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ORIGINAL ARTICLE

The unstable boundary: conditional classification of non-performing loan determinants and its implications for banking regulation

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Abstract
Research on the determinants of non-performing loans (NPLs) is organised around fixed classifications that assign each driver to a single category—macroeconomic, bank-specific or loan-specific. This paper argues that the assignment is not fixed, and that the instability has direct consequences for banking regulation. Whether a determinant operates through an endogenous or an exogenous channel—terms used here in a mechanism sense, relative to a specified unit of analysis, time horizon and institutional, capital or policy regime, rather than in the econometric sense—is conditional on those three dimensions. The argument is drawn from thirteen purposively selected primary studies whose reported findings, verified against the source text, are read together. Bank capital is reported to reduce NPLs in one setting, to raise them in another, and to do both either side of an estimated threshold within a single study. Economic growth is the dominant determinant in one study, insignificant in a second, positively signed in a third, and reverses its causal direction with the horizon in a fourth. Regulation is reported to bear no predictable relationship with NPLs. The paper distinguishes heterogeneity of effect from a change of channel, proposes a two-part test for the latter, and finds that the strongest cases rest on within-study variation in which sample, measure and estimator are held fixed. It argues that treating regulation, capital and provisioning as exogenous—across capital frameworks and across incurred-loss and expected-credit-loss provisioning regimes—risks misstating the relationships on which supervisory evaluation, provisioning policy and stress testing depend.

Keywords Non-performing loans · Credit risk · Determinants · Endogeneity · Bank regulation · Macroprudential policy

JEL Classification G21; G28; E44

Introduction

Non-performing loans (NPLs) remain a central concern for banking-sector stability and a recurring object of supervisory attention. A substantial empirical literature has developed to explain their formation and successive reviews have organised that literature by classifying determinants according to their origin: macroeconomic, bank-specific and loan-specific [1–3]. This scheme has proved useful as a means of synthesising diverse empirical findings and has structured research for decades. It also carries an implicit assumption: that each determinant occupies a fixed position within the classification and operates through a stable endogenous or exogenous channel.

This paper argues that the assumption does not hold. Whether a determinant operates through an endogenous or an exogenous channel is not an intrinsic property of the determinant itself but depends on the level of analysis, the time horizon and the institutional, capital or policy regime in which it is observed. A variable that is external to an individual bank may be generated by the banking system in aggregate; a determinant that appears exogenous over one horizon may be the delayed consequence of prior bank behaviour over another; and regulatory variables commonly treated as external frequently constitute responses to banking-sector conditions rather than independent influences. The endogenous–exogenous boundary is therefore conditional rather than fixed.

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Three distinctions govern the argument and should be stated at the outset. First, the terms endogenous and exogenous are used throughout in a mechanism sense, not an econometric one. A determinant operates through an endogenous channel, relative to a specified unit, horizon and regime, when the value it takes is generated by the decisions or condition of that unit; it operates through an exogenous channel when its value arrives from outside the unit. This is the sense in which Berger and DeYoung [4] described a bank's problem loans as exogenous to the extent that they stem from bad luck and endogenous to the extent that they stem from management's own actions. The channel is always relative: a variable may be exogenous to a bank and endogenous to the banking system. Second, this mechanism sense is distinct from econometric endogeneity—the correlation of a regressor with the error term that motivates instrumental-variable and dynamic-panel estimators. A regressor can be treated as econometrically exogenous while carrying a behavioural mechanism, and several of the studies drawn on below do exactly that: Hajja [5], for example, instruments capital as a weakly exogenous regressor while attributing its effect to moral hazard. Econometric endogeneity is a property of an estimating equation; the channel is a property of the relationship being estimated. Third, both are distinct from the origin-based taxonomy that the reviews apply, which sorts variables by where they are measured—in the bank's accounts, in the loan book or in the macroeconomy—and says nothing about the direction in which influence runs. The claim of this paper concerns the mechanism sense only: that the channel through which a determinant operates depends on the unit, horizon and regime at which it is observed, and that the origin taxonomy, by fixing the assignment, conceals this.

The argument is not simply that determinants behave differently across contexts. That observation is already familiar in empirical banking research and is commonly invoked to explain variation across countries, samples or estimation methods; it is the phenomenon of heterogeneous effects, and it is compatible with a fixed classification. The stronger claim advanced here is that context can determine the determinant's operative channel. A change of channel—referred to below as channel switching—is therefore defined more narrowly than a change of sign, magnitude or significance. It requires two things: (i) a relationship that differs across levels, horizons or regimes; and (ii) evidence, beyond that difference, that a different mechanism is at work—a reversal in the direction of influence, distinct mechanisms attributed by the source study to each context, or the determinant being generated by banking conditions in one context and not in the other. Feedback of this kind—output affecting NPLs while NPLs also affect output—is not evidence that one channel has displaced the other; it is evidence that both operate at once at the level of the system, so that the channel assignment depends on the level of analysis. It is that dependence, not the replacement of one mechanism by another, that is meant when feedback is offered as evidence of a change of channel. Coefficient heterogeneity, sign changes and non-linearities satisfy the first requirement but not, on their own, the second. Where only the first is met the paper claims heterogeneity; where the second rests on the interpretation of the source rather than on a direct estimate, the paper claims a candidate switch; and it reserves the unqualified term for cases in which both requirements are met by reported estimates. If the classificatory boundary is conditional in this sense, then some of the inconsistencies repeatedly reported in the NPL literature are not isolated empirical puzzles but evidence that a fixed taxonomy is being applied to relationships whose channels are themselves conditional.

