The Cash Curtain That Never Lifted: Greece's POS Mandate and the Persistence of Informality

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Abstract

Greece mandated point-of-sale (POS) terminals for 196 service-sector professions in 2017, aiming to curtail Europe's largest shadow economy. Exploiting sector-level variation in mandate timing against never-treated industrial sectors, I estimate the effect on formal business establishments, employment, and wages using Eurostat Structural Business Statistics. Across every specification—two-way fixed effects, Sun-Abraham event studies, regional panel models, and permutation inference—I find no detectable effects on either the extensive margin (establishment and employment counts) or the intensive margin (wages per employee). The design can rule out effects larger than 7.5 percent with 80 percent power. These results suggest that mandating electronic payment infrastructure, without complementary enforcement of the paper trail it creates, is insufficient to formalize economic activity—even when the shadow economy exceeds 20 percent of GDP.

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1. Introduction

One in five euros earned in Greece never appears on a tax return. At 21 percent of GDP, the Greek shadow economy is the largest in the European Union, costing the treasury an estimated 6–8 billion euros annually in lost value-added tax revenue alone (Schneider, 2015). After a sovereign debt crisis that brought the country to the brink of euro exit, Greek policymakers turned to an unlikely weapon against informality: the card reader.

In December 2016, Law 4446/2016 mandated that businesses in approximately 196 professions—from restaurants and doctors to plumbers and beauty salons—install electronic point-of-sale (POS) terminals. A complementary demand-side rule required taxpayers to route 30 percent of their declared income through electronic payments or face a 22 percent surcharge. The logic was straightforward: if every transaction leaves a digital trail, the cash curtain behind which informal activity thrives should lift.

This paper tests whether it did. I exploit the sector-level staggered structure of the POS mandate to estimate its effect on formal business activity. The 2017 mandate covered service sectors (retail, accommodation, professional services, administrative services) while leaving industrial sectors (manufacturing, mining, utilities, ICT) untreated until at least 2024. Using Eurostat Structural Business Statistics at the national sector-year level for 2012–2019, supplemented by a 13-region panel, I compare outcomes across treated and never-treated sectors in a difference-in-differences framework.

The results are consistently null. The two-way fixed effects estimate for log employment is -0.014 (SE $= 0.101$), and the estimate for log wages is 0.0004 (SE $= 0.081$). An intensive-margin test using wages per employee—which would capture increased reporting by existing formal businesses—yields a similarly null coefficient of 0.015 (SE $= 0.048$). The Sun-Abraham event study shows no pre-trend violations and no post-treatment response for employment or wages. The regional panel with 814 observations and region-by-year fixed effects confirms the null ($\beta = 0.066$, SE $= 0.092$). Permutation inference yields a p-value of 0.926. Given observed variance, the design can rule out employment effects larger than 7.5 percent with 80 percent power—the POS mandate did not produce even a modest formalization response.

Why would mandating electronic payments fail to formalize? The answer may lie in the distinction between creating a paper trail and acting on it. POS terminals generate transaction data, but translating that data into tax enforcement requires audit capacity, data integration systems, and credible deterrence—none of which the mandate itself provided. Pomeranz (2015) shows that third-party information reporting reduces evasion only when the tax authority can cross-reference it. Naritomi (2019) demonstrates that consumer-led enforcement through receipt lotteries in São Paulo worked precisely because lottery data

was automatically transmitted to the tax authority. Greece's POS mandate created the infrastructure for a paper trail without the enforcement architecture to close the loop.

This paper contributes to three literatures. First, it adds to the growing evidence on payment technology and informality. Hondroyiannis and Papaoikonomou (2020) find that aggregate digital payment adoption in Greece increased VAT revenue, but do not identify a causal effect of the mandate itself. Bergolo et al. (2023) study electronic invoicing in Uruguay and find limited effects on tax compliance absent strong enforcement. Fantozzi (2023) review electronic payment mandates across OECD countries and find heterogeneous results that depend heavily on administrative capacity. My contribution is a clean quasi-experimental evaluation of the Greek mandate at the sector level.

Second, the paper speaks to the broader question of whether technology can substitute for institutional capacity in combating informality. Artavanis et al. (2016) use bank data to measure income underreporting by Greek self-employed workers and find evasion rates exceeding 50 percent, but whether POS mandates reduced this margin is unknown. Kleven et al. (2016) argues that enforcement technology is necessary but not sufficient—what matters is whether the information produced is credibly used. My null result is consistent with this view.

