Demand Shocks and Firm Investment: Micro-evidence from fiscal retrenchment in Italy*

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Abstract

We study the effect of a persistent demand shock on corporate factor utilization. Our identification strategy leverages a legislative change designed to permanently reduce spending in certain targeted municipalities. This change generates an arguably-exogenous drop in the revenue of procurement firms, which differs depending on each firm's reliance for its revenue on procurement in the targeted municipalities. We find

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that firms responded to the demand shock by cutting capital rather than labor. We propose a theoretical mechanism based on the irreversibility of capital investment.

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1 Introduction

How do firms adjust production factors (i.e., capital and labor) after a persistent demand shock? At the theoretical level, if a shock is negative, most theories predict that both factors should be utilized less.[1] If there are adjustment costs, such as firing costs or capital irreversibility, theory suggests that factors should adjust more slowly[2] and, moreover, expectations about shock persistence are likely to play a role.[3] Turning to empirics, most of the macroeconomic evidence on how labor and capital adjust to shocks is aggregate and not causally identified. In fact, not much is known about the causal effect of demand variation on factor utilization due to: (1) the difficulty of identifying arguably exogenous demand shocks; and (2) the presence of general equilibrium effects, when the demand shocks happen to be large relative to the economy.

In this paper, we make progress on causal identification by studying an arguably exogenous sector-specific shock, firm-level data, and a small sector of the economy. The demand shock affected some firms more than others for arguably exogenous reasons; we leverage this cross-firm variation to get causal identification. Because we have firm-level data, we can subject our claim of exogeneity to rigorous challenges. Because the sector is small, the variation we rely on is unlikely to give rise to economy-wide confounders or general equilibrium effects. Finally, we leverage variation in capital rigidities and cross-firm variation in firing costs to assess whether capital rigidities or firing costs explain our findings.

The setting is as follows. A law was approved in 2008 that strengthened the enforcement of a pre-existing fiscal rule for Italian municipalities. The law only impacted municipalities with population greater than 5,000. We document that procurement in affected municipalities dropped sharply and persistently, relative to those not affected by the law. This drop represented a downward demand *shift* for procurement firms, and this shock was larger for firms with a greater *share* of revenues from affected municipalities.

[1] For example, in real business cycle models demand shocks are introduced as TFP shocks (see, e.g., Benhabib and Wen 2004, p. 515), and "in standard RBC models, a positive technology shock makes both labor and existing capital more productive" Rebelo (2005), p. 11. The same statement applies to network production models such as Acemoglu et al. (2012).

[2] For firing costs, see Sargent (1978) and Bentolilla and Bertola (1990). For capital adjustment costs see Bloom (2009), Shleifer and Vishny (2011).

[3] Bond and Van Reenen p. 4430.

We use this *shift-share* variation to get reduced form and instrumental variable estimates.

Our key finding is that firms respond to the persistent demand shock by cutting capital but not labor. To the best of our knowledge, these estimates are the most credibly identified in the existing literature because our shift-share variation is subjected to rigorous causality tests (pre-trends, placebo tests, etc.).

Our secondary finding concerns other margins along which firms adjust to the demand shock. We find that firms do not acquire alternative sources of revenue from non-impacted municipalities and are not more likely to declare bankruptcy. This feature provides some context for our findings, and suggests that they may generalize more readily to settings where firms do not make use of these margins.

We conclude by exploring possible mechanisms that could generate our main finding. The labor rigidity we document might be attributed to firing costs. However, the evidence from several distinct empirical approaches uniformly suggests that variation in firing costs does not correlate with the response to the shock. Instead, we propose an alternative theoretical mechanism based on the irreversibility of capital investment.

Related literature Nekarda and Ramey (2011) study the impact of variation in government purchases on the US economy. Their level of analysis is more aggregate than ours: an observation is a sector rather than a firm. Their instrument is year-on-year changes in aggregate government spending interacted with sectoral exposure to government demand. However, this instrument may be correlated with unobservables that affect firm performance across sectors and over time.[4] In contrast, our identification comes from "revenue-exposure to municipalities above vs. below the 5k threshold," where exogeneity of the shock to firm-level unobservables is much more plausible. A further point of difference is that Nekarda and Ramey's (2011) time variation comes from an aggregate shock (government spending), and so their estimates incorporate general equilibrium effects across sectors. By contrast, our micro-level analysis is based on a very sector-specific

[4]For example, if the US government purchases more weapons to fight a foreign war, it need *not* increase its purchases from all sectors in the same proportion (thus violating Nekarda and Ramey's identifying assumption, their eq. 8): furthermore, the US defense sector may concurrently benefit from an increase in "private" sales (perhaps from foreign governments fighting that same war), which is an unobservable shock to demand that is correlated with US government demand (another violation).

shock and thus isolates the direct effects of a demand shock. Finally, unlike Nekarda and Ramey (2011), we analyze the response to a demand shock we know to be *persistent*. These fundamental differences may help explain why our findings differ: whereas Nekarda and Ramey (2011) find that capital and labor co-move in response to a demand shock, we find no effect on labor.

Ferraz et al. (2015) study the effects of firm-level demand shocks on employment. Identification is achieved by comparing bidders that narrowly won and lost a Brazilian government procurement auction. While this source of identification is credible, their data lack information on investment, which is the key variable in our paper. In addition, their identification is based on a transitory shock (randomly losing one auction does not imply a reduction in the probability of winning in the future). Similarly, Guiso et al (2005) show that a large cross-section of Italian firms do not pass the burden of temporary productivity shocks through to their employee's wages. Collard-Wexler (2013) studies demand fluctuations in the ready-mix concrete industry. Compared to Collard-Wexler (2013), our analysis is less focused on market structure and more focused on firm-level financial outcomes.

Grembi et al. (2016) study the impact of an earlier (2001) *Patto di Stabilità* on municipal public finance.[5] Chiades and Mengotto (2013) study later versions of the *Patto di Stabilità* using the 5,000 population threshold. Bonfatti and Forni (2016) use the 5,000 population threshold. In these papers the dependent variables are municipal expenditures whereas in our paper they are firm-level outcomes.

Our paper also contributes to the literature on fiscal rules, because the variation in our paper comes from the tighter enforcement of a fiscal rule. This source of variation is policy-relevant because fiscal rules are increasingly common both at the national and at the sub-national level and are often weakly enforced.[6] Our results demonstrate empirically (for the first time to our knowledge) that enforcement is a key determinant of a fiscal rule's effectiveness and that it has an impact on a firm's investment.

[5]Notably, investment expenditures were exempted from the *Patto* up to 2004.

[6]In 2015, 92 countries had fiscal rules, up from seven in 1990. However, fiscal rules are often weakly enforced. Eyraud et al. (2018, p. 11) writes that "compliance with fiscal rules has been disappointing." Specifically regarding sub-national fiscal rules, Fredriksen (2013, p. 6) reports that "Monitoring and reporting of sub-central fiscal performance is poor and sanctions are not always credible or effective."

The rest of the paper is organized as follows. Section 2 describes the institutional background and the data. Section 3 shows that the legislative change generates a demand *shift* in some municipalities. Section 4 measures the *share* of firm revenue subject to the demand shift. Section 5 contains the estimates of the effect of the shift-share variation (firm-level demand shock) on capital and labor. Section 6 explores other adjustment margins. Section 7 explores several possible mechanisms for our findings and lands tentatively on capital adjustment costs. Section 8 concludes.

2 Institutional background and data sources

This section discusses the institutional background and the data sources we rely on. Appendix B contains detailed information on the data sources and the variables used.

2.1 The municipal procurement sector: institutional background

Our sector of interest is municipal public procurement. In Italy, municipal administrations provide roads, schools, and municipal buildings, and they are required to outsource this provision to private contractors via public tenders. The money for these public works is partly raised by the municipality itself, with the balance coming from grants (from the region, the central government, and the EU). Municipal procurement is a very small fraction of GDP (we estimate about 0.2%), and thus, a sectoral demand shock is unlikely to reverberate through the economy. This feature makes this sector a good laboratory to study the direct effects of the shock on firms, in isolation from hard-to-measure general equilibrium effects.

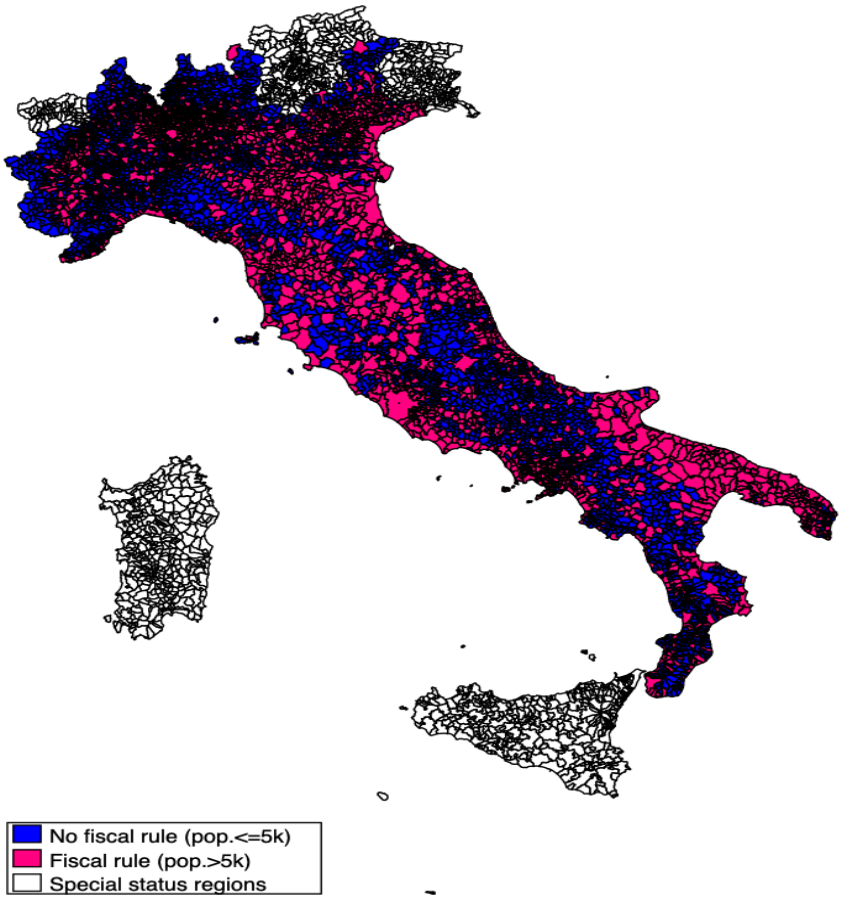
2.2 The legislative change

The *Patto di stabilità dei comuni* is a long-standing fiscal rule designed to check the growth in municipal spending, with a view to controlling municipal debt.[7] Only municipalities

[7]During our sample period, the *Patto*: required zero deficit and, moreover, a 20% ceiling on total spending growth (current plus capital, year-on-year); and it did not feature a "golden rule" exempting investments from its purview: see Guerra (2013, p. 954).

with population greater than 5,000 were subject to it during our sample period.

Figure 1: Italian municipalities with and without fiscal rule



Notes: Municipalities with population measured in 2001, the most recent census year before the reform. Special status regions, depicted in white, are exempt from the *Patto*; they are *Valle d'Aosta*, *Trentino Alto Adige*, *Friuli-Venezia Giulia*, *Sicilia* and *Sardegna*. Source: Authors' calculation on National Institute of Statistics data (ISTAT, 2012).

In August 2008, a law was unexpectedly passed that made enforcement of the *Patto* much stricter. For the first time, non-compliant municipalities suffered substantive cuts in government transfers, and restrictions to borrowing for investment; moreover, mayors and councilors in non-compliant municipalities received a 30% salary cut.[8] This new law aimed to permanently curb municipal spending. These penalties persisted throughout our sample period and beyond.[9] Following this law, municipalities with a population exceeding

[8]Law 133/2008 of August 6, 2008. Articles 77bis *comma* 20, and 61 *comma* 10.

[9]In 2015, for example, the mayor and councilors of Potenza were hit with the penalties: see Brancati

5,000 expected to see a drop in procurement due to the increased enforcement of the *Patto*. This is the "shift" component of the demand shock for firms.

Figure 1 shows the distribution of affected municipalities. In five "special status" regions, the *Patto* was optional, and two of them (Sicily and Sardinia) adopted it voluntarily. To guard against endogeneity, we drop all five regions from our main estimation sample. We then use the three non-adopting regions to perform placebo tests.

2.3 Municipal procurement data

Municipal budget data (Italian Ministry of Interior, 2012) do not allow us to precisely quantify municipal infrastructure spending. Therefore, we obtained proprietary procurement data from a private company that alerts procurement firms to upcoming tenders (Telemat, 2011). The procurement data includes all the municipal procurement contracts available in the proprietary data set, between 2004 and 2011.[10] We use this proprietary data to quantify yearly municipal procurement. Table 1, panel B reports the descriptive statistics of the municipal procurement market.[11] In addition, since the data contains information on the tender winners, we match the winners with our firm-level dataset (see next subsection). In this way, we can derive how much procurement business a given firm makes in any given municipality. This quantity will later be used to obtain a firm-level measure of the demand shock.

(2015).

[10]See Coviello and Mariniello (2014) and Decarolis and Giorgiantonio (2021) for a detailed description of the proprietary procurement data.

[11]On average, Italian municipalities put out to tender three contracts per year, for a total amount of 924,600 euros. The most frequently tendered contract type is road construction and maintenance (27%). The average tender attracts 29 bidders. There are 31,435 distinct winners in the database. Bids are expressed as a percentage rebate on a *valore stimato*: this is an estimate of the project's cost which is computed by a municipal engineer based on a government-issued price list. The average winning rebate is 17.01% of *valore stimato*, and the average *valore stimato* is 320,000 euros. On average, 54% of winners are incorporated in the tendering province.

