# Deposit Insurance and Discretion in Loan Loss Provisioning

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**Abstract**
Deposit insurance (DI) incentivizes bank risk-taking and regulatory scrutiny. Through both mechanisms, it should affect bank accounting. This paper studies how a recent, substantial expansion in U.S. DI coverage impacts a key accounting policy: discretion in banks' loan loss provision (LLP). We find strong, consistent evidence that affected banks post higher discretionary LLP, suggesting a capital-reducing or conservative bent. Results are strongest for those most exposed to the DI increase, those that increase risk most, and those subject to the most regulatory scrutiny. Our study is the first to show a direct impact of DI on bank accounting policy.

**Keywords:** Deposit Insurance, Accounting Discretion, Conservatism, Loan Loss Provision, Emergency Economic Stabilization Act
**JEL Codes:** G18, G21, G38, M43
**Data Availability:** Data are available from the public sources cited in the text.

## I. INTRODUCTION

Deposit insurance (DI) is an integral feature of nearly 150 countries' banking systems.[1] It protects investors by converting their deposits into a risk-free asset. As depositors' stake in the bank shifts to the deposit insurer, they have less incentive to monitor banks. Less depositor scrutiny pares the risk of bank runs, liquidity crises in which depositors push banks toward failure by withdrawing large sums (Diamond and Dybvig, 1983), but also allows banks to take more risk (Calomiris and Jaremski, 2019). Because of its ubiquity and weighty consequences, DI's effect on banking sector stability has been studied extensively.[2] However, no paper we know of explores its impact on accounting policy. This is surprising because managers set accounting policies with stakeholder preferences in mind. By handing uninsured depositors' stake to the deposit insurer and by encouraging bank risk-taking, DI can change preferences of the marginal stakeholder and, thus, influence accounting. Our paper shows for the first time how U.S. federal DI, a contested form of government intervention, impacts bank accounting: It leads bankers to exercise conservative discretion in recognizing loan losses.

The last major change in U.S. DI was activated by the Emergency Economic Stabilization Act of 2008 (EESA), which increased the DI ceiling from $100,000 per account to $250,000. Lambert et al. (2017) estimate that EESA insured an additional $500 billion in deposits. As a sharp, unanticipated shock, EESA offers an ideal setting to study the relationship between DI and accounting policy. Nearly all U.S. banks were affected. However, 126 Massachusetts state-chartered savings and cooperative banks were fully insured by state-run DI schemes initiated in

[1] https://www.iadi.org/en/deposit-insurance-systems/dis-worldwide/
[2] See, for example, Ronn and Verma (1986), Martinez-Peria and Schmukler (2001), Demirguc-Kunt and Detragiache (2002), Gropp and Vesala (2004), Anginer et al. (2014), or Calomiris and Jaremski (2019).

the 1930s (on top of FDIC insurance). These institutions serve as natural controls, against which we compare banks affected by EESA. Because assignment into treated and control groups occurs when banks incorporate – sometimes 200 years before EESA – our setting offers the plausible exogeneity needed for difference-in-differences (DID) estimation. So that treated banks better resemble controls, we reduce observable differences through propensity score matching. DID estimation mutes the impact of potential confounders like the financial crisis if they affect treated and control groups similarly.

The accounting policy we focus on is loan loss provision (LLP), banks' largest accrual by far (Beatty and Liao, 2014). U.S. Generally Accepted Accounting Principles require banks to recognize future loan loss if it is probable that the loans cannot be collected. Current accounting standards provide guidance for loss provisioning, but managers retain significant discretion in determining whether and when to recognize losses (SFAS 5 and 114).[3] Therefore, managerial discretion plays an integral role in banks' financial reporting decisions. Our paper studies whether exposure to the DI shock affects the magnitude and direction of managers' LLP discretion.

To measure LLP discretion, we model LLP as a function of its determinants, adopting an explanatory model from recent literature. Residuals capture the unexplained or discretionary component of LLP. We track the absolute and signed values of those residuals. A higher (lower) absolute value implies a greater (smaller) use of LLP discretion. Notably, discretion can be used for different purposes. Within regulatory boundaries, managers may underestimate loan losses and consequently under-accrue for them. We consider such discretion 'opportunistic' because it

[3] Statement of Financial Accounting Standards (SFAS) No. 5 Accounting for Contingencies: https://www.fasb.org/jsp/FASB/Document_C/DocumentPage?cid=1218220126761&acceptedDisclaimer=true
SFAS No. 114 Accounting by Creditors for Impairment of a Loan: https://www.fasb.org/jsp/FASB/Document_C/DocumentPage?cid=1218220123941&acceptedDisclaimer=true;
SFAS No. 114 Accounting by Creditors for Impairment of a Loan an amendment of FASB Statements No. 5 and 15: https://www.fasb.org/jsp/FASB/Document_C/DocumentPage?cid=1218220128771&acceptedDisclaimer=true

inflates earnings and capital. Conversely, managers may overestimate losses and book more LLP, eroding earnings and capital. Doing so equates to 'conservative LLP discretion'.[4] A more positive (negative) signed residual denotes more conservative (opportunistic) LLP discretion.

We expect DI to influence bank accounting through two channels. First, as discussed above, it increases bank risk-taking. Because risk amplifies earnings volatility, bankers now face greater incentives to manage earnings (Arya et al., 1998; Grant et al., 2009; Allini et al. 2025) and LLP discretion offers a natural tool to do so. Indeed, earnings management can be one explanation for the positive link between risk and LLP discretion found in prior research (Huizinga and Laeven, 2012; Bushman and Williams, 2012). Second, DI expands the deposit insurer's stake in the bank. Because regulators enforce the insurer's interests, the DI shock likely heightened regulatory scrutiny. Prior work describes regulatory compliance as a key motive for using LLP discretion (Ahmed et al., 1999; Altamuro and Beatty, 2010; Beatty and Liao, 2014; Dal Maso et al. 2018). Thus, we expect expanded DI coverage to increase LLP discretion. Furthermore, we expect the LLP discretion to be conservative (i.e., income- and capital-decreasing). The literature notes that risk compels stakeholders to demand conservative accounting (Beatty et al., 2008; Göx and Wagenhofer, 2009; Francis et al., 2013). Regulations appear to have a similar effect (Qiang, 2007; Lobo and Zhou, 2006).

[4] In our study, references to conservatism denote the conservative use of LLP discretion. That is, conservatism equates to income- and capital-decreasing actions. Such conservatism is related but not identical to the notion of "accounting conservatism" in the Basu (1997) sense, which reflects the asymmetry in the recognition of losses versus gains and is often measured using the Khan and Watts' (2009) C-Score. Lim et al. (2014) provide evidence that C-Scores and discretionary LLP are related. Our study does not focus on C-Score or other classic measures of accounting conservatism because they can only be computed for public banks, <5% of our sample.

Our baseline models suggest that DI does not affect the amount of LLP discretion banks use but leads them to shift toward more conservative discretion.[5] These results are internally consistent: The distribution of LLP discretion becomes more positive, shifting to the right. Lower negative values and higher positive ones cancel out, leading to more positive residuals but no significant change in their mean magnitude. In our sample, the mean bank-quarter's discretionary LLP is 14 basis points; our baseline findings suggest that roughly one quarter of that is attributable to DI. As further support of our baseline results, banks with the most deposits newly insured by EESA transition most toward conservative LLP discretion without changing overall levels of discretion. We verify results using several alternative specifications and placebo testing.

Next, we examine the hypothesized channels through which DI affects accounting: risk-taking and regulatory scrutiny. We subset banks by two proxies of risk, distance to default and nonperforming loans. Banks that increase risk most from the pre- to the post-shock period shift most toward conservative LLP discretion. They also increase LLP discretion the most, alluding to an effect that our baseline analysis may fail to detect. Next, we check for evidence of the regulatory scrutiny channel. The deposit insurer will likely care more about less capitalized banks, which face a greater default likelihood, and larger ones, which pose greater losses given default.[6] Because regulators enforce deposit insurer interests, we expect larger and less capitalized banks to experience more scrutiny. We find that the least capitalized and largest quintiles of banks increase conservative LLP discretion most, consistent with regulatory scrutiny as a driving mechanism. Our

[5] Although our baseline analysis detects no effect of DI on the amount of LLP discretion, several supplementary tests show evidence of a significantly positive relationship. To be conservative, we focus on our baseline results, which we believe to be the most econometrically sound.

[6] The largest quintile of banks in our matched sample is relatively small, with an average size of $927 million.

results are stronger for FDIC-supervised banks, where the insurer is the regulator and thus has the strongest incentives for diligence. Overall, both channels linking DI to accounting are supported.

To check external validity, we extend our tests into the larger (i.e., non-matched) set of banks. Beginning with all banks affected by EESA that meet our sample restrictions, we alternately designate 'treated' ones as the quintile most exposed to the DI shock, that increases risk most, and that is subject to the most regulatory scrutiny. All other banks serve as controls. In this broad sample, DI shock exposure, change in risk, and regulatory scrutiny explain both amount and direction of LLP discretion, as predicted.

Our analyses are carefully designed to alleviate the concern that the concurrent financial crisis, not the change in DI, leads treated and control banks to differentially change accounting policies. Preliminary panel regressions show that insured deposit levels relate to conservative LLP discretion even before designating banks as treated or control and when excluding the crisis years. Our baseline DID findings stem from a sample in which significant differences on a broad range of operational and structural factors are muted through matching. Chief among these are risk measures and, particularly, changes in risk from before to after the onset of the crisis. Ensuring that treated and control banks experience similar changes in risk alleviates concerns that the crisis affects them differently. Intensive margin analysis affirms that even when discarding control banks altogether, treated banks more affected by the shock shift more toward conservative LLP discretion than do other treated banks. Finally, a robustness test shows that when selecting treated banks to mimic the niche characteristics of the control sample as closely as possible, our findings continue to hold. This test increases the likelihood that, from a structural perspective, treated and control samples are poised to experience the crisis similarly. Although we cannot completely exclude the possibility that the financial crisis affects treated and control firms differently, we believe the extensive efforts described above greatly diminish that chance that such a differential impact could produce our main results, given they are consistent with other results across different samples and time periods.

Our contribution is to show that DI impacts bank accounting. In the vigorous debate around DI, scholars emphasize two effects: stabilizing bank liquidity (e.g. Diamond and Dybvig, 1983; Chernykh and Cole, 2011) and incentivizing bank risk-taking (e.g. Demirguc-Kunt and Huizinga, 2004; Calomiris and Jaremski, 2019). Our study informs this discussion by documenting two additional effects. We find some evidence that DI incentivizes more LLP discretion, which some researchers equate to opacity (Jiang et al., 2016; Yue et al., 2022). We also show robust evidence that DI leads to more conservative LLP discretion, consistent with Agarwal et al. (2014), Nicoletti (2018), Costello et al. (2019), Wheeler (2019), and Gallemore (2022), who associate regulatory actions with conservatism. On balance, our results concerning conservative LLP discretion suggests that the risk-taking behavior induced by DI is mitigated through the accounting channel; this is another consideration to regulators considering changes in DI.[7]

Prior research establishes a strong link between DI and bank risk-taking. Against this backdrop, our finding that DI associates with more conservative LLP discretion suggests that when banks take more risk, they recalibrate optimal disclosure toward conservatism. Such a move is to be expected because considerable theory and evidence connects increased risk with adverse consequences. For example, uncertainty dampens capital investments (Chortareas and Noikokyris, 2021; Doshi, Kumar, and Yerramilli, 2018) and increases cost of capital (Barth et al., 2013; Larson and Resutek, 2017). Strategic adjustments in various types of disclosures (including changes in

[7] Risk mitigation might occur in multiple channels. Our study indicates use of accounting policy. There may be other levers such as auditing (e.g., Brushwood et al. 2020; Krishnan and Zhang 2014).

conservatism) have been shown to mitigate consequences of this nature. An extensive literature finds that accounting conservatism, measured in diverse ways, has salutary effects including reducing cost of capital (Christensen et al., 2010; Garcia Lara et al., 2011; Li, 2015; Penman and Zhang, 2020). The literature also reports on complex regulatory dynamics such as the one we study, in which a regulatory action increases risk and therefore influences disclosure policy. Barth et al. (2017) find that the JOBS Act (like EESA, a regulatory intervention) contributes to coarsening the information environment and consequently firms find other ways to mitigate harm. This is similar to the disclosure response we find in the DI setting.

