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**Reading Between the Balance Sheets:
Earnings Quality as a Predictor of Deal Outcomes in Banking M&A**

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الْحَمْدُ لِلَّهِ

To my parents, for their love and every sacrifice they made so I could be here.

To my friends, who became my family.

To those who had to work twice as hard to belong, and still chose to dream.

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AI Disclosure Statement

This thesis was written independently by the author. During the drafting process, AI-assisted writing tools were used for text revision and improvement of clarity and coherence, and for structuring arguments. All content, analysis, interpretation of results, and academic judgements are the author's own. The author has independently verified the accuracy and reliability of all information included in this work.

Table of Contents

1. Introduction	1
1.1 Motivation and Background	1
1.2 The Gap in the Literature	2
1.3 Data, Main Findings and Structure of the Thesis	3
2. Literature Review.....	4
2.1 Introduction to the Chapter.....	4
2.2 Earnings Quality: Concept and Measurement.....	5
2.2.1 Measuring Earnings Quality: Accrual-Based Models	6
2.2.2 Consequences of Earnings Quality: Why Deal Completion May Be Affected	7
2.3 Accounting Discretion in Banking: The Loan Loss Provision.....	8
2.3.1 Critiques and Identification Challenges in the LLP Literature	10
2.4 Information Asymmetry and the Market for Corporate Control	11
2.4.1 Bank-Specific Financial Statement Variables Used in This Thesis	12
2.4.2 Payment Method as a Response to Information Asymmetry.....	13
2.5 Determinants of M&A Deal Completion	14
2.5.1 Alternative Explanations for Deal Non-Completion	15
2.6 Banking M&A: Motives, Patterns, and Cross-Country Differences.....	16
2.6.1 Cross-Border Banking M&A and Information Frictions.....	18
2.7 Synthesis and Hypothesis Development	19
2.7.1 Expected Signs and Testable Predictions.....	20
2.8 Chapter Summary.....	21
3. Institutional and Regulatory Context	22
3.1 Introduction to the Chapter.....	22
3.2 The U.S. Bank Merger Approval Framework	22
3.2.1 Antitrust Review in the United States: The Department of Justice	24
3.2.2 Prudential Capital Requirements: The Basel III Framework.....	24
3.3 The European Bank Merger Approval Framework	25
3.3.1 EU Competition Law: The Merger Regulation	27

3.3.2 Resolution Regimes: A Related but Distinct Framework	27
3.4 Comparing the Two Regimes: Implications for This Thesis.....	28
3.4.1 A Fourth Point: Dual-Track Approval and Timeline Considerations	29
3.4.2 Deposit Insurance and Depositor Confidence in the Approval Process	30
3.5 Historical Evolution: Bank M&A Regulation Since the 2008 Financial Crisis	31
3.5.1 Sample Period Coverage of Major Regulatory Milestones	33
3.6 Chapter Summary4.....	33
3.6.1 Implications for Variable Construction in Chapter 4	34
4. Data and Methodology.....	35
4.1 Introduction to the Chapter.....	35
4.2 Sample Construction	35
4.2.1 Data Extraction Methodology	36
4.3 The Dependent Variable: Deal Status.....	37
4.3.1 Timing of Deal Status Classification	38
4.4 Variable Description (Table 1).....	38
4.5 Sample Distribution (Table 2)	40
4.6 Variable Transformation: Logs, Ratios, and Winsorization.....	43
4.7 Summary Statistics (Table 3)	44
4.7.1 Full Sample.....	44
4.7.2 United States Sub-Sample	45
4.7.3 European Sub-Sample	46
4.7.4 Correlation and Multicollinearity Considerations	48
4.8 Empirical Model Specification.....	48
4.9 Chapter Summary.....	49
5. Baseline Empirical Results.....	50
5.1 Introduction to the Chapter.....	50
5.2 Table 4: Baseline Probit Results.....	50
5.3 Model Fit and the Sample-Size Trade-Off	52
5.4 Interpreting the Accounting-Quality Variables (H1)	53
5.5 Interpreting the Control Variables	54
5.6 A Result Contrary to Prediction: The Tier 1 Capital Ratio.....	55

5.7 Chapter Summary	56
6. Robustness Analysis.....	57
6.1 Introduction to the Chapter.....	57
6.2 United States versus Europe (H2)	57
6.2.1 Multicollinearity Diagnostics	59
6.3 Stricter Definition of Non-Completion	60
6.4 Alternative Capital-Adequacy Proxy.....	62
6.5 Synthesis: What This Evidence Does and Does Not Support	63
6.5.1 Practical Implications	64
6.6 Chapter Summary.....	65
7. Conclusion.....	65
7.1 Introduction to the Chapter.....	65
7.2 Summary of the Thesis.....	65
7.3 Summary of Empirical Findings	66
7.4 Contribution to Literature.....	67
7.5 Practical and Policy Implications	68
7.6 Limitations.....	68
7.7 Directions for Future Research.....	70
7.8 Concluding Remarks	71
References	72
Appendix: R code.....	75

ABSTRACT

This thesis investigates whether the accounting quality of target banks is associated with the completion status of banking mergers and acquisitions (M&A). Drawing on the theory of information asymmetry (Akerlof, 1970) and its application to M&A settings (Officer, Poulsen, and Stegemoller, 2009), the central argument is that targets with weaker accounting quality, reflected in less conservative loan loss provisioning, higher non-performing loans, and weaker capitalization, increase valuation uncertainty for the acquirer, making deal completion less likely.

Banks are a natural setting for this question. Unlike non-financial firms, their balance sheets contain accounting items, most notably loan loss provisions, over which managers exercise considerable discretion for income smoothing and signaling purposes (Kanagaretnam, Lobo, and Yang, 2004), making accounting quality both economically material and empirically tractable.

The empirical analysis draws on a sample of 392 banking M&A deals with targets headquartered in the United States and Europe, collected from Bloomberg and spanning fiscal years 2016 to 2025. Target-level accounting variables, including size, leverage, asset quality, profitability, and capital adequacy, are measured for the fiscal year immediately preceding the announcement, consistent with the bank M&A literature (DeYoung, Evanoff, and Molyneux, 2009). Given the sample's numerical weight toward U.S. targets, the baseline model is re-estimated on the European sub-sample separately to assess whether results generalize across regulatory environments.

Keywords: *earnings quality, loan loss provisions, banking M&A, deal completion, information asymmetry, probit model, United States, Europe*

1. Introduction

1.1 Motivation and Background

Mergers and acquisitions have long been a primary mechanism for the banking industry to restructure in response to competitive, regulatory, and technological pressures. Following the wave of consolidation that reshaped the U.S. and European banking landscapes from the 1990s onward, bank M&A activity has remained a persistent feature of the sector, driven by motives ranging from scale economies and geographic diversification to distressed-bank resolution and the pursuit of systemic scale (DeYoung, Evanoff, and Molyneux, 2009). Unlike M&A transactions in most other industries, bank mergers take place within a heavily regulated environment, where supervisory approval, capital adequacy requirements, and depositor protection considerations add layers of complexity and potential friction to the deal process. A transaction that is announced is therefore not guaranteed to be completed: deals can be renegotiated, delayed, or ultimately withdrawn, and understanding what drives this variation in deal outcomes is of direct relevance to acquirers, target shareholders, supervisors, and advisors alike.

A parallel and largely separate stream of research has examined the quality of the accounting information that firms report, and the extent to which reported earnings reflect a firm's underlying economic performance rather than the exercise of managerial discretion. Dechow, Ge, and Schrand (2010) provide a comprehensive review of the many proxies used to capture this construct, commonly referred to as earnings quality, including accrual-based measures, earnings persistence, smoothness, and timeliness, and document that the determinants and consequences of earnings quality vary substantially depending on the empirical setting and the accounting items involved. In the specific context of banks, this literature has a natural and well-studied focus: the loan loss provision. Because the provision for loan losses is inherently an accounting estimate, as banks must forecast expected credit losses on loans that have not yet defaulted, it affords bank managers considerable discretion, and a substantial body of evidence shows that this discretion is used both to smooth reported income over time and, under certain conditions, to signal private information about a bank's prospects to the market (Kanagaretnam, Lobo, and Yang, 2004).

These two literatures, bank M&A and bank accounting quality, have rarely been brought together empirically. Yet there are strong conceptual reasons to expect that the two are linked. An acquirer negotiating the purchase of a target bank relies substantially on the target's reported financial

statements to assess asset quality, capital adequacy, and the true economic value of the loan book being acquired. If those financial statements are of lower quality, that is, if the reported loan loss provisions and related asset-quality metrics are a less reliable representation of the target's actual credit risk, the acquirer faces greater valuation uncertainty during due diligence, greater scope for a subsequent breakdown in negotiations once further information is revealed, and a higher risk that the transaction will be renegotiated or abandoned altogether. This thesis develops and tests this argument directly.

1.2 The Gap in the Literature

The determinants of M&A deal completion have received sustained attention in the finance literature, particularly through the lens of contractual devices such as termination fees. Officer (2003) and Bates and Lemmon (2003) both show that including termination fee provisions is associated with a significantly higher probability that an announced deal is ultimately completed, interpreting this evidence as consistent with termination fees functioning as an efficient contractual mechanism that commits both parties to the transaction. This literature, however, is largely based on samples of non-financial firms and focuses on deal-level contractual features rather than on the target's accounting characteristics.

A separate literature examines how target-firm opacity and information asymmetry shape acquirer behavior. Officer, Poulsen, and Stegemoller (2009) show that acquirer returns are systematically higher in stock-swap acquisitions of targets that are difficult to value, consistent with Akerlof's (1970) analysis of adverse selection under quality uncertainty. This literature has focused primarily on acquirer returns and payment method, and has not been extended to banking, where the relevant information asymmetry is concentrated in a specific and well-understood accounting item. To the best of the author's knowledge, the direct link between target bank accounting quality and deal completion has not been empirically tested in a dedicated banking M&A sample. This gap motivates the present thesis.

The theoretical framework that underlies the empirical analysis is built on Akerlof's (1970) model of markets with asymmetric information. In banking, this asymmetry is concentrated in the allowance for loan losses and the related provision expense. A bank's management observes loan-by-loan information about borrower creditworthiness and default risk that is not observable to an outside acquirer during due diligence. Where managers use discretion over loan loss provisions to smooth

earnings or delay recognition of deteriorating asset quality, as documented by Kanagaretnam, Lobo, and Yang (2004), the resulting financial statements convey a distorted picture of the target's true condition. An acquirer that cannot fully unwind this distortion must either discount its valuation or abandon the transaction altogether once further information emerges during negotiations or regulatory approval. Both responses are consistent with a lower probability of deal completion for targets with weaker accounting quality.

The central research question of this thesis is therefore: does the accounting quality of a target bank, measured at the fiscal year preceding deal announcement, predict the completion status of an announced banking M&A transaction, and does this relationship hold consistently across the United States and European sub-samples? The severity of the information asymmetry problem plausibly differs between the two regions given differences in supervisory regimes, accounting standards, and disclosure practices, which motivates the sub-sample analysis in Chapter 6.

1.3 Data, Main Findings and Structure of the Thesis

The empirical analysis is based on a sample of 392 banking M&A deals with targets headquartered in the United States or Europe, extracted from Bloomberg. Target-level accounting variables are measured for the fiscal year immediately preceding each deal's announcement date, consistent with the convention of capturing information available to the acquirer at the time of evaluation. All monetary variables are expressed in US dollars using annual average exchange rates for the relevant fiscal year. The final sample spans fiscal years 2016 to 2025 and is numerically weighted toward United States targets, which motivates the sub-sample robustness analysis in Chapter 6.

The baseline probit regression yields qualified support for the central hypothesis. Financial leverage emerges as the most robust predictor of deal completion across all model specifications, consistent with the argument that higher leverage signals greater balance-sheet risk and increases acquirer uncertainty. The Tier 1 capital ratio is also statistically significant but carries a positive coefficient, contrary to the direction predicted by H1. This result is interpreted as reflecting the role of well-capitalized targets as attractive and credible counterparties, a mechanism that may operate alongside, rather than against, the information-asymmetry channel. The sub-sample analysis in Chapter 6 reveals that the relationship does not replicate uniformly across geographies, providing only partial support for H2 and suggesting that the regulatory and institutional environment moderates the link between accounting quality and deal outcomes.

This thesis makes three contributions to literature. First, it connects the bank accounting quality and M&A deal completion literatures, which have to date been examined largely in isolation. Second, it uses bank-specific accounting variables grounded in the loan-loss-provision literature rather than the generic accrual-based measures used in studies of non-financial firms. Third, the cross-country design directly addresses the concern that pooled results in global M&A samples may reflect U.S.-specific dynamics rather than a general relationship.

The remainder of the thesis is organized as follows. Chapter 2 reviews the relevant literature and develops formal hypotheses. Chapter 3 discusses the institutional and regulatory context of banking M&A in the United States and Europe. Chapter 4 describes the sample construction and methodology. Chapter 5 presents the baseline empirical results. Chapter 6 reports the robustness analysis. Chapter 7 concludes.

2. Literature Review

2.1 Introduction to the Chapter

This chapter reviews the two strands of literature underpinning this thesis's empirical analysis and develops the formal hypotheses first introduced in Section 1.3. Section 2.2 introduces the concept of earnings quality and its principal measurement approaches in the general corporate finance and accounting literature. Section 2.3 narrows the focus to banking specifically, reviewing the extensive literature on the loan loss provision as the primary channel of managerial accounting discretion in bank financial statements, since this channel forms the empirical basis for the accounting-quality proxies used later in Chapter 4. Section 2.4 turns to the theory of information asymmetry and its documented consequences for the market for corporate control, connecting the accounting-quality literature of Section 2.3 to the M&A setting. Section 2.5 reviews the literature on the determinants of M&A deal completion specifically. Section 2.6 reviews the banking M&A literature, including its cross-country dimension. Section 2.7 synthesizes these strands into the formal hypotheses tested in Chapters 5 and 6, and Section 2.8 summarizes the chapter.

2.2 Earnings Quality: Concept and Measurement

The term earnings quality does not have a single, universally accepted definition in the accounting literature. Dechow, Ge, and Schrand (2010), in what remains the most comprehensive review of the field, note that researchers have used a wide array of proxies to capture the construct, including the persistence of earnings over time, the magnitude and behavior of accruals, the smoothness of reported earnings relative to underlying cash flows, the timeliness with which economic losses are recognized, the tendency of firms to avoid reporting losses or earnings declines, investor responsiveness to reported earnings, and external indicators such as financial restatements and enforcement actions by securities regulators. Rather than proposing a single unifying proxy, Dechow, Ge, and Schrand (2010) argue that the appropriate measure of earnings quality is contingent on the decision context: a proxy well suited to studying valuation may be poorly suited to studying debt contracting, and a proxy developed for non-financial firms may not translate cleanly to financial institutions, whose balance sheets and income statements are structured very differently.

A closely related and partially overlapping literature addresses earnings management specifically, the intentional use of managerial discretion within generally accepted accounting principles to alter reported financial results, as opposed to the broader, not-necessarily-intentional variation in earnings quality that Dechow, Ge, and Schrand (2010) survey. Healy and Wahlen (1999) provide an influential synthesis of this literature from the perspective of accounting standard setters, defining earnings management as occurring when managers use judgment in financial reporting and in structuring transactions to alter financial reports, either to mislead stakeholders about the underlying economic performance of the firm or to influence contractual outcomes that depend on reported accounting numbers. Healy and Wahlen (1999) document that earnings management is most readily identified, empirically, in settings where the accounting item in question requires a substantial degree of subjective estimation, a condition that is unusually pronounced for a specific class of assets in the banking industry, discussed next.

Both literatures share a common empirical challenge: because earnings quality and earnings management are not directly observable, researchers must rely on proxies, and the validity of any given empirical result depends on how well the chosen proxy captures the underlying construct in the specific setting being studied. This thesis follows the approach, common in the banking literature reviewed in the next section, of exploiting an accounting setting in which the scope for managerial discretion is unusually well identified, the bank loan loss provision, rather than relying on generic, cross-industry accrual measures that are not well suited to financial institutions' balance sheets.

Because this thesis, like much of the bank accounting literature reviewed in Section 2.3, relies on a specific and well-identified accounting item, the loan loss provision, rather than on economy-wide discretionary accrual estimation, it is useful to situate this choice relative to the broader methodology used to measure earnings management in the general (non-bank) accounting literature. This is the subject of the remainder of this section.

2.2.1 Measuring Earnings Quality: Accrual-Based Models

The dominant empirical approach to measuring earnings management in non-financial firms is to estimate the discretionary component of total accruals, the portion of accruals not explained by the firm's underlying operating activity, and therefore attributable, at least in part, to managerial choice. Jones (1991) develops the foundational model in this tradition, estimating non-discretionary accruals as a function of the change in revenues and the level of gross property, plant, and equipment, and treating the residual from this estimation, the portion of total accruals not explained by these fundamentals, as the discretionary, and therefore potentially manipulated, component. Jones (1991) applies this model in the setting of U.S. import relief investigations, showing that firms under investigation by the International Trade Commission report income-decreasing discretionary accruals during the investigation period, consistent with managers using accounting discretion strategically in response to a specific, identifiable regulatory incentive.

Dechow, Sloan, and Sweeney (1995) subsequently evaluate the Jones (1991) model against several competing specifications, including simpler models that treat total accruals as entirely discretionary, and industry-based models that use the median accrual behavior of other firms in the same industry as the non-discretionary benchmark, and find that a modified version of the Jones model, which further adjusts non-discretionary accruals for the change in accounts receivable, exhibits the greatest statistical power to detect earnings management, including revenue-based earnings management. At the same time, Dechow, Sloan, and Sweeney (1995) document an important limitation common to all of the models they test: even the best-performing specification has low power to detect economically plausible magnitudes of earnings management, and all models tend to over-reject the null hypothesis of no earnings management when applied to samples of firms with extreme underlying financial performance, a finding that has motivated substantial subsequent methodological refinement in the broader earnings-management literature.

This accrual-based tradition, while foundational to the earnings-quality literature as a whole, is not well suited to the empirical setting of this thesis, for a reason already implicit in Section 2.3 below: the Jones (1991) model and its descendants are calibrated to the accrual structure of non-financial firms, inventories, receivables, and depreciable property, plant, and equipment, none of which is a first-order item on a bank's balance sheet. Applying a Jones-type model directly to banks would therefore risk misclassifying genuinely non-discretionary variation in bank accruals as discretionary, or vice versa, simply because the model's benchmark was never designed for a financial institution's accrual structure. For this reason, and consistent with the approach taken by the bank-specific accounting literature reviewed next, this thesis measures accounting quality directly through bank-specific balance sheet and income statement items, non-performing loans, the loan loss provision, reserve coverage, and capital adequacy, rather than through a generic, cross-industry discretionary accrual estimate. This choice is a direct methodological implication of the literature reviewed in Section 2.3, to which the chapter now turns.

2.2.2 Consequences of Earnings Quality: Why Deal Completion May Be Affected

A separate part of the review by Dechow, Ge, and Schrand (2010) addresses not the determinants of earnings quality but its documented economic consequences, and this discussion is directly relevant to the causal mechanism this thesis proposes between accounting quality and deal completion. Dechow, Ge, and Schrand (2010) summarize evidence that lower earnings quality is associated with a higher cost of both debt and equity capital, reflecting the fact that outside capital providers, unable to fully verify the reliability of reported earnings, demand compensation for the additional estimation risk they bear, an application, in the corporate financing context, of the same adverse-selection logic that Section 2.4 applies to the M&A context specifically. They further note that lower earnings quality is associated with less efficient capital allocation and with a higher likelihood of costly ex post outcomes such as financial restatement and regulatory enforcement, once the market's initial, imperfect assessment of a firm's reported earnings is subsequently revised considering new information.