The claim is drawn from the literature's own reported findings rather than imposed upon it. Bank capital is found to reduce NPLs in one banking system, to increase them in another, and to change sign either side of an estimated capital threshold within a single study. Economic growth, the determinant most confidently classified as exogenous, is reported as the dominant driver of NPLs in some studies, statistically insignificant or positively associated with NPLs in others, and reverses causal direction across time horizons in one. Regulation itself is reported to exhibit no stable relationship with NPLs. These findings are usually treated as isolated inconsistencies requiring better data or alternative estimation strategies. This paper argues instead that a recognisable subset of them—those in which the variation is observed within a single study and attributed by its authors to distinct mechanisms—is better read as evidence of a conditional classificatory boundary, and that the remainder, while consistent with that reading, establish heterogeneity rather than a change of channel.

The contribution is therefore directed at the classification rather than at the determinants themselves. The paper does not propose a replacement taxonomy. Instead, it argues that the existing taxonomy rests on an unstable boundary and that recognising this instability offers a parsimonious, though not the only, explanation for one of the literature's most persistent features: the failure of repeated studies to agree on the sign, magnitude or significance of many widely studied determinants. The argument also has direct implications for banking regulation. Regulation, capital and provisioning are among the determinants whose exogeneity is least secure, yet they are routinely modelled as external influences despite often constituting endogenous responses to banking-sector conditions. Treating them as fixed exogenous regressors risks misidentifying the relationships on which supervisory evaluation, provisioning policy and stress-testing frameworks depend.



Table 1 Reported findings on four groups of determinants, as stated by the source studies, with the type of evidence each supplies

Determinant	Study (context, period, measure)	Reported finding	Condition that sets the channel	Evidence
Bank capital	Berger & DeYoung (1997) [4]; US, 1985–1994; equity/gross assets	Full sample: lagged capital positively associated with NPLs; below-median-capital banks: capital decreases precede NPL increases; at low-capital banks NPL increases precede capital increases	Capital regime: moral-hazard (behavioural) channel in the low-capital subsample; feedback from asset quality to capital	H, W, D
	Hajja (2022) [5]; Malaysia, 2002–2011; equity/total assets	Concave: capital raises NPLs below a threshold of ~11%, reduces them above; NPLs Granger-cause capital, not the reverse	Capital regime: moral hazard below, disciplinary or regulatory effect above, as attributed by the source	H, W, D, C
	Ersoy (2021) [12]; Turkey, 2010–2019; total capital/RWA	Higher capital adequacy reduces NPLs (marginal in system GMM); read by the source as moral hazard	Institutional setting: same sign and mechanism as the Berger–DeYoung low-capital subsample	H
	Ferreira (2022) [13]; 80 countries, 1999–2019; total regulatory capital/RWA	Positive in the full sample and in both income sub-panels, 1999–2019; sign reverses between one-step and two-step estimates in the OECD sub-panel; post-2008 negative in the non-high-income and non-OECD sub-panels but still positive in the high-income and OECD sub-panels, same system-GMM estimator	Estimation window and income/OECD setting: the source reads the post-crisis negative sign against the increased role of regulators after the crisis, but finds it only outside the high-income and OECD groups	H, W
Economic growth	Beck et al. (2015) [2]; 75 countries, 2000–2010	Real GDP growth the main driver of NPL ratios	External macro shock at the system level	H
	Espinoza & Prasad (2010) [14]; GCC, 1995–2008	Significant inverse relation (real non-oil GDP); panel VAR finds short-lived feedback from NPLs to growth	Level and horizon: system-level feedback runs bank→macro	W, D
	Klein (2013) [15]; CESEE, 1998–2011	Both macro and bank-level factors drive NPLs; NPL increases feed back to GDP, credit, unemployment	Level and direction: channels operate jointly, feedback bank→macro	W, D
	Hajja (2022) [5]; Malaysia, 2002–2011	Higher GDP growth raises NPLs; read by the source as procyclical	Institutional setting: sign differs from the cross-country consensus	H
	Mishra et al. (2021) [16]; India, 2010–2019	GDP growth insignificant, although industrial production carries a significant negative coefficient (inflation, the policy rate and the exchange rate are also significant); the source concludes that cyclical booms and recessions have not influenced NPAs	Institutional setting: cyclical channel detected through industrial production rather than GDP growth	H
	Kartal et al. (2023) [17]; Turkey, 2005–2019	Growth explains NPLs in the long run; short-run linkage confined to the 2007–2009 crisis window, reported in the source's conclusion as NPLs causing growth	Time horizon: direction of causality reverses on the source's concluding reading	W, D
Credit growth	Vithessonthi (2016) [18]; Japan, 1993–2013	Positive and significant pre-crisis; negative and weakly significant (10%) post-crisis	Cyclical regime and horizon: time-varying impact	H, W
	Accornero et al. (2017) [19]; Italy, 2008–2015	Raw correlation largely reflects common borrower conditions; IV estimates indicate that shocks to NPLs reduce credit supply, an effect minor in sample	Identification and direction: once common factors are removed, influence runs from NPLs to credit rather than from credit growth to NPLs	W, D

Table 1 (continued)

Determinant	Study (context, period, measure)	Reported finding	Condition that sets the channel	Evidence
Provisioning & crisis policy	Ozili (2015) [9]; Europe, US, Asia, Africa, 2004–2013; incurred-loss era	Loan loss reserves and loan growth predict NPLs; regulation bears no predictable relationship to NPLs	Institutional setting: significance of each determinant is country-contingent	H
	Mishra et al. (2021) [16]; India, 2010–2019; incurred-loss prudential norms (Ind AS 109 deferred)	Provisions and contingencies positively associated with NPAs	Provisioning regime: consistent with a mechanical or a discretionary channel; does not discriminate	H
	Žunić et al. (2021) [23]; Bosnia & Herzegovina, 2012–2020; IFRS 9 from 2018, then the FBA's ECL decision	COVID-19 indicator negative and significant; loan moratoria prevented migration into the NPL category; provisions weakly positive	Policy as response: forbearance defers (source) or absorbs (this paper) the measured effect of the shock	W, P

