Bank Stress Test Disclosures, Private Information Production, and Price Informativeness [*]

Amanda Rae Heitz[†] P. Barrett Wheeler [‡]

August 31, 2021

[*]We thank Andrew Bird (discussant), John Campbell, Candace Jens, Itay Goldstein (discussant), Mathias Kronlund, Jon Pogach, Robert Prilmeier, Jason Sandvik, Jeff Traczynski, Alex Ufier, Will Waller, Da Xu, and participants at the Joint Virtual Workshop by the Research Group of the Basel Committee on Banking Supervision, 2021 FARS Midyear Meeting, Tulane University, and the Federal Deposit Insurance Corporation (FDIC) for helpful feedback. All errors are our own.

[†]Department of Finance, A.B. Freeman School of Business Tulane University Goldring/Woldenberg Complex 7 McAlister Dr. New Orleans, LA 70118-5645 phone: (504) 314-7575 e-mail: aheitz@ tulane.edu

[‡]Department of Accounting, A.B. Freeman School of Business Tulane University Goldring/Woldenberg Complex 7 McAlister Dr. New Orleans, LA 70118-5645 phone: (504) 862-8045 e-mail: pwheeler1@tulane.edu

Bank Stress Test Disclosures, Private Information Production, and Price Informativeness

---

Abstract

From 2015-2017, banks holding \$10-\$50 billion in assets were required to disclose a portion of the results of company-run stress tests mandated by the Dodd-Frank Act of 2010. While these disclosures were intended to increase bank transparency and promote financial system stability, recent theory models suggest that increased regulatory disclosure may have the unintended consequence of discouraging private information production and reducing the informativeness of equity prices. We find that the disclosure of stress test results by treatment banks is associated with a reduced analyst following of approximately 5%, driven primarily by the loss of experienced analysts. We also find that the earnings forecasts of analysts that continue to follow these banks exhibit decreased dispersion and contain less firm-specific information. Further, we find that, post-disclosure, bank equity prices become more synchronous with the entire stock market, indicating that their prices become less informative. Taken together, our results suggest an unintended consequence of stress test disclosures. Because equity prices can be informative to bank regulators, our study suggests that stress test disclosures can, paradoxically, reduce the information available to bank regulators, which could negatively affect financial system stability.

Keywords: Stress test, disclosure, market efficiency
JEL-Classification: G14, G21, G28.

---

1 Introduction

Since the financial crisis of 2007-2009, stress tests have become an important element of bank supervision in the United States. The goal of conducting stress tests is to assess banks' ability to withstand economic shocks, allowing supervisors to discipline banks' behavior and promote financial system stability. A key question regarding stress tests is the extent to which results should be made public. On the one hand, disclosing these results could be beneficial. For instance, disclosing these results could potentially enhance market discipline and increase confidence in the supervisory process (Goldstein and Sapra, 2013). Furthermore, because market participants have the ability to synthesize and incorporate information from many different sources into prices (Hayek, 1945; Grossman, 1976; Roll, 1984), releasing supervisory information to the public may enhance the informativeness of prices and provide new, valuable information to the regulator.

On the other hand, as detailed in Goldstein and Sapra (2013), there are potential costs associated with increased regulatory disclosure. Most notably, recent theoretical models suggest that more disclosure does not unambiguously lead to greater production of private information and more informative prices.[1] These models caution that releasing additional information into the market may reduce the incentives of market participants to produce private information and encourage them instead to rely on public information (Morris and Shin, 2002). This could ultimately lead to less informative market prices, potentially impeding the ability of regulators to use market prices as a supervisory tool (Bond et al., 2010; Bond and Goldstein, 2015; Goldstein and Yang, 2019). The extent to which stress test disclosures promote or discourage private information production remains an open empirical question.

[1] Goldstein and Sapra (2013) also discuss two additional potential costs of disclosure: 1) Disclosure may harm the operation of the interbank market and the provision of risk-sharing in this market, as modeled by Dang et al. (2017), and 2) Disclosure may impact the incentives of bank managers and lead them to make myopic actions to pass the test or act in their own self-interest (see Beatty and Liao (2014) and Bushman (2014) for further discussions).

From 2015-2017, under provisions updating the Dodd-Frank Act of 2010, all savings and loan holding companies, bank holding companies (BHCs), and complex financial institutions with capitalized assets between \$10 billion and \$50 billion were required to generate and disclose the results of *company-run* stress tests estimated under a set of "severely adverse" economic conditions set forth by the Federal Reserve. Meanwhile, the Federal Reserve had been conducting stress tests and disclosing the results for banks with greater than \$100 billion in assets since 2013 and greater than \$50 billion in assets since 2014. Banks with assets less than \$10 billion have never been required to conduct stress tests.

We use this setting to examine two questions. First, how does the public release of stress test results affect the production of private information? Second, how does revealing stress test information affect the overall price informativeness of the disclosing banks? To the best of our knowledge, this paper is the first to examine the impact that these company-reported stress test disclosures have on capital markets. Further, the existing empirical literature examining stress test disclosures is void of a study focusing on private information production and price informativeness. Given anecdotal and analytical evidence that market prices are an important determinant of government actions and policies,[2] this is an important link to understand.

We begin our analysis by examining whether self-reported stress test disclosures provided the market with new information by examining their stock market reactions.[3] One benefit of our setting, compared to previous empirical studies examining other U.S. or European stress test disclosures, is that banks disclosing their company-run Dodd Frank Act Stress Test (DFAST) results

[2]See both the discussion and theory model presented in Bond and Goldstein (2015).

[3]While our paper is the first to examine the impact that the self-reported Dodd Frank Act Stress Test (DFAST) disclosures had on banks with assets between \$10 billion and \$50 billion, other papers have empirically examined stock market reactions to other U.S. or European stress test announcements and found mixed results regarding the extent to which their content was informative (Petrella and Resti, 2013; Morgan et al., 2014; Candelon and Sy, 2015; Bird et al., 2015; Flannery et al., 2017; Fernandes et al., 2020)

self-reported them on different days.[4] The staggered nature of this disclosure provides identification and helps us directly quantify the stock market price responses to these disclosures.[5] For each bank, we calculate the five-day cumulative and absolute raw and abnormal returns where abnormal returns are calculated using the characteristic-matched benchmark of Daniel et al. (1997) (DGTW). Because the information contained in stress test disclosures could lead to either negative or positive announcement effects, absolute returns are a better metric for analyzing the information content of these disclosures. Consistent with the arrival of new information, both absolute returns and volume are positive and significant.

Next, we focus on the impact these company-run DFAST disclosures have on market participants' production of private information. Because we cannot directly observe and quantify the amount and quality of private information produced by all market participants, we focus our analysis on equity analysts and rely on existing proxies for the amount of private information that they produce. We conduct a difference-in-differences analysis, comparing disclosing banks with assets between the \$10 billion to \$50 billion disclosure threshold to non-disclosing banks with assets under \$10 billion, allowing us to examine the direct impact of this disclosure. Existing theory models suggest that the impact of increased disclosure on the amount of private information market participants produce is an empirical question. For example, the model proposed in Bond and Goldstein (2015) suggests that if the regulator discloses information about issues that investors are researching, that may induce investors to acquire less information on their own. However, disclosing information about matters that investors cannot research may spur them to produce

[4]In comparison, the results of the stress tests that the Federal Reserve conducts (SCAP, CCAR, DFAST for large banks) are released on a single day.

[5]As Goldstein and Leitner (2018) point out, one of the empirical challenges that researchers face is that initial stress test disclosures are often coupled with confounding events, such as other government regulations. In contrast, our sample period spans 2015-2017, which Flannery et al. (2017) refer to as a relatively "benign" banking environment. This "benign" banking environment is actually a strength of our study because it helps to alleviate the concern that confounding events are driving our findings.

more information.[6]

We begin by examining how stress test disclosures affect the number of analysts making earnings forecasts. We find that the number of analysts making earnings forecasts drops by approximately 0.81 analysts, on average, for treated banks after the initiation of stress test disclosures, which is approximately 5%. We decompose the number of analysts following the firm into "rookie" analysts that have made fewer than four quarterly earnings forecasts and seasoned analysts with more than four quarters of prior earnings forecasting experience. We find that, post-2015, the reduction in analyst following is almost entirely driven by the loss of seasoned analysts, while there is no meaningful difference in the number of "rookie" analysts. Subsequent analysis indicates that analysts with greater experience drop coverage.

We next examine how stress test disclosures affected properties of analysts' forecasts, specifically the mean forecast error and dispersion. Barron et al. (1998) develop a framework that decomposes analyst forecasts into information known only to individual analysts and information common to all analysts. In their framework, as the amount of private information analysts produce increases, the dispersion of forecasts increases. Consistent with a decrease in private information production, we find a decrease in forecast dispersion but no statistically significant change in forecast accuracy for treated banks in the disclosure period.

We expand on this finding by implementing two more sophisticated proxies for information production. First, following Barron et al. (1998), we create a measure of the amount of idiosyncratic bank information contained in analyst forecasts. The model uses forecast errors and dispersion to decompose analysts' forecasts into common and idiosyncratic information and quantify the amount of such types of information. We also calculate the deviation of analysts' forecasts from time-

[6] Other theoretical models are discussed in greater detail in Section 2.1. For example, McNichols and Trueman (1994) suggest that increased disclosure leads to greater private information production, while Morris and Shin (2002); Angeletos and Pavan (2007); Bond and Goldstein (2015); Goldstein and Yang (2019) suggest that market participants may be less likely to produce their own.

series predictions as a second measure of private information production (Altschuler et al., 2015). Consistent with stress test disclosures discouraging private information production, both measures indicate that, post-disclosure, analysts' forecasts contain less idiosyncratic information for treated banks.

Our primary measure of price informativeness is the firm-specific information contained within a bank's stock returns, calculated as the R^2 from a modified index-model regression.[7] Durnev et al. (2003) find future firm earnings explain share prices more for lower R^2, and a number of other studies have implemented R^2 as a proxy for idiosyncratic information.[8] We find that company-run stress test disclosures are negatively associated with idiosyncratic information, indicating that bank stock prices are less informative in the disclosure period for treated banks relative to control banks.

In robustness tests, we conduct a matched sample where we match treated banks to control banks based on terciles of capital ratios, market-to-book, and credit quality within the loan portfolio, as measured by the trailing four quarter average charge-off rate. We find results that are consistent with baseline specifications.

While our evidence suggests that stress test disclosures are associated with a decrease in private information production, it is unclear whether this decrease is *inefficient* because it reflects less information produced or *efficient* because information previously produced by market participants is now produced and disclosed by firms at a lower cost. Specifically, on the one hand, the increase in market synchronicity could reflect a negative consequence of reduced private information production, since a decrease in private information production is generally considered harmful in analytical models. On the other hand, if a bank's returns are a function of both the riskiness of its assets as well as overall market conditions, the increase in synchronicity may be the result of stress

[7]Within this regression framework, a lower R^2 indicates a lower ability for market-wide news to explain stock returns. Thus, bank returns are driven more by bank-specific information.

[8]For example, see Durnev et al. (2004), Jin and Myers (2006), Chen et al. (2007), and Hutton et al. (2009).

test disclosures revealing private information about the nature of a bank's assets such that bank returns become merely a function of the overall market.

To explore this alternative interpretation, we examine changes in both the level and volatility of banks' betas following the implementation of company-run stress test disclosures. Under this alternative interpretation, if company-run DFAST disclosures reveal the bank's asset quality to be higher (lower) than market participants had believed, the bank's equity prices would rise (fall) upon the disclosure announcement, and its beta would decline (increase) post-disclosure. However, the beta would be constant after the DFAST disclosure, even though it changes discretely as a result of the disclosure. Thus, returns would be a function only of market fluctuations affecting the value of its assets. We find no significant change in average betas relative to the control sample for banks with either positive or negative market reactions to their initial DFAST disclosures. Further, we find a slight increase in the volatility of betas. This evidence is inconsistent with this alternative explanation.