Applied to the M&A setting of this thesis, this consequences-side literature suggests a specific, testable channel through which target accounting quality could affect deal completion, distinct from, though complementary to, the pure information-asymmetry channel developed in Section 2.4. If lower accounting quality is associated with a higher cost of capital and a higher likelihood of subsequent negative revelations, as Dechow, Ge, and Schrand (2010) document in other settings, then

an acquirer conducting due diligence on a target with weak accounting quality faces not only greater difficulty verifying the target's current condition, but a heightened risk that new, negative information will surface between announcement and closing, precisely the kind of intervening event that both Officer (2003) and Bates and Lemmon (2003) identify as a proximate cause of deal renegotiation or withdrawal in their termination-fee samples. Sections 2.3 and 2.5 develop, respectively, the bank-specific version of the determinants side of this literature and the deal-completion evidence this consequences-side mechanism connects to.

2.3 Accounting Discretion in Banking: The Loan Loss Provision

Banks occupy a distinctive position within the earnings-quality literature because one of the largest and most consequential accounting estimates on their financial statements, the allowance for loan losses, and the corresponding periodic provision expense, is, by construction, forward-looking and highly discretionary. Unlike most accruals in non-financial firms, which relate to transactions that have already occurred, the loan loss provision requires bank management to estimate losses that have not yet been realized, based on private information about individual borrowers' creditworthiness that is not observable to outside parties, including auditors, regulators, and, critically for this thesis, prospective acquirers.

Beaver and Engel (1996) provide early evidence that the market prices the discretionary and non-discretionary components of loan loss allowances differently, implying that investors are, at least to some degree, able to distinguish provisioning that reflects a bank's underlying credit risk from provisioning that reflects managerial discretion, but also implying that the discretionary component itself carries information content that the market must infer rather than observe directly. Liu, Ryan, and Wahlen (1997) extend this line of enquiry by showing that the valuation implications of loan loss provisions differ systematically across banks and across fiscal quarters, depending on whether the provision appears to be signaling changes in credit quality or smoothing reported earnings, an important precursor to the joint-test approach later adopted by Kanagaretnam, Lobo, and Yang (2004), discussed below.

A parallel stream of research asks not merely whether the loan loss provision contains discretion, but what specifically motivates bank managers to exercise it. Ahmed, Takeda, and Thomas (1999) exploit a 1990 change in U.S. bank capital adequacy regulation to sharpen the identification of two competing motives, capital management, whereby provisions are used to manage regulatory capital ratios, and

earnings management, whereby provisions are used to smooth reported income, and find strong evidence in favour of the capital management channel, a finding directly relevant to this thesis's inclusion of the Tier 1 capital ratio and the equity-to-assets ratio as target-bank variables, since these variables are precisely the regulatory metrics that the capital management channel would predict to be systematically related to provisioning behavior.

The most direct antecedent to the empirical design of this thesis is Kanagaretnam, Lobo, and Yang (2004), who develop joint tests of two motives for discretionary loan loss provisioning: signaling, whereby undervalued banks use provisions to convey private information about future prospects to the market, and income smoothing, whereby banks use provisions to reduce the volatility of reported earnings around a target level. Kanagaretnam, Lobo, and Yang (2004) find that relative undervaluation is a significant driver of signaling-motivated provisioning, while deviation of pre-managed earnings from historical norms drives smoothing-motivated provisioning, and that these two motives can coexist within the same institution depending on its specific circumstances at a given point in time. For the purposes of this thesis, the central implication of this literature is that the loan loss provision reported by a target bank in the period preceding an M&A announcement is not a purely mechanical reflection of credit losses, but rather an accounting choice shaped by managerial incentives that an acquirer, during due diligence, cannot fully and costlessly unwind.

Beatty and Liao (2014), in a comprehensive review of the empirical bank accounting literature published in the years following the 2007–2009 financial crisis, organize this body of research around three themes: the relation between bank financial reporting and outside investors' valuation and risk assessments; the relation between financial reporting discretion, regulatory capital, and earnings management, of the type documented by Ahmed, Takeda, and Thomas (1999) and Kanagaretnam, Lobo, and Yang (2004); and banks' economic decisions under differing accounting and regulatory regimes. Beatty and Liao (2014) explicitly identify bank opacity, the degree to which outside parties cannot verify a bank's true asset quality from its published financial statements, as a first-order, unresolved issue in the literature, directly motivating the information-asymmetry framework developed in Section 2.4 of this thesis.

Because this thesis draws its sample from both the United States and Europe, it is also important to establish that the discretionary use of loan loss provisions is not a U.S.-specific phenomenon but has been documented internationally under differing accounting and regulatory regimes. Leventis, Dimitropoulos, and Anandarajan (2011) study a sample of European Union commercial banks reporting under IFRS and find evidence consistent with both earnings and capital management

through loan loss provisions, while Hasan and Wall (2004) provide cross-country comparisons of the determinants of loan loss allowances and document that, while the general relation between provisioning and observable credit-risk fundamentals holds across countries, the strength of that relation and, by implication, the residual scope for managerial discretion, varies with national accounting and supervisory regimes. This evidence directly motivates the cross-sample robustness test between the United States and Europe developed in H2 of this thesis: if the mechanism linking accounting quality to deal outcomes operates through a channel, discretionary provisioning, whose strength itself varies across regulatory regimes, the relationship documented in the full sample need not hold with equal strength in both sub-samples.

2.3.1 Critiques and Identification Challenges in the LLP Literature

The literature reviewed above should not be read as having reached full consensus on how to isolate the discretionary component of loan loss provisions, and it is important to be explicit about this for a thesis that relies on the reported values of provisions and related asset-quality variables directly, rather than attempting to decompose them into discretionary and non-discretionary parts. Beatty and Liao (2014) note that studies attempting to estimate discretionary loan loss provisions face an identification problem analogous to, but in some respects more severe than, the one facing the general accrual-based earnings-management literature discussed in Section 2.2.1: the non-discretionary, credit-risk-driven component of the provision is itself estimated using an econometric model, and any misspecification of that model, for example, omitting a relevant credit-risk determinant, or assuming a linear relation where the true relation is non-linear, will bias the residual, and therefore the resulting discretionary provision estimate, in ways that are difficult to detect from the data alone.

This thesis sidesteps this specific identification problem by design, at the cost of a corresponding limitation that is made explicit here. Rather than estimating a discretionary loan loss provision model of the type surveyed by Beatty and Liao (2014), which would require a longer historical panel per bank than the single pre-announcement fiscal year this thesis's data collection strategy provides, for the reasons discussed in Section 1.3, the empirical analysis in Chapter 5 uses the levels of non-performing loans, the loan loss provision, and the reserve ratio directly, interpreting higher and less conservative values of these variables, relative to the target's size and peer group, as indicative of weaker accounting quality. This is a coarser proxy than a fully specified discretionary-provision model would provide, and it does not distinguish, at the level of an individual observation, between a bank with genuinely poor underlying credit quality and a bank with good underlying credit quality

but aggressive, discretion-driven under provisioning. Both cases, however, are consistent with the mechanism this thesis tests: both leave an acquirer facing greater valuation uncertainty than it would face for a target with strong, conservatively provisioned asset quality. This point is revisited explicitly in the discussion of limitations in Chapter 7.

2.4 Information Asymmetry and the Market for Corporate Control

The theoretical foundation connecting accounting discretion to transaction outcomes is Akerlof's (1970) analysis of markets characterized by asymmetric information between buyers and sellers. As discussed in Section 1.3, Akerlof's central insight is that when sellers possess private information about the quality of the good being sold that buyers cannot costlessly verify, buyers rationally discount the price they are willing to pay to protect themselves against the risk of purchasing a low-quality good, a "lemon", and this discounting can, in equilibrium, drive high-quality sellers out of the market entirely. While Akerlof's original analysis was developed using the market for used cars, the framework has since been applied extensively to corporate finance settings in which one party to a transaction, typically the seller of a firm, or the firm itself, possesses superior information about its own value.

Within the M&A literature specifically, Officer, Poulsen, and Stegemoller (2009) provide direct evidence consistent with an Akerlof-type mechanism operating in acquisitions. They show that acquirer stock returns are significantly higher when the payment method is stock, rather than cash, in acquisitions of targets that are difficult to value, as proxied by R&D intensity and idiosyncratic stock return volatility, and interpret this finding through the lens of risk-sharing: when a target's true value is highly uncertain, exchanging acquirer stock for target ownership allows both sets of shareholders to share the risk that the acquirer has mispriced an opaque target, mitigating the adverse-selection problem that a pure cash offer would otherwise leave entirely with the acquirer. Officer, Poulsen, and Stegemoller (2009) further note that this effect is particularly pronounced, and more clearly identified, in transactions involving privately held targets, precisely because private targets are subject to less external monitoring and therefore exhibit weaker corroborating information than publicly traded ones, a consideration directly relevant to this thesis's own sample, given the large number of privately held or thinly covered target banks noted in Section 1.3.

Banking provides an unusually clean setting in which to extend this line of research, for a reason already established in Section 2.3: the primary source of a bank's information advantage over an

outside acquirer is concentrated in a specific, well-defined, and heavily studied accounting item, the loan loss provision and the related allowance for loan losses, rather than being diffused across many idiosyncratic and hard-to-observe assets, as is typically the case for non-financial firms with substantial intangible assets or R&D. This concentration allows the information-asymmetry channel proposed by Akerlof (1970) and documented empirically by Officer, Poulsen, and Stegemoller (2009) to be operationalized directly, using the same accounting variables, non-performing loans, the loan loss provision, reserve coverage, and capital adequacy, that the bank accounting literature reviewed in Section 2.3 has already identified as the locus of managerial discretion.

2.4.1 Bank-Specific Financial Statement Variables Used in This Thesis

The literature reviewed above, together with the general bank M&A literature discussed in Section 2.6, motivates the specific set of target-level accounting variables collected for this thesis and described in full in Chapter 4. It is useful to state explicitly, at this point, how each variable connects back to the literature just reviewed, since this connection is what allows the variable-description table required by the thesis supervisor (Chapter 4, Table 1) to cite a literature source for every variable rather than treating the choice of variables as an arbitrary list of items available from Bloomberg.

Total assets serves as the standard proxy for bank size in this thesis, consistent with its use throughout the bank M&A and bank consolidation literature (Berger, Demsetz, and Strahan, 1999; DeYoung, Evanoff, and Molyneux, 2009) as a control for the scale-related motives, cost efficiency, diversification, and the pursuit of systemic scale, that this literature identifies as first-order drivers of bank merger activity, independent of any accounting-quality channel. Financial leverage and the equity-to-assets ratio jointly capture a target's capital structure and capitalization, variables whose relation to loan loss provisioning behavior is documented directly by Ahmed, Takeda, and Thomas (1999) under the capital management hypothesis, and whose regulatory counterpart, the Tier 1 capital ratio, is the metric bank supervisors themselves use to assess capital adequacy, a channel through which weak capitalization could plausibly affect deal completion independently of, or in addition to, the accounting-discretion channel that is the central focus of this thesis.

Non-performing loans, the loan loss provision, and the reserve for loan losses relative to total loans jointly operationalize the accounting-discretion channel developed in Section 2.3: these are precisely the variables through which Beaver and Engel (1996), Liu, Ryan, and Wahlen (1997), Ahmed, Takeda, and Thomas (1999), and Kanagaretnam, Lobo, and Yang (2004) identify and measure discretionary

behavior in bank financial statements, and their inclusion in this thesis's variable set is a direct empirical implementation of that literature, rather than an independent methodological choice. Total deposits, net interest income, and total revenue capture the scale of a target's core deposit-taking and lending activity, the primary business of a commercial bank, and serve, alongside total assets, as controls for the possibility that any observed relation between accounting-quality variables and deal completion is confounded by the underlying scale or business mix of the target, rather than reflecting accounting discretion specifically. Finally, return on equity and net income capture target profitability, a variable with a dual role in this literature: it is both a standard control for target financial health, and a variable directly implicated in the signaling channel documented by Kanagaretnam, Lobo, and Yang (2004), whereby banks with relative undervaluation, which, all else equal, is more likely to be observed among lower-profitability targets, have a heightened incentive to use loan loss provisions to signal private information to the market.

2.4.2 Payment Method as a Response to Information Asymmetry

A further implication of the Officer, Poulsen, and Stegemoller (2009) framework, directly relevant to the empirical design of this thesis, concerns the choice of payment method, cash versus stock, or a mixture of the two, as an equilibrium response to the degree of information asymmetry surrounding a target. As summarized in Section 2.4, Officer, Poulsen, and Stegemoller (2009) show that acquirer stock returns are significantly higher when hard-to-value targets are acquired using stock rather than cash, and interpret stock consideration as a risk-sharing mechanism: because target shareholders who receive acquirer stock retain exposure to the combined firm's future performance, they bear part of the cost if the target turns out to have been overvalued at the time of acquisition, partially aligning the incentives of the two parties under uncertainty in a way that a pure cash payment does not.

This mechanism gives rise to a direct empirical prediction that this thesis's dataset is well placed to test as an extension to the baseline model: if weaker target accounting quality increases valuation uncertainty in the manner proposed by H1, acquirers facing weaker-accounting-quality targets should be more likely to structure the transaction using stock, or a mixed cash-and-stock consideration, rather than an all-cash offer, consistent with the risk-sharing logic of Officer, Poulsen, and Stegemoller (2009). Payment type is recorded for every deal in the sample used in this thesis (Chapter 4), which allows this prediction to be examined either as a robustness check on the main H1 specification, controlling for payment type as an additional deal-level covariate, on the logic that payment type may itself be an equilibrium response to the same accounting-quality variables that H1 relates directly to

deal completion or, time permitting, as a short standalone extension in Chapter 6 relating target accounting-quality variables to the choice of payment method directly, rather than to deal completion. The baseline specification in Chapter 5 takes the more conservative of these two approaches, including payment type as a control variable, and leaves the standalone extension as a candidate for the additional sensitivity analysis referenced in Section 1.3 depending on time and data availability.

2.5 Determinants of M&A Deal Completion

A separate literature examines directly why some announced M&A transactions are completed while others are renegotiated, delayed, or ultimately withdrawn, the dependent variable of central interest in this thesis. This literature has developed largely around the study of contractual devices designed to mitigate the risk of deal breakdown, most prominently termination fee provisions, which require one party, typically the target, to pay a pre-specified sum to the other if the transaction fails to close under specified conditions.

Officer (2003) provides evidence that mergers including a target termination fee are associated with significantly higher completion rates and higher deal premiums than mergers without such provisions, and interprets this finding as more consistent with termination fees serving as an efficient contracting device, compensating the acquirer for the cost of revealing valuable private information during negotiations, and thereby encouraging bidder participation, than with the competing hypothesis that termination fees are used opportunistically by target managers to protect favoured ("sweetheart") deals from competing bids. Bates and Lemmon (2003) reach a similar conclusion using an independent sample, likewise finding that the presence and structure of termination fee provisions is systematically related to the probability that an announced deal is subsequently completed, consistent with these provisions functioning as a risk-allocation mechanism between transacting parties operating under incomplete information about how the transaction, and the environment surrounding it, will evolve between announcement and closing.

This literature establishes an important point of departure for the present thesis: the existing evidence on deal completion is built almost entirely around deal-level contractual features, the presence, size, and structure of termination fees, rather than around target-level characteristics that might independently explain why a transaction carries a greater or lesser risk of breakdown in the first place. Termination fees are, in a sense, a symptom of anticipated deal risk: parties negotiate more robust contractual protections precisely when they perceive greater uncertainty about whether the transaction

will close. This thesis complements the termination-fee literature by asking a logically prior question, whether an observable characteristic of the target itself, namely its accounting quality, is systematically related to the same outcome (deal completion) that termination fees are designed to protect against, independent of whatever contractual provisions the parties may or may not have negotiated.

A related methodological point, relevant to the empirical strategy adopted in Chapter 5, concerns how deal completion is defined and measured. Both Officer (2003) and Bates and Lemmon (2003) treat deal completion as a binary outcome, completed versus not completed, estimated using discrete-choice models (typically probit or logit) that relate the probability of completion to deal- and firm-level covariates, an approach this thesis follows directly given that Bloomberg's own deal-status classification (Completed, Withdrawn, Terminated) is itself categorical rather than continuous. An important caveat, noted by both studies and equally relevant here, is that the observed sample of announced deals is not a random sample of all potential transactions: deals are announced only after some degree of prior negotiation and mutual agreement between the parties, meaning that any completion model estimated on announced deals implicitly conditions on a first-stage selection process, a firm's decision to announce a deal in the first place, that is not itself modeled. This thesis does not attempt to correct for this first-stage selection explicitly, consistent with the approach taken in the termination-fee literature it builds on, but the limitation is acknowledged here and returned to in the discussion of limitations in Chapter 7.

2.5.1 Alternative Explanations for Deal Non-Completion

Before turning to the banking-specific literature, it is important to acknowledge explicitly that target accounting quality is unlikely to be the only, or even the primary, determinant of whether an announced deal is completed, and that a careful empirical test of H1 must account for the most plausible competing explanations documented elsewhere in the literature this chapter has reviewed. Three alternative explanations are particularly relevant to the banking M&A setting of this thesis, and each has a corresponding control variable or design feature in the empirical strategy developed in Chapter 4.

The first alternative explanation is competition among bidders. A deal may fail to complete not because of any characteristic of the original target, but because a competing acquirer emerges with a superior offer, prompting the original bidder to walk away rather than raise its own bid. This channel

is deal-specific rather than target-specific, and while this thesis's dataset does not record whether a given transaction attracted competing bids, deal value and announcement-to-completion timing, both recorded in the sample, serve as partial proxies, since deals that attract competing interest tend to be larger and more protracted, consistent with the broader M&A literature on bidder competition. The second alternative explanation is regulatory intervention independent of target financial condition, for instance, antitrust concerns arising from market concentration in a specific local banking market, which are a function of the combined footprint of acquirer and target rather than of the target's accounting quality per se. Because this channel operates primarily through market overlap rather than target financial condition, it is not expected to be systematically correlated with the accounting-quality variables central to H1, though Chapter 7 returns to this point as a limitation, since the dataset does not allow this channel to be tested directly.

The third, and most important, alternative explanation to rule out is that any observed relation between target accounting quality and deal completion could instead reflect a broader dimension of target financial distress unrelated to accounting discretion specifically, that is, target banks with weak accounting quality may simply also be, on average, worse-performing or higher-risk banks along dimensions unrelated to earnings quality, and it may be this general distress, rather than accounting quality as such, that deters deal completion. The variable set described in Section 2.3.1 and detailed fully in Chapter 4 is designed explicitly to address this concern: by including target size, leverage, profitability, and business-mix variables alongside the accounting-quality variables central to H1, the baseline specification in Chapter 5 is able to assess whether the accounting-quality variables carry incremental explanatory power for deal completion, net of these more general measures of target financial condition, rather than merely proxying for target distress in general.

2.6 Banking M&A: Motives, Patterns, and Cross-Country Differences

Bank M&A activity has been a persistent and well-studied feature of the financial services industry since at least the early 1990s. Berger, Demsetz, and Strahan (1999), in a review covering over 250 studies, evaluate the causes and consequences of financial services industry consolidation within a value-maximization framework, and document evidence consistent with consolidation generating increases in market power in certain segments, improvements in profit efficiency and risk diversification on average, but little or no improvement in average cost efficiency, a mixed picture that has motivated substantial subsequent research into which specific deal and institutional

characteristics are associated with successful, value-creating combinations, as opposed to those that create no value or fail to complete at all.

