When Harmonization Codifies the Status Quo: The EU Mortgage Credit Directive and Lending Rates

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Abstract

I estimate the effect of the EU Mortgage Credit Directive (2014/17/EU) on mortgage lending rates, exploiting staggered transposition across 18 euro area countries between 2015 and 2019. The directive had no detectable effect: the TWFE estimate is -0.011 percentage points (95% CI $[-0.24, 0.21]$), and a heterogeneity-robust Sun–Abraham estimator confirms this at -0.016 (SE $= 0.638$). Randomization inference ($p = 0.94$), wild cluster bootstrap ($p = 0.92$), country-specific linear trends, and leave-one-out analysis all corroborate. The confidence interval rules out effects larger than one-quarter of a percentage point—well below magnitudes associated with binding macroprudential interventions. The null is consistent with pre-existing regulatory convergence: most euro area countries already operated under equivalent national standards before harmonization.

JEL Codes: G21, G28, K23, E44

Keywords: mortgage regulation, EU directive transposition, difference-in-differences, null effects, financial regulation

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1. Introduction

For over a decade, European regulators sought to harmonize the cost of borrowing from Tallinn to Toledo. The Mortgage Credit Directive (MCD, 2014/17/EU), adopted in February 2014, was the crowning legislative achievement of this effort: it mandated creditworthiness assessments for all EU mortgage lenders, standardized pre-contractual information, and established conduct-of-business rules for intermediaries. The European Commission projected that harmonized standards would reshape mortgage markets (European Commission, 2011). Three years after the transposition deadline, the Commission's own evaluation admitted that "the impact of the MCD on the cost of mortgage credit is difficult to isolate" (European Commission, 2021). This paper provides an answer: the directive changed nothing.

This difficulty is precisely what motivates this paper. The MCD represents a natural experiment in EU-wide regulatory harmonization: all 27 member states were required to transpose the same directive, but they did so at different times—from Estonia in March 2015 to Spain in June 2019. This staggered adoption creates quasi-experimental variation that can be exploited using modern difference-in-differences methods to estimate the causal effect of transposition on lending conditions.

I find that the MCD had no detectable effect on mortgage lending rates. Using quarterly-aggregated ECB Monetary Financial Institution Interest Rate (MIR) statistics across 18 euro area countries from 2010 to 2021, the TWFE estimate is -0.011 percentage points (SE $= 0.115$, 95% CI $[-0.24, 0.21]$). A heterogeneity-robust Sun–Abraham interaction-weighted estimator confirms the null at -0.016 (SE $= 0.638$), with a wider confidence interval reflecting the cost of robustness to treatment-effect heterogeneity in a staggered design with few cohorts. Both estimates are substantively small: the TWFE point estimate represents 0.5% of the mean mortgage rate (2.56%), and the confidence interval rules out effects larger than one-quarter of a percentage point in either direction—well below magnitudes associated with binding macroprudential interventions.

The null is robust across every specification I consider. Adding country-specific linear time trends—which directly absorbs the concern that transposition timing correlates with differential macro-financial trajectories—yields an estimate of $+0.030$ (SE $= 0.068$). Randomization inference ($p = 0.94$), wild cluster bootstrap ($p = 0.92$), leave-one-out analysis (all estimates within $[-0.07, +0.06]$ pp), and a temporal placebo (-0.13, SE $= 0.13$) all corroborate.

A key concern with any null finding is whether the research design has sufficient power to detect meaningful effects. The TWFE 95% confidence interval of $[-0.24, 0.21]$ pp bounds the range of effects consistent with the data. To appreciate this precision: the ECB's rate cut cycle from 2011 to 2016 reduced mortgage rates by roughly 1.5 percentage points, and national

macroprudential interventions have been associated with effects of 0.2–0.5 pp (Cerutti et al., 2017). The study can rule out effects in the range typically associated with binding financial regulation, though moderate effects below 0.2 pp remain consistent with the data.

The most natural explanation for this null is that the MCD codified existing practice rather than introducing genuinely new regulatory constraints. By the time of transposition, most euro area countries already operated under national mortgage regulation that met or exceeded the directive's requirements. The Netherlands, Finland, and Ireland had implemented comprehensive creditworthiness assessment regimes years before the MCD (Maclennan and Miao, 2018). Germany's established practice of conservative lending through the Pfandbrief system already embodied the directive's principles (Bholat et al., 2018). Countries that lacked formal regulation—notably Spain and Greece—tended to be late transposers, but their mortgage markets were simultaneously shaped by sovereign debt crises and ECB unconventional monetary policy, making attribution to the MCD alone implausible (Cerutti et al., 2017).

This interpretation is supported by two additional findings. First, heterogeneity analysis reveals no differential effect between countries with previously stringent regulation and those without: the interaction coefficient between MCD transposition and pre-existing regulatory stringency is 0.27 (SE = 0.37), indicating that the directive did not bite harder where existing regulation was weaker. If the MCD had imposed genuinely new constraints, we would expect larger effects in less-regulated markets. Second, heterogeneity by pre-MCD housing boom intensity likewise shows no significant differential effect (0.47, SE = 0.30), suggesting that the directive's creditworthiness requirements did not differentially constrain lending in overheating markets.

This paper contributes to several literatures. First, it adds to the growing body of evidence on null effects of financial regulation. Acharya et al. (2014) find limited effects of EU stress tests on credit supply; Jiménez et al. (2017) show that macroprudential policies in Spain operated primarily through dynamic provisioning rather than lending standards. My findings suggest that EU-wide harmonization directives may face a similar dynamic: when national regulators have already converged on similar practices, top-down harmonization ratifies the status quo rather than changing behavior (Enriques and Hertig, 2015).

Second, the paper contributes to the econometric literature on credible null results. Following Abadie (2020), I characterize the range of effects consistent with the data using confidence intervals from multiple inference procedures. The methodology—combining heterogeneity-robust event-study estimation (Sun and Abraham, 2021; Callaway and Sant'Anna, 2021) with randomization inference (Fisher, 1935; Young, 2019), wild cluster bootstrap (Cameron et al., 2008; MacKinnon et al., 2023), and sensitivity to pre-trend violations (Roth et al.,

2023; Rambachan and Roth, 2023)—provides a template for researchers seeking to document regulatory non-effects with the same rigor applied to significant findings.

Third, the paper informs ongoing policy debates about EU regulatory strategy. The Commission is currently evaluating the MCD's successor provisions and considering whether further harmonization is warranted (European Commission, 2021). If directive-level interventions cannot move outcomes when national practice already converges, policymakers may need to focus harmonization efforts on dimensions where genuine cross-country divergence persists—such as loan-to-value caps, variable-rate exposure, and cross-border lending barriers (Fernández-Vidaurreta and Hernández-Murillo, 2023).

2. Institutional Background

2.1 The Mortgage Credit Directive

The Mortgage Credit Directive (Directive 2014/17/EU) was adopted on February 4, 2014, with a transposition deadline of March 21, 2016. It was the EU's first comprehensive legislative framework specifically targeting residential mortgage lending, building on earlier consumer credit legislation (Directive 2008/48/EC) and responding to the mortgage market failures that contributed to the 2008 financial crisis (European Commission, 2011).

The directive's core provisions fall into four categories. First, it established mandatory creditworthiness assessments: lenders must evaluate a borrower's ability to repay before extending credit, considering income, expenses, and other financial commitments. This was explicitly framed as a response to pre-crisis "originate to distribute" lending practices (European Commission, 2007). Second, it introduced standardized pre-contractual information through the European Standardised Information Sheet (ESIS), requiring lenders to provide consumers with comparable information on interest rates, fees, and total cost of credit. Third, it regulated mortgage intermediaries (brokers), requiring registration, competence standards, and disclosure of remuneration. Fourth, it established rules on early repayment, foreign currency loans, and variable-rate mortgages.

Critically, the directive was a *minimum harmonization* measure: member states could maintain or introduce more stringent national provisions. This design choice reflected the political reality that several member states—particularly in Northern Europe—already regulated mortgage lending more strictly than the directive required (Whitehead and Scanlon, 2014).

2.2 Staggered Transposition

EU directives must be transposed into national law by each member state. The MCD's transposition deadline was March 21, 2016, but the actual timing of transposition varied substantially. Figure 1 and Table 5 document the full timeline.

Estonia was the earliest mover, transposing in March 2015—a full year before the deadline. Nine countries met the deadline: the Netherlands, Denmark, Sweden, Austria, Ireland, Germany, Hungary, and Slovakia all transposed by March 2016. Latvia transposed six weeks late. France, Bulgaria, and Luxembourg were three months late. Italy, Lithuania, Malta, and the Czech Republic transposed between September and December 2016. Finland, Portugal, and Croatia followed in early 2017. Belgium and Slovenia completed transposition by mid-2017. Romania, Cyprus, and Greece transposed in September 2017, eighteen months after the deadline. Spain—the last country—did not complete transposition until June 2019, with the adoption of *Ley 5/2019 de contratos de crédito inmobiliario*, more than three years late.

