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ABSTRACT

Consumption Responses to a Major Minimum Wage Increase: Evidence from Spain*

This paper investigates the effects of minimum wage increases on household consumption, focusing on Spain's 2019 minimum wage increase, which raised the floor on wages by an unprecedented 22.3 % in a low-inflation environment. Leveraging high-frequency, confidential transaction data from point-of-sale devices and credit card payments at the municipal level, we exploit geographic variation in exposure to the reform to identify its effects. We find that the increase led to a significant rise of 4.5 % in household consumption, with the largest gains concentrated in nonessential categories such as electronics, leisure, and spending at restaurants and hotels. We corroborate these findings using household-level data from the Spanish Household Budget Survey. Our findings can be rationalized by a simple model featuring nonhomothetic preferences.

JEL Classification: D12, E21, H31, J31, R20

Keywords: minimum wage, consumption, transaction data, discretionary spending, nonhomothetic preferences

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1 Introduction

The minimum wage has long been central to economic policy debates, with much of the discussion centered on its effects on employment, labor costs, and income distribution. While these labor market impacts have been extensively studied, especially since the groundbreaking work by Card and Krueger (1994), understanding the broader economic and social benefits of a minimum wage, such as poverty reduction, greater economic security, and improved worker well-being, requires that we look beyond these traditional outcomes.

One key channel through which these broader benefits emerge is household consumption. By raising the incomes of low-wage workers, minimum wage increases can significantly influence how low-income households allocate their spending—in terms of both total consumption and its composition. The extent to which a minimum wage provides immediate financial relief may be reflected in how much of the additional income is directed toward current consumption. The composition of such spending can also be revealing: Households' use of the extra income to meet essential needs would indicate tighter constraints than would spending on nonessential items. Analyzing these potential outcomes would yield important insight into how minimum wage policies affect low-income households. Moreover, a substantial body of research supports the view that consumption is a more reliable indicator of well-being than income (Deaton, 1992; Attanasio and Davis, 1996). Evaluating minimum wage policies through the lens of consumption, rather than income alone, thus offers a richer perspective on their welfare effects.

This paper examines the relationship between minimum wages and household consumption, using Spain's 2019 minimum wage reform as a case study. The policy introduced an unprecedented 22.3% increase in the statutory wage floor, one of the largest in recent European history (European Central Bank, 2022). In contrast to gradual, inflation-indexed adjustments, this substantial increase was explicitly aimed at strengthening the purchasing power of low-wage workers and was implemented in a context of low inflation, ensuring that most of the rise translated to meaningful real wage gains. The magnitude of the reform allows us to explore potential nonlinearities in the effects of the wage change on consumption behavior that would be difficult to detect in the case of more modest adjustments. Spain also provides a particularly relevant setting given the characteristics of its labor market. It has consistently stood as an outlier among advanced economies because of its persistently high unemployment rate—of 15.26% in 2018—and pronounced regional and demographic disparities in employment outcomes. These conditions introduce a degree of uncertainty regarding the net impact of a reform of this magnitude: Higher wages may boost consumption, but potential negative employment effects could offset these gains—particularly since the households and communities most affected by

the minimum wage often face more precarious labor market outcomes.

Our empirical strategy follows a two-pronged approach. First, we use high-frequency, confidential transaction data from point-of-sale (POS) terminals and credit card payments, provided by one of the largest commercial banks in Spain and a market leader in POS coverage. This dataset, available at the municipal level, enables us to exploit geographic variation in exposure to the minimum wage reform to estimate its impact on consumption. Second, we complement this analysis with household-level data from the Spanish Household Budget Survey, which allow us to examine consumption patterns among households directly affected by the policy. The combination of these municipal-level transaction data and household-level survey data allows us to comprehensively assess the minimum wage hike. It lets us capture both actual local spending behavior and detailed household-level responses while also cross-validating across independent data sources.

Our results indicate a significant increase of 4.5% in local aggregate consumption, primarily driven by notable rises in nonessential or discretionary spending—namely, on electronics (20.2%), leisure activities (11.7%), and restaurants and hotels (8.7%). Turning to the household-level data, we observe that the households affected by the reform increased their consumption by 4.6%. Although the consumption categories differ across the two datasets, the responses in the household-level data also appear to be concentrated in discretionary spending categories, including leisure, restaurants and hotels, and furniture. We find no evidence that the wage hike led to significant price increases, suggesting that the observed consumption effects were not driven by inflationary pressures.

To assess whether the observed consumption response was driven solely by income gains or partially offset by employment losses, we investigate whether the reform had any measurable impact on unemployment. Using municipal-level registered unemployment data and applying the same regional variation strategy as in our transaction data analysis, we find no significant increase in unemployment, suggesting that the estimated consumption response was driven entirely by the income gain.

Finally, to rationalize the observed shifts in consumption patterns, we use a simple theoretical framework with nonhomothetic preferences in which rising incomes allow households to reallocate spending toward discretionary goods.

The literature on minimum wages and employment is extensive, including contributions such as Card (1992); Card and Krueger (1994); Dube et al. (2010); Meer and West (2016); Clemens and Wither (2019); Cengiz et al. (2019); Dustmann et al. (2021); and Azar et al. (2024). There is also a growing body of work on the effects of minimum wages on income and income distribution (e.g., Dickens et al., 1999; Autor et al., 2016; Dube, 2019; Harasztosi and Lindner, 2019; Engbom and Moser, 2022). However, the literature

examining the relationship between minimum wages and consumption remains relatively recent and limited.

Studies of the US (Aaronson et al., 2012; Cooper et al., 2020; Alonso, 2022) and China (Dautović et al., 2024) have found that higher minimum wages can boost household spending, though the composition of consumption expenditures varies across countries. Aaronson et al. (2012), using household survey data covering the late 1980s to the mid-2000s, finds that minimum wage increases led to higher consumption of durable goods—particularly vehicles—among a small group of households with access to collateralized credit. Similarly, Cooper et al. (2020), using city-level data from 1999 to 2017 and exploiting variation in local minimum wages, find that minimum wage hikes led to increased spending on food, especially food away from home. Alonso (2022), using county-level grocery retail sales from 2006 to 2014, documents a positive effect of minimum wage increases on consumption of nondurables.

Relative to these studies, our contribution lies not only in analyzing an advanced economy with distinct institutional and social characteristics but also in exploiting sharper policy variation and more granular consumption data. In terms of results, we find substantial increases in spending on leisure and electronics, which were not emphasized in earlier studies. We observe increased spending in restaurants, consistent with Cooper et al.'s (2020) findings on food away from home, though we find no significant effects on food consumed at home, in contrast to Alonso (2022).

In contrast, our results differ markedly from those of Dautović et al. (2024), who uses a representative panel of urban Chinese households and finds that the income from minimum wage increases is spent mostly on health care and education—likely reflecting the ubiquity of intrafamily and intergenerational obligations in this context. While such obligations are also prevalent in Spain, such an effect may not be present there because of widespread access to publicly funded health care and education among low-income households.

A related literature has examined the effects of minimum wage hikes on prices. Studies using scanner data from the US, such as Leung (2021) and Renkin et al. (2022), find a positive relationship between minimum wage increases and grocery prices. Ashenfelter and Jurajda (2022) reach a similar conclusion using data from McDonald's restaurants. For Germany, Link (2024) finds that firms responded to minimum wage hikes by increasing prices more often, a response tied to their decision to preserve employment. While price effects are not the focus of our analysis, we find little difference between nominal and real consumption outcomes. This may reflect our consideration of a broader set of goods and services or could suggest that Spanish firms absorbed the cost increases through adjustments on other margins (Clemens, 2021). Price effects may also emerge with a lag,

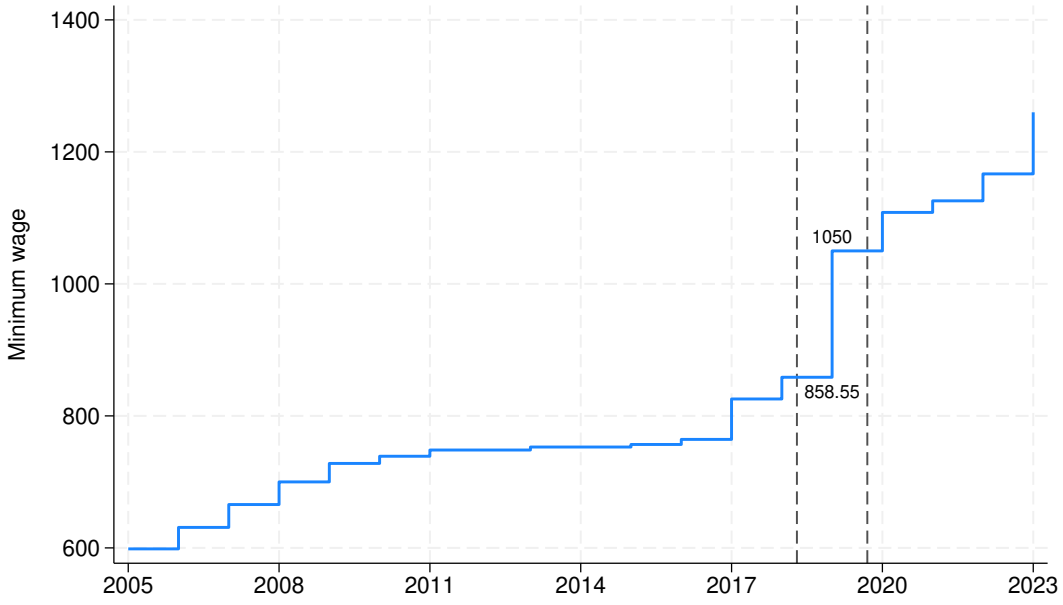
outside the horizon of our short-term analysis.

