Releasable or Relabeled? Positive-Neutral CCyB and Management-Buffer Crowd-Out*

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Abstract

When bank regulators create a releasable capital buffer in normal times, do banks add capital, or do they relabel capital they already intended to hold? I study this question using positive-neutral countercyclical capital buffer (PNR-CCyB) frameworks in Europe, where adoption is staggered across countries. I combine EEA bank-quarter capital data with country PNR adoption dates, macroprudential buffer rates, per-bank Pillar 2 requirements, and listed-bank payout data. I identify the effect by comparing treated banks to not-yet-treated controls under parallel trends in a staggered difference-in-differences design. After PNR adoption, the regulatory requirement rises by 0.97 percentage points and the voluntary buffer falls by 1.63 percentage points (1.17 to 1.63 across estimators); total CET1 does not significantly change. The composition of the CET1 stack shifts; its quantity does not. PNR-CCyB frameworks can therefore raise legal releasability without adding new capacity to bear losses.

Keywords: macroprudential policy, countercyclical capital buffer, bank capital, management buffers, payout policy.

JEL codes: G21, G28, E58.

1 Introduction

When regulators require banks to hold more releasable capital, do banks build new capital, or do they shrink the voluntary buffers they already hold above requirements? Which one happens determines whether macroprudential capital rules actually add loss-absorbing capacity or simply relabel capital banks were going to hold anyway. The

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question matters because a regulatory buffer and a voluntary buffer are not the same economic object even when both are measured in CET1 capital: voluntary buffers may be hard to draw down in stress — because of market stigma, supervisory expectations, distribution constraints, rating concerns, or uncertainty about future capital needs.

Positive-neutral countercyclical capital buffer (PNR-CCyB) frameworks are a direct response to this problem. Instead of setting the CCyB at zero in normal times and raising it only when cyclical risk is elevated, a PNR framework sets or targets a positive CCyB in normal times. The policy logic is simple: if part of the normal-times capital stack is inside a releasable macroprudential buffer, authorities can cut the requirement when stress arrives. The open empirical question is whether this policy increases the quantity of CET1 capital banks hold or instead changes the legal composition of a largely unchanged capital stack.

That distinction is central for macro-finance. Capital regulation affects the economy through bank balance sheets, but balance-sheet quantities and regulatory labels need not move together. If banks target a total CET1 ratio, a higher releasable requirement can crowd out the voluntary buffer: the CCyB component rises, the voluntary buffer falls by the same amount, and total CET1 is little changed. If banks instead target a fixed distance above requirements, total CET1 should rise with the new requirement. The policy can therefore improve legal usability without adding loss-absorbing capital. This paper tests that capital-stack margin.

The empirical setting is the recent adoption of positive-neutral CCyB frameworks in Europe. PNR adoption creates country-level variation in the timing and target size of normal-times releasable buffers. The setting is useful because the policy has a precise balance-sheet prediction: the measured requirement proxy should rise, and the response of the voluntary buffer and of total CET1 distinguishes new capital from relabelled capital. It is also difficult because adoption is not random. Countries choose PNR frameworks as part of broader macroprudential regimes, and adoption may reflect supervisory philosophy, bank-capital conditions, or pre-existing CCyB practices. The paper therefore does not treat PNR as a laboratory shock. It uses the policy timing to discipline a public-data accounting test.

I build a bank-quarter panel for large EEA banks that decomposes CET1 into a measured regulatory requirement proxy and the voluntary buffer above it. The panel combines EBA Transparency capital ratios, risk-weighted assets, and total assets; ESRB CCyB and systemic-buffer rates; ECB and bank-disclosure Pillar 2 requirements; manually validated PNR announcement and effective dates; and a FactSet payout workbook for listed banks. The main outcomes are measured in percentage points of risk-weighted assets: the regulatory requirement proxy, the CCyB component, the voluntary buffer, and total CET1.

The baseline design compares banks before and after their home country reaches the PNR effective date, absorbing bank fixed effects and common quarter shocks. The coefficient is the within-bank change after PNR adoption relative to the same-quarter change among banks in never-treated and not-yet-treated countries. Because treatment varies at the country-policy level, I report country-clustered and restricted wild country-cluster inference in addition to bank-clustered standard errors. Because adoption is staggered, I also report a group-time ATT diagnostic and a stacked-window DiD that compares each treated country with never-treated countries in a local window around implementation.

The main result is that PNR changes the composition of CET1 but not the quantity. After PNR adoption, the measured regulatory requirement proxy rises by 0.97 percentage points. The voluntary buffer falls by 1.63 percentage points in the baseline TWFE specification ($p = 0.015$ with bank clustering and $p = 0.023$ with country clustering). A group-time ATT diagnostic gives a same-signed voluntary-buffer decline of 1.17 percentage points. Because its standard errors use a cell-level approximation, I use it mainly to assess sign and magnitude rather than as the primary source of inference. The stacked-window DiD gives a similar decline of 1.33 percentage points ($p = 0.014$). Total CET1 does not significantly increase: the TWFE estimate is -0.64 percentage points ($p = 0.185$ with country clustering), the group-time ATT estimate is +0.06 percentage points ($p = 0.830$), and the stacked-window estimate is -0.33 percentage points ($p = 0.410$).

The 0.97-percentage-point increase in the measured requirement proxy is almost fully offset by the 1.63-percentage-point decline in the voluntary buffer. The estimated net change, 0.97 minus 1.63, is -0.66 percentage points, close to the -0.64 TWFE estimate for total CET1. The increase in the measured stack is concentrated in the CCyB component rather than in other measured requirements. This is what a fixed-total-CET1 or costly-buffer view predicts: the legally releasable part of the stack expands, but the total CET1 ratio does not clearly rise.

Voluntary-buffer declines are larger for banks with higher pre-treatment voluntary buffers and in countries with larger PNR targets. Interaction tests support the target-size margin more clearly than the initial-buffer margin. One-for-one relabelling would predict exactly this pattern — but the heterogeneity alone is not enough to pin the mechanism down.

Pre-treatment balance tables show no significant treated-control differences in total CET1, measured requirements, voluntary buffers, P2R, RWA, or total assets. Treated and other countries do differ in pre-treatment CCyB use, and lower pre-treatment total CET1 and lower pre-treatment CCyB rates predict explicit PNR status at the country level. Event-study pretrends are insignificant for the requirement proxy, the voluntary buffer, and total CET1. Leave-one-treated-country-out estimates keep the voluntary-buffer coefficient negative, while a restricted wild country-cluster bootstrap returns $p = 0.199$ on the baseline coefficient. The capital-stack pattern is robust; the policy-level causal claim is weaker.

In the listed-bank FactSet sample, total payout ratios do not fall after PNR adoption; the point estimate is positive but imprecise ($p = 0.170$), and the buyback-only data are sparse. Log constructed CET1 amount, log RWA, and log total assets do not move significantly either. These extensions do not pin down where the displaced voluntary buffer goes. They only show that the public data do not reveal payout cuts or a clean numerator/denominator adjustment large enough to overturn the capital-stack result.

The contribution is to measure an ex-post margin that is usually left conceptual. The paper is not a credit-register lending paper and does not claim to estimate real effects. It answers the prior balance-sheet question: when authorities create a normal-times releasable buffer, do banks add CET1 or reallocate their existing voluntary buffer within the capital stack? PNR calibration work can say how large a releasable buffer should be; this paper asks how banks' capital stacks actually move after implementation. Capital-requirement and lending papers can estimate credit-supply effects; this paper measures the buffer-composition margin that determines whether a requirement increase is likely to be new loss-absorbing capacity. The management-buffer literature shows that banks can offset higher requirements; this paper tests that crowd-out logic for a policy whose stated goal is legal usability.

The policy implication is a distinction, not a verdict on PNR. Positive-neutral CCyB frameworks may make capital more legally releasable by converting part of the voluntary buffer into a macroprudential one. That can be valuable if voluntary buffers are hard to draw down in stress. But the public EEA bank data do not show clear evidence that this conversion raises total CET1. The relevant question for policymakers is therefore not just how large a positive-neutral buffer to set, but whether the banks they oversee will treat it as new capital or as a label on capital they already had.

2 Related Literature

The question this paper answers — do positive-neutral CCyB frameworks add new capital, or relabel existing voluntary buffers? — sits at the intersection of four literatures.

The first studies positive-neutral CCyB design and calibration. Policy reports argue that positive-neutral frameworks build releasable macroprudential space before stress arrives (Basel Committee on Banking Supervision, 2024; European Central Bank and ESRB, 2025; Behn et al., 2023). Recent model-based work asks how large the positive-neutral buffer should be and how fast authorities should build it (Muñoz and Smets, 2025; Herrera et al., 2024). That literature makes the policy case for PNR but does not test the bank-level response.

Smaga (2024) comes closest to the bank-level question, asking whether EU banks held enough voluntary buffers ex ante to absorb a positive CCyB. I move that question from ex-ante capacity to the ex-post capital-stack response after adoption.

A second literature uses administrative credit data to measure loan-supply responses when capital requirements tighten or buffers are released (Aiyar et al., 2014; Jiménez et al., 2017; Fraisse et al., 2020; De Jonghe et al., 2020; Behn et al., 2025; Couaillier et al., 2025). These papers identify real effects. I study a prior balance-sheet object — whether the capital stack adds CET1 or relabels existing buffer — that the public-data design can measure without competing with credit-register identification.

