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ABSTRACT

Does Employment Protection Unprotect Workers? The Labor Market Effects of Job Reinstatements in Peru*

We investigate the labor market effects of the reestablishment of private-sector workers' right to reinstatement for unfair dismissals, which occurred in 2002 in Peru. Using data from Peruvian Household Surveys from 2004 to 2015, and the Specialized Employment Survey 1998-2001, we estimate a quasi-experimental difference-in-difference model. We find that this reestablishment is associated with increases in new contracting in the private sector, by 5.9 % for permanent hiring and 3.0 % for temporary hiring. By means of placebo tests, we only find a causal effect of the reinstatement on temporary hiring, not on permanent hiring. We also find a negative association between reinstatements and real wages of 3.9 %, but placebo tests indicate that this is not a causal effect. Our findings call into question the effectiveness of removing reinstatement laws as a policy to increase permanent hiring and wages.

JEL Classification: J23, J65, E24

Keywords: labor costs, employment, fixed-term contracts

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1 Introduction

In 2002, the Peruvian Supreme Court ruled in favor of the right to the reinstatement of workers who were dismissed unfairly. We estimate the effects of this ruling on permanent and temporary hiring and wages. We follow a quasi-experimental design that identifies a treatment group of workers in the private sector, who were affected by the ruling, and a control group of workers in other sectors, who were not affected, mainly public-sector workers with other hiring regimes. We find that the ruling was associated with a yearly increase of 3.0% of temporary contracts, 5.9% of permanent contracts, and a decline of real wages by 3.9%. However, only the increase in temporary hiring is plausibly causal, while the increase in permanent hiring and the decline in wages are not.

These small and mostly non-causal findings should be understood in the context that the Supreme Court’s reinstatement ruling was a very specific change in employment protection legislation,¹ not a comprehensive labor market reform, such as the reform in Colombia studied by Kugler (2004). These findings cast serious doubts on alleged perverse effects of employment protection legislation and on removing reinstatement laws as an effective policy tool to improving labor market conditions.

This is, of course, a very controversial matter. The top firm executives surveyed by the Global Competitive Index (Rendon and Vera, 2019) express their generalized concern about reinstatement laws and employment protection legislation overall. By stark contrast, more comprehensive and representative surveys, like the 2015 National Enterprise Survey (*Encuesta Nacional de Empresas*),² suggest that reinstatement laws only influence permanent hiring patterns for 31% of Peruvian firms.

The economic literature is not consensual regarding the unemployment effects of employment protection legislation. A series of studies finds that employment protection reduces firings at the expense of the perverse effect of lowering employment creation or increasing unemployment (Lazear, 1990; Heckman and Páges-Serra, 2000; Botero et al., 2004). However, Addison and Grosso (1996) and Kanbur and Ronconi (2018) find that the methods applied in those studies are not sufficiently robust. Moreover, Autor et al. (2006) suggest that the real employment effect of employment

¹Employee protection against dismissals rooted in their lack of productivity or any disciplinary incident was unchanged. Additionally, in theory, employers could block the possibility of a reinstatement by providing fired workers with a clear motive behind their dismissal.

²This survey by the National Institute of Statistics and Informatics of Peru is designed to represent small and large firms alike.

protection is lower than it was previously attributed. In the same line, Kugler and Pica (2008) find that a comprehensive labor reform in Italy reduces the probability of becoming employed of between 1 and 2.6 percentage points. Cahuc et al. (2016) also find small employment effects of employment protection for the French labor market, and, instead, a significant substitution of permanent for temporary contracts, who are not entitled to severance payments, are less productive, enjoy less job satisfaction, wages, and human capital accumulation.³ Similarly, Schivardi and Torrini (2008) and Hijzen et al. (2017) find a positive relationship between employment protection and temporary hiring in the Italian labor market. Employment protection has small effects on employment, but it has substantial effects on increasing temporary hiring at the expense of permanent hiring. These findings are established based on comprehensive variations of employment protection systems, such as comparisons between countries with different labor laws or full labor market reforms within a country. In comparison, the reestablishment of reinstatements in Peru in 2002 was very specific and short-scoped labor law variation.

The effects of employment protection in Peru have been recently revisited by Jaramillo (2019) who delves into the very same case study considered in this document. This author finds large and significant drops in permanent hiring, real wages and unionization rates after the ruling, which he attributes to the reestablishment of reinstatement laws. Our study differs from his in the selection of the treatment and control groups. Jaramillo (2019) estimates a double difference model with formal workers as the treatment group and informal workers as the control group, and the treatment date is assumed to be 2001. In Appendix C, we elaborate on some important threats to the validity of this identification strategy.

We organize the rest of this document as follows. The next section describes the institutional framework and the legal changes introduced by the Supreme Court's 2002 ruling regarding job reinstatements. Section 3 describes the data; Section 4 details our identification strategy. Section 5 discusses the results and presents several robustness checks. In Section 6, we develop counterfactual simulations of the number of temporary and permanent contracts that would have been observed in the absence of the ruling, based on our previous results. In Section 7, we address the potential wage effects of the ruling. Finally, Section 8 presents our main conclusions.

³See Booth et al. (2002), Dolado et al. (2002), Cingano et al. (2014) or Hijzen et al. (2017) for more details on these effects.

2 Institutional Framework

Fixed-term or temporary contracts have been legal in Peru since 1970, albeit their use was heavily restricted to jobs that were temporary in nature, with intense government supervision. In the 1990s, many employment protection mechanisms and previous restrictions to temporary hiring were drastically eased (RM 430-90-TR). The government eliminated job stability for future hiring by the extension of just causes for dismissals and introduced a severance payment regime whereby fired workers were entitled to a month's salary for each tenure year with a minimum payment of 3 months and a maximum of 12 (Rodríguez Gamero 2011). In 1995, the government expanded severance payments to all contracts, including those that began prior to this date (DL 26513) (Saavedra and Maruyama 2000). In 1996, severance payments increased slightly, to one-and-a-half month's salary per tenure year (DL 871). By 1997, temporary contracts were exempted from government approval, their maximum length was prolonged from 3 to 5 years, and their use was extended to more cases, such as task-specific hiring or hirings made by newly established firms.