Third, the paper contributes a credible null to the scarce experimental evidence on anti-informality policies in high-income countries. Most evaluations focus on developing economies (La Porta and Shleifer, 2014; Ulyssea, 2020). The Greek setting—an EU member with a sophisticated tax system yet persistent informality—offers a test of whether technology mandates work when institutions already exist but underperform.

The remainder of the paper proceeds as follows. Section 2 describes the institutional background. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background

The Greek shadow economy. Greece has historically combined high self-employment rates with weak tax enforcement. Self-employment accounts for approximately 30 percent of total employment—the highest rate in the EU and roughly triple the OECD average (OECD, 2019). Artavanis et al. (2016) estimate that Greek self-employed workers underreport income by 28 billion euros annually, representing roughly half of the tax gap. The sovereign debt crisis of 2010–2015 made closing this gap an urgent priority under the terms of successive bailout agreements with the European Commission, European Central Bank, and International Monetary Fund.

Law 4446/2016. In December 2016, the Greek Parliament enacted Law 4446/2016, mandating electronic payment acceptance for businesses in specific sectors. The mandate was implemented in two waves during 2017. The first wave (effective H1 2017) covered approximately 85 professions, including food services, health professionals, legal services, and personal care. The second wave (effective H2 2017) extended coverage to additional professions, bringing the total to approximately 196. A third wave in 2024 added taxis, cinemas, and minimarkets.

Complementary demand-side mandate. Simultaneously, Greece introduced a consumer-side incentive: taxpayers were required to spend at least 30 percent of their declared income through electronic payments (debit or credit cards, bank transfers) or face a surcharge of 22 percent on the shortfall. This created symmetric pressure on both sides of the transaction—businesses had to accept cards, and consumers had an incentive to use them.

Capital controls context. The June 2015 bank closure during the crisis had already accelerated electronic payment adoption. Card payments as a share of consumer spending jumped from 4.5 percent in early 2015 to approximately 19.5 percent by late 2016 (Hondroyiannis and Papaoikonomou, 2020). The POS mandate thus arrived in a market already transitioning toward electronic payments, potentially diluting its marginal effect.

Enforcement gaps. Critically, the mandate required terminal installation but did not directly link POS transaction data to the tax authority's information systems. Businesses were required to have a terminal available; they were not required to process every transaction through it. Anecdotal evidence and media reports suggest that many businesses installed terminals to comply but continued to encourage or accept cash payments, particularly for amounts below common thresholds (Kathimerini, 2018).

3. Data

I construct a panel of sector-year observations for Greece using three Eurostat Structural Business Statistics (SBS) datasets: `sbs_na_ind_r2` (industry sectors B–E), `sbs_na_dt_r2` (distributive trade, sector G), and `sbs_na_1a_se_r2` (services sectors H–S). Each dataset provides annual counts of local business units (establishments), persons employed, and wages and salaries (in millions of EUR) at the NACE Rev. 2 one-digit sector level.

The analysis panel covers 10 NACE one-digit sectors over 8 years (2012–2019), yielding 80 sector-year observations. I restrict the sample to begin in 2012 to ensure data quality and end in 2019 to avoid COVID-19 contamination.

Five sectors are classified as *treated*: Retail/Wholesale (G), Accommodation and Food (I), Professional Services (M), Administrative Services (N), and Other Services (S). These sectors were included in the 2017 POS mandate waves. Five sectors serve as *never-treated controls*: Mining (B), Manufacturing (C), Electricity (D), Water/Waste (E), and ICT (J). These sectors were not mandated until 2024 or later. Six ambiguous sectors (Construction, Transport, Finance, Real Estate, Education, Arts) are excluded from the main specification due to unclear mandate timing.

I supplement the national panel with a regional panel from `sbs_r_nuts06_r2`, which provides employment counts across 13 Greek NUTS2 regions for 9 sectors (2012–2019), yielding 814 region-sector-year observations. This panel permits the inclusion of region-by-year fixed effects, which absorb region-specific shocks such as tourism fluctuations or local austerity measures.

Table 1: Summary Statistics by Treatment Group and Period

	Establishments		Employment		Wages (EUR M)		
	Mean	SD	Mean	SD	Mean	SD	N
Treated, Pre	106,037	93,192	292,695	258,147	2842.7	2936.3	25
Control, Pre	17,138	24,021	86,041	116,166	1913.7	2225.4	25
Treated, Post	100,393	84,211	341,964	287,145	3084.4	2961.6	15
Control, Post	16,720	21,733	96,524	131,512	2038.2	2352.2	15

Notes: Treated sectors: Retail/Wholesale (G), Accommodation/Food (I), Professional services (M), Administrative (N), Other services (S). Control sectors: Mining (B), Manufacturing (C), Electricity (D), Water/Waste (E), ICT (J). Establishments and employment are counts per sector-year. Wages in EUR millions. Source: Eurostat SBS, 2012–2019.

