0.00389)
$Length_{24\to 35}\times$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×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.

Figure 6: Test the Smoothness of Control Variables

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis in Figure 12 shows the monthly percentages of different dummy variables .

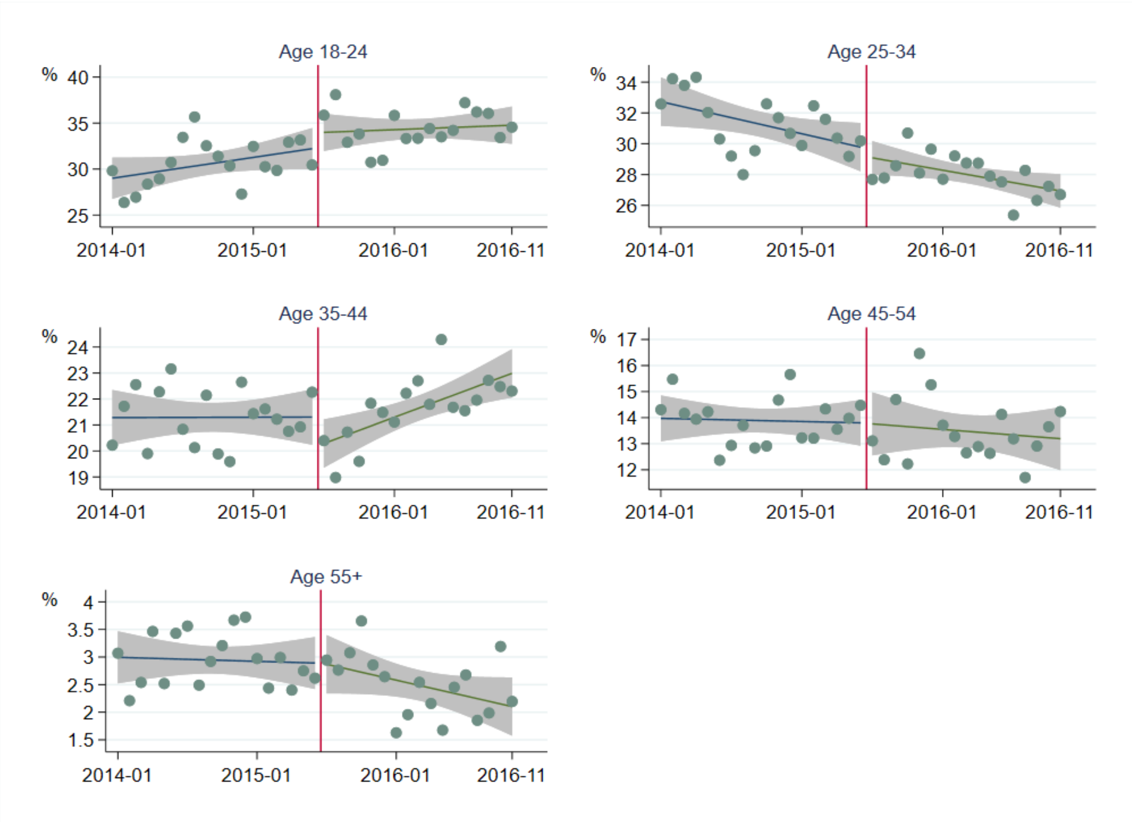

Figure 7: Test the Smoothness of Control Variables

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis in Figure 12 shows the monthly percentages of different age groups.

Figure 8: Test the Smoothness of Control Variables

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis in Figure 12 shows the monthly percentages of employees with different levels of salary.