Our paper contributes to the literature by providing empirical evidence on negative externalities associated with releasing regulatory information to the public. While increased disclosure is often viewed as a panacea, we find an unintended consequence of stress test disclosures in the form of a decrease in private information production, ultimately leading to less informative prices. Despite existing theory models generating predictions pertaining to the effect regulatory disclosures have on price informativeness, to the best of our knowledge, we are the first paper to test these predictions empirically. We find no evidence that these disclosures improve, and may even impede, the regulator's ability to glean idiosyncratic information contained in equity prices. In this respect, our paper suggests that regulators should be cautions with regard to the amount and type of stress test information that they require banks to publicly disclose

2 Background

2.1 Related Literature on Cost and Benefits of Stress Test Disclosure

The economic consequences of mandatory disclosure have been extensively debated (Healy and Palepu, 2001; Shleifer and Wolfenzon, 2002). While some studies document benefits to mandatory disclosure in securities,[9] others are more circumspect (see Leuz and Wysocki (2016) for survey evidence). Within the mandatory disclosure literature, a growing number of studies focus on the impact bank stress test disclosures have on financial system stability. Recently, a small but growing body of theoretical models identify the mechanisms through which stress test disclosures may lead to negative market consequences, and our paper empirically tests some of the mechanisms outlined in these theories.

Both policymakers and academics have acknowledged that one of the primary intended benefits of disclosing regulatory information to market participants is market discipline.[10] This disclosure gives market participants better insights into banks' risk exposures and promotes the incorporation of this information into stock prices, which reflect the aggregate information of many different market participants (Hayek, 1945; Grossman, 1976; Roll, 1984). Thus, banks should become more accountable to both supervisors and investors. This ultimately increases financial system stability by facilitating the monitoring and disciplining of banks' risk taking and reducing the chances that unexpected events will cause major systemic disruptions. As Goldstein and Sapra (2013) discuss, many proponents of disclosing stress test results link the most recent financial crisis to bank opacity. These proponents argue that if banks had properly disclosed their risk-taking decisions, market discipline would have penalized banks for taking on excessive risks and ex-ante

[9] Healy and Palepu (2001) survey the existing literature and find that a number of studies have concluded that increased disclosure can be associated with improved stock liquidity and cost of capital reductions.

[10] Underscoring this idea is the fact that market discipline is one of three pillars of the Basel III international regulatory framework.

reduced their incentives to take such risks (e.g., Bushman, 2014).[11]

A second benefit of disclosing regulatory information that goes hand-in-hand with market discipline is the ability of the regulator to gather information about bank safety and soundness from the prices of bank securities. As Gary Stern, the former President of the Federal Bank of Minneapolis, explained, "Raw market prices are nearly free to supervisors. This characteristic seems particularly important given that supervisory resources are limited and are diminishing in comparison to the complexity of large banking organizations."[12] Traders have incentives to quickly gather, generate, and trade on information in order to reap their own monetary profits, and as a result, regulators can benefit from this information production. By monitoring the prices of banks' securities in real time, regulators can quickly identify concerns regarding bank risk taking and/or solvency and take appropriate disciplinary action. The academic literature has established that market prices influence government actions (Feldman and Schmidt, 2003; Krainer and Lopez, 2004; Furlong et al., 2006), and policy proposals have called for bank supervisors to even make *greater* use of market prices (Evanoff and Wall, 2004; Herring, 2004).

However, recent theoretical models suggest that a potential cost of increased regulatory disclosure is that it discourages private information production under two broad scenarios. First, by releasing more information into the market, traders that had already been generating that information may lose some of their competitive advantage and therefore realize fewer gains from trading. As a result, market participants have less incentive to produce private information (Gao and Liang, 2013; Bond and Goldstein, 2015). Second, if regulators release more information, traders may become increasingly reliant on public information and produce less of their own (Morris and

[11]Michael (2004) also discusses that during the Savings and Loan (S&L) Crisis of the 1980s and highlights that SLs were not using market prices to value their assets, which would have arguably provided market discipline by highlighting the problem to outsiders much earlier.

[12]http://www.minneapolisfed.org/pubs/region/01-09/stern.cfm

Shin, 2002; Angeletos and Pavan, 2007),[13] ultimately crowding out private information in market prices.

In extreme cases, placing excessive weight on public information may lead to a coordination problem between traders, whereby market participants overreact to bad news, which can compromise stability, create suboptimal bank runs, or prevent efficient runs (He and Manela, 2016; Chen and Hasan, 2006).[14] Thus, the theory literature outlines a variety of reasons market participants may reduce their production of private information in response to increased disclosures, ultimately making market prices less informative and adversely affecting the ability of regulators to learn from them (Bond et al., 2010; Bond and Goldstein, 2015; Goldstein and Yang, 2019).

This paper is most closely related to theoretical models demonstrating the "dark side" of stress test disclosure. Despite the fact that there is a large literature surrounding the impact of stress test disclosures around the world, to the best of our knowledge, we are the first empirical paper to examine how stress test disclosures affect price informativeness. Understanding the effect of stress test disclosures is important given costs of producing such disclosures and the existing anecdotal and academic evidence that indicates that policymakers learn from prices.

2.2 Stress Testing Background

Before the financial crisis, stress testing was viewed as one of many risk management tools and was not yet a major component of bank supervisory programs (Hirtle and Lehnert, 2015). However, in the aftermath of the financial crisis, national authorities turned to bank stress tests as a credible

[13] In Bond and Goldstein (2015), the regulator can release different types of information. The model suggests that if the regulator discloses information about issues that investors are researching, that may induce investors to acquire less information on their own, but disclosing information about matters that investors cannot research may spur them to produce more information.

[14] The model proposed by Bouvard et al. (2015) suggests that releasing bank-specific information can enhance stability during crisis times but impede it during normal times. Morrison and White (2013) shows that regulatory transparency improves confidence ex ante but impedes regulators' ability to stem panics ex post.

means of both assessing the health of banks and communicating it to the public.[15] As part of the 2009 Supervisory Capital Assessment Program (SCAP), the first stress tests were administered to 19 of the largest U.S.-owned BHC's with more than $100 billion in assets in order to ensure that these banks had sufficient capital to withstand adverse macroeconomic conditions (Hirtle and Lehnert, 2015). Later, supervisors began to implement coordinated stress testing into a larger group of banks through the implementation of the Comprehensive Capital Analysis and Review (CCAR) in 2011 and the Dodd-Frank Act stress testing (DFAST) provisions first implemented in 2013.

The Dodd-Frank Wall Street Reform and Consumer Protection Act ("Dodd-Frank" Act), enacted on July 21, 2010, required the Federal Reserve to generate stress test results each year under three supervisory scenarios: baseline, adverse, and severely adverse for all banks with more than $50 billion in assets. The severely adverse scenario includes trajectories for 26 variables, including 14 variables that capture economic activity, asset prices, and interest rates in the U.S. economy and financial markets, plus three variables in each of four countries or country blocks.[16] The Federal Reserve adopted rules implementing these requirements and their public disclosures of the adverse and severely adverse scenarios in October 12, 2012,[17] though they allowed for a phase-in period for this regulation. This amendment also added the requirement that all regulated financial companies, including BHCs, with between $10 billion to $50 billion in capitalized assets must disclose their own bank-calculated estimates under the severely adverse scenario. The Federal Reserve publicly disclosed the results of their DFAST on March 7, 2013, which contained the results of the severely

[15]The importance of understanding bank health is highlighted in by existing studies showing the consequences of poor bank health. For example, capital inadequacy can lead to reduced bank lending (Bernanke et al., 1991; Bolton and Freixas, 2006; Beatty and Liao, 2011), deleveraging via asset sales potentially at fire sale prices (Hanson et al., 2011), increased risk-shifting incentives (Acharya et al., 2016), decreased probability of survival , competitive position, and market share (Berger and Bouwman, 2013), and increased borrowing costs and decreased availability of credit (Afonso et al., 2011; Kashyap and Stein, 1995, 2000)

[16]Real GDP growth, inflation and the U.S./foreign currency exchange rate are reported for the Euro area, the United Kingdom, developing Asia, and Japan.

[17]See 77 FR 62380 (October 12, 2012).

adverse case scenario for 18 of the banks subjects to SCAP.[18] The 2014 DFAST disclosures released on March 20, 2014, reported both the adverse and severely adverse case scenarios for 30 U.S.-owned BHC's with greater than $50 billion in assets. BHCs with assets between $10 billion to $50 billion conducted their own stress tests, and starting in 2015, they self-reported their severely adverse case scenarios either on their own websites or by filing a Form 8-K with the Securities and Exchange Commission.

While our paper focuses on DFAST, the Federal Reserve also has a complementary stress test program, CCAR. One of the primary goals of CCAR is to evaluate each BHC's ability to maintain adequate capital after taking its planned capital actions. During the financial crisis, many large BHCs had significantly reduced (or suspended) dividend payments (Hirtle, 2014), and once the turmoil surrounding the crisis had passed, there was a desire to resume these programs. The Federal Reserve implemented CCAR in 2011 to provide a framework to determine whether the largest and most complex 19 BHCs had sufficient capital to resume these distributions (Board of Governors of the Federal Reserve System, 2011). CCAR also provides a framework and tools for the Federal Reserve to annually assess a BHC's internal capital positions and planning processes, including the governance over their capital planning process, its policy governing capital actions, such as dividends, repurchases, and share issuances, and its bank-run stress test projections under the Federal Reserve's baseline, adverse, and severely adverse scenarios, as well as under two bank-determined scenarios (Hirtle and Lehnert, 2015). Starting in 2014, 30 BHCs with assets greater than $50 billion participated in CCAR, which was the same population participating in DFAST, and the results of CCAR are publicly disclosed every year.

The supervisory results for CCAR are very closely linked to the DFAST projections. Both tests include the same banks and both are based on the same baseline, adverse, and severely adverse

[18] Lehman Brothers was not included.

scenarios outlined by the Federal Reserve. They also include the same projections of the balance sheet, risk-weighted assets, and net income. However, they differ in their assumptions about the BHCs actions affecting capital. Under CCAR, each BHC specifies their own intended capital plan, including dividends and share repurchases. In contrast, DFAST uses stylized assumptions specified in the Dodd-Frank Act, which are based on historical dividend levels for each BHC and set share repurchases and share issuance at zero, except for issuances associated with employee compensation (Board of Governors of the Federal Reserve System, 2015a,b).

On February 3, 2017, Dodd-Frank was amended such that banks with assets less than $50 billion capitalized assets were no longer required to report their company-run DFAST results after their November 2017 releases. According to the same amendment, banks with assets less than $250 billion were no longer subject to annual DFAST or CCAR examinations, though banks with capitalized assets between $100 billion and $250 billion will still be tested periodically.[19] Appendix A gives a breakdown of banks that are stress tested each year under each type of stress test.

3 Hypothesis Development

As discussed in Section 2.1, it has been widely established that mandatory disclosures have the ability to impact capital markets in meaningful ways. There are a number of channels through which stress test disclosures may impact private information production and price informativeness. To further clarify this relationship, we develop two hypotheses in this section.

Market participants, including analysts, have greater incentives to produce private information when they have the ability to profit from that information, such as through either demand for their services or gains from trades (McNichols and Trueman, 1994; Healy and Palepu, 2001). It is possible that the disclosure of stress test results could attract analysts who believe that they

[19] See 82 Fed. Reg. 9308 (February 3, 2017)

can profit from a superior ability to analyze and interpret stress test data. However, recent theory models suggest that a potential cost of increased regulatory disclosure is that it discourages private information production under two broad scenarios. First, by preempting traders' information advantage from information acquisition, disclosure could reduce private information production (Gao and Liang, 2013; Bond and Goldstein, 2015). Second, if regulators release more information, traders may become increasingly reliant on public information and produce less of their own (Morris and Shin, 2002; Angeletos and Pavan, 2007), ultimately crowding out private information production. Thus, the effect stress test disclosures have on private information production is an empirical question, which leads us to our first null Hypothesis:

H1: Company-run stress test disclosures do not affect analyst private information production.

In addition to the aggregate *level* of private information produced, recent theory models have suggested that mandatory disclosure can affect the *type* of private information produced, which may affect the informativeness of prices. In both Gao and Liang (2013) and Goldstein and Yang (2019), price informativeness is a function of two types of information. The first type of information is information that an agent (such as a regulator or manager) knows, while the second is information that the agent would like to learn. In Gao and Liang (2013), the two types of information are positively correlated. Thus, the decrease in private information produced by market participants mentioned above ultimately leads to prices becoming less informative. However, in Goldstein and Yang (2019), the two types of information are substitutes. Under certain circumstances, when the agent releases more information pertaining to the known factor, market participants produce more information pertaining to the unknown factor. This leads to market prices being more informative along the dimension of the unknown factor.

