Essays in Empirical Finance and Macroeconomics:

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ESSAYS IN EMPIRICAL FINANCE AND MACROECONOMICS

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ESSAYS IN EMPIRICAL FINANCE AND MACROECONOMICS

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Abstract

In the wake of the financial crisis of 2007-2009, academics and policymakers have worked to empirically quantify macro-financial linkages. This dissertation contributes to this debate by covering two broad themes. First, substantial changes in bank regulation and supervision typically follow financial crises. Quantifying the impact of these new policies is of paramount importance to academics and policymakers. To this end, my research in this area sheds light on the ways in which changes in financial stability policy ultimately affect the economy. Bank stress testing has become a major tool of supervisory policy in the past decade. The first chapter, **The Real Effects of Stress Testing**, uses the introduction of annual stress testing of large U.S. banks in 2009 as a quasi-experiment to examine whether bank supervisory policies affect real economic activity. While stress-tested banks reduced their risk exposure to large corporate loans, foreign banks mostly offset this shock and enabled firms to continue

borrowing after the test. However, speculative grade firms that were highly exposed to stress-tested banks borrowed on worse terms after the test, and subsequently reduced fixed investment and employment. In contrast, highly exposed investment grade firms received new loans and expanded intangible investment. This paper provides insights into the effects of stress testing on the reallocation of risks in the financial system and the consequences for real economic activity.

The structure of the U.S. mortgage market has experienced dramatic changes in recent years, as Fannie Mae and Freddie Mac (the major government-sponsored enterprises or GSEs) faced substantial reforms to their business practices. An important feature of regulatory reform included changing the pricing of loan guarantees on mortgage-backed securities insured by the GSEs, in particular removing the subsidy paid by small lenders to large lenders in 2012. The second chapter of this dissertation, **Lender Cross-Subsidization and Credit Supply in the Fannie Mae MBS Market** (co-authored with Igor Karagodsky), shows that the removal of this subsidy resulted in a relative increase in mortgage lending by small lenders. However, states with relatively higher concentrations of large lenders experienced relative reductions in credit following the removal of these subsidies. This research underscores an important link between lender market power and credit supply.

Understanding the drivers of the fluctuations in bond returns is a central question in finance. Theoretically, unexpected bond returns should reflect either changes in expectations of future short-term rates or future compensation for risk. The third chapter of this dissertation, **Survey Forecasts and Bond Return Decompositions**, revisits this question using survey forecasts of professional economists to measure expectations of interest rates and returns, rather than with a statistical model. Two main results emerged from this analysis: (1) News about future short-term interest rates explains relatively more of the variation in unexpected excess bond returns for short-maturity bonds relative to long-maturity bonds. (2) The share of news explained by future short-term interest rates increases with horizon for all maturities. This analysis contributes to the recent academic literature that highlights the importance of subjective expectations in understanding asset-price movements.

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1 The Real Effects of Stress Testing

Abstract

I use the introduction of supervisory stress testing of large U.S. banks as a quasi-experiment to examine the compositional effect of macroprudential policy on the real economy. Following the 2009 round of U.S. bank stress tests (SCAP), tested banks altered their lending behavior in the syndicated loan market at the extensive margin. Firms that borrowed from tested banks in the syndicated loan market experienced larger reductions in tested bank shares in loan syndicates after the SCAP. On the whole, non-tested financial institutions offset these reductions in risk, smoothing shocks to firm borrowing and mitigating effects on firm real outcomes. Speculative grade firms faced tighter credit conditions as a consequence of the SCAP. My results imply that a 1 standard deviation increase in speculative grade firm exposure corresponded with an 8 pp lower probability of receiving a positive modification of an existing loan, a 3 pp decrease in fixed investment, and a 7 pp decrease in employment growth after the SCAP. In contrast, investment grade firms reliant on tested banks fully substituted reductions in tested bank risk by borrowing from foreign banks and subsequently expanded intangible investment. Firms accessing the syndicated loan market for the first time and firms with short lending relationships faced steeper reductions in borrowing and real outcomes.

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1.1 Introduction

Supervisory stress testing has emerged as the primary tool of macroprudential policy in the United States tasked with preserving stability of the financial system. While credited with restoring confidence in U.S. banks in the wake of the financial crisis of 2007-2009 (Bernanke et al. (2013)), stress testing has come under criticism from bankers for imposing additional capital requirements at the expense of credit allocation.[1] Recent studies have documented that banks reduced credit supply in response to certain stress tests, particularly to risky borrowers. Thus, an important question is whether firms are ultimately affected by their lenders being stress tested. If firms reliant on borrowing from tested banks substituted this loss of credit by obtaining funds from other lenders, then stress testing would not adversely firm outcomes. In that case, risks would only be redistributed in the financial system. Whether stress testing ultimately impacts firm borrowing and real economic activity remains an unexplored question in the literature.

In this paper, I exploit the introduction of supervisory stress testing of large U.S. banks as a quasi-experiment to identify the impact of macroprudential policy on the economy. Importantly, I examine the role that bank- and firm-level frictions played in the transmission of shocks from tested banks to firms. This paper first exam-

cial Research Ph.D. Symposium on Financial Stability, 2018 Northern Finance Association Ph.D. Session (Charlevoix), the 2018 BC/BU Green Line Macro Meeting, the 2018 International Banking, Economics and Finance Association Summer Meeting (Vancouver), the 2018 Canadian Economics Association Annual Conference (Montréal), the 2017 Southern Finance Association Annual Meetings (Key West), the 2017 Financial Management Association Doctoral Student Consortium (Boston), The Future of Bank Regulation: Bank Colloquium for Young Researchers at the Université de Limoges, and workshop participants at Boston College for helpful questions and comments. All errors are my own.

[1] Regarding the annual supervisory stress test, Bank of America CEO Brian Moynihan stated: "It will make you very safe. The question is whether it restricts lending." (Moynihan (n.d.))

ines whether there were changes in lending behavior in the syndicated loan market[2] by stress tested U.S. banks following the Supervisory Capital Assessment Program (SCAP), the first supervisory U.S. bank stress test. Relative to previous studies, I utilize both non-tested U.S. banks and foreign banks that were active lenders in this market in order to sharpen identification of the effect of stress testing on firms.

In the first part of this paper, I show that tested banks were significantly less likely to exit existing loan syndicates than other banks after the SCAP. However, tested banks were also significantly less likely to enter into new loan syndicates after the test. Given this contrasting lending behavior, I examine whether firm credit outcomes and the structure of loan syndicates changed after the SCAP. Firms with ex-ante greater reliance on tested banks experienced significant reductions in tested bank syndicate shares, but no difference in the terms of credit after the test. Foreign banks primarily offset these reductions in risk by entering into syndicates of highly exposed firms. In the second part of this paper, I quantify the impact of these shocks on firm-level outcomes. Utilizing a larger sample of firms that obtained syndicated loans before the SCAP I find that firms highly exposed to tested banks fully substituted their borrowing from other lenders, smoothing shocks to real business activity. However, changes in the composition of credit differentially affected firms as a function of their access to the bond market. Foreign banks fully substituted the reduction of risks by tested banks in syndicates of investment grade firms. These firms were

[2]Syndicated loans are an important form of corporate financing that fall on the spectrum between single-lender bank loans and debt. In a typical syndicated loan agreement, firms solicit funds from a "lead arranger," who handles the administrative duties, gathers "participant" lenders, and monitors and services the loan in exchange for a fee. A typical syndicate (deal) contains multiple tranches, consisting typically of revolving credit lines and term loans. Sufi (2007) provides a more detailed discussion of the syndicated loan market.

significantly more likely to obtain new loans after the SCAP and to increase intangible investment. In contrast, speculative grade firms were significantly less likely to borrow on similar terms after the SCAP, leading to reductions in fixed investment and employment growth.

Understanding how stress testing can aﬁect the real economy is an important topic of interest to academics and policymakers alike. Theories of stress testing offer ambiguous predictions of the impact on bank lending, as increases in capital and disclosure could potentially affect both aggregate credit or the composition of lending to the real economy either positively or negatively.[3] In fact, a growing empirical literature has studied how banks adjust lending in response to supervisory stress tests in the U.S.[4] In the context of large corporate loans, tested banks reduced their exposure to risks (Acharya, Berger, and Roman (2018), Lambertini and Mukherjee (2016)), although this behavior was partially offset by higher capital required by the tests (Pierret and Steri (2018)). Other papers have examined the impact of prudential policies on real outcomes in the context of European capital adequacy exercises: dynamic provisioning in Spain (Jiménez, Ongena, Peydró, and Saurina (2017)) and increases in capital ratios required by the European Banking Authority (Gropp, Mosk, Ongena, and Wix (2018)). Whether stress tests themselves affect the economy remains a largely unanswered empirical question. Furthermore, providing insights into the relative importance of frictions at the bank and firm level can inform policymak-

[3]Theoretical papers that have modeled the relationship between stress testing and bank lending decisions include Goldstein, Sapra, et al. (2014), citedogra2018, Shapiro and Zeng (2018).

[4]Recent papers have examined outcomes at the bank level (Shahhosseini (2014), Flannery, Hirtle, and Kovner (2017), Bassett and Berrospide (2018)) and state level (P. S. Calem, Correa, and Lee (2017), Cortés, Demyanyk, Li, Loutskina, and Strahan (2018)).

ers and guide theory.

A large theoretical and empirical literature has demonstrated that credit supply shocks can affect real economic activity, depending crucially on bank health and firm reliance on external financing. Thus, in order to discipline the analysis of the ultimate effect of stress testing on firm outcomes, I survey contrasting theoretical implications linking stress testing to bank lending decisions. The capital channel predicts that the response of bank lending depends on changes in the quantity and quality of capital resulting from stress tests. Higher capital can reduce risk-shifting incentives and strengthen market discipline, leading to increases in credit to safe borrowers. On the other hand, higher capital can engender increased risk-taking incentives due to strengthened monitoring, larger capital buffers, and increases in credit risk or reaching for yield to offset reductions in leverage risk. Higher capital could also reduce lending, especially to risky borrowers, due to reductions in uninsured creditors' incentives to monitor management or due to higher charter values. The disclosure channel also offers contrasting predictions as to whether tested banks might change their lending behavior due to the release of information about the methodology, inputs, and results of stress tests. On the one hand, increased disclosure of bank fundamentals could strengthen market discipline or result in banks allocating their lending towards assets more likely to pass the test, resulting in increases in lending to safe borrowers. On the other hand, lack of supervisory credibility, in combination with the identification of the set of systemically important institutions likely to be bailed out in future crises could encourage moral hazard lending. In addition, too much disclosure of information about bank fundamentals can impose endogenous costs on tested banks and lead to reductions in lending, for example by impairing incentives for liquidity creation or forcing banks to reduce lending in anticipation of higher future regulatory costs. Given the contrasting theoretical predictions from the capital channel and the disclosure channel, whether stress testing affects bank lending remains an empirical question.

This paper is structured to trace the transmission of changes in tested bank lending behavior to firm outcomes. In the first part of this paper, I utilize a difference-in-difference model to test whether the same firm experienced changes in lending outcomes from tested banks relative to other banks after the SCAP.[5] I document that tested banks were 8 pp significantly less likely to exit from pre-test loan syndicates relative to other banks after the SCAP, although the share of their exposure to loan syndicates did not change. Much of this continuation of existing lending relationships occurred for firms with bond market access. On the other hand, I find that tested banks were simultaneously 7 pp less likely to enter new loan syndicates than other banks. In particular, tested banks were 21 pp significantly less likely to enter into new lending relationships with speculative grade firms relative to other banks.

These observed changes in lending behavior by tested banks had important consequences for firms. In order to study outcomes at the firm level, I construct a measure of exposure to the stress test as a weighted average of tested bank shares in each firm's last pre-test syndicate. I first focus on the set of firms obtaining new loans in both the pre- and post-test periods (approximately 300 firms) in order to study changes

[5]The pre-test period includes all syndicated loan originations extended between January 2007 and January 2009. The post-test period include all syndicated loan originations extended between February 2009 and December 2010.

in syndicate outcomes at the intensive margin. For these firms, the composition of loan syndicates changed dramatically after the SCAP. Increasing test exposure for a firm one standard deviation above the mean resulted in a 15 pp reduction in tested bank shares in post-test syndicates, but did not lead in changes in credit outcomes for these firms. Foreign banks offset much of this reduction in tested bank shares, enabling firms with high exposure to tested banks to borrow on similar terms after the test. While I do not find evidence of changes in the terms of credit on average, there were important compositional effects across firms as a function of their access to the bond market. The majority of the reduction in tested bank shares for highly exposed investment grade firms was offset by foreign banks, leading to no changes in credit outcomes. In contrast, foreign banks and other U.S. banks more evenly offset the decline of tested banks in speculative grade firm syndicates, leading to decreases in the growth of syndicate size and stricter covenants after the SCAP for these firms. Thus, speculative grade firms faced relatively worse terms of credit in order to attract new lenders into their syndicates after the SCAP.

In the second part of the paper, I examine changes in firm outcomes at the extensive margin among the set of firms that borrowed in the syndicated loan market during the pre-test period (approximately 800 firms). Firms with relatively higher exposure to tested banks did not experience changes in borrowing after the SCAP, and thus did not differentially adjust real outcomes. However, investment grade firms relatively more exposed to tested banks experienced a 10 pp higher probability of borrowing after the SCAP relative to less exposed firms, primarily due to a higher probability of obtaining new loans. As a result, these firms increased intangible investment after the SCAP by 5 pp more than less exposed firms. In contrast, speculative grade firms relatively more exposed to tested banks faced significantly tighter credit conditions after the test. Increasing exposure to gap banks for a speculative grade firm one standard deviation above the mean resulted in an 8 pp lower probability of obtaining a positive modification, a 3 pp decrease in fixed investment, and a 7 pp reduction in employment growth after the SCAP. Thus, the transmission of shocks from tested banks to real business outcomes differed substantially between speculative grade firms and investment grade firms.

Estimation of a causal channel through which stress testing affects bank lending is challenged by a number of issues that complicate identification. First, selection into the SCAP was not random, as banks included in the early rounds of stress tests were larger than other non-tested U.S. banks. Second, the SCAP was implemented in the depths of the financial crisis when both firm demand and bank supply of credit were strained. Third, tested banks might have extended credit to certain types of firms, potentially biasing estimation of a causal effect. To address the first concern, I argue that the stress test announcement was plausibly exogenous to future lending outcomes, as the threshold for inclusion was based on an ex-ante (2008Q4) value of \$100 billion in assets, mitigating concerns that banks near the threshold manipulated their assets in anticipation of the test. Furthermore, the short time horizon between the announcement and implementation of the SCAP likely precluded banks from adjusting their portfolios quickly. In addition, expected loss rates on corporate loans under the severely adverse scenario were lower than other loan categories such as credit cards and mortgages. As such, I argue that much of the expected deterioration of capital forecast under stressed scenarios was likely not due to the set of syndicated loans considered in this paper. In terms of selection into the test, I use the fact that U.S. bank-holding companies with assets just below the threshold are a natural control group for tested banks just above the threshold (the treatment group). In addition, I utilize a dataset of foreign bank-holding companies with an active presence in the U.S. syndicated loan market as a natural control group for the set of large tested U.S. banks. Comparing pre-test balance sheet characteristics between the two groups minimizes concerns that the two groups differed on a number of observable dimensions. To address the second concern, I build a matched bank-firm-time dataset of syndicated loans with the important feature that the same firm borrowed both before and after the SCAP. I then utilize the Khwaja and Mian (2008) procedure of estimating changes in loan outcomes from tested banks relative to non-tested banks holding fixed all time-varying shocks to firm credit demand as a way of isolating the credit supply channel of stress testing. To address the third concern, I compare regression estimates for specifications with firm fixed effects relative to those with just firm controls to show that biases arising from tested banks lending to firms as a function of their risk are likely minimal among the set of firms obtaining new loans in both periods.

There are a number of issues that complicate the estimation of a causal effect of stress test-ing on firm outcomes. First, as the pre-test period contained the financial crisis and the post-test period the recovery from the recession, changes in the distribution of risks in loan syndicates across lenders, as well as borrowing and real firm business decisions could have been driven by fluctuations in the business cycle. Furthermore, observed outcomes could affect changes in credit demand due to macroeconomic conditions or idiosyncratic firm shocks. Second, firms highly reliant on borrowing from tested banks could have differed from less reliant firms, potentially due to tested banks concentrating their lending in the pre-test period to firms as a function of their risk. Thus, unobserved factors could be driving differences in firm-level out-comes between these two sets of firms rather than changes in credit extended by tested banks. Third, highly exposed firms could have experienced reductions in borrowing and adjusted firm outcomes in anticipation of the SCAP. I take a number of steps to aid identification of the causal effect of stress testing on firm outcomes. First, the main estimating equation is estimated in first differences in order to remove all shocks common to firms. In addition, I include a wide array of proxies for firm credit demand identified in the bank lending regressions in order to control for shocks to firm credit demand.[6] I also include characteristics of each firm's primary lead arranger and control for a measure of bank health and changes in TARP investments which were likely to affect the ability of firms to borrow. Second, I demonstrate that firm and deal characteristics did not differ dramatically across different levels of exposure to tested banks. Third, I present evidence to support the assumption that borrowing and firm real outcomes followed parallel trends for firms differentially exposed to tested banks. Therefore, the main identifying assumption throughout the second part of the paper is that stress testing uniquely affected firm-level outcomes conditional on the set of proxies for firm credit demand and firm exposure to other

[6] To illustrate this point, including the set of firm and syndicate controls increases the R-squared test statistic from 1% to 52% in the investment regressions, suggesting that these variables explain a large fraction of the variation in investment.

credit supply shocks.

In extensions of the baseline results, I demonstrate that tested banks that were found to have insufficient capital buffers as a result of the SCAP responded relatively more conservatively than other tested banks. However, firms relatively more exposed to these banks did not experience differential changes in borrowing outcomes. Additionally, I show that firms with long lending relationships and previous borrowers in the syndicated loan market were less affected than other firms.

Section 2 provides background on stress testing and reviews the empirical and theoretical literature. Section 3 discusses construction of the main dataset and summary statistics. Section 4 details changes in lending outcomes at the bank-firm level. Section 5 tests for changes in outcomes and the composition of loan syndicates at the intensive margin. Section 6 examines changes in borrowing at the extensive margin and tests for real effects. Section 7 extends the baseline analysis to explore heterogeneous effects, while Section 8 concludes.

1.2 Background

1.2.1 *Stress Testing*

Stress testing of financial institutions has emerged as one of the major prudential tools developed by regulators and supervisors in recent years.[7] While banks were required to conduct internal stress tests as part of the Basel capital accords, these tests typically entailed assessments of trading book exposures to market risks, rather than

[7] Tarullo (2014) provides a thorough overview of recent topics in macroprudential regulation and policies aimed at improving financial stability.

the sensitivity of capital ratios to changes in lending portfolios Wall (2014), Bookstaber, Cetina, Feldberg, Flood, and Glasserman (2014)).[8] Since the financial crisis of 2007-2009, the basic framework of supervisory stress tests has involved estimating bank fundamentals under baseline and adverse economic scenarios, determining expected capital levels under those scenarios, and disclosing results to the public (Hirtle and Lehnert (2015)). In contrast to more traditional supervision of financial institutions, a defining feature of modern supervisory stress testing has been that it not only assesses bank capital positions from a microprudential perspective, but also accounts for macroprudential risks.[9]

1.2.2 *SCAP*

The Supervisory Capital Assessment Program (SCAP) was the first supervisory stress test implemented in the United States in response to the financial crisis of 2007-2009 (Bernanke (2009); Hirtle, Schuermann, and Stiroh (2009)). At the time, the U.S. banking system was under considerable strain due to worsening macroeconomic conditions, depressed asset prices, and eroding capital positions (Bernanke et al. (2013)). In addition, bank financing was under pressure due to uncertainty about risks on (or off) bank balance sheets, and potential further depletion of bank capital. Among the responses to the crisis, the Federal Reserve implemented a number of

[8] For example, Basel II required banks to assess whether capital requirements under the internal ratings-based approach would change under stressed scenarios (BIS (2004a)). In practice, surveys of financial institutions found that banks were primarily conducting stress tests on their trading books, and were less likely to perform stress tests on loans (BIS (2004b)). Hurdles to implementing stress tests of loan portfolios included difficulty of marking loans to market, costs of data accumulation, and lack of broad risk management practices accounting for trading and loan book risks.

[9] Microprudential regulation involves setting rules to build capital buffers for individual institutions to with-stand idiosyncratic shocks. Macroprudential regulation involves setting rules so that the financial sector as a whole has sufficient capital to withstand system-wide shocks.

unconventional policies, including providing liquidity to key markets, the Treasury Department introduced the Troubled Asset Relief Program (TARP) with the goal of re-capitalizing banks, and the FDIC agreed to guarantee long-term bank debt and expand deposit insurance protection. In spite of these efforts, there was still considerable uncertainty about whether financial institutions would be nationalized or allowed to fail as in the case of Lehman Brothers.

The SCAP was announced in February 2009 with the aim of restoring confidence to financial markets by determining whether large U.S. banks would be able to maintain sufficient capital buffers to withstand an adverse economic shock and to continue lending in a crisis (of Governors of the Federal Reserve System (2009)). The SCAP departed significantly from traditional supervisory exercises (Bernanke et al. (2013)). First, only U.S. bank holding companies with assets exceeding $100 billion as of 2008Q4 were included in the test. Second, the SCAP was a forward-looking exercise that calculated projected losses, revenues, and capital ratios under multiple economic scenarios. In contrast, examinations and capital ratios were generally set based on backward-looking information. Third, the test was simultaneous, which allowed for supervisors to assess exposures across institutions and risks of contagion. Finally, results of the SCAP were disclosed publicly on May 7, 2009, in contrast to the traditional practice of keeping supervisory results confidential. The release included institution-level projections of pre-provision net revenue, allowance for loan and lease losses, and losses on different loan categories, securities holdings, and trading portfolios.[10] Importantly, supervisors also calculated pro forma 2010 year-end levels of capital and risk-weighted assets based on their projections of bank fundamentals under the adverse scenario. Resulting pro forma capital ratios were assessed relative to supervisory benchmarks of 6% of Tier 1 capital and 4% of Tier 1 Common capital as shares of risk-weighted assets. The SCAP release included estimates of each institution's capital gap, its supervisory capital level minus its pro forma capital level calculated under the adverse scenario. Ten of the nineteen institutions tested were deemed to be insufficiently capitalized to withstand the adverse scenario, resulting in an aggregate capital gap of $75 billion after accounting for 2009 Q1 performance and interim capital actions. The bulk of the capital needed to meet the SCAP requirement was in the form of Tier 1 Common capital, which nine institutions were subsequently able to successfully raise privately.[11]

One of the key implications of the SCAP was the massive recapitalization that occurred in the months following the test. As the majority of banks found themselves in precarious capital positions as a result of the financial crisis, there was a need to restore capital buffers in order to remain viable entities going forward. Importantly, the response to the SCAP differed between banks with and without a capital gap. For the set of banks without a capital gap, the main regulatory agencies provided them with the option of exiting TARP in June 2009 conditional on successfully raising capital in a common stock offering (for the Troubled Asset Relief Program (2011)).[12] The main

[10] The bank-holding companies with significant trading exposures were also subject to a market shock.

[11] Only GMAC required assistance from the Treasury through the Capital Assistance Program (CAP).

[12] As a condition of exiting TARP, these banks also paid back Treasury's holdings of preferred stock. Morgan Stanley, a positive capital gap bank, was also given the opportunity to exit TARP

reasons for exiting TARP were the stigma associated with government ownership and the inability to pay dividends and compensate management. The possibility of TARP exit was not revealed until after the SCAP. Banks with an insufficient capital buffer were required to submit plans for how they planned to address their capital gap by June 2009, and were given six months to do so. Capital gaps were generally filled with some combination of new stock issuance, conversion of preferred securities for common equity, and asset sales. Gap banks were also given the option to exit TARP gradually between late 2009 and 2011 by similarly demonstrating ability to raise private capital in equity issuances. Gap banks thus increased their capital both in response to the SCAP and as a condition of exiting TARP. Thus, one major difference between banks included in the SCAP and those not included was the increase of capital.

The main focus of this paper is the response to the SCAP, as I rely on the institutional details of that stress test to identify the effect of stress testing on firm outcomes. Importantly, the outcomes and methodology of the CCAR and DFAST differ substantially from the SCAP. Additional details of these tests are available in the appendix.

1.2.3 *Related Literature*

A growing literature has begun to assess the extent to which supervisory stress testing affects financial institutions. Early empirical papers studied the impact of stress tests on bank financial variables, such as the market responses to stress test

early.

announcements (Petrella and Resti (2013); Morgan, Peristiani, and Savino (2014); Candelon and Sy (2015); Fernandes and Pinheiro (2017); Gerhardt and Vander Vennet (2017); Flannery et al. (2017)). In addition, Kovner and Van Tassel (2018) find that the cost of capital increased for banks included in the SCAP, but fell for this set of banks after the DFAST. Theoretical models of stress testing have generally focused on the question of disclosure of test results (Goldstein et al. (2014)), and whether this disclosure could change the incentive of banks to allocate lending to projects of varying risks (Shapiro and Zeng (2018), Leitner and Williams (2017), Corona, Nan, and Zhang (2017)).

This paper relates more broadly to a third branch of the literature that has begun to quantify the effect of stress testing on bank lending. These papers utilize bank-level regressions to test for differential responses of tested banks following stress tests. Shahhosseini (2014) focuses on the largest U.S. bank-holding companies between 2005 and 2015 and finds that bank man-agers increased capital levels by either restructuring or removing non-performing loans from their balance sheets in order to pass the stress tests in the U.S. In addition, she shows that loan growth fell for tested banks relative to non-tested banks only following the SCAP and not sub-sequent tests. Acharya et al. (2018) use a similar dataset and find negative responses for lending driven by banks with capital gaps, especially for risky loan categories. Fernandes and Pinheiro (2017) build a dataset of the largest U.S. bank-holding companies as of 2014Q4 and find only an immediate negative response for lending for banks not passing the test. In contrast, Bassett and Berrospide (2018)look at large U.S. bank-holding companies between 2013 and 2016 find no evidence that banks required to raise additional capital reduced loan growth. However, they do find that some tested banks that increased capital ratios were able to increase loan growth for certain types of lending. Flannery et al. (2017) find little evidence in support of loan growth being affected by differences between the Fed's and tested bank's estimates of losses by loan category for later rounds of stress tests. In the context of Europe, Mésonnier and Monks (2015) focus on the 2011 and 2012 European Banking Authority (EBA)'s Capital Exercises and find that banks with relatively larger capital shortfalls subsequently reduced loan growth. Eber and Minoiu (2016) utilize the European Comprehensive Assessment announced in 2012, which was a stress test of Eurozone banks, and find that banks achieved higher capital ratios generally by not reducing lending. Gropp et al. (2018) use syndicated loan data to show that banks in the 2011 European Banking Authority's (EBA) capital exercise increased capital ratios by reducing lending rather than raising equity. Hirtle, Kovner, and Plosser (2018) find a causal link between supervision and bank performance.

More closely related to this paper, a number of recent papers have used disaggregated loan-level data in order to attempt to isolate the effect of stress testing from demand factors. Using a matched sample of syndicated loans to U.S. non- financial firms from U.S. bank-holding companies, Acharya et al. (2018) find that spreads increased and loan amounts and maturities decreased in syndicates with lead arrangers that were tested in the SCAP and sub-sequent CCARs. These results were driven by reductions in credit supply to risky borrowers. They also find some evidence that banks not passing the SCAP increased pricing and decreased loan size by less than did passing banks. Lambertini and Mukherjee (2016) also find that spreads increased for banks included in the SCAP and CCAR following the release of stress test results. Using a similar dataset of syndicated loans matched to U.S. bank-holding companies, Pierret and Steri (2018) show that while higher capital requirements due to stress tests created incentives to increase risk-taking, monitoring effectively dampened the magnitude of this effect. Other papers have used matched bank-firm samples of syndicated loans in Europe to study the effect of prudential policies on credit allocation. Gropp et al. (2018) find that lending decreased at the intensive margin for banks subject to the EBA's 2011 capital exercise relative to those not included. As a result, firms more exposed to EBA banks reduced total assets and fixed assets, and experienced lower sales following the exercise. Focusing on the Eurozone stress tests, Eber and Minoiu (2016) find some evidence that weakly capitalized tested banks responded by reducing credit supply. Two recent papers examine the effect of stress testing on other types of lending. P. S. Calem et al. (2017) study how U.S. bank-holding companies responded to stress tests by changing the supply of mortgage credit. They find that the share of jumbo mort-gage origination fell among tested banks, especially weakly capitalized banks. They also show that the share of speculative-grade term-loan originations declined following implementation of 2013 supervisory guidelines on leveraged lending. Cortés et al. (2018) show that small business lending declined in counties relatively more exposed to tested banks forecasted to have larger deteriorations in capital under stressed scenarios. Much of this decline in lending occurred in markets where tested banks had local branches and among relatively riskier loans. Small, non-tested banks entered markets so that state-level loan growth remained unchanged as a function of test exposure. Only one paper briefly explores the effects of stress testing on local economic conditions. Berger and Roman (2017) find some evidence that stress-tested banks affect local economic conditions by reducing business and personal bankruptcies, but find the opposite result for net job creation and net hiring at establishments.