DeYoung, Evanoff, and Molyneux (2009) update this picture with a review of the post-2000 bank M&A literature, covering over 150 studies, and identify several robust patterns: North American bank mergers can be efficiency-improving, although the associated evidence on shareholder wealth creation is mixed; European bank mergers appear, on the evidence they survey, to have produced both efficiency gains and shareholder value enhancement somewhat more consistently than their North American counterparts; and merger activity is robustly linked to CEO compensation incentives and to the pursuit of scale sufficient to be perceived as systemically important. Directly relevant to the design of this thesis, DeYoung, Evanoff, and Molyneux (2009) note that the great majority of the post-2000 empirical literature on bank M&A has been conducted on North American samples, with European evidence comparatively less abundant, a gap that motivates both the explicit inclusion of European targets in this thesis's sample and the sub-sample robustness test specified in H2.

This asymmetry in the existing literature, heavily weighted toward United States evidence, mirrors a concern that arises directly in the construction of the sample used in this thesis, discussed in Section 1.3: because Bloomberg's fundamental data coverage is itself more complete for United States banks than for smaller European institutions, any pooled empirical analysis of a global banking M&A sample risks being disproportionately influenced by United States observations, a possibility DeYoung, Evanoff, and Molyneux (2009) implicitly caution against when noting the comparative scarcity of European evidence in the literature to date. Testing the central hypothesis of this thesis separately within the United States and European sub-samples, as specified in H2, is therefore not merely a generic robustness exercise, but a direct response to a specific, previously documented imbalance in the literature this thesis seeks to extend.

Beyond the volume of available evidence, the United States and European banking markets also differ in the structure of the supervisory approval process that any announced bank merger must clear before it can be completed, a difference with a direct bearing on the mechanism this thesis tests. In the United States, bank mergers require approval from the relevant federal banking regulator (the Federal Reserve, the Office of the Comptroller of the Currency, or the Federal Deposit Insurance Corporation, depending on the charter type of the institutions involved), which evaluates, among other factors, the financial condition and prospects of the combined institution. In the European Union, bank mergers are subject to a comparable, though institutionally distinct, approval process, generally involving the European Central Bank's Single Supervisory Mechanism for significant institutions together with the

relevant national competent authority, alongside separate competition-law clearance. Chapter 3 discusses this institutional architecture, and its specific approval criteria, in full; the point relevant to this chapter is narrower: in both jurisdictions, supervisory approval is conditioned in part on an assessment of the target's financial soundness, meaning that weak target accounting quality could, in principle, affect deal completion not only through the private due-diligence channel emphasized by Officer, Poulsen, and Stegemoller (2009) and developed in Section 2.4, but also through a public, regulatory channel operating in parallel. This thesis's empirical design, based on observed deal outcomes rather than on the internal reasoning of either acquirers or supervisors, cannot separately identify which of these two channels, private due diligence or public supervisory scrutiny, drives any relationship it finds, a limitation made explicit here and revisited in Chapter 7.

2.6.1 Cross-Border Banking M&A and Information Frictions

A further strand of the bank M&A literature bears directly on the information-asymmetry mechanism at the core of this thesis: the study of cross-border, as opposed to domestic, bank mergers. Focarelli and Pozzolo (2001), using a dataset of almost 2,500 banks across 29 OECD countries, document that cross-border mergers and acquisitions are systematically rarer in banking than in other sectors of the economy, and attribute this pattern explicitly to the importance of information asymmetries in banking relationships, together with regulatory restrictions on foreign entry. Banks with cross-border shareholdings, they find, tend to be larger, more profitable, and based in countries with more developed banking markets, characteristics broadly consistent with an acquirer's ability to bear, or a target's ability to mitigate, the additional information costs that cross-border due diligence entails relative to a purely domestic transaction.

This evidence is doubly relevant to the present thesis. First, it provides independent, industry-level corroboration, obtained through a completely different empirical approach, using aggregate cross-border shareholding patterns rather than target-level accounting variables, of the same underlying mechanism this thesis tests directly: that information asymmetry is a first-order friction in banking M&A specifically, and not merely a generic feature of M&A transactions in any industry. Second, it has a direct implication for the empirical design of Chapter 4: because Focarelli and Pozzolo (2001) show that information frictions are already elevated in cross-border banking transactions relative to domestic ones, a target's accounting quality may plausibly interact with whether a given deal is domestic or cross-border. A target with weak accounting quality may pose a particularly acute information problem for a foreign acquirer, who lacks the local market knowledge that might

otherwise substitute, in part, for the missing accounting information available to a domestic acquirer. Chapter 4 therefore records, for every deal in the sample, whether the acquirer and target are headquartered in the same country, allowing this possibility to be explored as an extension to the baseline specification if the data permits.

2.7 Synthesis and Hypothesis Development

The literature reviewed in this chapter establishes three connected propositions. First, earnings quality is a real, economically meaningful, if imperfectly observable, construct, and the banking industry offers an unusually well-identified setting in which to measure it, because managerial discretion over asset-quality recognition is concentrated in a specific, heavily studied accounting item, the loan loss provision, rather than diffused across many hard-to-observe assets (Section 2.3). Second, information asymmetry of the type documented in this accounting-discretion literature has well-established consequences for transaction design and outcomes in the market for corporate control, operating through the adverse-selection mechanism first formalized by Akerlof (1970) and documented empirically in the M&A setting by Officer, Poulsen, and Stegemoller (2009) (Section 2.4). Third, while the determinants of M&A deal completion have been studied extensively at the contractual level, principally through the termination-fee literature of Officer (2003) and Bates and Lemmon (2003) and the determinants and patterns of bank M&A activity have been studied extensively at the industry level (Berger, Demsetz, and Strahan, 1999; DeYoung, Evanoff, and Molyneux, 2009), the specific link between a target bank's accounting quality and the completion status of its acquisition has not, to the author's knowledge, been directly tested (Section 2.5).

These three propositions motivate the formal hypotheses first stated in Section 1.3, restated here with their full theoretical grounding:

H1: Target banks with weaker accounting quality are less likely to complete their announced M&A deals.

H2: The relationship proposed in H1 holds in sign and is statistically significant when the model is estimated separately on the United States and European sub-samples.

2.7.1 Expected Signs and Testable Predictions

Translating H1 into a variable-by-variable set of predictions, to be tested formally in Chapter 5, the literature reviewed in this chapter motivates the following expected relationships between each target-level accounting variable and the probability of deal completion, holding the other variables and deal-level controls fixed. Non-performing loans and the loan loss provision are predicted to be negatively related to deal completion: higher values of either variable, particularly when large relative to a target's size and peer group, are consistent with weaker asset quality and a greater scope for the discretionary under-recognition of credit losses documented by Beaver and Engel (1996) and Kanagaretnam, Lobo, and Yang (2004), and therefore with the adverse-selection mechanism of Akerlof (1970) operating more strongly. The reserve for loan losses relative to total loans, conversely, is predicted to be positively related to deal completion, since a higher reserve ratio, holding non-performing loans fixed, indicates more conservative provisioning against known credit risk, the opposite of the discretionary under-provisioning behavior this thesis's mechanism predicts to deter deal completion.

Financial leverage and the Tier 1 capital ratio are both predicted to relate to deal completion through the capital management channel identified by Ahmed, Takeda, and Thomas (1999): higher leverage (equivalently, a lower equity-to-assets ratio) and a lower Tier 1 ratio are predicted to be negatively related to deal completion, reflecting both a weaker capital buffer against unexpected losses and, per the capital management hypothesis, a greater incentive for the target to have used provisioning discretion to manage a binding regulatory capital constraint. Return on equity and net income are predicted to be positively related to deal completion as general indicators of target financial health, though Section 2.3 notes that this relation may be non-monotonic at the margin, since Kanagaretnam, Lobo, and Yang (2004) document that it is specifically relative undervaluation, not low profitability per se, that drives signaling-motivated provisioning. Total assets, total deposits, net interest income, and total revenue, the scale variables discussed in Section 2.3.1, are not predicted to have a directional sign under H1 itself, since they proxy for target size and business mix rather than accounting quality; they are included as controls precisely so that any estimated relation between the accounting-quality variables above and deal completion can be interpreted net of target size, consistent with their role in the wider bank M&A literature (Berger, Demsetz, and Strahan, 1999; DeYoung, Evanoff, and Molyneux, 2009).

2.8 Chapter Summary

This chapter has reviewed the literature on earnings quality and its measurement, the specific and extensive literature on discretionary loan loss provisioning in banks, the theory of information asymmetry and its documented consequences for transaction design in the market for corporate control, the literature on the contractual determinants of M&A deal completion, and the literature on banking M&A activity and its cross-country patterns. Taken together, this literature identifies a clear and, to the author's knowledge, previously untested link between a well-established accounting-quality construct, the discretionary loan loss provision and related asset-quality metrics, and a well-established transaction outcome, M&A deal completion, in a setting, banking, where both constructs are unusually well measured.

Three points from this review are carried forward directly into the remaining chapters of the thesis. First, the choice of accounting-quality variables in Chapter 4: non-performing loans, the loan loss provision, and the reserve for loan losses relative to total loans, alongside leverage, capital, and profitability measures, is not an arbitrary selection from the fields available on Bloomberg, but a direct empirical implementation of the specific mechanisms identified in Sections 2.3 and 2.3.1. Second, the identification strategy adopted in Chapter 5, using variable levels rather than an estimated discretionary-provision residual, is a deliberate methodological choice made explicitly in Section 2.3.2, with a corresponding limitation that is acknowledged rather than concealed. Third, the sub-sample structure of the robustness analysis in Chapter 6, United States versus Europe, is motivated by specific, previously documented evidence that the strength of the provisioning-discretion channel varies across regulatory regimes (Hasan and Wall, 2004; Leventis, Dimitropoulos, and Anandarajan, 2011) and that the existing bank M&A literature is itself geographically imbalanced (DeYoung, Evanoff, and Molyneux, 2009), rather than being included as a generic robustness exercise for its own sake. Chapter 3 turns to the institutional and regulatory context within which this relationship is tested, before Chapter 4 presents the sample construction and methodology in full.

3. Institutional and Regulatory Context

3.1 Introduction to the Chapter

Chapter 2 established that supervisory approval is, alongside private due diligence, a second channel through which target accounting quality could plausibly affect deal completion, since banking regulators in both the United States and Europe explicitly assess the financial condition of merging institutions before authorizing a transaction. This chapter describes that institutional and regulatory background in enough detail to support the interpretation of results in Chapters 5 and 6, without attempting a treatise-length legal analysis of either jurisdiction. Section 3.2 describes the U.S. bank merger approval framework, including the parallel antitrust review conducted by the Department of Justice and the Basel III capital adequacy baseline against which target capitalization is assessed. Section 3.3 describes the corresponding European framework, centered on the European Central Bank's Single Supervisory Mechanism (SSM), together with the parallel EU competition-law review and the related, but distinct, bank resolution framework. Section 3.4 compares the two regimes directly and draws out the implications for this thesis's cross-country empirical design. Section 3.5 places both frameworks in historical context, tracing their development since the 2008 financial crisis, which is directly relevant given that this thesis's sample begins in fiscal year 2016. Section 3.6 summarizes the chapter.

3.2 The U.S. Bank Merger Approval Framework

Bank mergers in the United States are governed by the Bank Merger Act of 1960 (Public Law 86-463), signed by President Eisenhower on May 13, 1960, which requires that no insured bank may merge with another insured bank without the prior written consent of the appropriate federal banking agency, the Office of the Comptroller of the Currency (OCC) for national banks, the Board of Governors of the Federal Reserve System for state member banks and bank holding companies, or the Federal Deposit Insurance Corporation (FDIC) for state non-member banks, depending on the charter type of the resulting institution. Under the statute, the relevant agency must consider the financial history and condition of each of the merging banks, their capital adequacy, their future earnings prospects, the character of their management, the convenience and needs of the community to be served, and the effect of the transaction on competition, before determining whether the merger

is in the public interest. In practice, the agency responsible for the resulting institution also requests a report on the competitive effects of the transaction from the other federal banking agencies and from the Attorney General, whose Department of Justice separately evaluates the transaction under antitrust law.

Two elements of this framework are directly relevant to the empirical design of this thesis. First, capital adequacy and financial condition are explicit, statutory approval criteria, not merely factors an acquirer might voluntarily consider during private due diligence, meaning that a target bank with weak accounting quality, to the extent this is reflected in weaker measured capital adequacy or financial condition, faces a public regulatory hurdle in addition to whatever private valuation concerns an acquirer may have, consistent with the dual private/public channel discussed in Section 2.6. Second, and consistent with the FDIC's and OCC's own published merger-review procedures, an applicant that anticipates an unfavourable resolution of one of the statutory factors is frequently permitted to withdraw its application before a public denial is issued, rather than proceeding to a formal rejection. This institutional detail matters for how this thesis's dependent variable should be interpreted: a deal recorded by Bloomberg as "withdrawn," rather than as a formal regulatory denial, may in some cases reflect this kind of anticipated-denial withdrawal precisely, meaning that supervisory concerns about target financial condition, including accounting quality, can manifest empirically as a withdrawal rather than as an explicit rejection, without that distinction being visible in deal-level databases such as the one used in this thesis.

The U.S. regulatory framework has also been subject to recent revision. In September 2024, the OCC and FDIC each issued updated policy statements clarifying how they weigh the statutory factors under the Bank Merger Act when reviewing applications, and the OCC eliminated its previously available streamlined and expedited review procedures for qualifying transactions, while the Department of Justice announced that bank mergers would thereafter be evaluated under its generally applicable 2023 Merger Guidelines rather than the sector-specific 1995 Bank Merger Guidelines. Because this thesis's sample spans fiscal years 2016 to 2025, with deal announcement dates extending into 2026, the sample includes transactions reviewed under both the pre-2024 and the revised post-2024 procedural regime; this institutional shift is noted here as a potential source of regime-related heterogeneity in U.S. deal completion rates over the sample period, distinct from the accounting-quality channel that is the central focus of this thesis, and is revisited as a limitation in Chapter 7.

3.2.1 Antitrust Review in the United States: The Department of Justice

The prudential approval process described above operates alongside, and is procedurally distinct from, a separate antitrust review conducted by the Department of Justice (DOJ). As noted in Section 3.2, the Bank Merger Act requires the primary federal banking regulator to solicit a competitive-factors report from the Attorney General before acting on a merger application; the Department of Justice has historically evaluated these reports against sector-specific 1995 Bank Merger Guidelines, but announced in September 2024 that it would thereafter apply its generally applicable 2023 Merger Guidelines to the banking industry, supplemented by a short Banking Addendum identifying which portions of the general guidelines it considers most frequently relevant to bank mergers specifically. This shift is significant for the interpretation of deal outcomes in this thesis's later sample years, because the general 2023 Merger Guidelines apply a broader, more structurally oriented standard of competitive harm than the narrower, deposit-concentration-based screens that had characterized bank-specific antitrust review historically, potentially altering the antitrust risk profile of transactions announced after the policy change relative to earlier, otherwise comparable transactions in the sample.

For this thesis, the key point is that antitrust review constitutes a third possible channel of deal non-completion, alongside the prudential-approval channel and the private due-diligence channel developed in Chapter 2, and one that, unlike the other two, is driven primarily by the degree of market overlap between acquirer and target rather than by target accounting quality as such. A well-specified empirical test of H1 must therefore be interpreted with the possibility in mind that some non-completions in the U.S. sub-sample reflect antitrust concerns unrelated to accounting quality. Chapter 4 records deal value and target and acquirer geography for every observation, which provide partial, if imperfect, proxies for the degree of market overlap that antitrust review would scrutinize, given that the sample does not include a direct measure of local deposit-market concentration for each transaction.

3.2.2 Prudential Capital Requirements: The Basel III Framework

Both the U.S. and European approval frameworks assess target and combined-entity capital adequacy against a common international baseline: the Basel III framework developed by the Basel Committee on Banking Supervision (BCBS) following the 2008 financial crisis and first published in December 2010. Basel III requires internationally active banks to maintain Common Equity Tier 1 (CET1)

capital of at least 4.5 percent of risk-weighted assets, total Tier 1 capital of at least 6 percent, and total regulatory capital of at least 8 percent, with additional capital conservation and, for the largest institutions, systemic-importance buffers that raise effective requirements further above these baseline minimums. Basel III is not itself directly binding law; rather, it is implemented into domestic law separately by each jurisdiction, in the United States, principally through the interagency capital rules issued by the Federal Reserve, OCC, and FDIC, and in the European Union, principally through the Capital Requirements Directive and Capital Requirements Regulation referenced in Section 3.3.

The Tier 1 capital ratio collected as a target-level variable in this thesis (Chapter 4) is measured directly against this Basel III baseline, giving the variable a precise, internationally comparable regulatory meaning rather than being merely a descriptive financial ratio. In the United States, a bank whose capital ratios fall below the relevant Basel-implementing minimums is classified as undercapitalized under the Prompt Corrective Action framework codified at 12 CFR 208.43, triggering mandatory supervisory restrictions that include, directly relevant to this thesis, a restriction on the bank's ability to undertake new acquisitions without regulatory approval of a capital restoration plan. This provides a second, independent piece of institutional grounding, alongside the capital management evidence of Ahmed, Takeda, and Thomas (1999) discussed in Section 2.3, for treating the Tier 1 capital ratio as a variable directly relevant to deal completion: a target bank near or below the Basel III / Prompt Corrective Action thresholds is, by regulatory design, in a weaker position to be an acquisition target (or, in some cases, an acquirer) than one comfortably above them, independent of any accounting-discretion channel.

3.3 The European Bank Merger Approval Framework

Within the euro area, the supervisory approval of bank mergers is centered on the European Central Bank's Single Supervisory Mechanism (SSM), established under Council Regulation (EU) No 1024/2013. The SSM divides supervised banks into Significant Institutions (SIs), which the ECB supervises directly, and Less Significant Institutions (LSIs), which remain under the direct supervision of the relevant national competent authority (NCA), subject to the ECB's indirect oversight. A bank qualifies as significant if it meets at least one of several criteria, most notably total assets exceeding €30 billion, or a ratio of assets to national GDP exceeding 20 percent (unless total assets are below €5 billion), or if it is among the three largest credit institutions in its participating

member state or has requested or received direct public financial assistance from the European Stability Mechanism.

Under Article 4 of Regulation 1024/2013 and Article 24 of the Capital Requirements Directive (Directive 2013/36/EU), the ECB is responsible for assessing proposed acquisitions of "qualifying holdings" broadly, the direct or indirect acquisition of at least 10 percent of a credit institution's capital or voting rights, or any other means of exercising significant influence over its management in significant institutions, in cooperation with the relevant national competent authorities, which serve as the first point of contact for applications and carry out an initial assessment before forwarding them to the ECB. The ECB's Guide on the supervisory approach to consolidation in the banking sector further specifies that, for mergers and acquisitions among SSM-supervised entities, the ECB's assessment considers whether the resulting entity will remain able to comply with prudential capital requirements following the transaction, whether the merger's integration plan is feasible and sound from a prudential perspective, and whether the transaction increases the risk of money laundering or terrorist financing. A qualifying holding application must, by regulation, be authorized or formally objected to within 60 working days of a complete application being received, extendable by up to 30 additional working days in specified circumstances, giving a maximum statutory review window of 90 working days a materially more time-bound process, at least on paper, than the U.S. framework described in Section 3.2, which does not specify a comparable fixed statutory deadline for the primary federal regulator's decision.

