Do Minimum Wage Hikes Lead to Employment Destruction? Evidence from a Regression Discontinuity Design in Argentina*

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Abstract

In this paper, we evaluate a series of minimum wage hikes implemented in the early XXIst century in Argentina using administrative records of registered employment. We identify the effect of raising the minimum wage on job separations via a regression discontinuity design. More specifically, we compare the match destruction rates for a treatment group directly bound by the minimum wage hikes and a control group slightly out of its legal scope. We show that this method represents an improvement over previous ones because it reduces the incidence of Type-I error. We find that, when aggregated, these hikes had a precisely estimated zero effect on separation rates. However, the increases enacted in 2008 arise as an exception. They decreased separations by 4.8 percentage points (19%). These results suggest that the employment effects of minimum wages may not flow through employment destruction.

Keywords: Minimum wage, Regression Discontinuity Design.
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1 Introduction

Since the early XXth century, economists have shown concern over the living conditions of workers at the lower end of the income distribution. Krueger (2015) highlights at least three historical perspectives on the matter. Firstly, he refers to an "institutionalist" perspective that emerged in the 1930s and 1940s that argues that minimum wages are an adequate tool to improve the income and living conditions of the poorest[1]. In contrast, the "marginalist" perspective illustrated in Stigler (1946) suggests that the conclusion adopted by the "institutionalists" is unlikely. In contrast, they assert that one should expect minimum wages to be associated with lower employment levels concentrated in the poorest households.

The conflict between these two positions can be mapped to a disagreement about the structure of labor markets. If they are competitive, increasing the minimum wage should reduce the number of low-wage jobs. On the other hand, if they are not, increasing the wage floor raises the income of the poorest; without causing job losses. In light of this discussion, Krueger refers to a third perspective, where economists embark on a more agnostic approach and derive their conclusions purely on the basis of econometric evidence. Our paper adds to this very literature. We propose a novel identification strategy to evaluate the effects of nationwide (i.e., federal) minimum wage hikes. And illustrate its features by evaluating Argentina's minimum wage policy between 2003 and 2011.

Adding to this vast literature remains relevant given the salience of some instances of conflicting evidence. For example, a first group of studies based on aggregate time-series evidence reaches contradictory results. Brown et al. (1982) find that minimum wages reduce employment. However, Wellington (1991) extends the time series used in that study and finds a null result. This finding is remarkable, given that in Wellington (1991)'s larger sample, detecting an association should be easier; but, in this particular case, such an association does not become stronger; it disappears. Recent studies have abandoned time series data in favor of microdata. Mainly because comparing the employment levels of aggregate units (countries, states, regions, etc.) with different minimum wages may not be enough to distinguish a true causal effect from other underlying unobservable differences between these units (Currie and Fallick, 1996).

Card and Krueger (1994)'s landmark study arises in this context. These authors compare the employment levels of a state exposed to a minimum wage hike with a control state before and after the event. This difference-in-difference design makes it possible to eliminate unobservable but time-invariant differences over time between units, alleviating the concern embedded in time-series studies. Since then, this method has become the standard practice to measure the effect of sub-national minimum wages. For example, Dube et al. (2010) uses an extension of Card and Krueger's methodology to evaluate all state minimum wage increases in the United States between 1990 and 2006 by comparing states that increased their minimum wages with nearby states that did not. Other documents have relied on alternative sources of variation whenever a second difference based on geographical heterogeneity is unavailable. Examples include Dickens et al. (2014), which exploits age-specific minimum wages in the UK, and Ham (2018), which utilizes industry and company size heterogeneity in the minimum wage in Honduras.

Yet, applying similar research desings to evaluate nationwide (i.e., federal) minimum wage hikes is inherently more complicated, mainly because the definition of treatment and control units is uncertain. Card (1992) provides a clever attempt to solving this problem by exploiting the regional heterogeneity in the rates of "treated workers". He defines"treated workers" as those whose pre-hike wages lie between the old and new minimum wages. After the hike, their employers must decide between i) increasing their wages up to the new legal minimum or ii) terminate the employment relation. Currie and Fallick (1996) do a similar analysis using a panel survey. They conclude that compared to a control group comprised of all workers with pre-hike wages above the new minimum, treated workers are 3% more likely to lose their job a year after a minimum wage increase. This approach, or variations, has

[1]This conclusion is rooted in certain features of the labor market that collide with a standard definition of a competitive market. For example, the observed income disparities between equally skilled employees suggest a certain level of employer discretion in the wage-setting process.

been used extensively since then, therefore, we will refer to it as the "standard panel data approach" (Clemens and Wither, 2019; Dustmann et al., 2022; Caliendo et al., 2018). Our methodology builds upon these pieces, as we adopt the same definition of "treated worker".

Our main contribution is to leverage rich administrative data[2] on monthly wages and employer-employee match length, to develop a method that, like Card (1992) or Currie and Fallick (1996), compares the length of "treated matches" with those of "untreated matches". However, unlike those studies, our data allows us to carefully restrict the control group to maximize comparability between the two groups. Specifically, we employ a regression discontinuity design to compare treated matches' length with those of a more refined control group: matches with pre-hike wages *slightly* above the new minimum. We argue that this method solves one of the potential shortcomings of the standard panel data approach. Specifically, by comparing the outcomes of minimum wage jobs with other low-wage jobs, we minimize the contamination induced by the large, unobserved, and systematic differences between treated units and those in the upper echelons of the income distribution. Previously, Yuen (2003) also noted that the standard approach essentially compares the employment outcomes of low and high-income workers. Therefore, she offered a convincing solution to the problem by comparing treated workers in regions that increased their minimum wage with workers in the same wage range (i.e., also low-income workers) in regions that did not. However, this solution is inapplicable in contexts without sub-national heterogeneity in the minimum wage; this document provides an alternative for those cases.

Throughout this paper we provide supporting evidence for our choice of treatment and control groups in two dimensions. First, we perform a descriptive comparison between the treatment group and two potential control groups. The first one includes matches with pre-hike wages only marginally above the new minimum wage. And the second one features all labor relations with pre-hike wages above this threshold (à la standard method). We show that the first control group candidate more closely matches the observable features of the treatment group than the second candidate group defined following the standard method. Second, we take advantage of an unusual institutional feature. Namely, the fact that the Argentinian nominal minimum wage was unchanged throughout the 1990s allows us to run a series of placebo experiments where we simulate non-existent minimum wage hikes. While the standard method detects a spurious treatment effect, ours does not. A lack of statistical power cannot explain this result. Therefore, compared to the standard panel data approach, ours reduces the likelihood of type-I error.

We apply our regression discontinuity design on a series of minimum wage hikes enacted in Argentina between 2003 and 2011. We find that, overall, these hikes did not have a significant impact on job destruction. However, there are some exceptional events. The increase enacted in January 2004 is associated with a large increase in separation rates[3] of up to 9.2 percentage points (or 24%). However, further statistical testing shows that this finding is most likely explained by a non-linear relationship between wages and job destruction rather than by a true discontinuity. Conversely, the increases in August 2008 reduced job destruction rates by 4.8 pp. This result is robust to a series of alternative specifications and bandwidth choices.

This paper adds to the literature as follows. First, our methodology and findings are of general interest as many countries, other than Argentina, do not have any source of sub-national minimum wage heterogeneity and have administrative records similar to the ones we employ in this paper[4].

Second, our finding of a zero effect of raising the minimum wage seems consistent with recent findings in developing countries (Derenoncourt et al., 2021; Neumark and Corella, 2021). Nonetheless, as in Dube et al. (2010), we show that different minimum wage hikes can have heterogeneous impacts even within a single country. This result suggests that studies that aggregate multiple minimum wages as a single event should be careful not to incur in aggregation bias.

Third, we argue that Argentina's unique institutional features make our findings particularly in-

[2]To our knowledge, Cerimelo (2021) in the only preivous article that employs the same data.

[3]Throughout this paper we use the terms "separation" and "match destruction" interchangeably.

[4]For example, Spain's "Muestra Continua de Vidas Laborales", Germany's "Linked Employer-Employee Data" (LIAB), or Peru's "Planilla Electronica".

teresting. We provide evidence on the effectiveness of minimum wages as a policy to combat currently employed workers' purchase power erosion in a context marked by moderate inflation. This is a relevant lesson amidst the worlwide inflation surge observed in the aftermath of the Covid-19 pandemic. Moreover, our study joins a recent literature that studies how different institutional contexts shape the labor market effects of minimum wages. For example, Brummund and Strain (2020) show that minimum wage indexation reinforces these effects, mainly because it signals a commitment of permanently increasing the wage floor. Notably, our results differ from theirs in that we find no clear evidence of a pernicious effect of the minimum wage on job destruction. It is possible that this difference is due to the fact that, in our setting, there was no formal or clearly defined indexation. However, this is only a hypothesis which requires further testing. Finally, as mentioned above, the fact that Argentina's minimum wage did not change throughout the 1990s and early 2000s provides a rare opportunity for running falsification tests which support the credibility of our findings.

Lastly, we join very recent work focused on understanding the mechanisms behind the effects of minimum wages on employment margins. Our findings are complementary to those in Butschek (2022); Hirsch et al. (2015); Clemens et al. (2021) and Kudlyak et al. (2022). These authors find that employers respond to minimum wage hikes by either raising their hiring standards or by reducing existing and new vacancy postings. When taken together with our findings, it seems that the minimum wage improves labor market outcomes for currently employed individuals at the expense of the employment opportunities of the currently unemployed, as predicted by the insider-outsider theory (Lindbeck et al., 1989).

The rest of the document is structured as follows. In the next section we describe Argentina's minimum wage policy, and other complementary policies that might aid at interpreting our results. In Section 3, we describe the data and the sample selection. Section 4 details our strategy for identifying the causal effect of an increase in the minimum wage on job separations. Sections 5 and 6 present the results and conclude, respectively.

2 Institutional Setting

The Argentinian "Salario Mínimo Vital y Móvil" (henceforth, MW or minimum wage) was formally instituted in 1964. However, a prototype for a wage floor was promulgated by Juan Domingo Perón in 1945 when he served as Secretary of Labor and Welfare of the Nation ("Decreto 33.302/45"). The current legal minimum wage was enacted by "Ley 16.459". This legal document creates the minimum wage as follows[5]:

> *"A living, minimum, and mobile salary is established for every employee over 18 years old. "*

We highlight this section for two reasons. First, it sets an important precedent regarding the universal scope of the minimum wage. More recently, the increases in the minimum wage apply to those labor relations covered by Ley 20,744[6]. Subsequent updates to Ley 16.459 have also been clear in that regard. The second reason is that, even though it specifies that the minimum wage is a "mobile" figure, it does not establish any rule or criterion regarding its updating. In fact, between 1994 and 2002, the nominal minimum wage stayed fixed. It was only in 2003 when Néstor Kirchner's resumed the periodic updating in the aftermath of the 2001-2002 crisis. However, we will show that even after the increases initiated by Kirchner, minimum wages have not followed a predictable updating scheme, neither in terms of magnitude nor timing.

[5]The original text reads: *"Se establece un salario vital, mínimo y móvil para toda persona mayor de 18 años que trabaje por cuenta ajena bajo dependencia de un empleador."*

[6]There are 3 categories of employees that *a priori* could be out of the scope of the MW: public sector workers, family workers, and agricultural workers. Our data, does not include the first two, so we ignore this caveat. Agricultural workers, can deviate from Ley 20,744; Ley 26,727 specifies these. However, it clearly states that agricultural workers are subject to minimum wage legislation, thus, we do not exclude them from this study. Specifically, article 34 of Ley 26,727 establishes that: *"Minimum wage for job performance. Guaranteed minimum wage. Workers will be paid according to the work that they perform, but in no case may it be less than the minimum remuneration established by the National Agricultural Work Commission."*

Table 1: Minimum Wage Schedule in Argentina: 2003-2011

Effective Date	Nominal MW	Document	Announcement	Considered?
08/01/93	200	Res. 2/93 CNSVMM	07/26/93	No
07/01/03	250	Dec. 388/2003	07/10/03	Yes
08/01/03	260	Dec. 388/2003		
09/01/03	270	Dec. 388/2003		
10/01/03	280	Dec. 388/2003		
11/01/03	290	Dec. 388/2003		
12/01/03	300	Dec. 388/2003		
01/01/04	350	Dec. 1349/04	12/29/03	Yes
09/01/04	450	Res. 2/04 CNEPySMVyM	09/02/04	Yes
05/01/05	510	Res. 2/05 CNEPySMVyM	06/01/05	Yes
06/01/05	570	Res. 2/05 CNEPySMVyM		
07/01/05	630	Res. 2/05 CNEPySMVyM		
08/01/06	760	Res. 2/06 CNEPySMVyM	07/28/06	Yes
09/01/06	780	Res. 2/06 CNEPySMVyM		
11/01/06	800	Res. 2/06 CNEPySMVyM		
08/01/07	900	Res. 2/07 CNEPySMVyM	07/11/07	Yes
10/01/07	960	Res. 2/07 CNEPySMVyM		
12/01/07	980	Res. 2/07 CNEPySMVyM		
08/01/08	1200	Res. 3/08 CNEPySMVyM	07/28/08	Yes
12/01/08	1240	Res. 3/08 CNEPySMVyM		
08/01/09	1400	Res. 2/09 CNEPySMVyM	07/30/09	No
10/01/09	1440	Res. 2/09 CNEPySMVyM		
01/01/10	1500	Res. 2/09 CNEPySMVyM	08/05/10	No
08/01/10	1740	Res. 2/10 CNEPySMVyM		
01/01/11	1840	Res. 2/10 CNEPySMVyM	08/26/11	Yes
09/01/11	2300	Res. 2/11 CNEPySMVyM		