Relative to this empirical literature, I make a number of contributions on the impact of stress testing on bank and firm outcomes. In terms of bank lending outcomes, I examine whether tested banks altered their lending in the syndicated loan market after stress tests both at the intensive margin, by changing the size of existing loans, and at the extensive margin, either by exiting existing bank-firm relationships or by entering new bank-firm relationships. My paper complements Acharya et al. (2018), who document an important margin of adjustment in lending, namely through the reduction of exposure to risky borrowers in syndicated loans. As they focus their analysis only on lead arrangers, their result applies most strongly to firms whose largest lead arranger was a U.S. bank-holding company that lent to them both before and after stress tests. As such, their paper offers strong evidence to suggest that loan outcomes changed for firms reliant on lead arrangers that were included in stress tests.[13] Relative to their paper, I study lending outcomes among bank-firm pairs by incorporating all lenders in a syndicate (lead arrangers, co-agents, and participants) into my analysis. In addition, I study the behavior of the most active foreign bank-holding companies in the U.S. non- financial syndicated loan market, which enables me to compare changes in lending by the most active lenders in syndicates, regardless of U.S. affiliation. Including active foreign lenders and participants is particularly

[13] Lambertini and Mukherjee (2016) find similar results.

important in this context, as I am interested in studying how syndicate structure changed in response to stress tests, and whether these changes affected overall firm borrowing. Pierret and Steri (2018) also study how syndicated loan outcomes changed among bank-firm pairs, however they focus on separately identifying capital and monitoring channels of stress testing using the introduction of the DFAST. In contrast, my main focus in terms of bank lending is studying how loan outcomes changed at the extensive margin for tested banks relative to non-tested banks among bank-firm relationships. While I provide some evidence in the robustness section regarding changes in lending for later stress tests, my main analysis focuses on the response to the SCAP.

To my knowledge, this paper is the first to document how syndicate structure changed and whether firms adjusted real outcomes in response to changes in lending by stress-tested banks. My first main contribution is to investigate how the shares of different lenders changed following stress tests as a function of existing exposure to tested banks. I show that there is important heterogeneity in this response as a function of firm financial constraints. My second main contribution is to examine the extensive margin of firm borrowing outcomes. Rather than focus on firms obtaining syndicated loans in both pre- and post-test periods, I study how loan outcomes changed as a function of exposure to tested banks. By focusing on this wider set of firms, I am able to test whether firms substituted changes in borrowing from the syndicated loan market by issuing debt or equity. Lastly, this is the first paper to examine how firm-level real outcomes were affected by exposure to stress tested banks.

More broadly, this paper relates to the empirical literature studying the effects of credit supply shocks on real economic activity. Frictions at the bank and firm level produce differential responses of credit supply shocks to borrowers (Bernanke (1983), Kashyap, Stein, and Wilcox (1993), Gertler and Gilchrist (1994), Kashyap, Lamont, and Stein (1994), Peek and Rosengren (1997), Peek and Rosengren (2000), Kashyap and Stein (2000), Campello (2002), Ashcraft (2005), Ashcraft (2006), Ashcraft and Campello (2007), Gan (2007), Paravisini (2008), Chava and Purnanandam (2011), Adrian, Colla, and Song Shin (2013), Becker and Ivashina (2014)). This paper utilizes the technique developed by Khwaja and Mian (2008) to isolate credit supply shocks and examine effects on firm outcomes (Schnabl (2012), Iyer, Peydró, da Rocha-Lopes, and Schoar (2013), Jiménez, Ongena, Peydró, and Saurina (2014), Cingano, Manaresi, and Sette (2016)). In the context of syndi-cated lending, this paper relates most closely to Chodorow-Reich (2013), who shows that bank health causally affected non- financial firm employment outcomes following the financial crisis of 2007-2009. This paper also relates to the literature studying the effect of bank capital regulation on lending (Thakor (1996); Fraisse, Lé, and Thesmar (2015), Bahaj and Malherbe (2018), Chu, Zhang, and Zhao (2018)).

1.2.4 *Theoretical Implications*

The theoretical literature predicts an ambiguous between stress testing and lending, operating through two primary channels. The capital channel of stress testing predicts that lending behavior by tested banks can change in response to changes in the quantity and quality of capital required by stress tests.[14] Whether lending

[14]Insufficiently capitalized banks in the SCAP were required to issue equity in the months following the test, while the set of tested banks receiving TARP funds did as well as a precondition of exiting

increases or decreases depends crucially on the composition of credit allocation between safe and risky borrowers. On the one hand, higher levels of capital can result in increases in lending to relatively safer borrowers due to reduced risk-shifting incentives (Furlong and Keeley (1989), Coval and Thakor (2005)) or higher capital requirements (Bahaj and Malherbe (2018)). On the other hand, more capital could result in increases in lending to relatively riskier borrowers due to strengthened monitoring incentives (Holmstrom and Tirole (1997)), larger capital buffers (Bhattacharya and Thakor (1993), Berger and Udell (1994)), increases in credit risk (P. Calem and Rob (1999)) or reaching for yield incentives (Bahaj, Bridges, Malherbe, and O'Neill (2016)). Higher capital could also result in decreases in lending, especially to risky borrowers, if reductions in uninsured creditors' risk reduce the incentive to monitor bank management, weakening market discipline, and increasing borrowing costs for banks (Calomiris and Kahn (1991), Diamond and Rajan (2001)). Another consequence of higher capital is higher charter values, which lower the likelihood of future failure, leading to reductions in risky lending (Keeley (1990)).

The disclosure channel predicts that the release of information about the methodology, inputs, and results of stress tests could impact bank lending behavior. As with the capital channel, the degree to which overall lending might change due to increased disclosure depends on potential changes in the composition of credit. Increases in lending to relatively safer borrower could result from the ability of uninsured creditors to more effectively impose market discipline on bank management due to increased

government ownership. Thus, SCAP banks increased both the quantity and the quality of equity capital in the years following the first stress test. With the subsequent rounds of CCAR and DFAST, banks with insufficient internal stress test procedures and capital plans were required to reduce distributions.

disclosure of bank fundamentals (Goldstein et al. (2014), Balakrishnan and Ertan (2017)), through incentives to allocate portfolios towards loans more likely to pass the test (Dogra and Rhee (2018)),[15] or due to credibility of the supervisors and fiscal authorities conducting the stress test (Williams (2017)), Faria-e Castro, Martinez, and Philippon (2016)). In contrast, weak supervisory discipline can also encourage banks to allocate their lending towards risky borrowers by signalling favorable treatment on future tests (Shapiro and Zeng (2018)) or future bail-outs (Corona et al. (2017)). Encouraging banks to disclose bad news could lead to higher risk-taking, but allow regulatory agencies more control over future investment decisions (Harris and Raviv (2014)). Increased disclosure could also serve to identify the set of systemically important institutions, encouraging bank management to increase lending to risky borrowers due to moral hazard incentives (Acharya et al. (2018)). Too much disclosure could lead to reductions in lending, especially to risky borrowers, due to endogenous costs associated with the test Goldstein et al. (2014)). Disclosure of bank fundamentals can also reduce incentives for banks to create liquidity (Dang, Gorton, Holmström, and Ordonez (2017)). Additionally, being identified as Too-Big-to-Fail (TBTF) might force banks to reduce lending due to expectations of increased regulatory costs for these firms, such as the need to hold higher levels of capital in the future, and subsequently reducing risky lending on the margin (Acharya et al. (2018)).

[15]Leitner and Williams (2017) show that in spite of the latter incentives, revealing stress test models could still increase socially desirable lending. However, Orlov, Zryumov, and Skrzypacz (2018) argue that these increases in lending could come at the cost of an increase in systemic risk or re sales if adverse scenarios are too revealing.

1.3 Data

1.3.1 *Dataset Construction*

The main dataset in this paper combines data on syndicated loans with lender and firm characteristics. Syndicated loan data come from Loan Pricing Corporation's (LPC) Dealscan database, which is collected from SEC lings, originators, and news sources. Each deal (syndicate) is composed of at least one tranche (facility). I start by restricting the sample to include all completed term loans[16] and revolving credit facilities[17] extended to U.S. firms originated be-tween 2004 and 2016 in U.S. dollars, a selection which yields 69,045 unique facilities and 44,742 syndicates. I then exclude real estate and financial firms with SIC codes between 6011 and 6799 (10,946 facilities and 8,724 syndicates)[18] and facilities whose primary purpose is not for real investment (20,231 facilities and 9,857 syndicates).[19] After a nal screen for non-missing lender data, the main Dealscan dataset contains 37,845 facilities, 26,137 syndicates, 2,394 lenders, 11,537 firms, and 79,570 lender- firm pairs. Dealscan provides information on certain character-istics for each firm (name, SIC code, state, capital market status) and facility (names of lenders, loan size, pricing, maturity, purpose, start and end dates, collateral, covenants, amendments, and allocations). I then merge this dataset with Compustat following Chava and Roberts (2008) in order to obtain detailed

[16]Dealscan variable "loantype" equal to the following values: Delay Draw Term Loan, Term Loan, or Term Loan A through K.

[17]Dealscan variable "loantype" equal to the following values: 364-Day Facility, Revolver/Line < 1 Yr., Revolver/Line >= 1 Yr., Revolver/Term Loan, Demand Loan, Limited Line.

[18]Financial firms are excluded due to the fact that the crisis of 2007-2009 originated at those firms and changes in their lending might be particularly driven by shocks to their demand for credit.

[19]Dealscan variable "primarypurpose" equal to the following values: Corp. purposes, Work. cap., Capital expend., Equip. Purch., Proj. finance.

income statement, balance sheet, and cash ow data. This reduces the set of firms considerably, however the resulting coverage of firm-level variables is richer. Finally, I merge the set of lenders with Bankscope following Schwert (2018) and keep the most active lenders for which data are available, resulting in a total of 97 bank-holding companies based either in the U.S. or abroad.

The main unit of analysis in the first part of this paper is a firm-lender-facility triple. For each facility, Dealscan reports loan allocation shares when available. I fill in missing allocation shares using a censored regression of available shares on facility characteristics.[20] For the first part of my analysis, I focus exclusively on firms that borrowed in both pre- and post-test periods.[21]. This sample selection choice enables me to control for time-varying shocks to firm credit demand by observing how credit supply changed for the same firm from tested banks relative to non-tested banks. However, due both to limited sample size and the nature of the empirical exercise, I am unable to draw conclusions about the effects of stress testing on firm outcomes. As a result, the second part of my analysis focuses on a broader set of firms, namely firms that received a syndicated loan at some point in the pre-test period. This sample allows for a richer analysis of changes in firm-level outcomes as a function of ex-ante exposure to tested banks.

[20]This exercise follows De Haas and Van Horen (2013) and Le (2013). The results are robust to following the imputation method of Chodorow-Reich (2013) as well. Results from the censored regression are shown in Appendix Table A1.

[21]The pre-test period covers January 2007-December 2008 and the post-test period covers February 2009-December 2010

1.3.2 *Summary Statistics*

U.S.-based bank-holding companies with at least $100 billion in assets as of 2008Q4 were chosen for inclusion in the SCAP. In order to compare the behavior of tested banks with other banks, I collect data on the most active lenders in the U.S. non- financial syndicated loan market with sufficient available data in Bankscope.[22] Table 1 lists the set of tested banks included in the SCAP and the lower panel includes the set of other regional U.S. banks. Most of the largest global financial institutions are represented in this sample (Table 2). The sample of banks exhibits large variation across regions and size.

Table 3 presents summary statistics for outcome and control variables for the matched bank-firm analysis in this paper. For the set of firms borrowing in both periods, exposure to loan syndicates among bank-firm pairs decreased by 2 pp on average. Approximately 41% of bank-firm pairs that existed in each firm's last pre-test syndicate did not appear in the post-test period. Similarly, 45% of bank-firm pairs that existed in the post-test syndicate did not appear in the pre-test period. Tested banks were active in approximately 42% of all bank-firm pairs existing in the exit and entry regressions, while gap banks correspondingly represented less than one-third of pairs.

Table 4 gives summary statistics for outcome and control variables for the firm-level analysis in this paper. The upper panel of syndicate-level outcomes shows that the average firm borrowing in both the pre- and post-test period did not experience large changes in shares of different lenders. The second panel contains outcomes for

[22] I keep lenders with at least 25 facilities extended between 2007 and 2010.

the full sample of firms obtaining a syndicated loan between 2007 and 2008 studied in this paper. Almost one-half of these firms borrowed between February 2009 and December 2010, primarily by obtaining a new loan (36%) rather than a positive modification of an existing loan (14%). The average firm increased fixed investment by 18%, intangible investment by 20%, and employment growth by 4% between 2008 and 2011. The third panel contains regressors for the firm-level analysis. The average firm was approximately 61% "exposed" to tested banks, measured by the weighted-average expo-sure of these banks in firms' pre-test syndicates. Gap banks represented 45% of shares in the average firm's pre-test syndicate. Approximately half of the firms in the sample had a credit rating in December 2008 and one-fourth were considered investment grade (BBB- or higher). Revolving credit lines composed the largest share of syndicates (82%). Half of syndicates were collateralized, were used for corporate purposes, and contained at least one covenant. Over half of syndicates contained a previous lead arranger and previous lender, while 40% of syndicates contained lending relationships that were over 5 years old.

1.4 Lending Outcomes

The focus of this paper is the transmission of supervisory stress testing to the real economy via changes in bank behavior. This section uses a matched bank-firm dataset to test whether lending outcomes to the same firm changed for tested banks relative to other banks after the SCAP.

1.4.1 *Empirical Strategy*

A number of factors complicate identification of a causal effect of stress testing on lending. First, banks tested in the SCAP were larger than other U.S. banks that were not included. Second, the SCAP occurred in the depths of the financial crisis when both shocks to firm credit demand and bank credit supply were strained. Third, tested banks might have concentrated their lending to particular types of firms as a function of their risk, potentially biasing coefficient estimates.

To partially mitigate these issues, I utilize a matched bank-firm-time dataset with the important feature that the same firm borrowed in both pre- and post-test periods from tested banks and other lenders. The inclusion of firm fixed effects in a first-differenced model removes all time-varying shocks to credit demand, allowing for identification of changes in loan growth to the same firm from tested banks relative to other lenders (Khwaja and Mian (2008)), conditional on controlling for other shocks to credit supply. Furthermore, this framework allows for a test of the bias due to tested banks potentially lending to firms as a function of their risk. Calculating the difference in coefficient estimates obtained with a firm fixed effects model relative to a model with firm controls proxies for this bias. One limitation of this specification is that it is not possible to identify the total effect of stress testing on lending. Nevertheless, my analysis still provides insights into whether tested banks changed their lending behavior among bank-firm pairs. Subsequent sections explore whether changes in lending affected firms' overall ability to borrow.

Given that selection into the SCAP was not randomly assigned, a major concern of the analysis in this paper is that the set of tested banks might have differed on observable and un-observable dimensions in the years leading up to the SCAP. However, there are features of the criteria for inclusion into the test which help to mitigate concerns of selection bias. The SCAP was announced in February 2009 to include U.S. bank holding companies with end-2008 assets in excess of $100 billion. Therefore, it would not have been possible for banks to manipulate their balance sheets and reduce their assets in order to fall under the ex-ante threshold. Because of this exogenous inclusion criterion, financial institutions with assets just below the threshold serve as a natural control group for those institutions just above the threshold.[23] However, large banks are quite different from those near the threshold. For this reason, I focus on the market for syndicated lending to U.S. non- financial firms in which foreign lenders have a large presence. The natural control group for large U.S. bank-holding companies includes large foreign bank-holding companies that were active lenders of syndicated loans.[24] Results in the upper panel table 5 suggest that observable bank characteristics did not differ dramatically between tested banks and other banks in the pre-test period (2007-2008). The major differences included significantly lower non-performing loans as a share of gross loans and higher equity capital ratios for tested banks relative to other banks. To partially control for these and other differences, I include a full set of bank characteristics in subsequent lending regressions.

[23]Banks with assets between $50 and $100 billion and foreign banking organizations with a substantial U.S. presence were included in later rounds of the CCAR, but were not subject to supervisory stress testing under the SCAP.

[24]Large European banks were subject to supervisory stress tests in both 2009 and 2010. The 2009 stress test was generally viewed as being less credible than the SCAP, in part due to limited disclosure of test details or bank-level results (Ong and Pazarbasioglu (2013)). The 2010 stress test provided more information about bank-and country-level exposures. However, its release falls in the latter part of my sample (July 23, 2010).

In addition, I include measures of "bank health"[25] and the percentage point change in equity capital ratios due to investments by the U.S. government under Capital Purchase Program (TARP). These two measures capture differences in asset quality and capital ratios as a result of the financial crisis. Results in the lower panel of table 5 demonstrate that banks in the SCAP shifted the composition of their assets away from loans and toward securities and increased their capital ratios primarily through increases in common equity in the years after the SCAP.

A third feature of the SCAP aids identification of a causal effect. In early 2009 there was considerable uncertainty among market participants regarding nationalization of banks and whether this would be effective at aiding the economic recovery from the recession (Blue Chip March 1, 2009). Morgan et al. (2014) find significant equity price movements on important announcement dates for the SCAP, suggesting that the test announcement was plausibly exogenous to other factors that might have affected lending outcomes. In particular, they find significant abnormal returns for gap banks after Ben Bernanke clarified that banks would not be nationalized and after the results were announced. This short time period over which the stress test was implemented and conducted suggests that the set of tested banks were likely not able to adjust their portfolios ex-ante in a manner consistent with passing the test. In addition, expected loss rates on corporate loans under the severely adverse scenario were lower than other loan categories such as credit cards and mortgages. As such, I argue that much of the expected deterioration of capital forecast under stressed

[25] Bank health is calculated as the sum of weighted loan shares from bank b to all firms other than f in the post-test period relative to the pre-test period (Chodorow-Reich (2013)).

scenarios was likely not due to the set of syndicated loans considered in this paper.

The analysis in this section con firms and extends the prior literature addressing the effect of stress testing on lending. The baseline empirical framework models loan outcomes as a function of shocks to firm credit demand and lender credit supply (Khwaja and Mian (2008)). Applied to the syndicated loan market (Chodorow-Reich (2013)), I utilize a difference-in-difference model that tests whether the same firm borrowing in both pre- and post-test periods experienced changes in loan outcomes from tested banks relative to non-tested banks:

$$\text{Outcome}_{b,f} = \alpha_f + \beta_1 \text{Test}_b + \delta \text{Controls}_b + \gamma \text{Controls}_{b,f} + \epsilon_{b,f} \tag{1}$$

In this setting, I consider three loan outcomes of interest. In order to test for changes at the intensive margin of credit, I calculate the difference of loan size from lender b to firm f from the last pre-test syndicate (2007-2008) relative to the first new post-test syndicate (2009-2010) scaled by the total size of firm f's last pre-test syndicate. One potential concern of the estimates from regressions at the intensive margin is that they could suffer from biases due to measurement error if loan shares are systematically imputed incorrectly. To address this issue, I also test whether banks included in the SCAP changed their behavior at the "extensive margin." The first measure asks whether lenders removed credit entirely among existing loan syndicates.[26] The dependent variable in those regressions is a dummy variable equal to 1 if a bank-firm pair that existed in a pre-test syndicate did not exist in a post-test syndicate and 0

[26] Darmouni and Rodnyansky (2017) use a similar measure of loan renewal in the context of syndicated loan pairs in response to the Federal Reserve's large-scale asset purchases.

otherwise. Alternatively, tested banks might have reacted to stress tests by changing their propensity to enter into new lending relationships. In order to capture the entry dimension, I condition on all bank-firm pairs existing in the post-test syndicate with at least one tested and one non-tested bank in the syndicate. The dependent variable in those regressions is a dummy variable equal to 1 if a bank-firm pair newly existed in the post-test syndicate that did not exist in the pre-test syndicate and 0 otherwise.

This empirical specification contains an array of controls for factors that might affect loan outcomes. First, the inclusion of firm fixed effects removes all observable and unobservable firm-level heterogeneity that might drive firm credit demand (α_f). In this setting, only factors varying at the bank level or bank- firm level are identified. In this difference-in-difference framework, the main empirical challenge is to isolate treatment (being tested) from other shocks to bank fundamentals that might have affected credit supply. Inclusion of the log of bank assets controls for differences in the ability of relatively larger banks to extend loans at lower cost. Given observed differences in capital ratios and asset quality (Table 5), I include ratios of equity capital to assets and non-performing loans to gross loans. In addition, I also control for each bank's asset liquidity (securities/assets), profitability (return on assets), the liquidity of bank liabilities (deposits and short-term funds to assets), bank health, and the investment of TARP capital scaled by bank assets. This specification also includes interactions of all measures of bank risk with dummies for firm bond market access ($\text{Controls}_{b,f}$), in order to address the endogenous matching between banks and firms (Iosifidi and Kokas (2015)).

A key question of interest in this paper is whether tested banks changed their lending to risky firms, given that shocks to bank balance sheets primarily affect financially constrained firms (Kashyap et al. (1993), Gertler and Gilchrist (1994)). The main measure of financial constraints that I study in this paper is whether a firm received a credit rating just before the SCAP, and if so, whether that credit rating was speculative grade (below BBB-) or investment grade (BBB- and above). This measure is motivated by a literature demonstrating the differential effect of credit supply as a function of bond market access (Chodorow-Reich (2013), Becker and Ivashina (2014)).[27] This distinction is also important in that loans to firms with relatively higher credit ratings carried lower risk weights. Furthermore, institutional investors play a large role in providing credit to speculative grade firm syndicates (Nini (2017)). In order to examine whether test outcomes and firm risk differentially contributed to changes in bank lending behavior, I include interactions of all bank risk measures with dummies for rated firms and rated investment grade firms in additional specifications.

1.4.2 *Results*

Results from estimation of the baseline lending regressions are given in Table 6. First, I find that the same firm did not experience significant changes in loan exposure from tested banks relative to other banks after the SCAP (column 1). However, tested banks slightly increased their exposure in investment grade firm syndicates by 2 pp more than other banks (column 4). In terms of the extensive margin, I

[27] Chodorow-Reich (2013) also finds a significant transmission of shocks to bank health through small firms, however the sample that he utilizes encompasses a broader range of firms than the large corporate borrowers in my sample.

find that tested banks were 8 pp less likely to exit syndicates to the same firm than other banks (column 2), especially among firms with a credit rating (column 5). These results suggest that tested banks were significantly more likely to maintain existing lending relationships. On the other hand, I also find that tested banks were 7 pp less likely than non-tested banks to enter syndicates to firms borrowing in both periods (column 3). Much of this result was due to a significantly lower likelihood of entry into syndicates of speculative grade firms after the SCAP (column 6). Taken together, these results demonstrate that tested banks responded to the SCAP by altering credit at the extensive margin of lending, rather than the intensive margin. Tested banks maintained lending in existing borrower relationships, but behaved more conservatively by not entering new loan facilities. I explore whether these changes in lending affected firm borrowing in the following section.

Results from these regressions help to allay concerns that SCAP banks might have concentrated their lending to firms receiving favorable shocks to credit demand. While I have presented evidence that the criteria for being included in the SCAP were unrelated to firm characteristics, tested banks might have differed from other banks on unobservable dimensions. I test for these differences by comparing point estimates between the specification in Table 6 estimated with firm fixed effects and the specification in Appendix Table A6 estimated with firm controls. Comparing estimates between the models validates this assumption. Under additive separability of shocks to firm credit demand and the SCAP treatment indicator, the difference between these coefficients can capture the bias resulting from non-random matching between banks and firms. For all specifications I find that this bias is small.

Results in Table 7 provide some evidence that gap banks (those needing to fill capital gaps) responded more conservatively to the SCAP than non-gap banks. In particular, I find that the lower likelihood of exit among tested banks was driven mostly by non-gap banks. However, both sets of tested banks were significantly less likely to enter syndicates of speculative grade firms, although the magnitude of the total effect for gap banks was larger (Table 8). These results suggest that gap banks responded relatively more conservatively to being tested than non-gap banks in terms of maintaining and establishing lending relationships. While I do not necessarily conclude that stress testing affected lending behavior through precise channels, these results suggest that weaker tested banks reduced risk to risky firms after the SCAP.

1.5 Syndicate Outcomes

Results in the previous section demonstrated that tested banks responded to the SCAP by not exiting loan syndicates, particularly among rated firms, but also by not entering new syndicates of speculative grade firms. Whether these changes at the bank- firm level affected credit outcomes at the syndicate level remains an open question. In this section, I focus on the set of approximately 300 firms that obtained new loans in both the pre- and post-test periods. Among this sample of firms I study how exposure to tested banks affected credit supply at the intensive margin. Given the contrasting lending behavior among bank- firm pairs, I also explore whether syndicate structure changed and if firms might have substituted their borrowing from different lenders, leading to potential changes in credit outcomes.

Loan syndicates suffer from asymmetric information problems between borrowers and lenders, which gives rise to delegated monitors to overcome these frictions (Diamond (1984)). Nevertheless a moral hazard problem exists between "informed" and "uninformed" lenders (Holmstrom and Tirole (1997)). As a consequence, lead arrangers retain larger shares of syndicated loans and form more concentrated syndicates of relatively more opaque borrowers in order to mitigate these informational frictions (Lee and Mullineaux (2004), Jones, Lang, and Nigro (2005)), Sufi (2007), Ivashina (2009)). Risky firms are likely to be most affected by changes in lending behavior from tested banks that served as lead arrangers, since the loss of lending relationships could adversely affect the ability of firms to borrow on similar terms (Bharath, Dahiya, Saunders, and Srinivasan (2009)). Tested banks changing their propensity to lend as participants in syndicated loans could also affect firm borrowing. Commercial banks are relatively more active than non-banks in extending credit lines, especially to risky borrowers and as participant lenders, owing to the fact that deposit in ows serve as a hedge for liquidity risk (Gatev and Strahan (2009)). Thus, the loss of tested banks as participant lenders, due to more active liquidity risk management, could also affect the terms on which firms subsequently borrow.[28] This section provides evidence that the structure of loan syndicates and credit outcomes changed after the SCAP.

[28] Le (2013) finds that following the collapse of Lehman Brothers, lenders exposed to co-syndication with Lehman reduced their roles, while credit supply remained unchanged, suggesting that changes in borrowing outcomes is not necessarily obvious ex-ante.

1.5.1 *Firm Exposure Measures*

In order to study outcomes at the firm level, I first construct a measure of each firm's ex-ante exposure to tested banks, which is an average weighted by loan shares held by each bank b in firm f's last pre-test syndicate:[29]

$$\overline{Tested}_f = \sum_{b=1}^{B} \left(\frac{\sum_{s=1}^{2} L_{b,f,s,pre}}{L_{f,pre}} \times I(Tested_b = 1) \right) \tag{2}$$

where $I(Tested_b = 1)$ is an indicator variable equal to 1 if bank b was included in the SCAP and 0 if not. An advantage of this measure is that it assigns greater weights to exposure to tested banks that served as lead arrangers rather than participants. In addition, I also construct similar exposure measures based on the share of gap banks in each firm's last pre-test syndicate, as well as for the weighted average of exposure to tested banks as a function of the size of their capital gaps (capital gap/risk-weighted assets). Summary statistics of these exposure measures are given in Table 4.

1.5.2 *Empirical Strategy*

The main question of interest in this section is whether credit outcomes and syndicate structure changed for firms with relatively higher ex-ante exposure to tested banks. Identification of the causal effect of stress testing on changes in the terms of credit and syndicate structure is complicated by a number of factors. First, macroeconomic shocks could have strained the demand for firm borrowing, especially as the sample period in this paper contains the financial crisis of 2007-2009. Second, firms

[29] I sum over each bank's amount lent in each facility s.

ex-ante more reliant on tested banks could have differed from less reliant firms on both observable and unobservable dimensions. Third, tested banks could have concentrated their lending to particular types of firms, especially risky firms, potentially resulting in a downward bias of the main estimates. In addition, firms relatively more exposed to tested banks could have experienced reductions in lending in anticipation of being tested, violating the parallel trends assumption.

