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

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## Abstract

In this study, we examine the impact of eight minimum wage increases in Argentina during the early 21st century by analyzing administrative records of registered employment. Utilizing a regression discontinuity design, we compare job separation rates between a group affected by the minimum wage hikes and a control group slightly out of their legal scope. We show that this method improves upon previous methods by reducing the likelihood of Type-I error. Overall, we find that the minimum wage hikes had no significant effect on job separation rates. However, the increases implemented in 2008 resulted in a decrease of 4.8 percentage points (19%) in separations, indicating that the employment effects of minimum wages may not necessarily lead to job loss.

**Keywords:** Minimum wage, Regression Discontinuity Design.

**JEL Classification:** J31, J80, K31.

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# 1 Introduction

Since the early 20th 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<sup>1</sup>. In contrast, the “marginalist” perspective illustrated in Stigler (1946) suggests that the conclusion adopted by the “institutionalists” is unlikely. 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. Namely, we compare the probability of separation of employment relations with pre-hike wages between the old and new minima with those with pre-hike wages slightly above the new minimum.

We find that overall, these minimum wage hikes did not have an impact on employment destruction. Nonetheless, we find evidence of positive and negative effects in two particular cases. The increase enacted in January 2004 is associated with a large increase in separation rates<sup>2</sup> of up to 9.2 percentage points (or 24%). Conversely, the increases in August 2008 reduced job destruction rates by 4.8 pp. We offer descriptive evidence that allow us to hypothesize that minimum wages are more likely to trigger separations when they force employers to increase wages during times when wages in the low-wage labor market are on a downward trend.

Adding to the vast literature on the minimum wage 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.

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<sup>1</sup>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.

<sup>2</sup>Throughout this paper we use the terms “separation” and “match destruction” interchangeably.

Yet, applying similar research designs 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 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<sup>3</sup> 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. We complement this evidence with another set of placebo minimum wage hikes in which we assume that all minimum wage hikes occur six months in advance. Again, we show that our regression discontinuity design is more likely than the standard method to yield an unbiased null point estimate. Therefore, our approach reduces the likelihood of type-I error. We argue that this is an important contribution.

Previous studies have utilized similar identification techniques that include a restricted control group, mainly as a means to validate the standard panel data approach. See Choi et al. (2021) for an example. However, to the best of our knowledge, we are the first to provide empirical evidence of the magnitude of the bias when using the standard panel data approach. Our placebo experiments reveal that it can overestimate the match-destroying effects of the minimum wage by up to 22 percentage points. This finding calls into question how informative are estimates derived from that method.

This paper further 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

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<sup>3</sup>To our knowledge, Cerimelo (2021) in the only previous article that employs the same data.

paper<sup>4</sup>.

Second, our finding of a zero effect of raising the minimum wage is 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 interesting. 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 worldwide 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.

Finally, 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 could improve labor market outcomes for currently employed individuals, although we cannot rule out that such improvement is made at the expense of the employment opportunities of the currently unemployed, as predicted by the insider-outsider theory (Lindbeck et al., 1989). Our results are also consistent with the presence of several labor market phenomena which could be investigated in future research. First, with a strand of recent literature that finds a material degree of labor market power in other countries in Latin America (Amodio and De Roux, 2021; Amodio et al., 2022). Second, it is possible that Argentinian employers react to increases in the minimum wage by hiking up prices, as found in Peru by Castellares et al. (2022). Lastly, our findings could be driven by employees responding to the minimum wage by becoming more productive (Coviello et al., 2022). All in all, it seems that a theory that seeks to explain the relationship between minimum wages and employment should not assume straightforward that minimum wages cause disemployment, at least in Argentina.

The remainder of this document is organized as follows. In the next section, we provide an overview of Argentina’s minimum wage policy and other relevant policies that may aid in interpreting our results. In Section 3, we present information about the data and sample selection used in this study. Section 4 outlines our methodology for identifying the causal effect of an increase in the minimum wage on job separations. In Section 5, we present the results of our analysis on the effects of the minimum wage on job separations and in Section 6, we provide descriptive evidence on how wages evolve after a minimum wage hike. Additionally, in Section 7, we conduct a series of robustness checks to ensure the validity of our findings. Finally, in Section 8, we summarize our findings and provide conclusions.

## 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

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<sup>4</sup>For example, Spain’s “Muestra Continua de Vidas Laborales”, Germany’s “Linked Employer-Employee Data” (LIAB), Peru’s “Planilla Electronica”, or Brazil’s RAIS.

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		
08/01/03	260	Dec. 388/2003		
09/01/03	270	Dec. 388/2003	07/10/03	Yes
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	570	Res. 2/05 CNEPySMVyM	06/01/05	Yes
07/01/05	630	Res. 2/05 CNEPySMVyM		
08/01/06	760	Res. 2/06 CNEPySMVyM		
09/01/06	780	Res. 2/06 CNEPySMVyM	07/28/06	Yes
11/01/06	800	Res. 2/06 CNEPySMVyM		
08/01/07	900	Res. 2/07 CNEPySMVyM		
10/01/07	960	Res. 2/07 CNEPySMVyM	07/11/07	Yes
12/01/07	980	Res. 2/07 CNEPySMVyM		
08/01/08	1200	Res. 3/08 CNEPySMVyM		
12/01/08	1240	Res. 3/08 CNEPySMVyM	07/28/08	Yes
08/01/09	1400	Res. 2/09 CNEPySMVyM		
10/01/09	1440	Res. 2/09 CNEPySMVyM	07/30/09	No
01/01/10	1500	Res. 2/09 CNEPySMVyM		
08/01/10	1740	Res. 2/10 CNEPySMVyM	08/05/10	No
01/01/11	1840	Res. 2/10 CNEPySMVyM		
09/01/11	2300	Res. 2/11 CNEPySMVyM	08/26/11	Yes

current legal minimum wage was enacted by “Ley 16.459”. This legal document creates the minimum wage as follows<sup>5</sup>:

*“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<sup>6</sup>. 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 administration 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.

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

<sup>5</sup>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.*”

<sup>6</sup>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.*”

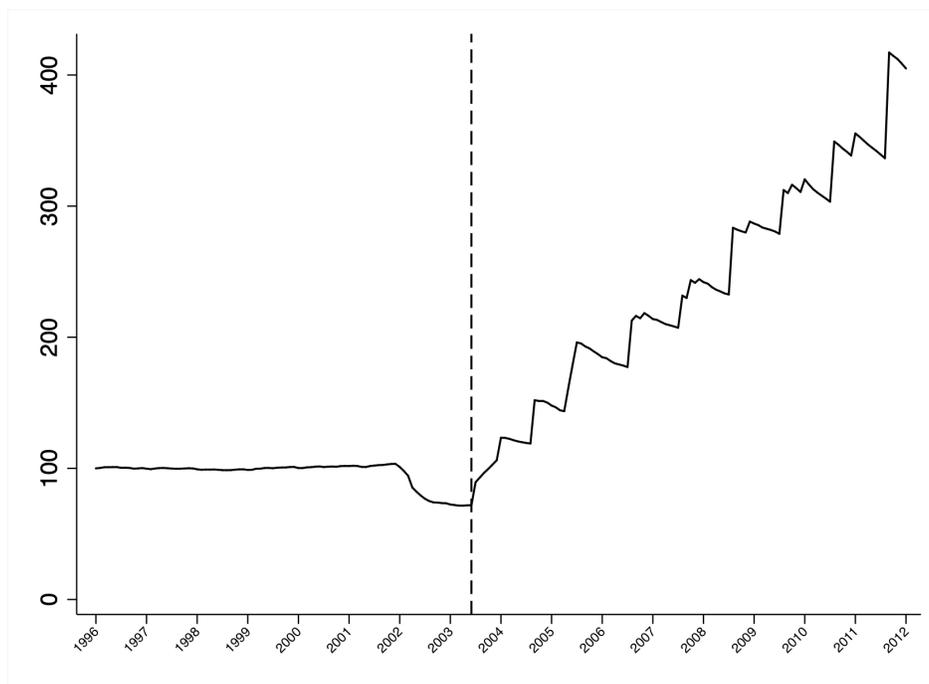
after their announcement.

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 by the 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 unconvincing<sup>7</sup>. 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 advanced notice of 60 business days before a dismissal. In addition, employers must make severance payments to dismissed employees that add up to 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 the mid-1990s to 2012.

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



Note: The figure shows the evolution of the real minimum wage between 1996 and 2012. We deflate the nominal minimum wage to 2008 ARS and then re-express them as a percentage of its 1996 value. The dashed vertical line marks the onset of the period during which the minimum wage was regularly increased.

<sup>7</sup>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.

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)<sup>8</sup>. 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 gross wages), economic sector, firm size, workers’ sex, their year of birth, and the province where they work. In Appendix A we elaborate on the definition of each variable.

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)<sup>9</sup>. 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<sup>10</sup>. 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.

In columns 2, 3, and 4; we show descriptive statistics for specific groups defined by their baseline (i.e., pre-hike) 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 slightly greater than the new minimum wage. This comprises a group with baseline wages marginally above the new minimum. More specifically, column 3 describes matches with pre-hike wages up to 100.59 ARS over the new minimum. We choose this figure owing to the fact that this is the optimal bandwidth for our regression discontinuity design, selected by the algorithm in Calonico et al. (2014), when the dependent variable is the dummy for

<sup>8</sup>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.

<sup>9</sup>This filtering process aims to eliminate part-time positions that would have higher wages when converted to full-time equivalents. This is crucial as we lack information on hourly wages or hours worked.

<sup>10</sup>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.

Table 2: Means of Key Variables.

Variable/Group	Full-Sample	Treated	Marginally Non-Treated	Non-Treated
	(1)	(2)	(3)	(4)
<b>Male (%)</b>	70.80	64.07	65.52	71.34
<b>Year of Birth</b>	1,968	1,971	1,970	1,968
<b>Large-Firm</b>	57.54	35.93	44.08	59.32
<b>Primary Sector</b>	6.75	16.04	12.75	5.99
<b>Buenos Aires</b>	60.01	51.24	55.01	60.73
<b>Match Destruction (%)</b>				
1 month	2.68	6.14	5.24	2.40
3 months	8.20	16.65	15.30	7.51
6 months	14.86	27.38	25.20	13.84
<b>Treated (%)</b>	7.52	100.00	0.00	0.00
<b>Obs.</b>	921,066	69,255	48,461	851,811

Note: Column (1) describes the full sample. Column (2), describes employer-employee matches with pre-hike wages between the old and new minima. Column (3), those with pre-hike wages at most 100.59 ARS over the new minimum. This figure is the optimal bandwidth selected using the algorithm in Calonico et al. (2014) when the dependent variable is a dummy for match destruction six months after a minimum wage hike. Column (4) includes all matches with wages above the new minimum wage before the hike.

match destruction six months after a minimum wage hike. Therefore, the units in column 3 represent the control group in our main regression analysis<sup>11</sup>. In column 4, we describe all matches with baseline wages above the new minimum<sup>12</sup>.

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 who are 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 D.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 standard method 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

<sup>11</sup>Note that we allow for different optimal bandwidth sizes for each month between 1 and 6 after a hike. The optimal bandwidth is very stable and ranges between 81 and 120 ARS. Moreover, the descriptive results are nearly identical if we use a control group made up of matches with pre-hike wages at most 0.25 times the difference between the old and new minima. These results are available upon request.

<sup>12</sup>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 baseline wages between 300 and 400 in column 3, and those with pre-hike wages greater than or equal to 300 in column 4.

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 directly targeted by the policy change.

## 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  $MW$  (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.

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

$TE$  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 legal minimum wages before and after the implementation of Dec. 388/2003 (See Table 1). 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.

$$Y_{i,t=k} = \beta_0 + \beta_{1,t=k} T_{i,t=0} + \beta_{2,t=k} (W - MW')_{i,t=0} + \beta_{3,t=k} (W - MW')_{i,t=0} \times T_{i,t=0} + \epsilon_{i,t=k}, \quad (2)$$

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  $Y_{i,t=k} = 0 \quad \forall k$ .  $T_i$  is a binary variable that indicates whether employment relationship  $i$  is in the treatment group, that is, if  $MW \leq W_{t=0} < MW'$ . In that sense,  $\beta_{1,t=k}$  is our parameter of interest. We make a separate estimation for each month since the new minimum wage is first enacted ( $k = 1$ ) to six months after ( $k = 6$ ).

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:  $(W - SM')_{i,t=0}^2$  and  $(W - SM')_{i,t=0}^2 \times T_{i,t=0}$ . We report the full results derived from the quadratic specification in Appendix B.

## 4.2 Identification Assumptions and Threats

The main identifying assumption is that assignment to treatment and control groups in the interval  $[MW' - h, MW' + h]$ , is as good as random. This assumption implies that, within the bandwidth, the determinants of the treatment assignment are uncorrelated to the error term<sup>13</sup>. 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)<sup>14</sup>. In that sense, the first threat to our identification strategy is the violation of the local randomization assumption.

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 D 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<sup>15</sup> 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 in the regressions 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 C. These tests look for significant differences in the density of the running variable  $(W_{i,t=0} - MW)$  on each side of the threshold. Significant density differences are often interpreted as endogenous self-selection into or out of the treatment group, therefore, they argue against the quasi-random assignment of the treatment around the threshold.

Conceptually, we do not expect these threats to randomization to be a particularly serious concern in our case for the following reasons. First, precisely manipulating 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 story of manipulation based on the anticipation of the timing or magnitude of the increase<sup>16</sup>. 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  $MW' - W_{i,t=0}$  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.

<sup>13</sup>The advantage of this is that it allows us to directly rule out alternative mechanisms such as inflation or macroeconomic fluctuations as they should not affect the treatment and control groups differentially. Hence, they do not contaminate our estimate of  $\beta_1$ .

<sup>14</sup>We will show that the results hold under some different bandwidths.

<sup>15</sup>Specifically, this would be observed as a “treatment effect” statistically equal to 0.

<sup>16</sup>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.