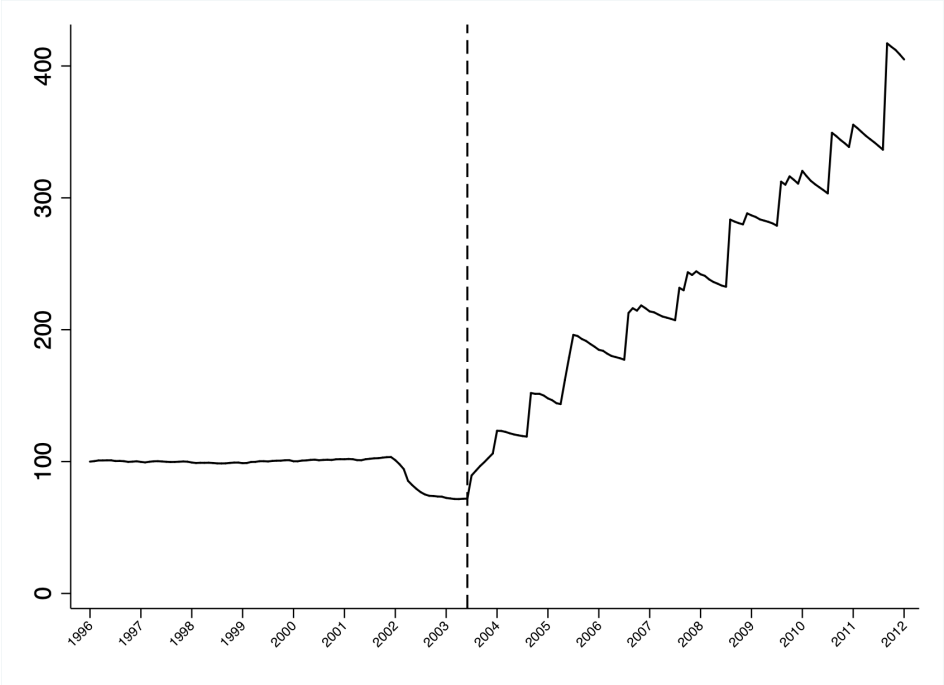
In Table 1, we show all minimum wage hikes enacted in Argentina since 2003 . We report the date when the increase was made effective and the updated value (i.e., the new minimum wage). This information will be highly relevant to understand the identification strategy described in Section 4. We also include columns for the legal document that announced the increase, and its publication date. This Table shows that minimum wage hikes in Argentina are generally made effective immediately after their announcement. Moreover, the legal documents that increase the minimum wage usually announce a small block, or sequence, of further increases to be implemented in the near future. Take, for example, the hikes enacted between July and December 2003. They were jointly announced in Dec.388/2003 on July 10, 2003. That is, employees and employers found out on this date about the minimum wage schedule from July to December of 2003. Dec.388/2003 increased the minimum wage from 200 to 250 ARS (local currency) immediately. Furthermore, it formalized a sequence of further increases that would leave the salary floor at 300 ARS by the end of the year. Consequently, by December, all labor relations with wages between 200 and 300 ARS in June 2003 would have to receive a wage increase to at least match the new minimum wage of 300 ARS or be destroyed. Finally, in Table 1 we include a column in which we indicate whether or not we consider said increase in this article. We only exclude the hikes enacted in August 2009 and August 2010. We do so because the small time frame between them and the next makes identification very problematic[7]. Between the start of one block and the other there is a difference of only 5 months. This is problematic because minimum wage hikes might not have an immediate effect on job destruction, mainly because of employment protection legislation: Argentinian law states that employers must provide notice 60 business days in advance of a dismissal. Moreover, severance payments are around 150 daily wages for workers with at least 5 years of seniority in the same position (Alaimo et al., 2017).

We complement Table 1 with Figure 1, where we show the trend of the real minimum wage from

[7]For example, the block of increases that began in August of 2009 ends in October of 2009. The next series of increases begins in of January 2010. Although we could have studied this hike as an independent event from the one in August of 2010 we decide not to because they were announced at the same time and identification might be contaminated by anticipation effects.

the mid-1990s to 2012.

Figure 1: Real Minimum Wage Trend: 1996-2012.



Note: We deflate the nominal minimum wage to 2008 ARS and then re-express them as a percentage of its 1996 value.

Showing this trend is particularly important because, given the inflationary history of Argentina in the 21st century, it might have been the case that inflation fully eroded these hikes. If this was the case, one should expect no labor market effects. However, Figure 1 rules out this possibility. By 2012, minimum wage workers in Argentina had a purchasing power around four times the one they had in 1996. In other words, wage floor increases implemented since Néstor Kirchner's government increased the value of the *real* minimum wage. Curiously, we can observe an almost identical pattern in Brazil, after Fernando Henrique Cardoso's government (Derenoncourt et al., 2021)[8]. Colombia and Chile also experienced a similar increase in the minimum wage since the turn of the century (Keifman and Maurizio, 2012).

3 Data

Data comes from the Longitudinal Sample of Registered Employment (henceforth, MLER). It is a 3% sample of all social security records. This sample is representative of the universe of registered (i.e., formal) labor relations in the Argentinian private sector from January 1996 to December 2015. Thus, the unit of observation is an employer-employee match. In that sense, it is similar to other matched data sets such as Spain's "Muestra Continua de Vidas Laborales", Germany's "Linked Employer-Employee Data" (LIAB), or Peru's "Planilla Electronica".

Our analysis spans the years between 2003 and 2011. The sample has information for over 450 thousand people and records their entire work history month by month. It is important to state that each data point in our data set represents a specific labor relation (i.e., an employer-employee match), rather than an employee by herself. In addition, the MLER includes information about wages (monthly

[8]The only difference is that, in the Argentine case, there was a stagnation in the minimum wage before the 2000s; in contrast, a strong depreciation was observed in Brazil.

gross wages), economic sector, firm size, workers' sex, their year of birth, and the province where they work.

We restrict our data to labor relations that are active before a change in the minimum wage (i.e., that have a positive wage); and that have a wage at least as high as the minimum at that time (i.e., the "old" minimum wage)[9]. In addition, we drop observations that do not have information on sex, age, province, or economic sector. Our final sample comprises 234,423 individuals in 921,066 employment relationships between 2003 and 2011[10]. The employment relationships in our data can be included in multiple events. For example, if an employment relationship that is active before one raise survives into the next, then we record it twice, once for each raise. In that sense, our data is an unbalanced panel of labor relations that were active one month before a minimum wage increase.

In Table 2 we present the means of the key variables. In column 1 we describe the full sample. In a typical employment relationship, a worker is more likely to be male, to have been born around 1968, to work in a large company (i.e., one with more than 50 workers), to work outside the primary sector (i.e., agriculture, mining, and fishing), and to work in GBA (i.e., Greater Buenos Aires, Capital Federal, or Buenos Aires). Job destruction rates 1, 3, and 6 months after a typical increase in the minimum wage amount to 2.7, 8.2, and 14.9% respectively. In addition, we show that the proportion of individuals with salaries between the old and the new minimum wage represents 7.52% of the sample.

Table 2: Means of Key Variables.

Variable/Group	Full-Sample	Treated	Marginally Non-Treated	Non-Treated
	(1)	(2)	(3)	(4)
Male (%)	70.80	64.07	66.19	71.34
Year of Birth	1968.47	1971.37	1970.73	1968.24
Large-Firm	57.54	35.93	44.09	59.32
Primary Sector	6.75	16.04	14.83	5.99
Buenos Aires	60.01	51.24	54.96	60.73
Match Destruction (%)				
1 month	2.68	6.14	4.97	2.40
3 months	8.20	16.65	14.83	7.51
6 months	14.86	27.38	24.79	13.84
Treated (%)	7.52	100.00	0.00	0.00
Obs.	921,066	69,255	19,372	851,811

Note: Column (2) includes workers with pre-hike wages between the old and new minima. Column (3), those with previous wages at most 0.25 times the difference between the old and new minima. Column (4) includes all workers with wages above the new minimum wage before the hike.

In columns 2, 3, and 4, we show descriptive statistics for specific groups defined by their baseline wages. In column 2, we describe labor relations with incomes between the old and new minimum wages; in column 3, those with pre-hike wages greater than the new minimum for an amount up to 0.25 times the difference between the old and new minima. This comprises a group with baseline wages marginally above the new minimum. In column 4, we describe all matches with baseline above the new minimum[11].

[9]This allows us to eliminate part-time jobs that could be paying "high" wages in their full-time equivalent. This filtering is necessary since we do not have information on hourly wages or hours worked.

[10]We will estimate our main with a sub-sample of this. Since the optimal bandwidth selection algorithm we use is strictly data-driven we do not impose any *ex-ante* restrictions based on baseline wages.

[11]As an example, consider the minimum wage increases in 2003 when the minimum wage rose from 200 to 300 ARS. We describe individuals with pre-hike wages between 200 and 300 ARS in column 2, those with pre-raise salaries between 300 and 325 (300+0.25[300-200]) in column 3, and those with pre-raise salaries -increase greater than or equal to 300 in column 4.

Comparing the degree of similarity between treated units in column 2 with those in columns 3 and 4 elucidates the main argument of this paper. Treated units (column 2) appear to be very different from untreated units (column 4) in all relevant dimensions. Workers in the upper echelons of the income distribution (column 4) are much more likely to be men (11.35%), slightly older, more likely to work in large companies (65.10%), less likely to work in the primary sector (62.66%) and more likely to be geographically located in Buenos Aires (18.52%). Likewise, they have much lower job destruction rates.

In contrast, a comparison between column 2, those directly affected by an increase in the minimum wage, and column 3, those marginally untreated, suggests a higher level of comparability between the two groups. Although some differences persist, they are much smaller than those observed between the Treated group (column 2) and the Non-Treated group (column 4). In Figure C.1 we provide a more formal proof on this line by showing that most demographic differences between the treatment and the control group increase monotonically with the size of the bandwidth. To the extent that the unobservable differences between worker types behave like the observable, our method will yield more accurate estimates than the standard panel data approach.

OLS estimates that compare treated workers with the entire population of untreated workers à la Currie and Fallick (1996) could overestimate the match destructing effects of the minimum wage. Namely, they could confound a true causal effect with the underlying unobservable differences between high and low-income workers. Comparing the groups in columns 2 and 4 would likely put too much weight on the fit of the relationship away from the neighborhood of the minimum wage threshold (Fujiwara, 2011). Instead, our identification strategy solves this problem by comparing low-income individuals affected by an increase in the minimum wage with other low-income individuals who are not affected.

4 Identification Strategy

4.1 Regression Discontinuity Design

Let $W_{t=0}$ be the nominal wage of an employment relation exactly one month *before* some increase in the minimum wage; that is, the "old" minimum wage. Similarly, let MW' be the nominal minimum wage *after* the raise (the "new" minimum wage), and $Y_{t=k}$ some outcome variable measured k months after the raise (e.g., job destruction). Following Fujiwara (2011), the treatment effect (TE) we seek to identify can be expressed as follows.

$$TE = \lim_{W_{t=0}\uparrow MW'} E[Y_{t=k}|W_{t=0}] - \lim_{W_{t=0}\downarrow MW'} E[Y_{t=k}|W_{t=0}] \tag{1}$$

TE identifies the treatment effect of changing from being a treated to a control match, for a match with a baseline wage equal to the new minimum, as long as the distribution of treatment effects is continuous at the threshold. We define the old minimum wage, MW, as the minimum wage in effect before the enactment of a minimum wage hike, or sequence of hikes. Likewise, MW' is the minimum wage that will be in effect at the end of all announced increases. For example, to evaluate the 2003 minimum wage increases, we define MW =200 and MW' =300; These are the minimum wages in force before and after the implementation of Dec. 388/2003 (See Table 2). Similarly, for the 2005 raises, we set MW =450 and MW' =630. We proceed identically for the rest.

The rationale behind this identification strategy is similar to that in Currie and Fallick (1996): faced with a minimum wage hike, employers of minimum wage workers have to either i) raise their salaries to at least match the new minimum wage or ii) terminate the employment relationship. Since the control group always satisfies $MW' \leq W_{t=0}$, this decision does not apply. In that sense, we compare a group whose wages are directly affected with a group whose wages are not.

