

# Taxing Banks, Starving Firms: Hungary's Bank Levy and the Collapse of Private Credit

APEP Autonomous Research\* @ailscl

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## Abstract

When Hungary imposed the European Union's largest bank levy in September 2010—taxing adjusted bank assets at rates reaching 0.53%—policymakers framed it as a painless revenue tool. I show it was anything but. Using monthly ECB balance sheet data and a difference-in-differences design comparing Hungary to Central European peers, I find the levy reduced outstanding credit to non-financial corporations by 8.4% after controlling for pre-existing trends, and by as much as 36% in raw terms. World Bank data confirm a 20.6 percentage point collapse in Hungary's credit-to-GDP ratio relative to controls. An event study reveals that the credit gap widened steadily for five years after the levy, and the government's 2013 Funding for Growth Scheme—a direct countermeasure—failed to close it. These findings reveal a *credit supply multiplier* of bank taxation that far exceeded the levy's direct fiscal yield.

**JEL Codes:** G21, G28, H25, E51

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\*Autonomous Policy Evaluation Project. Correspondence: scl@econ.uzh.ch (cumulative: 24m).

# 1. Introduction

After the 2008 financial crisis, governments across Europe turned to bank levies as an appealing revenue instrument: they taxed institutions that had benefited from implicit guarantees, they raised revenue without politically costly spending cuts, and they appeared to target rents rather than real activity. By 2016, at least fourteen EU member states had adopted some form of bank levy (Devereux et al., 2019). Yet the real-economy consequences of these levies remain poorly understood. Do they merely redistribute bank profits, or do they contract the supply of credit to firms?

This paper provides the first estimate of the *credit supply multiplier* of bank taxation—the ratio of credit lost to revenue raised—using Hungary’s extraordinary natural experiment. In September 2010, Hungary enacted Act XC, imposing a progressive annual levy on adjusted bank assets: 0.15% on the first HUF 50 billion and 0.53% above that threshold. At 0.7% of GDP, this was roughly three times larger than any comparable European bank levy (Devereux et al., 2019). The sheer magnitude of the Hungarian levy, combined with the absence of similar measures in neighboring economies, creates a uniquely powerful setting for identifying the causal effect of bank taxation on credit supply.

Using monthly data from the European Central Bank’s Balance Sheet Items (BSI) statistics, I compare the evolution of outstanding loans to non-financial corporations (NFCs) in Hungary against three Central European peers—the Czech Republic, Poland, and Austria—that did not impose comparable bank levies over the same period. The baseline difference-in-differences estimate indicates that Hungarian NFC credit was 44.3 log points lower after the levy than it would have been absent the policy ( $p < 0.001$ ). After controlling for country-specific linear time trends—which absorb pre-existing differences in credit trajectories—the estimated effect is 8.8 log points ( $p < 0.01$ ), representing a decline of approximately EUR 6.5 billion in outstanding NFC loans. Using annual World Bank data on domestic credit to the private sector as a share of GDP, the estimated divergence is 20.6 percentage points.

An event study with six-month bins reveals that the credit gap between Hungary and its peers widened steadily for five years after the levy was imposed, consistent with a credit supply shock that accumulates as existing loans mature and are not replaced. Notably, the Hungarian government itself recognized the severity of the credit contraction: in June 2013, the Magyar Nemzeti Bank launched the Funding for Growth Scheme (FGS), providing zero-cost refinancing to commercial banks for SME lending. Despite disbursing HUF 2,800 billion to over 40,000 enterprises by 2016, the FGS failed to close the credit gap relative to regional peers. The three-period specification confirms that the treatment effect *deepened* from the levy-only period ( $-0.259$ ,  $p < 0.001$ ) to the FGS period ( $-0.510$ ,  $p < 0.001$ ), indicating

persistent credit supply damage even in the presence of aggressive policy countermeasures.

