

The Mandate Gap: Financial Inclusion Directives and the Limits of Rights-Based Banking Policy

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Abstract

Over 20 million EU adults lacked bank accounts in 2014. The Payment Accounts Directive (2014/92/EU) mandated universal access, yet 18 of 27 member states missed the deadline. Exploiting staggered adoption, I apply Callaway-Sant'Anna difference-in-differences to provide the first causal evaluation. Including never-treated countries with pre-existing laws, transposition is associated with 5.3 percentage points lower internet banking ($p < 0.05$), with clean pre-trends. However, restricting to only not-yet-treated comparisons reduces the estimate to -1.0 ($p > 0.30$), revealing that the effect reflects faster digital convergence in Central European early-movers. The EU-wide mandate arrived too late to add value beyond organic convergence.

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1. Introduction

In September 2016, a remarkable bureaucratic milestone passed almost unnoticed: only one-third of EU member states had implemented a directive guaranteeing every resident the right to a basic bank account. The Payment Accounts Directive (2014/92/EU), adopted two years earlier to combat financial exclusion, was being ignored by the very governments that had voted for it. By the time Romania—the last holdout—transposed the directive in December 2017, the policy debate had moved on to digital payments and cryptocurrency regulation. Whether the mandate actually worked remained an open question.

This paper provides the first causal evaluation of the EU Payment Accounts Directive (PAD). The directive, adopted in July 2014 with a September 2016 transposition deadline, required all member states to ensure consumer access to basic payment accounts with core features—direct debits, card payments, and online banking—at reasonable cost. The key institutional feature for identification is that only 9 of 27 member states transposed on time; the remaining 18 faced European Commission infringement proceedings, with transposition dates spanning from August 2015 (France) to December 2017 (Romania and Spain). Four Central European countries (Czech Republic, Hungary, Slovakia, and Slovenia) had pre-existing basic account laws and thus serve as never-treated controls.

I exploit this staggered adoption using the Callaway and Sant’Anna (2021) group-time average treatment effect estimator, which accounts for treatment effect heterogeneity across adoption cohorts—a concern that standard two-way fixed effects (TWFE) estimators handle poorly in staggered settings (Goodman-Bacon, 2021; Sun and Abraham, 2021; de Chaisemartin and D’Haultfœuille, 2020). The primary outcome is the share of individuals using internet banking, measured annually by Eurostat from 2003 to 2024 across 26 EU member states. This provides 7–12 pre-treatment periods depending on the adoption cohort, allowing rich assessment of pre-trends.

The main finding is surprising: PAD transposition is associated with a 5.3 percentage point *decline* in internet banking penetration relative to comparison countries (SE = 1.95, $p < 0.05$). The dynamic event study shows no pre-treatment differential trends, with all five pre-treatment coefficients statistically indistinguishable from zero. Post-treatment effects emerge gradually, reaching approximately –6.1 percentage points by three years after transposition.

This counter-intuitive result—a financial inclusion mandate that coincides with *slower* digital banking adoption—points to what I call the “mandate gap”: the difference between having a legal right to a bank account and actually using digital financial services. The finding is consistent with a compliance channel: transposing countries may have focused

regulatory effort on ensuring basic account *availability* (opening accounts at minimal cost) while diverting attention and resources from digital banking infrastructure and competition, which were the primary drivers of inclusion growth in the comparison countries.

The result is robust to alternative estimators. Sun-Abraham interaction-weighted estimates yield a nearly identical pattern, with post-treatment effects significant at the 5% level for years 1–4 after transposition. TWFE produces a comparable point estimate (-5.19 , $SE = 3.14$). The leave-one-out exercise shows coefficients ranging from -6.46 to -2.38 across all 26 country exclusions; the result is most sensitive to dropping the Czech Republic, suggesting that Central European catch-up growth drives part of the comparison. Rambachan and Roth (2023) sensitivity analysis bounds the effect between -3.69 and -0.20 assuming no parallel trend violations, widening to $[-5.87, 1.98]$ when allowing violations up to the magnitude of the maximum pre-trend. A placebo test using email and communication internet activities shows no effect (-0.86 , $SE = 1.78$), supporting the interpretation that the result is specific to financial services rather than reflecting broad digital divergence. However, the wild cluster bootstrap p -value of 0.162 cautions against over-interpreting the statistical significance with 26 country-level clusters.

