# Current Expected Credit Loss (CECL) Model and Analyst Forecasts

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## Abstract

We investigate whether the adoption of the Current Expected Credit Loss (CECL) standard by U.S. banks affects three properties of financial analysts' loan loss provision forecasts: accuracy, dispersion, and coverage. We find that CECL adoption is associated with reduced accuracy and coverage, and increased dispersion, of analyst provision forecasts, indicating a deterioration in analyst forecast properties. We do not observe similar changes to the properties of pre-provision earnings forecasts, and we show that the main results vary cross-sectionally with proxies for the importance of the new standard to the bank, strengthening the link between CECL adoption and changes in analyst forecast properties. Finally, we provide evidence of muted stock market reactions to analysts' provision forecast revisions after CECL adoption, suggesting that investors perceive these forecasts to be less informative post-adoption. Collectively, our results contribute to existing research on the consequences of expected credit loss standards.

## 1. Introduction

This paper examines whether the new accounting standard for estimating credit losses—the Current Expected Credit Loss (CECL) model—affects financial analysts, who are among the most important and sophisticated users of accounting information. Given financial analysts' role as important information intermediaries in stock markets (Clement 1999) and academics' long-standing interest in learning about the use of accounting information by financial analysts (Schipper 1991), we investigate whether the adoption of CECL by U.S. banks affects the properties of financial analysts' loan loss provision forecasts. Before CECL implementation, the "incurred loss" (IL) standard required managers to delay credit loss recognition until it was "probable" that the loss had been "incurred." CECL, in contrast, requires managers to recognize, at loan origination, all credit losses expected to occur during the lifetime of a loan, without any recognition threshold or trigger events. The American Bankers Association has called CECL the "most sweeping change to bank accounting ever."[1] CECL has generated significant controversy since its proposal and has led to intense debates among financial institutions, regulators, academics, and members of Congress about its potential costs and benefits.[2] To evaluate the consequences of the new CECL standard, it is important to understand its impact on different market participants.

We investigate whether the adoption of CECL by U.S. banks affects three important properties of financial analysts' loan loss provision forecasts: accuracy, dispersion, and coverage. We study the impact of CECL adoption on the accuracy of analyst provision forecasts because prior studies show that accuracy is one of the most important aspects of analysts' forecasting performance (e.g., Gu and Wu 2003; Jackson 2005; Hong and Kubik 2003). In addition, we

[1] See "*ABA Position*" on CECL (https://www.aba.com/advocacy/our-issues/cecl-implementation-challenges).
[2] For example, some members of Congress (e.g., Rep. Blaine Luetkemeyer), banking trade groups (e.g., American Banker Association, Banking Policy Institute), and CEOs or CFOs of some large U.S. banks (e.g., Capital One, BB&T Corp.) all called for either a complete removal or at least an implementation delay of CECL.

examine the impact of CECL adoption on the dispersion of analyst provision forecasts because prior literature shows that analyst forecast dispersion reflects differences in opinion among investors (Diether, Malloy, and Scherbina 2002) or unpriced information risk arising when asset values are unobservable (Johnson 2004), and thus may have implications for the cross section of stock returns.[3] Lastly, we examine the impact of CECL adoption on analyst coverage of loan loss provisions, because analyst coverage is a common proxy for information dissemination and production (e.g., Shores 1990) and the quality of firms' information environment (Frankel and Li 2004; Shroff, Verdi, and Yu 2014).

It is unclear *ex ante* whether CECL adoption will affect the accuracy and dispersion of analyst provision forecasts. On the one hand, CECL adoption may increase (decrease) forecast accuracy (dispersion) for at least two reasons. First, CECL could provide financial statement users with more decision-useful information about expected credit losses and thus reduce analyst uncertainty about expected credit losses. Second, CECL adoption should more closely align the forecasting objectives of managers and analysts compared to under the IL regime since Beatty and Liao (2021) provide evidence that analysts' provision forecasts reflected future losses even under the IL regime. On the other hand, CECL adoption may decrease (increase) the accuracy (dispersion) of analyst provision forecasts, as CECL may significantly increase the uncertainty, subjectivity, and discretion in managers' provision estimates.

Similarly, it is unclear *ex ante* whether CECL adoption will affect analyst coverage of provision forecasts. On the one hand, CECL adoption could increase analyst forecast accuracy and/or decrease analyst uncertainty about expected credit losses as discussed above. CECL

[3] Divergence in beliefs can stem from two sources: information asymmetry (i.e., analysts having access to different information about the unobservable true loan losses) and disagreement (i.e., differences of opinion; that is, investors agree to disagree, perhaps because they use different models to process the same information set about loan losses, or because of psychological biases) (Fischer, Kim, and Zhou 2021).

adoption could also reduce analysts' information gathering costs due to expanded public disclosures about loan fundamentals (e.g., vintage disclosure). These aspects of CECL adoption may attract more analysts to provide provision forecasts. On the other hand, CECL adoption could increase analyst forecast errors and analyst uncertainty about expected credit losses as discussed above, making analysts less willing to provide provision forecasts.

We examine whether CECL adoption affects the accuracy, dispersion, and coverage of analyst provision forecasts using a difference-in-differences research design on a sample of U.S. banks during 2018–2021. Our sample consists of 205 treatment banks that are publicly listed non-smaller reporting companies (i.e., non-SRCs), which were originally required to adopt CECL in 2020 before the passage of the CARES Act in March 2020, and 140 control banks that are publicly listed SRCs, which were originally exempted from adopting CECL until 2023. We find that CECL adoption is associated with a decrease in the accuracy and coverage, and an increase in the dispersion, of analyst provision forecasts.

To mitigate concerns that our baseline results are not directly related to CECL adoption, but rather to other contemporaneous changes in the information environment of the treatment banks relative to the control banks, we conduct a falsification test using analysts' pre-provision earnings (EBLLP) forecasts. If our main results are not related to CECL adoption, then we should observe a similar decrease in accuracy and coverage, and a similar increase in dispersion, of analyst EBLLP forecasts following CECL adoption. However, we are unable to find evidence that the accuracy, dispersion, or coverage of analyst EBLLP forecasts changes after CECL adoption.

Another concern is that our main results are driven by different effects of macroeconomic uncertainty on treatment and control banks in the post-CECL period, which includes the onset of the COVID-19 pandemic. It is possible that the loan performance of treatment banks is more sensitive to changes in macroeconomic conditions relative to that of control banks, making it more difficult for analysts to forecast loan loss provisions for the treatment banks in the post-period. We conduct a falsification test using a proxy for the sensitivity of a bank's changes in nonperforming loans (NPL) to changes in GDP. We find no evidence that the effects of CECL adoption on analyst forecast properties are different for banks with higher versus lower NPL-GDP sensitivity, mitigating the concern that our results are driven by differences in the effects of macroeconomic uncertainty on treatment and control banks in the post-period.

We conduct several additional analyses to reinforce our baseline findings. First, we predict and find that the effects of CECL adoption vary with *ex ante* proxies for the importance of the CECL standard to the bank. Specifically, we find that CECL adoption is associated with (1) a larger increase in forecast errors and dispersion for banks with a larger CECL day-one impact on retained earnings (due to the incremental amount of credit losses a bank recognizes upon day one of CECL application), (2) a larger increase in forecast errors and dispersion and a larger decrease in coverage for banks with more consumer loans and unfunded loan commitments, and (3) a larger increase in forecast errors and dispersion for banks with riskier loans. Next, we examine the drivers of the increase in provision forecast dispersion associated with CECL adoption using the Barron, Kim, Lim, and Stevens (1998; BKLS hereafter) framework and find that the increase in provision forecast dispersion associated with CECL adoption is due to an increase in total uncertainty, rather than a decrease in the consensus in analysts' beliefs, and that CECL adoption is associated with a decrease in the precision of analysts' public, but not private, information. Further, when examining the effect of CECL adoption by year, we find that our main results hold both in 2020, which includes the February–April 2020 recession caused by the COVID-19 pandemic, and in 2021. Lastly, we find evidence of muted stock market reactions to analysts' provision forecast revisions after CECL adoption, suggesting the market perceives these forecasts to be less informative post-adoption.

We also conduct a series of sensitivity tests and find our baseline results are robust to various alternative specifications. First, our main findings are robust to (i) including additional controls for bank size, (ii) trimming the sample such that there is no statistical difference in total assets between treatment and control banks, (iii) using lagged total loans and lagged market value of equity as alternative scalars for defining forecast accuracy and dispersion, and (iv) using alternative measures of analyst coverage. Second, we conduct placebo tests using the 2008Q1–2009Q2 recession to mitigate concerns that our baseline results are driven by size differences between the treatment and control banks rather than by CECL adoption. Finally, we fail to find evidence consistent with a violation of the parallel trends assumption.

Our study contributes to the literature in several ways. First, we contribute to the broader accounting literature on the consequences of changes in major accounting standards by providing the first evidence of the impact of CECL adoption on financial analysts. Our finding that CECL adoption is associated with a degradation in the properties of analyst provision forecasts suggests that CECL may have reduced the quality of the research outputs from a key group of information intermediaries. Our results complement those in other recent studies that examine the adoption of an expected credit loss standard: Gee, Neilson, Schmidt, and Xie (2022), which finds that expected credit loss information on day one of CECL adoption is decision-useful for equity investors and that CECL credit loss allowances provide new information about credit losses to investors; López-Espinosa, Ormazabal, and Sakasai (2021), which finds expected-credit-loss-based provisions under IFRS 9 are more predictive of future bank risk; and Oberson (forthcoming), which finds IFRS 9 adoption leads to an improvement in the timeliness of loan loss recognition, an increase in the use of provisioning to smooth earnings, and some increases in the informativeness of provisioning for CDS pricing. Given the controversies and debates around expected credit loss standards and the important role of financial analysts as information intermediaries in the capital markets, our finding of degradation in the properties of analyst provision forecasts around CECL adoption contributes to a better understanding of the consequences of the new accounting standard from the perspective of financial analysts.

Second, our study also contributes to the literature on the impact of accounting standard changes on financial analysts. Several prior studies examine the effects of mandatory International Financial Reporting Standards (IFRS) adoption on the properties of analyst earnings forecasts (e.g., Byard, Li, and Yu 2011; Tan, Wang, and Welker 2011; Horton, Serafeim, and Serafeim 2013). Given that CECL represents the most important changes to bank accounting in decades, the impact of CECL adoption on financial analysts merits examination. Our findings suggest that CECL adoption is associated with a degradation in the properties of analyst provision forecasts. Beatty and Liao (2021) provide evidence suggesting that, under the IL standard, analysts sacrifice forecast accuracy to inform investors about expected credit losses. Our study suggests that the adoption of CECL, which could reduce the need for such a tradeoff, does not appear to improve the accuracy of analyst provision forecasts.

The remainder of our study proceeds as follows. Section 2 provides some institutional background about the IL model and the CECL model, including the debate about the CECL model. Section 3 explains the research design. Section 4 discusses sample selection and descriptive statistics. Section 5 discusses empirical results. Section 6 presents additional analyses and robustness checks. Section 7 concludes.

## 2. Institutional Background

### 2.1 The Road from the Incurred Loss Model to CECL

The previous incurred loss (IL) model delayed loan loss recognition by banks until it was "probable" that the loss had been "incurred." The 2007–2009 financial crisis revealed that the IL model led to loan loss recognition that was perceived to be "too little, too late," a problem that is widely believed to have amplified the depth and duration of the financial crisis (Basel Committee on Banking Supervision 2021). In response to calls from regulators and investors, the FASB and the IASB explored forward-looking alternatives for timelier recognition of credit losses. In June 2016, the FASB issued ASU 2016-13, *Financial Instruments—Credit Losses (Topic 326)*, which requires recognition of lifetime expected credit losses on the day of loan origination or acquisition.[4] The new model is commonly referred to as current expected credit loss or "CECL".[5] CECL brings about a sea change to how banks should recognize credit losses from loans and other debt instruments, and has been called by American Bankers Association the "most sweeping change to bank accounting ever".[6]

CECL was originally set to become effective for fiscal years beginning after December 15, 2022, for entities considered smaller reporting companies (SRCs) by the SEC, and for fiscal years beginning December 15, 2019, for non-SRCs.[7] However, in response to the outbreak of the

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[4] In June 2014, the IASB issued International Financial Reporting Standard 9 – Financial Instruments (IFRS 9), which uses the expected credit loss framework and became effective for annual periods beginning on or after January 1, 2018. There are significant differences between CECL and IFRS 9. Most importantly, CECL requires banks to recognize lifetime expected credit losses on all loans upon loan origination/acquisition; in contrast, upon loan origination/acquisition, IFRS 9 requires banks to recognize only expected credit losses resulting from default events that are possible within the next 12 months.

[5] CECL affects financial assets measured at amortized cost, including financing receivables (most notably loans held for investments), held-to-maturity (HTM) debt securities, purchased credit-deteriorated (PCD) assets, and trade receivables, as well as off-balance-sheet credit exposures (e.g., unfunded loan commitments). Since loans are the most significant asset class for a typical bank, banks' asset class that is most affected by CECL is loans held for investment.

[6] https://www.aba.com/advocacy/our-issues/cecl-implementation-challenges.

[7] SRCs are companies that have (i) public float of less than $250 million or (ii) annual revenues less than $100 million and either no public float or public float less than $700 million (https://www.sec.gov/smallbusiness/goingpublic/SRC).

COVID-19 pandemic, the Coronavirus Aid, Relief, and Economic Security ("CARES") Act, signed into law on March 27, 2020, gave non-SRCs the option to delay CECL adoption until 2021. For the non-SRCs that elected to delay adoption until 2021, the Consolidated Appropriations Act (2021), signed into law on December 27, 2020, gave them the further option to delay CECL adoption until 2022.[8]

## 2.2 Debates about CECL

Since its proposal, CECL has generated significant debates among financial institutions, regulators, academics, and members of Congress about its potential costs and benefits. Proponents of CECL, including bank regulators, applaud its timelier recognition of credit losses and greater transparency due to the incorporation of forward-looking information in managers' credit loss estimation. Opponents of CECL, particularly financial institutions and their trade groups, along with some members of Congress, vehemently sought the total removal or at least implementation delay of CECL. The main concerns expressed by the opponents of CECL include outsized implementation costs outweighing any potential benefits, increased subjectivity and discretion in credit loss estimation, increased earnings volatility, and increased procyclicality in loan loss provisioning and, hence, in bank lending. Although hundreds of publicly listed banks have already adopted CECL, the controversy about the potential costs and benefits of CECL remains.

## 2.3 Prior Literature

The most closely related paper to our study is Beatty and Liao (2021). Beatty and Liao (2021) examine analyst forecasts of loan loss provisions under the IL standard to understand potential cross-sectional differences in how the future implementation of CECL would affect provision timeliness. They find that analyst provision forecasts incrementally predict future

[8] See Sec. 540 a (2), Subtitle C of CAA at https://www.congress.gov/bill/116th-congress/house-bill/133/text.

nonperforming loans and market returns beyond the IL provisions recognized by banks, especially when banks are more constrained in their ability to predict future losses, analyst EPS forecast errors are higher, or analyst target price errors are lower. Their results suggest that under the IL standard, analysts, who are not constrained by the IL model, trade off forecast accuracy for informing the market by incorporating expected credit losses into their provision forecasts. Our study suggests that the adoption of CECL, which eliminates the need for such a tradeoff, does not appear to improve the accuracy of analyst provision forecasts.

