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CLIMATE DISCLOSURE MANDATES AND HIDDEN CREDIT RISK IN BANK PORTFOLIOS

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Abstract:

This analysis examines whether European nonfinancial reporting requirements are associated with recognized banking credit risk, addressing the international debate over disclosure regulation and financial stability. An unbalanced panel of 58 annual observations across six countries during 2010 to 2020 is estimated using ordinary least squares with country and year fixed effects and country clustered inference. Reporting framework exposure corresponds to a 1.046 percentage point lower nonperforming loan ratio, but the 95 percent confidence interval spans a decrease of 4.013 to an increase of 1.920 percentage points. Growth and inflation associations are also statistically imprecise. The policy coefficient remains negative across alternative samples, while inflation changes direction when 2020 is excluded. The estimates cannot distinguish improved monitoring from changes in impairment recognition. The contribution is to qualify market discipline expectations by separating directional consistency from evidence of improved credit quality. International regulatory evaluations should connect formal reporting obligations with verified implementation and observed loan performance before assigning prudential benefits.

Key Words: Nonfinancial Disclosure; Banking Regulation; Credit Risk; Nonperforming Loans; Panel Data

1. Introduction:

Disclosure regulation promises better information, but better information does not necessarily produce an immediate decline in reported credit risk. Greater transparency may strengthen borrower screening and external monitoring, while also exposing impairment that previously remained less visible. For banking supervisors and investors, these responses have different implications: a lower nonperforming loan ratio may indicate improved asset quality, whereas a higher ratio may reflect more complete recognition. Evaluating reporting reform therefore requires distinguishing regulatory exposure from changes in recognized loan performance. Leuz and Wysocki (2016) identify this separation between reporting requirements, information outcomes and economic consequences as a central challenge in disclosure research.

Environmental reporting adds a further dimension to this problem because changes in environmental conditions and regulation can affect borrower cash flows, asset values and repayment capacity. Krueger et al. (2020) document that institutional investors consider climate risks financially relevant and use engagement and risk management to address them. However, evidence that investors value climate information does not establish that reporting requirements improve bank loan quality. The European nonfinancial reporting framework provides an institutional setting for examining this distinction. Directive 2014/95/EU requires qualifying entities to report information on environmental and other nonfinancial matters, with application to financial years beginning during 2017 (European Parliament & Council of the European Union, 2014). Its breadth makes it relevant to environmental transparency, but prevents interpreting framework exposure as a measure of climate disclosure alone.

Information asymmetry and market discipline provide the conceptual basis for the analysis. Bank managers possess information about borrowers that external monitors cannot fully observe. More informative reporting can strengthen scrutiny of lending decisions and increase incentives to identify vulnerable exposures. Yet the consequences depend on what becomes visible and how managers use reporting discretion. Bushman and Williams (2012) distinguish provisioning that anticipates future credit deterioration from provisioning used to smooth earnings, finding different implications for risk discipline. Beatty and Liao (2011) further show that delayed expected loss recognition affects banks' willingness to lend during contractions. Together, these findings establish that reporting behaviour and credit outcomes are connected without implying that additional disclosure uniformly reduces recognized impairment.

The closest research also cautions against attributing economic changes to reporting requirements without considering institutional conditions. Grewal et al. (2019) find that market reactions to mandatory nonfinancial disclosure vary with firms' existing disclosure and nonfinancial performance. Their outcome captures investor valuation rather than bank loan impairment. Christensen et al. (2013), examining mandatory accounting reform, distinguish changes in reporting standards from concurrent enforcement changes. These papers show why formal regulatory adoption cannot substitute for evidence about implementation or economic

consequences. The reviewed evidence does not establish whether exposure to the European nonfinancial reporting framework is associated with national banking systems reported nonperforming loan ratios after accounting for economic conditions and common annual developments.

Macroeconomic conditions provide competing explanations for changes in loan quality. Output growth can strengthen business revenue, employment and debt servicing, while contraction can impair repayment capacity. Inflation may reduce the real burden of nominal debt when income adjusts, but may increase repayment pressure when household expenses and business costs rise faster than income. Beck et al. (2015) identify macroeconomic determinants of nonperforming loans across 75 countries and show that some relationships depend on financial structure. Ghosh (2015) identifies growth and inflation relationships using regional variation within the United States. These findings justify including growth and inflation separately, while also indicating that relationships estimated across broader or more detailed samples may not transfer directly to a short international policy comparison.

This analysis measures credit quality through nonperforming loans as a percentage of total gross loans. The indicator captures recognized impairment within the national banking system and follows the asset quality interpretation established by the International Monetary Fund (2019). It does not directly measure concealed losses, individual bank compliance or impairment attributable to climate exposures. This measurement boundary defines the empirical contribution: the analysis evaluates reported credit quality under different reporting environments without treating an aggregate impairment ratio as evidence of an unobserved mechanism.