Buffer usability is the third concern. Releasable buffers matter only if banks are willing and able to use them in stress, and several papers document why market stigma, supervisory pressure, payout restrictions, rating concerns, and uncertainty about future requirements make nominally releasable capital hard to draw down (Basel Committee on Banking Supervision, 2022; Behn et al., 2020; Andreeva et al., 2020; Abad et al., 2022; Couaillier et al., 2025). Positive-neutral CCyB frameworks respond by placing a releasable buffer in the normal-times stack. I show that formal usability can rise without total CET1 rising.

The fourth strand studies management buffers and target capital. Banks hold voluntary capital above minimum requirements because losses are uncertain, external equity is costly, market discipline and ratings matter, supervisors monitor capital planning, and distributions require headroom (Berger et al., 2008; Flannery and Rangan, 2008; Gropp and Heider, 2010; Couaillier, 2021). Abbondanza et al. (2025) show that banks can partly offset higher capital requirements by reducing voluntary buffers. I apply that crowd-out logic to PNR-CCyB, a policy whose stated aim is legal usability rather than higher requirements per se.

3 Institutional Background

Positive-neutral CCyB frameworks change the normal-times capital regime rather than only the cyclical activation rule. Under the conventional implementation, authorities set the CCyB at zero in normal times and raise it when cyclical systemic risks build. A positive-neutral framework treats a positive CCyB as the normal setting, even outside a severe credit boom, so that the authority has a buffer to release when stress arrives. PNR shifts more of the CET1 stack into a legally releasable macroprudential buffer.

Treatment dates are not the same as first-positive CCyB dates. Some countries had a positive CCyB long before formally adopting a positive-neutral framework. Sweden had a positive CCyB long before the Financial Supervisory Authority announced its positive-neutral approach in 2021 and restored the rate to 2 percent with application in 2023. Ireland had previously activated a positive CCyB, but its positive-neutral 1.5 percent framework dates from the 2023 announcement and 2024 implementation. The Czech Republic is a third case: I follow the BCBS classification and date the PNR framework to 2019, when Czech National Bank material described a non-zero CCyB rate for the usual level of cyclical risk. A first-positive CCyB date can capture an ordinary cyclical tightening, not a change to a positive-neutral regime.

I use PNR effective dates, not first-positive CCyB dates, as the baseline treatment. The effective date is when the capital stack is legally changed, while the announcement date is when banks may begin adjusting; I use announcement dates as a timing robustness check where national sources identify the framework or rate step. Countries with future effective dates remain not-yet-treated before implementation. I do not code Denmark and Norway as explicit-PNR treatment countries because the BCBS material classifies them as early-activation, no-target cases rather than target-rate PNR frameworks. Poland, Spain, Greece, and Hungary are in my classification, but their effective dates fall outside or at the end of the current sample and so do not contribute treated post observations in the baseline panel.

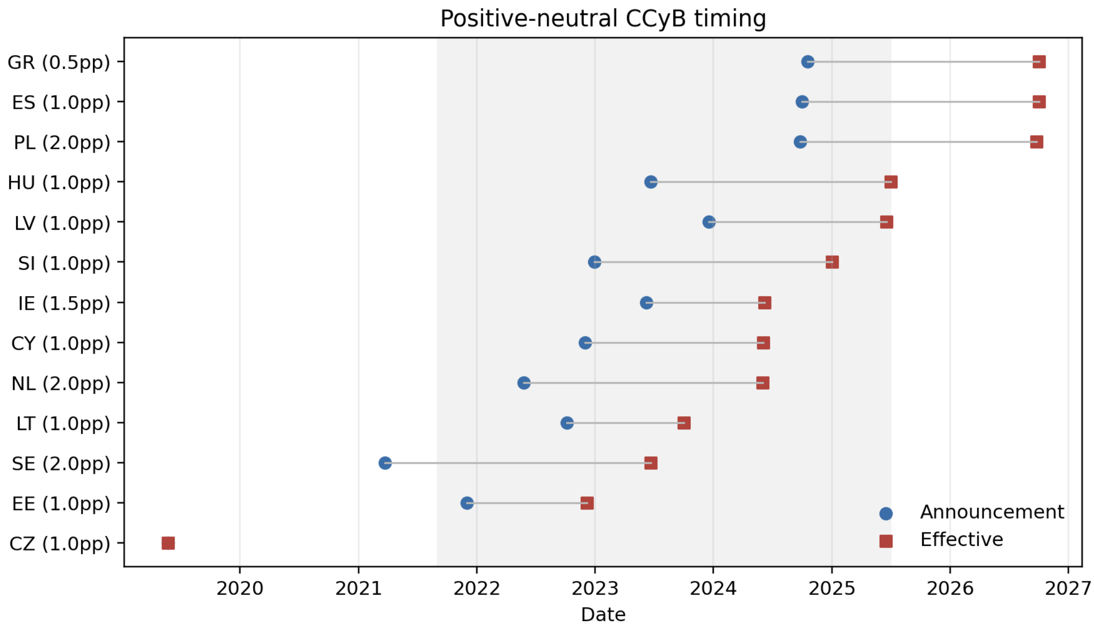
Figure 1 plots PNR announcement and effective dates against the sample window. Treated post observations come from countries whose effective dates fall inside the EBA Transparency window: Estonia, Sweden, Lithuania, the Netherlands, Cyprus, Ireland, Slovenia, and Latvia. Countries whose effective dates fall after 2025Q2 contribute control observations rather than treated post observations.

4 Capital-Stack Framework

The policy question can be written as a capital-stack adjustment problem. The observed CET1 ratio can be decomposed as:

$$\text{CET1}_{it} = \text{Pillar 1}_{it} + \text{P2R}_{it} + \text{CBR}_{it} + \text{Voluntary buffer}_{it},$$

Figure 1: Positive-neutral CCyB timing. The figure plots PNR announcement and effective dates for explicit-PNR countries. The shaded region is the main capital-stack sample period, 2021Q3–2025Q2. Labels report the country ISO code and target positive-neutral rate.



where the combined buffer requirement includes the capital conservation buffer, the country CCyB, and systemic buffers where applicable. If the PNR shock raises the combined buffer but total CET1 is unchanged, the voluntary component must fall.

The decomposition gives four empirical hypotheses. First, banks may target a fixed total CET1 ratio because equity is costly or market capital targets are sticky; a higher releasable requirement then crowds out the voluntary buffer, so measured requirements rise, the voluntary buffer falls, and total CET1 is flat. Second, banks may target a fixed distance above requirements, in which case the same policy raises total CET1 one-for-one. Third, banks may adjust distributions, retaining more earnings to rebuild the voluntary buffer; payout ratios then fall after PNR adoption. Fourth, banks shrink the denominator: RWA or assets decline relative to capital.

Table 1: Capital-stack channels and empirical predictions

Channel	Economic logic	Prediction in the data	Main test
New capital	Banks preserve the voluntary buffer above higher requirements	Requirements and total CET1 rise	Main capital-stack table
Management-buffer crowd-out	Banks target total CET1 and treat voluntary equity as costly	Requirements rise, the voluntary buffer falls, total CET1 is flat	Decomposition and estimator comparison
Payout adjustment	Banks retain more earnings to rebuild the voluntary buffer	Payout ratios fall after PNR adoption	Listed-bank payout table
Denominator adjustment	Banks reduce RWA or balance-sheet scale	RWA or assets fall relative to capital	Capital-amount and denominator table

Notes: The table states the competing capital-stack mechanisms before the regressions. The public data can test these balance-sheet predictions but cannot identify loan-level credit supply or welfare effects.

The public EEA bank data permit some tests but not others. They can measure the composition of the CET1 stack, total CET1 ratios, RWA, assets, and a listed-bank payout extension; they cannot measure supervisory capital plans, loan-level credit supply, borrower demand, or welfare. I therefore study the prior balance-sheet margin: whether positive-neutral buffers add new CET1 or relabel capital banks already held.

5 Data and Measurement

The unit of observation is a bank-quarter. EBA Transparency data provide bank capital ratios, risk-weighted assets, and total assets. ESRB data provide country-level CCyB rates and macroprudential buffer information. ECB SREP disclosures and bank publications provide per-bank Pillar 2 requirements. I hand-code the PNR treatment list from BCBS and national sources, recording the target positive-neutral rate, the announcement date where available, and the effective date.

The main dependent variables are expressed in percentage points of risk-weighted assets. The measured regulatory requirement proxy is defined as the sum of the Pillar 1 minimum, the capital conservation buffer, the Pillar 2 requirement, the country CCyB, and observed systemic buffers such as O-SII and SyRB. Only the CCyB component is naturally releasable; P2R and O-SII are included to measure the broader CET1 stack but should not be interpreted as releasable macroprudential capital. The voluntary buffer proxy is total CET1 minus the requirement proxy. Total CET1 is the fully loaded ratio where available; otherwise I use the transitional ratio.