2.4 Financial information on procurement firms

The AIDA database (AIDA, 2016) contains the yearly financial statements of all public and privately-owned Italian firms that are required to file a balance sheet. In addition, AIDA records the firms' sector (e.g., construction), where the firm is incorporated, and the year of incorporation. AIDA does not report whether a construction firm operates specifically in the public procurement sector. Since we care about firms that operate in public procurement, we restrict attention to the 3,482 AIDA firms that we can match to winners in the procurement-market database described in Section 2.3, and that won at least one infrastructure tender before 2008.[12] See Table 1, panel A for descriptive statistics.[13]

3 The shift: the legislative change

This section describes the "shift" component of our "shift-share" variation in firm-level demand. This shift is caused by the legislative change described in Section 2.2 and results in differential procurement cuts in municipalities with a population greater than 5,000, after 2007. We first quantify this differential decrease, and then show that this decrease is unanticipated.

[12] The match is performed using firm name and zip code of the municipality of incorporation. Many procurement-market winners are not found in AIDA, probably because they are partnerships and not corporations, and thus are not required to file a balance sheet. To evaluate the representativeness of our matched-firm sample, we compare matched and unmatched procurement winners based on observables: see Table D.1. The sample of matched winners shows some small but statistically significant differences from unmatched winners. These differences suggest that our matched-firms sample provides a good, but not perfect, representation of the entire sector.

[13] Before the demand shock takes effect, corporate revenues equal 3.184 million euros on average, only part of which originate from municipal procurement. Wages equal 405,300 euros. Fixed Assets equal 450,700 euros. About 1% of the firms in our matched sample declare bankruptcy every year.

Table 1: Descriptive statistics (pre-demand shock)

Stats	Mean	St.Dev.	p10	p50	p90	N
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: Firm data					
Revenues from Procurement (in 100,000)	4.542	11.25	0	0.355	12.43	10,311
Capital (in 1,000)	450.7	2,866	13.95	114.6	956.9	10,311
Labor (in 1,000)	405.3	1,454	39.76	199.9	802.7	10,311
Fisc.Rule.Exp.	19.11	25.44	0	9.202	53.92	10,311
Municipal exposure	23.41	26.90	1.684	12.94	65.82	10,311
Inc.in Fisc.Rule.Mun.	0.855	0.352	0	1	1	10,311
	Panel B: Municipal procurement data					
Total value of tenders (in 100,000)	9.246	61.49	0	1.592	18.47	25,384
N.Tenders	2.614	7.945	0	1	6	25,384
Avg. value of procurement (in 100,000)	3.204	4.237	0.635	2.085	6.614	15,566
Percent Roads	26.88	35.29	0	7.053	100	15,566
Number of bidders	29.33	25.93	5	22.89	61.50	7,941
Winning rebate (in %)	17.01	8.124	7.700	15.62	28.45	8,345
Winner from the same province	53.76	34.13	9.028	49.68	100	5,758

Notes: *Revenues from Procurement* is the value of procurement won by a firm in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros); Financial variables are deflated using KLEMS (2012) deflators. *Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities with fiscal rule and the firm's pre-demand shock revenues; *Municipal exposure* represents the ratio between the firm's value won in municipalities (with and without fiscal rule) and the firm's pre-2008 revenues; *Inc. in Fisc.Rule.Mun.* equals one for firms incorporated in municipality with fiscal rule. *Winning rebate* is the municipal-level average of the percentage rebate on the *Avg. value of procurement* (in Italian, *valore stimato*). The latter is an estimate of the project's cost which is computed by a municipal engineer based on a government-issued price list. Source: Statistics pre-demand shock for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011, for all Italian municipalities.

3.1 Measuring the decrease in procurement in large municipalities

To estimate the average impact of the legislative change on municipal infrastructure spending, we estimate the following econometric model:

$$y_{it} = \alpha + \delta Fisc.Rule_i * Post_t + \beta Fisc.Rule_i + \gamma Post_t + \mu X_{it} + \varepsilon_{it}, \quad (1)$$

where y_{it} represents infrastructure spending, $Fisc.Rule_i$ is an indicator variable for municipalities with a population above 5,000 (these are the municipalities targeted by the legislative change), and $Post_t$ indicates the years after 2007. Depending on the specification, X_{it} includes time-varying municipal population and its squared term, or the

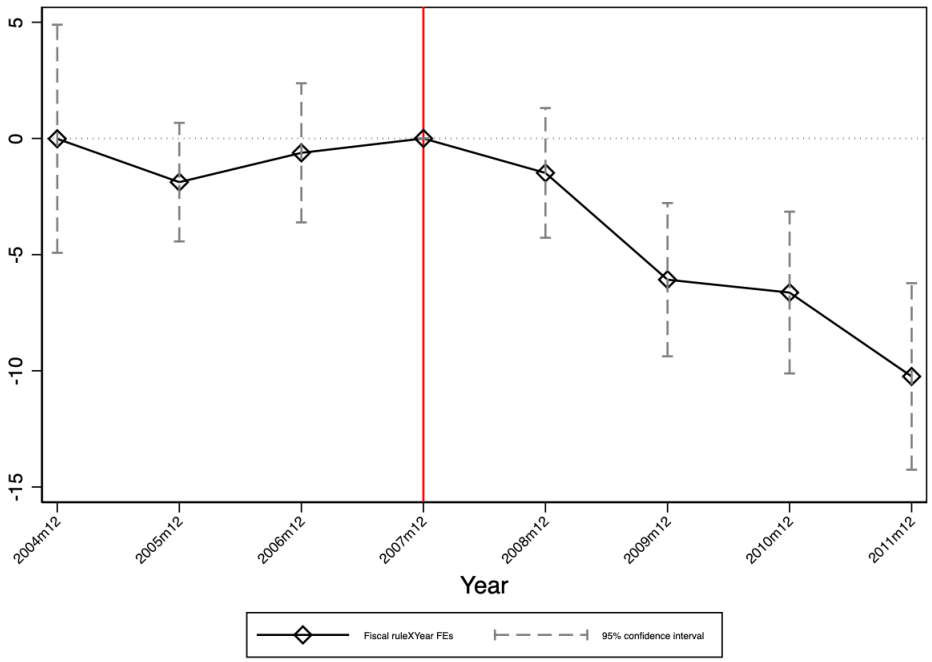
population in 2001 interacted with a linear time trend. We include municipal and time fixed effects. Standard errors are clustered at the municipal level. The coefficient δ is the main coefficient of interest: it represents the average impact of the legislative change.

The estimated coefficients of *Fisc.Rule*Post* (Table 2, columns 1 and 2) indicate that infrastructure spending decreased by roughly 23% in municipalities affected by the legislative changes, relative to the unaffected ones. This estimate is stable to including or excluding municipal- and time-fixed effects.

Figure 2 captures the dynamic effects of the legislative change on municipal procurement. This figure is obtained from a variant of equation (1) where the regressor $Fisc.Rule_i * Post_t$ is replaced by seven $Fisc.Rule_i * Year_t$ interaction terms, with 2007 being the omitted year. The estimated coefficients (see column 6 of Table 2) are depicted in Figure 2. As expected, after 2007 the value of procurement drops more sharply in municipalities that were impacted by the legislative change.[14]

[14]That the drop in spending can happen so quickly is consistent with the procurement law (*D.Lgs* 163/06), whereby three-year municipal procurement plans are revised on yearly basis.

Figure 2: Dynamic effects of the legislative change on municipal procurement



Notes: The figure reports leads and lags effects of the fiscal shock denoted by the interactions terms between the *Year2004-Year2011, excluding the year 2007* time dummies and the *Fisc.Rule* indicator on municipal procurement. Diamonds represent point estimates while dashed lines 95% confidence intervals. The vertical line indicates the last pre-treatment year. Source: Authors' calculation on procurement data, and municipal budget data from Italian Ministry of Interior for all municipalities between 2004-2011.

Table 2: Impact of the legislative change on infrastructure spending

Model	Difference-in-Difference Estimates				Parallel Trend	Leads&Lags	Placebo Regions
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Fisc.Rule$_i$*Post$_t$	-5.480***	-5.480***	-5.917***	-5.528***			1.701
	(1.199)	(1.199)	(1.993)	(1.205)			(1.438)
Post$_t$	-0.568***			-0.574***			
	(0.067)			(0.067)			
Fisc.Rule$_i$	21.308***			-2.575			
	(2.251)			(1.942)			
Population$_{i,t}$			2.548				
			(4.153)				
Population$^2_{i,t}$			-0.005***				
			(0.001)				
Pop.$_{i,2001}$*Year$_t$				0.001***			
				(0.0001)			
Pop.$^2_{i,2001}$*Year$_t$				-0.000***			
				(0.000)			
Fisc.Rule$_i$*Year$_t$					0.131		
					(0.772)		
Parallel trend test						0.200	
(p-value sig.)							
Fisc.Rule$_i$*Year2004$_t$						-0.015	
						(2.504)	
Fisc.Rule$_i$*Year2005$_t$						-1.882	
						(1.302)	
Fisc.Rule$_i$*Year2006$_t$						-0.620	
						(1.527)	
Fisc.Rule$_i$*Year2008$_t$						-1.484	
						(1.424)	
Fisc.Rule$_i$*Year2009$_t$						-6.078***	
						(1.683)	
Fisc.Rule$_i$*Year2010$_t$						-6.633***	
						(1.776)	
Fisc.Rule$_i$*Year2011$_t$						-10.243***	
						(2.048)	
Observations	50,768	50,768	50,768	50,768	25,384	50,768	4,440
Municipal FE	NO	YES	YES	NO	YES	YES	YES
Year FE	NO	YES	YES	NO	NO	YES	YES
Mean Y_treat-pre	24.09	24.09	24.09	24.09	24.09	24.09	16.44
Eff.Fisc.Rule on Treated (%)	-22.75	-22.75	-24.56	-22.95	0.542		10.34

Notes: The table reports difference-in-difference estimates of the effects of the legislative change on the annual total value of municipal tenders for infrastructure in all Italian municipalities. In each row, *Fisc.Rule* is an indicator variable for municipalities with population above the 5,000 population threshold and subject to the legislative change, and *Post* is an indicator for the years after 2007. Column 3 controls for time-varying municipal *Population* and its squared term in 1,000 inhabitants; column 4 for *Pop.*$_{2001}$ population in 1,000 inhabitants in 2001 interacted with the time trend (and its squared term). In column 4, the original estimated coefficient and standard error of the variable Pop.$^2_{i,2001}$*Year$_t$ are -1.22e-07 and 3.18e-08. Column 5 reports parametric tests for the parallel trend assumption, by checking the statistical significance of the interaction term pre-2008 *Fisc.Rule*Year*. These estimates are obtained in the pre-legislative change period and are used to test for the presence of linear pre-trends. In column 6, the regression includes leads and lags denoted by the interactions terms between the *Year2004-Year2011, excluding the year 2007* time dummies and the *Fisc.Rule* indicator. In this column *Parallel trend test (p-value sig.)* is the p-value of the joint test for all the leading terms equal zero. Column 7 restricts attention to "special status" regions that chose not to implement the national legislative change (Friuli, Trentino, and Val d'Aosta). Mean Y_treat-pre is the sample mean for treated municipalities pre-2008. *Eff.Fisc.Rule on Treated (%)* is the ratio between the estimated coefficient of *Fisc.Rule*Post* and Mean Y_treat-pre. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

3.2 No anticipation of the legislative change, and parallel trends

Visually, Figure 2 shows no evidence of anticipatory effects because the coefficients before 2008 are all small and not statistically different from zero. The parallel trend assumption is supported visually in Figure 2, and it is tested in Table 2, columns 5 and 6. In column 5, the assumption is tested parametrically in a model where the total value of municipal procurement is regressed on a linear time trend, a linear time trend interacted with Fisc.Rule, and municipal fixed effects, in the sample before 2008. The estimated coefficient of the interaction term is small and not statistically significant, suggesting that the parallel trend assumption is not rejected. In column 6, we perform a non-parametric version of this test by checking the statistical significance of the variable *Fisc.Rule* interacted with year dummies (2004-2006), in a model where the total value of municipal procurement is regressed on seven interaction terms $Fisc.Rule_i * Year_{2004}...Fisc.Rule_i * Year_{2011}$, year and municipal fixed effects, in the 2004-2011 sample. The lack of statistical significance of the pre-2007 individual coefficients, as well as the high p-value of the joint test, both indicate that the parallel trend assumption is not rejected.

To further support the notion that our estimates are not confounded by pre-existing trends that correlate with the impact of the 2008 law, we follow a procedure inspired by Kahn-Lang and Lang (2020) and Goldsmith-Pinkham et al. (2020). We look for X's that predict the impact of the 2008 law with a high R^2, and then toggle $X * time$ in and out of the specification, and check for the stability of the coefficients. An obvious candidate for X is "Population in the municipality pre-treatment," which by definition predicts the impact of the 2008 almost perfectly. Comparing the estimates across specifications that include (column 4 of Table 2) or exclude (columns 1 and 2 of Table 2) the regressor $X * time$ shows that the estimates are very stable. This is reassuring. Along the same lines, in Table C.3, we produce bias-adjusted estimates that formally account for the possible bias generated by the omission of this variable.[15] Bias-adjusted estimates are comparable to

[15]According to the Oster (2019) procedure, bias-adjusted estimates are computed using the following formula

$$\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}]\frac{R^2_{Max} - \widetilde{R}^2}{\widetilde{R}^2 - \mathring{R}^2}$$

with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being, respectively, the R^2s from the model with and without *Municipal pop. * Year*. The parameter δ is set to 1 under the assumption that firm specific time-varying unobservables are at least as important as the observables to

our main estimates, which is also reassuring.

Finally, one might be concerned that our estimates may be confounded by mean reversion: that is, large municipalities might have had a large increase in infrastructure spending pre-crisis and then, post-crisis, they mean-reverted to their initial level.[16] We believe that Figure 2, showing parallel trends for a full four years before treatment, does not point to mean-reversion. In addition, if "mean reversion for large municipalities" was a factor, then we would also expect to see it in placebo regions that were unaffected by the legislative change: however, there is no significant effect of "Post" in special status regions (Table 2, column 7), suggesting that mean reversion is probably not a factor.