In our setting, stress test disclosures contain information about the sensitivity of a bank's loan losses and regulatory capital to severely adverse economic condition. The degree of precision regulators have about this information in the absence of company-run DFAST disclosures is unclear. Further, there are many other factors affecting bank performance not addressed in these disclosures, including bank competition, anticipated loan growth, loan and deposit pricing, etc. If the regulator has precise information regarding what is contained within the disclosures, then DFAST disclosures could spur private information production along other dimensions where the regulator seeks knowledge. However, if their information is imprecise, then DFAST disclosures could discourage production of more precise information about banks' performance under severely adverse economic conditions.

Empirically, we can observe *overall* equity price informativeness but not the *relative* measures of informativeness pertaining to each type of information presented within these models. Thus, our hypothesis development has an additional level of complexity, since our measure of overall equity price informativeness will only capture the net effect of all types of information produced.

Within Gao and Liang (2013), the two types of information are complements, and an increase (decrease) in the production of one type of information will only magnify the overall effect on price informativeness. However, within the Goldstein and Yang (2019) framework, if a decrease (increase) in the production of one type of information is greater (less than) than the production of the other, the overall effect may be negative (positive). Thus, the overall effect stress test disclosures have on overall price informativeness is an empirical question, which forms our second null Hypothesis:

H2: Company-run stress test disclosures do not affect price informativeness.

4 Data and Empirical Design

4.1 Data

We obtain quarterly bank-level variables from quarterly consolidated financial statements filed with the Federal Reserve on FR Y-9C from 2011-2017 and daily equity prices from the Center for Research in Security Prices (CRSP). We acquire analyst forecast data and actual earnings data from the Institutional Brokers' Estimate System (IBES). We hand-collect company-run stress test disclosures, including their release dates, from bank websites, Form 8-K Securities and Exchange filings, and S&P Global Market Intelligence.

4.1.1 Analyst Forecasts

Analysts often update their earnings forecasts prior to earnings announcement, resulting in multiple earnings forecasts per analyst for a given earnings period. Our coarsest measure of private information production is the total number of analyst forecasts within a given earnings period, allowing for forecast revisions, *EPS_FCSTNUM*. A greater number of analysts following a firm may lead to greater firm-level private information production, and analysts that make updates more frequently may be doing so based on the information they produce. We also calculate the number of analysts providing earnings forecasts, *EPS_ANALYSTS*, and decompose the number of analysts following the firm into "rookie" analysts that have made fewer than four quarterly earnings forecasts, *ROOKIE_FCST*, and seasoned analysts with more than four quarters of prior earnings forecast experience, *SEASONED_FCST*.

Using the most recent earnings forecast for each analyst within a given earnings period, we calculate the mean and standard deviation of these forecasts (*EPS_MEAN* and *EPS_SD*), as well as the stated EPS, *EPS_ACTUAL*. Using these data, we calculate analyst earnings forecast dispersion and forecast error, *EPS_DISPERSION* and *EPS_FE*, for banks that have at least two analysts making forecasts, so our sample size is slightly reduced.

Forecast dispersion captures the level of consensus among analysts regarding future firm prospects (see Imhoff Jr and Lobo (1992)). As indicated in Equation 1, quarterly *EPS_ DISPERSION* is calculated as the ratio of the standard deviation of the quarterly EPS forecasts of analysts following a given bank divided by the price at the end of the previous quarter:

$$EPS\_DISPERSION_{b,q} = \frac{EPS\_SD_{b,q}}{Price_{b,q-1}} \tag{1}$$

We also calculate EPS forecast error, *EPS_ FE*, as the absolute value of the difference between the mean analyst EPS forecast and the actual EPS for a bank in a given quarter, normalized by the bank's share price at the end of the previous quarter, as shown in Equation 2:

$$EPS\_FE_{b,q} = \frac{\mid EPS\_MEAN_{b,q-1} - EPS\_ACTUAL_q \mid}{Price_{b,q-1}} \tag{2}$$

We report summary statistics for our full sample of banks with assets less than \$50 billion in Table 1. While the mean number of analysts making quarterly EPS forecasts in our sample is 6.9988, bigger banks have greater analyst following. For example, pre-2015, banks with less than \$10 billion in assets have an average of approximately 6.0485 (untabulated) analysts making forecasts. This is less than half of the analyst following that banks with assets between \$10 billion and \$50 billion have (16.4481 analysts).

4.1.2 Private Information Measures

While it is difficult to directly observe the amount of private, firm-specific information that analysts produce, we follow the methodology in Barron et al. (1998) to estimate the amount of firm-specific information in analysts' forecasts. Barron et al. (1998) decompose analysts' earnings forecasts into two components: information common to all analysts and information known only by individual analysts (i.e., idiosyncratic information). In their measure, common information is captured by the mean of all forecasts, and idiosyncratic information is reflected in the dispersion of analysts' forecasts. Based on this intuition, the amount of idiosyncratic information is estimated using Equation 3:

$$EPS\_PRIV\_INFO1_{b,q} = \frac{EPS\_SD_{b,q}}{((1 - 1/EPS\_ANALYSTS_{b,q}) \times EPS\_SD_{b,q} + EPS\_SE_{b,q})^2} \quad (3)$$

where *EPS_SD* denotes forecast dispersion and *EPS_SE* denotes the squared error of the consensus mean forecast, which reflects the imprecision of the information that is common among all analysts. Equation 3 shows that *EPS_PRIV_INFO1* is increasing in dispersion, *EPS_SD*, and decreasing in the precision of common information, *EPS_SE*. If analyst forecasts are driven entirely by common information, then then there will be no dispersion in individual analysts' forecasts, indicating that there is no private information (*EPS_PRIV_INFO1*=0). In the limiting case where there is only one analyst making a forecast, there is no private information (*EPS_PRIV_INFO1*=0), since common information and private information are one and the same. In the other case where the number of analysts approaches infinity, while forecast error and dispersion remain constant, this indicates that analyst forecasts contain less private information.

We also incorporate a second measure of private information, which is the deviation of analysts' forecasts from time-series predictions. Previous literature has shown that analyst forecasts predicting earnings are typically superior to time-series models (Brown et al., 1987), presumably because analysts incorporate information beyond historical financial information in making their forecasts. The magnitude of the deviation of the analysts' forecast from a time-series prediction will reflect the information incorporated into forecasts beyond the time-series estimate (Altschuler et al., 2015). We incorporate a seasonal random walk and compute this measure in Equation 4:

$$EPS\_PRIV\_INFO2_{b,q} = \frac{|EPS\_MEAN_{b,q} - EPS\_ACTUAL_{b,q-4}|}{Price_{b,q-1}} \tag{4}$$

To mitigate concerns that this measure is affected by acquisitions, we restrict our sample to banks that had asset growth rates of less than 20% over the last year. We present the summary statistics for these variables in Table 1.

4.1.3 Price Informativeness

Roll (1984) finds that only a small portion of price movements can be explained by contemporaneous public news and speculates that traders acting on nonpublic firm-specific information could be driving returns. Subsequently, a number of papers have examined the relation between information and stock price dynamics, focusing on the R^2 from a modified index-model regression to measure stock price informativeness (Morck et al., 2000; Durnev et al., 2003, 2004; Jin and Myers, 2006; Hutton et al., 2009). Within our setting, a lower R^2 indicates a lower ability for market-wide news to explain stock returns. This means that there is a greater degree of bank-specific information available, and therefore, prices are more informative. We follow the literature and compute a measure of quarterly R^2, *QuarterlyRSQ*, from a modified index-model regression framework shown in Equation 5:

$$r_{b,q} = \alpha + \beta_1 r_{m,q-1} + \beta_2 r_{m,q} + \beta_3 r_{m,q+1} + \gamma_1 r_{i,q-1} + \gamma_2 r_{i,q} + \gamma_3 r_{i,q+1} + \epsilon_{b,q} \tag{5}$$

where $r_{b,q}$, $r_{m,q}$, and $r_{i,q}$ are excess returns of the stock, the market, and the stock's industry during quarter q. Since R^2 ranges from 0 to 1, 1-R^2 is a measure of firm-specific volatility or lack of market synchronicity. We follow the literature (Morck et al., 2000; Hutton et al., 2009) in estimating the amount of idiosyncratic information in prices, *IDIOSYN*, using a logistic transformation of $QuarterlyRSQ$, as shown in Equation 6:

$$IDIOSYN_{b,q} = ln(\frac{1 - QuarterlyRSQ_{b,q}}{QuarterlyRSQ_{b,q}}) \tag{6}$$

where higher values of *IDIOSYN* indicates a greater amount of stock price informativeness.

4.1.4 Control Variables

Following prior literature, we control for a number of bank-level variables constructed using FR Y-9C reports. These variables include size, calculated as the natural logarithm of total bank assets (*LNASSETS*), market value of equity (MVE), calculated as the number of shares outstanding multiplied by price, market-to-book value of equity (MTB), calculated as the book value of equity divided by the market value of equity, and bank capital ($CAPITAL$), calculated as the book value of equity divided by total assets. We also control for net charge-offs (NCO), measured as the average net charge-offs over the last four quarters normalized by total loans from the previous quarter.

When examining stock price informativeness, we follow Hutton et al. (2009) and add three additional controls to account for a stock's skewness, volatility, and kurtosis over a calendar year. Additional information regarding the calculation of these variables can be found in Appendix B, and aggregate summary statistics are presented in Table 1.

4.2 Empirical Design

We test our main hypotheses using a difference-in-differences approach that compares changes in the variables of interest before versus after the release of company-run DFAST disclosures for treated (i.e., disclosing) banks compared to control (i.e., unaffected) banks.

As discussed in Section 2.2, the Federal Reserve had been conducting Dodd-Frank Act

Stress Tests on banks with assets of more than \$100 billion (\$50 billion) and disclosed the results of both their adverse and severely adverse scenarios starting in 2012 (2013). The Act was later updated such that all banks holding assets between \$10 billion and \$50 billion were required to conduct company-run stress tests and publicly disclose the results for the severely adverse scenario starting in 2015. In our primary analysis, we compare the treatment banks disclosing company-run stress tests to a control group that was never required to conduct or disclose any DFAST results.[20]

We define a variable, DISCLOSE, that is an indicator variable that takes a value of 1 for the years 2015 to 2017 and zero otherwise, and a second indicator variable, *TREAT*, that takes a value of 1 for banks with assets between \$10 billion and \$50 billion. Our main specification is a single-stage, bank-level regression as indicated by Equation 7:

$$\begin{aligned} DEPENDENT\_VARIABLE_{b,q} = \alpha_b + \gamma_q + \beta_1' \, DISCLOSE_{b,q} \times TREAT_{b,q} + \\ \beta_2' \, BANK\_CONTROLS_{b,q-1} + \epsilon_{b,q} \end{aligned} \tag{7}$$

where for a bank b in quarter q, α_b represents bank fixed effects and γ_q denotes year-quarter fixed effects. Because the bank and year-quarter fixed effects subsume the direct effects from *TREAT* and *POST*, respectively, they are omitted from the specifications. The bank fixed effects control for all time-invariant heteroteneity across banks, while the bank-quarter fixed effects remove overall time trends. *BANK_ CONTROLS* is a vector of time-varying bank controls that include log assets (*LNASSETS*), charge-offs (*NCO*), market value of equity (*MVE*), market-to-book (*MTB*), and *CAPITAL*, which are discussed in Section 4.1.4 and defined in Appendix B. We cluster our standard errors by bank.

In Figure 1, we plot the yearly average number of analysts for both treated and control firms

[20] In robustness tests mentioned in Section 7.2, we show that our results are qualitatively similar when we use large BHCs with total assets greater than \$50 billion where the Federal Reserve released DFAST results as a control group.

in order to examine the parallel trends assumption underlying a difference-in-differences analysis. We focus on the number of analysts in our parallel trend analysis since almost all of our other dependent variables of interest are functions of the total number of analysts, including the number of seasoned and rookie analysts, analyst forecast error and dispersion, and our primary measure of private information production. The vertical line in the figure is drawn at the year 2014 to indicate the final year before the company-run DFAST disclosures were initiated. Pre-2015, the number of analysts for both the treated and control groups appear approximately parallel. Meanwhile, starting in 2015, the rate at which the disclosing banks lose analysts starts decreasing at a more rapid rate than the control group.