Several measurement choices are conservative. I assign the country CCyB by bank home country rather than weighting it by geographic credit exposures. I approximate historical O-SII and systemic-risk buffer data from ESRB macroprudential sources. I expand P2R coverage backward using ECB snapshots and bank-level disclosures, and I validate or correct four low-confidence manual rows against primary sources (see data appendix). These choices introduce measurement error in the capital-stack proxy, so I treat the decomposition as an empirical proxy for the voluntary buffer rather than a complete Overall Capital Requirement (OCR) measure.

FactSet data provide the payout extension. I compute dividends as dividend per share times total shares, since the direct total-dividends field is unusable in the cached workbook. Buybacks come from the workbook's `new share buyback` sheet. FactSet covers net income and total shares broadly but covers buybacks for a smaller subset.

After the CET1 clean-range filter, the capital-stack sample covers 2021Q3–2025Q2, 132 banks, and 27 countries. Eight treated countries have post-treatment observations in the current sample: Cyprus, Estonia, Ireland, Lithuania, Latvia, the Netherlands, Sweden, and Slovenia. These countries account for 25 treated banks and 141 treated post-treatment bank-quarter observations in the capital-stack panel.

The EBA Transparency inputs use the 2022–2025 vintages. The ESRB buffer files provide country CCyB and systemic-buffer rates, and ECB SREP snapshots and bank disclosures provide P2R; I fill missing historical P2R with a backfill rule documented in the data appendix. I drop CET1 ratios outside the 5–50% range. RWA and total-assets outcomes are estimated only where the denominator is observed, and payout ratios are limited to listed banks with usable FactSet net income and total-shares fields and are winsorized as reported in the payout table.

Table 2: Sample construction and coverage

Sample object	Observations	Banks
Capital-stack bank-quarters	1,846	132
CET1 observations in clean range	1,846	132
Voluntary-buffer observations with extended P2R	1,127	87
Treated bank-quarters	141	25
Payout-ratio observations	1,484	28
Buyback observations	451	29

Notes: Capital-stack observations are bank-quarter rows. Payout observations are bank-security-quarter rows after merging the FactSet workbook to LEIs.

Table 2 reports the effective sample sizes. The capital-stack panel of 132 banks contains 25 treated banks and 141 treated post-treatment observations; the FactSet payout panel is limited to listed banks. The core crowd-out result is therefore a large-bank supervisory-data result, while the payout mechanism is identified on listed banks with usable FactSet fields.

6 Descriptive Evidence

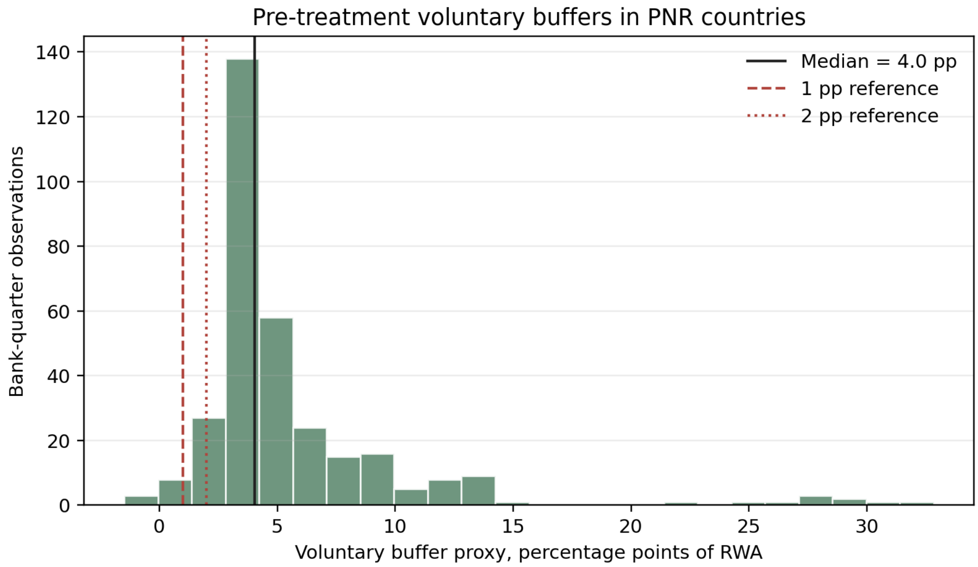
Banks in PNR countries enter the sample with voluntary buffers large enough to absorb a 1–2 percentage point releasable requirement without breaching CET1 targets (Figure 2). Management-buffer crowd-out is therefore mechanically possible before any regression is estimated.

Many treated-country banks had voluntary buffers larger than the target PNR increment before implementation (Figure 2). A higher releasable requirement can therefore be absorbed by drawing down existing voluntary capital, not by adding new CET1.

Raw means by ever-PNR status confound composition with policy: treated and other countries differ in pre-treatment CCyB use and in the timing of macroprudential regimes (Figure 3). I therefore turn to a fixed-effects design that isolates within-bank changes.

Figure 4 compares average CET1-stack composition before and after PNR effective dates

Figure 2: Pre-treatment voluntary buffers in PNR countries. The histogram uses 322 bank-quarter observations from explicit-PNR countries before their PNR effective dates with valid CET1 and measured-requirement proxy data. Voluntary buffers are measured as total CET1 minus the measured regulatory requirement proxy, in percentage points of risk-weighted assets. The vertical reference lines mark 1 and 2 percentage points, common target PNR rates in the treatment file.



for treated countries. The CCyB share is higher in the post period, and the voluntary buffer is lower; the same accounting pattern reappears in the within-bank regressions.

Policy variation is generated by a small number of treated countries, even though the panel contains many bank-quarter observations (Figure 5). I therefore report country-level inference checks alongside conventional bank-clustered standard errors.

Table 3: Pre-treatment balance

Variable	PNR	Other	Diff.	p-value
Total CET1	17.12	18.30	-1.18	0.204
Measured req.	10.00	10.12	-0.13	0.548
Voluntary buffer	7.05	8.26	-1.22	0.401
CCyB	0.15	0.44	-0.28	< 0.001
P2R	2.27	2.09	0.18	0.149
Log RWA	10.22	10.43	-0.21	0.396
Log assets	11.27	11.60	-0.33	0.223

Notes: Bank-level pre-treatment means. Capital-ratio variables are percentage points of RWA; size variables are log points. P-values are Welch tests of equality of means across banks.

Table 3 compares treated-country and other-country banks before PNR implementation. Treated banks do not have statistically different pre-treatment total CET1, measured requirements, voluntary buffers, P2R, RWA, or asset size at conventional levels. The

Figure 3: Capital-stack trends by PNR treatment status. The figure plots bank-quarter means for explicit-PNR countries and other EEA countries. Outcomes are measured in percentage points of risk-weighted assets. The treated group is defined by ever being in the explicit-PNR treatment file; countries with future effective dates are included in the treated-country descriptive series but are not coded as post-treated before implementation in the regressions.

