The Formalization Mirage: Electronic Sales Reporting and Enterprise Dynamics in the Czech Republic

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Abstract

In 2016–2018, the Czech Republic required all cash-handling businesses to transmit receipt data to the tax authority in real time—the Electronic Records of Sales (EET). A naïve cross-country difference-in-differences comparing Czech sectors to Visegrad neighbors suggests that EET reduced registered enterprises by 19 percent. However, this estimate is entirely an artifact of differential convergence trends: a placebo test on non-EET Czech sectors yields an identical coefficient, and permutation inference fails to reject the null ($p = 0.49$). Controlling for unit-specific linear trends reverses the sign, yielding a positive 8 percent effect consistent with the formalization channel documented in developing countries. I name this pattern the *formalization mirage*: cross-country designs applied to transition economies mistake convergence for policy effects, systematically overstating the costs of enforcement technology.

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1. Introduction

When the Czech Republic launched its Electronic Records of Sales (EET) system in December 2016, restaurant owners protested in the streets, opposition politicians called it a "state surveillance tool," and the hospitality industry predicted mass closures. Six years later, the incoming government abolished the entire system, declaring it an unnecessary burden on small business. The political narrative was clear: EET destroyed enterprises.

This paper shows that the empirical reality is more subtle—and more interesting—than either side claimed. Using Eurostat Structural Business Statistics across five Central European economies, I construct a panel of 324 industry-country units spanning 2008–2020 and exploit the EET's phased sector rollout as a staggered natural experiment. A standard two-way fixed effects (TWFE) regression yields a large negative estimate: Czech sectors covered by EET appear to have 19 percent fewer enterprises after the policy took effect. This finding would seem to vindicate the critics.

But the estimate is a mirage. Three diagnostic tests expose the problem. First, a Sun–Abraham event study (Sun and Abraham, 2021) reveals significant positive pre-trend coefficients stretching back a decade—Czech EET sectors were converging toward European levels long before the policy. Second, a placebo test applying the same empirical design to Czech sectors *not* covered by EET produces an effect of equal magnitude (-17.7 percent, $p < 0.01$). Third, randomization inference across 1,000 permutations yields $p = 0.49$, confirming that the observed coefficient is indistinguishable from chance under the null.

These failures trace to a single source: the Czech Republic, like other post-transition economies, experienced sustained convergence in formal enterprise counts toward Western European levels throughout 2008–2020. This convergence was sector-wide and preceded EET by years, but any cross-country design that fails to account for it will attribute the slowdown in catch-up growth to whatever policy happened to occur during the sample period (Roth et al., 2023).

Once I control for unit-specific linear time trends—absorbing the pre-existing convergence—the sign flips. The preferred estimate is $+8.0$ percent ($p < 0.01$), consistent with the formalization hypothesis: EET caused previously unregistered businesses to enter the formal sector (Naritomi, 2019; Ulyssea, 2018). This finding aligns with evidence from Brazil, Ethiopia, and Pakistan that enforcement technology increases formal registration rather than destroying enterprises (Pomeranz, 2015; Okunogbe and Pouliquen, 2022; Waseem, 2018).

This paper contributes to three literatures. First, it provides the first cross-country causal evaluation of the Czech EET system, extending the tax compliance technology literature (Slemrod, 2019; Kleven et al., 2011) to an EU member state. Prior work on EET has been limited to descriptive revenue analyses (Slavkova and Sedmihradska, 2019); I provide the first enterprise-level identification. Second, it introduces the concept of a *formalization mirage*—the systematic bias that arises when cross-country difference-in-differences designs are applied to transition economies without trend correction. This echoes methodological concerns raised by Rambachan and Roth (2023) and Goodman-Bacon (2021) about pre-trend sensitivity in staggered designs, but identifies a specific economic mechanism (convergence) rather than a purely statistical concern. Third, by studying the same policy in both a developed and developing-country framework, it provides a bridge between the large formalization literature focused on Latin America and South Asia (La Porta and Shleifer, 2014; Bruhn, 2011; Fajnzylber et al., 2011) and the European tax compliance literature.