We estimate the limits of Equation (1) non-parametrically using a local linear regression with a uniform kernel. That is; for a given bandwidth, h, we do a linear regression of Y_t on $W_{t=0}$ using only

the observations that satisfy $W_{t=0} \in [MW' - h, MW']$ for the limit when $W_{t=0} \uparrow MW'$ and those that satisfy $W_{t=0} \in [MW', MW' + h]$ for the limit when $W_{t=0} \downarrow MW'$. We implement this by estimating the following linear regression.

$$Y_{i,t=k} = \beta_0 + \beta_1 T_{i,t=0} + \beta_2 (W - MW')_{i,t=0} + \beta_3 (W - MW')_{i,t=0} \times T_{i,t=0} + \epsilon_{i,t=k}, \tag{2}$$

Here i indexes employment relations and t is a time index. The dependent variable, Y, is a binary variable that indicates whether the employment relationship i is destroyed k months after the increase; throughout the document we report the results for $k = \{1, 2, 3, 4, 5, 6\}$. To be more precise, we set $Y_{i,t=k} = 1$ from the first month when we fail to observe strictly positive wages for match i, and assume that it is destroyed thereafter. Before a match is destroyed $Y_{i,t=k} = 0$. Likewise, for matches that are not destroyed within a 6 month window $Y_{i,t=k} = 0 \ \forall \ k$. T_i is a binary variable that indicates whether employment relationship i is in the treatment group, that is, if $SM \leq W_{t=0} < MW'$. In that sense, β_1 is our parameter of interest.

We also include the variable $(W - MW')_{i,t=0}$ which denotes the nominal wage of the employment relationship i a month before the increase in the minimum wage centered around the new minimum wage. Finally, we include an interaction term between $T_{i,t=0}$ and $(W - SM')_{i,t=0}$. We compute all our results using the algorithm in Calonico et al. (2017) and, since by default in that method the treatment group is assumed to be to the right of the cut-off point and not to the left (as in our case), we present the results with the opposite sign. We also calculate quadratic specifications where we include the following explanatory variables: $(W - SM')^2_{i,t=0}$ and $(W - SM')^2_{i,t=0} \times T_{i,t=0}$. We report the full results derived from the quadratic specification in Appendix A.

4.2 Identification Assumptions and Threats

The main identifying assumption is that assignment to treatment and control groups in the interval $[SM' - h, SM' + h]$, is as good as random. In that sense, the selected bandwidth, h, is crucial. As usual in the literature, we use the optimal bandwidth proposed by Calonico et al. (2014)[12]. Informally, this bandwidth is computed in two steps: first, the asymptotic variance and bias of the regression discontinuity estimator (i.e., TE) are estimated using a preliminary bandwidth; then, these are incorporated in the expression for the mean squared error and an "optimal" bandwidth is chosen so as to minimize it. In that sense, the first threat to our identification strategy is the non-randomness (local) in the assignment to treatment.

This assumption is not directly testable, however, we present evidence that supports its fulfillment throughout the document. First, in Table 2 we show that, at the descriptive level, low-income individuals on both sides of the cut-off point are similar in observable terms. In Appendix C we will evaluate more formally whether there are significant differences by re-estimating the equation (2), but taking these same characteristics (gender, geographic location, economic sector and firm size) as the dependent variables. We interpret the absence of significant differences[13] on these observable variables as evidence in favor of local randomization. We do detect certain covariate imbalance. Nonetheless, we show that it becomes larger with bandwidth choice. Which argues for our estimator over the standard approach. Moreover, we find that our results are robust to including these covariates as suggested by Calonico et al. (2019).

Along the same lines, it is usual to couple these tests with "manipulation" tests in the spirit of Cattaneo et al. (2018) or McCrary (2008). To formally perform this analysis, we use the Cattaneo et al. (2018) method and report the results in Appendix B. These tests look for evidence of significant differences in the density of the running variable $(W_{i,t=0} - SM)$ on each side of the threshold. Any significant density difference, could be interpreted as possible self-selection into or out of the treatment group, therefore, it is a threat to the assumption of quasi-random assignment.

[12] We will show that the results hold under some different bandwidths.

[13] Specifically, this would be observed as a "treatment effect" statistically equal to 0.

Conceptually, we do not expect these threats to randomization to be a particularly serious concern in our case for the following reasons. First, the precisely manipulation the running variable is particularly complex given the discussion in Table 1. Most increases are announced retroactively and the magnitude of the increase is arbitrary in that it does not follow a pre-established formula; therefore, it is very difficult to build a history of manipulation based on the anticipation of the timing or magnitude of the increase[14]. Second, even if perfect anticipation was possible (based on rumors or informal political announcements), it remains unclear how and why exactly would any party wish to manipulate the treatment assignment. On the one hand, employees do not have the possibility of manipulating the running variable, since this would imply unilateral control over their own wages. On the other hand, employers have no incentive to manipulate treatment assignment: transferring a worker from the treatment group to the control group *before* the increase has a monetary cost equal to $SM' - W_{i,t=0}$ per month, and there is no clear reason why an employer would be willing to incur that cost before a raise; Transferring a worker from the control group to the treatment group is implausible, since it would imply reducing a worker's salary only to increase it again once the new minimum wage comes into force.

Summarizing, we can argue that even if we find evidence of significant differences in the allocation variable, this might be due to issues exogenous to the determination of the minimum wage. An argument based on the conscious, systematic and coordinated manipulation of the treatment allocation by workers and employers is remarkably difficult to believe. In addition, behavioral economics provides an additional reason to dismiss the relevance of significant differences in the density of the running variable around the cutoff. Specifically, there is evidence that wages are usually set at "round" numbers and that there is accumulation in particular figures, even in salary ranges very different from the minimum wage. For example, Dube et al. (2020) show that, for hourly earnings in the United States, the wage density at \$10 is 50 times larger than the density at \$9.90 or \$10.10. As shown in Table 1, all the minimum wages studied are set in round numbers, therefore, it is natural to find density peaks in figures like these. For example, for the January 2004 raise, it would be natural to find a peak around 350 since it is difficult to think of many people who have salaries equal to 349 or 351 ARS.

In Appendix B we show that there are some significant density differences in the running variable on both sides of the cut-off point, at least for some hikes. Despite the conceptual weakness of this counterargument to our identification strategy, it is important to try to understand the extent in which possible manipulation can bias our results. For this reason, we will incorporate "doughnut-hole" estimations where we exclude observations in a very small neighborhood of the cut-off. This is is a usual robustness check in the regression discontinuity literature (Almond and Doyle, 2011; Melnikov et al., 2020; Eggers et al., 2015). The "doughnut-hole" estimates follow a heuristic rule based on the assumption that manipulation would occur mostly for individuals who are extremely close to the cut-off point. In other words, the "doughnut" estimate is based on excluding those individuals for whom there is a suspicion of endogenous self/selection into the treatment given their proximity to the cut-off point.

5 Results

In this section we present the estimates of the match destruction effects of the minimum wage the Equation (2), which seek to identify the destruction of employment generated from the increases in the minimum wage. First, in subsection 5.1, we pool together the 8 minimum wage hikes described in Section 2 and we run Equation (2). We include "doughnut-hole" specifications to probe the robustness of our results to the potential manipulation issues that were mentioned in the previous section. We also include estimates derived from using different bandwidths and from incorporating a quadratic specification. In the section 5.2, we briefly analyze the heterogeneous treatment effects that is, we estimate Equation (2) independently for each increase to allow each minimum hake to have a differential effect on separation rates. Finally, subsection 5.3 we compare the performance of our method vis-à-

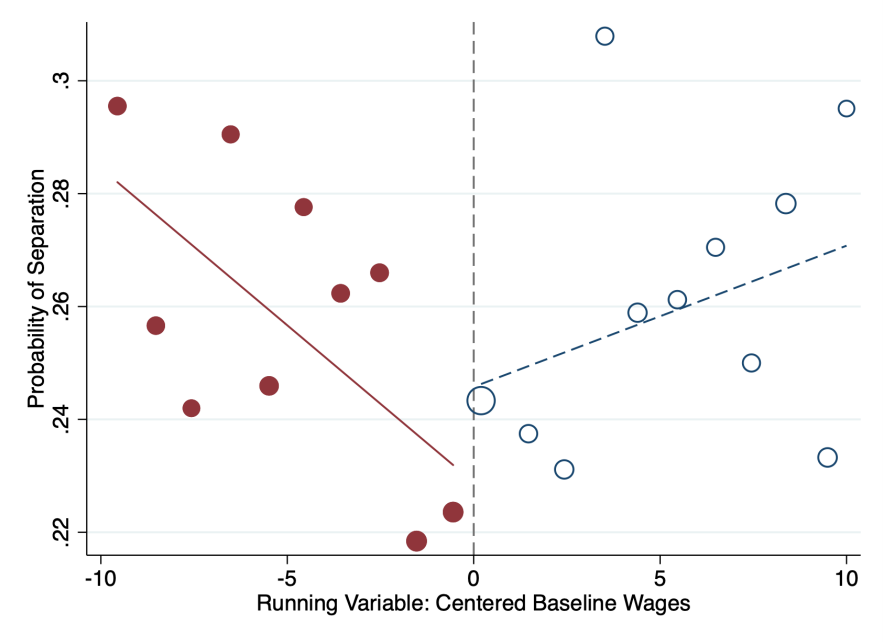
[14]For example, one could think that, given an early announcement, employers could increase the wages of their most productive workers up to the new minimum wage and freeze those of the least productive to fire them after the increase. However, such an argument contrasts sharply with Table 1.

vis the standard panel data approach in a placebo experiment. We provide a more complete set of results in Appendix A. We also report all density tests in Appendix B, tests for covariate continuity in Appendix C.

5.1 Pooled Results

Before reporting the results from estimating Equation (2), it is informative to graphically check for discontinuities in a small neighborhood around the threshold. In Figure 2, we plot the probabilities of match destruction against the centered pre-hike wage six months after the minimum wage hikes. We pool all 8 minimum wage hikes described in Section 2. We fit linear polynomials at each side of the cutoff. To clarify, the treated units are those to the left of the cutoff. We use an arbitrary bandwidth of 10 ARS at each side of the cutoff. Each circle represents a group of labor relations (*bin*) with salaries with a maximum difference of 1 ARS (i.e., *binwidth=1*). The regressions and the circles are weighted by the number of employment relationships they group together, such that groups with more workers are plotted with larger circles and receive a higher weights in the regressions.

Figure 2: Centered Minimum Wage and Match Destruction Rates for $k = 6$.



Note: we choose the 10 ARS bandwidth at each side of the cut-off arbitrarily. The dots denote averages, with a bin width of 1. Their size represents the number of observations. Treated units are at the left of the cutoff.

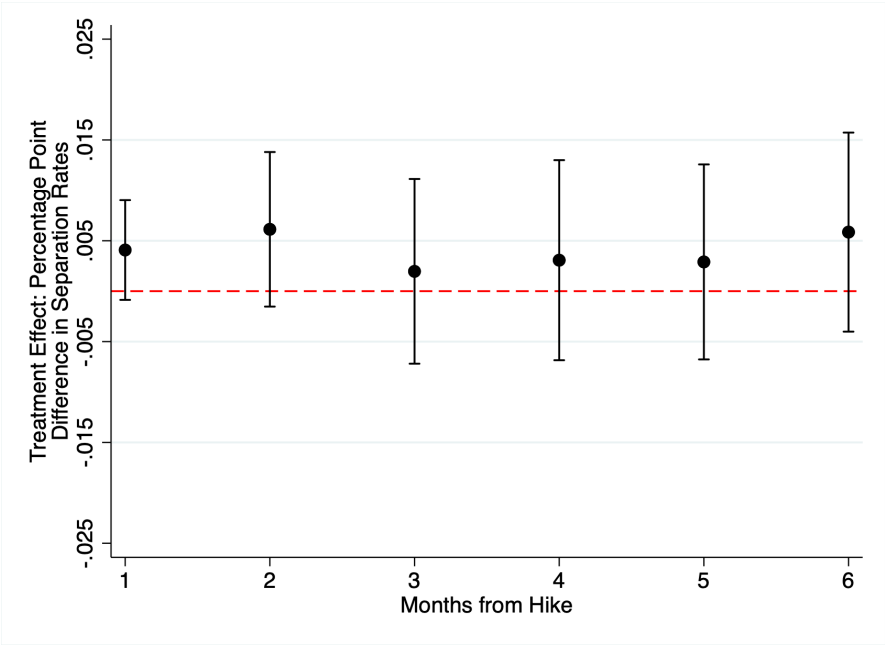
Figure 2 suggests that there might be a discontinuity in the outcome variable. Apparently, treated units have a lower job destruction rate than the control units 6 months after an increase in the minimum wage. It is important to highlight that this graphical motivation is made with an arbitrary bandwidth. Considering that, in the remainder of the section we present more rigorous calculations where we use the optimal bandwidth suggested by Calonico et al. (2014). In addition, we present a battery of robustness tests where we vary the bandwidth, the degree of the polynomial used for the estimation, and exclude observations around the thresholds to test the robustness of our claims.