The remainder of the paper is structured as follows. Section 2 describes the institutional background of the 2019 minimum wage reform in Spain. Section 3 outlines our empirical strategy and presents results based on municipal-level consumption data. Section 4 complements these findings with an analysis of household-level data. In Section 5, we present a theoretical framework consistent with our findings. Section 6 concludes and discusses implications.

2 Institutional background

Spain's 2019 minimum wage (MW) increase represented a major policy shift, reflecting broader efforts to address real wage stagnation that had persisted since the Great Recession. In the past, MW adjustments in Spain were typically moderate and incremental, aimed primarily at keeping pace with inflation. These changes were usually negotiated through tripartite discussions involving employers' associations, trade unions, and the government. The 2019 hike, however, stemmed from a political agreement rather than a consensus-based negotiation. The proposal emerged in mid-2018, during efforts to secure parliamentary support for the 2019 budget of the newly formed center-left government. In this context, the left-wing coalition Unidos Podemos made its support conditional on several social policy measures, including a substantial MW increase. The agreement was announced on October 11, 2018, approved by the Council of Ministers in December, and took effect on January 1, 2019.

Figure 1: Minimum wage in Spain



Implemented in a context of relatively low inflation (1.2% in 2018 and 0.7% in 2019), the reform introduced an unprecedented 22.3% increase in the minimum wage—the largest single-year rise in four decades. The annual MW rose from €10,302.60 in 2018 to €12,600.00 in 2019, corresponding to a monthly increase from €858.55 to €1,050.00 (Figure 1).[1]

The MW is set at the national level and applies uniformly to all workers, regardless of age, sector, or region, with proportional adjustments for part-time and temporary contracts. As a result, the increase affected a broad cross-section of the labor market. According to the Wage Structure Survey from the Spanish National Institute of Statistics (INE), 14.10% of workers earned between 0 and 1 times the MW in 2018. By 2019, this share had increased to 18.18%, reflecting the extensive reach of the reform and its economy-wide impact (INE, 2024).

3 Regional approach

3.1 Empirical strategy

The nationwide scope of the policy and its simultaneous implementation across all regions rule out the use of a natural control group. Instead, our identification strategy exploits geographic variation in exposure to the MW hike, measured by the share of individuals affected by the increase within each geographic unit. Following the approach of Card (1992), we compare outcomes across areas with differing levels of exposure to the reform.[2]

Recent econometric studies have shown that standard difference-in-differences (DiD) designs, may yield biased estimates in settings with continuous treatment and no stayers (de Chaisemartin et al., 2024; Callaway et al., 2024). To address this concern, we adopt the aggregation approach proposed by Callaway et al. (2024), which demonstrates how partial aggregation across treatment intensities can yield interpretable causal parameters.

Specifically, we classify our geographical units—municipalities—into two groups based on their exposure to the MW increase: those above and those below the median exposure level. This binary classification ensures balanced group sizes and enables a comparison between municipalities with high and low exposure, under two standard assumptions: (i) municipalities with a larger share of individuals affected by the reform experience a

[1] In Spain, annual salaries typically involve 14 payments, as they include two extra payments in Summer and at Christmas. These two may be disbursed at those times or prorated across the year. Accordingly, media outlets often report the monthly minimum wage as rising from €735.90 in 2018 to €900.00 in 2019, based on the 14-payment structure.

[2] Many other numerous studies exploit geographic variation in exposure to minimum wage changes, including Stewart (2002), Caliendo et al. (2018), Dustmann et al. (2021), and Jiménez (2023). This strategy is also widely used to assess the effects of other policies and economic shocks—for example, Black et al. (2005), Autor et al. (2013), and Yagan (2019), among many others.

stronger treatment effect; and (ii) in the absence of the reform, both groups would have followed parallel trends in the outcome variable.[3]

Using monthly data from January 2018 to December 2019, we estimate a DiD model to assess the causal effect of the MW increase on consumption. The baseline specification is:

$$\ln(c_{mt}) = \alpha_m + \beta_1(\text{Treat}_m \times \text{Post}_t) + \phi_{rt} + \epsilon_{mt}, \tag{1}$$

where $\ln(c_{mt})$ is the natural logarithm of real per capita consumption in municipality m at time t; α_m denotes municipality fixed effects, which control for time-invariant heterogeneity across municipalities; and Treat_m is a binary indicator equal to 1 if municipality m has above-median exposure to the MW increase. Post_t equals 1 for months in the post-reform period (2019) and 0 for the pre-reform period (2018). The term ϕ_{rt} represents region-by-time fixed effects, which account for time-varying local economic conditions, and ϵ_{mt} is the error term, clustered at the municipality level. The coefficient β_1 captures the average treatment effect on the treated (ATT) of the MW increase on consumption.

A key identifying assumption of the DiD approach is that treated and untreated municipalities would have followed parallel trends in the absence of the reform. To validate this assumption, we estimate a dynamic DiD model that examines outcome differences quarter by quarter, using the pre-treatment period as the baseline.[4] The dynamic specification is:

$$\ln(c_{mt}) = \alpha_m + \sum_{q=-4}^{4} \beta_q \cdot (\text{Treat}_m \times \text{Quarter}_q) + \phi_{rt} + \epsilon_{mt}, \tag{2}$$

where β_q represents the relative impact of the reform in quarter q, with $q = 1$ corresponding to the reform's implementation quarter. Coefficients for $q < 0$ allow us to test for pre-existing trends, while those for $q > 0$ capture the dynamic effects of the reform over time.

A potential concern for our identification strategy is the presence of geographical spillover effects, which could violate the Stable Unit Treatment Value Assumption (SUTVA), particularly given our use of relatively small municipality-level data. To mitigate this concern, we implement several robustness checks. First, we exclude summer months (July and August) to account for potential confounding effects of tourism. Second, we omit the smallest municipalities, which may be more susceptible to spillover effects due to their size. Third, we test the robustness of our results using larger geographical units, such as large urban areas. Finally, we confirm that our results remain robust to alternative

[3]For empirical applications of this identification strategy, see, for example, Bartik et al. (2019); Caliendo and Wittbrodt (2022).

[4]In this analysis, we keep using monthly data but present coefficients by quarter for better graphical representation.

definitions of exposure to the reform and to the inclusion of baseline controls.

3.2 Data sources and descriptive statistics

Exposure to the 2019 MW reform. To estimate the proportion of individuals potentially affected by the reform, we rely on the 2018 Personal Income Tax Sample from the Spanish Instituto de Estudios Fiscales (IEF). This dataset, obtained directly from the Spanish Tax Agency, contains detailed information on 3,011,866 personal tax declarations—representing 14% of the total. In particular, it includes data on annual labor income and, crucially for our identification strategy, the declarant's municipality of residence.

We define the affected population in a given municipality as the share of working-age individuals whose annual earnings fell between €8,000 and €12,600—the threshold established by the 2019 minimum wage reform.[5]

A potential limitation is that low-earning workers are generally not required to file tax returns if their income is below €22,000 (or €12,643 with multiple payers).[6] However, many still file voluntarily to claim refunds or deductions—such as those related to maternity, large families, or housing benefits. These incentives likely increased the coverage of low-income earners in the dataset. In any case, this limitation does not threaten the validity of our identification strategy, provided that the propensity of low-earning workers to voluntarily file tax returns does not systematically vary across municipalities.

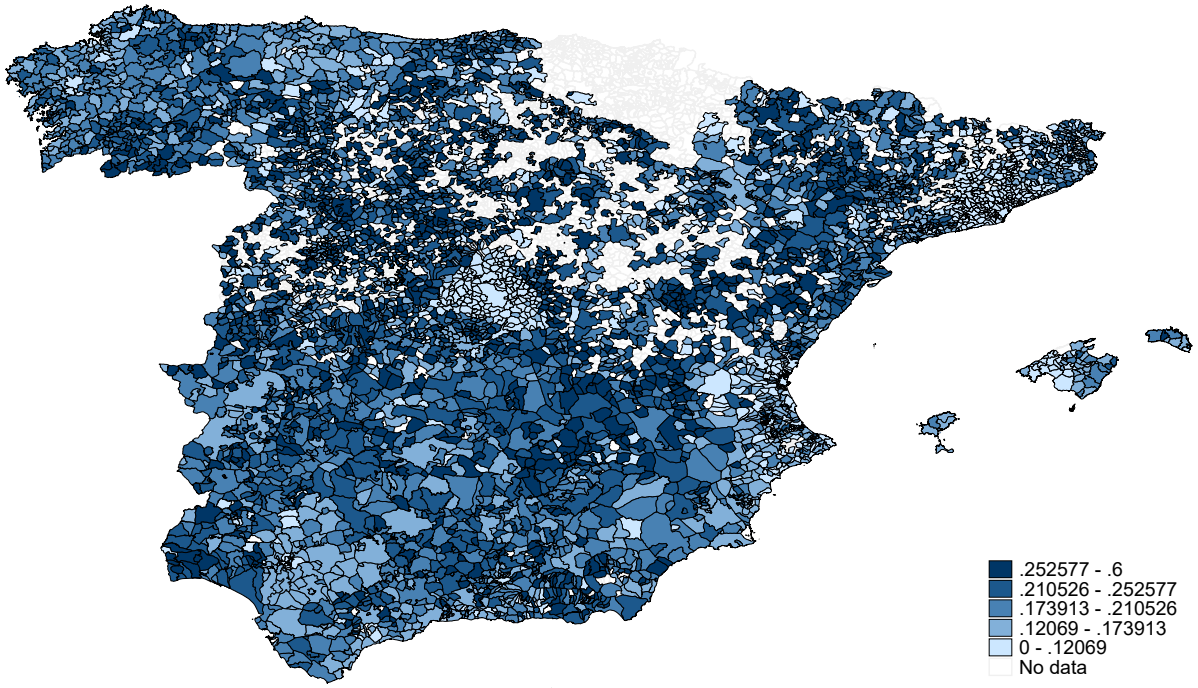
Figure 2 illustrates the degree of municipal exposure to the 2019 MW reform across Spain. The absence of data in the northern regions of Basque Country and Navarre reflects the fact that personal income taxes in these regions are collected by their respective regional tax agencies, rather than by the Spanish Tax Agency; as a result, our dataset does not include information for these areas. The empty zones in upper central Spain—often referred to as "Empty Spain"—are primarily located in Castilla y León and, to a lesser extent, in Castilla-La Mancha. These areas correspond to large numbers of sparsely populated municipalities. Of the 8,124 municipalities that existed in 2018, we are able to compute an exposure measure for 5,520.[7]

[5]The lower bound is set below the 2018 minimum wage to account for individuals who may have been affected by the reform despite not working full-time or for the full year. We later show that our results are robust to alternative definitions of exposure.