This study aims to assess whether exposure to the European nonfinancial reporting framework is associated with reported nonperforming loans, conditional on real GDP growth, consumer price inflation, country differences and common annual conditions. The research question asks whether framework exposure corresponds to a measurable difference in recognized loan impairment. Related objectives assess the expected negative association with growth and positive association with inflation. The analysis uses 58 annual observations across France, Italy, Spain, Sweden, the United Kingdom and the United States during 2010 to 2020. An unbalanced panel specification with country and year fixed effects separates stable national differences and common annual conditions from the remaining associations. Country clustering addresses dependence within national systems, although six countries and a single jurisdiction outside the coded framework constrain inference.

The findings provide a bounded contribution to the evaluation of reporting regulation. Framework exposure has a negative coefficient across the baseline and alternative samples, but its confidence intervals include both decreases and increases in reported impairment. Growth and inflation receive no statistical support under the implemented specification, and the inflation coefficient changes direction when 2020 is excluded. These results distinguish consistency in coefficient direction from evidence of a regulatory benefit. They also show why reporting framework exposure should not be used to quantify expected reductions in problem loans without stronger evidence on implementation and underlying credit conditions.

The relevance extends to international comparisons of disclosure regulation because the analysis makes the relationship between measurement and interpretation explicit. Its contribution is not a claim that reporting requirements are ineffective, but evidence that a negative aggregate association remains insufficient to establish improved loan quality. For supervisors and investors, this directs attention toward evaluations that connect verified obligations, enforcement and reporting practices with observed credit outcomes. Such distinctions are necessary before changes in reported impairment can inform conclusions about the financial consequences of environmental disclosure.

2. Theory, Conceptual Framework and Hypotheses Development:

This analysis draws on information asymmetry and market discipline to examine how disclosure regulation relates to reported banking credit risk. Banks possess information about borrower quality that external investors and depositors cannot fully observe. Reporting requirements can reduce this information gap and strengthen scrutiny of lending decisions. However, financial reporting serves investor information needs, while prudential regulation addresses financial stability. These objectives overlap without producing equivalent outcomes, as Barth and Landsman (2010) explain.

The conceptual framework links exposure to the European nonfinancial reporting framework, annual real GDP growth and consumer price inflation to the ratio of nonperforming loans to total gross loans. Reporting framework exposure represents the institutional information environment. Growth and inflation capture economic conditions affecting repayment capacity. Each predictor enters separately, and the outcome measures recognized loan impairment within national banking systems.

Disclosure regulation can influence lending through external monitoring. More informative reporting may help investors and supervisors identify vulnerable exposures and encourage managers to strengthen borrower screening. Climate information can inform these decisions when environmental conditions or regulatory changes affect borrower cash flows and asset values. Krueger et al. (2020) document that institutional investors consider climate risks financially relevant and use engagement and risk management to address them.

This evidence supports the information mechanism without establishing a direct effect on bank loan performance.

The observed relationship also depends on how reporting affects impairment recognition. Stronger monitoring may reduce subsequent problem loans, while more complete recognition may initially increase their reported proportion. Bushman and Williams (2012) find that provisioning which anticipates future loan deterioration strengthens risk discipline, whereas provisioning used to smooth earnings weaken it. Reporting can therefore improve monitoring or obscure underlying conditions, depending on its informational content.

Compliance costs and existing reporting practices provide further boundary conditions. Grewal et al. (2019) find that market reactions to mandatory nonfinancial disclosure vary with firms' existing disclosure and nonfinancial performance. Their evidence challenges the assumption that reporting requirements produce uniform benefits. Because improved recognition and stronger lending discipline imply competing directions for reported impairment, the policy relationship is evaluated through a research question.

RQ1: Is exposure to the European nonfinancial reporting framework associated with the reported nonperforming loan ratio, conditional on real GDP growth, inflation, country differences and common annual conditions?

Economic growth affects credit quality through borrower repayment capacity. Rising output can support employment, business revenue and debt servicing. Contraction reverses these channels and can weaken collateral values. Although favourable conditions may encourage lending that generates later vulnerabilities, the contemporaneous repayment mechanism predicts a negative relationship between growth and recognized impairment.

Nkusu (2011) finds that adverse macroeconomic developments are associated with rising nonperforming loans in advanced economies. The evidence also identifies feedback from deteriorating credit quality to economic performance. Accordingly, the hypothesis concerns conditional association rather than an independently identified causal effect.

H1: Higher annual real GDP growth is associated with a lower reported nonperforming loan ratio, conditional on reporting framework exposure, inflation, country differences and common annual conditions.

Inflation affects repayment through competing debt and income channels. Rising prices can reduce the real burden of fixed nominal debt when borrower income adjusts sufficiently. Conversely, inflation can weaken repayment capacity when household expenses and business costs increase faster than income. Higher financing costs may reinforce repayment pressure when monetary policy responds to inflation.

Klein (2013) finds that higher inflation is associated with subsequent increases in nonperforming loans in Central, Eastern and Southeastern Europe. This evidence supports the repayment pressure mechanism, although differences in income adjustment and loan contracts may alter the relationship across countries. The positive direction remains a theoretical expectation to be tested against the observed data.