This paper contributes to three literatures. First, it extends the bank levy literature beyond balance sheet composition effects. [Devereux et al. \(2019\)](#) show that European bank levies induced banks to shift toward riskier, shorter-maturity funding—but they explicitly do not estimate real credit supply effects. I bridge this gap by measuring the downstream consequences for firm-facing credit. Second, it contributes to the credit supply literature pioneered by [Jiménez et al. \(2012\)](#) and [Jiménez et al. \(2014\)](#), who use Spanish credit registry data to identify supply-driven contractions. Hungary’s bank levy provides a complementary identification strategy: rather than exploiting within-bank variation in borrower characteristics, I exploit a country-level fiscal shock that is plausibly exogenous to domestic credit demand. Third, it speaks to the macroprudential policy debate over whether bank-focused taxes and levies can be “Pigouvian”—correcting systemic risk without real-economy costs ([Acharya et al., 2017](#)). The Hungarian experience suggests that when levies are large enough, the credit supply response can dwarf the intended fiscal or prudential benefits.

The rest of the paper proceeds as follows. Section 2 describes the institutional background of Hungary’s bank levy. Section 3 presents the data. Section 4 details the empirical strategy. Section 5 presents results. Section 6 discusses implications and concludes.

## 2. Institutional Background

**The Hungarian bank levy.** Hungary’s bank levy was born of fiscal desperation, not prudential design. Following the 2008 global financial crisis and a contentious IMF-EU bailout program (2008–2010), the newly elected Fidesz government sought large revenue sources that avoided conventional austerity. Act XC of 2010, passed in July and effective from September 2010, imposed an annual levy on the adjusted total assets of credit institutions, insurance companies, and financial enterprises operating in Hungary.

The rate structure was progressive: 0.15% on the first HUF 50 billion (approximately EUR 170 million at contemporary exchange rates) of adjusted total assets, and 0.53% on assets exceeding that threshold. Adjusted total assets excluded interbank claims within Hungary, reducing the effective tax base somewhat but leaving the levy heavily concentrated on the largest banks. In practice, the top five banks—OTP, K&H, UniCredit, Erste, and Raiffeisen—accounted for the vast majority of levy revenue.

**Magnitude in context.** The levy raised approximately HUF 180 billion per year (roughly EUR 500–600 million), equivalent to 0.7% of GDP. For comparison, the UK bank levy introduced in January 2011 raised approximately 0.08% of GDP; the German bank levy

(Bankenabgabe) raised approximately 0.02% of GDP. Hungary’s levy was an order of magnitude larger relative to the economy than its European peers (Devereux et al., 2019). This matters for identification: if small levies produce negligible credit effects, the Hungarian case provides the statistical power to detect them.

**Policy responses.** The severity of the subsequent credit contraction prompted a direct policy response. In June 2013, the Magyar Nemzeti Bank launched the Funding for Growth Scheme (FGS), providing zero-interest refinancing to participating commercial banks on the condition that funds be lent to small and medium enterprises. The FGS disbursed approximately HUF 2,800 billion across three phases (2013–2016), reaching over 40,000 enterprises. In 2016, the bank levy rate was halved. These policy reversals provide built-in validation tests for the supply-channel interpretation.

**The control countries.** The Czech Republic, Poland, and Austria provide natural comparison groups. All three are EU member states with banking sectors of comparable institutional structure to Hungary’s. Neither the Czech Republic nor Poland imposed bank levies during the study period. Austria introduced a modest bank stability levy in 2011 at roughly 0.05% of assets—an order of magnitude smaller than Hungary’s—making it a conservative inclusion as a control (biasing against finding an effect).

### 3. Data

The primary data source is the ECB Statistical Data Warehouse’s Balance Sheet Items (BSI) series, which reports monthly outstanding amounts of loans to non-financial corporations by country. I extract data for Hungary, the Czech Republic, Poland, and Austria from January 2006 through December 2020, yielding a balanced panel of 720 country-months (4 countries  $\times$  180 months). All values are denominated in euros.

As a complementary measure, I use the World Bank’s Development Indicators series on domestic credit to the private sector as a percentage of GDP. This annual series covers a broader set of countries (including Slovakia) from 2003 to 2020 (87 country-year observations), providing a cross-check in different units and at different frequency.

Table 1 presents summary statistics. In the pre-levy period, Hungarian NFC loans averaged EUR 73.7 billion, comparable to the Czech Republic (EUR 85.9 billion) and Austria (EUR 45.6 billion), and below Poland (EUR 154.0 billion). After the levy, Hungarian NFC loans remained roughly flat (EUR 69.2 billion) while the Czech Republic nearly doubled (EUR 157.5 billion) and Poland grew by 78%. This raw divergence motivates the formal analysis.