In 2001, a group of around 500 unionized workers of *Telefónica del Perú* filed a Supreme Court lawsuit against their employer for being fired unfairly; that is, for causes unrelated to their productivity or their behavior.⁴ In July 2002, the Supreme Court ruled against the firm, allowing workers the option of reinstatement in their former jobs instead of receiving a severance payment (*Expediente N° 1124-2001-AA/TC*). This ruling in practice reestablished job reinstatements and annulled the previous regime, which only allowed for severance payments in cases of dismissal without expression of just cause (*General Labor Law, DL 728, Art. 34*).

This change explicitly affected only employees and firms in the private sector. The Supreme Court adopted a different set of criteria for workers outside the private sector (Farfán, 2011). In the public sector, severance payments and other employment protection mechanisms are far more restrictive than in the private sector. For instance, severance payments and job reinstatements are only an option for public sector workers who were hired for their jobs by public contests and who are dismissed as part of properly registered mass layoffs (Ugaz and Galicia 2015).⁵ Firing public

⁴The ruling that solved this lawsuit considered that the workers' unionization rights had also been violated, as most dismissed workers were union members (Neves 2015).

⁵An interested reader may resort to SERVIR's webpage for a comprehensive list of all regulations that govern labor relations within the public sector. It should be noted that under the job reinstatement category, all laws are specific in their scope: They only apply in case of mass layoffs. The original list of laws in Spanish can be retrieved from Servir (2017).

servants is only possible under specific conditions such as committing violent acts, negligence, and drug abuse, and job reinstatements are not allowed for at least 5 years after the layoff and never allowed for workers in the education sector (DL 276). Moreover, in 2010, and again in 2013, the Supreme Court ruled against the reinstatement of public sector workers hired under Service Administrative Contracts (*Contratos Administrativos de Servicios* or CAS). According to Servir (2017), 21% of public sector workers are hired under the DL 276, and 22% of workers are working under a CAS, implying that these workers have no right to reinstatement if fired unfairly.⁶ Only public employees who were hired under private sector regimes, 13% of all the public workforce were affected by the reinstatement ruling.

In sum, after the labor market flexibilization from the 1990s, the Supreme Court's 2002 ruling recognized and enforced for the first time the constitutional right of workers to be reinstated for unfair dismissals. This established a legal precedent in the private sector, without any implications for public sector workers. Yet, although job reinstatements are exceptionally infrequent events,⁷ they may affect permanent and temporary hiring, a possible implication that we test empirically in the next sections of this paper.

3 The Data

We use data from the National Household Survey (*ENAHU* or *Encuesta Nacional de Hogares*, in Spanish) from 2004 to 2015 and its predecessor, the Specialized Employment Survey (*ENIVE*, *Encuesta Especializada de Niveles de Empleo*, in Spanish) from 1998 to 2001. These surveys have been conducted periodically by the National Institute of Statistics and Computing since 1997 and consist of a nationally representative sample of approximately 30,000 different households each year, which feature demographic information (age, sex, educational attainment) and a detailed set of employment related questions, such as wages, contract type, tenure, and hours of work. Our sample includes urban wage earners who report having some form of contract signed after 1997. Our sample allows us to distinguish among three broad types

⁶Here 44% of public servants includes workers hired under special regimes like diplomats, school-teachers, and college professors, among others, who are not affected by the reinstatement ruling since their labor relations are not governed by the modified DL 728.

⁷In Peru, there are around 162 reinstatement requests by year (Jaramillo, 2019), while there are around 2,332,218 firms (INEI, 2018). Thus, in a given year only, 0.0069% of firms are expected to face a reinstatement request.

of contracts: permanent contracts, temporary contracts, and others. The final category includes screening period contracts, youth training contracts, apprenticeship contracts, professional service contracts, and special contracting regimes.

We retrieve the starting date of each contract by subtracting the tenure in months from the survey date. Thereby, we are able to recover information for the years 2002 and 2003 for which the questions regarding an employer’s contract type were not asked. Furthermore, we include a set of macroeconomic variables, including GDP growth and the value of exports, and match them to our main dataset by the contract start date. We provide further details on variable definitions in Appendix A.

[Figure 1 goes here.]

In Figure 1, we show total amounts for each type of hiring regime over time: permanent, temporary, and others. The steady rise in temporary hiring is evident since the 1990s (Pascó-Font and Saavedra 2001). On the one hand, Cuadros (2018) suggests that this increase is explained by the ease of temporary hiring, while on the other hand, some authors have suggested, without econometric evidence, that the actual cause lies in the increase of employment protection for permanent contracts in 1996 (Saavedra and Maruyama 2000).⁸ For his part, Jaramillo (2019) asserts that the expansion of temporary hiring is rooted in the reestablishment of reinstatement laws in 2002. In what follows, we will attempt to rigorously determine whether there is a causal relationship between reinstatement laws and the observed expansion of temporary hiring.

4 Identification Strategy

In this section, we propose an identification strategy based on defining groups of workers who were affected by the ruling as treatment groups and unaffected workers as a control group. Since Ashenfelter and Card (1985), this strategy has been adopted extensively to evaluate the labor market effects of a wide array of policies, from minimum wage hikes (Card and Krueger 1994) to the effects of changing the degree or the structure of employment protection regimes (Kugler 2004, Autor, Donohue and Schwab 2006).

⁸As previously stated, this increase in employment protection featured a modest increase in severance payments after their decrease in the early 1990s. Severance payments still remained lower than in the pre-Fujimori era.

The specification of our identification strategy is contained in the following equation:

$$y_i = \beta_0 + \beta_1 A_i + \beta_2 T_i + \beta_3 A_i \times T_i + X' \delta + F' \gamma + M' \alpha + \epsilon_i, \quad (1)$$

where y_i is an outcome variable like wages or a latent variable associated with a binary variable that takes the value of 1 when worker i was hired under a permanent (or a temporary) contract, A_i is a dummy variable that is equal to 1 whenever a workers' employment relationship began in July 2002 or after, T_i is another dummy variable that indicates whether worker i works in the private sector; that is, it denotes the treatment group. Subsequently, workers outside this sector, those with labor market regimes that do not allow reinstatements after unjustified dismissals, are defined as the control group. Namely, this group consists on public servants, special service firm workers, worker cooperative affiliates, and members of the armed forces or the police.

Coefficient β_3 that multiplies the interaction term $A_i \times T_i$ represents our difference in difference (diff-in-diff) estimator. We also have a vector of demographic control variables X (sex, age, educational attainment, and geographical domain), a vector F of firm-specific features (number of workers and economic sector), and a vector of macroeconomic variables M (year fixed-effects, GDP growth, and value of exports growth).