To partially address these concerns, for this analysis I restrict the sample in this section to the set of firms obtaining new loans in both pre- and post-test periods. Taking first differences removes macroeconomic shocks common to all firms. In addition, including the set of firm controls identified in bank lending regressions controls for observable shocks to firm credit demand. Furthermore, firm and deal characteristics are well balanced across different levels of exposure to tested banks (Appendix Tables A2 and A3), reducing concerns that tested banks might have systematically extended credit to firms as a function of their risk. Likewise, I show that shares of tested banks changed as a function of pre-test exposure only after the announcement of the SCAP, satisfying the parallel trends assumption (Appendix Figure 1).

The main estimating equation tests whether syndicate-level outcomes changed between firm f's last pre-test and first post-test syndicate as a function of that firm's exposure to tested banks and other firm, deal, and lead arranger characteristics:

$$\Delta \text{Outcome}_{f,pre,post} = \alpha^F + \beta_1^F \overline{\text{Tested}}_{f,pre} + \gamma^F \text{Controls}_{f,pre} + \eta_f \tag{3}$$

where α^F is a constant, $\overline{\text{Tested}}_{f,pre}$ is firm f's pre-test exposure measure, and $\text{Controls}_{f,pre}$ is a vector of pre-test firm and syndicate controls. Credit outcomes at the syndicate level include the growth rate of the size of the syndicate and a dummy variable equal to 1 if the number of covenants on firm f's first post-test syndicate increased relative to its last pre-test syndicate (0 otherwise). In order to study whether loan pricing changed, I also match the last pre-test facility of a certain type to the first post-test facility of the same type.[30] In addition to credit outcomes, I investigate whether shares of four main types of lenders changed as a function of firm test exposure: tested banks, non-tested U.S. banks, foreign banks, and non-banks.

Firm controls include dummies for 1-digit SIC code, Census region,[31] bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the aver-age maturity, collateral, purpose, the shares of revolving credit lines in the pre-test syndicate, previous lenders, lead arrangers, and whether lenders had a long relationship with the firm ($>$ 5 years). Lead arranger controls include lagged values of the log of bank assets, equity capital/assets, securities/assets, deposits and short-term funds/assets, return on assets, and non-performing loans/gross loans for the largest lead arranger in the syndicates. In addition, I include firm-level measures of exposure to bank health and changes in equity capital ratios due to TARP investments weighted by pre-test syndicate shares.

Shocks to lenders more adversely affect small and opaque firms (Gertler and

[30] For example, I match based on revolving credit lines or different tranches of term loans (A, B, etc.).

[31] Due to the small sample size, I am unable to include more disaggregated industry and geographic controls without dropping a large number of firms.

Gilchrist (1994)), suggesting that lenders could reduce their shares in syndicates of risky firms. This would be consistent with the evidence presented in the lending regressions. However, since lead arrangers hold relatively larger shares of syndicates in these firms (Sufi (2007)), syndicates could have become more concentrated with tested banks, especially for relationship borrowers, potentially smoothing shocks to credit outcomes.[32] Given these contrasting predictions, I also investigate whether there was a differential effect as a function of firm bond market access.

1.5.3 *Results*

Table 9 presents evidence that firm-level credit outcomes did not change at the intensive margin for firms with relatively higher test exposure. Test exposure coefficients are standardized and can be interpreted as the percentage point change in different outcomes for a firm with pre-test tested bank shares one standard deviation above the mean (30 pp). Thus, increasing pre-test exposure to tested banks by one standard-deviation above the mean did not correspond to significant changes in the growth rate of loan syndicates (column 1), stricter covenants (column 2), the growth rate of individual facilities (column 5), or changes in spreads on similar facilities (column 6). However, I do find evidence of significant compositional changes for firms based on their access to the bond market. To be precise, I find that increasing pre-test exposure to tested banks by one-standard deviation above the mean for speculative grade firms resulted in a 15 pp reduction in syndicate growth (column 3), an 8 pp

[32]In fact, Allen and Paligorova (2015) find that banks passed along liquidity shocks to public firms in Canada rather than private firms, while retaining larger shares in private loan syndicates due to higher returns.

higher likelihood of receiving stricter covenants (column 4), and a 14 pp reduction in facility growth rates (column 7) after the SCAP relative to less exposed firms. I do not find evidence of significant changes in loan pricing across firm types. Taken together, these results provide evidence of a contraction in credit supply for speculative grade firms that were highly exposed to tested banks.

An important question is whether the observed changes in credit outcomes could affect changes in the composition of loan syndicates after the SCAP. As risky firms are most likely to be affected by shocks to their lenders, the substitution of firms towards other types of lenders could have been accompanied by worsened terms of credit. Results from the syndicate structure analysis are given in Table 10. I find that the share of tested banks in post-test syndicates fell by 15 pp more for a firm with ex-ante exposure to tested banks one standard deviation above the mean. More than half of this reduction in tested bank shares was offset by foreign banks (10 pp), while non-banks and other U.S. banks marginally increased their shares (2 pp each). The reduction in tested bank shares in post-test syndicates occurred similarly across all firms as a function of their access to the bond market (column 5). However, the composition of changes in syndicate members differed substantially across these firms. The majority of the reduction in tested bank shares for investment grade firms relatively more exposed tested banks was filled by foreign banks (column 7). In contrast, foreign banks and non-bank lenders offset the decline of tested banks in syndicates of speculative grade firms relatively more exposed to tested banks (columns 6 and 7).

Much of these changes in syndicate structure were driven by changes at the extensive margin. For example, the large increase in foreign bank shares in investment grade firm syndicates was due to new foreign banks entering into syndicates relative to existing foreign banks exiting (Appendix Table A10). I find minimal changes in the composition of loan syndicates for existing lenders (Appendix Table A9), consistent with results from the lending regressions.

While credit supply shocks differed at the extensive margin between gap and non-gap banks, I do not find significant differences in firm-level credit outcomes for firms as a function of their exposure to gap banks (Appendix Table A7). Thus, I conclude that the observed changes in credit for speculative grade firms was due to reductions in risk from all tested banks rather than exclusively from gap banks. In Section 7 I explore in more depth some of the differences in firm-level outcomes as a function of exposure to gap banks.

The results from this analysis demonstrate clear segmentation of the syndicated loan market. Foreign bank shares increased in syndicates of all types of firms that were relatively more exposed to tested banks, but especially investment grade firms. In contrast, other US banks played a relatively larger role in offsetting part of the reduction for relatively riskier borrowers (speculative grade firms). Overall, this evidence is consistent with tested banks reducing both credit risk and liquidity risk after the SCAP.

1.6 Firm Outcomes

The main focus of this paper is to determine whether firms were affected by changes in lending behavior by tested banks. In order to examine whether there was a transmission of shocks from stress testing of banks to the real economy, I focus on set of approximately 800 firms that obtained syndicated loans in the pre-test period. Given the observed changes in lending at the intensive margin, in this section I explore whether firms experienced changes in their overall ability to borrow as a function of their exposure to tested banks. Then I test whether these firms subsequently adjusted real firm-level outcomes.

1.6.1 *Total Borrowing: Background*

The evidence presented in the previous section suggests that firms relatively more reliant on tested banks before the SCAP experienced large and significant declines in tested bank shares after the SCAP. The composition of syndicates shifted towards foreign banks for most firms, while other U.S. banks also increased their shares in syndicates of risky firms. Given that speculative grade firms with high exposure to tested banks experienced reductions in credit at the intensive margin, I test whether there was a similar change in credit at the extensive margin.

The loss of tested banks as lenders could have affected firms' ability to borrow on similar terms after the test. Changes in credit could be driven by the need of lead arrangers to increase spreads or change other non-price terms in order attract additional lenders in place of tested banks. Lending relationships in the syndicated loan market exhibit stickiness over time for both lead arrangers and participants (Chodorow-Reich (2013)). In fact, firms receive lower spreads on subsequent loans when borrowing from previous lenders (Bharath et al. (2009)). While non-bank financial institutions such as certain institutional investors could ll in the gap for firms in need of financing, they generally extend credit at a higher cost (Nandy and Shao (2010)), especially for financially constrained firms (Lim, Minton, and Weisbach (2014)). However, Nini (2017) finds that firms more reliant on borrowing institutional term loans before the financial crisis fully substituted this exposure by borrowing from the bond market, and thus experienced no significant change in investment. Becker and Ivashina (2014) find evidence of firms substituting towards bond issuance following credit supply shocks, suggesting that firms highly exposed to tested banks could potentially increase debt or equity to substitute the loss of credit. Given this evidence, whether firms relatively more exposed to tested banks experienced changes in borrowing remains an open question.

1.6.2 ***Total Borrowing: Empirical Strategy***

The main question studied in this section is whether firms relatively more exposed to tested banks experienced changes in their borrowing after the SCAP. Relative to the previous analysis of loan syndicates, I utilize a wider set of firms that either obtained a new loan in the two years prior to the SCAP or with loans outstanding at the time of the SCAP, to determine whether certain firms substituted their loss of credit by borrowing from other lenders or other sources. Many of the factors that complicate identification of a causal effect of stress testing on syndicate outcomes likely pose similar challenges for this sample of firms.[33] I take a number of steps to aid identification of the effect of stress testing on firm borrowing. As before, I include controls for firm- and syndicate-level variables that proxy for credit demand

[33] Section 5.2 discusses the issues related to endogeneity of these estimates.

and balance sheet controls of lead arrangers in each syndicate. In Appendix Tables A2 and A3 I demonstrate that firm and deal characteristics are balanced across different levels of firm exposure to tested banks. Additionally, in Appendix Table A4 I show that firm borrowing outcomes did not exhibit a systematic trend in the three years before the SCAP, reducing concerns that firm outcomes might not have followed parallel trends in the pre-test period.

The main estimating equation for this section models firm borrowing outcomes as a function of shocks to firm credit demand and firm exposure to credit supply shocks.

$$\Delta \text{Outcome}_{f,pre,post} = \alpha^F + \beta_1^F \overline{\text{Tested}}_{f,pre} + \gamma^F \text{Controls}_{f,pre} + \eta_f \tag{4}$$

where the set of controls are identical to those in the syndicate-level regressions, with the exception that I use dummies for 2-digit SIC codes and state headquarters of firms for this larger sample of firms. As syndicated loans are frequently renegotiated, I define borrowing in this market similar to Chodorow-Reich (2013) as either obtaining a new loan or a positive modification of an existing loan (increase in size, extension of maturity, or loosening of existing covenants). In addition, I separately test for changes in borrowing due either to obtaining a new loan or a positive modification. Regressions of these discrete outcomes are performed using probit estimation. Firms that do not borrow in the syndicated loan market could substitute to other types of borrowing. In order to test whether this type of substitution occurred, I re-estimate equation (4) using growth rates of the book value of equity and debt.[34] For the

[34] I take three-year harmonized growth rates in order to reduce the reliance of outliers.

regressions of syndicated loan outcomes, finding β_1^F equal to 0 would affect a full substitution of lenders in post-test syndicates for firms relatively more affected by exposure to tested banks (Khwaja and Mian (2008)). As in prior sections, I explore whether firms borrowed on similar terms after the SCAP as a function of their access to the bond market in order to test for differential effects across firms. Given the evidence presented so far, it remains an open question whether firm-level frictions magnified the effect of reductions in risk by tested banks.

1.6.3 *Total Borrowing: Results*

The aforementioned analysis provides evidence that tested banks shifted their exposure to safer firms following the SCAP. In addition, syndicates that were highly concentrated with tested banks before the SCAP adjusted to become less concentrated afterward, while increases in shares of new lenders differed across firms.

Table 11 contains estimates from the baseline firm borrowing regressions. I find no evidence to reject the null hypothesis that firms relatively more exposed to tested banks experienced no change in their probability of borrowing after the SCAP. The evidence presented in columns 1-3 suggests that highly exposed firms fully substituted the loss of tested banks by borrowing on similar terms after the SCAP. Firms also did not significantly increase equity or debt growth in the three years after the SCAP as a function of pre-test exposure to tested banks (columns 4 and 5).

Columns 6-10 of Table 11 offer evidence that firms faced different borrowing outcomes in response to the SCAP as a function of their access to the bond market. Investment grade firms that were relatively more exposed to tested banks experienced significantly higher likelihoods of borrowing than less exposed firms, driven primarily by higher likelihoods of obtaining new loans (columns 6 and 7). In contrast, speculative grade firms relatively more exposed to tested banks experienced an 8 pp lower probability of receiving positive modifications of existing loan than less exposed firms (column 8). This result is consistent with the evidence in Table 10 showing that these firms faced stricter covenants and obtained worse non-price terms of credit after the SCAP. Importantly, these firms did not substitute this relative reduction in credit by issuing new debt or equity after the test (columns 9 and 10).

On the whole, these results suggest that the reduction in risk by tested banks affected speculative grade firms adversely. However, investment grade firms that were relatively more exposed to tested banks bene tted from increased borrowing as tested banks tilted their lending towards safer firms.

1.6.4 *Real Outcomes: Background*

Did firms that were highly exposed to tested banks in the syndicated loan market adjust real outcomes after the test? A large empirical literature has demonstrated the importance of a credit channel affecting real economic outcomes (Peek and Rosengren (2000), Ashcraft (2005), Gan (2007), Almeida, Campello, Laranjeira, and Weisbenner (2011), Cingano et al. (2016)). In addition, theory predicts that credit supply shocks are most likely to affect firms reliant on external sources to finance spending and investment (Bernanke and Gertler (1989)). Firms reliant on external finance that are unable to borrow following shocks to their lenders respond by adjusting spending and investment (Holmstrom and Tirole (1997)). Chodorow-Reich (2013) shows that financially constrained firms responded to credit supply shocks by also reducing employment.

The evidence in this paper so far suggests that the change in the composition of syndicates away from tested banks after the SCAP did not affect overall firm borrowing, suggesting likely minimal effects on firm real outcomes. However, speculative grade firms that were relatively more exposed to tested banks experienced significant reductions in borrowing, while similarly exposed investment grade firms experienced significant increases in credit. Given these differences in borrowing outcomes, the focus of this section is to document how firm real outcomes changed as a function of exposure to tested banks and financial constraints.

1.6.5 ***Real Outcomes: Empirical Strategy***

This section examines whether firms that were affected by their ability to borrow adjusted real outcomes in response to the SCAP. The estimation framework is a difference-in-difference model that estimates changes in real outcomes for firms as a function of test exposure. The hurdles to identification of the causal effect of stress testing on firm outcomes are similar to those in the borrowing analysis. I argued in prior sections that concerns of selection bias and parallel trends are likely less relevant in this context.[35] However, the identifying assumption is that stress testing uniquely affected firm borrowing and real outcomes, conditional on controlling for firm credit demand. The main estimating equation tests whether firms responded to the SCAP

[35]The test of parallel trends for firm real outcomes is given in Table A5.

by altering real outcomes as a function of their exposure to tested banks:

$$\Delta\text{Outcome}_{f,pre,post} = \alpha^F + \beta_1^F \overline{\text{Tested}}_{f,pre} + \beta_2^F \overline{\text{Tested}}_{f,pre} \times \text{Risk}_{f,pre} + \gamma^F \text{Controls}_{f,pre} + \eta_f \tag{5}$$

The outcomes of interest in this paper are motivated by previous work quantifying firm-level real effects of credit supply shocks (Chodorow-Reich (2013), Fraisse et al. (2015), Cingano et al. (2016), Gropp et al. (2018)). The first outcome of interest is the three-year rate of fixed investment, calculated as cumulative capital expenditures over 2009-2011, scaled by the 2008 book value of assets for each firm. In addition to plants and equipment, firms could demand credit for intangible expenditures. The second outcome that I consider is intangible investment, defined as cumulative net acquisitions, expenditures on research and development, and advertising expenditures over 2009-2011, scaled by the 2008 book value of assets. The remaining outcomes studied are the harmonized growth rate of assets and employment, calculated between 2008 and 2011. I utilize the same proxies for firm demand and firm exposure to credit supply shocks (lead arranger characteristics) as in prior sections. In addition, I test for differential changes in real outcomes across firms with different bond market access.

1.6.6 ***Real Outcomes: Results***

Results for the regressions of firm real outcomes are given in table 12. Firms relatively more exposed to tested banks did not significantly adjust spending on capital expenditures, intangible expenditures, assets or employment following the SCAP, conditional on controlling for firm risk characteristics (columns 1-4). These results are consistent with the observed substitution of borrowing by these firms. Results in columns 5 through 8 demonstrate that there was important heterogeneity in the response of firm real outcomes to their ability to borrow after the SCAP. Consistent with the fact that highly exposed speculative grade firms borrowed on relatively less favorable terms after the SCAP, I find that these firms subsequently reduced fixed investment by 3 pp and employment growth by 7 pp relative to less exposed firms in the three years after the SCAP (columns 5 and 8). On the other hand, investment grade firms that were relatively more exposed to tested banks increased intangible investment by 5 pp more than less exposed firms after the test (column 6).

The results in this section suggest that firms relatively more exposed to tested banks did not experience changes in their borrowing after the SCAP, and thus did not adjust real outcomes. However, highly exposed speculative grade firms reduced fixed investment and employment growth in response to their loss of credit. Investment grade firms bene tted from increased access to credit from tested banks and subsequently increased intangible investment after the SCAP.

1.7 Extensions

1.7.1 ***Bank Heterogeneity: Overview***

This section tests for heterogeneity in the treatment effect. Tested banks could have adjusted their lending behavior differentially as a function of their performance on the test. I first consider whether bank- and firm-level outcomes changed as a function of the size of each bank's capital gap. Then I study the response of banks to the 2013 CCAR/DFAST exercise to determine if the observed response to the SCAP can be extended to later rounds of stress testing.

1.7.2 *Bank Heterogeneity: Capital Gaps (SCAP)*

The results of supervisory stress tests can potentially affect tested banks in different ways depending on the outcome of tests. Results in Table 7 suggest that banks with positive capital gaps for the most part did not differentially change their lending behavior relative to non-gap banks. However, the magnitudes of these estimates suggest a relatively more conservative lending response than among non-gap banks.[36] At the firm level, I do not find sufficient evidence to conclude that firms relatively more exposed to gap banks experienced significantly worse credit outcomes (Table A8), significantly different changes in syndicate structure (Table A11), or significant changes in borrowing (Table A13) or real outcomes (Table A15) relative to less exposed firms.

Stress tests provide information beyond simply classifying institutions based on deficiencies in their capital planning. In the context of the SCAP, I construct a proxy which captures the difference between capital ratios under the severely adverse scenario and current capital ratios:

$$\Delta CapRatio_{Actual-Fed} = CapRatio_{Actual} - CapRatio^{Stressed}_{Fed} \quad (6)$$

[36]36 Identification of the causal effect of failing a stress test on lending and firm outcomes is complicated by the fact that failing banks differed from passing banks not only on observable dimensions, like measures of bank health, but potentially on unobservable dimensions as well. Gap banks could have failed the SCAP likely because of weakness in their lending portfolios and reliance on risky firms. Thus the causal interpretation of these estimates is less straightforward than for estimates of stress testing itself.

This measure captures the percentage point increase in capital ratios that failing banks would be required to raise in response to the test. To be precise, it is calculated as each failing bank's capital gap (or SCAP "buffer") divided by its risk-weighted assets from the beginning of the test.[37] This measure excludes capital actions that occurred between the time banks were informed of their capital gap and the release of the results, and thus can be viewed as plausibly exogenous to any changes in bank behavior that might have occurred before the test. Another way to think of $\Delta CapRatio_{Actual-Fed}$ is each bank's likelihood of not having sufficient capital under a stressed scenario affecting all banks in the economy, or its contribution to systemic risk (Acharya et al. (2018)). For all of the main regressions, I interact the capital gap dummy with this measure. Results are given in Appendix Tables A8, A12, A14, and A16 and are generally similar to those found for the discrete measure of the capital gap, suggesting that banks with larger capital gaps responded more conservatively to the test than banks with smaller capital gaps, although firm-level outcomes were generally not significantly different than for firms less exposed to gap banks.

1.7.3 *Firm Heterogeneity: Overview*

This section tests for the importance of firm-level frictions in driving the results in this paper. Credit supply shocks can adversely affect firms when there is large information asymmetry between lenders and firms. I investigate the extent to which tested banks reduced their exposure to these firms, and whether firm-level outcomes were subsequently affected.

[37] Mésonnier and Monks (2015) utilize a similar measure for European stress tests

1.7.4 *Firm Heterogeneity: First Time Borrowers*

Lead arrangers hold larger shares in syndicates of opaque borrowers as a means of addressing the moral hazard problem with participant lenders (Sufi (2007)). As a way to test for the importance of these frictions in the transmission of shocks to firms, I include interactions of a first-time borrower dummy with tested bank measures in all specifications to allow for these differential effects. First, I do not find significant differences in credit outcomes for firms that were highly exposed to tested banks and were first time borrowers (Table A17). However, syndicates for these unrated and speculative grade firms became more concentrated with U.S. banks after the test (Table A19). Table A21 suggests that first time borrowers were significantly less likely to borrow after the test, and consequently reduced fixed investment and employment growth (Table A23). These adverse effects were especially pronounced among speculative grade firms.

1.7.5 *Firm Heterogeneity: Lending Relationships*

Relationships between lenders and borrowers in the syndicated loan market are highly persistent over time, both for lead arrangers and participants (Chodorow-Reich (2013)). Bank-dependent firms benefit by borrowing from highly capitalized banks, smoothing shocks to the real economy (Schwert (2018)). Moreover, the loss of lending relationships can adversely affect firm borrowing outcomes (Bharath et al. (2009)). To examine the importance of lending relationships, I re-estimate all specifications with additional interactions of a lending relationship dummy (equal to

1 for lender- firm pairs lasting longer than 5 year, 0 otherwise) and the tested bank regressors. I do not find strong evidence to suggest that firms with high exposure to tested banks and longer lending relationships experienced differential changes in credit outcomes after the SCAP (Table A18) or syndicate structure (Table A20). At the firm level, I find that investment grade firm with high exposure to tested banks and long lending relationships were significantly more likely to borrow (Table A22) and increase intangible investment after the test (Table A24).

1.8 Conclusion

Banks included in the early rounds of U.S. stress tests shifted their lending toward relatively safer firms after the SCAP. This resulted in a change in the composition of syndicate members away from tested banks, but did not dramatically affect overall borrowing and firm outcomes. However, the response differed importantly across firms as a function of their access to the bond market. Foreign banks offset much of the change in post-test syndicates for investment grade firms that were highly exposed to tested banks, resulting in increases in borrowing and investment for these firms. Speculative grade firms highly reliant on tested banks experienced increases in syndicate shares across non-tested U.S. banks and foreign banks. As a result, these firms did not borrow on similar terms after the test and responded by reducing investment and employment.

Bank stress testing plays an important role in improving financial stability and restoring confidence in the banking system. However, it can also result in reductions in exposures to risk by banks included in stress tests. The change in composition of risk in post-test syndicates of large corporate firms provides insights into how macro-prudential policies can reallocate risks in the financial system, while not dramatically affecting the real economy.

1.9 Tables

Table 1: List of U.S. Bank-Holding Companies

Passing Tested Banks	Failing Tested Banks
American Express Company	Ally Financial Inc.
The Bank of New York Mellon Corporation	Bank of America Corporation
BB&T Corporation	Citigroup Inc.
Capital One Financial Corporation	Fifth Third Bancorp
The Goldman Sachs Group, Inc.	KeyCorp
JP Morgan Chase & Co.	Morgan Stanley
MetLife, Inc.	The PNC Financial Services Group, Inc.
State Street Corporation	Regions Financial Corporation
U.S. Bancorp	SunTrust Banks, Inc.
Wells Fargo & Co.	
Other Banks	**Other Banks**
Associated Bank	Huntington Bancshares
BOK Financial	Jefferies
Brown Brothers Harriman	M&T Bank
CapitalSource Finance	Marshall & Ilsley Corp
CIT Group	Mid first Bank
City National Bank	Northern Trust Corp
Cobank	PrivateBancorp
Comerica	Raymond James Financial
First Tennessee National	Silicon Valley Bancshares
FirstMerit Bank	UMB Bank
Frost National Bank	Webster Bank
General Electric Capital	Zions Bancorporation
Hancock Bank	

Table 2: List of Foreign Bank-Holding Companies

Bank Name	Country	Bank Name	Country
Raiffeisen Zentralbank	Austria	Societe Generale	France
ANZ Banking Group	Australia	Barclays	Great Britain
Westpac Banking	Australia	HSBC Banking Group	Great Britain
Dexia Bank	Belgium	Lloyds Banking Group	Great Britain
KBC Group	Belgium	Royal Bank of Scotland	Great Britain
BMO Capital Markets	Canada	Standard Chartered Bank	Great Britain
CIBC	Canada	Allied Irish Banks	Ireland
RBC Capital Markets	Canada	Bank of Ireland Group	Ireland
Scotiabank	Canada	Bank Hapoalim	Israel
Toronto Dominion Bank	Canada	Bank Leumi Le-Israel	Israel
Credit Suisse	Switzerland	Israel Discount Bank	Israel
UBS	Switzerland	Intesa Sanpaolo	Italy
Bank of China	China	UniCredit	Italy
Bank of Communications	China	Mitsubishi UFJ Financial	Japan
BayernLB	Germany	Mizuho Financial	Japan
Commerzbank	Germany	Sumitomo Mitsui Financial	Japan
Deutsche Bank	Germany	ABN AMRO Bank	Netherlands
DZ Bank	Germany	Fortis Bank	Netherlands
HSH Nordbank	Germany	ING Group	Netherlands
Landesbank Hessen-Thuringen	Germany	Rabobank	Netherlands
NordLB Group	Germany	DNB	Norway
Portigon	Germany	Nordea Bank	Sweden
BBVA	Spain	United Overseas Bank	Singapore
Banco Santander	Spain	Cathay United Bank	Taiwan
BNP Paribas	France	Chang Hwa Commercial Bank	Taiwan
CM-CIC	France	First Commercial Bank of Taiwan	Taiwan
Credit Agricole	France	Hua Nan Commercial Bank	Taiwan
Natixis	France		

Table 3: Matched Bank-Firm Summary Statistics

Bank-Firm Outcomes	Number	Mean	Std. Dev.	Min	p25	p50	p75	Max
Loan Growth Rate (Pct Change)	2398	-0.02	0.11	-0.35	-0.07	-0.03	0.01	0.61
Exit (D)	2446	0.41	0.49	0.00	0.00	0.00	1.00	1.00
Enter (D)	2707	0.45	0.50	0.00	0.00	0.00	1.00	1.00

Bank-Firm Regressors	Number	Mean	Std. Dev.	Min	p25	p50	p75	Max
Tested (D)	5133	0.42	0.49	0.00	0.00	0.00	1.00	1.00
Gap (D)	5133	0.27	0.44	0.00	0.00	0.00	1.00	1.00
Gap Size / Risk-Weighted Assets	5153	0.00	0.01	0.00	0.00	0.00	0.00	0.02
Log Bank Assets	4835	20.19	1.35	15.16	19.27	20.58	21.28	22.08
Equity/Assets	4835	0.07	0.03	0.01	0.04	0.06	0.09	0.17
Securities/Assets	4835	0.33	0.18	0.00	0.18	0.31	0.42	0.86
Return on Assets	4829	0.01	0.01	-0.10	0.01	0.01	0.01	0.07
Non-Performing Loans/Gross Loans	4507	0.01	0.01	0.00	0.00	0.01	0.01	0.10
Deposits and S-T Funds/Assets	4814	0.65	0.15	0.11	0.59	0.69	0.76	0.92
CPP Investments/Assets	4835	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Bank Health	5153	0.01	0.00	0.00	0.01	0.01	0.01	0.03
Collateral (D)	5153	0.41	0.49	0.00	0.00	0.00	1.00	1.00
Corporate Purpose (D)	5153	0.70	0.46	0.00	0.00	1.00	1.00	1.00
Maturity	5153	40.65	19.53	0.00	28.00	39.00	60.00	178.00
Log Firm Assets	5153	22.19	1.61	17.89	20.97	21.97	23.37	26.36
Book Value of Leverage	5153	2.77	5.47	0.03	1.01	1.61	2.74	63.76
Cash/Assets	5153	0.07	0.11	0.00	0.01	0.04	0.09	1.00
Tangible Assets/Assets	5153	0.49	0.27	0.00	0.26	0.48	0.72	0.95
Profits/Assets	5153	0.14	0.06	-0.04	0.10	0.13	0.17	0.41
Rated (D)	5153	0.70	0.46	0.00	0.00	1.00	1.00	1.00
Investment Grade (D)	5153	0.42	0.49	0.00	0.00	0.00	1.00	1.00

This table provides summary statistics of the main outcome and explanatory variables for the matched bank- firm analysis. Dummy variables are denoted by (D).