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. 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. Dube et al. (2020) show that the density of hourly earnings in the United States at \$10 is 50 times larger than 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 is 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 C, we show that there are some significant density differences in the running variable on both sides of the cut-off, 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 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 relies 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 Equation 2 which seeks to identify the effects of raising the minimum wage on employment destruction. First, in subsection 5.1, we pool together the 8 minimum wage hikes described in Section 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 section 5.2, we briefly analyze the heterogeneous treatment effects; that is, we estimate Equation 2 independently for each increase to allow each minimum hike to have a differential impact on separation rates. We provide a more complete set of results in Appendix B. We also report all density tests in Appendix C, tests for covariate smoothness and the results for specifications with controls in Appendix D.

### 5.1 Pooled Results

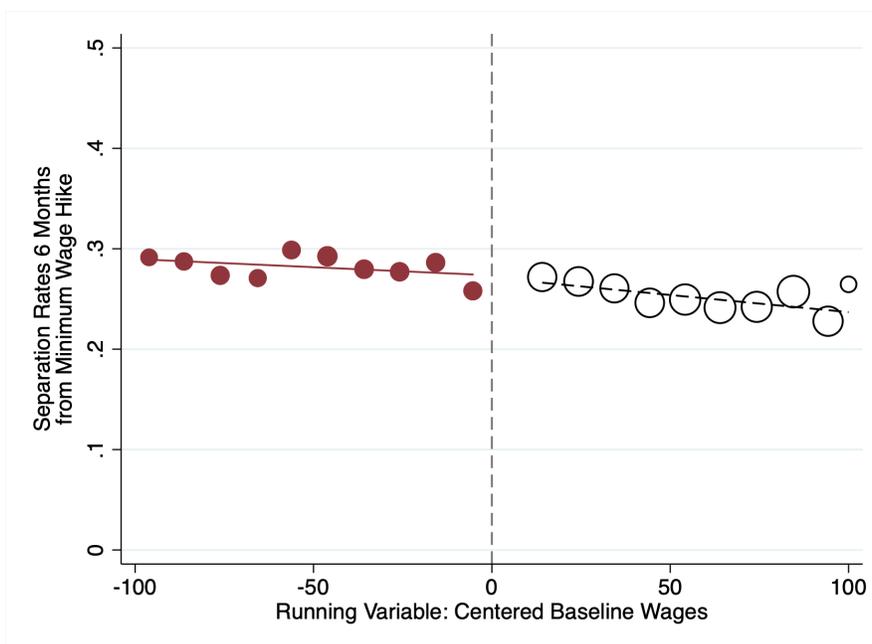
Before reporting the results from estimating Equation 2, it is informative to graphically check for discontinuities in a small neighborhood around the threshold. In Figure 2, we plot the probabilities of match destruction against the centered pre-hike wage six months after the minimum wage hikes. We 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 the optimal bandwidth of 100.59 ARS at each side of the cutoff, derived following Calonico et al. (2014). Each circle represents a group of labor relations (*bin*) with salaries with a maximum difference of 10 ARS (i.e., *binwidth*=10). 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 suggests that there might not be a discontinuity in the outcome variable. Apparently, treated units have an almost identical job destruction rate than the control units 6 months after an increase in the minimum wage. In the remainder of the section we present more rigorous calculations

of our treatment effect of interest and 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.

Figure 3 presents the estimates of the effects of a minimum wage hike on job destruction, that is,  $\beta_1$  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 wage floor did not trigger job separations. To further this claim, we compute the minimum effect detectable effect under standard levels of statistical significance using the regression discontinuity design sample. We can rule out any match destruction effects larger than 0.73 percentage points six months after a hike.

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

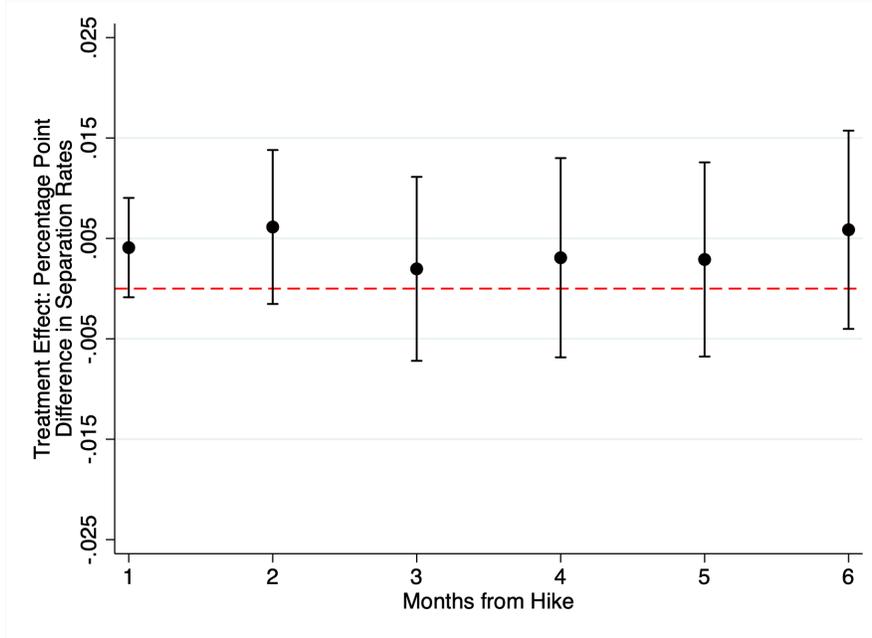


Note: This plot shows the percentage of matches that were destroyed within 6 months after a minimum wage hike is enacted for each bin. We choose the 100.59 ARS bandwidth at each side of the cut-off following Calonico et al. (2014). The dots denote averages, with a bin width of 10 ARS. Their size represents the number of observations. Treated units are at the left of the cutoff.

In Table 3 we present a first battery of robustness checks. In column (1) we present the original estimate, which corresponds to the coefficient six months from a hike 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 are economically negligible and statistically insignificant.

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

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



Note: Each marker in the table represents  $\beta_{1,t=k}$  for  $k = \{1, 2, 3, 4, 5, 6\}$ . That is, the estimated month by month percentage point difference in separation rates between the treatment and control groups, as calculated using Equation 2. These groups were selected using the bandwidth selection method outlined in Calonico et al. (2014). These estimates reflect the treatment effects for each month between the first and sixth months following the implementation of a new minimum wage. The lines in the table indicate the conventional 90% confidence intervals for the treatment effects.

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 very small: it implies that increasing the minimum wage by 29.3% (simple average in nominal terms) only increases separation rates by 1 percentage point. 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 B.1, we plot how does the treatment effect of interest varies with bandwidth size and show that it equals zero for smaller bandwidths and that it only becomes positive and statistically significant once we increase the bandwidth above 160 ARS. This is above the optimal bandwidth of around 100 ARS. The point estimates increase monotonically after this point which 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 argue that this is because introducing higher income observations introduces bias. Section 6 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.

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 (0.006)	0.002 (0.007)	0.000 (0.008)	-0.009 (0.008)	0.012** (0.005)	-0.01 (0.009)
Doughnut?	No	1 ARS	5 ARS	No	No	No
Optimal Bandwidth $\times$	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: The table shows alternative specifications for Equation 2. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within 6 months following a minimum wage hike. In Column 1, we report the estimates of our standard specification as a benchmark. Columns 2 and 3 report the estimates obtained by excluding observations with baseline wages close to the minimum wage cutoff; 1 and 5 ARS, respectively. Columns 4 and 5 present the estimates obtained by restricting the sample to observations with wages between 0.5 and 1.5 times the bandwidth. Finally, Column 6 shows the estimates obtained by adding a second-order term for the running variable, pre-hike wages, and its interaction with the treatment dummy. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

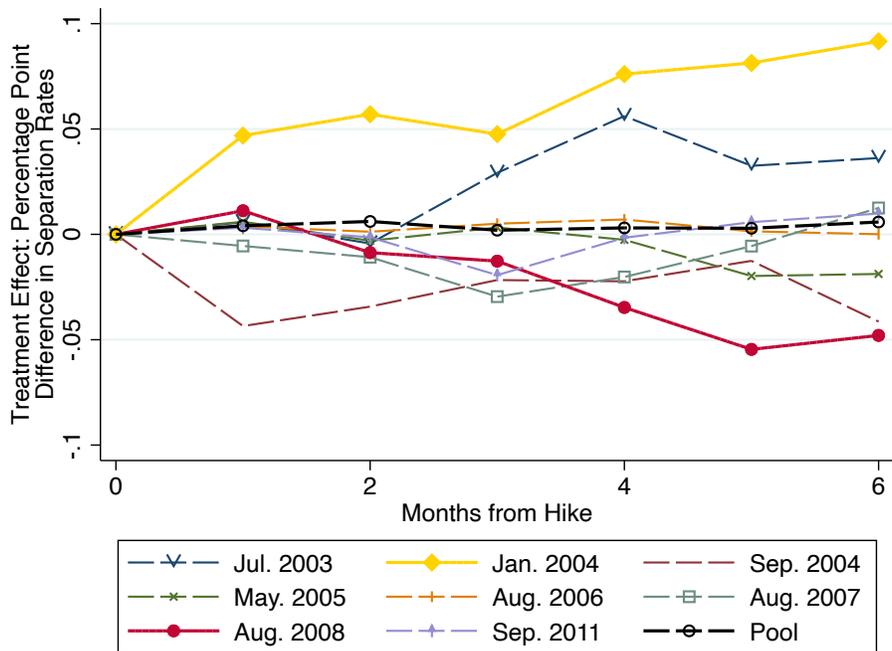
We show the path of  $\beta_1$  up to 6 months after each hike. Solid lines represent paths with a significant endpoint (those that have a statistically significant  $\beta_1$  at  $k = 6$ ). Dashed lines denote paths with a null endpoint. The complete set of results including point estimates and standard errors can be found in Appendix B (Table B.1). We also include further robustness checks in Tables B.2, B.3, and D.2; and in Figure B.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. This argues against a causal effect of raising the minimum wage on job destruction. We do detect a pattern in the sign of the point estimates. We see that the first minimum wage hikes, those in July 2003 and January 2004, had a non-negative association with separation rates. That is, they are associated with higher job separation rates. Conversely, those that occurred after September 2004 yield non-positive point estimates, which imply lower job destruction. Albeit, it is important to point out that the magnitude of the point estimates is usually close to zero and not statistically significant.

Two events stand out as exceptions: January 2004 and 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 (Table B.1) and to bandwidth choice (Table B.3). Nonetheless, it ceases to be significant if we use a quadratic fit instead of a linear one (Table B.2). 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 C.2g, there is no evidence of abnormal density at any side of the cutoff, therefore, we can disregard the doughnut-hole estimates. This result does not argue that the increase in the minimum wage created jobs. Rather, it 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 than what they would have had in the counterfactual scenario without an increase in the minimum wage. In other words, it seems

Figure 4: Path Plot of  $\beta_1$  for each Minimum Wage Hike.



Note: The graph shows the percentage point difference in separation rates between the treatment and control groups after each minimum wage hike considered, as per Table 1. The point estimates come from estimating Equation 2 using an outcome-specific optimal bandwidth, calculated following Calonico et al. (2014). We exclude confidence intervals for visualization purposes. The full set of results with standard errors is reported in Appendix B. Solid lines represent paths with an statistically significant endpoint ( $\beta_{1,t=6}$ ).

that this hike lowered separations, but an important caveat is that we cannot make further claims regarding its effect on overall employment levels.

In Tables D.3 and D.4 we also report heterogeneous effects by sex, place of residence, and industry. We find that males, residents of Buenos Aires, and workers in the utilities and transport sectors seem to be particularly more responsive to minimum wage hikes than the rest. That is, the minimum wage hikes that did impact separation rates mostly did so among these worker groups.

In Figure B.2, we decompose the outcome variable into two different dummies so that we can distinguish between the disemployment and the reallocation effects of the minimum wage. Specifically, we re-estimate equation 2 separately for a dummy that indicates whether a worker who lost their job due to a hike found another job within 6 months, and another dummy for those who remained unemployed or informally employed. In Appendix A we provide further detail on how we construct these variables. We find that the effects are typically around zero, which supports our main findings. Additionally, we observe that the association between the minimum wage hike in January 2004 and job separations is mainly due to job loss, as most individuals who lost their jobs were not able to find new employment by June of the same year.

## 6 Wages and the Minimum Wage

In this section, we show the evolution of nominal wages after a minimum wage hike. We opt out of performing a detailed econometric analysis in favor of a conservative description of the data. This is due to the fact that our research design suffers from a similar econometric problem as the standard panel data approach when it comes to uncovering the wage effects of the minimum wage. Namely, if we were to estimate equation 2 using the change in wages between  $k = 0$  and  $k = \{1, 2, 3, 4, 5, 6\}$  as a dependent variable we will end up with an equation in which the outcome and the treatment dummy are both co-determined by baseline wages. This could potentially generate an artificial correlation between both variables, thus rendering any result uninformative. Therefore we will limit ourselves to displaying how wages evolve for the treatment and control groups after each hike; and abstain from making any causal claim. Still, we believe that this section illuminates our previous findings in at least two ways.