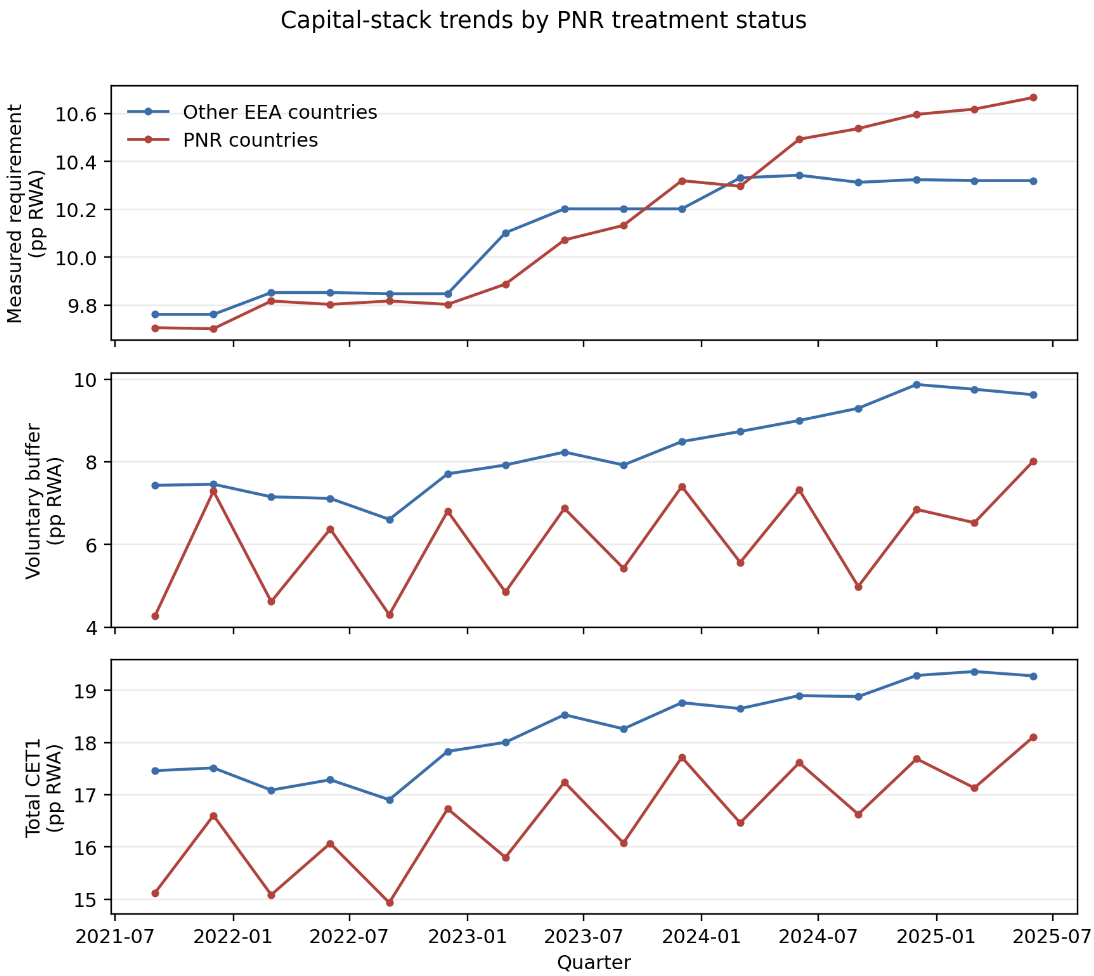


Figure 4: Average CET1-stack composition in treated countries. The figure plots treated-country bank-quarter means before and after PNR effective dates. Pillar 1, the capital conservation buffer, P2R, the CCyB, and systemic buffers form the measured requirement proxy; the voluntary buffer is the residual between total CET1 and the measured requirement proxy. The figure is descriptive and does not absorb bank or quarter fixed effects. Because treated-country composition changes over time and post-treatment observations are concentrated in a small number of countries, it should not be interpreted as an estimate of the PNR effect.

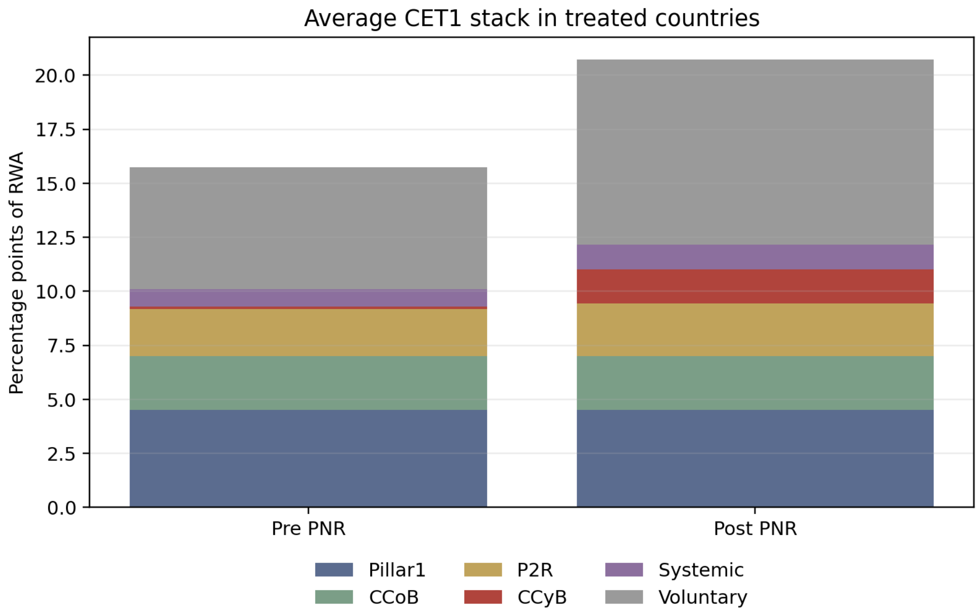
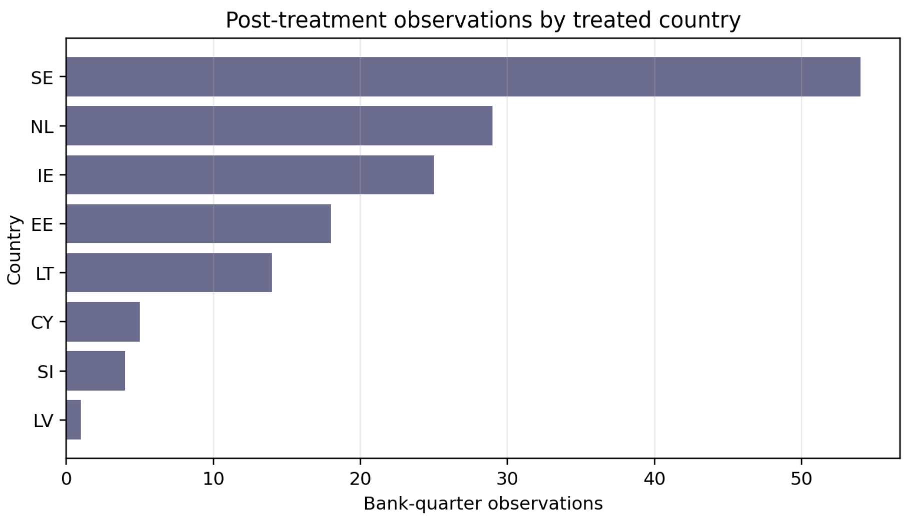


Figure 5: Treated post observations by country. The figure reports the number of bank-quarter observations with PNR treatment switched on in the capital-stack panel. Treatment varies at the country-policy level, even though the panel contains bank-quarter observations.



one sharp difference is the CCyB: other countries have higher average pre-treatment CCyB rates. That imbalance is consistent with the institutional fact that some non-PNR countries, including early-activation cases, already used positive CCyB rates without a target-rate PNR framework.

Table 4: Country-level PNR adoption diagnostics

Predictor	Estimate	p-value	Countries
Voluntary buffer	-0.09	0.476	17
Total CET1	-0.18**	0.025	27
Measured req.	-0.08	0.436	17
CCyB	-0.18***	< 0.001	27
Log RWA	-0.11	0.221	27
Number of banks	-0.08	0.192	27

Notes: Each row is a separate linear-probability regression of explicit-PNR country status on a standardized pre-treatment country characteristic. Robust HC1 p-values are reported. The small country count makes these diagnostics descriptive.

Table 4 reports a complementary country-level diagnostic. The estimates in the table are not first-stage causal regressions; the country count is small and each row is a separate linear-probability regression on a different pre-treatment characteristic. Even so, explicit PNR adoption is correlated with lower pre-treatment total CET1 and lower pre-treatment CCyB rates. This reinforces the main identification caveat: PNR adoption is a policy choice made by countries with different macroprudential regimes, not a randomized shock.

7 Empirical Design

The empirical difficulty is to separate a capital-stack response to PNR from other country-level macroprudential or bank-capital trends. Adoption is not random: countries choose PNR frameworks when they want more releasable macroprudential space, and banks in those countries may differ in profitability, risk, or supervisory scrutiny. The baseline design therefore asks a narrower within-bank question: after a country reaches its PNR effective date, does the same bank hold a different composition of CET1 relative to banks facing the same quarter's shocks in countries that are never treated or not yet treated?

The baseline specification is:

$$Y_{it} = \alpha_i + \lambda_t + \beta\,\mathrm{PNRPost}_{c(i)t} + \varepsilon_{it},$$

where Y_{it} is a capital-stack or payout outcome for bank i in period t, α_i are bank fixed effects, and λ_t are period fixed effects. The treatment indicator turns on after the bank's home country reaches its PNR effective date. Countries with future effective dates are not coded as treated before implementation. The coefficient β measures the average within-bank change in the outcome after PNR adoption relative to the same period's change among banks in non-treated or not-yet-treated countries.

The preferred outcomes map directly to the capital-stack mechanism. If PNR creates new capital, the measured requirement proxy should rise and total CET1 should rise. If PNR crowds out the voluntary buffer, the measured requirement proxy should rise, the voluntary buffer should fall, and total CET1 should be statistically flat. The coefficient on the measured requirement proxy therefore checks policy bite, while the coefficients on the voluntary buffer and total CET1 distinguish new capital from relabeled capital.

Since PNR adoption varies at the country-time level, inference is potentially more fragile than bank-level clustering suggests. I therefore report bank-clustered standard errors as a baseline, but also report country-clustered, two-way clustered, and restricted wild country-cluster bootstrap inference. The coefficient is stable across these approaches, but the wild-bootstrap p-value is weaker. For this reason, I interpret the estimates as public-data evidence consistent with capital-stack crowd-out rather than as definitive causal estimates.

Staggered adoption creates a second threat: TWFE estimates can mix comparisons across cohorts when treatment effects vary over event time. I therefore report group-time ATT estimates as a staggered-adoption diagnostic. Treatment cohorts are defined by the home country's PNR effective quarter, with the control group restricted to never-treated banks. For each cohort and event quarter, the estimator compares treated banks' change from the quarter before treatment with never-treated banks' change over the same window. The reported average post ATT is a treated-observation-weighted average over event quarters 0 through 4.

Standard errors use a pooled cell-variance approximation that does not fully account for country-policy-level clustering, so I interpret the ATT p-values descriptively and rely on the country-clustered and wild-cluster TWFE results for the main statistical caution.