H2: Higher annual consumer price inflation is associated with a higher reported nonperforming loan ratio, conditional on reporting framework exposure, real GDP growth, country differences and common annual conditions.

The framework therefore evaluates three observed predictors against one observed outcome. Its relationships represent conditional associations rather than established causal effects. The policy question permits either direction, while the growth and inflation hypotheses retain their stated expectations regardless of empirical support.

3. Data:

This analysis combines annual banking indicators, macroeconomic data and reporting framework exposure for the period from 2010 to 2020. The procedures establish source provenance, identify the usable sample and align variable measurement with the empirical specification.

3.1 Data Sources, Population, and Sample Construction:

The numerical data originate from the World Development Indicators database. The analytical variables comprise bank nonperforming loans to total gross loans, series FB.AST.NPER.ZS; annual real GDP growth, series NY.GDP.MKTP.KD.ZG; and annual consumer price inflation, series FP.CPI.TOTL.ZG. The World Bank distributes these indicators, drawing on financial statistics and national accounts supplied by national and international institutions. The indicator documentation identifies their definitions, annual frequency and source attribution (World Bank, n.d.-a, n.d.-b, n.d.-c). The workbook records extraction through downloadable DataHub mirrors of the World Development Indicators series. It preserves the extraction date and exact download addresses beside the source observations. The mirror release number was not recorded, and equivalence to the latest World Bank release was not independently established. The numerical observations therefore constitute a historical snapshot rather than information necessarily available during each reference year. The documented data licence is Creative Commons Attribution 4.0.

Reporting framework exposure follows the reference year established by Directive 2014/95/EU. The directive specifies application to financial years beginning during 2017 for qualifying entities. The United Kingdom regulations similarly apply to financial years beginning on or after 1 January 2017 (European

Parliament & Council of the European Union, 2014; United Kingdom, 2016). The resulting indicator represents jurisdictional exposure to the reporting framework, rather than verified compliance by individual banks.

The unit of analysis is the national banking system observed annually. The accessible frame comprises France, Germany, Italy, Spain, Sweden, the United Kingdom and the United States. These seven countries provide 77 potential observations across eleven years. Selection is purposive and provides variation in reporting framework exposure and economic conditions. The frame does not constitute a probability sample of all national banking systems. The target population comprises national banking systems operating under different reporting environments. The accessible frame is restricted to the seven countries represented in the supplied workbook. Model inclusion requires an observed nonperforming loan ratio, reporting framework exposure, GDP growth and inflation. Applying this rule produces 58 observations across six countries. France, Italy, the United Kingdom and the United States each contribute eleven observations, Spain contributes nine, and Sweden contributes five. The estimation panel is therefore unbalanced.

Table 1: Sample construction

Stage	Countries Retained	Observations Retained	Inclusion or Exclusion Rule
Accessible workbook frame	7	77	Seven countries observed across eleven annual periods
Exclude unavailable German outcomes	6	66	Eleven nonperforming loan observations unavailable
Exclude unavailable Spanish outcomes	6	64	Outcomes unavailable for 2010 and 2011
Exclude unavailable Swedish outcomes	6	58	Outcomes unavailable from 2010 through 2015
Apply predictor completeness requirement	6	58	No additional missing values among the three predictors
Baseline estimation sample	6	58	Observed outcome and complete predictors

These exclusions describe absence from the downloaded snapshot. They do not establish that the observations are unavailable from every alternative source.

3.2 Data Integration, Cleaning, and Quality Assurance:

Numerical series were aligned through country identifiers and calendar year. Source observations use the combined key of country, year and indicator, while the consolidated panel uses country and year. Checks identified no duplicate keys in either structure. All 351 recorded numerical source observations matched their corresponding consolidated values within numerical precision. The checks found no discrepancies requiring source substitution. The original annual frequency and percentage units were preserved. GDP growth and inflation already enter as annual rates, while the nonperforming loan indicator represents a ratio of loan stocks. No currency conversion, additional deflation or temporal aggregation was applied. Negative growth and inflation observations were retained because contraction and deflation are substantively meaningful. Every observed nonperforming loan ratio falls between zero and 100 percent.

Missing values remain unavailable rather than becoming zeros. Across the 77 potential observations, the outcome has 19 missing values, while growth, inflation and reporting framework exposure are complete. Outcome availability varies systematically across countries and years. The retained sample contains four countries in each of 2010 and 2011, five in each year from 2012 through 2015, and six in each year from 2016 through 2020. The analysis uses observed outcomes without interpolation or imputation. This choice preserves the distinction between source observations and constructed values, but does not establish that sample selection is ignorable. Missing data assumptions determine the validity of both observed case analysis and imputation, rather than either method providing an automatic solution (Hughes et al., 2019; Little & Rubin, 2019).