5 Empirical Results

5.1 Information Content of Disclosures

First, we examine whether company-run stress test disclosures contained information that was new to the market. A number of other papers examining the release of stress test information in Europe or for Federal Reserve-conducted stress tests conducted on large U.S. BHCs (both SCAP and CCAR) have largely concluded that these disclosures conveyed new information to market participants (Petrella and Resti, 2013; Morgan et al., 2014; Candelon and Sy, 2015; Bird et al., 2015; Flannery et al., 2017; Fernandes et al., 2020). The Federal Reserve releases SCAP and DFAST BHC results in a single day, raising concerns that a confounding event or other regulatory announcement drives these results, as discussed in Goldstein and Leitner (2018). However, \$10 billion to \$50 billion banks that were required to perform company-run (DFAST) stress tests disclosed the results on different days. The staggered nature of these disclosures helps alleviate this concern.

We calculate five-day cumulative raw and DGTW-adjusted returns around all DFAST disclosure dates, including both company-run and Federal Reserve-conducted stress tests,[21] and we present the results in Table 2, Panel A. In Row 1, we present the results of the full sample of all stress test disclosures, including both company-run and those conducted by the Federal Reserve. We report raw returns (Column 2) and DGTW-adjusted returns (Column 4).

We also present the results for subsamples of only company-run DFAST disclosures (Row 2), only Federal Reserve DFAST disclosures (Row 3), only the first release of company-run stress test disclosures (Row 4), and only subsequent releases of company-run stress test disclosures (Row 5). While fewer banks are included in the Federal Reserve-conducted stress tests, there are more disclosures and corresponding announcement dates because these banks typically release DFAST disclosures twice each year, one annual and one midyear report. We find that for all subsamples, except for the initial non-Fed DFAST disclosures, the number of banks with positive returns is greater than the number with negative abnormal returns, though just under half of the observations are negative for all types of sub-samples. Since stress test releases can convey either positive or negative information, comparing announcement-day returns to zero may not be an appropriate. Thus, while both raw and DGTW-adjusted returns are not always statistically different from zero, this does not necessarily indicate a lack of new information conveyed to the market. In Panel B, we retain the 20 trading days before and after DFAST announcement days, and we compute the five-day absolute value of our raw and DGTW-adjusted returns. If these company-run disclosures convey information to the market, regardless of whether the information is positive or negative, the absolute value of returns should be higher than days without news. The variable *Disclosure Window* is an indicator variable that takes a value of 1 for days in -2 to 2 trading days surrounding company-run DFAST announcements. We also control for other event-windows surrounding important events.

[21] When stress test disclosures are unavailable through 8-K filings, 8-K filings, or S&P Global Market intelligence, we contacted the bank's investor relations departments directly using the contact information whenever it is available. In many cases, we received responses, but for some banks that we believe were stress tested, we are unable to discover the dates of the stress test releases, so this small number of banks is omitted from these CAR tests.

These include analogous five-day trading windows surrounding all earnings announcements *Earnings Announcements*, the 2016 Presidential election (11/8/2016), and the day the Tax Cuts and Jobs Act passed in the House of Representatives (11/17/2017). For both raw and DGTW-adjusted returns, the coefficient on *Disclosure Window* is positive and statistically significant. In Panel B, we also show that abnormal volume is positive and statistically significant during the disclosure window. Taken together, the results in Table 2 suggest that company-run DFAST disclosures convey new information to the market.

5.2 Analyst Following, Forecast Error, and Dispersion

As discussed in Section 3, market participants, such analysts, have greater incentives to produce private information when they have the ability to profit from that information, such as through gains from trades or increased demand for their services. However, stress test disclosures could reduce analysts' advantage generating private information by making more information available to the public. Thus, the effect of stress test disclosures on analyst following is unclear ex ante. In this section, we analyze the impact that company-run stress test disclosures have on the number of analysts making EPS forecasts and present the results in Table 3.

The coefficient on the interaction term $DISCLOSE \times TREAT$ is negative and significant. This is consistent with the initiation of company-run stress test disclosures leading to a decline in analyst following for treated banks relative to control banks. The results in Column 1 indicates that, on average, treated banks lose about 0.81 analysts making a quarterly EPS forecasts in the disclosure period. In terms of economic significance, this represents a reduction in analyst following of approximately 5% from the pre-disclosure-period treated firm average of 16.4481 analysts. This result contrasts the findings in Flannery et al. (2017), who find an increase in analyst following after the Federal Reserve started disclosing stress test results for large banks through either CCAR or SCAP. In Column 2, we find that, on average, analysts make fewer forecasts post-disclosure, suggesting that either there is less information available for them to use in their forecasts or that they are producing less private information.

Next, we decompose the number of analysts into analysts that have made fewer than four quarterly forecasts, coined "rookie" analysts, and "seasoned" analysts that have previously made more than four forecasts, and we show the results in Table 4. In Column 1, the negative interaction term indicates that the reduction of analysts is driven almost entirely by disclosing firms losing more seasoned analysts, who could have acquired more firm-specific knowledge over time. Furthermore, the results in Column 2 indicates that the stress test disclosures did not significantly impact the level of rookie analysts. The results from Tables 3 and 4 suggest that banks releasing DFAST disclosures were able to retain fewer seasoned analysts than the group of control banks that did not have such disclosure requirements.

The theory model presented in Barron et al. (1998) allows us to use analyst forecasts to examine whether these company-run DFAST disclosures increased the amount and precision of common information available to all analysts. The model assumes that analysts receive two signals: one common to all market participants and a second private one. Within their model, if analysts are producing more private information, they are more dispersed. In Table 5, we find that company-run DFAST disclosures are associated with no statistically significant change forecast errors, though analyst forecasts are less dispersed. In the next section, we continue to follow the framework outlined in Barron et al. (1998), using the properties of analyst forecasts to examine how stress test disclosures affect the amount of private, bank-specific information that is contained in these forecasts.

5.3 Analyst Private Information Production

Analyst forecasts are a function of both public and privately produced information, and existing theory models have indicated that despite providing more public information, increased disclosures could make market participants less likely to produce their own information (Morris and Shin, 2002; Angeletos and Pavan, 2007; Bond and Goldstein, 2015; Goldstein and Yang, 2019). We first follow Barron et al. (1998) to calculate a measure of the level of idiosyncratic earnings information contained in analysts' forecasts and also analyze our second private information measure, the magnitude of the deviation of the analysts' forecasts from a time-series prediction. Both the construction and intuition behind these variables is discussed in Section 4.1.2, and we present the results in Table 6. Column 1 examines *EPS_PRIV_INFO1* and indicates that the coefficient on the interaction term $DISCLOSE \times TREAT$ is negative and significant, indicating that analysts produced less idiosyncratic information related to earnings for treated banks post-disclosure. Our results are consistent when we analyze the time-series measure of private information, *EPS_PRIV_INFO2*, as indicated by Column 2. This suggests that, post-disclosure, the increase in analyst consensus shown in Table 5 is *not* a result of analysts incorporating *more* bank-specific information into forecasts but *less*, consistent with both analyst herding and Barron et al. (1998).

Taken together, our evidence regarding analyst following, forecast error and dispersion, and more sophisticated measures of private information production rejects null Hypothesis 1. That is, the results presented in Tables 3 - 6 suggest that disclosure of company-run stress tests leads to a decrease in analyst private information production.

5.4 Price Informativeness

In this section, we examine how company-run stress test disclosures affect the overall informativeness of market prices. As discussed in Section 2.1, regulators need to make decisions, such as whether to take enforcement actions against a bank, in real time. Rather than waiting for updated accounting information included in quarterly regulatory filings, regulators can acquire real-time information about bank performance and solvency from market prices.

Based on our findings in Sections 5.2 and 5.4, the model presented in Gao and Liang (2013) would predict that stress test disclosures are associated with a decrease in the informativeness of stock prices. However, the link to overall price informativeness within the Goldstein and Yang (2019) framework is more nuanced. Within their model, there are two types information: one type that is known to a regulator and a second type of information that the regulator cares to learn. Market participants can use their resources to produce either type of information (or both). If information known to the regulator is sufficiently precise, then the regulator can release information pertaining to the known type so market participants will direct their efforts towards producing the second (unknown) type of information. As a result, because market participants direct their efforts to producing more of the second type of information, prices contain more of the unknown type of information, and the regulator can learn information they did not know from market prices.

While this model is related to our setting, there are some notable differences. First, individual banks, as opposed to the regulator, are both running stress tests and disclosing their results. It is unclear whether the regulator has precise information pertaining to the bank's performance under the severely adverse scenario prior to disclosure. If the regulator's information is precise, the model suggests that the regulator can strategically disclose stress test information so that market participants will produce more information regarding other bank attributes about which the regulator desires knowledge. Thus, it is possible that, even if stress test disclosures discourage private information production regarding the risks in the loan portfolio, this decrease in private information could be offset by an increase in private information production on other dimensions not addressed in these disclosures, such as bank competition, anticipated loan growth, or loan and deposit pricing.

Empirically, we cannot decompose our measure of price informativeness, *IDIOSYN*, into the two different types of information in the model. Since *IDIOSYN* is a measure of overall stock price informativeness, it reflects information impounded into prices related to stress test disclosures as well as all other types of information that are not easily observable or quantifiable. While the results presented in Table 3 and Table 6 indicate that stress test disclosures discourage analysts from producing private information about earnings, it is unclear whether this spurs greater private information production pertaining to other types of information. Furthermore, analysts are just one type of market participant that is producing information, and, thus, the impact of DFAST disclosures on overall private information production is unclear. Overall stock price informativeness will decrease (increase) if the reduction in private information produced by all market participants related to the contents of stress tests exceeds (is less than) any increase in other privately produced information.

We analyze stock price informativeness utilizing the same empirical framework presented in Equation 7, though we follow Hutton et al. (2009) and add additional yearly stock price controls, as reflected in Equation 8.

$$\begin{aligned} IDIOSYN_{b,q} = \alpha_b + \gamma_q + \beta_1' \, DISCLOSE_{b,q} \times TREAT_{b,q} + \\ \beta_2' \, BANK\_CONTROLS_{b,q-1} + \beta_3' \, STOCK\_CONTROLS_{b,y-1} + \epsilon_{b,q} \end{aligned} \tag{8}$$

The bank level-controls are identical to those presented in Equation 7 with the addition of controls for volatility, skewness, and kurtosis that are computed over the previous year. We present the results in Table 7. The effect of $DISCLOSE \times TREAT$ is negative and statistically significant. This indicates that post-disclosure, price informativeness has decreased for treated banks relative to control banks. This result suggests that the reduction in privately produced information related to stress tests by all market participants exceeds any increase in privately produced information pertaining to other types of information. Despite these disclosures reducing information asymmetry between the regulator and market participants, the regulator is ultimately able to infer less information from market prices.

6 Additional Analysis

6.1 Interpreting the Decline in Price Informativeness

As outlined in Section 5.4, banks that publicly disclose their company-run DFAST results realize a reduction in price informativeness relative to the group of non-disclosing banks. Consistent with analytical models on disclosure and private information production, we interpret this decline as a negative unintended consequence of disclosing the results of company-run stress tests.

However, an alternative interpretation is that stress test disclosures are an efficient, less costly substitute for information previously produced privately by market participants. Under this interpretation, the increase in return synchronicity with the market is due to the revelation of private information via stress test disclosures. To illustrate, assume that a bank's earnings and stock returns are generated by the interaction of the risk of a bank's assets and overall market conditions. Banks with a safer assets (e.g., Treasury securities in an extreme case), will have relatively stable prices because their assets are less sensitive to changes in the aggregate economy. Thus, banks perceived to have safer assets will have higher equity prices, lower returns, and low betas, due to the low sensitivity of their asset values to market conditions. Conversely, banks perceived to have riskier assets will have lower equity prices, higher returns, and a high beta, due to the high sensitivity of their asset values to market conditions.

In the absence of any DFAST disclosures, market participants receive noisy signals via existing regulatory and other disclosures and attempt to infer the risk of each bank's assets quality. As new information arrives about the risk of bank's assets, both the bank's equity price and beta respond. That is, beta is not constant. If DFAST disclosures provide sufficiently precise incremental information about the risk of a bank's assets, then the riskiness of a bank's assets becomes public knowledge. If market participants learn that the bank's assets are safer than they had anticipated, the bank's stock prices will rise and its beta will fall post-disclosure. Conversely, if assets are revealed to be riskier than anticipated, the stock price reaction will be negative and beta will increase post-disclosure. Further, and importantly to this alternative interpretation, because asset risk is now public knowledge, the bank's stock returns will simply be a function of market information, thereby improving the fit of a market model and reducing the volatility of the bank's beta.