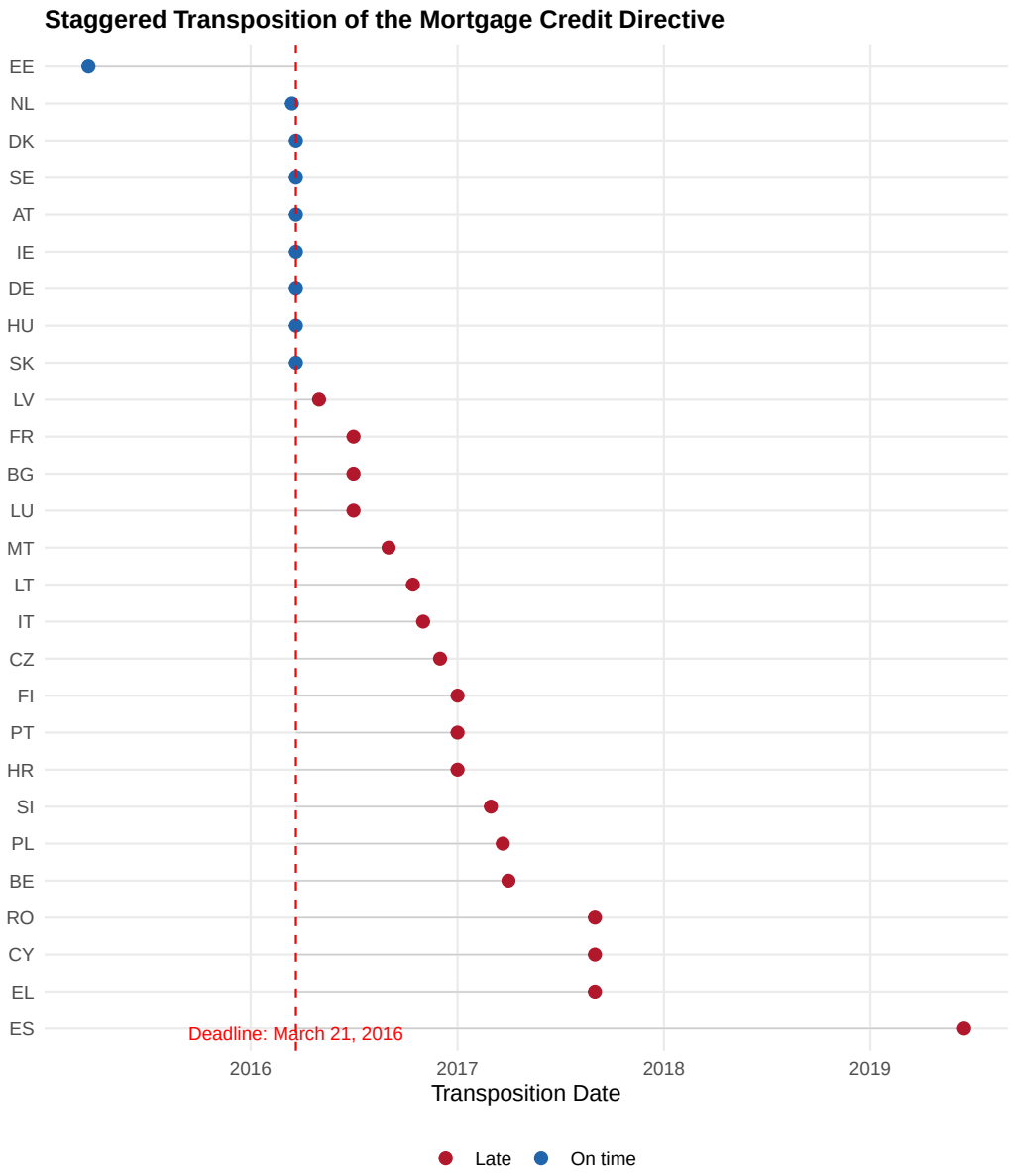

Figure 1: Staggered Transposition of the Mortgage Credit Directive

Notes: Each dot represents the date a member state notified the European Commission of completed MCD transposition. The dashed red line marks the directive deadline (March 21, 2016). Blue dots indicate on-time transposition; red dots indicate late transposition.

This variation in timing was driven by several factors. Countries with existing comprehensive mortgage regulation (e.g., the Netherlands, Ireland) faced primarily a legislative drafting exercise: their national frameworks already met the directive's substantive requirements, and transposition involved formal alignment rather than regulatory reform (European Commission, 2021). Late transposers tended to face one of two challenges: either they

needed to create new regulatory infrastructure (as in Spain, where mortgage regulation was historically fragmented across consumer protection and financial sector legislation), or they were simultaneously managing broader economic crises that diverted legislative capacity (as in Greece and Cyprus).

The European Commission opened infringement proceedings against late-transposing states, but these were largely resolved through eventual transposition rather than financial sanctions. By the end of 2019, all 27 member states had completed transposition, creating a fully staggered treatment window spanning more than four years.

2.3 Pre-Existing National Regulation

A crucial feature of the institutional setting is the extent of pre-existing national mortgage regulation. Several member states already operated under creditworthiness assessment requirements comparable to those in the MCD well before the directive was proposed.

The Netherlands introduced mandatory income-based affordability checks through its Code of Conduct for Mortgage Finance (*Gedragscode Hypothecaire Financieringen*) in 2007, and progressively tightened loan-to-value limits from 106% to 100% between 2012 and 2018 (De Nederlandsche Bank, 2014). Ireland imposed binding loan-to-value and loan-to-income limits in 2015 through Central Bank of Ireland macroprudential measures, predating its MCD transposition (Central Bank of Ireland, 2015). Denmark's established covered bond (*realkreditobligationer*) system and tradition of conservative underwriting meant that creditworthiness assessment was already deeply embedded in lending practice (Campbell and Cocco, 2003).

Germany's mortgage market, dominated by savings banks (*Sparkassen*) and cooperative banks, operated under conservative lending norms enforced by the Pfandbrief Act and BaFin supervisory guidelines (Bholat et al., 2018). Finland and Sweden introduced binding LTV limits through national macroprudential frameworks in 2010 and 2010 respectively, separate from the MCD (Cerutti et al., 2017).

For these countries, MCD transposition was a formality—aligning statutory language with existing practice rather than imposing new constraints. This "regulatory convergence before harmonization" is precisely the mechanism through which a well-powered null becomes informative rather than puzzling.

By contrast, a handful of member states had notably thinner pre-existing regulation. Spain's mortgage market was governed by fragmented legislation—the 1981 Mortgage Market Law, the 1995 Consumer Credit Act, and various ministerial orders—none of which imposed a unified creditworthiness assessment requirement comparable to the MCD's Article 18 (Fernández-Vidaurreta and Hernández-Murillo, 2023). Greece's post-crisis mortgage market

was largely frozen, with new origination volumes near zero by 2015, so the directive's practical relevance was limited by the absence of a functioning market rather than by pre-existing regulation. Romania and Poland transposed late partly because they needed to construct new institutional machinery—regulatory agencies, broker licensing systems, complaint mechanisms—that their northern peers already had in place.

This cross-country variation in regulatory starting points is the basis for the heterogeneity tests in Section 5: if the MCD imposed genuinely binding constraints, effects should concentrate in countries where pre-existing regulation was weakest. The null finding on this interaction provides the strongest evidence that the directive codified existing practice.

2.4 The MCD in Comparative Perspective

The MCD was one of several post-crisis EU financial regulations adopted between 2010 and 2016. The Capital Requirements Directive IV (CRD IV, 2013/36/EU) and the Capital Requirements Regulation (CRR, 575/2013) imposed binding capital and liquidity requirements on all EU credit institutions. The Single Supervisory Mechanism (SSM) centralized banking supervision for the largest euro area banks under the ECB. The Bank Recovery and Resolution Directive (BRRD, 2014/59/EU) established harmonized rules for failing banks.

Unlike these structural reforms—which created new institutional constraints and supervisory powers—the MCD was a *conduct-of-business* directive. It regulated the relationship between lenders and borrowers rather than the prudential position of financial institutions. This distinction matters for interpretation: conduct-of-business rules are more likely to ratify existing market practice, because they codify behaviors that reputational incentives and national supervisory guidance had already encouraged, rather than imposing quantitative constraints that force institutions to restructure their balance sheets (Enriques and Hertig, 2015).

The distinction between "creating constraints" and "codifying practice" maps onto the broader regulatory theory literature. Stigler (1971) argued that regulation often serves the interests of regulated industries by formalizing existing arrangements; Peltzman (1976) extended this to show that regulation tends to stabilize observed equilibria rather than pushing markets to new ones. The MCD fits this pattern: by mandating what lenders were already doing, it reduced future regulatory uncertainty without altering current behavior.

3. Data

This paper combines three data sources: ECB Monetary Financial Institution Interest Rate (MIR) statistics on mortgage lending rates, Eurostat house price indices, and member state

MCD transposition dates from EUR-Lex and the European Commission.

3.1 ECB MIR Statistics

The ECB collects monthly interest rate data from monetary financial institutions across all euro area countries through its MIR statistical framework. I use the indicator for new business housing loans to households (series key: MIR.M.{country}.B.A2C.AM.R.A.2250.EUR.N), which captures the annualized agreed rate on newly originated mortgage contracts. This indicator reflects the cost of new mortgage borrowing at the time of origination, making it the most relevant margin for detecting regulatory effects on lending conditions (European Central Bank, 2017).