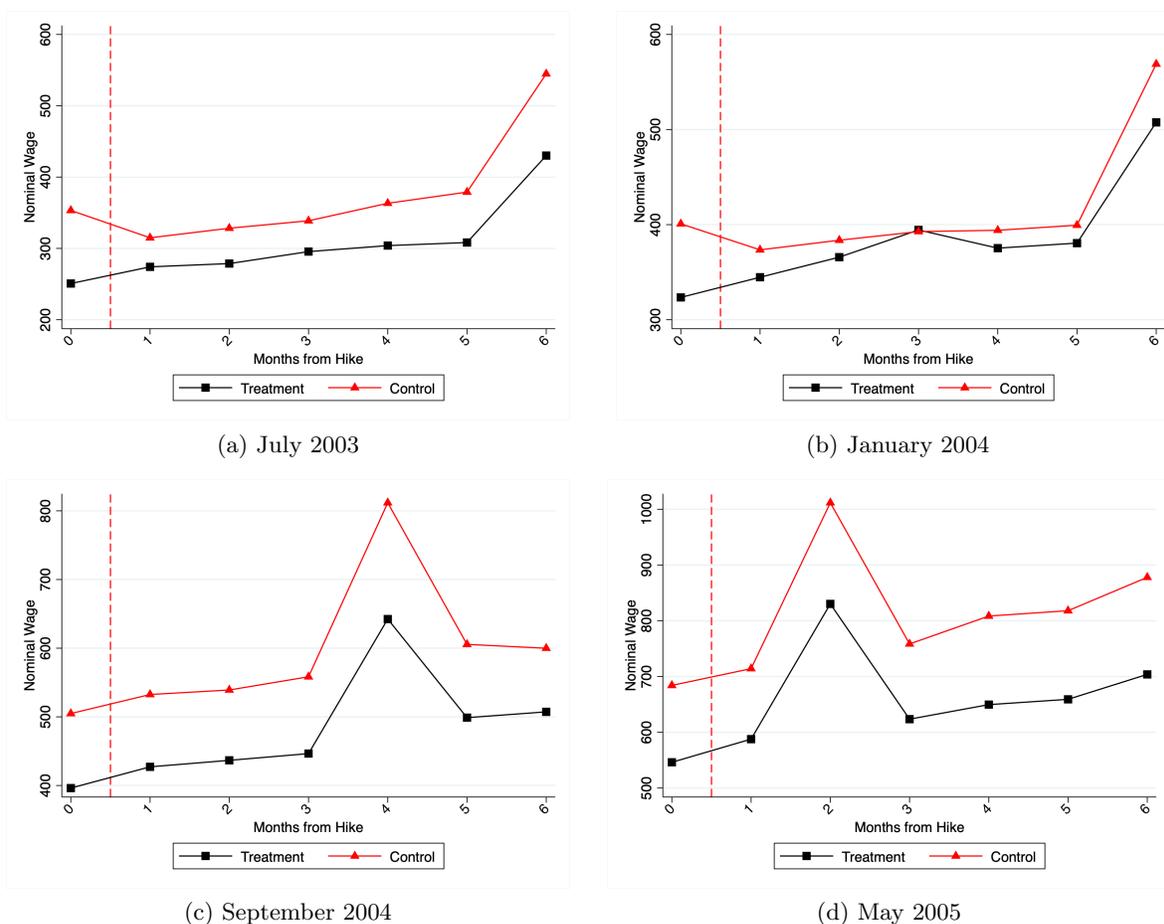
First, our identification strategy is based on assuming that our treatment group are workers whose wages are bound to increase following a minimum wage hike. In contrast, we assume that the control group are workers whose wages are not tied to the minimum wage, at least not legally. Thus, this empirical exercise offers evidence on the compliance with wage floor raises, implied by our definition of treatment and control groups. Second, so far we have shown that the minimum wage has an overall null effect on job separations. Nonetheless, we do detect some events that escape this rule. We hope that by exploring post-minimum wage hike wage patterns we are able to further understand when is this policy more likely to trigger match destruction.

Figure 5 plots the average nominal wages of the treatment and control groups for selected minimum wage hikes. The full set of results are available in Figure B.4. To clarify, the treatment group are those selected by the algorithm in Calonico et al. (2014). Namely, those with pre-hike wages below and above the new minimum wage by at most one time the hike-specific optimal bandwidth.

The first takeaway of this figure is that nominal wages effectively increase for the treatment group following the enactment of a wage floor raise. Therefore, it seems that our selection of a treatment group comprised of labor relations that are either terminated or that receive a wage raise after a hike has some empirical grounds.

The second takeaway from this figure is that, in contrast with the treatment group, wage trends for the control group do not show a clear pattern. We argue that this might provide suggestive evidence that hints on why do we sometimes find increased job destruction after a minimum wage hike. When comparing the July 2003 and January 2004 minimum wage hikes —both events had positive separation

Figure 5: Nominal Minimum Wage Trends by Group. Selected Events.



Note: The graph illustrates the change in average nominal wages for two groups of workers before and after selected minimum wage hikes. Month 0 denotes the month before the minimum wage hike is enacted, months 1 to 6 denotes the number of months in which the new minimum wage has been in effect. In black squares we show the evolution of nominal wages for the treatment group comprised of workers with baseline wages below the new minimum wage for at most one optimal bandwidth. In red triangles we show the evolution of nominal wages for the control group comprised of workers with baseline wages above the new minimum wage for at most one optimal bandwidth. We compute a hike-specific optimal bandwidth following Calonico et al. (2014).

rates— with September 2004 and May 2005 —which had no significant effect— we see that in the former control group wages trended downwards, while the opposite holds for the latter. This is also true for every event following 2005, as can be seen in Figures B.4.

In other words, we see that in the earliest minimum wage hikes, wages in the control group dropped immediately after the hike<sup>17</sup>, whilst wages in the treatment group increased, signaling compliance with the regulation. For the following events, all wages trended up and no evidence of employment destruction is found. Thus, it seems that the minimum wage leads to job destruction whenever it forces the wages of minimum wage jobs to go against the overall trend in the low-wage job market. In Figure B.6 we report the results from a similar exercise using real wages and both conclusions hold.

<sup>17</sup>This is likely to be rooted in the fact that these hikes occurred very shortly after the 2001-2002 crisis in a time period where the GDP had not fully recovered. In Figure B.5 we show the real value of the GDP between 2000 and 2012. The fact that the minimum wage hikes in 2003 and early 2004 were enacted in a time period when productivity was notably low might explain why wages in the control group fell immediately after their enactment.

## 7 Robustness Checks

In this section we provide two separate sets of further robustness checks. First, in subsection 7.1 we compare the performance of our method vis-à-vis the standard panel data approach in a series of placebo experiments. Then, in section 7.2 we resort to an alternative identification strategy to assess if our main insights hold.

### 7.1 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  $\beta_1$ , run both specifications, and inspect which one leads to a closer point-estimate. Given that  $\beta_1$  is, by definition, an unknown parameter, a test like this is unfeasible in most circumstances. However, we can approximate a situation in which the value of  $\beta_1$  is trivially known: a placebo experiment.

If we-incorrectly-assume that a minimum wage hike is implemented when none actually is, we can confidently assert that  $\beta_1$  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 August between 1996 and 2000; similar to the one actually implemented in 2003. Given that there might be concerns that the performance of the Argentinian economy in this period was too dissimilar to that of the early 2000s to provide a particularly credible placebo, we offer another set of placebo experiments in which incorrectly assume that the minimum wage hikes in Table 1 occurred six months in advance. We report the estimates for assuming placebo experiments in 1998 and 2008 in Table 4 and send the rest to Appendix B. We choose to report the 2008 placebo because we observe our most credible non-zero result on that date. Therefore, the results from this placebo experiment directly test its validity.

Columns (1) to (6) identify the estimated impact at different time windows. For both placebo experiments, 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. This is particularly reassuring for the 2008 placebo. It shows that our main result is likely to be a causal effect of the minimum wage rather than the result of a pre-existing difference between the treatment and control groups.

Summarizing, Table 4 shows that the bias induced by the standard panel data approach can be quite large: up to 16 percentage points. 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

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

Model/ $k$	1	2	3	4	5	6
<b>a) Placebo Experiment: 1998</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.021*** (0.004)	0.051*** (0.005)	0.074*** (0.006)	0.100*** (0.007)	0.113*** (0.007)	0.164*** (0.009)
Obs.	93,561	93,561	93,561	93,561	93,561	93,561
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.006 (0.011)	0.013 (0.017)	0.026 (0.020)	0.028 (0.021)	0.003 (0.019)	0.004 (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
<b>b) Placebo Experiment: 2008</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.014*** (0.002)	0.041*** (0.004)	0.061*** (0.004)	0.079*** (0.005)	0.084*** (0.005)	0.094*** (0.006)
Obs.	126,249	126,249	126,249	126,249	126,249	126,249
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.001 (0.008)	0.003 (0.014)	0.006 (0.016)	-0.002 (0.019)	-0.009 (0.019)	-0.004 (0.020)
Obs.	6,685	7,557	7,575	6,527	7,377	7,496
Power(0.09)	1.000	0.986	0.932	0.839	0.846	0.820

Note: The table shows the results from placebo estimations of Equation 2 in time periods where the minimum wage was not raised. In panel a, we assume a non-existent minimum wage increase from 200 to 300 ARS in August of 1998. In panel b, we assume a non-existent minimum wage hike in February 2008, six months prior to the effective minimum wage hike enacted in August 2008. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ .

treatment groups, such as unobserved job-specific heterogeneity in turnover rates.

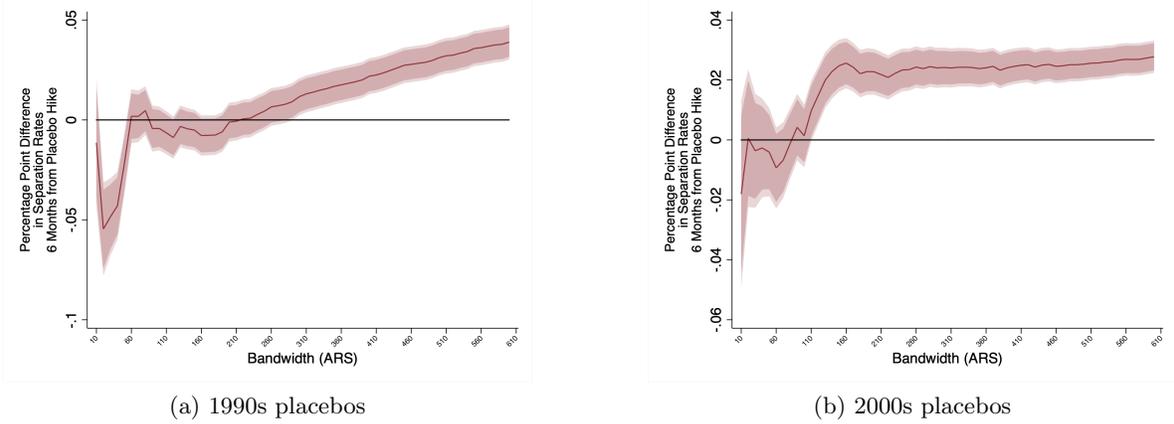
Given that our empirical strategy employs a relatively small number of observations, it is possible that the claims from Table 4 are a result of a lack of statistical power. In other words, a concern embedded in our method is that it might minimize bias at the expense of statistical power. To address this, we present power calculations for a treatment effect of 0.09, the average found by the standard panel data approach in 1998. 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 Tables B.4 and B.5 we show the results for a series of additional placebo tests. The conclusions are largely unchanged. Our specification fully eliminates bias in 27 out of the 36 placebo experiments for the 1990s and reduces its magnitude by between 13 and 80% in our worst performing scenarios where a spurious correlation is detected. Similarly, for the placebos in the 2000s, we show that the standard panel data approach incurs in type-I error in 53 out of the 54 placebo experiments that we run. Conversely, our regression discontinuity design only makes a false discovery in 12 out of 54. In these 12 spurious correlations our regression discontinuity design reduces the size of the bias from the standard panel data approach by between 42 and 68%.

When inspecting these scenarios more closely, we see that these poor results are an anomaly. In Figure 6 we pool both sets of placebo experiments separately and plot the point estimates for  $\beta_1$  in equation 2 for different bandwidth sizes. In panel a, we report the results for the 1990s placebos. Our estimator performs very adequately for bandwidths below 300 ARS<sup>18</sup>. For bandwidths above this figure we can observe how the bias becomes statistically significant and increases monotonically with bandwidth size. Likewise, for the placebo experiments in the 2000s we see that our estimates are unbiased for bandwidths below 110 ARS. The fact that the bandwidth we use in our main analysis is

<sup>18</sup>This is more than 100% the value of the baseline minimum wage. Thus, it represents an extremely large bandwidth.

Figure 6: Effect of Placebo Minimum Wage Hikes on Separation Rates.



Note: The reported point estimates come from estimating Equation 2 for two sets of placebo experiments. 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. Panel a shows the results of pooling together all records from 1996 and 2000 and assuming a false minimum wage hike from 200 to 300 ARS every august. Panel b reports the point estimates obtained from assuming that all minimum wage hikes in Table 1 occurred 6 months in advance.

below this figure is reassuring.

## 7.2 Comparison with an Alternative Method

To add another layer of credibility to our main findings we resort to an alternative method in the recent literature and test whether it yields similar results. Namely, we estimate the following specification based on Dustmann et al. (2022):

$$Y_{i,t=6} = \gamma_0 + \sum_j 1[W_{i,t=0} \in b_j] \delta_j + \Phi X_i + v_{i,t=6}, \quad (3)$$

Here  $Y$  is a dummy that takes the value of 1 if match  $i$  was destroyed at some point six months from any of the minimum wage hikes studied hitherto. Unlike Dustmann et al. (2022), we are estimating the effects of many minimum wage hikes during a time period with high volatility in the real value of the local currency. Therefore, constructing bins based on nominal monetary units might results in groups comprised of matches with very different purchasing powers. To work around this issue, we divide the observations at each side of the threshold into 10 groups with roughly the same number of observations<sup>19</sup>. Thus, the first group is comprised of the first 10% of matches with earnings between the old and the new minimum wage. The eleventh groups includes the first 10% of observations with earnings above the new minimum wage. We denote each of these groups as a bin,  $b_j$ . Therefore, the indicator variables in the summation term at the right hand side of equation 3 take the value of 1 whenever baseline wages for a match fall within each bin. Crucially, we do not restrict the sample to those observations within the regression discontinuity design sample.

We set  $\delta_{11} = 0$ . Therefore, the excluded category is bin number 11, hence, each  $\delta$  coefficient can be interpreted as the difference in separation rates between each bin and the separation rates of the 10% lowest earning workers above the new minimum wage.  $\gamma_0$  is an intercept and  $X$  is a vector of controls that includes minimum wage hike fixed effects, date of birth and dummies for being male, for working in the agrarian sector, for working in Buenos Aires, and for firms with more than 50 employees. Lastly,  $v$  is an error term.

<sup>19</sup>We apply this procedure at each side of the cutoff independently, so that the number of observations in each group below the threshold does not need to be identical to the number of observations in each group above the threshold.

Figure 7 shows the results and couples them with those obtained from pooling all placebo experiments described in the previous subsection. Namely, the placebo experiments come from simulating non-existent minimum wage hikes from 200 to 300 ARS every August between 1996 and 2000, and from assuming that the 2000s minimum wage hikes occurred six months before their effective implementation.

There are a series of important takeaways from this empirical exercise<sup>20</sup>. First, this methodology confirms our main findings: separation rates are identical between matches right before and after the cutoff.