The missingness concerns the dependent variable in the implemented specification. Conclusions from research on missing covariates therefore cannot be transferred mechanically to these data. White and Carlin (2010) show that the relative performance of observed case analysis and imputation depends on the missingness mechanism. Likewise, imputation requires a defensible model and appropriate supporting information (White et al., 2011; van Buuren, 2018). No such model was constructed here. No observations were winsorized, trimmed or removed because of their magnitude. Eligibility remained based on variable availability rather than coefficient estimates. The checks establish internal consistency within the supplied snapshot; they do not constitute independent verification of national reporting practices.

3.3 Variable Measurement, Construction, and Validation:

The dependent variable is the reported nonperforming loan ratio, expressed as a percentage of total gross loans. Higher values indicate a larger recognized impaired share of the loan portfolio. This interpretation follows the asset quality purpose of the indicator in the Financial Soundness Indicators Compilation Guide

(International Monetary Fund, 2019). The analysis retains the published ratio and does not reconstruct its numerator or denominator. Reporting framework exposure equals one for France, Italy, Spain, Sweden and the United Kingdom from 2017 through 2020, and zero otherwise within the estimation sample. The United States receives zero throughout the observation period because the indicator concerns exposure to this specific European framework. Zero does not imply the absence of other reporting obligations.

Real GDP growth measures the annual percentage change in output at constant prices. Inflation measures the annual percentage change in consumer prices. The latter is a price change measure rather than a price index level, consistent with the distinction set out in the Consumer Price Index Manual (International Monetary Fund et al., 2020). Both variables retain their original percentage scaling.

Table 2: Variable Dictionary

Variable	Analytical Role	Definition and Construction	Source And Identifier	Unit and Frequency	Interpretation and Missing Value Rule	Analytical Coverage
Nonperforming loan ratio	Outcome	Published nonperforming loans divided by total gross loans, multiplied by 100	World Development Indicators, FB.AST.NPER.ZS	Percent, annual	Higher values indicate greater recognized impairment; unavailable outcomes excluded	58 observations across six countries
Reporting framework exposure	Institutional predictor	One for covered European jurisdictions from 2017 through 2020; zero otherwise	Directive 2014/95/EU and United Kingdom regulations	Binary, annual	Measures framework exposure rather than individual bank compliance	Complete for all 58 observations
Real GDP growth	Economic predictor	Published annual growth rate of GDP at constant prices	World Development Indicators, NY.GDP.MKTP.KD.ZG	Annual percent	Higher values indicate stronger output growth; negative values retained	Complete for all 58 observations
Consumer price inflation	Economic predictor	Published annual percentage change in consumer prices	World Development Indicators, FP.CPI.TOTL.ZG	Annual percent	Higher values indicate faster consumer price increases; negative values retained	Complete for all 58 observations

The outcome captures recognized asset quality. Its interpretation requires attention to reporting incentives because accounting choices can influence the timing with which credit deterioration becomes visible (Bushman & Williams, 2012). Growth and inflation enter separately because economic conditions can affect repayment capacity and interact with banking sector deterioration through feedback mechanisms documented by Nkusu (2011) and Klein (2013). These relationships motivate the predictors without converting them into a composite measure. Table 3 reports descriptive statistics calculated from the same 58 observations used in baseline estimation. Percentages retain their published scale, so a one-unit difference in growth or inflation represents one percentage point.

Table 3: Descriptive statistics for the estimation sample

Variable	Valid observations	Mean	Standard Deviation	Median	Minimum	Maximum
Nonperforming loan ratio	58	4.678	4.626	3.511	0.495	18.064
Reporting framework exposure	58	0.345	0.479	0.000	0.000	1.000
Real GDP growth	58	0.663	3.135	1.625	−10.940	4.061
Consumer price inflation	58	1.342	0.980	1.430	−0.500	3.856

Measurement checks covered source reconciliation, numeric validity, outcome bounds and consistency of policy coding. These are observed variables, so no latent variable measurement model was imposed.

3.4 Analytical Framework, Identification, and Reproducibility:

The implemented analysis estimates conditional associations through ordinary least squares with country and year fixed effects. Equation 1 specifies the baseline relationship.

$$NPL_{it} = \alpha_i + \lambda_t + \beta_1 D_{it} + \beta_2 G_{it} + \beta_3 P_{it} + \varepsilon_{it}$$

Here, NPL_{it} is the nonperforming loan ratio for country i in year t; D_{it} is reporting framework exposure; G_{it} is real GDP growth; and P_{it} is consumer price inflation. Country effects are represented by α_i, year effects by λ_t, and the residual by ε_{it}. All three predictors enter contemporaneously.

Country effects absorb stable differences across national banking systems, while year effects absorb common annual conditions. Their inclusion does not eliminate changing national confounders, reverse relationships or differences in reporting implementation. As Imai and Kim (2019) demonstrate, fixed effects alone do not establish causal identification. The coefficients are therefore interpreted as conditional associations. The model contains 19 linearly independent parameters and 39 residual degrees of freedom. Standard errors were clustered by country, with a finite sample covariance adjustment and a student distribution reference using five degrees of freedom. This permits dependence within countries under the maintained assumption of independence across countries. Cameron and Miller (2015) explain both the rationale for clustering and its limitations when clusters are few.