Table 5: 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	(7) 2 months
$Length_{12\to23}$	0.157***	0.169***	0.180***	0.179***	0.171***	0.175***	0.181***
	(0.0109)	(0.00725)	(0.0110)	(0.0119)	(0.0131)	(0.0152)	(0.0185)
$Length_{24\to35}$	0.266***	0.250***	0.256***	0.266***	0.257***	0.249***	0.269***
	(0.0146)	(0.00956)	(0.0147)	(0.0159)	(0.0176)	(0.0205)	(0.0244)
Post Reform	-0.0158	-0.0234**	-0.0189	-0.0161	-0.0167	-0.0308	0.0179
	(0.0150)	(0.0101)	(0.0145)	(0.0163)	(0.0183)	(0.0221)	(0.0298)
$Length_{12\to23}$×Post Reform	0.0566***	0.0687***	0.0539***	0.0578***	0.0578***	0.0548***	0.0340
	(0.0144)	(0.00980)	(0.0142)	(0.0160)	(0.0178)	(0.0206)	(0.0253)
$Length_{24\to35}$×Post Reform	0.0505***	0.0376***	0.0215	0.0110	0.0211	0.0311	0.0144
	(0.0193)	(0.0133)	(0.0190)	(0.0219)	(0.0244)	(0.0281)	(0.0341)
Month	0.00259*	0.00344***	-0.00276	0.00495	0.00784	0.0148*	-0.0143
	(0.00146)	(0.000956)	(0.00288)	(0.00374)	(0.00517)	(0.00833)	(0.0166)
Month×Post Reform	-0.00424*	-0.00340**	0.0147***	-0.00757	-0.0168**	-0.0215*	0.0106
	(0.00238)	(0.00135)	(0.00372)	(0.00522)	(0.00724)	(0.0116)	(0.0233)
# Interruptions	-0.00220	-0.0589***	-0.0571***	-0.0464***	-0.0401***	-0.0326**	-0.0411**
	(0.00990)	(0.00701)	(0.0102)	(0.0117)	(0.0132)	(0.0154)	(0.0184)
Female	-0.00937	-0.0125**	-0.0233***	-0.0188**	-0.0149	-0.0220*	-0.0192
	(0.00840)	(0.00538)	(0.00766)	(0.00872)	(0.00968)	(0.0113)	(0.0139)
Immigrant	-0.0271**	-0.0226***	-0.0244**	-0.0162	-0.0156	-0.00907	-0.0154
	(0.0122)	(0.00802)	(0.0114)	(0.0127)	(0.0144)	(0.0167)	(0.0202)
Constant	0.140***	0.157***	0.140***	0.142***	0.137***	0.147***	0.109***
	(0.0235)	(0.0120)	(0.0177)	(0.0199)	(0.0223)	(0.0271)	(0.0358)
Control for							
Education, Age, Sector	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Average Salary	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Monthly Dummy	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Yearly Dummy	Yes	No	No	No	No	No	No
N	55,021	39,381	19,615	14,378	11,648	8,711	5,843
F-test							
$\alpha_1 + \alpha_2 = 0$	5.854	15.84	4.481	5.066	3.890	0.941	2.470
p-value	0.0155	6.91e-05	0.0343	0.0244	0.0486	0.332	0.116
$\alpha_1 + \alpha_3 = 0$	2.759	0.956	0.0146	0.0472	0.0275	0.000103	0.638
p-value	0.0967	0.328	0.904	0.828	0.868	0.992	0.424

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 6: Sensitivity Checks: Different Polynomial Choices in the RD Design