3.3 Robustness

First, we check for sorting around the 5,000 population municipal threshold. Figure C.1 indicates no evidence of any statistically significant jump in the distribution of the municipal population around the 5,000 population threshold. Second, our results are comparable in magnitude and statistical significance if we consider a less parsimonious model and control for the log of municipal population and for binned categories of population (see Table O.1). Third, we re-estimate Table 2 using log and inverse hyperbolic sine transformations of the dependent variable (the latter helps guard against zeros in the data). The results are robust; see Table O.2.[17] Finally, in Table C.1, we restrict the sample to tighter windows around the treatment threshold. As the window tightens (1k-10k, 3k-7k, and 4k-6k) around the 5k threshold, the point estimates decrease somewhat but they do not vanish (attaining -17%, statistically significant at the 10% level, in the tightest, least-numerous window).[18] This stability around the threshold supports our interpretation that the procurement drop is due to the legislative change, though we note that it is the *average* treatment effect that provides the "shift" in demand.

estimate treatment effects.

[16] We thank an anonymous referee for this observation.

[17] In Table O.3, we repeat the analysis considering the years after 2008 as "Post" and find evidence that is comparable in size and magnitude to our main evidence.

[18] In Table O.4, we report descriptive statistics of infrastructure spending before the *Patto* around the treatment threshold.

4 The share: fraction of firm revenue exposed to the legislative change

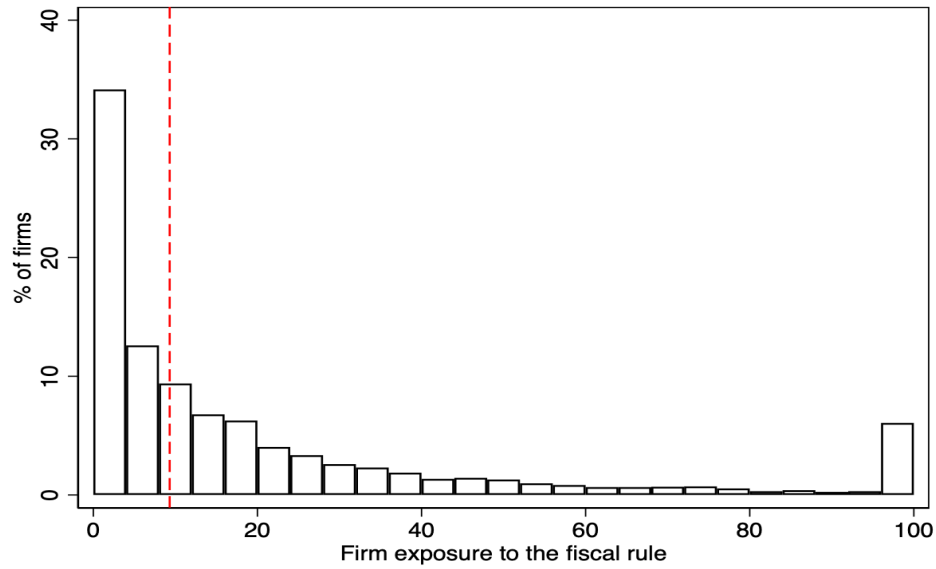
This section describes the "share" component of our "shift-share" variation in firm-level demand. This share, which we call "exposure" to the legislative change, is defined as the percentage of a firm's pre-2008 revenues that originated from procurement in the municipalities targeted by the 2008 legislative change. If revenue origination is somewhat persistent, more-exposed firms are expected to suffer a disproportionate demand reduction after 2008.

We define a firm's *exposure to the legislative change* $Fisc.Rule.Exp_i$ as the value of procurement won by firm i in municipalities with a population greater than 5,000, as a percentage of the firm's total revenues, before 2008. Figure 3 plots the frequency of firms by exposure to the legislative change. The median firm's exposure is just 9%; 23% of firm revenues comes from municipal procurement; and 85% of these companies are incorporated in treated municipalities (see the descriptive statistics in Table 1, panel A). Thus, corporations that operate in the municipal procurement sector are revenue diversified.

5 Main results: shift-share variation

In this section, we introduce our shift-share variable, which captures the degree to which a firm is impacted by the legislative change. We then document the absence of differential pre-trends (i.e., we show that the shift-share variable does not correlate with pre-trends in firm-level outcomes). We then analyze in reduced form how the shift-share variable impacts firm revenues, capital, and labor. Next, we argue that the share-shift variable is a valid instrument for a permanent demand shock. Finally, we use the shift-share variable to obtain IV estimates of the effect of a permanent demand shock on capital and labor.

Figure 3: Heterogeneity in firm exposure to the demand shock



Notes: A firm's exposure to the legislative change is defined as the value of procurement won by a firm in municipalities with population greater than 5,000, as a percentage of the firm's total revenues, before 2008. The sample median is 9% (vertical dashed line) and the standard deviation is 25.41%. Source: Authors' calculation on public works data and AIDA (2016) data.

5.1 The shift-share variable

The shift-share variable $Fisc.Rule.Exp_i * Post$ is created by interacting firm i's *exposure to the legislative change* variable defined in Section 4, with a dummy that equals 1 after the legislative change. We estimate the following model:

$$y_{it} = \alpha + \delta Fisc.Rule.Exp_i * Post_t + \beta Fisc.Rule.Exp_i + \gamma Post_t + \mu_c \cdot t + \varepsilon_{it}, \quad (2)$$

where y_{it} is the variable of interest (revenues, capital, or labor) in levels.[19] To control for municipal-level time trends, we introduce municipality-of-incorporation time trends $\mu_c \cdot t$.[20] In our preferred specification, we also add firm-specific and time fixed effects. Standard errors are clustered at firm level.[21]

[19] In Table O.2, we report estimates of the effects of the demand shock on revenues from procurement, capital and labor expressed in logs, and also applying the inverse hyperbolic sine transformation. We find comparable evidence when we consider these alternative specifications.

[20] Note that this variable is identified as the firms' municipality of incorporation need not be the same as the municipality where companies conduct their municipal business.

[21] In Table O.5, panel A, we repeat the analysis with clustering at the municipality-of-incorporation level and find evidence that is comparable in magnitude and statistical significance to our baseline estimates.

5.2 Reduced form estimates: effect of the legislative change on revenues, capital, and labor

As expected, procurement revenue drops more sharply for more-exposed firms after 2007: the estimated coefficient δ from equation (2) is negative in all three specifications: that is, without firm- and year-fixed effects, with them, and in our preferred specification, which further features municipality-of-incorporation time trends (see Table 3, columns 1-3). A one-standard-deviation increase in $Fisc.Rule.Exp_i$, when multiplied by the coefficient in Table 3, column 3, yields $25.44 * (-0.098) = -2.5$, corresponding to a drop of 250,000 euros in annual value of procurement won, or 72% of the average value of municipal infrastructure procurement won. The year-by-year impact of our shift-share variable is reported in column 4 and plotted in Figure 4 panel 1 (top-left): as expected, exposure to municipal procurement is more harmful to procurement revenue after 2008.[22]

Capital decreases more sharply for more-exposed firms after 2007: the estimated coefficient δ is negative across specifications (Table 3, columns 6-9). Based on the estimate from column 8, one standard deviation in $Fisc.Rule.Exp_i$ decreases capital by 16%. This figure is obtained by multiplying $25.44 * (-3.792) = -96.5$, corresponding to a drop of 96.5 thousand euros, or 16% of average physical assets. Column 10 reports the year-by-year impact of exposure, and the estimates are plotted in Figure 4, panel 2 (top-right): greater exposure leads to greater de-capitalization after 2007.

Labor follows a different pattern from revenues and capital: it does *not* decrease more

In panel B, we repeat the analysis controlling for regional demand shocks by adding a time-varying control for the fraction of cities exposed to *Patto* in the region of incorporation of the company (and its interaction term with the variable $Post_t$) and find evidence that is comparable in magnitude and statistical significance to our baseline estimates.

[22] To address concerns about measurement error in our $Fisc.Rule.Exp_i$ variable, we replicate our analysis using different definitions of firm-level exposure. First, we change the denominator to "total revenues from procurement" rather than "total revenues." The point estimates are very similar, but statistical significance is lost because the sample shrinks considerably (total revenues from procurement are zero for many firms in at least one year before the fiscal rule); results are available on request. Second, in Table O.6 in the online appendix, we compute $Fisc.Rule.Exp_i$ based on shorter windows: two years (columns 1-5) and one year (columns 6-10) before 2008. The estimated effects are comparable to the effect obtained in our baseline estimates. We thank an anonymous referee for suggesting these robustness checks.

sharply for more-exposed firms after 2007. Across all specifications, the estimated coefficient δ is not significantly different from zero (Table 3, columns 11-14). Column 15 reports the year-by-year impact of exposure, and the estimates are plotted in Figure 4, panel 3 (bottom-left).

5.3 Testing the identifying assumptions: no anticipation, and parallel trends

To visually gauge anticipation effects, Figure 4 plots the estimated coefficients and confidence intervals of the dynamic effects of the *Patto*. These effects are obtained by replacing the regressor $Fisc.Rule.Exp_i * Post_t$ in equation (2) with seven $Fisc.Rule.Exp_i * Year_t$ interaction terms, with 2007 being the omitted year. Visual inspection of the confidence intervals depicted in Figure 4 should alleviate any concern regarding anticipation effects. A joint test of the coefficients for each of the outcomes (revenues from procurement, capital, and labor) formally confirms this (see Table 3).

In Table 3, column 4, we check whether more- and less-exposed firms share the same trend in revenue from procurement, capital, and labor.[23] We test this parametrically by regressing each outcome before 2008 on a linear time trend interacted with the variable $Fisc.Rule.Exp_i$. The hypothesis of no-pretrends is not rejected for any of the variables (see Table 3, columns 4, 9, and 14). We also look for pre-trends non-parametrically by checking the statistical significance of the variable $Fisc.Rule.Exp_i$ interacted with the year dummies (2004-2006). The lack of statistical significance of all but one of the individual coefficients, as well as the high p-value of the joint test (see Table 3, columns 5, 10, and 15), indicate that the parallel trend assumption is not rejected.

As in Section 3.2, we also look for regressors that predict treatment: in this case, exposure to the fiscal rule. The predictors with the highest R^2 are municipal fixed effects: regressing *Fisc.Rule.Exp.* on municipal FE returns an R^2 of 0.26 (Table D.3, column 6). We believe there can be many reasons why firms located in municipality A may be more likely than firms located in municipality B to do business with a treated municipality, and we do not seek to explain these reasons here. Rather, we note that our $R^2 = 0.26$ fares well

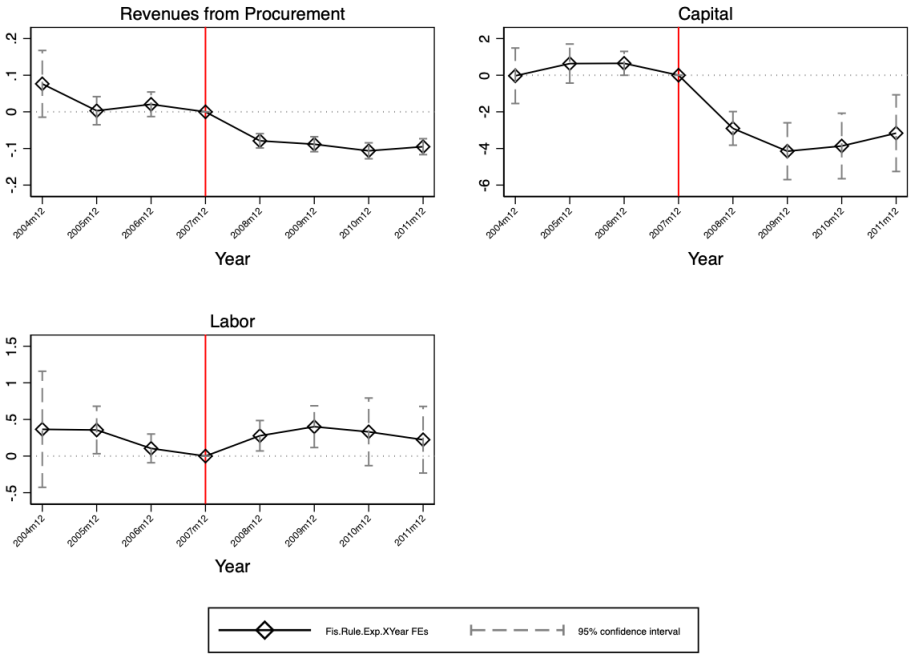
[23]The standard test for the common-trend assumption needs to be adapted to our setting because our treatment variable (exposure) is continuous and not binary.

within the context of Bartik instruments.[24] Comparing the estimates across specifications that include (columns 3, 8, 13 of Table 3) or exclude (columns 2, 7, 12 of Table 3) the regressor $X * time$, we see that the latter estimates are either very similar, when the dependent variables are "Revenues from procurement" or "Labor," or actually *increase* in magnitude when the dependent variable is "Capital." Because we see no attenuation when we fail to control for $X * time$, we believe our estimates, which control for $X * time$, are actually conservative.

We conclude by following Oster's (2019) procedure. Table D.4, row 2-4, displays estimates with no city-year FE (column 1), with city-year FE (column 4), and bias-adjusted estimates (column 7). Reassuringly, most of our estimates of β_s are stable across specifications. This stability suggests that firm-specific time-varying unobservable factors do not represent a major source of bias for our results.

[24]For comparison, Goldsmith-Pinkham et al. (2020, p. 2611) deem an R^2 of 0.46 "quite high," and the set of R^2's $\{0.15, 0.095, 0.21, 0.24, 0.02, 0.29\}$ collectively "fair" (p. 2617, referencing their Table 5).