In order to recover unbiased estimates of β_3 , we require the fulfillment of two key assumptions. First, it is crucial that the treatment and control groups share a common pre-treatment trend β_1 . In case this assumption, commonly known as “parallel trends assumption,” is not met, β_3 would be confounding the treatment effect with preexisting trend differences. Analogously, if the ruling treatment alters self-selection patterns between treatment and control groups, by, for example, attracting more productive workers to the treatment group, estimates of β_3 would not disentangle the pure treatment effect from the changed mix of workers' productivities.

5 Results

In Table 1, we present the number of contracts by sector, a descriptive diff-in-diff of the effects of the ruling on the distribution of workers by contract type.

[Table 1 goes here]

We observe that the ruling coincided with an increase in the permanent hiring of around 98,000 new permanent contracts yearly, which represents an increase of 11.77

percentage points in the relative importance of this type of contract. Meanwhile, temporary hiring also increased by 463,000 or 3.96 percentage points. Conversely, we see that other forms of hiring decreased both in absolute and relative terms. Overall, we see that employment grew by 522,000 new jobs each year for the treatment group.

[Table 2 goes here]

In Table 2, we compare the sociodemographics of both groups before and after the ruling. The differences in observable attributes do not change substantially after the ruling, which suggests that self-selection patterns have remained unchanged. We can also observe, as already noted by Paz and Urrutia (2015), that real wages as well as average tenure declined in both sectors. There is no other notable compositional change, neither in terms of educational attainment nor in the spatial distribution of the Peruvian workforce. We also see, in Figure 2, that both permanent and temporary hiring fell before the ruling for both groups, evidencing the fulfillment of the parallel trend requirement.

[Table 3 goes here]

In Table 3, we report Probit estimation results for Equation (1). We show the effects of the ruling on permanent hiring on one side and its effects on temporary hiring on the other. This is the first study that directly quantifies the effects of this particular ruling on temporary hiring in Peru. We report the number of observations for each estimation, the average predicted pre-treatment probability of being hired under a permanent (or temporary) contract for the treatment group and the marginal effect of the interaction term $A \times T$ with its respective standard deviation. We follow this format throughout this entire document. We include control variables for sex, age, educational attainment, geographical domain, value of exports, GDP growth, economic sector, firm size, and year fixed effects.

The 2002 reinstatement ruling was associated with a 5 percentage point increase in the probability of being hired under a permanent contract in the short run, with a 7.1 percentage point increase in the medium run, and with an increase of 17.6 percentage points, in the long run. Overall, the ruling was associated with a 5.9 percentage point increase in the probability of starting a permanent employment relation. Notably, this effect has the opposite sign than the one reported by Jaramillo (2019).

As far as temporary hiring, we register a drop of 0.2 percentage point in the probability of being hired with a temporary contract in the short run, which is not

statistically significant. We also find an increase of 7.7 percentage points in the medium run and a 11.3 percentage point increase in the long run. All in all, the ruling was associated with an increase of 3 percentage points in the probability of starting a temporary labor relation. This result is strikingly similar to the range of 2 percentage points and 2.5 percentage points found by Hijzen et al. (2017) for the effects of a comprehensive employment protection regime change on temporary hiring in Italy.

Our results indicate that the ruling coincided with an increase of both temporary and permanent hiring, which suggests that these two types of hiring are not substitutes, but complements. There is a simultaneous increase in the likelihood of starting both temporary and permanent contracts compensated by a relative decrease in the use of other forms of hiring. Moreover, the ruling effect on permanent hiring is 5.9 percentage points, larger than its 3 percentage point effect on temporary hiring. We have also replicated this estimation specific subsamples defined by demographic profiles, educational attainment, firm size, and economic sector; see Appendix B. This disaggregation corroborates that the ruling’s association with permanent hiring is usually stronger than its association with temporary hiring.

As a robustness check, we perform a stricter definition of our control group to only workers who were definitely not affected by the reestablishment of reinstatements. We assign workers employed by private companies to the treatment group and restrict our control group to workers of the public administration or the armed forces. These workers, if fired, cannot be reinstated for at least five years, except those in the education sector for which firing is permanent (*Law of the Administrative Career and Wages of the Public Sector, DL 276, Art. 13*). Since 13% of public servants were hired under private sector laws (Servir 2017), sending workers hired by public companies, who may have been hired under private sector laws, to the treatment group aims to minimize the bias that may arise from having these possibly treated workers in the control group.

[Table 4 goes here]

In Table 4, we see that the marginal effects for our interaction term in this estimation are very similar to those of our main specification. The main coefficients remain positive and highly statistically significant, and their magnitude is higher for permanent hiring than for temporary hiring, 6.2 and 5.2 percentage points, respectively. This test corroborates that our results are not driven by an incorrect selection of treatment and control groups.

We assess whether our results are contaminated by the inclusion of part-time workers who are not eligible for severance payments or for reinstatements in case of being dismissed without expression of just cause (*General Labor Law, Art. 22*).

[Table 5 goes here]

Table 5 reports the results from reestimating our main model with a restricted sample of full-time workers.⁹ Once again, the main coefficients are positive, statistically significant, and their magnitude is higher for permanent hiring than for temporary hiring, 5.5 and 4.3 percentage points, respectively.

We also discard the possibility that including informal sector workers bias our results, because their noncompliance with the law sets them outside the scope of the reinstatement ruling, as Jaramillo (2019) asserts.

[Table 6 goes here]

Results from Table 6 of estimations with a restricted sample of only formal sector workers are in line with our main results. We see an increase in permanent hiring of 6.3 percentage points coupled with an increase of 1.9 percentage points in the probability of starting a temporary labor relation.

The results from Tables 3 to 6 corroborate that, if the ruling had a positive effect on temporary hiring, its magnitude would be very small, between 1.8 and 4.3 percentage points, and it would not be associated with a negative effect on permanent hiring, but with a positive effect. In that sense, we do not find evidence of a *perverse* effect of reinstatements.

Finally, in Table 7, we run a placebo test to verify whether our results reflect an underlying causal relationship between job reinstatements and hiring and are not just the result of a spurious correlation. To determine whether our estimates are capturing a pre-ruling trend, we assume that the ruling took place on different dates rather than the actual date.