Because we study a change in DI, our paper relates to research linking accounting and deposit flows. Lo (2015) and McIntyre and Zhang (2020) show that uninsured deposit flows associate with bank transparency. Chen et al. (2022), meanwhile, shows that depositors respond more to bank performance in transparent banks. Jiang et al. (2022) find that deposit windfalls dissuade banks from certain voluntary disclosure whereas Wu (2022) finds that the same windfalls associate with timelier LLP. This literature connects to our study by tracing the interplay between bank accounting and depositor behavior. Unlike these papers, however, our focus is not on how accounting policies attract depositors or respond to deposit flows but how DI, a widespread and contentious regulatory policy, shapes bank accounting.

## II. HYPOTHESIS DEVELOPMENT

Uninsured depositors lose money if their bank defaults. This feature motivates them to monitor bank risk-taking and, if risks become excessive, discipline banks by withdrawing money, demanding higher deposit rate premiums, or both (Flannery, 1998; Goldberg and Hudgins, 2002; Bennett et al., 2015). By shielding depositors from loss, DI loosens this safety valve, allowing banks to take more risk. Empirical work consistently relates DI to more bank risk (e.g. Ioannidou and Penas, 2010; Calomiris and Jaremski, 2019).[8]

Greater risk should affect LLP discretion by increasing bankers' incentives to manage earnings. Because risk increases earnings volatility, riskier firms manage earnings more (Arya et al., 1998; Grant et al., 2009). In the banking sector, several studies confirm the use of LLP discretion to manage earnings (Kanagaretnam et al., 2004; Beck and Narayanamoorthy, 2013; Kilic et al., 2013). Consistent with this notion, Bushman and Williams (2012) and Huizinga and Laeven (2012) show that riskier banks use more accounting discretion.

Regulatory scrutiny poses another channel through which DI should impact LLP discretion. Bank regulators promote financial system stability and protect the FDIC's insurance fund via diligent supervision. By increasing insurer liabilities, EESA expands regulators' incentives for diligence as agents of the deposit insurer.[9] Prior research establishes regulatory compliance as a prime motive for bank LLP discretion (Kim and Kross, 1998; Ahmed et al., 1999; Huizinga and Laeven, 2012; Beatty and Liao, 2014).

To summarize, DI increases bank risk, which increases earnings volatility, which should increase banks' incentives to manage earnings, and consequently, use LLP discretion. It also expands the deposit insurer's liability, which likely leads to more supervisory diligence. To satisfy greater regulatory demands, banks may exercise more LLP discretion. These considerations inform our first hypothesis:

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[8] DI's effect on risk can be understood from an options pricing perspective, as well (Merton 1977).

[9] We coarsely test this conjecture using Barth, Caprio, and Levine's (2016) survey evidence on bank regulation for 180 countries (untabulated). We find a statistically significant cross-country correlation of 0.25 between "Supervisory Power," which measures whether bank supervisors have the authority to take specific actions to prevent and correct problems, and "Funding Insured," which measures the fraction of a country's deposits that are insured in an explicit DI scheme. This result suggests that regulators supervise banks more stringently when a larger fraction of deposits are insured.

**H1: Banks that experience increased DI coverage increase discretionary LLP relative to other banks.**

When firms become riskier, their stakeholders, particularly creditors, demand more conservative accounting (Beatty et al., 2008; Göx and Wagenhofer, 2009; Francis et al., 2013). Indeed, Zhang (2008), Nikolaev (2010), and Kravet (2014) associate debt with conservative accounting. By recognizing timely losses, firms enable better creditor monitoring. Kim et al. (2013) and Balakrishnan et al. (2016) posit that equity-holders also demand more conservative accounting in response to greater firm risk.

Bank managers can use LLP discretion to estimate losses more conservatively. Indeed, bank risk consistently associates with earnings- and capital-reducing LLP discretion (Delis et al., 2018; Kim et al., 2019; Kim et al., 2020; Danisewicz et al., 2021). Regulators have also been shown to prefer conservative accounting (Qiang, 2007; Lobo and Zhou, 2006). Therefore, we expect heighted regulatory scrutiny from the DI ceiling increase to compel banks toward more conservative LLP discretion. These considerations inform our second hypothesis:

**H2: Banks, which experience greater DI coverage, use more conservative LLP discretion, relative to other banks.**

## III. RESEARCH DESIGN

In this section, we introduce our variables of interest, empirical setting, and identification strategy. We then discuss our sample.

### 3.1 Discretionary LLP

Following a long stream of literature, we capture a bank-quarter's LLP discretion as the unexplained portion of its LLP. To measure it, we adapt Nicoletti's (2018) LLP prediction model to our setting. Her model builds on Beatty and Liao's (2014) suggested specification by including state and time fixed effects. We estimate the following model via ordinary least squares (OLS):

$$LLP_{b,t} = \alpha_1 DNPL_{b,t+1} + \alpha_2 DNPL_{b,t} + \alpha_3 DNPL_{b,t-1} + \alpha_4 DNPL_{b,t-2} + \alpha_5 EBLLP_{b,t}$$
$$+\alpha_6 TIER1_{b,t-1} + \alpha_7 LSIZE_{b,t-1} + \alpha_8 DLOAN_{b,t} + \mu_s + \tau_t + \epsilon_{b,t} \qquad (1)$$

where subscripts *b*, *t*, and *s* index the bank, quarter, and bank headquarter state, respectively. LLP is multiplied by 1000 so that coefficients can be better interpreted. Each unit, therefore, represents 10 basis points of the bank's lagged loan portfolio. *DNPL* captures changes in the quality of the underlying loan portfolio. Earnings before LLP and taxes (*EBLLP*) and the Tier 1 risk-based capital ratio (*TIER1*) capture earnings and capital management incentives, respectively. Bank size (*LSIZE*) and loan growth (*DLOAN*) provide key operational controls. Variables are defined further in Appendix A. We include quarter-fixed effects, $\tau_t$, to account for macroeconomic events common to all banks in a particular period and bank headquarter state-fixed effects, $\mu_s$, to absorb persistent regional differences. Appendix B, Column 1 reports coefficient estimates from this regression. Past, present, and future changes in nonperforming loans relate positively to LLP, as does profitability and size. Tier 1 capital and loan growth relate negatively.

Residuals from Equation (1) measure the discretionary component of LLP, with each unit, again, representing 10 basis points of the bank's lagged loan portfolio. From these residuals, we construct two variables. Their absolute value, *absDLLP*, captures the amount of LLP discretion that management exercises in a quarter; larger quantities denote more discretion. Their signed values, *DLLP*, capture how managers employ that discretion. Positive (negative) residuals denote greater (lower) LLP than the explanatory model predicts, consistent with conservative (opportunistic) LLP discretion.

### 3.2 Empirical Setting and Identification Strategy

On October 3, 2008, President Bush signed the Emergency Economic Stabilization Act (EESA) to combat the ongoing financial crisis. The Act was precipitated by catastrophic events such as the failure of a major investment bank (Lehman), the largest insurance company (AIG), and difficulties at other important financial institutions (Fannie Mae and Freddie Mac). Its main purpose was to arrest deteriorating credit market conditions. Section 136 of EESA increased the DI ceiling from $100,000 to $250,000 per account.[10] According to then-FDIC Chairman, Sheila Bair, the provision was meant to "help consumers maintain confidence in the banking system."[11] DI expansion was one tool in the EESA toolkit, which included troubled asset purchases, debt guarantees, and corporate governance reforms. Fein (2008) provides the context for this legislation and discusses its features.

Although EESA applied to deposit accounts at all FDIC-insured institutions, depositors of 126 Massachusetts state-chartered savings banks and cooperatives experienced no change in coverage. Their deposits were already fully insured through private Massachusetts DI schemes, the Depositors Insurance Fund (DIF) and Share Insurance Fund (SIF), respectively.[12] Both schemes were initiated during the Great Depression.[13] Like FDIC insurance, DIF/SIF insurance is risk-priced. Membership is mandatory for all Massachusetts savings banks and cooperatives just as FDIC insurance is required for all U.S. banks. Danisewicz et al. (2022) study Massachusetts state-

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[10] Initially set to expire in 2009, the protection was extended in May 2009 for another four years and made permanent by the Dodd-Frank Act in July 2010. Although it took a year and half to establish permanence, it would be politically inexpedient to offer broad protections and soon retract them. Moreover, lobbying for permanence began almost immediately (link below). Therefore, it is plausible that bankers and depositors quickly adjusted to the new limit https://www.reuters.com/article/financial-deposits/keeping-us-deposit-insurance-higher-gains-momentum-idUKLNE51200N20090203

[11] https://www.fdic.gov/news/news/press/2008/pr08093.html

[12] One may argue that our control banks also experienced a change in regulatory scrutiny. The FDIC deposit ceiling increased to $250,000 for these banks, as well. However, we note that total level of scrutiny – federal regulators tasked with protecting the FDIC insurance fund and Massachusetts state regulators tasked with protecting the DIF/SIF – did not change. Federal regulators were simply tasked with protecting a larger liability and state regulators a smaller one.

[13] In 2020, the Share Insurance Fund merged into the Depositors Insurance Fund.

chartered savings banks, concluding that the DIF provides sufficient oversight to reduce the potential moral hazard of unlimited insurance.

Not only were Massachusetts state-chartered savings banks and cooperatives insulated from the substantial DI increase, but their exclusion was exogenously pre-determined well before EESA.[14] The presence of an unaffected group of banks enables counterfactual analysis. We exploit this setting using difference-in-differences (DID) estimation. The main threat to validity stems from cross-sectional confounders: The control sample could be a poor estimate of how the treated sample would evolve absent the shock. In our setting, controls (Massachusetts state-chartered savings banks and cooperatives) differ from treated units (all other banks) in size, risk, capitalization, and many other factors. Because of these differences, EESA could plausibly affect the two groups heterogeneously.

We address this concern six ways. First, our primary tests propensity score match (PSM) on 22 variables to select subsamples of treated and control banks indistinguishable across observable traits. Similar observables increase the likelihood that the shock affects both samples comparably, beyond DI considerations. Second, we present evidence to support the parallel trend assumption, the notion that outcomes would evolve similarly for treated and control groups absent the DI increase. Third, in robustness analysis, we assemble a different subsample of treated banks that meet two of three conditions defining controls: (1) *non-Massachusetts* state-chartered savings banks or cooperatives; (2) Massachusetts *federally chartered* savings banks or cooperatives; and (3) Massachusetts state-chartered *commercial banks*. This alternate treated sample, which

[14] Some of the banks in our control group were incorporated in the early 1800s. Their choice to incorporate in MA as state-chartered savings banks could not have anticipated the state-run deposit insurance fund initiated in the Great Depression, let alone EESA. Even for subsequently incorporated control banks, the decision is likely exogenous to the 2008 shock, which followed an unforeseen financial crisis.

approximates the niche characteristics of our controls, provides identical inference as our baseline tests. We perform another test in which the treated sample is propensity score matched on an even larger set of bank characteristics and consistent results also hold. Fourth, intensive margin analysis on only treated banks shows that results are strongest for those most affected by the DI increase. By excluding controls altogether, we sidestep the issue of their peculiarity. Fifth, all of our tests include bank-fixed effects, and therefore rely on within-bank variation to identify the impact of DI on bank LLP discretion.[15] In doing so, estimates are shielded from many omitted factors like risk appetite or funding stability, which may differ across treated and control subsets (deHaan, 2021). Sixth, the relationship we find in our quasi-natural experiment also exists in basic panel tests, so they are unlikely to be an artifact of poorly matched treated and control samples.

A second threat to identification stems from our sample period, the financial crisis years. To explain our results, crisis-related confounders must not only affect only our baseline treated and control subsamples differently, but the two alternate treated and control subsamples described above, as well. They must also affect treated banks more exposed to the DI shock differently than treated banks less exposed to it, which is even more restrictive. Expanding our sample period offers the strongest backstop against this concern. Before presenting our DID estimates, we introduce panel regressions that associate the fraction of a bank's insured deposits with our outcome variables over a 75-quarter period. These associations preview our baseline estimates and continue to hold over a sample period excluding crisis years. Therefore, our results do not appear driven by the crisis. Additional analysis presented in Section 4.5 further assuages this concern.

### 3.3 Sample Selection and Descriptive Statistics

[15] Results of similar sign, significance, and magnitude obtain without bank-fixed effects.