The paper proceeds as follows. Section 2 describes the EET policy and its institutional context. Section 3 presents the data. Section 4 develops the empirical strategy. Section 5 reports results. Section 6 discusses implications.

2. Institutional Background and Policy Setting

The Electronic Records of Sales (Elektronická evidence tržeb, or EET) was enacted as Act No. 112/2016 Coll. and required businesses receiving cash payments to transmit transaction data to the Czech Financial Administration in real time. Each receipt was assigned a unique fiscal identification code by the tax server before being issued to the customer. The explicit goal was to reduce VAT evasion in cash-intensive sectors, which the Czech Ministry of Finance estimated at 15–20 billion CZK annually (roughly 1.5 percent of tax revenue).

Phased rollout. The EET was implemented in four waves, each covering a different set of NACE sectors:

- **Phase 1** (December 1, 2016): Accommodation and food service (NACE Section I)—approximately 59,000 enterprises.
- **Phase 2** (March 1, 2017): Wholesale and retail trade (NACE Section G)—approximately 230,000 enterprises.
- **Phase 3** (March 1, 2018): Transport (H), professional services (M), and agriculture (A).
- **Phase 4** (June 1, 2018): Manufacturing (C) and remaining sectors.

This phased structure is the source of identifying variation: sectors entered the EET system at different dates, creating natural treatment cohorts for a staggered difference-in-differences design. Sectors not yet phased in serve as controls for those already covered.

Compliance mechanism. EET imposed two margins of adjustment on businesses. First, a *direct compliance cost*: firms needed to acquire certified electronic equipment and internet connectivity, with median setup costs estimated at 5,000–15,000 CZK. Second, and more consequentially, a *transparency cost*: businesses that had previously underreported cash receipts could no longer do so without detection. This created an incentive for marginal enterprises—those whose profitability depended on evasion—to either exit or restructure.

Political economy and abolition. The EET was a signature policy of the center-left Sobotka government. Following the 2021 elections, the center-right Fiăla coalition suspended EET enforcement and formally abolished the system effective January 1, 2023. This introduction-and-abolition symmetry provides an additional identification opportunity, which I exploit in a reversal test using 2022–2025 Czech Statistical Office data.

Comparison countries. Slovakia, Poland, Hungary, and Austria serve as control countries. None implemented a comparable nationwide real-time electronic reporting mandate during the 2008–2020 sample period. Slovakia and Hungary use electronic cash registers but without real-time server verification. Poland introduced a voluntary electronic receipt system (Paragon+) only in 2020. Austria's Registrierkassenpflicht (2016) applies to cash registers but lacks the real-time server transmission that defines EET.

3. Data

3.1 Enterprise Panel

The main dataset draws on Eurostat's Structural Business Statistics (SBS), which reports the number of active enterprises by NACE Rev. 2 division and country on an annual basis. I combine four SBS tables covering industry, services, construction, and trade to construct a comprehensive panel at the 2-digit NACE division level (69 unique divisions) for five countries: Czech Republic, Slovakia, Poland, Hungary, and Austria. The sample spans 2008–2020, yielding a balanced panel of 324 division-country units observed over 13 years (4,212 observations).

Each Czech division inherits its parent NACE section's EET phase timing. Thirty-nine Czech divisions fall into EET-covered sectors (C, G, H, I, M), forming the treated group. The remaining 285 units—Czech non-EET sectors and all foreign sectors—constitute the control group.

3.2 Supplementary Data

Three additional sources support mechanism and robustness tests:

- **Business demography** (Eurostat *bd_9ac_l_form_r2*): Annual enterprise births and deaths by sector and country, 2008–2020. Permits decomposition of the net effect into entry and exit channels.
- **VAT revenue** (Eurostat *gov_10a_taxag*): Annual government tax revenue by type and country, 1995–2024. Tests whether EET increased VAT collections.
- **Czech Statistical Office** (CZSO datasets 140133q22–q25): Quarterly enterprise counts by sector and municipality, 2022–2025. Enables a reversal test around the January 2023 abolition.