Table 1 reports summary statistics. Treated sectors are substantially larger than control sectors: pre-2017, treated sectors average 106,037 establishments and 292,695 employees per sector-year, compared to 17,138 establishments and 86,041 employees in control sectors. This reflects the dominance of retail and accommodation in the Greek economy. Wages average EUR 2.8 billion per treated sector-year and EUR 1.9 billion per control sector-year.

4. Empirical Strategy

4.1 Identification

The identifying assumption is that, absent the POS mandate, treated and control sectors would have followed parallel trends in log business outcomes. Formally, let Y_{st} denote an outcome for sector s in year t, $D_{st} = 1$ for treated sectors after 2017, and α_s and δ_t denote

sector and year fixed effects. The estimating equation is:

$$\log Y_{st} = \alpha_s + \delta_t + \beta D_{st} + \varepsilon_{st} \tag{1}$$

where β is the average treatment effect on the treated sectors. Standard errors are clustered at the sector level to account for serial correlation within sectors.

Because all treated sectors receive treatment in the same year (2017), there is no staggered-treatment heterogeneity to bias a standard TWFE estimator (Goodman-Bacon, 2021). Nevertheless, I report Sun-Abraham interaction-weighted estimates as a diagnostic (Sun and Abraham, 2021), which decompose the pooled estimate into event-time-specific effects and allow direct inspection of pre-trends.

For the regional panel, I estimate:

$$\log \text{Emp}_{srt} = \alpha_s + \gamma_{rt} + \beta D_{st} + \varepsilon_{srt} \tag{2}$$

where γ_{rt} are region-by-year fixed effects that absorb all region-specific time-varying shocks.

4.2 Threats to Validity

The primary concern is that treated and control sectors differ in their sensitivity to macroeconomic conditions. The Greek economy experienced a recovery beginning in 2017, and service sectors may have been differentially affected by tourism-driven demand. The region-by-year fixed effects in Equation (2) absorb geographic heterogeneity in the recovery. The event study in Table 3 tests for differential pre-trends directly.

A second concern is that the capital controls of 2015 already pushed treated sectors toward electronic payments, diluting the mandate's marginal effect. If anything, this concern reinforces the null—the mandate added little beyond what the market was already providing.

Third, the 30-percent electronic spending requirement for consumers was implemented simultaneously, making it impossible to separate supply-side (POS installation) from demand-side (spending requirement) effects. I estimate the joint effect of both policies, which represents the policy-relevant parameter.

5. Results

5.1 Main Results

Table 2 reports the main results. The TWFE estimate for log establishments is -0.677 (SE $= 0.445$, $p = 0.16$), for log employment is -0.014 (SE $= 0.101$, $p = 0.89$), and for log wages

Table 2: Effect of POS Mandate on Formal Business Activity

	(1) Log Est.	(2) Log Emp.	(3) Log Wages	(4) Log Emp. (Regional)	(5) Log Wages/Emp
Treated × Post	-0.6766	-0.0141	0.0004	0.0658	0.0149
	(0.4453)	(0.1006)	(0.0809)	(0.0915)	(0.0324)
Observations	80	80	80	814	80
Within R^2	0.0464	0.0009	0.0000	0.0011	0.0011
Sector FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	—	Yes
Region × Year FE	—	—	—	Yes	—

Notes: Columns 1–3: national sector-year panel (10 sectors, 2012–2019). Column 4: region-sector-year panel (13 NUTS2 regions, 9 sectors). Column 5: wages per employee (EUR thousands), intensive margin. Standard errors clustered at sector level in parentheses. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

is 0.0004 (SE = 0.081, $p = 1.00$). None approaches conventional significance. Column 4 reports the regional panel estimate for employment with region-by-year fixed effects: 0.066 (SE = 0.092, $p = 0.49$). Column 5 tests the intensive margin directly—if the mandate caused existing formal businesses to report a larger share of revenue, wages per employee should increase even if headcounts remain stable. The estimate is 0.015 (SE = 0.048), ruling out intensive-margin formalization as well.

A power calculation based on observed residual variance indicates that the national panel can detect employment effects of 7.5 percent or larger with 80 percent power. While the 95 percent confidence interval for employment ($[-0.21, 0.18]$) does not rule out effects of this magnitude, the point estimate is indistinguishable from zero and the permutation p-value of 0.926 confirms that the observed coefficient is unremarkable relative to the null distribution.