Our baseline sample period extends from 4Q2005 to 4Q2011, twelve quarters before EESA to twelve quarters after. Data come mainly from the FDIC's Statistics on Depository Institutions, which reports financial information for all U.S. banks and thrifts. We supplement with several variables from the Census Bureau, Federal Housing Finance Agency, Treasury, and other FDIC datasets. To reduce survivorship bias, we begin with all banks that exist in both pre- and post-shocks periods. Those with 25 percent or more missing values for any variable in our study are discarded.[16] To avoid including unrepresentative banks, we eliminate those that, at any point in our sample period, report total assets below $25 million. We also drop banks that experience a quarterly change in total assets of 10 percent or more over our sample period; such changes likely reflect acquisitions, which skew operating performance. Both restrictions are common in the literature (e.g. Gatev and Strahan, 2006). Finally, we use PSM, detailed below, to select treated banks that resemble controls. Our final sample includes 7,738 (1,830) observations on 314 treated (74 control) banks. Table 1 summarizes these sample restrictions.

– INSERT TABLE 1 ABOUT HERE –

The variables used in our PSM reflect 22 operational, structural, and geographic characteristics. To measure scale and performance, we include the bank's logged assets, its equity-to-asset ratio, and return on assets before LLP and taxes (*LSIZE; EA; EBLLP*). To measure liquidity and funding structure, we include securities to assets (*SEC*), and core deposits, large deposits (>$100K), and demand deposits, each deflated by total deposits (*COREDEPOSIT; LGDEPOSIT; DMDDEPOSIT*). To measure loan portfolio structure, we include 1-4 family residential real estate loans, commercial and industrial loans, and commercial real estate loans,

[16] Given the paucity of control banks in our setting, our tests are susceptible to statistical power issues and outlier problems. To avoid losing more observations, we interpolate missing values via cubic spline function. Before doing so, we winsorize continuous variables at their 1% tails to mitigate the impact of outliers.

each deflated by total loans (*RRELOAN; CILOAN; CRELOAN*). To measure risk, we include logged Z-score (*LZSCORE;* the sum of equity-to-assets and income-to-assets divided by the five-period standard deviation of income-to-assets), LLP, nonperforming loans, and write-offs deflated by total loans (*LLP; NPL; WOFF*). To capture exposure to the 2007-2009 financial crisis, we include the change in each bank's average *LZSCORE* and *NPL* from the pre- to the post-shock period (*CHGLZSCORE; CHGNPA*).[17] To capture economic conditions of the bank's customers, we include the unemployment rate and logged housing price index for its main office county (*UNEMP; LHPI*). To capture regulatory and structural factors, we include four indicators, respectively set to one if the bank opted out of the Transaction Account Guarantee Program (*TAG OO*)[18], received Troubled Asset Relief Program (*TARP*) funds, is publicly traded (*PUBLIC*), and is owned by a bank holding company (HC). Appendix A defines these variables in more detail.

To execute the PSM, each variable is averaged over the pre-shock period (4Q2005-3Q2008) for each bank. An indicator flagging one for banks in the control sample is regressed on the averaged variables via logistic regression. Fitted values reflect probabilities of selection into the control group, based on observable characteristics. We match each control bank to treated ones with similar probabilities, selecting up to five treated matches for each control without replacement. To ensure match quality, we require treated and control bank propensity scores differ by 1 percent or less.[19]

[17] One might be concerned about matching on ex post information. We prefer this specification as it helps ward off a more serious concern – that the crisis affected treated and control banks differently. However, our results are identical if we exclude both change-in-risk variables from our PSM.

[18] The Troubled Asset Guarantee Program (TAGP) was implemented around the time of EESA. It included two components: the Temporary Liquidity Guarantee Program (TLGP), which fully insured noninterest-bearing transaction accounts, and the Debt Guarantee Program (DGP), which insured bank-issued debt. 1,099 banks, including 42 treated and nine control banks in our final sample, opted out of TAGP. Unlike the DI shock we explore, the TAG program was discontinued in 2010. For this reason and also because firms could select into the TAG program, we choose not to integrate it into our quasi-experiment.

[19] Our results hold for one-to-three matching within a 5 percent caliper as well as matching with replacement.

– INSERT TABLE 2 ABOUT HERE –

Table 2 summarizes PSM results. We begin with 3,722 treated and 95 control banks that meet our sample selection requirements. Two-sample t-tests identify significant pre-shock differences in 17 of 22 matching variables. For example, the average treated bank is 40 percent smaller and three times as profitable as the average control bank.[20] After matching, statistically significant differences dissipate across all variables except *EBLLP* and *LHPI*. The two-basis point difference in profitability and 0.02 log point difference in LHPI are both economically small. Importantly, the proportion of large deposits (>$100,000) to total deposits is nearly identical for both groups, at about one third; control banks did not have more large deposits just because they were fully insured, consistent with Danisewicz et al. (2022). Because our matched treated and control samples are very similar, macro-economic events around the time of the shock are likely to affect them similarly. For example, the U.S. was well into a mortgage crisis by 4Q2008, but because treated and control banks' average nonperforming loans that quarter were both 1 percent of total assets (unreported), it is unlikely that one group was significantly more exposed to the crisis. Likewise, TARP was implemented in 2008 but 2-3 percent of both groups opted into TARP. In sum, we expect confounding events to affect treated and control banks similarly.[21]

To further check the comparability between treated and control banks, we examine their pre-shock trends along our outcome variables. Panels A and B of Figure 1 respectively graph *absDLLP* and *DLLP* differences over our sample period. Differences are estimated by regressing *absDLLP*, and *DLLP* on the interaction between the treated and year dummies, omitting the year before the

[20] Exp(12.02)/Exp(12.53)-1=-40% and 0.30/0.10=300%

[21] We also examine the geographic distribution of matched treated banks (untabulated). By definition, controls are all in Massachusetts so it is reassuring that matched treated banks generally come from other Eastern states with large cities. Specifically, ten states contribute 64% of our treated sample: Pennsylvania, New Jersey, Illinois, Connecticut, New York, Ohio, Wisconsin, Massachusetts, Maryland, and Florida.

shock as the reference year. Specifications include bank and year fixed effects. Before the shock, both groups exercise similar levels of LLP discretion (Panel A) and exercise that discretion to adjust earnings by similar amounts (Panel B). Pre-shock similarity suggests that the two groups are reasonable counterfactuals. After the shock, the two groups continue to exercise similar amounts of LLP discretion (Panel A), but treated banks use discretion to inflate LLP, reducing income and capital (Panel B). Figure 1 suggests that the parallel trend assumption, crucial for DID validity, likely holds and that significant differences in *DLLP* emerge around the shock date.

– INSERT FIGURE 1 ABOUT HERE –

Table 3 describes our final sample. Our measure of LLP discretion, the difference between LLP and its predicted value, is around 14 basis points (=1.382/1000). On average, this discretion is used to increase income and capital by 5 basis points. The mean bank-quarter experiences a 7-basis point increase in nonperforming loans and a pre-LLP ROA of roughly a quarter of a percent. It holds almost a fifth of its risk-weighted assets in Tier-1 capital. It has around $261 million (=EXP(12.47)*1000) in assets and grows loans grow by 0.7 percent per quarter. Our sample is slightly larger, better capitalized, and more profitable than samples in related papers (e.g. Bushman and Williams, 2015 or Nicoletti, 2018) because we focus on a subset of the banking universe: Massachusetts state-chartered savings and cooperative banks and similar institutions.

– INSERT TABLE 3 ABOUT HERE –

To understand the distribution of our dependent variables, Figure 2 plots their average pre- to post-EESA changes for treatment and control groups. For each bank, we measure pre- to post-EESA change in *absDLLP* by subtracting average pre-EESA *absDLLP* from average post-EESA *absDLLP*. For all treated banks matched to the same control bank, differences are averaged once more to ensure the same number of treated and control observations. Panel A plots the distributions of these differences for treated (dashed line) and control (solid line) subsamples. Panel B does the same for *DLLP*. These figures preview our main findings in a univariate setting: Treated banks shift toward LLP discretion by 1.1 (≈ (1.2284-1.1231)*10) basis points more than control banks (Panel A) but they use that discretion to suppress LLP by about 3.6 (≈ (-0.5794 - (-0.8867))*10) basis points less than controls. In other words, they use LLP discretion more conservatively.

– INSERT FIGURE 2 ABOUT HERE –

## IV. EMPIRICAL ANALYSIS

In this section, we document the association between LLP discretion and DI over a broad sample. We then take strides toward establishing causality through our main empirical tests. We proceed to explore the intensive margin and the channels through which this relationship likely operates. Finally, we discuss measures taken to avoid endogeneity concerns.

### 4.1 Broad Sample Associations

To motivate our baseline analysis, we first assess the empirical relationship between DI and LLP discretion in a broad panel of bank-quarters. We include all banks that meet our sample selection criteria but extend the sample to 75 quarters centered on 4Q2008, to understand general relationships over a longer time-series. Over this sample of 123,310 observations, we estimate the following regression:

$$Y_{b,t} = \beta_1 INSDEP_{b,t} + \gamma' Controls + \mu_b + \tau_t + \epsilon_{b,t} \qquad (2)$$

where *b* and *t* index the bank and quarter, respectively. The dependent variable alternates between *absDLLP* and *DLLP*. *INSDEP* captures the fraction of a bank's deposits below the FDIC limit at the time. Chen et al. (2018) highlight potential bias from using residuals as dependent variables. We adopt their suggested solution by including all control variables from the first-stage, Equation

(1), in this second-stage regression. This specification includes bank- and quarter-fixed effects to be consistent with our main tests. Standard errors are clustered by bank due to the bank-specific, persistent nature of LLP (Nicoletti, 2018). The coefficient of interest is $\beta_1$, which measures the relationship between insured deposits and LLP discretion. Results are presented in Table 4.

-- INSERT TABLE 4 ABOUT HERE –

Column 1 shows that when a larger fraction of a bank's deposits is insured, that bank exercises more LLP discretion, in support of H1 and consistent with McIntyre and Zhang (2020). Column 2 shows that banks tend to use more income- and capital-decreasing discretion in periods of greater DI coverage, supporting H2. We also test whether the crisis period drives these results as LLP discretion has been shown to increase during the crisis (Huizinga and Laeven, 2012). Columns 3 and 4 exclude the crisis-years (2007-2010) and qualitatively similar results emerge. We conclude that the crisis cannot fully explain the relationships in Columns 1 and 2.

**4.2 Propensity Score Matched Difference in Differences**

The results above, though illustrative, suffer from endogeneity concerns. To whatever extent banks can cater to a wealthier deposit base, they can select into more insured deposits. To bypass this issue, we adopt the identification strategy described in Section 3.2: PSM DID with bank- and quarter-fixed effects. In Equation (2), we replace *INSDEP* with *TREATPOST*, an indicator equal to one for treated banks in every quarter after 3Q2008. Table 5 presents results.

-- INSERT TABLE 5 ABOUT HERE –

An insignificant $\beta_1$ in Column 1 implies that treated and control banks experience similar pre- to the post-EESA changes in the amount of LLP discretion. This result does not support H1. Column 2 shows that relative to controls, treated banks transition more toward conservative LLP discretion as they experience more DI. Consistent with H2, $\beta_1$ is highly significant (t-value of

2.769) and economically meaningful. The average bank-quarter's LLP in our sample is 8.88 basis points of its lagged loan portfolio (unreported). Column 2 suggests that the DI shock leads to 38 percent (=0.336/0.888) higher LLP levels, ceteris paribus. This result relates to Huang's (2021) main finding that deregulation led public banks to accept less conservative accounting from their borrowers. We show that under a likely more scrupulous regulatory regime, banks opt for more conservative LLP discretion.

Turning to control variables, *DNPL* correlates positively with *absDLLP* and negatively with *DLLP* when significant. Facing deteriorating loan portfolios, bankers exercise more LLP discretion (Column 1) to recognize fewer losses (Column 2), consistent with Huizinga and Laeven (2012). *EBLLP* relates positively to *absDLLP* and *DLLP*, consistent with higher earnings allowing for more LLP. *TIER1* does not load significantly in either column. Size predicts marginally less discretion and its less conservative application. Finally, loan growth curbs discretion but encourages its more conservative use.