To explore this alternative interpretation for a decline in price informativeness, we examine changes in treatment banks' beta and the volatility of beta during the disclosure period relative to the pre-period. We begin by using the capital asset pricing model (CAPM) to calculate quarterly betas for each sample bank based on daily returns in the last 60 trading days of each quarter. We also calculate the volatility of beta, calculated daily using trailing 60-day returns, during each quarter.

The results of our analysis of beta and beta volatility are presented in Table 8. In Panel A of Table 8, we focus on treatment banks and examine the change in beta for banks that have a positive market response to their first DFAST disclosure and those that have a negative market response. In Column 1, we include all quarters in our sample period from 2011-2017, and in Column 2 we include only the years 2014 and 2015. We do not find evidence of a significant change in beta during the disclosure period for either banks with positive or negative DFAST disclosure responses.

In Panel B of Table 8, we analyze both beta and the volatility of beta relative to control firms in the disclosure period. In Column 1, we find that beta increases for treatment firms relative to control firms, on average, consistent with the negative average market response to the initial company-run DFAST disclosures documented in Table 2. However, in Column 2, we find weak evidence that beta volatility *increases* for treatment firms relative to control firms. This is inconsistent with the alternative interpretation discussed above. Thus, we do not believe that our results are driven by an efficient substitution of DFAST disclosures for private information produced by market participants.

6.2 Characteristics of Analysts Leaving

In Section 5.2, we show that, compared to control banks, treated banks lose an average of 5% of analysts making quarterly EPS forecasts during the disclosure period. In this section, we examine characteristics of analysts that drop coverage in the years that company-run stress disclosures are required.

Using the IBES detail file, we first examine the number of analysts covering each bank at the end of 2014, the year before the initiation of the stress test disclosures. In Table 9, we show that in 2014, there were a total of 678 (496) analysts covering banks with assets of less than $10 billion (between $10 - $50 billion) in 2014. Of these analysts, 417 (261) were still covering banks with assets under $10 billion (between $10-$50 billion) in 2017, while 299 (197) had dropped coverage.

In Panel A, we compute the number of years of experience that each analyst has making forecasts at the end of 2014. For treated banks, we find that, on average, analysts dropping coverage of treated banks are more experienced (12.1472 years) than those maintaining coverage (10.6385 years), and this difference is significant at the 5% level. Control banks with assets of under $10 billion exhibit the opposite pattern. For this group, analysts retaining coverage are more experienced than those dropping coverage, and this difference is also significant at the 5% level.

In Panel B, we examine differences in forecast accuracy for analysts maintaining and dropping coverage, where forecast accuracy is calculated using each analyst's last forecast of fourth-quarter 2014 earnings. Larger values of forecast accuracy indicate less accurate forecasts. Just as in Table 5, our sample size is slightly decreased from the one presented in Panel A due to the absence of lagged price information for a small number of observations. In line with Table 5, we find that for both treated and control banks, there is no difference in forecast accuracy between analysts maintaining and dropping coverage.

6.3 Other Market Participants

In this section, we examine an alternative measure of price informativeness. The probability of informed trading (PIN) measure was first developed by Easley et al. (1996, 1997a,b) and has been implemented in a number of other studies (see Chen et al. (2007) for a review). Derived from a structural microstructure model, this measure estimates in stock trades from order flow data. This measure is designed to capture the probability of informed trading in a stock, and stocks with high levels of PIN have more information coming from private sources than public ones. Chen et al. (2007) conclude that, like the price synchronicity measure implemented in Section 5.4, PIN measures the private information in price that is not otherwise available to managers. Their analysis finds that stocks with high levels of private information (high PIN) provide managers with new information and meaningfully affects their investment decisions.

More recently, Duarte et al. (2020) analyzes several structural microstructure models, including the PIN (Easley et al., 1996) and APIN ((Duarte and Young, 2009)) models. They also provide new, generalized extensions of the PIN model and Odders-White and Ready (2008) models (coined GPIN and OWR respectively). Compared to traditional PIN and APIN measures, GPIN relies on flow to identify private information arrival and allows expected daily turnover from noise trading to be random, and the OWR model uses both order information and returns to identify private information. The authors conclude that PIN and APIN models are unreliable, though GPIN and OWR models are better proxies for information asymmetry and private information arrival.

Within the context of company-run stress tests, it is possible that measures of informed trading convey information to regulators, analogous to the way informed traders convey information to managers in Chen et al. (2007). Following Duarte et al. (2020), we analyze both GPIN and OWR models. The authors make annual parameter estimates available on their website[22], and we merge those parameter estimates to the fourth quarter Y-9C data for all banks in our sample. We implement the framework in Equation 7 on an annual basis and present the results in Table 10.

The coefficient on the interaction term $DISCLOSE \times TREAT$ is negative for both GPIN and OWR. However, we note that the dramatic reduction in our sample size for this analysis greatly reduces the power of the test. Coupled with the results presented in Table 7, none of the three measures of informed trading suggest that releasing company-run DFAST disclosures improve the regulator's ability to learn from equity prices.

7 Robustness

7.1 Matched Sample

Our baseline analysis uses all banks with assets of less than \$10 billion as a control sample. However, there may be concerns that not all of these banks are comparable to our treated banks. In this section, we conduct a matched sample analysis between treatment and control banks. Specifically, we break our sample into terciles of capital, credit quality (average charge-offs), and market-to-book. We match all eligible treated banks with assets of between \$10 billion - \$50 billion to control banks based on the three criteria and keep all matching banks. This reduces our sample to 5,359 bank-

[22]See: https://edwinhu.github.io/pin/

quarters. Of these 5,359 bank-quarters, 4,039 bank-quarters are in the control sample, and 1,321 are treated. Compared to our original sample of 6,772 observations, 1,408 control bank-quarters and 5 treatment bank-quarters are unmatched.

We present the results of our matched sample analysis in Table 11. The results are consistent with those presented in Tables 3 - 7. Specifically, our matched sample indicates that the number of analysts making forecasts and total forecasts declines, and this decline is driven by seasoned analysts. As indicated by the lack of significance on *EPS_FE*, analysts do not become significantly more accurate, yet dispersion declines. Both measures of private information are negative and statistically significant, and the decline in price informativeness (*IDIOSYN*) is also statistically significant.

7.2 Comparison to Fed-DFAST Banks

Our previous analysis has compared our treated banks that were required to disclose company-run stress tests to a group of banks that was never required to make such disclosures. In this analysis, we compare the set of treated banks to a set of control banks where the Federal Reserve both conducted and released DFAST results. We limit the treated sample to banks with assets exceeding $10 billion to facilitate comparison, though these results are also consistent when examining all banks with assets over $25 billion. As discussed in Section 2.2, the Federal Reserve conducted and disclosed the adverse and severely adverse scenario DFAST results for banks with assets above $100 billion in 2013 (19 banks) and those with assets greater than $50 billion in 2014 (30 banks), so we limit our sample to 2014-2017. This analysis has two shortcomings. First, our pre-period is very short, and second, both treatment and control groups are very small. We contine to define treated banks as those that made company-run DFAST disclosures.

Nonetheless, the results presented in Table 12 are qualitatively consistent with our previous analysis. This sample is over 85% smaller than the sample used for our primary analysis, which leads to a substantial reduction in power. While the results are mostly consistent with our previous analysis, not all coefficients demonstrate statistical significance. When treated banks start disclosing company-run DFAST results, they have fewer analysts making forecasts, and forecast errors and dispersion both decrease. Furthermore, both proxies for private information indicate that analysts are producing less private information, and analyst forecasts contain less private, firm-specific idiosyncratic information.

8 Conclusion

Our paper empirically examines some of the potential costs and benefits associated with the disclosure of company-run DFAST results. One of the most established benefits of releasing supervisory information to market participants is the enhanced ability for market participants to provide market discipline. In contrast, recent papers have highlighted a negative consequence of releasing more regulatory information and caution that market participants may become more reliant on public information as opposed to producing their own potentially valuable private information. We find that these disclosures provide new information to market participants. This could reduce information asymmetry between market participants and disclosing banks, facilitating more effective market discipline. However, we also find evidence that stress test disclosures are associated with a reduction in analysts following and decrease in the private, bank-specific information that analysts produce.

Further, we find that, post-disclosure, equity prices become less informative. This suggests that any increase in private information produced by market participants unrelated to the information contained in company-run DFAST disclosures does not exceed the decline in private information production caused by these disclosures. In this respect, if a regulator is attempting to garner greater information from equity prices, our paper suggests that regulators should exercise caution in releasing supervisory information, as this can reduce private information production and overall price informativeness.

References

Acharya, V. V., H. Mehran, and A. V. Thakor. 2016. Caught between Scylla and Charybdis? Regulating bank leverage when there is rent seeking and risk shifting. *The Review of Corporate Finance Studies* 5:36–75.

Afonso, G., A. Kovner, and A. Schoar. 2011. Stressed, not frozen: The federal funds market in the financial crisis. *The Journal of Finance* 66:1109–1139.

Altschuler, D., G. Chen, and J. Zhou. 2015. Anticipation of management forecasts and analystsâ private information search. *Review of Accounting Studies* 20:803–838.

Angeletos, G.-M., and A. Pavan. 2007. Efficient use of information and social value of information. *Econometrica* 75:1103–1142.

Barron, O. E., O. Kim, S. C. Lim, and D. E. Stevens. 1998. Using analysts' forecasts to measure properties of analysts' information environment. *Accounting Review* pp. 421–433.

Beatty, A., and S. Liao. 2011. Do delays in expected loss recognition affect banks' willingness to lend? *Journal of Accounting and Economics* 52:1–20.

Beatty, A., and S. Liao. 2014. Financial accounting in the banking industry: A review of the empirical literature. *Journal of Accounting and Economics* 58:339–383.

Berger, A. N., and C. H. Bouwman. 2013. How does capital affect bank performance during financial crises? *Journal of Financial Economics* 109:146–176.

Bernanke, B. S., C. S. Lown, and B. M. Friedman. 1991. The credit crunch. *Brookings Papers on Economic Activity* 1991:205–247.

Bird, A., S. A. Karolyi, T. G. Ruchti, and A. Sudbury. 2015. Bank regulator bias and the efficacy of stress test disclosures. *Working Paper* .

Board of Governors of the Federal Reserve System. 2011. Comprehensive capital analysis and review: overview and objectives.
`http://www.federalreserve.gov/newsevents/press/bcreg/bcreg20110318a1.pdf`.

Board of Governors of the Federal Reserve System. 2015a. Comprehensive capital analysis and review 2015: assessment framework and results.
`http://www.federalreserve.gov/newsevents/press/bcreg/bcreg20150311a1.pdf`.

Board of Governors of the Federal Reserve System. 2015b. Dodd-Frank Act Stress Test 2015: Supervisory Stress Test Methodology and Results.
`http://www.federalreserve.gov/newsevents/press/bcreg/bcreg20150305a1.pdf`.

Bolton, P., and X. Freixas. 2006. Corporate finance and the monetary transmission mechanism. *The Review of Financial Studies* 19:829–870.

Bond, P., and I. Goldstein. 2015. Government intervention and information aggregation by prices. *The Journal of Finance* 70:2777–2812.

Bond, P., I. Goldstein, and E. Prescott. 2010. Market-based corrective actions. *The Review of Financial Studies* 23:781â–820.

Bouvard, M., P. Chaigneau, and A. D. Motta. 2015. Transparency in the financial system: Rollover risk and crises. *The Journal of Finance* 70:1805–1837.

Brown, L. D., R. L. Hagerman, P. A. Griffin, and M. E. Zmijewski. 1987. An evaluation of alternative proxies for the market's assessment of unexpected earnings. *Journal of Accounting and Economics* 9:159–193.

Bushman, R. M. 2014. Thoughts on financial accounting and the banking industry. *Journal of Accounting and Economics* 58:384–395.

Candelon, B., and A. N. Sy. 2015. *How did markets react to stress tests?* International Monetary Fund.

Chen, Q., I. Goldstein, and W. Jiang. 2007. Price informativeness and investment sensitivity to stock price. *The Review of Financial Studies* 20:619–650.

Chen, Y., and I. Hasan. 2006. The transparency of the banking system and the efficiency of information-based bank runs. *Journal of Financial Intermediation* 15:307–331.

Dang, T. V., G. Gorton, B. Holmström, and G. Ordonez. 2017. Banks as secret keepers. *The American Economic Review* 107:1005–29.

Daniel, K., M. Grinblatt, S. Titman, and R. Wermers. 1997. Measuring mutual fund performance with characteristic-based benchmarks. *The Journal of Finance* 52:1035–1058.