Findings are reported as stated by the source studies and were verified against the source text. The fourth column is the present paper's interpretation of the condition under which the reported channel obtains and is not attributed to the source. The final column codes the evidence type: H = heterogeneity, a relationship that differs across studies or contexts; W = within-study variation, with sample, measure and estimator held fixed; D = an estimated reversal in the direction of influence, or feedback from NPLs to the determinant (the latter establishing coexistence of the two channels at the system level; Sect. "Introduction"); C = candidate channel switch, distinct mechanisms attributed by the source to different contexts; P = policy response that alters what the data record. Only rows carrying W together with D or C meet both parts of the definition of channel switching given in Sect. "Introduction". The table is illustrative rather than exhaustive: its purpose is to show that a determinant's sign, significance and operative channel vary with the level of analysis, the time horizon and the institutional, capital or policy regime in which it is examined

This is a conceptual argument rather than a systematic review, and it does not aim at exhaustive coverage. Recent systematic and bibliometric reviews already document the breadth of the literature; the aim here is different: to show that the literature's own reported evidence points towards a conditional structure that existing classifications do not capture. The evidence is drawn from thirteen primary studies, listed in Table 1, selected purposively on four criteria. Each study estimates the relationship between NPLs and at least one of three heavily studied determinants—bank capital, economic growth and credit growth—or one of the two regulatory variables of most direct supervisory relevance, provisioning and crisis policy. Each reports a finding that bears on the conditionality set out in Sect. "The conditional boundary" in at least one of three ways: variation across sub-samples, sub-periods or an estimated threshold; an estimate of the direction of influence; or a policy response that alters what the data record. Preference was given to studies that report such variation within a single analysis, with sample, measure and estimator held fixed, over studies that would supply only a contrast with another paper, because only the former can meet the second requirement of the definition above. Finally, the set as a whole spans high-income, emerging-market and cross-country settings, so that no single institutional setting supplies the pattern. Every finding reported in Table 1 was verified against the source text. The set is illustrative rather than representative: it was assembled to establish that the conditional pattern exists and can be identified, not to estimate how common it is, and the paper does not claim that stable findings are absent from the literature. Studies reporting a stable relationship do not contradict the argument, which concerns the classificatory operation rather than the frequency of any particular result; but the selection cannot, by construction, say how often the operation fails, and Sect. "Implications for empirical inconsistency" sets out the alternative explanations against which the cases must be weighed.

The literature records the conditionality while the reviews absorb it

The determinants of NPLs have been synthesised several times, and each synthesis organises the evidence around the origin-based categories. Manz [6] provides a systematic review of NPL determinants structured around macroeconomic, bank- and loan-specific themes. Notably, these themes are described as overlapping, followed by a call for a deeper understanding of their interaction—an acknowledgement that the categories are porous, though one that stops short of questioning whether the assignment of a determinant to a category is itself stable. Naili and Lahrichi

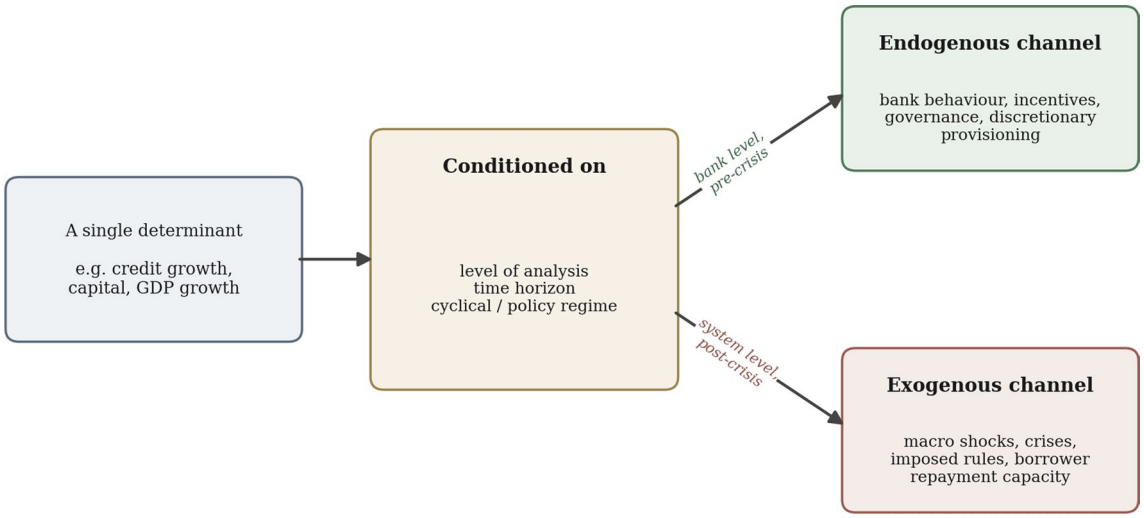


Fig. 1 The conditional boundary. The same determinant routes through an endogenous or an exogenous channel, in the mechanism sense defined in Sect. "Introduction", depending on the level of analysis, the time horizon and the institutional, capital or policy regime; the assignment is not a fixed property of the variable. The edge labels illustrate a bank's own lending decision; for a macroeconomic determinant the level-of-analysis assignment runs the other way (Sect. "The conditional boundary")

[7] review the determinants of bank credit risk and likewise structure the field by the location of explanatory variables, across bank-specific, macroeconomic and industry-related determinants. Ozili [8] offers a bibliometric mapping of NPL research worldwide and catalogues emerging determinants—governance, fintech, financial inclusion, country and political risk—added to, rather than re-sorted within, the inherited scheme. A parallel strand of cross-country work adopts an explicit internal-versus-external dichotomy but treats each variable's assignment to one side or the other as fixed. The present paper takes Manz's observation one step further: the overlap Manz documents is not a feature of the categories to be resolved by studying their interaction, but a symptom of aggregating a relationship whose channel is conditional, which is why the interaction research programme proposed has proved difficult to complete.