The positive $\beta_1$ in Column 2 can have three, non-mutually exclusive explanations. When managers exercise conservative LLP discretion, they could exercise larger amounts of it; when they exercise opportunistic discretion, they could exercise smaller amounts; or they can become more likely to choose conservative over opportunistic discretion. In other words, results could reflect larger positive residuals, smaller negative ones, or greater likelihood of positive residuals. We test these explanations in Columns 3-5. In Column 3 (4), the dependent variable is *absDLLP* and the sample includes only observations with positive (negative) residuals; in Column 5, the dependent variable is an indicator equal to one if the residual is positive, zero otherwise, and we use the full sample.[22]

Columns 3-5 support the second explanation: Relative to controls, when treated banks exercise discretion to reduce LLP, they do so by less after EESA (Column 4). The insignificantly positive coefficient in Column 3 weakly suggests DI-shocked bank managers use higher conservative LLP discretion, on average, than control bank managers; the insignificantly positive coefficient in Column 5 weakly suggests DI-shocked banks become more likely to exercise conservative discretion than control banks. The effects in Columns 3 and 5, although statistically insignificant, can combine to offset the significantly negative effect in Column 4 and explain why *absDLLP* does not change significantly (Column 1) while *DLLP* increases (Column 2).

### 4.3 Intensive Margin of LLP Discretion

We next explore DI's effect on the intensive margin of LLP discretion. If different outcomes between treated and control banks follow from greater DI coverage, then banks more exposed to the shock should increase LLP discretion more. To test this, we drop control banks from our sample, as their exposure to the shock is zero, by definition. For remaining banks, we construct a variable, newly insured deposits (*NIDEP*), equal to the fraction of 3Q2008 deposits in the \$100,000-\$250,000 range. Banks are sorted into *NIDEP* quintiles, with the highest quintile being most exposed to the shock.[23]

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[22] We choose a linear probability model over a logit or probit because of our fixed effects structure. Logit and probit models poorly accommodate nonlinearities (Wooldridge 2010).

[23] Although deposits in the \$100K-\$250K range were fully insured as of 4Q2008, banks continued to report according to the old, \$100K insurance threshold through 2Q2009. We adopt Lambert, Noth, and Schüwer's (2017) approach to estimating newly insured deposits, aware of this limitation. We subtract the value of 3Q2009 deposits above the \$250K DI ceiling from the value of 3Q2008 deposits above the \$100K DI ceiling. That amount, scaled by 3Q2008 total deposits, approximates the fraction of a bank's deposits exposed to the DI shock. We see no reason that the noise from this lagged reporting change should vary predictably between banks. Thus, our quintile designations should be unbiased.

Figure 3 illustrates how our dependent variables change from the pre- to the post-shock period for each quintile. Striped black (solid grey) bars denote average changes in *absDLLP* (*DLLP*). We report the average *NIDEP* for each quintile on the x-axis. This graph shows that *absDLLP* and *DLLP* are highest for the highest *NIDEP* quintile: Banks most exposed to the DI shock increase discretion the most and change LLP discretion least opportunistically.

-- INSERT FIGURE 3 ABOUT HERE –

To formalize this test, we create an indicator variable, *Q5NIDEPPOST*, that equals one for the highest quintile of newly insured deposits in the post-EESA period. We re-estimate Equation (2) after replacing *NIDEP* with *Q5NIDEPPOST*. One can consider this specification an alternative DID design, in which the control group (Q1-4) comprises banks whose DI coverage increased *less* than the treated group's (Q5) coverage.

Table 6 presents the results in Columns 1 and 2, not reporting covariates for brevity. An insignificant $\beta_1$ in Column 1 suggests that banks most exposed to the DI shock adjust LLP discretion similarly to other banks. In Column 2, a positive $\beta_1$ implies that banks most affected by the DI shock shift more toward conservative LLP discretion. Both results are consistent with Table 5, suggestion an effect of DI on the intensive margin of LLP discretion.[24]

– INSERT TABLE 6 ABOUT HERE –

### 4.4 Possible Mechanisms

Next, we explore the channels through which DI could affect LLP discretion. These tests resemble our preceding analysis but use different variables to partition banks into quintiles. The risk-taking channel predicts that our findings should be stronger for banks that increase risk more.

[24] Inferences are identical when designating the highest tercile and quartile as treated and when using a continuous measure of *NIDEP*.

We measure the change in risk as a bank's pre- to post-shock change in z-score and in nonperforming loans. Changes are computed by averaging each variable in each of the pre- and post-shock periods and taking the difference. In the first set of tests, we partition banks into quintiles by these change-in-risk variables. The regulatory scrutiny channel predicts that our results should be strongest for banks that matter most to the deposit insurer because regulators will focus limited attention on these banks. Less capitalized banks likely face greater regulatory scrutiny because a smaller buffer separates them from default and, therefore, insurance payout. Larger banks also probably matter more because, conditional on default, payout would be greater. Our second set of tests partitions banks into quintiles by average pre-shock *TIER1* and *LSIZE*.

Figure 4 presents univariate evidence on both channels. Panels A and B relate LLP discretion to changes in z-score and nonperforming loans, respectively. Both panels present strong, nearly linear trends suggesting that across the change-in-risk distribution, higher values are associated with stronger effects for *absDLLP* and *DLLP*. Panel C shows that the least capitalized quintile of banks reduce discretion by less than the next four quintiles. The cross-section of bank size (Panel D) presents somewhat mixed evidence as the middle quintile uses the most discretion and uses it most conservatively. Size may be a weaker proxy for regulatory scrutiny given that even banks in the largest quintile of size in our sample hold less than $1 billion in assets.

-- INSERT FIGURE 4 ABOUT HERE –

We confirm these results in multivariate tests. The remaining columns (3 to 10) of Table 6 show results from estimating a modified Equation (2), where *INSDEP* is replaced with indicators for the most risk-increasing quintiles, *Q5CHGRISKPOST*, (Q1 for change in z-score, Q5 for change in nonperforming loans) and the quintiles subject to the greatest regulatory scrutiny, *Q5REGSCRUTPOST*, (Q1 for capitalization, Q5 for size). Covariates are included but unreported for brevity. Columns 3-6 show that DI-shocked banks that increased risk most from the pre- to the post-shock period incrementally increase LLP discretion and its conservative use. These results support both H1 and H2. Columns 7-10 show that banks likely subject to the greatest regulatory scrutiny adjust the amount of LLP discretion no differently than other banks but pivot more toward conservative discretion, in support of H2.

In unreported analysis, we conduct another test of whether regulatory scrutiny explains the DI's effect on LLP discretion. The FDIC not only manages the DI fund but also supervises – i.e. acts as the regulator for – a substantial fraction of U.S. banks, roughly half of our PSM sample. As the deposit insurer, the FDIC has more incentive for scrupulous supervision and should thus exercise more diligence. If so, our results should be stronger for FDIC supervised banks, which is what we find. This test offers additional evidence that regulatory scrutiny is a likely mechanism.

One may wonder why banks that adopt riskier operations (a consequence of DI reducing depositor monitoring) would simultaneously select more conservative accounting policies. In fact, this paradox in our setting has precedent. Barth et al. (2017) show that when the JOBS Act increased information uncertainty for a newly created class of firms, these same firms adopted more voluntary disclosures to offset it. Manchiraju et al (2021) document that when Universal Demand Laws weaken investors' legal protections, allowing firms to pursue riskier operations, they responded by increasing conservative reporting. In our setting, EESA facilitated greater bank risk-taking (Lambert et al., 2017) and our focal firms also responded by shifting toward more conservative accounting. Although the data preclude us to definitively inferring banks' rationales, we find it unsurprising that firms would recalibrate their optimal accounting policy to smooth out negative consequences from greater risk-taking. This should be especially true when financial statement users include regulators, whose preference against excessive risk is well known (Qiang, 2007; Lobo and Zhou, 2006).

Overall, these tests contextualize our baseline results by tracing the channels through which DI affects bank accounting. Banks that experience two key outcomes from higher DI – more risk and greater regulatory scrutiny – adjust accounting policies accordingly.

### 4.5 Addressing the financial crisis as an alternate explanation

In this subsection, we highlight our extensive efforts to alleviate the concern that the financial crisis explains results. Table 4, Column 2 shows that a bank-quarter's level of insured deposits positively predicts its use of conservative LLP discretion. This result is highly significant; holds over a long period of time (1999-2018); and holds in a sample that deliberately excludes the 2007-2009 crisis (Table 4, Column 4). Because this relationship obtains without considering treated and control samples and outside the crisis years, it is unlikely that the crisis' heterogeneous impact on treated and control banks fully explains our results.

Our baseline methodology addresses the issue more comprehensively. To improve the odds that treated and control banks experience the crisis comparably, we match on a suite of over 20 covariates. These include multiple dimensions of risk (NPL, LLP, Z-score), economic environment (unemployment rate and HPI), deposit and loan portfolio structures, and other key attributes. After matching, none of the group differences are economically significant and all but two are statistically insignificant. By ensuring that both groups experienced similar changes in Z-score and NPL from the pre- to the post-shock period, we substantially reduce the odds that the crisis, not differential exposure to the DI shock, drives accounting policy changes.

Next, in Table 6, we exclude control banks altogether and, instead, identify treated banks that were most exposed to the DI shock, those with the largest fraction of their deposits the $100k-

$250k range. These are compared to all other banks in our treatment sample. While the differential impact of the DI shock on these two groups is obvious, there is no reason to expect that the crisis affects the two groups differently. Replicating our tests over this sample yields identical inferences.

Robustness analysis below presents a final safeguard against the crisis-related alternative explanation. We construct a third sample, matching via structural characteristics. This process ensures that the 'specialness' of our controls (i.e. Massachusetts state-chartered savings and cooperative banks) is replicated as closely as possible in the new treated sample. Doing so reduces confounding heterogeneity in bank structure. That is, if the crisis differentially affects savings banks or nationally chartered ones, the issue is muted in this sample. Again, results hold.

To summarize, the relationships we document hold in a broad panel of banks outside the crisis period. Matching on many observable dimensions, most notably pre-to-post-shock change in risk, reduces the chance that the crisis affects treated and control banks differentially. Tests on multiple samples that are, to various degrees, shielded from the crisis' potentially heterogeneous impact produce identical inference. In our view, these reasons, especially when considered together, render it far more likely that our results reflect DI than differential exposure to the crisis.

## V. ADDITIONAL ANALYSES

### 5.1 Robustness Checks

Table 7, Panel A presents results using three alternative measures of discretionary LLP instead of Nicoletti's (2018) adapted specification. The first two columns use Kanagaretnam, Krishnan, and Lobo's (2010) model, which additionally controls for loan loss reserves and the composition of banks' loan portfolios. Columns (3) and (4) employ an adapted Bushman and Williams' (2012) specification, which factors in changes in local economic conditions. Columns (5) and (6) use

Basu, Vitanza, and Wang's (2020) specification, which controls for charge offs and recoveries when estimating discretionary LLP as well as the asymmetric impact of nonperforming loan changes on LLP. We refer the reader to the original papers for these models' full rationale.

Re-estimating Equation (3) yields $\beta_1$ coefficients with the same sign as our baseline ones in five of six cases. The exception is Column 3, Bushman and Williams' (2012) *absDLLP* regression. Here, a marginally positive $\beta_1$ suggests that treated banks use more discretion, post-shock, providing another clue that DI may actually lead bankers to increase LLP discretion, despite our baseline results' insignificance.

– INSERT TABLE 7 ABOUT HERE –

We also test whether our results are sensitive to the particular subset of treated banks selected via PSM in Columns (7) and (8). To address this concern, we alternatively match controls to treated banks that share two of three defining characteristics. Recall that control banks are (1) Massachusetts-headquartered, (2) state-chartered, (3) savings or cooperative banks. For these tests, our revised sample of treated banks constitute:

1. *Non-Massachusetts*, state-chartered savings and cooperative banks,
2. Massachusetts, *federally-chartered* savings and cooperative banks, and
3. Massachusetts, state-chartered *commercial* banks.

Using this mixed counterfactual should improve internal validity (Roberts and Whited, 2013).

Caliendo and Kopeinig (2008) suggest that that PSM is more reliable with more covariates. In Columns (9) and (10), we augment our baseline PSM model by including twelve additional matching variables. Additional operational variables include cash/assets, measuring liquidity, and loans/assets, measuring the importance of lending to the bank; additional lending and deposit portfolio characteristics include loans to individuals/loans, certificates of deposit/deposits, savings deposits/deposits, and deposits from individuals and corporations/deposits; additional risk measures include deposits/loans, which coarsely proxies for interest rate risk, allowance for loan and lease losses/loans, which reflects credit risk, tier 1 capital/assets which measures capacity to absorb losses, and risk-weighted assets/assets, which measures overall asset risk; additional measures of crisis impact include pre-to-post-shock changes in the unemployment rate and log HPI. The cost of matching on more covariates is fewer matches. This procedure yields 161 (51) within the 1 percent caliper. In Table 7, we refer to the first sample as "Structural Match" and the expanded PSM as "Extra Variable PSM." For both dependent variables under both matching schemes (Columns 7-10), $\beta_1$ resembles estimates in the first two columns of Table 5. Overall, our baseline results remain robust to different discretionary LLP constructs and to different matching schemes.