I also estimate a stacked-window DiD diagnostic. For each treated-country effective quarter, I build a stack containing that country and the never-treated countries from four quarters before to four quarters after implementation. The stacked specification includes stack-by-bank and stack-by-quarter fixed effects, so identification comes from treated-country banks changing within the same local window relative to never-treated banks in that stack. This design does not solve country-level endogeneity, but it reduces reliance on comparisons across distant cohorts and periods.

The stacked-window sample repeats never-treated control banks across stacks when they are eligible controls for multiple treated-country windows. Standard errors are clustered by country to allow arbitrary correlation within country across stacks, but this diagnostic should still be interpreted as a robustness exercise rather than the main source of inference. Outcome-specific sample sizes differ because voluntary-buffer outcomes require the measured requirement proxy and extended P2R coverage, while total CET1, RWA, and assets are available in the broader capital-stack panel.

I also report timing checks. First, I shift treatment one year earlier within the pre-treatment sample. A significant placebo effect would suggest that treated banks were already reducing voluntary buffers before the policy became effective. Second, I replace effective-date treatment with PNR-specific announcement-date treatment. This check asks whether the adjustment occurs when the policy is announced rather than when the requirement applies.

The identifying assumption is that, absent PNR adoption, treated banks would not have experienced a relative decline in voluntary buffers after the same dates. The design mitigates common quarter shocks with period fixed effects and time-invariant bank differences with bank fixed effects, but it cannot rule out all country-time confounding. The paper therefore relies on the accounting decomposition, the estimator comparison, and the pre-treatment diagnostics rather than claiming a fully exogenous policy shock.

8 Main Results

The policy raises measured CET1 requirements, but banks absorb the increase by drawing down the voluntary buffer rather than by raising total CET1. I report the main estimates in Table 5, with three diagnostics — a stack decomposition, an estimator comparison, and a stacked-window DiD — in the tables that follow.

Table 5: Capital-stack response to positive-neutral CCyB adoption

Outcome	Estimate	SE	Bank p	Country p	Wild p	Observations
Voluntary buffer	-1.63**	0.66	0.015	0.023	0.199	1,127
Measured requirement proxy	0.97***	0.17	< 0.001	0.001		1,846
Total CET1 ratio	-0.64	0.43	0.139	0.185		1,846
Placebo voluntary buffer	-0.67	0.53	0.207			1,063

Notes: Estimates are percentage points of risk-weighted assets. All specifications include bank and period fixed effects. Bank p-values use bank-clustered standard errors; country p-values use country-clustered standard errors. The wild p-value is the restricted country-cluster bootstrap p-value for the voluntary-buffer coefficient, using seed 20260519 and 999 replications. Stars denote $^{***}p < 0.01$, $^{**}p < 0.05$, and $^{*}p < 0.10$.

Table 5 asks whether PNR adoption changes the quantity of CET1 or the composition of the CET1 stack. The measured regulatory requirement proxy increases by 0.97 percentage points after PNR adoption, with $p < 0.001$ (bank-clustered) and $p = 0.001$ (country-clustered), confirming policy bite. The voluntary buffer falls by 1.63 percentage points; $p = 0.015$ (bank-clustered) and $p = 0.023$ (country-clustered). Total CET1 does not significantly increase: the point estimate is -0.64 percentage points, with $p = 0.139$ (bank-clustered) and $p = 0.185$ (country-clustered). The pre-treatment placebo for the voluntary buffer is also not significant ($p = 0.207$).

These magnitudes line up with the accounting identity. The requirement-proxy increase of 0.97 percentage points and the voluntary-buffer change of -1.63 percentage points sum to a net change of -0.66 percentage points, almost identical to the estimated change in total CET1 of -0.64 percentage points. The new measured requirement component is offset by a lower voluntary buffer rather than by higher total CET1: this is the core crowd-out result.

Table 6: Decomposition of the measured regulatory stack

Outcome	Estimate	SE	p-value	Observations	Countries
CCyB component	0.96***	0.14	< 0.001	1,846	27
Other measured requirements	-0.10	0.06	0.113	1,127	17
Measured requirement proxy	0.88***	0.23	0.001	1,127	17
Voluntary buffer proxy	-1.63**	0.65	0.023	1,127	17
Total CET1 ratio	-0.64	0.47	0.185	1,846	27

Notes: Estimates are percentage points of risk-weighted assets from bank and period fixed-effect specifications with standard errors clustered by country. The CCyB component is the policy margin; other measured requirements include the non-CCyB components of the measured regulatory requirement proxy.

Table 6 addresses a measurement threat: the broad requirement proxy could be moving because of unrelated P2R or systemic-buffer changes rather than the PNR policy margin. The increase is concentrated in the CCyB component, while other measured requirements do not significantly offset it. This supports the interpretation that the main estimate captures the positive-neutral CCyB margin rather than unrelated regulatory-stack movements.

Table 7 addresses the staggered-treatment threat. It compares the country-clustered TWFE estimate with the group-time ATT average post effect. The measured requirement proxy and CCyB component rise under both estimators, while the voluntary buffer falls. The group-time voluntary-buffer estimate is smaller than the TWFE estimate, -1.17 percentage points rather than -1.63 percentage points. Total CET1 is not statistically different from zero and changes sign across estimators. Direction and magnitude are stable across estimators. With few treated cohorts, however, country-policy-level inference is imprecise, so I interpret the estimates as public-data evidence consistent

Table 7: Compact estimator comparison

Outcome	TWFE, country cluster	Group-time ATT
Measured regulatory requirement proxy	0.88*** (0.23; p = 0.001)	0.57*** (0.03; p < 0.001)
CCyB component	0.96*** (0.14; p < 0.001)	0.62*** (0.02; p < 0.001)
Voluntary buffer proxy	-1.63** (0.65; p = 0.023)	-1.17*** (0.23; p < 0.001)
Total CET1 ratio	-0.64 (0.47; p = 0.185)	0.06 (0.30; p = 0.830)
CET1 / total assets	-0.37* (0.18; p = 0.051)	-0.38*** (0.08; p < 0.001)

Notes: Estimates are percentage points. Each cell reports the estimate with stars, standard error in parentheses, and p-value. Stars are attached to estimates, not p-values. TWFE estimates include bank and period fixed effects with country-clustered standard errors. Group-time ATT rows are diagnostic estimates: their p-values use the project cell-level variance approximation and should not be interpreted as the paper's primary country-policy-level inference.

with capital-stack crowd-out rather than as definitive causal evidence.

Table 8: Stacked-window DiD diagnostics

Outcome	Estimate	SE	p-value	Obs.
Voluntary buffer	-1.33**	0.48	0.014	3133
Measured req.	0.66***	0.13	< 0.001	3133
Total CET1	-0.33	0.40	0.410	5122
Log RWA	0.01	0.01	0.265	5122
Log assets	0.05	0.03	0.141	4318

Notes: Each row stacks four quarters before and after each treated-country effective quarter, comparing treated-country banks with never-treated-country banks. Never-treated control observations may appear in multiple stacks. Specifications include stack-by-bank and stack-by-quarter fixed effects; standard errors cluster by country. Outcome samples differ because voluntary-buffer outcomes require measured-requirement coverage. Capital-ratio estimates are percentage points of RWA; log outcomes are log points.

Table 8 adds the stacked-window diagnostic. The voluntary buffer falls by 1.33 percentage points ($p = 0.014$) and the measured requirement proxy rises by 0.66 percentage points ($p < 0.001$). Total CET1, log RWA, and log assets do not move significantly. The stacked design therefore preserves the main composition result while making the comparison more local in event time.

9 Robustness and Interpretation

I organize the robustness section around named objections rather than around alternative specifications for their own sake. The central objections, in the order I address them below, are that treatment varies across few country cohorts, PNR adoption is not random, and the public requirement proxy is measured with error.

Table 10 addresses the main inference threat: treatment varies at the country-time level,

Table 9: Identification threats and diagnostics

Threat	Why it matters	Diagnostic	Result
Non-random adoption	Countries choose PNR regimes	Balance/adoption diagnostics	Mixed; cautious language
Few cohorts	Policy varies by country	Country and wild clustering	Wild p-value weaker
Staggered adoption	TWFE can mix cohorts	Group-time ATT; stacked DiD	Similar sign and magnitude
Measurement error	Public proxy approximates OCR	Alternative buffer definitions	Estimates remain negative
Anticipation	Banks may adjust early	Announcement and placebo timing	Weaker; placebo insignificant
Influential countries	One country could drive results	Leave-one-treated-country-out	Sign stable; precision sensitive
Early-activation controls	DK/NO may be imperfect controls	Exclude Denmark/Norway	Estimate remains negative

Notes: The table summarizes the main identification threats and the diagnostics used in the paper. The diagnostics discipline the interpretation but do not turn PNR adoption into a randomized shock.

Table 10: Inference sensitivity for voluntary-buffer crowd-out

Inference	Estimate	SE	p-value	Wild p-value	Clusters
bank cluster	-1.63**	0.66	0.015	0.199	87
country cluster	-1.63**	0.65	0.023	0.199	17
bank + quarter cluster	-1.63**	0.62	0.019		16
country + quarter cluster	-1.63**	0.62	0.019		16

Notes: Estimates are percentage points. Wild p-values use a restricted Rademacher wild cluster bootstrap at the country level with 999 replications. The wild bootstrap p-value should be read as a conservative country-policy-level inference check, not as a change in the estimated magnitude.

while the panel has many bank observations. I report bank clustering for continuity, but country clustering and the restricted wild country-cluster bootstrap better match the policy variation. The point estimate is unchanged across covariance estimators because the specification is unchanged. The p-values are not: country-clustered and two-way clustered p-values remain below 0.05, while the restricted wild bootstrap p-value is 0.199. The coefficient is economically meaningful and stable, but statistical certainty is sensitive to policy-level inference.