[6]These thresolds were also in place in 2018

[7]To reduce noise, we exclude municipalities with fewer than 10 tax declarations. In a robustness check, we further restrict the sample to municipalities with at least 100 tax declarations.

Figure 2: Exposure to the MW 2019 reform



Notes: Share of tax declarations from people aged $16-65$ with income in the range €8,000-€12,600. Data is available for 5,520 municipalities.

The exposure measure ranges from 0 to 60%, revealing substantial heterogeneity across municipalities. As expected, large municipalities—particularly those in the metropolitan areas of Madrid, Barcelona and Valencia—tend to exhibit lower exposure to the reform.[8]

Consumption Data. For the consumption data, we use confidential, anonymized transaction records at the municipality level, drawn from credit card and Point of Sale (POS) operations provided by Banc Sabadell, one of Spain's largest banks. Banc Sabadell is also the market leader in POS devices, with an estimated 20% share of the national market.[9]

Our dataset contains weekly transaction data disaggregated across 48 consumption categories. For the purposes of our analysis, we aggregate these data to a monthly frequency, covering the period from January 2018 to December 2019, with a primary focus on overall consumption. To explore heterogeneity in spending responses, we also group the 48 categories into 11 broader consumption groups.

The transaction data from POS devices identify the nationality of the credit card

[8] A few municipalities show no individuals affected by the reform. As long as these municipalities are relatively less exposed than those in the upper part of the distribution, this does not pose a problem for our empirical strategy. We later validate our exposure measure by examining its correlation with average municipal gross wages from an independent data source. Most of municipalities showing 0% exposure are indeed high-income, low-population municipalities (see Figure 4 below)

[9] According to the Asociación Española de Banca, Banc Sabadell is the fourth-largest Spanish bank by assets, following Santander, BBVA, and CaixaBank (AEA, 2023).

used and include purchases made with any credit or debit card, regardless of the issuing bank. To minimize the influence of tourist-related spending and more accurately assess the impact of the MW reform on domestic consumption, we exclude transactions made with foreign cards. In addition, we use customer-side transaction data from Banc Sabadell credit and debit cards. Our analysis, therefore, includes total expenditures recorded by Banc Sabadell POS devices using Spanish cards, as well as transactions made with Banc Sabadell-issued cards. To avoid double-counting, our dataset excludes transactions made with Banc Sabadell cards from the POS dataset when combining it with card-level data.[10] All consumption values are expressed in 2021 euros, adjusted using the Consumer Price Index (CPI) provided by the INE, disaggregated by province, month, and ECOICOP product classification.[11]

Table 1: Consumption composition

Components	% Total consumption 2018	% Total consumption 2019
Food and clothing	32.1	34.7
Home and car	6.1	5.9
Furnishing	14.0	14.2
Health and education	8.8	8.7
Transportation	10.9	9.6
Travel	4.3	3.7
Communication	0.6	0.5
Electronics	3.8	3.8
Leisure	5.0	4.8
Restaurants and hotels	13.4	13.2
Other	1.0	1.0
Avg. per capita consumption	38.2	41.0

Notes: Consumption composition using data from our econometric sample (1,999 municipalities). Food and clothingHome and car covers housing-related expenses and vehicle repairs. Furnishing includes expenditures on home improvement, furniture, houseware, and other retail goods. Health and education encompass expenses related to health and education. Transportation includes spending on petrol stations, parking, various modes of transportation, and tolls. Travel considers expenditures on plane tickets and travel-related lodging. Communication covers telecommunications expenses. Electronics covers spending on electronic devices and photography equipment. Leisure comprises expenditure on products related to sports, toys, lotteries, pets, and other recreational items. Restaurants and hotels include spending at bars, restaurants, hotels. Other incorporates expenses on insurance, donations, taxes, and ATMs.

Table 1 presents the composition of consumer spending across the 11 broad consumption groups using monthly per capita averages, as measured through observed transactions, for the years 2018 and 2019. A key feature of the data is the relative stability of spending patterns across the two years, with only minor fluctuations in category shares. The largest category is "Food and clothing," which are typically considered basic goods and consistently account for just over 30% of total expenditure. In contrast, "Furnishing" and "Restaurants and hotels" are more discretionary in nature, each representing

[10] Tourist spending may also involve domestic consumers; we describe how we address this issue in a later section.

[11] ECOICOP—the European Classification of Individual Consumption According to Purpose—is a standardized system for categorizing household expenditures. Although Banc Sabadell does not report consumption data using this classification, we manually align the expenditure categories with the closest ECOICOP groups or subgroups.

nearly 15% of total spending. "Transportation" and "Health and education" each make up slightly less than 10%. Average monthly per capita consumption, based on recorded transactions, increased from €38.2 in 2018 to €41 in 2019.

3.3 Econometric sample and validation of the exposure measure

Econometric Sample. Using tax declarations, we calculate the share of individuals affected by the 2019 MW reform across 5,520 municipalities. However, several constraints require us to reduce the number of municipalities included in our empirical analysis.

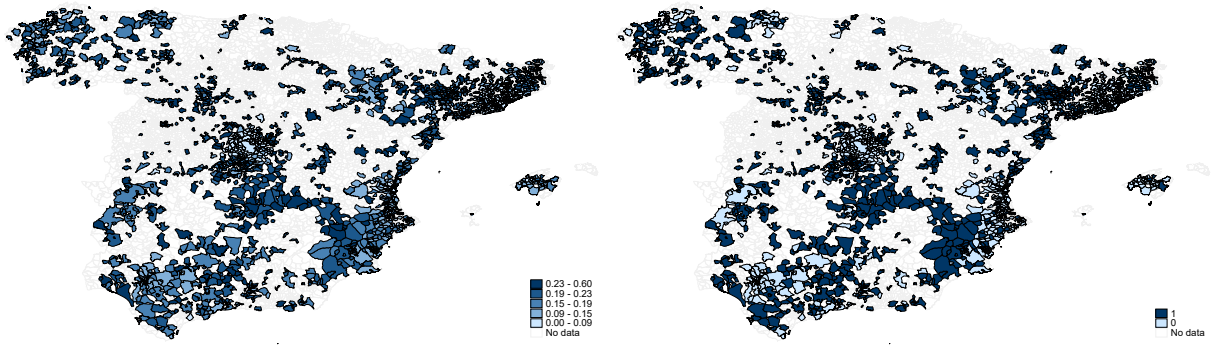
The first constraint arises from our consumption data. To ensure consistency, we limit our analysis to municipalities with a balanced panel of monthly consumption observations throughout the study period (January 2018 to December 2019), preventing composition effects from biasing our results. Additionally, the data provided by the BS is extracted directly by their technology department without pre-treatment or control for outliers. To reduce noise, we exclude municipalities with extremely low per capita consumption values—specifically, those in bottom 5% of the distribution—as well as the top 5% of municipalities with the largest consumption variations.

A second constraint relates to our identification strategy and concerns potential geographical spillovers. National tourism poses a risk, as touristic locations, often dominated by low-wage service-sector jobs, may appear in the upper range of our exposure distribution. If these areas attract national tourists, our estimates could be biased upward by capturing increased consumption from low-income workers temporarily relocating from control to treatment municipalities. To address this, we conservatively exclude potentially touristic municipalities, defined as those where average summer per capita consumption is at least 50% higher than the rest of the year.[12]

Figure 3.a displays the exposure measure for our econometric sample. After applying these selection criteria, the number of municipalities decreases from 5,520 to 1,999. Despite this reduction, our sample still covers 78% of the total population—approximately 36 million people—suggesting that the excluded municipalities are predominantly smaller. Figure 3.b presents the binary classification of municipalities into treatment and control groups.

[12]We later show that our results are robust to alternative thresholds and to the exclusion of the smallest municipalities, which are likely most susceptible to geographical spillovers.