As in the United States, prudential capital adequacy is an explicit approval criterion under the European framework, meaning that a target bank's capitalization and, by extension, any accounting discretion that has been used to manage measured capital ratios the capital management channel documented by Ahmed, Takeda, and Thomas (1999) and reviewed in Section 2.3, is directly relevant to whether the ECB's prudential assessment of the post-merger entity is favourable. Because Less Significant Institutions fall outside the ECB's direct assessment and remain subject to national, rather than supranational, approval criteria, and because approval criteria and procedural timelines can vary somewhat across the national competent authorities responsible for LSIs, the European sub-sample used in this thesis is not subject to a fully homogeneous regulatory process even within Europe, a further institutional source of heterogeneity, distinct from the U.S.-versus-Europe distinction itself, that is acknowledged here and returned to in the limitations discussion of Chapter 7.

3.3.1 EU Competition Law: The Merger Regulation

Parallel to the prudential SSM approval process described above, and structurally analogous to the U.S. Department of Justice antitrust review discussed in Section 3.2.1, bank mergers in the European Union that meet specified turnover thresholds are separately subject to competition-law review by the European Commission under the EU Merger Regulation (Council Regulation (EC) No 139/2004, the "EUMR"). The Commission has exclusive jurisdiction over concentrations that meet the EUMR's turnover-based "EU dimension" thresholds; transactions below these thresholds may instead fall under the merger control rules of individual member states, subject to a referral mechanism that allows cases to be moved between the Commission and national competition authorities in either direction under specified conditions. Where the Commission does have jurisdiction, an initial ("Phase I") review is followed, in cases raising sufficiently serious competitive concerns, by an in-depth Phase II investigation lasting 90 working days, extendable to 125 working days in certain circumstances, during which the Commission assesses whether the transaction would significantly impede effective competition in the internal market, in particular through the creation or strengthening of a dominant position.

This dual-track structure, prudential approval under the SSM (or the relevant NCA) running alongside, but procedurally separate from, competition approval under the EUMR (or national competition law), directly mirrors the U.S. structure of prudential approval by the primary federal banking regulator alongside antitrust review by the DOJ, discussed in Section 3.2.1. This parallel is useful for the interpretation of H2 in Chapter 6: both jurisdictions require an announced bank merger to clear two procedurally distinct hurdles, one assessing prudential soundness, where target accounting quality is directly relevant, and one assessing competitive effects, where it is not, meaning that the basic institutional architecture within which the accounting-quality channel of H1 operates is broadly comparable across the two sub-samples, even though, as Section 3.4 discusses, the specific agencies, statutory deadlines, and procedural details differ.

3.3.2 Resolution Regimes: A Related but Distinct Framework

A further related institutional feature, distinct from the merger approval frameworks discussed above, but occasionally relevant to how a weak target bank's situation is ultimately resolved, is the bank resolution regime available to supervisors when a bank is failing or likely to fail outside the context

of a negotiated, voluntary acquisition. In the United States, resolution authority for large, complex financial institutions was strengthened by the Dodd-Frank Act following the 2008 crisis, building on the FDIC's long-standing role as receiver for failed insured depository institutions. In the European Union, the equivalent framework is the Bank Recovery and Resolution Directive (BRRD, Directive 2014/59/EU) and, within the banking union, the Single Resolution Mechanism (SRM), whose central authority, the Single Resolution Board (SRB), became fully operational in 2016 and is explicitly modeled, in part, on the pre-existing FDIC framework. Under both regimes, a bank determined to be failing, or likely to fail, can be resolved, restructured, sold, or wound down, with losses allocated to shareholders and certain creditors through a "bail-in" mechanism, through a distinct legal process rather than through the ordinary, voluntary M&A approval channels described in Sections 3.2 and 3.3.

This distinction matters for the interpretation of this thesis's sample, and is noted here rather than developed further, since the resolution framework itself falls outside the scope of the voluntary, announced M&A transactions this thesis studies. It is nonetheless conceptually important: a target bank with sufficiently weak accounting quality and underlying financial condition that it approaches the threshold of being deemed "failing or likely to fail" may, in principle, exit the sample of voluntary announced deals entirely, being resolved through the BRRD/SRB or FDIC receivership framework instead of, or as an alternative to, being acquired through a negotiated transaction of the kind recorded in this thesis's Bloomberg-sourced dataset. To the extent this occurs, it introduces a form of sample selection at the weakest end of the target accounting-quality distribution, the very weakest targets may never appear as an announced, voluntary M&A deal at all, which would, if anything, work against finding a strong relationship under H1 in this thesis's sample, since the most severe cases of weak accounting quality are structurally less likely to be observed as ordinary announced transactions in the first place. This possibility is revisited in the discussion of sample limitations in Chapter 7.

3.4 Comparing the Two Regimes: Implications for This Thesis

Three points of comparison between the U.S. and European frameworks described above are directly relevant to the interpretation of the cross-sample robustness test (H2) specified in Chapter 2. First, both regimes make capital adequacy and financial condition explicit, statutory or regulatory approval criteria, providing institutional support for this thesis's central premise that target accounting quality, to the extent it affects measured capital and asset-quality metrics, can plausibly affect deal completion

through a public supervisory channel, not only through the private due-diligence channel emphasized by Officer, Poulsen, and Stegemoller (2009) and reviewed in Section 2.4. This supports the plausibility of H1 in both sub-samples, rather than only in one.

Second, the two regimes differ in their institutional structure in ways that could moderate the strength of this channel. The European framework, at least for Significant Institutions, operates under a single supranational supervisor with a codified statutory review timeline, whereas the U.S. framework distributes primary responsibility across three separate federal agencies depending on charter type, with an additional, separately timed antitrust review by the Department of Justice, and without a comparable fixed statutory decision deadline. If a more fragmented, less time-bound approval process is associated with greater scope for a deal to be renegotiated or to lapse for reasons unrelated to target accounting quality specifically, for instance, due to protracted inter-agency coordination, the strength of the relationship proposed in H1 could differ between the two sub-samples for institutional reasons that have nothing to do with the underlying accounting-quality mechanism this thesis tests. This possibility is a further, institutionally grounded reason, alongside the accounting-regime evidence of Hasan and Wall (2004) and Leventis, Dimitropoulos, and Anandarajan (2011) already discussed in Section 2.3, to test H1 separately in each sub-sample under H2, rather than assuming *ex ante* that any effect found in the pooled sample generalizes symmetrically across the two regulatory environments.

Third, the European framework's Significant/Less Significant Institution distinction has a direct sample-composition implication: a material share of the European targets in this thesis's dataset, given their smaller average size relative to the €30 billion asset threshold noted in Section 3.3, likely fall under Less Significant Institution status and are therefore approved at the national, rather than the ECB, level. This thesis's European sub-sample is consequently better described as being subject to a family of related, EU-law-derived national approval regimes operating under the same broad prudential framework, rather than a single, fully uniform supervisory process, a nuance that qualifies, without undermining, the clean U.S.-versus-Europe institutional comparison drawn in this section, and one that Chapter 7 revisits explicitly when discussing the limits of what the H2 sub-sample test can and cannot establish.

3.4.1 A Fourth Point: Dual-Track Approval and Timeline Considerations

A fourth point of comparison follows directly from Sections 3.2.1 and 3.3.1: both jurisdictions require an announced bank merger to clear two procedurally separate approval tracks, prudential and

competition-based, rather than a single unified process. This dual-track structure has a direct empirical implication for how deal duration, the time elapsed between announcement and completion (or withdrawal), should be interpreted in this thesis's sample. Because prudential approval and competition approval typically proceed on separate, only loosely coordinated timelines within each jurisdiction, a longer observed deal duration need not, by itself, indicate that a transaction faced a target-accounting-quality problem specifically; it could equally reflect the additional time consumed by a routine but procedurally separate competition review, particularly for larger transactions more likely to raise market-overlap concerns. This thesis's dataset, described in Chapter 4, records the deal value associated with each transaction, which correlates with the likelihood of triggering a more intensive Phase II (EU) or extended DOJ (U.S.) competition review; controlling for deal value in the baseline specification of Chapter 5 therefore serves the dual purpose already noted in Section 2.3.1, controlling for target scale and additionally, partially absorbing this dual-track timeline effect, to the extent it is correlated with transaction size.

A related, more specific timeline observation concerns the comparison between the European qualifying-holding review window, a statutory maximum of 90 working days under Regulation 1024/2013, as described in Section 3.3 and the EU competition-law Phase II window, which can itself extend to 125 working days under the EUMR, as described in Section 3.3.1. Because these two review clocks run in parallel rather than being formally synchronized, the effective minimum time to regulatory clearance for a European bank merger that triggers both a Phase II competition review and a full prudential qualifying-holding assessment is bounded below by whichever of the two processes takes longer, not by their sum, but is also not guaranteed to conclude in lockstep. No directly comparable, single, codified maximum timeline exists on the U.S. side for the combined prudential-plus-antitrust process described in Section 3.2, since the Bank Merger Act itself, unlike the SSM Regulation, does not specify a fixed statutory decision deadline for the primary federal banking regulator. This asymmetry in procedural time-boundedness between the two jurisdictions is noted here as a further, purely institutional source of potential heterogeneity in deal duration between the U.S. and European sub-samples, distinct from and not to be confused with the accounting-quality-driven heterogeneity in deal completion that is the substantive focus of H1 and H2.

3.4.2 Deposit Insurance and Depositor Confidence in the Approval Process

A final institutional feature relevant to this thesis's variable set concerns deposit insurance, since total deposits are one of the target-level variables collected in Chapter 4 as a measure of core banking-

activity scale. In the United States, the FDIC provides standard deposit insurance coverage of \$250,000 per depositor, per insured bank, for each recognized account ownership category, a limit last raised from \$100,000 to its current level in 2010 as part of the Dodd-Frank Act reforms discussed in Section 3.5. In the European Union, Directive 2014/49/EU harmonizes deposit guarantee coverage across member states at €100,000 per depositor per bank, a level reached in two steps, first to €50,000 in mid-2009 and then to €100,000 by the end of 2010, in direct response to the deposit-flight episodes observed during the 2008 crisis; the United Kingdom, outside the EU framework since Brexit, maintains a broadly comparable but separately set limit of £85,000 under its own Financial Services Compensation Scheme.

Deposit insurance is relevant to the bank merger approval process discussed in Sections 3.2 and 3.3 in a specific way: both the U.S. Bank Merger Act's "convenience and needs of the community" factor and the ECB's prudential assessment of a proposed combination are, in part, concerned with whether the merger preserves depositor confidence and continuity of access to insured deposits, particularly for a target bank whose weaker accounting quality might, in the absence of the transaction, have raised doubts about its standalone viability. A merger that moves a weak target's deposit base under the umbrella of a stronger, better-capitalized acquirer can be, and often is, viewed favourably by supervisors on exactly this dimension, even where the same weak accounting quality that motivates supervisory concern also, per H1, makes the transaction less likely to be completed on commercial terms acceptable to the acquirer. This tension, between a supervisory preference for resolving weak-bank situations through acquisition where possible, and an acquirer's commercial reluctance to complete a deal with a target of uncertain accounting quality, is not directly testable with this thesis's data, but is noted here as a further reason why the net relationship between target accounting quality and deal completion predicted under H1 should be understood as an empirical, not a purely mechanical, question: the public-interest and private-commercial incentives at play do not necessarily point in the same direction.

3.5 Historical Evolution: Bank M&A Regulation Since the 2008 Financial Crisis

Because this thesis's sample begins in fiscal year 2016, meaning deals announced from 2017 onward, per the t-1 measurement convention described in Section 1.3, it is useful to place both regulatory frameworks described above in their historical context, since neither reached its current form until several years after the 2008 financial crisis, and elements of both continued to evolve throughout the

sample period itself. The 2008 crisis prompted a broadly parallel policy response on both sides of the Atlantic, centered on two related objectives: strengthening bank capital requirements *ex ante*, and building credible resolution tools to end the "too big to fail" problem *ex post*, so that a large bank's failure could be managed without a repeat of the extraordinary, *ad hoc* public interventions that characterized the crisis itself.

On the capital side, this response produced the Basel III framework discussed in Section 3.2.2, initially published by the Basel Committee in December 2010 and phased in gradually across participating jurisdictions over the following decade, with certain elements of the framework's finalization not fully phased in until as late as 2027 in some jurisdictions' implementation timelines. On the resolution side, the response produced the Dodd-Frank Act in the United States, enacted in 2010, and, with a discernible lag, the EU's Bank Recovery and Resolution Directive, whose legislative development began around 2009-10 but which was not enacted until May 2014, with its central implementing authority, the Single Resolution Board, becoming fully operational only in January 2016, almost exactly at the start of the fiscal-year window covered by this thesis's sample. The European Central Bank's own Single Supervisory Mechanism, the direct-supervision framework described in Section 3.3, likewise did not commence operations until November 2014, meaning that the entire European institutional architecture this thesis relies on for its regulatory-context discussion was still comparatively new, relative to the far longer-standing U.S. framework dating to 1960, at the point where this thesis's sample period begins.

This historical sequencing has two implications for the interpretation of results in Chapters 5 and 6. First, because the European supervisory and resolution architecture was still being operationalized in the early years of the sample period, the Single Resolution Board's own initial performance in this period has been described in independent assessments as still developing its full operational capacity, any accounting-quality-to-deal-completion relationship estimated on the earliest European observations in the sample may be less precisely measured, or subject to somewhat different institutional conditions, than the same relationship estimated on more recent European observations, where the SSM and SRM frameworks have had a decade or more to mature. Second, and more generally, this reinforces the case, already made on separate grounds in Sections 2.3, 2.6, and 3.4, for treating year effects as an explicit control in the empirical specification of Chapter 5, rather than assuming that the institutional environment governing deal completion was static across the full 2016-2025 fiscal-year window covered by the sample.

3.5.1 Sample Period Coverage of Major Regulatory Milestones

To make the historical discussion above directly usable when interpreting the empirical results of Chapters 5 and 6, it is useful to map the major regulatory milestones described in this chapter onto the fiscal-year window covered by this thesis's sample. The sample's target-level accounting variables are measured at fiscal year-end for fiscal years 2016 through 2025 (Section 1.3), corresponding to deal announcements from 2017 through 2026. The Single Supervisory Mechanism became operational in November 2014, and the Single Resolution Board assumed full authority in January 2016, meaning the entire European sub-sample falls within the SSM/SRM era, with no pre-SSM European observations against which to benchmark. The EU's harmonized €100,000 deposit guarantee level, discussed in Section 3.4.2, was likewise already fully in place, having been reached by the end of 2010. On the U.S. side, the Dodd-Frank Act and the associated \$250,000 FDIC deposit insurance limit had been in force since 2010, again predating the entire sample window; the one material U.S. regulatory shift internal to the sample period is the September 2024 OCC/FDIC/DOJ policy revision discussed in Section 3.2.1, which affects only the final one to two fiscal years of announcements in the sample (deals announced in late 2024 through 2026).

Basel III itself presents a more gradual picture: the framework's core minimum ratios (Section 3.2.2) were fully phased in by 2019 across most major jurisdictions, but certain elements of its final calibration, the output floor and related revisions agreed by the Basel Committee in December 2017, were not required to be fully phased in until as late as 2027 in some implementing jurisdictions' timelines, meaning the entire sample period falls within a period of continued, gradual tightening of the effective Basel III capital baseline against which the Tier 1 capital ratio variable in this thesis is implicitly benchmarked. Taken together, this timeline confirms that the two sub-samples' regulatory environments are not fully static across the sample window, but that the great majority of the relevant institutional architecture, the SSM, the SRM, harmonized deposit insurance, and the core Basel III minimums, was already fully in place before the sample begins, leaving year effects to absorb primarily the residual, more gradual regulatory drift documented in this section, together with ordinary macroeconomic and credit-cycle variation over the ten fiscal years the sample spans.

3.6 Chapter Summary

This chapter has described the statutory and regulatory framework governing bank merger approval

in the United States, centered on the Bank Merger Act of 1960, the tripartite OCC/Federal Reserve/FDIC prudential approval structure, and the parallel antitrust review conducted by the Department of Justice, and in Europe, centered on the ECB's Single Supervisory Mechanism, its qualifying-holding assessment procedure under Regulation 1024/2013, and the parallel competition-law review conducted by the European Commission under the EU Merger Regulation. Both frameworks make target capital adequacy and financial condition explicit approval criteria, assessed in both jurisdictions against the common Basel III capital baseline, providing institutional grounding for this thesis's central hypothesis that accounting quality can affect deal completion through a public, supervisory channel in addition to the private due-diligence channel developed in Chapter 2. Both frameworks also operate a related, but analytically distinct, bank resolution regime, Dodd-Frank/FDIC receivership in the United States and the BRRD/Single Resolution Board in Europe and a harmonized deposit insurance scheme, which together bear on how supervisors weigh depositor protection against target accounting-quality concerns when evaluating a proposed merger.

At the same time, the two frameworks differ in institutional structure, statutory timelines, and internal homogeneity, a single supranational supervisor versus three separate federal agencies, a codified 90-working-day review window versus no comparable fixed deadline, and a European sub-sample that is itself institutionally heterogeneous between Significant and Less Significant Institutions, in ways that motivate testing the central hypothesis separately in each sub-sample under H2, rather than assuming the U.S. and European results must coincide. The historical review in Section 3.5 further shows that the European supervisory architecture was still comparatively new at the start of this thesis's sample period, reinforcing the case for treating year effects as an explicit control in the empirical specification that follows.

3.6.1 Implications for Variable Construction in Chapter 4

Three specific implications of this chapter carry forward directly into the variable construction described in Chapter 4. First, because both jurisdictions benchmark capital adequacy against the same Basel III minimums described in Section 3.2.2, the Tier 1 capital ratio and equity-to-assets ratio collected for both U.S. and European targets are directly comparable across the two sub-samples, despite the differences in the surrounding approval architecture, an important precondition for the pooled and sub-sample regressions estimated in Chapters 5 and 6 to be meaningfully compared. Second, because deal value is shown in Section 3.4.1 to plausibly proxy for the intensity of the parallel competition-law review in both jurisdictions, it is retained as a standard control variable in the

baseline specification, consistent with its role as a size control already motivated on separate grounds in Section 2.3.1. Third, because Section 3.5 shows that the institutional environment for European deals shifted materially around 2014-2016, while the U.S. environment shifted materially around 2024, Chapter 5's empirical specification includes year (or year-group) fixed effects for both sub-samples, allowing the estimated H1 relationship to be interpreted net of these largely mechanical, institutionally driven shifts in approval procedure, rather than having them confounded with the accounting-quality channel that is the substantive object of this thesis.

Chapter 4 now turns from this qualitative institutional background to the quantitative construction of the sample and variables used to test H1 and H2 directly.

4. Data and Methodology

4.1 Introduction to the Chapter

This chapter describes the construction of the sample and variables used to test H1 and H2, and previews the empirical model estimated in Chapter 5. Section 4.2 describes the sample construction process, from the initial Bloomberg deal universe to the final 392-deal working sample. Section 4.3 discusses the dependent variable, deal status. Section 4.4 presents the variable-description table (Table 1) required by the thesis supervisor. Section 4.5 presents the sample's distribution by year, region, and deal outcome (Table 2), together with three summary figures. Section 4.6 describes the log, ratio, and winsorization transformations applied to the raw variables ahead of the regression analysis. Section 4.7 presents summary statistics for the transformed variable set, for the full sample and the United States and European sub-samples separately (Table 3). Section 4.8 previews the empirical model specification carried forward into Chapter 5, and Section 4.9 summarizes the chapter.