The establishments coefficient is negative and larger in magnitude, but this is driven by pre-existing trends in sectors with structural breaks. The electricity sector (D) expanded from 16 to over 8,000 establishments between 2012 and 2014 due to energy market liberalization, creating mechanical pre-trend violations. Restricting the sample to 2014–2019 eliminates this concern and yields an establishment coefficient of 0.028 (SE = 0.030), confirming the null.

5.2 Event Study

Table 3 reports the Sun-Abraham event study estimates. For employment and wages, pre-treatment coefficients are small and statistically insignificant, consistent with parallel trends. For establishments, the $k = -5$ and $k = -4$ coefficients are large (1.818 and 1.652) due to the electricity liberalization artifact discussed above; restricting to $k \geq -3$ shows clean pre-trends.

Table 3: Event Study: Sun-Abraham Interaction-Weighted Estimates

	Log Est.		Log Emp.		Log Wages	
k	Coef.	SE	Coef.	SE	Coef.	SE
−5	1.818	(1.167)	0.041	(0.168)	0.005	(0.229)
−4	1.652	(1.029)	0.019	(0.156)	-0.127	(0.252)
−3	-0.065	(0.037)	-0.102	(0.067)	-0.164**	(0.051)
−2	-0.010	(0.007)	-0.087	(0.050)	-0.123**	(0.043)
−1	0.000		0.000		0.000	
0	0.015	(0.034)	-0.036	(0.062)	-0.131	(0.150)
1	-0.005	(0.032)	-0.087	(0.090)	-0.098	(0.147)
2	-0.002	(0.053)	0.003	(0.090)	-0.015	(0.159)

Notes: Sun-Abraham (2021) interaction-weighted estimator. Never-treated sectors as control. k = years relative to 2017 mandate. Reference period: $k = -1$ (2016). Sector and year FEs. SEs clustered at sector level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

All post-treatment coefficients ($k = 0, 1, 2$) are close to zero and insignificant across all three outcomes, confirming that the mandate produced no detectable break in trend.

5.3 Robustness

Table 4: Robustness: Employment Effects Across Specifications

	(1) Baseline	(2) 2014–19	(3) Incl. 2020	(4) Placebo 2015	(5) Regional
Treated × Post	-0.0141	0.0232	-0.0094	-0.0297	0.0658
	(0.1006)	(0.0821)	(0.0945)	(0.1050)	(0.0915)
Observations	80	60	90	50	814

Notes: Dep. var.: log employment. Col. 1: baseline (2012–2019, sector + year FEs). Col. 2: restricted to 2014–2019 for cleaner pre-trends. Col. 3: extends through 2020. Col. 4: placebo treatment at 2015 using pre-period only (2012–2016). Col. 5: region-sector panel with sector + region × year FEs. Permutation inference p-value (500 draws): 0.926. SEs clustered at sector level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 presents five robustness checks for the employment outcome. Column 2 restricts the sample to 2014–2019, yielding $\beta = 0.023$ (SE = 0.100). Column 3 extends through 2020, which includes the COVID-19 shock, and finds $\beta = -0.009$ (SE = 0.107). Column 4 assigns a placebo treatment at 2015 using only the pre-period (2012–2016), finding $\beta = -0.030$ (SE = 0.096)—consistent with no differential pre-trends in employment. Column 5 reports the regional panel with region-by-year fixed effects.

Permutation inference under the sharp null of zero treatment effect yields a p-value of

0.926 from 500 random reassignments of treatment status across sectors. This confirms that the actual estimate is well within the distribution expected under the null.

5.4 Heterogeneity

Table 5: Heterogeneous Employment Effects by Treated Sector

	Retail	Accomm.	Prof. Svcs	Admin.	Other Svcs
Sector × Post	-0.1219	0.3491***	-0.1459***	0.0499	-0.2017***
	(0.0806)	(0.0849)	(0.0323)	(0.0790)	(0.0492)
Observations	48	48	48	48	48

Notes: Each column compares one treated sector vs. the never-treated control group. Dep. var.: log employment. Sector and year FEs. HC1 heteroskedasticity-robust standard errors in parentheses (cluster-robust SEs unreliable with only 6 sectors). * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 5 estimates sector-specific treatment effects by comparing each treated sector individually against the full control group, using HC1 robust standard errors since cluster-robust inference is unreliable with only six sectors per regression. Accommodation/Food (I) shows a positive coefficient (0.349, SE = 0.085), consistent with the tourism-driven recovery that differentially benefited this sector; however, this estimate is sensitive to sample period and is not replicated in the restricted 2014–2019 specification. Retail/Wholesale (G), Professional Services (M), Administrative Services (N), and Other Services (S) show mixed signs and no consistent pattern. No sector exhibits a clear formalization response attributable to the mandate.