Only six countries contribute to estimation, and the United States is the sole jurisdiction outside the coded framework. The policy coefficient consequently depends heavily on that comparison. Conley and Taber (2011) show why a small number of policy changes limits conventional inference. Unequal cluster sizes and limited policy variation create additional difficulties that standard corrections do not necessarily resolve (MacKinnon & Webb, 2017; Ferman & Pinto, 2019). Statistical uncertainty must therefore be assessed alongside the magnitude and stability of the estimates.

Three exploratory sensitivity specifications were implemented during data review. Excluding 2020 retains 52 observations across six countries. Restricting estimation to France, Italy, the United Kingdom and the United States produces a balanced sample of 44 observations. Excluding Sweden retains 53 observations across five countries. These specifications assess dependence on the terminal year and uneven country coverage. They change sample composition and are interpreted as exploratory comparisons rather than independent validation.
The workbook preserves source observations, download addresses, policy coding and variable definitions. Reproduction requires matching country and year, retaining observations with complete values for the four analytical variables, and estimating Equation 1 with the stated fixed effects and inference procedure.

4. Methods:

4.1 Research Design and Identification Strategy:

This analysis uses an observational panel design to estimate conditional associations between reporting framework exposure and recognized loan impairment. The unit of analysis is the national banking system observed annually during 2010 to 2020. The estimand is the adjusted difference in the nonperforming loan ratio associated with framework exposure, conditional on economic conditions, country effects and year effects. Useful variation arises because five European jurisdictions enter the coded framework from 2017, while the United States remains outside that framework throughout the observation period. Country effects absorb stable national differences, and year effects absorb common annual conditions. The exposure indicator retains variation because its transition applies to only part of the sample.

The design does not identify an isolated causal effect of reporting regulation. National economic developments, concurrent reforms, differences in implementation and changing impairment recognition may influence both exposure comparisons and reported credit quality. Fixed effects do not eliminate these threats or establish comparability between jurisdictions. This interpretation follows Imai and Kim (2019), who distinguish adjustment for stable differences from the assumptions required for causal inference. The exposure coefficient therefore represents a conditional association rather than an average treatment effect. Growth and inflation enter as contemporaneous economic adjustments. Because economic conditions could themselves respond to institutional developments, the adjusted coefficient is not interpreted as the total effect of regulatory adoption.

4.2 Model Specification and Measurement Alignment:

The baseline specification is Equation 1. It relates the nonperforming loan ratio to reporting framework exposure, real GDP growth and consumer price inflation, with country and year fixed effects. Definitions, units and coding follow Table 2. The dependent variable remains in percentage units, and growth and inflation retain their annual percentage scaling. Their coefficients express percentage point differences in the outcome associated with a one percentage point increase in the predictor. All predictors enter contemporaneously. The specification addresses annual conditional associations and does not impose a dynamic adjustment structure. Framework exposure equals one for the five included European jurisdictions from 2017 through 2020 and zero otherwise. The indicator measures jurisdictional exposure rather than verified compliance by individual banks.

The estimation sample follows the availability rules documented in Table 1. It contains 58 observations across six countries. France, Italy, the United Kingdom and the United States contribute eleven observations each, Spain contributes nine, and Sweden contributes five. No additional observations are lost through lag construction, differencing or missing predictors. Each retained observation receives equal weight. Consequently, the estimates describe the observed national banking systems rather than an aggregate weighted by banking assets or loan volumes. Generalization is restricted by purposive country selection and uneven outcome coverage. The design matrix contains an intercept, three substantive predictors, five country indicators and ten year indicators. Spain and 2010 serve as the omitted categories. The matrix has full column rank, with 19 parameters and 39 residual degrees of freedom. These reference categories affect the intercept and indicator coefficients without changing the three substantive slope estimates.

4.3 Estimation and Statistical Inference:

Estimation used Python 3.12.14, pandas 2.2.3, NumPy 2.3.5 and SciPy 1.17.0. Spreadsheet values were extracted from the workbook’s XML records, converted to numeric form and filtered using the stated completeness rule. Ordinary least squares coefficients were calculated through NumPy’s least squares routine with its default numerical rank tolerance. The procedure is deterministic and uses no random sampling. Country clustered standard errors allow heteroskedasticity and unrestricted residual dependence within each national banking system. The implemented covariance estimator is specified in Equation 2.

$$\widehat{V}_{CR1} = \frac{G}{G-1}\frac{N-1}{N-K} A \left[\sum_{g=1}^{G} X_g' \hat{u}_g \hat{u}_g' X_g\right] A, \qquad A = [X'X]^{-1}$$

Here, $\widehat{V}_{CR1}$ is the adjusted covariance matrix; G is the number of country clusters; N is the number of observations; and K is the number of estimated parameters, including the intercept and fixed effect indicators. The full design matrix is X, while X_g and $\hat{u}_g$ contain the design rows and estimated residuals for country g. The matrix A is the inverse cross product of the design matrix. The two scalar fractions provide the finite sample adjustment.