Duarte, J., E. Hu, and L. Young. 2020. A comparison of some structural models of private information arrival. *Journal of Financial Economics* 135:795–815.

Duarte, J., and L. Young. 2009. Why is PIN priced? *Journal of Financial Economics* 91:119–138.

Durnev, A., R. Morck, and B. Yeung. 2004. Value enhancing capital budgeting and firm-specific stock returns variation. *The Journal of Finance* 59:2013–611.

Durnev, A., R. Morck, B. Yeung, and P. Zarowin. 2003. Does greater firm-specific return variation mean more or less informed stock pricing? *Journal of Accounting Research* 41:797–836.

Easley, D., N. M. Kiefer, and M. O'HARA. 1996. Cream-skimming or profit-sharing? The curious role of purchased order flow. *The Journal of Finance* 51:811–833.

Easley, D., N. M. Kiefer, and M. O'Hara. 1997a. The information content of the trading process. *Journal of Empirical Finance* 4:159–186.

Easley, D., N. M. Kiefer, and M. O'Hara. 1997b. One day in the life of a very common stock. *The Review of Financial Studies* 10:805–835.

Evanoff, D. D., and L. D. Wall. 2004. Subordinated debt as bank capital: a proposal for regulatory reform. *Federal Reserve Bank of Chicago Economic Perspectives* 24:40–53.

Feldman, R., and J. Schmidt. 2003. Supervisory use of market data in the federal reserve system. Tech. rep., Working Paper, Federal Reserve Bank of Minneapolis.

Fernandes, M., D. Igan, and M. Pinheiro. 2020. March madness in Wall Street:(What) does the market learn from stress tests? *Journal of Banking & Finance* 112:105250.

Flannery, M., B. Hirtle, and A. Kovner. 2017. Evaluating the information in the federal reserve stress tests. *Journal of Financial Intermediation* 29:1–18.

Furlong, F. T., R. Williams, et al. 2006. Financial market signals and banking supervision: Are current practices consistent with research findings. *Federal Reserve Bank of San Francisco Economic Review* pp. 17–29.

Gao, P., and P. J. Liang. 2013. Informational feedback, adverse selection, and optimal disclosure policy. *Journal of Accounting Research* 51:1133–1158.

Goldstein, I., and Y. Leitner. 2018. Stress tests and information disclosure. *Journal of Economic Theory* 177:34–69.

Goldstein, I., and H. Sapra. 2013. Should banks' stress test results be disclosed? An analysis of the costs and benefits. *Foundations and Trends in Finance* .

Goldstein, I., and L. Yang. 2019. Good disclosure, bad disclosure. *Journal of Financial Economics* 131:118–138.

Grossman, S. 1976. On the efficiency of competitive stock markets where trades have diverse information. *The Journal of Finance* 31:573–585.

Hanson, S. G., A. K. Kashyap, and J. C. Stein. 2011. A macroprudential approach to financial regulation. *Journal of Economic Perspectives* 25:3–28.

Hayek, F. A. 1945. The use of knowledge in society. *The American Economic Review* 35:519–530.

He, Z., and A. Manela. 2016. Information acquisition in rumor-based bank runs. *The Journal of Finance* 71:1113–1158.

Healy, P. M., and K. G. Palepu. 2001. Information asymmetry, corporate disclosure, and the capital markets: A review of the empirical disclosure literature. *Journal of Accounting and Economics* 31:405–440.

Herring, R. J. 2004. The subordinated debt alternative to Basel II. *Journal of Financial Stability* 1:137–155.

Hirtle, B. 2014. Bank holding company dividends and repurchases during the financial crisis. *FRB of New York Staff Report* .

Hirtle, B., and A. Lehnert. 2015. Supervisory stress tests. *Annual Review of Financial Economics* 7:339–355.

Hutton, A. P., A. J. Marcus, and H. Tehranian. 2009. Opaque financial reports, R2, and crash risk. *Journal of Financial Economics* 94:67–86.

Imhoff Jr, E. A., and G. J. Lobo. 1992. The effect of ex ante earnings uncertainty on earnings response coefficients. *The Accounting Review* pp. 427–439.

Jin, L., and S. C. Myers. 2006. R2 around the world: New theory and new tests. *Journal of Financial Economics* 79:257–292.

Kashyap, A. K., and J. C. Stein. 1995. The impact of monetary policy on bank balance sheets. In *Carnegie-Rochester Conference Series on Public Policy*, vol. 42, pp. 151–195. Elsevier.

Kashyap, A. K., and J. C. Stein. 2000. What do a million observations on banks say about the transmission of monetary policy? *The American Economic Review* 90:407–428.

Krainer, J., and J. A. Lopez. 2004. Incorporating equity market information into supervisory monitoring models. *Journal of Money, Credit and Banking* pp. 1043–1067.

Leuz, C., and P. D. Wysocki. 2016. The economics of disclosure and financial reporting regulation: Evidence and suggestions for future research. *Journal of Accounting Research* 54:525–622.

McNichols, M., and B. Trueman. 1994. Public disclosure, private information collection, and short-term trading. *Journal of Accounting and Economics* 17:69–94.

Michael, I. 2004. Accounting and financial stability. *Financial Stability Review* 16:118–128.

Morck, R., B. Yeung, and W. Yu. 2000. The information content of stock markets: why do emerging markets have synchronous stock price movements? *Journal of Financial Economics* 58:215–260.

Morgan, D. P., S. Peristiani, and V. Savino. 2014. The information value of the stress test. *Journal of Money, Credit and Banking* 46:1479–1500.

Morris, S., and H. S. Shin. 2002. Social value of public information. *The American Economic Review* 92:1521–1534.

Morrison, A. D., and L. White. 2013. Reputational contagion and optimal regulatory forbearance. *Journal of Financial Economics* 110:642–658.

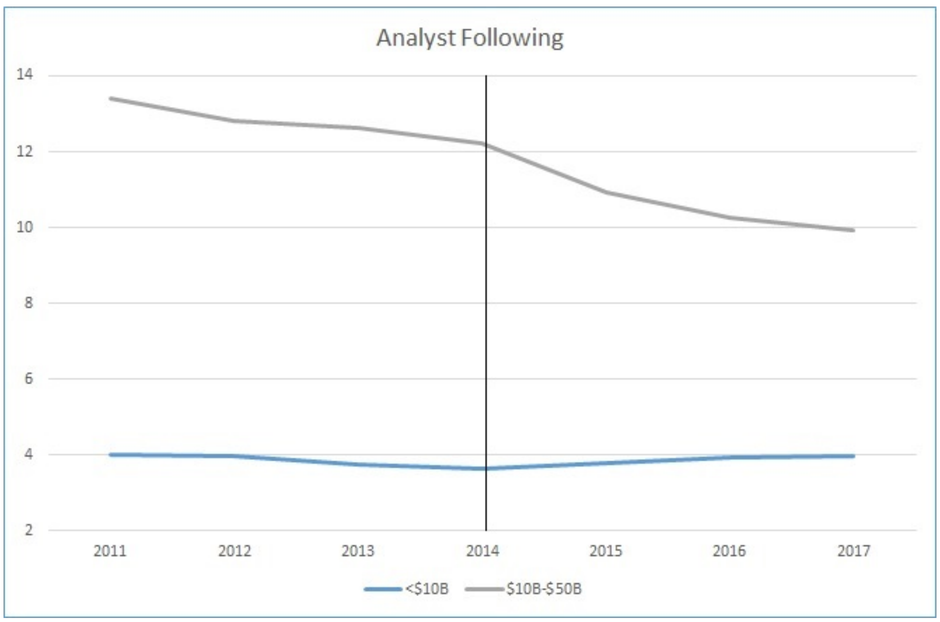
Odders-White, E. R., and M. J. Ready. 2008. The probability and magnitude of information events. *Journal of Financial Economics* 87:227–248.

Petrella, G., and A. Resti. 2013. Supervisors as information producers: Do stress tests reduce bank opaqueness? *Journal of Banking & Finance* 37:5406–5420.

Roll, R. 1984. A simple implicit measure of the effective bid-ask spread in an efficient market. *The Journal of Finance* 39:1127–1139.

Shleifer, A., and D. Wolfenzon. 2002. Investor protection and equity markets. *Journal of Financial Economics* 66:3–27.

Figure 1. Trends in Analyst Following
This table compares analyst following each year for treated banks that are required to both administer and disclose DFAST results and control banks that are never required to conduct such stress tests. Data quantifying the number of analysts come from the IBES Summary file.



Banks holding assets less than $50 billion

Table 1. Summary Statistics

This table shows the summary statistics for the key variables of interest defined in Appendix B. For each variable, we show the mean, standard deviation, 25th percentile, median, 75th percentile, maximum value, and number of observations in Columns 1-8, respectively, for the full sample of banks with assets less than $50 billion. Our sample period spans 2011-2017.

	(1) Mean	(2) SD	(3) Min	(4) P25	(5) Med	(6) P75	(7) Max	(8) N
EPS_ANALYSTS	6.9988	5.5451	1.0000	3.0000	6.0000	10.0000	34.0000	6,772
SEASONED_FCST	6.1031	4.9251	0.0000	2.0000	5.0000	8.0000	29.0000	6,772
ROOKIE_FCST	1.1196	1.2570	0.0000	0.0000	1.0000	2.0000	10.0000	6,772
EPS_FCSTNUM	7.2956	5.7761	1.0000	3.0000	6.0000	10.0000	42.0000	6,772
EPS_ABS_FE	0.0111	0.0449	0.0000	0.0006	0.0015	0.0037	0.3440	6,516
EPS_DISPERSION	0.0070	0.0284	0.0000	0.0006	0.0011	0.0024	0.2137	5,942
EPS_PRIVINFO1	0.0039	0.0151	0.0000	0.0002	0.0005	0.0017	0.1274	5,849
EPS_PRIVINFO2	0.0349	0.1659	0.0000	0.0009	0.0023	0.0060	1.2647	6,428
GPIN	0.4244	0.1491	0.0765	0.3083	0.4831	0.5320	0.6801	543
OWR	0.5102	0.2344	0.0015	0.3590	0.5472	0.6603	1.0000	543
IDIOSYN	1.0691	1.3305	-2.0947	0.1762	0.8134	1.7330	7.7041	6,761
NCO	0.0017	0.0025	-0.0003	0.0002	0.0006	0.0021	0.0131	6,772
LNASSETS	15.1358	1.0604	13.2691	14.2655	15.0179	15.8959	17.7491	6,772
MTB	1.1684	0.4789	0.2287	0.8759	1.1490	1.4083	3.0994	6,772
CAPITAL	0.1108	0.0297	0.0494	0.0926	0.1070	0.1247	0.2705	6,772
SIGMA	0.0232	0.0142	0.0064	0.0142	0.0176	0.0269	0.1251	6,740
SKEW	0.2416	0.7101	-4.0293	-0.0668	0.1647	0.4819	7.8608	6,736
KURT	3.4632	5.7463	-0.4423	1.1346	1.9993	3.4726	119.7090	6,736

Table 2. Return Results
Panel A presents the results for five-day raw and characteristic-matched benchmark of Daniel et al. (1997) (DGTW) cumulative abnormal returns around stress test disclosure dates. The columns in Panel A indicate the number of stress test disclosures of each type, five-day raw returns, the number of positive and negative instances of five-day raw returns, five-day DGTW-adjusted returns, and the number of positive and negative instances of five-day DGTW-adjusted returns. In Panel A, we report the results for disclosure dates for all stress tested banks (Federal Reserve and non-Federal Reserve DFAST disclosures) in Row 1 along with sub-samples consisting of Non-Federal Reserve DFAST disclosures (Row 2), Federal Reserve DFAST disclosures (Row 3), the first release of the non-Federal Reserve DFAST disclosures (Row 4), and subsequent releases for non-Federal Reserve DFAST disclosures (Row 5). In Panel B, we report the five-day raw and DGTW-adjusted returns, along with volume. In Panel B, we retain 20 trading days around all company-run DFAST disclosures. The variable *Disclosure Window* represents the window from -2 to 2 trading days surrounding company-run DFAST disclosures. We also control for analogous five day trading windows surrounding all earnings announcements, the 2016 Presidential Election (11/8/2016), and the the day the Tax Cuts and Jobs Act passed in the House of Representatives (11/17/2017). In Panel B, standard errors are clustered at the bank-level. Our sample period spans 2011-2017, and robust t-statistics are in parentheses. Significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