Figure 4: Effect of firm exposure on revenues from procurement, capital and labor



Notes: *Revenues from Procurement* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS (2012) deflators. *Fisc.Rule.Exp*YearFE* is the estimated coefficient of the interaction term *Fiscal Rule Exposure* and the year dummies (*Year2004-Year2011*) in a model where outcomes are regressed on: firm and year dummies; the *Fiscal Rule Exposure* variable; the interaction terms; and municipality-of-incorporation time trends (see Columns 5, 10 and 15 of Table 3). Dashed lines are the associated 95% confidence intervals. A firm's exposure to the legislative change *(Fisc.Rule.Exp.)* is defined as the value of procurement won by a firm in municipalities with population greater than 5,000, as a percentage of the firm's total revenues, before 2008. Vertical lines indicate the last pre-treatment year. Source: Authors' calculation on public works data and AIDA (2016) data.

Table 3: Reduced form estimates: effect of exposure on revenues from procurement, capital and labor

De.Var.	Rev.Proc	Rev.Proc	Rev.Proc	Rev.Proc	Rev.Proc	Capital	Capital	Capital	Capital	Capital	Labor	Labor	Labor	Labor	Labor
Model	OLS	FE	FE	Parallel Trend	Leads&Lags	OLS	FE	FE	Parallel Trend	Leads&Lags	OLS	FE	FE	Parallel Trend	Leads&Lags
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Fisc.Rule.Exp*Post	-0.107***	-0.102***	-0.098***			-4.343***	-5.304***	-3.792***			0.139	-0.094	0.215		
	(0.007)	(0.007)	(0.008)			(0.573)	(0.683)	(0.826)			(0.213)	(0.207)	(0.169)		
Fisc.Rule.Exp	0.115***					-6.630***					-5.361***				
	(0.007)					(1.267)					(0.633)				
Post	0.035					399.455***					13.411				
	(0.137)					(34.975)					(14.411)				
Fisc.Rule*Year				0.006					-0.118					0.000	
				(0.014)					(0.161)					(0.098)	
Parallel trend test					0.314					0.128					0.138
(p-value joint sig.)															
Fisc.Rule.Exp*Year2004					0.076					-0.031					0.366
					(0.046)					(0.773)					(0.404)
Fisc.Rule.Exp*Year2005					0.003					0.636					0.357**
					(0.020)					(0.544)					(0.165)
Fisc.Rule.Exp*Year2006					0.021					0.650*					0.105
					(0.017)					(0.332)					(0.100)
Fisc.Rule.Exp*Year2008					-0.079***					-2.905***					0.278***
					(0.010)					(0.469)					(0.106)
Fisc.Rule.Exp*Year2009					-0.088***					-4.151***					0.401***
					(0.010)					(0.792)					(0.145)
Fisc.Rule.Exp*Year2010					-0.106***					-3.864***					0.331
					(0.011)					(0.913)					(0.235)
Fisc.Rule.Exp*Year2011					-0.095***					-3.164***					0.223
					(0.011)					(1.068)					(0.232)
Observations	22,855	22,798	22,798	10,025	22,798	22,855	22,798	22,798	10,025	22,798	22,855	22,798	22,798	10,025	22,798
Company FE	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
Year FE	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES	NO	YES	YES	YES	YES
CityFE*Trend	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES	NO	NO	YES	YES	YES
Mean Y	3.444	3.444	3.444	3.444	3.439	618.2	618.2	618.2	618.2	619.4	411.2	411.2	411.2	411.2	411.9
St.Dev.Fisc.Rule	25.44	25.44	25.44	25.44		25.44	25.44	25.44	25.44		25.44	25.44	25.44	25.44	
Eff.Fisc.Rule.Exp (%)	-78.96	-75.06	-72.20	-72.20		-17.87	-21.82	-15.61	-15.61		0.862	-0.584	1.329	1.329	

Notes: The table reports estimates of the effects of exposure to the legislative change on firms revenues from procurement, capital accumulation and labor: *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS (2012) deflators. *Fisc.Rule.Exp* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. *Post* is an indicator for the years after 2007. *Year2004-Year2011* are time dummies interacted with *Fisc.Rule.Exp*. When denoted with *Yes* estimates include Company, Year and municipality-of-incorporation time trends *(CityFE*Trend)*. In Columns 4, 9, 14 *Fisc.Rule.Exp*Year* is the interaction term between *Fisc.Rule.Exp* and a linear trend and it is used to test for parametric trends in the pre-2008 sample. In Columns 5, 10, 15 *Parallel trend test (p-value joint sig.)* is the p-value of the joint test for all the leading terms equal zero, and it is used to test for non-parametric pre-2008 trends. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule and Mean Y. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

5.4 The shift-share variable as an instrument for persistent demand variation

In this section we argue that the shift-share variable $Fisc.Rule.Exp_i * Post_t$ is a valid instrument for persistent demand variation.

Strength of the first stage The first stage is strong (Table 3, column 3), which qualifies our instrument as relevant.

Validity of the exclusion restriction and multiple treatment concerns The exclusion restriction is satisfied as a matter of logic: the statutory effect of the 2008 law was merely to curb municipal spending. Therefore, we can say that the legislative change operated exclusively through the procurement spending channel. All other indirect effects are caused by reduced municipal procurement spending.

Now, we argue that there is no multiple treatment problem: the effect of the shift-share instrument is not confounded by unobserved correlates of the legislative change. The argument here is necessarily somewhat ad-hoc because there is no principled way of ruling out all conceivable confounders. However, we believe that the argument is compelling in its totality.

1. *Drop in municipal procurement unaffected by post-2007 municipal financial distress*

   Table C.2, column 1 shows that the size of the drop in municipal procurement is stable in magnitude and statistical significance if we control for central government transfers to municipalities and for municipal tax revenues. This procedure is crude, however, because it controls for variables that may be endogenous to the treatment: therefore, in column 2 we control for the pre-treatment value of transfers and tax revenues interacted with the dummy "Post;" and in column 3 we further allow transfers/taxes to trend differentially for the treatment vs control group in the pre-periods.[25] The coefficients remain stable in both specifications. This gives us confidence that the 2008 law, and not some other economic hardship, is responsible for the differential drop in procurement.

[25] We thank an anonymous referee for suggesting this procedure.

2. *Absence of correlation between instrument and pre-2008 municipal procurement*

   We argued in Section 5.3 that municipal procurement trends pre-2008 did not correlate with whether or not a municipality would be impacted by the shift component of our instrument.

3. *Absence of correlation between instrument and pre-2008 firm-level outcomes*

   We argued in Section 5.3 that more- and less-exposed firms share the same trend in revenues from procurement, in capital, and in labor. Because the shift-share variable does not correlate with pre-trends, it is plausible that the variation provided by the shift-share instrument is "as good as randomly assigned." This claim is further supported by the fact that the estimated changes in revenue from procurement, capital, and labor, are robust to the inclusion of firm fixed effects and municipality-of-incorporation time trends (Table 3 columns 1 vs. 2, 6 vs. 7, 11 vs 12).

4. Placebo tests

   If our instrument was correlated with unobserved confounders, we would expect these confounders to shift procurement in placebo municipalities that were not impacted by the legislative change.[26] We perform three placebo tests on municipal procurement. The first test is on municipalities located in the special status regions that did not adopt the *Patto*. In this sample, procurement should *not* increase differentially post-2007 if the municipality's population exceeds 5,000. Reassuringly, the estimated effect is small and not statistically different from zero (Table 2, column 7). The second test restricts our main (i.e., non-special status) sample to the pre-2008 period, creates a grid of placebo population thresholds (from 1,000 to 10,000),[27] and runs a diff-in-diff interacting these placebo thresholds with "Post-T," where T are placebo years. Since no municipalities were impacted pre-2008, we expect no effect. Figure 5, panel A reports the p-values of the interaction terms and shows no statistical significance. The third and final municipal placebo test is displayed in Panel B. We drop from our main sample all the municipalities with a population above 5,000. For the remaining municipalities, which are not impacted, we generate 10 population

[26] We thank a referee for suggesting these placebo tests.

[27] Municipalities below 10,000 inhabitants represent approximately 90% of the Italian municipalities: see Figure C.2.

placebo thresholds, and interact them with "Post-2007." Reassuringly, we find no effect. These placebo tests show that the estimated revenue drop is driven precisely by the 5,000 population threshold after 2007, and that the estimated effect is not present in "special status" municipalities.

We also perform a firm-level placebo test to guard against firm-level confounders. In Table D.5, the sample is firms located in special status regions that did not adopt the *Patto*, and we use a "placebo shift-share instrument" that is the fraction of those firms' revenues originating from municipalities in their own region with a population greater than 5,000 interacted with the dummy "Post-2007." Reassuringly, we find no effect of the placebo instrument.

Taken together, the placebo tests are strong evidence that the estimated effect is due to the legislative change, and not to some other municipal-level or firm-level confounder.

Interpretation: source of permanent demand variation We conclude that the variable $Fisc.Rule.Exp_i * Post_t$ is a valid instrument for the revenues from procurement; that is, for the observed "demand shock" experienced by procurement firms. Knowing that our instrument is a (permanent) change in the law allows us to interpret the IV estimates as the firm's response to *permanent* (as opposed to temporary) demand shock. We will return to this point in the next section.

5.5 Instrumental variables estimates

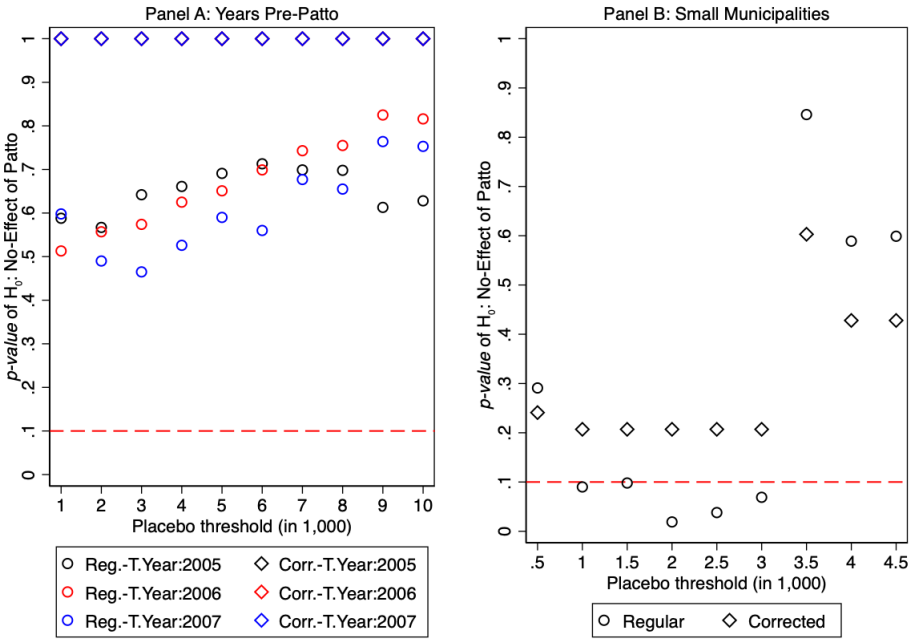
In this section, we use the variable $Fisc.Rule.Exp_i * Post_t$ as an instrument for demand. The two-stage model is given by equation (2), together with:

$$x_{it} = \beta_1 + \beta_2 y_{it} + \mu_c \cdot t + \omega_{it}, \tag{3}$$

where x_{it} is either capital or labor, and y_{it} is procurement revenue for firm i in year t. The rest of the variables are the same as in equation (2).

Table 4 reports the estimation results. Labor is statistically unaffected by demand variation, both in our OLS and IV estimates. In contrast, capital decreases by 38,799-52,190 euros for every 100,000 euros of permanent decrease in demand. The (inverse)

Figure 5: Placebo tests



Notes: The vertical axis represents the *p-value* of the interaction term "Pop. threshold * Post" in a diff-in-diff regression where the dependent variable is procurement spending. Diamonds are Benjamin et al. (2006) *p-values* that correct for multiple hypothesis testing. Panel A retains all the municipalities in our sample, but the years post-2007 are dropped because they were "treated." Within this sample, we create a grid of ten placebo populations thresholds (horizontal axis) and the diff-in-diff specification interacts each threshold with "Post-T" where $T \in \{2005, 2006, 2007\}$ is one of three placebo years (hence every point on the horizontal axis has three p-values: in Panel A there are in fact three diamonds for every threshold but they overlap perfectly at 1). In Panel B we drop the municipalities with more than 5,000 residents because they were "treated," and then generate 10 placebo population thresholds (horizontal axis). The "Post" years in Panel B are those after 2007. None of diamonds is below the 10% threshold. For completeness, we also report standard *p-values* as circles: those, too, do not show a pattern of statistical significance.

output elasticity of capital is approximately 2.[28]

Table 4 indicates that the decrease in demand due to the *Patto* causes a decrease in capital, and it has no impact on labor. In Section 7, we will show that this is precisely the pattern to be expected in a model with capital irreversibility, when firms are hit by a *permanent* demand shock. It is important for the interpretation, then, that our IV estimates are based exclusively on permanent variation because they reflect the portion of demand variation due to a permanent change in the law.[29]

[28]The (inverse) output elasticity of capital is $\frac{\delta k}{\delta y} \cdot \frac{y}{k}$. Capital decreases, so $\frac{\delta k}{\delta y} \approx 0.39$-$0.52$. The average output and capital in the sample equal 3M and 620k respectively, so $y/k \approx 5$. Therefore, the (inverse) output elasticity of capital equals $5.03 * 0.39 \approx 2$.

[29]Further evidence that the demand shock is permanent is obtained by inspecting the post-2007 coeffi-

Interestingly, if the shock had been temporary, the theory could not have rationalized the pattern in Table 4 (refer to Proposition 1, part 2). In light of this theoretical result, it is intriguing that the patterns in the OLS estimates (no statistically significant effect on both capital and labor) are different than in the IV. This difference is what is expected if the OLS picked up mostly temporary demand variation, and the IV picked up exclusively persistent variation in demand.