Figure 3 presents the estimates of the effects of a minimum wage hike on job destruction, that is, β_1 in Equation (2) or "TE" in Equation (1). Again, we jointly analyze the eight increases considered

in the period 2003 to 2011. The estimated coefficient for 6 months after the increase indicate that the treatment group had a 0.6 percentage point higher probability of job destruction than the control group, employment relationships with wages marginally above the new minimum wage. However, this difference is very small and is not statistically significant at conventional levels. Shorter time windows yield identical results. All the coefficients shown are extremely small and are never statistically significant. Therefore, it seems that the increases in the MW did not trigger job separations.

In Table 3 we present a first battery of robustness checks. In column (1) we present the original estimate, which corresponds to the sixth month coefficient in Figure 3. In columns (2) and (3) we estimate two "doughnut-hole" specifications where we exclude observations with a distance to the cutoff smaller than 1 and 5 ARS. In both cases, the estimated coefficients economically negligible and statistically insignificant.

Figure 3: Effects of the Minimum Wage on Match Destruction Rates. Regression Discontinuity Designs with Optimal Bandwidth.



Note: Each marker is a point estimate for β_1 in Equation (2). The lines denote conventional 90% confidence intervals.

In columns (4) and (5), we modify the bandwidth to 0.5 and 1.5 times the optimal. For the smaller bandwidth the impact on job destruction remains insignificant. However, with a bandwidth equal to 1.5 times the optimal, the coefficient of interest becomes positive. It indicates that the treatment could have significantly increased job destruction by 1.2 percentage points for the treatment group, compared to the control group. Although the effect is positive, it should be noted that it is of a small magnitude: it is a destruction of 1 percentage point of employment triggered by an average minimum wage increase of 29.3% (simple average in nominal terms). On the other hand, in line with what was previously argued, as more observations are incorporated at the right side of the bandwidth, it is possible that the estimation bias grows. Mainly because this essentially implies making comparisons between low and high-income workers who are very likely to differ in unobservables. In Figure A.1 we show that the treatment effect is zero for smaller bandwidths and that is only becomes positive and statistically significant once we increase the bandwidth above 160 ARS. Moreover, the point estimates increase monotonically after this point. This is consistent with our main claim. When we expand the bandwidth, our point estimates converge to those found by the standard panel data approach. We

Table 3: Effects of the Minimum Wage on Match Destruction Rates for $k = 6$. Alternative Specifications.

	(1)	(2)	(3)	(4)	(5)	(6)
Treatment Effect ($T_{i,t=6}$)	0.006	0.002	0.000	-0.009	0.012**	-0.01
	(0.006)	(0.007)	(0.008)	(0.008)	(0.005)	(0.009)
Doughnut?	No	1 ARS	5 ARS	No	No	No
Optimal Bandwidth ×	1	1	1	0.5	1.5	1
Quadratic Term?	No	No	No	No	No	Yes
Obs.	87,502	65,222	57,631	43,078	126,097	80,541

Note: Standard errors between parenthesis.
*** p<0.01, ** p<0.05, * p<0.1.

argue that this is because the additional high income observations introduce bias. Section 5.3 discusses this issue in depth.

5.2 Heterogeneous Treatment Effects

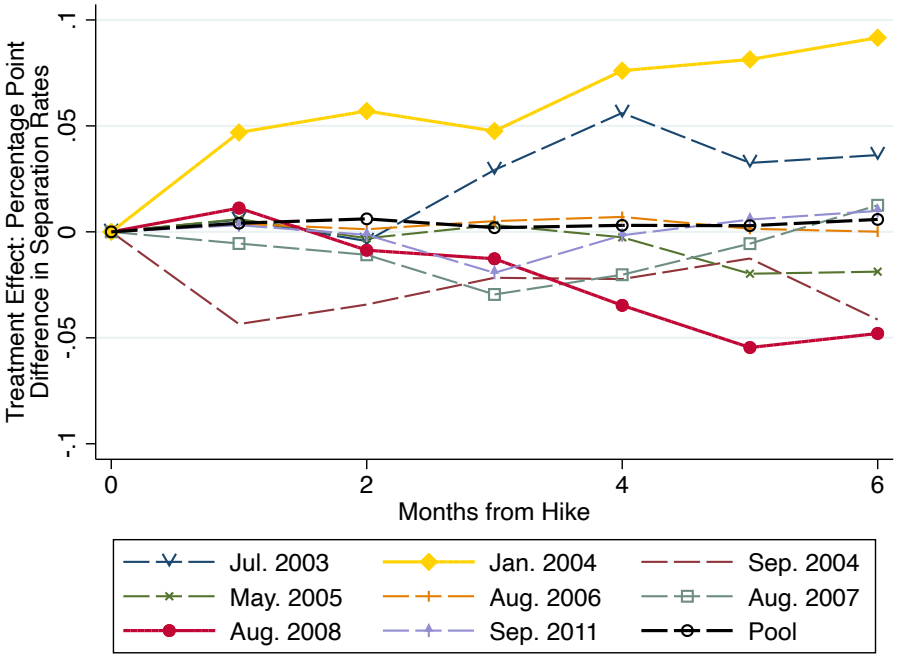
Given that our pooled results suggest that these minimum wage hikes had null effects on job separations, it is important to inquire whether this is actually the case or if we are witnessing the results from aggregation bias. Namely, the 8 events studied hitherto could have impacts of the opposite sign that counteract each other when aggregation is performed. After all, we are studying a very long period of time in a country that has experienced economic booms and busts. To test this, in Figure 4 we present the treatment effect estimates for each of the increases separately. We show the path of β_1 up to 6 months after each hike. We present the paths that have a statistically significant β_1 at $k = 6$ in solid lines (those that had a significant effect six months after the hike) and those that do not, in dashed lines. The complete set of results including point estimates and standard errors can be found in Appendix A (Table A.1). We also include further robustness checks in Tables A.2, A.3, and C.2; and in Figure A.1. In this section we limit ourselves to summarizing the most relevant findings.

The joint effect (under the "Pool" label, and which replicates Figure 3) is not statistically nor economically significant. In general, most coefficients are found around the zero line. We interpret this as evidence against a causal effect of raising the minimum wage on job destruction.

However, two events stand out as exceptions: the one in January 2004 and the one in August 2008. Both events yield large and statistically significant results with opposing signs. Six months after the hike in January of 2004, the treatment group had a job destruction rate 9.2 p.p. higher than that of the control group. This coefficient is highly statistically significant as it has p-value of less than 0.01. This result is robust to doughnut hole specifications and to bandwidth choice. Nonetheless, it ceases to be significant if we use a quadratic fit instead of a linear one. That is, we cannot rule out that the linear regression discontinuity design is confounding a nonlinear relationship between earnings and job destruction with a true causal effect.

The August 2008 increase, on the other hand, shows a different result: a plausibly causal drop of 4.8 percentage points in job destruction 6 months after the increase. That is, it seems that this raise in the minimum wage triggered a "protection" of employment which is statistically significant at the 10% level. This result is robust to a quadratic specification and to different bandwidths, although it loses significance in the doughnut-hole specification. However, it is important to note that, as shown in Figure B.2g, there is no evidence of abnormal density at any side of the cutoff, therefore, we can disregard the doughnut-hole estimates. An additional caveat is that this result does not argue that the increase in the minimum wage created jobs. This result suggests that labor relations directly affected by this particular increase in the minimum wage had a probability of being terminated 4.8 p.p. lower

Figure 4: Path Plot of β_1 for each Minimum Wage Hike.



Note: We exclude confidence intervals for visualization purposes. The full set of results with standard errors is reported in Appendix A. Solid lines represent paths with an statistically significant endpoint ($\beta_{1,t=6}$).

than what they would have had in the counterfactual scenario without an increase in the minimum wage. In other words, it seems that this hike lowered separations, but an important caveat is that we cannot make further claims regarding its effect on overall employment levels.

5.3 Placebo Experiments

One of the main reasons that motivates the use of a regression discontinuity design arises from the possible bias we might generate when estimating the Equation 2 via ordinary least squares. The greatest concern is that labor relations with salaries close to the minimum may differ from those with higher salaries in systematic, large, or even unobservable ways. For instance, minimum wage bound employees may naturally have higher turnover rates than others (Even and Macpherson, 2003). In Table 2 we present some evidence of this from observable characteristics: compared to all high-income workers, there are more women in the treatment group and on average they are younger, they are more engaged in the primary sector, they work in smaller companies and they reside less in Greater Buenos Aires. These differences in observable characteristics suggest that there may be other unobservable variables that correlate with the probability of working in a job at or near the minimum wage.

Table 2 provides an optimistic message in regards to the performance of our estimation strategy. Compared to the standard panel data approach, our preferred control group more closely matches the observable characteristics of the treatment group. And while this evidence might suggest that our method leads to better estimates than the standard method, this conjecture is largely un-testable. To perform a test, we would need to know the true value of β_1, run both specifications, and inspect which one leads to a closer point-estimate. Given that β_1 is, by definition, an unknown magnitude, a test like this is unfeasible in most circumstances. However, we can approximate a situation in which the value of β_1 is trivially known: a placebo experiment.

If we-incorrectly-assume that a minimum wage hike is implemented when none actually is, we can

Table 4: Standard Panel Data Approach v. RDD Performance in a Placebo Experiment.

Model/k	1	2	3	4	5	6
Standard Panel Data (OLS)						
Treatment Effect	0.021***	0.051***	0.074***	0.100***	0.113***	0.164***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)
Obs.	93,561	93,561	93,561	93,561	93,561	93,561
Regression Discontinuity Design						
Treatment Effect	0.006	0.013	0.026	0.028	0.003	0.004
	(0.011)	(0.017)	(0.020)	(0.021)	(0.019)	(0.024)
Obs.	5,718	5,492	5,784	5,795	8,759	6,456
Power(0.09)	1.000	0.939	0.874	0.801	0.886	0.706

Note: we assume a non-existent minimum wage increase from 200 to 300 ARS in August of 1998. Standard errors between parenthesis. *** p<0.01, ** p<0.05, * p<0.1

confidently assert that β_1 equals zero. Fortunately, Argentina in the 1990s offers a unique opportunity to perform several placebo experiments along these lines. Since 1993, the nominal minimum wage remained fixed at 200 ARS before being increased to 300 ARS in 2003. We will run several placebo experiments in which we assume a minimum wage hike from 200 to 300 ARS every in August between 1996 and 2000; similar to the one actually implemented in 2003. We report the estimates for the 1998 placebo and send the rest to Appendix A.

Columns (1) to (6) identify the estimated impact at different time windows. The treatment effect (i.e., the differential impact on job destruction produced by the increase in the minimum wage with respect to untreated workers), estimated via Ordinary Least Squares is economically relevant and statistically significant for all time horizons. On the other hand, with our Regression Discontinuity Design, these effects are much smaller and lose statistical significance.

These results show that the bias induced by the standard panel data approach can be quite large. In a period where the minimum wage was not increased, this method finds significant job destruction for labor relations with wages close to the minimum wage. As we mentioned, this bias potentially arises from unobservable differences between the control and treatment groups, such as unobserved job-specific heterogeneity in turnover rates.

Given that our empirical strategy employs a relatively small number of observations, it is possible that the claims from Table 4 are a result of a lack of statistical power. In other words, a concern embedded in our method is that it might minimize bias at the expense of statistical power. To address this concern, we present power calculations for a treatment effect of 0.09, the average found by the standard panel data approach. Although this varies according to the time horizon, the statistical power for our method ranges from 0.706 to almost 1. In all but one specification it is above the acceptable threshold of 0.8. The small statistical power in that exceptional specification is non-trivial. It suggests that administrative records are needed in order to detect an effect. Household or labor market surveys may not have enough density close to the cutoff to allow for informative statistical inference.

In Table A.4 we show the results for a series of additional placebo tests. The results are largely unchanged. Our specification fully eliminates bias in 27 out of the 36 placebo experiment. Moreover, our regression discontinuity design reduces its magnitude by between 13 and 80% in our worst performing scenarios. When inspecting these scenarios more closely, we see that these poor results are an anomaly. Figure A.2 shows that our estimator performs very adequately for bandwidths below 300 ARS[15]. Conversely, the standard panel data approach is biased in all 36 placebo experiments and the magnitude of the bias can be as high as 18.3 percentage points.

[15]This is more than 100% the value of the baseline minimum wage.

6 Discussion

In this paper, we develop a regression discontinuity design to evaluate the effects of nationwide minimum wage hikes on match destruction. Using underexploited administrative records of registered employment, we apply this method to most minimum wage increases implemented in Argentina between 2003 and 2011.

Overall, our main findings suggest that minimum wage hikes do not increase separation rates. Therefore, it seems that the mechanisms that links minimum wages and aggregate employment may not usually be rooted in job destruction. This result is in line with recent research suggesting that employers respond to minimum wage hikes by reducing new hires or increasing the productivity threshold needed for new matches. Thus, we highlight the need to consider general equilibrium effects to better understand the effects of minimum wages on aggregate employment. That is, we must consider the effects of minimum wages from job flows into and out off employment.