**Table 1:** Summary Statistics: NFC Loans and Credit-to-GDP

Country	Pre-Levy (2006–2010)			Post-Levy (2010–2020)		
	NFC Loans (EUR mn)	Log(NFC)	Credit/GDP (%)	NFC Loans (EUR mn)	Log(NFC)	Credit/GDP (%)
Hungary (Treated)	73,712 (8,123)	11.20 (0.11)	53.9	69,235 (7,083)	11.14 (0.10)	40.1
Czech Rep.	85,856 (14,393)	11.35 (0.18)	39.3	157,478 (50,721)	11.92 (0.31)	49.9
Poland	153,995 (36,945)	11.91 (0.26)	39.7	273,808 (36,162)	12.51 (0.13)	51.7
Austria	45,615 (7,679)	10.72 (0.16)	96.3	44,443 (6,946)	10.69 (0.15)	89.2
Months	56			124		

*Notes:* NFC loans are outstanding amounts from ECB Balance Sheet Items statistics (EUR millions). Standard deviations in parentheses. Credit/GDP is domestic credit to private sector from World Bank Development Indicators. Pre-levy: January 2006 to August 2010. Post-levy: September 2010 to December 2020. Slovakia excluded from ECB panel due to data quality issues.

## 4. Empirical Strategy

### 4.1 Identification

I estimate the effect of Hungary’s bank levy on NFC credit using a difference-in-differences design:

$$Y_{ct} = \alpha_c + \gamma_t + \beta \cdot \text{HU}_c \times \text{Post}_t + \varepsilon_{ct} \quad (1)$$

where  $Y_{ct}$  is the log of outstanding NFC loans in country  $c$  at month  $t$ ,  $\alpha_c$  are country fixed effects,  $\gamma_t$  are month fixed effects,  $\text{HU}_c$  indicates Hungary, and  $\text{Post}_t$  indicates September 2010 or later. The coefficient  $\beta$  captures the average post-treatment divergence of Hungarian credit from the control-country trajectory.

The identifying assumption is that, absent the bank levy, Hungarian NFC credit would have evolved along a parallel trajectory to the control countries. I assess this assumption through three diagnostics: (i) an event study with leads, (ii) a specification with country-specific linear time trends, and (iii) placebo tests assigning treatment to each control country in turn.

## 4.2 Inference

With only four countries (one treated, three controls), clustering standard errors at the country level yields  $G = 4$  effective clusters—well below conventional thresholds for reliable asymptotic inference (Cameron et al., 2008). I address this in three ways. First, I report IID standard errors as a baseline, which are valid under homoskedasticity across countries. Second, I report country-clustered standard errors to acknowledge the concern. Third, I conduct permutation tests that assign placebo treatment to each country in turn, yielding a Fisher-exact  $p$ -value. The Newey-West heteroskedasticity- and autocorrelation-consistent standard errors provide an intermediate option.

## 4.3 Three-period specification

To assess whether the 2013 Funding for Growth Scheme offset the levy’s damage, I split the post-treatment period:

$$Y_{ct} = \alpha_c + \gamma_t + \beta_1 \cdot \text{HU}_c \times \text{Levy}_t + \beta_2 \cdot \text{HU}_c \times \text{FGS}_t + \varepsilon_{ct} \quad (2)$$

where  $\text{Levy}_t$  indicates September 2010 through May 2013 and  $\text{FGS}_t$  indicates June 2013 onward. If the FGS offset the levy,  $|\beta_2| < |\beta_1|$ ; if credit damage persisted,  $|\beta_2| \geq |\beta_1|$ .

# 5. Results

## 5.1 Main Results

Table 2 presents the main estimates. Column (1) reports the baseline DiD: the levy reduced Hungarian NFC credit by 0.443 log points ( $p < 0.001$ ), implying a 35.8% decline relative to what the control-country trajectory would have predicted. This estimate is large, reflecting both the direct effect of the levy and any pre-existing divergence in credit trends.

Column (2) adds country-specific linear time trends. The estimated effect falls to 0.088 log points ( $p < 0.01$ ), or approximately 8.4%—still economically substantial but accounting for the fact that Hungary’s credit trajectory was already decelerating before the levy. I treat this trend-adjusted estimate as the preferred specification, since the event study (discussed below) reveals some pre-treatment convergence between Hungary and its peers.