What is striking is that the primary literature does not share this confidence. The problem was named at the outset. Berger and DeYoung [4], in the paper that established the endogenous vocabulary of this field through the bad luck, bad management, skimping and moral hazard hypotheses, close by proposing, as a direction for future research, an attempt "to decompose the determinants of loan quality into endogenous versus exogenous factors". The decomposition was posed as an open problem in 1997. So far as the reviews record, it has not been performed. What the subsequent literature supplies instead is a taxonomy of where variables originate, which is a different thing entirely, and which the founding paper did not claim to be equivalent.

Nor is the difficulty merely unresolved; it has been repeatedly rediscovered and then set aside. Ozili [9], examining bank-specific, regulatory and macroeconomic determinants across four continents, concludes that the significance and predictive power of each factor—across all three categories at once—depends on regional and, more strongly, on country-specific factors. That is a finding that bears on classification as much as on determinants: whether a variable matters at all is contingent on the institutional setting in which it is observed. The same study reports that regulation bears no predictable relationship to NPLs, offers one explanation and leaves alternatives to future research. Neither result is taken up as a question about classification in the reviews that followed, including the bibliometric survey conducted a decade later by the same author [8].

This paper takes the accumulated reversals seriously. The claim is not that the literature has failed to notice that determinants behave inconsistently—it has noticed repeatedly, and has recorded each instance as a local puzzle, a robustness caveat, or a call for further work. The claim is that a subset of these instances share a structure, that the structure is the conditionality of the endogenous–exogenous boundary, and that a classification which fixes the boundary cannot accommodate them. What follows is not a competing list of determinants but an argument about the classificatory operation itself: the boundary it draws moves, the movement can be identified in the reported evidence, and it recurs across determinants that the taxonomy places in different categories.

The conditional boundary

Three sources of conditionality recur across the NPL evidence; they are summarised schematically in Fig. 1.

Level of analysis

The exogeneity of a determinant depends on whether the unit of observation is the individual bank or the banking system. Aggregate credit conditions, asset prices and lending rates are external to any single institution, which takes them as given when it makes lending decisions. They are not external to the banking sector as a whole, whose collective behaviour helps generate them. A system-wide loosening of credit standards inflates asset prices and borrowing capacity, which later unwinds as the very downturn that subsequent studies record as a macroeconomic shock. The same variable is thus exogenous at the level of the firm and endogenous at the level of the system, and a classification that does not specify the level cannot say which. The point is not hypothetical: Beck et al. [2], recommending that exchange rates be incorporated into macro-stress-test scenarios, observe that the recommendation "presents a challenge to macroeconomic modelling which often abstracts from asset prices and (endogenous) exchange rates"—the parenthesis is theirs.

Time horizon

Exogeneity is also a function of the window over which a determinant is observed. Over a short horizon, a macroeconomic contraction is a shock that strikes banks from outside. Over a longer horizon, that same contraction may be the lagged product of a preceding credit expansion driven by the banks themselves. The determinant that looks external within a single business cycle becomes endogenous once the cycle that produced it is brought into view. Classification by origin implicitly fixes a horizon and then forgets that it has done so.

Regime and policy response

The clearest case, and the one most relevant to a regulatory readership, is the determinant a fixed taxonomy most consistently mishandles. Capital requirements, supervisory intensity, forbearance measures and monetary responses are filed as institutional or external determinants, yet they are reactions to the state of the banking system rather than conditions imposed upon it independently of that state. Regulation tightens because banks took risk; forbearance is extended because losses are mounting; provisioning rules bind differently as credit quality turns. Treating these responses as exogenous regressors confounds the policy with the conditions that elicited it and assigns to an external category a determinant that is endogenous by construction. A determinant's channel may also depend on the regime prevailing within the bank itself—most sharply, as Sect. "Determinants whose channel is conditional" discusses, on the level of capital it holds—and on the rules under which capital and provisions are measured. The regime dimension is itself heterogeneous. Capital is measured under different frameworks: an unweighted ratio of equity to assets, a risk-weighted regulatory ratio under a standardised approach, or one under an internal-ratings-based approach in which the weights depend on the bank's own risk estimates. Provisioning is governed either by an incurred-loss standard, under which a loss is recognised once a trigger event has occurred, or by an expected-credit-loss standard, under which forward-looking estimates and management judgement enter by design. Sects. "Determinants whose channel is conditional" and "Implications for banking regulation and supervision" return to both.

Determinants whose channel is conditional

The instability is not abstract, and it does not require unusual studies to display it. Table 1 collects the reported findings of the thirteen studies selected in Sect. "Introduction" on three of the most heavily studied determinants—bank capital, economic growth and credit growth—together with provisioning and crisis policy. The final column codes the kind of evidence each row supplies: H, a relationship that differs across studies or contexts (heterogeneity); W, variation observed within a single study with sample, measure and estimator held fixed; D, an estimated reversal in the direction of influence, or feedback from NPLs to the determinant; C, a candidate channel switch, in which distinct mechanisms are attributed by the source to different contexts; and P, a policy response that alters what the data record. Only rows carrying W together with D or C meet both parts of the definition in Sect. "Introduction"; rows carrying H alone establish heterogeneity. Read as a fixed taxonomy would read them, the rows contradict one another. Read as conditional channels, they need not, and the codes make explicit which rows carry the argument and which merely fail to contradict it.