4.2 Sample Construction

The sample was constructed from Bloomberg's deal-screening functionality, which was used to identify all announced mergers and acquisitions with a banking-sector target headquartered in the United States or in Western Europe (defined as the EU-27, the United Kingdom, Switzerland, and Norway), announced between January 1, 2017, and August 2026. This initial screen returned 2,740

deals. For each deal, target-level accounting variables were then extracted from Bloomberg for the fiscal year immediately preceding the announcement date (t-1), using point-in-time fundamental data requests rather than the most recently available fiscal year at the time of data collection, consistent with the measurement convention discussed in Section 1.3.

Two screening steps were applied to this initial universe before arriving at the final working sample. First, deals for which the target's Bloomberg security identifier could not be resolved, or for which fewer than six of the eight core accounting variables (Total Assets, Financial Leverage, Non-Performing Loans, the Reserve for Loan Losses / Total Loans ratio, Loan Loss Provisions, Total Deposits, Net Interest Income, and Total Revenue) were available, were excluded. This data-completeness screen reflects a known limitation of Bloomberg's fundamental data coverage, discussed in Section 1.3: smaller, privately held, or thinly covered target banks, disproportionately common among European targets and among United States community banks, are less likely to have complete fundamental data available. Second, an additional set of target-level variables, Total Equity, Return on Equity, the Tier 1 Capital Ratio, Equity/Total Assets, and Net Income, was collected for the deals surviving the first screen to support the accounting-quality proxies developed in Chapter 2.

The resulting working sample comprises 392 deals. All monetary variables are expressed in a common currency (US dollars); for targets headquartered outside the United States, local-currency Bloomberg figures were converted to USD using annual average exchange rates for the relevant fiscal year, obtained from the Federal Reserve's H.10/G.5A release and from the U.S. Internal Revenue Service's published yearly average exchange rates. Four deals, involving Icelandic targets, could not be converted due to the unavailability of a reliable annual average ISK/USD rate for the relevant fiscal years, and are flagged accordingly in the working dataset; these four observations are retained in the descriptive analysis of this chapter but are excluded from any regression specification in Chapter 5 that includes monetary (level) variables, since their inclusion would introduce a currency-comparability error rather than genuine cross-sectional variation.

4.2.1 Data Extraction Methodology

Target-level accounting variables were extracted using Bloomberg's BDP ("Bloomberg Data Point") function within the Excel Add-in, rather than the BDH ("Bloomberg Data History") function, for reasons specific to this thesis's t-1 measurement convention. BDH is designed to return a time series of values over a specified date range and is best suited to applications requiring the full historical

trajectory of a variable. BDP, by contrast, returns a single point-in-time value for security and field, subject to whatever overrides are supplied, and is both computationally lighter and more directly suited to extracting exactly one fiscal-year observation per target, as this thesis requires. Each variable was extracted using an override structure that fixed the fiscal year to the target's announcement year minus one (EQY_FUND_YEAR) and the reporting frequency to annual (FUND_PER=Y), ensuring that every observation in the sample reflects the same, precisely defined point in each target's reporting history, rather than the most recently available fiscal year at the time of extraction, which would have varied mechanically with the date on which the data was pulled rather than with the deal's own timeline.

Data extraction proceeded in stages, consistent with standard practice from Bloomberg samples: an initial deal universe was identified through Bloomberg's deal-screening tool, target securities were resolved from Bloomberg's ticker-based identifiers (rather than target company names, which do not reliably resolve to a unique Bloomberg security), and accounting variables were then extracted in batches, with each batch checked for completeness and internal consistency, including a check for two accounting fields (the loan loss provision and the reserve-for-loan-losses ratio) whose Bloomberg-provided labels were found, in an earlier iteration of the extraction, to have been transposed relative to their correct mnemonics, and were corrected before finalizing the working sample used in this thesis.

4.3 The Dependent Variable: Deal Status

The dependent variable in this thesis is the completion status of each announced deal, as classified by Bloomberg into one of three categories: Completed, Withdrawn, or Terminated. Consistent with the approach taken by Officer (2003) and Bates and Lemmon (2003), discussed in Section 2.5, the baseline empirical specification in Chapter 5 collapses this three-category outcome into a binary indicator equal to one if the deal was Completed and zero otherwise (Withdrawn or Terminated), estimated using a probit or logit model. This collapsing is a deliberate simplification: Bloomberg's Withdrawn and Terminated categories are not always applied with a fully consistent distinction across deal types and jurisdictions in the underlying data, and the number of Terminated observations in the working sample (16, discussed in Section 4.5) is too small to support a separate multinomial specification with adequate statistical power. Chapter 6 revisits this simplification directly, re-estimating the baseline specification with Withdrawn and Terminated deals excluded entirely (a

stricter definition of non-completion) as a robustness check, and discusses whether the results are sensitive to how the two non-completion categories are treated.

4.3.1 Timing of Deal Status Classification

A further methodological detail concerns the point in time at which a deal's status is observed. Because deal status is a Bloomberg field that updates as a transaction progresses, a deal announced several years ago and still shown as "pending" at the time of an early data pull might subsequently be reclassified as Completed, Withdrawn, or Terminated by the time of a later pull. The working sample used in this thesis reflects deal status as of the final data extraction date (August 2026), rather than status as of a fixed number of months after each deal's announcement. This introduces a mechanical asymmetry: deals announced very close to the data collection cutoff (the small number of 2026 observations visible in Figure 4.1) have had less time to reach a final resolution than deals announced in 2017, and a small number of these very recent deals may still be genuinely pending at the time of writing, rather than having a settled outcome that has simply not yet been recorded. Deals whose Bloomberg status remained "pending" as of the extraction date were excluded from the working sample entirely during the data-completeness screening described in Section 4.2, since a pending deal has not yet resolved to either of the two outcomes (completion or non-completion) that the dependent variable is designed to capture; this exclusion is noted here as a further, minor source of the gap between the initial 2,740-deal universe and the 392-deal working sample, alongside the accounting-data completeness screen that is the primary driver of that gap.

4.4 Variable Description (Table 1)

The dependent variable is Deal Status, a binary indicator equal to one if the deal was completed and zero otherwise, as described in Section 4.3. The main independent variables are the accounting quality proxies: $\ln(\text{Non-Performing Loans})$, $\text{Reserve for Loan Losses} / \text{Total Loans}$, and $\text{Loan Loss Provisions}$, motivated by the earnings-quality literature reviewed in Chapter 2. The remaining variables, $\ln(\text{Total Assets})$, Financial Leverage, ROE, Tier 1 Capital Ratio, $\text{Equity} / \text{Total Assets}$, $\ln(\text{Deal Value})$, and Payment Type, serve as control variables, capturing target size, capital structure, profitability, and deal characteristics.

Variable	Definition	Bloomberg Field / Transformation	Literature Source
ln(Total Assets)	Natural log of total book assets, target FY t-1	BS_TOT_ASSET, log-transformed	Berger, Demsetz & Strahan (1999); DeYoung, Evanoff & Molyneux (2009)
Financial Leverage	Total assets / total equity, target FY t-1 (x)	FNCL_LVRG	Ahmed, Takeda & Thomas (1999)
ln(Non-Performing Loans)	Natural log of non-performing loans, target FY t-1	BS_NON_PERFORM_LOANS, log-transformed	Beaver & Engel (1996); Kanagaretnam, Lobo & Yang (2004)
Reserve for Loan Losses / Total Loans	Loan loss reserve as % of gross loans, target FY t-1	RSRV_FOR_LOAN_LOSS_TO_TOT_LOAN	Liu, Ryan & Wahlen (1997); Hasan & Wall (2004)
Loan Loss Provisions	Period provision expense for loan losses, target FY t-1, USD mm	IS_PROV_FOR_LOAN_LOSS	Ahmed, Takeda & Thomas (1999); Kanagaretnam, Lobo & Yang (2004)
Deposits / Total Assets	Customer deposits as % of total assets, target FY t-1	BS_CUSTOMER_DEPOSITS / BS_TOT_ASSET, author's calculation	Berger, Demsetz & Strahan (1999)
ln(Total Revenue)	Natural log of total operating revenue, target FY t-1	NET_REV, log-transformed	DeYoung, Evanoff & Molyneux (2009)
ROE	Net income / average shareholders' equity, target FY t-1 (%)	RETURN_COM_EQY	Kanagaretnam, Lobo & Yang (2004)
Tier 1 Capital Ratio	Tier 1 regulatory capital / risk-weighted assets, target FY t-1 (%)	BS_TIER1_CAP_RATIO	Basel Committee on Banking Supervision (2010); Ahmed, Takeda & Thomas (1999)
Equity / Total Assets	Book equity as % of total assets, target FY t-1	Author's calculation (Equity/Assets)	Ahmed, Takeda & Thomas (1999)
ln(Deal Value)	Natural log of announced total transaction value, USD mm	Bloomberg deal screening, log-transformed	Officer (2003); Bates & Lemmon (2003)
Deal Status (dependent variable)	Completed / Withdrawn / Terminated, as of data collection date	Bloomberg deal screening	Officer (2003); Bates & Lemmon (2003)
Target / Acquirer Country	Country of headquarters of target/acquirer	Bloomberg deal screening	DeYoung, Evanoff & Molyneux (2009); Focarelli & Pozzolo (2001)
Payment Type	Cash / Stock / Cash and Stock / Undisclosed	Bloomberg deal screening	Officer, Poulsen & Stegemoller (2009)

Table 1 — Variable Description

Note: All Bloomberg fields are measured at the target fiscal year-end immediately preceding the deal announcement date (t-1). Monetary variables are converted to USD as described in Section 4.2. The literature source column lists the primary paper(s) motivating the variable's inclusion, as developed in Chapter 2. Full citations appear in the References section.

4.5 Sample Distribution (Table 2)

Table 2 reports the distribution of the 392-deal sample by announcement year and region. The sample is weighted toward United States targets (246 deals, 62.8%) relative to European targets (146 deals, 37.2%), a composition discussed in Section 1.3 as a direct consequence of Bloomberg's fuller fundamental-data coverage of larger, more established U.S. institutions relative to smaller European ones, and the motivation for the United States versus Europe robustness test specified in H2.

Announcement Year	United States	Europe	Total
2017	37	21	58
2018	31	13	44
2019	35	16	51
2020	12	16	28
2021	43	15	58
2022	16	15	31
2023	12	7	19
2024	24	20	44
2025	32	21	53
2026	4	2	6
Total	246	146	392

Table 2a — Sample Distribution by Announcement Year and Region

Note: 2026 reflects deals announced through August 2026, the data collection cutoff, and is therefore a partial year.

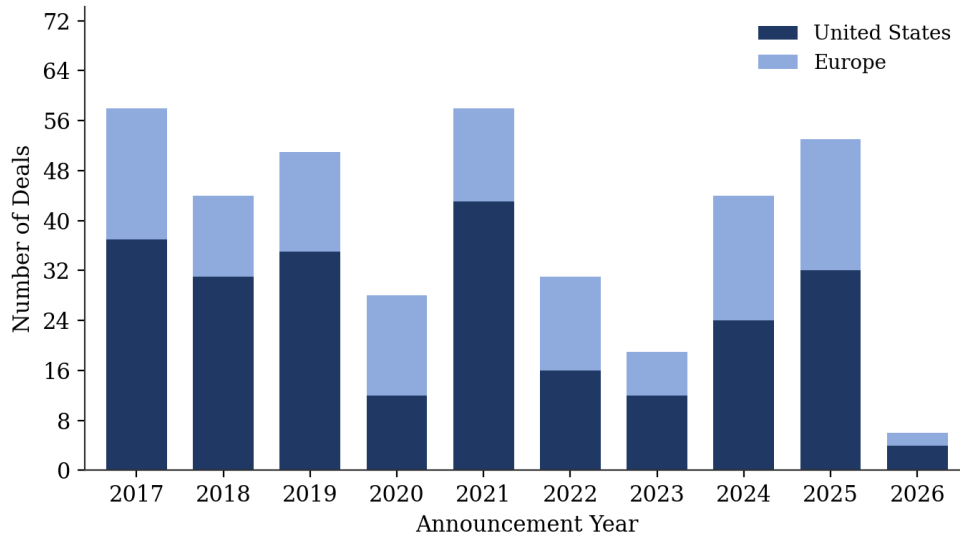


Figure 4.1 — Sample Distribution by Announcement Year and Region

The year-by-year pattern in Figure 4.1 shows two visible troughs, in 2020 and 2023, and two visible peaks, in 2017 and 2021. The 2020 trough is consistent with the broader disruption to global M&A activity during the COVID-19 pandemic; a full account of the macroeconomic drivers of deal volume is outside the scope of this thesis, but the pattern reinforces the case, developed in Sections 3.5 and 3.5.1, for including year fixed effects in the baseline empirical specification, so that the estimated relationship between target accounting quality and deal completion is not confounded with these broader cyclical swings in overall deal activity.

Table 2b reports deal status by region and reveals a pattern directly relevant to H2. The completion rate is substantially higher in the United States sub-sample (94.7%) than in the European sub-sample (69.9%), with both the withdrawal rate (23.3% versus 2.8%) and the termination rate (6.8% versus 2.4%) markedly higher in Europe. This descriptive pattern does not, by itself, establish anything about the accounting-quality mechanism central to H1, it may equally reflect the institutional differences discussed in Chapter 3, sample composition differences between the two regions, or factors entirely unrelated to either, but it is a first indication that pooling the United States and European sub-samples without allowing for region-specific effects would likely be inappropriate, reinforcing the empirical strategy, specified in Section 4.8 below, of estimating the baseline model both on the pooled sample and separately by region.

Region	Completed	Withdrawn	Terminated	Total
United States	233 (94.7%)	7 (2.8%)	6 (2.4%)	246
Europe	102 (69.9%)	34 (23.3%)	10 (6.8%)	146
Full Sample	335 (85.5%)	41 (10.5%)	16 (4.1%)	392

Table 2b — Deal Status by Region

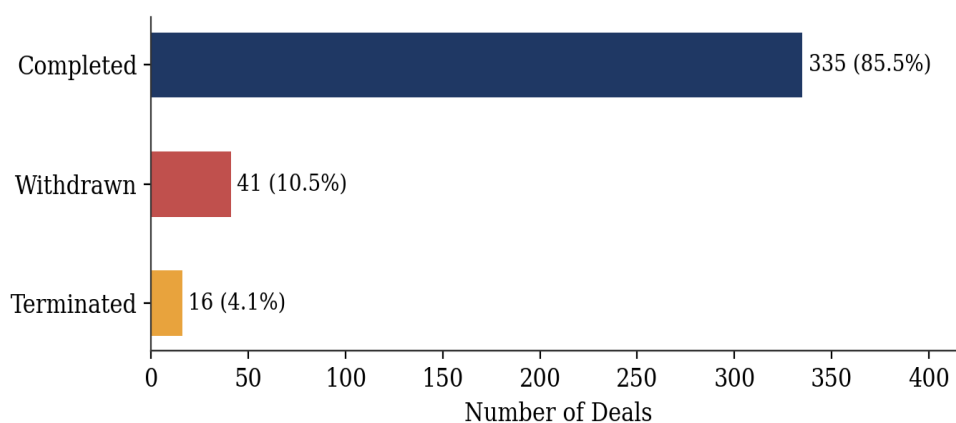


Figure 4.2 — Deal Status Composition (Full Sample, n = 392)

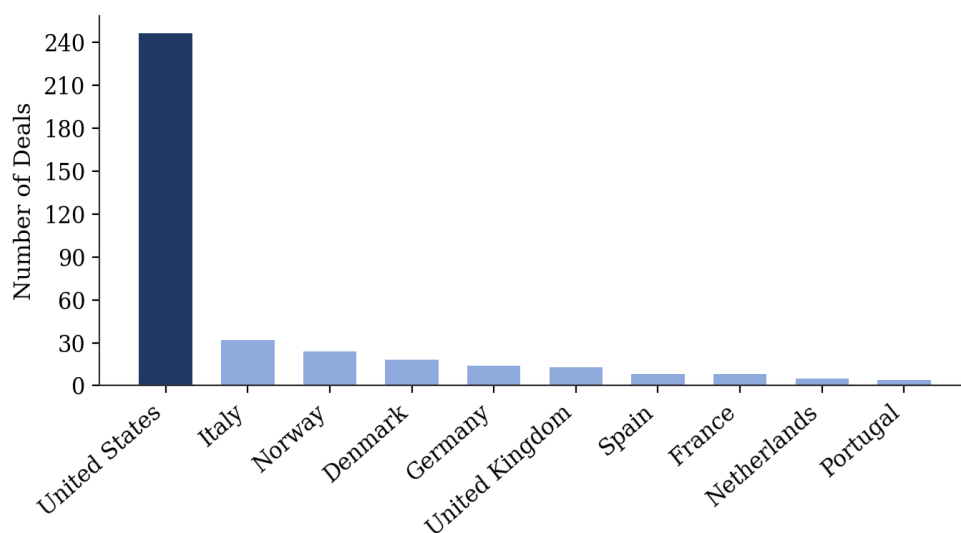


Figure 4.3 — Target Country Distribution (Top 10 of 20 Countries Represented)

Figure 4.3 shows that, beyond the United States, the largest single contributors to the European sub-sample are Italy (32 deals), Norway (24 deals), and Denmark (18 deals), together accounting for just under half of all European observations; the remaining European deals are spread across a further

seventeen countries, most contributing fewer than ten deals each. This concentration is noted here as a further sample-composition detail relevant to the interpretation of the European sub-sample results in Chapter 6: the European estimate is not evenly weighted across countries, and results should not be read as equally representative of every European banking market.

4.6 Variable Transformation: Logs, Ratios, and Winsorization

Before summary statistics are reported, three transformations are applied to the raw Bloomberg variables described in Section 4.4, following standard practice in the empirical accounting and finance literature and consistent with the specific guidance received from the thesis supervisor. First, all variables expressed in absolute monetary terms that span several orders of magnitude across the sample, Total Assets, Total Revenue, Non-Performing Loans, and Deal Value, are converted to natural logarithms. Raw monetary levels of this kind are heavily right-skewed by construction, since bank size in the sample ranges from small community and savings banks to institutions with well over a trillion dollars in assets (Table 3a); estimating a regression directly on unlogged levels would let a small number of very large institutions dominate the estimation, whereas the log transformation compresses this range and is standard for size-related variables in the banking literature (Berger, Demsetz, and Strahan, 1999).

Second, two variables are converted from absolute levels to ratios to make them comparable across targets of very different sizes. Total Deposits are expressed as Deposits / Total Assets rather than in USD millions, capturing a target's reliance on deposit funding independent of its overall scale. Total Equity, Net Interest Income, and Net Income are dropped entirely from the variable set used in the empirical analysis: Total Equity is retained only in ratio form, through the Tier 1 Capital Ratio and the Equity/Total Assets ratio already present in Table 1, rather than also being included as a separate absolute-level variable; Net Interest Income and Net Income are excluded as separate control variables because their information content is judged to be substantially captured by Return on Equity, which is itself a function of net income relative to equity, so that including both would risk introducing near-mechanical collinearity into the regression without adding independent explanatory content. Non-Performing Loans could not be converted to a ratio of total loans, since Bloomberg's Reserve for Loan Losses / Total Loans field is itself already a pre-calculated ratio that does not expose the underlying total loans figure separately; Non-Performing Loans is therefore retained as a log-

transformed level variable, following the same logic applied to the other absolute monetary variables, rather than as a ratio.