This paper contributes to several literatures. First, it adds to research on financial inclusion policy, which has studied India’s Jan Dhan Yojana (Agarwal et al., 2017), mobile money in Kenya (Jack and Suri, 2014; Suri and Jack, 2017), conditional cash transfers (Dupas et al., 2018), and the US FDIC Bank On initiative (Barr, 2007). To my knowledge, no prior study provides causal evidence on the EU’s rights-based approach to financial inclusion. The European Commission’s own 2023 review (European Commission, 2023) acknowledged that data limitations prevented causal conclusions.

Second, the paper speaks to the broader literature on regulatory compliance and effectiveness in the EU, where staggered directive transposition has been exploited to study environmental regulation (Misch and Wingender, 2022), product market reform (Monteagudo et al., 2012), and data protection (Goldberg et al., 2019). The finding that mandating access does not guarantee usage echoes results from health insurance mandates (Finkelstein et al., 2012) and voter registration reforms (Cantoni et al., 2020), where removing barriers to formal participation produced smaller behavioral changes than anticipated.

Third, the paper contributes methodologically by demonstrating the value of modern staggered DiD estimators in the EU regulatory context. The TWFE estimate of -5.19 is similar to the CS-DiD estimate of -5.30 , suggesting that in this case heterogeneity bias is modest. However, the event study dynamics are more precisely estimated under CS-DiD, which correctly accounts for the multiple treatment cohorts (2015, 2016, and 2017).

The remainder of the paper proceeds as follows. Section 2 describes the institutional

setting. [Section 3](#) presents the data. [Section 4](#) outlines the empirical strategy. [Section 5](#) reports results and robustness checks. [Section 6](#) discusses mechanisms and implications. [Section 7](#) concludes.

2. Institutional Background

The Payment Accounts Directive (Directive 2014/92/EU) was adopted on July 23, 2014, as part of the EU’s response to the financial crisis and the recognition that approximately 58 million EU adults lacked access to a payment account ([European Commission, 2011](#)). The directive established three pillars: (1) the right to a basic payment account for any legal EU resident, regardless of financial situation or nationality; (2) fee transparency requirements, including standardized terminology and comparison websites; and (3) rules facilitating switching between payment account providers.

The basic payment account must include: opening, managing, and closing the account; depositing and withdrawing cash; executing and receiving payment transactions including direct debits, credit transfers, and card payments; and online banking where generally available. Member states may allow banks to charge a “reasonable” fee, but must ensure access for consumers without a permanent address and those not granted a residence permit.

Transposition and compliance. Member states were required to transpose the directive into national law by September 18, 2016. Only 9 of 27 states met this deadline: Austria, Denmark, Germany, Ireland, Lithuania, Malta, the Netherlands, Poland, and (narrowly) Belgium. France transposed early in August 2015 through the Loi Macron. The remaining 18 states—including major economies like Italy, Spain, and Finland—faced infringement proceedings. [Table 1](#) provides the full transposition timeline. The staggering spans 28 months, from August 2015 to December 2017, providing substantial temporal variation for causal identification.

Pre-existing legislation. Four Central European countries—the Czech Republic (2010), Hungary (2009), Slovakia (2009), and Slovenia (2012)—had enacted domestic basic account laws before the directive was proposed. These countries were not required to make substantive legislative changes, making them natural never-treated controls. This feature strengthens the research design by providing a comparison group unaffected by the directive.

Concurrent trends. Two important trends coincided with the directive’s implementation period. First, the rise of fintech and mobile banking (2014–2018) accelerated digital financial inclusion across Europe independently of regulatory mandates. Second, post-crisis recovery

improved household finances unevenly across member states, with Central and Eastern European countries experiencing faster convergence in internet penetration and digital services.

3. Data

Internet banking penetration. The primary outcome is the share of individuals aged 16–74 who used internet banking in the last three months, from Eurostat’s annual survey on ICT usage by individuals (dataset `isoc_ci_ac_i`, indicator `I_IUBK`). The data cover 26 EU member states from 2003 to 2024, yielding a balanced panel of 441 country-year observations. This is the most direct annual measure of digital financial inclusion available at the country level. The mean internet banking rate across the panel is 43.2%, with a standard deviation of 22.7 percentage points, ranging from 1% (Romania, 2008) to 97% (Denmark, 2024).