3.3 Summary Statistics

Table 1: Summary Statistics

	N	Divisions	Mean Enterprises	SD Enterprises	Mean ln(Ent.)	SD ln(Ent.)
AT (Control)	884	68	4,634	8,090	-Inf	NaN
CZ EET Sectors	507	39	17,201	25,714	8.37	2.08
CZ Non-EET Sectors	312	24	13,292	28,526	7.97	1.99
HU (Control)	858	66	8,567	14,407	7.64	2.08
PL (Control)	884	68	24,505	48,814	8.55	2.14
SK (Control)	767	59	6,556	13,364	7.35	1.86

Notes: Panel of 2-digit NACE divisions × countries, 2008–2020. Enterprise counts from Eurostat Structural Business Statistics (SBS). CZ EET sectors are NACE sections C, G, H, I, M, phased into electronic reporting between December 2016 and June 2018. Non-EET sectors are NACE sections B, D, E, F, J, K, L, N, S. Control countries are Austria, Hungary, Poland, and Slovakia.

4. Empirical Strategy

4.1 Identification

I exploit the phased EET rollout as a staggered difference-in-differences design. The unit of observation is a 2-digit NACE division × country pair. Treatment is binary: $D_{it} = 1$ if unit i is a Czech division in an EET-covered sector and year t falls at or after the phase-in date.

The identifying assumption is parallel trends: absent EET, enterprise counts in Czech covered sectors would have evolved on the same trajectory as the control group. I assess this assumption using three approaches: a Sun–Abraham event study, a placebo test on non-EET Czech sectors, and randomization inference.

4.2 Estimation

The baseline TWFE specification is:

$$\ln(\text{Enterprises})_{it} = \alpha_i + \gamma_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{1}$$

where α_i are unit fixed effects and γ_t are year fixed effects. Standard errors are clustered at the unit level.

Because of pre-existing convergence trends (documented in Section 5.1), my preferred specification augments Equation 1 with unit-specific linear time trends:

$$\ln(\text{Enterprises})_{it} = \alpha_i + \delta_i \cdot t + \gamma_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{2}$$

For heterogeneity-robust estimation, I use the Sun–Abraham decomposition (Sun and Abraham, 2021), which decomposes the overall effect into cohort-specific treatment effects and aggregates using the interaction-weighted estimator to avoid negative weighting problems documented by Goodman-Bacon (2021).

4.3 Threats to Validity

Differential convergence. The central threat is that Czech sectors may have been converging toward Western European levels for structural reasons (EU accession, institutional reform, capital deepening) unrelated to EET. I address this with unit-specific trends and the non-EET placebo.

Concurrent policies. The Czech Republic implemented other business-environment reforms during 2016–2018, including changes to VAT rates and social security contributions. These affect all Czech sectors equally and are absorbed by the year fixed effects. The identifying variation comes from the *differential* timing of EET across sectors.

Anticipation. Businesses may have adjusted before their sector's phase-in date. The Sun–Abraham event study allows me to test for anticipation effects in the period immediately preceding treatment.

5. Results

5.1 Main Results

Table 2: Effect of Electronic Records of Sales on Enterprise Counts

	(1) TWFE	(2) Unit Trends	(3) Short Window	(4) CZ vs SK	(5) CZ Only
EET Treatment	-0.190***	0.080***	-0.091***	-0.424***	-0.040
	(0.034)	(0.029)	(0.024)	(0.051)	(0.063)
Unit FE	Yes	Yes	Yes	Yes	Yes
Year FE	Yes	Yes	Yes	Yes	Yes
Unit Trends	No	Yes	No	No	No
Pre-period	2008–16	2008–16	2013–16	2008–16	2008–16
Countries	5	5	5	CZ, SK	CZ
N	4190	4190	2576	1586	819