Our study is also related to prior literature that examines the impact of accounting standard changes on the properties of analyst forecasts. Several studies explore the impact of mandatory IFRS adoption on analyst forecasts. Tan et al. (2011) find that mandatory IFRS adoption increases following by foreign analysts and improves the forecast accuracy of foreign analysts. Byard et al. (2011) find that mandatory IFRS adoption is associated with a reduction in analyst forecast errors and dispersion in countries with both strong enforcement and large differences between local GAAP and IFRS. Horton et al. (2013) find that mandatory IFRS adopters experience a greater reduction in consensus forecast errors than voluntary adopters. The collective evidence from these studies suggests that mandatory IFRS adoption improves the quality of information intermediation by financial analysts and as a result improves firms' information environments.

Our paper is also related to recent studies examining the impact of the adoption of CECL or IFRS 9, with the latter being the standard issued by the IASB that also uses the expected credit losses approach.[9] Gee et al. (2022) find that expected credit loss information on day one of CECL adoption in the U.S. is decision-useful for equity investors and that CECL credit loss allowances

[9] There is also a stream of literature in which researchers develop their own models to estimate expected credit losses prior to CECL implementation, e.g., Covas and Nelson (2018), Harris, Khan, and Nissim (2018), Lu and Nikolaev (2021), and Wheeler (2021).

provide new information about credit losses to investors. Bable, Wong, and Wynes (2022) interviewed 13 users and trade group members with knowledge of and experience with CECL, including 8 sell-side analysts. The authors find that analysts are highly critical of CECL, believing that CECL disclosures are confusing and require more research than under the IL model to understand the true effects of CECL on the financial statements. López-Espinosa et al. (2021) find that credit loss provisions under IFRS 9 are more predictive of future bank risk than provisions under the IL standard, especially in countries with deteriorating credit conditions. Lejard, Paget-Blanc, and Casta (2021) find that, after IFRS 9 adoption, sovereign credit ratings (impaired loans) become a more (less) important determinant of loan loss allowances, and that there is increased heterogeneity in the measurement of provisions. Onali, Ginesti, Cardillo, and Torluccio (forthcoming) document positive market reactions to IFRS 9 adoption events. To the best of our knowledge, our study is the first to examine the impact of CECL adoption on the properties of analyst provision forecasts.

### 2.4 Empirical Predictions

It is unclear *ex ante* whether CECL adoption will affect the accuracy and dispersion of analyst provision forecasts. On the one hand, CECL adoption may improve these forecast properties for at least two reasons. First, the main stated objective of the CECL standard is "to provide financial statement users with more decision-useful information about the expected credit losses" (see p. 1 of ASU 2016-13). Accordingly, analysts may find bank managers' loan loss provision estimates and the accompanying disclosures under CECL to be more decision-useful than those under the IL model, leading to an improvement in forecast accuracy. Second, CECL adoption eliminates the misalignment in the approaches of bank managers and financial analysts to estimating provisions that exist in the IL regime. Prior to CECL adoption, bank managers were unable to incorporate expected losses into their provision estimates as they were bound by the "probable" recognition threshold, while analyst provision forecasts already reflected expected credit losses (Beatty and Liao 2021). Thus, evidence in Beatty and Liao (2021) suggests that, prior to CECL adoption, analysts sacrificed forecast accuracy for informativeness (which is consistent with Louis, Sun, and Urcan 2013). After CECL adoption, analysts no longer need to sacrifice forecast accuracy for informing the market, since both analyst forecasts and the actual provisions are based on expected credit losses. Thus, the approach of analysts and managers in estimating provisions should be better aligned under the CECL standard than under the IL model, which would lead to an improvement in the accuracy of analyst provision forecasts. Further, the increase in the decision-usefulness of accounting information under CECL may also lead to a reduction in analysts' uncertainty about expected credit losses and/or reduce the need for analysts to collect idiosyncratic information to inform their provision estimation, which would lead to a reduction in analyst provision forecast dispersion.

On the other hand, CECL adoption may decrease (increase) the accuracy (dispersion) of analyst provision forecasts, as CECL may significantly increase the uncertainty, subjectivity, and discretion in managers' provision estimates. CECL requires consideration of forward-looking information. Managers must incorporate reasonable and supportable forecasts about future macroeconomic conditions when estimating expected credit losses. The amount of expected credit losses recognized by managers depends critically on the optimism or pessimism in their macroeconomic predictions and assumptions (e.g., GDP growth, unemployment rates). Further, CECL allows an entity to use judgment in determining the estimation method that the manager uses, rather than prescribing specific methods for measuring expected credit losses (see ASC 326-

20-30-3 and 326-20-55-7).[10,11] To the extent that analysts' macroeconomic forecasts and assumptions underlying their provision forecasts differ from those used by bank managers or analysts do not use the same estimation methods as bank managers, or both, CECL adoption may reduce the accuracy of analyst provision forecasts. Furthermore, the incorporation of forward-looking information in estimating loan loss provisions under CECL may make loan loss provisions more volatile under CECL than under the IL model, which could reduce analyst forecast accuracy. In addition, the potential greater subjectivity and discretion in bank managers' provision estimates under CECL may reduce the decision-usefulness of banks' CECL information, which could lead to an increase in the extent to which analysts acquire idiosyncratic information and, accordingly, greater analyst provision forecast dispersion in the CECL regime than in the IL regime.

Similarly, it is unclear *ex ante* whether CECL adoption will affect analyst coverage of provision forecasts. Financial analysts face time, energy, and resource constraints (e.g., Clement 1999; deHaan, Shevlin, and Thornock 2015; Hirshleifer, Levi, Lourie, and Teoh 2019; Harford, Jiang, Wang, and Xie 2019), and their decision to cover a firm is affected not only by these constraints (Lee and So 2017) but also by their career concerns associated with forecast inaccuracy (Hong and Kubik 2003). To the extent that CECL adoption increases analyst forecast accuracy and/or reduces analyst uncertainty about expected credit losses, CECL adoption may attract more analysts to provide provision forecasts. However, CECL adoption may make it more costly for analysts to provide provision forecasts since they need to fully incorporate forward-looking information into their provision forecasts under CECL. To the extent that these incremental costs

[10] The different methods mentioned in ASU 2016-13 include vintage analysis, probability-of-default method discounted cash flow methods, loss-rate method, and roll-rate method (see FASB 2016, p. 109, para. 326-20-30-3).
[11] The potential subjectivity of CECL estimates has been one of the biggest concerns expressed about the standard. Even equity investors, who generally support CECL, are concerned that CECL "opens considerable room for manipulation" and that the estimation methodology chosen by an entity "may become too complex to understand and to challenge" (https://www.cfainstitute.org/-/media/documents/comment-letter/2010-2014/20130910.ashx).

exceed the benefit of providing provision forecasts, analysts may reduce their coverage of provision forecasts. Further, to the extent that CECL reduces the accuracy of analyst provision forecasts, analysts may also reduce their coverage of provision forecasts due to career concerns.

## 3. Research Design

We employ a difference-in-differences research design to examine the impact of CECL adoption on the properties of analyst provision forecasts. Our treatment banks are non-SRC banks because, before the passage of the CARES Act in March 2020, they were required to adopt CECL in 2020. Our control banks are SRC banks because they are not required to adopt CECL until 2023. To test whether CECL adoption affects the accuracy, dispersion, and coverage of analyst provision forecasts, we estimate the following model using ordinary least squares:

$$\begin{aligned} Accuracy/Dispersion/Coverage_{i,t} = \alpha_0 + \alpha_1 CECL_i \times Post_{i,t} + \alpha_2 Post_{i,t} \\ + Controls + Bank\ FE + Year\ FE + \varepsilon_{i,t} \end{aligned} \quad (1)$$

where subscript $i$ indexes the bank and $t$ indexes the quarter. The dependent variable is either forecast accuracy (|*LLPForecastError*|), dispersion (*LLPForecastDispersion*), or coverage (*LogNumLLPEst*). Following prior literature (e.g., Dhaliwal, Radhakrishnan, Tsang, and Yang 2012), we use analyst forecast error as an inverse measure of forecast accuracy. We define |*LLPForecastError*| as the absolute difference between the consensus (mean) analyst provision forecast and the actual provision for the bank-quarter, scaled by lagged total assets.[12] We define *LLPForecastDispersion* as the standard deviation of the individual analyst provision forecasts

[12] The prior literature on EPS forecast errors (e.g., Dhaliwal et al. 2012) calculates forecast errors as the difference between forecasted EPS and actual EPS, scaled by stock price: |*ForecastEPS – ActualEPS*|/*Price*. Since the two EPS numbers are calculated as earnings divided by shares outstanding and stock price is calculated as the market value of equity divided by shares outstanding, this can be rewritten as follows: |*$ForecastEarnings – $ActualEarnings*|/*MVE*. Thus, the forecast error is the difference between the forecasted and actual earnings (in dollars) divided by firm size (i.e., the market value of equity). Our measure of provision forecast error is analogous, except that we use lagged total assets, rather than the market value of equity, as the deflator. As shown in the robustness tests in Section 6.5, our results on analyst forecast accuracy and dispersion are robust to scaling the measures of forecast accuracy and dispersion by lagged total loans and lagged market value of equity.

included in the consensus, scaled by lagged total assets.[13] Finally, we define *LogNumLLPEst* as the natural logarithm of the number of analyst provision forecasts included in the consensus. *CECL* is an indicator variable equal to one for our treatment bank observations (i.e., the non-SRC banks) and zero for our control bank observations (i.e., the SRC banks)[14]. For treatment banks, *Post* is an indicator variable equal to one if the bank has adopted CECL by the beginning of quarter t, and zero otherwise. For control banks, *Post* is an indicator variable equal to one for 2020Q1 and thereafter, and zero otherwise.[15, 16]

Following prior literature that examines analysts' earnings forecasts (e.g., Tan et al. 2011; Dhaliwal et al. 2012; Horton et al. 2013; Campbell, Khan, and Pierce 2021), we include the following control variables, all measured one quarter prior to the measurement of the dependent variable: market-to-book ratio (*MTB*), stock return volatility (*RetVol*), whether a bank's financial statements are audited by a Big Four auditor (*BigN*), whether a bank reported negative earnings during the quarter (*Loss*), the most recent quarter-over-quarter change in loan loss provision recognized by a bank ($\Delta LLP$),[17] and bank size measured as the natural logarithm of total assets (*LogAssets*). As banks comprise the entirety of our sample, we also control for the following

[13] For the forecast dispersion analyses, we require that a minimum of two forecasts are included in the consensus.
[14] Companies classified as Emerging Growth Companies (EGC) by the SEC are not required to adopt CECL until 2023 unless they lose their EGC status before 2023 (https://www.sec.gov/smallbusiness/goingpublic/EGC). We classify non-SRC EGCs that have not adopted CECL during our sample period as control banks.
[15] The indicator variable *Post* is not subsumed by year fixed effects in equation (1) because there are 10 non-calendar year-end banks that adopt CECL during quarters 2–4 of 2020 and *Post* is defined based on the bank's adoption date. For example, if a non-calendar year-end bank adopts CECL in 2020Q2, then *Post* is equal to one in 2020Q2 and thereafter, but equal to zero in 2020Q1 and before.
[16] Since the majority of our treatment banks adopt CECL in 2020Q1, for control banks, we define *Post* to be the same as these treatment banks. We conduct a robustness test where, for the control banks, we randomly assign different "adoption dates," for purposes of defining *Post*, in the same proportions as the treatment banks in terms of their actual adoption dates. More specifically, for treatment banks, 76.1% adopt in 2020Q1, 0.5% adopt in 2020Q2, 2.0% adopt in 2020Q3, 2.4% adopt in 2020Q4, and 19.0% adopt in 2021Q1. In the robustness test, we set *Post* equal to 1 in 2020Q1 and thereafter for 76.1% of the control banks, we set *Post* equal to 1 in 2020Q2 and thereafter for 0.5% of the control banks, and so on. Our main results are robust to this alternative specification.
[17] This variable is calculated as loan loss provision recognized by a bank in quarter t-1 minus loan loss provision recognized by the bank in quarter t-2, scaled by lagged total assets. This variable mirrors the control variable of the most recent change in earnings used in prior literature that studies analyst forecasts of earnings (e.g., Hope 2003).

factors that are specific to banks and may have an impact on analyst forecast properties: (1) capital adequacy (*Tier1Ratio*, which is the Tier 1 regulatory capital ratio), and (2) loan portfolio composition (*ConsumerLoans*, which is the proportion of a bank's total assets that is consumer loans; and *RealEstateLoans*, which is the proportion of a bank's total assets that is real estate loans). Since analyst forecast accuracy and dispersion may be affected by analyst coverage of provision forecasts, we additionally control for the natural logarithm of the number of analysts issuing provision forecasts (*LogNumLLPEst*) when the dependent variable is forecast accuracy or dispersion. Finally, we control for bank and year fixed effects to account for bank- and time-invariant factors, respectively, and cluster standard errors at the bank level.

## 4. Sample Selection, Data, and Summary Statistics

To construct our main sample, we start with all publicly traded U.S. banks (identified using SIC codes 6000 – 6299) for which we can identify analyst loan loss provision forecasts and other bank-level variables available from the "Companies" dataset in S&P Capital IQ Pro ("SPCIQ," formerly SNL Financial). We further require that the banks have CRSP and Compustat coverage for the calculation of certain control variables.[18] Since prior to the passage of the CARES Act only non-SRCs were required to adopt CECL in 2020, we next classify banks into SRC and non-SRC groups. To identify banks' SRC status, we use the SEC's criteria for SRC status (see footnote 8 in Section 2.1) and approximate public float using market value of equity from CRSP. To reduce the potential for misclassification, we hand collect SRC status from the 10-K/Q filing for 2019Q4 for the subset of banks for which our approximation has the highest potential for misclassification.[19]

[18] See Appendix A for details regarding the construction and data sources of control variables.
[19] We set our hand collection thresholds conservatively to minimize errors in approximating public float. For banks with less than \$100 million in annual revenue, we hand collect SRC status if the bank has less than \$1 billion market value of equity (from CRSP), relative to the SEC's threshold of less than \$700 million of public float. For banks with at least \$100 million in annual revenue, we hand collect SRC status if the bank has less than \$400 million market value of equity, relative to the SEC's threshold of less than \$250 million public float.

To identify CECL adoption dates and CECL day-one impacts, we first attempt to extract the data from the "Regulated Depositories (U.S.)" dataset in SPCIQ.[20] When the reported CECL day-one impact in SPCIQ is either zero or missing for a non-SRC bank, we hand collect the bank's CECL day-one impact data and adoption date from its regulatory filings (i.e., FR Y-9C and call reports) and SEC filings (e.g., 10-Q filings).

Our final sample consists of 4,444 bank-quarters with analyst provision forecasts in SPCIQ during our 2018–2021 sample period, from 205 treatment banks and 140 control banks.[21] Among the treatment bank observations, 1,752 (1,306) bank-quarters are from the pre-period (post-period), while among the control bank observations, 718 (668) are from the pre-period (post-period). We winsorize all continuous variables at the 1st and 99th percentiles to reduce the influence of outliers. While we do not require a constant sample of banks for our main tests (i.e., we keep observations for banks when *Post* is always equal to one or always equal to zero), in an untabulated robustness test, we find that our main results are unaffected when we require a constant sample of banks (i.e., when we require at least one observation in both the pre- and post-periods).

Panel A (B) of Table 1 presents summary descriptive statistics separately for the pre-period and the post-period for the CECL (control) banks. For the average treatment bank in the pre-period (post-period), the absolute provision forecast error is equivalent to 2.3 (7.6) percent of total assets, provision forecast dispersion is equivalent to 1.0 (3.3) percent of total assets, and the number of analysts providing provision forecasts is 5.698 (5.741).