Capital: the same determinant, both signs, and a threshold

Bank capital is simultaneously a bank's own choice and a regulatory constraint, and the literature reports it acting in both directions. It is also measured in several ways, and the measure matters for what follows: an unweighted ratio of equity to total assets, a regulatory ratio of total capital to risk-weighted assets, or a core (Tier 1) ratio, where under the Basel framework the risk weights themselves depend on whether a standardised or an internal-ratings-based approach is applied [10, 11]. Berger and DeYoung [4], using equity to



gross total assets, find in their full sample that lagged capital is positively associated with subsequent NPLs, but that for banks below the median capital ratio decreases in capital precede increases in NPLs, which they read as thinly capitalised banks responding to moral hazard incentives by taking greater portfolio risk. They stress that their hypotheses are not mutually exclusive, that any one may dominate in a subset of banks, and that their Granger-causality results are associations rather than proof of causation. They also observe that at low-capital banks rising NPLs precede rising capital, perhaps under pressure from regulators or capital markets—a feedback from asset quality to the regulatory variable. Ersoy [12], studying twenty-two Turkish commercial banks with a risk-weighted capital adequacy ratio, finds that increases in the ratio reduce NPLs and reads the negative sign, as Berger and DeYoung read theirs, as support for the moral hazard hypothesis: low-capital banks take more risk. Ferreira [13], across eighty countries over 1999–2019 with the ratio of total regulatory capital to risk-weighted assets, finds a positive association in her full-sample system-GMM estimates and in both income sub-panels over the full period, a sign that reverses between one-step and two-step estimates in her OECD sub-panel, and after 2008 a negative association in the non-high-income and non-OECD sub-panels alongside a continued positive one in the high-income and OECD sub-panels—a contrast she reads against the increased role of regulatory authorities after the crisis, concluding that capital adequacy contributed to lower NPLs only outside the high-income and OECD groups.

Read as fixed determinants, these findings sit uneasily together. Hajja [5] supplies the one case in which both signs appear within a single estimation. Studying nineteen Malaysian commercial banks with an equity-to-assets ratio, he finds the capital function to be concave: increasing capital initially raises NPLs, under what he identifies as the moral hazard effect—a label he attaches to the opposite sign from that in Berger and DeYoung and Ersoy—until a threshold is reached, beyond which further capital reduces NPLs, under what he calls the disciplinary or regulatory effect. He locates the turning point at an equity-to-assets ratio of around eleven per cent. Within that study, then, the sign of capital's effect is not a property of capital but of the capital regime, and the two regimes are attributed to two distinct mechanisms—one behavioural and internal to the bank, one imposed by regulation. That is a within-study sign reversal with mechanisms attributed by the source, and Table 1 records it as a candidate switch. Two limits should be stated. The mechanisms are Hajja's interpretation rather than a direct estimate, and his own Granger tests run from NPLs to capital rather than the reverse. And because his threshold is defined on an unweighted equity ratio, it cannot be used to reconcile Ersoy and Ferreira, whose measures are risk-weighted regulatory ratios; Berger and DeYoung's ratio is also unweighted, but their sample, period and estimator differ. The contrasts among the four studies establish heterogeneity, and part of that heterogeneity may be measurement rather than mechanism. What the four studies establish jointly is narrower than a single reconciled curve but more secure: within every study that partitions its sample by capital level or estimation window, the reported relationship between capital and NPLs changes, and in two of them the change is accompanied by a feedback from NPLs to capital for which a fixed taxonomy has no place.

Economic growth: the determinant most confidently classified as external

If any determinant were unconditionally exogenous, it would be aggregate output. The evidence does not support that reading. Beck et al. [2] conclude that real GDP growth had been the principal driver of NPL ratios over the preceding decade. Espinoza and Prasad [14] find a significant negative relationship between real non-oil GDP and NPLs in the Gulf Cooperation Council—and then, in a panel vector autoregression, estimate a strong though short-lived feedback effect running from NPLs back to non-oil growth, with a semi-elasticity they put at around 0.4, which they expect to be nonlinear beyond a threshold. Klein [15] finds NPLs in Central, Eastern and South-Eastern Europe attributable to both macroeconomic conditions and bank-specific factors, and finds that increases in NPLs significantly affect credit, real GDP growth, unemployment and inflation in subsequent periods. In both studies the external variable is estimated to be partly generated by the banking system it is said to act upon.

The sign and significance are no more stable than the direction. Hajja [5] finds higher GDP growth *raises* NPLs in Malaysian commercial banks, a positive coefficient he interprets as procyclical behaviour of NPLs and reads as consistent with prior evidence from five European countries. Mishra et al. [16] find GDP growth to be an insignificant determinant of Indian non-performing assets—although industrial production carries a significant negative coefficient, so that a cyclical channel is detected through that indicator rather than through output growth—and note explicitly that this contrasts with most prior studies, concluding that cyclical booms and recessions have not significantly influenced NPAs in that setting. Kartal et al. [17], using wavelet coherence on Turkish data, find that economic growth explains NPLs in the long run while a separate short-run linkage appears only in the 2007–2009 crisis window, which the authors' conclusion reports as running from NPLs to economic growth—on that reading, the direction of causality itself reverses with the horizon and the crisis window.

Ferreira [13], whose growth variable is the level of real GDP per capita rather than its growth rate, concludes that it is a more important determinant than the bank-level and regulatory variables in every panel she estimates, although after 2008 it loses significance in one of the two estimates reported for each of the OECD and non-OECD sub-panels.