Figure 3: Exposure to the MW 2019 reform: Econometric sample

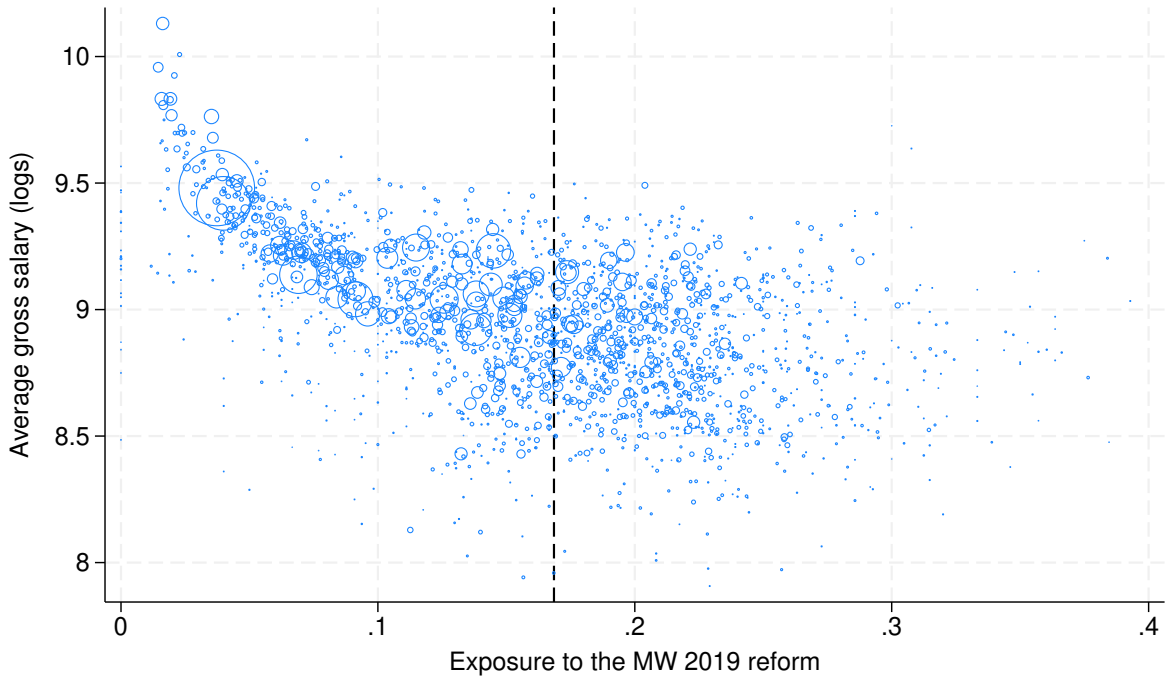


(a) Continuous (b) Dummy

Notes: Share of tax declarations from people aged 16 – 65 with income in the range €8,000-€12,600. The econometric sample includes 1,999 municipalities, covering 78% of the total population (approximately 36 million people).

Validity of the exposure to the 2019 MW reform. The validity of our treatment variable relies on income tax data, which entails two key limitations. First, tax declarations lack granularity in measuring detailed labor income (e.g., hours or months worked), as they are reported annually. Second, estimates for smaller municipalities may be prone to noise due to sparse observations, potentially introducing measurement error into our exposure measure.

Figure 4: Average salary and exposure to MW 2019 reform



Notes: The size of each circle is proportional to the population of the municipality.

To assess whether our exposure measure accurately captures the degree of exposure to the 2019 MW reform, we compare it with an alternative measure: the average municipality gross salary in 2018, obtained from the INE. We hypothesize that municipalities with lower exposure to the reform will tend to have higher average gross salaries. This expectation is grounded in the idea that areas with higher wages are less likely to employ low-wage workers and, therefore, are less affected by the MW increase. Figure 4 plots the average gross salary per municipality against our measure of exposure to the 2019 MW reform. The figure confirms the expected negative relationship between the two, providing visual validation of our exposure measure. This pattern strengthens our confidence in the treatment variable, particularly given that we rely on a binary distinction between control and treated units.

For completeness, Table 2 compares pre-reform characteristics between municipalities with low and high exposure to the MW reform. As expected, municipalities more exposed to the reform have a lower gross income per capita (€2,366.87 less, on average). They also exhibit higher unemployment rates and lower income inequality, suggesting a less dynamic labor market with a more compressed income distribution.

Table 2: Pre-reform characteristics (2018)

	Low exposure		High exposure		
	Mean	Median	Mean	Median	Difference
Unemployment rate	8.67	8.21	10.10	9.63	-1.42***
	(3.52)		(4.05)		
Population	26,074.25	4628	10,127.23	3528	15,947.01***
	(128,525.33)		(21,896.74)		
Share 16-65	65.29	65.90	64.92	65.54	0.37*
	(3.63)		(4.23)		
Share women	49.54	49.91	49.11	49.35	0.43***
	(2.05)		(2.03)		
Gross income pc	14,655.63	14,357.50	12,288.76	12,052.00	2,366.87***
	(3,703.03)		(2,410.57)		
Gini	30.08	29.80	28.96	28.90	1.12***
	(3.38)		(2.65)		
Municipalities	1000		999		

Notes: Low (high) exposure includes municipalities below (above) the median exposure to the MW 2019 reform.

A notable difference is observed in population size, with municipalities in the control group having a higher average population (15,947 residents). However, this gap narrows significantly when considering the median, suggesting that the difference is primarily driven by the presence of large urban centers within the control group.

To ensure the robustness of our findings, we conduct additional checks, demonstrating

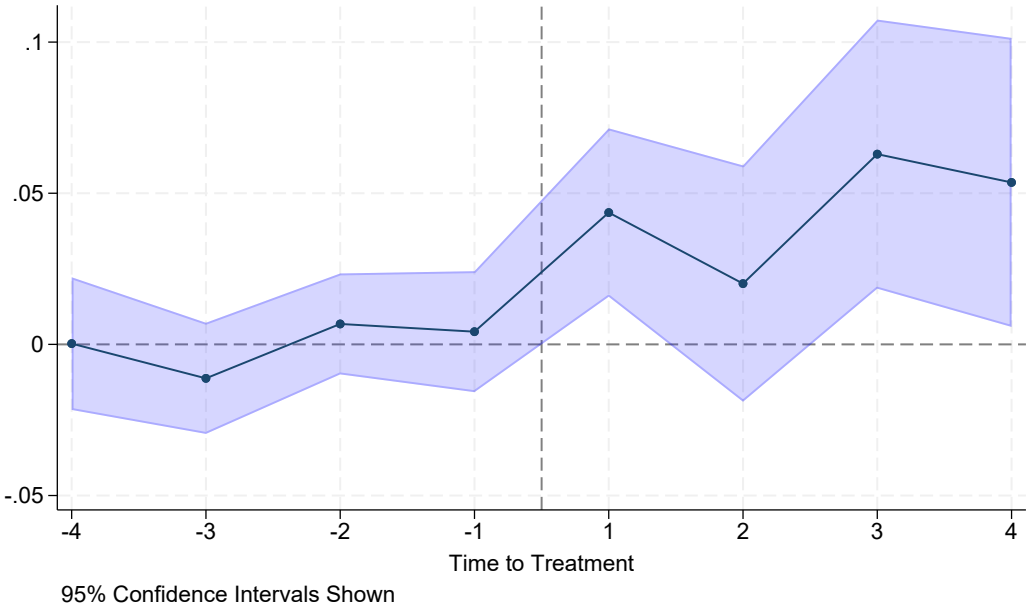
that our results remain consistent when accounting for baseline characteristics interacted with time fixed effects.[13] This approach accounts for potential time-varying heterogeneity across municipalities, helping ensure that underlying trends do not drive observed differences between treated and control municipalities.

3.4 The impact on consumption

3.4.1 Baseline results

Figure 5 presents the quarterly coefficients from the event study based on equation (2). The first key finding is the lack of statistically significant coefficients during the pre-intervention period. This suggests that consumption trends in high- and low-exposure municipalities did not diverge before the implementation of the minimum wage reform, providing support for the parallel trends assumption.

Figure 5: Event study: Consumption



Notes: Standard errors are clustered at the municipality level. The regression includes municipality and region-time fixed effects. We only consider municipalities with a balanced panel of 24 observations. 1,999 municipalities. 47,976 observations.

Following the reform, we observe a notable increase in consumption in treated municipalities. In the first quarter after the reform, consumption rises by nearly 5%, marking a significant and immediate response to the minimum wage increase. This effect persists throughout the year, though the magnitude of the coefficient for the second quarter is slightly smaller and less precisely estimated.

[13] Baseline controls include the 2018 values of the logarithm of population, the share of working-age population, and the share of women.

Table 3 presents DiD results based on equation (1). Column (1) displays the coefficient from our baseline regression, showing that, on average, municipalities more exposed to the 2019 MW reform increased their consumption by 4.5% in 2019 relative to the control group. The remaining columns explore the sensitivity of our results to alternative specifications.

Columns (2) and (3) examine the robustness of our results to the inclusion of controls and the use of a continuous treatment variable. Column (2) introduces a set of predetermined controls interacted with time-fixed effects, showing that the coefficient remains stable. Column (3) uses the original continuous measure of exposure, which also indicates a positive effect of the MW reform on consumption.[14]

Columns (4) and (5) test the robustness of our results using alternative exposure measures and sources of variation. In column (4), exposure is defined as the share of individuals with an annual income below €12,600, irrespective of age. While this measure introduces some noise by including groups such as pensioners who are not directly affected by the reform, it benefits from a larger sample of income tax declarations. The estimated impact is smaller, with a coefficient of 0.026, but remains highly significant. In column (5), we refine the treatment and control group classification by using the median exposure within each region, ensuring a similar number of treated and control units within regions. The coefficient is slightly smaller than in the baseline, likely reflecting comparisons between more similar municipalities, but remains highly significant.

Columns (6) and (7) explore the potential influence of tourism on our results. Column (6) excludes the peak tourist months of July and August, showing no significant change in the coefficient of interest. Column (7) relaxes the threshold for excluding municipalities with high seasonal consumption, increasing it from 50% to 100%. Again, we observe no significant changes in the coefficient, suggesting that our results are not driven by seasonal tourism effects.

Columns (8) to (10) address potential geographical spillover effects, particularly in smaller municipalities. Columns (8) and (9) restrict the sample to municipalities with more than 100 income tax declarations and those with populations exceeding 1,000 inhabitants, respectively. The coefficients, 5.3% and 4.3%, closely align with the baseline estimate. Column (10) uses urban areas as the geographical unit, reducing the sample to 73 units. Despite the smaller sample, the effect of the 2019 MW reform remains positive, with a coefficient of 3.6%, consistent with the baseline analysis.