[Table 7 goes here]

⁹To be more specific, we drop workers who do not report their weekly working hours or who report working less than 20 hours a week. This cutoff was chosen as the *General Labor Law* defines part-time workers as those who work 4 or less hours a day, which we approximate to 20 hours a week.

The signs of the coefficients for A in the placebo tests indicate that before the ruling there is a downward trend in permanent hiring as well as an upward trend in temporary hiring. Whereas the coefficient for the interaction term $A \times T$ for the 1998 placebo test is statistically significant for permanent hiring, it is not for temporary hiring. Hence, while our results for temporary hiring are robust to placebo testing, our results for permanent hiring are not. This implies that the reinstatement ruling indeed caused an increase in temporary hiring, but not necessarily an increase in permanent hiring.

6 Effects on Hiring Levels

Our estimation results allow us to calculate the counterfactual levels of permanent and temporary hiring that would have been observed if the treatment group had followed the same trend as the control group; that is, if the treatment group was not affected by the reinstatement ruling. We predict the probability of being hired under a permanent or a temporary contract, assuming that the interaction term $A \times T$ equates zero. Once we obtain the predicted probabilities for each time frame, we multiply it by the observed level of aggregate employment for each year. In our prediction, we abstract from possible aggregate employment effects and only consider effects on the distribution of new permanent and temporary contracts.¹⁰

These counterfactual trajectories would have occurred, if the reinstatement ruling had a causal effect; yet we should bear in mind that our previous results only suggest a causal relationship for temporary but not for permanent hiring.

[Figure 2 goes here]

In Figure 2, we can see that counterfactual permanent and temporary hiring levels are lower than their actual levels. Consequently, we can state that for 2010 the reinstatement ruling caused an increase in temporary hiring by 62,000 contracts and coincided with an increase of 172,000 permanent contracts, which cannot be attributed to the ruling.

¹⁰The descriptive evidence presented in previous pages suggests that the reestablishment of reinstatement laws was associated with an increase in aggregate employment.

7 Effects on Wages

In this section, we determine the effects of the reinstatement ruling on real wages. To that purpose, we estimate Equation (1) setting y_i equal to the natural logarithm of worker i 's wage, expressed in 2009 soles (Peruvian currency).

[Table 8 goes here]

In Table 8, we show descriptive diff-in-diff of the wage effect of the ruling. We observe that real average monthly wages in the private sector fell by 225 soles or 1%, with respect to other sectors. In Table 9, we test whether this result holds after controlling for workers' demographics, employer characteristics, and macroeconomic variables. We also evaluate the non-randomness of our results by assuming a false treatment date on year 2000.

[Table 9 goes here]

We see that the ruling was associated with a statistically significant drop of 3.9% in real wages. This association is smaller than the one found by Jaramillo (2019), whose single difference estimates point to a 28% drop in real wages allegedly caused by the ruling. Notably, the relatively low effect that we find is in line with previous research. Leonardi and Pica (2006) who found that a more comprehensive Italian labor market reform, which established workers' reinstatement rights and increased severance payments, decreased wages by approximately 2%. Our result is, however, not robust to placebo testing as we find a similar effect when we set an arbitrary year as the treatment date. Hence, there is a wage decline that coincides but is not caused by the 2002 reinstatement ruling, as we can see wage declines since at least the year 2000, as documented by Paz and Urrutia (2015).

8 Conclusions

We have estimated a diff-in-diff model of the labor market effects of the 2002 Peruvian Supreme Court ruling in favor of workers' reinstatements for unfair dismissals. We have exploited that this ruling only affected the private sector to define private sector workers as the treatment group and workers of other sectors, mainly public sector workers, as the control group.¹¹

¹¹Our main results also include workers hired by public companies as part of the control group. However, since it is possible for workers of these companies to be hired under private sector laws,

This quasi-experiment has shown a sizable and statistically significant increase in temporary and permanent hiring after the ruling. Temporary hiring in the private sector increased by 3 percentage points, which accounts to 172,000 jobs yearly. Whilst, permanent hiring also increased, by 5.9 percentage points, that is, 62,000 jobs annually. We also have found that the ruling is associated with a decline in real wages of 3.9%. These estimated effects are not only small, particularly when compared to those found by Jaramillo (2019), but also mostly non-causal correlations, reflective of a very specific and short-scoped change in labor market legislation, as it was the reinstatement Supreme Court’s ruling. We only have established a causal effect of the ruling on the increase in temporary hiring, but not on permanent hiring. We rather have shown evidence that the ruling was associated with an increase in permanent hiring within the private sector. We have found no evidence about perverse effects of employment protection, as represented by reinstatement laws, on the Peruvian labor market.

Our findings have the implication that removing reinstatement laws is not a policy that will foster permanent job creation or increase wages in Peru.

one of our robustness checks is designed to test the sensitivity of our results to transferring these workers to the treatment group instead; the results from this test are qualitatively identical and are very similar in magnitude to our main results.

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Appendix

In the present Appendix, we explain the definitions for the main variables used in our analysis. We also detail their sources when they are different than ENAHO or ENIVE, described in Section 3. We also replicate our main results for particular groups defined by demographic profile, educational attainment, age, firm size, and industry. Lastly, we describe and discuss an alternative quasi-experiment consisting of assuming that formal sector workers are the treatment group and informal sector workers are the control group.

A Definition of the Variables

- **Contract Type:** For this variable, we draw our data from questions “ocptipco” extracted from ENIVE and question “p551a” from ENAHO. A worker was hired under a permanent contract if he reported having a “open-ended contract” and a fixed-temporary contract, if he answered having a “fixed term contract.” Any other answer is re-coded as “others,” except from “no contract” and missing values, which are excluded from the analysis.
- **Contract Start Date:** For this variable, we subtract from the survey date, the number of years, and months that the worker has been working in the same job, as obtained from variables “p513a1” and “p513a2” from ENAHO, and variables “ocptiean” and “ocptieme” from ENIVE.
- **Business Formality Status:** For surveys conducted after 2004, we use the National Institute of Statistics and Computing the complete series for variable “emplpsec,” which determines, based on the adequate register with the tax authorities, the formality status of the firms in which surveyed workers reported working.
- **Labor Formality Status:** For surveys conducted after 2004 we asked the National Institute of Statistics and Computing to send us the complete series for variable “ocupinf”, for the previous years we construct an informality variable based on the same criteria described in *Cuenta Satélite de la Economía Informal*.
- **Private Sector:** A worker was in the private sector if he reported working for a private employer in questions “p510” or “ocptraba.” He was outside the private sector when he reported working for the Armed Forces, Police, Public Administration, a Public Company, a Workers’ Cooperative, or a Special Service Firm.
- **Quarterly GDP Variation:** This variable was extracted from the Peruvian Central Bank’s time series: <https://estadisticas.bcrp.gob.pe/estadisticas/series/trimestrales/resultados/PN03503MQ/html/1998-3/2015-4/>.
- **Monthly Export Value Variation:** This variable was extracted from the Peruvian Central Bank’s time series: <https://estadisticas.bcrp.gob.pe/estadisticas/series/mensuales/resultados/PN01448BM/html/1998-1/2015-12/>.