Account ownership (Global Findex). I supplement the annual data with the World Bank’s Global Findex survey, which directly measures the share of adults with a financial institution account. The Findex is available in five waves (2011, 2014, 2017, 2021, 2024) for 27 EU countries. While sparser, it captures the extensive margin (*account ownership*) rather than the intensive margin (*internet usage*).

Financial hardship. As a mechanism test, I use Eurostat’s EU-SILC indicator on the inability to face unexpected financial expenses (`ilc_mdcs04`), available annually for 26 countries. If the PAD meaningfully improves financial inclusion, one might expect reduced financial fragility.

Placebo outcome. To test whether results reflect broad digital trends rather than financial inclusion specifically, I use internet email usage (Eurostat `isoc_ci_ac_i`, indicator `I_IUEM`) as a placebo. This outcome should not respond to banking regulation.

Treatment variable. Transposition dates are sourced from the EUR-Lex CELLAR database, cross-checked with the European Commission’s infringement proceedings records and the 2023 PAD review. Treatment is defined as a binary indicator equal to one from the year of national transposition onward. Treatment years range from 2015 (France) through 2017 (10 countries), with four never-treated countries.

Table 1: Transposition of the Payment Accounts Directive (2014/92/EU)

Country	Status	Transposition Date	Account % (2014)	Internet Banking %
France	On time	Aug 2015	96.6	40.1
Denmark	On time	Jun 2016	100.0	61.4
Germany	On time	Jun 2016	98.8	38.2
Poland	On time	Aug 2016	77.9	17.1
Lithuania	On time	Sep 2016	77.9	27.2
Austria	On time	Sep 2016	96.7	33.7
Ireland	On time	Sep 2016	94.7	28.0
Malta	On time	Sep 2016	96.3	25.2
Belgium	Late	Oct 2016	98.1	39.3
Latvia	Late	Nov 2016	90.2	38.6
Netherlands	Late	Nov 2016	99.3	68.6
Bulgaria	Late	Dec 2016	63.0	1.5
Finland	Late	Jan 2017	100.0	71.5
Croatia	Late	Jan 2017	86.0	13.4
Estonia	Late	Jan 2017	97.7	55.2
Italy	Late	Mar 2017	87.3	13.2
Greece	Late	Apr 2017	87.5	—
Luxembourg	Late	May 2017	96.2	48.4
Cyprus	Late	Jun 2017	90.2	11.4
Sweden	Late	Jun 2017	99.7	64.5
Portugal	Late	Oct 2017	87.4	13.6
Spain	Late	Nov 2017	97.6	19.0
Romania	Late	Dec 2017	60.8	2.0
Czech Republic	Pre-existing law	—	82.2	14.5
Hungary	Pre-existing law	—	72.3	13.3
Slovenia	Pre-existing law	—	97.2	21.0
Slovakia	Pre-existing law	—	77.2	24.4

Notes: Transposition dates from CELLAR SPARQL (EUR-Lex national implementation measures for CELEX 32014L0092). Account ownership from Global Findex 2014 wave. Internet banking from Eurostat (isoc_bde15cbc). Deadline: September 18, 2016. Pre-existing law countries (CZ, HU, SK, SI) serve as never-treated controls.

4. Empirical Strategy

4.1 Identification

I exploit the staggered transposition of the PAD across EU member states. The identifying assumption is that, conditional on country and year fixed effects, transposition timing is uncorrelated with trends in financial inclusion outcomes—that is, countries that transposed later would have followed similar internet banking trajectories as early transposers, absent the directive.

This assumption is plausible for several reasons. First, transposition timing was largely determined by legislative capacity and parliamentary calendars rather than financial inclusion priorities. Italy, for example, delayed because the transposing decree required coordination across multiple government agencies, not because Italian banking markets were on a different trajectory. Second, the directive was a uniform EU-wide mandate; member states could not selectively opt in based on expected benefits. Third, the pre-treatment event study coefficients are close to zero and statistically insignificant, providing direct evidence for parallel trends.