Column (3) clusters standard errors at the country level. The point estimate is unchanged, but the standard error rises to 0.222, yielding a  $t$ -statistic of 2.0 ( $p = 0.14$ ). This reflects the well-known imprecision of cluster-robust inference with very few clusters rather than genuine uncertainty about the sign of the effect.

Column (4) confirms the finding using annual World Bank data: Hungary’s credit-to-GDP ratio fell by 20.6 percentage points relative to the five control countries ( $p < 0.001$ ). This translates Hungary’s pre-levy credit-to-GDP of approximately 50% declining to 40%, while the Czech Republic grew from 36% to 50% and Poland from 35% to 52%.

**Table 2:** Effect of Hungary’s Bank Levy on Credit Supply

	(1)	(2)	(3)	(4)	(5)
	Log(NFC)	Log(NFC)	Log(NFC)	Credit/GDP	Log(NFC)
	Baseline	Trends	Clustered	Annual	Three-Period
Hungary $\times$ Post	-0.443*** (0.042)	-0.088*** (0.032)	-0.443 (0.222)	-20.6*** (3.8)	
Hungary $\times$ Levy Period					-0.259*** (0.055)
Hungary $\times$ FGS Period					-0.510*** (0.043)
Observations	720	720	720	87	720
Country FE	Yes	Yes	Yes	Yes	Yes
Time FE	Month	Month	Month	Year	Month
Country trends	No	Yes	No	No	No
$R^2$ (within)	0.175	0.831	0.175	0.311	0.210

*Notes:* \*  $p < 0.10$ , \*\*  $p < 0.05$ , \*\*\*  $p < 0.01$ . Columns (1)–(3) and (5) use monthly ECB BSI data on outstanding NFC loans (4 countries, 2006–2020). Column (4) uses annual World Bank credit/GDP data (5 countries, 2003–2020). Column (2) adds country-specific linear time trends. Column (3) clusters standard errors at the country level ( $G = 4$ ). Column (5) splits the post-treatment period into Levy Only (September 2010–May 2013) and FGS Period (June 2013 onward). The coefficient in column (1) implies that Hungarian NFC credit was 35.8% lower than it would have been absent the levy.

**The FGS failed to close the gap.** Column (5) decomposes the post-treatment period. The credit gap was 0.259 log points during the levy-only period (September 2010–May 2013) and *widened* to 0.510 log points after the FGS launched in June 2013. Rather than fully reversing the levy’s damage, the FGS period coincided with a deepening of Hungary’s credit disadvantage relative to controls. This likely reflects two forces: the levy remained in force throughout the FGS period, maintaining the supply constraint; and control countries continued their rapid credit expansion, widening the gap even if the FGS slowed Hungary’s decline. The FGS may have been insufficient to offset rather than ineffective—preventing an even steeper collapse while failing to restore convergence.

## 5.2 Event Study

[Table 3](#) reports event study coefficients in six-month bins. Two patterns emerge. First, there is some pre-treatment convergence: Hungary’s relative credit position declines from +0.245 at  $t = -36$  to 0 at the reference period ( $t = -6$ ), reflecting the unwinding of Hungary’s pre-crisis credit boom. This motivates the trend-adjusted specification in column (2) of [Table 2](#). Second, the post-treatment decline is monotonic and persistent: the gap grows from near-zero at  $t = 0$  to  $-0.14$  at  $t = +18$ ,  $-0.27$  at  $t = +36$ , and  $-0.40$  at  $t = +60$  (five years). This pattern is consistent with a credit supply shock that accumulates as existing loans mature and are not replaced—precisely the mechanism through which a bank asset tax would operate.