Third, every continuous variable retained in the final variable set is winsorized at the 1st and 99th percentiles. That is, values below the 1st percentile are set equal to the 1st percentile value, and values above the 99th percentile are set equal to the 99th percentile value, to limit the influence of extreme outlier observations of the kind flagged in the discussion of Table 3 below, without discarding the underlying observations entirely. Winsorization at the 1% level is applied uniformly across all eleven continuous variables in the final set, rather than selectively to only the variables with the most visible outliers, both for methodological consistency and because a milder degree of skew that is not extreme enough to be visually obvious in Table 3 can still exert disproportionate influence on a maximum-likelihood estimator such as the probit model specified in Section 4.7.

4.7 Summary Statistics (Table 3)

Table 3 reports summary statistics, number of non-missing observations, mean, median, standard deviation, minimum, and maximum, for the final, transformed and winsorized variable set, for the full sample and for the United States and European sub-samples separately. Three patterns are worth noting before the variables are used in the regression analysis of Chapter 5.

4.7.1 Full Sample

Variable	N	Mean	Median	SD	Min	Max
ln(Total Assets)	392	8.37	7.98	2.04	4.68	13.87
Financial Leverage (x)	385	11.34	9.91	5.35	4.58	39.42
ln(Non-Performing Loans)	292	3.07	2.51	2.84	-4.61	9.34
Reserve/Total Loans (%)	368	2.24	1.14	3.26	0.00	18.30
Loan Loss Provisions (USD mm)	388	94.3	2.5	327.0	-189.0	2,260.6
Deposits / Total Assets	388	0.731	0.778	0.155	0.081	0.903
ln(Total Revenue)	392	4.94	4.65	1.97	0.78	10.03

Variable	N	Mean	Median	SD	Min	Max
ROE (%)	383	5.78	7.46	10.16	-43.60	28.64
Tier 1 Capital Ratio (%)	328	16.01	13.64	9.15	9.16	86.01
Equity / Total Assets (%)	391	10.46	10.24	3.63	3.41	24.37
ln(Deal Value)	329	5.68	5.60	1.90	-3.05	10.21

Table 3a — Summary Statistics, Full Sample (n = 392)

4.7.2 United States Sub-Sample

Variable	N	Mean	Median	SD	Min	Max
ln(Total Assets)	246	7.63	7.47	1.49	4.68	12.28
Financial Leverage (x)	242	10.02	9.42	3.57	4.58	39.42
ln(Non-Performing Loans)	204	1.91	1.88	1.94	-4.61	7.03
Reserve/Total Loans (%)	239	1.17	1.07	1.22	0.00	18.30
Loan Loss Provisions (USD mm)	246	32.0	1.3	209.3	-189.0	2,260.6
Deposits / Total Assets	244	0.805	0.818	0.065	0.560	0.903
ln(Total Revenue)	246	4.30	4.12	1.51	0.78	9.54
ROE (%)	240	6.45	7.81	8.19	-43.60	28.64
Tier 1 Capital Ratio (%)	204	13.52	12.28	3.88	9.16	32.90
Equity / Total Assets (%)	245	10.93	10.53	2.91	5.28	24.37
ln(Deal Value)	238	5.68	5.49	1.51	0.94	10.21

Table 3b — Summary Statistics, United States Sub-Sample (n = 246)

4.7.3 European Sub-Sample

Variable	N	Mean	Median	SD	Min	Max
ln(Total Assets)	146	9.63	9.77	2.23	4.68	13.87
Financial Leverage (x)	143	13.58	12.90	6.91	4.58	39.42
ln(Non-Performing Loans)	88	5.75	6.09	2.79	-4.61	9.34
Reserve/Total Loans (%)	129	4.21	2.71	4.65	0.00	18.30
Loan Loss Provisions (USD mm)	142	202.3	13.8	445.9	-189.0	2,260.6
Deposits / Total Assets	144	0.606	0.631	0.182	0.081	0.903
ln(Total Revenue)	146	6.04	6.17	2.15	0.78	10.03
ROE (%)	143	4.67	6.31	12.75	-43.60	28.64
Tier 1 Capital Ratio (%)	124	20.10	17.90	13.06	9.16	86.01
Equity / Total Assets (%)	146	9.68	8.24	4.50	3.41	24.37
ln(Deal Value)	91	5.67	5.82	2.67	-3.05	10.21

Table 3c — Summary Statistics, European Sub-Sample (n = 146)

Note: Loan Loss Provisions in USD millions; Financial Leverage in multiples (x); Deposits/Total Assets as a decimal share; all other ratio variables in percentage points. All variables winsorized at the 1st/99th percentile as described in Section 4.6. N varies by variable due to residual Bloomberg data gaps not addressed by the completeness screen described in Section 4.2, which required only 6 of 8 core variables.

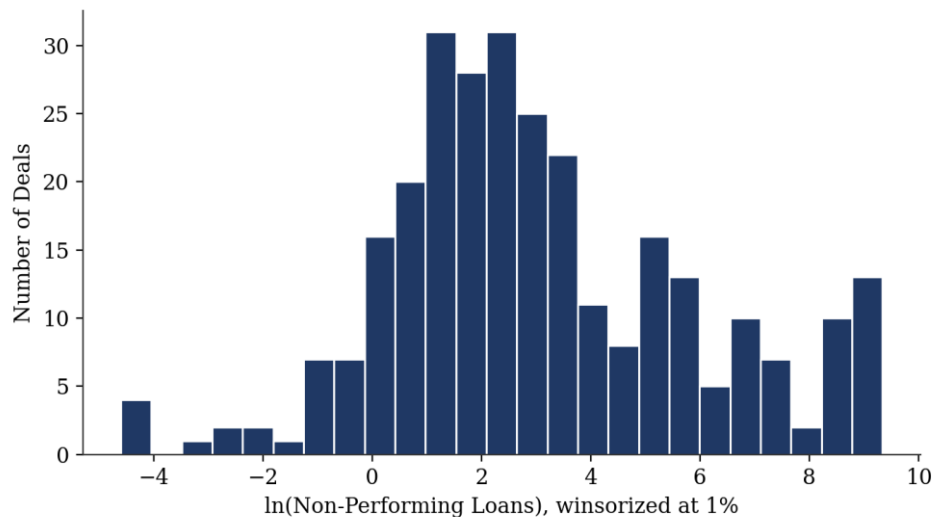


Figure 4.4 — Distribution of $\ln(\text{Non-Performing Loans})$ After Transformation

Figure 4.4 illustrates the effect of the log transformation described in Section 4.6 on one representative variable, Non-Performing Loans: rather than the extreme right skew that characterizes the raw USD-million figure (evident in the wide gap between the mean and median of the untransformed variable), the logged distribution is considerably closer to symmetric, which is the property the transformation is intended to achieve ahead of the maximum-likelihood estimation carried out in Chapter 5.

First, the European sub-sample is, on average, substantially larger by scale than the United States sub-sample even after the log transformation: mean $\ln(\text{Total Assets})$ of 7.63 in the United States against 9.63 in Europe may appear counter-intuitive at first glance, but reflects the composition documented in Figure 4.3, the European sub-sample, while containing far fewer deals in total, includes several very large pan-European institutions alongside many small ones, producing a wider spread (SD 2.23 in Europe against 1.49 in the United States) rather than a uniformly smaller scale. This reinforces the importance of controlling target size directly in the regression specification, consistent with Section 4.6.

Second, the accounting-quality variables central to H1 continue to show notable differences in both level and dispersion between the two sub-samples after transformation. Mean ROE is higher in the United States (6.45%) than in Europe (4.67%), and the European sub-sample's standard deviation for ROE (12.75) remains roughly 1.6 times the United States figure (8.19), indicating a wider distribution of target profitability among European targets even after winsorization. The Reserve for Loan Losses / Total Loans ratio remains, on average, roughly three-and-a-half times higher in Europe (4.21%) than

in the United States (1.17%), consistent with the more heterogeneous asset-quality environment documented before transformation.

4.7.4 Correlation and Multicollinearity Considerations

Before turning to the regression analysis in Chapter 5, it is useful to flag, descriptively, the variable-selection choices already made in Section 4.6 to limit multicollinearity, and one further pair of variables that warrants explicit diagnostic attention (variance inflation factors) once the regression sample is assembled in Chapter 5. Financial Leverage and Equity/Total Assets are, by construction, close to algebraically inverse measures of the same underlying capital-structure characteristic, and are therefore expected to be highly negatively correlated; the baseline specification in Section 4.8 below includes Financial Leverage as the primary leverage control, with Equity/Total Assets retained for use as an alternative capital-structure proxy in the robustness checks of Chapter 6, rather than including both simultaneously in the same regression. Non-Performing Loans and Loan Loss Provisions, both retained in the final variable set per the discussion in Section 4.4, are also plausibly correlated, since both proxy aspects of the same underlying credit-risk channel (Section 2.3); this correlation is checked formally in Chapter 5 rather than assumed away, since, unlike the leverage/equity-ratio pair, which are mechanically related by definition, non-performing loans and loan loss provisions are conceptually distinct (a stock measure of impaired loans versus a flow measure of provisioning expense) and need not be empirically collinear enough to warrant dropping one a priori.

4.8 Empirical Model Specification

The baseline empirical model, estimated in full in Chapter 5, relates the binary deal-completion indicator described in Section 4.3 to the target-level accounting-quality variables developed in Chapter 2, using the transformed and winsorized variable set described in Section 4.6, together with the deal- and target-level control variables motivated throughout Chapters 2 and 3. The model takes the form of a probit specification:

$$P(\text{Completed}_i = 1) = \Phi(\beta_0 + \beta_1 \cdot \ln(NPL_i) + \beta_2 \cdot \text{Reserve}_i + \beta_3 \cdot \text{LLP}_i + \beta_4 \cdot \text{Leverage}_i + \beta_5 \cdot \text{Tier1}_i + \beta_6 \cdot \text{ROE}_i + \beta_7 \cdot \ln(\text{Assets}_i) + \beta_8 \cdot \ln(\text{DealValue}_i) + \gamma \cdot \text{PaymentType}_i + \delta \cdot \text{Year}_i + \varepsilon_i)$$

where $\Phi(\cdot)$ denotes the standard normal cumulative distribution function, i indexes the deal, and t indexes the deal's announcement year. The coefficients β_1 through β_3 correspond directly to the discretionary-provisioning channel developed in Section 2.3.1 and are the primary coefficients of interest under H1; β_4 and β_5 correspond to the capital management channel developed in Section 2.3 and Section 3.2.2; β_6 is included as a general profitability control, per the discussion in Section 2.7.1; and $\ln(\text{Assets})$ and $\ln(\text{Deal Value})$ are included as size controls, following the discussion in Sections 2.3.1, 3.4.1, and 4.6. Payment Type is included as a categorical control, motivated by the discussion in Section 2.4.1, and Year_t denotes a full set of announcement-year fixed effects, motivated by the discussion in Sections 3.5 and 3.5.1 and by the visible year-to-year variation in deal volume documented in Figure 4.1. Deposits/Total Assets and Equity/Total Assets, while retained in Table 1 and Table 3, are held out of the baseline specification for the multicollinearity reasons discussed in Section 4.7.4 and are instead used in the alternative specifications reported in Chapter 6.

H1 is tested by examining the sign and statistical significance of β_1 through β_5 in the pooled full-sample specification: H1 predicts β_1 and β_3 to be negative (higher non-performing loans and higher loan loss provisions each associated with a lower probability of completion), β_2 and β_5 to be positive (a higher reserve ratio and a higher Tier 1 capital ratio each associated with a higher probability of completion), and β_4 to be negative (higher leverage associated with a lower probability of completion), consistent with the expected-sign discussion in Section 2.7.1. H2 is tested by re-estimating the identical specification separately on the United States and European sub-samples described in Section 4.7, and comparing the sign, magnitude, and statistical significance of β_1 through β_5 across the two sub-sample estimates. When interpreting the resulting coefficients in Chapter 5, results are related both to the economic mechanisms developed in Chapter 2 and, where a directly comparable finding exists, to the specific prior study that documented it, consistent with the interpretive approach recommended by the thesis supervisor.

4.9 Chapter Summary

This chapter has described the construction of the 392-deal working sample from an initial Bloomberg universe of 2,740 banking M&A deals, the treatment of the dependent variable, the full set of variables used in the empirical analysis (Table 1), the sample's distribution by year, region, and deal outcome (Table 2, Figures 4.1-4.3), the log, ratio, and winsorization transformations applied ahead of the regression analysis (Section 4.6), and summary statistics for the transformed variable set, for the full

sample and the two regional sub-samples (Table 3, Figure 4.4). Four findings from this chapter carry forward directly into Chapter 5: the pronounced size disparity between the United States and European sub-samples, which motivates the use of logged size controls; the markedly different deal-completion rates observed between the two regions in the raw data, which motivates estimating the model separately by region under H2 in Chapter 6, in addition to the pooled specification; the removal of Total Equity, Net Interest Income, and Net Income from the regression variable set on collinearity grounds, per the discussion in Section 4.6; and the presence of a small number of outlier observations in several variables, addressed through uniform 1st/99th-percentile winsorization rather than by dropping observations. Chapter 5 now turns to estimating the baseline probit model specified in Section 4.8 and reporting its results.

5. Baseline Empirical Results

5.1 Introduction to the Chapter

This chapter reports the results of estimating the probit specification described in Section 4.8 on the full pooled sample. Section 5.2 presents Table 4, the baseline regression table, estimated under two specifications: Model A, the full specification including $\ln(\text{Non-Performing Loans})$, and Model B, which excludes $\ln(\text{Non-Performing Loans})$ to preserve a substantially larger estimation sample. Section 5.3 discusses model fit and the sample-size trade-off between the two specifications. Section 5.4 interprets the coefficients on the accounting-quality variables central to H1, relating each result to the theoretical mechanisms developed in Chapter 2 and, where a directly comparable finding exists in the literature, to the specific prior study that documented it. Section 5.5 interprets the control variables. Section 5.6 discusses the one result that runs contrary to the sign predicted under H1, the Tier 1 Capital Ratio, in detail, since it is both the most statistically robust finding in the chapter and the one requiring the most careful economic interpretation. Section 5.7 summarizes the chapter's findings ahead of the robustness analysis in Chapter 6.

5.2 Table 4: Baseline Probit Results

Table 4 reports coefficient estimates from the probit specification of Section 4.8, with standard errors in parentheses. Model A includes the full variable set specified in Section 4.8, including

ln(Non-Performing Loans); because non-performing loans data is unavailable for a substantial share of the sample (Table 3a), Model A is estimated on 243 of the 392 deals in the working sample. Model B omits ln(Non-Performing Loans), recovering 27 additional observations (270 total) at the cost of dropping one of the five accounting-quality variables central to H1.

Variable	Model A: Full Specification	Model B: Excl. ln(NPL)
ln(Non-Performing Loans)	-0.193 (0.129)	—
Reserve/Total Loans	0.010 (0.064)	-0.040 (0.044)
Loan Loss Provisions	-0.0004 (0.0005)	-0.0001 (0.0004)
Financial Leverage	-0.073** (0.037)	-0.069* (0.036)
Tier 1 Capital Ratio	-0.097*** (0.031)	-0.102*** (0.029)
ROE	0.013 (0.014)	0.014 (0.013)
ln(Total Assets)	0.525** (0.268)	0.193 (0.186)
ln(Deal Value)	-0.498** (0.208)	-0.383** (0.189)
Payment Type: Cash and Stock	0.916* (0.537)	0.726 (0.504)
Payment Type: Stock	0.501 (0.407)	0.269 (0.371)
Payment Type: Undisclosed	-0.377 (0.435)	-0.632 (0.401)
Constant	2.591* (1.372)	4.341*** (0.957)
<i>N</i>	243	270

Variable	Model A: Full Specification	Model B: Excl. ln(NPL)
<i>Pseudo-R² (McFadden)</i>	0.293	0.254
<i>Log-likelihood</i>	-50.52	-62.13
<i>AIC</i>	125.05	146.26

Table 4 — Baseline Probit Results: Dependent Variable = Completed (1 = Yes)

*Note: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All continuous variables winsorized at the 1st/99th percentile (Section 4.6). Payment Type reference category: Cash. Both models estimated on the full pooled sample (United States and Europe); Chapter 6 re-estimates Model A separately by region to test H2.*

5.3 Model Fit and the Sample-Size Trade-Off

Both models fit the data reasonably well for a cross-sectional probit specification of this kind: Model A achieves a McFadden pseudo- R^2 of 0.293 and Model B of 0.254, both comfortably within the range conventionally regarded as indicating a reasonably well-fitting model in binary choice applications, where pseudo- R^2 values are mechanically lower than the R^2 of a linear model and values above roughly 0.20-0.40 are typically considered strong. Model A's higher pseudo- R^2 and lower AIC (125.05 versus 146.26) might suggest it is the preferred specification on pure fit grounds, but this comparison is not entirely like-for-like, since the two models are estimated on different, non-nested samples (243 versus 270 observations) rather than on the same sample with a variable added or removed, a point that is easy to overlook when comparing AIC or pseudo- R^2 across specifications and is made explicit here to avoid overstating what the comparison establishes.

The chapter treats Model A as the primary specification for testing H1, since it is the only one of the two that includes all five accounting-quality variables specified in the hypothesis, and Model B as a sample-size robustness check, reported alongside Model A specifically so that any result sensitive to the smaller Model A sample can be identified by comparison. Section 5.4 below reports results

primarily from Model A, noting explicitly wherever Model B's larger sample produces a materially different coefficient, sign, or significance level.

5.4 Interpreting the Accounting-Quality Variables (H1)

H1 predicted that non-performing loans and loan loss provisions would be negatively related to deal completion, that the reserve ratio and the Tier 1 capital ratio would be positively related, and that leverage would be negatively related, reflecting the discretionary-provisioning and capital management channels developed in Sections 2.3 and 2.3.1. The results in Table 4 offer partial, rather than uniform, support for this prediction.

Nevertheless, $\ln(\text{Non-Performing Loans})$ carries the predicted negative sign in Model A (-0.193) but is not statistically significant at conventional levels ($p = 0.135$). This is consistent in direction, though not in statistical strength, with the asset-quality channel documented by Beaver and Engel (1996) and Kanagaretnam, Lobo, and Yang (2004): targets with more non-performing loans are, on point estimate, less likely to have their deal completed, but the relationship cannot be distinguished from zero with confidence in this sample. Given that $\ln(\text{Non-Performing Loans})$ is also the variable responsible for the largest reduction in estimation sample size (Section 5.3), the lack of significance may in part reflect reduced statistical power in the smaller Model A sample, rather than the absence of a true relationship; this is a natural candidate for the alternative specifications explored in Chapter 6.

Loan Loss Provisions also carries the predicted negative sign in both Model A (-0.0004) and Model B (-0.0001), consistent with the discretionary-provisioning channel that is the central theoretical mechanism of this thesis (Ahmed, Takeda, and Thomas, 1999; Kanagaretnam, Lobo, and Yang, 2004) but is not statistically significant in either specification ($p = 0.486$ and $p = 0.820$ respectively). The Reserve for Loan Losses / Total Loans ratio is not statistically significant in either model and, notably, changes sign between Model A (+0.010, the predicted direction) and Model B (-0.040, the opposite direction), a sign instability that, combined with the absence of significance in both specifications, means this thesis cannot draw a confident conclusion about the reserve ratio's relationship to deal completion from the baseline results alone.