Panel A: Disclosure Window Returns

	N	Raw	Positive / Negative	DGTW	Positive / Negative
All Banks	406	-0.0046	217 / 189	-0.0013	214 / 192
		(-2.50)**		(-0.89)	
Non-Fed DFAST	136	-0.0015	71 / 65	0.0008	73 / 63
		(-0.50)		(0.35)	
Fed DFAST	270	-0.0062	146 / 124	-0.0023	141 / 129
		(-2.57)**		(-1.33)	
First Release (Non-Fed)	51	-0.0084	24 / 27	-0.0035	23 / 28
		(-2.10)**		(-0.9)	
Subsequent Releases (Non-Fed)	85	0.0027	47 / 38	0.0034	50 / 35
		(0.78)		(1.09)	

Panel B: Five-day absolute Returns and Volume around Company-Run DFAST Disclosures

	Raw	DGTW	Volume
Disclosure Window	0.0009	0.0011	0.0005
	(1.73)*	(2.75)***	(2.22)**
Earnings Announcement	0.0018	0.0022	0.0012
	(3.11)***	(5.03)***	(3.65)***
2016 Presidential Election	0.0120	0.0057	0.0019
	(23.51)***	(9.30)***	(5.58)***
Tax Cuts and Jobs Act Window	-0.0019	0.0006	-0.0010
	(-3.31)***	(0.81)	(-1.78)*
Constant	0.0099	0.0075	0.0065
	(135.31)***	(106.22)***	(112.10)***
Bank FE	Yes	Yes	Yes
SE Cluster	Bank	Bank	Bank
Observations	5,130	5,130	5,130
R-squared	0.1258	0.0999	0.1720

Table 3. Number of Analysts Making Forecasts and Total Analyst Forecasts
This table reports the OLS regression results where the dependent variable is the number of analysts making Earnings Per Share forecasts (*EPS_ANALYSTS*) in Columns 1-2 or the total number of analyst forecasts (*EPS_FCSTNUM*) for banks with assets of less than \$50 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between \$10 billion and \$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and year-quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

	(1) EPS_ANALYSTS	(2) EPS_FCSTNUM
DISCLOSE x TREAT	-0.8130	-0.7670
	(-2.07)**	(-2.05)**
LNASSETS	2.3008	2.2310
	(5.53)***	(5.26)***
NCO	-14.5046	-15.4160
	(-0.48)	(-0.51)
MVE	-0.0000	-0.0000
	(-2.11)**	(-1.99)**
MTB	0.0271	-0.0099
	(0.09)	(-0.03)
CAPITAL	8.4666	9.5439
	(1.60)	(1.78)*
Year-Quarter FE	Yes	Yes
Observations	6,772	6,772
R-squared	0.9320	0.9361

Table 4. Number of Seasoned and Rookie Analysts
This table reports the OLS regression results where the dependent variable is either the number of seasoned (*SEASONED_FCST*) or "rookie" (*ROOKIE_FCST*) analysts making Earnings Per Share forecasts for banks with assets of less than \$50 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between \$10 billion and \$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and year-quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

	(1) SEASONED_FCST	(2) ROOKIE_FCST
DISCLOSE x TREAT	-0.6260	-0.1351
	(-2.00)**	(-0.97)
LNASSETS	1.9085	0.3600
	(5.68)***	(2.64)***
NCO	-9.9104	-4.4375
	(-0.36)	(-0.48)
MVE	-0.0000	-0.0000
	(-2.83)***	(-0.47)
MTB	-0.0172	-0.0338
	(-0.07)	(-0.38)
CAPITAL	6.8649	2.1294
	(1.69)*	(1.15)
Year-Quarter FE	Yes	Yes
Observations	6,772	6,772
R-squared	0.9148	0.3779

Table 5. Analyst Forecast Error and Dispersion

This table reports the OLS regression results where the dependent variables are analyst Earnings Per Share Forecast Error and Dispersion (*EPS_FE* and *EPS_DISPERSION*. We examine banks with assets less than $50 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between $10 billion and $50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and year-quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

	(1) EPS_FE	(2) EPS_DISPERSION
DISCLOSE x TREAT	-0.0047	-0.0051
	(-1.56)	(-2.68)***
LNASSETS	0.0208	-0.0002
	(2.53)**	(-0.53)
NCO	5.0888	0.0105
	(4.81)***	(1.78)*
MVE	0.0000	0.0000
	(2.77)***	(1.87)*
MTB	-0.0224	-0.0147
	(-3.95)***	(-4.46)***
CAPITAL	-0.5826	-0.3902
	(-4.35)***	(-4.00)***
Year-Quarter FE	Yes	Yes
Observations	5,849	5,942
R-squared	0.5934	0.6417

Table 6. Private Information Production
This table reports the OLS regression results where the dependent variable the amount of private firm-specific information produced by analysts as measured through earnings per share forecasts using either the methodology of Barron et al. (1998), *EPS_PRIVATE_INFO1*, or time-series predictions using a seasonal random walk, *EPS_PRIVATE_INFO2*, for banks with assets less than $50 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between $10-$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

	(1) EPS_PRIV_INFO1	(2) EPS_PRIV_INFO2
DISCLOSE x TREAT	-0.0033	-0.0193
	(-2.66)***	(-1.69)*
LNASSETS	0.0058	0.0447
	(2.15)**	(1.30)
NCO	1.8816	26.7660
	(5.20)***	(4.39)***
MVE	0.0000	0.0000
	(1.12)	(0.86)
MTB	-0.0068	-0.0250
	(-3.50)***	(-1.07)
CAPITAL	-0.1568	-1.1716
	(-3.93)***	(-2.86)***
Year-Quarter FE	Yes	Yes
Observations	5,849	4,357
R-squared	0.3604	0.5153

Table 7. Market Synchronicity

This table reports the OLS regression results where the dependent variable is the degree of idiosynchratic information in stock returns (*IDIOSYN*), where higher values mean that a bank's returns are less synchronous with the market and thus are driven by more idiosyncratic information. Following Hutton et al. (2009), *IDIOSYN* is calculated as ln($\frac{1-QuarterlyRSQ}{QuarterlyRSQ}$), where $QuarterlyRSQ$ is alculated as the coefficient of determination from a regression of firm excess returns on market and industry excess returns, where the model is defined as $r_q = \alpha + \beta_1 r_{m,q-1} + \beta_2 r_{m,q} + \beta_3 r_{m,q+1} + \gamma_1 r_{i,q-1} + \gamma_2 r_{i,q} + \gamma_3 r_{i,q+1} + \epsilon_q$ where r_q, $r_{m,q}$, and $r_{i,q}$ are excess returns of the stock, the market, and the stock's industry during quarter q. Our sample contains banks with assets less than \$50 billion. The variable *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between \$10-\$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

	(1) IDIOSYN
DISCLOSE x TREAT	-0.2756
	(-4.13)***
LNASSET	-0.8095
	(-9.15)***
NCO	26.8120
	(2.74)***
MVE	0.0000
	(3.38)***
MTB	-0.1884
	(-2.31)**
CAPITAL	1.1959
	(0.89)
VOLATILITY	0.4306
	(0.15)
SKEW	-0.0172
	(-0.79)
KURTOSIS	-0.0013
	(-0.48)
Year-Quarter FE	Yes
Observations	6,736
R-squared	0.7232

Table 8. Company-Run DFAST Disclosures and Bank-level Beta Analysis
This table reports the OLS regression results where the dependent variable is bank-level beta for each bank that files a company-run DFAST disclosure. For each bank, we use the capital asset pricing model (CAPM) to calculate quarterly betas for each sample bank based on daily returns in the last 60 trading days of each quarter. We also calculate the volatility of beta, calculated daily using trailing 60-day returns, during each quarter. Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017. In Panel A, we analyze changes in betas for treated banks with positive and negative disclosure window returns around their first DFAST disclosures. The variable *Positive CAR* is an indicator variable that takes the value of 1 if the five-day CAR surrounding the bank's first disclosed company-run DFAST disclosure was positive. *DISCLOSE* $\times$ *Positive CAR* is the interaction between the *DISCLOSE* and *Positive CAR* variables. In Panel B, we examine changes in beta and the volatility of beta for treated banks relative to control banks, where *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between $10-$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Bank fixed effects are included in all regressions, and year-quarter fixed effects are included in Panel B. The year-quarter fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

Panel A: Treated Bank Sample

	(1) 2011-2017 β	(2) 2014-2015 β
DISCLOSE x Positive CAR	0.0068	0.0044
	(0.20)	(0.13)
DISCLOSE	0.1684	0.0063
	(1.28)	(0.05)
LNASSET	0.5316	0.1039
	(2.58)**	(0.47)
NCO	32.9249	24.7846
	(1.39)	(0.47)
MVE	0.0000	-0.0000
	(0.55)	(-0.43)
MTB	0.0632	0.0266
	(0.31)	(0.11)
CAPITAL	-0.6432	0.0747
	(-0.30)	(0.02)
Bank FE	Yes	Yes
Observations	1,098	354
R-squared	0.7228	0.6308

Panel B: Treated and Control Comparisons

	(1) 2011-2017 β	(2) 2011-2017 $\sigma(\beta)$
DISCLOSE x TREAT	0.2191	0.1709
	(3.41)***	(1.95)*
LNASSET	0.3357	0.2212
	(7.33)***	(2.50)**
NCO	13.9548	109.5419
	(2.25)**	(6.21)***
MVE	-0.0000	0.0000
	(-2.29)**	(1.13)
MTB	0.2759	-0.3399
	(6.82)***	(-2.51)**
CAPITAL	0.7847	-5.6413
	(1.61)	(-3.34)***
Bank FE	Yes	Yes
Year-Quarter FE	Yes	Yes
Observations	6,054	6,054
R-squared	0.6564	0.6344

Table 9. Analyst Characteristics

This table reports univariate tests examining the characteristics of analysts that maintain coverage during the disclosure period and those that drop coverage. Panel A focuses on the total number of years of experience of each analyst as of the end of 2014, and Panel B highlights the forecast accuracy of analysts at the end of 2014. Column 1 indicates the number of assets the bank is holding. Column 2 indicates the total number of analysts covering each type of bank at the end of 2014. Columns 3 and 4 contain a breakdown of the number of 2014 analysts that drop and maintain coverage over the disclosure period, and their respective means for each variable are given in Columns 5 and 6. We compute the difference in Column 7 and present the two-tailed t-test statistic and p-value in Columns 8 and 9.

(1) Bank Asset Size	(2) Total Analysts in 2014	(3) Number of 2014 Analysts Staying	(4) Number of 2014 Analysts Dropping	(5) Mean of Analysts Staying	(6) Mean of Analysts Dropping	(7) Difference	(8) test statistic	(9) two-tailed p-value
Panel A: Analyst Experience (years)								
< $10 billion	678	417	261	11.7687	10.6420	1.1267	2.2351	0.0257
$10 - $50 billion	496	299	197	10.6385	12.1472	-1.5087	-2.5076	0.0125
Panel B: Forecast Accuracy								
< $10 billion	666	411	255	0.0025	0.0025	0.0000	0.0806	0.9358
$10 - $50 billion	478	292	186	0.0015	0.0016	0.0001	-0.5984	0.5499

Table 10. Other Market Participants

This table reports the OLS regression results where the dependent variable is the structural alpha parameter from the GPIN and OWR models presented in Duarte et al. (2020). Our sample period spans 2011-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and quarter fixed effects are included in all regressions, and these fixed effects subsume the direct effects of *TREAT* and *POST*, which are omitted. All other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

	(1) GPIN	(2) OWR
DISCLOSE x TREAT	-0.0177	-0.0057
	(-0.61)	(-0.10)
LNASSETS	-0.4965	14.4406
	(-0.07)	(1.05)
NCO	-0.0000	0.0000
	(-0.90)	(1.46)
MVE	-0.0534	0.0129
	(-1.77)*	(0.20)
MTB	0.0451	-0.0313
	(1.34)	(-0.60)
CAPITAL	-0.7530	0.4043
	(-1.29)	(0.49)
Year-Quarter FE	Yes	Yes
Observations	543	543
R-squared	0.5881	0.4781

Table 11. Matched Sample Robustness

This table reports the OLS regression results for our matched sample of banks. Treated banks (between \$10 billion and \$50 billion) are matched to a control sample based on tercile of capital, market-to-book, and credit quality, as represented by the average charge-off rate of the trailing four quarters. The dependent variables are the number of analysts making earnings per share forecasts (*EPS_ANALYSTS*), the total number of analyst forecasts (*EPS_FCSTNUM*), number of seasoned (*SEASONED_FCST*) or "rookie" (*ROOKIE_FCST*) analysts making Earnings Per Share forecasts, along with analyst earnings earnings per share forecast error (*EPS_FE*), dispersion (*EPS_DISPERSION*), the amount of private firm-specific information produced by analysts as measured through earnings per share forecasts using either the methodology of Barron et al. (1998), *EPS_PRIVATE_INFO1*, or time-series predictions using a seasonal random walk, *EPS_PRIVATE_INFO2*, and synchronicity with the market (*IDIOSYN*) for banks with assets greater than \$15 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between \$10 billion and \$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2013-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and year-quarter fixed effects are included in all regressions, and other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * $p < 0.10$, ** $p < 0.05$, and *** $p < 0.01$.