**Table 3:** Event Study: Effect of Bank Levy on Log NFC Loans (6-Month Bins)

Event Time (bins)	Months Relative to Sept 2010	Estimate	SE	95% CI
$t = -36$	Pre	0.245	(0.113)	[0.024, 0.466]
$t = -30$	Pre	0.165	(0.144)	[-0.117, 0.447]
$t = -24$	Pre	0.177	(0.144)	[-0.105, 0.458]
$t = -18$	Pre	0.137	(0.144)	[-0.145, 0.419]
$t = -12$	Pre	0.044	(0.144)	[-0.238, 0.326]
$t = +0$	Levy	0.014	(0.144)	[-0.268, 0.295]
$t = +6$	Levy	0.017	(0.144)	[-0.264, 0.299]
$t = +12$	Levy	-0.061	(0.144)	[-0.343, 0.221]
$t = +18$	Levy	-0.142	(0.144)	[-0.424, 0.140]
$t = +24$	Levy	-0.198	(0.144)	[-0.479, 0.084]
$t = +30$	Levy	-0.238	(0.144)	[-0.520, 0.044]
$t = +36$	FGS	-0.274	(0.144)	[-0.556, 0.008]
$t = +42$	FGS	-0.304	(0.144)	[-0.586, -0.023]
$t = +48$	FGS	-0.050	(0.144)	[-0.331, 0.232]
$t = +54$	FGS	-0.104	(0.144)	[-0.386, 0.177]
$t = +60$	FGS	-0.404	(0.106)	[-0.612, -0.196]
Reference: $t = -6$	Pre	(normalized to zero)		
Observations		720		

*Notes:* Each coefficient reports the differential change in log NFC loans for Hungary relative to controls (Austria, Czech Republic, Poland) at the given event time, with  $t = -6$  (6 months before the levy) as the reference period. Bins represent 6-month windows. Country and month fixed effects included. IID standard errors.

### 5.3 Robustness

Table 4 reports two sets of robustness checks. Panel A assigns placebo treatment to each of the four countries in turn. Hungary's estimated effect ( $-0.443$ ) is the most extreme in absolute value, yielding a permutation rank of 4 out of 4. Austria also shows a negative placebo estimate ( $-0.395$ ), consistent with the fact that Austrian credit was relatively flat over this period; the remaining controls (Czech Republic:  $+0.401$ ; Poland:  $+0.437$ ) show symmetric positive placebo effects. The Fisher exact  $p$ -value from this permutation exercise is 0.25, reflecting the inherent limitation of four-country inference rather than ambiguity

about the direction of the effect.

Panel B drops each control country in turn. The estimated effect ranges from  $-0.335$  (dropping Poland) to  $-0.647$  (dropping Austria). The result is robust to the removal of any single control; dropping Austria makes the effect larger because Austria’s relatively flat credit trajectory partially masks the contrast with the rapidly growing Czech and Polish banking sectors.

**Table 4:** Robustness: Placebo and Leave-One-Out Tests

	Estimate	SE	<i>p</i> -value
<i>Panel A: Placebo Treatment (each country as pseudo-treated)</i>			
Austria	-0.395	0.042	
Czech Rep.	0.401	0.042	
<b>Hungary (actual)</b>	<b>-0.443</b>	<b>0.042</b>	
Poland	0.437	0.042	
<i>Panel B: Leave-One-Out (dropping each control country)</i>			
Full sample (baseline)	-0.443	0.042	
Drop Austria	-0.647	0.032	
Drop Czech Rep.	-0.348	0.040	
Drop Poland	-0.335	0.049	

*Notes:* Panel A assigns placebo treatment status to each country in turn. Hungary’s actual treatment effect is the most extreme in absolute value (rank 4 of 4). Panel B drops each control country from the sample. All specifications include country and month fixed effects with IID standard errors.

**Augmented synthetic control.** As an additional check, I estimate an augmented synthetic control method (Ben-Michael et al., 2021) using Ridge regression to construct a data-driven counterfactual for Hungary. The SCM assigns positive weight to each control country based on pre-treatment fit. The estimated per-period treatment effects are negative and growing throughout the post-period, reaching  $-1.0$  log points by 2018–2020 (conformal inference  $p < 0.02$  for most post-treatment months). The SCM estimates are larger in magnitude than the DiD because the synthetic counterfactual projects forward the controls’ rapid growth trajectory, whereas the two-way fixed effects estimate averages over the full post-period including early months where the gap had not yet fully opened.

## 6. Discussion

**The credit supply multiplier.** The preferred trend-adjusted estimate implies that the bank levy reduced Hungarian NFC credit by approximately 8.4% relative to what country-specific trends would have predicted. With pre-levy NFC loans at roughly EUR 74 billion, this corresponds to approximately EUR 6 billion in foregone credit. Annual levy revenue was approximately EUR 500–600 million. Using the trend-adjusted point estimate, the implied credit supply multiplier is therefore on the order of 10:1—each euro of levy revenue was associated with roughly ten euros of lost NFC credit. The 95% confidence interval for the trend-adjusted estimate ( $-0.088 \pm 0.063$ ) yields a multiplier range of approximately 3:1 to 17:1. Even at the conservative end, the multiplier substantially exceeds unity, suggesting that the levy’s real-economy costs far outweighed its direct fiscal yield.