[20] The CECL day-one impact captures the cumulative-effect adjustment for the changes in the allowance for credit losses, net of any related deferred tax assets and excluding the initial allowance gross-up for any purchased credit-deteriorated assets, recognized in retained earnings as of the adoption date (SPCIQ Keyfield 319094).

[21] 156 of the 205 CECL banks adopted CECL on January 1, 2020. Eight of these 205 banks elected to delay CECL adoption until 2022, as allowed under the Consolidated Appropriations Act (2021); our results are robust to dropping these eight banks from the sample.

## 5. Empirical Results

### 5.1 Baseline Analyses

Table 2 presents the results from estimating equation (1). In column 1 where the dependent variable is |*LLPForecastError*|, the coefficient on *CECL* × *Post* is significantly positive. This indicates that CECL adoption is associated with an increase in forecast errors and, thus, a decrease in forecast accuracy. In column 2 where the dependent variable is *LLPForecastDispersion*, the coefficient on *CECL* × *Post* is significantly positive. This suggests that CECL adoption is associated with an increase in forecast dispersion. In column 3 where the dependent variable is *LogNumLLPEst*, the coefficient on *CECL* × *Post* is significantly negative. This indicates that CECL adoption is associated with a decrease in the number of analysts providing provision forecasts, suggesting a reduction in analyst coverage of provision forecasts following CECL adoption. In terms of economic magnitude, these coefficients imply an increase in |*LLPForecastError*| and *LLPForecastDispersion* of 2.1 percent and 1.4 percent of lagged total assets, respectively, and a decrease of approximately one analyst issuing a provision forecast. These effect sizes represent 0.4, 0.6, and 0.5 standard deviations of the within-fixed-effect variation in the three dependent variables, respectively.[22] Thus, these effects are both statistically and economically significant.

### 5.2 Falsification Tests

Panel A of Table 3 presents the results of the falsification test that estimates equation (1)

[22] Following the suggestions of Mummolo and Peterson (2018), we report economic significance both in terms of the dependent variables' units and in terms of the dependent variables' within-fixed-effect standard deviations. |*LLPForecastError*| and *LLPForecastDispersion* are scaled by lagged total assets, so the coefficients on *CECL* x *Post* in Table 2 can be interpreted as a percentage of lagged total assets. For *LogNumLLPEst*, we calculate the effect size in terms of the number of analyst provision forecasts by taking the exponential of the coefficient on *CECL* x *Post* in Table 2. To calculate the dependent variables' within-fixed-effect standard deviations, we first regress the dependent variables on bank and year fixed effects. Then, we calculate the standard deviation of the residual from those regressions (untabulated). The effect sizes are calculated by dividing the coefficients on *CECL* x *Post* in Table 2 by the within-fixed-effect standard deviations of the respective dependent variables.

using analyst forecasts of earnings before loan loss provisions (EBLLP), which represents the part of earnings that should not be affected by CECL adoption. Columns 1–3 of Table 3 mirror columns 1–3 of Table 2. Across all three columns, none of the coefficients on *CECL* × *Post* is statistically different from zero. Given that CECL adoption should not affect EBLLP, our inability to detect a significant change in the properties of analyst EBLLP forecasts mitigates the concern that our results for analyst provision forecasts are driven by CECL banks and the control banks being differentially affected by the COVID-19 pandemic. The results of this falsification test using analyst EBLLP forecasts also help corroborate our conclusion that CECL adoption is associated with a decrease (an increase) in the accuracy (dispersion) and a decrease in the coverage of analyst provision forecasts.

To mitigate the concern that the differing results in Table 2 (analyst provision forecasts) versus Panel A of Table 3 (analyst EBLLP forecasts) are driven by sample differences, Panel B of Table 3 reports the results of robustness tests using the same sample in Panel A of Table 3 to re-estimate the regression models from Table 2; the results in Panel B of Table 3 are similar to those reported in Table 2. The contrasting results in Panel A versus Panel B of Table 3, both of which are based on the same sample, help support our conclusion that the changes in the attributes of analysts' loan loss provision forecasts reported in Table 2 are more likely to be driven by CECL adoption rather than the uncertainty driven by the onset of the COVID-19 pandemic. Since our treatment period coincides with the COVID-19 pandemic, we do offer the caveat that if the pandemic caused a bigger increase in uncertainty about expected credit losses relative to uncertainty about incurred credit losses, but did not cause a bigger increase in uncertainty about EBLLP for the treatment banks relative to the control banks, then our falsification test using EBLLP would not rule out the alternative explanation that the COVID-19 pandemic, rather than the adoption of CECL, leads to our findings.

To further mitigate the concern that our baseline results are driven by differential effects of the COVID-19 pandemic on treatment versus control banks, rather than the adoption of CECL, our next falsification test examines whether the main results differ for banks with higher versus lower sensitivities of loan performance to changes in macroeconomic conditions (*GDPBeta*). To calculate *GDPBeta*, we regress quarterly changes in non-performing loans ($\Delta NPL$) on lagged quarterly changes in GDP ($\Delta GDP$). We estimate these regressions by bank over the 2013–2019 time period. *GDPBeta* is the bank's estimated coefficient on $\Delta GDP$ obtained from these regressions, multiplied by -1 so that *GDPBeta* is increasing in the bank's NPL-GDP sensitivity. We first validate this measure by regressing $\Delta NPL_{t}$ on $GDPBeta_{t-1}$, $\Delta GDP_{t-1}$, and $GDPBeta \times \Delta GDP_{t-1}$ with our main sample. If *GDPBeta* captures meaningful variation in a bank's NPL-GDP sensitivity, then we expect a significantly negative coefficient on $GDPBeta \times \Delta GDP_{t-1}$. This would suggest that when changes in GDP are positive (negative), banks with higher NPL-GDP sensitivities experience more negative (positive) changes in NPL relative to banks with lower NPL-GDP sensitivities. The results of these validation tests are presented in Panel C of Table 3; the three columns report results with no additional controls (column 1), the addition of bank and year fixed effects (column 2), and the addition of all controls and fixed effects from Table 2 (column 3). As expected, we find a significantly negative coefficient on the interaction term $GDPBeta \times \Delta GDP$ across all three columns of Panel C in Table 3, suggesting that our proxy of NPL-GDP sensitivity is a valid measure of the sensitivity of a bank's loan performance to macroeconomic fluctuations.

If the COVID-19 pandemic is driving the results in Table 2, we would expect the main results to be stronger for banks with higher NPL-GDP sensitivity, since these banks are most likely to experience a significant increase in uncertainty related to loan losses due to the pandemic. We test this conjecture by adding *GDPBeta×Post* and *GDPBeta×CECL×Post* to equation (1) and reporting the results in Panel D of Table 3. Across the three columns of Panel D, none of the coefficients on *GDPBeta×CECL×Post* are statistically different from zero, while all three coefficients on *CECL×Post* are statistically significant and maintain the same sign as in the baseline results. Thus, it does not appear that our baseline results are driven by differential sensitivity of loan performance to changes in macroeconomic conditions across the treatment and control banks. This helps to further mitigate the concern that our main results are driven by the COVID-19 pandemic, rather than the adoption of CECL.

## 6. Additional Analyses and Robustness Tests

### 6.1 Cross-sectional Tests

We conduct three tests to examine whether the cross-sectional variation in the effect of CECL adoption on the properties of analyst provision forecasts is consistent with *ex ante* expectations of the importance of the new standard to the banks. First, we expect CECL's impact on analyst provision forecasts to be greater when CECL's day-one impact on retained earnings is greater. Upon CECL adoption, banks are required to record a cumulative "day-one" adjustment to credit loss allowances and retained earnings. The day-one adjustment to retained earnings reflects the after-tax effects of day-one CECL application. The more negative the CECL day-one impact on retained earnings, the greater the increase in credit loss allowances upon day one of CECL adoption and, hence, the greater the potential impact of CECL adoption on the bank after day one. Therefore, we expect the impact of CECL adoption on the properties of analyst provision forecasts to be greater when the CECL day-one impact on retained earnings is more negative. We also expect CECL's impact on analyst provision forecasts to be greater for banks with more consumer loans and more unfunded loan commitments. This is because (1) Gee et al. (2022) find that CECL day-one impacts on expected credit losses are positively associated with the proportion of a bank's total assets that is consumer loans, and (2) CECL is expected to have a significant impact on the allowance for unfunded loan commitments.[23] Lastly, we expect CECL's impact on analyst forecasts to be greater for banks with riskier loan portfolios. This is because we expect the difference between current expected credit losses and incurred losses to be greater, and hence the impact of CECL adoption to be greater, for riskier loans than for safer loans. We use the average interest rate charged by a bank on its total loan portfolio to capture the overall riskiness of its loans.

Table 4 presents the results of the three cross-sectional tests. In Panel A, the test variable is the interaction term between *Day1Impact* and the indicator variable *Post*. *Day1Impact* is the CECL day-one impact on retained earnings, multiplied by -1 so that a more positive *Day1Impact* suggests a greater increase in credit loss allowances upon day one CECL adoption, scaled by lagged total assets (i.e., assets at the end of the quarter immediately preceding CECL adoption).[24] As predicted, the coefficient on *Day1Impact* × *Post* is significantly positive (negative) in column 1 (2) where the dependent variable is forecast error (dispersion). This suggests that the increase in provision forecast errors and dispersion is greater for banks with greater day-one impacts. The coefficient on *Day1Impact* × *Post* is statistically insignificant ($t$ = -1.21) in column 3 where the dependent variable is analyst coverage of provision forecasts. In Panel B, the test variable is the three-way interaction term *HighImpact* × *CECL* × *Post*, where *HighImpact* is an indicator variable

[23] For example, FORVIS (formerly BKD) suggests that the effect of CECL adoption on the allowance for unfunded commitments often exceeded the effect on the allowance for funded loans: https://www.forvis.com/alert-article/2021/01/cecl-implementation-eight-takeaways.

[24] Since *Day1Impact* is only defined for CECL banks, the interaction term *Day1Impact* x *Post* represents the incremental effect of CECL for different magnitudes of *Day1Impact*. To ease interpretation, *Day1Impact* is standardized to have a mean of zero and a standard deviation of one. Thus, *CECL* × *Post* would represent the impact of CECL for banks with an average day-one impact (i.e., the standardized version of *Day1Impact* equals zero) and *Day1Impact* × *Post* would capture the incremental effect of CECL for banks with different magnitudes of the day-one impact.

set to 1 if the bank's 2019Q4 total consumer loans, scaled by total assets, and unfunded loan commitments, scaled by total assets, are in the top quartile of the sample, and 0 otherwise. As expected, the coefficient on *HighImpact* × *CECL* × *Post* is statistically significant in all three columns, with the same sign as the coefficient on *CECL* × *Post* in each column. This suggests that the changes in analyst provision forecast attributes associated with CECL adoption are greater for the treatment banks with more consumer loans and more unfunded loan commitments.

In Panel C, the test variable is the three-way interaction term *HighIntIncome* × *CECL* × *Post*, where *HighIntIncome* is an indicator variable set to one if the bank's 2019Q4 interest income, scaled by total loans, is in the top quartile of the sample, zero otherwise. The coefficient on *HighIntIncome* × *CECL* × *Post* is significantly positive (negative) in column 1 (3) for the forecast accuracy (coverage) test, with the same sign as the coefficient on *CECL* × *Post* in the respective column. This suggests that the decrease in analyst provision forecast accuracy and coverage associated with CECL adoption is greater for the treatment banks with riskier loan portfolios. Overall, the results of the three cross-sectional tests suggest that the effect of CECL adoption on the properties of analyst provision forecasts is a function of *ex ante* expectations of the importance of the CECL standard to the banks.

6.2 BKLS Decomposition

We next examine whether the increase in analyst provision forecast dispersion associated with CECL adoption is driven by a decrease in the commonality in analysts' beliefs, an increase in total uncertainty, or both. BKLS theoretically shows that analyst forecast dispersion is a function of both the commonality in analysts' forecast errors (i.e., the consensus in their beliefs) and the precision of their information (i.e., the inverse of total uncertainty). Specifically, expected analyst forecast dispersion can be expressed as one minus commonality (i.e., diversity) times total uncertainty (i.e., lack of precision). That is,

$$D = (1 - \rho) \text{ x } V \tag{2}$$

where $D$ is expected analyst forecast dispersion, $\rho$ measures the commonality in analysts' information, as captured by the across-analyst correlation in forecasts errors (also known as BKLS consensus or BKLS correlation), and $V$ is total uncertainty.

Since total uncertainty reflects the sum of the uncertainty associated with analysts' private information and the uncertainty associated with information common to all analysts, BKLS further show that under certain assumptions, $\rho$ captures the precision of analysts' common information to the precision of their total information:

$$\rho = h/(h + s) \tag{3}$$

where $h$ is the precision of analysts' common information, and $s$ is the precision of analysts' idiosyncratic information.

To understand whether the increase in forecast dispersion associated with CECL adoption is driven by an increase in total uncertainty, a decrease in analyst consensus, or both, we replace *LLPForecastDispersion* in equation (1) with *TotalUncertainty,* which is the variable $V$ in equation (2), and *Consensus,* which is the variable $\rho$ in equation (2). To further understand whether CECL adoption is associated with a change in the precision of analysts' public (i.e., common) versus idiosyncratic information, we replace *LLPForecastDispersion* in equation (1) with the precision of analysts' common information (*PublicPrecision*, which is the variable $h$ in equation (3)) and the precision of analysts' idiosyncratic information (*PrivatePrecision*, which is the variable $s$ in equation (3)). We present the results from these estimations in Table 5.

In column 1 (2) of Table 5, where the dependent variable is *TotalUncertainty* (*Consensus*), the coefficient on *CECL* × *Post* is significantly positive (insignificant). This suggests that the increase in forecast dispersion associated with CECL adoption is driven by an increase in total uncertainty, rather than a decrease in consensus. In column 3 (4) of Table 5, where the dependent variable is *PublicPrecision* (*PrivatePrecision*), the coefficient on *CECL* × *Post* is significantly (insignificantly) negative. This suggests that CECL adoption is associated with a significant decrease in the precision of analysts' common information, but an insignificant change in the precision of their idiosyncratic information.

6.3 By-year Analyses of the Post-Period

We conduct by-year analyses of the post-period to assess if our baseline results are only concentrated in 2020, during which a recession related to the COVID-19 pandemic occurred. Both macro and micro uncertainty rises sharply in recessions (Bloom 2014), making it more challenging for analysts to forecast provisions. For our by-year analyses, we define *Post2020* (*Post2021*), which is an indicator variable set to one if *Post* is equal to 1 and the year is 2020 (2021), and zero otherwise. Across all three columns in Panel A of Table 6, the estimated coefficients on *CECL* × *Post2020* and *CECL* × *Post2021* are statistically significant and have the same sign, which suggests that our baseline results are found not only in 2020 but also in 2021. Importantly, when we compare the magnitudes of the coefficients on *CECL* × *Post2020* and *CECL* × *Post2021,* we find that the former is significantly greater than the latter for the forecast dispersion test but not for the forecast accuracy or coverage tests. Thus, there is evidence of a deterioration in analyst provision forecast properties outside of the recessionary year 2020.