Table 11: Country-placebo diagnostic

Real est.	Cluster p	Reps	Placebo p	Mean	5-95 pct.
-1.63	0.023	99	0.030	0.04	[-1.03, 1.40]

Notes: The diagnostic randomly assigns the observed number of treated countries to countries in the panel and draws placebo effective dates from the treated-date distribution. The placebo p-value is the share of placebo estimates with absolute value at least as large as the real voluntary-buffer estimate. This is a diagnostic, not a formal randomization test.

Table 11 adds a country-placebo diagnostic. I randomly assign the observed number of treated countries to countries in the panel and draw placebo effective dates from the observed treated-date distribution. Only 3.0 percent of placebo estimates are at least as large in absolute value as the baseline voluntary-buffer estimate. The diagnostic is supportive because the real estimate sits in the tail of randomized country assignments, but it is not a formal randomization test: actual PNR adoption is a policy choice, not random assignment.

Table 12: Leave-one-treated-country-out sensitivity

Dropped country	Estimate	SE	p-value	Observations
CY	-2.07***	0.37	< 0.001	1,113
EE	-1.60**	0.70	0.036	1,111
IE	-1.47	1.07	0.192	1,061
LT	-1.69**	0.65	0.021	1,115
LV	-1.63**	0.65	0.023	1,127
NL	-1.08	0.87	0.237	1,060
SE	-1.63**	0.65	0.023	1,127
SI	-1.65**	0.67	0.027	1,103

Notes: Outcome is the voluntary-buffer proxy. Each row drops one treated country and clusters standard errors by country.

Table 12 addresses country influence. The voluntary-buffer estimate remains negative in every leave-one-out exercise (dropping one treated country at a time). Dropping the Netherlands or Ireland — important treated cohorts in the sample — makes the estimate smaller and statistically weak, but the sign does not flip.

Table 13: Robustness summary for voluntary-buffer crowd-out

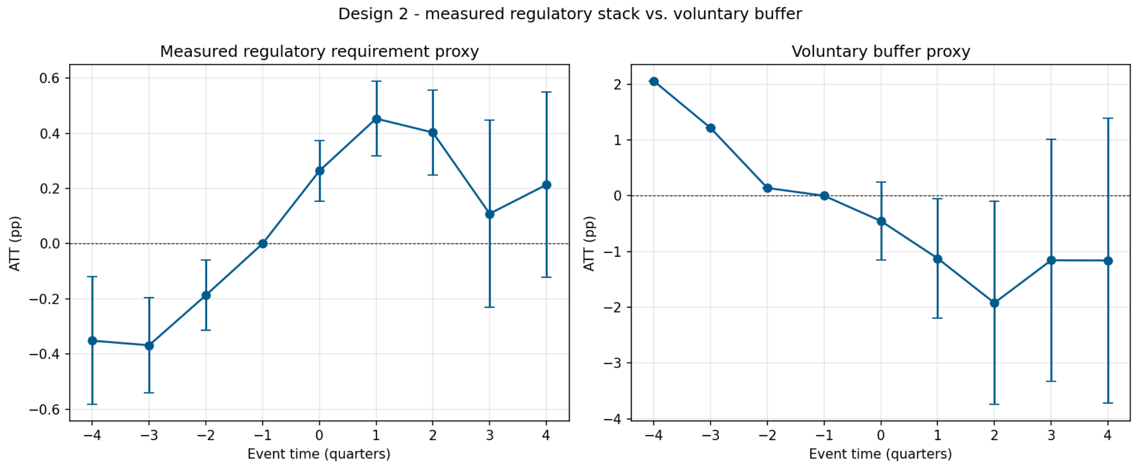
Check	Estimate	SE	p-value	Observations
Baseline voluntary buffer	-1.63**	0.65	0.023	1,127
No-P2R voluntary proxy	-1.60***	0.57	0.009	1,846
CET1 minus CCyB only	-1.60***	0.57	0.009	1,846
Exclude Czech Republic	-1.63**	0.65	0.023	1,127
Exclude Denmark/Norway controls	-1.63**	0.65	0.023	1,127
Exclude 2021 stress quarters	-1.55**	0.65	0.030	1,005
Fully loaded CET1 only	-1.68**	0.62	0.015	987
Winsorized voluntary buffer	-1.55**	0.64	0.029	1,127

Notes: All rows use bank and period fixed effects with standard errors clustered by country.

Table 13 groups the remaining threats. The voluntary-buffer estimate could reflect the P2R component of the proxy, the Czech treatment-date classification, the Denmark/Norway exclusion choice, early-sample stress quarters, transitional CET1 measurement, or outliers. I check each in turn, and the estimate remains negative under every check. The main limitation is therefore not sign fragility; it is precision with few treated country cohorts.

Figure 6 shows the same decomposition in event time. The event-study path matches the regression estimates, but it is not a standalone parallel-trends test: voluntary-buffer

Figure 6: Event-study evidence on the measured regulatory stack and voluntary buffer. The figure plots event-time estimates around PNR effective dates for the measured requirement proxy and the voluntary-buffer proxy. Units are percentage points of risk-weighted assets. Treatment is defined by the PNR effective quarter. The figure is a group-time ATT event-study with never-treated banks as controls; pointwise 95 percent confidence intervals use the pooled cell-variance approximation. The omitted event-time category is the last pre-treatment quarter.



point estimates drift downward before treatment, confidence intervals are wide, and the group-time pretrend test is not significant.

The group-time ATT event-study diagnostic supports this interpretation for the constructed stack variables. The average post-treatment ATT is $+0.57$ percentage points for the measured requirement proxy and -1.17 percentage points for the voluntary buffer, matching the group-time ATT column in Table 7.

The associated p-values use the pooled cell-variance approximation introduced in the empirical design and should be read descriptively. Diagonal pre-trend tests are not significant for the measured requirement proxy ($p = 0.776$), the voluntary buffer ($p = 0.509$), or total CET1 ($p = 0.693$). The CCyB component shows pre-movement, which is expected because some authorities raise the CCyB rate before the formal PNR effective date. These tests have limited power: the number of treated cohorts and treated post-treatment event cells is small.

Table 15 reports the timing check. Effective-date treatment produces the main negative voluntary-buffer estimate. The PNR-specific announcement-date estimate is also negative but smaller and not statistically significant. I read this as weakening any claim that banks adjust at the first public communication; it does not overturn the main result because the capital requirement binds at implementation.

Table 14: Group-time ATT event-study diagnostics

Outcome	Average post ATT	SE	ATT p-value	Pretrend p-value	Interpretation
Measured regulatory requirement proxy	0.57***	0.03	< 0.001	0.776	No significant pre-trend
Voluntary buffer proxy	-1.17***	0.23	< 0.001	0.509	No significant pre-trend
Total CET1 ratio	0.06	0.30	0.830	0.693	No significant pre-trend
CCyB component	0.62***	0.02	< 0.001	< 0.001	Pre-movement expected

Notes: Estimates are percentage points of risk-weighted assets. Average post ATT is the same weighted average of post-treatment group-time cells reported in the estimator-comparison table. Cells are weighted by treated observations. Group-time ATT rows are diagnostic estimates: their p-values use the project's pooled cell-variance approximation and should not be interpreted as the paper's primary country-policy-level inference. Stars and p-values on ATT estimates should be read descriptively. Pretrend p-values are diagonal joint tests over pre-treatment event cells from the project group-time ATT diagnostic and have limited power with few treated cohorts. The CCyB component can move before formal PNR effective dates because some authorities raise CCyB rates before the positive-neutral framework becomes effective.

Table 15: Treatment timing robustness

Specification	Estimate	SE	p-value	Observations	Banks
Effective date, voluntary buffer	-1.63**	0.66	0.015	1,127	87
Announcement date, voluntary buffer	-0.39	0.41	0.345	1,127	87
Effective date, total CET1	-0.64	0.43	0.139	1,846	132
Announcement date, total CET1	-0.20	0.30	0.515	1,846	132

Notes: Estimates are percentage points. Announcement dates use PNR-framework-specific dates where manually validated; effective dates are the baseline treatment.

10 Mechanism, Transmission, and Limits

The regressions identify a capital-stack pattern, not a complete transmission mechanism. The framework in Table 1 predicts that a fixed-total-CET1 response should appear as higher measured requirements, lower voluntary buffers, and little movement in total CET1. The main tables match that pattern. A fuller macro-finance paper would then trace where the compressed voluntary buffer goes — into lower retained earnings, higher payouts, slower CET1 growth, lower RWA, or reduced lending. The public data partially cover those margins, so I treat the diagnostics below as mechanism evidence rather than core identification.