- Opinion on Reinstatement Laws: Retrieved from the National Enterprise Survey of 2015, question M8P7.
- Monthly Wage: Constructed from a series of variables that indicate the frequency and amount of each payment received by the worker as a wage. We only consider each worker’s main occupation. Monthly wages were deflated using the average Consumer Price Index for each year extracted from the following link: <https://estadisticas.bcrp.gob.pe/estadisticas/series/mensuales/resultados/PN01270PM/html/1998-1/2015-12/>.

B Results by Sociodemographic Group

Estimations for particular demographic groups are presented in Tables B1 and B2.

[Table B1 goes here]

[Table B2 goes here]

We find negative effects of the reinstatement ruling on permanent hiring for some demographic groups; yet none of these effects is statistically significant. Moreover, we detect particularly large and positive effects on fixed-term hiring for high school incompletes, 12.6 percentage points. For college incompletes for whom we find a positive effect of 12.5 percentage points. Conversely, we find no effects for workers with post-graduate education, for those aged between 56 and 65 years, for those working for small firms with less than 11 workers, or for construction workers.

C An Alternative Quasi-Experiment Based on Labor Formality

We discuss an alternative identification strategy in which formal workers are the treatment group and informal workers are the control group, as used by Jaramillo (2019) who follows Kugler’s (2004) research to evaluate the Colombian Labor Market Reform of 1990. That reform substantially reduced severance payments, extended the definition of a fair dismissal, and simplified the administrative procedures for mass layoffs. By noticing that practically no worker had been left out of the scope of that reform, Kugler (2004) proposed using informal workers as a control group, because they are out of the scope of labor market legislation, including the items related to employment protection. This was a major reform of labor market regulation, vastly more comprehensive than our case study for Peru, which was limited to the *de facto* application of reinstatement laws.

[Table C1 goes here]

We first evaluate whether the reinstatement ruling was associated with resorting of workers between sectors that could threaten the validity of this quasi-experiment. Table C1 shows that there were no notable compositional changes between informal

and formal sectors before and after the ruling in several dimensions. It follows that any bias resulting from a coinciding resorting of workers would be very small in magnitude. However, these results indicate that informal workers in our sample are not representative of informal workers as a whole. While only 39% of informal workers are dependent wage earners (INEI 2017), in our sample, this figure rises to 100%. Moreover, according to INEI (2017), only 20% of informal workers attained some form of higher education, while in our sample 36% did so. Workers defined as informal in our sample are wage earners, have some form of labor contract, and are relatively well educated; they are not part of the group of low-skilled independent workers typically observed in the informal sector.

The fact that 100% of informal workers in our sample - and in Jaramillo's (2019) - has some form of labor contract is not trivial. Even if our data label as informal those workers who do not have employer-financed health insurance, this does not mean that their formal employers can nullify their contracts arbitrarily without consequences, because formal firms are explicitly affected by the ruling.¹²

[Figure C1 goes here]

In Figure C1, we observe that for every year after the ruling the share of informal workers, as defined until now, who are employed by formal firms is above 92%. That is to say, this control group is made up of workers who are not only covered by an employment contract, but their contracts are signed by private and formal firms. Recalling our institutional framework, the ruling explicitly modified the relevant legislation for formal firms and workers alike,¹³ which implies that workers hired by formal firms, despite their undercompliance with particular items of the labor code, are not necessarily excluded from the scope of reinstatement laws.

[Figure C2 goes here]

In Figure C2, we graph the number of permanent and temporary hires before and after the ruling for informal and formal workers to assess the fulfillment of the parallel trends assumption. Before the ruling, in permanent hiring, there is a downward trend in the formal sector and an upward trend in the informal sector. This pattern occurs also in temporary hiring. Very clearly, control and treatment groups do not exhibit parallel trends before the treatment. A diff-in-diff estimator of β_3 in Equation (1) would spuriously attribute the pre-existing trend divergence to the reinstatement ruling, leading to its overestimation.

Summarizing, the control group in this alternative quasi-experiment, defined as informal workers, does not provide an adequate benchmark because it does not follow the same pre-treatment trend as the formal sector group. Moreover, informal workers may have been affected by the treatment, as most of them are hired by formal firms which are explicitly under the scope of the legislative change studied in this document.

¹²Diaz et al. (2018) suggest that formality involves jointly business and labor formality. It is possible for an informal worker, e.g., a worker without employer-financed health insurance, to be formal in the business sense, if he or she has a formal contract with a properly registered firm.

¹³The private sector labor code or Ley 728 states explicitly that it covers all firms and workers subject to the labor regime of the private activity.

Table 1. Contracts per Year by Sector
Before and After the Reinstatement Ruling

Variables	Private Sector			Others			Dif in Diff
	Before	After	Dif	Before	After	Diff	
Contracts (thousand/year)							
Permanent	292	213	-79	243	66	-177	98
Temporary	357	959	602	209	348	139	463
Others	102	144	42	76	157	81	-39
Total	751	1,316	565	528	571	43	522
Contracts (%)							
Permanent	38.88	16.19	-22.69	46.02	11.56	-34.46	11.77
Temporary	47.54	72.87	25.33	39.58	60.95	21.37	3.96
Others	13.58	10.94	-2.64	14.39	27.50	13.11	-15.75

Table 2. Workers' Demographic Attributes by Sector
Before and After the Reinstatement Ruling

Variables	Private Sector		Others	
	Before	After	Before	After
Monthly Wages	1,624	1,336	1,282	1,219
Tenure	5.25	1.90	6.28	1.87
Weekly Work Hours	48.07	46.57	38.63	42.19
Female (%)	31.51	36.62	45.73	41.76
Age	30.64	30.60	30.94	33.47
Years of Schooling	12.62	12.73	13.91	13.11
Resides in Lima	64.42	60.36	38.16	33.98

Note: Wages are expressed in 2009 soles.