We further conduct the BKLS decomposition in the by-year analyses to understand what drives the greater increase in forecast dispersion following CECL adoption in 2020 compared to 2021; we report the results in Panel B of Table 6. The coefficient estimates on *CECL* × *Post2020* are significantly greater than those on *CECL* × *Post2021* only in column 1 (where the dependent variable is total uncertainty), but not for the remaining three columns (where the dependent variables are consensus, precision of public information, and precision of private information, respectively). Overall, these results suggest that the greater increase in forecast dispersion in 2020 relative to 2021 is due to greater total uncertainty in 2020.

## 6.4 The Impact of CECL on the Informativeness of Analyst Provision Forecasts

We next examine whether stock market reactions to analyst provision forecast revisions are consistent with investors perceiving CECL adoption to be associated with a deterioration in the properties of analyst provision forecasts. To the extent that CECL increases forecast errors and dispersion while reducing the coverage of analyst provision forecasts, investors may perceive analyst provision forecasts to be less informative after CECL adoption. In addition, investors' access to banks' expanded public disclosure of information related to expected credit losses and loan fundamentals may also reduce the relative importance and informativeness of analyst provision forecasts in the CECL regime. As a result, the announcements of analysts' provision forecast revisions may lead to smaller reductions in information asymmetry and smaller stock market responses after CECL adoption. To examine this, we follow the framework of Amiram, Owens, and Rozenbaum (2016), which examines the impact of analyst forecast announcements on information asymmetry, while using a difference-in-difference research design. Specifically, we estimate the following model using OLS:

$$\begin{aligned} DV_{i,d} = {} & \alpha_0 + \alpha_1 PreRevision1_{i,d} + \alpha_2 Revision0_{i,d} + \alpha_3 Revision1_{i,d} + \alpha_4 Post_{i,t} \\ & + \alpha_5 PreRevision1_{i,d} \times Post_{i,t} + \alpha_6 Revision0_{i,d} \times Post_{i,t} + \alpha_7 Revision1_{i,d} \times Post_{i,t} \\ & + \alpha_8 CECL_i \times PreRevision1_{i,d} \times Post_{i,t} \\ & + \alpha_9\, CECL_i \times Revision0_{i,d} \times Post_{i,t} + \alpha_{10}\, CECL_i \times Revision1_{i,d} \times Post_{i,t} \\ & + Controls + Bank\text{-}Analyst\ FE + Day\ FE + \varepsilon_{i,d} \end{aligned} \tag{4}$$

Subscripts *i*, *d*, and *t* index bank, day, and quarter, respectively. The dependent variable, *DV,* is either *BASpread*, a proxy for information asymmetry defined as the percent quoted bid-ask spread based on Daily Trade and Quote (DTAQ) data, or *AbsAbRet*, a proxy for stock price reaction defined as the daily absolute market-adjusted stock return. Equation (4) includes indicators identifying day d0, d+1, and d-1 (with d0 being the announcement day of an analyst provision forecast revision, and d-2 being the benchmark day). Specifically, *Revision0* is an indicator variable equal to one if day d is the announcement day of an analyst provision forecast revision (i.e., day d0), and zero otherwise. We also include *Revision1*, an indicator variable equal to one if day d is the trading day immediately after the announcement day (i.e., day d+1) and zero otherwise, because market reactions may extend into a second day. *PreRevision1* is an indicator variable equal to one if day d is the trading day immediately before the announcement day (i.e., day d-1), and zero otherwise. Since the tests utilize four daily observations around the forecast revision date (i.e., [d-2, d+1]), the dependent variables two days before the announcement day are captured by the intercept $\alpha_0$. Thus, the coefficients on the day indicators *PreRevision1*, *Revision0*, and *Revision1* capture the change in bid-ask spread or absolute abnormal stock return on those days relative to two days before the announcement day. Our variables of interest in equation (4) are the three-way interaction terms *CECL* × *Revision0* × *Post* and *CECL* × *Revision1* × *Post*.

Following Amiram et al. (2016), we control for market makers' processing costs by including daily stock price (*Price*; see Stoll 1978); stock liquidity, which is shown by Demsetz (1968) to affect inventory holding costs, by including average daily stock turnover in the prior quarter (*Turnover*); inventory risk by including the standard deviation of daily stock returns in the prior quarter (*RetVol*); and the natural logarithm of market capitalization (*LogMVE*). To control for market makers' potential adjustment to depth to protect against inventory risk or information asymmetry, we include daily quoted depth (*Depth*). To further control for inventory risk and differential news content, we include daily trading volume (*Volume*). We further control for news content by including the absolute magnitude of analyst provision forecast revisions (*AbsRevisionMagnitude*). Furthermore, when the dependent variable is daily bid-ask spread, we include daily absolute abnormal stock returns as an additional control for news content; when the dependent variable is daily absolute abnormal stock returns, we control for daily bid-ask spread. We control for the horizon of the forecast (*LogHorizon*), defined as the natural logarithm of the number of days between the consensus forecast date and the quarterly earnings announcement date. We control for bank-analyst fixed effects and day fixed effects, and cluster standard errors by bank-analyst. We obtain intra-day data on quoted bid-ask spread and depth from a WRDS dataset called "Millisecond Intraday Indicators by WRDS" (both of which have been computed by WRDS based on millisecond TAQ data following the methodology in Holden and Jacobsen, 2014)[25].

One shortcoming of equation (4) is that it does not consider the direction of stock price changes or analyst provision forecast revisions, nor does it link the magnitude of stock price changes to the magnitude of analyst provision forecast revisions. To address this design issue, we examine the relation between signed abnormal stock returns and the magnitude of analyst provision forecast revisions by estimating the following regression:

$$\begin{aligned} AbRet_{[d0,\, d+1]i,t} = {} & \alpha_0 + \alpha_1 Revision_{i,t} + \alpha_2 Post_{i,t} + \alpha_3 Revision_{i,t} \times Post_{i,t} \\ & + \alpha_4 Revision_{i,t} \times CECL_i + \alpha_5 Revision_{i,t} \times CECL_i \times Post_{i,t} \\ & + Controls + Bank\text{-}Analyst\ FE + Day\ FE + \varepsilon_{i,t} \end{aligned} \quad (5)$$

Subscripts *i* and *t* index bank and quarter, respectively. The dependent variable, *AbRet*, is the cumulative market-adjusted stock return from the day of to one trading day after the announcement of an analyst forecast revision for bank *i*'s provision for quarter *t*. *Revision* is the revision to an

[25] The specific variable we use from the "Millisecond Intraday Indicators by WRDS" dataset is named "QuotedSpread_Percent_tw" in the dataset, which is defined as time-weighted percent quoted spread during market hours. Our results are robust to using the daily effective bid-ask spread, which is the "EffectiveSpread_Percent_tw" variable from the WRDS dataset.

individual analyst's provision forecast, measured as the analyst's most recent forecast minus her/his current forecast, scaled by lagged total assets, with a positive (negative) value of *Revision* indicating a decrease (an increase) in the provision estimate and hence good (bad) news. The test variable is *Revision* × *CECL* × *Post*. In addition to including the same set of control variables as in equation (1), equation (5) also controls for the time horizon of the forecasts (*LogHorizon*). We report summary statistics for the variables used in the tests of equations (4) and (5) in Table 7.

Columns 1 and 2 of Table 8, Panel A report OLS estimations of equation (4) using daily bid-ask spreads (*BASpread*) and absolute abnormal stock returns (*AbsAbRet*), respectively, from day d-1 to day d+1 as the dependent variables. In column 1, the estimated coefficients on both *CECL* × *Revision0* × *Post* and *CECL* × *Revision1* × *Post* are significantly positive, indicating that CECL adoption is associated with a decrease in the ability of analyst provision forecast revisions to reduce information asymmetry. In column 2, while the coefficient on *CECL* × *Revision0* × *Post* is insignificant, the coefficient on *CECL* × *Revision1* × *Post* is significantly negative, suggesting that CECL adoption is associated with muted absolute stock price reactions to forecast revisions.

Panel B of Table 8 reports OLS estimations of equation (5), with the dependent variable being the cumulative market-adjusted stock return around the announcement of an analyst provision forecast revision ($AbRet_{[t0,t+1]}$). The coefficient on *CECL* × *Revision* × *Post* is significantly negative, indicating that CECL adoption is associated with muted signed stock price reactions to signed analyst provision forecast revisions. Taken together, the results of the market reaction tests in Table 8 suggest that investors perceive analyst provision forecasts to be less informative post CECL adoption.

### 6.5 Robustness Tests

We conduct four sets of robustness tests. The first robustness test involves the inclusion of additional controls for bank size in equation (1). We conduct this test because SRCs (non-SRCs) comprise our control (treatment) observations, which could lead to a concern that our main findings are driven by size differences between the two groups. The inclusion of the natural logarithm of total assets (*LogAssets*) as a control variable in equation (1) should help mitigate this concern. The relation between forecast properties and bank size, however, could be nonlinear, and could differ in the pre- versus post-period. To further mitigate the size concern, we additionally control for the square and cubic forms of *Assets* (*AssetsSquared* and *AssetsCubed*, respectively) as well as their interaction terms with the *Post* indicator variable in equation (1). We present the results in Panel A of Table 9. Across all three columns, our results are robust to the inclusion of these additional size controls, as the coefficients on *CECL* × *Post* remain statistically significant and maintain the same sign as those in the main analyses reported in Table 2.

To further allay concerns that our main results are driven by differences in size between treatment and control banks, we test whether our main results are robust to trimming the sample based on total assets until there is no statistical difference in the mean of total assets between treatment and control banks. Specifically, we first calculate the bank's largest value of total assets during the sample period. Then, using this value, we drop the largest (smallest) 40% of treatment (control) banks.[26] Finally, using this subsample of banks that are more comparable in terms of size, we re-run equation (1) and also conduct tests of differences in the mean of total assets across treatment and control banks. We report the results of our second robustness test in Panel B of Table 9. Across all three columns, the coefficients on *CECL* × *Post* remain statistically significant and maintain the same sign as those in the main analyses reported in columns 1–3 of Table 2.

[26] For this robustness test, we dropped the largest (smallest) treatment (control) banks in increments of 5% until the difference in the mean of total assets between treatment and control banks was insignificant. Using this approach, dropping the largest (smallest) 40% of treatment (control) banks was the fewest number of observations we could drop while achieving no statistical difference in the mean of total assets between treatment and control banks.

Furthermore, at the bottom of Panel B, we find that the difference in the mean of total assets between treatment and control banks is statistically insignificant (*p*-value between 0.221 and 0.506).

In our main analysis, we use lagged total assets as the scalar for defining our provision forecast error variable |*LLPForecastError*| and dispersion variable *LLPForecastDispersion*. In our third robustness test, we use lagged total loans ($TotalLoans_{t-1}$) and lagged market value of equity ($MVE_{t-1}$) as alternative scalars and report results in columns 1–2 (3–4) of Panel C of Table 9. Across all four columns, the coefficients on *CECL* × *Post* remain statistically significant and maintain the same sign as those in the main analyses reported in columns 1 and 2 of Table 2.

In our primary tests, we measure analyst coverage as the number of analysts providing loan loss provision forecasts in SPCIQ. In our fourth robustness test, we measure analyst coverage in three alternative ways and report the results in Panel D of Table 9. In column 1 of Panel D, we measure coverage as the natural logarithm of the number of analyst EPS forecasts included in the consensus in the IBES database (*LogNumEPSEst*) and we utilize the same sample used in our main analysis in column 3 of Table 2 (although we drop five of the 4,444 observations because of missing IBES coverage). In columns 2 and 3 of Panel D, rather than dropping observations with missing *LogNumLLPEst* or *LogNumEPSEst*, we include those observations and set the number of forecasts to zero. In column 2, the dependent variable is the natural logarithm of one plus the number of analyst LLP forecasts from SPCIQ that are included in the consensus (*LogNumLLPEst*). Column 3 of Panel C is analogous to column 2 using the number of IBES EPS forecasts (*LogNumEPSEst*). Across all three columns, the coefficients on *CECL* × *Post* remain statistically significant and maintain the same sign as those tabulated in column 3 of Table 2.

## 6.6 Placebo Tests

One concern with comparing non-SRC and SRC banks as treatment and control banks is that the treatment banks are larger than control banks. It is possible that larger banks are more affected by any crisis period, which could mean that analysts face greater forecasting difficulties for larger banks during recessionary periods. This concern would apply to our setting since the post-period includes the 2020Q1 recession caused by the COVID-19 pandemic. While we already use a falsification test based on analyst EBLLP forecasts (Section 5.1), a by-year analysis (Section 6.3), and additional robustness tests related to size (Section 6.5) to mitigate these concerns, we further mitigate these concerns via placebo tests using the 2008Q1–2009Q2 recession (which preceded CECL). We use two approaches for identifying non-SRC (i.e., CECL) and SRC (i.e., control) banks for the placebo tests. One approach is to retain banks' SRC classification used in our main analyses and use this same classification in the placebo period; Panel A of Table 10 presents the results using this "maintain classification approach." The other approach is to reclassify banks as SRC vs. non-SRC using the SEC's classification during the placebo period, which is consistent with the original CECL rollout schedule;[27] Panel B of Table 10 presents the results using this "reclassification approach." Unfortunately, analyst provision forecast data are not available in SPCIQ until 2008Q3, which precludes using the period before 2008Q1 as the pre-period for the placebo test. Thus, the sample period for our placebo test starts with the recessionary period of 2008Q3–2009Q2, followed by a non-recessionary period of 2009Q3–2010Q2. *PlaceboPre* is an indicator variable equal to one for the recessionary quarters 2008Q3–2009Q2, and zero for the non-recessionary quarters 2009Q3–2010Q2.

In column 1 across the two panels of Table 10, neither of the coefficients on *CECL* ×

[27] In 2018, the SEC raised the thresholds in the smaller reporting company (SRC) definition, as defined in Item 10(f)(1) of Regulation S-K (see https://www.sec.gov/corpfin/amendments-smaller-reporting-company-definition).

*PlaceboPre* is statistically different from zero where the dependent variable is provision forecast error. Thus, the treatment banks exhibit no evidence of differentially greater provision forecast error than the control banks during the recessionary period of 2008Q3–2009Q2 relative to the non-recessionary period of 2009Q3–2010Q2. In column 2 of Table 10, where the dependent variable is provision forecast dispersion, the coefficient on *CECL* × *PlaceboPre* is not statistically different from zero using the maintain classification approach in Panel A, but is significantly negative using the reclassification approach in Panel B (-0.021, $t$ = -1.76), with the latter result indicating that the non-SRC banks experienced *smaller* forecast dispersion during the recessionary period compared to the non-recessionary period. This latter result is inconsistent with the alternative explanation that the increase in forecast dispersion after CECL adoption, which we report in Table 2, is driven by the recession related to the COVID-19 pandemic.

In column 3 of Table 10, where the dependent variable is provision forecast coverage (*LogNumLLPEst*), the coefficient on *CECL* × *PlaceboPre* is significantly negative in both panels. This suggests that provision forecast coverage is higher in the non-recessionary period of 2009Q3–2010Q2 than in the recessionary period of 2008Q3–2009Q2 for the non-SRCs relative to the SRCs. This result suggests a recovery (i.e., increase) in provision forecast coverage after the recession ended in 2009Q2 for the non-SRCs relative to the SRCs, which is inconsistent with the results of our by-year analyses presented in column 3 of Table 6, Panel A. More specifically, the results in Table 6 suggest that forecast coverage was *not* incrementally higher during the non-recessionary period of 2021 than during the recessionary period of 2020 for the non-SRCs relative to the SRCs. Put differently, our by-year analyses in Section 6.3 suggest no evidence of recovery (i.e., increase) in forecast coverage for the non-SRCs relative to the SRCs after the 2020 recession ended, which is inconsistent with what we observe in the placebo test.