Table 4: Firm-Level Summary Statistics

Syndicate-Level Outcomes	Number	Mean	Std. Dev.	Min	p25	p50	p75	Max
Test Share	307	-0.01	0.28	-1.00	-0.15	0.00	0.11	1.00
Non-Test US Bank Share	307	0.01	0.17	-1.00	-0.03	0.00	0.03	1.00
Non-US Bank Share	307	0.01	0.23	-1.00	-0.09	0.00	0.10	1.00
Non-Bank Share	307	0.00	0.14	-1.00	0.00	0.00	0.00	1.00
Lead Test Share	307	0.04	0.31	-1.00	-0.07	0.01	0.19	1.00
Lead Non-Test US Bank Share	307	0.01	0.14	-1.00	0.00	0.00	0.00	1.00
Lead Non-US Bank Share	307	0.02	0.21	-1.00	0.00	0.00	0.00	1.00
Lead Non-Bank Share	307	0.00	0.11	-1.00	0.00	0.00	0.00	1.00
Firm-Level Outcomes	**Number**	**Mean**	**Std. Dev.**	**Min**	**p25**	**p50**	**p75**	**Max**
Borrow (D)	897	0.47	0.50	0.00	0.00	0.00	1.00	1.00
New Loan (D)	897	0.36	0.48	0.00	0.00	0.00	1.00	1.00
Positive Modification (D)	897	0.14	0.35	0.00	0.00	0.00	0.00	1.00
Harmonized 3-year Equity Growth Rate	752	0.12	0.48	-1.99	-0.06	0.14	0.36	1.87
Harmonized 3-year Debt Growth Rate	778	0.08	0.43	-1.70	-0.14	0.06	0.28	1.82
Tangible Investment Rate 3-year	776	0.18	0.22	0.00	0.06	0.11	0.20	1.71
Intangible Investment Rate 3-year	711	0.20	0.41	-1.23	0.01	0.09	0.26	4.96
Harmonized 3-year Assets Growth Rate	780	0.10	0.35	-1.71	-0.07	0.09	0.26	1.66
Harmonized 3-year Emp Growth Rate	744	0.04	0.32	-1.61	-0.08	0.02	0.17	2.00
Harmonized 3-year Sales Growth Rate	778	0.01	0.35	-1.66	-0.15	0.01	0.18	2.00
Firm-Level Regressors	**Number**	**Mean**	**Std. Dev.**	**Min**	**p25**	**p50**	**p75**	**Max**
Test Exposure	897	0.61	0.29	0.00	0.43	0.62	0.85	1.00
Gap Exposure	897	0.45	0.29	0.00	0.26	0.42	0.61	1.00
Gap Size Exposure	897	0.01	0.00	0.00	0.00	0.01	0.01	0.02
Lead Test Exposure	897	0.36	0.31	0.00	0.16	0.28	0.49	1.00
Threshold Test Exposure	897	0.08	0.16	0.00	0.00	0.00	0.11	1.00
Rated (D)	897	0.49	0.50	0.00	0.00	0.00	1.00	1.00
Investment Grade (D)	897	0.25	0.43	0.00	0.00	0.00	0.00	1.00
Log Firm Assets	897	21.02	1.78	16.08	19.74	20.88	22.19	27.76
Book Value of Leverage	897	2.19	4.39	0.03	0.73	1.24	2.14	75.17
Cash/Assets	897	0.11	0.15	0.00	0.02	0.05	0.13	1.00
Tangible Assets/Assets	897	0.44	0.26	0.00	0.23	0.42	0.64	0.95
Profits/Assets	897	0.12	0.11	-0.74	0.08	0.12	0.17	0.71
First Time Borrower (D)	897	0.65	0.48	0.00	0.00	1.00	1.00	1.00
Share Revolver	897	0.82	0.34	0.00	0.83	1.00	1.00	1.00
Share Term A Loan	897	0.03	0.15	0.00	0.00	0.00	0.00	1.00
Number of Covenants	897	0.94	1.19	0.00	0.00	0.00	2.00	5.00
Share Collateralized	897	0.50	0.50	0.00	0.00	0.85	1.00	1.00
Maturity	897	48.74	20.17	0.00	36.00	60.00	60.00	168.00
Share Corporate Purpose	897	0.58	0.49	0.00	0.00	1.00	1.00	1.00
Share Previous Lead	897	0.55	0.29	0.00	0.31	0.47	0.79	1.00
Share Previous Lender	897	0.67	0.38	0.00	0.44	0.84	1.00	1.00
Share Long Relationship	897	0.40	0.38	0.00	0.00	0.38	0.75	1.00
Log Lead Bank Assets	897	6.90	1.13	1.84	6.45	7.32	7.48	8.26
Lead Bank Equity/Assets	897	0.08	0.03	0.01	0.06	0.08	0.09	0.24
Lead Bank Securities/Assets	897	0.33	0.15	0.00	0.25	0.32	0.38	0.77
Lead Bank Dep and S-T Funds/Assets	897	0.67	0.14	0.00	0.62	0.69	0.71	1.53
Lead Bank Return on Assets	897	0.01	0.00	-0.01	0.01	0.01	0.01	0.04
Lead Bank NPL/Gross Loans	897	0.01	0.01	0.00	0.00	0.00	0.01	0.04
Lead Bank Exposure	897	0.48	0.30	0.04	0.24	0.37	0.66	1.00

This table provides summary statistics of the main outcome and explanatory variables for the syndicate-level and firm-level analysis. Dummy variables are denoted by (D).

Table 5: Bank Balance-Sheet Characteristics

Pre-SCAP Bank Characteristics (2007-2008)

Characteristic	Non-Tested	Tested	Difference	p-value
Total Assets ($ Bln USD)	607.46	612.27	-4.82	0.98
Securities / Assets	30.87	30.88	-0.01	1.00
Gross Loans / Assets	53.82	49.32	4.49	0.37
Non-Performing Loans / Gross Loans	1.93	0.99	0.94	0.02
Return on Assets	0.47	0.40	0.07	0.79
Equity Capital / Assets	5.71	8.08	-2.37	0.00
Deposits and Short-Term Funds / Assets	64.55	60.57	3.98	0.40
Tangible Common Equity / Tangible Assets	4.48	4.06	0.42	0.51

Adjustment of Bank Characteristics (2009-2010 vs. 2007-2008)

Characteristic	Non-Tested	Tested	Difference	p-value
Total Assets	0.09	0.05	0.04	0.48
Securities	0.08	0.25	-0.17	0.11
Gross Loans	0.10	-0.04	0.14	0.03
Non-Performing Loans	0.87	1.47	-0.60	0.00
Risk-Weighted Assets	-0.01	0.02	-0.03	0.64
Tangible Assets	0.09	0.05	0.04	0.50
Securities / Assets	0.17	4.85	-4.68	0.00
Gross Loans / Assets	0.14	-3.42	3.56	0.04
Risk-Weighted Assets / Assets	-0.06	-0.06	0.00	0.97
Non-Performing Loans / Gross Loans	1.80	2.34	-0.54	0.37
Return on Assets	-0.31	-0.47	0.16	0.46
Total Equity	0.13	0.21	-0.08	0.33
Tangible Common Equity	0.22	0.38	-0.16	0.10
Total Deposits and Short-Term Funds	0.10	0.01	0.08	0.23
Total Equity / Assets	0.33	0.95	-0.62	0.08
Total Deposits and Short-Term Funds / Assets	0.65	-0.73	1.38	0.33
Tangible Common Equity / Tangible Assets	0.35	1.15	-0.80	0.03

The upper panel of this table includes summary statistics of tested and not tested financial institutions averaged over 2007-2008. Means of bank characteristics are displayed in columns 1 (non-tested including new CCAR entrants) and 2 (tested only SCAP banks). Differences of mean values are given in column 3 and p-values from t-tests of the equality of means in column 4. The lower panel of this table includes summary statistics of average differences of bank characteristics between 2009-2010 relative to 2007-2008.

Table 6: Bank Lending: Matched Bank-Firm Regressions (SCAP: Testing)

	Dependent Variables:					
	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry
Regressors:	(1)	(2)	(3)	(4)	(5)	(6)
Tested	0.01	-0.08**	-0.07*	0.01	-0.01	0.04
	(0.01)	(0.03)	(0.04)	(0.02)	(0.06)	(0.06)
× Rated				0.02	-0.13	-0.21**
				(0.01)	(0.08)	(0.08)
× Rated × Inv Grade				0.01	0.01	0.10
				(0.01)	(0.08)	(0.07)
E[Dep Var | Tested = 1]						
Speculative Grade Firms				0.01	-0.14**	-0.18**
				(0.01)	(0.07)	(0.07)
Investment Grade Firms				0.02*	-0.12***	-0.08
				(0.01)	(0.05)	(0.05)
Firm FE	Y	Y	Y	Y	Y	Y
Firm Controls	Y	Y	Y	Y	Y	Y
Firm × Controls	Y	Y	Y	Y	Y	Y
Bank-Firms	2083	2131	2335	2083	2131	2335
R-squared	0.50	0.47	0.37	0.51	0.49	0.38

This table provides estimates from regressions of loan outcomes on firm, facility, and bank characteristics. The dependent variable in columns 1 and 4 is the change in the size of bank b's commitment to firm f's first post-test syndicate (2009-2010) relative to its last pre-test syndicate (2007-2008) scaled by the total size of the last pre-test syndicate. The dependent variable in columns 2 and 5 is a dummy equal to 1 if a bank- firm pair existing in the last pre-test syndicate (2007-2008) did not exist in the first post-test syndicate (2009-2010) and 0 otherwise. The dependent variable in columns 3 and 6 is a dummy equal to 1 if a bank- firm pair existing in the first post-test syndicate (2009-2010) newly entered the syndicate and 0 if a bank- firm pair existing in the first post-test syndicate (2009-2010) also existed in the last pre-test syndicate (2007-2008). Tested is a dummy equal to 1 if bank b was included in the SCAP, 0 if not. Firm controls include dummies for 2-digit SIC code, state, bond market access (rated), investment grade credit rating, and lagged values of the log of assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Deal controls include the syndicate's average maturity, total number of covenants, the number of lead arrangers and total lenders, and dummies for collateral, whether the loan was used for corpo-rate purposes, and year that the pre-test syndicate was extended. Bank controls include the share of Capital Purchase Program capital pledged to bank b scaled by total assets as of 2008Q4 and standardized lagged values of the log of bank assets, equity capital/assets, securities/total assets, deposits and short-term funds/total assets, return on assets, non-performing loans/gross loans, and bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f). Bank- firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. The sample of loans in columns 1-2 and 4-5 includes all loan syndicates with at least one lead arranger and one participant, and at least one tested bank and one non-tested bank in each pre-test syndicate (the loan growth regressions also Winsorize at the 2nd and 99th percentile values of loan growth). The sample of loans in columns 3 and 6 includes all syndicate with at least one lead arranger and one participant, and at least one tested bank and one non-tested bank in each post-test syndicate. The post-test period covers Feb 2009-Dec 2010 and the pre-test period cover January 2007-January 2009. Standard errors given below coefficient estimates are clustered at the bank level. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 7: Bank Lending: Matched Bank-Firm Regressions (SCAP: Gap)

	Dependent Variables:					
	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry
Regressors:	(1)	(2)	(3)	(4)	(5)	(6)
Tested	0.01	-0.10**	-0.03	-0.01	0.01	0.08
	(0.01)	(0.04)	(0.05)	(0.02)	(0.06)	(0.05)
× Rated				0.02	-0.17*	-0.22**
				(0.02)	(0.10)	(0.08)
× Rated × Inv Grade				0.00	-0.02	0.06
				(0.01)	(0.08)	(0.07)
Tested × Gap	-0.01	0.03	-0.06	0.00	-0.04	-0.09
	(0.01)	(0.02)	(0.05)	(0.02)	(0.04)	(0.06)
× Rated				-0.01	0.08	0.01
				(0.02)	(0.09)	(0.05)
× Rated × Inv Grade				0.00	0.06	0.07*
				(0.01)	(0.07)	(0.04)
Firm FE	Y	Y	Y	Y	Y	Y
Lender Controls	Y	Y	Y	Y	Y	Y
Firm × Lender Controls	Y	Y	Y	Y	Y	Y
Bank-Firms	2083	2131	2335	2083	2131	2335
R-squared	0.50	0.47	0.37	0.51	0.49	0.38

This table provides estimates from regressions of loan outcomes on firm, facility, and bank characteristics. The dependent variable in columns 1 and 4 is the change in the size of bank b's commitment to firm f's first post-test syndicate (2009-2010) relative to its last pre-test syndicate (2007-2008) scaled by the total size of the last pre-test syndicate. The dependent variable in columns 2 and 5 is a dummy equal to 1 if a bank- firm pair existing in the last pre-test syndicate (2007-2008) did not exist in the first post-test syndicate (2009-2010) and 0 otherwise. The dependent variable in columns 3 and 6 is a dummy equal to 1 if a bank- firm pair existing in the first post-test syndicate (2009-2010) newly entered the syndicate and 0 if a bank- firm pair existing in the first post-test syndicate (2009-2010) also existed in the last pre-test syndicate (2007-2008). Tested is a dummy equal to 1 if bank b was included in the SCAP, 0 if not. Gap is a dummy equal to 1 if bank b was included in the SCAP and found to have a positive capital gap, 0 if not. Firm controls include dummies for 2-digit SIC code, state, bond market access (rated), investment grade credit rating, and lagged values of the log of assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Deal controls include the syndicate's average maturity, total number of covenants, the number of lead arrangers and total lenders, and dummies for collateral, whether the loan was used for corporate purposes, and year that the pre-test syndicate was extended. Bank controls include the share of Capital Purchase Program capital pledged to bank b scaled by total assets as of 2008Q4 and standardized lagged values of the log of bank assets, equity capital/assets, securities/total assets, deposits and short-term funds/total assets, return on assets, non-performing loans/gross loans, and bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f). Bank- firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. The sample of loans in columns 1-2 and 4-5 includes all loan syndicates with at least one lead arranger and one participant, and at least one tested bank and one non-tested bank in each pre-test syndicate (the loan growth regressions also Winsorize at the 2nd and 99th percentile values of loan growth). The sample of loans in columns 3 and 6 includes all syndicate with at least one lead arranger and one participant, and at least one tested bank and one non-tested bank in each post-test syndicate. The post-test period covers Feb 2009-Dec 2010 and the pre-test period cover January 2007-January 2009. Standard errors given below coefficient estimates are clustered at the bank level. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 8: Bank Lending: Total Effects from Matched Bank-Firm Regressions (SCAP: Gap)

	Dependent Variables:					
	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry
Total Effects:	(1)	(2)	(3)	(4)	(5)	(6)
E[DepVar | Tested = 1]						
Non-Gap Banks						
All Firms	0.01	-0.10**	-0.03			
	(0.01)	(0.04)	(0.05)			
Unrated Firms				-0.01	0.01	0.08
				(0.02)	(0.06)	(0.05)
Speculative Grade Firms				0.02	-0.16**	-0.14*
				(0.01)	(0.08)	(0.08)
Investment Grade Firms				0.02**	-0.18***	-0.08
				(0.01)	(0.05)	(0.05)
Gap Banks						
All Firms	0.01	-0.07**	-0.10*			
	(0.01)	(0.03)	(0.05)			
Unrated Firms				-0.01	-0.03	-0.01
				(0.02)	(0.06)	(0.07)
Speculative Grade Firms				0.01	-0.12	-0.22***
				(0.01)	(0.08)	(0.08)
Investment Grade Firms				0.01	-0.09*	-0.08
				(0.01)	(0.05)	(0.05)

This table provides estimates of total effects from regressions of loan outcomes on firm, facility, and bank characteristics in Table 7. Standard errors given below coefficient estimates are clustered at the bank level. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 9: Firm-Level Intensive Margin Regressions (SCAP: Testing)

	Dependent Variables:							
	Syndicate-Level				Facility-Level			
	Syndicate Growth	Covenant Increase	Syndicate Growth	Covenant Increase	Facility Growth	ΔSpread	Facility Growth	ΔSpread
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.06	0.01	-0.04	0.00	0.05	5.90	0.09	-7.40
× Rated	(0.07)	(0.02)	(0.09)	(0.02)	(0.06)	(9.21)	(0.10)	(14.24)
			-0.11	0.08			-0.23	7.59
× Rated			(0.10)	(0.04)			(0.12)	(16.26)
			0.21	-0.07			0.27	9.86
× Inv Grade			(0.13)	(0.05)			(0.08)	(26.21)
	E[Dep Var | High Test Exp]							
Speculative Grade Firms			-0.15**	0.08**			-0.14*	0.19
			(0.07)	(0.04)			(0.07)	(14.33)
Investment Grade Firms			0.06	0.01			0.13	10.05
			(0.14)	(0.06)			(0.10)	(19.67)
Firm and Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm Bank Controls	N	N	Y	Y	N	N	Y	Y
R-squared	0.21	0.16	0.26	0.25	0.21	0.23	0.35	0.36
Number of Firms	298	330	298	330	233	250	233	250

This table provides estimates from regressions of firm-level borrowing outcomes on firm, facility, and bank characteristics. The dependent variable in columns 1 and 3 is the percent change in the size of firm f's first post-test syndicate (2009-2010) relative to its last pre-test syndicate (2007-2008). The dependent variable in columns 2 and 4 is a dummy equal to 1 if the number of covenants in firm f's first post-test syndicate (2009-2010) exceeded the number in its last pre-test syndicate (2007-2008). Coefficient estimates reported in columns 2 and 4 are marginal effects. The dependent variable in columns 5 and 7 is the percent change in the size of firm f's first post-test facility (2009-2010) relative to its last pre-test facility (2007-2008), where facilities from different periods are matched according to their type (revolving credit line, term loan A, B, ...). The dependent variable in columns 6 and 8 is the basis point change in firm f's first post-test facility (2009-2010) relative to its last pre-test facility (2007-2008), where facilities from different periods are matched according to their type (revolving credit line, term loan A, B, ...). Test Exp equals firm f's exposure to tested banks in its last pre-test syndicate (weighted by loan shares). Firm controls include dummies for 1-digit SIC code, Census region, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, number of lead arrangers and lenders in the last pre-test syndicate, the share of previous lead arrangers and lenders in the last pre-test syndicate, whether lenders had a long relationship with the firm (¿ 5 years), and whether the firm borrowed from the syndicated loan market for the first time. Bank controls include standardized values of the largest lead arranger's lagged log of bank assets, equity capital/assets, securities/assets, deposits and short-term funds/assets, return on assets, non-performing loans/gross loans, bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f), and the weighted average of the size of Capital Purchase Program investments scaled by total assets in 2008Q4 (for U.S. banks). Bank- firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. The sample of loans in all columns includes the set of firms that either obtained a new loan in both periods or received some form of modification of an existing loan in the post-test period. Growth rates in columns 1, 3, 5, and 7 are Winsorized at the 90th percentile (growth rates exceeding 193% and 220%, respectively). Changes in spreads in columns 6 and 8 exclude values exceeding 400 bp increases. The post-test period covers Feb 2009-Dec 2010 and the pre-test period cover January 2007-January 2009. Standard errors given below coefficient estimates are clustered at the level of the largest lead arranger in each pre-test syndicate. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 10: Syndicate Structure Regression Results (SCAP: Testing)

	Dependent Variables: Change in Syndicate Shares							
	Tested Bank	Other US Bank	Foreign Bank	Non-Bank	Tested Bank	Other US Bank	Foreign Bank	Non-Bank
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.15	0.02	0.10	0.02	-0.13	0.03	0.09	0.01
	(0.02)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.01)
× Rated					-0.02	0.03	0.00	0.00
					(0.03)	(0.02)	(0.04)	(0.02)
× Rated × Inv Grade					-0.05	-0.08	0.11	0.02
					(0.05)	(0.02)	(0.03)	(0.03)
E[Dep Var | High Test Exp]								
Speculative Grade Firms					-0.15***	0.06***	0.08***	0.01
					(0.02)	(0.01)	(0.02)	(0.02)
Investment Grade Firms					-0.21***	-0.02	0.20***	0.03
					(0.05)	(0.02)	(0.03)	(0.02)
Firm and Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.36	0.26	0.26	0.25	0.41	0.31	0.34	0.34
Number of Firms	331	331	331	331	331	331	331	331
Number of Clusters	23	22	23	23	23	22	23	23

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The dependent variable is the change in the share of different lenders in firm f's first post-test syndicate minus the share of the same type of lender in its last pre-test syndicate, where shares are calculated using loan weights. Test Exp equals firm f's exposure to tested banks in its last pre-test syndicate (weighted by loan shares). Firm controls include dummies for 1-digit SIC code, Census region, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, number of lead arrangers and lenders in the last pre-test syndicate, the share of previous lead arrangers and lenders in the last pre-test syndicate, whether lenders had a long relationship with the firm (> 5 years), and whether the firm borrowed from the syndicated loan market for the first time. Bank controls include standardized values of the largest lead arranger's lagged log of bank assets, equity capital/assets, securities/assets, deposits and short-term funds/assets, return on assets, non-performing loans/gross loans, bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f), and the weighted average of the size of Capital Purchase Program investments scaled by total assets in 2008Q4 (for U.S. banks). Bank- firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. Standard errors given below coefficient estimates are clustered at the level of the largest lead arranger in each pre-test syndicate. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 11: Total Borrowing Regression Results (SCAP: Testing)

	Dependent Variables:									
	Borrow	New Loan	Modify	Equity Growth	Debt Growth	Borrow	New Loan	Modify	Equity Growth	Debt Growth
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Test Exp	0.01	0.01	-0.01	-0.01	0.00	0.00	0.01	0.00	0.01	-0.02
	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.05)	(0.04)	(0.04)	(0.03)	(0.03)
× Rated						-0.05	-0.02	-0.08	-0.05	0.01
						(0.07)	(0.06)	(0.04)	(0.06)	(0.05)
× Rated × Inv Grade						0.14	0.09	0.14	0.00	0.03
						(0.04)	(0.04)	(0.07)	(0.06)	(0.06)
E[Dep Var | High Test Exp]										
Spec Grade						-0.05	-0.01	-0.08***	-0.04	-0.01
						(0.04)	(0.04)	(0.03)	(0.05)	(0.04)
Invest Grade						0.10***	0.09***	0.06	-0.04	0.03
						(0.03)	(0.03)	(0.05)	(0.03)	(0.04)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.23	0.25	0.26	0.21	0.21	0.25	0.28	0.29	0.25	0.22
Number of Firms	751	747	716	692	714	751	747	716	692	714
Number of Clusters	30	30	29	29	29	30	30	29	29	29

This table provides estimates from regressions of borrowing outcomes on firm and deal characteristics. The dependent variable in columns 1 and 6 is a dummy equal to 1 if firm f received either a new loan or a positive modification in its first syndicate after the SCAP (over 2009-2010) and 0 if not. The dependent variable in columns 2 and 7 is a dummy equal to 1 if firm f received a new loan in its first syndicate after the SCAP (over 2009-2010) and 0 if not. The dependent variable in columns 3 and 8 is a dummy equal to 1 if firm f received a positive modification in its first syndicate after the SCAP (over 2009-2010) and 0 if not. The dependent variable in columns 4, 5, 9, and 10 are the three-year harmonized growth rates (over 2009-2011) of equity and debt, respectively. Test Exp equals firm f's exposure to tested banks in its last pre-test syndicate (weighted by loan shares). Firm controls include dummies for 2-digit SIC code, state, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, number of lead arrangers and lenders in the last pre-test syndicate, the share of previous lead arrangers and lenders in the last pre-test syndicate, whether lenders had a long relationship with the firm (¿ 5 years), and whether the firm borrowed from the syndicated loan market for the first time. Bank controls include standardized values of the largest lead arranger's lagged log of bank assets, equity capital/assets, securities/assets, deposits and short-term funds/assets, return on assets, non-performing loans/gross loans, bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f), and the weighted average of the size of Capital Purchase Program investments scaled by total assets in 2008Q4 (for U.S. banks). Bank- firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. Standard errors given below coefficient estimates are clustered at the level of the largest lead arranger in each pre-test syndicate. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table 12: Firm Real Outcomes (SCAP: Testing)

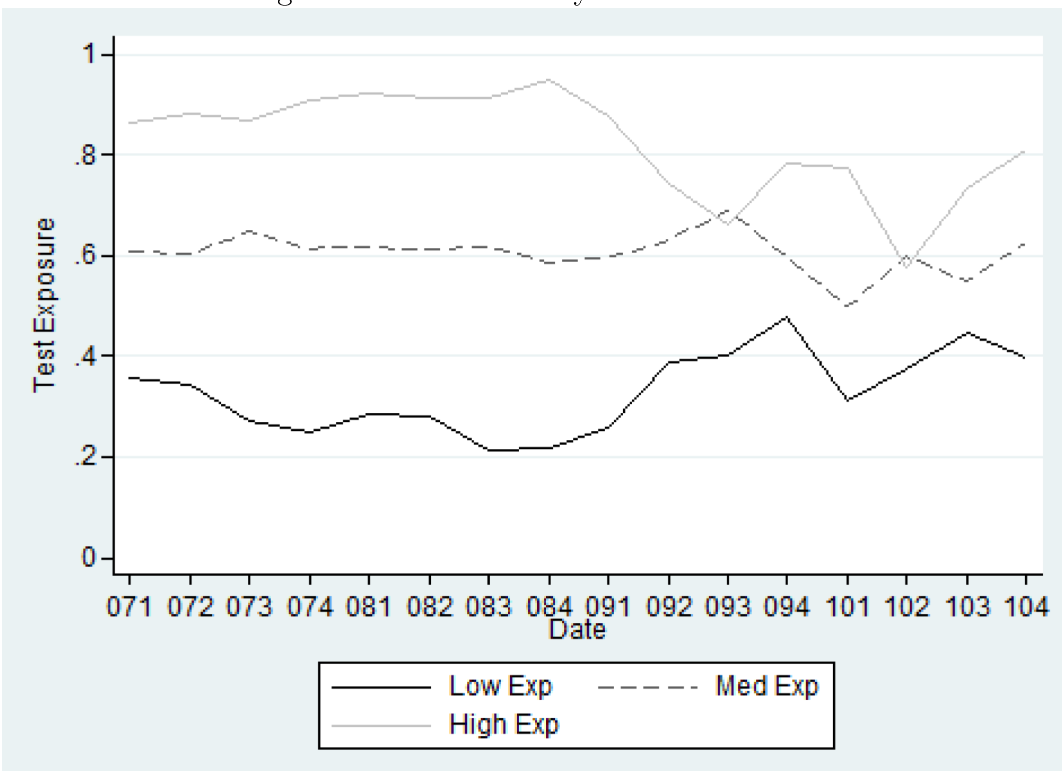
Explanatory Variables	Dependent Variables (3-Year Growth Rates): Fixed Invest (1)	Int Invest (2)	Assets (3)	Emp (4)	Fixed Invest (5)	Int Invest (6)	Assets (7)	Emp (8)
Test Exp	-0.01	0.02	0.01	0.00	-0.01	0.01	0.01	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)
× Rated					-0.02	-0.01	-0.05	-0.07
					(0.02)	(0.03)	(0.04)	(0.03)
× Rated × Inv Grade					0.03	0.04	0.06	0.07
					(0.02)	(0.04)	(0.04)	(0.03)
E[Dep Var | High Test Exp]								
Spec Grade Firms					-0.03*	0.01	-0.03	-0.07**
					(0.01)	(0.03)	(0.04)	(0.03)
Invest Grade Firms					0.00	0.05***	0.03	0.01
					(0.01)	(0.01)	(0.03)	(0.04)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.54	0.37	0.24	0.21	0.55	0.38	0.25	0.23
Number of Firms	753	687	753	719	753	687	753	719
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm real outcomes on firm and deal characteristics. The dependent variable in columns 1 and 5 is firm f's 3-year investment rate, calculated as capital expenditures from 2009-2011 divided by total assets in 2008. The dependent variable in columns 2 and 6 is firm f's 3-year investment rate, calculated as capital expenditures from 2009-2011 divided by total assets in 2008. The dependent variables in columns 3 and 7 are firm f's harmonized 3-year growth rate (from 2009-2011 relative to 2008) of assets. The dependent variables in columns 4 and 8 are firm f's harmonized 3-year growth rate (from 2009-2011 relative to 2008) of employment. Test Exp equals firm f's exposure to tested banks in its last pre-test syndicate (weighted by loan shares). Firm controls include dummies for 2-digit SIC code, state, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, number of lead arrangers and lenders in the last pre-test syndicate, the share of previous lead arrangers and lenders in the last pre-test syndicate, whether lenders had a long relationship with the firm (¿ 5 years), and whether the firm borrowed from the syndicated loan market for the first time. Bank controls include standardized values of the largest lead arranger's lagged log of bank assets, equity capital/assets, securities/assets, deposits and short-term funds/assets, return on assets, non-performing loans/gross loans, bank health (lending done in the post-test period to all firms other than f divided by lending done in the pre-test period to all firms other than f), and the weighted average of the size of Capital Purchase Program investments scaled by total assets in 2008Q4 (for U.S. banks). Bank-firm controls include interactions of all bank characteristics with dummies for whether a firm was rated before the SCAP and whether a rated firm held an investment grade credit rating. Standard errors given below coefficient estimates are clustered at the level of the largest lead arranger in each pre-test syndicate. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

1.10 Appendix

1.10.1 *Figures*

Figure A.1: Trends in Syndicate Structure



This figure shows trends of syndicate structure across terciles of pre-test firm exposure to SCAP banks between 2007Q1 and 2010Q4.