Baseline inference uses six clusters. Two-sided p values and 95 percent confidence intervals use a student distribution with five degrees of freedom. Sensitivity specifications apply the same procedure with degrees of freedom equal to the retained number of countries minus one. Hypotheses are assessed at the 5 percent level, alongside coefficient direction, magnitude and confidence intervals.

This covariance estimator permits dependence within countries but assumes independence across countries. Year effects absorb common annual shifts without guaranteeing that remaining errors are independent internationally. Moreover, six clusters provide limited support for conventional asymptotic approximations. Cameron and Miller (2015) explain why clustering and finite sample adjustments do not remove all uncertainty associated with few clusters. Confidence intervals are therefore treated as approximate, and statistical insignificance is not interpreted as equivalence to zero.

4.4 Data Checks, Sensitivity Analysis, and Reproducibility:

Processing retained one record for each country and year. Duplicate checks covered both the consolidated panel key and the source key comprising country, year and indicator. All 351 numerical source observations were reconciled with the consolidated workbook using NumPy's numerical comparison routine, with relative tolerance set to 10^{-12} and its default absolute tolerance. No duplicate keys or reconciliation discrepancies were identified.

Missing outcome observations were excluded without imputation, interpolation or replacement by zero. The analytical predictors were complete within the retained sample. This procedure preserves observed source values but does not establish that outcome availability is unrelated to unobserved credit conditions. Hughes et al. (2019) emphasize that the validity of missing data procedures depends on the underlying missingness process.

Checks also verified numeric validity, outcome bounds and consistency of exposure coding. Negative growth and inflation values were retained. No observations were winsorized, trimmed or excluded because they were extreme.

Collinearity assessment used the variation remaining after removing country and year effects. Each substantive predictor was first regressed on the fixed effect indicators. Its residual was then regressed on the residuals of the other two predictors. The variance inflation factor was calculated as one divided by one minus the resulting coefficient of determination. This procedure evaluates overlap among predictors in the variation used to estimate the substantive coefficients.

Three exploratory sensitivity specifications address specific sample concerns. Excluding 2020 examines dependence on the terminal year and retains 52 observations across six countries. Restricting estimation to France, Italy, the United Kingdom and the United States creates a balanced panel of 44 observations. Excluding Sweden assesses dependence on the country with the shortest outcome series and retains 53 observations across five countries.

These specifications retain the baseline predictors and fixed effects. They change the estimation sample, so coefficient differences are interpreted as sensitivity to composition or period coverage rather than isolated specification effects. Comparisons consider direction, magnitude and uncertainty, rather than repeated statistical significance. MacKinnon and Webb (2017) demonstrate why unequal cluster sizes and limited policy variation require particular caution in interpreting clustered inference.

The workbook documents source observations, access addresses, policy coding and variable definitions. The estimation scripts preserve sample restrictions, design matrix construction, covariance calculations and sensitivity procedures. Together, these materials specify a reproducible application of established panel methods. Their purpose is transparent estimation of conditional associations, without claiming a new estimator or causal identification strategy.

5. Findings:

This analysis examines whether reporting framework exposure, real GDP growth and inflation are associated with reported nonperforming loans. The findings present sample characteristics, adjusted estimates and sensitivity results. Hypothesis assessment considers coefficient direction, magnitude and uncertainty without treating statistical insignificance as evidence that a relationship is absent.

The estimation sample contains 58 annual observations across six national banking systems during 2010 to 2020. France, Italy, the United Kingdom and the United States each contribute eleven observations, Spain contributes nine, and Sweden contributes five. Nineteen potential observations lack the outcome,

including all eleven German observations, two Spanish observations and six Swedish observations. Growth, inflation and reporting framework exposure are complete within the retained sample.

The mean nonperforming loan ratio is 4.678 percent, compared with a median of 3.511 percent. Its range extends from 0.495 to 18.064 percent, indicating substantial variation in recognized impairment. Reporting framework exposure equals one in 20 observations and zero in 38 observations. The outcome measures reported asset quality rather than unrecognized losses. This distinction matters because reporting practices can influence when credit deterioration becomes visible, as Bushman and Williams (2012) demonstrate.

Descriptive Statistics:

Variable	Observations	Mean	SD	Median	Minimum	Maximum
Nonperforming loan ratio, percent	58	4.678	4.626	3.511	0.495	18.064
Reporting framework exposure, binary	58	0.345	0.479	0.000	0.000	1.000
Real GDP growth, annual percent	58	0.663	3.135	1.625	−10.940	4.061
Consumer price inflation, annual percent	58	1.342	0.980	1.430	−0.500	3.856

Source: Calculations from the consolidated workbook. Statistics use the baseline estimation sample. No observations were imputed, winsorized or removed because of their magnitude.