We do see that two particular events escape this generalization. The minimum wage increase in January 2004 increased the rate of job destruction by up to 24% (or 9.2 percentage points), six months after the increase. While the wage floor raise in August of 2008 reduced it by 19% (or 4.8pp). These findings suggest that i) it is possible for the minimum to impact separations, even when this does not seem to be the norm; and ii) practicioners should be cautious of aggregation bias when pooling together multiple minimum wage hikes and evaluating them as a single event.

We provide extensive evidence to support the use for our identification strategy over the standard panel data approach. Namely, we show that our method compares treated units with a much more comparable control group and that it performs significantly better in placebo experiments. In spite of this, we also recognize a fe challenges that practicioners should be mindful when applying similar quasi-experimental designs. First, even when we show that covariate imbalance is greatly reduced vis a vis the standard approach, it is not fully eliminated. And second, in our placebo experiments we show that a small number specifications lack the statical power to provide informative statistical inference. While this result can be easily mapped to the well understood bias-variance tradeoff, it highlights the need for large sample sizes to perform similar excercises. While these can be found in administrative records of registered employment, it is quite likely that household surveys lack the density for this kind of analysis.

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Appendix

A Full Results and Further Robustness Checks

This appendix serves two purposes. First, we present the results in as much detail as possible. We show the results of estimating the equation (2) for time windows of one to 6 months after every hike. We also include the full set of results for the pooled specification. Second, we include additional robustness checks. We show the results for "doughnut-hole" specifications where we exclude observations very close to the cut-off point to test the robustness of our main results to the manipulation of the assignment variable. Furthermore, we show the sensitivity of our main results to re-estimating equation (2) using a quadratic polynomial fit, as described in Section 4. Finally, we test the sensitivity of our results to different bandwidth choices. We also include a series of additional placebo experiments and test the performance of our identification strategy in placebo tests under different bandwidth choice.

Table A.1 presents our main results in detail in the section entitled "No Hole". We find that overall, Argentina's minimum wage policy in the 2003-2011 period had no effect on job destruction. However, two episodes stand out above the rest. For January 2004, we detected strong job destruction: treatment group employment relationships were 9.2 percentage points more likely to be destroyed 6 months after the raise than the control group. For August 2008 we detected an effect with the opposite sign: 6 months after the increase, the treatment group had a 4.8 percentage point lower probability of being destroyed than the control group.

The rest of Table A.1 shows that the result corresponding to January 2004 is robust to the possible manipulation of the assignment variable since the results are qualitatively identical and quantitatively similar in the specifications with holes of 1 and 5 ARS. For the result corresponding to August 2008, the results when excluding the donut holes are quantitatively similar to the main ones but lose statistical significance. However, this result must be taken with a grain of salt as it does not directly affect the main results, since in Appendix B we show that there is no indication that there has been manipulation in the assignment variable in this case study. In other words, for the August 2008 increase, the results with a doughnut hole are not very informative because there is no need to check its robustness against manipulation, since no indications of its existence are directly observed.

In Table A.2 we present our main results, with the linear fit presented in the equation (2), and a robustness test in which we apply a quadratic fit. We show that while the coefficient for the hike in January 2004 loses statistical significance; the effect found for August 2008 does not.

In Table A.3 we show the sensitivity of our results to different bandwidth choices. Specifically, we show the results for optimal bandwidth, half optimal, and 1.5 times optimal. In all cases our main claims are robust to different bandwidths, except for the pooled specification 6 months after the hike, where we detect an increase in separation rates. In Figure A.1 we delve deeper into this result. We plot the treatment effect for the pooled specification for all bandwidth sizes between 10 an 600 ARS in 10 by 10 increments. We show that the treatment effect is not statistically different from zero for bandwidths below 160 ARS. It is important to highlight that this is a very large figure as it represents between 9.2 and 80% of the running variable. Quite notably, this Figure also provides some grounds for our main claims. Expanding the bandwidth implies making a comparison between low and high income workers. In the extreme case, when the bandwidth is as large as the support of the running variable, our estimates should converge to the standard method. This explains why our point estimate of interest increases monotonically with the size of the bandwidth.

We finish this section by extending the evidence of the performance of our estimators in placebo experiments. In Table A.4 we report a series of placebo experiments. Our method always performs better than the standard. Although in some specifications we have statistical power below 80. Finally, in Figure A.2 we show that our estimates, correctly, yield a null effect of placebo experiments for most bandwidths below 310 ARS; except for a small region between 20 and 50 ARS. Altogether, this evidence implies that i) our method performs substantially better than the standard and ii) it is possible to find large and systematic differences in separation rates between low and high wage workers even in the

absence of a minimum wage hike. Therefore, further research should avoid confounding this pattern with a causal effect of the minimum wage.

Table A.1: Effects of the Minimum Wage on Match Destruction Rates. Full Results and Doughnut-Hole Specifications.

Event/Doughnut-Hole	No Hole						1 ARS						5 ARS					
k	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
Pool																		
Treatment Effect	0.004	0.006	0.002	0.003	0.003	0.006	0.005	-0.003	-0.008	-0.006	0.005	0.002	0.008**	-0.001	-0.006	-0.000	-0.003	-0.000
Standard Error	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.003)	(0.005)	(0.006)	(0.007)	(0.006)	(0.007)	(0.004)	(0.006)	(0.007)	(0.007)	(0.008)	(0.008)
Obs.	101,789	77,345	70,516	71,341	84,012	87,502	80,724	63,597	59,637	60,366	94,249	65,222	71,565	56,338	58,275	59,965	58,642	57,631
July 2003																		
Treatment Effect	0.006	-0.004	0.029	0.056	0.033	0.036	-0.008	-0.019	0.006	0.042	0.021	0.030	0.008	0.005	0.116*	0.201**	0.136*	0.179**
Standard Error	(0.019)	(0.025)	(0.034)	(0.037)	(0.033)	(0.035)	(0.022)	(0.027)	(0.040)	(0.042)	(0.037)	(0.039)	(0.029)	(0.042)	(0.070)	(0.079)	(0.073)	(0.083)
Obs.	3,921	3,557	2,130	2,119	2,958	2,794	3,457	3,404	1,890	1,967	2,747	2,591	2,635	2,281	1,237	1,183	1,372	1,218
January 2004																		
Treatment Effect	0.047**	0.057**	0.048*	0.076***	0.081***	0.092***	0.054**	0.049*	0.041	0.072**	0.077**	0.091***	0.064**	0.037	0.049	0.076*	0.097**	0.117***
Standard Error	(0.021)	(0.025)	(0.028)	(0.029)	(0.030)	(0.030)	(0.023)	(0.027)	(0.030)	(0.031)	(0.031)	(0.031)	(0.028)	(0.035)	(0.041)	(0.041)	(0.039)	(0.039)
Obs.	4,503	4,393	4,167	4,271	4,372	4,366	4,275	4,211	4,046	4,056	4,211	4,213	3,726	3,295	2,985	3,133	3,368	3,411
September 2004																		
Treatment Effect	-0.044**	-0.034	-0.022	-0.022	-0.013	-0.041	-0.046*	-0.043	-0.020	-0.032	-0.027	-0.060	-0.077	-0.024	0.005	0.033	0.013	0.043
Standard Error	(0.018)	(0.024)	(0.023)	(0.025)	(0.031)	(0.034)	(0.024)	(0.031)	(0.027)	(0.030)	(0.037)	(0.041)	(0.073)	(0.076)	(0.066)	(0.072)	(0.089)	(0.091)
Obs.	2,607	2,787	3,933	3,918	2,985	2,496	2,060	2,328	3,548	3,389	2,561	2,203	744	981	1,317	1,309	1,127	1,148
May 2005																		
Treatment Effect	0.006	-0.003	0.003	-0.003	-0.020	-0.019	0.005	-0.001	0.015	-0.002	-0.013	-0.005	0.004	-0.011	0.000	-0.007	-0.034	-0.028
Standard Error	(0.009)	(0.012)	(0.014)	(0.015)	(0.015)	(0.018)	(0.010)	(0.014)	(0.015)	(0.018)	(0.018)	(0.020)	(0.012)	(0.017)	(0.019)	(0.024)	(0.025)	(0.026)
Obs.	8,153	8,265	8,325	9,729	10,367	8,525	7,402	7,529	8,235	7,257	7,911	7,097	7,224	6,656	7,394	5,799	5,938	6,087
August 2006																		
Treatment Effect	0.004	0.001	0.005	0.007	0.001	0.000	-0.004	-0.004	-0.012	-0.014	-0.012	-0.008	-0.005	-0.021	-0.017	-0.022	-0.023	-0.013
Standard Error	(0.009)	(0.013)	(0.015)	(0.017)	(0.017)	(0.018)	(0.011)	(0.014)	(0.017)	(0.019)	(0.020)	(0.021)	(0.014)	(0.018)	(0.022)	(0.024)	(0.025)	(0.026)
Obs.	7,145	8,484	8,565	8,045	8,633	8,536	6,162	7,632	7,168	7,455	7,512	7,442	5,016	6,100	5,841	5,801	5,907	6,003
August 2007																		
Treatment Effect	-0.006	-0.011	-0.030	-0.020	-0.006	0.013	-0.000	-0.006	-0.036	-0.021	-0.011	0.010	-0.003	-0.026	-0.048	-0.027	-0.013	0.003
Standard Error	(0.011)	(0.018)	(0.022)	(0.026)	(0.025)	(0.024)	(0.012)	(0.019)	(0.025)	(0.029)	(0.027)	(0.025)	(0.016)	(0.027)	(0.037)	(0.040)	(0.042)	(0.036)
Obs.	5,105	4,562	3,745	3,641	4,361	5,214	5,180	4,439	3,404	3,362	4,080	4,934	4,058	3,099	2,535	2,646	2,609	3,572
August 2008																		
Treatment Effect	0.011	-0.009	-0.013	-0.035	-0.055**	-0.048*	0.012	0.003	-0.013	0.007	-0.017	-0.008	0.026	0.008	0.010	0.009	-0.016	-0.002
Standard Error	(0.012)	(0.017)	(0.022)	(0.022)	(0.024)	(0.026)	(0.013)	(0.020)	(0.023)	(0.026)	(0.027)	(0.030)	(0.018)	(0.024)	(0.029)	(0.031)	(0.038)	(0.043)
Obs.	5,169	4,701	3,841	4,634	4,415	3,991	4,641	4,113	4,069	3,711	3,960	3,446	3,711	3,807	3,573	3,625	2,803	2,593
September 2011																		
Treatment Effect	0.003	-0.001	-0.019	-0.002	0.006	0.010	-0.004	-0.015	-0.034*	-0.001	-0.000	0.011	0.007	-0.006	-0.026	0.016	0.019	0.039
Standard Error	(0.010)	(0.014)	(0.017)	(0.018)	(0.020)	(0.020)	(0.011)	(0.016)	(0.018)	(0.019)	(0.021)	(0.021)	(0.012)	(0.018)	(0.023)	(0.024)	(0.024)	(0.027)
Obs.	6,205	6,303	6,092	6,451	6,326	6,582	5,872	5,898	5,866	6,066	6,240	6,269		5,746	5,418	4,757	5,221	5,492
5,010																		

Note: Standard errors between brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A.2: Effects of the Minimum Wage on Match Destruction Rates. Linear and Quadratic Fits.