Taken together, the results of our placebo tests using the crisis preceding CECL help mitigate the concern that our main findings may be driven by treatment and control banks being affected differently by the 2020Q1 COVID-19 recession due to their size differences.

### 6.7 Probing the Parallel Trends Assumption

Our difference-in-differences research design relies on the parallel trends assumption that differences in the properties of analyst provision forecasts between the treatment and control banks would have remained unchanged had the treatment banks not adopted CECL. While this assumption is untestable, we examine trends in the properties of analyst provision forecasts in the pre-period to gauge its plausibility. To do so, we define an indicator variable for each of the four quarters of 2019 (e.g., the indicator variable *2019Q1* equals one for 2019Q1, and zero otherwise), with 2018 serving as the benchmark. Our test variables are the interaction term between the CECL indicator and each of these four quarter indicators (e.g., *CECL* × *2019Q1*). We report the results from this diagnostic analysis in Table 11. Across all three columns in Table 11, only one of the coefficients on the interaction terms is statistically different from zero, with the only exception being *CECL* × *2019Q2* when the dependent variable is forecast dispersion (0.002, $t$ = 1.82). The magnitude of the coefficient, however, is only about one-seventh of the coefficient of 0.014 on *CECL* × *Post* in our main analysis reported in column 2 of Table 2. Overall, we find little evidence inconsistent with the reasonableness of the parallel trends assumption.

## 7. Conclusion

We examine how the adoption of the Current Expected Credit Loss (CECL) standard, which imposes the most significant changes to bank accounting in decades, affects the properties of financial analysts' loan loss provision forecasts. Under the previous incurred loss model, managers are required to delay credit loss recognition until it is "probable" that the loss has been "incurred." In contrast, CECL requires managers to recognize all lifetime expected credit losses upon loan origination or acquisition. To our knowledge, we provide the first empirical evidence on the impact of CECL adoption on three important properties of financial analyst forecasts of loan loss provision: accuracy, dispersion, and coverage. We find that CECL adoption is associated with a reduction in the accuracy and coverage, and an increase in the dispersion, of analyst provision forecasts, suggesting a deterioration in the attributes of financial analysts' provision forecasts after CECL adoption. While our study contributes to existing research by documenting potential costs of expected credit loss standards, our results do not suggest that the CECL standard is undesirable. Rather, our study provides some evidence, from the perspectives of financial analysts, that can become part of a complex mosaic of the various costs and benefits of the CECL standard.

Despite our falsification tests using pre-provision earnings and our evidence that the degradation in the properties of analyst provision forecasts after CECL adoption occurs not only in 2020 (which encompasses the short-lived recession caused by the COVID-19 pandemic), but also in 2021, we offer the caveat that we cannot fully rule out the possibility that our main finding may be at least partly driven by the pandemic and the resulting greater macroeconomic uncertainty. Thus, we caution against the generalization of our findings to periods in a more "normal" economic environment.

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## Appendix A: Variable Definitions (in Alphabetical Order)

| Variable | Definition |
|---|---|
| 2019Q1–2019Q4 | 2019Q1 (2019Q2, 2019Q3, 2019Q4) is an indicator variable equal to 1 if the quarter is 2019Q1 (2019Q2, 2019Q3, 2019Q4) and 0 otherwise. |
| $AbRet_{[t0,t+1]}$ | Cumulative market-adjusted stock return (calculated using *ret* and *vwretd* from CRSP) measured during the window [t0, t+1], where t0 is set to the announcement date of an analyst forecast revision (or, the next available trading date if the revision date is not a trading day) for bank *i*'s provision for quarter *t*; t+1 is set to the next trading day immediately after t0. |
| AbsAbRet | Daily absolute market-adjusted stock returns in percent form: (*ret* – *vwretd*) x 100 from CRSP. |
| AbsRevisionMagnitude | The absolute magnitude of analyst provision forecast revisions, measured as the absolute value of the difference between an analyst's current provision forecast and the most recent previous provision forecast, scaled by lagged total assets (SPCIQ Keyfield 280297). Individual analyst forecasts are manually downloaded for each bank from the SPCIQ website under the Detailed Estimates tab. |
| AssetsCubed | The cubic form of total assets in millions (SPCIQ Keyfield 280297). |
| AssetsSquared | The square form of total assets in millions (SPCIQ Keyfield 280297). |
| BASpread | Time-weighted percent quoted spread during market hours. It is the "QuotedSpread_Percent_tw" variable from the "Millisecond Intraday Indicators by WRDS" dataset, which has been computed by WRDS following the methodology in Holden and Jacobsen (2014). The weights are based on the amount of time during a trading day that the spreads are in force. |
| BigN | An indicator variable equal to 1 if a bank is audited by a Big-N auditor as of the most recent fiscal year and 0 otherwise (calculated using *au* from Compustat). |
| CECL | An indicator variable equal to 1 for our treatment banks (i.e., the non-SRC banks), and 0 for our control banks (i.e., the SRC banks), except for Panel B of Table 10. In Panel B of Table 10, we redefine *CECL* based on the bank's SRC/non-SRC status during the period of the placebo test. |
| Consensus | BKLS consensus, measured as the across-analyst correlation in analyst forecasts errors, and computed following Barron et al. (1998): $Consensus = (se - d/n)/TotalUncertainty$ where $n = \exp(LogNumLLPEst)$, $d = LLPForecastDispersion^2$, $se = LLPForecastError^2$ |
| ConsumerLoans | Total consumer loans outstanding (SPCIQ Keyfield 290161), scaled by total assets (SPCIQ Keyfield 280297). |
| Day1Impact | The CECL day-one impact on retained earnings (SPCIQ Keyfield 319094, supplemented by hand collection from regulatory filings and SEC EDGAR filings as described in the manuscript), scaled by total assets at the end of the quarter immediately before adoption (SPCIQ 280297) and multiplied by -1 so that a more positive *Day1Impact* suggests a greater increase in credit loss allowances upon day-one CECL adoption. The CECL day one impact on retained earnings captures the cumulative-effect adjustment for the changes in the allowance for credit losses, net of any related deferred tax assets and excluding the initial allowance gross-up for any purchased |

| | |
|---|---|
| | credit-deteriorated assets, recognized in retained earnings as of the adoption date. |
| Depth | Daily quoted depth, defined as the average of the time-weighted best offer share depth during market hour (which is the "BestOfrDepth_Share_tw" variable from the "Millisecond Intraday Indicators by WRDS" dataset) and best bid share depth during market hour (which is the "BestBidDepth_Share_tw" variable from the same dataset). |
| EBLLPForecastDispersion | Dispersion of analyst pre-provision earnings (EBLLP) forecasts, defined as the standard deviation of the individual analyst EBLLP forecasts included in the consensus (SPCIQ Keyfield 333394), scaled by lagged total assets (SPCIQ Keyfield 280297). |
| \|EBLLPForecastError\| | Forecast errors in analyst forecasts of pre-provision earnings (EBLLP), defined as the absolute difference between the consensus (mean) analyst EBLLP forecast (SPCIQ Keyfield 333390) and the actual EBLLP for the bank-quarter (defined as earnings plus the loan loss provision, where earnings are SPCIQ Keyfield 329294 (Keyfield 329255 if Keyfield 329294 is missing) and the loan loss provision is defined the same as $\Delta LLP$), scaled by lagged total assets (SPCIQ Keyfield 280297) following Beatty and Liao (2021). |
| ΔGDP | The percentage change in GDP during the quarter. These data are obtained from the Bureau of Economic Analysis ("Table 1.1.5 Gross Domestic Product"). |
| GDPBeta | The sensitivity of a bank's $\Delta NPL$ to $\Delta GDP$. To calculate *GDPBeta*, we first run the following regression: <br> $\Delta NPL_t = \beta_0 + \beta_1 \Delta GDP_{t-1} + \varepsilon_t$ <br> This regression is run by bank over the 2013–2019 time period. *GDPBeta* is the bank's coefficient on $\Delta GDP$ (i.e., $\beta_1$) obtained from this regression, multiplied by -1 so that *GDPBeta* is increasing in the bank's NPL-GDP sensitivity. |
| HighImpact | An indicator variable set to 1 if the bank's 2019Q4 total consumer loans (SPCIQ Keyfield 290161), scaled by total assets (SPCIQ Keyfield 280297), and unused commitments (SPCIQ Keyfield 281191), scaled by total assets (SPCIQ Keyfield 280297), are both in the top quartile of the sample, 0 otherwise. |
| HighIntIncome | An indicator variable set to 1 if the bank's 2019Q4 interest income (SPCIQ Keyfield 280322), scaled by total loans (SPCIQ Keyfield 290178), is in the top quartile of the sample, 0 otherwise. |
| ΔLLP | The loan loss provision in the current quarter minus the loan loss provision in quarter t-1, scaled by total assets (SPCIQ Keyfield 280297). For the loan loss provision, we first use SPCIQ Keyfield 271740; if Keyfield 271740 is missing, we use Keyfield 272026 and then Keyfield 309178 as needed to preserve sample size. |
| LLPForecastDispersion | Dispersion of analyst loan loss provision forecasts, defined as the standard deviation of the individual analyst provision forecasts included in the consensus (SPCIQ Keyfield 332442), scaled by lagged total assets (SPCIQ Keyfield 280297). |
| \|LLPForecastError\| | Forecast errors in analyst forecasts of loan loss provisions, which we use as an inverse measure of analyst forecast accuracy. It is defined as the absolute difference between the consensus (mean) analyst provision forecast (SPCIQ Keyfield 332438 and the actual provision for the bank- |

| | |
|---|---|
| | quarter (defined the same as $\Delta LLP$), scaled by lagged total assets (SPCIQ Keyfield 280297) following Beatty and Liao (2021). |
| LogAssets | The natural logarithm of total assets in millions (SPCIQ Keyfield 280297). We report the untransformed variable (i.e., *Assets*) in Table 1 and Table 9, Panel A. |
| LogHorizon | The natural logarithm of the number of days between the analyst's forecast revision date (obtained from the SPCIQ website as described in the definition of *AbsRevisionMagnitude*) and the quarterly earnings announcement date (*rdq* from Compustat). We report the untransformed variable (i.e., *Horizon*) in Table 7. |
| LogMVE | The natural logarithm of the market value of equity in millions (*prccq* x *cshoq* from the Compustat Fundamentals Quarterly table; if those variables are missing, we use *prccm* x *cshoq* from the Compustat Security Monthly table). We report the untransformed variable (i.e., *MVE*) in Table 7 and we use *MVE* in thousands as an alternative scalar in Panel C of Table 9. |
| LogNumEBLLPEst | The natural logarithm of the number of pre-provision earnings forecasts (EBLLP) included in the consensus (SPCIQ Keyfield 333395). |
| LogNumEPSEst | The natural logarithm of the number of analyst EPS forecasts included in the consensus (*numest* from the Statistics Summary table in IBES when *measure* equals "EPS" and *fiscalp* equals "QTR"). |
| LogNumLLPEst | The natural logarithm of the number of analyst provision forecasts included in the consensus (SPCIQ Keyfield 332443). We report the untransformed variable (i.e., *NumLLPEst*) in Table 1. |
| Loss | An indicator variable equal to 1 if net income (SPCIQ Keyfield 280344) is negative for the quarter and 0 otherwise. |
| MTB | The market value of equity divided by the book value of equity. The market value of equity is calculated as described for *LogMVE*. The book value of equity is calculated as total shareholders' equity (*seqq* from Compustat; if missing, we use (i) *ceqq* + *pstkq* and then (ii) *atq* – *ltq* – *mibq* from Compustat) minus preferred shareholders equity (*pstkq* in Compustat). To preserve sample size, if the components of *MTB* are missing from Compustat, we use the market value of equity (SPCIQ Keyfield 275838, multiplied by 1,000) and book value of equity (SPCIQ Keyfield 280318) from SPCIQ. |
| $\Delta NPL$ | The change in non-performing loans (i.e., the sum of non-accrual loans and loans 90 days past due; SPCIQ Keyfields 281530 and 281489, respectively) during the quarter scaled by lagged total assets (SPCIQ Keyfield 280297). |
| PlaceboPre | An indicator variable equal to one for the recessionary quarters 2008Q3–2009Q2 and zero for the non-recessionary quarters of 2009Q3–2010Q2. |
| Post | An indicator variable equal to one if the bank has adopted CECL by the beginning of the quarter, and zero otherwise. |
| Post2020 (Post2021) | *Post2020* (*Post2021*) is an indicator variable equal to one if *Post* is equal to one and the year is 2020 (2021), and zero otherwise. |
| PreRevision1 | An indicator variable equal to one if day d is the trading day immediately prior to the date of the analyst's provision forecast revision (i.e., day d-1), and zero otherwise. Individual provision forecast revisions are obtained from the SPCIQ website as described in the definition of *AbsRevisionMagnitude*. |
| Price | Stock price on day d (abs(*prc*) from CRSP). |

| | |
|---|---|
| PrivatePrecision | The precision of analysts' idiosyncratic information, computed following Barron et al. (1998), and then divided by 1,000,000 for presentation purposes: <br> $PrivatePrecision = d/TotalUncertainty^2$ <br> where $n = \exp(LogNumLLPEst)$ <br> $d = LLPForecastDispersion^2$ <br> $se = LLPForecastError^2$ |
| PublicPrecision | The precision of analysts' public information, computed following Barron et al. (1998), and then divided by 1,000,000 for presentation purposes: <br> $PublicPrecision = (se - d/n)/TotalUncertainty^2$ <br> where $n = \exp(LogNumLLPEst)$ <br> $d = LLPForecastDispersion^2$ <br> $se = LLPForecastError^2$ |
| RealEstateLoans | Total real estate loans outstanding (SPCIQ Keyfield 290155), scaled by total assets (SPCIQ Keyfield 280297). |
| RetVol | The standard deviation of daily stock return (*ret* from CRSP) during the quarter. |
| Revision | The signed magnitude of analyst provision forecast revisions, measured as the analyst's most recent provision forecast minus the current provision estimate, scaled by lagged total assets; a positive (negative) value of *Revision* indicates a decrease (increase) in the provision estimate and thus good (bad) news. Individual provision forecast revisions are obtained from the SPCIQ website as described in the definition of *AbsRevisionMagnitude*. |
| Revision0 | An indicator variable equal to one if day d is the trading day of the analyst's provision forecast revision (i.e., day d0), and zero otherwise. Individual provision forecast revisions are obtained from the SPCIQ website as described in the definition of *AbsRevisionMagnitude*. |
| Revision1 | An indicator variable equal to one if day d is the trading day immediately after the date of the analyst's provision forecast revision (i.e., day d+1), and zero otherwise. Individual provision forecast revisions are obtained from the SPCIQ website as described in the definition of *AbsRevisionMagnitude*. |
| Tier1Ratio | The tier 1 risk-based capital ratio (SPCIQ Keyfield 280216). |
| TotalLoans | Total loans outstanding in thousands (SPGMI Keyfield 290178). We use this variable as an alternative scalar in Panel C of Table 9. |
| TotalUncertainty | Total uncertainty, which reflects the sum of the uncertainty associated with analysts' private information and the uncertainty associated with information common to all analysts, computed following Barron et al. (1998), and then multiplied by 1,000,000 for presentation purposes: <br> $TotalUncertainty = (1 - 1/n)*d + se$ <br> where $n = \exp(LogNumLLPEst)$ <br> $d = LLPForecastDispersion^2$ <br> $se = LLPForecastError^2$ |
| Turnover | The average daily share turnover during the quarter, where share turnover is calculated as trading volume (*vol* from CRSP) divided by the number of shares outstanding (*shrout* from CRSP, multiplied by 10). |
| Volume | Trading volume on day d in millions(*vol* from CRSP). |