	(1)	(2)	(3)	(4)	(5)	(6)
Order of Polynomials	1	2	3	1	2	3
$Length_{12\rightarrow 23}$	0.170***	0.170***	0.170***	0.158***	0.158***	0.158***
	(0.00602)	(0.00602)	(0.00602)	(0.00982)	(0.00982)	(0.00982)
$Length_{24\rightarrow 35}$	0.246***	0.246***	0.246***	0.260***	0.260***	0.260***
	(0.00797)	(0.00797)	(0.00797)	(0.0132)	(0.0132)	(0.0132)
Post Reform	-0.0182**	-0.0311*	-0.0161	-0.0175	-0.0173	-0.0337
	(0.00889)	(0.0171)	(0.0240)	(0.0145)	(0.0271)	(0.0378)
$Length_{12\rightarrow 23}\times$Post Reform	0.0669***	0.0671***	0.0671***	0.0595***	0.0596***	0.0595***
	(0.00819)	(0.00819)	(0.00819)	(0.0130)	(0.0130)	(0.0130)
$Length_{24\rightarrow 35}\times$Post Reform	0.0347***	0.0348***	0.0349***	0.0492***	0.0493***	0.0492***
	(0.0113)	(0.0113)	(0.0113)	(0.0178)	(0.0178)	(0.0178)
# Interruptions	-0.0613***	-0.0612***	-0.0612***	-0.00900	-0.00902	-0.00900
	(0.00593)	(0.00593)	(0.00593)	(0.00913)	(0.00913)	(0.00913)
Female	-0.0141***	-0.0142***	-0.0142***	-0.0114	-0.0114	-0.0114
	(0.00451)	(0.00451)	(0.00451)	(0.00768)	(0.00768)	(0.00768)
Immigrant	-0.0243***	-0.0243***	-0.0242***	-0.0290***	-0.0290***	-0.0291***
	(0.00667)	(0.00667)	(0.00667)	(0.0111)	(0.0111)	(0.0111)
Constant	0.199***	0.212***	0.189***	0.145***	0.150***	0.174***
	(0.0134)	(0.0191)	(0.0317)	(0.0221)	(0.0309)	(0.0504)
$Month$	0.00235***	0.00527*	-0.00630	0.00277**	0.00339	0.0160
	(0.000790)	(0.00296)	(0.0128)	(0.00128)	(0.00470)	(0.0202)
$Month\times$Post Reform	-0.00148	-0.00388	0.0128	-0.00431**	-0.00600	-0.0243
	(0.00129)	(0.00456)	(0.0206)	(0.00207)	(0.00718)	(0.0326)
$Month^2$		0.000147	-0.00137		2.57e-05	0.00168
		(0.000144)	(0.00163)		(0.000228)	(0.00258)
$Month^2\times$Post Reform		-0.000183	0.000448		3.34e-05	-0.000650
		(0.000266)	(0.00124)		(0.000416)	(0.00195)
$Month^3$			-5.35e-05			5.85e-05
			(5.73e-05)			(9.07e-05)
$Month^3\times$Post Reform			9.09e-05			-9.97e-05
			(0.000111)			(0.000177)
Observed Controls	Yes	Yes	Yes	Yes	Yes	Yes
Firm-fixed Effect	No	No	No	Yes	Yes	Yes
N	55,021	55,021	55,021	55,021	55,021	55,021
F-test						
$\alpha_1+\alpha_2=0$	23.28	4.114	4.326	7.121	2.343	0.456
p-value	1.40e-06	0.0425	0.0375	0.00762	0.126	0.500
$\alpha_1+\alpha_3=0$	1.693	0.0368	0.534	2.655	1.127	0.147
p-value	0.193	0.848	0.465	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 7: Placebo Test for July 2014 and July 2016

	Jul 2014		Jul 2016	
	PS	PD	PS	PD
$Length_{12\to23}$	0.165***	-0.0257***	0.245***	-0.00705
	(0.0118)	(0.00826)	(0.0113)	(0.00727)
$Length_{24\to35}$	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)
$Length_{12\to23}\times$Post Reform	-0.0186	0.0174	-0.0133	-0.00316
	(0.0160)	(0.0110)	(0.0153)	(0.00969)
$Length_{24\to35}\times$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×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 8: 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
$Length_{12\to23}$	0.218***	0.175***	0.147***	-0.00131	-0.00570***	-0.00867***
	(0.00167)	(0.00165)	(0.00180)	(0.00105)	(0.00107)	(0.00120)
$Length_{24\to35}$	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)
$Length_{12\to23}\times$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)
$Length_{24\to35}\times$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×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 9: Sensitivity Checks: Exclude Salary in the Control Variables

	(1) PS	(2) PS	(3) PS	(4) PS (Tri)
$Length_{12\to23}$	0.209***	0.200***	0.177***	0.176***
	(0.00607)	(0.00604)	(0.00980)	(0.0109)
$Length_{24\to35}$	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)
$Length_{12\to23}$×Post Reform	0.0616***	0.0658***	0.0574***	0.0538***
	(0.00845)	(0.00832)	(0.0131)	(0.0144)
$Length_{24\to35}$×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. *** Denotes significance at the 1% level, ** denotes significance at the 5% level and * denotes significance at the 10% level.

Table 10: Table for Age Effects

	(1) PS	(2) PS	(3) PS
Post Reform	0.00831	-0.0327	0.0132
	(0.0637)	(0.0328)	(0.0367)
$Length_{12\rightarrow 23}$×Post Reform	0.0595***	0.0596***	-0.0298
	(0.0130)	(0.0130)	(0.0513)
$Length_{24\rightarrow 35}$×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×$Length_{12\rightarrow 23}$			0.118**
			(0.0526)
Age(25-34)×Post Reform×$Length_{12\rightarrow 23}$			0.0823
			(0.0531)
Age(35-44)×Post Reform×$Length_{12\rightarrow 23}$			0.0740
			(0.0538)
Age(45-54)×Post Reform×$Length_{12\rightarrow 23}$			0.0692
			(0.0554)
Age(18-24)×Post Reform×$Length_{24\rightarrow 35}$			0.123*
			(0.0684)
Age(25-34)×Post Reform×$Length_{24\rightarrow 35}$			0.117*
			(0.0691)
Age(35-44)×Post Reform×$Length_{24\rightarrow 35}$			0.0970
			(0.0702)
Age(45-54)×Post Reform×$Length_{24\rightarrow 35}$			0.0477
			(0.0720)
Other Controls	Yes	Yes	Yes
N	55,021	55,021	55,021