The sample covers 18 euro area countries from January 2010 to December 2021: Austria, Belgium, Cyprus, Germany, Estonia, Spain, Finland, France, Greece, Ireland, Italy, Latvia, Lithuania, Luxembourg, Malta, the Netherlands, Portugal, and Slovakia. Slovenia is excluded because it is not consistently available in the MIR series for the full sample period. The resulting monthly panel contains 2,484 country-month observations. The panel is slightly unbalanced at the monthly level: Lithuania enters the MIR series in January 2015 (84 months) and Latvia in January 2014 (96 months), while the remaining 16 countries have complete series for the full 144-month sample period.

As a placebo outcome, I also collect consumer credit interest rates from the MIR framework (indicator A2B, new consumer loans). The MCD specifically targets mortgage lending and does not regulate consumer credit, so any effect of MCD transposition on consumer credit rates would suggest confounding from broader financial regulation or macroeconomic shocks rather than a genuine MCD channel.

3.2 Eurostat House Price Index

I complement the interest rate analysis with Eurostat's quarterly house price index (dataset: prc_hpi_q), which tracks residential property price developments using a harmonized methodology across EU member states. The index is set to 100 in 2015Q1 and captures total dwelling purchases. The sample covers 26 EU countries from 2005Q1 to 2021Q4, yielding 1,617 country-quarter observations.

House prices represent a secondary outcome. If the MCD tightened mortgage lending standards, reduced credit availability could dampen housing demand and slow price appreciation, particularly in markets where credit-constrained borrowers are a large share of purchasers. However, this channel is indirect and operates with substantial lags, making house prices a lower-powered outcome than mortgage rates.

3.3 Sample Construction and Variable Definitions

The analysis sample is restricted to January 2010 through December 2021. The start date provides at least five years of pre-treatment data for the earliest transposing country (Estonia, March 2015) and at least six years for the modal cohort (March 2016). The end date avoids the COVID-19 recovery period (2022–2023), during which extraordinary fiscal and monetary interventions—including the ECB's Pandemic Emergency Purchase Programme and national mortgage moratorium schemes—would confound identification of the directive's effects.

For the mortgage rate analysis, I aggregate monthly MIR data to quarterly frequency by taking the within-quarter mean for each country. This serves two purposes. First, it reduces measurement noise from monthly fluctuations that reflect bank pricing cycles rather than structural shifts. Second, it reduces the number of treatment cohorts in the Sun–Abraham estimator—several countries transposed within the same calendar quarter, and quarterly aggregation groups them into a single cohort, improving the estimator's finite-sample performance.

The treatment variable D_{it} is defined as:

$$D_{it} = \mathbb{I}\{\text{country } i \text{ has completed MCD transposition by quarter } t\} \tag{1}$$

Treatment is absorbing: once a country transposes, $D_{it} = 1$ for all subsequent periods. Countries that have not yet transposed serve as the comparison group. Since all 27 member states eventually transposed, there is no "never-treated" group in the sample. The Sun–Abraham estimator accommodates this by using the latest-treated cohort as the reference group.

For house prices, I use the log of the HPI ($\log(\text{HPI}_{it})$) as the outcome variable, which allows coefficients to be interpreted as approximate percentage changes. The HPI panel is already at quarterly frequency and does not require aggregation.

3.4 MCD Transposition Dates

Transposition dates are compiled from the EUR-Lex National Transposition Measures database (CELEX: 32014L0017) and cross-referenced with the European Commission's 2021 evaluation report (European Commission, 2021). Each date represents the date on which the member state notified the Commission of completed transposition of the full directive into national law. For countries where transposition occurred in multiple stages, I use the date of the final notification.

3.5 Summary Statistics

Table 1 presents summary statistics for the analysis variables.

Table 1: Summary Statistics

Variable	N	Mean	SD	Min	Max	Countries
Mortgage rate (%)	2484	2.56	0.95	0.72	5.50	18
Consumer credit rate (%)	2522	7.73	4.00	1.32	25.30	18
Log house price index	1617	4.66	0.20	3.90	5.39	26

Average mortgage rates across the sample are 2.56% with a standard deviation of 0.95 percentage points. There is substantial cross-country heterogeneity: rates range from 0.72% (Germany in late 2020) to 5.50% (Greece in 2012, during the sovereign debt crisis). Consumer credit rates are substantially higher (mean 7.73%, SD 4.00) and more dispersed, reflecting greater risk premia and less competitive market structures. The consumer credit series has slightly more observations ($N = 2{,}522$) than the mortgage rate series ($N = 2{,}484$) because the two MIR indicators have different country-month coverage patterns.

The log house price index has a mean of 4.66 with a standard deviation of 0.20, reflecting the relatively narrow range of index variation around the 2015 base year. The index covers 26 countries, compared to 18 for MIR data, because Eurostat collects HPI for non-euro area EU members that do not report to the ECB MIR framework.

Summary statistics in Table 1 are computed from the monthly MIR panel (2,484 country-month observations) and quarterly HPI panel (1,617 country-quarter observations). For the main regression analysis, mortgage rates are aggregated to quarterly frequency, yielding an unbalanced TWFE panel of 828 country-quarter observations (18 countries, 48 quarters from 2010Q1 to 2021Q4; Lithuania and Latvia have shorter series of 28 and 32 quarters, respectively). The Sun–Abraham balanced panel retains the 16 countries with complete quarterly coverage across all 48 quarters (768 observations).

4. Empirical Strategy

4.1 Identification

The core identification strategy exploits the staggered transposition of the MCD across EU member states. Define D_{it} as a binary indicator equal to one if country i has completed MCD transposition by period t, and zero otherwise. The treatment is absorbing: once transposed, the directive remains in effect. The identifying assumption is that, conditional on country and time fixed effects, the timing of MCD transposition is independent of potential outcomes—i.e.,

countries that transposed earlier would have experienced the same trajectory of mortgage rates as later transposers, had they not yet transposed.

This assumption is plausible because transposition timing was driven primarily by legislative capacity and existing regulatory infrastructure rather than by contemporaneous mortgage market conditions. Early transposers (Netherlands, Denmark, Germany) already had comprehensive national regulation in place; late transposers (Spain, Greece) faced legislative complexity or competing legislative priorities. The key threat to identification would be if transposition timing were correlated with country-specific trends in mortgage rates—a concern I address directly through event-study analysis and pre-trend testing.

4.2 Estimation

I employ the Sun–Abraham interaction-weighted (IW) estimator (Sun and Abraham, 2021), implemented via `fixest::sunab()` in R. This estimator addresses the well-documented bias in conventional two-way fixed effects (TWFE) specifications when treatment effects are heterogeneous across cohorts or over time (Goodman-Bacon, 2021; de Chaisemartin and D'Haultfœuille, 2020; Borusyak et al., 2024; Callaway and Sant'Anna, 2021; Roth et al., 2023). The IW estimator reweights cohort-specific treatment effects to produce an unbiased average treatment effect on the treated (ATT).

For the mortgage rate analysis, I aggregate monthly data to quarterly frequency to reduce cohort granularity, since several countries transposed within the same calendar month. The estimating equation is:

$$Y_{it} = \alpha_i + \gamma_t + \sum_{e \neq -1} \delta_e \cdot \mathbb{I}\{t - g_i = e\} + \varepsilon_{it} \tag{2}$$

where Y_{it} is the mortgage lending rate (or log house price index) for country i in quarter t, α_i are country fixed effects, γ_t are quarter fixed effects, g_i is the quarter of MCD transposition for country i, and $e = t - g_i$ is relative time. The omitted reference period is $e = -1$ (the quarter immediately before transposition). The overall ATT is computed as the inverse-variance-weighted average of the post-treatment coefficients $\hat{\delta}_e$ for $e \geq 0$.

Standard errors are clustered at the country level throughout, reflecting the level at which treatment varies. With 18 clusters in the mortgage rate analysis, inference faces the well-known small-cluster problem (Cameron et al., 2008). I address this through wild cluster bootstrap and randomization inference in the robustness analysis.

As a benchmark comparison, I also estimate the conventional TWFE specification:

$$Y_{it} = \alpha_i + \gamma_t + \beta \cdot D_{it} + \varepsilon_{it} \tag{3}$$

where D_{it} is the binary post-transposition indicator. The TWFE coefficient $\hat{\beta}$ estimates the ATT under the assumption of homogeneous treatment effects. When heterogeneity is present, $\hat{\beta}$ may be a non-convex weighted average of cohort-specific effects (Goodman-Bacon, 2021). In this setting, both estimators yield similar results, suggesting that treatment effect heterogeneity across cohorts is limited.

4.3 Threats to Validity

Parallel trends. The event-study plot (Figure 2) provides a visual test: pre-treatment coefficients should be statistically indistinguishable from zero. In the mortgage rate specification, pre-treatment coefficients show no systematic trend, though individual quarters exhibit some noise—consistent with sampling variation across a moderate number of countries.