1.10.2 *Tables*

Table A.1: Censored Regression of Bank Allocation Shares

Regressors	Dependent Variable: Actual Bank Allocation Share (1)
log of Loan Size	-4.33***
	(0.05)
Loan Maturity	-0.46***
	(0.00)
Lead Arranger (D)	11.30***
	(0.14)
Co-Agent (D)	2.71***
	(0.07)
Number of Lenders	-0.13***
	(0.00)
Constant	97.91***
	(1.06)
Origination Year Controls	Y
Bank-Firms	56,490
R-squared	0.48

This table provides estimates from censored regressions of actual loan allocations on facility-level characteristics for all loans used for either corporate or working capital purposes extended to U.S. non-financial firms between 2004 and 2016.

Table A.2: Firm Balancing on Observable Characteristics Split by Test Exposure

	Percentile of Test Exposure					Percentile of Test Exposure				
	1	2	3	4		1	2	3	4	
	mean	mean	mean	mean	std dev	mean	mean	mean	mean	std dev
	All Firms					Unrated Firms				
Revolver Share	0.74	0.83	0.86	0.86	0.34	0.78	0.87	0.90	1.00	0.29
Term Loan A Share	0.05	0.04	0.03	0.01	0.15	0.05	0.04	0.02	0.00	0.15
Term Loan B Share	0.21	0.13	0.11	0.13	0.31	0.17	0.09	0.08	0.00	0.26
Number of Covenants	1.08	1.00	0.97	0.73	1.19	1.08	1.25	0.75	4.00	1.27
log of Firm Assets	20.94	21.64	21.43	20.03	1.79	19.69	20.33	19.69	18.48	1.24
BV of Leverage	1.97	2.65	2.20	1.74	4.28	1.39	2.33	1.30	0.70	3.75
Cash/Assets	0.12	0.07	0.09	0.14	0.15	0.18	0.08	0.15	0.01	0.18
Tangible Assets/Assets	0.46	0.43	0.45	0.41	0.26	0.42	0.38	0.40	0.32	0.27
Profits/Assets	0.10	0.12	0.14	0.13	0.11	0.07	0.14	0.13	0.18	0.13
Corporate Purpose Share	0.57	0.60	0.65	0.51	0.49	0.45	0.48	0.46	0.00	0.50
Maturity	48.51	50.10	51.79	44.69	20.27	46.17	51.24	44.77	36.00	19.78
Collateralized Share	0.64	0.44	0.37	0.59	0.50	0.74	0.53	0.57	1.00	0.49
Previous Lead Share	0.53	0.41	0.45	0.79	0.29	0.61	0.38	0.70	0.43	0.32
Previous Lender Share	0.56	0.76	0.77	0.60	0.38	0.42	0.67	0.55	0.43	0.42
Long Relationship Share	0.30	0.46	0.49	0.33	0.38	0.19	0.29	0.29	0.43	0.36
	Speculative Grade Firms					Investment Grade Firms				
Revolver Share	0.67	0.66	0.78	0.59	0.41	0.76	0.92	0.90	0.97	0.30
Term Loan A Share	0.03	0.02	0.04	0.04	0.14	0.06	0.02	0.02	0.00	0.15
Term Loan B Share	0.30	0.32	0.18	0.37	0.40	0.18	0.06	0.08	0.03	0.27
Number of Covenants	1.34	1.12	1.22	1.19	1.26	0.81	0.66	0.63	0.48	0.82
log of Firm Assets	21.69	21.31	21.50	21.23	1.10	23.18	23.26	22.93	22.75	1.28
BV of Leverage	3.21	3.11	2.35	4.65	6.21	1.86	2.60	2.39	1.79	2.32
Cash/Assets	0.05	0.06	0.07	0.09	0.09	0.06	0.07	0.08	0.06	0.08
Tangible Assets/Assets	0.56	0.51	0.44	0.45	0.25	0.43	0.49	0.43	0.54	0.23
Profits/Assets	0.13	0.11	0.11	0.11	0.09	0.15	0.14	0.15	0.16	0.06
Corporate Purpose Share	0.60	0.63	0.65	0.74	0.47	0.79	0.70	0.79	0.76	0.43
Maturity	57.42	57.86	53.18	56.54	17.57	43.93	42.88	47.68	48.50	21.98
Collateralized Share	0.86	0.77	0.62	0.67	0.44	0.12	0.12	0.08	0.03	0.29
Previous Lead Share	0.45	0.44	0.47	0.66	0.27	0.39	0.41	0.50	0.56	0.21
Previous Lender Share	0.67	0.76	0.81	0.82	0.28	0.75	0.85	0.90	0.88	0.27
Long Relationship Share	0.32	0.41	0.50	0.42	0.34	0.58	0.66	0.72	0.65	0.32
	Small Firms					Large Firms				
Revolver Share	0.77	0.88	0.91	1.00	0.28	0.71	0.82	0.86	0.82	0.36
Term Loan A Share	0.04	0.03	0.00	0.00	0.11	0.05	0.04	0.03	0.03	0.16
Term Loan B Share	0.19	0.09	0.09	0.00	0.26	0.24	0.14	0.11	0.15	0.33
Number of Covenants	0.91	1.16	0.71	4.00	1.29	1.18	0.89	1.09	0.76	1.13
log of Firm Assets	18.97	19.49	19.01	18.48	0.84	22.09	22.26	21.90	21.58	1.34
BV of Leverage	1.51	1.88	1.62	0.70	4.82	2.17	3.04	2.48	1.85	3.96
Cash/Assets	0.22	0.08	0.18	0.01	0.21	0.08	0.06	0.08	0.09	0.09
Tangible Assets/Assets	0.38	0.42	0.38	0.32	0.27	0.49	0.45	0.43	0.49	0.25
Profits/Assets	0.05	0.12	0.12	0.18	0.14	0.13	0.12	0.14	0.14	0.09
Corporate Purpose Share	0.41	0.44	0.39	0.00	0.49	0.65	0.65	0.70	0.69	0.47
Maturity	44.16	48.90	42.74	36.00	20.23	52.37	49.51	51.10	50.46	19.98
Collateralized Share	0.88	0.68	0.69	1.00	0.44	0.55	0.34	0.34	0.36	0.49
Previous Lead Share	0.76	0.42	0.81	0.43	0.31	0.41	0.41	0.45	0.61	0.25
Previous Lender Share	0.32	0.65	0.50	0.43	0.44	0.68	0.80	0.80	0.78	0.31
Long Relationship Share	0.18	0.25	0.27	0.43	0.38	0.38	0.52	0.54	0.46	0.36

This table provides estimates of averages of firm characteristics split by quartiles of test exposure. The last column is the standard deviation of each variable.

Table A.3: Firm Balancing on Observable Characteristics Split by Gap Exposure

	Percentile of Test Exposure					Percentile of Test Exposure				
	1	2	3	4		1	2	3	4	
	mean	mean	mean	mean	std dev	mean	mean	mean	mean	std dev
	All Firms					Unrated Firms				
Revolver Share	0.77	0.81	0.85	0.86	0.34	0.80	0.85	0.90	0.90	0.29
Term Loan A Share	0.04	0.04	0.02	0.02	0.15	0.04	0.07	0.02	0.01	0.15
Term Loan B Share	0.20	0.14	0.12	0.12	0.31	0.16	0.08	0.08	0.09	0.26
Number of Covenants	0.97	1.08	0.97	0.78	1.19	0.85	1.41	1.03	0.56	1.27
log of Firm Assets	20.78	21.85	21.42	19.99	1.79	19.52	20.42	20.26	19.18	1.24
BV of Leverage	1.91	2.34	2.49	1.83	4.28	1.32	2.37	1.38	1.23	3.75
Cash/Assets	0.13	0.07	0.08	0.14	0.15	0.18	0.10	0.10	0.18	0.18
Tangible Assets/Assets	0.45	0.46	0.44	0.41	0.26	0.41	0.38	0.43	0.39	0.27
Profits/Assets	0.10	0.13	0.14	0.12	0.11	0.07	0.14	0.16	0.10	0.13
Corporate Purpose Share	0.56	0.63	0.61	0.52	0.49	0.44	0.46	0.55	0.41	0.50
Maturity	46.37	50.73	51.39	46.62	20.27	43.38	51.91	51.99	39.54	19.78
Collateralized Share	0.64	0.40	0.41	0.59	0.50	0.75	0.54	0.41	0.72	0.49
Previous Lead Share	0.59	0.41	0.47	0.71	0.29	0.73	0.35	0.45	0.88	0.32
Previous Lender Share	0.55	0.76	0.76	0.61	0.38	0.42	0.63	0.69	0.45	0.42
Long Relationship Share	0.31	0.48	0.47	0.33	0.38	0.22	0.25	0.34	0.26	0.36
	Speculative Grade Firms					Investment Grade Firms				
Revolver Share	0.66	0.60	0.74	0.69	0.41	0.79	0.90	0.95	0.91	0.30
Term Loan A Share	0.03	0.05	0.01	0.04	0.14	0.03	0.03	0.02	0.02	0.15
Term Loan B Share	0.32	0.35	0.25	0.26	0.40	0.18	0.07	0.04	0.07	0.27
Number of Covenants	1.29	1.14	1.28	1.18	1.26	0.76	0.72	0.59	0.50	0.82
log of Firm Assets	21.57	21.60	21.61	20.96	1.10	23.06	23.48	22.98	22.60	1.28
BV of Leverage	3.31	2.94	2.83	4.23	6.21	2.07	1.97	2.20	2.41	2.32
Cash/Assets	0.06	0.06	0.07	0.09	0.09	0.07	0.05	0.06	0.08	0.08
Tangible Assets/Assets	0.55	0.48	0.49	0.43	0.25	0.44	0.48	0.46	0.50	0.23
Profits/Assets	0.13	0.12	0.09	0.12	0.09	0.14	0.14	0.15	0.15	0.06
Corporate Purpose Share	0.67	0.66	0.57	0.72	0.47	0.79	0.72	0.80	0.73	0.43
Maturity	55.02	57.96	57.46	54.52	17.57	43.00	41.98	50.10	47.89	21.98
Collateralized Share	0.81	0.79	0.68	0.65	0.44	0.12	0.05	0.12	0.07	0.29
Previous Lead Share	0.47	0.44	0.50	0.61	0.27	0.41	0.41	0.50	0.53	0.21
Previous Lender Share	0.71	0.75	0.80	0.81	0.28	0.71	0.90	0.93	0.84	0.27
Long Relationship Share	0.31	0.42	0.45	0.48	0.34	0.55	0.72	0.73	0.61	0.32
	Small Firms					Large Firms				
Revolver Share	0.81	0.86	0.89	1.00	0.28	0.72	0.81	0.85	0.84	0.36
Term Loan A Share	0.01	0.06	0.01	0.00	0.11	0.05	0.05	0.02	0.03	0.16
Term Loan B Share	0.17	0.09	0.10	0.00	0.26	0.24	0.14	0.13	0.13	0.33
Number of Covenants	0.69	1.36	0.72	4.00	1.29	1.08	0.99	1.06	0.79	1.13
log of Firm Assets	18.85	19.55	19.04	18.48	0.84	22.15	22.28	21.93	21.47	1.34
BV of Leverage	1.38	1.83	1.71	0.70	4.82	2.35	2.34	2.61	2.25	3.96
Cash/Assets	0.23	0.10	0.16	0.01	0.21	0.08	0.06	0.07	0.10	0.09
Tangible Assets/Assets	0.38	0.40	0.39	0.32	0.27	0.49	0.47	0.44	0.46	0.25
Profits/Assets	0.05	0.13	0.11	0.18	0.14	0.13	0.14	0.13	0.14	0.09
Corporate Purpose Share	0.40	0.41	0.42	0.00	0.49	0.67	0.67	0.64	0.70	0.47
Maturity	37.79	51.20	44.80	36.00	20.23	50.13	50.56	52.91	49.84	19.98
Collateralized Share	0.86	0.69	0.69	1.00	0.44	0.48	0.35	0.39	0.37	0.49
Previous Lead Share	0.86	0.41	0.76	0.43	0.31	0.44	0.41	0.47	0.56	0.25
Previous Lender Share	0.34	0.57	0.53	0.43	0.44	0.70	0.79	0.81	0.75	0.31
Long Relationship Share	0.23	0.21	0.27	0.43	0.38	0.38	0.53	0.53	0.46	0.36

This table provides estimates of averages of firm characteristics split by quartiles of gap exposure. The last column is the standard deviation of each variable.

Table A.4: Trends in Firm Borrowing Outcomes 2006-2008

Explanatory Variables	Borrow = 1 (1) 2006	(2) 2007	(3) 2008	Equity Growth Rate (1-yr) (4) 2006	(5) 2007	(6) 2008	Debt Growth Rate (1-yr) (7) 2006	(8) 2007	(9) 2008
Test Exp $\uparrow 1\sigma$	-0.07***	-0.01	0.01	0.01	-0.02	-0.01	-0.02	-0.01	0.00
	(0.02)	(0.03)	(0.04)	(0.02)	(0.02)	(0.03)	(0.02)	(0.01)	(0.03)
R-squared	0.44	0.34	0.52	0.24	0.31	0.33	0.30	0.30	0.33
Number of Firms	534	601	617	534	599	598	534	608	617
Unrated									
Test Exp $\uparrow 1\sigma$	-0.08**	-0.07**	0.04	0.04	-0.01	-0.01	-0.01	0.00	-0.03
	(0.04)	(0.03)	(0.05)	(0.03)	(0.02)	(0.03)	(0.03)	(0.03)	(0.04)
Spec Grade									
Test Exp $\uparrow 1\sigma$	-0.07	0.05	-0.09	-0.01	-0.02	-0.01	-0.01	-0.04	0.03
	(0.07)	(0.06)	(0.05)	(0.03)	(0.05)	(0.05)	(0.03)	(0.03)	(0.04)
Inv Grade									
Test Exp $\uparrow 1\sigma$	-0.08**	0.07	0.01	-0.03	-0.03	0.03	-0.06*	0.01	0.06*
	(0.03)	(0.06)	(0.08)	(0.03)	(0.03)	(0.05)	(0.03)	(0.03)	(0.03)
R-squared	0.44	0.35	0.52	0.24	0.31	0.33	0.30	0.30	0.34
Number of Firms	534	601	617	534	599	598	534	608	617
Med/Large									
Test Exp $\uparrow 1\sigma$	-0.11***	0.02	0.00	-0.01	-0.03	0.00	-0.03	-0.03	0.03
	(0.02)	(0.04)	(0.05)	(0.02)	(0.05)	(0.04)	(0.02)	(0.02)	(0.03)
Small									
Test Exp $\uparrow 1\sigma$	-0.02	-0.04	0.02	0.04*	0.00	-0.02	0.00	0.01	-0.03
	(0.03)	(0.04)	(0.05)	(0.02)	(0.02)	(0.03)	(0.03)	(0.02)	(0.04)
R-squared	0.44	0.34	0.52	0.24	0.31	0.33	0.30	0.30	0.33
Number of Firms	534	601	617	534	599	598	534	608	617

This table provides estimates of the expected change in rm borrowing outcomes calculated from rm borrowing regressions (unreported). All regressions include firm and deal controls. Firm controls include dummies for 2-digit SIC code, state, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, the shares of revolving credit lines in the pre-test syndicate, previous lenders, lead arrangers, and whether lenders had a long relationship with the firm (¿ 5 years). Each estimate is the expected value of the change in the dependent variable going from a firm at the 10th percentile of test exposure to one at the 90th percentile of test exposure. Marginal effects and pseudo R-squared values are reported for columns 1 through 3. Borrow is a dummy variable equal to 1 if a firm either obtained a new loan or a positive modi cation of an existing loan in a given year. The harmonized growth rates of equity and debt are taken relative to 2004 values of each. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively

Table A.5: Trends in Firm Real Outcomes 2006-2008

	Borrow = 1			Equity Growth Rate (1-yr)			Debt Growth Rate (1-yr)		
Explanatory Variables	(1) 2006	(2) 2007	(3) 2008	(4) 2006	(5) 2007	(6) 2008	(7) 2006	(8) 2007	(9) 2008
Test Exp ↑ 1σ	-0.01	-0.01*	-0.01	-0.01	-0.03***	0.00	-0.01	-0.02	0.00
	(0.00)	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.02)
R-squared	0.62	0.53	0.55	0.33	0.36	0.4	0.31	0.29	0.35
Number of Firms	709	717	728	710	726	738	665	680	690
Unrated									
Test Exp ↑ 1σ	0.00	-0.01	0.00	0.00	-0.02	-0.02	0.00	-0.02	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)
Spec Grade									
Test Exp ↑ 1σ	-0.01	-0.02	-0.03	-0.01	-0.01	0.02	-0.02	-0.01	-0.02
	(0.01)	(0.01)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.02)	(0.01)
Inv Grade									
Test Exp ↑ 1σ	-0.01	-0.01	0.00	-0.04*	-0.01	0.00	-0.03*	-0.01	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.03)	(0.01)	(0.02)	(0.01)
R-squared	0.62	0.53	0.55	0.33	0.29	0.35	0.31	0.29	0.53
Number of Firms	709	717	728	710	680	690	665	680	717
Med/Large									
Test Exp ↑ 1σ	-0.01	-0.01	-0.01	-0.03	-0.05***	0.01	-0.02	-0.01	0.00
	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.02)
Small									
Test Exp ↑ 1σ	0.00	-0.01	0.00	0.01	0.00	0.00	0.00	-0.03	-0.01
	(0.01)	(0.01)	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)
R-squared	0.62	0.53	0.55	0.33	0.37	0.4	0.31	0.29	0.35
Number of Firms	709	717	728	710	726	738	665	680	690

This table provides estimates of the expected change in firm real outcomes calculated from firm outcome regressions (unreported). All regressions include firm and deal controls. Firm controls include dummies for 2-digit SIC code, state, bond market access, investment grade credit rating, and lagged values of log assets, book value of leverage, cash/assets, tangible assets/assets, and profits/assets. Syndicate controls include the average maturity, collateral, purpose, the shares of revolving credit lines in the pre-test syndicate, previous lenders, lead arrangers, and whether lenders had a long relationship with the firm (¿ 5 years). Each estimate is the expected value of the change in the dependent variable going from a firm at the 10th percentile of test exposure to one at the 90th percentile of test exposure. The investment rate is calculated as capital expenditures over a given year scaled by 2004 assets. The harmonized growth rates of assets, employment, and sales are taken relative to 2004 values of each. ***, **, and * denote significance at the 1, 5, and 10% levels, respectively.

Table A.6: Bank Lending: Matched Bank-Firm Regressions (SCAP: Testing)

	Dependent Variables:					
	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry	$\frac{\Delta \text{Loan Size}_{b,f}}{\text{Deal Amt}_f}$	Exit	Entry
Regressors:	(1)	(2)	(3)	(4)	(5)	(6)
Tested	0.01	-0.09*	-0.07*	-0.01	0.01	0.02
	(0.01)	(0.04)	(0.04)	(0.02)	(0.07)	(0.06)
× Rated				0.02	-0.20	-0.19
				(0.03)	(0.08)	(0.09)
× Rated × Inv Grade				0.01	0.06	0.08
				(0.02)	(0.08)	(0.06)
E[Dep Var | Tested = 1]						
Speculative Grade Firms				0.01	-0.19**	-0.17**
				(0.01)	(0.07)	(0.06)
Investment Grade Firms				0.01	-0.13***	-0.09*
				(0.01)	(0.04)	(0.05)
Firm Controls	Y	Y	Y	Y	Y	Y
Firm × Controls	Y	Y	Y	Y	Y	Y
Bank-Firms	2083	2131	2335	2083	2131	2335
R-squared	0.17	0.28	0.19	0.18	0.29	0.21

This table provides estimates from regressions of loan outcomes on firm, facility, and bank characteristics. The specifications are identical to those in Table 7 with the except of being estimated with only firm controls and not firm fixed effects.

Table A.7: Firm-Level Intensive Margin Regressions (SCAP: Gap)

	Dependent Variables:							
	Syndicate-Level				Facility-Level			
	Syndicate Growth	Covenant Increase	Syndicate Growth	Covenant Increase	Facility Growth	ΔSpread	Facility Growth	ΔSpread
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.04	0.02	-0.01	-0.01	0.03	7.98	0.11	-18.84
	(0.08)	(0.04)	(0.11)	(0.03)	(0.07)	(14.24)	(0.16)	(15.61)
× Rated			-0.19	0.11			-0.58	85.14
			(0.14)	(0.06)			(0.20)	(22.94)
× Rated × Inv Grade			0.31	-0.04			0.62	-29.16
			(0.16)	(0.10)			(0.11)	(28.15)
Test Exp × Gap Exp	-0.03	-0.01	-0.04	0.02	0.02	-2.77	-0.01	11.69
	(0.06)	(0.03)	(0.07)	(0.03)	(0.05)	(10.22)	(0.10)	(16.86)
× Rated			0.11	0.11			0.45	-96.59
			(0.10)	(0.06)			(0.18)	(21.88)
× Rated × Inv Grade			-0.17	-0.04			-0.45	33.52
			(0.13)	(0.10)			(0.18)	(25.31)
E[Dep Var | High Test and Gap Exp]								
All Firms	-0.07	0.01			0.06	5.21		
	(0.07)	(0.02)			(0.05)	(8.61)		
Unrated Firms			-0.05	0.00			0.10	-7.14
			(0.08)	(0.02)			(0.09)	(14.19)
Spec Grade Firms			-0.13*	0.08**			-0.04	-18.59
			(0.08)	(0.04)			(0.09)	(13.21)
Invest Grade Firms			0.01	-0.02			0.13	-14.23
			(0.17)	(0.05)			(0.11)	(10.78)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	Y	Y	N	N	Y	Y
R-squared	0.21	0.16	0.25	0.25	0.21	0.23	0.38	0.39
Number of Firms	298	330	298	330	233	250	233	250

This table provides estimates from regressions of firm-level borrowing outcomes on firm, facility, and bank characteristics. The specifications are identical to those in Table 9.

Table A.8: Firm-Level Intensive Margin Regressions (SCAP: Gap Size)

	Dependent Variables:							
	Syndicate-Level				Facility-Level			
	Syndicate Growth	Covenant Increase	Syndicate Growth	Covenant Increase	Facility Growth	ΔSpread	Facility Growth	ΔSpread
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.05	0.00	-0.04	-0.02	0.04	2.55	0.10	-15.72
	(0.09)	(0.03)	(0.13)	(0.03)	(0.09)	(11.25)	(0.16)	(15.78)
× Rated			-0.11	0.09			-0.46	55.07
			(0.17)	(0.07)			(0.22)	(18.57)
× Rated × Inv Grade			0.28	-0.04			0.52	-5.45
			(0.19)	(0.09)			(0.17)	(26.66)
Test Exp × Gap Exp	-0.02	0.02	0.00	0.03	0.02	5.05	-0.01	9.51
	(0.05)	(0.02)	(0.08)	(0.03)	(0.05)	(6.76)	(0.10)	(16.68)
× Rated			0.00	0.09			0.32	-64.16
			(0.13)	(0.07)			(0.27)	(20.76)
× Rated × Inv Grade			-0.15	-0.04			-0.39	-10.33
			(0.25)	(0.09)			(0.22)	(21.67)
E[Dep Var | High Test and Gap Exp]								
All Firms	-0.07	0.02			0.06	7.60		
	(0.06)	(0.02)			(0.04)	(8.63)		
Unrated Firms			-0.04	0.01			0.10	-6.21
			(0.07)	(0.02)			(0.07)	(15.45)
Spec Grade Firms			-0.15**	0.08**			-0.04	-15.31
			(0.08)	(0.04)			(0.12)	(14.81)
Invest Grade Firms			-0.02	-0.01			0.09	-31.08***
			(0.18)	(0.07)			(0.11)	(10.53)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	Y	Y	N	N	Y	Y
R-squared	0.21	0.16	0.25	0.25	0.21	0.23	0.37	0.38
Number of Firms	298	330	298	330	233	250	233	250

This table provides estimates from regressions of firm-level borrowing outcomes on firm, facility, and bank characteristics. The specifications are identical to those in Table 9.