**Table 1**
Summary Statistics for Loan Loss Provision Forecast Properties Sample

**Panel A: CECL Banks**

| | Pre-Period | | | | | | Post-Period | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | N | Mean | Std. Dev. | P25 | P50 | P75 | N | Mean | Std. Dev. | P25 | P50 | P75 |
| $\lvert LLPForecastError\rvert_t$ | 1,752 | 0.023 | 0.039 | 0.005 | 0.011 | 0.024 | 1,306 | 0.076*** | 0.081 | 0.022 | 0.050 | 0.099 |
| $LLPForecastDispersion_t$ | 1,655 | 0.010 | 0.017 | 0.003 | 0.005 | 0.008 | 1,259 | 0.033*** | 0.047 | 0.009 | 0.017 | 0.037 |
| $NumLLPEst_t$ | 1,752 | 5.698 | 2.918 | 4.000 | 5.000 | 7.000 | 1,306 | 5.741 | 2.905 | 4.000 | 5.000 | 7.000 |
| $MTB_{t-1}$ | 1,752 | 1.434 | 0.477 | 1.113 | 1.356 | 1.643 | 1,306 | 1.193*** | 0.518 | 0.850 | 1.097 | 1.358 |
| $RetVol_{t-1}$ | 1,752 | 0.017 | 0.008 | 0.013 | 0.015 | 0.018 | 1,306 | 0.028*** | 0.014 | 0.018 | 0.024 | 0.035 |
| $BigN_{t-1}$ | 1,752 | 0.515 | 0.500 | 0.000 | 1.000 | 1.000 | 1,306 | 0.568** | 0.496 | 0.000 | 1.000 | 1.000 |
| $Loss_{t-1}$ | 1,752 | 0.007 | 0.086 | 0.000 | 0.000 | 0.000 | 1,306 | 0.044*** | 0.204 | 0.000 | 0.000 | 0.000 |
| $\Delta LLP_{t-1}$ | 1,752 | 0.031 | 0.615 | -0.106 | 0.000 | 0.138 | 1,306 | -0.062*** | 1.363 | -0.615 | -0.088 | 0.327 |
| $Tier1Ratio_{t-1}$ | 1,752 | 12.956 | 2.664 | 11.300 | 12.250 | 13.680 | 1,306 | 12.841 | 2.153 | 11.430 | 12.390 | 13.810 |
| $ConsumerLoans_{t-1}$ | 1,752 | 0.044 | 0.076 | 0.004 | 0.013 | 0.053 | 1,306 | 0.045 | 0.080 | 0.004 | 0.013 | 0.052 |
| $RealEstateLoans_{t-1}$ | 1,752 | 0.488 | 0.179 | 0.401 | 0.515 | 0.609 | 1,306 | 0.423*** | 0.172 | 0.337 | 0.438 | 0.545 |
| $Assets_{t-1}$ | 1,752 | 62,997.490 | 263,406.800 | 4,640.140 | 8,145.850 | 20,541.850 | 1,306 | 94,310.730*** | 315,381.900 | 6,868.540 | 14,968.150 | 35,327.790 |
| $TotalUncertainty_t$ | 1,655 | 0.232 | 1.153 | 0.004 | 0.015 | 0.072 | 1,259 | 1.712*** | 3.689 | 0.081 | 0.343 | 1.255 |
| $Consensus_t$ | 1,655 | 0.640 | 0.380 | 0.419 | 0.793 | 0.943 | 1,259 | 0.688*** | 0.371 | 0.513 | 0.865 | 0.968 |
| $PublicPrecision_t$ | 1,655 | 80.799 | 191.855 | 4.534 | 28.909 | 84.598 | 1,259 | 19.120*** | 110.219 | 0.363 | 1.624 | 6.812 |
| $PrivatePrecision_t$ | 1,655 | 155.222 | 416.375 | 0.910 | 10.780 | 94.109 | 1,259 | 39.149*** | 257.167 | 0.037 | 0.335 | 3.031 |

**Table 1** (*continued* )

**Panel B: Non-CECL Banks**

| | Pre-Period | | | | | | Post-Period | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | N | Mean | Std. Dev. | P25 | P50 | P75 | N | Mean | Std. Dev. | P25 | P50 | P75 |
| $\|LLPForecastError\|_t$ | 718 | 0.023 | 0.039 | 0.006 | 0.014 | 0.025 | 668 | 0.054*** | 0.063 | 0.014 | 0.035 | 0.070 |
| $LLPForecastDispersion_t$ | 535 | 0.007 | 0.007 | 0.003 | 0.005 | 0.008 | 485 | 0.018*** | 0.019 | 0.007 | 0.012 | 0.024 |
| $NumLLPEst_t$ | 718 | 2.897 | 1.909 | 1.000 | 3.000 | 4.000 | 668 | 2.632 | 1.467 | 1.000 | 2.000 | 3.000 |
| $MTB_{t-1}$ | 718 | 1.363 | 0.335 | 1.140 | 1.306 | 1.530 | 668 | 1.003*** | 0.320 | 0.774 | 0.963 | 1.132 |
| $RetVol_{t-1}$ | 718 | 0.016 | 0.006 | 0.012 | 0.015 | 0.019 | 668 | 0.028*** | 0.016 | 0.016 | 0.024 | 0.036 |
| $BigN_{t-1}$ | 718 | 0.099 | 0.299 | 0.000 | 0.000 | 0.000 | 668 | 0.043** | 0.204 | 0.000 | 0.000 | 0.000 |
| $Loss_{t-1}$ | 718 | 0.025 | 0.156 | 0.000 | 0.000 | 0.000 | 668 | 0.016 | 0.127 | 0.000 | 0.000 | 0.000 |
| $\Delta LLP_{t-1}$ | 718 | 0.011 | 0.569 | -0.113 | 0.000 | 0.138 | 668 | -0.004 | 0.995 | -0.403 | 0.000 | 0.294 |
| $Tier1Ratio_{t-1}$ | 718 | 13.477 | 2.867 | 11.530 | 12.580 | 14.830 | 668 | 13.695 | 2.748 | 11.935 | 13.000 | 14.840 |
| $ConsumerLoans_{t-1}$ | 718 | 0.025 | 0.046 | 0.002 | 0.009 | 0.026 | 668 | 0.015** | 0.027 | 0.002 | 0.007 | 0.018 |
| $RealEstateLoans_{t-1}$ | 718 | 0.607 | 0.129 | 0.526 | 0.605 | 0.706 | 668 | 0.564*** | 0.124 | 0.483 | 0.567 | 0.643 |
| $Assets_{t-1}$ | 718 | 4,409.538 | 20,885.780 | 1,224.065 | 1,630.542 | 2,197.166 | 668 | 2,472.738 | 1,735.346 | 1,512.002 | 2,051.564 | 2,783.783 |
| $TotalUncertainty_t$ | 535 | 0.228 | 1.290 | 0.007 | 0.023 | 0.061 | 485 | 0.641*** | 1.702 | 0.035 | 0.144 | 0.468 |
| $Consensus_t$ | 535 | 0.655 | 0.432 | 0.442 | 0.870 | 0.969 | 485 | 0.641 | 0.429 | 0.367 | 0.835 | 0.969 |
| $PublicPrecision_t$ | 535 | 53.798 | 170.196 | 5.158 | 21.064 | 54.852 | 485 | 14.350*** | 83.237 | 0.679 | 3.086 | 10.416 |
| $PrivatePrecision_t$ | 535 | 150.718 | 441.862 | 0.520 | 5.425 | 56.040 | 485 | 44.872*** | 251.873 | 0.084 | 0.883 | 10.126 |

This table reports descriptive statistics and univariate tests of differences in the mean for the main samples, with the sample of CECL banks reported in Panel A and non-CECL banks reported in Panel B. Tests of differences in the mean are based on OLS regressions with the *Post* indicator included in the model to allow for clustering of standard errors by firm. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 2**
Effects of CECL adoption on analyst forecasts of loan loss provision

| Dependent Variable: | Column (1)<br>*\|LLPForecastError\|* | Column (2)<br>*LLPForecastDispersion* | Column (3)<br>*LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.005 | -0.008** | 0.079* |
| | (-0.71) | (-2.30) | (1.86) |
| **CECL x $Post_t$** | **0.021*** ** | **0.014*** ** | **-0.129*** ** |
| | **(4.31)** | **(6.64)** | **(-3.60)** |
| $MTB_{t-1}$ | 0.005** | 0.004** | -0.028 |
| | (2.48) | (2.50) | (-1.18) |
| $RetVol_{t-1}$ | -0.003** | 0.001 | -0.068*** |
| | (-1.99) | (0.64) | (-8.81) |
| $BigN_{t-1}$ | 0.001 | 0.003 | -0.006 |
| | (0.14) | (0.88) | (-0.10) |
| $Loss_{t-1}$ | 0.120*** | 0.032*** | -0.042 |
| | (7.62) | (3.42) | (-1.24) |
| $\Delta LLP_{t-1}$ | -0.002 | 0.003*** | 0.023*** |
| | (-1.05) | (5.68) | (4.48) |
| $LogNumLLPEst_t$ | -0.002 | 0.006*** | / |
| | (-0.66) | (4.53) | / |
| $Tier1Ratio_{t-1}$ | 0.005* | 0.000 | 0.016 |
| | (1.86) | (0.08) | (0.67) |
| $ConsumerLoans_{t-1}$ | 0.003 | 0.008* | -0.003 |
| | (0.30) | (1.73) | (-0.05) |
| $RealEstateLoans_{t-1}$ | 0.024*** | 0.007** | 0.081* |
| | (3.81) | (2.41) | (1.90) |
| $LogAssets_{t-1}$ | -0.046*** | -0.026*** | 0.138 |
| | (-3.65) | (-3.54) | (1.48) |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.03% | 36.06% | 85.23% |

This table reports the results of regressions examining the impact of CECL adoption on the attributes of analysts' LLP forecasts. *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Column 1, the dependent variable, *|LLPForecastError|*, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2, the dependent variable, *LLPForecastDispersion*, is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3, the dependent variable, *LogNumLLPEst*, is the natural log of the number of analyst provision forecasts included in the consensus. Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 3**
Falsification tests: EBLLP Forecasts and GDP Beta

**Panel A: Falsification Tests: Earnings Before Loan Loss Provision Forecasts**

| Dependent Variable: | Column (1) *\|EBLLPForecastError\|* | Column (2) *EBLLPForecastDispersion* | Column (3) *LogNumEBLLPEst* |
|---|---|---|---|
| $Post_t$ | 0.002 | 0.002 | 0.044 |
| | (0.23) | (0.98) | (0.78) |
| **CECL x $Post_t$** | **-0.005** | **-0.002** | **-0.049** |
| | **(-0.99)** | **(-1.27)** | **(-1.09)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 3,730 | 2,710 | 3,730 |
| Adjusted $R^2$ | 23.41% | 32.00% | 79.08% |

**Panel B: Replication of Main Results: Loan Loss Provision Forecasts**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.012 | -0.008* | 0.034 |
| | (-1.58) | (-1.86) | (0.72) |
| **CECL x $Post_t$** | **0.025*** ** | **0.016*** ** | **-0.085** ** |
| | **(5.02)** | **(5.18)** | **(-2.13)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 3,730 | 2,710 | 3,730 |
| Adjusted $R^2$ | 43.73% | 34.16% | 82.27% |

**Table 3** (*continued* )

**Panel C: Validation of GDP Beta**

| Dep. Var.: $\Delta NPL_t$ | Column (1) | Column (2) | Column (3) |
|---|---|---|---|
| GDPBeta | -0.004 | / | / |
| | (-1.55) | / | / |
| $\Delta GDP_{t-1}$ | -0.013*** | -0.007** | -0.005 |
| | (-4.72) | (-2.29) | (-1.40) |
| **GDPBeta x $\Delta GDP_{t-1}$** | **-0.005**** | **-0.005**** | **-0.005*** |
| | **(-2.04)** | **(-1.99)** | **(-1.78)** |
| Controls | No | No | Yes |
| Bank Fixed Effects | No | Yes | Yes |
| Year Fixed Effects | No | Yes | Yes |
| N | 4,444 | 4,444 | 4,444 |
| Adjusted $R^2$ | 1.06% | 3.12% | 4.91% |

**Panel D: GDP Beta Falsification Test**

| Dependent Variable: | Column (1) \|*LLPForecastError* \| | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.005 | -0.008** | 0.077* |
| | (-0.66) | (-2.34) | (1.81) |
| GDPBeta x $Post_t$ | 0.002 | -0.001 | -0.019 |
| | **(0.88)** | **(-0.75)** | **(-0.91)** |
| **CECL x $Post_t$** | **0.020***** | **0.014***** | **-0.126***** |
| | **(4.20)** | **(6.68)** | **(-3.50)** |
| **GDPBeta x CECL x $Post_t$** | **-0.003** | **0.001** | **0.000** |
| | **(-0.64)** | **(0.69)** | **(0.00)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.02% | 36.03% | 85.24% |

This table reports the results of falsification tests using analysts' earnings before loan loss provision (EBLLP) forecasts and firms' GDP betas. Panel A reports the results of the falsification tests that examine attributes of EBLLP forecasts, while Panel B reports the results of robustness tests that examine attributes of loan loss provision forecasts using a constant sample compared to Panel A. Panel C reports the results of validation tests of the GDP beta measure. Panel D reports the results of the falsification tests that examine cross-sectional differences in the main results based on firms' GDP beta measures. *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Column 1 Panel A (Panel B), the dependent variable, |*EBLLPForecastError* | (|*LLPForecastError* |), is the absolute difference between the consensus analyst EBLLP (provision) forecast, based on the mean, and the actual EBLLP (provision) for the quarter, scaled by lagged total assets. In Column 2 Panel A (Panel B), the dependent variable, *EBLLPForecastDispersion* (*LLPForecastDispersion* ), is the standard deviation of analyst EBLLP (provision) forecasts included in the consensus, scaled by lagged total assets. In Column 3 Panel A (Panel B), the dependent variable, *LogNumEBLLPEst* (*LogNumLLPEst* ), is the natural log of the number of analyst EBLLP (provision) forecasts included in the consensus. In Panel C, $\Delta NPL$ ($\Delta GDP$ ) is the change in non-performing loans, scaled by lagged total assets (percentage change in GDP). In Panels C and D, *GDPBeta* is calculated by running the following regression by firm over the time period 2013 - 2019:

$$\Delta NPL_t = \beta_0 + \beta_1 \Delta GDP_{t-1} + \varepsilon_t$$

*GDPBeta* is the estimated coefficient on $\Delta GDP_{t-1}$ (i.e., $\beta_1$) from this regression, multiplied by -1 so that *GDPBeta* is increasing in the bank's NPL-GDP sensitivity. Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 4**
Cross-sectional tests: Day-1 impact and consumer loans

**Panel A: Day-1 Impact Cross-Sectional Test**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | 0.003 | -0.004 | 0.066 |
| | (0.33) | (-1.11) | (1.52) |
| **CECL x $Post_t$** | **0.010*** | **0.009***** | **-0.109***** |
| | **(1.87)** | **(3.64)** | **(-2.85)** |
| **Day1Impact x $Post_t$** | **0.012***** | **0.006***** | **-0.021** |
| | **(3.76)** | **(3.08)** | **(-1.21)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.72% | 36.72% | 85.25% |