Second, the matches with earnings in the top 20% (bins 19 and 20) have extremely low separation rates. This is more than 15 percentage points lower than the matches that more closely resemble the control group in our regression discontinuity design; that is, the lowest earning matches in the control group (bin 11). This result is very likely to be unrelated to the minimum wage given that the employment trends for the richest workers in the country should be independent of the minimum wage. The standard panel data approach, which considers this observations, might produce biased estimates by putting too much weight on these matches who are, not only very different to minimum wage matches, but also to others in the control group. This supports the main rationale behind our main empirical strategy.

Third, comparing the results obtained when analyzing real minimum wage hikes (black circles) with those obtained from running equation 3 in the 1990s placebo experiments (red triangles) also corroborates our findings. The difference in separation rates between every bin in the control group and the lower earning matches in the treatment group (bin 11) are small, they range from virtually zero to around 3 percentage points. Note however, that in the placebo experiments, where no minimum wage hike was truly enacted, these gaps are substantially larger. They can be as high as 15 percentage points. Thus, it seems that, if anything, the gaps in separation rates between the treatment and the control group are smaller when the minimum wage increases. This argues for a non-positive effect of minimum wage hikes on employment destruction, at least for these set of placebo experiments.

We also show the results from the 2000s placebo experiment (gold squares). The results are qualitatively similar. The separation rates of treated matches are very similar to those of the lower earning matches in the control group. And notably, the differences in separation rates between the treatment and the control group are closer after the effective minimum wage hikes than in the placebo experiments.

## 8 Discussion

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

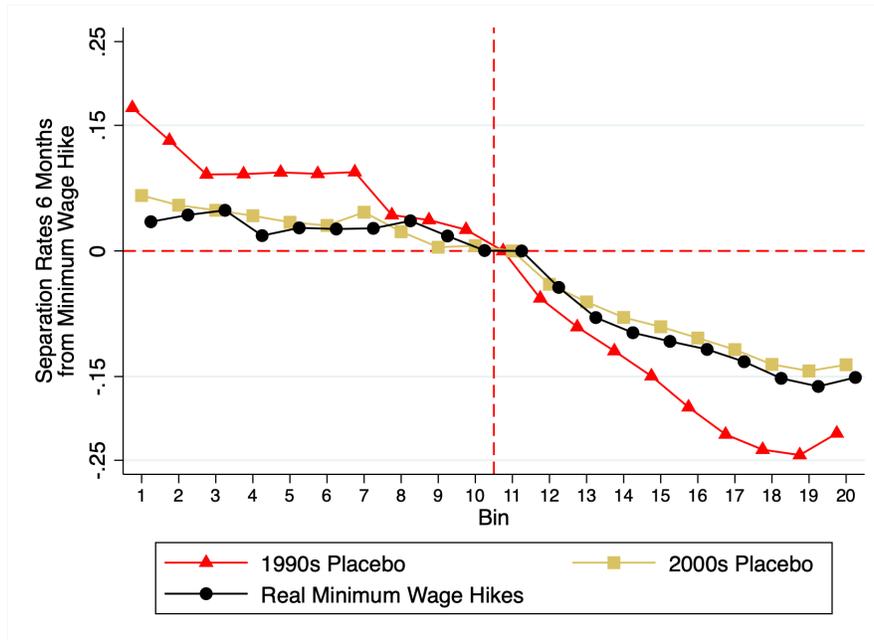
Overall, our main findings suggest that minimum wage hikes do not increase separation rates. Therefore, it seems that the mechanisms that links minimum wages and aggregate employment may not usually be rooted in job destruction. This result is in line with recent research suggesting that employers respond 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

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<sup>20</sup>The full set of results in table format with standard errors is available upon request.

Figure 7: Match Destruction Rates Six Months From Hike by Position in the Pre-hike Wage Distribution. Real Minimum Wage Hikes vs Placebo Experiments.



Note: The black circles in the graph depict the estimates of  $\delta_j$  in equation 3 using the minimum wage hikes outlined in Table 1. These estimates represent the average separation rates six months after each hike for a group of matches comprised of 10% of all matches on either side of the cutoff. The analysis controls for minimum wage hike fixed effects, date of birth, and demographic factors such as gender, sector of employment, location, and firm size. Confidence intervals are not shown for visualization purposes. The red triangles represent the results obtained by assuming a false minimum wage hike from 200 to 300 ARS every August between 1996 and 2000, while the gold squares represent the point estimates obtained from assuming that all minimum wage hikes in Table 1 occurred 6 months in advance.

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) practitioners should be cautious of aggregation bias when pooling together multiple minimum wage hikes and evaluating them as a single event. We have found suggestive evidence that minimum wages are more likely to associate with increased separations whenever they force employers to raise wages in contexts marked by low productivity and decreasing wages in the low-wage labor market.

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 the challenges that practitioners 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 statistical power to provide informative statistical inference. While this result can be easily mapped to the well understood bias-variance trade-off, it highlights the need for large sample sizes to perform similar exercises. 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 Variable Definition

In this appendix we describe how we construct each variable used in the analysis. We will also provide a small set of complementary descriptive results. For our main analysis, we used the following variables:

- Active match: we consider that a match is active at some point in time whenever it has positive wages at least as high as the current minimum wage.
- Inactive matches: to clarify, we consider that a match is inactive in three scenarios. First, when it have no information for wages (i.e., a missing value). Second, when it has wages exactly equal to zero. As per the data documentation, this implies that a worker is on-leave. This is a harmless assumption due to the fact that this is an extremely infrequent case. For example, the share of workers on leave in the raw data before the first minimum wage hike is only 0.20%, six months later, this figure is still well below 1% (0.34%). Third, we assume that a match is inactive if before a minimum wage hike it has a wage below the legal minimum wage. We assume that this corresponds to part-time workers. Importantly, we do not impose this restriction to employment relations post minimum wage-hike.
- Employment destruction  $k$  months from a hike: we assume that an employment relation is destroyed whenever it is active before a minimum wage hike and becomes inactive at any point between the minimum wage hike and  $k$ . Once a match is destroyed we assume that it remains destroyed thereafter.
- Wages: our measure of wages is the total gross compensation received by a match at any given month. It includes remunerative amounts (salary, additional annual salary, wages, fees, tips, bonuses and additional supplements that are usual and regular in nature) and non-remunerative amounts.
- Treatment dummy: dummy variable equal to 1 for matches with pre-hike wages at or above the old minimum wage and strictly below the new minimum wage.
- Buenos Aires dummy: dummy variable equal to 1 for employment relations developing in Greater Buenos Aires, Capital Federal, or Buenos Aires.
- Firm size: dummy that indicates employment relations that occur in a firm with at least 50 employees.
- Primary sector: dummy that indicates matches in the agriculture, fishing and mining sectors.
- Disemployment: dummy variable that indicates whether a worker who held an active match that was destroyed within six months following a minimum wage hike did not have an active match by the sixth month. This variable, which is measured at the worker level, was merged with the match-level data used in the main analyses using the worker IDs provided in the data.
- Reallocation: dummy variable that indicates whether a worker who held an active match that was destroyed within six months following a minimum wage hike had an active match by the sixth month. This variable, which is measured at the worker level, was merged with the match-level data used in the main analyses using the worker IDs provided in the data.
- Real GDP: data comes from the World Bank. The raw data is expressed in constant 2015 US\$. We re-express it as a percentage of its 2000 value.

Moreover, in Table D.4 we disaggregate our economic sector variable to provide more insights about the potential heterogeneous effect of minimum wage hikes on employment destruction across sectors. We code the sectors as follows:

- Agriculture, Fishing and Mining (Primary sector): includes those three sectors exactly.
- Utilities and Transport: includes matches related to the supply of electricity, gas, water, transportation, storage, and communication.
- Manufacturing and Construction: includes those two sectors exactly.
- Others: includes all matches dedicated to trade, repairs, hotels, restaurants, financial intermediation, real estate, teaching, health and social services, and personal services.

## B Full Results and Additional 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 B.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 B.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 C 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 B.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 B.3 we show the sensitivity of our results to different bandwidth choices. Specifically, we show the results for using bandwidths equal to 1, 0.5, and 1.5 times the optimal bandwidth. 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 B.1 we delve deeper into this result. We plot the treatment effect for the pooled specification for all bandwidth sizes between 10 and 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 cutoff. 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.

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

Event/Doughnut-Hole <i>k</i>	No Hole						1 ARS						5 ARS					
	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>																		
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
<b>July 2003</b>																		
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
<b>January 2004</b>																		
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
<b>September 2004</b>																		
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
<b>May 2005</b>																		
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
<b>August 2006</b>																		
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
<b>August 2007</b>																		
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
<b>August 2008</b>																		
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
<b>September 2011</b>																		
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

Note: The table shows alternative specifications for Equation 2. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within  $k$  months following a minimum wage hike. Three sets of estimates are provided: the "No Hole" title presents the results for the standard specification as a benchmark; "1 ARS" and "5 ARS" titles report the estimates obtained by excluding observations with baseline wages that are close to the minimum wage cutoff, with 1 and 5 ARS respectively. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

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

Event/Polynomial Order <i>k</i>	Linear						Quadratic					
	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>												
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
<b>July 2003</b>												
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
<b>January 2004</b>												
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
<b>September 2004</b>												
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
<b>May 2005</b>												
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
<b>August 2006</b>												
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
<b>August 2007</b>												
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
<b>August 2008</b>												
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
<b>September 2011</b>												
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

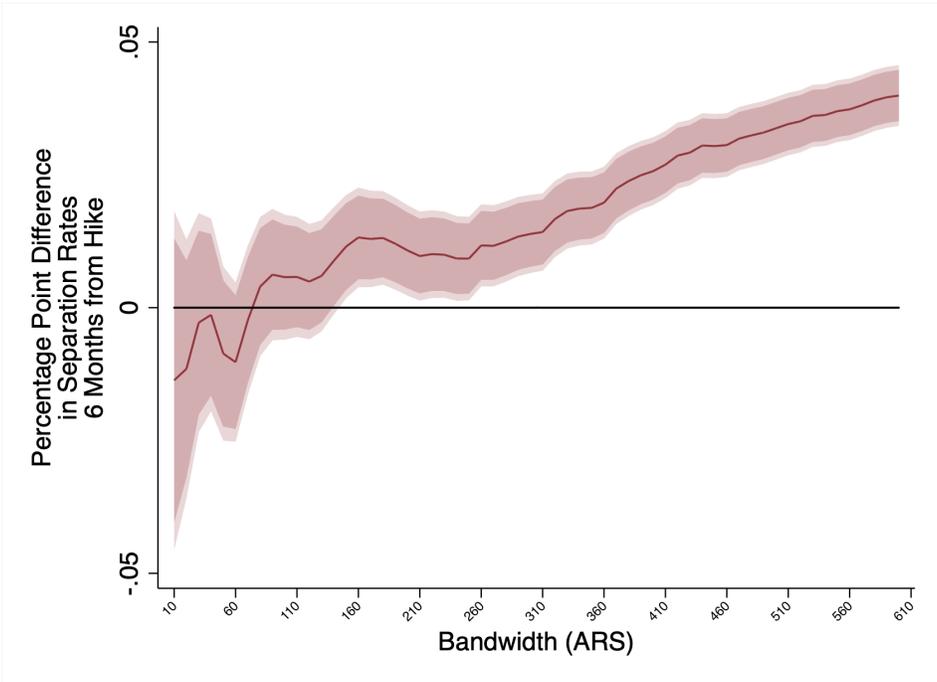
Note: The table shows alternative specifications for Equation 2. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within *k* months following a minimum wage hike. Two sets of estimates are provided: the "Linear" title presents the results for the standard specification as a benchmark; the "Quadratic" title reports the estimates obtained by including a quadratic term of the running variable and its interaction with the treatment dummy. Standard errors between parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

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

Event/Bandwidth <i>k</i>	Optimal						0.5 · Optimal						1.5 · Optimal					
	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>																		
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
<b>July 2003</b>																		
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
<b>January 2004</b>																		
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
<b>September 2004</b>																		
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
<b>May 2005</b>																		
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
<b>August 2006</b>																		
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
<b>August 2007</b>																		
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
<b>August 2008</b>																		
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
<b>September 2011</b>																		
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

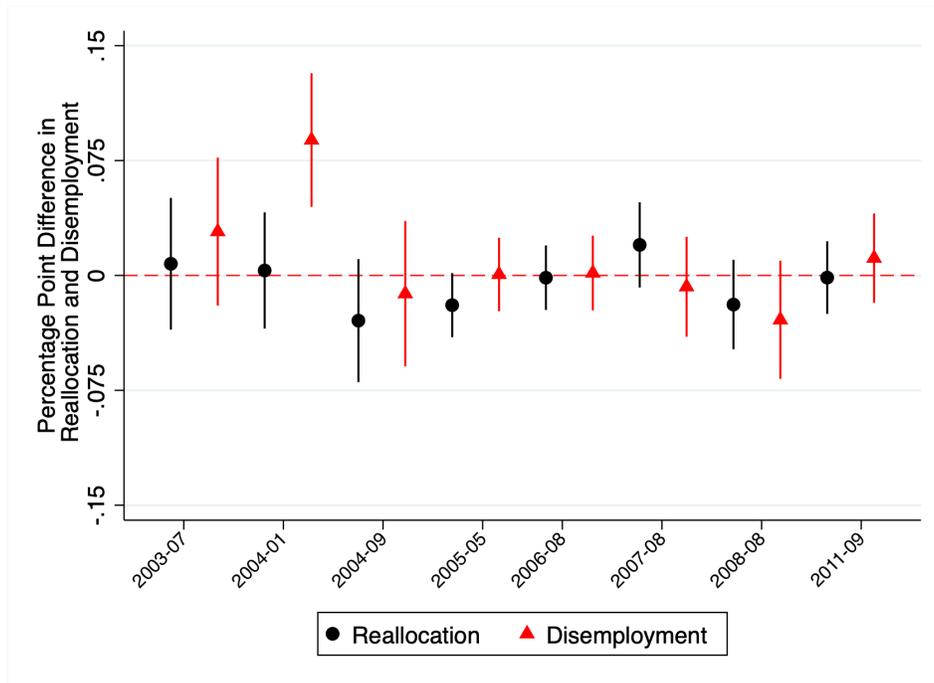
Note: The table shows alternative specifications for Equation 2. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within  $k$  months following a minimum wage hike. Three sets of estimates are provided: the “Optimal” title presents the results for the standard specification using the bandwidth derived using Calonico et al. (2014) as a benchmark; “0.5 · Optimal” and “1.5 · Optimal” titles report the estimates obtained by estimating Equation 2 using half and one-and-a-half times the optimal bandwidth. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

Figure B.1: Effect of a Minimum Wage Hike on Match Destruction 6 Months From Hike for Different Bandwidths.