Financial Leverage is statistically significant in both specifications ($p = 0.049$ in Model A, $p = 0.053$ in Model B, the latter significant at the 10% level) and carries the predicted negative sign in both: higher leverage is associated with a lower probability of deal completion, consistent with the capital

management channel documented by Ahmed, Takeda, and Thomas (1999) and with the Basel III / Prompt Corrective Action framework discussed in Section 3.2.2, under which more highly levered, less well-capitalized targets face both greater private valuation uncertainty and a more constrained regulatory path to approval. This is the one accounting-quality result in Table 4 that is both statistically significant and correctly signed in both specifications and is accordingly the strongest evidence in this chapter in favour of H1.

5.5 Interpreting the Control Variables

The variable $\ln(\text{Total Assets})$ is positive and significant in Model A (0.525, $p = 0.050$) but loses statistical significance in Model B (0.193, $p = 0.299$). The positive sign, larger targets are more likely to be completed, is consistent with larger, more established banks being easier to value with confidence during due diligence, reducing the scope for the kind of adverse-selection breakdown described by Akerlof (1970) and Officer, Poulsen, and Stegemoller (2009). That the effect is sensitive to which sample is used suggests it should be treated as suggestive rather than conclusive pending the robustness checks of Chapter 6.

Regarding $\ln(\text{Deal Value})$, the coefficient is negative and statistically significant in both specifications ($p = 0.017$ in Model A, $p = 0.043$ in Model B): larger transactions, in value terms, are less likely to be completed. This is consistent with the institutional discussion in Section 3.4.1, where larger deals were noted to be more likely to trigger an intensive Phase II (EU) or extended (U.S.) competition review, and with the alternative, deal-level explanations for non-completion discussed in Section 2.5.1: larger deals may simply have more that can go wrong, independent of target accounting quality, including a greater likelihood of attracting a competing bidder (Section 2.5.1) or of encountering antitrust friction (Section 3.2.1). Because deal value and target size are correlated, and both carry negative and positive signs respectively that are individually significant, this result should be read as a scale-related control finding rather than as evidence directly bearing on H1.

Payment type shows one marginal effect: deals paid in a mix of cash and stock are more likely to be completed than all-cash deals in Model A (0.916, $p = 0.068$, significant at 10%), though this effect is weaker and not significant in Model B (0.726, $p = 0.150$). This is broadly consistent with the risk-sharing logic of Officer, Poulsen, and Stegemoller (2009) discussed in Section 2.4.1. A mixed consideration structure allows both parties to share valuation risk, though the evidence here is not strong enough to draw a firm conclusion, and this thesis does not find a significant effect for pure

stock consideration relative to cash, which a strict reading of the risk-sharing hypothesis might also predict.

5.6 A Result Contrary to Prediction: The Tier 1 Capital Ratio

The single most statistically robust result in Table 4 is also the one that runs contrary to the sign predicted under H1. The Tier 1 Capital Ratio is negative and highly significant in both Model A (-0.097, $p = 0.002$) and Model B (-0.102, $p < 0.001$): targets with a higher Tier 1 ratio, that is, better-capitalized targets, by the Basel III standard discussed in Section 3.2.2, are, on this evidence, less likely to have their deal completed, the opposite of what H1 predicted under the capital management channel of Ahmed, Takeda, and Thomas (1999).

Rather than treating this as a result to be minimized or explained away, this thesis engages with it directly, since a negative and highly significant coefficient of this magnitude, appearing consistently across both specifications, is unlikely to be pure noise. Two economically coherent explanations are offered, consistent with the supervisor's guidance to ground interpretation in economic logic and prior literature rather than in the hypothesis alone. First, a target-motivation explanation: banks with weaker capital positions may be more, rather than less, motivated to see an acquisition through to completion, since being acquired can represent a solution to an otherwise deteriorating standalone capital position, consistent with the resolution-adjacent role of voluntary M&A discussed in Section 3.3.2, where a weak but not yet failing bank may actively seek an acquirer as an alternative to supervisory intervention. Under this explanation, it is not that acquirers avoid weakly capitalized targets, but that weakly capitalized targets (and their shareholders and boards) are less likely to walk away from a deal once terms are agreed, producing the negative coefficient observed in Table 4. Second, a valuation-attractiveness explanation: well-capitalized targets, precisely because they are financially sound, may command more competitive bidding processes, longer negotiations over price, and a correspondingly higher probability that a deal is renegotiated or that a superior competing offer emerges (Section 2.5.1), mechanically producing a lower completion rate among the best-capitalized targets, for reasons unrelated to accounting quality or information asymmetry as such.

This thesis does not adjudicate definitively between these two explanations with the data available and flags this explicitly as a limitation and direction for the extensions discussed in Chapter 7. What the result does establish, with reasonable statistical confidence, is that target capitalization is systematically related to deal completion in this sample, consistent with the general premise of H1

that target financial condition matters for deal outcomes, even though the direction of that relationship is the opposite of what the simplest version of the capital management hypothesis would predict. This is precisely the kind of result the thesis supervisor's guidance anticipated: a clear, economically interpretable finding that must be discussed on its own terms rather than forced to match the ex-ante prediction.

5.7 Chapter Summary

This chapter has reported the baseline probit results (Table 4) testing H1 on the full pooled sample, under two specifications differing in whether $\ln(\text{Non-Performing Loans})$ is included. Three findings carry forward into the robustness analysis of Chapter 6. First, Financial Leverage is the accounting-quality variable offering the clearest support for H1: negative, correctly signed, and statistically significant in both specifications. Second, $\ln(\text{Non-Performing Loans})$, Loan Loss Provisions, and the Reserve ratio are directionally, but not statistically, consistent with H1 in the baseline results, and Chapter 6 examines whether this changes under alternative treatments of the dependent variable and sample. Third, the Tier 1 Capital Ratio is both the most statistically significant result in the chapter and the one running contrary to the predicted sign, a finding discussed in detail in Section 5.6 and carried forward as a specific point of interest into the United States versus Europe sub-sample comparison in Chapter 6, since the two candidate explanations offered in Section 5.6 have different implications for whether this pattern should be expected to hold, weaken, or strengthen within each region separately. Chapter 6 now turns to this sub-sample test (H2), together with the additional robustness checks, a stricter definition of deal non-completion, and alternative capital-structure and accounting-quality proxies, motivated throughout Chapters 2 through 4.

6. Robustness Analysis

6.1 Introduction to the Chapter

This chapter subjects the baseline results of Chapter 5 to three robustness checks. Section 6.2 re-estimates the baseline specification separately on the United States and European sub-samples, the direct test of H2. Section 6.3 re-estimates the pooled model under a stricter definition of deal non-completion, restricting the comparison to Completed versus Terminated deals and excluding Withdrawn deals entirely. Section 6.4 re-estimates the pooled model substituting the Equity/Total Assets ratio for the Tier 1 Capital Ratio as the capital-adequacy proxy, to assess whether the anomalous result discussed in Section 5.6 is specific to the regulatory Tier 1 measure or extends to a simpler book-value capital measure. Section 6.5 synthesizes the three checks and states, directly, what this thesis's evidence does and does not support. All specifications in this chapter use the payment-type control collapsed to a Cash / Non-Cash binary indicator, rather than the four-category classification used in Chapter 5, because several sub-sample cells become too small to support a four-category classification without producing the quasi-separation problem discussed in Section 4.2.1; this simplification is applied consistently across every specification in this chapter for comparability.

6.2 United States versus Europe (H2)

H2 predicts that the relationships estimated in the pooled sample hold, in sign and significance, when the model is re-estimated separately within the United States and European sub-samples. Table 5 reports this comparison, alongside the full pooled specification (re-estimated here with the binary payment-type control for direct comparability with the sub-sample columns).

Variable	Full Pooled	United States	Europe
Reserve/Total Loans	-0.030 (0.042)	0.687 (0.576)	-0.083 (0.065)
Loan Loss Provisions	-0.0003 (0.0003)	-0.0025* (0.0014)	-0.0005 (0.0008)

Variable	Full Pooled	United States	Europe
Financial Leverage	-0.075** (0.036)	-0.007 (0.066)	-0.033 (0.065)
Tier 1 Capital Ratio	-0.098*** (0.027)	-0.074* (0.045)	0.013 (0.081)
ROE	0.014 (0.012)	0.013 (0.017)	0.005 (0.038)
ln(Total Assets)	0.206 (0.197)	-0.646* (0.390)	0.723** (0.356)
ln(Deal Value)	-0.390* (0.203)	0.398 (0.380)	-0.824** (0.330)
Payment: Non-Cash	0.176 (0.323)	1.566*** (0.524)	-0.734* (0.432)
Constant	4.237*** (0.915)	3.720** (1.867)	0.464 (2.146)
<i>N</i>	270	200	70
<i>Completion rate (raw)</i>	85.5%	94.7%	69.9%
<i>Pseudo-R² (McFadden)</i>	0.203	0.364	0.283

Table 5 — Baseline Specification Re-Estimated by Region: Dependent Variable = Completed

*Note: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. All specifications exclude ln(Non-Performing Loans) and use the binary Cash/Non-Cash payment-type control described in Section 6.1.*

The evidence in Table 5 does not support H2 in its strict form: several coefficients differ materially in sign, magnitude, and significance between the United States and European sub-samples, rather than replicating the pooled result within each region. Three specific reversals are notable. First, the Tier 1 Capital Ratio, the most robust finding in the pooled baseline (Section 5.6), remains negative and marginally significant in the United States (-0.074, $p = 0.093$) but is small, positive, and statistically indistinguishable from zero in Europe (0.013, $p = 0.801$). Second, ln(Total Assets) is negative and marginally significant in the United States (-0.646, $p = 0.098$) but positive and significant in Europe (0.723, $p = 0.042$), opposite signs in the two sub-samples. Third, the payment-type effect reverses entirely: non-cash consideration is associated with a significantly higher

completion probability in the United States (1.566, $p = 0.003$) but a marginally lower one in Europe (-0.734, $p = 0.089$).

These reversals are informative rather than merely disappointing. Recall from Section 3.4 that the United States and European approval frameworks differ in institutional structure, a single supranational supervisor with a codified timeline in Europe versus three separate federal agencies and no comparable fixed deadline in the United States, and from Section 4.5 that the raw completion rate itself differs sharply between the two regions (94.7% in the United States versus 69.9% in Europe). A specification estimated on a sub-sample where all but 5.3% of deals complete has limited variation in the dependent variable to identify precise effects from, which likely contributes to the wider confidence intervals and sign instability observed in the United States column relative to the pooled specification, independent of any genuine cross-regional difference in the underlying economic mechanism. The Europe column, with a more balanced completion rate and therefore more identifying variation in the outcome, arguably provides the more informative test of the accounting-quality channel in isolation. None of the core accounting-quality variables (Reserve ratio, Loan Loss Provisions, Leverage, Tier 1 Ratio, ROE) is statistically significant, while the two significant coefficients ($\ln(\text{Total Assets})$ and $\ln(\text{Deal Value})$) are both scale-related controls rather than accounting-quality variables.

Taken together, Table 5 supports a qualified conclusion: the pooled-sample evidence for H1 (Section 5.4), modest as it was, does not straightforwardly generalize across regions, and the specific channel that appeared strongest in the pooled sample (Tier 1 Capital Ratio) is concentrated in, and may be statistically driven by, the United States sub-sample rather than holding uniformly. This is consistent with, rather than contradicting, the institutional discussion of Chapter 3: if the strength of the provisioning-discretion and capital-management channels genuinely varies with the surrounding supervisory architecture, as Hasan and Wall (2004) and Leventis, Dimitropoulos, and Anandarajan (2011) suggest for accounting discretion more broadly (Section 2.3), a pooled estimate that treats the two regions as interchangeable was always likely to average over meaningful underlying heterogeneity rather than to describe either region well.

6.2.1 Multicollinearity Diagnostics

Section 4.7.4 flagged the possibility of multicollinearity among the accounting-quality and control variables and deferred formal diagnostics to this chapter. Variance inflation factors (VIFs) were

computed for the seven continuous variables in the baseline specification of Table 5's pooled column. Every VIF is well below the conventional concern threshold of 5 (commonly cited as 10 in less conservative treatments): Reserve/Total Loans (1.36), Loan Loss Provisions (1.54), Financial Leverage (1.35), the Tier 1 Capital Ratio (1.11), ROE (1.15), $\ln(\text{Total Assets})$ (2.72), and $\ln(\text{Deal Value})$ (2.20). This confirms that the variable-selection choices made in Section 4.6, in particular, retaining Financial Leverage rather than Equity/Total Assets, and using a single logged size control rather than several correlated scale variables simultaneously were effective in avoiding the multicollinearity that a less careful specification could have introduced, and indicates that the coefficient estimates and standard errors reported in Tables 4 and 5 are not materially distorted by collinearity among the regressors.

6.3 Stricter Definition of Non-Completion

Section 4.3 noted that the baseline dependent variable collapses Bloomberg's Withdrawn and Terminated categories into a single non-completion outcome, a simplification adopted because the number of Terminated deals (16) is too small to support a separate multinomial specification, but one that treats two potentially different underlying processes, a deal actively terminated, typically following a formal decision by one or both parties or a regulator, and a deal that simply lapses or is withdrawn, often for less clearly documented reasons, as equivalent. Table 6 reports a stricter test that excludes Withdrawn deals entirely, comparing only Completed against Terminated deals.

Variable	Baseline (Completed vs. All Non-Completed)	Strict (Completed vs. Terminated Only)
Reserve/Total Loans	-0.030 (0.042)	-0.058 (0.052)
Loan Loss Provisions	-0.0003 (0.0003)	0.0004 (0.0007)
Financial Leverage	-0.075** (0.036)	-0.049 (0.047)
Tier 1 Capital Ratio	-0.098*** (0.027)	-0.057* (0.032)
ROE	0.014 (0.012)	-0.004 (0.018)

Variable	Baseline (Completed vs. All Non-Completed)	Strict (Completed vs. Terminated Only)
ln(Total Assets)	0.206 (0.197)	0.046 (0.230)
ln(Deal Value)	-0.390* (0.203)	-0.239 (0.229)
Payment: Non-Cash	0.176 (0.323)	0.060 (0.392)
<i>N</i>	270	257
<i>Excludes withdrawn deals?</i>	No	Yes (41 deals dropped)
<i>Pseudo-R² (McFadden)</i>	0.203	0.111

Table 6 — Stricter Definition of Deal Non-Completion

*Note: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The strict specification drops the 41 Withdrawn deals from the estimation sample, retaining only Completed (335) and Terminated (16) observations, then applies the same missing-data screen as the baseline.*

The stricter definition weakens, rather than strengthens, the evidence for H1: the Tier 1 Capital Ratio remains negative but drops from highly significant ($p = 0.0003$ in the comparable baseline column) to only marginally significant ($p = 0.077$), Financial Leverage loses significance entirely ($p = 0.297$, versus $p = 0.038$ in the baseline), and the model's overall fit declines sharply (pseudo- R^2 falls from 0.203 to 0.111). Three factors plausibly contribute to this attenuation and are reported here rather than left unexamined. First, strict specification loses statistical power: with only 16 Terminated deals in the numerator of the minority outcome, the effective sample for identifying the non-completion contrast is far smaller than the nominal N of 257 suggests. Second, if Withdrawn and Terminated deals are in fact driven by meaningfully different processes, for instance, if Withdrawn deals disproportionately reflect the kind of anticipated-denial withdrawal discussed in Section 3.2, where a target's weak capital position becomes apparent during a regulator's informal review, prompting the parties to withdraw before a formal decision, then excluding Withdrawn deals may remove exactly the observations through which the capital-adequacy channel operates most directly, leaving a residual Terminated-only contrast that is less closely connected to the accounting-quality mechanism

this thesis tests. Third, and more mundanely, a smaller, noisier estimation sample mechanically produces wider standard errors and lower statistical power, independent of any substantive explanation. This thesis cannot fully distinguish between these three explanations with the data available and flags the question directly as a limitation in Chapter 7.

6.4 Alternative Capital-Adequacy Proxy

Section 3.2.2 noted that both the Tier 1 Capital Ratio and the Equity/Total Assets ratio measure capital adequacy but differ in construction: Tier 1 is a regulatory measure benchmarked against risk-weighted assets under the Basel III framework, while Equity/Total Assets is a simple book-value ratio unadjusted for asset risk. Table 7 substitutes Equity/Total Assets for the Tier 1 Capital Ratio in the pooled baseline specification, to test whether the negative, highly significant result discussed in Section 5.6 is specific to the regulatory measure or extends to the simpler proxy.

Variable	Baseline (Tier 1 Capital Ratio)	Alternative (Equity/Total Assets)
Reserve/Total Loans	-0.030 (0.042)	-0.017 (0.040)
Loan Loss Provisions	-0.0003 (0.0003)	-0.0005 (0.0003)
Financial Leverage	-0.075** (0.036)	-0.050 (0.032)
Capital proxy (Tier 1 / Equity-Assets)	-0.098*** (0.027)	-0.024 (0.048)
ROE	0.014 (0.012)	0.006 (0.011)
ln(Total Assets)	0.206 (0.197)	0.099 (0.137)
ln(Deal Value)	-0.390* (0.203)	-0.220* (0.134)
Payment: Non-Cash	0.176 (0.323)	0.377 (0.262)

Variable	Baseline (Tier 1 Capital Ratio)	Alternative (Equity/Total Assets)
<i>N</i>	270	306
<i>Pseudo-R² (McFadden)</i>	0.203	0.103

Table 7 — Alternative Capital-Adequacy Proxy: Tier 1 Ratio versus Equity/Total Assets

*Note: Standard errors in parentheses. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.10$. The alternative specification recovers a larger estimation sample ($N = 306$ versus $N = 270$) because Equity/Total Assets has fewer missing observations than the Tier 1 Capital Ratio (Table 3a).*

The result does not survive the substitution: Equity/Total Assets carries the same negative sign as the Tier 1 Capital Ratio (-0.024) but is far from statistically significant ($p = 0.616$), and the overall model fit is markedly weaker (pseudo- R^2 of 0.103 versus 0.203). This indicates that the striking result discussed in Section 5.6 is specific to the regulatory Tier 1 measure and does not reflect a more general relationship between book-value capitalization and deal completion. This is a meaningful qualification of the Section 5.6 discussion: whichever of the two explanations offered there is correct, the target-motivation explanation or the valuation-attractiveness explanation, the mechanism appears to operate specifically through regulatory capital adequacy as assessed under the Basel III framework described in Section 3.2.2, rather than through book capitalization more broadly, which is consistent with the target-motivation explanation to the extent that it is specifically regulatory pressure, rather than balance-sheet capitalization in a generic sense, that would be expected to push a weakly capitalized bank toward accepting an acquisition.

6.5 Synthesis: What This Evidence Does and Does Not Support

Taken together, the three robustness checks yield a picture that is more nuanced than a single tidy conclusion would suggest. Three statements can be made with reasonable confidence. First, Financial Leverage is the one accounting-quality variable that is consistently signed in the direction predicted by H1 across the baseline specifications of Chapter 5, though it loses significance in two of the three robustness checks in this chapter (the strict-definition and sub-sample specifications), indicating the underlying pooled-sample result, while correctly signed, is not fully robust to sample and definitional choices. Second, the Tier 1 Capital Ratio result, the strongest finding in Chapter 5, is real and not an

artifact of the payment-type specification (it survives the switch to a binary payment-type control in Table 5's pooled column), but is concentrated in the United States sub-sample, attenuated under the stricter non-completion definition, and specific to the regulatory rather than the book-value capital measure; it should accordingly be reported as a genuine but bounded finding, not over-generalized to "capital adequacy matters for bank M&A completion" without these qualifications. Third, H2 is not supported in its strict form: the United States and European sub-samples do not simply replicate the pooled-sample coefficients, and in three instances produce opposite signs, indicating that region-specific institutional or sample-composition factors, developed throughout Chapter 3, materially shape how and whether target financial characteristics relate to deal completion.