Notes: Dependent variable is ln(number of enterprises) at the 2-digit NACE division × country × year level. Treatment is 1 for Czech divisions in EET-covered sectors (NACE C, G, H, I, M) after their respective phase-in date. Standard errors clustered at the division × country level in parentheses. Column (2) includes division-specific linear time trends. Column (3) restricts the pre-period to 2013–2016. Column (4) uses only Czech Republic and Slovakia. Column (5) uses only Czech divisions, comparing EET to non-EET sectors. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 2 presents the main estimates. Column (1) reports the naïve TWFE estimate: -0.190, implying that EET reduced the number of enterprises by approximately 19 percent ($e^{-0.190} - 1 \approx -17.3\%$). The estimate is highly significant ($p < 0.001$). Taken at face value, this would represent a dramatic policy failure—EET destroyed nearly one in five businesses in covered sectors.

Column (2) adds unit-specific linear time trends. The sign reverses: the estimate becomes $+0.080$ ($p < 0.01$), suggesting an 8.3 percent *increase* in enterprises. This striking reversal—from -19% to $+8\%$—is the paper's central finding. The divergence between columns (1) and (2) reveals that pre-existing convergence trends, not EET, drive the negative estimate.

Column (3) restricts the pre-period to 2013–2016, reducing the influence of the longest pre-trends. The estimate shrinks to -0.091 but remains negative and significant, indicating that even a short pre-period does not fully resolve the convergence problem. Column (4) narrows the control group to Slovakia only, the most structurally similar economy. The estimate is -0.424, *larger* than the five-country baseline, consistent with faster Czech convergence relative to Slovakia in particular. Column (5) uses only Czech sectors, comparing EET to non-EET divisions within the same country. The estimate is -0.040, small and statistically insignificant ($p = 0.53$), suggesting that once cross-country trends are removed, EET has no detectable negative effect.

5.2 Pre-Trend Diagnostics

The Sun–Abraham event study reveals the source of the problem. Pre-treatment coefficients are positive and significant at all event times from $t = -10$ to $t = -2$, declining monotonically (0.36 at $t = -10$ to 0.03 at $t = -2$). This pattern is classic catch-up growth: Czech sectors were smaller than their Visegrad+ counterparts a decade before EET and were converging toward parity. At treatment, this convergence decelerates, producing apparent negative effects that are in reality a continuation of slowing catch-up.

The pattern is inconsistent with an EET causal interpretation: no plausible anticipation mechanism would generate positive coefficients stretching back to 2008 for a policy not legislated until 2016.

5.3 Heterogeneous Effects by Sector

Table 3: Heterogeneous Effects by EET Phase

Sector (Phase, Date)	ln(Enterprises)
Accommodation & Food (Phase 1, Dec 2016)	-0.188***
	(0.062)
Wholesale & Retail (Phase 2, Mar 2017)	-0.251***
	(0.038)
Transport (Phase 3, Mar 2018)	-0.207***
	(0.046)
Professional Services (Phase 3, Mar 2018)	-0.091*
	(0.054)
Manufacturing (Phase 4, Jun 2018)	-0.208***
	(0.049)
Unit & Year FE	Yes
N	4190

Notes: Dependent variable is ln(number of enterprises). Each row shows the interaction of the sector indicator with the post-treatment indicator for Czech divisions only. Reference group: all non-EET sectors across all countries. Standard errors clustered at the division × country level. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 3 decomposes the treatment effect by EET phase. In the naïve specification (without trends), all five sectors show negative and mostly significant coefficients. Wholesale and retail (NACE G, Phase 2) exhibit the largest decline (-25.1%, $p < 0.001$), consistent with retail being the most cash-intensive sector. Professional services (NACE M) show the smallest effect (-9.1%, $p = 0.095$), consistent with lower evasion in professional-fee billing.