Event/Polynomial Order	Lineal						Cuadrático					
k	1	2	3	4	5	6	1	2	3	4	5	6
Todos												
Treatment Effect	0.004	0.006	0.002	0.003	0.003	0.006	0.004	-0.002	-0.014*	-0.017**	-0.018**	-0.010
Standard Error	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.005)	(0.006)	(0.008)	(0.008)	(0.009)	(0.009)
Obs.	101,789	77,345	70,516	71,341	84,012	87,502	90,713	91,350	79,076	77,058	74,990	80,541
July 2003												
Treatment Effect	0.006	-0.004	0.029	0.056	0.033	0.036	0.014	0.013	0.025	0.053	0.047	0.045
Standard Error	(0.019)	(0.025)	(0.034)	(0.037)	(0.033)	(0.035)	(0.025)	(0.032)	(0.036)	(0.040)	(0.040)	(0.042)
Obs.	3,921	3,557	2,130	2,119	2,958	2,794	4,539	4,285	4,366	4,177	4,377	4,088
January 2004												
Treatment Effect	0.047**	0.057**	0.048*	0.076***	0.081***	0.092***	0.005	0.023	0.044	0.067	0.046	0.050
Standard Error	(0.021)	(0.025)	(0.028)	(0.029)	(0.030)	(0.030)	(0.031)	(0.037)	(0.041)	(0.044)	(0.044)	(0.044)
Obs.	4,503	4,393	4,167	4,271	4,372	4,366	4,872	4,731	4,476	4,297	4,547	4,616
September 2004												
Treatment Effect	-0.044**	-0.034	-0.022	-0.022	-0.013	-0.041	-0.032	-0.025	-0.009	0.003	0.005	-0.013
Standard Error	(0.018)	(0.024)	(0.023)	(0.025)	(0.031)	(0.034)	(0.021)	(0.032)	(0.034)	(0.037)	(0.041)	(0.042)
Obs.	2,607	2,787	3,933	3,918	2,985	2,496	4,259	3,590	3,944	3,896	3,474	3,563
May 2005												
Treatment Effect	0.006	-0.003	0.003	-0.003	-0.020	-0.019	0.014	0.009	0.001	-0.020	-0.034	-0.018
Standard Error	(0.009)	(0.012)	(0.014)	(0.015)	(0.015)	(0.018)	(0.011)	(0.018)	(0.021)	(0.023)	(0.024)	(0.026)
Obs.	8,153	8,265	8,325	9,729	10,367	8,525	10,874	7,528	7,896	8,060	8,039	8,406
August 2006												
Treatment Effect	0.004	0.001	0.005	0.007	0.001	0.000	0.017	0.038*	0.040	0.050*	0.029	0.041
Standard Error	(0.009)	(0.013)	(0.015)	(0.017)	(0.017)	(0.018)	(0.012)	(0.021)	(0.025)	(0.027)	(0.026)	(0.029)
Obs.	7,145	8,484	8,565	8,045	8,633	8,536	8,829	6,540	6,667	6,748	7,492	6,886
August 2007												
Treatment Effect	-0.006	-0.011	-0.030	-0.020	-0.006	0.013	-0.007	-0.015	-0.030	-0.031	-0.023	0.024
Standard Error	(0.011)	(0.018)	(0.022)	(0.026)	(0.025)	(0.024)	(0.014)	(0.020)	(0.023)	(0.028)	(0.029)	(0.028)
Obs.	5,105	4,562	3,745	3,641	4,361	5,214	8,099	8,146	7,828	6,726	7,067	8,074
August 2008												
Treatment Effect	0.011	-0.009	-0.013	-0.035	-0.055**	-0.048*	0.018	-0.011	-0.029	-0.033	-0.050*	-0.084***
Standard Error	(0.012)	(0.017)	(0.022)	(0.022)	(0.024)	(0.026)	(0.015)	(0.020)	(0.023)	(0.027)	(0.027)	(0.027)
Obs.	5,169	4,701	3,841	4,634	4,415	3,991	6,557	7,659	7,424	6,895	7,882	9,082
September 2011												
Treatment Effect	0.003	-0.001	-0.019	-0.002	0.006	0.010	0.005	-0.010	-0.029	-0.012	0.005	0.013
Standard Error	(0.010)	(0.014)	(0.017)	(0.018)	(0.020)	(0.020)	(0.013)	(0.017)	(0.020)	(0.024)	(0.025)	(0.025)
Obs.	6,205	6,303	6,092	6,451	6,326	6,582	6,921	9,174	8,805	7,245	8,127	8,330

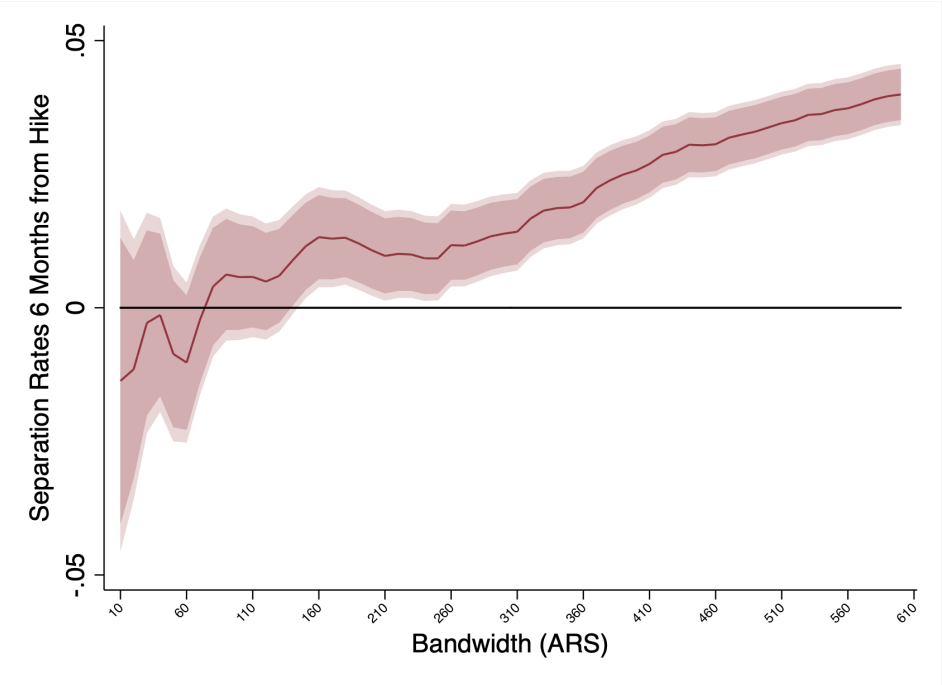
Note: Standard errors between brackets. *** p<0.01, ** p<0.05, * p<0.1.

Table A.3: Effects of the Minimum Wage on Match Destruction Rates. Sensitivity to Bandwidth Choice.

Event/Bandwidth	Optimal						0.5 · Optimal						1.5 · Optimal					
k	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
Pool																		
Treatment Effect	0.004	0.006	0.002	0.003	0.003	0.006	0.005	-0.004	-0.006	-0.007	-0.013	-0.009	0.006**	0.007*	0.003	0.005	0.006	0.012**
Standard Error	(0.003)	(0.005)	(0.006)	(0.006)	(0.006)	(0.006)	(0.004)	(0.006)	(0.008)	(0.008)	(0.008)	(0.008)	(0.002)	(0.004)	(0.005)	(0.005)	(0.005)	(0.005)
Obs.	101,789	77,345	70,516	71,341	84,012	87,502	51,039	37,939	34,568	34,963	41,306	43,078	150,268	112,905	103,832	104,996	122,104	126,097
July 2003																		
Treatment Effect	0.006	-0.004	0.029	0.056	0.033	0.036	0.019	0.036	0.009	0.034	0.038	0.032	0.002	-0.012	0.005	0.020	0.003	0.009
Standard Error	(0.019)	(0.025)	(0.034)	(0.037)	(0.033)	(0.035)	(0.026)	(0.033)	(0.046)	(0.051)	(0.045)	(0.047)	(0.016)	(0.020)	(0.029)	(0.031)	(0.027)	(0.028)
Obs.	3,921	3,557	2,130	2,119	2,958	2,794	1,977	1,790	1,084	1,078	1,490	1,411	6,795	6,204	3,119	3,101	4,521	4,277
January 2004																		
Treatment Effect	0.047**	0.057**	0.048*	0.076***	0.081***	0.092***	0.019	0.051	0.080**	0.099**	0.066	0.077*	0.036*	0.048**	0.041	0.073***	0.072***	0.075***
Standard Error	(0.021)	(0.025)	(0.028)	(0.029)	(0.030)	(0.030)	(0.028)	(0.034)	(0.039)	(0.040)	(0.040)	(0.041)	(0.020)	(0.024)	(0.026)	(0.028)	(0.028)	(0.028)
Obs.	4,503	4,393	4,167	4,271	4,372	4,366	2,444	2,352	2,151	2,236	2,335	2,320	5,849	5,660	5,299	5,447	5,633	5,617
September 2004																		
Treatment Effect	-0.044**	-0.034	-0.022	-0.022	-0.013	-0.041	-0.013	-0.004	0.005	0.022	0.004	-0.017	-0.023	-0.027	-0.000	-0.003	-0.016	-0.035
Standard Error	(0.018)	(0.024)	(0.023)	(0.025)	(0.031)	(0.034)	(0.025)	(0.034)	(0.032)	(0.035)	(0.042)	(0.046)	(0.015)	(0.020)	(0.019)	(0.021)	(0.025)	(0.028)
Obs.	2,607	2,787	3,933	3,918	2,985	2,496	1,300	1,374	1,941	1,930	1,467	1,243	3,955	4,236	5,963	5,933	4,539	3,775
May 2005																		
Treatment Effect	0.006	-0.003	0.003	-0.003	-0.020	-0.019	0.018	0.009	0.009	-0.010	-0.019	-0.017	-0.003	0.003	0.004	-0.005	-0.024*	-0.011
Standard Error	(0.009)	(0.012)	(0.014)	(0.015)	(0.015)	(0.018)	(0.012)	(0.016)	(0.019)	(0.020)	(0.021)	(0.024)	(0.007)	(0.010)	(0.012)	(0.012)	(0.012)	(0.014)
Obs.	8,153	8,265	8,325	9,729	10,367	8,525	4,033	4,130	4,160	4,745	5,172	4,284	12,437	12,699	12,806	15,515	16,500	13,368
August 2006																		
Treatment Effect	0.004	0.001	0.005	0.007	0.001	0.000	0.024*	0.034*	0.031	0.017	0.020	0.024	0.003	0.001	0.002	0.002	0.001	-0.003
Standard Error	(0.009)	(0.013)	(0.015)	(0.017)	(0.017)	(0.018)	(0.013)	(0.018)	(0.021)	(0.024)	(0.024)	(0.025)	(0.008)	(0.011)	(0.013)	(0.014)	(0.015)	(0.015)
Obs.	7,145	8,484	8,565	8,045	8,633	8,536	3,486	4,069	4,114	3,763	4,141	4,102	10,621	12,033	12,147	11,417	12,248	12,094
August 2007																		
Treatment Effect	-0.006	-0.011	-0.030	-0.020	-0.006	0.013	-0.015	-0.029	-0.048	-0.027	-0.019	0.023	-0.011	-0.011	-0.025	-0.014	-0.008	0.006
Standard Error	(0.011)	(0.018)	(0.022)	(0.026)	(0.025)	(0.024)	(0.016)	(0.025)	(0.031)	(0.036)	(0.034)	(0.033)	(0.009)	(0.014)	(0.018)	(0.021)	(0.020)	(0.019)
Obs.	5,105	4,562	3,745	3,641	4,361	5,214	2,589	2,337	1,811	1,735	2,137	2,627	7,590	6,894	5,749	5,496	6,658	7,727
August 2008																		
Treatment Effect	0.011	-0.009	-0.013	-0.035	-0.055**	-0.048*	0.010	-0.001	-0.029	-0.031	-0.058*	-0.080**	0.002	-0.004	-0.025	-0.036**	-0.044**	-0.039*
Standard Error	(0.012)	(0.017)	(0.022)	(0.022)	(0.024)	(0.026)	(0.015)	(0.023)	(0.031)	(0.030)	(0.033)	(0.037)	(0.010)	(0.014)	(0.018)	(0.018)	(0.020)	(0.022)
Obs.	5,169	4,701	3,841	4,634	4,415	3,991	2,507	2,310	1,812	2,257	2,099	1,881	7,812	7,290	5,742	7,186	6,783	6,039
September 2011																		
Treatment Effect	0.003	-0.001	-0.019	-0.002	0.006	0.010	-0.006	-0.020	-0.033	-0.028	-0.019	-0.010	-0.005	-0.002	-0.019	-0.002	-0.007	0.016
Standard Error	(0.010)	(0.014)	(0.017)	(0.018)	(0.020)	(0.020)	(0.014)	(0.019)	(0.023)	(0.024)	(0.027)	(0.027)	(0.009)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)
Obs.	6,205	6,303	6,092	6,451	6,326	6,582	3,104	3,164	3,013	3,233	3,178	3,341	8,855	9,114	8,663	9,325	9,164	9,522

Note: Standrad errors between brackets. *** p<0.01, ** p<0.05, * p<0.1.

Figure A.1: Effect of a Minimum Wage Hike on Match Destruction 6 Months From Hike for Different Bandwidth Choice.



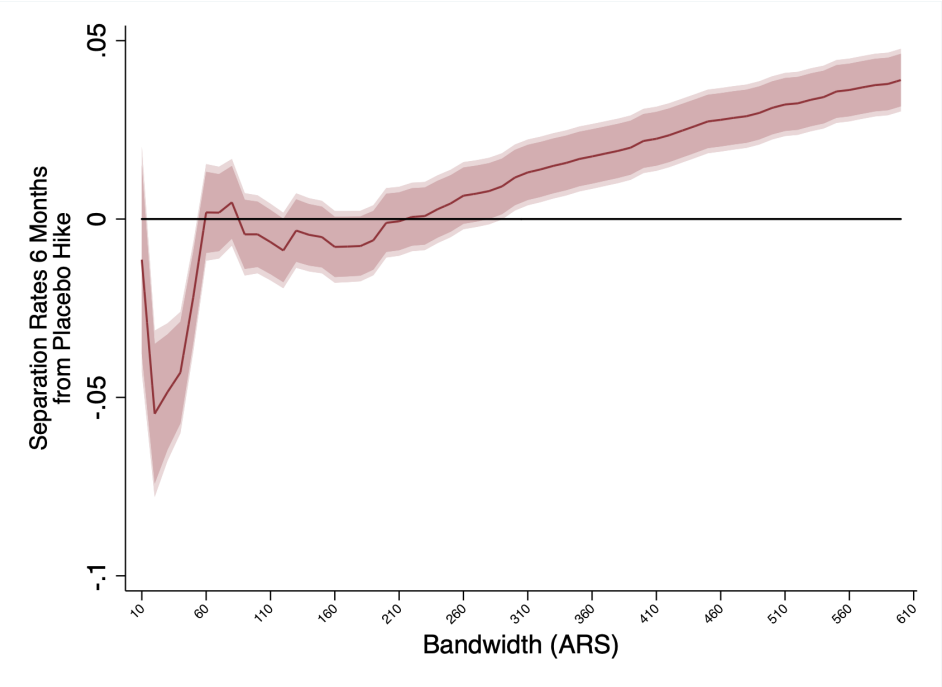
Note: The reported point estimates come from estimating Equation 2. We show the results for different bandwidths between 10 and 600 ARS in 10 by 10 increments. Dark shaded area represents 90%, light shaded area 95% conventional confidence intervals.