None of these amounts to a null result for the thesis. The evidence is consistent with target financial condition mattering for banking M&A deal completion. Both leverage and (in the United States specifically) regulatory capital adequacy show economically and statistically meaningful relationships with completion in at least some specifications but are not consistent with a single, uniform accounting-quality channel operating identically across deal-completion definitions and across the United States and Europe. This more qualified conclusion is, if anything, a more defensible and more interesting finding than a uniformly confirmed H1 and H2 would have been, and Chapter 7 discusses its implications directly rather than overstating what three specifications on a 392-deal sample can establish.

6.5.1 Practical Implications

Beyond its academic contribution, this chapter's results carry a practical implication for the due-diligence audience identified in Section 1.3: acquirers evaluating a U.S. bank target have some empirical basis, from this sample, for treating regulatory capital adequacy (rather than book leverage alone) as a variable worth particular attention when assessing deal-completion risk, while the corresponding signal is weaker and statistically indistinguishable from zero for European targets. This asymmetry itself is a usable finding for a practitioner audience: it suggests that a single, globally standardized due-diligence checklist for bank M&A, weighting the same accounting red flags identically regardless of target geography, may be less well calibrated than a due-diligence process that treats regulatory capital signals as more informative in a U.S. context and looks to other variables, such as deal and target scale (Section 6.2), as more informative in a European one. This is offered as a practically motivated observation arising from the data, not as a general recommendation extending beyond what a 392-deal, ten-year sample can support.

6.6 Chapter Summary

This chapter has re-estimated the baseline probit specification of Chapter 5 under three robustness checks: separately by region (Table 5, testing H2), under a stricter definition of non-completion (Table 6), and with an alternative capital-adequacy proxy (Table 7). The central finding is one of qualified rather than uniform support for H1, and non-support for H2 in its strict form: Financial Leverage offers the most consistent, if not universally significant, evidence in the direction predicted by H1. The Tier 1 Capital Ratio result from Chapter 5 is real but concentrated in the United States sub-sample and specific to the regulatory capital measure; and the United States and European sub-samples produce materially different coefficient patterns rather than replicating one another. Chapter 7 concludes the thesis, discussing these findings' implications, the limitations of the analysis made explicit throughout Chapters 1 through 6, and directions for future research.

7. Conclusion

7.1 Introduction to the Chapter

This final chapter draws together the thesis's motivation, methodology, and findings, and closes with an honest assessment of what the analysis does and does not establish. Section 7.2 summarizes the thesis. Section 7.3 restates the empirical findings on H1 and H2 in full, integrating the results of Chapters 5 and 6. Section 7.4 revisits the thesis's contribution to the literature first outlined in Section 1.3. Section 7.5 discusses practical and policy implications. Section 7.6 sets out the limitations of the analysis, gathering points raised throughout Chapters 1 through 6 into a single, comprehensive discussion. Section 7.7 outlines directions for future research, and Section 7.8 offers brief concluding remarks.

7.2 Summary of the Thesis

This thesis sets out to test whether the accounting quality of a target bank, proxied by non-performing loans, loan loss provisions, reserve coverage, leverage, and regulatory capital adequacy, is associated with the completion status of an announced banking merger or acquisition. The motivation, developed

in Chapter 1, rests on a specific and, to the author's knowledge, previously untested combination of two well-established literatures: the extensive accounting literature on discretionary loan loss provisioning in banks (Beaver and Engel, 1996; Ahmed, Takeda, and Thomas, 1999; Kanagaretnam, Lobo, and Yang, 2004), and the theory of information asymmetry and its documented consequences for transaction design in the market for corporate control (Akerlof, 1970; Officer, Poulsen, and Stegemoller, 2009). Chapter 2 developed this theoretical link in full and formalized it into two hypotheses, H1, relating target accounting quality to deal completion, and H2, testing whether this relationship holds consistently across the United States and European sub-samples, while Chapter 3 established the institutional and regulatory context, in both jurisdictions, within which supervisory approval interacts with target financial condition.

Chapter 4 described the construction of an extraction of a 392-deal sample of banking M&A transactions announced between 2017 and 2026, with target-level accounting variables measured at the fiscal year preceding announcement and drawn from Bloomberg, and detailed the log, ratio, and winsorization transformations applied to the raw variables ahead of estimation. Chapters 5 and 6 estimated a probit model relating deal completion to this variable set, first on the pooled sample (Chapter 5) and then under three robustness checks: separately by region, under a stricter definition of non-completion, and with an alternative capital-adequacy proxy (Chapter 6).

7.3 Summary of Empirical Findings

The evidence assembled in Chapters 5 and 6 supports H1 only partially and does not support H2 in its strict form. Three findings, taken together, characterize the overall pattern of results.

First, Financial Leverage is the accounting-quality variable offering the most consistent support for H1: negatively and significantly related to deal completion in the pooled baseline specification (Section 5.4), correctly signed across every specification estimated in this thesis, though it loses statistical significance under the stricter non-completion definition and in both regional sub-samples individually (Section 6.2, Section 6.3), a pattern consistent with a real but modest underlying effect that requires the full pooled sample's statistical power to detect with confidence, rather than with the effect being spurious.

Second, non-performing loans, loan loss provisions, and the reserve ratio, the three variables most directly implementing the discretionary-provisioning channel that is the theoretical core of this thesis (Section 2.3.1), are directionally consistent with H1 in most, though not all, specifications, but are

not statistically significant in any of them. This thesis is explicit that this constitutes weak, rather than absent, evidence: a coefficient that is correctly signed but insignificant is not the same finding as a coefficient that is significant with the wrong sign, and Section 5.4 discussed the specific role that missing non-performing-loans data, and the resulting loss of roughly 38 percent of the working sample in the fullest specification, plausibly plays in this result.

Third, the Tier 1 Capital Ratio produced the single most statistically robust result in the thesis (Section 5.6), but with a sign opposite to the one predicted under H1, a finding this thesis chose to explain rather than suppress or explain away. Chapter 6 showed that this result is concentrated in the United States sub-sample (Section 6.2), is attenuated under a stricter non-completion definition (Section 6.3), and is specific to the regulatory Tier 1 measure rather than extending to a simpler book-value capital ratio (Section 6.4). Two theoretically grounded explanations were offered in Section 5.6: that weakly capitalized targets may be more motivated to see an acquisition through to completion as an alternative to supervisory intervention (Section 3.3.2), or that well-capitalized targets attract more competitive, and therefore more fragile, deal processes (Section 2.5.1). This thesis's data cannot adjudicate definitively between these two explanations, a limitation discussed further in Section 7.6.

H2, that the relationship documented in the pooled sample holds, in sign and significance, separately within the United States and Europe, is not supported. Section 6.2 documented three specific sign or significance reversals between the two sub-samples, and argued that this divergence is itself informative, consistent with the institutional differences between the U.S. and European bank merger approval frameworks developed in Chapter 3, and with the sharply different raw completion rates observed in the two regions (94.7% in the United States versus 69.9% in Europe, Section 4.5), which independently affects the statistical power available to estimate precise sub-sample coefficients.

7.4 Contribution to Literature

This thesis' contribution, first outlined in Section 1.3, can now be restated in light of the actual findings rather than only the ex-ante research design. The thesis connects the bank accounting-quality literature (Section 2.3) and the M&A deal-completion literature (Section 2.5) empirically, in a banking-specific setting where the theorized information-asymmetry mechanism (Akerlof, 1970; Officer, Poulsen, and Stegemoller, 2009) is unusually concretely measurable through the loan loss provision and related variables. The empirical result of this connection is not a clean confirmation of the hypothesized channel, but a more textured finding: partial, leverage-driven support for the general

proposition that target financial condition matters for deal completion, a striking but regionally concentrated and definition-sensitive capital-adequacy result that runs contrary to the simplest version of the capital management hypothesis (Ahmed, Takeda, and Thomas, 1999), and clear evidence against pooling the United States and European bank M&A markets without allowing for their distinct institutional architectures (Chapter 3). Each of these findings is, in its own way, informative for future work in this specific intersection of the literature, discussed further in Section 7.7.

7.5 Practical and Policy Implications

For acquirers and their advisors, the leverage result (Section 5.4) offers modest empirical support for treating target leverage as a due-diligence red flag associated with elevated deal-completion risk in banking M&A specifically, consistent with existing practitioner intuition but here given empirical grounding in an extracted sample rather than asserted based on case-study evidence alone. The regional asymmetry in the capital-adequacy result (Section 6.2, Section 6.5.1) carries a more specific implication: a due-diligence process calibrated on U.S. bank M&A experience may weight regulatory capital signals more heavily than is warranted for a European target, and vice versa for the scale-related variables that proved more informative in the European sub-sample.

For supervisors, the institutional discussion of Chapter 3 and the empirical finding that capital adequacy is significantly related to deal completion in the United States sub-sample together suggest that the explicit role of capital adequacy in the statutory and regulatory approval criteria described in Sections 3.2 and 3.3 is not merely a formal requirement but is reflected in which deals actually proceed to completion, though Section 5.6's discussion of the target-motivation explanation cautions against reading this as straightforwardly meaning that supervisors block weakly capitalized targets from being acquired; if anything, the evidence points toward the opposite reading in this sample. This distinction matters for how a supervisor might interpret a change in the completion rate of weakly capitalized-bank acquisitions over time.

7.6 Limitations

This thesis has been explicit throughout the limitations of its data and methodology at the point each

limitation became relevant; this section gathers those points into a single, comprehensive discussion, organized into four categories.

Sample limitations. The working sample is constructed from Bloomberg-covered banks and is therefore skewed toward larger, more established, and more often publicly listed institutions (Section 1.3); the very weakest targets, those at genuine risk of failure, may exit the population of voluntary announced deals entirely through the resolution mechanisms discussed in Section 3.3.2, before ever appearing in a dataset of this kind. The sample is also numerically dominated by United States targets (Section 4.5), and the completion-rate imbalance between the two regions (94.7% versus 69.9%) independently limits the statistical power available to estimate precise, well-identified coefficients within the United States sub-sample specifically (Section 6.2).

Variable construction limitations. Non-performing loans could not be expressed as a ratio of total loans, since Bloomberg's Reserve for Loan Losses / Total Loans field does not expose the underlying total loans figure separately (Section 4.6), and was instead log-transformed; this, combined with substantial missingness in the underlying Bloomberg field, materially reduces the estimation sample whenever it is included (Section 5.3). Non-monetary currency conversion (Section 4.2) relies on annual average exchange rates rather than transaction-date-specific rates, introducing a degree of approximation into cross-country monetary comparisons that a more resource-intensive, transaction-level FX matching exercise could in principle reduce. Four Icelandic-krona-denominated deals could not be converted to USD at all and are excluded from any specification using monetary variables.

Measurement and identification limitations. The accounting-quality proxies used in this thesis are level-based rather than derived from an estimated discretionary-provision residual, a deliberate choice explained and defended in Section 2.3.2, but one that does not distinguish, at the level of an individual observation, between a target with genuinely poor credit quality and one with aggressive, discretion-driven under provisioning. The collapsing of Bloomberg's Withdrawn and Terminated categories into a single non-completion outcome in the baseline specification (Section 4.3), and the sensitivity of the results to this choice documented in Section 6.3, indicate that the dependent variable itself is measured with some irreducible ambiguity given the sample size available. More generally, this thesis identifies associations, not causal effects: the probit specification of Section 4.8 does not rule out the possibility that both weak accounting quality and deal non-completion are jointly driven by an omitted third factor, for instance, unobserved target distress along dimensions the sample's controls do not fully capture, a concern partially, but not fully, addressed by the size, profitability, and payment-type controls included in the specification (Section 2.5.1).

Scope limitations. Following the thesis supervisor's guidance (Chapter 4), this thesis does not construct an accounting-based bank stability index (Z-score) or a market-based Distance-to-Default measure, both of which were raised in early discussions with the supervisor as illustrative examples of alternative earnings-quality proxies rather than as required components of the analysis; a Z-score of the kind originally discussed would in any case have required a multi-year panel of target-level data that this thesis's single-fiscal-year (t-1) data collection strategy does not provide (Section 1.3), and a Distance-to-Default measure would have been feasible only for the sub-sample of publicly listed targets with available equity market data, a further sample restriction this thesis's timeline did not permit exploring.

7.7 Directions for Future Research

Four extensions follow directly from the limitations discussed in Section 7.6 and are noted here as concrete directions for research building on this thesis rather than as generic suggestions. First, extending the data collection to a multi-year panel per target, rather than the single pre-announcement fiscal year used in this thesis, would allow both a formally estimated discretionary loan loss provision residual, of the kind reviewed in Section 2.3.2, and an accounting-based stability index such as the Z-score originally discussed with the thesis supervisor, directly addressing the scope limitation noted above. Second, restricting attention to the sub-sample of publicly listed targets would allow a market-based Distance-to-Default measure to be constructed and compared against the accounting-based proxies used in this thesis, testing directly whether the two approaches yield consistent evidence, in the spirit of the robustness logic the supervisor originally proposed. Third, the regional divergence documented in Chapter 6 could be investigated more directly with an interaction-term specification, interacting each accounting-quality variable with a United States indicator in a single pooled model, rather than estimating fully separate sub-sample models, which would allow a formal statistical test of whether the cross-regional differences in Table 5 are themselves statistically significant, something this thesis's separate-sample approach does not directly establish. Fourth, incorporating deal-level information not available in this thesis's dataset, specifically, whether a transaction attracted competing bids, and the precise timeline of prudential and antitrust review discussed in Section 3.4.1, would allow the alternative explanations for deal non-completion discussed in Section 2.5.1 to be tested directly rather than addressed only through control variables and qualitative discussion.

7.8 Concluding Remarks

This thesis sets out to test a specific, previously unexamined link between bank accounting quality and the completion of banking mergers and acquisitions. The evidence it produces is neither a clean confirmation nor a clean rejection of that link: leverage behaves as theory predicts: several accounting-quality variables point in the theoretically predicted direction without reaching conventional statistical significance; the most statistically robust result runs contrary to the simplest theoretical prediction and required a genuine economic explanation rather than a mechanical one; and the relationship does not hold uniformly across the United States and Europe, in a pattern that is itself consistent with the institutional differences this thesis documented in detail. Reporting this mixed pattern honestly, rather than emphasizing only the results consistent with the initial hypotheses, is itself the methodological standard this thesis has tried to hold itself to throughout and is, ultimately, a more useful and more credible contribution to the literature than a tidier but less faithful account of what an extraction of a 392-deal sample of banking M&A transactions actually shows.

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Board of Governors of the Federal Reserve System. H.10/G.5A: Foreign Exchange Rates. Federal Reserve statistical release.

U.S. Internal Revenue Service. Yearly Average Currency Exchange Rates. [irs.gov](https://www.irs.gov).

Appendix: R code

This appendix presents the complete R code used to estimate the probit models in Chapters 5 and 6, executed in R via RStudio. The underlying dataset (target-level accounting variables, already log-transformed, ratio-converted, and winsorized as described in Section 4.6) is provided as a single CSV file. It was generated from the Bloomberg-sourced Excel workbook described in Chapter 4 using Python for data cleaning and transformation, then imported into R for estimation. The code is organized into four parts, corresponding to Table 4 (Chapter 5) and Tables 5, 6, and 7 (Chapter 6).

A.1 Data Import and Preparation

```
library(readxl)

# Import the pre-cleaned, pre-transformed dataset
data <- read_excel("MA_Thesis_Regression.xlsx")

# Clean column names (spaces --> dots)
names(data) <- make.names(names(data))

# Collapse the rare 'Cash or Stock' payment-type category into
# 'Undisclosed' to avoid quasi-complete separation (Section 4.2.1)
data$PaymentType_clean <- ifelse(
  data$Payment.Type %in% c("Cash or Stock"),
  "Undisclosed", data$Payment.Type
)

# Further binary payment-type control used in Chapter 6
# sub-sample and robustness specifications
data$PaymentType_binary <- ifelse(
  data$Payment.Type == "Cash", "Cash", "Non-Cash"
)

# Region indicator for the H2 sub-sample test
```

```
data$Region <- ifelse(
  data$Target.Country == "United States",
  "United States", "Europe"
)
```

A.2 Table 4 — Baseline Probit Models (Chapter 5)

```
# Model A: full specification including ln(NPL)
model_A <- glm(
  Completed ~ ln_NPL_w + ReserveRatio_w + LLP_mm_w +
    FinancialLeverage_w + Tier1_w + ROE_w +
    ln_TotalAssets_w + ln_DealValue_w +
    factor(PaymentType_clean),
  data = data, family = binomial(link = "probit")
)
summary(model_A)

# Model B: excludes ln(NPL) to preserve estimation sample
model_B <- glm(
  Completed ~ ReserveRatio_w + LLP_mm_w +
    FinancialLeverage_w + Tier1_w + ROE_w +
    ln_TotalAssets_w + ln_DealValue_w +
    factor(PaymentType_clean),
  data = data, family = binomial(link = "probit")
)
summary(model_B)

# McFadden pseudo-R2 for both models
pseudoR2 <- function(m) 1 - (m$deviance / m$null.deviance)
```

```
pseudoR2(model_A); pseudoR2(model_B)
```

A.3 Table 5 — United States vs. Europe Sub-Samples (Chapter 6, H2)

```
formula_noNPL <- Completed ~ ReserveRatio_w + LLP_mm_w +  
  FinancialLeverage_w + Tier1_w + ROE_w +  
  ln_TotalAssets_w + ln_DealValue_w +  
  factor(PaymentType_binary)  
  
model_pooled <- glm(formula_noNPL, data = data,  
  family = binomial(link = "probit"))  
  
model_US <- glm(formula_noNPL,  
  data = subset(data, Region == "United States"),  
  family = binomial(link = "probit"))  
  
model_EU <- glm(formula_noNPL,  
  data = subset(data, Region == "Europe"),  
  family = binomial(link = "probit"))  
  
summary(model_pooled); summary(model_US); summary(model_EU)  
  
# Multicollinearity check (Section 6.2.1)  
library(car)  
vif(model_pooled)
```

A.4 Table 6 — Stricter Definition of Non-Completion (Chapter 6)

```
# Exclude Withdrawn deals; compare Completed vs. Terminated only  
data_strict <- subset(data, Deal.Status != "Withdrawn")
```

```

model_strict <- glm(formula_noNPL, data = data_strict,
                    family = binomial(link = "probit"))
summary(model_strict)

```

A.5 Table 7 — Alternative Capital-Adequacy Proxy (Chapter 6)

Substitute Equity/Total Assets for the Tier 1 Capital Ratio

```

formula_altcap <- Completed ~ ReserveRatio_w + LLP_mm_w +
  FinancialLeverage_w + EquityAssets_w + ROE_w +
  ln_TotalAssets_w + ln_DealValue_w +
  factor(PaymentType_binary)

```

```

model_altcap <- glm(formula_altcap, data = data,
                    family = binomial(link = "probit"))
summary(model_altcap)

```