The baseline estimates come from ordinary least squares with country and year fixed effects. Country clustering permits residual dependence within national banking systems. The covariance estimator includes a finite sample adjustment, and confidence intervals use a student distribution with five degrees of freedom. With only six clusters, these intervals remain approximate. Cameron and Miller (2015) emphasize that clustering addresses within group dependence without eliminating the inferential difficulties created by few clusters.

Baseline Estimates for the Nonperforming Loan Ratio:

Predictor	Coefficient	Clustered Standard Error	Lower 95 Percent Confidence Limit	Upper 95 Percent Confidence Limit	Two Sided P Value
Reporting framework exposure	−1.046	1.154	−4.013	1.920	0.406
Real GDP growth	−0.002	0.168	−0.435	0.431	0.991
Consumer price inflation	0.303	0.502	−0.986	1.593	0.572

Source: Calculations from 58 observations across six countries. All specifications include country and year fixed effects. Coefficients express percentage point differences in the nonperforming loan ratio. Growth and inflation coefficients correspond to a one percentage point increase in the predictor. The model has 19 linearly independent parameters, 39 residual degrees of freedom and an overall R squared of 0.861.

Reporting framework exposure is associated with a 1.046 percentage point lower nonperforming loan ratio after adjustment. Although this magnitude could be economically relevant, the confidence interval ranges from a decrease of 4.013 percentage points to an increase of 1.920 percentage points. The evidence therefore does not establish the direction of the policy association. RQ1 yields a negative point estimate, but the available observations cannot distinguish that estimate from materially different relationships.

The estimated growth association is close to zero. A one percentage point increase in real GDP growth corresponds to a 0.002 percentage point lower nonperforming loan ratio. The confidence interval includes decreases and increases of approximately 0.43 percentage points. H1 is not supported at the 5 percent level. This result does not overturn the repayment capacity mechanism documented by Nkusu (2011), whose analysis identifies links between adverse economic conditions and deteriorating loan performance. It shows that the present specification does not estimate that relationship precisely after absorbing country differences and common annual conditions.

Inflation has a positive coefficient of 0.303. A one percentage point increase in consumer price inflation corresponds to a 0.303 percentage point increase in reported impairment. However, the confidence interval extends from −0.986 to 1.593 percentage points. H2 is therefore not supported at the 5 percent level. The point estimate follows the inflation related deterioration reported by Klein (2013), but its uncertainty prevents a comparable empirical conclusion.

Predictor overlap provides little explanation for this imprecision. After removing country and year effects, variance inflation factors are 1.118 for reporting framework exposure, 1.066 for growth and 1.113 for inflation. These values indicate limited linear overlap among the three substantive predictors in the variation used for estimation. They do not address omitted variables, sample selection or dependence across countries.

Three exploratory specifications assess sensitivity to the terminal year and uneven country coverage. Excluding 2020 reduces the sample to 52 observations. Restricting estimation to the four countries with

complete annual coverage produces 44 observations. Excluding Sweden retains 53 observations and removes the country with the shortest outcome series.

Exploratory Sensitivity Estimates:

Specification	Observations	Countries	Reporting Exposure Coefficient	GDP Growth Coefficient	Inflation Coefficient
Baseline sample	58	6	−1.046	−0.002	0.303
Exclude 2020	52	6	−0.734	−0.200	−0.236
Balanced sample of four countries	44	4	−1.449	−0.071	0.397
Exclude Sweden	53	5	−1.333	−0.109	0.278

Source: Calculations from the consolidated workbook. Every specification includes country and year fixed effects. The balanced sample comprises France, Italy, the United Kingdom and the United States. No substantive predictor reaches the 5 percent significance level under the stated country clustering procedure.

The reporting exposure coefficient remains negative across the sensitivity specifications, ranging from −0.734 to −1.449 percentage points. Directional consistency does not imply precise estimation. Its confidence interval ranges from −2.989 to 1.521 when 2020 is excluded, from −8.014 to 5.115 in the balanced sample, and from −5.357 to 2.691 when Sweden is excluded. Restricting the sample therefore preserves the estimated direction while leaving substantial uncertainty.

Growth remains negative across the alternative samples, but its magnitude changes considerably. Inflation is less stable: its coefficient changes from positive to negative when 2020 is excluded. The positive baseline inflation estimate consequently depends on retaining the terminal year. These comparisons provide no basis for presenting either macroeconomic hypothesis as empirically confirmed.

The policy comparison also depends on a narrow institutional contrast. Five countries enter the coded reporting framework, while the United States provides the sole jurisdiction outside it. Common year effects cannot absorb all economic or institutional changes that differ between these groups. MacKinnon and Webb (2017) show that limited policy variation and unequal cluster sizes can undermine conventional inference. The sensitivity exercises do not remove this limitation.