[14] To obtain the average treatment effect on the treated and make this coefficient comparable to previous estimates, we multiply by the average treatment intensity (0.165). The implied ATT is 5.1%, closely aligning with the original estimates.

Table 3: Robustness: Consumption

	Baseline	Controls	Cont.	Alt. exp.	Reg. median
	(1)	(2)	(3)	(4)	(5)
Treat x Post 2019	0.045	0.046	0.307	0.026	0.022
	(0.007)***	(0.007)***	(0.058)***	(0.007)***	(0.006)***
Mean outcome	2.62	2.62	2.62	2.62	2.62
Observations	47976	47976	47976	47976	47976
Municipalities	1999	1999	1999	1999	1999

	No summer	Sum. ratio > 2	N> 100	Pop.$> 1,000$	Urban Areas
	(6)	(7)	(8)	(9)	(10)
Treat x Post 2019	0.043	0.043	0.053	0.043	0.036
	(0.008)***	(0.006)***	(0.007)***	(0.007)***	(0.009)***
Mean outcome	2.61	2.53	2.76	2.67	3.60
Observations	39980	62592	31920	38136	1752
Municipalities	1999	2608	1330	1589	73

Notes: Standard errors are clustered at the municipality level. The "Baseline" column represents the benchmark estimation from the main analysis. Column (2) includes baseline controls (i.e., the 2018 values of the logarithm of population, the share of working-age population, and the share of women) interacted with time fixed effects. Column (3) uses a continuous measure of exposure. The "Alt. exp." column defines exposure as the share of individuals with an annual income below €12,600. Column (5) separates municipalities into high- and low-exposure groups within each region. Column (6) omits data from July and August. Column (7) excludes municipalities where summer consumption is, on average, at least twice the consumption in other months. Columns (8) and (9) restrict the sample to municipalities with more than 100 income tax declarations and a population exceeding 1,000, respectively. Column (10) considers large urban areas as the geographical unit.

3.4.2 Consumption heterogeneities

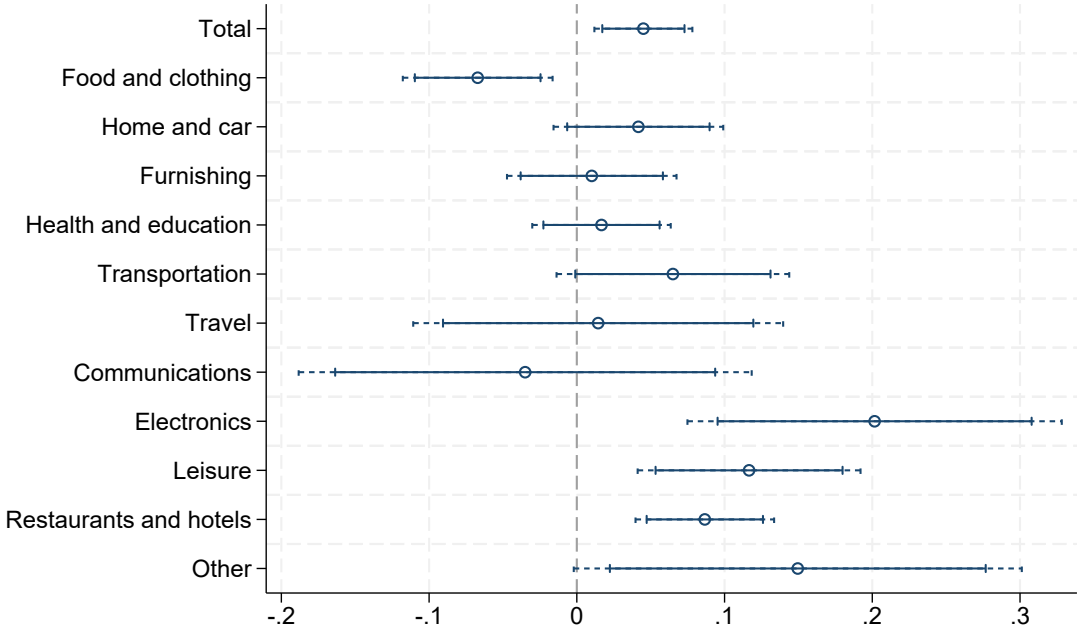
Having established the overall positive impact of the minimum wage reform on aggregate consumption, we now examine whether this effect varies across different spending categories. Understanding how households reallocate their expenditures in response to an income increase provides valuable insights into consumption patterns and the underlying channels through which the policy operates.

In this section, we analyze the impact of the 2019 MW reform across eleven broad consumption categories. However, the more we disaggregate our data, the greater the likelihood that some municipalities will have missing values for specific categories in a given month. To maintain the sample as comparable as possible to that used in our baseline analysis, we consider the average per capita consumption by municipality for each category over the years 2018 and 2019.[15]

Figure 6 presents the DiD results, revealing significant heterogeneity across consumption groups. The overall increase in consumption observed earlier is primarily driven by higher expenditures on electronics (20.2%), leisure (11.7%), and restaurants and hotels (8.7%). This suggests that the additional household income generated by the MW reform was directed toward improving living conditions and increasing participation in leisure activities outside the home.

[15]Table A.1 in the Appendix provides details on the number of municipalities included for each category.

Figure 6: DiD: Consumption components



Notes: 90% (solid line) and 95% (dashed line) confidence intervals. Standard errors clustered at the municipality level in parentheses. All the regressions include municipality and region-time fixed effects.

In contrast, we observe a notable decline in spending on food and clothing (6.7%). This reduction, coupled with the rise in expenditures on restaurants and hotels, suggests a potential substitution effect, where households may be shifting from home-prepared meals to dining out. We will explore this issue in greater detail in the analysis with household data.[16]

Finally, no significant effects are detected for the other categories. The stability in transportation and communications spending may reflect the lower income elasticity of these categories, as such expenses are often tied to professional or essential needs rather than discretionary choices. Similarly, the lack of response in health and education expenditures is unsurprising, given that Spain's public healthcare and education systems provide comprehensive, high-quality coverage, thereby insulating these categories from the direct effects of income fluctuations. Similarly, the absence of significant effects in housing-related categories, such as home and car, and furnishing, may be attributed to indivisibilities; these items often require substantial one-time expenditures, such as purchasing new furniture or undertaking renovation projects, making them less responsive to short-term income changes.

[16] Additionally, we observe a sizable but less precisely estimated increase in expenditures on other expenditure (15.0%).

3.4.3 The role of prices

From a theoretical perspective, firms may pass through part of the increase in the minimum wage to higher prices as a response to rising production costs or increased demand from workers earning higher wages.

In our benchmark analysis, we use real consumption measures by deflating nominal expenditure data with the province-, month-, and ECOICOP-specific CPI from INE to control for price effects. While this deflation method should account for substantial price variation induced by the minimum wage reform, it does not capture potential price differences across municipalities within the same province.

Since municipality-level price indices are unavailable, we address this limitation by re-examining the impact of the 2019 MW reform on consumption using nominal consumption figures. The rationale is that if price effects are substantial, we would expect a notable discrepancy between our original estimates—which control for province-ECOICOP product price variations over time—and the new results based on nominal consumption. If no significant discrepancy is observed, it is unlikely that price effects play a meaningful role in our baseline analysis. This is because price discrepancies are expected to be larger when comparing municipalities across different provinces than when examining variation within the same province.

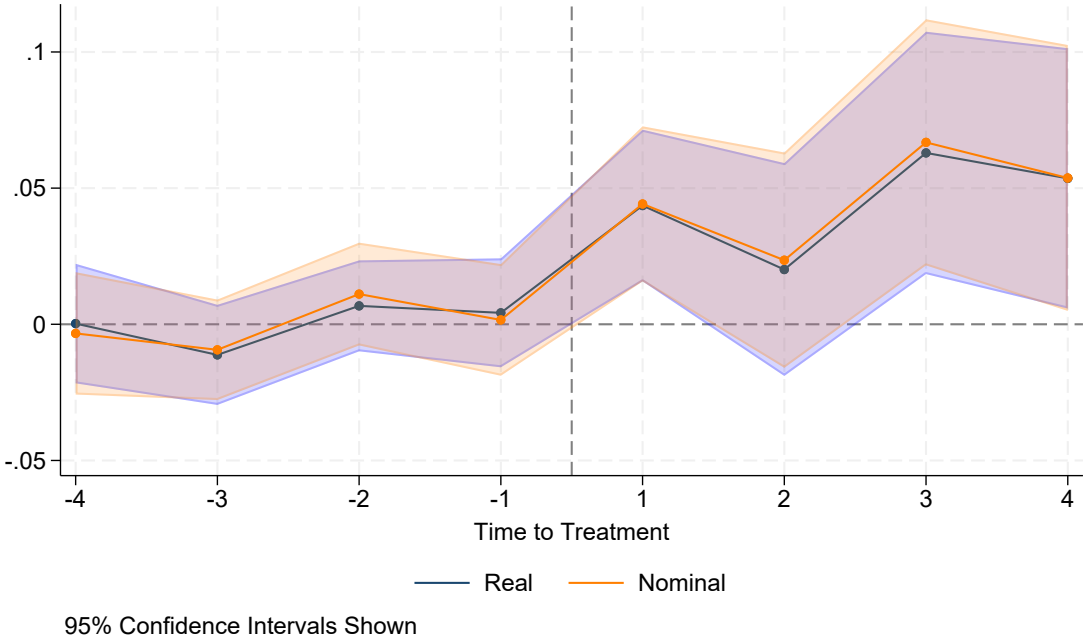
Figure 7 compares our original event-study results (based on real consumption) with those obtained using nominal consumption. The estimates remain stable across specifications, showing no significant time patterns or differences between real and nominal consumption trends. The absence of meaningful discrepancies between real and nominal consumption supports the conclusion that price adjustments at the municipality level within provinces are minimal.