Table A.9: Syndicate Structure Regression Results (SCAP: Existing Lenders)

	Dependent Variables: Change in Syndicate Shares Lenders Existing in Both Periods									
Explanatory Variables	Total (1)	Tested Bank (2)	Other US Bank (3)	Foreign Bank (4)	Non-Bank (5)	Total (6)	Tested Bank (7)	Other US Bank (8)	Foreign Bank (9)	Non-Bank (10)
Test Exp	-0.03	-0.06	-0.01	0.04	0.00	-0.01	-0.06	0.00	0.05	0.00
	(0.02)	(0.01)	(0.00)	(0.01)	(0.00)	(0.02)	(0.02)	(0.01)	(0.01)	(0.00)
× Rated						-0.02	0.00	0.00	-0.01	-0.01
						(0.05)	(0.04)	(0.01)	(0.03)	(0.00)
× Rated × Inv Grade						0.00	-0.03	0.01	0.00	0.01
						(0.09)	(0.07)	(0.01)	(0.04)	(0.00)
	E[Dep Var | High Test Exp]									
Spec Grade Firms						-0.02	-0.05*	0.00	0.04	-0.01*
						(0.04)	(0.03)	(0.00)	(0.02)	(0.00)
Invest Grade Firms						-0.02	-0.08	0.01	0.04	0.00
						(0.06)	(0.05)	(0.01)	(0.03)	(0.00)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y	Y	Y
R-squared	0.31	0.27	0.17	0.25	0.30	0.33	0.30	0.27	0.29	0.36
Number of Firms	331	331	331	331	331	331	331	331	331	331
Number of Clusters	24	24	24	24	24	24	24	24	24	24

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.10: Syndicate Structure Regression Results (SCAP: Entering - Exiting Lenders)

	Dependent Variables: Change in Syndicate Shares Entering Lenders - Exiting Lenders									
	Total	Tested	Other US	Foreign	Non-	Total	Tested	Other US	Foreign	Non-
Explanatory		Bank	Bank	Bank	Bank		Bank	Bank	Bank	Bank
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Test Exp	0.03	-0.09	0.03	0.06	0.02	0.01	-0.07	0.04	0.04	0.01
	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.04)	(0.02)	(0.04)	(0.01)
× Rated						0.02	-0.03	0.03	0.01	0.01
						(0.05)	(0.05)	(0.02)	(0.04)	(0.02)
× Rated						0.00	-0.03	-0.10	0.11	0.01
× Inv Grade						(0.09)	(0.05)	(0.02)	(0.05)	(0.03)
E[Dep Var | High Test Exp]										
Spec Grade Firms						0.02	-0.10***	0.06***	0.04	0.02
						(0.04)	(0.04)	(0.01)	(0.03)	(0.02)
Invest Grade Firms						0.02	-0.13***	-0.03*	0.16***	0.03
						(0.06)	(0.03)	(0.02)	(0.03)	(0.02)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y	Y	Y
R-squared	0.31	0.25	0.27	0.24	0.25	0.33	0.28	0.32	0.31	0.33
Number of Firms	331	331	331	331	331	331	331	331	331	331
Number of Clusters	24	24	24	24	24	24	24	24	24	24

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.11: Syndicate Structure Regression Results (SCAP: Gap)

	Dependent Variables: Change in Syndicate Shares							
	Tested Bank	Other US Bank	Foreign Bank	Non-Bank	Tested Bank	Other US Bank	Foreign Bank	Non-Bank
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.15	0.02	0.10	0.03	-0.13	0.02	0.10	0.01
	(0.03)	(0.01)	(0.02)	(0.02)	(0.03)	(0.02)	(0.03)	(0.01)
× Rated					-0.04	0.05	-0.05	0.04
					(0.06)	(0.02)	(0.04)	(0.03)
× Rated × Inv Grade					-0.02	-0.09	0.15	-0.04
					(0.05)	(0.02)	(0.04)	(0.04)
Test Exp × Gap Exp	0.00	0.01	0.00	-0.01	-0.01	0.02	-0.01	0.00
	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
× Rated					0.02	-0.04	0.06	-0.05
					(0.05)	(0.02)	(0.05)	(0.03)
× Rated × Inv Grade					-0.06	0.01	-0.04	0.09
					(0.06)	(0.02)	(0.03)	(0.05)
	E[Dep Var | High Test Exp and Gap Exp]							
All Firms	-0.15***	0.03*	0.11***	0.02				
	(0.02)	(0.01)	(0.02)	(0.02)				
Unrated Firms					-0.13***	0.04*	0.09**	0.01
					(0.03)	(0.02)	(0.04)	(0.01)
Spec Grade Firms					-0.15***	0.05***	0.10***	0.00
					(0.02)	(0.01)	(0.02)	(0.02)
Invest Grade Firms					-0.22***	-0.03	0.20***	0.05
					(0.05)	(0.02)	(0.02)	(0.04)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.31	0.25	0.27	0.24	0.33	0.28	0.32	0.31
Number of Firms	331	331	331	331	331	331	331	331
Number of Clusters	24	24	24	24	24	24	24	24

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.12: Syndicate Structure Regression Results (SCAP: Gap Size)

	Dependent Variables: Change in Syndicate Shares							
	Tested Bank	Other US Bank	Foreign Bank	Non-Bank	Tested Bank	Other US Bank	Foreign Bank	Non-Bank
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.15	0.02	0.10	0.02	-0.13	0.03	0.10	0.01
	(0.03)	(0.01)	(0.02)	(0.01)	(0.04)	(0.02)	(0.03)	(0.01)
× Rated					-0.02	0.04	-0.04	0.01
					(0.05)	(0.02)	(0.04)	(0.03)
× Rated × Inv Grade					-0.04	-0.10	0.15	-0.01
					(0.05)	(0.02)	(0.04)	(0.04)
Test Exp	0.00	0.00	0.00	0.00	0.00	0.01	-0.01	0.00
× Gap Size Exp	(0.02)	(0.01)	(0.02)	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)
× Rated					-0.01	-0.03	0.05	-0.02
					(0.04)	(0.02)	(0.03)	(0.03)
× Rated × Inv Grade					-0.03	0.03	-0.06	0.06
					(0.06)	(0.02)	(0.04)	(0.06)
E[Dep Var | High Test Exp and Gap Size Exp]								
All Firms	-0.15***	0.02	0.10***	0.02				
	(0.02)	(0.01)	(0.02)	(0.02)				
Unrated Firms					-0.13***	0.04	0.09**	0.01
					(0.03)	(0.02)	(0.04)	(0.01)
Spec Grade Firms					-0.15***	0.05***	0.10***	0.01
					(0.02)	(0.01)	(0.02)	(0.02)
Invest Grade Firms					-0.22***	-0.02	0.19***	0.05
					(0.06)	(0.02)	(0.02)	(0.05)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.36	0.26	0.26	0.25	0.41	0.31	0.35	0.34
Number of Firms	331	331	331	331	331	331	331	331
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.13: Total Borrowing Regression Results (SCAP: Gap)

	Dependent Variables:									
	Borrow	New Loan	Modify	Equity Growth	Debt Growth	Borrow	New Loan	Modify	Equity Growth	Debt Growth
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Test Exp	-0.03	-0.01	-0.02	-0.02	-0.02	-0.07	-0.04	-0.01	-0.01	-0.02
	(0.02)	(0.02)	(0.02)	(0.02)	(0.02)	(0.04)	(0.04)	(0.04)	(0.02)	(0.02)
× Rated						0.01	0.01	-0.08	-0.11	-0.06
						(0.09)	(0.09)	(0.06)	(0.07)	(0.07)
× Rated						0.27	0.22	0.17	0.12	0.07
× Inv Grade						(0.09)	(0.09)	(0.08)	(0.06)	(0.09)
Test Exp × Gap Exp	0.05	0.03	0.02	0.01	0.05	0.10	0.07	0.01	0.02	0.07
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)
× Rated						-0.08	-0.04	-0.01	0.06	0.03
						(0.06)	(0.07)	(0.05)	(0.07)	(0.06)
× Rated						-0.21	-0.22	-0.06	-0.09	-0.06
× Inv Grade						(0.11)	(0.10)	(0.07)	(0.09)	(0.07)
E[Dep Var | High Test Exp and Gap Exp]										
All Firms	0.03	0.03	0.00	-0.01	0.03*					
	(0.03)	(0.03)	(0.03)	(0.02)	(0.02)					
Unrated						0.03	0.03	0.00	0.00	0.04*
						(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
Spec Grade Firms						-0.04	0.00	-0.08**	-0.05	0.02
						(0.05)	(0.05)	(0.03)	(0.06)	(0.04)
Invest Grade Firms						0.01	0.00	0.03	-0.02	0.03
						(0.05)	(0.05)	(0.04)	(0.03)	(0.05)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.23	0.25	0.26	0.21	0.20	0.26	0.29	0.29	0.24	0.22
Number of Firms	751	747	716	668	691	751	747	716	668	691
Number of Clusters	30	30	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm borrowing outcomes on firm and deal characteristics. The specifications are identical to those in Table 11.

Table A.14: Total Borrowing Regression Results (SCAP: Gap Size)

	Dependent Variables:									
	Borrow	New Loan	Modify	Equity Growth	Debt Growth	Borrow	New Loan	Modify	Equity Growth	Debt Growth
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Test Exp	-0.02	0.00	-0.01	-0.02	0.00	-0.06	-0.02	-0.02	-0.01	-0.01
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
× Rated						0.05	0.03	-0.03	-0.09	-0.04
						(0.11)	(0.10)	(0.07)	(0.06)	(0.05)
× Rated						0.15	0.11	0.11	0.07	0.04
× Inv Grade						(0.09)	(0.09)	(0.10)	(0.06)	(0.07)
Test Exp × Gap	0.04	0.02	0.01	0.02	0.04	0.09	0.05	0.02	0.02	0.05
Size Exp	(0.02)	(0.02)	(0.01)	(0.03)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)
× Rated						-0.14	-0.07	-0.08	0.03	0.01
						(0.08)	(0.09)	(0.05)	(0.07)	(0.04)
× Rated						-0.03	-0.05	0.04	0.02	-0.02
× Inv Grade						(0.11)	(0.11)	(0.13)	(0.08)	(0.05)
E[Dep Var | High Test Exp and Gap Size Exp]										
All Firms	0.03	0.02	0.00	0.00	0.03					
	(0.03)	(0.03)	(0.02)	(0.02)	(0.02)					
Unrated						0.03	0.02	0.00	0.00	0.04
						(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
Spec Grade Firms						-0.06	-0.02	-0.10***	-0.05	0.01
						(0.04)	(0.04)	(0.03)	(0.06)	(0.05)
Invest Grade Firms						0.05	0.04	0.05	0.03	0.04
						(0.06)	(0.05)	(0.07)	(0.03)	(0.04)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.23	0.25	0.26	0.21	0.20	0.26	0.29	0.29	0.25	0.22
Number of Firms	751	747	716	668	691	751	747	716	668	691
Number of Clusters	30	30	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm borrowing outcomes on firm and deal characteristics. The specifications are identical to those in Table 11.

Table A.15: Firm Real Outcomes (SCAP: Gap)

Explanatory Variables	Dependent Variables (3-Year Growth Rates): Fixed Invest (1)	Int Invest (2)	Assets (3)	Emp (4)	Fixed Invest (5)	Int Invest (6)	Assets (7)	Emp (8)
Test Exp	-0.02	0.00	-0.01	-0.02	-0.02	-0.01	-0.01	-0.04
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
× Rated					0.00	-0.04	-0.07	0.00
					(0.02)	(0.05)	(0.07)	(0.07)
× Rated × Inv Grade					0.02	0.10	0.10	0.01
					(0.02)	(0.06)	(0.08)	(0.05)
Test Exp × Gap Exp	0.01	0.03	0.04	0.03	0.02	0.04	0.06	0.06
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.02)	(0.02)	(0.01)
× Rated					-0.03	0.05	0.04	-0.08
					(0.02)	(0.04)	(0.06)	(0.05)
× Rated × Inv Grade					0.01	-0.07	-0.07	0.08
					(0.02)	(0.05)	(0.08)	(0.09)
	E[Dep Var | High Test Exp and Gap Exp]							
All Firms	-0.01	0.03	0.03	0.01				
	(0.01)	(0.02)	(0.02)	(0.02)				
Unrated Firms					0.00	0.02	0.04**	0.02
					(0.01)	(0.02)	(0.02)	(0.02)
Spec Grade Firms					-0.03**	0.03	0.00	-0.06**
					(0.02)	(0.03)	(0.04)	(0.03)
Invest Grade Firms					0.00	0.05**	0.04	0.03
					(0.02)	(0.02)	(0.04)	(0.06)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.54	0.37	0.25	0.17	0.55	0.39	0.27	0.19
Number of Firms	753	687	743	715	753	687	743	715
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm real outcomes on firm and deal characteristics. The specifications are identical to those in Table 12.

Table A.16: Firm Real Outcomes (SCAP: Gap Size)

Explanatory Variables	Fixed Invest (1)	Int Invest (2)	Assets (3)	Emp (4)	Fixed Invest (5)	Int Invest (6)	Assets (7)	Emp (8)
	Dependent Variables (3-Year Growth Rates):							
Test Exp	-0.02	0.01	0.00	-0.01	-0.02	0.00	-0.01	-0.03
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
× Rated					0.00	0.02	-0.04	0.00
					(0.02)	(0.04)	(0.05)	(0.06)
× Rated × Inv Grade					0.02	0.04	0.05	-0.01
					(0.03)	(0.05)	(0.06)	(0.04)
Test Exp × Gap Size Exp	0.01	0.01	0.04	0.03	0.02	0.03	0.05	0.05
	(0.01)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)	(0.02)	(0.01)
× Rated					-0.03	-0.04	-0.01	-0.09
					(0.02)	(0.03)	(0.05)	(0.04)
× Rated × Inv Grade					0.02	0.00	0.01	0.13
					(0.02)	(0.03)	(0.06)	(0.07)
	E[Dep Var | High Test Exp and Gap Size Exp]							
All Firms	0.00	0.02	0.04*	0.02				
	(0.01)	(0.02)	(0.02)	(0.02)				
Unrated Firms					0.00	0.02	0.04**	0.02
					(0.01)	(0.02)	(0.02)	(0.01)
Spec Grade Firms					-0.03**	0.00	0.00	-0.07***
					(0.02)	(0.03)	(0.04)	0.00
Invest Grade Firms					0.00	0.05*	0.04	0.03
					(0.02)	(0.02)	(0.04)	(0.06)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.54	0.37	0.25	0.17	0.55	0.39	0.27	0.19
Number of Firms	753	687	743	715	753	687	743	715
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm real outcomes on firm and deal characteristics. The specifications are identical to those in Table 12.

Table A.17: Firm-Level Intensive Margin Regressions (SCAP: First Time Borrowers)

	Dependent Variables:							
	Syndicate-Level				Facility-Level			
	Syndicate Growth	Covenant Increase	Syndicate Growth	Covenant Increase	Facility Growth	ΔSpread	Facility Growth	ΔSpread
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.03	0	-0.01	-0.01	0.07	2.68	0.11	-14.56
	(0.08)	(0.03)	(0.08)	(0.03)	(0.07)	(11.15)	(0.09)	(14.55)
× Rated			-0.11	0.09			-0.2	0.54
			(0.11)	(0.04)			(0.13)	(15.56)
× Rated × Inv Grade			0.22	-0.07			0.28	1.05
			(0.13)	(0.06)			(0.08)	(21.79)
Test Exp × First Time	-0.08	0.02	-0.09	0.02	-0.05	7.48	-0.03	17.45
	(0.08)	(0.08)	(0.08)	(0.07)	(0.11)	(13.52)	(0.09)	(17.24)
× Rated			0.05	0.09			-0.04	-1.57
			(0.08)	(0.04)			(0.09)	(15.29)
× Rated × Inv Grade			-0.07	-0.07			-0.05	47.3
			(0.10)	(0.06)			(0.13)	(32.97)
	E[Dep Var | High Test Exp and First Time]							
All Firms	-0.10	0.02			0.02	10.17		
	(0.08)	(0.06)			(0.09)	(11.55)		
Unrated Firms			-0.1	0.02			0.07	2.89
			(0.11)	(0.05)			(0.12)	(18.79)
Spec Grade Firms			-0.16*	0.03			-0.17	1.85
			(0.09)	(0.06)			(0.13)	(15.76)
Invest Grade Firms			-0.02	-0.03			0.06	50.21*
			(0.17)	(0.10)			(0.16)	(25.71)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	Y	Y	N	N	Y	Y
R-squared	0.21	0.16	0.25	0.25	0.21	0.23	0.35	0.38
Number of Firms	298	330	298	330	233	250	233	250

This table provides estimates from regressions of firm-level borrowing outcomes on firm, facility, and bank characteristics. The specifications are identical to those in Table 9.

Table A.18: Firm-Level Intensive Margin Regressions (SCAP: Long Relationship)

	Dependent Variables:							
	Syndicate-Level				Facility-Level			
	Syndicate Growth	Covenant Increase	Syndicate Growth	Covenant Increase	Facility Growth	ΔSpread	Facility Growth	ΔSpread
Explanatory Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.06	0.01	-0.04	-0.01	0.1	12.35	0.12	2.87
	(0.08)	(0.03)	(0.10)	(0.02)	(0.06)	(9.26)	(0.09)	(14.33)
× Rated			-0.18	0.03			-0.31	15.62
			(0.07)	(0.03)			(0.10)	(18.54)
× Rated × Inv Grade			0.15	0.03			0.29	-6.59
			(0.19)	(0.06)			(0.22)	(34.27)
Test Exp ×	0.01	0.01	-0.04	0	-0.14	-16.62	-0.09	-29.22
Long Relationship	(0.07)	(0.05)	(0.11)	(0.06)	(0.08)	(13.83)	(0.09)	(16.16)
× Rated			0.22	0.03			0.22	-10.57
			(0.10)	(0.03)			(0.07)	(20.44)
× Rated × Inv Grade			-0.05	0.03			-0.1	25.11
			(0.10)	(0.06)			(0.27)	(28.94)
	E[Dep Var | High Test Exp and Long Relationship]							
All Firms	-0.05	0.02			-0.03	-4.27		
	(0.07)	(0.04)			(0.07)	(14.02)		
Unrated Firms			-0.07	-0.01			0.04	-26.35
			(0.08)	(0.05)			(0.12)	(18.37)
Spec Grade Firms			-0.04	0.12***			-0.05	-21.31
			(0.10)	(0.03)			(0.08)	(20.21)
Invest Grade Firms			0.07	0.01			0.14	-2.79
			(0.10)	(0.06)			(0.12)	(20.68)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	Y	Y	N	N	Y	Y
R-squared	0.21	0.16	0.25	0.25	0.22	0.23	0.37	0.37
Number of Firms	298	330	298	330	233	250	233	250

This table provides estimates from regressions of firm-level borrowing outcomes on firm, facility, and bank characteristics. The specifications are identical to those in Table 9.

Table A.19: Syndicate Structure Regression Results (SCAP: First Time Borrowers)

Explanatory Variables	Tested Bank (1)	Other US Bank (2)	Foreign Bank (3)	Non-Bank (4)	Tested Bank (5)	Other US Bank (6)	Foreign Bank (7)	Non-Bank (8)
	Dependent Variables: Change in Syndicate Shares							
Test Exp	-0.16	0.01	0.13	0.02	-0.14	0.02	0.11	0.01
	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)	(0.03)	(0.02)
× Rated					-0.04	0.02	0.01	0.00
					(0.03)	(0.01)	(0.03)	(0.02)
× Rated × Inv Grade					-0.04	-0.08	0.10	0.02
					(0.05)	(0.02)	(0.03)	(0.03)
Test Exp × First Time	0.02	0.04	-0.07	0.00	0.01	0.04	-0.05	-0.01
	(0.04)	(0.03)	(0.04)	(0.02)	(0.05)	(0.03)	(0.03)	(0.02)
× Rated					0.08	-0.01	-0.08	0.01
					(0.04)	(0.02)	(0.03)	(0.02)
× Rated × Inv Grade					-0.04	0.01	0.03	0.00
					(0.08)	(0.02)	(0.06)	(0.02)
E[Dep Var | High Test Exp and First Time Borrower]								
All Firms	-0.14***	0.05**	0.07**	0.02				
	(0.03)	(0.02)	(0.03)	(0.01)				
Unrated Firms					-0.13***	0.06***	0.06	0.00
					(0.05)	(0.02)	(0.04)	(0.01)
Spec Grade Firms					-0.09**	0.08*	0.00	0.01
					(0.04)	(0.04)	(0.04)	(0.03)
Invest Grade Firms					-0.16*	0.00	0.13**	0.03
					(0.08)	(0.02)	(0.05)	(0.03)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.36	0.27	0.27	0.25	0.42	0.31	0.36	0.34
Number of Firms	331	331	331	331	331	331	331	331
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.20: Syndicate Structure Regression Results (SCAP: Long Relationship)

	Dependent Variables: Change in Syndicate Shares							
Explanatory Variables	Tested Bank (1)	Other US Bank (2)	Foreign Bank (3)	Non-Bank (4)	Tested Bank (5)	Other US Bank (6)	Foreign Bank (7)	Non-Bank (8)
Test Exp	-0.14	0.03	0.09	0.02	-0.12	0.03	0.09	0.01
	(0.03)	(0.02)	(0.03)	(0.01)	(0.04)	(0.02)	(0.04)	(0.01)
× Rated					-0.03	0.05	-0.01	0.00
					(0.04)	(0.02)	(0.04)	(0.02)
× Rated × Inv Grade					-0.07	-0.09	0.15	0.01
					(0.05)	(0.03)	(0.06)	(0.03)
Test Exp ×	-0.03	-0.01	0.03	0.01	-0.02	0.00	0.01	0.01
Long Relationship	(0.04)	(0.04)	(0.04)	(0.02)	(0.05)	(0.04)	(0.03)	(0.02)
× Rated					0.03	-0.05	0.01	0.01
					(0.02)	(0.01)	(0.03)	(0.01)
× Rated × Inv Grade					0.01	0.03	-0.05	0.01
					(0.03)	(0.01)	(0.05)	(0.03)
	E[Dep Var | High Test Exp and Long Relationship]							
All Firms	-0.17***	0.02	0.12***	0.03				
	(0.03)	(0.03)	(0.03)	(0.03)				
Unrated Firms					-0.15***	0.04	0.09**	0.02
					(0.04)	(0.04)	(0.04)	(0.02)
Spec Grade Firms					-0.15***	0.03	0.09***	0.02
					(0.04)	(0.03)	(0.03)	(0.03)
Invest Grade Firms					-0.21***	-0.03	0.20***	0.04
					(0.05)	(0.03)	(0.03)	(0.03)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.36	0.26	0.26	0.25	0.41	0.31	0.35	0.34
Number of Firms	331	331	331	331	331	331	331	331
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of syndicate structure on firm and deal characteristics. The specifications are identical to those in Table 10.

Table A.21: Total Borrowing Regression Results (SCAP: First Time Borrower)

Explanatory Variables	Borrow (1)	New Loan (2)	Modify (3)	Equity Growth (4)	Debt Growth (5)	Borrow (6)	New Loan (7)	Modify (8)	Equity Growth (9)	Debt Growth (10)
	Dependent Variables:									
Test Exp	0.08	0.07	0.03	-0.02	0.00	0.08	0.07	0.04	-0.01	0.00
	(0.04)	(0.03)	(0.04)	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)	(0.03)	(0.03)
× Rated						-0.03	-0.01	-0.08	-0.07	-0.04
						(0.06)	(0.05)	(0.04)	(0.06)	(0.04)
× Rated						0.10	0.07	0.12	0.06	0.03
× Inv Grade						(0.04)	(0.04)	(0.06)	(0.05)	(0.06)
Test Exp	-0.17	-0.13	-0.09	0.02	0.05	-0.16	-0.13	-0.10	0.01	0.05
× First Time	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.04)	(0.05)	(0.04)	(0.04)
× Rated						-0.08	-0.06	0.00	0.00	0.03
						(0.04)	(0.04)	(0.03)	(0.06)	(0.04)
× Rated						0.08	0.05	0.06	0.02	-0.08
× Inv Grade						(0.07)	(0.06)	(0.04)	(0.06)	(0.03)
E[Dep Var | High Test Exp and First Time Borrower]										
All Firms	-0.08***	-0.06**	-0.06*	0.00	0.05					
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)					
Unrated						-0.08*	-0.06	-0.06	0.00	0.05**
						(0.05)	(0.05)	(0.04)	(0.03)	(0.02)
Spec Grade Firms						-0.19***	-0.13*	-0.14***	-0.06	0.04
						(0.07)	(0.07)	(0.05)	(0.06)	(0.04)
Invest Grade Firms						-0.01	0.00	0.05	0.01	0.00
						(0.05)	(0.05)	(0.07)	(0.03)	(0.06)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.24	0.26	0.26	0.21	0.20	0.27	0.29	0.29	0.24	0.22
Number of Firms	751	747	716	668	691	751	747	716	668	691
Number of Clusters	30	30	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm borrowing outcomes on firm and deal characteristics. The specifications are identical to those in Table 11.

Table A.22: Total Borrowing Regression Results (SCAP: Long Relationship)

Explanatory Variables	Borrow (1)	New Loan (2)	Modify (3)	Equity Growth (4)	Debt Growth (5)	Borrow (6)	New Loan (7)	Modify (8)	Equity Growth (9)	Debt Growth (10)
	Dependent Variables:									
Test Exp	-0.02	-0.01	-0.02	0.02	0.03	-0.01	-0.01	-0.01	0.01	0.03
	(0.03)	(0.03)	(0.03)	(0.03)	(0.02)	(0.05)	(0.04)	(0.04)	(0.03)	(0.02)
× Rated						-0.08	-0.04	-0.10	-0.05	-0.01
						(0.09)	(0.07)	(0.07)	(0.05)	(0.04)
× Rated						0.09	0.03	0.20	0.06	-0.04
× Inv Grade						(0.08)	(0.07)	(0.09)	(0.04)	(0.07)
Test Exp	0.07	0.07	0.03	-0.09	-0.03	0.05	0.05	0.04	-0.06	-0.02
× Long Relation	(0.04)	(0.05)	(0.04)	(0.04)	(0.04)	(0.06)	(0.07)	(0.04)	(0.05)	(0.04)
× Rated						0.06	0.04	0.01	-0.02	-0.03
						(0.03)	(0.03)	(0.04)	(0.04)	(0.03)
× Rated						0.05	0.06	-0.09	0.01	0.09
× Inv Grade						(0.06)	(0.06)	(0.08)	(0.04)	(0.03)
	E[Dep Var | High Test Exp and Long Relationship]									
All Firms	0.05	0.06	0.01	-0.07**	0.00					
	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)					
Unrated						0.04	0.05	0.03	-0.04	0.01
						(0.08)	(0.07)	(0.05)	(0.05)	(0.04)
Spec Grade Firms						0.02	0.05	-0.06	-0.12**	-0.03
						(0.03)	(0.04)	(0.04)	(0.05)	(0.06)
Invest Grade Firms						0.16***	0.14***	0.05	-0.04	0.02
						(0.03)	(0.03)	(0.05)	(0.04)	(0.02)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	N	Y	Y	Y	Y	Y
R-squared	0.23	0.25	0.26	0.22	0.19	0.26	0.28	0.29	0.25	0.22
Number of Firms	751	747	716	668	691	751	747	716	668	691
Number of Clusters	30	30	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm borrowing outcomes on firm and deal characteristics. The specifications are identical to those in Table 11.

Table A.23: Firm Real Outcomes (SCAP: First Time Borrowers)

Explanatory Variables	Fixed Invest (1)	Int Invest (2)	Assets (3)	Emp (4)	Fixed Invest (5)	Int Invest (6)	Assets (7)	Emp (8)
	Dependent Variables (3-Year Growth Rates):							
Test Exp	0.01	-0.01	0.01	0.01	0.01	-0.02	0.02	0.02
	(0.01)	(0.02)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.02)
× Rated					0.00	0.00	-0.04	-0.06
					(0.02)	(0.03)	(0.05)	(0.03)
× Rated × Inv Grade					0.02	0.05	0.05	0.05
					(0.02)	(0.04)	(0.04)	(0.03)
Test Exp	-0.04	0.06	0.01	-0.02	-0.03	0.07	0.01	-0.03
× First Time	(0.01)	(0.03)	(0.04)	(0.04)	(0.01)	(0.03)	(0.04)	(0.05)
× Rated					-0.06	0.01	0.00	-0.02
					(0.01)	(0.04)	(0.03)	(0.02)
× Rated × Inv Grade					0.02	-0.04	-0.04	0.03
					(0.01)	(0.03)	(0.02)	(0.05)
	E[Dep Var | High Test Exp and First Time Borrower]							
All Firms	-0.03**	0.05*	0.03	-0.01				
	(0.01)	(0.03)	(0.03)	(0.03)				
Unrated Firms					-0.02	0.05	0.03	-0.01
					(0.02)	(0.03)	(0.03)	(0.03)
Spec Grade Firms					-0.08***	0.06*	-0.01	-0.09*
					(0.02)	(0.03)	(0.04)	(0.05)
Invest Grade Firms					-0.05***	0.07**	0.01	0.00
					(0.02)	(0.03)	(0.04)	(0.08)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.54	0.38	0.24	0.16	0.56	0.39	0.26	0.18
Number of Firms	753	687	743	715	753	687	743	715
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm real outcomes on firm and deal characteristics. The specifications are identical to those in Table 12.

Table A.24: Firm Real Outcomes (SCAP: Long Relationship)

	Dependent Variables (3-Year Growth Rates):							
	Fixed	Int	Assets	Emp	Fixed	Int	Assets	Emp
Explanatory	Invest	Invest			Invest	Invest		
Variables	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Test Exp	-0.01	0.01	0.03	0.00	-0.01	0.01	0.03	0.00
	(0.01)	(0.02)	(0.02)	(0.02)	(0.02)	(0.03)	(0.02)	(0.02)
× Rated					-0.03	0.00	-0.02	-0.04
					(0.02)	(0.03)	(0.04)	(0.03)
× Rated × Inv Grade					0.02	0.01	0.00	0.02
					(0.02)	(0.04)	(0.04)	(0.03)
Test Exp × Gap Size Exp	0.02	0.02	-0.01	0.01	0.01	0.01	0.00	0.02
	(0.02)	(0.02)	(0.04)	(0.03)	(0.02)	(0.02)	(0.04)	(0.03)
× Rated					0.03	-0.01	-0.04	-0.06
					(0.01)	(0.02)	(0.02)	(0.02)
× Rated × Inv Grade					-0.01	0.04	0.07	0.08
					(0.01)	(0.02)	(0.03)	(0.03)
E[Dep Var | High Test Exp and Long Relationship]								
All Firms	0.00	0.03*	0.01	0.01				
	(0.02)	(0.01)	(0.03)	(0.03)				
Unrated Firms					0.00	0.02	0.03	0.02
					(0.02)	(0.02)	(0.04)	(0.02)
Spec Grade Firms					-0.01	0.01	-0.04	-0.07
					(0.02)	(0.04)	(0.05)	(0.04)
Invest Grade Firms					0.01	0.06***	0.04	0.02
					(0.02)	(0.02)	(0.03)	(0.04)
Firm Controls	Y	Y	Y	Y	Y	Y	Y	Y
Deal Controls	Y	Y	Y	Y	Y	Y	Y	Y
Bank Controls	Y	Y	Y	Y	Y	Y	Y	Y
Firm-Bank Controls	N	N	N	N	Y	Y	Y	Y
R-squared	0.54	0.37	0.24	0.16	0.55	0.39	0.26	0.19
Number of Firms	753	687	743	715	753	687	743	715
Number of Clusters	30	30	30	30	30	30	30	30

This table provides estimates from regressions of firm real outcomes on firm and deal characteristics. The specifications are identical to those in Table 12.