Table 4: Instrumental variable estimates

Dep.Var.	Capital	Capital	Capital	Capital	Labor	Labor	Labor	Labor
Model	OLS	IV	OLS	IV	OLS	IV	OLS	IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Rev.Proc	-0.778	52.190***	-1.210	38.799***	-2.226	0.929	-2.704	-2.198
	(2.503)	(7.617)	(2.750)	(8.958)	(1.958)	(2.038)	(2.181)	(1.740)
First-Stage F-Stat		552.4		323		552.4		323
Observations	22,798	22,798	22,798	22,798	22,798	22,798	22,798	22,798
Mean Y	619.4	619.4	619.4	619.4	411.9	411.9	411.9	411.9
Mean Rev.Proc	4.542				4.542			
Company	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
CityFE*trend	No	No	Yes	Yes	No	No	Yes	Yes

Notes: The table reports estimates of the effects of revenues from procurement (*Rev.Proc.*) on firms capital accumulation (*Capital*) and labor (*Labor*): *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS (2012) deflators. All the columns report FE estimates with firm and year fixed effects. When denoted with *Yes* estimates are obtained including municipality-of-incorporation time trends *(CityFE*trend)*. Odd (Even) columns report OLS (IV) estimates (using $Fisc.Rule.Exp_i * Post_t$ as an instrument for *Rev.Proc.*). *Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, and *Post* indicates the years after 2007. *First-Stage F-Stat* is the first stage statistics for the relevance of the instrument. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

cients in Figure 2. All of the coefficients are negative, decreasing, and statistically significant, indicating that the legislative change has a persistent and growing impact on municipal expenditures in the post-2007 years.

6 Other adjustment margins

When hit by a demand shock, firms may adjust on margins other than capital and labor. They may seek new sources of revenue or, *in extremis*, declare bankruptcy. In this section we show that our firms were not successful in replacing lost revenue with revenue from non-impacted municipalities, nor were they more likely to declare bankruptcy if hit by the demand shock.

Seeking new revenue from non-Patto municipalities In this section, we are concerned about the possibility that firms that conducted business in municipalities that were targeted by the 2008 law may, after 2008, start bidding more often in non-targeted municipalities. To explore the economic significance of this effect, we leverage the procurement market data. Table C.4 presents the estimates of equation (1) when the dependent variable is municipal-level averages of tender outcomes. The outcomes of interest are measures of tender participation and competition: number of bidders (columns 1, 2), winning rebate (columns 3, 4), and the fraction of tendered value won by local firms: that is, firms located in the same province as the tendering municipality (columns 5, 6). We find that tenders held by municipalities targeted by the 2008 legislative change tend to have slightly *more* bidders and slightly *higher* winning rebates after 2007. Furthermore, we detect no effects on the value won by local firms: columns 5-6 show no statistically significant change. Overall, we read the evidence as follows. As expected, targeted municipalities experienced an increase in competition per tender, relative to non-treated ones. The fact that the value won by local firms *did not decrease* in non-targeted municipalities is persuasive evidence that affected firms did not poach on "non-targeted territory:" that is, we do not see firms adjusting this margin in response to the demand shock.

Bankruptcy Table D.6 reports estimates of the impact of an increase in our shift-share variable on the yearly probability that a firm may declare bankruptcy. Multiplying the estimate in column 2 by a standard deviation in exposure to $Fisc.Rule.Exp_i$ yields 0.009*25.44=0.23; that is, a 0.23 percentage points increase in the exit rate of corporations that operate in the municipal procurement sector. After dividing this by the 2.26% average exit rate, we get a 9.861% increase in the exit probability relative to its baseline.[30] This

[30]We cannot provide results on the effect of $Fisc.Rule.Exp_i$ on the entry rate in Table D.6 because the definition of exposure to treatment limits the sample to corporations that existed before the demand

effect is 10 times smaller and not statistically different from zero if we include municipality of incorporation-by-year fixed effects (column 3). Thus, we find no evidence of a sizable "bankruptcy effect" of the fiscal rule.

7 Mechanisms

Section 5 has shown that firms cut capital, but not labor, in response to a permanent demand shock. What kind of theory would rationalize the evidence? In this section, we address this question intuitively, and refer the reader to Appendix A for a formal treatment.

A theory without adjustment costs cannot rationalize the evidence because, under the mild assumption that inputs are normal, we expect capital and labor to co-move in response to demand shocks. A theory with firing costs could rationalize the evidence. However, empirical evidence based on measuring variation in firing costs seems to reject this mechanism in our setting. The first proxy for firing costs is the duration of labor trials in the court of appeal where the firm is incorporated (ISTAT, 2012).[31] Interacting this proxy with our shift-share variable suggests that firing costs do not influence the response of capital and labor to the demand shock (Table D.7, panel A). The second proxy is whether a firm has more than 15 employees: a threshold above which stricter employment protection law (EPL) applies.[32] To ensure comparability, we restrict the sample to firms with employment between 10 and 20 workers before 2008, and create an EPL dummy for firms with average employment greater than 15 workers. Interacting this dummy with our shift-share variable again suggests that firing costs do not influence the response of capital and labor to the demand shock (Table D.7, panel B). Finally, we consider a form of labor that is not subject to firing costs: that is, outsourced services.[33] If firing costs

shock hit.

[31] Our measure of duration of trials is obtained as the ratio between the total backlog of labor disputes and the cases disposed in 2007 (and multiplied by 365), since data on exact duration does not exist for the years prior to 2008. This measure is used as a proxy for firing costs in Gianfreda and Vallanti (2017) for Italy and similar judicial data is used in Fraisse et al. (2015) to estimate the impact of firing costs on labor flows in France.

[32] See, for example, Garibaldi and Violante 2005, Schivardi and Torrini 2008, Lenzu and Manaresi 2018.

[33] For example, a school builder might hire a subcontractor to plant a garden, and also hire professional services such as lawyers, accountants, etc.

were the cause of the labor rigidity we measure, we should observe outsourced services to drop after the demand shock. However, we do not observe this (Table D.8). In sum, a theory of firing costs could rationalize the evidence, however, direct empirical evidence on firing costs seems to reject this theory as an explanation for the observed co-variation patterns of capital and labor.

A theory where capital is irreversible can rationalize the evidence when capital and labor are substitutes, provided that the demand shock is persistent. Since our estimates come from a persistent demand shock, this is our preferred theory. It is widely accepted that de-mobilizing sector-specific capital in response to a sector-level demand shock is difficult.[34] In Appendix A we provide a model where capital is irreversible, and capital and labor are substitutes. This model can rationalize the evidence through the following intuitive mechanism. Suppose a firm has to make a capital decision in period 1 that cannot be reversed in period 2, and the firm experiences a negative period-1 demand shock that portends an even worse period-2 shock. Then it is optimal for the firm to immediately reduce capital *beyond the level warranted by the period-1 shock*. This allows the firm immediately to reach a capital level suitable to period 2 (recall that capital cannot be adjusted in period 2), and to attenuate the period-1 distortion by replacing capital with labor in period 1 (this is where input substitutability matters). Thus, we expect capital to fall sharply in period 1, and labor to pick up the slack in period 1 (theoretically, labor could even increase in period 1). In period 2, both capital and labor will be very low (note that we do not observe this period in our data).

A placebo test based on rented (as opposed to owned) capital provides support for our preferred theory. Rented capital is easier to demobilize than owned capital, and is a possible substitute for owned capital. In the polar case where rented capital is *perfectly reversible*, rented capital is analogous to labor within our model. Therefore, if our model is correct, we should find that rented capital, like labor, was not cut as a response to the shock. This is indeed our finding (Table D.9). This evidence lends credibility to the irreversibility of owned capital as an explanation for our findings.

In sum, while the goal of this paper is not to provide or test a theoretical model, this section argues that the evidence in Section 5 is consistent with firms adjusting substitute

[34]See, for example, Shleifer and Vishny (2011), and Bloom (2009) who argues that, in modeling firm-level response to shocks, it is more important to account for capital than labor adjustment costs.

production factors subject to capital irreversibility, in response to a persistent demand shock portending worse news for the future.

8 Conclusions

We study the effect of a persistent demand shock on corporate factor utilization. Our identification strategy leverages a legislative change designed to permanently reduce spending in certain targeted municipalities. This change generates an arguably-exogenous drop in the revenue of procurement firms, which differs depending on each firm's reliance for its revenue on procurement in the targeted municipalities. Because we are able to pinpoint the source of demand variation, we are able to argue that the variation is persistent (in the sense that the legislative change was not reversed) and that it does not generate general-equilibrium confounding effects (because municipal procurement is a tiny fraction of GDP). Due to this combination of features, our research design delivers what we believe are the so far best-identified causal estimates of how individual firms adjust their capital and labor in response to a permanent demand shock.

Our main finding is that firms respond to a persistent demand shock by cutting capital rather than labor. This labor rigidity could be attributed to firing costs; however, using several empirical approaches, we find that variation in firing costs does not correlate with the response to the shock. Therefore, we proposed an alternative theoretical mechanism based on the irreversibility of capital investment.

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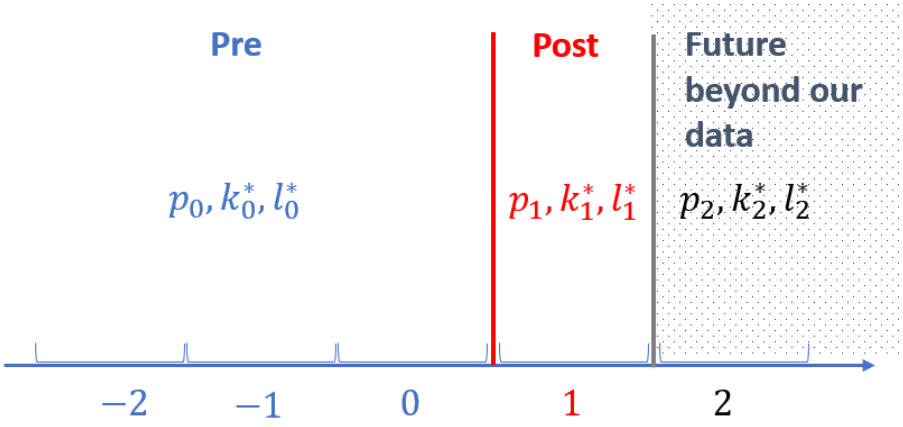
Appendix

A Analytical appendix

The timing is as depicted in Figure 6. The pre-period corresponds to pre-2008; period 1 is interpreted as 2008-11 (what we call "Post"); and period 2 is interpreted as the future beyond our data window. The parameters p_0, p_1, p_2 are interpreted as demand levels. Denote by k_t^*, l_t^* the firm's optimally-chosen capital and labor levels in period t. The evidence from Section 5 may then be expressed as follows.

Figure 6: Conceptual framework



Definition 1. ***("the evidence")*** *We refer to the constellation* $p_1 < p_0$, $l_1^* = l_0^*$ *and* $k_1^* < k_0^*$, *as "the evidence."*

A.1 No adjustment costs

Consider a firm that operates a production technology

$$y = f(k, l),$$

that satisfies the mild assumption that inputs are normal.[35] With no adjustment costs, the firm's problem is a succession of static maximization problems of the form:

$$\max_{k_t,l_t} p_t f\left(k_t, l_t\right) - rk_t - wl_t, \tag{4}$$

where r and w are exogenous factor prices. Because inputs are normal, the optimal k_t^* and l_t^* must co-move with p_t, hence, if p_0 and p_1 are such that $k_1^* < k_0^*$ then it must also be $l_1^* < l_0^*$. Therefore we have the following result.

Lemma 1. ***(the evidence cannot be rationalized without adjustment costs)*** *Suppose k_t and l_t solve (4) and the production function f has normal inputs. Then, regardless of p_0 and p_1, it is not possible that $l_1^* = l_0^*$ and $k^* < k_0^*$.*

This lemma implies that a model without adjustment costs cannot rationalize the evidence. We turn to a model with adjustment costs.

A.2 Labor adjustment costs

A labor adjustment cost of the form $C \cdot |l_t - l_{t-1}|$ can, if added to problem (4), rationalize the evidence. However, we find that variation in C (as proxied by the duration of labor trials, or by whether a firm is large enough to be unionized, or by whether labor is outsourced) fails to correlate with $\Delta l^* = l_1^* - l_0^*$ and $\Delta k^* = k^* - k_0^*$. Refer to the discussion of the empirical evidence in Section 7

A.3 Capital adjustment costs

Assume the representative firm operates a linear CES technology of the form:

$$f\left(k, l\right) = \sqrt{k^\rho + l^\rho}.$$

We restrict attention to the case $\rho \in (0, 1)$, meaning that inputs are substitutes. We set w equal to r to avoid corner solutions in the region where ρ is close to 1. We divide the firm's decision problem into a pre- and a post-demand shock phase. The fact that the firm solves these two problems separately embodies the assumption that the post- regime is unanticipated.

[35] Inputs are normal if they co-move along the expansion paths. Expansion paths are the loci of k and l that give the least-cost production of any given y, for given factor prices. Any homotetic production function has normal inputs, for example.

The firm's "pre-demand shock" problem We assume that the environment is stationary before the shock, that is, $p_0 = p_{-1} = p_{-2} = ...$ In a stationary environment adjustment costs do no matter because no adjustment is ever needed. Therefore the optimal strategy in a stationary environment solves the following time-invariant problem:

$$\max p_0 \sqrt{k^\rho + l^\rho} - rk - rl. \tag{5}$$

When solving (5), firms do not anticipate the future period-1 shock.

The firm's "post-demand shock" problem Starting in period 1 the environment is no longer stationary: we have $p_1, p_2 \neq p_0$. The firm's "post-problem" is:

$$\max_{k,l_1,l_2} p_1 \sqrt{k^\rho + (l_1)^\rho} + p_2 \sqrt{k^\rho + (l_2)^\rho} - 2rk - r(l_1 + l_2). \tag{6}$$

This maximization problem embodies the assumption that, while labor is acquired on the spot market in every period (no labor rigidities), capital lasts for both periods (it is irreversible once acquired; for this reason, k is not time-subscripted and the price of capital is doubled). This admittedly stark setting approximates a more realistic setting in which capital is durable but depreciates progressively, the firm can choose to augment its capital stock in every period, and the capital stock cannot be sold on the market. Separately, because p_1 and p_2 are known at the beginning of period 1, problem (6) captures an environment where the unanticipated period-1 shock is fully informative about the period-2 shock.