**5.2 Placebo Tests**

Placebo tests provide our final internal validity check. If our results truly reflect the DI ceiling increase heterogeneously affecting treated and control banks, they should obtain only when comparing treated and control groups (or, as in Table 5, when comparing more and less treated banks) and only around the actual DI ceiling increase. We test the first condition by randomly matching our actual sample of treated banks to a placebo-control sample that also consists of treated banks. We then re-estimate Equation (3). We test the second condition by selecting placebo-event quarters 12 quarters before and 12 quarters after 4Q2008 and re-estimating Equation (3). Panel B of Table 7 shows that across all three placebo tests, insignificant relationships obtain between *TREATEDPOST* and both discretionary LLP measures.

**5.3 External Validity**

Finally, we explore our findings' external validity. Table 4 presents preliminary evidence that the relationship between insured deposits and *DLLP* exists over the broad universe of banks, not only for banks in our quasi-experiment. It also alludes to a relationship between insured deposits and *absDLLP*. However, these tests suffer more from endogeneity concerns.

To understand the effects of DI in a more representative sample of banks without the concerns of Table 4, we replicate Table 6 using the broad sample of banks that meet our sample criteria. Table 8 presents our findings. In Column 1 and 2, we test whether a bank's change in *absDLLP* and *DLLP* relates to its level of *NIDEP*. In Columns 3 and 4 (5 and 6), we test whether it relates to change in z-score (nonperforming loans). In Columns 7 and 8 (9 and 10), we test whether it relates to bank capitalization (size). Again, controls are unreported for brevity. Our results are likely robust if, in the broader sample, we see stronger effects in banks with more *NIDEP* and banks that change risk more and experience greater regulatory scrutiny.

– INSERT TABLE 8 ABOUT HERE –

Unlike our baseline analysis, results are significant for both *absDLLP* and *DLLP* in this specification. That is, DI appears to affect the amount of LLP discretion in the larger sample, supporting H1, as well as the direction. This presents more evidence than DI may affect LLP discretion overall despite our baseline analysis, which has lower statistical power.

## VI. CONCLUSION

Since its adoption by the U.S. in 1933, DI has spread around the world, becoming an integral feature of most banking systems. Domestically, EESA further entrenched DI by raising the insured deposit limit from $100,000 to $250,000. Given the banking sector's importance to market economies and DI's centrality to the banking ecosystem, research has studied various DI-related issues. To our knowledge, however, DI's impact on accounting policy has not been examined. This is surprising given that most analyses of DI focus on issues such as risk-taking and monitoring, to which financial reporting choices are central.

Our study uses EESA as a quasi-experiment to examine the effect of DI on accounting discretion. EESA diminished the role of uninsured depositors as bank monitors, often cited as a negative consequence of DI. Would the additional risk banks take in response to DI influence accounting? Would less depositor monitoring drive them to act opportunistically about accounting choices, or would more regulatory scrutiny constrain managerial discretion?

We explore these issues by focusing on the discretionary component of the most important bank accrual, the loan loss provision. We find some evidence that affected banks increase discretion and consistent, strong evidence that they exercise discretion more conservatively. That is, bankers subject to more DI use this important accrual to reduce income and capital. To the best of our knowledge, how DI affects bank financial reporting has not been discussed in prior research.

From the policy perspective, our results suggest that regulators in the U.S. provide the proper oversight and monitoring to limit banks' opportunistic reporting, especially when the moral hazard from DI incentivizes greater risk. In doing so, we echo Demirgüc-Kunt and Kane (2002), Gropp and Vesala (2004), and Danisewicz et al. (2022) in highlighting that effective regulation can overcome the problems associated with DI-induced moral hazard. Overall, our finding provides an important policy implication to accounting standard setters and bank regulators.

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**APPENDIX A: Variable Definitions.**

This table defines the variables in our analysis. Unless otherwise specified, variables come from the FDIC's statistics on depository institutions dataset; their names in that data are included in parentheses. All income statement items are annualized.

| Dependent Variables | Definitions |
|---|---|
| LLP | Loan loss provision (elnatr) times 1000 divided by lagged total loans (lnlsgr). |
| DLLP | Discretionary loan loss provisions measured as residuals from Nicoletti's (2018) adapted model: LLP(t) = f(EBLLP(t), DNPL(t+1), DNPL(t), DNPL(t-1), DNPL(t-2), DLOAN(t), TIER1(t-1), SIZE(t-1), quarter and state fixed effects). Variables from Nicoletti's model are defined below. |
| absDLLP | Absolute value of DLLP. |
| posDLLP | An indicator equal to one if DLLP is positive, 0 otherwise. |

| Independent Variables | Definitions |
|---|---|
| INSDEP | The fraction of a bank-quarter's deposits below the FDIC insurance limit at the time. Banks estimate this variable (depins). |
| TREATPOST | An indicator equal to one for observations in the treated sample after 3Q2008. Treated banks are defined as those propensity score matched to Massachusetts state-chartered savings and cooperative banks. |
| Q5NIDEPPOST | An indicator equal to one for banks in the highest quintile of newly insured deposits after 3Q2008. A bank's newly insured deposits are measured as the fraction of its deposits between $100K and $250K at 3Q2008. Because banks continued to report according to $100K insurance threshold up to 3Q2009, we adopt Lambert, Noth, and Schüwer's (2017) approach to estimating newly insured deposits, aware of this limitation. We subtract the value of 3Q2009 deposits above the $250K DI ceiling from the value of 3Q2008 deposits above the $100K DI ceiling. That amount, scaled by 3Q2008 total deposits, approximates the fraction of a bank's deposits that were newly insured. |
| Q5REGSCRUTPOST | An indicator equal to one for banks in the highest quintile of regulatory scrutiny after 3Q2008. We alternate between two measures of regulatory scrutiny, tier 1 capital (rbc1rwaj; less capitalized banks should incur greater scrutiny) and total assets (at; larger banks should incur greater scrutiny). |
| Q5INCRISKPOST | An indicator equal to one for banks in the highest quintile of pre- to post-EESA change in risk. We alternate between two measures of increased risk, change in z-score (banks whose z-scores decline more become riskier) and change in nonperforming loans (banks whose nonperforming loans ratios increase more become riskier). |

| Control Variables | Definitions |
|---|---|
| DNPL | Quarterly change in nonperforming loans (nclnls) divided by lagged total loans (lnlsgr). |
| EBLLP | Net income before extraordinary items (ibefxtr) minus LLP (elnatr) divided by lagged total loans (lnlsgr). |
| TIER1 | Tier 1 risk-adjusted capital ratio (rbc1rwaj). |
| LSIZE | Natural logarithm of total assets (at). |
| DLOAN | Quarterly change in total loans (lnlsgr) divided by lagged total loans (lnlsgr). |

**APPENDIX A (Continued)**

| Matching Variables | Definitions |
|---|---|
| EA | Equity capital (eq) divided by total assets (at). |
| SEC | Total securities (sc) divided by total assets (at). |
| COREDEPOSIT | Core deposits (coredep) divided by total deposits (dep). |
| LGDEPOSIT | Deposits above $100,000 (deplgamt) divided by total deposits (dep). |
| DMDDEPOSIT | Demand deposits (ddt) divided by total deposits (dep). |
| RRELOAN | 1-4 family real estate loans (lnreres) divided by total loans (lnlsgr). |
| CILOAN | Commercial and industrial loans (lnci) divided by total loans (lnlsgr). |
| CRELOAN | Commercial real estate loans (lnrenres) divided by total loans (lnlsgr). |
| LZSCORE | Natural logarithm of z-score, which is computed as the sum of net income (netinc) and equity capital (eq), each scaled by total assets (at), divided by the 12-quarter rolling standard deviation of net income (netinc). This is a measure of distance to default. |
| NPL | Nonperforming loans (nclnls) divided by total assets (at). |
| WOFF | Loan and lease charge-offs (ntlnls) divided by total loans (lnlsgr). |
| CHGLZSCORE | Change in average log z-score from the pre- to the post-shock period. This variable is computed by averaging LZSCORE in each of two periods, 4Q2005:3Q2008 and 4Q2009:4Q2011, and taking the difference between the two averages. |
| CHGNPL | Change in average NPL from the pre- to the post-shock period. This variable is computed by averaging NPL in each of two periods, 4Q2005:3Q2008 and 4Q2009:4Q2011, and taking the difference between the two averages. |
| UNEMP | Quarter-end unemployment rate for the county of the bank's headquarters. Source: Census Bureau. |
| LHPI | Quarter-end housing price index for the county of the bank's headquarters. Source: Federal Housing Finance Agency. |
| TAG OO | An indicator equal to 1 if the bank opted out of the FDIC's 2008 Transaction Account Guarantee Program, 0 otherwise. Source: FDIC. |
| TARP | An indicator equal to 1 if the bank received funds through the Capital Purchase Program, 0 otherwise. Source: U.S. Treasury. |
| PUBLIC | An indicator equal to 1 if the bank is publicly traded, 0 otherwise. Source: Federal Reserve Bank of New York |
| HC | An indicator equal to 1 if the bank is part of a holding company structure (hcmult), 0 otherwise. |

**APPENDIX B: Explanatory Model to Obtain Discretionary LLP**

| Variable | (1)<br>$LLP_t$ |
|---|---|
| $DNPL_{t+1}$ | 7.659*** |
| | (3.566) |
| $DNPL_t$ | 26.109*** |
| | (10.754) |
| $DNPL_{t-1}$ | 39.645*** |
| | (17.621) |
| $DNPL_{t-2}$ | 39.184*** |
| | (17.115) |
| $EBLLP_t$ | 40.999*** |
| | (4.406) |
| $TIER1_{t-1}$ | -3.665*** |
| | (-15.163) |
| $SIZE_{t-1}$ | 0.302*** |
| | (13.292) |
| $DLOAN_t$ | -15.381*** |
| | (-28.517) |
| | |
| Observations | 93,897 |
| Adjusted R-squared | 0.206 |
| Fixed Effects | State+Quarter |
| SE Clusters | Bank |

This table reports coefficients from an OLS regression of Equation (1). The dependent variable is LLP, loan loss provision. DNPL is the quarterly change in nonperforming loans Control variables include past, present, and future changes in nonperforming loans (*DNPL*)*,* earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (TIER1) are scaled by lagged total loans (risk-weighted assets). Control variables are scaled by lagged total loans except *TIER1*, which the scaled by risk-weighted assets, and *SIZE* and *DLOAN*, which are unadjusted. Column 1 is our prediction model. Residuals from this model are our dependent variables in subsequent regressions. Column 2 augments this model with the difference-in-differences variable, *TREATPOST*, an indicator set to one after 3Q2008 for all banks other than Massachusetts state-chartered savings and cooperative banks. Refer to Appendix A for further variable definitions. Models include state and quarter fixed effects. Standard errors are clustered by bank. *, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

# Figures and Tables

**Figure 1: Treated and control bank trends in absDLLP and DLLP.**

This figure plots coefficients and their 95% confidence intervals from regressions tracking average annual differences between treated and control banks' LLP discretion. The dependent variable is *absDLLP (DLLP*) in Panel A (B). The independent variables are an indicator, *TREAT* (equal to one for treated banks), year dummies, and these dummies' interaction with *TREAT*. The year before EESA's passage, 2007, is the reference year.