2 Lender Cross-Subsidization and Credit Supply in the Fannie Mae MBS Market

(Joint with Igor Karagodsky)

Abstract

Large market-share sellers of mortgages to Fannie Mae received discounts on mortgage insurance payments (guarantee fees) relative to small market-share sellers until 2012. These guarantee-fee discounts created incentives for small sellers to originate and sell mortgages to large sellers, rather than directly to Fannie Mae, foregoing the opportunity to service loans and establish lending relationships with local borrowers. We exploit an exogenous change in guarantee fees charged between large and small market-share sellers of mortgages to identify the impact of these cross-lender subsidies on credit supply. In response to the removal of these subsidies, small market-share sellers increased their share of originations of single-family mortgages sold directly to Fannie Mae relative to large sellers. Preliminary evidence suggests that as a consequence, states with high concentrations of large market-share sellers experienced relative reductions in credit quantities, suggesting that these subsidies previously increased credit supply.

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2.1 Introduction

Fannie Mae and Freddie Mac, the major government-sponsored enterprises (GSEs), are a dominant presence in the single-family mortgage finance market in the United States. The GSEs insure credit risk on loans backing mortgage-backed securities (agency MBS) in exchange for "guarantee fees," a fraction of principal and interest payments on each loan. While this business model proved lucrative during the housing boom period of 2002-2006, the increase in defaults and foreclosures following the financial crisis of 2007-2009 exposed the GSEs to tremendous losses on insured loans. Following government conservatorship of the GSEs in 2008, the Federal Housing Finance Agency (FHFA) and Congress have begun to enact policies to reduce the GSEs' burden on taxpayers by gradually shrinking their market share in the single-family MBS market. While the GSEs have begun to align pricing of guarantee fees with underlying credit risk, certain borrowers, lenders, and products continue to receive large subsidies in the agency MBS market. Quantifying the impact of these subsidies has become an important question of interest in the mortgage finance literature. Whether the removal of these subsidies affects credit supply remains an unexplored question.

This paper studies the unwinding of one particular subsidy in the Fannie Mae single-family MBS market, that of offering guarantee-fee discounts to large-volume sellers of MBS relative to small-volume sellers. Large-volume sellers relied primarily on swap transactions with Fannie Mae, the exchange of pools of mortgages of particular characteristics for MBS. In contrast, small-volume sellers utilized cash window sales, the direct purchase of mortgages by Fannie Mae in exchange for cash. Guarantee-fee pricing depended crucially on the volume of mortgages sold through each channel, as fees paid on MBS swaps were significantly lower than those paid on cash window sales for many years, in part due to negotiations between Fannie Mae and large-volume sellers (FHFA (2013)). In late 2012, the FHFA directed Fannie Mae to increase guarantee fees on MBS swap transactions by relatively more than cash window sales, effectively removing subsidies paid by small-volume sellers to large-volume sellers.

Recent research has begun to quantify the effect of geographic and credit-risk subsidies across borrowers of GSE mortgages (Hurst, Keys, Seru, and Vavra (2016), Dagher and Sun (2016), Gete and Zecchetto (2017)). While the GSEs have begun to increase risk-based pricing of guarantee fees, geographic subsidies still remain in place. We contribute to this literature by quantifying the impact of removing lender-based subsidies. This research has important implications for mortgage finance reform, as small sellers typically sold mortgages to large lenders due to this subsidy, foregoing the opportunity to service mortgages and establish lending relationships with local borrowers. Only recently have small sellers begun to increase their business with the GSEs, resulting in increases in GSE counterparty credit risk (FHFA (2014)), but more even distribution of risks across lenders.

In the first part of the paper, we quantify whether the implementation of guarantee-fee parity was effective at changing the incentives of small-volume sellers to sell mortgages to Fannie Mae relative to large-volume sellers. We find that the likelihood of selling to the cash window for sellers with market shares ranked between 26 and 100 increased by 5 percentage points more than for large sellers ranked between 1 and 5.

In the second part of the paper, we investigate whether small-volume sellers differentially allocated more of their credit in the 30-year Fannie Mae MBS market toward the cash window relative to swap transactions following the guarantee-fee policy change. We find that small-volume sellers increased the relative share of cash-window volume sales by 14-18 percentage points more than did large-volume sellers. Much of this relative allocation of credit was driven by observably less risky loans (high credit score and low LTV ratio).

Given these results, we provide some preliminary evidence to investigate the aggregate effect of this change in lending behavior. At the state-month level, we average loan size and interest rates, and aggregate total loan volume. In addition, we calculate a measure of market concentration of large-volume sellers by squaring market shares of sellers in the 1-5 rank of the distribution in a given state-month pair. We find that going from a state at the 10th percentile (2.1%) of large-volume seller concentration to one at the 90th percentile (12.8%) resulted in a 1.2 percentage point decline in average loan size and a 5.9 percentage point decline in total loan volume in a given state following the implementation of guarantee fee parity across sellers. In contrast, we find no such change in credit supply for interest rates. These results offer preliminary evidence that the removal of lender-based subsidies resulted in the relative loss of credit for borrowers in states that were highly reliant on large-volume sellers, suggesting that the existence of these subsidies previously contributed to increases in credit supply.

2.2 Background

2.2.1 *Fannie Mae MBS Market*

The Federal National Mortgage Association (Fannie Mae) is one of the government-sponsored enterprises (GSEs) tasked with increasing liquidity in the secondary mortgage market in the U.S.[38] Its main business model involves the issuance of mortgage-backed securities (MBS) on pools of mortgages that it purchases from financial institutions in exchange for a guarantee of the timely payment of principal and interest to investors. Strict credit quality guidelines ensure that only conforming mortgages are eligible to be acquired by Fannie Mae.[39] In exchange for effectively assuming the credit risk on pools of mortgages underlying its MBS, Fannie Mae is compensated with "guarantee fees," which are payments on a fixed percentage of the outstanding principal balance on a mortgage.[40]

Before the financial crisis of 2007-2009, Fannie Mae benefitted from the implicit backing of the U.S. government, which enabled it to borrow at a small spread above Treasury rates (Frame and White (2005)). With easy access to low-cost borrowing rates on its debt, this created strong moral hazard incentives whereby Fannie Mae bought increasingly risky securities, such as Alt-A and subprime MBS. In addition, Fannie Mae was able to set guarantee fees below the level required by private in-

[38]The other major GSEs involved in housing finance include the Federal Home Loan Mortgage Corporation (Freddie Mac), the Government National Mortgage Association (Ginnie Mae), and the Federal Home Loan Banks. This paper focuses on Fannie Mae due to superior data provided on mortgages underlying its MBS and its role as the largest GSE.

[39]Conforming mortgages have limits on size ($417,000 except in high-cost areas), credit score, and loan-to-value and debt-to-income ratios.

[40]Guarantee fees increased from approximately 20 bp in the mid-2000s to over 60 bp in recent years (FHFA July 16, 2013).

surance companies to earn a profit. This business model was quite lucrative during the housing boom when default rates were low. However, as defaults on mortgages increased during the early stages of the financial crisis, Fannie Mae became insolvent and was placed into government conservatorship in 2008 (Frame, Fuster, Tracy, and Vickery (2015)). In the post-crisis period, Fannie Mae's regulator, the FHFA, has begun to increase guarantee fees paid on MBS in order to both remove the agencies' market share and to encourage participation from the private sector in the conforming MBS market. Nevertheless, Fannie Mae and the other GSEs have maintained large market shares in the MBS market in recent years (Hurst et al. (2016)).

There are two main channels through which Fannie Mae provides liquidity to lenders.[41] In an MBS swap transaction, sellers bundle and deliver loans of similar characteristics to Fannie Mae in exchange for MBS (Figure 13). While sellers in swap transactions may deliver loans originated through the retail channel (i.e. by the institution itself), they also pool loans originated by third parties, either a correspondent lender or broker. When acquired from a third party, the originating institution typically sells the mortgage to the seller "servicing released," foregoing the ability to service the loan. Thus, the seller typically retains MSR in MBS swap transactions, giving the institution the ability to establish a relationship with and market future products to mortgage borrowers. Historically, benefits of swap transactions included relatively low guarantee fees paid by large sellers, which enabled them to issue large volumes of MBS at a comparative advantage to private-label MBS. The main cost

[41] Financial institutions that sell mortgages to Fannie Mae through either channel are referred to as "sellers." "Servicers" are the institutions that administer the loans, for example by collecting monthly mortgage payments, managing the relationship with mortgagors, and remitting payments to the trust (Fannie Mae) in exchange for servicing fees.

to a seller of utilizing swap transaction includes having to re-underwrite third-party mortgages, buyback provisions on loans failing to meet Fannie Mae credit quality guidelines, and having to monitor third-party originators.

In a cash window (whole loan conduit) transaction, Fannie Mae purchases mortgages directly from sellers for cash and bundles them into MBS, which they subsequently sell to investors (Figure 14). Sellers typically retain MSR on mortgages that they sell in these transactions, but forego the ability to sell MBS to investors. Benefits of the cash window include quick liquidity to lenders, reduced warehousing risk, less concern about structuring MBS in order to abide by Fannie Mae's guidelines, and the option to retain MSR. However, cash-window sellers pay guarantee fees upfront for lower proceeds than they would get in a swap transaction. As a result, the cash window is more frequently utilized by small sellers.

2.2.2 *Cross-Subsidization*

An important market friction in the agency MBS market throughout the 2000s was cross-subsidization across different market segments. For example, guarantee fees did not fully reflect risks across loan product, geographic region, borrower, and seller characteristics.[42] In the after-math of the financial crisis, the GSEs began to implement risk-based pricing of guarantee fees to eliminate these cross subsidies.

The focus of this paper is the Fannie Mae policy of charging higher guarantee fees to small-volume sellers of MBS relative to large-volume sellers. In particular,

[42]For example, major cross-subsidies included: fixed 15-year and adjustable-rate mortgage MBS subsidizing fixed 30-year MBS; non-judicial foreclosure states subsidizing judicial foreclosure states; low loan-to-value (LTV) ratios and high credit scores subsidizing high LTVs and low credit scores; small-volume sellers subsidized large-volume sellers.

guarantee fees charged via the cash window exceeded those charged in swap transactions. In addition, sellers with large market share were able to negotiate guarantee-fee discounts directly with Fannie Mae due to their market power (FHFA (2013)).[43] This policy discouraged small financial institutions from selling mortgages directly to Fannie Mae. For many years, large-volume sellers exploited this pricing difference by buying directly from small sellers and "aggregating" pools of MBS to be sold through swap transactions (FHFA (2014)).[44] Small financial institutions benefitted from these sales due to the superior sales price offered by aggregators relative to sales through the cash window. However, as mentioned previously, these institutions typically sold mortgages to aggregators servicing released, and thus were unable to benefit from servicing relationships with mortgage borrowers. From Fannie Mae's perspective, this system reduced their counterparty credit risk by not having to monitor the smallest sellers and remained a lucrative business model through the housing boom, as relatively low default rates on single-family mortgages enabled them to easily cover their cost of capital.

Guarantee fees increased between 2008 and 2011, consistent with the aim of encouraging private-sector participation in the secondary mortgage market. These increases resulted in the gradual reduction of risk-based cross-subsidization. One such policy change resulted from the Temporary Payroll Tax Cut Continuation Act

[43] "The Enterprises traditionally charged high-volume mortgage sellers guarantee fees that were lower than those charged to low-volume sellers. The Enterprises did so, in part, because large lenders were able to negotiate reductions in fees based upon the large volume of loans they were able to deliver." (FHFA (2013), p. 23)

[44] "By combining their own mortgages with those originated by smaller lenders, the aggregators received larger guarantee fee discounts from the Enterprises. In turn, the aggregators passed along a portion of their discounted guarantee fees to smaller lenders in the form of better prices than the Enterprises could offer them." (FHFA (2014), p.14)

of 2011, which increased upfront guarantee fees by at least 10 basis points above the 2011 average on single-family MBS, effective April 1, 2012. This fee increase was used by Congress to raise revenue as a means of offsetting payroll tax cuts. The Federal Housing Finance Authority (FHFA)[45] issued guidance on August 31, 2012 which instructed the agencies to further increase guarantee fees by 10 basis points, particularly to reduce differences between 15-year and 30-year MBS, and between large-volume and small-volume sellers. The policy became effective on November 1, 2012 for the cash window and December 1, 2012 for swap transactions. This policy was effective at narrowing the gap in guarantee fees between high market-share and low market-share sellers (Figure 15).

2.2.3 *Literature Review*

A growing literature has considered the implications of GSE cross-subsidies on transfers of wealth across different demographics.[46] Hurst et al. (2016) demonstrate the lack of geographic risk-based pricing of mortgages in GSE loans, even though private-label mortgage rates do price regional default risk ex-ante. This constant interest-rate policy results in a large transfer in wealth from regions experiencing positive economic shocks to those experiencing negative shocks, and this transfer benefits middle-income households more that are reliant on the GSEs for mortgage finance. Dagher and Sun (2016) find that credit supply at the jumbo-loan size threshold falls for judicial relative to non-judicial foreclosure states, although the GSEs mitigate the

[45]The FHFA is the regulator of Fannie Mae and Freddie Mac.

[46]Other related papers in this area include Jeske, Krueger, and Mitman (2013), Elenev, Landvoigt, and Van Nieuwerburgh (2016), and J. Kim and Wang (2018). In addition, a number of papers have studied the implicit funding subsidy provided to the GSEs (Passmore, Sparks, and Ingpen (2002)).

aggregate effects using cross-subsidies. Gete and Zecchetto (2017) use a model of the housing market to show that the removal of credit-risk subsidies in GSE mortgages increases wealth inequality by benefitting high-income households and homeowners. Our paper also relates to a literature showing that the GSE policies affect credit supply and the structure of MBS. Around the GSE jumbo loan cut-off, credit supply changes discretely for more liquid and more concentrated lenders (Loutskina and Strahan (2009), Loutskina and Strahan (2011)). Adelino, Frame, and Gerardi (2017) document within-deal performance differences between loans destined for the GSEs relative to other non-GSE mortgages backing the same securities. Relative to these papers we focus on the GSE policy of cross-subsidizing lenders, and not on the distributional implications for borrowers. In addition, we show that these GSE cross-subsidies affected the composition of credit within the GSE mortgage market.

Our also paper relates to a broad literature studying the effect of securitization on the incentives for retention of mortgage risk. Demiroglu and James (2012) show that originator-sponsor and originator-servicer affiliation in private-label MBS deals results in significantly lower deal default rates, but also lower yield spread. Other papers show more generally that the originate-to-distribute model resulted in excessive origination of low-quality mortgages (Keys, Mukherjee, Seru, and Vig (2009),Keys, Mukherjee, Seru, and Vig (2010), Purnanandam (2010), Keys, Seru, and Vig (2012), Nadauld and Sherlund (2013), Jiang, Nelson, and Vytlacil (2013), Bubb and Kaufman (2014), Rajan, Seru, and Vig (2015), Begley and Purnanandam (2016)). Relative to these papers, we study how GSE cross-subsidies affected the decision to retain servicing. Importantly, we can make this distinction from pure risk retention, as the credit risk on GSE loans is fully insured, unlike in the market for private-label MBS.

2.3 Data and Summary Statistics

The main dataset that we utilize in our analysis is monthly loan- and pool-level data on conventional long-term, single-family MBS generally maturing or due in 30 years or less, downloaded using the Fannie Mae PoolTalk® portal for securities issued between 2012 and 2013. For each loan, we keep at-issuance data on loan size, the original interest rate, the name of the seller and servicer, credit score, the loan-to-value (LTV) ratio, month of first payment, state in which the property resides, loan purpose (purchase or re finance), and occupancy status (principal, second, or investor). In addition, for pools beginning in June 2012, we also observe the origination channel (retail, correspondent, or broker), property type (single-family, condo, etc.), and whether the borrower is a first-time buyer. For each pool, we also keep the pool number and whether the MBS was sold through the cash window or through a swap transaction.[47] While many of the sellers in our dataset are non-banks, we collect for the commercial bank sample data from the CALL reports of balance sheets and income. The main bank characteristics that we keep are bank assets and ratios of deposits, equity capital, cash, and non-performing loans to assets.

Summary statistics for the main loan-level dataset are given in Table 13. Each row contains averages split by seller market shares of key variables of interest and standard deviations given in brackets below. On average, utilization of the cash window and originator/serivcer affiliation are higher for smaller sellers relative to larger

[47]Before June 2013, cash window loans were denoted by the pool prefix "AB."

sellers. In addition, small-volume sellers sell higher LTV and lower credit score mortgages than do large-volume sellers. However, they also sell more retail and fewer refinance mortgages. At first glance, it is not obvious that the riskiness of mortgage pools necessarily differed across sellers of different market shares. Nevertheless, we include a full set of controls for loan and borrower risk in all specifications.

Following the implementation of guarantee-fee parity, total issuance of 30-year fixed-rate MBS volumes increased (Figure 16). This increase was driven primarily by increases in cash window volume sales, which took some of the market share (Figure 17) from swap transactions, especially in the months after the announcement.[48] In addition, Figures 18 and 19 show that much of this increase in the volume of MBS issuance and relative changes in market share occurred for small and medium sellers relative to large sellers. While all sellers increased cash window volume sales in the months following the policy change (Figure 20), this increase was especially pronounced for small- and extra-small volume sellers. Figure 9 shows that much of the eventual increase in cash-window market share was driven by these small sellers. Overall, these figures provide visual evidence that the implementation of guarantee-fee parity resulted in relative shifts in MBS issuance toward the cash window.

[48]Note that the increase in guarantee fees for the cash window occurred in November 2012 (month 2 in all figures) and for swap transaction in December 2012 (month 3 in all figures).

2.4 Loan-Level Evidence

2.4.1 *Cash Window Sales*

A key relationship we want to estimate in this paper is between cross-subsidization and credit supply. Identification of the main relationship is complicated due to reverse causality. On the one hand, sellers might pool and sell relatively more mortgages destined for swap trans-actions rather than through the cash window due to guarantee-fee pricing differentials. On the other hand, Fannie Mae might charge different guarantee fees to sellers in response to their issuance of MBS. To identify this relationship, we focus on the implementation of guarantee-fee parity between large-volume and small-volume sellers in late-2012. Summary statistics in the previous section suggest that in aggregate small sellers increased MBS sales to the Fannie Mae cash window relative to large sellers following the implementation of guarantee-fee parity. In this section, we investigate whether this behavior applied at the loan level as well. The main outcome of interest for this empirical model is whether a loan is sold to the cash window or not. Our main loan-level estimating equation for loan i sold by seller s in year-month t is the following:

$$\begin{aligned}
\text{Cash Window Sale}_{i,s,t} &= \alpha + \beta_1 \text{Market Share Dummies}_{s,t-1} \\
&+ \beta_2 \text{Market Share Dummies}_{s,t-1} \times \text{Post}_t + \gamma \text{Controls}_{i,t} \\
&+ \delta \text{Controls}_{i,t} \times \text{Post}_t + \zeta \text{Seller Controls}_{s,t-1} + \alpha_s + \alpha_t + \epsilon_{i,s,t}
\end{aligned}$$

where α_s and α_t are seller and year-month fixed effects, Market Share Dummies$_{s,t-1}$ equal 1 for sellers with medium (6-25), small (26-100), and extra-small (100+) market shares in the previous month and 0 otherwise, Post_t is a dummy equal to 1 after August 2012 and 0 otherwise, $\text{Controls}_{i,t}$ are borrower- and loan-specific risk characteristics, and Seller $\text{Controls}_{s,t-1}$ are lagged quarter values of seller characteristics for institutions for whom data are available. The coefficients of interest from these regressions are β_2, which capture the likelihood of selling to the cash window for non-large sellers relative to large sellers after the implementation of guarantee-fee parity, conditional on risk characteristics. Formally, we test the null hypothesis that the likelihood of being sold to the cash window did not change for medium, small, and extra-small sellers relative to large sellers following the removal of volume-based cross-subsidies (H1).

Results from loan-level regressions are given in Table 14. Each regression is estimated using OLS with the inclusion of seller fixed effects.[49] Using the full sample of sellers (columns 1-2), we find strong evidence to reject the null hypothesis (H1) for small sellers. Following the removal of volume-based cross-subsidies, the likelihood of a small seller (26-100) selling to the cash window increased by 5 percentage points more than for a large seller conditional on risk characteristics. This result is robust to the inclusion of additional characteristics available only after June 2012 (column 2), and the inclusion of time-varying seller controls (columns 3 and 4). Much of the relative change in cash window utilization seems to have occurred for small sellers and not for medium or extra-small sellers.

[49] All results are robust to estimation via Probit.

2.4.2 *Borrower Heterogeneity*

In this section we explore whether the increase in likelihood of cash window sales for small-relative to large-volume sellers was driven by ex-ante riskier borrowers. In particular, we split the sample based on proxies of loan riskiness identified in the literature: low credit score threshold (FICO score below 660) and high loan-to-value ratio (LTV ratio above 80). Results from loan-level regressions split by borrower characteristics are given in Tables 15 and 16. Each regression is estimated using OLS with the inclusion of seller fixed effects. Across both the full and bank samples, we again find strong evidence to reject the null hypothesis (H1) for small sellers. Following the removal of volume-based cross-subsidies, the likelihood of a small seller (26-100) selling to the cash window increased by 4-7 percentage points more than for a large seller conditional on risk characteristics for both the high- and low-credit score samples. We similarly find that across the high- and low-LTV samples, the likelihood of sale to the cash window is 4-7 percentage points higher for small relative to large sellers. We conclude that this increased sale to the cash window was not necessarily driven by marginally risky borrowers.

2.4.3 *Servicing Retention and Compensation*

One of the main factors cited in the removal of volume-based cross-subsidies was the ability of small-volume sellers to be able to retain servicing rights, thus reducing their reliance on sales to aggregators.[50] We test whether sellers were more

[50] "Lenders are more readily able to follow the historically traditional organic growth path from a pure servicing-released model, to a servicing-retained model with subservicing, and finally to an end-to-end origination to servicing model in the hopes of capitalizing on the customer relationship,

likely to retain servicing on loans originated by the same institution following the introduction of guarantee-fee parity. For these regressions, we split the sample into cash-window and MBS swap sales, but due to data limitations we use the full sample of sellers, rather than the commercial bank sample. Results are presented in Table 17 columns 1 and 2. Overall, we do not find evidence to reject the null hypothesis that smaller sellers did not retain servicing on loans originated through the retail channel by more than did larger sellers. While the coefficient magnitudes for the cash window sample (column 1) do support this hypothesis, coefficient estimates are not statistically different from zero. While we do not find significant differences between the likelihood of retain servicing for sellers as a function of market share after the implementation of guarantee-fee parity, we hypothesize that smaller sellers might have been compensated relatively more on retained loans than large sellers after the policy change. To proxy for servicer compensation, we take the difference between the interest rate on each loan and the pass-through rate for the MBS pool. Conditional on seller fixed effects and the riskiness of the loan, this variable should proxy for the servicing fees going to the seller. Results are given in columns 3 and 4 in Table 17. Overall, the coefficient magnitudes do support the hypothesis that smaller sellers earned more in servicing on retained loans, but the point estimates do not allow for us to reject the null hypothesis of no change.

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the servicing cash flows, and the re finance or the next purchase opportunity." (MBA (2015))

2.5 Aggregate Evidence

2.5.1 *Cash Window and MBS Swap Issuance*

We have provided loan-level evidence to suggest that on the margin, small-volume sellers were more likely to sell to the cash window than large-volume sellers following the implementation of guarantee-fee parity across sellers. With the removal of this cross-subsidy, we test whether sellers allocated relatively more credit to the cash window than to MBS swaps as a function of market share. Given that the gap in guarantee fees narrowed as a function of seller market share, we would expect that small sellers were more affected by this policy change relative to medium-sized sellers.

In order to estimate the causal relationship of cross-subsidization on MBS issuance, we consider a standard model of credit allocation which models an institution's volume of MBS issuance (both to the cash window and to swap transactions) as a function of shocks to the supply and demand for credit. Similar to Loutskina and Strahan (2009) and Loutskina and Strahan (2011), we take differences in volumes between the cash window and swaps at the seller-month level to purge unobserved heterogeneity that might affect the demand for MBS in either market. Dividing by the total volume of MBS issuance for each seller, the outcome of interest is then the share of cash-window relative to swap volume issuance. Our main estimating equation for seller s in year-month t is the following:

$$\frac{\Delta(\text{CW-Swap})_{s,t}}{\text{Vol Total Issuance}_{s,t}} = \alpha + \beta_1 \text{Market Share Dummies}_{s,t-1}$$

$$+ \beta_2 \text{Market Share Dummies}_{s,t-1} \times \text{Post}_t + \gamma \text{Controls}_{s,t}$$

$$+ \delta \text{Seller Controls}_{s,t-1} + \alpha_s + \alpha_t + \epsilon_{s,t}$$

where s and t are seller and year-month fixed effects, Market Share Dummies$_{s,t-1}$ equal 1 for sellers with medium (6-25), small (26-100), and extra-small (100+) market shares in the previous month and 0 otherwise, Post$_t$ is a dummy equal to 1 after August 2012 and 0 otherwise, Controls$_{s,t}$ are averages of the characteristics of the pool of MBS issued by seller s in year-month t, and Seller Controls$_{s,t-1}$ are lagged quarter values of seller characteristics for institutions for whom data are available. The coefficients of interest from these regressions are β_2, which capture the relative change in shares of cash window relative to swap issuance for non-large sellers relative to large sellers after the implementation of guarantee-fee parity, conditional on risk characteristics. Formally, we test the null hypothesis that medium, small, and extra-small sellers did not change the share of MBS issued through the cash window relative to swaps by more than did large sellers following the removal of volume-based cross-subsidies (H2).

Results from the regression in equation (1) are given in Table 18. Each model is estimated after performing the within-transformation across sellers to remove all time-invariant heterogeneity at the seller level. Across all specifications, we can reject the null hypothesis (H2) that small sellers did not differentially allocate credit to the cash window relative to large sellers. Sellers with market share in the 26-100 range (small) increased their share of cash window relative to swap MBS volume issuance by 14-18 percentage points more than did sellers in the 1-5 range of the distribution (large) following guarantee-fee parity. This result holds with the inclusion of month fixed effects, controls for the average riskiness of loan pools, and controls for time-varying seller characteristics for the commercial bank sample (column 4). We also find that extra-small sellers marginally increased their relative cash window issuance as well, although this result is less robust than for small sellers. Finally, we find little evidence to reject the null hypothesis for medium sellers. Overall, these results indicate that the FHFA implementation of guarantee-fee parity across sellers was effective at encouraging a relative allocation of credit toward the cash window, especially for small sellers.

2.5.2 *Sales Split by Borrower Heterogeneity*

In the prior analysis, we established that small-volume sellers increased the allocation of their sales to Fannie Mae relatively more to the cash window than did large-volume sellers. In this section, we explore whether this was driven by a relative change by borrower characteristics. For each specification we calculate relative shares of cash window and MBS swap volume sales by high/low credit scores (660 FICO score threshold) and LTV ratios (80%). Results from this exercise are given in Table 19. Overall, we find that much of the relative increase in cash window sales by small and extra-small sellers relative to large sellers was driven by increases in high credit score and low LTV loans (columns 2 and 4), rather than by observably riskier loans (columns 1 and 3).

2.5.3 *Credit Supply and Large-Seller Concentration*

In this section, we try to quantify the aggregate effects of the removal of volume-based cross-subsidies on borrowers. For each state, we calculate the average loan size and interest rate, as well as the sum of all loan volume in a given month. We also calculate the Herfindahl-Hirschman Index for large sellers in a given month, which we include as a lag in each regression. We then regress these credit supply outcomes in a state- fixed effects model with controls for month, and average borrower and loan characteristics in a given state-month. Results from this exercise are given in Table 20. Going from a state at the 10th percentile (2.1%) of large-volume seller concentration to one at the 90th percentile (12.8%) results in a 1.2 percentage point decline in the average loan size and a 5.9 percentage point decline in the total loan volume in a given state following the implementation of guarantee fee parity across sellers. We find no such change in credit supply for interest rates. These results suggest that the relative increase in cash window sales by small sellers did not offset the overall reduction in credit for states that were relatively more reliant on large-volume sellers. Nevertheless, we do not necessarily argue for a causal interpretation of these results given the difficulty of fully controlling for time-varying shocks to borrower demand in geographic regions.

2.6 Conclusion

This study is the first to evaluate the impact of removing volume-based cross-subsidies in the Fannie Mae MBS market. On the margin, small sellers benefitted from differentially favorable guarantee-fee pricing by allocating relatively more credit to the cash window than to swap transactions. Nevertheless, this did not significantly change the incentive to retain servicing or to earn higher servicing fees on mortgages for which servicing rights were retained. We provide some evidence that credit supply fell in states with relatively higher concentration of large-volume sellers, although we do not entirely rule out alternative explanations. In conclusion, our estimates provide some context for considering the implications for credit supply of removing cross-subsidies in a highly-liquid market.