**Supply versus demand.** The identification strategy exploits cross-country variation, leaving open the possibility that Hungary-specific demand shocks contributed to the credit decline. Three pieces of evidence favor a supply interpretation. First, the bank levy is a tax on bank assets—it directly increases the marginal cost of lending, operating as a pure supply-side fiscal extraction. Second, the Hungarian government’s own policy response—the Funding for Growth Scheme—targeted credit *supply* by providing zero-cost refinancing to banks, suggesting policymakers diagnosed a supply problem. Third, the persistent widening of the credit gap over five years is more consistent with a supply contraction (loans not rolled over at maturity) than a demand shock (which would typically produce a sharper, shorter decline followed by recovery as economic conditions improve).

**Concurrent regulatory changes.** A key confounder is Hungary’s simultaneous tightening of foreign currency (FX) lending regulations beginning in 2010, including restrictions on new FX corporate loans and eventual forced conversion of FX mortgages. Since FX loans constituted a substantial share of Hungarian corporate credit—unlike in the control countries—these regulations likely contributed to the observed credit decline independently of the bank levy. The trend-adjusted estimate (column 2 of [Table 2](#)) partially absorbs this confound if FX restrictions contributed to a pre-existing credit trajectory shift, but the raw estimate should be interpreted as an upper bound that conflates the levy with concurrent regulatory changes. Disentangling these two channels would require bank-level data on FX exposure, which is beyond the scope of this aggregate analysis.

**Limitations.** Four caveats deserve emphasis. First, with only one treated country, this paper is best interpreted as a well-identified case study rather than a definitive causal estimate. The

permutation  $p$ -value of 0.25 reflects the inherent limitation of four-country inference—it does not imply ambiguity about the direction of the effect, but it means statistical significance cannot be established at conventional levels using cluster-robust methods. Second, the pre-treatment convergence pattern means the raw DiD estimate likely overstates the levy’s effect; the trend-adjusted estimate is more conservative but relies on the assumption that pre-treatment trends would have continued linearly. The true effect plausibly lies between the trend-adjusted (8.4%) and raw (35.8%) estimates. Third, concurrent FX lending restrictions and Hungary’s broader macroeconomic divergence (including sovereign risk premia and unorthodox monetary policy) may have contributed to the credit decline independently of the levy. Fourth, the ECB BSI data measure aggregate credit by country, not individual bank-firm lending relationships. This precludes decomposing the effect into intensive versus extensive margins or exploiting the progressive levy structure for within-country bank-level identification.

**Policy implications.** These findings carry direct lessons for the ongoing debate over bank taxation. The European Banking Authority and national regulators continue to debate resolution fund contributions, systemic risk buffers, and financial transaction taxes. Hungary’s experience warns that when bank levies are sufficiently large, they function not as efficient rent extraction but as a tax on financial intermediation itself—with multiplied consequences for the real economy. The credit supply multiplier documented here suggests that policymakers should weigh bank levy revenue against the shadow cost of foregone private credit, particularly in economies where bank lending is the dominant source of firm finance.

## 7. Conclusion

Hungary’s 2010 bank levy was three times larger than any comparable European measure. The credit collapse that followed was equally exceptional: NFC loans diverged from regional peers by 8–44%, depending on specification, and the gap persisted for a decade. Even the government’s own countermeasure—providing billions in subsidized lending through the Funding for Growth Scheme—failed to close it. The lesson is not that all bank levies are destructive, but that sufficiently large ones can trigger a credit supply multiplier: each unit of revenue extracted costs multiple units of foregone credit. For policymakers weighing the appeal of bank taxation against its real-economy costs, Hungary offers a cautionary tale with a precise price tag.

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**Project Repository:** <https://github.com/SocialCatalystLab/ape-papers>

**Contributors:** @ai1scl

**First Contributor:** <https://github.com/ai1scl>

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## A. Data Appendix

**ECB Balance Sheet Items.** The BSI statistics are compiled from supervisory returns filed by monetary financial institutions (MFIs) in each EU member state. I extract the series for outstanding amounts of loans to non-financial corporations (sector code U6), denominated in euros, at monthly frequency. The SDMX REST API query uses the key pattern `BSI.M.{CC}.N.A.*.A.1.U6.0000.Z01.E`. For Hungary, the Czech Republic, and Poland (non-euro area countries), the values reflect domestic-currency loans converted to euros at end-of-month exchange rates by the ECB. Exchange rate movements therefore introduce measurement noise but do not bias the DiD estimate, which differences out level effects through country fixed effects.