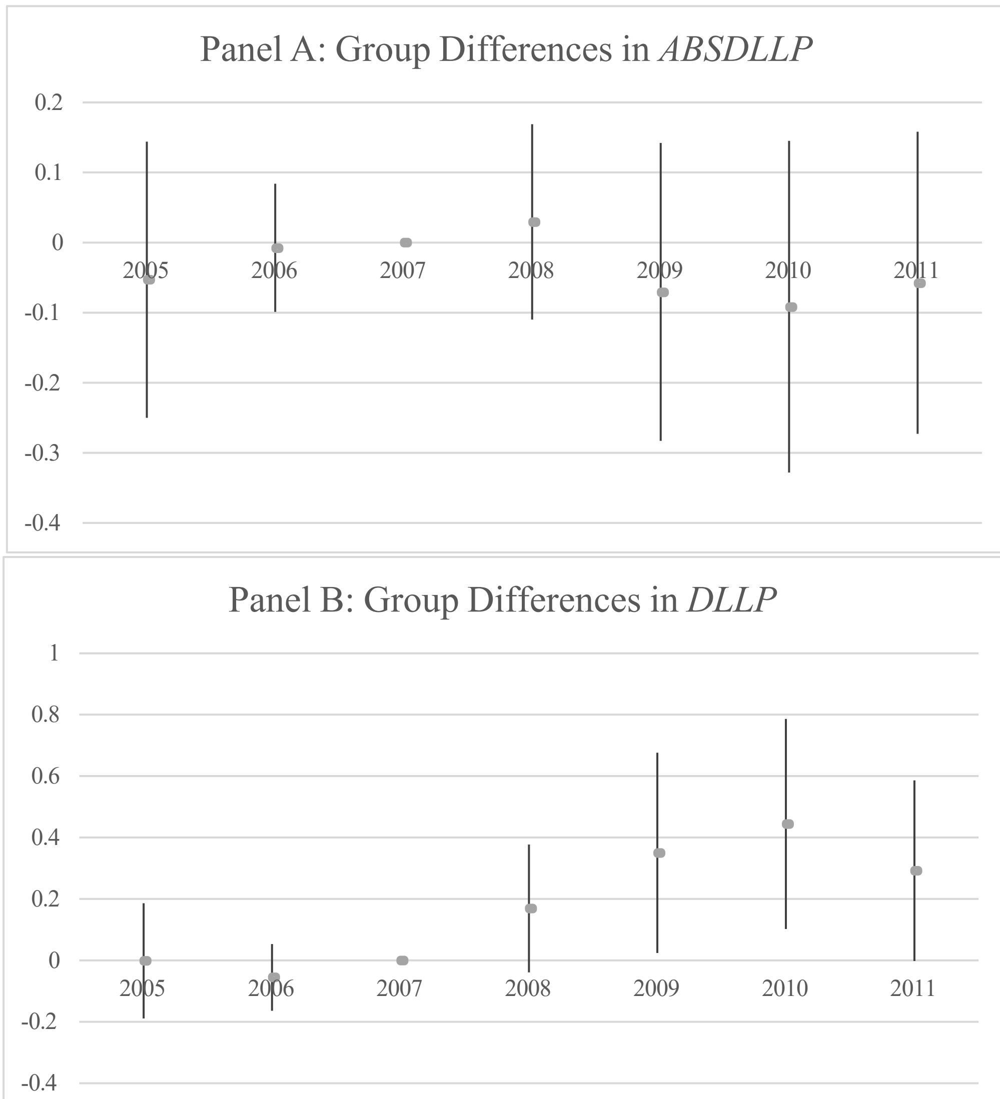

**Figure 2: Pre- to Post-EESA Changes in Outcome Variables by Treatment Status**

This figure plots the distribution pre- to post-EESA changes in *absDLLP* (Panel A) and *DLLP* (Panel B) for each bank in treated (dashed line) and control (solid line) subsamples. Mean values for each group are denoted with a vertical bar and labeled at the top.

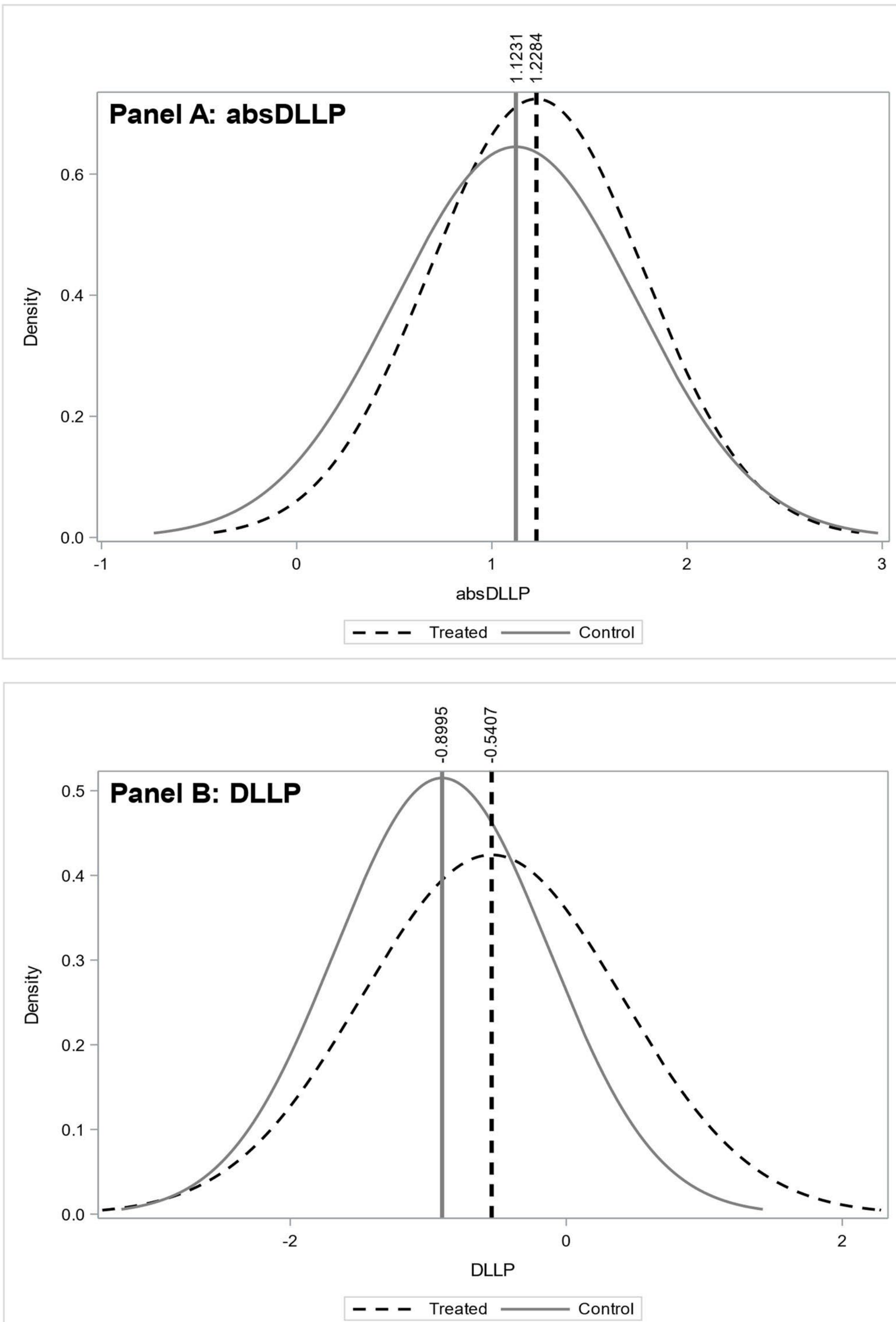

**Figure 3: Mean change in *absDLLP* and *DLLP* by newly insured deposit quintiles.**

This figure plots mean change in *absDLLP* (black striped bars) and *DLLP* (solid grey bars) from the pre- to the post-EESA period for banks in various quintiles of newly insured deposits. A bank's newly insured deposits are measured as the fraction of its deposits between $100K and $250K at 3Q2008. Appendix A details our methodology in estimating newly insured deposits. The y-axis measures the average change in *absDLLP* and *DLLP*; the x-axis shows the average level of newly insured deposits for banks in each quintile.

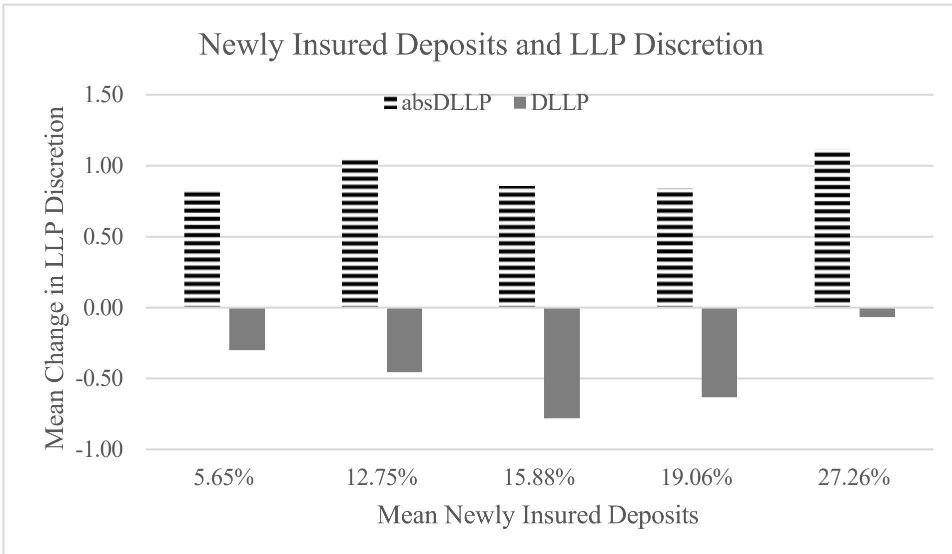

**Figure 4: Mean change in *absDLLP* and *DLLP* by change-in-risk and regulatory scrutiny quintiles.**

This figure plots mean change in *absDLLP* (black striped bars) and *DLLP* (solid grey bars) from the pre- to the post-EESA period for banks in various quintiles of four variables. Those variables include two measures of change in risk, change in z-score (Panel A) and change in nonperforming loans (Panel B), and two measures of regulatory scrutiny, Tier 1 capital (Panel C) and bank size (Panel D). The y-axis measures the average change in *absDLLP* and *DLLP*; the x-axis measures the average values of the partitioning variable for each quintile.

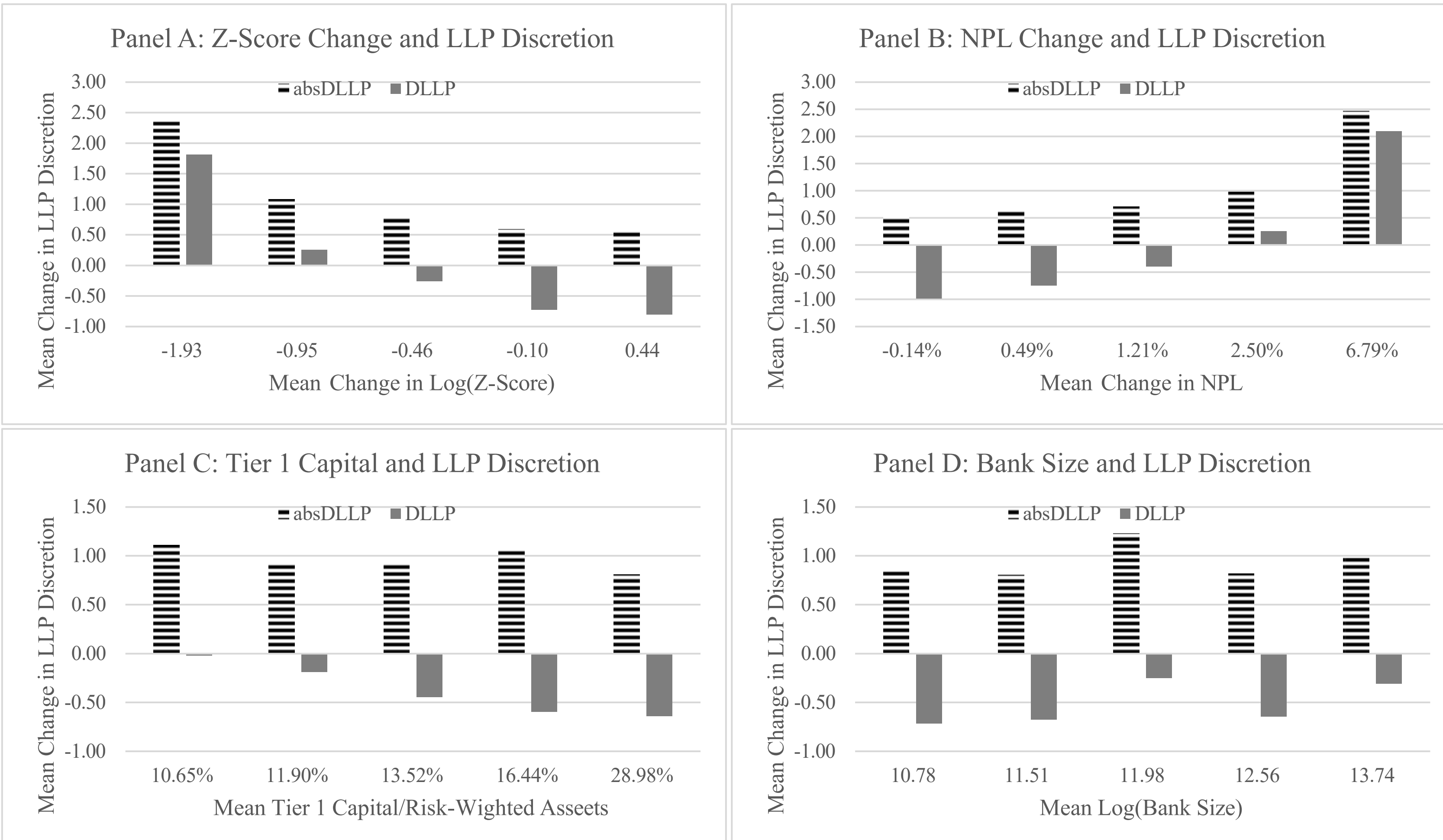

**Table 1: Sample Selection**

This table outlines our sample selection procedures. We begin with the full universe of bank-quarter observations in the FDIC's Statistics on Depository Institutions dataset between 4Q2005 and 4Q2011. Banks with 25 percent or more missing values for any variable in our study are dropped, as are those with under $25 million in total assets in any quarter within the sample period, and those that experience quarterly growth in assets of 10 percent or more, as this proxies for merger activity. The remaining banks in our control subsample are propensity score matched to those in the treated subsample.