Table 3. Diff-in-Diff Effects of Reinstatement on Permanent and Temporary Hiring
All Workers

Time Frame	Permanent			Temporary		
	N	Percentage	Mg. Effect	N	Percentage	Mg. Effect
Short Run	41,633	0.383	0.050 ^a (0.008)	41,671	0.502	-0.002 (0.010)
Medium Run	43,763	0.383	0.071 ^a (0.007)	43,797	0.503	0.077 ^a (0.009)
Long Run	21,857	0.383	0.176 ^a (0.020)	21,874	0.503	0.113 ^a (0.014)
Total Effect	77,890	0.382	0.059 ^a (0.005)	77,937	0.503	0.030 ^a (0.008)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Short Run. 2002-2008; Medium Run. 2009-2013; Long Run. 2014-2015. Robust standard errors in parentheses.

Table 4. Diff-in-Diff Effects of Reinstatement on Permanent and Temporary Hiring
Private Sector Ley 276

Time Frame	Permanent			Temporary		
	N	Percentage	Mg. Effect	N	Percentage	Mg. Effect
Short Run	41,633	0.368	0.053 ^a (0.008)	41,671	0.517	-0.014 (0.010)
Medium Run	43,763	0.368	0.074 ^a (0.007)	43,797	0.518	0.068 ^a (0.010)
Long Run	21,857	0.368	0.251 ^a (0.025)	21,874	0.518	0.100 ^a (0.014)
Total Effect	77,890	0.368	0.062 ^a (0.006)	77,937	0.518	0.018 ^b (0.008)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Short Run. 2002-2008; Medium Run. 2009-2013; Long Run. 2014-2015. Robust standard errors in parentheses.

Table 5. Diff-in-Diff Effects of Reinstatement on Permanent and Temporary Hiring
Full-Time Workers Only

Time Frame	Permanent			Temporary		
	N	Percentage	Mg. Effect	N	Percentage	Mg. Effect
Short Run	37,908	0.383	0.046 ^a (0.008)	37,950	0.504	0.008 (0.010)
Medium Run	39,942	0.384	0.069 ^a (0.007)	39,976	0.504	0.089 ^a (0.010)
Long Run	19,888	0.384	0.169 ^a (0.021)	19,907	0.504	0.129 ^a (0.015)
Total Effect	71,477	0.383	0.055 ^a (0.006)	71,534	0.504	0.043 ^a (0.009)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Short Run. 2002-2008; Medium Run. 2009-2013; Long Run. 2014-2015. Robust standard errors in parentheses.

Table 6. Diff-in-Diff Effects of Reinstatement on Permanent and Temporary Hiring
Formal-Sector Workers Only

Time Frame	Permanent			Temporary		
	N	Percentage	Mg. Effect	N	Percentage	Mg. Effect
Short Run	29,810	0.459	0.050 ^a (0.010)	29,815	0.515	-0.013 (0.010)
Medium Run	31,084	0.460	0.072 ^a (0.009)	31,093	0.514	0.063 ^a (0.010)
Long Run	15,656	0.460	0.181 ^a (0.026)	15,665	0.514	0.095 ^a (0.018)
Total Effect	53,018	0.458	0.063 ^a (0.007)	53,018	0.515	0.019 ^b (0.008)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Short Run. 2002-2008; Medium Run. 2009-2013; Long Run. 2014-2015. Robust standard errors in parentheses.

Table 7. Placebo Tests: Marginal Effects of Reinstatements on the Percentage of Permanent and Temporary Hiring for False Treatment Dates

Contract Type: Year Variables	Permanent		Temporary	
	Ruling 2002	Placebo 1998	Ruling 2002	Placebo 1998
A : After	-0.075 ^a (0.011)	-0.550 ^a (0.014)	0.052 ^a (0.018)	0.419 ^a (0.017)
T : Private	-0.101 ^a (0.006)	-0.095 ^a (0.013)	0.083 ^a (0.009)	0.092 ^a (0.021)
$A \times T$: After and Private	0.059 ^a (0.005)	0.035 ^a (0.013)	0.030 ^a (0.008)	0.014 (0.020)
Percentage	0.382	0.501	0.503	0.433
Number of Observations	77,890	77,890	77,937	77,937

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Robust standard errors in parentheses.

Table 8. Real Wages and Log-Wages by Sector Before and After the Reinstatement Ruling

Variables	Private Sector			Others			Dif in
	Before	After	Dif	Before	After	Dif	Dif
Wages	1,624	1,336	-288	1,282	1,219	-63	-225
ln(Wages)	7.03	6.94	-0.09	6.96	6.88	-0.08	-0.01

Note: Monthly wages in 2009 soles.

Table 9. Diff-in-Diff Effects of Reinstatement on Log Real Wages

Year Variables	Ruling 2002	Placebo 2000
A : After	-0.047 ^b (0.023)	-0.157 ^a (0.017)
T : Treated	0.061 ^a (0.012)	0.067 ^a (0.016)
$A \times T$: After and Treated	-0.039 ^a (0.011)	-0.040 ^a (0.014)
Number of Observations	63,383	63,383
R^2	0.395	0.395

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level.

Robust standard errors in parentheses.