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.

Figure B.2: Reallocation and Disemployment Effects of a Minimum Wage Hike 6 Months From Hike.



Note: The point estimates presented in this table are obtained by estimating Equation 2 using two dummy variables. The black circles represent the effects of minimum wage hikes on a dummy indicating whether workers who lost their jobs were able to find another job within 6 months of the hike. The red triangles represent the disemployment effects of the minimum wage. Additional information on the construction of these variables can be found in Appendix A. Both point estimates and 90% confidence intervals are reported for each minimum wage hike. It is important to note that the point estimates for each hike sum up to  $\beta_1$  for  $k = 6$ .

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

Model/ $k$	1	2	3	4	5	6
<b>a) 1996</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.031*** (0.004)	0.063*** (0.005)	0.083*** (0.006)	0.098*** (0.007)	0.107*** (0.008)	0.183*** (0.009)
Obs.	79,430	79,430	79,430	79,430	79,430	79,430
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.005 (0.012)	0.003 (0.016)	0.001 (0.020)	-0.013 (0.023)	-0.019 (0.024)	-0.047 (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>b) 1997</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.020*** (0.004)	0.039*** (0.005)	0.059*** (0.006)	0.071*** (0.007)	0.092*** (0.007)	0.156*** (0.009)
Obs.	86,393	86,393	86,393	86,393	86,393	86,393
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.008 (0.010)	-0.012 (0.015)	-0.042** (0.018)	-0.061*** (0.019)	-0.055*** (0.020)	-0.136*** (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
<b>c) 1998</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.021*** (0.004)	0.051*** (0.005)	0.074*** (0.006)	0.100*** (0.007)	0.113*** (0.007)	0.164*** (0.009)
Obs.	93,561	93,561	93,561	93,561	93,561	93,561
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.006 (0.011)	0.013 (0.017)	0.026 (0.020)	0.028 (0.021)	0.003 (0.019)	0.004 (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
<b>d) 1999</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.018*** (0.003)	0.051*** (0.005)	0.073*** (0.006)	0.087*** (0.006)	0.097*** (0.007)	0.163*** (0.008)
Obs.	93,241	93,241	93,241	93,241	93,241	93,241
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.003 (0.010)	-0.003 (0.014)	0.010 (0.017)	0.020 (0.019)	0.042* (0.025)	0.000 (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
<b>e) 2000</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.017*** (0.003)	0.041*** (0.005)	0.057*** (0.006)	0.073*** (0.006)	0.084*** (0.007)	0.138*** (0.008)
Obs.	92917	92917	92917	92917	92917	92917
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.014 (0.011)	0.004 (0.016)	-0.013 (0.019)	-0.025 (0.02)	-0.035 (0.021)	-0.101*** (0.027)
Obs.	5075	6232	5743	6186	6041	4650
Power(0.09)	1.000	0.961	0.886	0.846	0.798	0.585
<b>f) Pool</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.021*** (0.002)	0.049*** (0.002)	0.069*** (0.003)	0.086*** (0.003)	0.099*** (0.003)	0.160*** (0.004)
Obs.	445,542	445,542	445,542	445,542	445,542	445,542
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.001 (0.005)	-0.002 (0.008)	-0.011 (0.010)	-0.017* (0.010)	-0.021** (0.009)	-0.056*** (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

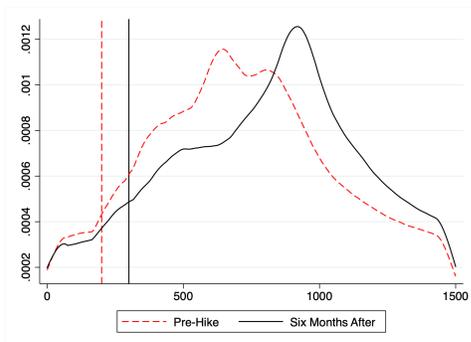
Note: The table shows the results from placebo estimations of Equation 2 in time periods where the minimum wage was not raised. Namely, we assume non-existent minimum wage increase from 200 to 300 ARS every August between 1996 and 2000. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within  $k$  months following a placebo minimum wage hike. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

Table B.5: Standard Panel Data Approach v. RDD Performance in Placebo Experiments. False Minimum Wage Hikes 6 Months Prior to the Effective Enactment.

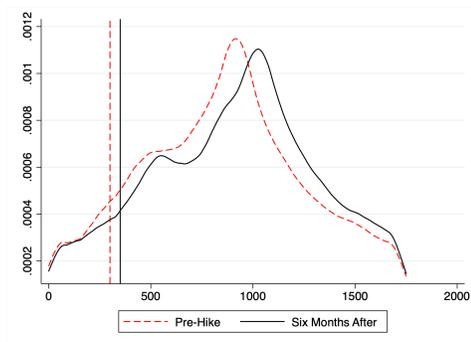
Model/ <i>k</i>	1	2	3	4	5	6
<b>a) July 2003</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.073***	0.106***	0.160***	0.205***	0.220***	0.224***
	(0.006)	(0.008)	(0.009)	(0.010)	(0.011)	(0.011)
Obs.	88,363	88,363	88,363	88,363	88,363	88,363
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.021	-0.041	-0.019	-0.010	-0.025	-0.015
	(0.021)	(0.027)	(0.032)	(0.035)	(0.035)	(0.036)
Obs.	3,243	2,969	2,707	2,514	2,606	2,521
Power(0.09)	0.858	0.627	0.484	0.401	0.396	0.381
<b>b) January 2004</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.053***	0.084***	0.122***	0.157***	0.163***	0.162***
	(0.007)	(0.009)	(0.011)	(0.013)	(0.014)	(0.014)
Obs.	88,553	88,553	88,553	88,553	88,553	88,553
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.012	0.020	0.023	0.036	0.028	0.022
	(0.014)	(0.018)	(0.021)	(0.023)	(0.024)	(0.025)
Obs.	5,605	5,380	5,512	5,264	5,402	5,331
Power(0.09)	0.974	0.867	0.776	0.656	0.641	0.597
<b>c) September 2004</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.053***	0.098***	0.108***	0.123***	0.167***	0.170***
	(0.004)	(0.006)	(0.007)	(0.008)	(0.011)	(0.011)
Obs.	92,686	92,686	92,686	92,686	92,686	92,686
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.008	0.015	0.046*	0.052*	0.054*	0.059**
	(0.019)	(0.028)	(0.027)	(0.029)	(0.031)	(0.030)
Obs.	2,665	2,729	3,238	3,086	3,891	4,284
Power(0.09)	0.891	0.532	0.563	0.502	0.457	0.484
<b>d) May 2005</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.003	0.019***	0.030***	0.045***	0.059***	0.071***
	(0.002)	(0.003)	(0.004)	(0.004)	(0.005)	(0.005)
Obs.	94,485	94,485	94,485	94,485	94,485	94,485
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.003	0.004	0.003	0.000	0.016	0.018
	(0.005)	(0.011)	(0.012)	(0.014)	(0.015)	(0.016)
Obs.	14,941	8,129	9,717	8,556	9,229	9,099
Power(0.09)	1.000	0.999	0.996	0.980	0.968	0.939
<b>e) August 2006</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.022***	0.045***	0.066***	0.082***	0.089***	0.095***
	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)
Obs.	106,979	106,979	106,979	106,979	106,979	106,979
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.002	-0.022*	-0.038**	-0.037*	-0.043**	-0.049***
	(0.008)	(0.012)	(0.018)	(0.019)	(0.018)	(0.019)
Obs.	9,632	8,501	5,914	6,013	7,507	7,835
Power(0.09)	1.000	0.999	0.939	0.897	0.912	0.887
<b>f) August 2007</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.017***	0.040***	0.063***	0.076***	0.083***	0.092***
	(0.003)	(0.004)	(0.005)	(0.006)	(0.006)	(0.006)
Obs.	115,140	115,140	115,140	115,140	115,140	115,140
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.007	0.013	-0.001	0.002	-0.008	-0.008
	(0.009)	(0.012)	(0.017)	(0.019)	(0.019)	(0.021)
Obs.	7,509	8,787	7,009	6,898	7,182	6,558
Power(0.09)	1.000	0.997	0.944	0.886	0.858	0.802
<b>g) August 2008</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.014***	0.041***	0.061***	0.079***	0.084***	0.094***
	(0.002)	(0.004)	(0.004)	(0.005)	(0.005)	(0.006)
Obs.	126,249	126,249	126,249	126,249	126,249	126,249
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.001	0.003	0.006	-0.002	-0.009	-0.004
	(0.008)	(0.014)	(0.016)	(0.019)	(0.019)	(0.020)
Obs.	6,685	7,557	7,575	6,527	7,377	7,496
Power(0.09)	1.000	0.986	0.932	0.839	0.846	0.820
<b>h) September 2011</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.014***	0.040***	0.058***	0.072***	0.080***	0.094***
	(0.003)	(0.004)	(0.005)	(0.005)	(0.006)	(0.006)
Obs.	139,206	139,206	139,206	139,206	139,206	139,206
<b>Regression Discontinuity Design</b>						
Treatment Effect	-0.004	-0.016	-0.031**	-0.028*	-0.032*	-0.028
	(0.008)	(0.012)	(0.014)	(0.016)	(0.017)	(0.018)
Obs.	9,827	10,268	10,267	9,284	8,857	8,577
Power(0.09)	1.000	0.997	0.982	0.935	0.902	0.872
<b>i) Pool</b>						
<b>Standard Panel Data (OLS)</b>						
Treatment Effect	0.028***	0.058***	0.083***	0.100***	0.119***	0.128***
	(0.001)	(0.001)	(0.002)	(0.002)	(0.002)	(0.002)
Obs.	851,661	851,661	851,661	851,661	851,661	851,661
<b>Regression Discontinuity Design</b>						
Treatment Effect	0.003	-0.002	-0.007	-0.005	0.001	-0.005
	(0.004)	(0.005)	(0.006)	(0.006)	(0.008)	(0.008)
Obs.	50,430	54,771	57,218	59,958	46,311	52,637
Power(0.09)	1.000	1.000	1.000	1.000	1.000	1.000

Note: The table shows the results from placebo estimations of Equation 2 in time periods where the minimum wage was not raised. Namely, we falsely assume that the minimum wage hikes in Table 1 occurred six months in advance. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within *k* months following a placebo minimum wage hike. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

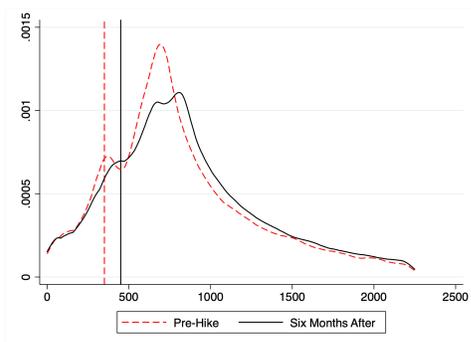
Figure B.3: Distribution of Nominal Wages. Event by Event.



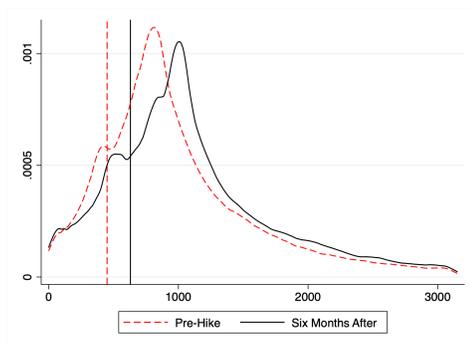
(a) July 2003



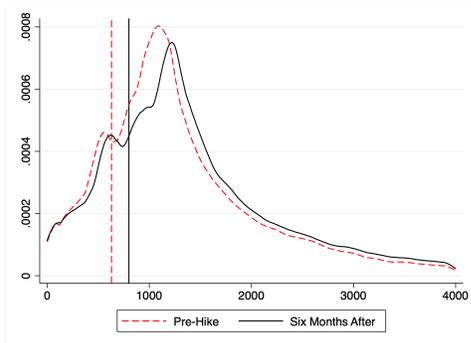
(b) January 2004



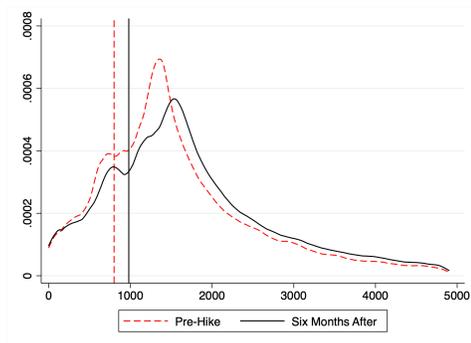
(c) September 2004



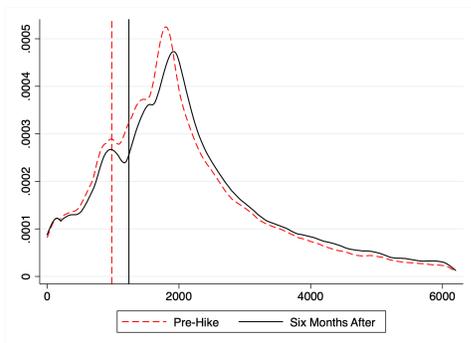
(d) May 2005



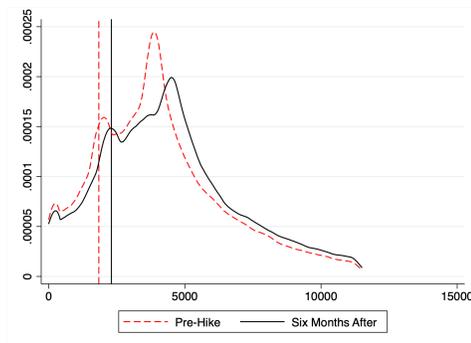
(e) August 2006



(f) August 2007



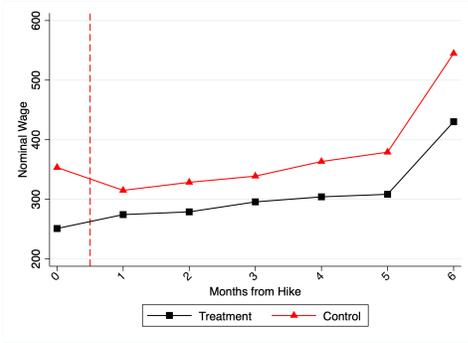
(g) August 2008



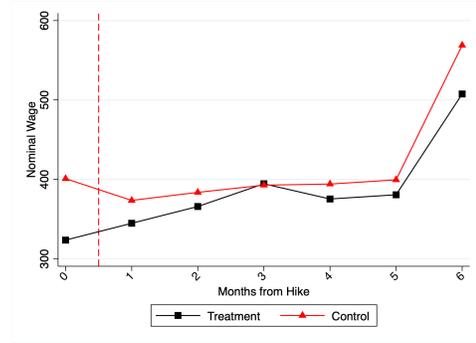
(h) September 2011

Note: The figures illustrate the distribution of nominal wages one month prior to each minimum wage hike and six months after their implementation. These figures are based on the raw data, including matches with earnings below the minimum wage. For visualization purposes, observations with wages above 5 times the new minimum wage have been removed. The vertical dashed and solid lines indicate the pre-hike minimum wage and the effective nominal minimum wages six months from the change respectively.