**Panel B: Consumer Loans and Unfunded Commitments Cross-Sectional Test**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.004 | -0.007** | 0.078* |
| | (-0.65) | (-2.15) | (1.82) |
| HighImpact x $Post_t$ | -0.005 | 0.003 | 0.161*** |
| | (-1.04) | (1.38) | (4.02) |
| **CECL x $Post_t$** | **0.019***** | **0.011***** | **-0.129***** |
| | **(3.82)** | **(5.35)** | **(-3.51)** |
| **HighImpact x CECL x $Post_t$** | **0.020*** | **0.023***** | **-0.145**** |
| | **(1.69)** | **(4.26)** | **(-2.57)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.11% | 37.46% | 85.23% |

**Table 4** (*continued*)

**Panel C: Interest Income Cross-Sectional Test**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.008 | -0.008** | 0.052 |
| | (-1.02) | (-2.35) | (1.03) |
| HighIntIncome x $Post_t$ | 0.010 | 0.001 | 0.094 |
| | (1.20) | (0.24) | (1.64) |
| **CECL x $Post_t$** | **0.017*** ** | **0.014*** ** | **-0.091** ** |
| | **(3.01)** | **(6.10)** | **(-2.04)** |
| **HighIntIncome x CECL x $Post_t$** | **0.021*** | **0.000** | **-0.142*** |
| | **(1.85)** | **(-0.06)** | **(-1.87)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.72% | 36.02% | 85.26% |

This table reports the results of cross-sectional regressions examining how the impact of CECL adoption on the attributes of analysts' LLP forecasts varies by the bank's reported day-1 impact (Panel A) and the bank's level of consumer loans (Panel B). *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Panel A, *Day1Impact* is the effect of day-one CECL adoption on retained earnings, multiplied by -1 so that the variable is increasing in the day-one impact of CECL adoption, and scaled by total assets at the end of the quarter immediately before the adoption of CECL. In Panel B, *HighImpact* is an indicator variable set to 1 if the bank's 2019Q4 total consumer loans, scaled by total assets, and unused commitments, scaled by total assets, are in the top quartile of the sample, 0 otherwise. In Panel C, *HighIntIncome* is an indicator variable set to 1 if the bank's 2019Q4 interest income, scaled by total loans, is in the top quartile of the sample, 0 otherwise. Across both panels, in Column 1, the dependent variable, |*LLPForecastError*|, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2, the dependent variable, *LLPForecastDispersion*, is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3, the dependent variable, *LogNumLLPEst*, is the natural log of the number of analyst provision forecasts included in the consensus. Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 5**
Effects of CECL adoption on total uncertainty, consensus, and the precision of public and private information

| Dependent Variable: | Column (1) *TotalUncertainty* | Column (2) *Consensus* | Column (3) *PublicPrecision* | Column (4) *PrivatePrecision* |
|---|---|---|---|---|
| $Post_t$ | 0.022 | 0.022 | 34.079* | 27.423 |
| | (0.08) | (0.40) | (1.88) | (0.76) |
| **CECL x $Post_t$** | **0.827*** ** | **0.060** | **-37.741*** ** | **-29.819** |
| | **(4.96)** | **(1.61)** | **(-2.79)** | **(-0.95)** |
| $MTB_{t-1}$ | 0.469*** | -0.010 | -5.038 | 7.729 |
| | (4.63) | (-0.60) | (-0.93) | (0.70) |
| $RetVol_{t-1}$ | -0.299*** | -0.030*** | -3.111 | -1.147 |
| | (-4.22) | (-3.34) | (-1.20) | (-0.19) |
| $BigN_{t-1}$ | 0.174 | -0.041 | -12.528 | 17.125 |
| | (0.72) | (-0.82) | (-1.23) | (0.58) |
| $Loss_{t-1}$ | 6.174*** | 0.117** | 39.039** | -0.517 |
| | (7.49) | (2.21) | (2.10) | (-0.03) |
| $\Delta LLP_{t-1}$ | -0.021 | -0.029*** | -0.023 | 0.792 |
| | (-0.32) | (-4.22) | (-0.01) | (0.29) |
| $LogNumLLPEst_t$ | -0.015 | -0.072*** | -10.217* | -12.396 |
| | (-0.12) | (-4.46) | (-1.67) | (-0.87) |
| $Tier1Ratio_{t-1}$ | 0.142 | 0.017 | 1.942 | 3.140 |
| | (1.18) | (0.93) | (0.20) | (0.19) |
| $ConsumerLoans_{t-1}$ | 0.309 | -0.094* | 7.825 | 69.516 |
| | (0.54) | (-1.68) | (0.25) | (1.48) |
| $RealEstateLoans_{t-1}$ | 0.745*** | 0.046 | -22.571 | -80.584** |
| | (3.17) | (1.07) | (-1.30) | (-2.20) |
| $LogAssets_{t-1}$ | -1.643*** | 0.012 | 46.253 | -4.828 |
| | (-2.67) | (0.14) | (1.46) | (-0.07) |
| Bank Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| N | 3,934 | 3,934 | 3,934 | 3,934 |
| Adjusted $R^2$ | 37.96% | 6.28% | 29.48% | 16.17% |

This table reports the results of regressions examining the impact of CECL adoption on components of LLP forecast dispersion following Barron et al. (1998). *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. Below is a summary of how each of the four dependent variables are calculated:

Column 1 - $TotalUncertainty = (1 - 1/n)*d + se$
Column 2 - $Consensus = (se - d/n)/TotalUncertainty$
Column 3 - $PublicPrecision = (se - d/n)/TotalUncertainty^2$
Column 4 - $PrivatePrecision = d/TotalUncertainty^2$
where: $n = \exp(LogNumLLPEst)$
$d = LLPForecastDispersion^2$
$se = LLPForecastError^2$

Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 6**
Analysis of loan loss provision properties: 2020 versus rest of post-period

**Panel A: LLP Forecast Errors, Dispersion, and Number of Estimates**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| (1) Post2020 | -0.004 | -0.014*** | 0.077* |
| | (-0.50) | (-3.83) | (1.75) |
| (2) Post2021 | -0.022* | -0.011*** | 0.170*** |
| | (-1.93) | (-3.50) | (2.75) |
| **(3) CECL x Post2020** | **0.023*** ** | **0.025*** ** | **-0.147*** ** |
| | **(3.47)** | **(7.29)** | **(-3.80)** |
| **(4) CECL x Post2021** | **0.018*** ** | **0.003** ** | **-0.110*** ** |
| | **(3.84)** | **(2.16)** | **(-2.69)** |
| **F-Test: (4) - (3) = 0** | **-0.005** | **-0.022*** ** | **0.037** |
| | **(0.53)** | **(42.66)** | **(1.19)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.06% | 37.11% | 85.24% |

**Panel B: LLP Forecast Uncertainty, Consensus, and Precision**

| Dependent Variable: | Column (1) *TotalUncertainty* | Column (2) *Consensus* | Column (3) *PublicPrecision* | Column (4) *PrivatePrecision* |
|---|---|---|---|---|
| (1) Post2020 | -0.267 | 0.041 | 35.051* | 24.088 |
| | (-0.94) | (0.68) | (1.83) | (0.63) |
| (2) Post2021 | -0.712 | 0.038 | 45.101 | 23.969 |
| | (-1.42) | (0.39) | (0.95) | (0.52) |
| **(3) CECL x Post2020** | **1.426*** ** | **0.024** | **-40.977*** ** | **-23.623** |
| | **(5.54)** | **(0.54)** | **(-2.78)** | **(-0.72)** |
| **(4) CECL x Post2021** | **0.256* ** | **0.094** ** | **-34.617** ** | **-35.697** |
| | **(1.68)** | **(2.11)** | **(-2.17)** | **(-0.91)** |
| **F-Test: (4) - (3) = 0** | **-1.170*** ** | **0.070** | **6.360** | **-12.074** |
| | **(20.65)** | **(1.98)** | **(0.19)** | **(0.11)** |
| Controls | Yes | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| N | 3,934 | 3,934 | 3,934 | 3,934 |
| Adjusted $R^2$ | 38.50% | 6.30% | 29.44% | 16.13% |

This table reports the results of regressions examining the impact of CECL adoption on the attributes of analysts' LLP forecasts in 2020 versus the rest of the post-period. *Post2020* (*Post2021*) is an indicator variable equal to 1 if *Post* is equal to one and the year is 2020 (2021), 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Column 1 of Panel A, the dependent variable, |*LLPForecastError*|, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2 of Panel A, the dependent variable, *LLPForecastDispersion*, is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3 of Panel A, the dependent variable, *LogNumLLPEst*, is the natural log of the number of analyst provision forecasts included in the consensus. Below is a summary of how each of the four dependent variables are calculated in Panel B:

Column 1 - $TotalUncertainty = (1 - 1/n)*d + se$

Column 2 - $Consensus = (se - d/n)/TotalUncertainty$

Column 3 - $PublicPrecision = (se - d/n)/TotalUncertainty^2$

Column 4 - $PrivatePrecision = d/TotalUncertainty^2$

where: $n = \exp(LogNumLLPEst)$

$d = LLPForecastDispersion^2$

$se = LLPForecastError^2$

The bottom of Panels A and B report the results of F-tests that examine whether the coefficients on *CECL* x *Post2020* and *CECL* x *Post2021* are equal. Standard errors are clustered by firm. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 7**
Summary Statistics for Market Reactions Sample

**Panel A: CECL Banks**

| | Pre-Period | | | | | | Post-Period | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | N | Mean | Std. Dev. | P25 | P50 | P75 | N | Mean | Std. Dev. | P25 | P50 | P75 |
| $BASpread_d$ | 32,326 | 30.558 | 46.684 | 7.158 | 15.215 | 32.130 | 26,058 | 33.213 | 44.600 | 9.115 | 20.299 | 38.996 |
| $AbsAbRet_d$ | 32,326 | 1.405 | 1.480 | 0.446 | 0.966 | 1.809 | 26,058 | 1.917*** | 1.839 | 0.621 | 1.340 | 2.598 |
| $AbsRevisionMagnitude_r$ | 32,326 | 0.104 | 0.207 | 0.020 | 0.047 | 0.098 | 26,058 | 0.361*** | 0.450 | 0.067 | 0.198 | 0.476 |
| $Horizon_r$ | 32,326 | 85.390 | 49.138 | 72.000 | 91.000 | 92.000 | 26,058 | 70.009*** | 44.603 | 29.000 | 86.000 | 91.000 |
| $Price_d$ | 32,326 | 44.122 | 34.595 | 22.800 | 34.400 | 53.100 | 26,058 | 46.071 | 43.255 | 20.020 | 32.960 | 53.460 |
| $MVE_{t-1}$ | 32,326 | 12,994.130 | 39,315.940 | 984.311 | 2,420.840 | 5,865.255 | 26,058 | 14,692.430 | 39,736.550 | 1,172.406 | 2,696.925 | 6,811.290 |
| $RetVol_{t-1}$ | 32,326 | 0.016 | 0.007 | 0.013 | 0.015 | 0.018 | 26,058 | 0.028*** | 0.014 | 0.018 | 0.024 | 0.033 |
| $Turnover_{t-1}$ | 32,326 | 0.562 | 0.305 | 0.338 | 0.493 | 0.733 | 26,058 | 0.614*** | 0.319 | 0.382 | 0.537 | 0.771 |
| $Depth_d$ | 32,326 | 739.027 | 1,554.341 | 180.147 | 232.485 | 459.818 | 26,058 | 666.735 | 1,423.059 | 171.021 | 216.879 | 373.725 |
| $Volume_d$ | 32,326 | 1.987 | 4.588 | 0.123 | 0.395 | 1.458 | 26,058 | 2.303* | 5.218 | 0.168 | 0.471 | 1.629 |
| $AbRet_{[t0,t+1]}$ | 8,531 | 0.209 | 3.357 | -1.505 | 0.173 | 2.023 | 6,563 | 0.309 | 3.945 | -1.824 | 0.071 | 2.423 |
| $Revision_t$ | 8,531 | 0.009 | 0.209 | -0.024 | 0.016 | 0.065 | 6,563 | 0.114*** | 0.510 | -0.034 | 0.081 | 0.303 |

**Panel B: Non-CECL Banks**

| | Pre-Period | | | | | | Post-Period | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Variable | N | Mean | Std. Dev. | P25 | P50 | P75 | N | Mean | Std. Dev. | P25 | P50 | P75 |
| $BASpread_d$ | 4,445 | 159.129 | 116.940 | 73.015 | 119.941 | 211.472 | 6,448 | 180.416*** | 127.699 | 81.517 | 139.653 | 246.906 |
| $AbsAbRet_d$ | 4,445 | 1.389 | 1.388 | 0.440 | 0.966 | 1.868 | 6,448 | 2.082*** | 2.056 | 0.654 | 1.430 | 2.774 |
| $AbsRevisionMagnitude_r$ | 4,445 | 0.085 | 0.077 | 0.033 | 0.064 | 0.108 | 6,448 | 0.336*** | 0.367 | 0.083 | 0.219 | 0.448 |
| $Horizon_r$ | 4,445 | 97.946 | 52.776 | 84.000 | 91.000 | 94.000 | 6,448 | 82.870*** | 46.077 | 48.000 | 88.000 | 92.000 |
| $Price_d$ | 4,445 | 22.545 | 10.515 | 15.760 | 20.760 | 25.300 | 6,448 | 20.248*** | 11.804 | 13.250 | 17.640 | 23.520 |
| $MVE_{t-1}$ | 4,445 | 270.407 | 144.745 | 179.895 | 229.734 | 316.241 | 6,448 | 269.305 | 203.084 | 149.371 | 213.026 | 327.023 |
| $RetVol_{t-1}$ | 4,445 | 0.016 | 0.005 | 0.012 | 0.016 | 0.020 | 6,448 | 0.028*** | 0.015 | 0.017 | 0.024 | 0.033 |
| $Turnover_{t-1}$ | 4,445 | 0.210 | 0.168 | 0.123 | 0.169 | 0.238 | 6,448 | 0.249*** | 0.148 | 0.154 | 0.220 | 0.299 |
| $Depth_d$ | 4,445 | 374.496 | 417.571 | 201.582 | 265.989 | 380.993 | 6,448 | 308.343*** | 340.822 | 169.857 | 218.728 | 314.229 |
| $Volume_d$ | 4,445 | 0.030 | 0.058 | 0.007 | 0.015 | 0.031 | 6,448 | 0.050*** | 0.158 | 0.012 | 0.023 | 0.047 |
| $AbRet_{[t0,t+1]}$ | 1,653 | 0.105 | 2.914 | -1.331 | 0.154 | 1.687 | 1,581 | 0.513** | 4.158 | -1.649 | 0.323 | 2.669 |
| $Revision_t$ | 1,653 | 0.023 | 0.139 | -0.036 | 0.027 | 0.079 | 1,581 | 0.082*** | 0.463 | -0.084 | 0.078 | 0.307 |

This table reports descriptive statistics and univariate tests of differences in the mean for the market reaction tests samples, with the sample of CECL banks reported in Panel A and non-CECL banks reported in Panel B. Tests of differences in the mean are based on OLS regressions with the *Post* indicator included in the model to allow for clustering of standard errors by firm. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 8**
Market reactions to loan loss provision forecast revisions