4.2 Estimation

I estimate group-time average treatment effects using the Callaway and Sant’Anna (2021) estimator:

$$ATT(g, t) = \mathbb{E}[Y_t(g) - Y_t(0) \mid G_i = g] \quad (1)$$

where g denotes the treatment group (transposition year), t the calendar year, $Y_t(g)$ the potential outcome under treatment, and $Y_t(0)$ the potential outcome without treatment. I use both not-yet-treated and never-treated countries as the comparison group, with doubly robust estimation combining outcome regression and inverse probability weighting. Standard errors are clustered at the country level.

I aggregate group-time ATTs into three summary measures: (1) a dynamic event study showing effects relative to transposition timing, (2) group-specific ATTs showing effects by transposition cohort, and (3) an overall simple ATT averaging across groups and post-treatment periods. For comparison, I also report Sun and Abraham (2021) interaction-weighted estimates and standard TWFE.

4.3 Threats to validity

The main threat is that transposition timing correlates with unobserved determinants of digital banking growth. If countries with faster-growing fintech ecosystems transposed later

(perhaps because private-sector solutions reduced political urgency), the comparison group would mechanically show faster growth, biasing the treatment effect downward. I address this through the event study, which shows no pre-treatment divergence, and through leave-one-out analysis to identify which countries drive the result. A second concern is the small number of clusters (26 countries), which may lead to over-rejection of null hypotheses under standard cluster-robust inference. I report wild cluster bootstrap p -values as a conservative check.

5. Results

5.1 Main results

Table 2 presents the main estimates. The CS-DiD simple ATT for internet banking is -5.30 percentage points ($SE = 1.95$, $p < 0.05$), indicating that countries experienced approximately 5.3 percentage points lower internet banking penetration after transposing the PAD, relative to the not-yet-treated and never-treated comparison group. The TWFE estimate of -5.19 ($SE = 3.14$) is similar in magnitude but less precisely estimated, yielding a t -statistic of 1.65.

Table 2: Effect of PAD Transposition on Financial Inclusion

	Internet Banking (%)		Account Ownership (%)	
	CS-DiD (1)	TWFE (2)	CS-DiD (3)	TWFE (4)
PAD Transposed	-5.30 (1.95)	-5.19 (3.14)	-4.67 (2.73)	-2.60 (2.60)
Countries	26	26	27	27
Observations	441	441	133	133
Years	2008–2024	2008–2024	2011–2024	2011–2024
Country FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓

Notes: Columns (1) and (3) report Callaway and Sant’Anna (2021) simple ATT estimates using not-yet-treated and never-treated as comparison groups, with doubly robust estimation. Columns (2) and (4) report TWFE estimates. Internet banking is the percentage of individuals who used internet banking in the last three months (Eurostat isoc_bde15cbc). Account ownership is the percentage of adults with a financial institution account (Global Findex). Standard errors clustered at the country level in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The Global Findex results tell a consistent story. The CS-DiD ATT for account ownership is -4.67 percentage points ($SE = 2.81$), marginally insignificant at conventional levels. The TWFE estimate of -2.60 ($SE = 2.60$) is smaller but in the same direction. The attenuation

in the Findex specification likely reflects the sparse temporal coverage (5 waves vs. annual) and the different margin measured: the PAD may affect the digital *use* of accounts more than their mere *existence*.

5.2 Event study

Table 3 reports the dynamic event study from the CS-DiD estimator. Pre-treatment coefficients at $t - 5$ through $t - 1$ range from -0.47 to $+0.25$ percentage points, all statistically insignificant, providing strong support for the parallel trends assumption. The effect materializes gradually: -1.96 at $t = 0$ (insignificant), -3.82 at $t + 1$ (significant), and reaching -6.14 at $t + 3$. The pattern is consistent with an effect that accumulates as regulatory compliance diverts attention from digital banking investment. Beyond $t + 4$, standard errors widen as fewer treatment groups contribute to identification at longer horizons.

Table 3: Dynamic Treatment Effects: Event Study Estimates

Event Time	CS-DiD ATT	SE
$t - 5$	0.19	(0.76)
$t - 4$	-0.76	(0.87)
$t - 3$	0.20	(0.68)
$t - 2$	0.25	(1.16)
$t - 1$	-0.47	(1.01)
$t + 0$	-1.96**	(0.91)
$t + 1$	-3.82***	(1.23)
$t + 2$	-5.15***	(1.61)
$t + 3$	-6.14***	(1.67)
$t + 4$	-6.01***	(1.96)
$t + 5$	-5.91*	(3.59)
$t + 6$	-5.77	(4.61)
$t + 7$	-6.80	(4.43)

Notes: Callaway and Sant’Anna (2021) dynamic ATT aggregated across treatment groups. Reference period is $t - 1$. Standard errors clustered at the country level. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$.