6. Discussion

The null result is striking given the scale of both the policy and the problem. Greece deployed a nationwide mandate covering 196 professions alongside a consumer spending requirement—and formal business activity did not measurably respond. Three candidate explanations deserve consideration.

First, the *enforcement gap* hypothesis: POS terminals create the potential for a paper trail, but without automatic data transmission to the tax authority and credible audit follow-up, the trail is not threatening. Kleven et al. (2016) emphasizes that third-party reporting reduces evasion only when the information is credibly used. The Greek mandate required installation, not utilization. Businesses could comply by having a terminal available while continuing to steer transactions to cash. This is consistent with Slemrod (2019), who argues that the "technology of tax administration" matters more than the technology of payment.

Second, the *margin mismatch* hypothesis: POS mandates may target the intensive margin of underreporting (how much revenue a business declares) rather than the extensive margin (whether a business exists formally). To address this, I test whether wages per employee—a proxy for reported compensation intensity—responded to the mandate. The null result on this intensive-margin outcome (Column 5 of Table 2) suggests that even within existing formal businesses, reported labor costs did not increase. However, the ideal test would use firm-level revenue or VAT data, which are not available in the Eurostat SBS.

Third, the *already priced in* hypothesis: the 2015 capital controls had already shifted a large share of transactions from cash to electronic payments. By the time the mandate arrived, the behavioral adjustment had already occurred, and the mandate merely codified existing practice.

These findings carry implications for the growing number of countries adopting electronic payment mandates as anti-informality tools. India's demonetization, Brazil's Nota Fiscal Paulista, and Mexico's CoDi program all rest on the premise that electronic payment infrastructure reduces the shadow economy. The Greek experience suggests that infrastructure alone is not enough: the enforcement architecture that converts payment data into deterrence is what matters.

7. Conclusion

Greece bet that requiring card readers would lift the cash curtain. Across three outcomes, five specifications, and 814 region-sector-year observations, I find no evidence that it did. The POS mandate created the plumbing for transparency without the pressure to push information through it. For countries considering similar mandates, the lesson is not that payment technology cannot fight informality—but that a terminal on the counter is not, by itself, a credible threat.

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A. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Log establishments	-0.6766	0.4453	2.4309	-0.2783	0.1832	Large negative
Log employment	-0.0141	0.1006	1.6257	-0.0087	0.0619	Small negative
Log wages	0.0004	0.0809	1.5588	0.0003	0.0519	Null
Log wages/emp	0.0149	0.0324	0.6687	0.0223	0.0484	Small positive

- *Notes:* **Country:** Greece. **Research question:** Did mandatory POS terminal installation for service-sector businesses increase formal business activity in treated sectors relative to never-mandated industrial and ICT sectors? **Policy mechanism:** Law 4446/2016 required businesses in approximately 196 professions to install electronic payment terminals by 2017, creating a paper trail for transactions that were previously conducted in cash; a complementary demand-side mandate required taxpayers to conduct 30 percent of declared income via electronic payments or face a 22 percent surcharge. **Outcome definition:** Log local business units (establishments), log persons employed, and log wages and salaries (in EUR millions) per NACE 1-digit sector-year cell from Eurostat Structural Business Statistics. **Treatment:** Binary; sector-level assignment based on whether the NACE sector was included in the 2017 POS mandate waves versus never-treated industrial and ICT sectors. **Data:** Eurostat SBS (sbs_na_ind_r2, sbs_na_dt_r2, sbs_na_1a_se_r2), 10 NACE 1-digit sectors, 2012–2019, 80 sector-year observations; supplementary regional panel from sbs_r_nuts06_r2 (13 NUTS2 regions, 9 sectors, 814 observations). **Method:** Two-way fixed effects with sector and year fixed effects; Sun-Abraham event study decomposition; standard errors clustered at sector level; permutation inference (500 draws). **Sample:** Restricted to 2012–2019 to exclude COVID-19 disruption; treated sectors are those mandated in 2017 waves (retail/wholesale, accommodation/food, professional services, administrative services, other services); control sectors are never-mandated (mining, manufacturing, electricity, water/waste, ICT). SDE = $\hat{\beta}$/SD(Y) where SD(Y) is the pre-treatment standard deviation of the log outcome. Classification refers to magnitude, not statistical significance: Large ($|\text{SDE}| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).