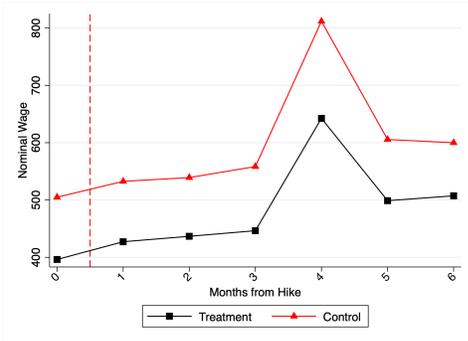
Figure B.4: Nominal Wage Trends by Group. Event by Event.



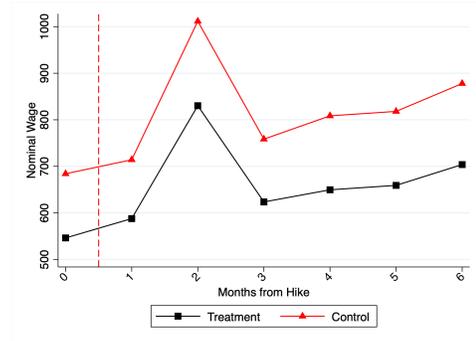
(a) July 2003



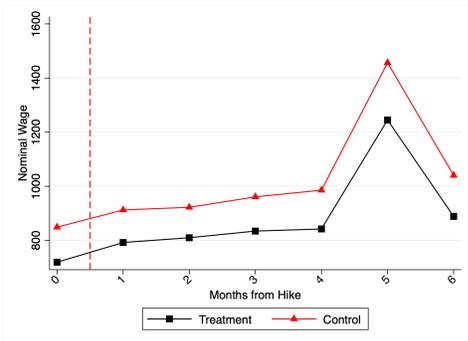
(b) January 2004



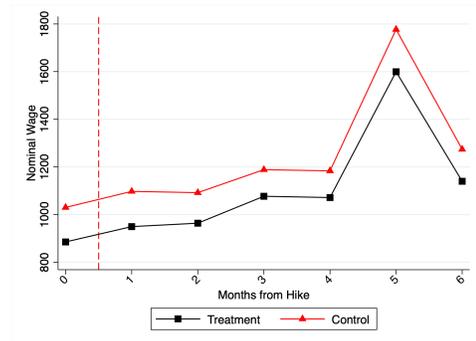
(c) September 2004



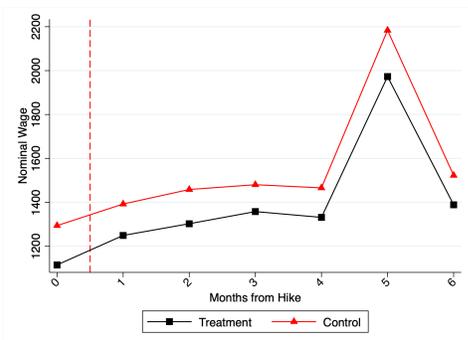
(d) May 2005



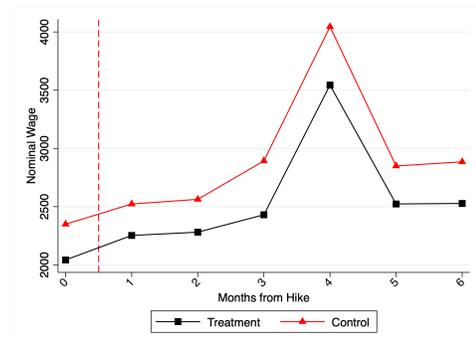
(e) August 2006



(f) August 2007



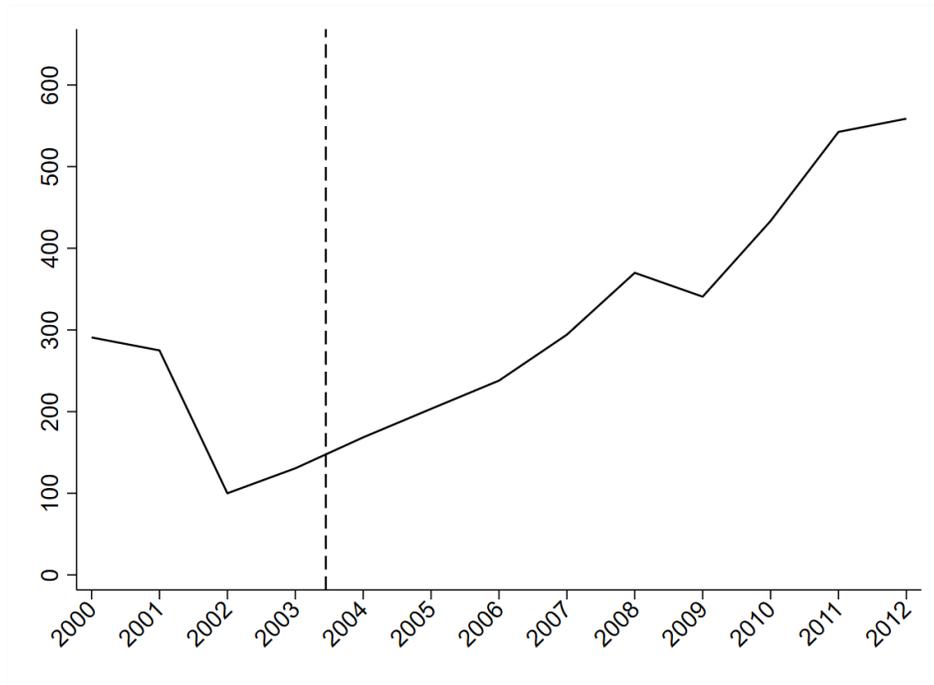
(g) August 2008



(h) September de 2011

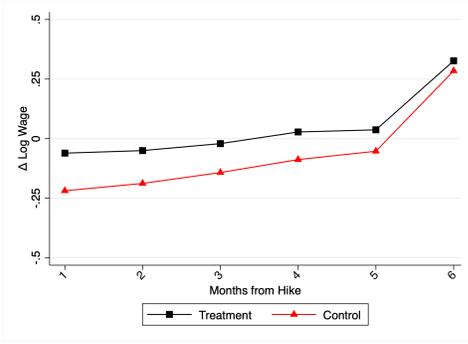
Note: The graph illustrates the change in average nominal wages for two groups of workers before and after every minimum wage hike. Month 0 denotes the month before the minimum wage hike is enacted, months 1 to 6 denotes the number of months in which the new minimum wage has been in effect. In black squares we show the evolution of nominal wages for the treatment group comprised of workers with baseline wages below the new minimum wage for at most one optimal bandwidth. In red triangles we show the evolution of nominal wages for the control group comprised of workers with baseline wages above the new minimum wage for at most one optimal bandwidth following Calonico et al. (2014). We compute a hike-specific optimal bandwidth following Calonico et al. (2014).

Figure B.5: Real GDP Trend: 2000-2012.

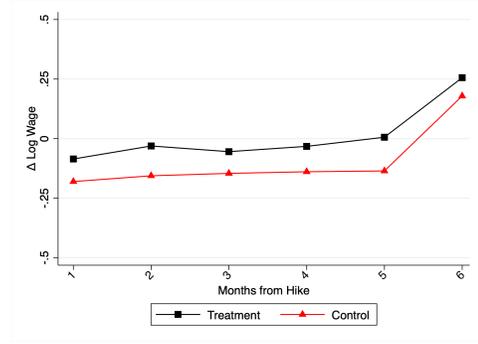


Note: The figure shows the evolution of the real GDP between 2000 and 2012. We deflate the nominal minimum wage to 2008 ARS and then re-express them as a percentage of its 2000 value. The dashed vertical line marks the onset of the period during which the minimum wage was regularly increased.

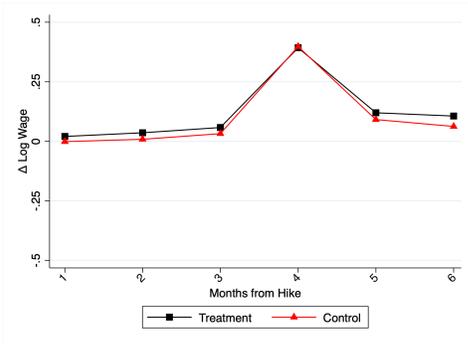
Figure B.6: Real Wage Growth with Respect to  $t = 0$  by Group. Event by Event.



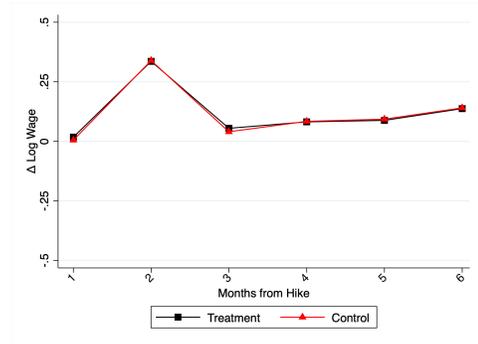
(a) July 2003



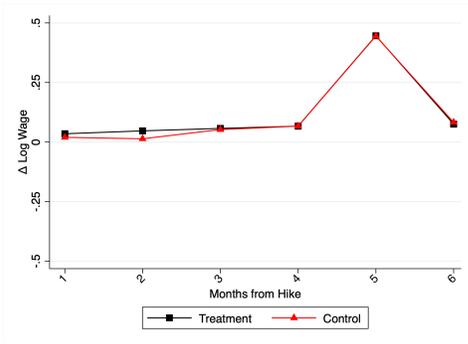
(b) January 2004



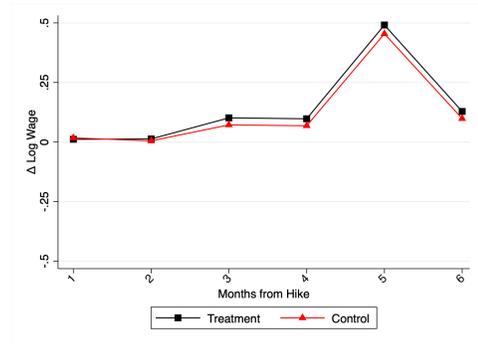
(c) September 2004



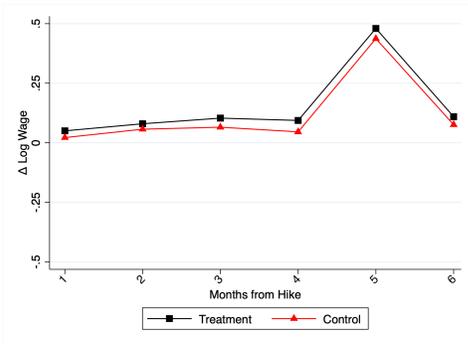
(d) May 2005



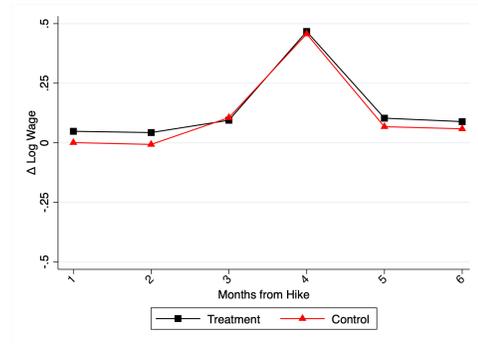
(e) August 2006



(f) August 2007



(g) August 2008



(h) September de 2011

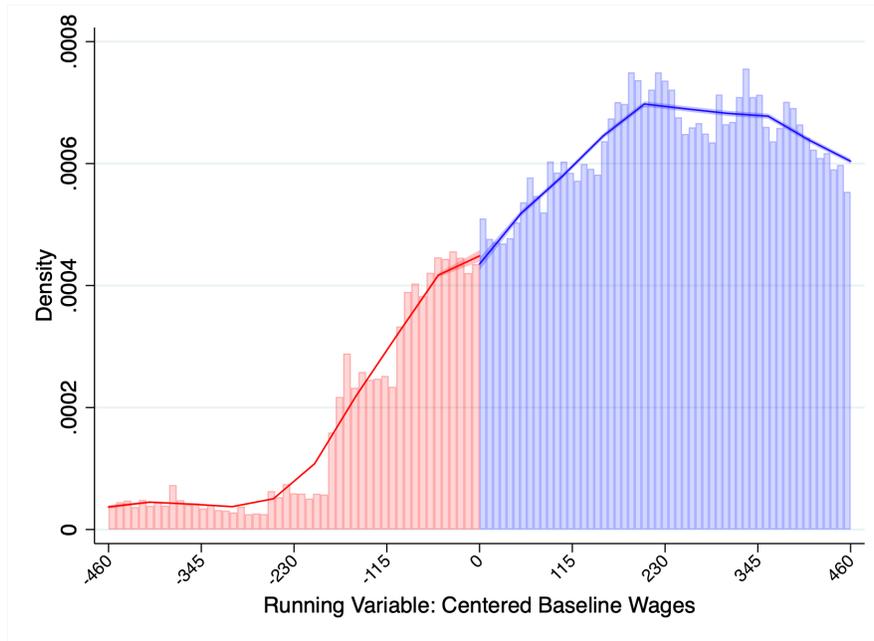
Note: The figures show the cumulative difference in average log wages with respect to the month before each minimum wage hike by group. We plot the results for the treatment group using a black square, those of the control group with a red triangle. Nominal wages are deflated to 2008 ARS.