Across the six growth studies in Table 1, then, economic growth reduces NPLs, dominates all other determinants, raises NPLs, is insignificant, and swaps causal direction with the observation window. Not all of that spread carries the same weight. The contrasts between Beck et al., Hajja, Mishra et al. and Ferreira are contrasts across studies that differ in sample, period, measure and estimator; they establish heterogeneity and admit the ordinary methodological explanations. The findings of Espinoza and Prasad, Klein and Kartal et al. are different in kind: each estimates, within its own data, that the direction of influence between output and NPLs is not fixed, and each meets the second requirement of the definition in Sect. "Introduction" by a direct estimate rather than by interpretation. What the within-study evidence supports is narrower and more precise: in Espinoza and Prasad and in Klein, the macro channel and the feedback from NPLs coexist at the level of the system, so that output is exogenous to any one bank and partly endogenous to the banking system; and in Kartal et al. the direction of transmission also depends on whether the horizon is short enough to isolate the shock from the credit cycle that helped generate it. What the cross-study evidence adds is that the sign and significance of the same variable also vary with the institutional setting—its capital regime, its currency exposure, its degree of intermediation—in ways that are consistent with the argument but do not by themselves establish it.

Credit growth

Vithessonthi [18] finds that credit growth has a positive and statistically significant effect on Japanese NPLs before the global financial crisis, while after its onset the coefficient is negative and significant only at the ten per cent level, which he describes as a weak association and interprets as evidence that the effect of credit growth on loan quality varies over time. The variation is within a single study, but it is a change of sign across cyclical regimes rather than an estimated change of mechanism, and Table 1 codes it accordingly. At the level of the individual bank, credit growth is a behavioural lending choice; in aggregate it drives the credit cycle that later returns as an external shock. Accornero et al. [19] sharpen the point into a statement about identification. Controlling for borrower conditions, they find the link between NPL ratios and credit growth becomes tenuous to non-existent, and attribute the raw correlation to common factors—declining firm profitability, investment opportunities and credit demand—driving both. Yet their instrumental-variable estimates indicate that exogenous shocks to NPLs do causally reduce credit supply, an effect they judge quantitatively minor in Italy over their sample. Their conclusion is explicitly conditional: whether NPLs constitute a problem at all cannot, they argue, be judged without a strategy that identifies the shocks banks face and a reliable estimate of their nature and size. Here the two senses distinguished in Sect. "Introduction" bear on one result without collapsing into each other: which way influence runs between credit growth and NPLs—the channel in the mechanism sense—can be read off only once the econometric question of identification has been settled, and the answer is that, with common factors removed, the estimated influence runs from NPLs to credit rather than the reverse.

Provisioning

Ozili [9] finds loan loss reserves, loan growth and the loan-to-asset ratio, together with the state of the economy, to be important determinants of NPLs with significant explanatory power, and reports evidence of procyclical NPL behaviour indicating cyclical interactions and macro-financial linkages. Provisioning enters the literature both as a rule-bound institutional requirement and as an item managers adjust across the cycle, and which of these dominates depends on the accounting regime. Under an incurred-loss standard, a provision is recognised when an objective trigger event has occurred; the rule fixes the timing, and discretion enters mainly through the recognition of the trigger. Under an expected-credit-loss standard such as IFRS 9 [20], provisions are forward-looking by design, incorporate macroeconomic scenarios and require significant judgement about staging and significant increases in credit risk, and in practice about the use of management overlays; discretion is not a departure from the rule but part of it. The channel is therefore set by the institutional context rather than by the variable, and it is set differently under the two regimes. The regime also has to be checked against the sample. Ozili's data end in 2013, before expected-credit-loss accounting standards took effect in any jurisdiction in his sample. Mishra et al. [16] find provisions and contingencies positively associated with NPAs in India over 2010–2019, a period in which Indian banks remained under the Reserve Bank's incurred-loss prudential norms because the transition to Ind AS 109 was deferred in 2018 and then indefinitely in 2019, with final expected-loss provisioning directions issued in April 2026 and taking effect from April 2027 [21, 22]. The finding is consistent with either a mechanical or a discretionary reading and discriminates between neither—which is precisely the interpretive difficulty a fixed classification conceals.



Crisis and policy response

Žunić et al. [23] examine quarterly data for the banking sector of Bosnia and Herzegovina over 2012–2020 and find that improvement in NPLs is largely attributable to improving economic conditions, while NPL growth is associated, weakly, with growth in loan loss provisions. On COVID-19—as close to a clean exogenous shock as this literature offers—their pandemic indicator carries a negative and significant coefficient, the opposite of what they expected, and they report that the impact on NPLs is not yet adequately noticeable because the loan moratoria introduced by the Banking Agency largely prevented loans from migrating into the non-performing category; the real effect, they expect, will appear only after the moratorium period expires. They also record that regulators across the region eased the classification of credit risk and altered the period after which a loan is treated as uncollectible. The interpretation this paper draws—that the measured effect of the shock was not biased by the policy response but absorbed by it—is the paper's rather than the source's, and it rests on their reported reasoning. What the data record is not the shock but the shock net of the intervention that answered it, and the intervention was itself a response to the shock. There is no observation of the exogenous determinant that is not already conditioned on the endogenous reply. A regime note belongs here too: the sample straddles the adoption of IFRS 9 in Bosnia and Herzegovina from 2018 and the Federation Banking Agency's expected-credit-loss decision, with minimum provisioning rates [24], and the study addresses neither, so its provisioning result is estimated across two regimes.