	(1) EPS_ANALYSTS	(2) EPS_FCSTNUM	(3) SEASONED_FCST	(4) ROOKIE_FCST	(5) EPS_FE	(6) EPS_DISPERSION	(7) EPS_PRIV_INFO1	(8) EPS_PRIV_INFO2	(9) IDIOSYN
DISCLOSE x TREAT	**-0.9083**	**-0.8506**	**-0.7294**	-0.1199	-0.0039	**-0.0040**	**-0.0027**	**-0.0181**	**-0.2487**
	(-2.34)**	**(-2.26)****	**(-2.34)****	(-0.84)	(-1.36)	**(-2.13)****	**(-2.20)****	**(-1.70)***	**(-4.05)*****
LNASSETS	2.1732	2.1425	1.8191	0.3263	0.0186	0.0117	0.0073	0.0569	-0.7033
	(4.80)***	(4.65)***	(5.00)***	(2.07)**	(2.24)**	(1.85)*	(2.45)**	(1.52)	(-8.25)***
NCO	-27.8419	-22.3855	-21.4355	0.2679	5.4278	3.8176	1.7397	28.4316	33.7146
	(-0.76)	(-0.61)	(-0.67)	(0.02)	(5.06)***	(5.18)***	(5.22)***	(3.85)***	(3.16)***
MVE	-0.0000	-0.0000	-0.0000	-0.0000	0.0000	0.0000	0.0000	0.0000	0.0000
	(-1.84)*	(-1.72)*	(-2.65)***	(-0.33)	(2.72)***	(1.89)*	(1.00)	(0.75)	(3.08)***
MTB	-0.2380	-0.2850	-0.2210	-0.0699	-0.0213	-0.0150	-0.0069	-0.0298	-0.1258
	(-0.74)	(-0.89)	(-0.87)	(-0.65)	(-3.26)***	(-3.74)***	(-3.34)***	(-1.40)	(-1.54)
CAPITAL	6.7742	7.2909	5.0023	1.9461	-0.5128	-0.3624	-0.1451	-1.0141	1.8975
	(1.16)	(1.21)	(1.14)	(0.88)	(-3.89)***	(-3.75)***	(-3.73)***	(-2.51)**	(1.38)
Equity Controls	No	No	No	No	No	No	No	No	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	5,359	5,359	5,359	5,359	4,791	4,838	4,791	3,611	5,331
R-squared	0.9345	0.9392	0.9178	0.3888	0.6062	0.6406	0.3648	0.5169	0.7279

Table 12. Large Banks as the Control Group

This table reports the OLS regression results where the dependent variables are the number of analysts making earnings per share forecasts (*EPS_ANALYSTS*), the total number of analyst forecasts (*EPS_FCSTNUM*), number of seasoned (*SEASONED_FCST*) or "rookie" (*ROOKIE_FCST*) analysts making Earnings Per Share forecasts, along with analyst earnings earnings per share forecast error (*EPS_FE*), dispersion (*EPS_DISPERSION*), the amount of private firm-specific information produced by analysts as measured through earnings per share forecasts using either the methodology of Barron et al. (1998), *EPS_PRIVATE_INFO1*, or time-series predictions using a seasonal random walk, *EPS_PRIVATE_INFO2*, and synchronicity with the market (*IDIOSYN*) for banks with assets greater than $15 billion. *TREAT* is an indicator variable that takes a value of 1 if the bank has assets between $25-$50 billion and is therefore required to conduct a company-run stress test and disclose the results. Our sample period spans 2013-2017, and the *DISCLOSE* variable is an indicator variable that takes a value of 1 during the years 2015-2017, and $DISCLOSE \times TREAT$ is the interaction between the *DISCLOSE* and *TREAT* variables. Firm and year-quarter fixed effects are included in all regressions, and other variable definitions are defined in Appendix B. Standard errors are adjusted for cluster effects at the bank level. Robust t-statistics are in parentheses. Significance is denoted by * p <0.10, ** p < 0.05, and *** p<0.01.

	(1) EPS_ANALYSTS	(2) EPS_FCSTNUM	(3) SEASONED_FCST	(4) ROOKIE_FCST	(5) EPS_FE	(6) EPS_DISPERSION	(7) EPS_PRIV_INFO1	(8) EPS_PRIV_INFO2	(9) IDIOSYN
DISCLOSE x TREAT	-0.5824	-0.2590	-0.3044	0.0631	0.0002	-0.0004	-0.0003	-0.0030	-0.1735
	(-0.81)	(-0.36)	(-0.43)	(0.59)	(0.79)	(-1.29)	(-0.15)	(-1.82)*	(-2.51)**
LNASSETS	3.7913	3.4836	3.5009	-0.1711	-0.0013	-0.0000	-0.0024	0.0059	36.7920
	(3.68)***	(3.24)***	(3.50)***	(-0.75)	(-1.14)	(-0.66)	(-0.76)	(1.99)**	(1.90)*
NCO	-140.5611	-100.9468	-132.8024	14.0929	0.4089	0.0004	1.6522	2.3421	-0.0000
	(-0.70)	(-0.50)	(-0.74)	(0.40)	(0.88)	(0.80)	(1.22)	(1.61)	(-1.01)
MVE	-0.0000	-0.0000	-0.0000	-0.0000	-0.0000	0.8481	-0.0000	0.0000	-0.0247
	(-0.14)	(-0.21)	(-0.22)	(-0.02)	(-0.36)	(1.89)*	(-0.08)	(0.66)	(-0.25)
MTB	-1.4497	-1.4147	-1.2244	-0.1733	-0.0018	-0.0000	-0.0003	-0.0007	-0.4987
	(-2.31)**	(-2.33)**	(-2.10)**	(-1.14)	(-3.20)***	(-0.15)	(-0.19)	(-0.32)	(-3.63)***
CAPITAL	-14.9631	-15.0745	-16.8092	0.2802	-0.0044	-0.0013	-0.0321	0.0643	1.5911
	(-1.02)	(-1.05)	(-1.24)	(0.11)	(-0.38)	(-2.17)**	(-0.85)	(1.66)	(0.72)
Equity Controls	No	No	No	No	No	No	No	No	Yes
Year-Quarter FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,492	1,492	1,492	1,492	1,484	1,484	1,484	1,279	1,487
R-squared	0.9551	0.9589	0.9513	0.1842	0.2898	0.4238	0.4907	0.1722	0.6669

A Stress Test History

This table reports the assets size reporting threshold for each type of stress test along with the number of banks stress tested by year.

	SCAP		CCAR		Federal-Reserve DFAST		Company-run DFAST	
	Asset threshold	Banks	Asset threshold	Banks	Asset threshold	Banks	Asset threshold	Banks
2009	>$100 billion	19						
2010								
2011			>$100 billion	19				
2012			>$100 billion	19				
2013			>$100 billion	18	>$100 billion	18		
2014			>$50 billion	30	>$50 billion	30		
2015			>$50 billion	31	>$50 billion	31	$10-$50 billion	51
2016			>$50 billion	33	>$50 billion	33	$10-$50 billion	54
2017			>$50 billion	33	>$50 billion	33	$10-$50 billion	58

B Variable Descriptions

Variable	Definition	Source
CAPITAL	Book value of equity (bhck3210) divided by total bank assets (bhck2170)	Y-9C
DISCLOSE	*DISCLOSE* is an indicator variable that takes a value of 1 during the years 2015-2017	
EPS_ACTUAL	Actual earnings per share	S&P Global Market Intelligence
EPS_ANALYSTS	Number of analysts providing earnings per share forecasts	IBES
EPS_DISPERSION	Standard deviation of analyst earnings per share forecasts normalized by share price at the end of the previous quarter	IBES
EPS_FCSTNUM	Total number of quarterly analyst earnings per share forecasts, allowing for analyst forecast revisions	IBES
EPS_FE	Earnings per share forecast error is the absolute value of the difference between the mean analyst earnings per share forecast and the actual earnings per share normalized by price at the end of the previous quarter, calculated as $\frac{\lvert EPS\_MEAN_{b,q-1} - EPS\_ACTUAL_{b,q}\rvert}{Price_{b,q-1}}$	IBES
EPS_MEAN	Average earnings per share forecast across all analysts	IBES
EPS_PRIV_INFO1	Following Barron et al. (1998), this variable measures the precision of idiosyncratic information in analysts' earnings per share forecasts, calculated as $\frac{EPS\_SD_q}{((1-1/EPS\_ANALYSTS_{b,q}) \times EPS\_SD_{b,q} + EPS\_SE_q)^2}$	IBES
EPS_PRIV_INFO2	The magnitude of the deviation of the average analysts' forecast from a time-series prediction based on a seasonal random walk normalized by price at the end of the previous quarter, calculated as $\frac{\lvert EPS\_MEAN_{b,q} - EPS\_ACTUAL_{b,q-4}\rvert}{Price_{b,q-1}}$	IBES
EPS_SD	Standard deviation of the consensus mean earnings per share forecast	IBES
EPS_SE	Squared error of the consensus mean earnings per share forecast, calculated as the square of the difference between *EPS_MEAN* and *EPS_ACTUAL*	IBES
GPIN	Annual structural parameter estimate from the GPIN model in Duarte et al. (2020). Compared to a standard PIN model, GPIN relies on order flow to identify private information arrival and allows expected daily turnover from noise trading to be random, while keeping the same information structure as the PIN model.	Edwin Hu's Website
IDIOSYN	This variable indicates the degree of idiosyncratic information in a firm's stock returns. Following Hutton et al. (2009), it is calculated as $\ln(\frac{1-QuarterlyRSQ_{b,q}}{QuarterlyRSQ_{b,q}})$	CRSP
KURTOSIS	Kurtosis of daily returns over the calendar year	CRSP
MTB	Market value of equity divided by book value of equity (bhck3210), where market value of equity is calculated as shares outstanding (shrout) multiplied by price (prc)	Y-9C and CRSP
MVE	Market value of equity is calculated as shares outstanding multiplied by price	Y-9C and CRSP

NCO	Average net charge-offs, calculated as gross charge-offs (bhck4635) minus recoveries (bhck4605), over the trailing four quarters normalized by last quarter's total loans	Y-9C
OWR	Annual structural parameter estimate from the Odders-White and Ready (2008) (OWR) model implemented in Duarte et al. (2020). The OWR model uses both order information and returns to identify private information.	Edwin Hu's Website
QuarterlyRSQ	Calculated as the coefficient of determination from a regression of firm excess returns on market and industry excess returns, where the model is defined as $r_{b,q} = \alpha + \beta_1 r_{m,q-1} + \beta_2 r_{m,q} + \beta_3 r_{m,q+1} + \gamma_1 r_{i,q-1} + \gamma_2 r_{i,q} + \gamma_3 r_{i,q+1} + \epsilon_q$ where r_q, $r_{m,q}$, and $r_{i,q}$ are excess returns of the stock, the market, and the stock's industry during quarter q.	CRSP
ROOKIE_FCST	Number of "rookie" analysts making forecast where a "rookie" is considered an analyst that has made less than four quarterly earnings forecasts at the same bank	IBES
SEASONED_FCST	Number of seasoned analysts making forecast where a seasoned analyst is considered an analyst that has previously made four quarterly earnings forecasts at the same bank	IBES
SKEW	Skewness of daily returns over the calendar year	CRSP
TOTAL_ASSETS	Total bank assets (bhck2170)	Y-9C
TREAT	*TREAT* is an indicator variable that takes a value of 1 if the bank has assets between \$10-\$50 billion and is therefor required to conduct a company-run stress test and disclose the results.	
VOLATILITY	Standard deviation of daily returns over the calendar year	CRSP