Proposition 1. ***(capital adjustment costs rationalize the evidence)*** *Denote the solutions to the "pre- problem" (5) and the "post-problem" (6) by (k_0^*, l_0^*) and (k^*, l_1^*, l_2^*), respectively.*

1. ***(the evidence can be rationalized)*** *Given any two positive numbers a and $b \in [a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}, a)$, there exist shocks $p_0 > p_1$ such that $(k_0^*, l_0^*) = (a, a)$ solves the "pre-problem" and $(k^*, l_1^*) = (b, a)$ are part of the solution to the "post-problem."*

2. ***(the evidence can only be rationalized if the negative demand shock portends even worse future shocks)*** *If $l_1^* = l_0^*$ and $k^* < k_0^*$, it must be $p_0 > p_1 > p_2$.*

Proof. **The pre-problem** The firm's first order conditions in the pre- problem (5) are:

$$k \quad : \quad (k_0)^{\rho-1} \frac{p_0 \rho}{2\sqrt{(k_0)^\rho + (l_0)^\rho}} = r \tag{7}$$

$$l \quad : \quad (l_0)^{\rho-1} \frac{p_0 \rho}{2\sqrt{(k_0)^\rho + (l_0)^\rho}} = r \tag{8}$$

implying a symmetric equilibrium

$$k_0^* = l_0^*.$$

Substitute k for l into (7) and isolate k to get:

$$k_0^* = \left(\frac{p_0\rho}{2r\sqrt{2}}\right)^{\frac{2}{2-\rho}}. \tag{9}$$

When $\rho \to 1$ we have:

$$k_0^* = l_0^* \to \frac{1}{2}\left(\frac{p_0}{2r}\right)^2. \tag{10}$$

The post-problem The first order conditions in the firm's post- problem (6) read:

$$k \quad : \quad k^{\rho-1}\left(\frac{\rho p_1}{2\sqrt{k^\rho + (l_1)^\rho}} + \frac{\rho p_2}{2\sqrt{k^\rho + (l_2)^\rho}}\right) = 2r \tag{11}$$

$$l_1 \quad : \quad (l_1)^{\rho-1}\frac{\rho p_1}{2\sqrt{k^\rho + (l_1)^\rho}} = r \tag{12}$$

$$l_2 \quad : \quad (l_2)^{\rho-1}\frac{\rho p_2}{2\sqrt{k^\rho + (l_2)^\rho}} = r \tag{13}$$

Proof of part 2. To show that $p_1 > p_2$, combine (11-13) to get:

$$(k^*)^{1-\rho} = \frac{(l_1^*)^{1-\rho} + (l_2^*)^{1-\rho}}{2} \tag{14}$$

Since in our case $k^* < k_0^* = l_0^* = l_1^*$, it must be that $l_2^* < k^* < l_1^*$. Note that the LH sides of (12-13) both equal the same decreasing (because $\rho \in (0,1)$) function of l_t multiplied by p_t. Since the two LHS must be equal and $l_1^* > l_2^*$, it must be $p_1 > p_2$.

Let us now show that $p_0 > p_1$. The LH sides of (8) and (12) must be equal. By assumption $l_0^* = l_1^*$ and $k_0^* > k^*$, therefore (because $\rho \in (0,1)$) it must be $p_0 > p_1$.

Proof of part 1.

Substitute $k_0^* = a$ into (9) to get a unique $\overline{p}_0$. Substituting $l_1^* = a$ and $k^* = b$ into equation (14) yields:

$$l_2^* = \left[2\,(b)^{1-\rho} - (a)^{1-\rho}\right]^{\frac{1}{1-\rho}} \stackrel{\text{def}}{=} c$$

Nonnegativity of l_2^* requires $2\,(b)^{1-\rho} \geq (a)^{1-\rho}$, i.e.:

$$b \geq a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}.$$

Having selected l_2^* as the solution of (14) guarantees that, if (12-13) hold, then (11) also holds. Thus, we have reduced the problem to selecting two numbers $\overline{p}_1, \overline{p}_2$ that solve

(12-13), i.e.:

$$\begin{aligned}(a)^{\rho-1}\frac{\rho p_1}{2\sqrt{b^\rho+(a)^\rho}} &= r\\ (c)^{\rho-1}\frac{\rho p_2}{2\sqrt{b^\rho+(c)^\rho}} &= r.\end{aligned}$$

Isolating p_1, p_2 from the above equations yields the desired $\overline{p}_1, \overline{p}_2$. Since $a > b$, by virtue of part 1 it is the case that $\overline{p}_0 > \overline{p}_1$. □

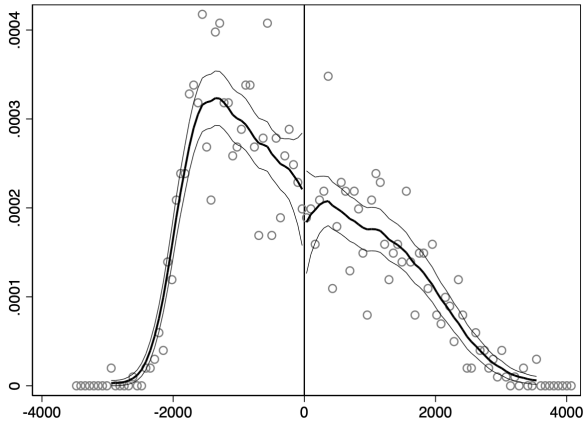
Part 1 says that the evidence from Definition 1, that is, $p_1 < p_0$, $l_1^* = l_0^*$ and $k_1^* < k_0^*$, can be rationalized. The additional feature that $k_0^* = l_0^*$ in part 1 is not a restriction, it is an equilibrium result that follows from the assumption that factor prices are equal and the production function is symmetric. The restriction that $b > a\left(\frac{1}{2}\right)^{\frac{1}{1-\rho}}$ in part 1 is a technical assumption needed to ensure that period-2 labor is nonnegative: it means that the period-2 shock should not be too severe. The intuition behind part 1 is that the firm anticipates the worse period-2 shock by reducing capital sharply in period 1, and compensates with labor to meet the desired production level in period 1. This asymmetric adjustment helps the firm cope with the constraint that capital is irreversible once acquired, whereas labor is purchased on the spot market. The substitutability between capital and labor ($\rho > 0$) allows for this compensation. The inequality $p_0 > p_1 > p_2$ in part 2 captures a negative period-1 demand shock which portends an even worse shock in period 2: Proposition 1 part 2 shows that this shock configuration is implied by the existing evidence.

B Variables, Descriptions, and Sources

Variable	Description	Source
Municipalities		
Total value of procurement	Is the annual total value of municipal contests for public works.	Telemat (2011)
N.Tenders	Is the number of tenders in a municipality in a year.	Telemat (2011)
Avg. value of procurement	Is the average value of the contests tendered in a municipality in a year computed using the engineers' estimates of the value of the contest.	Telemat (2011)
Percent Roads	Is the fraction of roads' contests	Telemat (2011)
Winning rebate	Is the offer that won the procurement, which represents the percentage discount over the engineer's estimate of the value of the tender. A higher offers represents lower municipal procurement costs.	Telemat (2011)
Number of bidders	Is the number of bibbers participating to the tender.	Infoplus (2012)
Winner from the same province	Is the value won by firms from the same province of the municipality running the contest over the value of contests by year and municipality.	Authors' calculation on Telemat (2011) data.
Transfers	Are the annual transfers to the municipality by the central governments (state and region).	Italian Ministry of Interior (2012).
Tax revenues	Are the annual tax revenues of the municipality.	Italian Ministry of Interior (2012)
Population	Is the municipal population.	ISTAT (2012).
Firm balance sheet.		AIDA (2016)
Bankruptcy/Exit	Probability that a company goes bankrupt in a given year obtained with the year fo last official submission of the balance sheet.	Variable: *Anno ultimo bilancio.*
Rev.Proc	Is the value of procurement contests won in a year (in 100,000 euros).	Authors' calculation on Information provider data
Capital	Total annual physical assets net of depreciation (in 1,000 euros). See Italian Civil Code Art. 2424 for details.	Variable: *Totale Immobilizzazioni Materiali.*
Labor	Total annual wages (in 1,000 euros) net of benefits and severance funds. See Italian Civil Code Art. 2424 for details.	Variable: *Totale salari e stipendi.*
O.Services	Are the firm total costs for outsourced services (in 1,000 euros)	Variable: *Servizi*
Rented Capital	Are the firm total costs for rented capital (in 1,000 euros)	Variable: *Godimento beni di terzi*

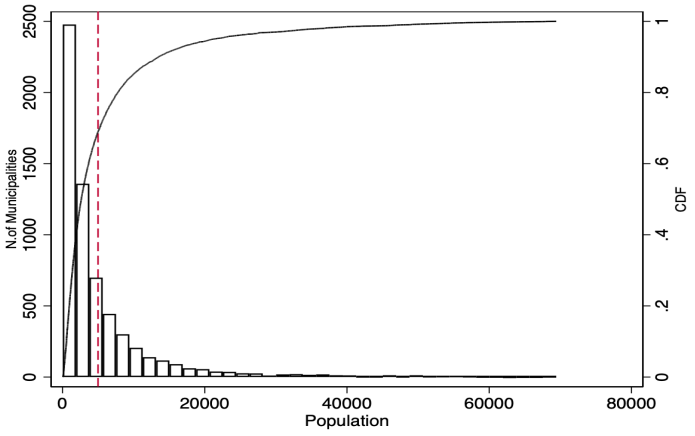
C Additional municipal analysis (the shift)

Figure C.1: No sorting around the fiscal-rule population threshold



Notes. Distribution of the municipal population around the threshold in Italian municipalities with population between 3,000 and 7,000 inhabitants in 2007. Circles represent the difference between the municipal population and the 5,000 threshold (vertical line). Circles are average observed values, the bold solid line is a kernel estimate (see McCrary, 2008), and the two thin lines are 95% confidence intervals. Discontinuity estimate, log difference in height, (and standard errors are -.15 (.26), respectively. Source: Statistics for all the public works tendered between 2004 and 2011 in Italy with population between 3,000 and 7,000 inhabitants in 2007.

Figure C.2: Distribution of Municipalities by Population



Note. All Italian municipalities with less than 68,000 inhabitants in 2007. These municipalities represent the 99% of the approximately 6,800 municipalities.

Table C.1: Impact of the fiscal shock on infrastructure spending for different windows around the 5k threshold

	Main Effect	Parallel Trend	Leads&Lags	Main Effect	Parallel Trend	Leads&Lags	Main Effect	Parallel Trend	Leads&Lags
Municipalities		1k-10k			3k-7k			4k-6k	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Fisc.Rule*Post	-1.024***			-0.642			-1.104*		
	(0.307)			(0.411)			(0.573)		
Fisc.Rule*Year		0.236			0.113			-0.061	
		(0.169)			(0.232)			(0.304)	
Fisc.Rule*2004			-0.694			-0.228			0.596
			(0.545)			(0.743)			(0.970)
Fisc.Rule*2005			-0.677			-0.446			0.263
			(0.538)			(0.704)			(0.988)
Fisc.Rule*2006			-0.404			-0.002			1.436
			(0.605)			(0.814)			(1.193)
Fisc.Rule*2008			-0.103			-0.004			-0.492
			(0.651)			(0.817)			(1.057)
Fisc.Rule*2009			-1.260**			-1.155			-0.761
			(0.632)			(0.789)			(1.202)
Fisc.Rule*2010			-1.537**			-0.779			-0.255
			(0.661)			(0.872)			(1.204)
Fisc.Rule*2011			-2.973***			-1.306*			-0.615
			(0.562)			(0.730)			(0.864)
Municipalities		4,056			1,520			713	
Observations	32,448	16,224	32,448	12,160	6,080	12,160	5,704	2,852	5,704
Municipal FE	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	NO	YES	YES	NO	YES	YES	NO	YES
Mean $Y\_treat - pre$	7.596			6.906			6.472		
Eff.Fisc.Rule(%)	-13.49			-9.296			-17.06		
Parallel trend (p-value)			0.807			0.806			0.524

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. *Fisc.Rule*Year* is the interaction between *Fisc.Rule* and the linear trend. These estimates are obtained in the pre-2008 sample and are used to test for the presence of linear pre-trends. *Parallel trend (p-value)* is the *p-value* for the joint statistical significance of the leads effect of the demand shock. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy with population between 1,000 and 10,000 (cols. 1 and 2); 3,000 and 7,000 (cols. 3 and 4); 4,000 and 6,000 (cols. 5 and 6).