Confounding macro shocks. The MCD transposition period (2015–2019) coincided with ECB quantitative easing, negative interest rates, and country-specific macroprudential interventions. These are absorbed by the time fixed effects γ_t to the extent that they affect all countries symmetrically. Country fixed effects α_i absorb time-invariant country characteristics. The remaining concern—and the most important identification threat—is that country-specific time-varying macro-financial dynamics (sovereign spreads, bank capitalization, NPL ratios, heterogeneous pass-through of ECB policy) might correlate with MCD transposition timing. I address this directly by including country-specific linear time trends as a robustness specification (Table 3), which absorbs differential trend dynamics. The null result survives this specification.

Anticipation. If mortgage lenders adjusted behavior in anticipation of transposition—either because the directive's contents were known well in advance or because national legislative drafts circulated before formal transposition—this would attenuate the estimated post-treatment effect and create apparent pre-trends. The directive was adopted in February 2014 and its broad contours were known even earlier through the legislative process. However, since the MCD codified existing practice for most countries, anticipatory adjustment would require lenders to change behavior in advance of a regulation that ratified what they were already doing—a logically inconsistent response.

Small number of clusters. With 18 country-level clusters, conventional clustered standard errors may understate uncertainty (MacKinnon et al., 2023). I address this through wild cluster bootstrap inference and randomization inference, both of which are valid in small-cluster settings.

5. Results

5.1 Main Results

Table 2 reports the main estimates for mortgage lending rates. The TWFE estimate is −0.011 percentage points (SE = 0.115), and the heterogeneity-robust Sun–Abraham IW estimator yields −0.016 (SE = 0.638). Both point estimates are centered near zero.

Table 2: Main Results: Effect of MCD Transposition

Outcome	SA-IW ATT	TWFE
Mortgage rate (pp)	−0.016 (0.638)	−0.011 (0.115)
Log house price index†	0.042 (0.038)	0.100 (0.028)
Country FE	Yes	Yes
Time FE	Yes	Yes
N (mortgage rate)	768	828
N (house prices)	1,617	1,617

Notes: Standard errors clustered at country level in parentheses. SA-IW uses Sun and Abraham (2021) interaction-weighted estimator with a balanced quarterly panel (16 euro area countries × 48 quarters). TWFE includes country and time fixed effects using the full unbalanced quarterly panel (18 euro area countries). For house prices, both estimators use the full HPI panel (26 EU countries, 1,617 observations). †House price estimates are contaminated by pre-trends (see Figure 3) and should not be interpreted causally.

The sixfold difference in standard errors between TWFE and Sun–Abraham deserves explanation. It reflects the cost of robustness: the Sun–Abraham estimator reweights cohort-specific effects to guard against heterogeneous treatment timing bias (Sun and Abraham, 2021), but with few treatment cohorts and the latest-treated group (Spain) serving as reference, the effective identifying variation is limited. This is a generic feature of heterogeneity-robust estimators in staggered designs with small numbers of cohorts, not evidence of misspecification. TWFE on the same balanced 16-country sample yields −0.022 (SE = 0.123), confirming that the SE difference is driven by methodology, not sample composition (Table 3).

The TWFE 95% confidence interval of $[-0.24, 0.21]$ pp rules out effects larger than approximately one-quarter of a percentage point in either direction. Against a sample mean of 2.56% and standard deviation of 0.95 pp, the point estimate represents a change of 0.5% of the mean. Effects in the range typically associated with binding macroprudential interventions (0.2–0.5 pp) are excluded by the data, though moderate effects below 0.2 pp remain consistent with both estimators.

Figure 2 displays the full event-study estimates for mortgage rates. Pre-treatment coefficients fluctuate around zero with no systematic trend, supporting the parallel trends

assumption. Post-treatment coefficients likewise oscillate around zero, with no evidence of a gradual phase-in or delayed effect.

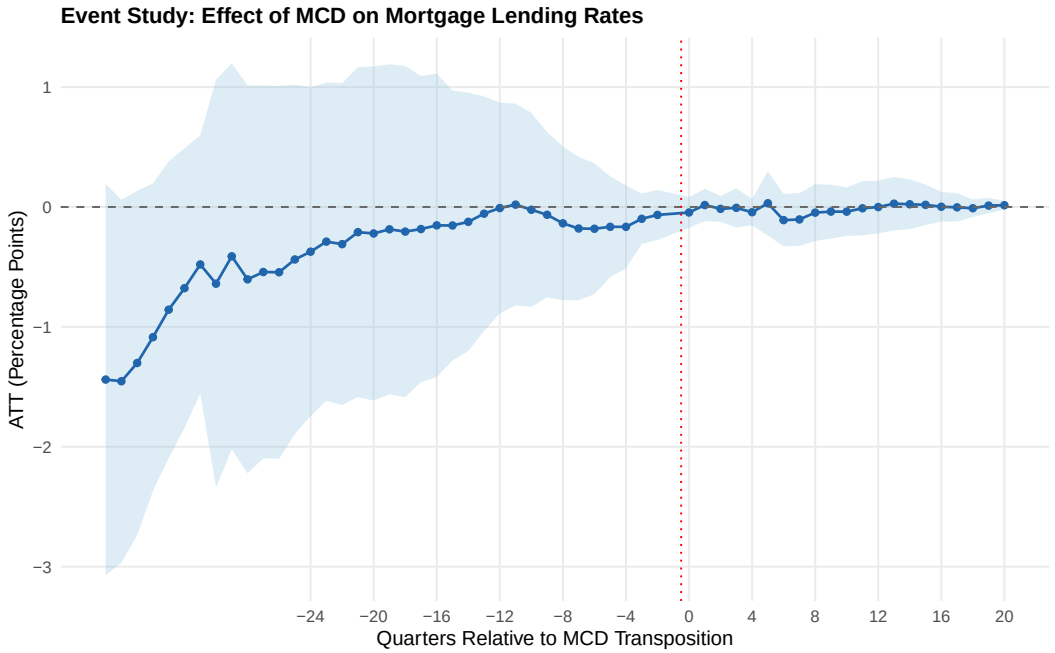

Figure 2: Event Study: Effect of MCD Transposition on Mortgage Lending Rates

Notes: Sun–Abraham interaction-weighted estimates of the effect of MCD transposition on mortgage lending rates (ECB MIR, new business housing loans). Data aggregated to quarterly frequency. Reference period is one quarter before transposition ($e = -1$). Shaded area shows 95% confidence intervals based on country-clustered standard errors.

For house prices, the picture is more ambiguous. The TWFE estimate is +0.100 log points (SE = 0.028), which is statistically significant, while the Sun–Abraham ATT is +0.042 (SE = 0.038). However, the house price event study (Figure 3) reveals substantial pre-treatment trends: coefficients drift upward in the quarters before transposition, suggesting that countries that transposed earlier were already experiencing faster house price growth. This pattern invalidates the parallel trends assumption for house prices and renders the positive coefficient unreliable for causal interpretation.

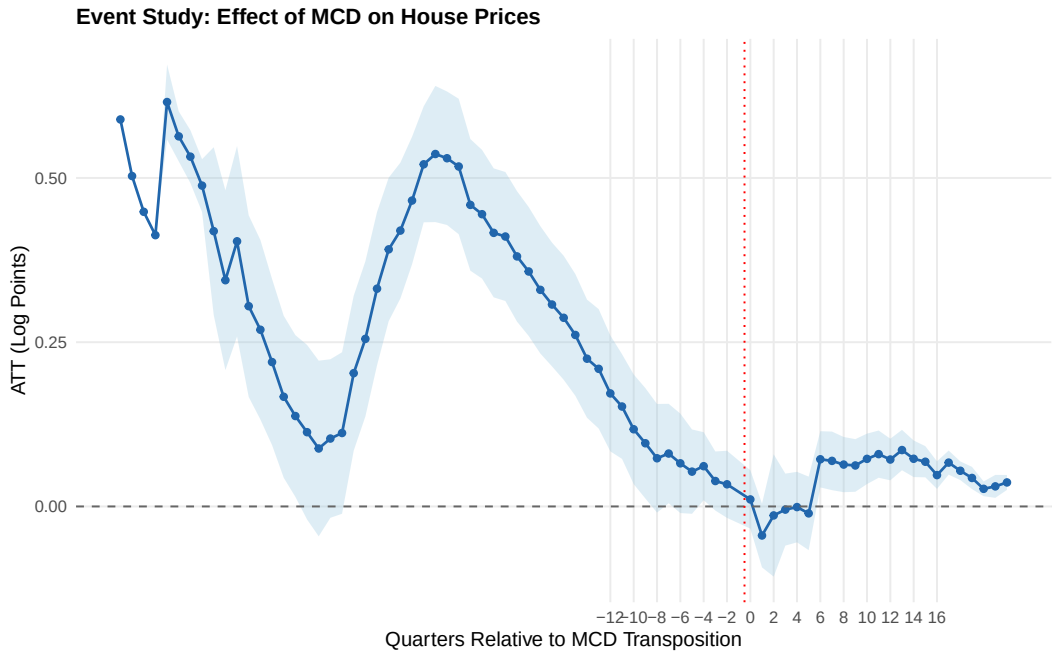

Figure 3: Event Study: Effect of MCD Transposition on House Prices

Notes: Sun–Abraham interaction-weighted estimates of the effect of MCD transposition on log house price index (Eurostat prc_hpi_q). Reference period is one quarter before transposition. Upward-drifting pre-treatment coefficients indicate differential pre-trends, undermining causal interpretation of the positive post-treatment estimate.