Implications for empirical inconsistency

The rows of Table 1 bear on a well-known feature of the NPL literature: the same determinant is reported with different signs and magnitudes across studies, and sometimes as insignificant where it was elsewhere decisive. The standard explanations are methodological, and they deserve to be taken seriously before a conditional boundary is invoked. Four are relevant here. Sampling and period differences: studies drawn from different countries and windows may simply estimate different parameters, and cross-study contrasts such as those between Beck et al., Hajja and Mishra et al. on growth cannot rule this out. Measurement: two of the capital studies in Table 1 use an unweighted equity ratio and two use a risk-weighted regulatory ratio, and an apparent reversal of sign across studies may partly reflect the measure rather than the mechanism. Estimator and specification: Ferreira's capital coefficient changes sign between one-step and two-step system-GMM estimates on the same OECD sub-panel, and Accornero et al. find that the credit-growth correlation weakens once borrower conditions are controlled for, so omitted variables and dynamic bias can manufacture inconsistency without any change of channel. And reporting: primary studies that find a stable relationship are less likely to remark on it, so a set of studies assembled for variation, as this one was, will overstate how often variation occurs.

These explanations account for much of the cross-study spread, and the paper does not claim otherwise; that is why Table 1 codes such rows as heterogeneity. What they do not readily account for is the within-study evidence. Differences of sample and period across studies cannot reverse the estimated direction of causality between two series within one dataset according to the frequency band examined, as in Kartal et al.; they do not produce feedback from NPLs to output in a panel vector autoregression estimated on the same data that record the forward effect, as in Espinoza and Prasad and Klein; and they do not place a sign reversal at an estimable threshold with a distinct mechanism attributed to each side, as in Hajja. If a determinant's channel is conditional on level, horizon and regime, then pooling estimates across those conditions without allowing the channel to vary will produce the pattern observed: coefficients that change sign across regimes, lose significance across institutional settings and resist synthesis. The inconsistency is then partly signal as well as noise, evidence that a context-dependent relationship is being forced through a context-free category. Some disputes in the literature are, on this reading, not waiting to be settled by a better dataset; they are artefacts of classification, and they ease once the determinant is treated as a determinant-in-context rather than a determinant in isolation. Ozili's [9] conclusion that the significance of each factor depends on country-specific conditions is not only a limitation of his sample. It may be the general case, stated in the particular.

The object that can be coherently classified, then, is not the determinant but the determinant together with the context that fixes its channel. This has an immediate corollary for synthesis. A synthesis that pools reported coefficients without modelling the contextual moderators identified here—level, horizon and regime—risks averaging across distinct channels rather than across estimates of a common effect, and its central tendency can then be mistaken for a structural constant when it is a mixture whose value depends on the composition of the study sample. It also has a corollary for primary work: reporting that a determinant is significant is incomplete without specifying the level, horizon and regime under which the finding obtains because those are constitutive of the effect rather than incidental to it.



Implications for banking regulation and supervision

The conditionality of the boundary is not only an interpretive nicety. It bears directly on regulatory practice, because the determinants whose exogeneity is least secure—regulation, capital and provisioning—are precisely the ones supervisors rely on as instruments. Three implications follow, and each is anchored in a finding reported above.

First, the evaluation of regulation. Cross-country evidence on whether regulatory and supervisory design reduces NPLs is often read as if regulation were exogenous to bank conditions. It is not: regulation tightens in response to prior risk-taking, so its cross-sectional correlation with credit quality blends the effect of the rule with the conditions that prompted it. The literature exhibits the symptom. Ozili [9] finds that regulation bears no predictable relationship to NPLs, offers one explanation and leaves alternatives to future research. Ferreira [13] finds that the regulatory capital ratio—the instrument most directly intended to strengthen banks against credit losses—is associated with higher NPLs across eighty countries over 1999–2019, and after 2008 with lower NPLs in the non-high-income and non-OECD sub-panels but still with higher NPLs in the high-income and OECD sub-panels—a pattern she reads against the increased role of regulatory authorities after the crisis, concluding that capital adequacy contributed to lower NPLs only outside those two groups. The reverse-channel reading is the alternative: regulatory capital is highest where it has been raised in answer to deteriorating asset quality, so that the positive coefficient recovers the condition rather than the cure, and the post-crisis change records a period in which capital was raised across the board rather than in response to bank-specific deterioration. Neither reading can be established from her cross-country estimates alone; the point is that the second is not available at all if the ratio is entered as an exogenous regressor. The capital framework adds a further layer. Under a risk-weighted regime the denominator of the ratio moves with the bank's own risk assessments, most directly under the internal-ratings-based approach, so that the regulatory variable is partly generated by the conditions it is meant to measure [10, 11]; under an unweighted leverage ratio it is not. Evidence on capital and NPLs therefore has to be read against the framework and approach in force, and comparisons across Basel I, II and III regimes, or across standardised and internal-ratings banks, are comparisons across channels as well as across coefficients. The same check that Sect. "Determinants whose channel is conditional" applies to the provisioning studies applies here: taken together, the windows of the capital studies in Table 1 run across successive Basel frameworks, and none of the four partitions its estimates by the framework in force. Evidence-based rule design that ignores the reverse channel will misestimate the marginal contribution of supervision and may read as ineffective a regime that is in fact responding to conditions it did not create.