Table 11: Table for Educational Effects

	(1) PS	(2) PS	(3) PS
Post Reform	-0.0177	-0.0129	-0.0232
	(0.0144)	(0.0163)	(0.0170)
$Length_{12\to23}$×Post Reform	0.0592***	0.0598***	0.0755***
	(0.0130)	(0.0130)	(0.0182)
$Length_{24\to35}$×Post Reform (γ_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)×Post Reform		-0.00529	0.00688
		(0.0132)	(0.0157)
Edu (High)×Post Reform		-0.0116	0.0115
		(0.0159)	(0.0195)
Edu (Medium)×Post Reform×$Length_{12\to23}$			-0.0221
			(0.0202)
Edu (High)×Post Reform×$Length_{12\to23}$			-0.0267
			(0.0242)
Edu (Medium)×Post Reform×$Length_{24\to35}$			-0.0201
			(0.0273)
Edu (High)×Post Reform×$Length_{24\to35}$(γ_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 12: 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)
$Length_{12\to23}$×Post Reform	0.0781***	0.0775***	0.144***
	(0.0101)	(0.0102)	(0.0505)
$Length_{24\to35}$×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×Post Reform		0.0294	0.0682**
		(0.0312)	(0.0337)
S3×Post Reform		0.0339	0.0836**
		(0.0316)	(0.0345)
S4×Post Reform		0.0151	0.00686
		(0.0598)	(0.0822)
S5×Post Reform		0.0600	0.196**
		(0.0651)	(0.0949)
S6×Post Reform		0.00507	0.0398
		(0.0311)	(0.0335)
S7×Post Reform		0.0346	0.112***
		(0.0382)	(0.0426)
S8×Post Reform		-0.00388	0.106
		(0.0916)	(0.118)
S2×Post Reform×$Length_{12\to23}$			-0.0599
			(0.0515)
S3×Post Reform×$Length_{12\to23}$			-0.0785
			(0.0521)
S4×Post Reform×$Length_{12\to23}$			0.000754
			(0.0984)
S5×Post Reform×$Length_{12\to23}$			-0.149
			(0.112)
S6×Post Reform×$Length_{12\to23}$			-0.0587
			(0.0513)
S7×Post Reform×$Length_{12\to23}$			-0.136**
			(0.0612)
S8×Post Reform×$Length_{12\to23}$			-0.354**
			(0.145)
S2×Post Reform×$Length_{24\to35}$			-0.145*
			(0.0791)
S3×Post Reform×$Length_{24\to35}$			-0.164**
			(0.0797)
S4×Post Reform×$Length_{24\to35}$			-0.0593
			(0.139)
S5×Post Reform×$Length_{24\to35}$			-0.356***
			(0.136)
S6×Post Reform×$Length_{24\to35}$			-0.121
			(0.0790)
S7×Post Reform×$Length_{24\to35}$			-0.192**
			(0.0938)
S8×Post Reform×$Length_{24\to35}$			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.

Table 13: Parametric Estimates for Transitions to PS and PD Including Part-time Jobs

	(1) PS	(2) PS	(3) PS	(4) PD	(5) PD	(6) PD
$Length_{12\to23}$	0.206***	0.165***	0.152***	-0.0138***	-0.0154***	-0.0181***
	(0.00441)	(0.00435)	(0.00659)	(0.00316)	(0.00319)	(0.00482)
$Length_{24\to35}$	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)
$Length_{12\to23}$×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)
$Length_{24\to35}$×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.