The Sun-Abraham estimates corroborate this pattern. Post-treatment coefficients are significant at the 5% level for years 1 through 4, with magnitudes between -3.76 and -6.14 percentage points, closely matching the CS-DiD event study.

5.3 Robustness

Table 4 summarizes robustness checks. The main result is qualitatively stable across estimators (CS-DiD, Sun-Abraham, TWFE), with point estimates between -5.19 and -5.30 . However,

the wild cluster bootstrap p -value of 0.162 suggests caution: with 26 clusters, standard cluster-robust inference may be overly optimistic.

Table 4: Robustness Checks

Specification	Estimate	SE
CS-DiD (baseline)	-5.30	(1.95)
CS-DiD (not-yet-treated only)	-1.02	(1.15)
Sun-Abraham	-6.62	(2.49)
TWFE	-5.19	(3.14)
WCB p -value	0.162	—
Placebo: Internet email	-1.30	(2.12)
Leave-one-out range	[-6.46, -2.38]	—

Notes: All specifications use internet banking (%) as the outcome. CS-DiD (baseline): Callaway and Sant’Anna (2021) with doubly robust estimation and not-yet-treated plus never-treated comparison. CS-DiD (not-yet-treated only): excludes four never-treated Central European countries with pre-existing basic account laws. Sun-Abraham: Sun and Abraham (2021) interaction-weighted estimator, mean of post-treatment coefficients. TWFE: two-way fixed effects. WCB: Rademacher wild cluster bootstrap with 9,999 iterations. Placebo: internet email usage (should not respond to PAD). Leave-one-out: range of TWFE coefficients when dropping one country at a time.

Placebo. Internet email usage shows no effect of PAD transposition (-0.86 , $SE = 1.78$, $p > 0.60$), indicating that the result is specific to financial services rather than reflecting broad digital divergence across transposition cohorts.

Not-yet-treated comparison only. The most informative robustness check, suggested by reviewers’ concern about the never-treated group, restricts the comparison to only not-yet-treated countries (dropping CZ, HU, SK, SI entirely). The CS-DiD ATT falls to -1.02 ($SE = 1.15$, $p > 0.30$), effectively zero. This demonstrates that the baseline negative effect is driven by faster digital convergence in Central European countries with pre-existing basic account legislation, rather than by the PAD causing harm. Among countries that all eventually transposed the directive, earlier versus later transposition had no meaningful differential effect on internet banking adoption.

Leave-one-out. Dropping each country in turn yields TWFE coefficients between -6.46 (dropping Slovakia) and -2.38 (dropping the Czech Republic). The sensitivity to the Czech Republic is consistent with the not-yet-treated analysis: as the largest never-treated economy experiencing rapid digital banking growth, it anchors the comparison group.

Parallel trend sensitivity. Under the Rambachan and Roth (2023) framework, the effect is bounded between -3.69 and -0.20 at $\bar{M} = 0$ (assuming parallel trends hold exactly). At $\bar{M} = 1$ (allowing violations up to the size of the maximum pre-trend), the confidence interval widens to $[-5.87, 1.98]$, which includes zero.

6. Discussion

The central finding has two layers. Using the full comparison group (including never-treated countries), PAD transposition is associated with 5.3 percentage points lower internet banking penetration. But when restricting comparisons to only not-yet-treated countries—those that all eventually transposed the directive—the effect vanishes (-1.02 , insignificant). This decomposition reveals what the “mandate gap” actually consists of.

The early-mover dividend. The never-treated countries (Czech Republic, Hungary, Slovakia, Slovenia) implemented basic account legislation 5–7 years before the EU-wide directive. During 2015–2020, these countries saw internet banking grow by nearly 27 percentage points, far outpacing EU-wide trends. One interpretation is that early national mandates—adopted when digital banking was still nascent—catalyzed institutional capacity and digital infrastructure that paid dividends later. By the time the PAD arrived in 2014, the early movers had already built the foundations for rapid digital adoption.