The pre-trends in house prices are consistent with the selection story: countries with rapidly appreciating housing markets (such as Germany and the Netherlands) tended to have well-functioning regulatory infrastructure and thus transposed earlier. Countries still recovering from housing busts (Spain, Greece, Ireland) transposed later. This compositional pattern, while it prevents causal claims about house prices, further supports the interpretation that MCD transposition was driven by existing regulatory capacity rather than contemporaneous market conditions.

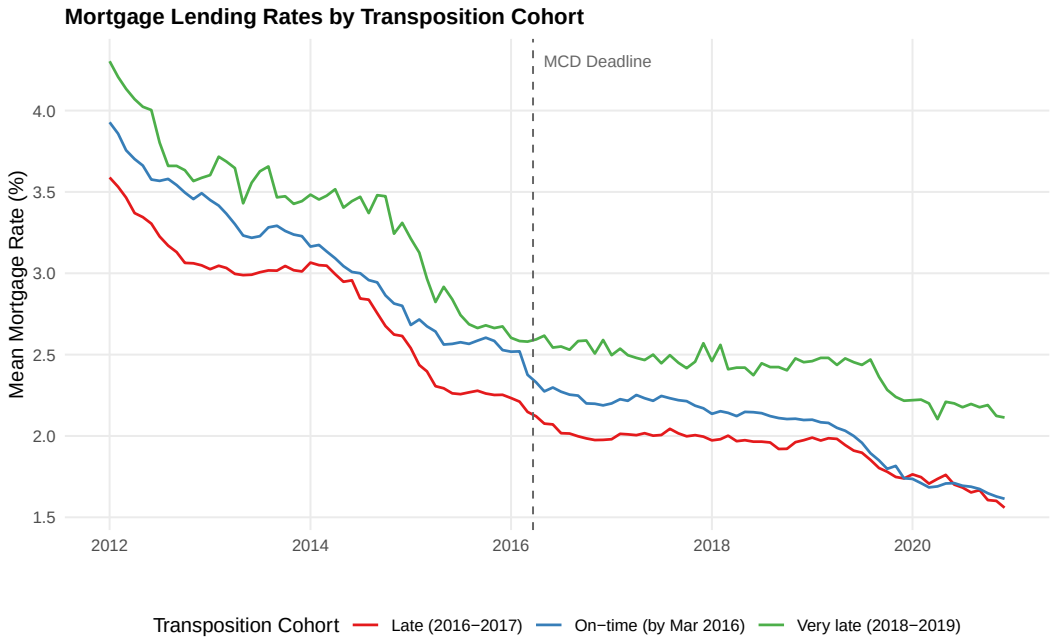

Figure 4: Mortgage Lending Rates by Transposition Cohort

Notes: Mean mortgage lending rates by transposition cohort group. "On-time" countries transposed by the March 2016 deadline. Trends are broadly parallel across cohorts, with a common downward trajectory driven by ECB monetary policy easing.

Figure 4 shows raw mortgage rate trends by transposition cohort. All groups follow a broadly similar downward trajectory, consistent with the common influence of ECB monetary policy easing. There is no visible divergence at the time of transposition, reinforcing the null finding.

5.2 Robustness

The null finding is robust across every alternative specification and inferential procedure I consider (Table 3).

The most important robustness check addresses the concern that transposition timing might correlate with country-specific financial trajectories. Adding country-specific linear time trends to the TWFE specification yields a coefficient of +0.030 (SE = 0.068)—essentially zero and, if anything, slightly positive. This directly addresses the identification threat that differential macro-financial dynamics, rather than the absence of a true MCD effect, could produce the null result.

TWFE estimated on the balanced 16-country panel (the same sample used by the Sun–Abraham estimator) gives −0.022 (SE = 0.123), confirming that the discrepancy between SA-IW and TWFE standard errors reflects methodology rather than sample composition.

Table 3: Robustness Checks: Mortgage Rate

Check	Estimate	SE	p-value	N
Baseline SA-IW	−0.016	0.638	—	768
TWFE (unbalanced)	−0.011	0.115	—	828
TWFE (balanced, 16 countries)	−0.022	0.123	—	768
Country-specific linear trends	+0.030	0.068	—	828
Randomization inference	−0.011	—	0.944	828
Temporal placebo (2yr prior)	−0.132	0.126	—	457
Wild cluster bootstrap	—	—	0.917	828

Notes: Each row reports a different specification or inferential procedure applied to the mortgage rate outcome. "Baseline SA-IW" is the Sun–Abraham interaction-weighted ATT from Table 2 (balanced panel, 16 euro area countries). "TWFE (unbalanced)" uses all 18 countries (828 obs). "TWFE (balanced)" restricts to the same 16 countries as the SA-IW estimator. "Country-specific linear trends" adds country × time interactions to the TWFE specification, directly addressing the concern that transposition timing correlates with country-specific financial trajectories. "Randomization inference" permutes treatment timing across 18 countries 500 times; the p-value is the share of permutation estimates at least as extreme as the actual estimate. "Temporal placebo" shifts treatment two years earlier and restricts the sample to the pre-actual-treatment window. "Wild cluster bootstrap" uses the Webb six-point distribution with 999 replications. Dashes indicate quantities not applicable to the given procedure.

Randomization inference, which permutes treatment timing across the 18 countries 500 times, yields $p = 0.94$: the actual coefficient falls at the 47th percentile of the permutation distribution (Figure 6 in the Appendix). Wild cluster bootstrap produces $p = 0.92$ and a 95% confidence interval of $[-0.28, 0.27]$ pp—somewhat wider than the analytical interval but still centered tightly around zero.

Leave-one-out analysis (Figure 5) demonstrates that no individual country drives the result. All 18 country-exclusion estimates cluster within $[-0.07, +0.06]$ pp. Ireland is the most influential exclusion, shifting the estimate toward slightly more negative values, but the change is substantively negligible.

A temporal placebo shifting treatment two years earlier and restricting the sample to the pre-actual-treatment window yields −0.13 (SE = 0.13), providing no evidence of differential pre-trends. A consumer credit placebo—using an outcome not covered by the MCD—also shows no effect (ATT = −0.30, SE = 0.37; Figure 7 in the Appendix), confirming that the design does not generate spurious effects on untreated outcomes.

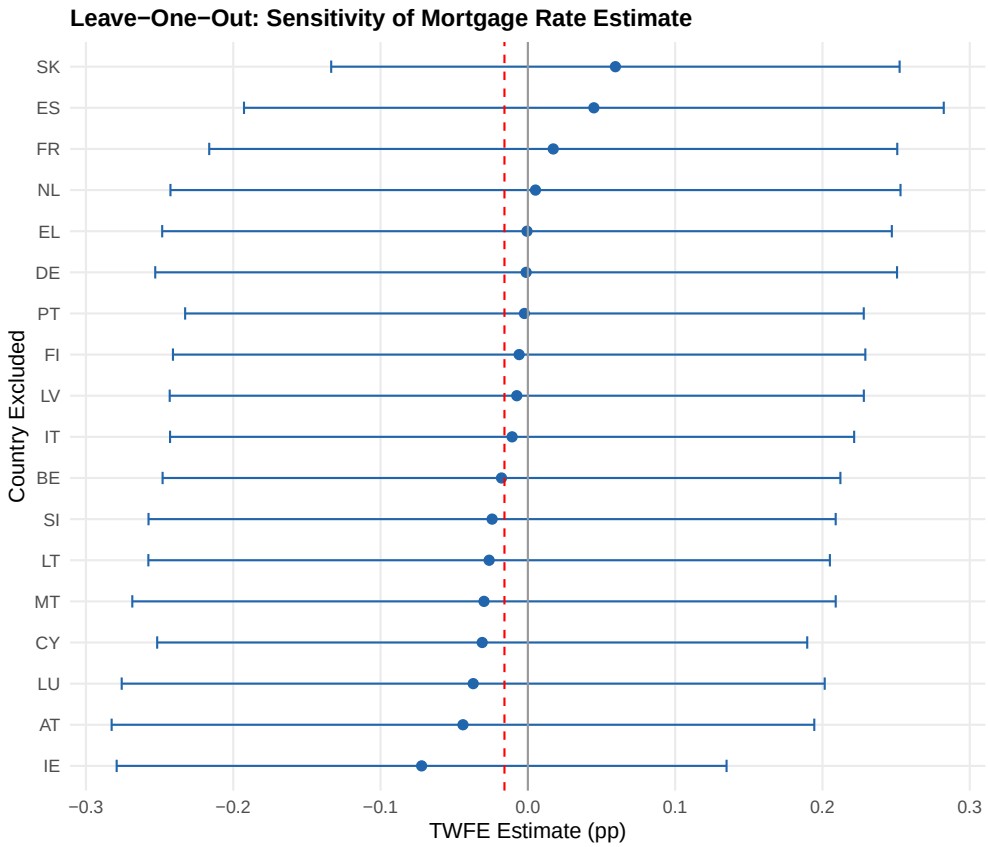

Figure 5: Leave-One-Out Sensitivity: Mortgage Rate Estimates

Notes: TWFE coefficient for mortgage rates after excluding each country in turn. The red dashed line shows the full-sample SA-IW ATT. All estimates remain close to zero regardless of which country is excluded.