Table A.4: Standard Panel Data Approach v. RDD Performance in a Placebo Experiments.

Model/k	1	2	3	4	5	6
a) 1996						
Standard Panel Data (OLS)						
Treatment Effect	0.031***	0.063***	0.083***	0.098***	0.107***	0.183***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.008)	(0.009)
Obs.	79,430	79,430	79,430	79,430	79,430	79,430
Regression Discontinuity Design						
Treatment Effect	0.005	0.003	0.001	-0.013	-0.019	-0.047
	(0.012)	(0.016)	(0.020)	(0.023)	(0.024)	(0.029)
Obs.	4,970	6,410	5,005	4,601	4,626	4,244
Power(0.09)	0.994	0.945	0.787	0.687	0.653	0.507
b) 1997						
Standard Panel Data (OLS)						
Treatment Effect	0.020***	0.039***	0.059***	0.071***	0.092***	0.156***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)
Obs.	86,393	86,393	86,393	86,393	86,393	86,393
Regression Discontinuity Design						
Treatment Effect	-0.008	-0.012	-0.042**	-0.061***	-0.055***	-0.136***
	(0.010)	(0.015)	(0.018)	(0.019)	(0.020)	(0.028)
Obs.	7,899	6,512	6,350	6,347	7,072	4,461
Power(0.09)	1.000	0.972	0.919	0.855	0.837	0.571
c) 1998						
Standard Panel Data (OLS)						
Treatment Effect	0.021***	0.051***	0.074***	0.100***	0.113***	0.164***
	(0.004)	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)
Obs.	93,561	93,561	93,561	93,561	93,561	93,561
Regression Discontinuity Design						
Treatment Effect	0.006	0.013	0.026	0.028	0.003	0.004
	(0.011)	(0.017)	(0.020)	(0.021)	(0.019)	(0.024)
Obs.	5,718	5,492	5,784	5,795	8,759	6,456
Power(0.09)	1.000	0.939	0.874	0.801	0.886	0.706
d) 1999						
Standard Panel Data (OLS)						
Treatment Effect	0.018***	0.051***	0.073***	0.087***	0.097***	0.163***
	(0.003)	(0.005)	(0.006)	(0.006)	(0.007)	(0.008)
Obs.	93,241	93,241	93,241	93,241	93,241	93,241
Regression Discontinuity Design						
Treatment Effect	-0.003	-0.003	0.010	0.020	0.042*	0.000
	(0.010)	(0.014)	(0.017)	(0.019)	(0.025)	(0.026)
Obs.	7,498	8,213	7,736	7,346	4,492	5,651
Power(0.09)	1.000	0.981	0.924	0.868	0.653	0.601
e) 2000						
Standard Panel Data (OLS)						
Treatment Effect	0.017***	0.041***	0.057***	0.073***	0.084***	0.138***
	-0.003	-0.005	-0.006	-0.006	-0.007	-0.008
Obs.	92917	92917	92917	92917	92917	92917
Regression Discontinuity Design						
Treatment Effect	0.014	0.004	-0.013	-0.025	-0.035	-0.101***
	-0.011	-0.016	-0.019	-0.02	-0.021	-0.027
Obs.	5075	6232	5743	6186	6041	4650
Power(0.09)	1	0.961	0.886	0.846	0.798	0.585
f) Pool						
Standard Panel Data (OLS)						
Treatment Effect	0.021***	0.049***	0.069***	0.086***	0.099***	0.160***
	(0.002)	(0.002)	(0.003)	(0.003)	(0.003)	(0.004)
Obs.	445,542	445,542	445,542	445,542	445,542	445,542
Regression Discontinuity Design						
Treatment Effect	-0.001	-0.002	-0.011	-0.017*	-0.021**	-0.056***
	(0.005)	(0.008)	(0.010)	(0.010)	(0.009)	(0.012)
Obs.	24,290	25,360	22,711	25,091	35,968	23,688
Power(0.09)	1.000	1.000	1.000	1.000	1.000	0.998

Note: we assume a non-existent minimum wage increases from 200 to 300 ARS every August. Standard errors between parenthesis.
*** p<0.01, ** p<0.05, * p<0.1

Figure A.2: Effect of a Placebo Minimum Wage Hike on Match Destruction 6 Months From Hike for Different Bandwidth Choice.



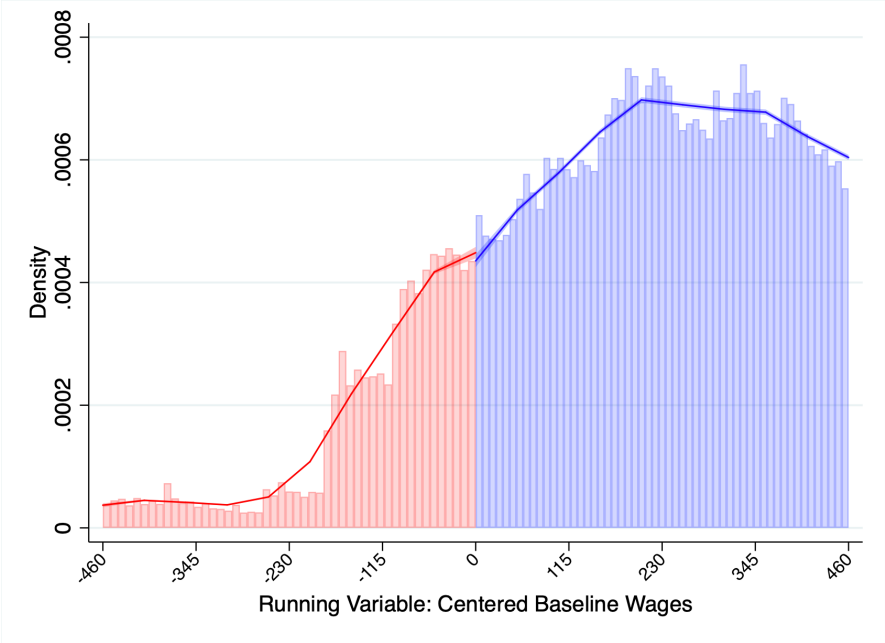
Note: The reported point estimates come from estimating Equation 2 in a placebo experiment where we pool together all records from 1996 and 2000 and assume a false minimum wage from 200 to 300 ARS every august. We show the results for different bandwidths between 10 and 600 ARS in 10 by 10 increments. Dark shaded area represents 90%, light shaded area 95% conventional confidence intervals.

B Density Tests

In this appendix, we show formal density difference tests for the running variable, pre-hike wages centered on the new minimum wage $(W - MW')_{i,t=0}$, at each side of the cutoff point. Typically, significant density differences are interpreted as potential manipulation of the treatment status. We use the Cattaneo et al. (2018) test mainly because it represents an improvement over McCrary (2008) in that it does not require pre-bining the observations.

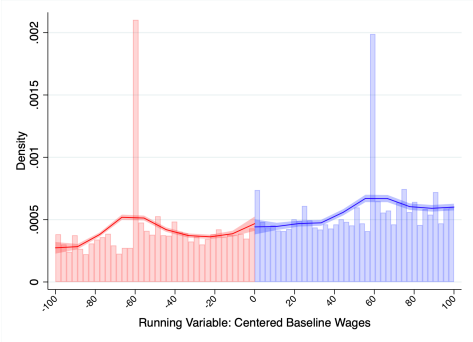
Below we graphically report the graphical tests and their associated T-statistics and p-values. We use second degree polynomials to construct the point estimator for the density at each point and also its standard deviation.

Figure B.1: Density Test. Pool.



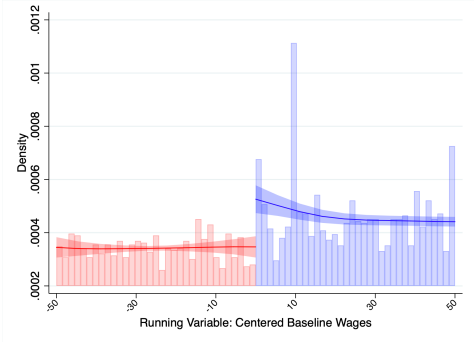
Note: T=-1.78, P-value=0.08.

Figure B.2: Density Test. Event by Event.



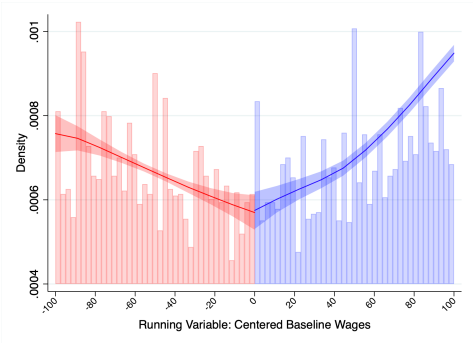
Note: T=1.360, P-value=0.174.

(a) July 2003



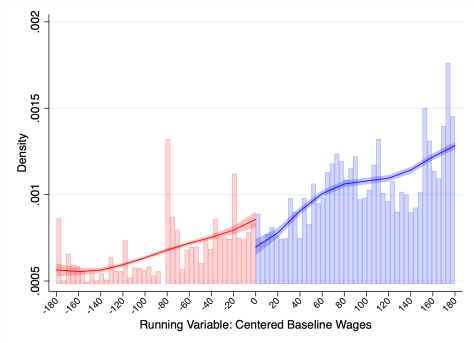
Note: T=4.995, P-value=0.000.

(b) January 2004



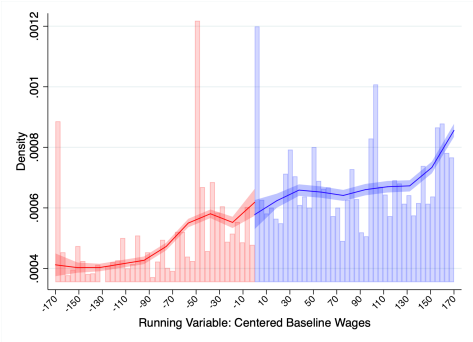
Note: T=0.313, P-value=0.754.

(c) September 2004



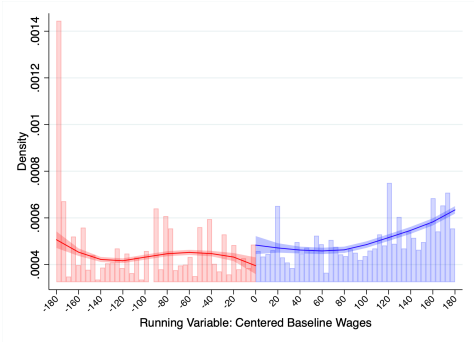
Note: T=-4.783, P-value=0.000.

(d) May 2005



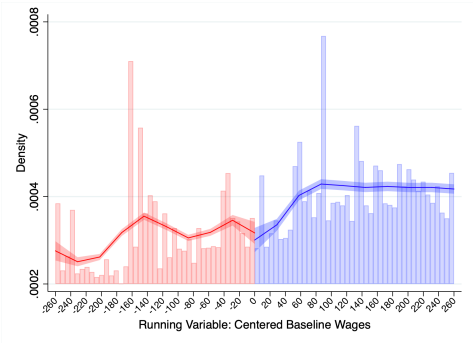
Note: T= 2.205, P-value= 0.027.

(e) August 2006



Note: T=3.305, P-value=0.001.

(f) Agust 2007



Note: T=-0.836, P-value=0.403.

(g) August 2008

Note: T=1.088, P-value=0.277.

(h) September de 2011

C Covariate Smoothness

It is usual to couple the results of a regression discontinuity design with covariate smooth. Since these designs assume local randomization in the treatment assignment, it is not necessary to include covariates directly in the estimation. However, verifying that there are no systematic differences in observable terms in the treatment and control groups reinforces the credibility of this assumption. Next, we carry out a battery of tests in which we estimate the equation (2), but taking a series of observable characteristics as the dependent variable. Table C.1 presents these results.

We find that, in the pooled specification, there are some differences mainly in geographic location, gender and economic sector. We disregard the differences in year of birth because, despite being statistically significant, they are economically negligible. We also found that these differences originate mainly in the last two case studies: August 2008 and September 2011. These results highlight the importance of formally contrasting the similarity of the treatment and control groups. Even when our method improves inter-group comparability[16] compared to the standard panel data approach, some differences still persist.