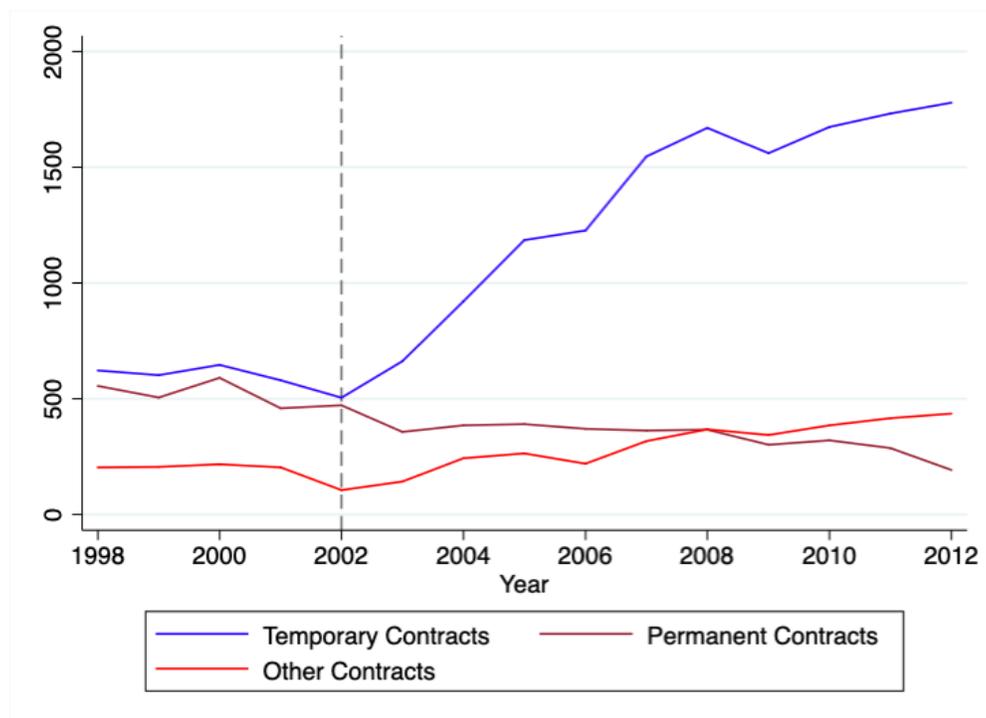


Figure 1: Number of New Hires by Contract Type and Start Year (Thousands)

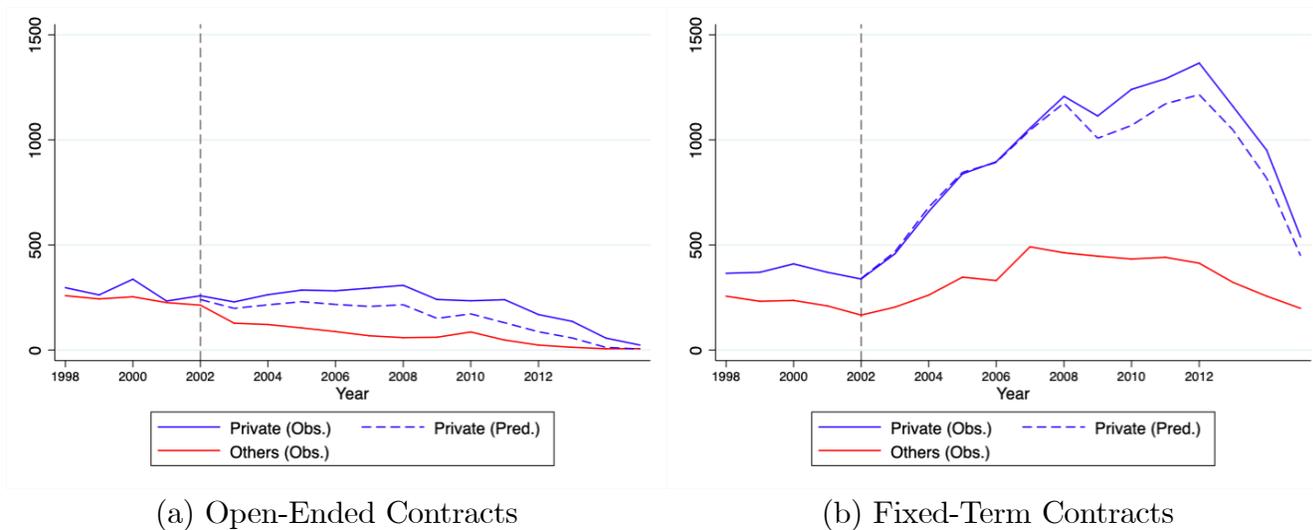


Figure 2: Number of New Hires by Start Year and Sector, Private and Others (Thousands). Observed and Counterfactual Trajectories: the Treatment Group Behaves as the Control Group After the Reinstatement Ruling

Table B1. Effects of the Reinstatement Ruling on Hiring by Demographic Attribute and Educational Attainment

	Percentage	Marginal Effect
Males		
Permanent	0.392	0.055 ^a (0.007)
Temporary	0.503	0.037 ^a (0.010)
Females		
Permanent	0.364	0.059 ^a (0.009)
Temporary	0.500	0.020 (0.014)
Lima (Metropolitan)		
Permanent	0.478	0.065 ^a (0.014)
Temporary	0.403	0.055 ^a (0.018)
Out of Lima		
Permanent	0.329	0.056 ^a (0.006)
Temporary	0.560	0.007 (0.010)
HS Incompletes		
Permanent	0.312	0.043 ^a (0.016)
Temporary	0.610	0.126 ^a (0.026)
HS Graduates		
Permanent	0.345	0.031 ^a (0.011)
Temporary	0.562	0.088 ^a (0.016)
Incomplete Non-College Tertiary Degree		
Permanent	0.381	0.017 (0.023)
Temporary	0.498	0.072 ^b (0.037)
Complete Non-College Tertiary Degree		
Permanent	0.437	0.054 ^a (0.013)
Temporary	0.460	0.015 (0.018)
College Dropouts		
Permanent	.373	0.046 ^b (0.020)
Temporary	0.457	0.125 ^a (0.033)
College Graduates		
Permanent	0.433	0.076 ^a (0.012)
Temporary	0.418	0.008 (0.018)
Post-Graduate Degree		
Permanent	0.651	0.050 (0.037)
Temporary	0.299	-0.008 (0.049)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level. Robust standard errors in parentheses.