Second, provisioning and procyclicality. Loan-loss provisioning is at once a regulatory requirement and a managerial lever, and the balance between the two is set by the provisioning regime. Under the incurred-loss approach that governed most of the samples in Table 1, a provision is recognised only once an objective loss event has occurred; the rule is backward-looking, and managerial discretion operates mainly in the timing of recognition, which the accounting literature finds to be used to smooth income and to delay loss recognition ahead of downturns [25]. Under expected-credit-loss standards—IFRS 9 from 2018 in most jurisdictions that apply it [20], with the Basel Committee's supervisory guidance on their implementation [26]—provisions are forward-looking, incorporate macroeconomic forecasts and require significant judgement about the staging of exposures and, in practice, about the use of management overlays. Discretion is thereby moved from the margins of the rule into its centre, and the behavioural channel that incurred-loss provisioning admitted only through timing is present by design. The evidence in Table 1 is almost entirely from the incurred-loss era: Ozili's [9] sample ends in 2013; the Indian banks in Mishra et al. [16] remained under incurred-loss prudential norms throughout 2010–2019, with final expected-loss provisioning directions issued in April 2026 and taking effect from April 2027 [21, 22]; and Žunić et al. [23] straddle the Bosnian transition without modelling it [24]. None of these results discriminates between the mechanical and the discretionary channel, because the observable is identical under both. Treating provisions as an exogenous control variable therefore obscures the behavioural and forbearance channels through which they also operate, and supervisory inferences drawn from provisioning levels risk misreading managed figures as fundamentals. Under expected-loss regimes the difficulty is sharper rather than resolved: the forward-looking component is precisely the one in which judgement is exercised, so that the provisioning variable is endogenous to management's expectations by construction. Recognising provisioning as a conditional channel—mechanical with discretion at the margin under incurred-loss standards, and mechanical and discretionary at once under expected-loss standards—is a precondition for interpreting it correctly in supervisory monitoring.

Third, macroprudential calibration and stress testing. Stress tests treat macroeconomic paths as exogenous scenarios imposed on the banking system. The evidence surveyed above indicates that, at the level of the system and over horizons long enough to include the credit cycle, they



are not. Espinoza and Prasad [14] estimate a feedback effect running from NPLs to non-oil growth and expect it to be nonlinear beyond a threshold; Klein [15] finds NPL increases significantly affecting GDP growth, credit, unemployment and inflation in subsequent periods; Beck et al. [2], recommending richer scenarios, concede that macroeconomic modelling often abstracts from endogenous exchange rates. Scenario design that suppresses the bank-to-macro feedback risks understating tail risk where the downturn is partly of the system's own making. Žunić et al. [23] supply the complementary warning for crisis calibration: where forbearance defers or absorbs a shock, the historical relationship between the shock and measured credit quality is a relationship between the shock and the policy response, and a model estimated on such data will tend to forecast the response rather than the loss. Conditionality implies that supervisory models should be state-contingent and horizon-aware. The practical counsel for regulators mirrors the methodological one for researchers: the determinant is not the unit to target; the determinant-in-context is.

Conclusion

The central assumption underlying much of the literature on non-performing loan determinants is that each determinant occupies a fixed position within a stable classificatory scheme, operating through either an endogenous or an exogenous channel. This paper has argued that the assumption does not hold unconditionally. The evidence reviewed here indicates that a determinant's operative channel—in the mechanism sense defined in Sect. "Introduction"—is not an intrinsic property of the variable itself but depends on the level of analysis, the time horizon and the institutional, capital or policy regime in which it is observed. The apparent contradictions that recur throughout the literature—bank capital reducing NPLs in some settings and increasing them in others, economic growth changing sign or reversing causal direction, and regulation exhibiting no stable relationship with NPLs—are therefore not all isolated anomalies. Some are ordinary heterogeneity, explicable by sample, measure and method; a smaller set, observed within single studies and attributed by their authors to distinct mechanisms, share a structure that a fixed classification cannot represent.

Exogeneity is conditional on the level at which the banking system is observed, the horizon over which it is examined, and the capital, institutional and policy regime prevailing when it is measured. A number of ostensibly external determinants—regulation, supervision and provisioning among them—are responses to banking-sector conditions rather than independent of them, and the degree to which this holds itself varies with the capital framework and the provisioning standard in force. Recognising the conditionality is a precondition for any classification that hopes to do more than relabel, and for any supervisory model that hopes to read its instruments correctly. Berger and DeYoung [4] proposed the decomposition of loan-quality determinants into endogenous and exogenous factors as a research programme in 1997. The intervening decades suggest why it has not been completed: the decomposition cannot be performed unconditionally, because the boundary it presupposes is not fixed.

Several research directions follow. First, channel switching as defined here is in principle measurable: rather than absorbing sign reversals into estimation error, future work could test directly whether a determinant's sign, significance or direction of influence varies with the level of analysis, the time horizon and the capital or cyclical regime, holding sample, measure and estimator fixed, in the manner in which Hajja [5] locates a threshold in the capital function or Kartal et al. [17] locate a reversal of causality across frequency bands. Second, the endogeneity of regulation and provisioning invites designs that model the policy response explicitly—instrumenting supervisory intensity or rule changes by their triggers—so that the marginal effect of an instrument can be separated from the conditions that prompted it. Third, the bank-to-macro feedback estimated by Espinoza and Prasad [14] and Klein [15] could be built into stress-test scenario design, allowing the most damaging downturns to be represented as partly system-generated rather than purely exogenous. Fourth, supervisory monitoring of provisioning could seek to distinguish the mechanical from the discretionary component, since the two carry different signals about underlying credit quality despite an identical observable, and the transition from incurred-loss to expected-loss standards offers a natural setting in which to do so. Fifth, and most directly, the conditions identified here—level, horizon and regime—are candidate moderators for a systematic re-examination of the determinants literature, in which apparent disagreement is treated as data about the boundary rather than noise about the coefficient, and in which the alternative explanations set out in Sect. "Implications for empirical inconsistency" are tested rather than assumed away. Pursuing these questions is the route by which a conditional, context-aware classification of NPL determinants might eventually be constructed.

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