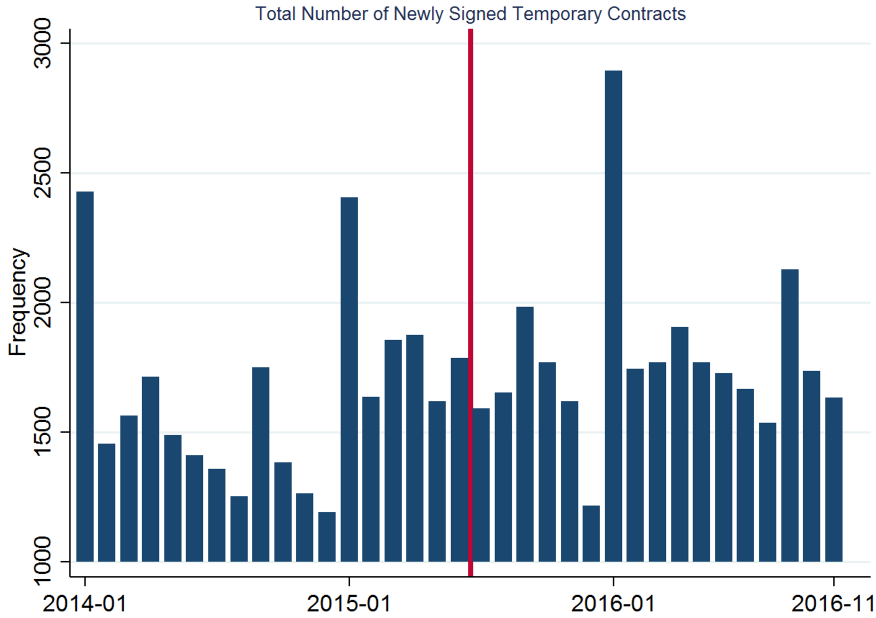

Figure 9: The Frequency of Newly Signed Temporary Contracts

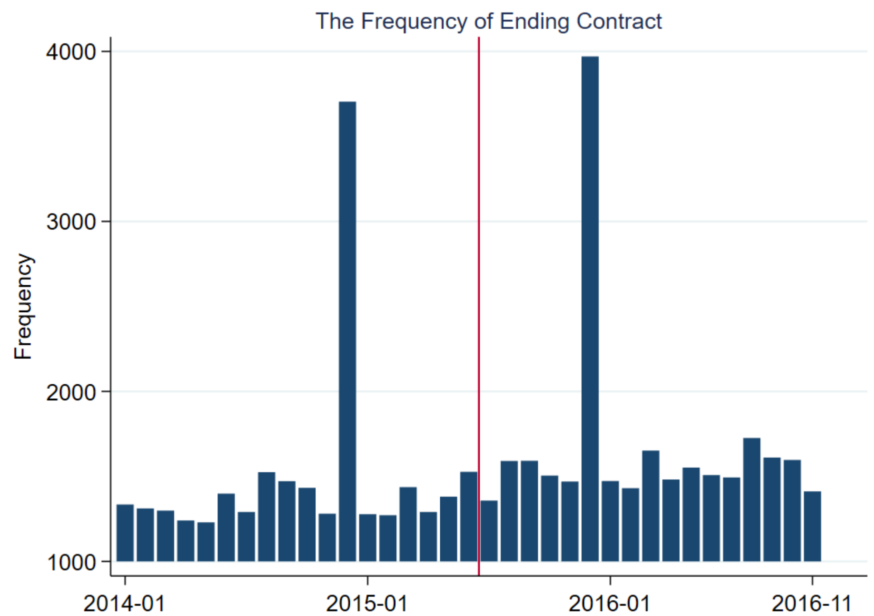

Figure 10: The Frequency of Ending Contracts

Note: The vertical red line represents the date when the policy reform takes effect.

Table 14: 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.