| | Treated | | Control | | Total | |
|---|---|---|---|---|---|---|
| Condition | Banks | Obs. | Banks | Obs. | Banks | Obs. |
| Full sample, 4Q2005 - 4Q2011 | 7,915 | 191,576 | 124 | 3,061 | 8,039 | 194,637 |
| Less than 25% missing values | 7,847 | 190,087 | 124 | 3,061 | 7,971 | 193,148 |
| Over $25mm | 6,957 | 169,767 | 123 | 3,036 | 7,080 | 172,803 |
| No merger activity | 3,722 | 91,552 | 95 | 2,345 | 3,817 | 93,897 |
| Propensity score matched | 314 | 7,738 | 74 | 1,830 | 388 | 9,568 |

**Table 2: Propensity Score Matching**

This table summarize our propensity score matching procedure. Banks are matched along log total assets (*LSIZE*); book equity to assets (*EA*); earnings before loan loss provision (*EBLLP*); securities (SAV); core deposits (*COREDEPOSIT*); large deposits (*LGDEPOSIT*); demand deposits (*DMDDEPOSIT*); residential real estate loans (*RRELOAN*); commercial and industrial loans (*CILOAN*); commercial real estate loans (*CRELOAN*); the natural logarithm of z-score (*LZSCORE*); loan loss provision (*LLP*); nonperforming loans (*NPL*); write-offs (*WOFF*); pre-to-post-shock change in *NPA* (*CHGNPA*); ); pre-to-post-shock change in *LZSCORE* (*CHGZSCORE*); unemployment rate and log housing price index at the bank's main office county (*UNEMP*; *LHPI*); whether the bank opted out of the TAG program (*TAG OO*); whether it accepted TARP funds (*TARP*); whether it is publicly traded (*PUBLIC*); and whether it is owned by a holding company (*HC*). Refer to Appendix A for further variable definitions. The number of banks and their mean values for each variable are reported before and after matching as are p-values from two-tailed t-tests on mean differences. Continuous variables are winsorized at their 1 percent tails before matching.

| | Before Matching | | | | | After Matching | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| | Banks | | Means | | | Banks | | Means | | |
| Variable | Treatment | Control | Treatment | Control | p-value | Treatment | Control | Treatment | Control | p-value |
| LSIZE | 3,722 | 95 | 12.02 | 12.53 | (0.00) | 314 | 74 | 12.47 | 12.45 | (0.90) |
| EA | 3,722 | 95 | 10.69% | 10.66% | (0.93) | 314 | 74 | 11.05% | 10.69% | (0.43) |
| EBLLP | 3,722 | 95 | 0.30% | 0.10% | (0.00) | 314 | 74 | 0.13% | 0.11% | (0.04) |
| SAV | 3,722 | 95 | 21.64% | 22.81% | (0.31) | 314 | 74 | 22.35% | 21.33% | (0.51) |
| COREDEPOSIT | 3,722 | 95 | 81.47% | 81.30% | (0.76) | 314 | 74 | 82.14% | 81.67% | (0.54) |
| LGDEPOSIT | 3,722 | 95 | 34.66% | 33.75% | (0.24) | 314 | 74 | 31.80% | 32.94% | (0.32) |
| DMDDEPOSIT | 3,722 | 95 | 12.13% | 6.82% | (0.00) | 314 | 74 | 6.76% | 7.14% | (0.52) |
| RRELOAN | 3,722 | 95 | 34.19% | 67.68% | (0.00) | 314 | 74 | 64.92% | 65.73% | (0.74) |
| CILOAN | 3,722 | 95 | 12.38% | 3.79% | (0.00) | 314 | 74 | 4.23% | 4.10% | (0.79) |
| CRELOAN | 3,722 | 95 | 21.13% | 15.65% | (0.00) | 314 | 74 | 16.67% | 16.39% | (0.85) |
| LZSCORE | 3,722 | 95 | 4.99 | 5.32 | (0.00) | 314 | 74 | 5.34 | 5.27 | (0.37) |
| LLP | 3,722 | 95 | 0.05% | 0.02% | (0.00) | 314 | 74 | 0.02% | 0.02% | (0.35) |
| NPL | 3,722 | 95 | 0.98% | 0.52% | (0.00) | 314 | 74 | 0.62% | 0.61% | (0.93) |
| WOFF | 3,722 | 95 | 0.04% | 0.01% | (0.00) | 314 | 74 | 0.01% | 0.01% | (0.35) |
| CHGNPA | 3,722 | 95 | -0.64 | -0.84 | (0.05) | 314 | 74 | -0.72 | -0.77 | (0.62) |
| CHGLZSCORE | 3,722 | 95 | 2.34% | 1.50% | (0.00) | 314 | 74 | 1.67% | 1.62% | (0.87) |
| UNEMP | 3,722 | 95 | 4.97% | 4.62% | (0.00) | 314 | 74 | 4.73% | 4.69% | (0.70) |
| LHPI | 3,363 | 95 | 4.93 | 5.05 | (0.00) | 314 | 74 | 5.03 | 5.05 | (0.08) |
| TAG OO | 3,722 | 95 | 14.86% | 9.47% | (0.08) | 314 | 74 | 14.01% | 10.81% | (0.47) |
| TARP | 3,722 | 95 | 7.05% | 2.11% | (0.00) | 314 | 74 | 2.28% | 2.70% | (0.83) |
| PUBLIC | 3,722 | 95 | 5.84% | 4.21% | (0.50) | 314 | 74 | 4.43% | 5.41% | (0.71) |
| HC | 3,722 | 95 | 74.05% | 24.12% | (0.00) | 314 | 74 | 28.48% | 26.24% | (0.69) |

**Table 3: Descriptive Statistics**

This table describes the sample we use through our baseline analysis. Dependent variables include the absolute and signed values of discretionary loan loss provision (*absDLLP* and *DLLP*), measured as residuals from Equation (1). Control variables include nonperforming loans (*DNPL*), earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*LSIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (TIER1) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. All variables are winsorized at their 1 percent tails.

| | | Mean | St. Dev. | 25% | Median | 75% |
|---|---|---|---|---|---|---|
| Dependent Variables: | absDLLP | 1.382 | 1.776 | 0.477 | 1.004 | 1.765 |
| | DLLP | -0.533 | 2.186 | -1.526 | -0.749 | 0.014 |
| Control Variables: | DNPL | 0.07% | 0.64% | -0.11% | 0.00% | 0.21% |
| | EBLLP | 0.23% | 0.31% | 0.11% | 0.22% | 0.34% |
| | TIER1 | 18.84% | 9.25% | 12.50% | 16.04% | 21.67% |
| | LSIZE | 12.47 | 1.03 | 11.72 | 12.46 | 13.14 |
| | DLOAN | 0.70% | 2.92% | -1.08% | 0.54% | 2.32% |

### Table 4: DI and LLP Discretion

This table reports coefficients from OLS estimates of Equation (2). The dependent variable is the absolute value of discretionary loan loss provision in Columns 1 and 3 (*absDLLP*) and the signed value (*DLLP*) in Columns 2 and 4. *DLLP* is measured as the residual from Equation (1). For Columns 1 and 2, we include all banks that meet the size and merger restrictions over 75 quarters. For Columns 3 and 4, we exclude the crisis-years (2007-2010). *INSDEP* measures the fraction of a bank's deposits below the FDIC insurance limit in place at the time. Control variables include past, present, and future changes in nonperforming loans (*DNPL*), earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (TIER1) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. Continuous variables are winsorized at their 1 percent tails. Models include bank and quarter fixed effects. Standard errors are clustered by bank. *, **, *** denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

| | (1) | (2) | (3) | (4) |
|---|---|---|---|---|
| | Full Sample | | Sample excludes the crisis-years | |
| Variable | absDLLP | DLLP | absDLLP | DLLP |
| ***$INSDEP_t$*** | ***1.131**** | ***1.880**** | ***0.610**** | ***0.898**** |
| | ***(6.419)*** | ***(7.857)*** | ***(4.753)*** | ***(5.176)*** |
| $DNPL_{t+1}$ | -0.485 | -0.483 | -3.735*** | 1.380 |
| | (-0.409) | (-0.324) | (-4.093) | (1.205) |
| $DNPL_t$ | 5.499*** | -0.161 | -3.171*** | 1.584 |
| | (4.075) | (-0.095) | (-2.873) | (1.149) |
| $DNPL_{t-1}$ | 13.562*** | 0.259 | 3.024*** | 1.553 |
| | (10.144) | (0.159) | (2.783) | (1.173) |
| $DNPL_{t-2}$ | 14.714*** | -0.033 | 4.962*** | 1.274 |
| | (11.206) | (-0.020) | (5.105) | (1.046) |
| $EBLLP_t$ | 49.317*** | 23.756*** | 44.777*** | 15.073** |
| | (8.592) | (3.006) | (10.328) | (2.461) |
| $TIER1_{t-1}$ | -1.209*** | -0.398 | -1.019*** | -0.388* |
| | (-5.876) | (-1.405) | (-6.095) | (-1.710) |
| $LSIZE_{t-1}$ | 0.102* | 0.364*** | 0.018 | 0.143** |
| | (1.782) | (4.573) | (0.422) | (2.417) |
| $DLOAN_t$ | -5.876*** | 0.655** | -3.164*** | 0.234 |
| | (-24.834) | (2.158) | (-19.818) | (1.130) |
| Observations | 123,310 | 123,310 | 95,584 | 95,584 |
| Adjusted R-squared | 0.241 | 0.102 | 0.205 | 0.123 |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| SE Clusters | Bank | Bank | Bank | Bank |