To provide a more formal test of this conjecture, in Figure C.1 we show that the differences between the control and the treatment group increase with bandwidth size. That is, as we deviate from our regression discontinuity into the standard panel data approach, the differences between groups become larger (in absolute value).

Even when it is clear that our method represents an improvement over the standard, it is still important to address how sensitive are our results to correcting for covariate imbalance. In Table C.2, we control for sex, year of birth, and dummies for working in Buenos Aires, in large firms, and in the primary sector. Our main results hold under this empirical excercise.

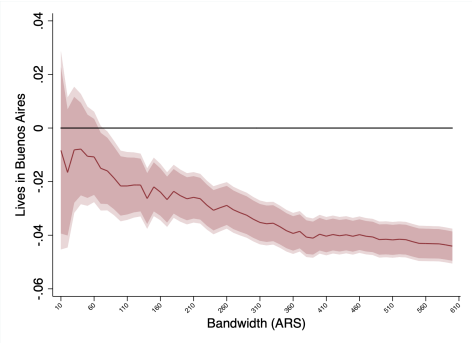
[16] See Table 2.

Table C.1: Covariate Smoothness Test

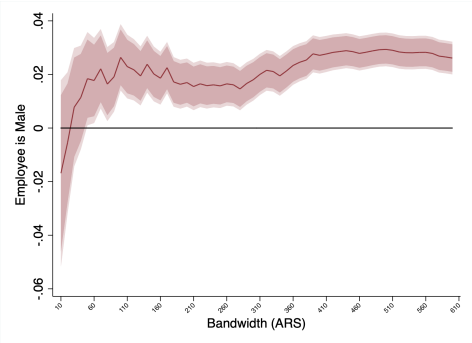
Event/Covariate	Buenos Aires	Male	Date of Birth (Year)	Firm Size> 50	Primary Sector
Pool					
Treatment Effect	-0.021***	0.019**	0.466**	-0.014	0.037***
Standard Error	(0.008)	(0.008)	(0.217)	(0.009)	(0.007)
Obs.	68,643	50,297	51,164	40,325	32,667
July 2003					
Treatment Effect	0.029	-0.064*	1.054	0.033	0.050**
Standard Error	(0.039)	(0.035)	(0.845)	(0.041)	(0.024)
Obs.	2,348	2,950	3,130	2,195	2,593
January 2004					
Treatment Effect	0.040	0.028	0.835	-0.033	0.034*
Standard Error	(0.030)	(0.030)	(0.769)	(0.031)	(0.019)
Obs.	4,493	4,162	4,308	4,064	4,563
September 2004					
Treatment Effect	0.071**	-0.027	0.621	0.053	-0.020
Standard Error	(0.033)	(0.043)	(0.709)	(0.036)	(0.021)
Obs.	3,518	1,763	4,334	2,848	2,515
May 2005					
Treatment Effect	0.018	0.001	0.336	0.039	-0.005
Standard Error	(0.023)	(0.020)	(0.580)	(0.025)	(0.023)
Obs.	6,586	7,579	6,719	5,566	2,824
August 2006					
Treatment Effect	-0.002	-0.086***	-0.958	-0.018	0.045
Standard Error	(0.025)	(0.028)	(0.733)	(0.026)	(0.029)
Obs.	5,728	3,722	4,292	5,129	1,883
August 2007					
Treatment Effect	-0.008	0.088***	-0.016	0.055	0.027
Standard Error	(0.029)	(0.029)	(0.684)	(0.035)	(0.023)
Obs.	4,391	3,885	5,037	3,070	3,406
August 2008					
Treatment Effect	-0.067**	0.041	0.303	-0.095***	0.087***
Standard Error	(0.031)	(0.027)	(0.668)	(0.033)	(0.023)
Obs.	4,136	4,291	5,169	3,123	2,981
September 2011					
Treatment Effect	-0.075***	0.071***	0.640	-0.108***	0.251***
Standard Error	(0.021)	(0.024)	(0.746)	(0.029)	(0.031)
Obs.	8,099	4,917	4,070	3,273	1,542

Note: Standard errors between brackets. *** p<0.01, ** p<0.05, * p<0.1.

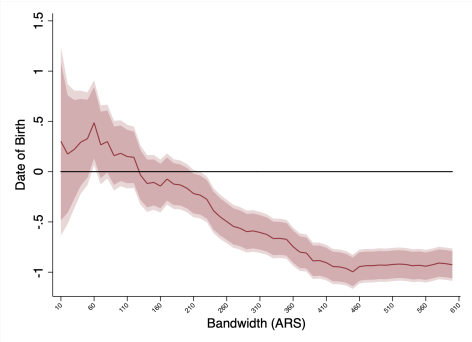
Figure C.1: Covariate Imbalance for Different Bandwidth Choice.



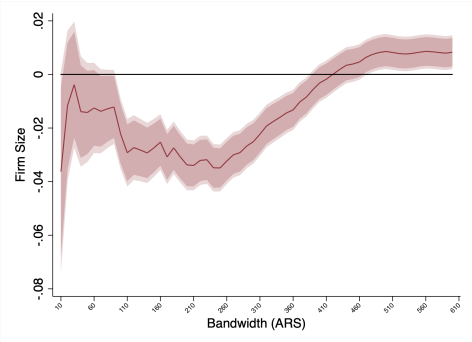
(a) GBA.



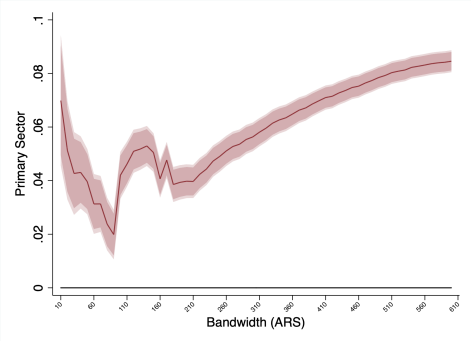
(b) Male.



(c) Date of Birth (Year).



(d) Firm Size > 50.



(e) Primary Sector.

Note: The reported point estimates come from estimating Equation 2 but taking the covariates as dependent variables. We show the results for different bandwidths between 10 and 600 ARS in 10 by 10 increments. Dark shaded area represents 90%, light shaded area 95% conventional confidence intervals.

Table C.2: Effect of a Minimum Wage Hike on Match Destruction. Specification with Covariates.

Event/Bandwidth	Optimal						0.5 × Optimal						1.5 × Optimal					
k	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
Pool																		
Treatment Effect	0.006*	0.005	0.004	0.006	-0.004	0.006	0.004	-0.001	-0.006	-0.006	-0.008	-0.006	0.006**	0.009***	0.008**	0.010**	0.006	0.012**
Standard Error	(0.003)	(0.004)	(0.005)	(0.005)	(0.007)	(0.006)	(0.005)	(0.006)	(0.007)	(0.007)	(0.009)	(0.008)	(0.003)	(0.004)	(0.004)	(0.004)	(0.006)	(0.005)
Obs.	78,006	87,111	87,945	95,237	58,942	86,392	38,323	43,545	43,888	47,812	28,928	43,125	113,458	126,119	127,155	139,945	88,432	124,950
July 2003																		
Treatment Effect	0.001	0.004	0.033	0.037	0.018	0.027	0.030	0.033	0.024	0.042	0.040	0.049	-0.000	-0.017	0.003	0.007	-0.010	-0.001
Standard Error	(0.020)	(0.026)	(0.035)	(0.037)	(0.031)	(0.035)	(0.027)	(0.035)	(0.047)	(0.050)	(0.042)	(0.047)	(0.016)	(0.021)	(0.029)	(0.031)	(0.025)	(0.029)
Obs.	3,467	3,116	2,029	2,027	3,124	2,542	1,745	1,594	1,035	1,035	1,600	1,295	6,011	5,520	2,967	2,967	5,531	3,902
January 2004																		
Treatment Effect	0.046**	0.050**	0.044	0.070**	0.068**	0.078***	0.003	0.051	0.070*	0.093**	0.062	0.059	0.035*	0.043*	0.034	0.064**	0.062**	0.067**
Standard Error	(0.021)	(0.025)	(0.028)	(0.029)	(0.029)	(0.030)	(0.029)	(0.034)	(0.040)	(0.041)	(0.040)	(0.040)	(0.020)	(0.024)	(0.027)	(0.028)	(0.028)	(0.028)
Obs.	4,220	4,124	3,894	3,994	4,145	4,193	2,289	2,191	1,991	2,087	2,210	2,258	5,423	5,284	4,886	5,067	5,318	5,394
September 2004																		
Treatment Effect	-0.030*	-0.020	-0.017	-0.012	-0.014	-0.013	0.003	0.008	0.028	0.031	0.039	0.002	-0.021	-0.013	0.001	-0.002	-0.001	-0.019
Standard Error	(0.018)	(0.024)	(0.023)	(0.024)	(0.028)	(0.032)	(0.024)	(0.033)	(0.032)	(0.033)	(0.038)	(0.043)	(0.015)	(0.019)	(0.019)	(0.020)	(0.023)	(0.026)
Obs.	2,588	2,800	3,740	4,091	3,546	2,832	1,289	1,376	1,850	2,029	1,733	1,396	3,918	4,251	5,672	6,341	5,341	4,320
May 2005																		
Treatment Effect	0.008	-0.002	0.004	-0.003	-0.018	-0.018	0.024**	0.013	0.007	-0.009	-0.019	-0.018	-0.002	0.010	0.007	-0.006	-0.025**	-0.019
Standard Error	(0.009)	(0.012)	(0.015)	(0.015)	(0.015)	(0.016)	(0.012)	(0.016)	(0.020)	(0.021)	(0.021)	(0.022)	(0.007)	(0.010)	(0.013)	(0.012)	(0.012)	(0.013)
Obs.	7,755	7,587	7,319	9,162	10,337	9,807	3,844	3,785	3,676	4,490	5,169	4,872	11,807	11,649	11,092	14,537	16,366	15,636
August 2006																		
Treatment Effect	0.003	0.003	0.003	0.008	-0.001	0.001	0.024*	0.028	0.027	0.024	0.014	0.007	0.003	0.002	0.002	0.004	-0.000	-0.004
Standard Error	(0.010)	(0.013)	(0.015)	(0.017)	(0.018)	(0.019)	(0.014)	(0.018)	(0.021)	(0.024)	(0.025)	(0.026)	(0.008)	(0.011)	(0.013)	(0.015)	(0.015)	(0.016)
Obs.	5,744	7,749	7,860	7,251	7,854	7,639	2,860	3,613	3,667	3,384	3,661	3,569	8,882	10,977	11,112	10,313	11,098	10,814
August 2007																		
Treatment Effect	-0.008	-0.009	-0.017	-0.012	-0.005	0.017	-0.009	-0.019	-0.045	-0.037	-0.034	0.032	-0.004	-0.017	-0.012	-0.015	0.001	0.016
Standard Error	(0.010)	(0.017)	(0.020)	(0.023)	(0.023)	(0.022)	(0.014)	(0.024)	(0.028)	(0.033)	(0.033)	(0.031)	(0.008)	(0.014)	(0.017)	(0.019)	(0.019)	(0.018)
Obs.	6,931	5,025	4,402	4,222	4,642	5,899	3,434	2,527	2,248	2,070	2,347	2,899	10,410	7,442	6,630	6,435	6,977	8,910
August 2008																		
Treatment Effect	0.018	-0.001	-0.010	-0.016	-0.041*	-0.040	0.025	0.008	-0.020	-0.037	-0.051	-0.069*	0.009	0.004	-0.017	-0.036*	-0.030	-0.041*
Standard Error	(0.012)	(0.017)	(0.021)	(0.024)	(0.024)	(0.027)	(0.015)	(0.023)	(0.030)	(0.034)	(0.034)	(0.038)	(0.010)	(0.014)	(0.017)	(0.020)	(0.020)	(0.022)
Obs.	4,835	4,529	3,948	3,842	4,214	3,768	2,364	2,217	1,858	1,809	2,005	1,783	7,394	7,031	6,022	5,762	6,470	5,662
September 2011																		
Treatment Effect	0.007	-0.006	-0.022	-0.002	-0.000	0.006	0.006	-0.008	-0.037*	-0.036	-0.014	0.012	0.001	-0.002	-0.013	-0.005	0.000	0.022
Standard Error	(0.010)	(0.015)	(0.017)	(0.018)	(0.020)	(0.020)	(0.014)	(0.020)	(0.022)	(0.025)	(0.026)	(0.027)	(0.009)	(0.012)	(0.014)	(0.015)	(0.016)	(0.016)
Obs.	6,021	5,866	6,194	6,018	6,195	6,318	3,001	2,885	3,119	3,001	3,119	3,170	8,578	8,237	8,973	8,573	8,975	9,139

Nota: Standard errors between brackets. *** p<0.01, ** p<0.05, * p<0.1. Includes controls for sex, geographical location, year of birth, firm size and economic sector.