## C 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.

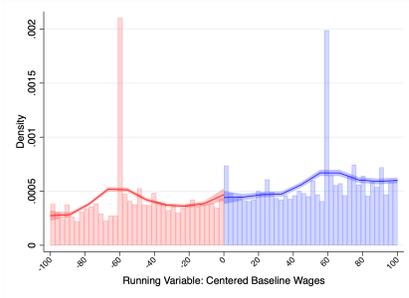
Below we graphically report the graphical tests and their associated  $T$ -statistics and p-values. We use second degree polynomials to construct the point estimator for the density at each point and also its standard deviation.

Figure C.1: Density Test. Pool.



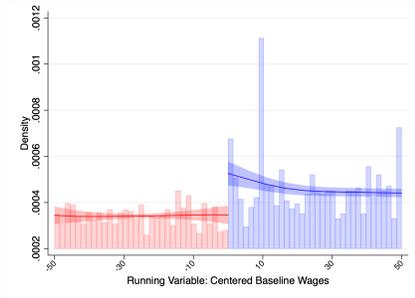
Note: This figure presents the results of a density difference test for the running variable, pre-hike wages centered on the new minimum wage  $(W - MW')_{i,t=0}$ , on either side of the cutoff point as per Cattaneo et al. (2018). The test combines all the minimum wage hikes presented in Table 1 considered in the study. The test statistic  $T=-1.78$ , and the P-value=0.08.

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



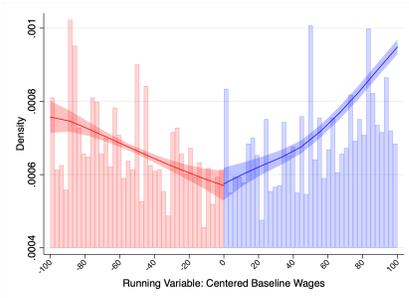
T=1.360, P-value=0.174.

(a) July 2003



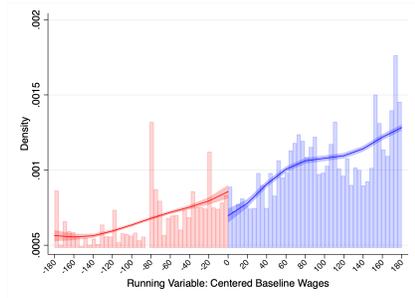
T=4.995, P-value=0.000.

(b) January 2004



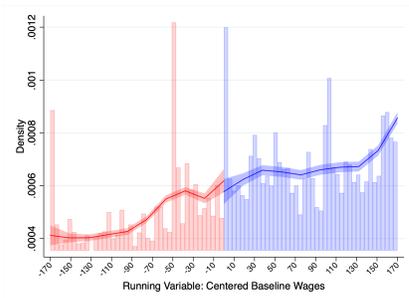
T=0.313, P-value=0.754.

(c) September 2004



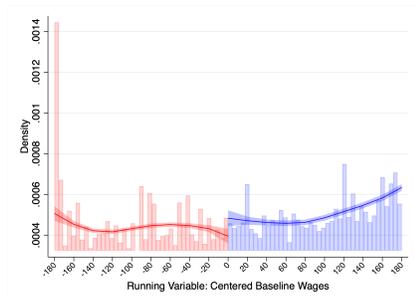
T=-4.783, P-value=0.000.

(d) May 2005



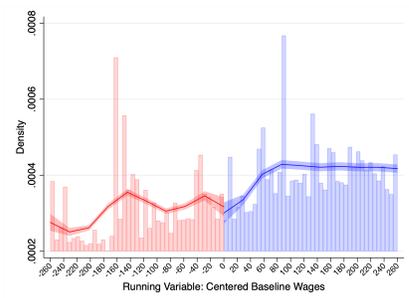
T= 2.205, P-value= 0.027.

(e) August 2006



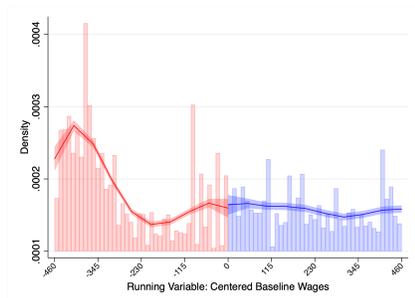
T=3.305, P-value=0.001.

(f) August 2007



T=-0.836, P-value=0.403.

(g) August 2008



T=1.088, P-value=0.277.

(h) September de 2011

Note: This figure displays the results of a density difference test for the running variable, pre-hike wages centered on the new minimum wage  $(W - MW')_{i,t=0}$ , on either side of the cutoff point as per Cattaneo et al. (2018). The test is conducted separately for each event presented in Table 1 considered in the study. The test statistics and P-values are reported under the graph for each event.

## D Covariate Smoothness

It is usual to couple the results of a regression discontinuity design with tests for covariate smoothness. 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 D.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<sup>21</sup> compared to the standard panel data approach, some differences still persist.

To provide a more formal test of this conjecture, in Figure D.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 D.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 exercise.

Finally, in Tables D.3 and D.4 we provide estimates of the effect of minimum wage hikes on different sub-samples defined by their demographic profiles and industry. We show that the effects of minimum wage hikes are concentrated on workers who are male, who work in Buenos Aires, and that work in the Utilities and Transportation industries.

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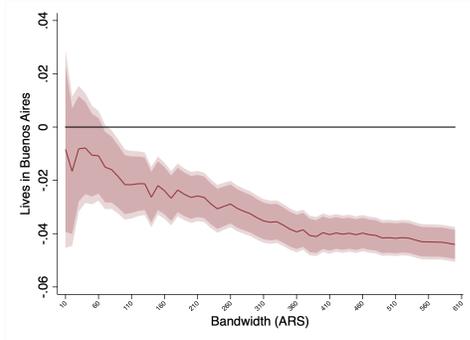
<sup>21</sup>See Table 2.

Table D.1: Covariate Smoothness Test

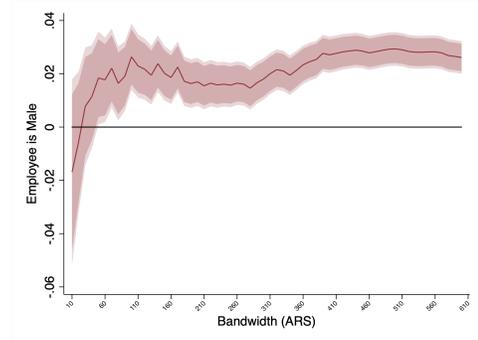
Event/Covariate	Buenos Aires	Male	Date of Birth (Year)	Firm Size > 50	Primary Sector
<b>Pool</b>					
Treatment Effect	-0.021***	0.019**	0.466**	-0.014	0.037***
Standard Error	(0.008)	(0.008)	(0.217)	(0.009)	(0.007)
Obs.	68,643	50,297	51,164	40,325	32,667
<b>July 2003</b>					
Treatment Effect	0.029	-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
<b>January 2004</b>					
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
<b>September 2004</b>					
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
<b>May 2005</b>					
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
<b>August 2006</b>					
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
<b>August 2007</b>					
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
<b>August 2008</b>					
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
<b>September 2011</b>					
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: The table presents alternative specifications for Equation 2 in which the outcome variable has been replaced with a series of characteristics of the employer-employee match. These include dummies for the location of the match (Buenos Aires), the gender of the worker, a variable indicating the worker's year of birth, dummies for firm size, and dummies for working in the primary sector. Additional information on how these variables were constructed can be found in Appendix A. Standard errors between parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

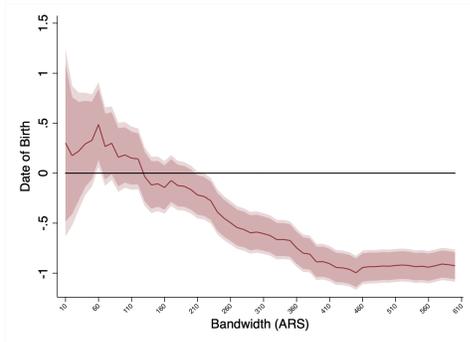
Figure D.1: Covariate Imbalance for Different Bandwidth Choice.



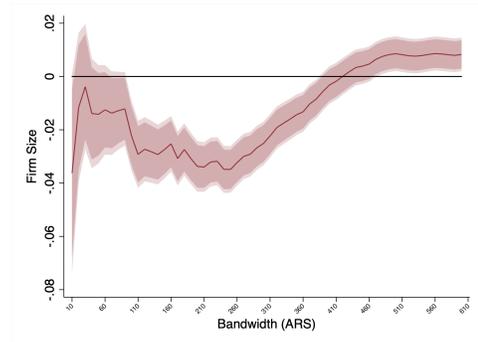
(a) GBA.



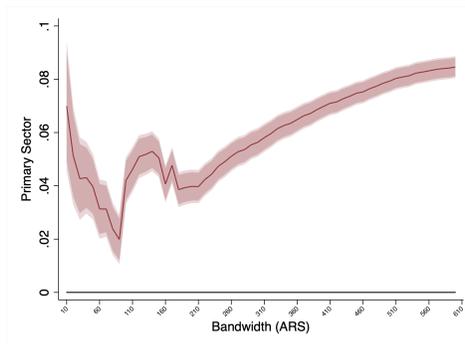
(b) Male.



(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. These include dummies for the location of the match (Buenos Aires), the gender of the worker, a variable indicating the worker's year of birth, dummies for firm size, and dummies for working in the primary sector. Additional information on how these variables were constructed can be found in Appendix A. 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 D.2: Effect of a Minimum Wage Hike on Match Destruction. Specification with Covariates.

Event/Bandwidth $k$	Optimal						$0.5 \times$ Optimal						$1.5 \times$ Optimal					
	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>																		
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
<b>July 2003</b>																		
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
<b>January 2004</b>																		
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
<b>September 2004</b>																		
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
<b>May 2005</b>																		
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
<b>August 2006</b>																		
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
<b>August 2007</b>																		
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
<b>August 2008</b>																		
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
<b>September 2011</b>																		
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

Note: The table shows alternative specifications for Equation 2. The outcome in each column is a dummy variable that indicates whenever an employer-employee match was terminated at some point within  $k$  months following a minimum wage hike. Three sets of estimates are provided: the "Optimal" title presents the results for the standard specification using the bandwidth derived using Calonico et al. (2014) as a benchmark; " $0.5 \cdot$  Optimal" and " $1.5 \cdot$  Optimal" titles report the estimates obtained by estimating Equation 2 using half and one-and-a-half times the optimal bandwidth. We include controls for sex, geographical location, year of birth, firm size and economic sector following Calonico et al. (2019). Additional information on how these variables were constructed can be found in Appendix A. Standard errors between parenthesis. \*\*\*  $p < 0.01$ , \*\*  $p < 0.05$ , \*  $p < 0.1$ . Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

Table D.3: Effect of a Minimum Wage Hike on Match Destruction. By Sex and Place of Residence.