Overall, these findings reinforce the robustness of our baseline analysis, confirming that the observed consumption effects are primarily driven by real changes in spending rather than price adjustments. This is consistent with theoretical expectations that minimum wage increases have limited inflationary effects, particularly at the local level.

3.5 The labor market

The labor market response to minimum wage increases has been widely studied, yet theoretical predictions remain ambiguous, depending on factors such as labor market competition, labor demand elasticity, and income-consumption elasticity. While our primary focus is on the impact of the MW on consumption, we include this section on labor market outcomes for completeness. Specifically, we analyze the effect of Spain's 2019 MW reform on unemployment using the same methodology as in our consumption analysis, providing

Figure 7: Event study: The role of prices



Notes: Standard errors are clustered at the municipality level. The regression includes municipality and region-time fixed effects. We only consider municipalities with a balanced panel of 24 observations. 1,999 municipalities. 47,976 observations.

a broader perspective on the reform's economic implications.

This section examines the unemployment rate, calculated monthly at the municipality level as the ratio of registered unemployed individuals to the working-age population.[17]

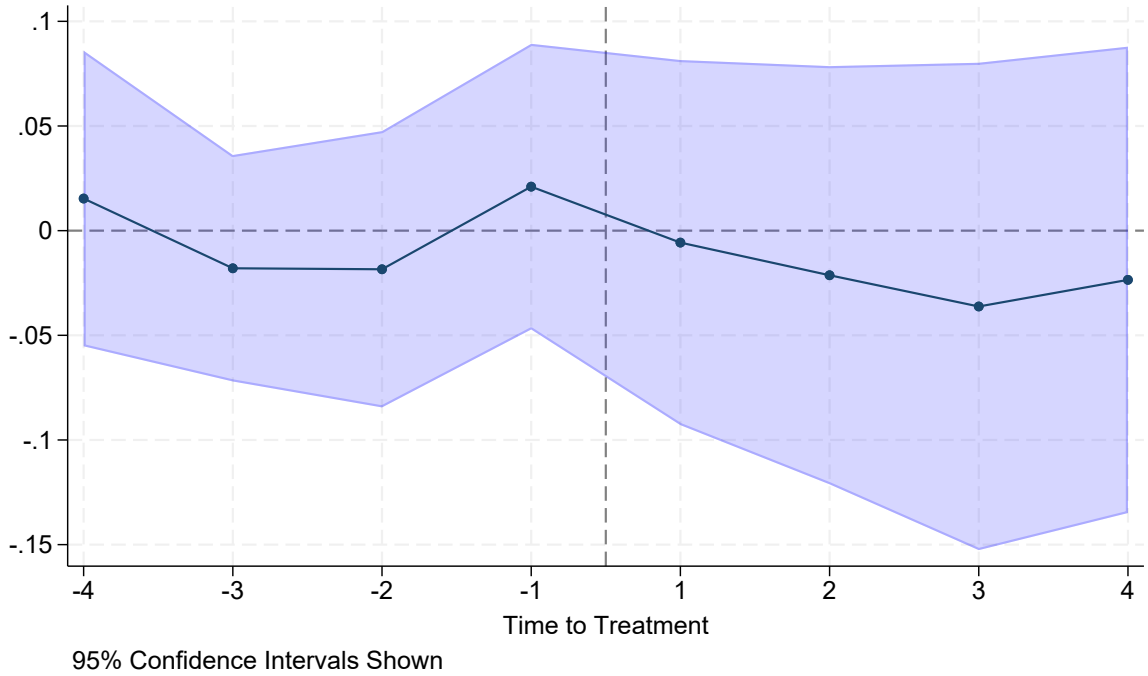
Figure 8 presents the results of our event study based on equation (2), using the unemployment rate (in percentage) as the dependent variable for our econometric sample of 1,999 municipalities. As in the consumption analysis, we find no significant pre-reform differences between treated and control municipalities. However, we also detect no meaningful post-reform effects. The unemployment rate in municipalities more exposed to the MW increase remained stable following the 22.3% wage hike.

Comparing Figures 5 and 8, we observe that while the MW increase had an immediate positive effect on consumption, it had no discernible impact on unemployment in the short term. This suggests that the 2019 MW reform did not result in job losses. Furthermore, the rise in consumption among low-wage workers may have spurred economic activity, potentially offsetting any negative labor market effects.[18]

[17]Monthly registered unemployment data comes from the Public Employment Service (SEPE), while working-age population estimates are sourced annually from the INE. Since labor force data is only available at the province level, we use the working-age population as the denominator. Adjusting for the labor force at the province level does not affect our results.

[18]Table A.2 in the Appendix confirms the robustness of our findings across alternative specifications. Tables A.3 and A.4 show no economically significant heterogeneities in unemployment by age or sector. Additionally, Figure A.1 and Table A.5 reveal that when using new contract registrations as an alternative

Figure 8: Event study: Unemployment rate



Notes: Standard errors are clustered at the municipality level. The regression includes municipality and region-time fixed effects. We only consider municipalities with a balanced panel of 24 observations. 1,999 municipalities. 47,976 observations.

4 Household data

4.1 Data and empirical strategy

As discussed in the previous section, our prior analysis may be subject to two key concerns: the validity of our measure of exposure to the 2019 reform and the quality of the consumption data, as well as potential geographical spillovers across municipalities that could violate the SUTVA. Despite the robustness of our earlier findings, we further assess the impact of the 2019 MW reform on consumption using household-level data and an alternative identification strategy.

Specifically, we leverage the Spanish Household Budget Survey (EPF), an extensive annual survey conducted by the INE to analyze household consumption patterns and provide detailed insights into living conditions.[19] Although the EPF includes a panel component, the dataset does not disclose which households are repeated or newly surveyed. Consequently, we treat it as a repeated cross-section. Empirically, we estimate the following specification:

labor market indicator, we similarly find no significant effect of the MW reform.

[19]We exclude individuals over age 60 from our sample since minimum wage policies primarily affect active labor force participants, and older individuals are more likely to be retired or have attenuated labor market attachment.

$$\ln(c_{ht}) = \beta_0 + \beta_1 \text{Treat}_h + \beta_2 \text{Post}_t + \beta_3 (\text{Treat}_h \times \text{Post}_t) + \gamma \mathbf{X}_{ht} + \epsilon_{ht}, \tag{3}$$

where $\ln(c)_{ht}$ is the natural logarithm of household h's real consumption expenditure in period t. Consumption is expressed in real terms using the 2-digit ECOICOP CPI and adjusted per capita using the OECD equivalence scale. Treat_h is a binary indicator equal to 1 if the household belongs to the treatment group and 0 otherwise. Post_t is a dummy variable set to 1 for the post-reform period (2019) and 0 otherwise. The interaction term $\text{Treat}_h \times \text{Post}_t$ captures the difference-in-differences effect of the reform.

Households are classified as affected by the reform if the primary earner earns less than €1,500 per month.[20] The control group consists of households earning between €2,000 and €3,000 per month.[21]

$\mathbf{X}_{ht}$ represents a vector of control variables, including geographical factors (region fixed effects and municipality size dummies in five categories), demographic characteristics (age in three groups, gender, and education level in four groups), household structure (number of children and partnership status of the primary earner), job characteristics (sector in four groups and occupation in six categories), and wealth proxies (homeownership and residential area dummies in seven groups).

4.2 Results

Table 4: Consumption: Household Budget Survey

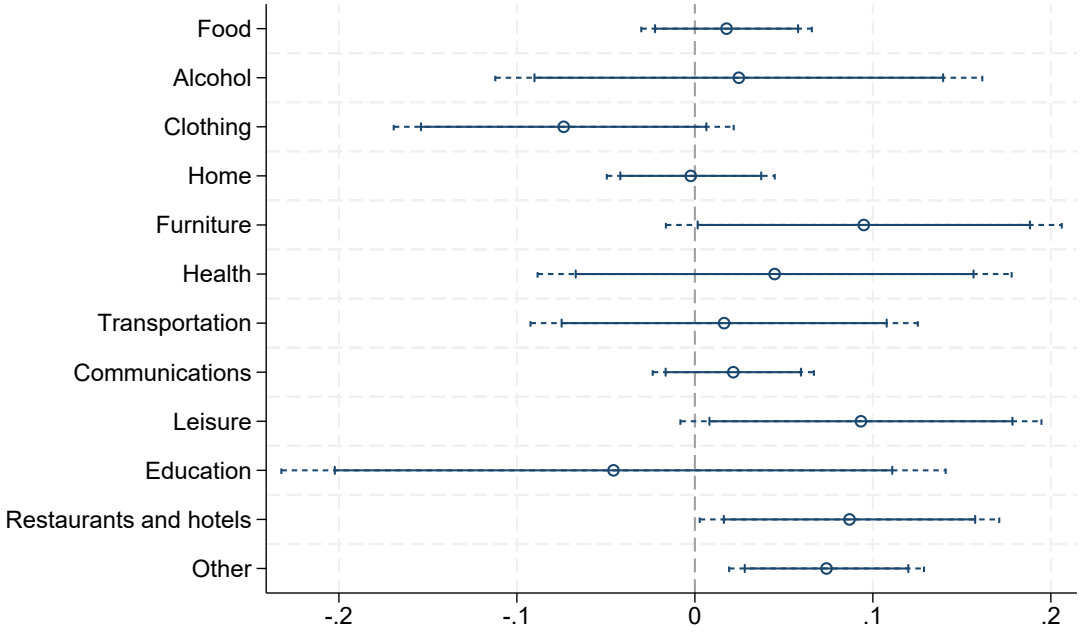
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main analysis (2018-2019)						
Treat x Post 2019	0.051	0.052	0.049	0.048	0.045	0.046
	(0.019)***	(0.019)***	(0.018)***	(0.018)***	(0.018)**	(0.018)***
Mean outcome	9.30	9.30	9.30	9.30	9.30	9.30
Observations	11210	11210	11210	11210	11210	11210
Panel B: Placebo (2017-2018)						
Treat x Post 2018	0.024	0.024	0.015	0.023	0.021	0.020
	(0.019)	(0.019)	(0.018)	(0.018)	(0.018)	(0.018)
Mean outcome	9.29	9.29	9.29	9.29	9.29	9.29
Observations	11746	11746	11746	11746	11746	11746
Geographical controls	No	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes	Yes
Household structure controls	No	No	No	Yes	Yes	Yes
Job characteristics controls	No	No	No	No	Yes	Yes
Wealth controls	No	No	No	No	No	Yes

Notes: Robust standard errors in parentheses.