Table C.2: Robustness: Impact of the fiscal shock on infrastructure spending controlling for transfers and tax revenues

	(1)	(2)	(3)
Fisc.Rule*Post	-5.247***	-6.459**	-5.915**
	(1.162)	(2.913)	(2.998)
Fisc.Rule	5.152**	4.287	4.287
	(2.586)	(2.953)	(2.951)
Post	-0.538***	-0.606***	-1.162***
	(0.076)	(0.181)	(0.155)
Observations	47,326	47,322	47,322
Mean Y_treat-pre	24.54	24.54	24.54
Eff.Fisc.Rule on Treated (%)	-21.38	-26.32	-24.10

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. In Column 1 we include as controls yearly transfers (in 100k) that represents the transfer to the municipality by central governments (state and region), and yearly taxes (in 100k) that represent municipal tax revenues; in Column 2, we include only pre-*Patto* values of the transfers and taxes interacted with *Post*; in Column 3 we also allow them to trend differentially for treatment vs control group in the pre-period. All the regressions control for population in 1,000 inhabitants. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

Table C.3: Bias adjusted municipal level estimates

Model Desc.	Short: No $Pop._{i,2001} \cdot Year_t$ $\mathring{\beta}$ $(St.\mathring{E}rror)$ $[\mathring{R}^2]$ (1)			Long: Yes $Pop._{i,2001} \cdot Year_t$ $\widetilde{\beta}$ $(St.\tilde{E}rror)$ $[\widetilde{R}^2]$ (2)			Bias-Adjusted β^* R^2_{Max}=1.3·$\widetilde{R}^2$ (3)
All	-5.48	(1.2)	[.0295]	-5.53	(1.2)	[.648]	-5.54
1k-10k	-1.02	(.307)	[.0319]	-1.03	(.307)	[.0412]	-1.04
3k-7k	-.642	(.411)	[.0111]	-.645	(.411)	[.0131]	-.652
4k-6k	-1.1	(.573)	[.004]	-1.11	(.573)	[.005]	-1.11

Notes: In column 1(2), estimates are obtained without (with) including the variable $Pop._{i,2001}$ interacted with the time trend in a model that includes *Fisc.Rule* the indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants), and *Post* the indicator for the years after 2007, and their interaction term. In column three bias-adjusted estimates β^* are computed with the following formula, and implemented in STATA with the .ADO file PSACALC2: $\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}]\frac{R^2_{Max}-\widetilde{R}^2}{\widetilde{R}^2-\mathring{R}^2}$ with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being the R^2_s from the model with and without$Pop._{i,2001} \cdot Year_t$, respectively. δ is set to 1 under the assumption that city specific time-varying unobservables are at least as important as the observables to estimate treatment effects. Rows reports estimates for all, 1k-10k, 3k-7k, and 4k-6k municipalities.

Table C.4: Checking for treatment spillovers in the procurement market

Dep.Var.	N.Bidders		Winning Rebate		Winners from the same province	
Model	OLS	FE	OLS	FE	OLS	FE
	(1)	(2)	(3)	(4)	(5)	(6)
Fisc.Rule*Post	2.620**	1.380	1.002***	0.852***	0.592	-0.657
	(1.137)	(1.294)	(0.292)	(0.319)	(1.308)	(1.559)
Post	5.919***		2.698***		6.183***	
	(0.807)		(0.227)		(1.012)	
Fisc.Rule	8.263***		1.522***		-16.864***	
	(0.679)		(0.251)		(1.011)	
Observations	13,597	12,395	16,277	15,072	11,278	9,959
Municipal FE	NO	YES	NO	YES	NO	YES
Year FE	NO	YES	NO	YES	NO	YES
Mean Y_treat-pre	32.77	32.77	17.67	17.67	47	47
Eff.Fisc.Rule on Treated (%)	7.993	4.212	5.672	4.824	1.259	-1.397

Notes: The table reports estimates of the effects of the demand shock on municipal procurement outcomes: *N.Bidders* is the number of competitors submitting an offer; *Winning-Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. Odd (even) columns report OLS (FE) estimates (with municipal and year fixed effects). Mean Y_treat-pre and St.Dev. Y_treat-pre are the sample mean and standard deviation for treated municipalities pre-2008. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

D Additional analysis of firm's responses

Table D.1: Descriptive statistics by matched and unmatched firms (pre-fiscal demand shock)

Sample	Unmatched	Matched	Diff.
	(1)	(2)	(3)
Share Proc.Won	.25	.75	.50
Proc.Won in *Patto* Mun.	0.78	0.79	0.01
N.Tenders	1.3	1.51	.21
Val.Proc.	4.69	5.44	.75
Roads	.27	.32	.06
N.Bidders	34.54	37.66	3.12
Winning rebate	18.08	17.82	-.26
Winner same prov.	.45	.48	.03

Notes: Table reports statistics at contract level by matched and unmatched firms. *Share Proc.Won* is the share of procurement; *Proc.Won in Patto Mun.* indicates contracts won in a Municipality with population above 5k. *N.Tenders* is the number of tenders won; *Val.Proc.* is value of tenders ; *Roads* indicates co*N.Bidders* is the number of competitors submitting an offer; *Winning Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. *Winner same prov.* indicates winners of the contracts from the same province of the municipality.

Table D.2: Descriptive statistics for matched and unmatched firms (pre-fiscal demand shock)

Stats	Mean	St.Dev.	p10	p50	p90	N
	(1)	(2)	(3)	(4)	(5)	(6)
Proc.Won in *Patto* Mun.	0.782	0.413	0	1	1	39,801
N.Tenders	1.355	2.106	0.250	0.750	2.750	39,801
Val.Proc.	4.876	10.04	0.500	2.281	10.34	38,052
Roads	0.281	0.449	0	0	1	39,801
N.Bidders	35.31	28.77	8.257	28	71.24	35,666
Winning rebate	18.01	10.33	6.946	16.01	32.69	37,494
Winner same prov.	0.458	0.498	0	0	1	39,234

Notes: Table reports statistics at contract level for the sample of matched and unmatched firms. *Proc.Won in Patto Mun.* indicates contracts won in a Municipality with population above 5k. *N.Tenders* is the number of tenders won; *Val.Proc.* is value of tenders ; *Roads* indicates co*N.Bidders* is the number of competitors submitting an offer; *Winning Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. *Winner same prov.* indicates winners of the contracts from the same province of the municipality.

Table D.3: Correlates of exposure to the demand shock: firm outcomes and geographical characteristics

Charact.	Capital	Labor	Mun. of Inc.>5k	Region FE	Province FE	City FE
	(1)	(2)	(3)	(4)	(5)	(6)
Coefficient	-0.001	-0.002*	10.167***			
	(0.000)	(0.001)	(0.988)			
R2	0.003	0.009	0.017	0.070	0.104	0.259

Notes: The table reports the estimated coefficients from OLS regressions of *Fisc.Rule.Exp* on: average firms outcomes before 2009: *Capital* are the firm total annual physical assets (in 1,000 euros), *Labor* are the firm total personnel costs (in 1,000 euros); and on firms geographical characteristics: *Mun of Inc. >5k* is an indicator that equals one for firms incorporated in municipalities with population above 5k and exposed to the fiscal rule; *Region FE* are region-of-incorporation fixed effects, *Province FE* are province-of-incorporation fixed effects, and *Municipal FE* are municipality of incorporation fixed effects. Financial variables are deflated using KLEMS deflators. SEs, in parenthesis, are clustered at the firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2008.

Table D.4: Bias-adjusted firm level estimates

Model	Short: No cityFE*trend			Long: Yes cityFE*trend			Bias-Adjusted β^*
Desc.	$\mathring{\beta}$ ($St.\mathring{E}rror$) [$\mathring{R}^2$]			$\widetilde{\beta}$ ($St.\widetilde{E}rror$) [$\widetilde{R}^2$]			R^2_{Max}=1.3·$\widetilde{R}^2$
	(1)			(2)			(3)
Rev.Proc	-.107	(.007)	[.049]	-.106	(.008)	[.181]	-.105
Capital	-4.34	(.573)	[.007]	-3.07	(.541)	[.114]	-2.37
Labor	.139	(.213)	[.007]	.262	(.220)	[.109]	.328

Notes: In column 1(2), estimates are obtained without (with) municipality-of-incorporation time trends estimating a model that includes *Fisc.Rule.Exp.* which represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, a dummy *Post* indicating years after 2007 and their interaction term. In column three bias-adjusted estimates β^* are computed with the following formula, and implemented in STATA with the .ADO file PSACALC2: $\beta^* \approx \widetilde{\beta} - \delta[\mathring{\beta} - \widetilde{\beta}] \frac{R^2_{Max} - \widetilde{R}^2}{\mathring{R}^2 - \widetilde{R}^2}$ with R^2_{Max} computed as in column 4 of Table 5, pg. 202 of Oster (2019) with $\widetilde{R}$ and $\mathring{R}$ being the R^2_s from the model with and without city-year FE, respectively. δ is set to 1 under the assumption that firm specific time-varying unobservables are at least as important as the observables to estimate treatment effects. Rows reports estimates for: *Rev.Proc.*, which is the value of procurement won in a year (in 100,000 euros); *Capital*, which are the firm total annual physical assets (in 1,000 euros); *Labor* which are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table D.5: Estimates of the effect of exposure on revenues from procurement, capital and labor for firms incorporated in special status regions

Dep.Var.	Rev.Proc	Capital	Labor	Capital	Labor
Model	FE	FE	FE	FE-IV	FE-IV
	(1)	(2)	(3)	(4)	(5)
Fisc.Rule.Exp.XPost	-0.032	-5.143	0.442		
	(0.040)	(5.676)	(1.808)		
Rev.Proc				160.606	-13.788
				(282.486)	(56.873)
Observations	676	676	676	676	676
Company FE	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES
CityFE*trend	YES	YES	YES	YES	YES
Mean Y	3.757	1126	704	1126	704
St.Dev.Fisc.Rule	11.23	11.23	11.23	0.632	0.632
Eff.Fisc.Rule.Exp. (%)	-9.575	-5.132	0.705		
First-Stage F-Stat				0.284	0.284

Notes: In columns 1-3, the table reports estimates of the effects of exposure to the fiscal demand shock on firms revenues from procurement, capital accumulation and labor for firms incorporated in special status regions. Columns 4,5 report IV estimates using *Fisc.Rule.ExpXPost* as an instrument for *Rev.Proc.* First-Stage F-Stat is the first stage statistics for the relevance of the instrument. *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp* represents the exposure to the fiscal demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. All estimates include Company, Year and city of incorporation-by-year fixed effects. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coeffcient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule* and Mean Y. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011 and incorporated in special status regions *(Friuli, Trentino, and Val d'Aosta)* .

Table D.6: Bankruptcy

Dep.Var.	Exit	Exit	Exit
Model	OLS	FE	FE-HT
	(1)	(2)	(3)
Fisc.Rule.Exp.XPost	-0.001	0.009	0.001
	(0.007)	(0.008)	(0.010)
Fisc.Rule.Exp.	0.005		
	(0.004)		
Post	2.464***		
	(0.225)		
Observations	22,855	22,798	22,798
Company FE	No	Yes	Yes
Year FE	No	Yes	Yes
CityFEXtrend	No	No	Yes
Mean Y	2.262	2.262	2.262
St.Dev.Fisc.Rule	25.44	25.44	25.44
Eff.Exposure (			
%)	-1.431	9.861	1.106

Notes: The table reports estimates of the effects of the demand shock on firm Bankruptcy/exit defined as the probability of going bankrupt in a given year. *Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2007. Col. 1 (2) [3] reports OLS (FE) [FE-HT] estimates (with firm and year fixed effects) [municipality-of-incorporation time trends]. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule on Treated (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule and Mean Y. SEs are clustered at firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

Table D.7: Use of inputs not affected by firing costs

Dep.Var.	Rev.Proc	Capital	Labor
Model	FE-HT	FE-HT	FE-HT
	(1)	(2)	(3)
	Panel A: High duration of labor disputes		
Fisc.Rule.Exp.XPostXDur.Lav	-0.0000	0.004	-0.0001
	(0.000)	(0.003)	(0.001)
Fisc.Rule.Exp.XPost	-0.105***	-4.257***	0.093
	(0.010)	(1.237)	(0.238)
	Panel B: Large firms subject to EPL		
Fisc.Rule.Exp.XPostXEPL	-0.030	3.055	-0.879
	(0.060)	(1.941)	(0.708)
Fisc.Rule.Exp.XPost	-0.106***	-1.468*	0.608**
	(0.026)	(0.871)	(0.292)
Company FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes

Notes: The table reports estimates of the effects of exposure to the demand shock on firms revenues from procurement, capital and labor. *Rev.Proc* are the revenues from procurement (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators. *Fisc.Rule.Exp.XPostXDur.Lav. (Fisc.Rule.Exp.XPostXEPL)* is a triple interaction term between the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, the dummy post, and the average duration of labor disputes in the court of appeal where the firm is incorporated, in 2007 (a dummy variable for with more than 15 workers subject to employment protection laws). In column 1, the original estimated coefficient and standard error of the variable *Fisc.Rule.Exp.XPostXDur.Lav.* are -3.60e-06 and .0000275. In Panel B, we consider the sub-sample of firms with employment between 10 and 20 workers before 2008. All estimates include firm, year and municipality-of-incorporation time trends and the interaction term *PostXDur.Lav (PostXEPL)*. SEs are clustered at firm level. Significance at the 10% (*), at the 5% (*), and at the 1% (**). Source: Statistics for procurement companies that won at least one auction before 2008 and observed between 2004 and 2011.