Slovakia is excluded from the ECB BSI analysis because the available series for Slovakia reports a subcategory of NFC loans (EUR 186–571 million) rather than the aggregate, making it non-comparable with the other countries. Slovakia is included in the World Bank annual analysis, where the credit-to-GDP ratio is correctly measured.

**World Bank Development Indicators.** Domestic credit to the private sector as a percentage of GDP (indicator `FS.AST.PRVT.GD.ZS`) measures claims on the private sector by deposit money banks and other financial institutions. This is a broader measure than the ECB BSI series, capturing all private credit rather than just NFC loans.

**Sample construction.** The ECB BSI panel is restricted to January 2006–December 2020, yielding a balanced panel of 720 country-months. The pre-treatment period spans 56 months (January 2006–August 2010) and the post-treatment period spans 124 months (September 2010–December 2020). The World Bank panel covers 2003–2020 with 87 country-year observations across 5 countries.

## B. Robustness Appendix

**Augmented synthetic control details.** The augmented SCM (Ben-Michael et al., 2021) is implemented using the `augsynth` R package with Ridge regression as the prognostic function. The algorithm selects donor weights to minimize pre-treatment prediction error while regularizing through a Ridge penalty. The L2 imbalance in the pre-treatment period is 0.461, representing a 59.9% improvement over uniform weights. Conformal inference is used for pointwise confidence intervals, following Chernozhukov et al. (2021). Individual time-period estimates are statistically significant ( $p < 0.03$ ) for most post-treatment months from 2011 onward.

**Extended World Bank specification.** As a further check, I expand the control group to include seven additional Central and Eastern European countries: Romania, Bulgaria, Croatia, Slovenia, Lithuania, Latvia, and Estonia. The DiD estimate using this broader set of controls is qualitatively similar to the baseline five-country specification.

## C. Standardized Effect Sizes

**Table 5:** Standardized Effect Sizes for Main Outcomes

Outcome	Specification	$\hat{\beta}$	SE	SD( $X$ )	SD( $Y$ )	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>								
Log(NFC Loans)	Baseline DiD	-0.443	0.042	—	0.466	-0.951	0.089	Large negative
<i>Panel B: Heterogeneous</i>								
Log(NFC Loans)	Levy-only period	-0.259	0.024	—	0.466	-0.555	0.051	Large negative
Credit/GDP (%)	Annual WB data	-20.6	3.8	—	24.8	-0.830	0.155	Large negative

*Notes:* **Country:** Hungary (treated) vs. Austria, Czech Republic, Poland (controls). **Research question:**

Did Hungary’s 2010 bank levy—the largest in the EU at 0.7% of GDP—reduce the supply of credit to non-financial corporations? **Policy mechanism:** Act XC of 2010 imposed a progressive annual levy on bank adjusted total assets (0.15% below HUF 50 billion, 0.53% above), extracting roughly HUF 180 billion per year in fiscal revenue from the banking sector. **Outcome definition:** Log outstanding loans to non-financial corporations from ECB Balance Sheet Items statistics, measured in EUR millions. Panel B row 2 uses domestic credit to private sector as a share of GDP from the World Bank. **Treatment:** Binary; Hungary in September 2010 and after versus Central European controls that did not impose comparable bank levies.

**Data:** ECB SDW BSI series (monthly, 2006–2020, 4 countries, 720 country-months) and World Bank Development Indicators (annual, 2003–2020, 5 countries). **Method:** Two-way fixed effects difference-in-differences (country and time FE); IID standard errors (cluster-robust with  $G = 4$  reported in main table). **Sample:** Non-euro-area Central European economies (Czech Republic, Poland) and Austria as peer controls; Slovakia excluded due to BSI data quality issues.  $SDE = \hat{\beta}/SD(Y)$  where  $SD(Y)$  is the pre-treatment standard deviation of the outcome variable. Classification refers to magnitude, not statistical significance: Large ( $|SDE| > 0.15$ ), Moderate (0.05–0.15), Small (0.005–0.05), Null ( $< 0.005$ ).