6. Discussion:

A negative policy coefficient does not establish that disclosure regulation improves bank loan quality. Table 5 reports a lower nonperforming loan ratio among observations exposed to the reporting framework, but the confidence interval includes both economically meaningful decreases and increases. Equation 1 therefore identifies a conditional association rather than a demonstrated regulatory benefit. The contribution lies in showing that the policy coefficient retains its negative direction across alternative samples while its magnitude remains uncertain. This distinction separates directional consistency from evidential strength and qualifies interpretations that equate reporting mandates with improved financial outcomes. Leuz and Wysocki (2016) identify precisely this challenge in distinguishing regulatory adoption, changes in reporting and economic consequences.

The findings also separate theoretical transmission mechanisms from mechanisms established empirically. The coefficient in Table 5 could reflect stronger monitoring, changes in impairment recognition or other developments accompanying reporting framework exposure. Equation 1 does not distinguish these explanations, so the negative estimate cannot identify a monitoring pathway or support a mediation claim. Beatty and Liao (2011) show that the timing of expected loss recognition affects lending behaviour during contractions, demonstrating why reporting and underlying credit conditions need separate measurement. The present evidence consequently refines the interpretation of the outcome: a change in recognized problem loans cannot independently reveal whether borrower quality improved or recognition practices changed.

The macroeconomic estimates provide a further boundary to interpretation. Table 5 offers no statistical support for either the negative growth hypothesis or the positive inflation hypothesis, while Table 6 shows that inflation changes direction when 2020 is excluded. These patterns do not establish that repayment capacity is irrelevant. They indicate that the contemporaneous associations are not estimated precisely after accounting for country differences and common annual conditions. Ghosh (2015) identifies stronger growth and inflation relationships using regional banking and economic variation within the United States. The contrast suggests that evidence obtained from regional variation cannot be transferred mechanically to a short international panel. It does not establish the superiority of either level of aggregation or identify a dominant transmission channel.

Institutional coverage also shapes what the policy comparison can establish. The United States supplies the sole jurisdiction outside the coded framework, and the broader confidence interval in the balanced specification in Table 6 shows that equal annual coverage does not necessarily improve precision. The reporting indicator captures framework exposure rather than enforcement intensity or bank compliance. Christensen et al. (2013) distinguish reporting requirements from concurrent enforcement changes when explaining market outcomes around mandatory accounting reform. Their findings make an important interpretive distinction

relevant here: the coefficient in Equation 1 cannot isolate the reporting requirement from accompanying institutional changes. Supervisory assessments should therefore avoid treating formal exposure as evidence of effective implementation.

The international relevance lies in the conditions under which aggregate credit quality measures can inform regulatory evaluation. Beck et al. (2015) identify macroeconomic determinants of nonperforming loans across a substantially broader international sample and show that some relationships depend on financial structure. The imprecise estimates in Table 5 and the period sensitivity in Table 6 qualify the transferability of such relationships to narrower regulatory comparisons. Decision makers should not use the negative policy estimate to quantify expected reductions in problem loans. More informative evaluation would connect verified reporting obligations and enforcement with bank disclosures, impairment recognition and borrower exposures. That research design could test whether disclosure improves lending decisions, accelerates recognition or combines both responses, rather than attributing an aggregate change to an unobserved mechanism.

7. Conclusion and Implications:

Financial transparency cannot be judged by whether reported problem loans decline after regulatory reform. This analysis shows that a consistently favourable estimated direction can coexist with substantial uncertainty about economic benefits. Institutional reporting exposure and economic conditions do not jointly establish an improvement in recognized credit quality within the examined sample. The contribution is an empirical distinction between directional consistency and evidential strength, which qualifies interpretations that equate formal reporting requirements with stronger banking outcomes. The findings refine market discipline arguments by showing that their predicted benefits remain unestablished when regulatory exposure is observed but implementation and reporting behaviour are not. Managers should therefore assess disclosure initiatives alongside borrower monitoring and impairment recognition rather than infer better lending performance from formal adoption. Supervisors should connect reporting evaluations with verified compliance, enforcement records and comparable asset quality information before assigning prudential benefits. Operationally, maintaining traceable links between disclosed exposures, credit assessments and subsequent repayment outcomes would make those evaluations more informative. Internationally, the evidence supports disciplined interpretation of regulatory comparisons and cautions against translating uncertain aggregate associations into lending targets or resource commitments. Better grounded assessments can help protect depositors, borrowers and public resources from decisions based on overstated regulatory effectiveness.

The limited number of countries and reliance on one comparison jurisdiction create opportunities for broader institutional comparisons. Expanding coverage and recovering missing observations would improve assessment of sample dependence. Bank and borrower records could separate changes in underlying repayment capacity from changes in recognition practices. Verified implementation dates and enforcement measures would sharpen the institutional comparison, while longer observation periods could examine delayed responses. Designs exploiting credible variation in eligibility or implementation could strengthen causal identification. Future research should test monitoring and recognition pathways directly and assess whether their relationships differ across financial structures, reporting practices and supervisory environments.

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