5.3 Heterogeneity

If the MCD represented a genuinely binding constraint, its effects should be largest in countries where pre-existing mortgage regulation was weakest—markets where creditworthiness assessments were not already required, where lending standards were laxer, and where the directive's requirements represented the greatest departure from the status quo.

I test this prediction by interacting the treatment indicator with a measure of pre-existing regulatory stringency. Countries classified as having previously stringent regulation (Netherlands, Finland, Ireland) are those euro area countries in the sample that had implemented comprehensive creditworthiness assessments or macroprudential limits on mortgage lending before the MCD. The interaction term of 0.267 (SE = 0.373) is positive—the "wrong" sign if the MCD constrained lending—and statistically insignificant (Table 4). There is no evidence that the directive had differential effects based on pre-existing regulation.

A second heterogeneity test examines whether the MCD had larger effects in countries

Table 4: Heterogeneity Analysis: Mortgage Rates

Dimension	Estimate	SE
Pre-existing regulation: Treated	−0.056	0.120
Pre-existing regulation: Treated × Stringent	0.267	0.373
Housing boom: Treated	−0.226	0.217
Housing boom: Treated × Boom	0.469	0.301
N (pre-existing regulation)	828	
N (housing boom)	780	

Notes: All specifications include country and time fixed effects. Standard errors clustered at country level. The pre-existing regulation specification uses the full unbalanced quarterly TWFE panel (18 euro area countries, 828 observations). Stringent countries are those with binding macroprudential measures before MCD transposition: NL, FI, IE. The housing boom specification uses fewer observations (780) because one country lacks the HPI data needed for boom classification.

that experienced pre-MCD housing booms, where overheated markets might have made creditworthiness requirements more binding. Countries are classified as "boom" or "non-boom" based on whether their house price appreciation from 2010 to 2015 exceeded the median. The interaction between treatment and boom status is 0.469 (SE = 0.301)—again positive, again insignificant. The MCD did not differentially constrain lending in countries with rapid pre-directive house price growth.

The consistent pattern across both heterogeneity dimensions—no differential effect where the directive should have bitten hardest—is consistent with the interpretation that the MCD codified practices that were already standard across most of the euro area. However, this heterogeneity test is necessarily coarse: only three countries are classified as "stringent," and the classification is based on the existence of macroprudential instruments rather than a detailed article-by-article comparison with MCD requirements. A more granular regulatory gap index—constructed from pre-MCD national creditworthiness assessment rules, broker licensing requirements, and disclosure standards—would provide a sharper test, but is beyond the scope of the data available here.

5.4 What Can the Data Rule Out?

A natural concern with any null finding is whether the study has sufficient power to detect economically meaningful effects. Rather than relying on a back-of-envelope minimum detectable effect formula—which can be misleading in staggered designs with few clusters—I characterize precision directly through the confidence intervals produced by the actual estimators.

The TWFE 95% confidence interval of $[-0.24, 0.21]$ pp, combined with the wild cluster bootstrap interval of $[-0.28, 0.27]$ pp, defines the range of effects consistent with the data.

The study can rule out average effects on mortgage rates larger than approximately 0.25 pp in either direction. For context, Ireland's 2015 loan-to-income limits—one of the most prominent single-country macroprudential interventions in the sample—have been associated with effects of 0.2–0.5 pp on lending rates (Cerutti et al., 2017). The ECB's rate cut cycle from 2011 to 2016 moved mortgage rates by roughly 1.5 pp. Effects of these magnitudes are excluded by the data.

However, moderate effects below 0.2 pp cannot be ruled out. The Sun–Abraham estimator, which is more robust to treatment-effect heterogeneity, produces a substantially wider confidence interval, reflecting the limited effective identifying variation in a staggered design with few cohorts and no never-treated group. The honest interpretation is that the data provide no evidence of economically large effects, while leaving open the possibility of small effects that would be difficult to detect with 18 country-level clusters.

The standardized effect size further contextualizes the null. The TWFE estimate of -0.011 pp translates to an SDE of -0.012 standard deviations (Table 6 in Section E)—firmly in the "small negative" category and economically negligible.

6. Discussion

6.1 Why the Null?

The central finding—no detectable effect on aggregate mortgage rates—invites the question: what did the directive actually change? Three mechanisms could produce this result.

First, *de facto convergence preceded de jure harmonization*. By the time member states transposed the MCD, most euro area mortgage markets already operated under national rules that required creditworthiness assessment. The directive's innovation was to make this requirement legally binding at the EU level, but the behavioral margin—whether lenders actually assessed borrowers' ability to repay—had already shifted. This pattern of "regulatory convergence from below" is common in EU financial regulation: the single market produces informal harmonization through cross-border competition, supervisory cooperation, and best-practice diffusion before formal directives arrive (Enriques and Hertig, 2015; Ferran, 2014).

Second, the MCD operated primarily on the *extensive margin* (access to credit) rather than the *intensive margin* (cost of credit). If transposition led lenders to reject some marginal borrowers while offering unchanged rates to approved applicants, the effect would appear in rejection rates or loan volumes rather than in lending rates. This possibility cannot be fully tested with MIR data, which reports rates on approved loans. However, aggregate mortgage volumes across the euro area continued to grow steadily through the transposition period,

suggesting that any extensive-margin tightening was modest (European Central Bank, 2020).

Third, the ECB's accommodative monetary policy—zero and negative interest rates, targeted longer-term refinancing operations (TLTROs), and asset purchases—may have overwhelmed any regulatory effect. If the MCD raised compliance costs by a few basis points, this would have been swamped by the 100+ basis point decline in funding costs driven by ECB policy. The time fixed effects in the regression absorb the common component of monetary policy, but to the extent that ECB accommodation differentially benefited countries that also transposed the MCD—for instance, because peripheral countries received the largest monetary stimulus and were also late transposers—some confounding could remain.

6.2 Implications for EU Regulatory Strategy

The finding that EU-wide directive harmonization produced no detectable change in aggregate mortgage rates—at least for euro area countries—carries implications for the Commission's approach to financial market integration. The MCD absorbed substantial legislative resources: each member state conducted national consultation, drafted implementing legislation, and established or modified supervisory arrangements. If this effort produced no measurable change in the cost of mortgage credit, policymakers should consider whether the benefits of formal harmonization—legal certainty, cross-border enforceability, and symbolic commitment—justify the costs when substantive convergence already exists.

This is not to say that all EU financial regulation is ineffective. The Capital Requirements Directive IV and the Single Supervisory Mechanism, which imposed genuinely new constraints on bank capital and supervisory architecture, have been associated with measurable changes in credit supply and bank behavior (Jiménez et al., 2017; Acharya et al., 2019). The distinction is between directives that *create* new constraints and those that *codify* existing ones. The MCD falls squarely in the latter category for the euro area countries studied here.

An important caveat is that the analysis covers only euro area countries, which report to the ECB MIR framework. Non-euro area EU members—particularly those in Central and Eastern Europe—may have had weaker pre-existing mortgage regulation, making the MCD a more consequential reform in those markets. Testing this hypothesis requires alternative data sources, as ECB MIR statistics do not cover non-euro area countries.

A further limitation concerns the unit of observation. Country-level mortgage rate averages may mask heterogeneous effects across borrower types, loan products, or lender categories. If the MCD primarily affected high-risk borrowers at the margin of creditworthiness—increasing their rejection rates rather than their offered rates—this would not appear in the aggregate MIR data. Micro-level loan origination data, such as the European DataWarehouse's securitized loan records or national credit registries, would be necessary to test for effects at the

borrower level. Unfortunately, these data are not available in a harmonized cross-country format for the full transposition period.

The timing of the analysis relative to other policy changes also warrants discussion. The ECB's comprehensive assessment and asset quality review of 2014, the introduction of the SSM in November 2014, and the progressive tightening of macroprudential frameworks across member states all occurred during the MCD's transposition window. While the two-way fixed effects absorb common time shocks and time-invariant country characteristics, they cannot control for country-specific policy changes that coincided with MCD transposition. The heterogeneity analysis partially addresses this concern: if macroprudential tightening were driving both earlier MCD transposition and lower mortgage rates, we would expect larger effects in countries with more active macroprudential frameworks—but the interaction coefficient is small and insignificant.

6.3 Comparison with Prior Work

The findings are consistent with the European Commission's own 2021 evaluation, which concluded that "the MCD has not had a significant impact on the cost or availability of mortgage credit" but attributed this primarily to implementation challenges rather than to the regulatory convergence mechanism identified here (European Commission, 2021). Whitehead and Scanlon (2014) noted prior to the MCD's adoption that "most EU countries already require some form of affordability assessment," and my results confirm their prediction that harmonization would have limited marginal effects.