Table 16: Heterogeneity in voluntary-buffer response

Split	Estimate	p-value	Obs.	Banks
High pre-treatment voluntary buffer	-2.56***	< 0.001	537	42
Low pre-treatment voluntary buffer	-0.84	0.379	580	42
High target PNR country	-2.21***	< 0.001	867	66
Low target PNR country	0.41	0.777	994	75
Listed / FactSet coverage	-1.30	0.192	666	52
No FactSet coverage	-1.77***	< 0.001	461	35

Notes: Each row estimates the baseline bank and quarter fixed-effects specification within the indicated subsample. Estimates are percentage points of RWA. Standard errors are clustered by country; stars are based on p-values.

Table 16 asks whether the voluntary-buffer response is concentrated where the framework predicts more room for crowd-out. The decline is much larger among banks with high pre-treatment voluntary buffers, -2.56^{***} percentage points ($p < 0.001$), than among banks with low pre-treatment voluntary buffers, -0.84 percentage points ($p = 0.379$). It is also concentrated in high-target PNR countries, where the estimate is -2.21^{***} percentage points ($p < 0.001$), compared with 0.41 percentage points ($p = 0.777$) in low-target PNR countries. This is the main mechanism evidence in the current public-data design, but it is suggestive rather than definitive: formal interaction tests are imprecise for the initial-buffer split.

In split-sample diagnostics not shown here, banks with high pre-treatment buffers also show a decline in total CET1, so the subsample result is not a clean one-for-one relabeling estimate. The narrower reading is that voluntary-buffer compression is not evenly spread across the sample; it concentrates where crowd-out is most feasible. Table 17 provides the formal difference test for the split-sample patterns.

The high-target PNR interaction in Table 17 is negative and marginally significant ($p = 0.075$), consistent with larger target buffers producing larger voluntary-buffer compression. The high-pre-treatment-buffer interaction is also negative but not significant

Table 17: Interaction tests for voluntary-buffer heterogeneity

Moderator	PNR x moderator	SE	p-value	Obs.
High pre-treatment buffer	-1.05	0.81	0.213	1127
Pre-treatment buffer (z-score)	-0.15	0.30	0.613	1117
High PNR target	-2.68*	1.41	0.075	1127
Listed / FactSet coverage	-0.13	0.95	0.892	1127

Notes: Each row estimates the baseline bank and quarter fixed-effects specification with PNRPost, the moderator, and their interaction. The reported coefficient is the interaction term for the voluntary-buffer outcome. Estimates are percentage points of RWA; standard errors are clustered by country.

($p = 0.213$). The pattern is strongest for PNR target size. The pre-treatment-buffer split is descriptive; interaction precision is too weak to identify a differential treatment effect.

Table 18: Payout response after positive-neutral CCyB adoption

Outcome	Estimate	SE	p-value	Observations	Banks
Total payout / net income	16.95	12.01	0.170	391	27
Total payout / net income, positive NI	16.68	12.29	0.187	375	26
Dividends / net income	0.59	0.64	0.362	391	27
Log buybacks	-0.51	0.90	0.578	153	21

Notes: Ratio outcomes are in percentage points. Dividends are computed as dividend per share times total shares. Buyback coverage is sparse: the buyback-only regression has 153 observations across 21 banks. All specifications include bank and period fixed effects, with standard errors clustered by bank.

For listed banks, Table 18 asks whether payouts fell after PNR adoption by enough to rebuild voluntary buffers. The point estimates suggest they did not: total payout rises by 16.95 percentage points in the all-bank listed sample ($p = 0.170$) and by a similar amount in the positive-net-income sample ($p = 0.187$). The standard errors are too wide to discipline the direction, but the sign points up rather than down. Dividend-only payout barely changes, and the buyback regression rests on 153 observations across 21 banks — too sparse to support inference.

The payout sample is therefore not strong enough to identify where the displaced voluntary buffer goes; it provides no evidence of payout cuts large enough to rebuild voluntary buffers. The capital-stack decomposition remains the paper's main evidence, and allocating the displacement across dividends, buybacks, retained earnings, issuance, or other balance-sheet choices is left for future work with richer data.

To check whether the ratio result is driven by the capital numerator, the RWA denominator, or balance-sheet scale, I run the decomposition in Table 19. Total assets come from the EBA Transparency *Total Assets* field. Log CET1 amount, log RWA, and log

Table 19: Capital quantity and denominator channels

Outcome	Estimate	SE	p-value	Units
CET1 ratio	-0.64	0.47	0.185	pp
Log constructed CET1 amount	-0.01	0.04	0.764	log points
Log RWA	0.03	0.02	0.212	log points
Log total assets	0.06	0.05	0.200	log points
CET1 / total assets	-0.37*	0.18	0.051	pp
RWA / total assets	-0.97	0.70	0.179	pp

Notes: CET1 amount is constructed as CET1 ratio times RWA. Total assets come from the EBA Transparency "Total Assets" field. Level CET1 amount and RWA regressions are generated in the replication output but omitted here because the raw amount unit is not cleanly interpretable across EBA vintages.

total assets do not move significantly. CET1 relative to total assets falls marginally ($p = 0.051$); RWA relative to assets does not move significantly ($p = 0.179$). The balance-sheet decomposition therefore reveals no clean numerator or denominator channel. The ratio-stack result remains the strongest evidence: measured requirements rise, voluntary buffers fall, and total CET1 ratios do not significantly increase.

The current data also do not support a real-effects analysis comparable to the bank-lending-channel literature. EBA Transparency data provide balance-sheet aggregates, but they do not separate loan demand from loan supply and do not match banks to borrowers. I therefore do not interpret the flat total-assets and RWA estimates as evidence that PNR has no lending effect. They only show that the public bank-quarter panel does not reveal a clean aggregate denominator response over the same window. A credit-register design would be needed to ask whether the change in capital-stack composition affects loan supply.

11 Discussion

Positive-neutral CCyB adoption changes the composition of bank capital more clearly than it changes the level. A regulator can make a larger share of the capital stack formally releasable, but banks respond by holding a smaller voluntary buffer above requirements. This pattern is consistent with banks targeting a total capital ratio or treating voluntary equity as costly, rather than mechanically preserving a fixed voluntary buffer.

Positive-neutral CCyB frameworks can still improve resilience: if voluntary buffers are difficult to use in stress, shifting part of the capital stack into a legally releasable buffer is an improvement in usability even when the total CET1 ratio is unchanged. For calibration, the lesson is that authorities should account for endogenous voluntary-buffer compression when interpreting how much new loss-absorbing capacity a positive-neutral buffer creates.

The main limitations are measurement and timing. The public-data capital stack is a proxy for the supervisory Overall Capital Requirement (OCR). Because the treatment is country-level, PNR adoption may be correlated with broader macroprudential policy choices. The announcement-date test is weaker than the effective-date test because the requirement only binds at implementation. These limitations restrict the claim to capital-stack composition around PNR implementation, not a complete welfare assessment of positive-neutral frameworks.

Heterogeneity supports the mechanism but does not fully identify it. The split-sample estimates show larger voluntary-buffer compression among banks with high pre-treatment voluntary buffers and in countries with higher PNR targets. Formal interaction tests provide clearer support for the target-size margin than for the initial-buffer margin: the high-target interaction is negative and significant at the 10 percent level, while the high-buffer interaction is negative but imprecise.

Future work could test the mechanism more directly with richer bank-level data on profitability, capital planning, and supervisory guidance — for example, whether banks with higher equity costs or tighter payout incentives drive the crowd-out response.

12 Conclusion

Positive-neutral CCyB frameworks are designed to create releasable capital in normal times. In this sample, they raise the measured requirement proxy but do not significantly increase total CET1. Voluntary buffers fall by 1.17 to 1.63 percentage points after adoption, depending on the estimator, offsetting the increase. The estimate remains negative across leave-one-treated-country-out checks and alternative buffer definitions, although the country-cluster wild bootstrap p-value exceeds conventional thresholds. A pre-treatment placebo is insignificant, and the listed-bank payout sample shows no evidence of payout cuts large enough to rebuild voluntary buffers.

Macroprudential authorities should distinguish between the legal usability of capital and the quantity of capital banks hold. Positive-neutral frameworks raise legal releasability without raising the total amount. This is what voluntary-buffer crowd-out implies; country-policy-level statistical inference, however, is fragile.

Table 20: Positive-neutral CCyB treatment countries

Country	ISO	Target PNR (pp)	Effective date
Czech Republic	CZ	1.0	2019-05-23
Estonia	EE	1.0	2022-12-07
Sweden	SE	2.0	2023-06-22
Lithuania	LT	1.0	2023-10-01
Netherlands	NL	2.0	2024-05-31
Cyprus	CY	1.0	2024-06-02
Ireland	IE	1.5	2024-06-07
Slovenia	SI	1.0	2025-01-01
Latvia	LV	1.0	2025-06-18
Hungary	HU	1.0	2025-07-01
Poland	PL	2.0	2026-09-24
Spain	ES	1.0	2026-10-01
Greece	GR	0.5	2026-10-01

Notes: Treatment dates come from the manually locked BCBS/national-source treatment file. Denmark and Norway are excluded from the explicit-PNR table because the treatment file classifies them as early-activation, no-target cases.