Table B2. Effects of the Reinstatement Ruling on Hiring
by Age, Firm Size, and Economic Sector

Group	Percentage	Marginal Effect
17-25 Years Old		
Permanent	0.334	0.074 ^a (0.010)
Temporary	0.529	0.050 ^a (0.016)
26-35 Years Old		
Permanent	0.417	0.061 ^a (0.009)
Temporary	0.486	0.025 ^b (0.013)
36-45 Years Old		
Permanent	0.424	0.036 ^a (0.013)
Temporary	.480	0.067 ^a (0.019)
46-55 Years Old		
Permanent	0.374	0.047 ^b (0.021)
Temporary	0.514	-0.011 (0.032)
56-65 Years Old		
Permanent	00.337	0.020 (0.046)
Temporary	0.472	0.060 (0.073)
1-10 Employees		
Permanent	0.323	0.098 (0.088)
Temporary	0.458	0.087 (0.092)
11-100 Employees		
Permanent	0.335	0.045 ^c (0.025)
Temporary	0.525	0.050 (0.033)
More than 100 Employees		
Permanent	0.439	0.041 ^a (0.007)
Temporary	0.501	0.063 ^a (0.010)
Manufacturing		
Permanent	0.409	-0.088 (0.088)
Temporary	0.529	0.247 ^b (0.103)
Construction, Water, and Sanitation		
Permanent	0.262	-0.001 (0.024)
Temporary	0.63	0.053 (0.040))
Services		
Permanent	0.352	0.090 ^a (0.007)
Temporary	0.476	0.043 ^a (0.011)

Note: ^a significant at the 1% level, ^b significant at the 5% level and ^c significant at the 10% level. Robust standard errors in parentheses.

Table C1. Workers' Demographic Attributes
Before and After the Reinstatement Ruling

Variables	Formal		Informal	
	Before	After	Before	After
Monthly Wages	1,604	1,486	994	934
Tenure	6.21	2.32	3.63	1.05
Weekly Work Hours	44.13	46.11	44.36	43.54
Female (%)	36.93	36.16	39.17	42.18
Age	31.00	31.65	29.86	31.10
Years of Schooling	13.30	12.97	12.60	12.58
Resides in Lima	54.03	54.92	51.87	47.37

Note: Wages are expressed in 2009 soles.

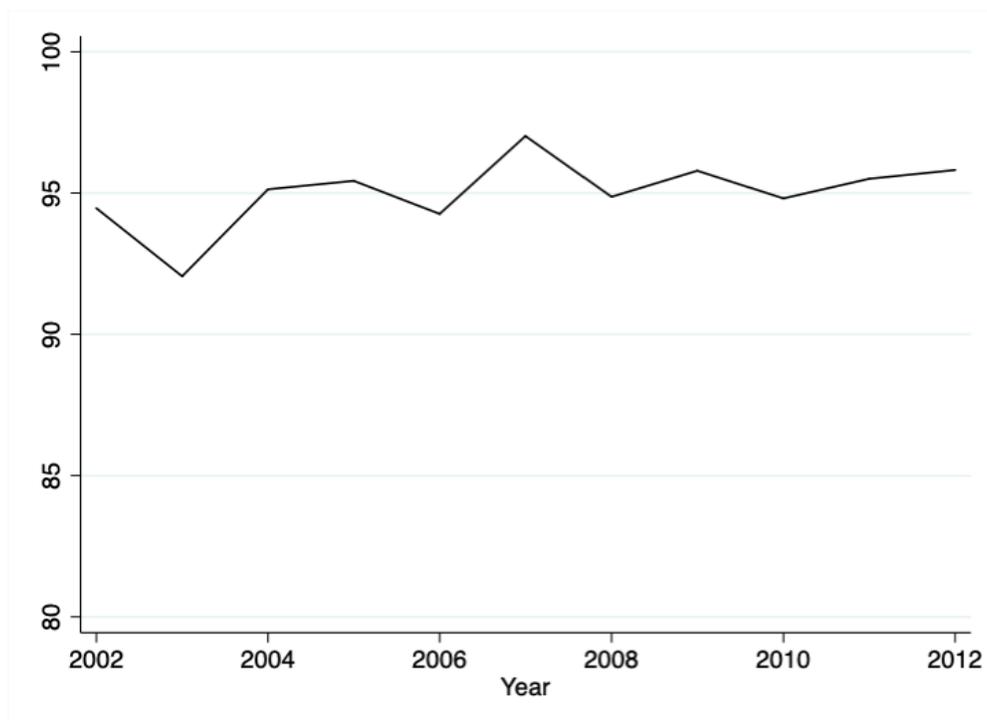


Figure C1: Percentage of Informal Workers Employed by Formal Firms by Year

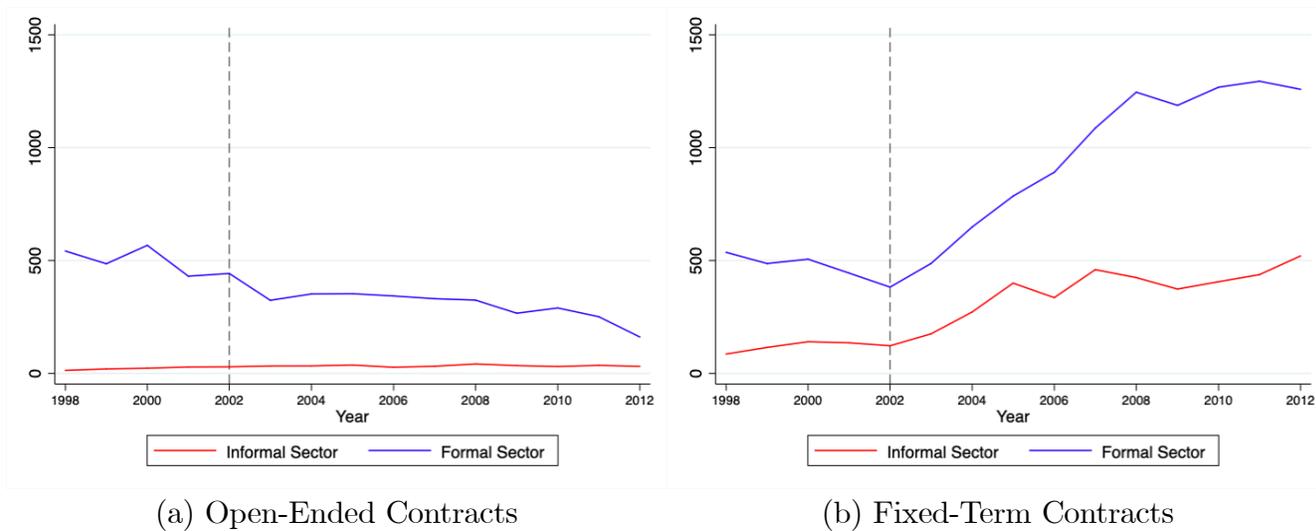


Figure C2: Number of New Hires by Start Year and Sector, Formal and Informal (Thousands)