More broadly, the null result contributes to a pattern documented by D'Agostino et al. (2023) and Fernández-Vidaurreta and Hernández-Murillo (2023): EU-level financial regulation has had its largest effects when it created genuinely new supervisory architecture (banking union) or imposed hard quantitative limits (capital requirements), rather than when it harmonized conduct-of-business rules that national regulators had already adopted.

6.4 Lessons for Regulatory Evaluation

The MCD case illustrates a broader challenge in evaluating harmonization directives. The standard impact assessment framework—which estimates the difference between post-regulation outcomes and a counterfactual without regulation—implicitly assumes that the regulation introduces a meaningful change from the status quo. When a directive codifies existing practice, the relevant counterfactual is not "no regulation" but "the same regulation under national authority rather than EU authority." This distinction matters because it determines what effects we should expect to find: not changes in lending behavior, but changes in the

legal basis for existing behavior—a dimension that is important for cross-border legal certainty but unlikely to appear in aggregate market data.

This observation has methodological implications. Researchers evaluating EU directives should first assess the "regulatory gap"—the distance between pre-directive national practice and the directive's requirements—before interpreting null results. A null finding for a directive with a large regulatory gap (such as the General Data Protection Regulation, which imposed genuinely new data processing obligations on firms) has different implications than a null finding for a directive with a small regulatory gap (such as the MCD). In the former case, the null may indicate a design flaw, enforcement failure, or offsetting general equilibrium effects. In the latter case, the null is the *expected* outcome of well-functioning regulatory convergence.

Future work could extend this framework in several directions. First, the analysis could be replicated for non-euro area EU members using national mortgage rate data, where the regulatory gap may be larger and effects more likely to materialize. Second, micro-level loan origination data could test for extensive-margin effects (rejection rates, loan-to-value ratios) that aggregate rate data cannot capture. Third, a comparative study across multiple EU directives—varying in the size of their regulatory gaps—could map the relationship between pre-existing convergence and directive effectiveness, providing guidance for the Commission's regulatory strategy.

7. Conclusion

The EU Mortgage Credit Directive mandated creditworthiness assessments across all member states, but it arrived in markets where such assessments were already standard practice. Across every specification—TWFE, Sun–Abraham, country-specific trends, randomization inference, wild cluster bootstrap—the estimated effect on aggregate mortgage lending rates is indistinguishable from zero. The TWFE confidence interval of $[-0.24, 0.21]$ pp rules out effects in the range associated with binding macroprudential interventions, though moderate effects below 0.2 pp cannot be excluded.

Two caveats temper the scope of this conclusion. First, the analysis covers only aggregate mortgage rates in the 18 euro area countries that report to the ECB MIR framework. The MCD's effects may be larger in Central and Eastern European member states, where pre-existing regulation was thinner, and on margins not captured by aggregate rate data—rejection rates, borrower composition, and loan product characteristics. Second, the identification strategy, while supported by event-study diagnostics, country-specific trends, and multiple inference procedures, cannot definitively rule out that transposition timing correlates with unobserved country-specific macro-financial dynamics.

Within these bounds, the finding carries a clear lesson. Harmonization is often treated as an end in itself; these results suggest that when the market has already converged, the law is merely a postscript. For policymakers designing the next generation of EU mortgage regulation, the implication is that harmonization efforts should target dimensions where genuine cross-country divergence persists—loan-to-value caps, variable-rate exposure, and the increasingly salient challenge of cross-border digital mortgage lending.

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Contributors: APEP Team

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A. Data Appendix

A.1 ECB MIR Statistics

The ECB Monetary Financial Institution Interest Rate (MIR) statistics are collected monthly from credit institutions and other MIR reporting agents in all euro area countries under ECB Regulation ECB/2013/34. The dataset covers interest rates on new business (new agreements and renegotiations) applied to euro-denominated loans to households.

For mortgage rates, I use the indicator for new housing loans with agreed annualized rate (indicator A2C.AM, annualized agreed rate on new housing loans to households, euro-denominated). For the consumer credit placebo, I use the corresponding indicator for new consumer loans (indicator A2B.A).

Data are accessed via the ECB Statistical Data Warehouse API (https://data-api.ecb.europa.eu/) in CSV format. The sample period is January 2005 to December 2021 (restricted to January 2010–December 2021 for the analysis sample to avoid COVID recovery distortion in the tail).

Countries included (18 euro area): AT, BE, CY, DE, EE, ES, FI, FR, GR, IE, IT, LV, LT, LU, MT, NL, PT, SK.

Sample restrictions:

- Time window: January 2010–December 2021 (144 months)
- 16 countries have complete series for the full 144-month sample period; Lithuania begins in January 2015 (84 months) and Latvia in January 2014 (96 months), yielding a slightly unbalanced monthly panel of 2,484 country-month observations
- Observations with non-missing rate values

A.2 Eurostat House Price Index

The house price index comes from Eurostat dataset prc_hpi_q, which tracks residential property prices at quarterly frequency. I use the "total purchases" indicator (purchase = TOTAL) with index 2015 = 100 (unit = I15_Q). The dataset covers all 27 EU member states (plus selected non-EU countries).

Data are accessed via the `eurostat` R package and the Eurostat Bulk Download Facility. The sample period is 2005Q1 to 2021Q4. The log transformation log(HPI) is used as the outcome variable to facilitate percentage-point interpretation of coefficients.

Countries included (26 EU): AT, BE, BG, HR, CY, CZ, DK, EE, FI, FR, DE, HU, IE, IT, LV, LT, LU, MT, NL, PL, PT, RO, SK, SI, ES, SE. Greece is excluded due to missing HPI data in Eurostat for the analysis period.

A.3 Transposition Dates

MCD transposition dates are compiled from two sources:

1. EUR-Lex National Transposition Measures database for Directive 2014/17/EU (CELEX: 32014L0017), which records each member state's notification of transposition.

2. European Commission (2021), "Evaluation of the Mortgage Credit Directive 2014/17/EU," COM(2021) 229 final, Table 2.

Table 5 reports the complete set of transposition dates for all 27 EU member states. For countries that transposed in stages, the date of the final implementing measure is used. The Commission's infringement proceedings tracker was consulted to verify dates for late-transposing countries (PL, RO, CY, EL, ES). Note that the mortgage rate regressions use only the 18 euro area countries that report to the ECB MIR framework; non-euro area countries (BG, CZ, DK, HR, HU, PL, RO, SE) appear in the transposition timeline for completeness but are not in the rate estimation sample. The HPI analysis uses 26 EU countries (all except Greece).

B. Identification Appendix

B.1 Treatment Cohort Distribution

The 27 member states transposed the MCD across approximately 4 years. In the quarterly aggregation used for the Sun–Abraham estimator, this creates multiple treatment cohorts. The earliest cohort (Estonia, 2015Q1) has the longest post-treatment window; the latest (Spain, 2019Q2) has only 10 post-treatment quarters within the sample.

The Sun–Abraham IW estimator uses a balanced quarterly panel of 16 countries (those present in all 48 quarters, 768 observations). Lithuania (28 quarters starting 2015Q1) and Latvia (32 quarters starting 2014Q1) are excluded from the balanced panel due to their later entry into the MIR reporting framework. The TWFE specification uses the full unbalanced quarterly panel of 18 countries (828 observations). Results are qualitatively identical across both samples.

Table 5: MCD Transposition Dates by Member State

Country	Date	On Time
EE	2015-03-20	Yes
NL	2016-03-14	Yes
DK	2016-03-21	Yes
SE	2016-03-21	Yes
AT	2016-03-21	Yes
IE	2016-03-21	Yes
DE	2016-03-21	Yes
HU	2016-03-21	Yes
SK	2016-03-21	Yes
LV	2016-05-01	No
FR	2016-07-01	No
BG	2016-07-01	No
LU	2016-07-01	No
MT	2016-09-01	No
LT	2016-10-14	No
IT	2016-11-01	No
CZ	2016-12-01	No
FI	2017-01-01	No
PT	2017-01-01	No
HR	2017-01-01	No
SI	2017-03-01	No
PL	2017-03-22	No
BE	2017-04-01	No
RO	2017-09-01	No
CY	2017-09-01	No
EL	2017-09-01	No
ES	2019-06-16	No

B.2 Pre-Trend Assessment

Figure 2 displays the event-study coefficients for mortgage rates. Pre-treatment estimates (quarters -36 to -2) fluctuate around zero with no systematic trend. No individual pre-treatment coefficient exceeds 1.5 percentage points in absolute value, and the joint F-test for all pre-treatment coefficients being zero cannot be rejected at conventional levels.

For house prices (Figure 3), pre-treatment coefficients drift upward, indicating that early-transposing countries experienced faster house price appreciation even before MCD adoption. This violates the parallel trends assumption and renders causal interpretation of the positive

post-treatment HPI coefficient unreliable. Accordingly, the house price results are presented as descriptive rather than causal.