[20] Since the EPF reports income in intervals, we include all households that could potentially be affected by the reform.

[21] We exclude households earning between €1,500 and €2,000 to mitigate potential spillover effects within the control group.

Figure 9: Consumption components: Household Budget Survey



Notes: 90% (solid line) and 95% (dashed line) confidence intervals. Robust standard errors in parentheses.

Panel A in Table 4 presents the results of interest, showing that individuals affected by the reform increased their consumption by 4.6% in 2019 compared to those in the control group, consistent with results obtained using geographical variation. Panel B shows a placebo test examining whether households affected by the 2019 reform exhibited different consumption behavior between 2017 and 2018, a period when the minimum wage increased modestly by 4%. No significant effect is found, supporting the parallel trends assumption.[22]

Finally, Figure 9 presents the disaggregated results by 2-digit ECOICOP consumption categories. Each coefficient corresponds to β_3 from equation (3), estimated using consumption in a specific ECOICOP group as the dependent variable.[23] While the categories are not fully comparable—since Banc Sabadell does not report consumption using the ECOICOP classification—we find that the increase in overall consumption is primarily driven by higher spending on leisure, and restaurants and hotels, echoing the results from our geographical analysis. Additionally, we observe a positive effect in the "Other" category, which, in this dataset, includes other discretionary goods such as jewelry and cosmetics.[24] Finally, this analysis also reveals a positive impact on furniture expenditures.

[22]The EPF provides weighting factors to produce nationally representative estimates. Although we do not weight our consumption measures in this analysis, Appendix Table A.6 confirms that the results are robust to weighting, showing even a larger consumption response.

[23]See Table A.7 in the Appendix for detailed regression results.

[24]"Other" corresponds to ECOICOP group 12.

Interestingly, the Sabadell consumption category "Food and clothing," which previously showed a negative effect, is disaggregated into three distinct groups in the EPF data: Food, Alcohol, and Clothing. While we find no significant effect on Food and Alcohol consumption, the Clothing category exhibits a negative coefficient, statistically significant at the 13% level. This result suggests that the earlier findings are not driven by a reduction in food consumption. Consequently, it appears that the 2019 MW reform did not lead to a substitution effect, such as households shifting from home-cooked meals to dining out.

5 Conceptual framework

To interpret our empirical findings, we require a model that incorporates non-homothetic preferences. Luckily, the literature offers a well-established framework for this: the Stone–Geary utility function, originally proposed by Geary (1950) and Stone (1954). This specification captures the idea that individuals must first meet a minimum level of consumption for certain basic goods before allocating additional income to discretionary items.[25] With Stone-Geary utility function, a representative minimum wage consumer has preferences over N goods:

$$U(C_1, \ldots, C_N) = \prod_{i=1}^{N} (C_i - \bar{C}_i)^{\alpha_i},$$

where C_i denotes consumption of good i, $\bar{C}_i$ is the subsistence level of good i, and $\alpha_i > 0$ reflects preference weights that satifiy the Cobb-Douglas property $\sum_{i=1}^{N} \alpha_i = 1$. If prices p_i, consumption levels C_i, and disposable income MW satisfy the budget constraint $\sum_{i=1}^{N} p_i C_i = \text{MW}$, one can easily derive the well-known demand function in this setup:

$$C_i = \bar{C}_i + \frac{\alpha_i}{p_i} \left(\text{MW} - \sum_{j=1}^{N} p_j \bar{C}_j \right),$$

This demand function shows that the consumption level of good i, C_i, consists of two parts. The first part, $\bar{C}_i$, is the minimum or subsistence amount of good i. The second part depends on the consumer's income available after covering the total subsistence costs of all goods: $\text{MW} - \sum_{j=1}^{N} p_j \bar{C}_j$. Thus, the first priority of minimum wage income is to cover the subsistence consumption level of each good—a threshold that is presumably highly heterogeneous across goods. The remaining income is then allocated across goods

[25] For simplicity, we use a static framework to illustrate how the composition of the consumption bundle adjusts in response to a minimum wage increase. We abstract from changes in total consumption, which are straightforward under the assumption of a permanent income increase—the scenario considered here.

according to the preference weight α_i, adjusted for the price p_i of good i.

For simplicity, we can group all goods into two categories: B, which includes all basic goods, and D, which includes only discretionary goods. Then, the demand for each category is:

$$C_B = \bar{C}_B + \frac{\alpha_B}{p_B}\left(\text{MW} - p_B\bar{C}_B - p_D\bar{C}_D\right)$$
$$C_D = \bar{C}_D + \frac{\alpha_D}{p_D}\left(\text{MW} - p_B\bar{C}_B - p_D\bar{C}_D\right)$$

Therefore, as disposable income MW increases, consumption beyond subsistence rises in proportion to $\frac{\alpha_i}{p_i}$. Our empirical results are consistent with parameter values in which basic goods have relatively high subsistence levels $\bar{C}_B$, reflecting essential needs that must be met first, and relatively low preference weights α_B for consumption beyond subsistence. In contrast, discretionary goods have low or negligible subsistence levels $\bar{C}_D$, but relatively high preference weights α_D. As a result, discretionary goods absorb a larger share of the additional income from the minimum wage, leading to a greater increase in their consumption ΔC^D, conditional on basic needs having been met prior to the minimum wage increase.

This framework helps explain why minimum wage increases may primarily stimulate spending on discretionary goods—such as leisure, electronics, and food away from home—while consumption of basic goods shows a smaller or muted response. This pattern may seem counterintuitive or differ from the experience in other countries, where minimum wages fail to cover basic needs or where individuals may lack any access to non-earned income sources.

6 Conclusions

This paper presents new evidence on how minimum wage increases affect household consumption, drawing on the substantial minimum wage hike implemented in Spain in 2019. Using a combination of high-frequency municipal-level transaction data from credit cards, point of sale records, and household survey data, we assess the extent to which higher wages translate into increased consumer spending.

Our findings with transaction data indicate that the reform led, on average, to a 4.5% aggregate rise in local consumption, with the effect being particularly pronounced in discretionary spending categories such as electronics, leisure, and restaurants and hotels. These results suggest that low-wage workers responded to higher incomes by reallocating their spending toward non-essential goods and services. These findings are consistent

with those obtained from household-level survey data. Additionaly, we find no evidence of adverse employment effects, which indicates that minimum wage increases boosted consumption without being offset by significant job losses. We also address potential concerns regarding price changes, showing that the observed increase in consumption was not driven by inflationary effects.

Our findings are consistent with the behavior of MW earners with Stone-Geary preferences, who direct additional income from a permanent MW increase toward discretionary goods because their subsistence needs are already met, as suggested by the muted consumption response of basic goods in our analysis.

Taken together, our findings suggest that substantial MW increases can enhance welfare not only by raising the overall level of consumption, but also by shifting its composition—away from strictly subsistence goods and toward a broader range of discretionary items that may offer greater utility to low-income workers. These consumption opportunities are typically out of reach under inadequately low minimum wages, but might become accessible when earnings rise just above the subsistence threshold.

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APPENDIX: Supplementary tables and figures

Table A.1: DiD: Consumption components

	Total	Food and clothing	Home and car	Furnishing	Health and education	Transportation
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post 2019	0.045	–0.067	0.042	0.010	0.017	0.065
	(0.017)***	(0.026)***	(0.029)	(0.029)	(0.024)	(0.040)
Mean outcome	2.62	0.93	–0.31	–0.33	–0.64	0.45
Observations	3998	3722	3258	3314	3450	3388
Municipalities	1999	1861	1629	1657	1725	1694

	Travel	Communications	Electronics	Leisure	Restaurants and hotels	Other
	(7)	(8)	(9)	(10)	(11)	(12)
Treat x Post 2019	0.014	–0.035	0.202	0.117	0.087	0.150
	(0.064)	(0.078)	(0.065)***	(0.038)***	(0.024)***	(0.077)*
Mean outcome	–1.48	–3.06	–1.21	–1.04	0.43	–3.18
Observations	1518	1256	2430	2718	3822	1212
Municipalities	759	628	1215	1359	1911	606

Notes: Standard errors clustered at the municipality level in parentheses. All regressions include municipality and region-time fixed effects. * p<0.1, ** p<0.05, *** p<0.01.