**Table 5: Change in LLP Discretion around DI Ceiling Increase**

This table reports coefficients from OLS estimates of a modified Equation (2), where the independent variables is replaced with *TREATEDPOST*, an indicator equal to one for all treated banks after 3Q2008. Treated banks are defined as those propensity score matched to Massachusetts state-chartered savings and cooperative banks (controls). The dependent variable is the absolute value of discretionary loan loss provision (*absDLLP*) in Columns 1, 3, and 4; the signed value (*DLLP*) in Column 2; and an indicator equal to one for positive *DLLP* values in Column 5. Columns 1, 2, and 5 use the full sample whereas Column 3 (4) includes only observations with positive (negative) *DLLP*. *DLLP* is measured as the residual from Equation (1). Control variables include past, present, and future changes in nonperforming loans (*DNPL*), earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (TIER1) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. Continuous variables are winsorized at their 1 percent tails. Models include bank and quarter fixed effects. Standard errors are clustered by bank. *, **, *** denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

| | (1) | (2) | (3) | (4) | (5) |
|---|---|---|---|---|---|
| | Full Sample | Full Sample | DLLP>0 | DLLP<0 | Full Sample |
| Variable | absDLLP | DLLP | absDLLP | absDLLP | posDLLP |
| ***TREATPOST$_t$*** | ***-0.068*** | ***0.336**** | ***0.212*** | ***-0.146**** | ***0.012*** |
| | ***(-0.836)*** | ***(2.769)*** | ***(0.517)*** | ***(-3.413)*** | ***(0.411)*** |
| DNPL$_{t+1}$ | -4.649 | -18.601*** | -8.314 | 6.053*** | -3.096*** |
| | (-1.205) | (-3.708) | (-0.857) | (4.539) | (-4.376) |
| DNPL$_t$ | 8.831 | -17.180** | 7.282 | 18.448*** | -4.067*** |
| | (1.625) | (-2.446) | (0.533) | (12.342) | (-5.206) |
| DNPL$_{t-1}$ | 19.974*** | -18.571*** | 26.321** | 26.604*** | -4.671*** |
| | (4.015) | (-2.897) | (2.101) | (17.036) | (-6.651) |
| DNPL$_{t-2}$ | 29.237*** | -8.557 | 57.867*** | 26.741*** | -4.863*** |
| | (6.252) | (-1.299) | (4.511) | (17.289) | (-6.572) |
| EBLLP$_t$ | 50.326** | 50.004* | 94.561** | 11.688** | 5.504* |
| | (2.485) | (1.679) | (2.547) | (2.256) | (1.722) |
| TIER1$_{t-1}$ | -1.818 | 0.269 | -5.835 | -3.229*** | 1.808*** |
| | (-1.266) | (0.153) | (-1.520) | (-5.429) | (5.128) |
| SIZE$_{t-1}$ | -0.574* | -1.517*** | -2.871*** | 0.334** | -0.308*** |
| | (-1.945) | (-4.159) | (-2.936) | (2.270) | (-3.777) |
| DLOAN$_t$ | -10.187*** | 9.726*** | -12.934*** | -13.866*** | 3.972*** |
| | (-10.354) | (7.730) | (-4.577) | (-36.405) | (20.169) |
| Observations | 9,568 | 9,568 | 2,350 | 7,189 | 9,568 |
| Adjusted R-squared | 0.332 | 0.234 | 0.354 | 0.729 | 0.313 |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| SE Clusters | Bank | Bank | Bank | Bank | Bank |

**Table 6: Cross-sectional Analysis of LLP Discretion over Treated Sample**

This table reports coefficients from OLS estimates of a modified Equation (2), where the independent variable is replaced as follows: In Columns 1 and 2, it is an indicator for the top quintile of 'newly insured deposits', those in the \$100K-\$250K range as of 3Q2008; in Columns 3 and 4, it is our first indicator for the top quintile of change-in-risk, pre- to post-EESA change in z-score; in Column 5 and 6, it is our second indicator for the top quintile of change-in-risk, pre- to post-EESA change in the nonperforming loan ratio; in Column 7 and 8, it is our first indicator for the top quintile of regulatory scrutiny, Tier 1 Capital/ Risk-weighted assets; in Columns 9 and 10, it is our second indicator for the top quintile of regulatory scrutiny, bank size. This sample includes only treated banks, defined as those propensity score matched to Massachusetts state-chartered savings and cooperative banks (controls). The dependent variable is absolute value of discretionary loan loss provision (*absDLLP*) in Columns 1, 3, 5, 7, 9. and the signed value (*DLLP*) in Columns 2, 4, 6, 8, 10. *DLLP* is measured as the residual from Equation (1). All specifications include the following controls, unreported for brevity: past, present, and future changes in nonperforming loans (*DNPL*), earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (*TIER1*) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. Continuous variables are winsorized at their 1 percent tails. Models include bank and quarter fixed effects. Standard errors are clustered by bank. *, **, *** denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

| | (1)<br>absDLLP | (2)<br>DLLP | (3)<br>absDLLP | (4)<br>DLLP | (5)<br>absDLLP | (6)<br>DLLP | (7)<br>absDLLP | (8)<br>DLLP | (9)<br>absDLLP | (10)<br>DLLP |
|---|---|---|---|---|---|---|---|---|---|---|
| ***Q5NIDEPPOST$_t$*** | ***0.204*** | ***0.577***** | | | | | | | | |
| | ***(1.290)*** | ***(2.323)*** | | | | | | | | |
| ***Q5CHGRISKPOST$_t$*** | | | ***0.880****** | ***1.232****** | ***1.256****** | ***2.442****** | | | | |
| | | | ***(4.666)*** | ***(4.400)*** | ***(4.420)*** | ***(6.878)*** | | | | |
| ***Q5REGSCRUTPOST$_t$*** | | | | | | | ***0.078*** | ***0.635***** | ***0.019*** | ***0.315***** |
| | | | | | | | ***(0.311)*** | ***(2.028)*** | ***(0.185)*** | ***(1.980)*** |
| Observations | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 | 7,738 |
| Adjusted R-squared | 0.320 | 0.230 | 0.328 | 0.239 | 0.331 | 0.256 | 0.319 | 0.229 | 0.319 | 0.228 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| S.E. Clusters | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank |

**Table 7: Robustness Tests**

This table reports coefficients from OLS estimates of a modified Equation (2), where the independent variables is replaced with *TREATEDPOST*, an indicator equal to one for all treated banks after 3Q2008. Treated banks are defined as those propensity score matched to Massachusetts state-chartered savings and cooperative banks (controls). Panel A reports the alternative specifications of DLLP measures and sample matching criteria. The dependent variable is the absolute value of discretionary loan loss provision (*absDLLP*) in odd numbered columns and the signed value (*DLLP*) in even numbered columns; In Columns 1 and 2 of Panel A, dependent variables are the absolute and signed values of discretionary loan loss provision (DLLP) computed as residuals from Kanagaretnam, Lim, and Lobo's (2010b) model (KKL10). In Columns 3 and 4, dependent variables are the absolute and signed values of DLLP computed as residuals from Bushman and Williams' (2012) model (BW12). In Columns 5 and 6, dependent variables are the absolute and signed values of DLLP computed as residuals from Basu, Vitanza, and Wang (2020) model (BVW20). In the remaining columns of Panel A and in Panel B, the dependent variables are the absolute and signed residuals from Equation (1). In Columns 7 and 8, a treated subsample is selected to include all Massachusetts state-chartered commercial banks, Massachusetts federally-chartered savings banks, and non-Massachusetts state-chartered savings banks. Columns 9 and 10 use an alternate propensity score matching scheme that includes additional matching variables listed in Section 5.1. Panel B reports the placebo tests. Columns 1 and 2 re-estimate Equation (3) with randomly assigning treated and control banks to treated and control placebo-samples. Columns 3 and 4 (5 and 6) re-estimate Equation (3) with placebo-event quarters 12 quarters before (12 quarters after) EESA. All specifications include the following controls, unreported for brevity: past, present, and future changes in nonperforming loans (*DNPL*)*,* earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (*TIER1*) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. Continuous variables are winsorized at their 1 percent tails. Models include bank and quarter fixed effects. Standard errors are clustered by bank. *, **, *** Denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

**Panel A - Alternative Specifications of Discretionary LLP and Sample Matching Criteria**

| | KKL10 | | BW12 | | BVW20 | | Structural Match | | Extra Variable PSM | |
|---|---|---|---|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) | (7) | (8) | (9) | (10) |
| | absDLLP | DLLP | absDLLP | DLLP | absDLLP | DLLP | absDLLP | DLLP | absDLLP | DLLP |
| ***TREATPOST$_t$*** | ***-0.069*** | ***0.162**** | ***0.170**** | ***0.333**** | ***-0.045*** | ***0.185**** | ***-0.079*** | ***0.455**** | ***0.082*** | ***0.374**** |
| | ***(-1.181)*** | ***(3.705)*** | ***(1.848)*** | ***(2.775)*** | ***(-0.586)*** | ***(3.574)*** | ***(-0.913)*** | ***(3.504)*** | ***(0.673)*** | ***(2.066)*** |
| Observations | 9,568 | 9,568 | 9,568 | 9,568 | 9,568 | 9,568 | 7,743 | 7,743 | 5,133 | 5,133 |
| Adjusted R-squared | 0.278 | 0.072 | 0.210 | 0.257 | 0.264 | 0.056 | 0.329 | 0.231 | 0.305 | 0.184 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| S.E. Clusters | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank |

**Panel B – Placebo Tests**

| | Random T/C Assignment | | 12 Quarters Earlier | | 12 Quarters Later | |
|---|---|---|---|---|---|---|
| | (1) | (2) | (3) | (4) | (5) | (6) |
| | absDLLP | DLLP | absDLLP | DLLP | absDLLP | DLLP |
| ***TREATPOST$_t$*** | ***0.137*** | ***0.073*** | ***0.026*** | ***-0.043*** | ***-0.265*** | ***-0.284*** |
| | ***(1.255)*** | ***(0.529)*** | ***(0.645)*** | ***(-0.725)*** | ***(-1.376)*** | ***(-0.866)*** |
| Observations | 9,568 | 9,568 | 7,575 | 7,575 | 8,179 | 8,179 |
| Adjusted R-squared | 0.332 | 0.233 | 0.304 | 0.135 | 0.390 | 0.321 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| S.E. Clusters | Bank | Bank | Bank | Bank | Bank | Bank |

**Table 8: Cross-sectional Analysis over Full Sample**

This table reports coefficients from OLS estimates of a modified Equation (2), where the independent variable is replaced as follows: In Columns 1 and 2, it is an indicator for the top quintile of 'newly insured deposits', those in the $100K-$250K range as of 3Q2008; in Columns 3 and 4, it is our first indicator for the top quintile of change-in-risk, pre- to post-EESA change in z-score; in Column 5 and 6, it is our second indicator for the top quintile of change-in-risk, pre- to post-EESA change in the nonperforming loan ratio; in Column 7 and 8, it is our first indicator for the top quintile of regulatory scrutiny, Tier 1 Capital/ Risk-weighted assets; in Columns 9 and 10, it is our second indicator for the top quintile of regulatory scrutiny, bank size. This sample includes all treated banks that meet our sample selection criteria prior to propensity score matching. The dependent variable is absolute value of discretionary loan loss provision (*absDLLP*) in Columns 1, 3, 5, 7, 9. and the signed value (*DLLP*) in Columns 2, 4, 6, 8, 10. *DLLP* is measured as the residual from Equation (1). All specifications include the following controls, unreported for brevity: past, present, and future changes in nonperforming loans (*DNPL*), earnings before loan loss provision (*EBLLP)*, Tier 1 capital (*TIER1*), the natural logarithm of total assets (*SIZE*), and the quarterly change in loans (*DLOAN*). *DNPL* and *EBLLP* (*TIER1*) are scaled by lagged total loans (risk-weighted assets). Refer to Appendix A for further variable definitions. Continuous variables are winsorized at their 1 percent tails. Models include bank and quarter fixed effects. Standard errors are clustered by bank. *, **, *** denote two-tailed significance at the 10 percent, 5 percent, and 1 percent levels, respectively.

| | (1) absDLLP | (2) DLLP | (3) absDLLP | (4) DLLP | (5) absDLLP | (6) DLLP | (7) absDLLP | (8) DLLP | (9) absDLLP | (10) DLLP |
|---|---|---|---|---|---|---|---|---|---|---|
| ***Q5NIDEPPOST*** | ***0.348**** | ***0.572**** | | | | | | | | |
| | ***(5.758)*** | ***(6.527)*** | | | | | | | | |
| ***Q5CHGRISKPOST*** | | | ***1.613**** | ***2.610**** | ***1.695**** | ***3.063**** | | | | |
| | | | ***(20.202)*** | ***(23.953)*** | ***(22.222)*** | ***(31.328)*** | | | | |
| ***Q5REGSCRUTPOST*** | | | | | | | ***0.394**** | ***0.937**** | ***0.200**** | ***0.872**** |
| | | | | | | | ***(6.292)*** | ***(10.377)*** | ***(3.329)*** | ***(9.902)*** |
| Observations | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 | 91,552 |
| Adjusted R-squared | 0.266 | 0.150 | 0.281 | 0.178 | 0.284 | 0.190 | 0.266 | 0.153 | 0.265 | 0.152 |
| Controls | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes | Yes |
| Fixed Effects | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr | Bank+Qtr |
| S.E. Clusters | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank | Bank |