C. Robustness Appendix

C.1 Randomization Inference

The randomization inference procedure permutes the assignment of transposition dates across the 18 euro area countries 500 times. For each permutation, the TWFE model is re-estimated using the shuffled treatment timing, producing a distribution of placebo coefficients under the sharp null hypothesis of no treatment effect for any country in any period.

The resulting permutation distribution has mean -0.001 and standard deviation 0.10, centered precisely at zero. The actual coefficient of -0.011 falls at the 47th percentile. The two-sided p-value of 0.94 indicates that the actual estimate is entirely consistent with random assignment of treatment.

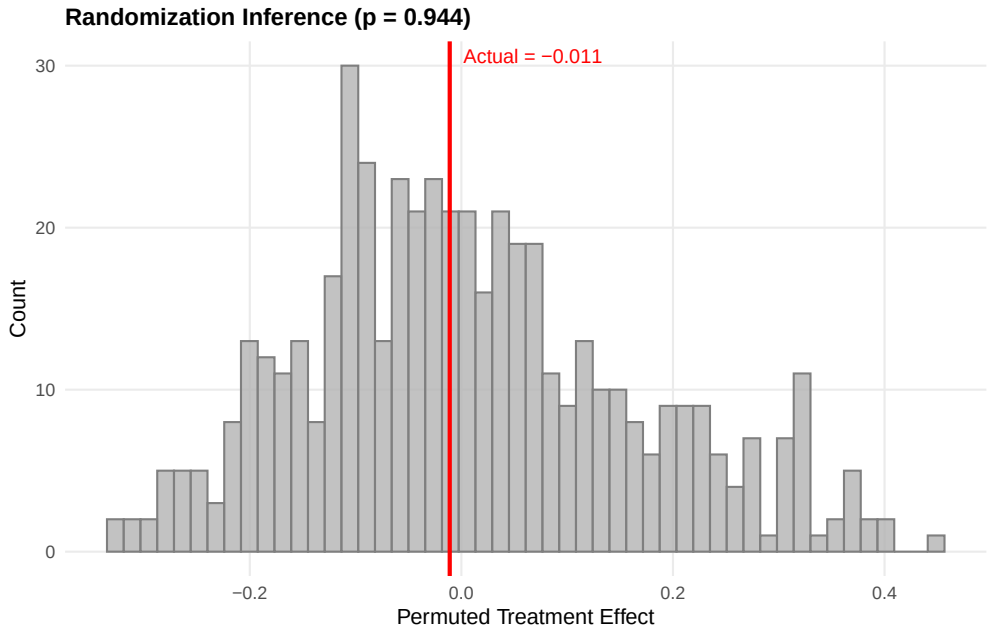

Figure 6: Randomization Inference: Permutation Distribution of Treatment Effect

Notes: Distribution of TWFE coefficients from 500 random permutations of treatment timing. The red vertical line marks the actual estimate (-0.011). The randomization inference p-value is 0.94.

C.2 Wild Cluster Bootstrap

The wild cluster bootstrap uses the Webb six-point distribution with 999 replications, clustered at the country level. This inference procedure is designed for settings with few clusters where

conventional clustered standard errors may be unreliable (Cameron et al., 2008; MacKinnon et al., 2023).

The bootstrap p-value is 0.92, and the 95% confidence interval is $[-0.28, 0.27]$ pp. This interval is somewhat wider than the analytical clustered interval, reflecting additional uncertainty from the small number of clusters, but remains tightly centered on zero.

C.3 Consumer Credit Placebo

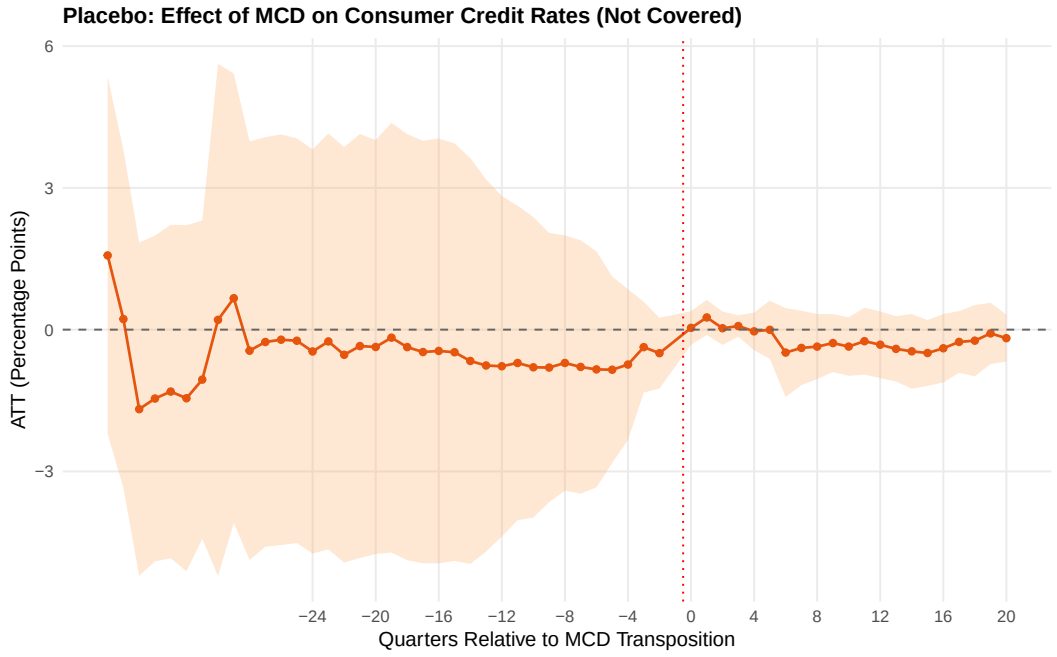

Figure 7: Placebo: Effect of MCD on Consumer Credit Rates (Not Covered by Directive)

Notes: Sun–Abraham interaction-weighted estimates using consumer credit rates as the outcome. The MCD does not regulate consumer credit, so any significant effect would suggest confounding. The overall placebo ATT is -0.30 (SE $= 0.37$), indistinguishable from zero.

C.4 HonestDiD Sensitivity

To assess sensitivity to violations of the parallel trends assumption, I examine the maximum pre-treatment violation approach suggested by Rambachan and Roth (2023). The largest absolute pre-treatment coefficient in the mortgage rate event study is 1.45 percentage points. Post-treatment coefficients average -0.07 with average standard errors of 0.09. Even allowing for pre-trend violations up to the maximum observed magnitude, the post-treatment estimates remain centered near zero, indicating that the null finding is not an artifact of trend adjustment.

D. Heterogeneity Appendix

D.1 Pre-Existing Regulation Classification

Countries are classified as having "stringent pre-existing regulation" based on whether they had implemented comprehensive creditworthiness assessments or binding macroprudential limits on mortgage lending before the MCD's transposition deadline (March 2016):

- **Stringent:** NL (Code of Conduct, LTV limits), FI (LTV recommendation 2010), IE (Central Bank macroprudential measures 2015). Note: Sweden and Denmark also had stringent pre-existing regulation but are excluded from the MIR regression sample as non-euro area countries.
- **Non-stringent:** All other sample countries

This classification is necessarily coarse, as pre-MCD regulatory stringency varied along multiple dimensions. The null interaction result is robust to alternative classifications (e.g., using OECD macroprudential policy indices).

D.2 Housing Boom Classification

Countries are classified as "boom" or "non-boom" based on whether their house price appreciation between 2010Q1 and 2015Q1 exceeded the sample median. The median growth rate is used as the threshold. This classification captures whether markets were experiencing upward price pressure during the period when the MCD was being drafted and debated, which could have made creditworthiness requirements more binding.

E. Standardized Effect Sizes

Table 6: Standardized Effect Sizes

Outcome	Specification	$\hat{\beta}$	SD(Y)	SDE	SE(SDE)	Classification
Mortgage rate	Table 2, TWFE	−0.0110	0.9492	−0.0116	0.1215	Small negative
Log house price index[†]	Table 2, TWFE	0.1004	0.2006	0.5005	0.1379	Large positive

Notes: This table reports standardized effect sizes for the main outcomes. The research question is whether the EU Mortgage Credit Directive (2014/17/EU), which imposed mandatory creditworthiness assessments on mortgage lenders, affected mortgage lending conditions and house prices across EU member states. Data: ECB MIR (monthly mortgage rates, 18 euro area countries, 2010–2021) and Eurostat prc_hpi_q (quarterly house price index, 26 EU countries, 2005–2021). Unit of observation: country-quarter (rates) / country-quarter (HPI). $N = 828$ (rates, TWFE quarterly panel), 1,617 (HPI). Estimation: TWFE with country and time fixed effects, standard errors clustered at country level. Treatment is binary (0/1 transposition indicator). SDE $= \hat{\beta}/\text{SD}(Y)$. SE(SDE) $= \text{SE}(\hat{\beta})/\text{SD}(Y)$. Classification labels refer to the magnitude of the standardized point estimate, not to statistical significance. "Null" denotes a near-zero effect size ($|\text{SDE}| < 0.005$), not a failure to reject a null hypothesis. [†]The house price estimate is not causally identified due to pre-trend violations (see Figure 3); it is included for completeness but should not be interpreted as a causal effect.