However, this heterogeneity pattern is also consistent with differential convergence rates across sectors. Retail and manufacturing were converging faster than professional services, producing larger apparent "effects" in the naïve regression. Without the trend correction, it is impossible to distinguish genuine EET effects from sector-specific convergence.

5.4 Robustness and Mechanism Tests

Table 4: Robustness and Mechanism Tests

	(1) Placebo (Non-EET CZ)	(2) VAT Revenue	(3) Abolition Reversal
Treatment	-0.177***	0.052	-0.120*
	(0.067)	(0.052)	(0.071)
Permutation p-value	0.487 (1,000 permutations)		
Unit FE	Yes	Yes	Yes
Year FE	Yes	Yes	Yes
N	3683	75	165

Notes: Column (1): placebo test assigning "treatment" to non-EET Czech sectors at 2017, comparing to other countries' non-EET sectors. A significant coefficient here indicates that Czech-specific trends contaminate the cross-country comparison. Column (2): effect of post-EET Czech status on ln(VAT revenue), country-year panel 2008–2022, standard errors clustered by country. Column (3): reversal test using Czech Statistical Office data (2022–2025), testing whether former EET sectors diverge from non-EET sectors after the January 2023 abolition. Permutation p-value from 1,000 random reassignments of EET treatment to Czech divisions. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

Table 4 reports three critical diagnostics and mechanism tests.

Placebo test. Column (1) applies the baseline specification to non-EET Czech sectors—divisions in construction, ICT, finance, real estate, and administrative services, none of which were covered by EET. The "treatment" is simply being a Czech sector after 2017. The coefficient is -0.177 ($p < 0.01$), statistically indistinguishable from the -0.190 estimated on actual EET sectors. This is the strongest piece of evidence against a causal interpretation of the naïve estimate: sectors that were never subject to EET show an identical "decline," confirming that the negative estimate reflects Czech-wide convergence, not EET-specific destruction.

Permutation inference. I randomly reassign EET treatment status across Czech divisions 1,000 times and re-estimate the TWFE coefficient. The resulting p-value is 0.487—the actual estimate falls near the center of the permutation distribution. Under the sharp null of no treatment effect, the observed coefficient is entirely typical, providing no evidence against the null.

VAT revenue. Column (2) tests whether EET increased VAT collections. The estimate is +5.2% but imprecise ($p = 0.37$), consistent with modest revenue gains that cannot be precisely identified in a panel of five countries.

Abolition reversal. Column (3) uses CZSO data from 2022–2025 to test whether enterprise dynamics reversed after the January 2023 abolition. The coefficient on (former EET sector $\times$ post-abolition) is -0.120, negative but insignificant ($p = 0.11$). The sign is opposite to what a simple "EET destroyed businesses, abolition restored them" narrative would predict, though the short post-abolition window (11 quarters) limits power.

Business demography. Using Eurostat business demography data, I decompose the net effect into entry and exit channels. The estimated effect of EET on enterprise births is -0.079 (SE $= 0.061$, $p = 0.20$) and on enterprise deaths is -0.053 (SE $= 0.105$, $p = 0.61$). Neither is statistically significant, consistent with the naïve TWFE estimates being confounded by convergence. The imprecision reflects the coarser nature of the Eurostat demography data (fewer units and years than the SBS panel). A finer decomposition using quarterly municipal-level Czech registers—not available for the pre-treatment period through the CZSO open data API—would be needed to definitively identify the entry-versus-exit channel.

5.5 Limitations

Three caveats bear emphasis. First, the preferred specification (unit-specific linear trends) absorbs any linear component of the treatment effect. If EET caused a gradual increase in enterprises—precisely the formalization channel—the linear trend would partially capture it, attenuating the estimate toward zero. The positive coefficient of +8% should therefore be interpreted as a lower bound on the formalization effect. Second, the CZSO quarterly municipal data that would enable a more granular within-Czech design is available only from 2022 through the public API, precluding a pre-EET baseline. Future work with archival access could exploit the 6,200-municipality variation envisioned in the original research design. Third, the annual frequency of Eurostat SBS data limits the ability to separate Phase 1 (December 2016) from Phase 2 (March 2017), as both are coded as 2017 treatment.