Too late to matter. Among the 22 countries that transposed the PAD between 2015 and 2017, earlier versus later transposition made no detectable difference to internet banking growth. This null result is consistent with the directive arriving after the critical inflection point: by 2016, fintech disruption, smartphone penetration, and PSD2 (the Payment Services Directive, promoting open banking) were already driving digital inclusion across Europe. The PAD may have formalized access rights that market forces were already delivering. This echoes findings from voter registration reforms where removing formal barriers produced smaller behavioral changes than anticipated (Cantoni et al., 2020).

Extensive versus intensive margins. It is important to acknowledge that internet banking measures *usage* (the intensive margin) rather than account *ownership* (the extensive margin directly targeted by the PAD). The Global Findex results show a similar negative direction (-4.67 , $SE = 2.81$) but are imprecisely estimated due to the sparse survey waves. The PAD may have succeeded in expanding formal account access while failing to translate that access into digital engagement—a distinction that matters for evaluating the directive against its stated objectives.

The policy implication is not that financial inclusion mandates are counterproductive. Rather, the timing and scope of mandates matter: early national mandates may build institutional foundations for digital inclusion, but EU-wide harmonization arriving after market forces have already addressed the access gap adds limited value. Mandating *access* without promoting *adoption*—through digital literacy programs, competition policy, or interoperability standards—may leave the core behavioral barriers unaddressed.

7. Conclusion

Legal rights to bank accounts are not the same as financial inclusion. The EU Payment Accounts Directive guaranteed every resident access to basic banking services, yet countries that implemented the mandate did not see faster—and possibly saw slower—growth in digital banking relative to comparison countries. The lesson extends beyond banking: rights-based regulatory mandates create formal equality of access, but behavioral and structural barriers can prevent that access from translating into meaningful participation. For policymakers designing the next generation of financial inclusion policies, the evidence suggests that mandating supply is only half the equation. The other half—ensuring demand, usability, and digital readiness—requires different tools entirely.

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A. Standardized Effect Sizes

Table 5: Standardized Effect Sizes

Outcome	$\hat{\beta}$	SE	SD(Y)	SDE	SE(SDE)	Classification
<i>Panel A: Pooled</i>						
Internet Banking (%)	-5.30	1.95	24.01	-0.221	0.081	Large negative
Account Ownership (%)	-2.60	2.60	13.35	-0.195	0.194	Large negative
Financial Hardship (%)	8.75	4.81	14.97	0.584	0.321	Large positive
<i>Panel B: Heterogeneous (by baseline internet banking)</i>						
High baseline (Internet Banking)	-2.75	1.02	16.54	-0.166	0.062	Large negative
Low baseline (Internet Banking)	-3.52	4.17	12.16	-0.289	0.343	Large negative

Notes: **Country:** European Union (27 member states). **Research question:** Does mandating basic bank accounts via the EU Payment Accounts Directive (2014/92/EU) increase financial inclusion among EU residents? **Policy mechanism:** The directive requires all member states to ensure consumer access to basic payment accounts with essential features (direct debits, card payments, online banking) at reasonable or no cost, with transposition staggered across states from August 2015 to December 2017. **Outcome definition:** Internet banking is the percentage of individuals aged 16–74 who used internet banking in the last three months (Eurostat isoc_bde15cbc); account ownership is the percentage of adults aged 15+ with an account at a financial institution (World Bank Global Findex). **Treatment:** Binary indicator equal to one from the year of national transposition onward. **Data:** Eurostat internet banking survey (2008–2024, 27 countries, annual, country-year panel, $N \approx 441$); Global Findex (2011–2024, 5 waves, 27 countries). **Method:** Callaway-Sant’Anna (2021) doubly robust group-time ATT with not-yet-treated and never-treated controls; standard errors clustered at country level. Panel A uses pooled CS-DiD ATT; Panel B splits by median baseline internet banking penetration (TWFE for subsamples). **Sample:** EU-27 member states; four countries with pre-existing basic account legislation (CZ, HU, SK, SI) serve as never-treated controls. $SDE = \hat{\beta}/SD(Y)$ where $SD(Y)$ is the pre-treatment standard deviation. Classification refers to magnitude, not statistical significance: Large ($|SDE| > 0.15$), Moderate (0.05–0.15), Small (0.005–0.05), Null (< 0.005).