Event/Group <i>k</i>	Males						Females						Buenos Aires						Out of Buenos Aires					
	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>																								
Treatment Effect	0.005	0.003	0.003	0.005	0.006	0.002	0.002	0.004	-0.001	0.004	0.004	0.011	0.009*	0.006	-0.000	0.003	-0.017*	-0.018*	0.002	0.003	0.005	0.011	0.009	0.015
Standard Error	(0.004)	(0.006)	(0.006)	(0.006)	(0.008)	(0.008)	(0.005)	(0.007)	(0.008)	(0.008)	(0.009)	(0.008)	(0.005)	(0.006)	(0.006)	(0.007)	(0.010)	(0.011)	(0.005)	(0.007)	(0.008)	(0.008)	(0.009)	(0.009)
Obs	58,449	59,203	67,912	68,773	52,377	55,669	34,064	31,382	32,145	37,354	33,015	38,736	42,385	50,998	53,078	51,164	28,819	27,310	41,731	38,062	37,446	37,713	38,609	34,826
<b>July 2003</b>																								
Treatment Effect	0.045*	0.026	0.020	0.061*	0.071*	0.070*	-0.045	-0.067*	-0.040	-0.043	-0.057	-0.050	0.009	-0.017	-0.050	-0.063	-0.046	-0.059	0.000	0.011	0.017	0.074	0.075	0.074
Standard Error	(0.024)	(0.030)	(0.033)	(0.036)	(0.036)	(0.037)	(0.027)	(0.035)	(0.050)	(0.048)	(0.046)	(0.047)	(0.023)	(0.030)	(0.037)	(0.038)	(0.038)	(0.039)	(0.028)	(0.036)	(0.041)	(0.046)	(0.047)	(0.049)
Obs	2,851	2,783	2,748	2,652	2,830	2,776	1,511	1,418	923	1,145	1,313	1,335	3,178	2,365	1,968	2,142	2,267	2,337	1,705	1,623	1,639	1,546	1,545	1,459
<b>January 2004</b>																								
Treatment Effect	0.041	0.056	0.059	0.081**	0.083**	0.075*	0.043	0.049	0.024	0.056	0.055	0.075*	0.070**	0.090***	0.079**	0.114***	0.099***	0.095**	0.024	0.019	0.002	0.049	0.054	0.091**
Standard Error	(0.029)	(0.034)	(0.038)	(0.038)	(0.039)	(0.039)	(0.031)	(0.037)	(0.040)	(0.042)	(0.043)	(0.043)	(0.028)	(0.034)	(0.037)	(0.038)	(0.038)	(0.039)	(0.034)	(0.040)	(0.045)	(0.044)	(0.044)	(0.044)
Obs	2,906	2,896	2,894	2,899	2,858	2,879	1,952	1,808	1,800	1,832	1,847	1,911	2,715	2,676	2,666	2,651	2,648	2,620	1,818	1,797	1,735	1,904	1,989	1,953
<b>September 2004</b>																								
Treatment Effect	-0.035	-0.034	-0.019	-0.018	-0.033	-0.064	-0.020	0.006	0.022	-0.003	-0.030	-0.024	-0.010	-0.027	0.023	0.008	-0.005	-0.011	-0.053**	-0.024	-0.045	0.006	-0.040	-0.025
Standard Error	(0.025)	(0.031)	(0.029)	(0.032)	(0.035)	(0.041)	(0.021)	(0.025)	(0.029)	(0.033)	(0.042)	(0.046)	(0.020)	(0.028)	(0.027)	(0.030)	(0.033)	(0.035)	(0.025)	(0.032)	(0.038)	(0.038)	(0.042)	(0.047)
Obs	1,728	2,007	2,939	2,877	2,700	2,039	1,303	1,855	1,825	1,590	1,317	1,176	2,282	1,840	2,602	2,648	2,520	2,435	1,334	1,752	1,617	1,802	1,684	1,378
<b>May 2005</b>																								
Treatment Effect	0.012	-0.002	0.012	0.005	-0.012	-0.013	-0.011	0.005	-0.014	-0.037	-0.042	-0.041	-0.001	-0.009	-0.006	-0.012	-0.013	-0.019	0.011	0.002	0.023	-0.002	-0.021	-0.021
Standard Error	(0.012)	(0.016)	(0.019)	(0.021)	(0.022)	(0.022)	(0.012)	(0.018)	(0.021)	(0.026)	(0.028)	(0.030)	(0.011)	(0.017)	(0.020)	(0.021)	(0.022)	(0.023)	(0.013)	(0.018)	(0.020)	(0.024)	(0.025)	(0.026)
Obs	5,178	5,233	5,271	5,305	5,272	5,746	3,362	2,603	2,799	2,603	2,566	2,329	4,467	4,110	4,197	4,657	5,039	5,153	3,908	3,884	4,513	3,759	3,910	3,816
<b>August 2006</b>																								
Treatment Effect	0.012	0.006	0.008	0.052*	0.040	0.050	0.004	0.009	0.006	-0.001	0.012	0.015	0.001	-0.002	0.004	0.015	0.010	0.010	0.013	0.008	-0.002	0.000	-0.003	-0.002
Standard Error	(0.011)	(0.018)	(0.019)	(0.027)	(0.029)	(0.031)	(0.016)	(0.023)	(0.026)	(0.029)	(0.035)	(0.037)	(0.012)	(0.018)	(0.021)	(0.022)	(0.023)	(0.025)	(0.014)	(0.020)	(0.023)	(0.028)	(0.028)	(0.029)
Obs	5,384	4,556	5,671	3,112	3,056	2,810	2,036	2,169	2,275	2,197	1,689	1,631	4,161	4,254	4,339	4,645	4,630	4,431	3,203	3,749	3,927	3,288	3,656	3,682
<b>August 2007</b>																								
Treatment Effect	0.010	-0.005	-0.032	-0.022	0.017	0.039	-0.035**	-0.041	-0.042	-0.044	-0.057	-0.019	-0.022	-0.020	-0.018	-0.045	-0.038	-0.022	0.013	0.006	-0.024	0.018	0.019	0.064*
Standard Error	(0.016)	(0.025)	(0.030)	(0.033)	(0.034)	(0.033)	(0.017)	(0.029)	(0.033)	(0.037)	(0.037)	(0.041)	(0.014)	(0.026)	(0.030)	(0.035)	(0.035)	(0.033)	(0.016)	(0.023)	(0.030)	(0.032)	(0.031)	(0.034)
Obs	3,033	2,474	2,264	2,281	2,522	2,824	1,698	1,596	1,605	1,522	1,725	1,516	3,884	2,369	2,293	2,099	2,284	2,947	2,092	2,465	1,885	2,093	2,671	2,291
<b>August 2008</b>																								
Treatment Effect	0.025*	-0.019	-0.033	-0.048*	-0.061**	-0.088***	-0.002	0.011	-0.005	-0.014	-0.035	-0.014	0.030*	-0.006	-0.063**	-0.068**	-0.098***	-0.144***	-0.010	0.002	0.011	0.008	0.009	0.014
Standard Error	(0.015)	(0.021)	(0.024)	(0.027)	(0.029)	(0.030)	(0.017)	(0.027)	(0.029)	(0.033)	(0.038)	(0.041)	(0.017)	(0.023)	(0.029)	(0.031)	(0.035)	(0.041)	(0.016)	(0.024)	(0.028)	(0.032)	(0.034)	(0.036)
Obs	3,162	3,095	3,102	3,171	3,069	3,060	2,314	2,025	2,318	2,087	1,647	1,714	2,758	2,756	2,220	2,303	2,057	1,646	2,590	2,283	2,306	2,140	2,163	2,023
<b>September 2011</b>																								
Treatment Effect	0.000	-0.002	-0.012	-0.011	-0.005	0.007	-0.007	-0.019	-0.016	0.009	0.013	0.028	0.006	0.003	-0.002	0.014	-0.007	-0.014	-0.002	-0.013	-0.042	-0.013	-0.018	0.006
Standard Error	(0.011)	(0.018)	(0.022)	(0.023)	(0.025)	(0.026)	(0.020)	(0.027)	(0.030)	(0.033)	(0.036)	(0.038)	(0.014)	(0.019)	(0.023)	(0.026)	(0.030)	(0.033)	(0.014)	(0.021)	(0.027)	(0.028)	(0.033)	(0.036)
Obs	4,693	4,234	3,937	4,234	4,293	4,114	1,826	1,618	1,558	1,591	1,704	1,680	3,319	3,714	3,171	3,185	2,582	2,456	3,000	2,992	2,526	2,753	2,164	1,992

Note: The table shows alternative specifications for Equation 2 in which we restrict the sample to specific sub-groups. We define these groups by sex and geographical location. Additional information on how these variables were constructed can be found in Appendix A. Standard errors between parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.

Table D.4: Effect of a Minimum Wage Hike on Match Destruction. By Industry.

Event/Industry k	Agriculture, Fishing and Mining (Primary Sector)						Utilities and Transport						Manufacturing and Construction						Others					
	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6	1	2	3	4	5	6
<b>Pool</b>																								
Treatment Effect	-0.012	-0.018	-0.027*	-0.014	-0.014	-0.018	0.044**	0.050**	0.041*	0.039	0.010	-0.004	0.012	0.017*	0.010	0.006	0.008	0.014	0.007*	0.006	0.004	0.007	0.007	0.009
Standard Error	(0.008)	(0.013)	(0.015)	(0.016)	(0.018)	(0.019)	(0.019)	(0.023)	(0.024)	(0.027)	(0.027)	(0.027)	(0.007)	(0.010)	(0.011)	(0.013)	(0.013)	(0.014)	(0.004)	(0.005)	(0.006)	(0.006)	(0.007)	(0.007)
Obs	9,783	9,233	8,813	8,925	8,313	7,710	3,937	4,314	4,406	4,038	4,416	4,596	22,426	21,562	22,375	19,332	21,113	17,950	67,720	63,737	62,508	63,026	51,499	63,652
<b>July 2003</b>																								
Treatment Effect	0.028	0.005	0.029	0.177**	0.197**	0.143	0.263***	0.230**	0.174*	0.137	0.136	0.145	0.007	0.037	0.039	-0.005	-0.003	-0.008	0.001	-0.021	-0.026	-0.036	-0.027	-0.008
Standard Error	(0.054)	(0.078)	(0.085)	(0.089)	(0.089)	(0.092)	(0.079)	(0.096)	(0.100)	(0.102)	(0.102)	(0.102)	(0.032)	(0.041)	(0.046)	(0.048)	(0.049)	(0.049)	(0.023)	(0.035)	(0.041)	(0.042)	(0.039)	(0.044)
Obs	526	459	458	484	451	440	444	426	424	426	426	426	1,832	1,627	1,567	1,497	1,626	1,679	2,498	1,683	1,459	1,620	1,959	1,608
<b>January 2004</b>																								
Treatment Effect	0.112*	0.040	-0.005	-0.033	-0.022	-0.009	0.166	0.220**	0.243**	0.271**	0.250**	0.241**	-0.002	-0.013	-0.016	-0.006	-0.025	-0.015	0.048*	0.072**	0.070**	0.109***	0.114***	0.125***
Standard Error	(0.062)	(0.080)	(0.087)	(0.091)	(0.091)	(0.090)	(0.102)	(0.111)	(0.114)	(0.116)	(0.115)	(0.115)	(0.052)	(0.061)	(0.063)	(0.066)	(0.065)	(0.065)	(0.025)	(0.029)	(0.033)	(0.034)	(0.035)	(0.035)
Obs	446	464	454	462	466	466	394	375	404	364	364	375	1,286	1,278	1,288	1,278	1,292	1,281	3,150	3,100	2,988	3,070	3,046	3,052
<b>September 2004</b>																								
Treatment Effect	-0.111	-0.036	-0.088	-0.050	-0.059	-0.076	0.042	0.119	0.121	0.164**	0.091	0.097	-0.042	-0.050	-0.062	-0.038	-0.034	-0.052	-0.031	-0.029	0.012	0.009	0.003	-0.014
Standard Error	(0.076)	(0.106)	(0.096)	(0.114)	(0.111)	(0.121)	(0.050)	(0.079)	(0.078)	(0.081)	(0.091)	(0.095)	(0.032)	(0.041)	(0.045)	(0.049)	(0.051)	(0.054)	(0.021)	(0.028)	(0.027)	(0.028)	(0.031)	(0.037)
Obs	216	281	357	274	297	251	476	385	461	467	443	424	1,294	1,306	1,346	1,404	1,326	1,246	1,729	1,815	2,603	2,790	2,770	2,076
<b>May 2005</b>																								
Treatment Effect	-0.003	-0.014	0.014	0.015	0.012	0.053	0.073*	0.052	0.027	0.014	-0.053	-0.057	0.018	-0.001	0.008	0.026	-0.002	0.025	0.002	0.003	-0.001	-0.017	-0.024	-0.027
Standard Error	(0.024)	(0.030)	(0.037)	(0.040)	(0.042)	(0.045)	(0.044)	(0.058)	(0.059)	(0.073)	(0.069)	(0.073)	(0.022)	(0.030)	(0.038)	(0.041)	(0.037)	(0.048)	(0.011)	(0.015)	(0.018)	(0.019)	(0.021)	(0.021)
Obs	1,495	1,556	1,358	1,358	1,381	1,268	456	504	583	441	602	595	1,438	1,746	1,613	1,578	2,158	1,435	4,414	4,686	4,705	5,663	5,294	5,556
<b>August 2006</b>																								
Treatment Effect	-0.023	-0.029	0.003	0.027	0.014	0.005	0.087**	0.101	0.094	0.110	0.081	0.044	0.001	-0.005	-0.019	0.000	-0.029	-0.014	0.007	0.012	0.015	0.012	0.019	0.016
Standard Error	(0.015)	(0.030)	(0.037)	(0.041)	(0.043)	(0.045)	(0.039)	(0.067)	(0.077)	(0.084)	(0.088)	(0.094)	(0.024)	(0.031)	(0.038)	(0.041)	(0.045)	(0.046)	(0.012)	(0.018)	(0.022)	(0.024)	(0.026)	(0.027)
Obs	1,968	1,722	1,360	1,278	1,337	1,342	388	409	424	393	391	380	1,552	2,070	1,902	1,753	1,580	1,619	3,895	3,625	3,708	3,629	3,251	3,484
<b>August 2007</b>																								
Treatment Effect	0.013	0.037*	0.049*	0.087***	0.090***	0.093***	0.058	-0.008	-0.034	-0.016	-0.043	-0.035	-0.006	-0.035	-0.063	-0.061	-0.074*	-0.057	-0.023*	-0.025	-0.049*	-0.017	-0.026	-0.007
Standard Error	(0.013)	(0.021)	(0.026)	(0.032)	(0.032)	(0.033)	(0.064)	(0.078)	(0.077)	(0.078)	(0.090)	(0.089)	(0.023)	(0.037)	(0.043)	(0.050)	(0.043)	(0.041)	(0.013)	(0.022)	(0.027)	(0.024)	(0.030)	(0.030)
Obs	2,369	2,137	2,001	1,907	2,103	2,140	424	435	476	553	477	497	2,122	1,631	1,431	1,161	1,798	2,175	3,067	2,779	2,413	3,853	2,784	3,141
<b>August 2008</b>																								
Treatment Effect	-0.027**	-0.036	-0.021	-0.025	-0.052	-0.004	-0.037	-0.010	-0.125	-0.146	-0.200**	-0.218**	0.056**	0.051	0.025	-0.001	0.020	0.023	-0.000	-0.014	-0.037	-0.050*	-0.067**	-0.090***
Standard Error	(0.013)	(0.026)	(0.027)	(0.030)	(0.034)	(0.035)	(0.059)	(0.078)	(0.078)	(0.091)	(0.097)	(0.103)	(0.025)	(0.032)	(0.041)	(0.045)	(0.047)	(0.051)	(0.014)	(0.022)	(0.025)	(0.028)	(0.029)	(0.033)
Obs	1,445	1,406	1,678	1,659	1,553	1,810	278	318	353	330	330	306	1,775	1,609	1,399	1,475	1,474	1,394	3,802	3,170	3,107	3,109	2,993	2,630
<b>September 2011</b>																								
Treatment Effect	-0.030**	-0.055***	-0.107***	-0.132***	-0.117***	-0.102***	0.046	0.060	0.050	0.048	0.020	0.074	0.008	0.023	0.002	0.043	0.072*	0.112***	0.006	-0.009	-0.014	0.001	0.011	0.045
Standard Error	(0.012)	(0.020)	(0.026)	(0.029)	(0.031)	(0.032)	(0.040)	(0.052)	(0.058)	(0.060)	(0.063)	(0.072)	(0.022)	(0.033)	(0.033)	(0.036)	(0.039)	(0.043)	(0.016)	(0.022)	(0.022)	(0.026)	(0.029)	(0.028)
Obs	2,181	2,309	2,186	2,094	2,058	2,056	590	629	622	620	601	524	1,967	1,845	2,317	2,215	2,232	1,966	2,512	2,417	3,605	3,255	3,057	3,381

Note: The table shows alternative specifications for Equation 2 in which we restrict the sample to specific sub-groups. We define these groups by industry. Additional information on how these variables were constructed can be found in Appendix A. Standard errors between parenthesis. \*\*\* p<0.01, \*\* p<0.05, \* p<0.1. Differences in the number of observations are a result of selecting different optimal bandwidths for each specification.