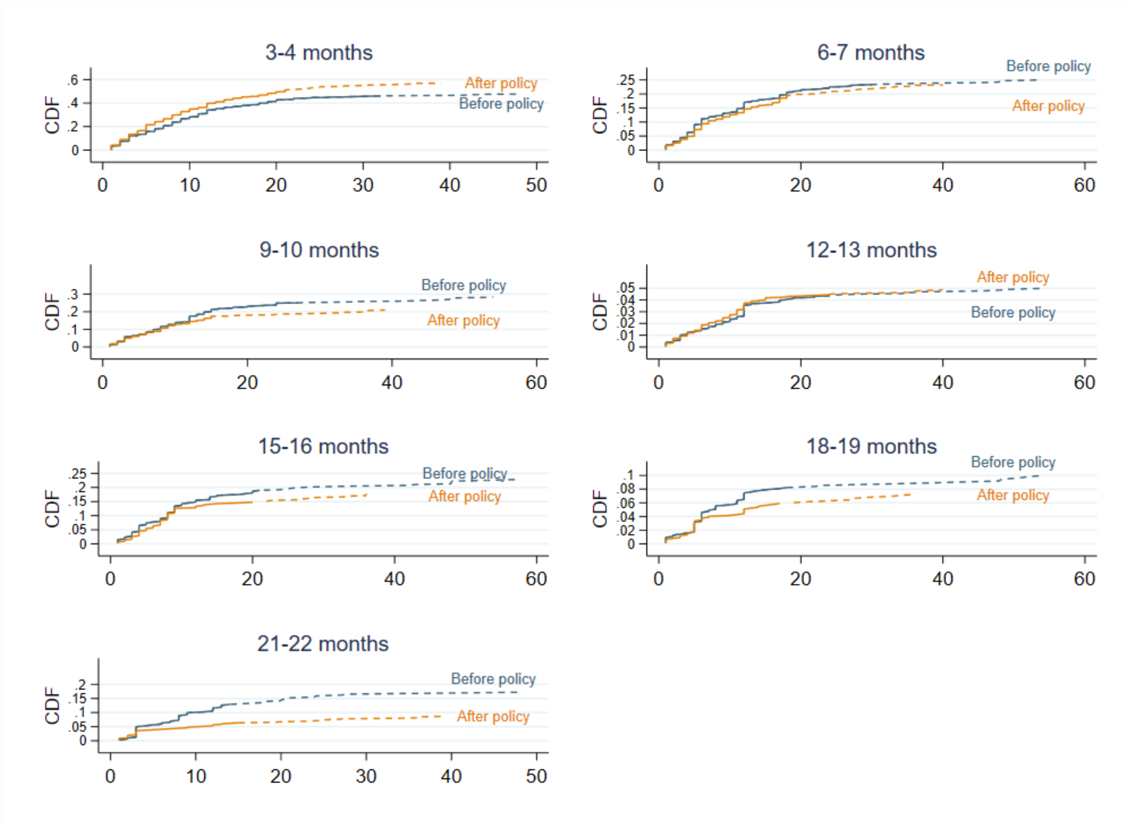

Figure 11: The Empirical CDF of the Length of the Renewed Contract

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 subfigure presents the CDF of different lengths of contracts. The blue line and orange line draw the CDFs before and after the policy, 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.

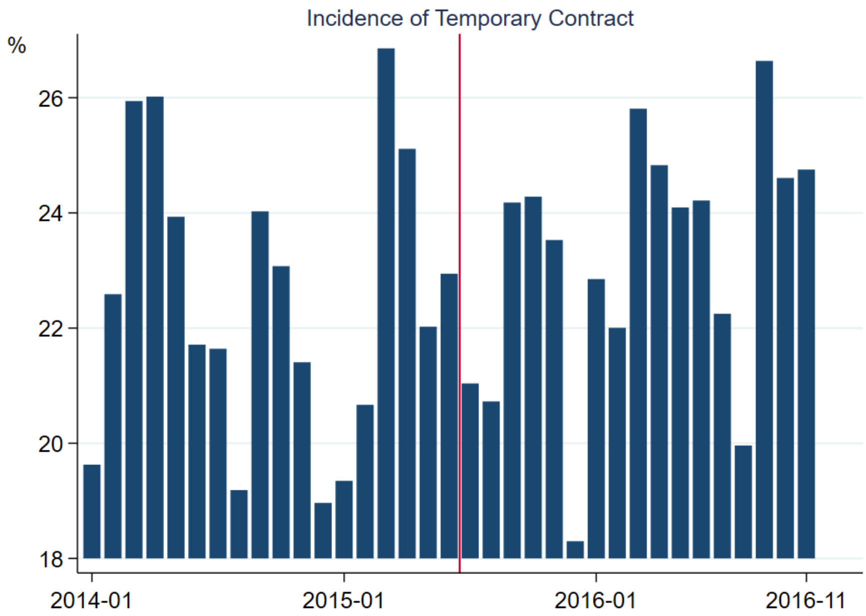

Figure 12: Incidence of Temporary Contracts

Note: The vertical red line represents the date when the policy reform takes effect. The vertical axis in Figure 12 shows the monthly percentage of signing a temporary working contract after receiving unemployed benefits.

Table 15: 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 16: Parametric Estimates for the Policy Effect on the Lengths of Renewed Contracts

	(1)	(2)	(3)	(4)
$Length_{12\to23}$	1.069	1.026	1.059	1.014
	(0.737)	(0.742)	(0.739)	(0.746)
$Length_{24\to35}$	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)
$Length_{12\to23}$×Post Reform	-0.990	-0.995	-0.928	-0.915
	(0.922)	(0.925)	(0.915)	(0.920)
$Length_{24\to35}$×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×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 A1: 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
SPOLISDIENSTVERBAND	Full-time or part-time employment	1	Full-time

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.