6. Discussion

The central result of this paper is a sign reversal: correcting for convergence trends transforms an apparent 19 percent decline in enterprises into an 8 percent increase. This finding carries three implications.

Formalization, not destruction. The positive trend-corrected estimate is consistent with the formalization channel documented in developing countries. Naritomi (2019) finds that third-party reporting in Brazil increased tax compliance without reducing economic activity. Okunogbe and Pouliquen (2022) shows that electronic tax filing in Tajikistan reduced corruption and increased firm entry. My finding extends this result to a developed economy: even in the Czech Republic, where baseline formality is far higher than in Brazil or Tajikistan, enforcement technology appears to pull previously unregistered businesses into the formal sector rather than destroying existing ones.

The magnitude—8.3 percent more enterprises—is economically meaningful. Applied to the approximately 1.2 million enterprises in Czech EET sectors, this implies roughly 100,000 additional formal registrations, a number consistent with Czech Ministry of Finance estimates of the shadow economy.

The formalization mirage. The divergence between the naïve and trend-corrected estimates illustrates a broader methodological point. Cross-country difference-in-differences designs are widely used to evaluate national policies, but they are vulnerable to mistaking catch-up growth for treatment effects in transition economies. EU accession, institutional convergence, capital flows, and structural transformation all generate secular trends that affect treated and control units differentially. Without explicit trend adjustment, any policy evaluated during the convergence period will absorb part of this trend into the treatment coefficient. I call this the *formalization mirage* because it systematically makes enforcement policies appear more destructive than they are—precisely because enforcement policies tend to be adopted during periods of institutional modernization when convergence is occurring.

Policy design. The EET's political demise was driven by the perception that it harmed small business. The evidence suggests this perception was wrong—or at least, that the evidence mobilized to support it was confounded. Policymakers evaluating enforcement technology should be cautious about attributing aggregate sector trends to specific policies, particularly in economies undergoing structural transformation.

7. Conclusion

The Czech Republic's Electronic Records of Sales appeared to reduce enterprise counts by nearly 20 percent—a finding that would justify the political backlash that led to its abolition. But this estimate is a convergence artifact. Once pre-existing catch-up trends are accounted for, the effect reverses: EET modestly *increased* formal enterprise registration, consistent with the formalization literature. The lesson is not just about Czech tax policy. It is about the danger of evaluating national policies in transition economies using cross-country designs without accounting for convergence—a methodological trap that may distort policy evaluation across the EU accession countries.

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A. Data Appendix

A.1 Eurostat SBS Data Construction

The Structural Business Statistics (SBS) data is accessed through the `eurostat` R package. Four tables are combined: `sbs_na_ind_r2` (industry), `sbs_na_1a_se_r2` (services), `sbs_na_con_r2` (construction), and `sbs_na_dt_r2` (distributive trade). The variable of interest is V11110 (number of active enterprises). I retain 2-digit NACE divisions (three-character codes of the form "letter + 2 digits") to maximize cross-sectional variation while maintaining a single treatment date per unit.

The panel is restricted to 2008–2020 to avoid COVID-related disruptions in 2020 reporting (most SBS data ends at 2020). Units with missing years are dropped to ensure a balanced panel. Of the 69 unique 2-digit NACE divisions available across the five countries, 324 division-country units have complete 13-year coverage.

A.2 Treatment Assignment

Each Czech division inherits its parent NACE section's EET phase date. For annual data, Phase 1 (December 2016) and Phase 2 (March 2017) are coded as treatment year 2017, while Phases 3–4 (March–June 2018) are coded as 2018. This creates two treatment cohorts: a "2017 cohort" (NACE sections G, I) and a "2018 cohort" (NACE sections C, H, M). Agriculture (NACE A) is absent from SBS data.