Table A.2: Robustness: Unemployment

	Baseline	Controls	Cont.	Alt. exp.	Reg. median
	(1)	(2)	(3)	(4)	(5)
Treat x Post 2019	–0.022	–0.032	–0.067	–0.136	–0.034
	(0.019)	(0.020)	(0.158)	(0.018)***	(0.014)**
Mean outcome	9.17	9.17	9.17	9.17	9.17
Observations	47976	47976	47976	47976	47976
Municipalities	1999	1999	1999	1999	1999

	No summer	Sum. ratio > 2	N> 100	Pop.$> 1,000$	Urban Areas
	(6)	(7)	(8)	(9)	(10)
Treat x Post 2019	–0.023	–0.021	0.029	0.023	–0.036
	(0.021)	(0.018)	(0.018)	(0.017)	(0.048)
Mean outcome	9.23	9.10	9.99	9.71	11.63
Observations	39980	62592	31920	38136	1752
Municipalities	1999	2608	1330	1589	73

Notes: *Notes*: Standard errors are clustered at the municipality level. The "Baseline" column represents the benchmark estimation from the main analysis. Column (2) includes baseline controls (i.e., the 2018 values of the logarithm of population, the share of working-age population, and the share of women) interacted with time fixed effects. Column (3) uses a continuous measure of exposure. The "Alt. exp." column defines exposure as the share of individuals with an annual income below €12,600. Column (5) separates municipalities into high- and low-exposure groups within each region. Column (6) omits data from July and August. Column (7) excludes municipalities where summer consumption is, on average, at least twice the consumption in other months. Columns (8) and (9) restrict the sample to municipalities with more than 100 income tax declarations and a population exceeding 1,000, respectively. Column (10) considers large urban areas as the geographical unit.

Table A.3: DiD: Unemployment

	Total	Male	Female	< 25	$25 - 45$	> 45
	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Controlling for municipality and region-time fixed effects						
Treat x Post 2019	–0.022	–0.008	–0.026	0.166	0.036	–0.044
	(0.019)	(0.022)	(0.028)	(0.067)**	(0.024)	(0.023)*
Panel B: Adding controls for municipality characteristics interacted with time fixed effects						
Treat x Post 2019	–0.032	–0.016	–0.038	0.163	0.027	–0.057
	(0.020)	(0.022)	(0.028)	(0.069)**	(0.024)	(0.023)**
Mean outcome	9.17	7.28	11.23	5.64	6.54	10.97
Observations	47,976	47,976	47,976	47,976	47,976	47,976
Municipalities	1999	1999	1999	1999	1999	1999

Notes: Standard errors clustered at the municipality level in parentheses. Panel A accounts for municipality and region-time fixed effects. Panel B further includes 2018 data on population (log), the percentage of women, and the percentage of individuals aged 16 to 65 interacted with time dummies.

Table A.4: DiD: Unemployment (cont.)

	Agriculture	Industry	Construction	Services	Other
	(1)	(2)	(3)	(4)	(5)
Panel A: Controlling for municipality and region-time fixed effects					
Treat x Post 2019	–0.018	–0.020	0.000	0.023	–0.007
	(0.007)***	(0.006)***	(0.005)	(0.015)	(0.004)*
Panel B: Adding controls for municipality characteristics interacted with time fixed effects					
Treat x Post 2019	–0.018	–0.022	0.001	0.017	–0.009
	(0.007)***	(0.006)***	(0.006)	(0.015)	(0.004)**
Mean outcome	0.65	1.04	0.81	6.02	0.65
Observations	47,976	47,976	47,976	47,976	47,976
Municipalities	1999	1999	1999	1999	1999

Notes: Standard errors clustered at the municipality level in parentheses. Panel A accounts for municipality and region-time fixed effects. Panel B further includes 2018 data on population (log), the percentage of women, and the percentage of individuals aged 16 to 65 interacted with time dummies.

Table A.5: DiD: New contracts

		By type			By sector			
		Open-ended						
	Total	Initial	Converted	Fixed-term	Agriculture	Industry	Construction	Services
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Panel A: Controlling for municipality and province-time fixed effects								
Treat x Post 2019	0.041	0.012	0.010	0.019	–0.002	0.011	0.017	0.016
	(0.107)	(0.015)	(0.008)	(0.103)	(0.071)	(0.023)	(0.008)**	(0.065)
Panel B: Adding controls for municipality characteristics interacted with time fixed effects								
Treat x Post 2019	0.033	0.010	0.010	0.014	–0.001	0.007	0.018	0.009
	(0.108)	(0.014)	(0.008)	(0.105)	(0.071)	(0.024)	(0.008)**	(0.067)
Mean outcome	5.60	0.29	0.22	5.09	1.23	1.00	0.31	3.07
Observations	47,976	47,976	47,976	47,976	47,976	47,976	47,976	47,976
Municipalities	1999	1999	1999	1999	1999	1999	1999	1999

Notes: Standard errors clustered at the municipality level in parentheses. Panel A accounts for municipality and region-time fixed effects. Panel B further includes 2018 data on population (log), the percentage of women, and the percentage of individuals aged 16 to 65 interacted with time dummies.

Table A.6: Consumption: Household Budget Survey (with population weights)

	(1)	(2)	(3)	(4)	(5)	(6)
Panel A: Main analysis (2018-2019)						
Treat x Post 2019	0.087	0.084	0.084	0.088	0.085	0.088
	(0.038)**	(0.027)***	(0.026)***	(0.024)***	(0.024)***	(0.023)***
Mean outcome	15.88	15.88	15.88	15.88	15.88	15.88
Observations	11210	11210	11210	11210	11210	11210
Panel B: Placebo (2017-2018)						
Treat x Post 2018	–0.014	–0.011	–0.001	0.017	0.016	0.015
	(0.037)	(0.026)	(0.026)	(0.023)	(0.023)	(0.023)
Mean outcome	15.86	15.86	15.86	15.86	15.86	15.86
Observations	11746	11746	11746	11746	11746	11746
Geographical controls	No	Yes	Yes	Yes	Yes	Yes
Demographic controls	No	No	Yes	Yes	Yes	Yes
Household structure controls	No	No	No	Yes	Yes	Yes
Job characteristics controls	No	No	No	No	Yes	Yes
Wealth controls	No	No	No	No	No	Yes

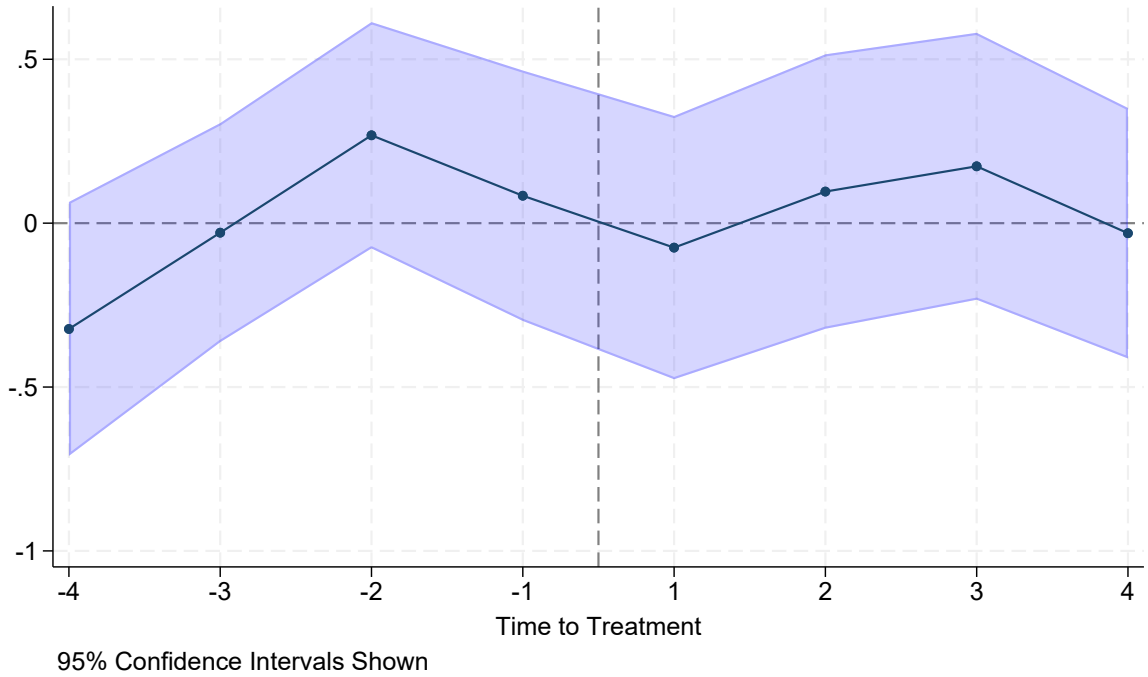
Notes: Robust standard errors in parentheses.

Table A.7: Consumption components: Household Budget Survey

	Food	Alcohol	Clothing	Home	Furniture	Health
	(1)	(2)	(3)	(4)	(5)	(6)
Treat x Post 2019	0.018	0.025	–0.074	–0.002	0.095	0.045
	(0.024)	(0.070)	(0.049)	(0.024)	(0.057)*	(0.068)
Mean outcome	7.47	5.08	6.30	7.19	5.68	5.52
Observations	11162	7991	9502	11147	10944	8306
	Transportation	Communications	Leisure	Education	Restaurants	Other
	(7)	(8)	(9)	(10)	(11)	(12)
Treat x Post 2019	0.016	0.022	0.093	–0.046	0.087	0.074
	(0.056)	(0.023)	(0.052)*	(0.095)	(0.043)**	(0.028)***
Mean outcome	7.01	6.02	6.20	5.45	6.91	6.69
Observations	10656	11023	9962	4561	10119	11171

Notes: Robust standard errors in parentheses.

Figure A.1: Event study: New contracts



Notes: Standard errors are clustered at the municipality level. The regression includes municipality and region-time fixed effects. We only consider municipalities with a balanced panel of 24 observations. 1,999 municipalities. 47,976 observations.