Table D.8: Effects of the demand shock on outsourced services

Model	O.Services OLS (1)	O.Services FE (2)	O.Services FE (3)	O.Services Parellel Trend (4)	O.Services Leads&Lags (5)
Fisc.Rule.Exp.XPost	4.426**	3.025*	2.393***		
	(1.938)	(1.717)	(0.927)		
Fisc.Rule.Exp.	-18.280***				
	(2.110)				
Post	-150.932**				
	(66.386)				
Fisc.Rule*Year				0.343	
				(0.472)	
Fisc.Rule.Exp.*Year2004					-0.615
					(1.894)
Fisc.Rule.Exp.*Year2005					0.049
					(0.897)
Fisc.Rule.Exp.*Year2006					-0.304
					(0.876)
Fisc.Rule.Exp.*Year2008					1.560***
					(0.479)
Fisc.Rule.Exp.*Year2009					1.972**
					(0.950)
Fisc.Rule.Exp.*Year2010					2.773**
					(1.150)
Fisc.Rule.Exp.*Year2011					2.765**
					(1.118)
Observations	22,855	22,798	15,884	10,025	22,798
Company FE	NO	YES	YES	YES	YES
Year FE	NO	YES	YES	YES	YES
CityXYearFE	NO	NO	YES	YES	YES
Mean Y	1216	1216	1216	1216	1218
St.Dev.Fisc.Rule	25.44	25.44	25.44	25.44	
Eff.Fisc.Rule.Exp. (%)	9.256	6.327	5.006	5.006	
No-pre-trend					0.907

Notes: The table reports estimates of the effects of exposure to the demand shock on the firm total costs for outsourced services (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2008. *Year2004-Year2011* are time dummies interacted with *Fisc.Rule.Exp.* All estimates include firm and year fixed effects. Columns 3-5, include municipality-of-incorporation time trends. In Column 4 *Fisc.Rule.Exp*Year* is the interaction term between *Fisc.Rule.Exp* and a linear trend and it is used to test for linear trends in the pre-2008 sample. *Parallel trend test (p-value)* is the p-value of the joint test for all the leading terms equal zero. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule and Mean Y. Significance at the 10% (*), at the 5% (*), and at the 1% (**). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table D.9: Effects of the demand shock on rented capital

	Rented Capital	Rented Capital	Rented Capital	Rented Capital	Rented Capital
Model	OLS	FE	FE	Parellel-Trend	Leads&Lags
	(1)	(2)	(3)	(4)	(5)
Fisc.Rule.Exp.XPost	0.220*	0.139	0.119		
	(0.128)	(0.127)	(0.125)		
Fisc.Rule.Exp.	-1.884***				
	(0.169)				
Post	-10.078				
	(8.300)				
Fisc.Rule*Year				0.002	
				(0.089)	
Fisc.Rule.Exp.*Year2004					0.152
					(0.237)
Fisc.Rule.Exp.*Year2005					0.070
					(0.134)
Fisc.Rule.Exp.*Year2006					0.067
					(0.085)
Fisc.Rule.Exp.*Year2008					0.115
					(0.114)
Fisc.Rule.Exp.*Year2009					0.133
					(0.130)
Fisc.Rule.Exp.*Year2010					0.182
					(0.176)
Fisc.Rule.Exp.*Year2011					0.299*
					(0.167)
Observations	22,855	22,798	22,798	10,025	22,798
Company FE	NO	YES	YES	YES	YES
Year FE	NO	YES	YES	YES	YES
CityXYearFE	NO	NO	YES	YES	YES
Mean Y	150.5	150.5	150.5	150.5	150.7
St.Dev.Fisc.Rule	25.44	25.44	25.44	25.44	
Eff.Fisc.Rule.Exp. (%)	3.727	2.353	2.008	2.008	
N.Firms		3425	3425	3174	3425
No-pre-trend					0.833

Notes: The table reports estimates of the effects of exposure to the demand shock on the firm total costs for rented capital (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2008. *Year2004-Year2011* are time dummies interacted with *Fisc.Rule.Exp.* All estimates include firm and year fixed effects. Columns 3-5, include municipality-of-incorporation time trends. In Column 4 *Fisc.Rule.Exp*Year* is the interaction term between *Fisc.Rule.Exp* and a linear trend and it is used to test for linear trends in the pre-2008 sample. *Parallel trend test (p-value)* is the p-value of the joint test for all the leading terms equal zero. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule and Mean Y. Significance at the 10% (*), at the 5% (*), and at the 1% (**). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Online Appendix

Table O.1: Impact of the fiscal shock on infrastructure spending controlling for different functions of municipal population

	(1)	(2)	(3)
Fisc.Rule*Post	-5.917***	-5.531***	-5.660***
	(1.993)	(1.244)	(1.313)
Population	0.003		
	(0.004)		
Population^2	-0.000***		
	(0.000)		
Log-Pop		2.150	1.196
		(2.566)	(2.410)
Observations	50,768	50,768	50,768
Sample	All	All	All
Municipal FE	YES	YES	YES
Year FE	YES	YES	YES
Mean Y_treat-pre	24.09	24.09	24.09
Eff.Fisc.Rule on Treated (%)	-24.56	-22.96	-23.50

Notes: The table reports estimates of the effects of the demand shock on the average annual total value of procurement for public works in all Italian municipalities. In each of the rows, *Fisc.Rule* is an indicator variable for municipalities with population above the fiscal rule population threshold (5,000 inhabitants) and *Post* is an indicator for the years after 2007. In Column 1, we control for population in 1,000 inhabitants and its square; In Column 2, for log-population and in Column 3 we also include 12 dummies for binned categories of population size. SEs are clustered at municipal level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for all the public works tendered between 2004 and 2011 in Italy.

Table O.2: Robustness: different data transformations

	Municipal data			Firm data								
Dep.Var.	Tot.Val.Proc.	Tot.Val.Proc.	Tot.Val.Proc.	Rev.Proc	Rev.Proc	Rev.Proc	Capital	Capital	Capital	Labor	Labor	Labor
Dep.Var.Spec	Levels	Logs	IHS	Levels	Logs	IHS	Levels	Logs	IHS	Levels	Logs	IHS
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Fisc.Rule.*Post	-5.480***	-0.184***	-0.303***									
	(1.199)	(0.029)	(0.025)									
Fisc.Rule.Exp*Post				-0.102***	-0.279***	-0.206***	-5.304***	-0.055***	-0.047***	-0.094	0.017*	-0.017
				(0.007)	(0.020)	(0.009)	(0.683)	(0.014)	(0.012)	(0.207)	(0.009)	(0.013)
Observations	50,768	26,735	50,768	22,798	7,199	22,798	22,798	18,968	22,798	22,798	18,535	22,798
Unit FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Mean Y_treat-pre	24.09	2.381	2.631	3.439	1.502	0.874	619.4	4.930	5.491	411.9	5.347	5.726

Notes: The table reports estimates of the effects of exposure to the demand shock on municipal spending for procurement, and firms revenues from procurement, capital accumulation and labor: *Tot.Val.Proc.* is the annual total value of municipal tenders for infrastructures in all Italian municipalities; *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). These variables are expressed in levels, in logs or in IHS depending on the specification. Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues, and it is expressed in levels, in logs or in IHS depending on the specification. In each of the rows, *Post* is an indicator for the years after 2007. In Cols.1-3 estimates are for municipal data and *Unit FE* are municipal fixed effects, while in Cols.4-12 for firms and *Unit FE* are firm fixed effects. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table O.3: Robustness: Post > 2008

Dep.Var.	Tot.Val.Proc.	Rev.Proc	Capital	Labor
	(1)	(2)	(3)	(4)
Fisc.RuleXPost	-6.851***			
	(1.178)			
Fisc.Rule.Exp.XPost		-0.080***	-4.538***	-0.122
		(0.005)	(0.633)	(0.197)
Observations	50,768	22,798	22,798	22,798
Mean Y	23.91	3.439	619.4	411.9
Eff.Fisc.Rule on Treated (%)	-28.65			
Eff.Fisc.Rule.Exp. (%)		-60.30	-18.98	-0.767

Notes: The table reports estimates of the effects of exposure to the demand shock on municipal spending for procurement, and firms revenues from procurement, capital accumulation and labor: *Tot.Val.Proc.* is the annual total value of municipal tenders for infrastructures in all Italian municipalities; *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the legislative change computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. *Post* is an indicator for the years after 2008. In column 1 estimates are for municipal data, while in columns. 2-4 for firms. Mean Y is the sample mean for each dep.var. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table O.4: Balance tests for the municipal data

Dep.Var.	Tot.Val.Tenders	N.Tenders	Avg.Value Proc.	Works:Roads	Num. of Bidders	Winning Rebate	Winners local
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
				Panel A: Full-Sample			
Fiscal rule (pop.>5k)	24.597	5.058	4.804	33.629	33.786	19.038	48.798
No fiscal rule (pop.≤5k)	2.66	1.06	2.883	31.246	26.474	17.818	66.405
p-value Diff.	<0.01	<0.01	<0.01	0.061	<0.01	<0.01	<0.01
Obs.	6346	6346	3584	3584	1995	2627	1783
				Panel B: 1k-10k			
Fiscal rule (pop.>5k)	7.917	2.259	4.007	34.126	31.288	17.888	56.133
No fiscal rule (pop.≤5k)	3.3	1.271	3.019	31.306	26.905	17.714	63.961
p-value Diff.	<0.01	<0.01	<0.01	0.107	<0.01	0.703	<0.01
Obs.	4056	4056	2321	2321	1166	1623	1038
				Panel C: 3k-7k			
Fiscal rule (pop.>5k)	7.195	2.085	3.758	32.35	28.712	17.624	56.198
No fiscal rule (pop.≤5k)	4.855	1.645	3.455	31.845	28.656	17.854	62.045
p-value Diff.	<0.01	<0.01	0.298	0.841	0.979	0.733	0.074
Obs.	1520	1520	982	982	575	731	485
				Panel D: 4k-6k			
Fiscal rule (pop.>5k)	6.119	1.893	3.54	30.32	27.336	17.583	57.779
No fiscal rule (pop.≤5k)	5.214	1.709	3.605	33.698	27.635	17.163	63.123
p-value Diff.	0.252	0.261	0.886	0.343	0.921	0.598	0.248
Obs.	713	713	462	462	285	342	227

Notes: *N.Tenders* is the number of tenders in a municipality in a year; *Avg.Value of procurement* is the average value of tenders in a municipality in a year computed using the engineers' estimates of the value of the works; *Roads* is the fraction of all tenders for road constructions; *N.Bidders* is the number of competitors submitting an offer; *Winning-Rebate* is the winning offer, which represents the percentage discount over the engineer's estimate of the value of the works. *p-value Diff.* are the *p-values* for the statistical differences of the sample means. *p-value Diff.* < 0.01 are approximations of the original *p-values*. Source: Statistics for all the public works tendered in 2007 in Italy.

Table O.5: Results are robust to different clustering of standard errors, and controlling for local demand shocks

Dep.Var.	Rev.Proc	Rev.Proc	Capital	Capital	Labor	Labor
	(1)	(2)	(3)	(4)	(5)	(6)
	Panel A: cluster at the municipality of incorporation level					
Fisc.Rule.Exp.XPost	-0.102***	-0.098***	-5.304***	-3.792***	-0.094	0.215
	(0.007)	(0.008)	(0.596)	(0.639)	(0.214)	(0.173)
Eff.Fisc.Rule.Exp. (%)	-75.18	-72.31	-21.78	-15.58	-0.583	1.327
	Panel B: controlling for regional demand shocks					
Fisc.Rule.Exp.XPost	-0.107***	-0.104***	-5.528***	-4.127***	-0.172	0.097
	(0.008)	(0.010)	(0.838)	(1.094)	(0.266)	(0.215)
Eff.Fisc.Rule.Exp. (%)	-80.17	-77.86	-21.82	-16.29	-1.023	0.579
Observations	22,798	22,798	22,798	22,798	22,798	22,798
Company FE	No	Yes	No	Yes	No	Yes
Year FE	No	Yes	No	Yes	No	Yes

Notes: The table reports estimates of the effects of exposure to the legislative change on firms outcomes: *Rev.Proc.* are the revenues from procurement (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS deflators.*Fisc.Rule.Exp.* represents the exposure to the demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock and the firm's pre-demand shock revenues. In each of the rows, *Post* is an indicator for the years after 2007. Odd (even) columns report estimates with firm and year fixed effects (add municipality-of-incorporation time trends). Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coefficient of *Fisc.Rule.Exp.*Post**St.Dev.Fisc.Rule and Mean Y. In Panel A, SEs are clustered at the municipality level. In Panel B, all the regressions include the fraction of cities exposed to *Patto* in the region of incorporation of each company (and its interaction with the *Post* dummy) and SEs are clustered at the firm level. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction before 2009 and observed between 2004 and 2011.

Table O.6: Estimates of the effect of exposure on revenues from procurement, capital and labor with shorter windows for the computation of Fisc.Rule.Exp.

Dep.Var.	Rev.Proc	Capital	Labor	Capital	Labor	Rev.Proc	Capital	Labor	Capital	Labor
Model	FE	FE	FE	FE-IV	FE-IV	FE	FE	FE	FE-IV	FE-IV
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Fisc.Rule.Exp.XPost	-0.081***	-3.931***	0.090			-0.051***	-4.546***	-0.226		
	(0.007)	(1.045)	(0.186)			(0.007)	(1.644)	(0.461)		
Rev.Proc				48.235***	-1.108				89.461***	4.455
				(13.245)	(2.277)				(33.982)	(9.145)
Observations	18,562	18,562	18,562	18,562	18,562	12,694	12,694	12,694	12,694	12,694
Company FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Year FE	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
CityFE*trend	YES	YES	YES	YES	YES	YES	YES	YES	YES	YES
Mean Y	3.766	642.7	426.4	642.7	426.4	4.228	695.7	446.5	695.7	446.5
St.Dev.Fisc.Rule	29.16	29.16	29.16	0.632	0.632	33.83	33.83	33.83	0.632	0.632
Eff.Fisc.Rule.Exp. (%)	-63.10	-17.83	0.618			-40.65	-22.10	-1.715		
First-Stage F-Stat				215.6	215.6				65.75	65.75

Notes: In columns 1-3 (6,8), the table reports estimates of the effects of exposure to the fiscal demand shock on firms revenues from procurement, capital accumulation and labor for firms incorporated in special status regions. Columns 4,5 (9,10) report IV estimates using *Fisc.Rule.ExpXPost* as an instrument for *Rev.Proc.* First-Stage F-Stat is the first stage statistics for the relevance of the instrument. *Rev.Proc.* is the value of procurement won in a year (in 100,000 euros); *Capital* are the firm total annual physical assets (in 1,000 euros); *Labor* are the firm total personnel costs (in 1,000 euros). Financial variables are deflated using KLEMS (2012) deflators. In Cols. 1-5 (6-10), *Fisc.Rule.Exp* represents the exposure to the fiscal demand shock computed as the ratio between the firm's value won in municipalities hit by the demand shock in 2006 and 2007 (in 2007) and the firm's average revenues in the same period (in 2007). All estimates include Company, Year and city of incorporation-by-year fixed effects. Mean Y is the sample mean for each dep.var. *Eff.Fisc.Rule.Exp (%)* is the ratio between the estimated coeffcient of *Fisc.Rule.Exp.*Post*St.Dev.Fisc.Rule* and Mean Y. Significance at the 10% (*), at the 5% (**), and at the 1% (***). Source: Statistics for procurement companies that won at least one auction in 2007 and observed between 2004 and 2011.