**Panel A: Bid-Ask Spread and Market-Adjusted Stock Returns**

| Dependent Variable: | *BASpread* $_d$ | | *AbsAbRet* $_d$ | |
|---|---|---|---|---|
| Variable | Coefficient | T-Stat | Coefficient | T-Stat |
| $Post_t$ | 25.761*** | 4.38 | -0.114 | -1.23 |
| $PreRevision1_d$ | 5.428* | 1.81 | 0.177*** | 2.92 |
| $Revision0_d$ | 9.610*** | 2.90 | 0.140** | 2.50 |
| $Revision1_d$ | 1.832 | 0.54 | 0.324*** | 4.44 |
| CECL x $Post_t$ | -16.491*** | -2.91 | 0.050 | 0.64 |
| CECL x $PreRevision1_d$ | -3.674 | -1.21 | -0.146** | -2.30 |
| CECL x $Revision0_d$ | -7.755** | -2.33 | 0.121** | 2.01 |
| CECL x $Revision1_d$ | -0.316 | -0.09 | 0.212*** | 2.68 |
| $PreRevision1_d$ x $Post_t$ | -6.024 | -1.51 | 0.017 | 0.19 |
| $Revision0_d$ x $Post_t$ | -13.194*** | -3.12 | 0.211** | 2.49 |
| $Revision1_d$ x $Post_t$ | -9.855** | -2.30 | 0.054 | 0.53 |
| CECL x $PreRevision1_d$ x $Post_t$ | 5.863 | 1.45 | -0.079 | -0.84 |
| **CECL x $Revision0_d$ x $Post_t$** | **12.184*** ** | **2.87** | **-0.137** | **-1.48** |
| **CECL x $Revision1_d$ x $Post_t$** | **7.161*** | **1.66** | **-0.379*** ** | **-3.47** |
| $AbsRevisionMagnitude_r$ | 1.146*** | 2.67 | 0.101*** | 8.89 |
| $LogHorizon_r$ | -1.182*** | -3.11 | -0.035*** | -3.75 |
| $Price_d$ | 0.023 | 0.02 | -0.332*** | -7.56 |
| $LogMVE_{t-1}$ | 8.162** | 2.11 | 0.619*** | 6.52 |
| $RetVol_{t-1}$ | 2.315*** | 3.57 | 0.053*** | 3.80 |
| $Turnover_{t-1}$ | -9.018*** | -11.69 | -0.001 | -0.09 |
| $Depth_d$ | 4.113*** | 6.69 | -0.137*** | -6.57 |
| $Volume_d$ | -5.894*** | -11.96 | 0.907*** | 15.35 |
| $AbsAbRet_d$ | 6.708*** | 17.50 | / | / |
| $BASpread_d$ | / | / | 0.342*** | 20.78 |
| Clustered SE | Bank-Analyst | | Bank-Analyst | |
| Bank-Analyst FE | Yes | | Yes | |
| Day FE | Yes | | Yes | |
| N | 69,277 | | 69,277 | |
| Adjusted $R^2$ | 68.13% | | 21.36% | |

**Table 8** (*continued* )

| **Panel B: Signed Revision Magnitude and Signed Returns** | | |
|---|---|---|
| Dep. Var.: *AbRet* $_{[t0,t+1]}$ | Coefficient | T-Stat |
| $Revision_r$ | -0.310 | -1.33 |
| $Post_t$ | 0.389 | 1.13 |
| CECL x $Post_t$ | 0.003 | 0.01 |
| CECL x $Revision_r$ | 0.500** | 1.96 |
| $Revision_r$ x $Post_t$ | 0.606** | 2.29 |
| **CECL x $Revision_r$ x $Post_t$** | **-0.745**** | **-2.57** |
| $LogHorizon_r$ | 0.257*** | 7.15 |
| $MTB_{t-1}$ | -0.550*** | -5.62 |
| $RetVol_{t-1}$ | -0.058 | -0.99 |
| $BigN_{t-1}$ | -0.615* | -1.71 |
| $Loss_{t-1}$ | -0.066 | -0.20 |
| $\Delta LLP_{t-1}$ | -0.300*** | -7.97 |
| $LogNumLLPEst_t$ | 0.189** | 2.14 |
| $Tier1Ratio_{t-1}$ | -0.197** | -2.03 |
| $ConsumerLoans_{t-1}$ | -0.132 | -0.28 |
| $RealEstateLoans_{t-1}$ | -0.843*** | -3.76 |
| $LogAssets_{t-1}$ | 0.565 | 1.15 |
| Clustered SE | Bank-Analyst | |
| Bank-Analyst FE | Yes | |
| Day FE | Yes | |
| N | 18,328 | |
| Adjusted $R^2$ | 2.66% | |

This table reports the results of regressions examining the impact of CECL adoption on market reactions to analysts' LLP forecast revisions. *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Panel A, *PreRevision1d* *Revision0d* , and *Revision1d* are indicator variables equal to 1 on the day before, the day of, and the day after an analyst's LLP forecast revision, respectively; the indicators are equal to 0 two days before an analyst's LLP forecast revision. In Panel B, *Revision* is the change in the analyst's LLP forecast (i.e., previous estimate minus current estimate) scaled by lagged total assets. Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 9**
Robustness tests - Bank size and alternative dependent variable definitions

**Panel A: Additional Size Controls**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.003 | -0.003 | 0.063 |
| | (-0.38) | (-0.81) | (1.44) |
| **CECL x $Post_t$** | **0.020***** | **0.010***** | **-0.109***** |
| | **(3.79)** | **(4.89)** | **(-2.90)** |
| $Assets_{t-1}$ | -0.241*** | -0.272*** | -0.954* |
| | (-3.32) | (-4.09) | (-1.80) |
| $AssetsSquared_{t-1}$ | 0.509** | 0.603** | 4.436*** |
| | (2.15) | (2.61) | (2.67) |
| $AssetsCubed_{t-1}$ | -0.273* | -0.334** | -2.537** |
| | (-1.91) | (-2.35) | (-2.51) |
| $Assets_{t-1}$ x $Post_t$ | 0.051 | 0.083*** | -0.119 |
| | (1.31) | (3.34) | (-0.84) |
| $AssetsSquared_{t-1}$ x $Post_t$ | -0.132 | -0.247* | -0.167 |
| | (-0.66) | (-1.86) | (-0.21) |
| $AssetsCubed_{t-1}$ x $Post_t$ | 0.083 | 0.169 | 0.300 |
| | (0.50) | (1.52) | (0.46) |
| Additional Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 |
| Adjusted $R^2$ | 42.06% | 37.99% | 85.45% |

**Panel B: Trimming Sample Based on Bank Size**

| Dependent Variable: | Column (1) *\|LLPForecastError\|* | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $Post_t$ | -0.003 | -0.010*** | 0.061 |
| | (-0.47) | (-3.29) | (1.09) |
| **CECL x $Post_t$** | **0.018***** | **0.004**** | **-0.090*** |
| | **(3.11)** | **(2.53)** | **(-1.78)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 2,676 | 2,364 | 2,676 |
| Adjusted $R^2$ | 37.11% | 44.59% | 78.14% |

| Trimmed Sample | Mean *Assets* CECL = 0 | Mean *Assets* CECL = 1 | Difference: T-Stat (P-Val.) |
|---|---|---|---|
| Columns (1) and (3) | **4,960.883** | **7,167.422** | **1.23 (0.221)** |
| Column (2) | **5,928.221** | **7,512.890** | **0.67 (0.506)** |

**Table 9** (*continued* )

**Panel C: Alternative Scalars for Forecast Errors and Dispersion**

| Scalar: | *TotalLoans* $_{t-1}$ | | *MVE* $_{t-1}$ | |
|---|---|---|---|---|
| Dependent Variable: | Column (1)<br>\|*LLPForecastError* \| | Column (2)<br>*LLPForecastDispersion* | Column (3)<br>\|*LLPForecastError* \| | Column (4)<br>*LLPForecastDispersion* |
| $Post_t$ | -0.009 | -0.009** | 0.065 | -0.032 |
| | (-0.94) | (-2.01) | (0.96) | (-0.86) |
| **CECL x $Post_t$** | **0.037*** ** | **0.024*** ** | **0.143*** ** | **0.100*** ** |
| | **(5.40)** | **(7.34)** | **(3.08)** | **(4.46)** |
| Controls | Yes | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes | Yes |
| N | 4,444 | 3,934 | 4,444 | 3,934 |
| Adjusted $R^2$ | 42.81% | 33.67% | 46.33% | 42.09% |

**Panel D: Alternative Measures for Analyst Coverage**

| Dependent Variable: | Column (1)<br>*LogNumEPSEst* | Column (2)<br>*LogNumLLPEst* | Column (3)<br>*LogNumEPSEst* |
|---|---|---|---|
| $Post_t$ | 0.117*** | 0.058* | 0.089*** |
| | (3.07) | (1.96) | (3.66) |
| **CECL x $Post_t$** | **-1.76*** ** | **-0.093*** ** | **-0.130*** ** |
| | **(-4.93)** | **(-3.80)** | **(-5.50)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 4,439 | 5,734 | 5,734 |
| Adjusted $R^2$ | 96.33% | 91.88% | 97.93% |

This table reports the results of robustness tests for the main results. Panel A reports the results of the tests including additional size controls. Panel B reports the results of tests using a trimmed sample based on bank size. Panel C reports the results of the tests using alternative scalars for forecast errors and forecast dispersion. Panel D reports the results of the tests using alternative analyst coverage proxies. Across all four panels, *Post* is an indicator variable equal to 1 if the bank has adopted CECL by the beginning of the quarter, 0 otherwise. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Column 1, Panels A and B, the dependent variable, |*LLPForecastError* |, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2, Panels A and B, the dependent variable, *LLPForecastDispersion* , is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3, Panels A and B, the dependent variable, *LogNumLLPEst* , is the natural log of the number of analyst provision forecasts included in the consensus. In Panel B, the sample is trimmed based on bank size. Specifically, we drop the top 40% (bottom 40%) of treatment (control) banks based on asset size and test the robustness of our main results to this trimmed sample. At the bottom of Panel B, we report the results of tests of differences in the mean of bank size (i.e., total assets) across treatment and control banks; these results are based on OLS regressions with the *CECL* indicator included in the model to allow for clustering of standard errors by bank. In Panel C, Columns 1 and 2 (Columns 3 and 4) use lagged total loans (lagged market value of equity) as the scalar for |*LLPForecastError* | and *LLPForecastDispersion* . In Column 1 Panel D, *LogNumEPSEst* is the natural log of the number of analyst EPS forecasts included in the consensus using IBES. In Column 2 Panel D, rather than dropping observations with missing *LogNumLLPEst* , those observations are included and the dependent variable is the natural log of 1 plus the number of analyst LLP forecasts included in the consensus. Column 3 Panel D is analogous to Column 2 using the number of IBES EPS forecasts. Standard errors are clustered by firm. All continuous independent variables are standardized to have a mean of zero and a standard deviation of one for ease of interpretation. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 10**
Placebo tests

**Panel A: 2008 Financial Crisis Placebo Tests - Maintain SRC Classification Approach**

| Dependent Variable: | Column (1) $\lvert LLPForecastError \rvert$ | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $PlaceboPre_t$ | 0.023 | -0.011 | -0.076 |
| | (0.52) | (-0.81) | (-1.10) |
| **CECL x $PlaceboPre_t$** | **-0.060** | **-0.002** | **-0.252***** |
| | **(-1.35)** | **(-0.18)** | **(-3.16)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 1,074 | 847 | 1,074 |
| Adjusted $R^2$ | 33.31% | 48.11% | 83.53% |

**Panel B: 2008 Financial Crisis Placebo Tests - Reclassification of SRC Approach**

| Dependent Variable: | Column (1) $\lvert LLPForecastError \rvert$ | Column (2) *LLPForecastDispersion* | Column (3) *LogNumLLPEst* |
|---|---|---|---|
| $PlaceboPre_t$ | -0.048 | 0.002 | 0.009 |
| | (-1.42) | (0.14) | (0.11) |
| **CECL x $PlaceboPre_t$** | **-0.001** | **-0.021*** | **-0.226***** |
| | **(-0.04)** | **(-1.76)** | **(-2.77)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 1,701 | 1,197 | 1,701 |
| Adjusted $R^2$ | 36.13% | 42.50% | 82.73% |

This table reports the results of 2008 financial crisis placebo tests for the main results. Panel A reports the results where the same treatment banks are used during the placebo period. Panel B reports the results using non-SRC banks as of the period of the placebo tests as the pseudo-treatment group. *PlaceboPre* is an indicator variable equal to 1 for quarters 2008Q3 - 2009Q2, 0 for quarters 2009Q3 - 2010Q2. In Panel A, *CECL* is an indicator variable equal to 1 for non-SRC banks during the period of the main tests (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Panel B, *CECL* is an indicator variable equal to 1 if the bank was classified as a non-SRC during the period of the placebo test, 0 otherwise. In Column 1, the dependent variable, $\lvert LLPForecastError \rvert$, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2, the dependent variable, *LLPForecastDispersion*, is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3, the dependent variable, *LogNumLLPEst*, is the natural log of the number of analyst provision forecasts included in the consensus. Standard errors are clustered by firm. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.

**Table 11**
Parallel trends test

| Dependent Variable: | Column (1)<br>*\|LLPForecastError\|* | Column (2)<br>*LLPForecastDispersion* | Column (3)<br>*LogNumLLPEst* |
|---|---|---|---|
| 2019Q1 | 0.003 | -0.002* | 0.031 |
| | (0.74) | (-1.93) | (1.06) |
| 2019Q2 | 0.003 | -0.003*** | -0.047 |
| | (0.83) | (-2.63) | (-1.03) |
| 2019Q3 | 0.002 | -0.002** | -0.017 |
| | (0.46) | (-2.08) | (-0.39) |
| 2019Q4 | 0.002 | -0.002** | -0.208*** |
| | (0.55) | (-2.17) | (-4.52) |
| **CECL x 2019Q1** | **-0.002** | **0.001** | **-0.038** |
| | **(-0.49)** | **(0.85)** | **(-1.35)** |
| **CECL x 2019Q2** | **-0.003** | **0.002*** | **-0.034** |
| | **(-0.79)** | **(1.82)** | **(-0.74)** |
| **CECL x 2019Q3** | **0.002** | **0.000** | **-0.049** |
| | **(0.59)** | **(0.48)** | **(-1.11)** |
| **CECL x 2019Q4** | **0.002** | **0.000** | **-0.051** |
| | **(0.36)** | **(0.01)** | **(-1.03)** |
| Controls | Yes | Yes | Yes |
| Bank Fixed Effects | Yes | Yes | Yes |
| Year Fixed Effects | Yes | Yes | Yes |
| N | 2,294 | 2,047 | 2,294 |
| Adjusted $R^2$ | 33.12% | 44.24% | 93.24% |

This table reports the results of the parallel trends tests for the main results. *2019Q1*, *2019Q2*, *2019Q3*, and *2019Q4* are indicator variables equal to 1 if the quarter is 2019Q1, 2019Q2, 2019Q3, and 2019Q4, respectively; the indicators are equal to 0 during 2018. *CECL* is an indicator variable equal to 1 for non-SRC banks (i.e., before the CARES Act, the bank was originally required to adopt CECL in 2020), 0 otherwise. In Column 1, the dependent variable, |*LLPForecastError*|, is the absolute difference between the consensus analyst provision forecast (based on the mean) and the actual provision for the quarter, scaled by lagged total assets. In Column 2, the dependent variable, *LLPForecastDispersion*, is the standard deviation of analyst provision forecasts included in the consensus, scaled by lagged total assets. In Column 3, the dependent variable, *LogNumLLPEst*, is the natural log of the number of analyst provision forecasts included in the consensus. Standard errors are clustered by firm. ***, **, and * represent statistical significance at the 1%, 5%, and 10% levels, respectively (two-tailed test). See Appendix A for variable definitions.