Table 21: Country-level treatment audit

ISO	Target	Announce	Effective	Banks	Post obs.	Role
NL	2.0	2022-05-25	2024-05-31	8	29	post
ES	1.0	2024-10-01	2026-10-01	10	0	no post
SE	2.0	2021-03-22	2023-06-22	5	45	post
CY	1.0	2022-12-01	2024-06-02	2	5	post
CZ	1.0	2019-05-23	2019-05-23	0	0	no post
GR	0.5	2024-10-18	2026-10-01	5	0	no post
HU	1.0	2023-06-22	2025-07-01	2	0	no post
IE	1.5	2023-06-07	2024-06-07	6	25	post
EE	1.0	2021-11-30	2022-12-07	2	18	post
LV	1.0	2023-12-18	2025-06-18	1	1	post
LT	1.0	2022-10-05	2023-10-01	2	14	post
PL	2.0	2024-09-24	2026-09-24	2	0	no post
SI	1.0	2022-12-30	2025-01-01	3	4	post
DK		2018-03-14		3	0	early control
NO		2013-12-12		3	0	early control

Notes: The table summarizes the treatment file and the role each country plays in the current EBA capital-stack sample. Denmark and Norway are early-activation/no-target cases, not explicit target-rate PNR treatment countries.

A Positive-Neutral CCyB Treatment Coding

The full source audit, including URLs and country-specific notes, is provided in the replication package as `pnr_treatment_source_audit.md` and `pnr_treatment_audit_table.csv`. The PDF reports the compact treatment-role table to preserve readability.

Table 20 lists the explicit-PNR countries used in the baseline treatment file. The empirical timing distinction is important: first-positive CCyB dates can predate formal PNR frameworks by several years in some countries. The paper therefore uses PNR effective dates in the baseline and PNR-specific announcement dates in the timing robustness table. Countries with future effective dates are not coded as treated before implementation; in the current sample, Poland, Spain, and Greece remain not-yet-treated controls before their 2026 effective dates.

For the Czech Republic, the coding relies on the BCBS effective-date classification and ECB secondary documentation, supported by CNB material from May 2019 describing a non-zero CCyB rate of around 1 percent when cyclical risks are at usual levels. I do not use the Czech announcement date as a primary-source treatment date in the baseline.

B Construction of the Measured Regulatory Requirement Proxy

The measured regulatory requirement proxy is:

$$\text{MeasuredReq}_{it} = P1_{it} + CCoB_{it} + P2R_{it} + CCyB_{c(i)t} + Systemic_{it}.$$

The voluntary buffer proxy is:

$$\text{VoluntaryBuffer}_{it} = CET1_{it} - \text{MeasuredReq}_{it}.$$

Only the CCyB component is naturally releasable. The broader measured requirement proxy is included to measure the bank's CET1 stack and voluntary headroom, not to define legally releasable capital. The proxy excludes unobserved supervisory guidance and any bank-specific legal details not recoverable from public EBA, ESRB, ECB, and bank-disclosure sources. Further documentation is in `docs/capital_stack_measurement.md`.

Table 22: Event-time coefficients from group-time ATT diagnostic

Outcome	Event time	Estimate	SE	p-value	95% CI low	95% CI high	Treated obs.
CCyB	-4	-0.19**	0.08	0.022	-0.34	-0.03	19
CCyB	-3	-0.20***	0.05	< 0.001	-0.29	-0.11	21
CCyB	-2	-0.02	0.02	0.428	-0.06	0.03	21
CCyB	0	0.27***	0.06	< 0.001	0.15	0.39	22
CCyB	1	0.72***	0.05	< 0.001	0.62	0.81	21
CCyB	2	0.72***	0.05	< 0.001	0.62	0.83	19
CCyB	3	0.71***	0.06	< 0.001	0.60	0.82	19
CCyB	4	0.81***	0.06	< 0.001	0.70	0.93	19
Measured req.	-4	-0.12	0.17	0.488	-0.46	0.22	10
Measured req.	-3	-0.01	0.09	0.953	-0.17	0.16	12
Measured req.	-2	0.05	0.07	0.431	-0.08	0.19	12
Measured req.	0	0.46***	0.17	0.006	0.13	0.78	12
Measured req.	1	0.61***	0.08	< 0.001	0.44	0.77	11
Measured req.	2	0.61***	0.09	< 0.001	0.44	0.78	10
Measured req.	3	0.59***	0.10	< 0.001	0.39	0.80	10
Measured req.	4	0.84***	0.10	< 0.001	0.64	1.05	10
Voluntary buffer	-4	0.60	0.56	0.277	-0.48	1.69	10
Voluntary buffer	-3	0.54	0.53	0.308	-0.50	1.59	12
Voluntary buffer	-2	-0.11	0.36	0.758	-0.81	0.59	12
Voluntary buffer	0	-0.65*	0.36	0.070	-1.35	0.05	12
Voluntary buffer	1	-0.98*	0.55	0.072	-2.05	0.09	11
Voluntary buffer	2	-1.07	0.93	0.252	-2.89	0.76	10
Voluntary buffer	3	-0.44	1.11	0.688	-2.61	1.72	10
Voluntary buffer	4	-1.01	1.30	0.436	-3.57	1.54	10
Total CET1	-4	0.78	0.80	0.332	-0.79	2.34	19
Total CET1	-3	0.82	1.17	0.481	-1.46	3.11	21
Total CET1	-2	0.08	0.65	0.906	-1.19	1.34	21
Total CET1	0	0.33	0.60	0.576	-0.84	1.51	22
Total CET1	1	0.25	0.87	0.776	-1.46	1.95	21
Total CET1	2	0.01	1.08	0.991	-2.10	2.12	19
Total CET1	3	1.22	1.93	0.526	-2.55	4.99	19
Total CET1	4	1.32	2.05	0.519	-2.70	5.34	19

Notes: Estimates are percentage points of risk-weighted assets. Event time -1 is the omitted category. The table omits that zero-normalized row.

C Event-Study Diagnostics

Table 22 reports the event-time coefficients behind the compact group-time ATT diagnostic in the main text. The omitted category is event time -1.

D Full Estimator Comparison

Table 23: Full estimator comparison

Outcome	Estimator	Estimate	SE	p-value	Treated obs./cells	Controls
Measured regulatory requirement proxy	TWFE, bank cluster	0.88***	0.13	< 0.001	71	never-treated and not-yet-treated banks
Measured regulatory requirement proxy	TWFE, country cluster	0.88***	0.23	0.001	71	never-treated and not-yet-treated banks
Measured regulatory requirement proxy	Group-time ATT, never-treated controls	0.57***	0.03	< 0.001	53	never-treated banks
CCyB component	TWFE, bank cluster	0.96***	0.07	< 0.001	141	never-treated and not-yet-treated banks
CCyB component	TWFE, country cluster	0.96***	0.14	< 0.001	141	never-treated and not-yet-treated banks
CCyB component	Group-time ATT, never-treated controls	0.62***	0.02	< 0.001	100	never-treated banks
Voluntary buffer proxy	TWFE, bank cluster	-1.63**	0.66	0.015	71	never-treated and not-yet-treated banks
Voluntary buffer proxy	TWFE, country cluster	-1.63**	0.65	0.023	71	never-treated and not-yet-treated banks
Voluntary buffer proxy	Group-time ATT, never-treated controls	-1.17***	0.23	< 0.001	53	never-treated banks
Total CET1 ratio	TWFE, bank cluster	-0.64	0.43	0.139	141	never-treated and not-yet-treated banks
Total CET1 ratio	TWFE, country cluster	-0.64	0.47	0.185	141	never-treated and not-yet-treated banks
Total CET1 ratio	Group-time ATT, never-treated controls	0.06	0.30	0.830	100	never-treated banks
CET1 / total assets	TWFE, bank cluster	-0.37**	0.17	0.031	126	never-treated and not-yet-treated banks
CET1 / total assets	TWFE, country cluster	-0.37*	0.18	0.051	126	never-treated and not-yet-treated banks
CET1 / total assets	Group-time ATT, never-treated controls	-0.38***	0.08	< 0.001	85	never-treated banks

Notes: TWFE estimates include bank and period fixed effects. Group-time ATT rows are diagnostic estimates using never-treated banks as controls and a treated-observation-weighted average over post-treatment event cells. Estimates are percentage points. Treated observations/cells is the number of treated bank-quarter observations for TWFE rows and the sum of treated observations across post-treatment event cells for group-time ATT rows. Group-time ATT p-values use the project cell-level variance approximation and should not be interpreted as the paper's primary country-policy-level inference.

Table 23 reports the full estimator audit behind the compact main-text comparison. It includes the bank-clustered TWFE rows, country-clustered TWFE rows, group-time ATT rows, treated observation or cell counts, and control-group definitions.

E P2R Validation

The extended P2R panel uses ECB SREP snapshots and bank disclosures. Four manual rows were flagged for validation. ING 2018 is validated at 1.75 percent. Société Générale 2018 and BPCE 2018 are corrected to 1.50 percent. ABN AMRO 2019 is corrected to 1.75 percent based on ABN AMRO's statement that its 2020 P2R of 2.00 percent was 0.25 percentage points higher than in 2019. These corrections are documented in `docs/p2r_validation_2026-05-19.md`.

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