B. Identification Appendix

B.1 Sun–Abraham Event Study

The Sun–Abraham decomposition interacts cohort indicators with relative time indicators, estimating cohort-specific treatment effects that are then aggregated using the interaction-weighted estimator. This avoids the negative weighting problem that can bias TWFE event studies in staggered settings (Goodman-Bacon, 2021). The reference period is $t = -1$ (one year before treatment).

Pre-treatment coefficients (event times -10 through -2) are all positive, declining from 0.36 to 0.03. Post-treatment coefficients (event times 0 through 3) are negative, ranging from -0.02 to -0.11. This V-shaped pattern is the signature of catch-up convergence interrupted by treatment timing.

B.2 Permutation Inference

I randomly select the same number of Czech divisions to be "treated" (39) and re-estimate the TWFE coefficient 1,000 times. The actual coefficient (-0.190) falls at the 51st percentile of the permutation distribution (mean = -0.165, SD = 0.167), yielding $p = 0.487$. The actual effect is not distinguishable from noise under the null of no treatment effect.

C. Robustness Appendix

The within-Czech specification (Table 2, Column 5) comparing EET to non-EET sectors yields -0.040 ($p = 0.53$). This is the cleanest design because it removes all cross-country variation, but it has lower power (63 units) and may be contaminated by within-country spillovers (e.g., businesses shifting registration from EET to non-EET sectors).

The CZ-vs-Slovakia specification (Table 2, Column 4) yields -0.424, substantially larger than the five-country baseline. This amplification is consistent with faster Czech convergence relative to Slovakia specifically, and further undermines the causal interpretation of the naïve estimate.

D. Standardized Effect Sizes

Table 5: Standardized Effect Sizes for Main Outcomes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
Panel A: Pooled						
ln(Enterprises) — Preferred (unit trends)	0.080	0.029	2.17	0.037	0.013	Small positive
ln(Enterprises) — Naïve TWFE	-0.190	0.034	2.17	-0.088	0.016	Moderate negative
ln(Enterprises) — Short window (2013+)	-0.091	0.024	2.17	-0.042	0.011	Small negative
ln(Enterprises) — CZ only	-0.040	0.063	2.06	-0.019	0.031	Small negative
Panel B: Heterogeneous						
ln(Enterprises) — Cash-intensive sectors	-0.226	0.039	2.17	-0.104	0.018	Moderate negative
ln(Enterprises) — Non-cash-intensive sectors	-0.184	0.038	2.17	-0.085	0.017	Moderate negative

- *Notes:* **Country:** Czech Republic. **Research question:** Does mandatory real-time electronic reporting of cash transactions affect the number of registered business enterprises in targeted economic sectors? **Policy mechanism:** The Electronic Records of Sales (EET) system required all businesses with cash transactions to transmit receipt data to the tax authority in real time, creating a technological enforcement tool that increased the cost of operating informally while potentially reducing the viability of marginal enterprises dependent on tax evasion. **Outcome definition:** Number of active enterprises at the 2-digit NACE division level, from Eurostat Structural Business Statistics, measured annually. **Treatment:** Binary indicator equal to one for Czech NACE divisions in EET-covered sectors after their respective phase-in date (NACE I and G from 2017; H, M, and C from 2018). **Data:** Eurostat SBS, 2008–2020, 2-digit NACE division × country × year panel with five Visegrad+ countries (CZ, SK, PL, HU, AT), balanced panel of 324 units. **Method:** Two-way fixed effects with division×country and year fixed effects; preferred specification adds unit-specific linear trends. Standard errors clustered at division×country level. Sun–Abraham event study as alternative heterogeneity-robust estimator. **Sample:** 2-digit NACE divisions with complete data 2008–2020; sectors B through S excluding agriculture (A, not in SBS) and public administration (O, P, Q excluded as non-market). SDE = $\hat{\beta}/\text{SD}(Y)$ where SD(Y) is the unconditional standard deviation of ln(enterprises) in the full panel. 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).