2.7 Figures

Figure 13: MBS Swap Transaction

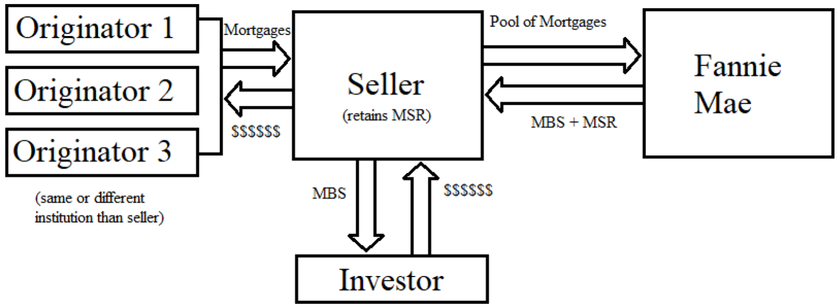


Figure 14: Cash Window Transaction

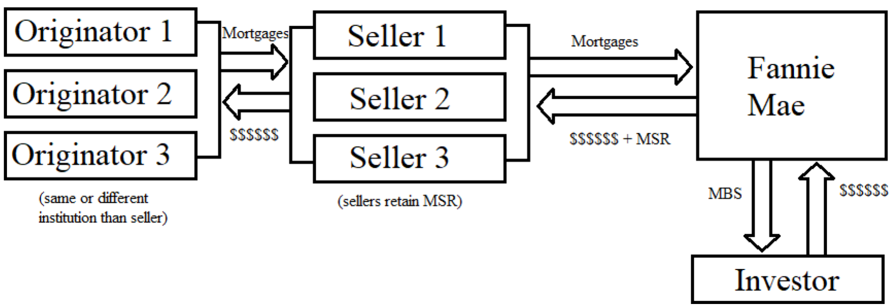
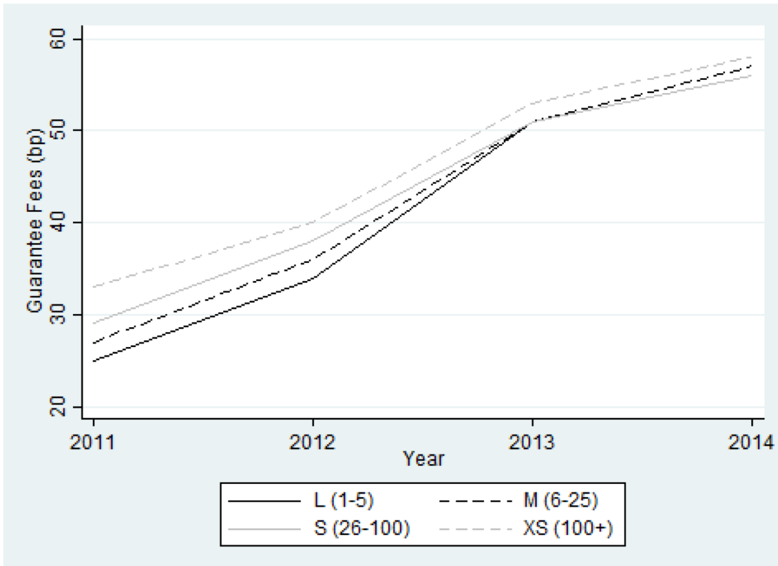


Figure 15: Single-Family Guarantee Fees by Acquisition-Volume Group



FHFA Report: Fannie Mae and Freddie Mac Single-Family Guarantee Fees in 2014

Figure 16: 30-Year Fannie Mae Total Volume MBS Issuance

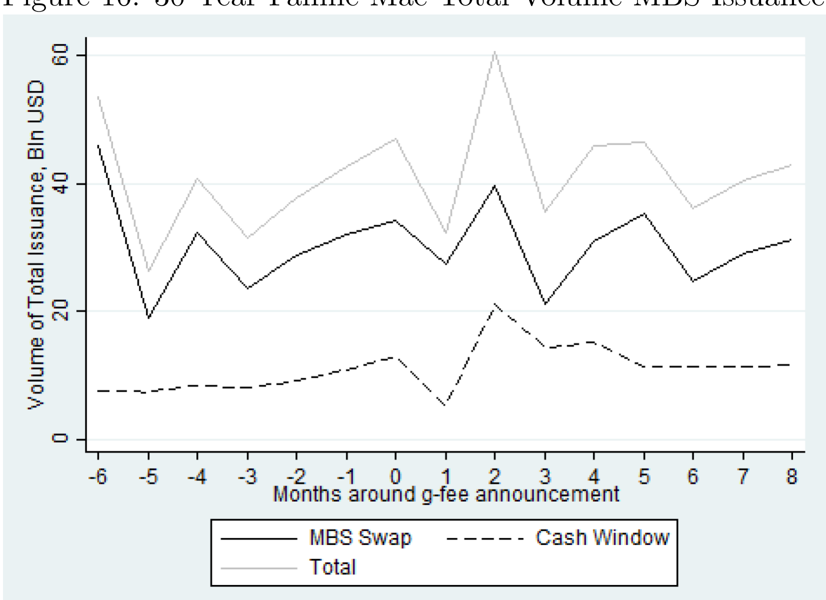


Figure 17: Market Shares of 30-Year Fannie Mae Total Volume MBS Issuance

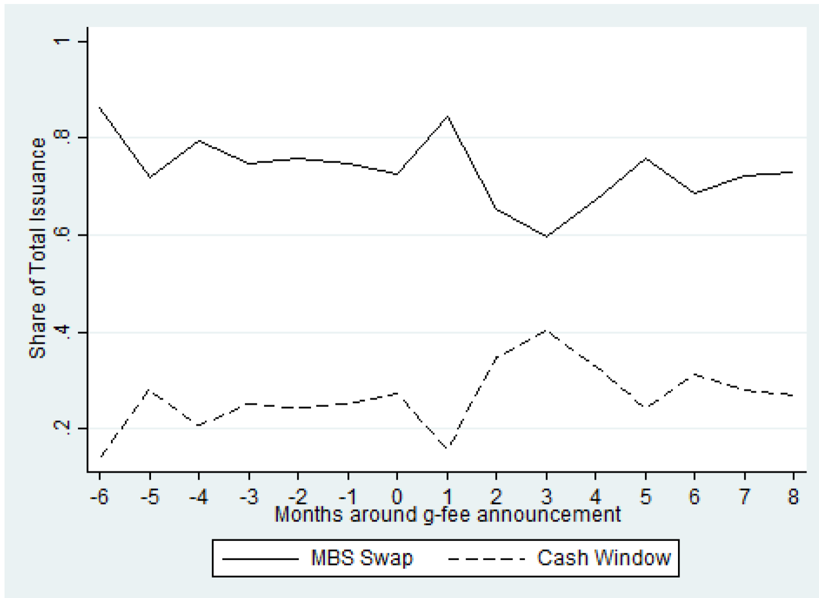


Figure 18: 30-Year Fannie Mae Total Volume MBS Issuance by Seller Size

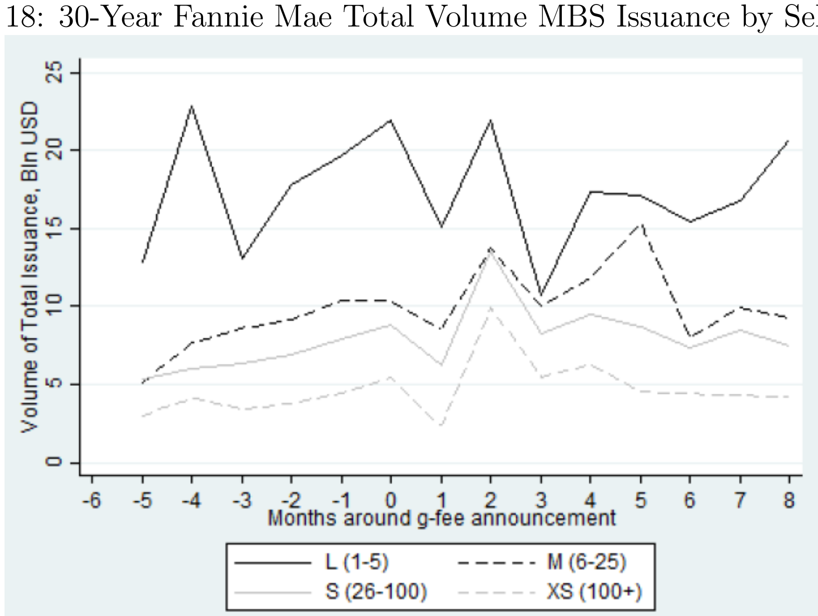


Figure 19: Market Shares of 30-Year Fannie Mae Total Volume MBS Issuance by Seller Size

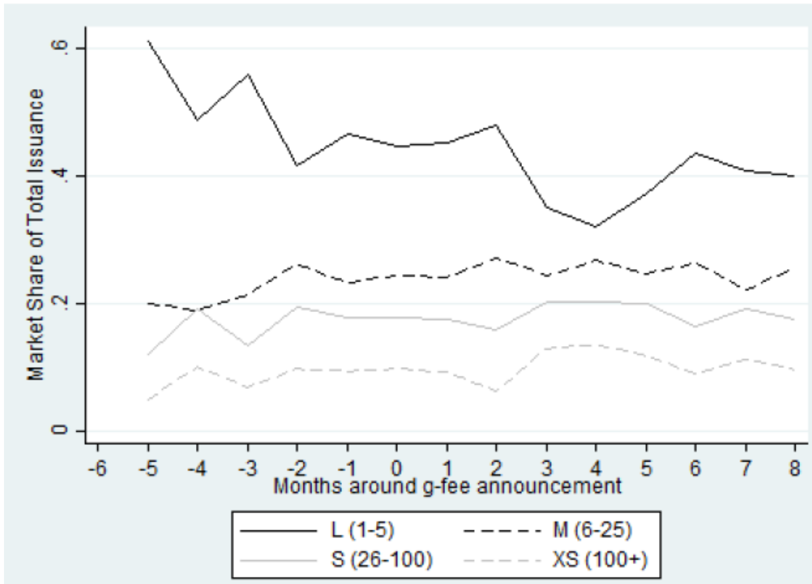


Figure 20: 30-Year Fannie Mae Cash Window MBS Issuance by Seller Size

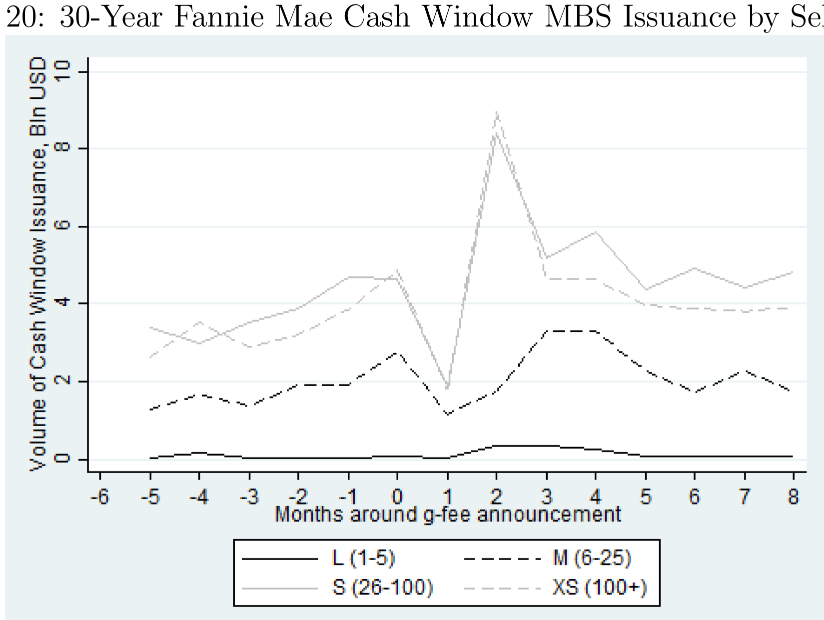
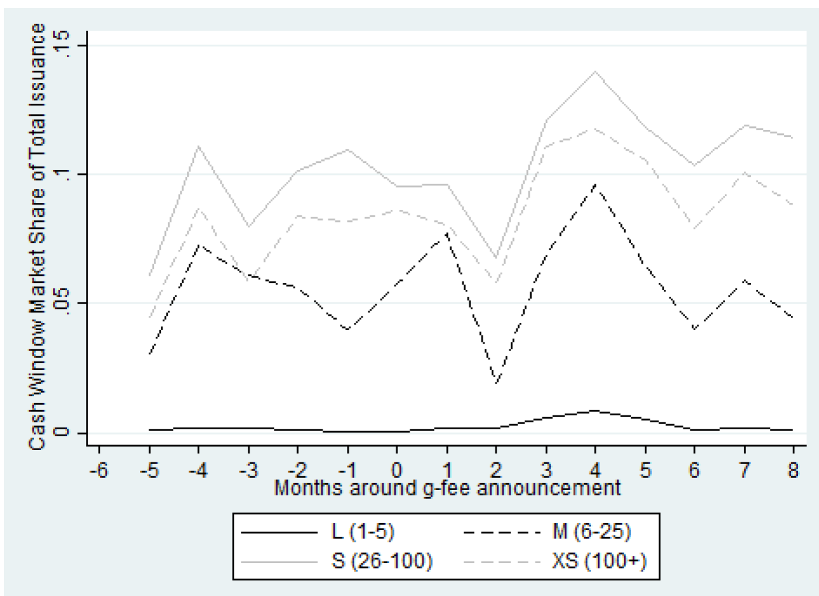


Figure 21: Cash Window Market Shares of 30-Year Fannie Mae Total Volume MBS Issuance by Seller Size



2.8 Tables

Table 13: Mean Loan-Level Summary Statistics

Market Share Group:	Large	Medium	Small	Extra-Small
Cash Window Sale (D)	0.01	0.18	0.54	0.86
	(0.08)	(0.39)	(0.50)	(0.35)
Same Originator-Servicer	0.50	0.49	0.68	0.82
	(0.50)	(0.50)	(0.47)	(0.39)
Interest Rate - Pass-Through Rate	0.57	0.57	0.53	0.51
	(0.19)	(0.19)	(0.18)	(0.19)
LTV Ratio	73.10	74.08	74.48	76.00
	(17.39)	(17.54)	(16.62)	(16.76)
Credit Score	756.16	753.19	758.19	754.70
	(48.96)	(49.91)	(43.29)	(45.45)
Refinance (D)	0.71	0.76	0.63	0.63
	(0.45)	(0.43)	(0.48)	(0.48)
Number of Units	1.03	1.03	1.04	1.03
	(0.24)	(0.25)	(0.29)	(0.24)
Retail Channel Origination (D)	0.50	0.50	0.69	0.82
	(0.50)	(0.50)	(0.46)	(0.39)
First-Time Buyer (D)	0.10	0.08	0.13	0.12
	(0.30)	(0.26)	(0.33)	(0.33)

Table 14: Likelihood of Cash Window Sale

	Dependent Variable: Sale to Cash Window = 1			
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	0.01	0.02	0.00	0.00
	(0.03)	(0.03)	(0.01)	(0.01)
Small Seller × Post	0.05***	0.04**	0.06*	0.06*
	(0.02)	(0.02)	(0.03)	(0.04)
Extra-Small Seller × Post	0.02	0.02	0.03	0.02
	(0.01)	(0.01)	(0.02)	(0.02)
Sample	Full	Full	Banks	Banks
Loan and Borrower Controls	Y	Y	Y	Y
Additional Controls	N	Y	N	Y
Seller Controls	N	N	Y	Y
Month Fixed Effects	Y	Y	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	2,146,802	1,653,902	1,209,913	905,410
Number of Clusters	369	369	134	134
R-squared (within)	0.05	0.02	0.07	0.04

This table presents estimates from linear regressions of cash window sales on loan, borrower, and seller characteristics. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period in columns 1 and 3 includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Due to data availability, the sample period in columns 2 and 4 includes loans sold to Fannie Mae between June 2012 and April 2013. Loan and borrower controls include the original LTV ratio, original credit score, number of units, the percentage of the mortgage that is insured privately, and dummy variables for origination month, property type, occupancy status, loan purpose, and state. In addition, all loan and borrower controls are interacted with the post dummy. Additional controls include dummies for whether the loan was originated through the retail channel, mortgage property type, and whether the borrower was a first-time buyer, as well as interactions of each with the post dummy. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. Columns 1 and 2 contain the full sample of sellers while columns 3 and 4 contain only the sample of commercial banks for which data are available. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 15: Likelihood of Cash Window Sale Split by Credit Score

	Dependent Variable: Sale to Cash Window = 1			
	High Credit Score		Low Credit Score	
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	0.02	0.00	0.01	0.01
	(0.03)	(0.01)	(0.02)	(0.01)
Small Seller × Post	0.04**	0.06*	0.04**	0.04**
	(0.02)	(0.04)	(0.02)	(0.03)
Extra-Small Seller × Post	0.02	0.02	0.01	0.02
	(0.01)	(0.02)	(0.01)	(0.02)
Sample	Full	Banks	Full	Banks
Loan and Borrower Controls	Y	Y	Y	Y
Seller Controls	N	Y	N	Y
Month Fixed Effects	Y	Y	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	1,582,535	861,992	71,367	43,418
Number of Clusters	369	134	368	133
R-squared (within)	0.02	0.04	0.02	0.05

This table presents estimates from linear regressions of cash window sales on loan, borrower, and seller characteristics. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period in columns 1 and 3 includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Due to data availability, the sample period in columns 2 and 4 includes loans sold to Fannie Mae between June 2012 and April 2013. Loan and borrower controls include the original LTV ratio, original credit score, number of units, the percentage of the mortgage that is insured privately, and dummy variables for origination month, property type, occupancy status, loan purpose, and state. In addition, all loan and borrower controls are interacted with the post dummy. Additional controls include dummies for whether the loan was originated through the retail channel, mortgage property type, and whether the borrower was a first-time buyer, as well as interactions of each with the post dummy. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. Columns 1 and 2 contain the full sample of sellers while columns 3 and 4 contain only the sample of commercial banks for which data are available. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 16: Likelihood of Cash Window Sale Split by Loan-to-Value Ratio

	Dependent Variable: Sale to Cash Window = 1			
	High LTV		Low LTV	
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	0.02	0.00	0.02	-0.01
	(0.03)	(0.01)	(0.04)	(0.01)
Small Seller × Post	0.05***	0.06	0.04**	0.07*
	(0.02)	(0.04)	(0.02)	(0.04)
Extra-Small Seller × Post	0.01	0.01	0.02	0.02
	(0.01)	(0.02)	(0.01)	(0.02)
Sample	Full	Banks	Full	Banks
Loan and Borrower Controls	Y	Y	Y	Y
Seller Controls	N	Y	N	Y
Month Fixed Effects	Y	Y	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	469,833	255,026	1,184,069	650,384
Number of Clusters	368	133	369	134
R-squared (within)	0.02	0.04	0.02	0.04

This table presents estimates from linear regressions of cash window sales on loan, borrower, and seller characteristics. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period in columns 1 and 3 includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Due to data availability, the sample period in columns 2 and 4 includes loans sold to Fannie Mae between June 2012 and April 2013. Loan and borrower controls include the original LTV ratio, original credit score, number of units, the percentage of the mortgage that is insured privately, and dummy variables for origination month, property type, occupancy status, loan purpose, and state. In addition, all loan and borrower controls are interacted with the post dummy. Additional controls include dummies for whether the loan was originated through the retail channel, mortgage property type, and whether the borrower was a first-time buyer, as well as interactions of each with the post dummy. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. Columns 1 and 2 contain the full sample of sellers while columns 3 and 4 contain only the sample of commercial banks for which data are available. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 17: Servicing Retention and Compensation

Dependent Variables:	Originator-Servicer Affiliation		Interest Rate - Pass-Through Rate	
Sample:	Cash Window	MBS Swap	Cash Window	MBS Swap
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	0.05	-0.01	0.03	-0.01
	(0.06)	(0.03)	(0.04)	(0.03)
Small Seller × Post	0.06	0.03	0.03	0.03
	(0.04)	(0.02)	(0.04)	(0.02)
Extra-Small Seller × Post	0.04	-0.03	0.01	0.04
	(0.04)	(0.02)	(0.04)	(0.03)
Sample	Full	Full	Full	Full
Loan and Borrower Controls	Y	Y	Y	Y
Seller Controls	N	N	N	N
Month Fixed Effects	Y	Y	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	434,017	1,182,176	293,044	633,776
Number of Clusters	353	93	345	91
R-squared (within)	0.03	0.11	0.07	0.07

This table presents estimates from linear regressions of originator/servicer dummies (columns 1 and 2) and the difference between loan interest rates and pass-through rates (columns 3 and 4) on loan, borrower, and seller characteristics split by borrower credit score. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period includes June 2012 through April 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Loan and borrower controls include the original LTV ratio, original credit score, number of units, the percentage of the mortgage that is insured privately, and dummy variables for origination month, property type, occupancy status, loan purpose, and state. In addition, all loan and borrower controls are interacted with the post dummy. Additional controls include dummies for whether the loan was originated through the mortgage property type, and whether the borrower was a first-time buyer, as well as interactions of each with the post dummy. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. All columns contain the full sample of sellers. The samples in columns 1 and 3 condition on sale to the cash window, while columns 2 and 4 condition on MBS swap sales. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 18: Relative Change in Cash Window vs. MBS Swap Volumes

Dependent Variable:	$\frac{\Delta \text{Vol(CW-Swaps)}}{\text{Vol(Total)}}$			
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	0.03	0.02	0.02	0.01
	(0.06)	(0.06)	(0.06)	(0.02)
Small Seller × Post	0.16***	0.14***	0.15***	0.18**
	(0.05)	(0.05)	(0.05)	(0.09)
Extra-Small Seller × Post	0.07*	0.06	0.06*	0.08**
	(0.04)	(0.03)	(0.03)	(0.04)
Sample	Full	Full	Banks	Banks
Loan Pool Controls	N	Y	Y	Y
Seller Controls	N	N	N	Y
Month Fixed Effects	N	N	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	4,185	4,185	4,185	1,576
Number of Clusters	352	352	352	133
R-squared (within)	0.02	0.03	0.04	0.09

This table presents estimates from regressions of the difference between cash window volume and MBS swap volume scaled by total volume of 30-year single-family MBS sales on pool and seller characteristics. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Loan pool controls include the average LTV ratio, credit score, number of units, and shares of single-family, occupancy status, loan purpose, and Census regions for each seller in a given month. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. All regressions also include the unemployment rate in each state averaged across all loans for each seller in a given month. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 19: Relative Change in Volumes: Split by Credit Scores and LTV ratios

Dependent Variables:	$\frac{\Delta \text{Vol(CW-Swaps)}}{\text{Vol(Total)}}$			
Sample:	Low Credit Score	High Credit Score	High LTV Ratio	Low LTV Ratio
Explanatory Variables:	(1)	(2)	(3)	(4)
Medium Seller × Post	-0.01	0.03	-0.07**	0.09*
	(0.01)	(0.06)	(0.04)	(0.06)
Small Seller × Post	0.00	0.15***	(0.02)	0.18***
	(0.01)	(0.05)	(0.03)	(0.05)
Extra-Small Seller × Post	0.00	0.07**	-0.06**	0.12***
	(0.01)	(0.03)	(0.04)	(0.04)
Sample	Full	Full	Full	Full
Loan Pool Controls	Y	Y	Y	Y
Seller Controls	N	N	N	N
Month Fixed Effects	Y	Y	Y	Y
Seller Fixed Effects	Y	Y	Y	Y
Number of Observations	4,185	4,185	4,185	4,185
Number of Clusters	352	352	352	352
R-squared (within)	0.17	0.04	0.23	0.08

This table presents estimates from regressions of the difference between cash window volume and MBS swap volume scaled by total volume of 30-year single-family MBS sales on pool and seller characteristics. Medium, small, and extra-small sellers denote financial institutions in the 6-25, 25-100, and 100+ market shares of the Fannie Mae 30-year single-family MBS market. The omitted category in each regression includes large (1-5) sellers. The sample period includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Loan pool controls include the average LTV ratio, credit score, number of units, and shares of single-family, occupancy status, loan purpose, and Census regions for each seller in a given month. Seller controls are available for all commercial banks and include lagged values of the log of total assets, and ratios of deposits, equity capital, cash, and non-performing loans-to-assets by seller-quarter. All regressions also include the unemployment rate in each state averaged across all loans for each seller in a given month. Standard errors are clustered by seller. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

Table 20: State-Level Credit Supply

Dependent Variable:	log(Average Loan Size)	Average Interest Rate	log(Total Loan Volume)
Explanatory Variables:	(1)	(2)	(3)
Large-Seller Concentration × Post	-0.11*	-0.02	-0.54**
	(0.06)	(0.06)	(0.20)
Average State-Level Characteristics	Y	Y	Y
Number of Observations	663	663	663
Number of Clusters	51	51	51
R-squared (within)	0.62	0.98	0.82

This table presents estimates from regressions of average loan size, average interest rates, and total loan volume issuance by state-month. Each regression includes state fixed effects, month fixed effects, average state-level borrower and loan characteristics (with post-November 2012 interactions), and state unemployment rates. Large HHI Index equals the Herfindahl-Hirschman Index for large sellers in a given month (sum of squared market shares, taking a value between 0 and 1). The sample period includes March 2012 through May 2013, and the Post variable takes a value equal to 1 after November 2012 and 0 else (note that September and October 2012 are omitted from the regression). Standard errors are clustered by state. ***, **, and * indicate significance at the 1, 5, and 10% levels, respectively.

3 Survey Forecasts and Bond Return Decompositions

Abstract

Unexpected excess bond returns can be decomposed into news about future short-term interest rates and news about future excess bond returns. This paper uses consensus survey forecasts to directly measure expectations of future interest rates and provides two new preliminary empirical facts: (1) News about future short-term interest rates explains relatively more of the variation in unexpected excess bond returns for short-maturity bonds relative to long-maturity bonds. (2) The share of news explained by future short-term interest rates increases with horizon for all maturities.

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3.1 Introduction

What drives fluctuations in bond returns? Under the Expectations Hypothesis of the term structure of interest rates, long-term bond yields should fully reflect expectations of short-term interest rates over the life of the bond. However, a long literature in finance has found strong evidence against the validity of the hypothesis (Fama and Bliss (1987), Campbell and Shiller (1991), Cochrane and Piazzesi (2005)).

A starting point for studying the drivers of the variance of bond returns is the decomposition that states that unexpected excess bond returns must reflect either unexpected changes in expectations of future short-term interest rates (interest-rate news) or future excess returns (excess-return news). Under the Expectations Hypothesis, interest-rate news should be the primary driver of the unconditional variance of unexpected returns on long-term bonds. Campbell and Ammer (1993) find that excess-return news and inflation news (as a component of interest-rate news) explained large fractions of the unconditional variance of excess bond returns in the 1970s and 1980s. Using survey forecasts to directly proxy for expectations, Duffee (2018) finds less of a role for inflation news, while Crump, Eusepi, and Moench (2018) strongly reject the Expectations Hypothesis at long horizons for the level and changes in realized forward rates.

Given this recent evidence concerning the importance of survey forecasts, this paper revisits the fundamental question of the validity of the Expectations Hypothesis in the context of variance decompositions of bond returns. Relative to the existing literature, this paper measures expectations of *both* unexpected bond returns and future short-term interest rates using survey forecasts in order to understand the relative importance of interest-rate news *across horizons.* Two new empirical facts emerge from this analysis:

1. *Interest-rate news explains relatively more of the variation in unexpected excess bond returns for short-maturity bonds relative to long-maturity bonds.* For example, at a one-month horizon, these shares are approximately 89%, 41%, and 11% for 6-month, 2-year, and 10-year bonds, respectively.

2. *The relative importance of interest-rate news increases with horizon for all maturities.* For example, the variance share of unexpected excess returns on 5- and 10-year bonds explained by interest-rate news increases from 16% and 11% at a one-month horizon to over 50% and 33% after eighteen months, respectively.

This paper takes the approach of measuring expectations of interest rates using survey forecasts of market participants. A number of recent papers have employed survey forecasts in similar settings as alternatives to Vector Autoregressions (VAR) that impose the assumption of full-information rational expectations (Duffee (2018), Buraschi, Piatti, and Whelan (2018), Crump et al. (2018)). Relative to these papers, I exploit strong correlations of the term structure of interest-rate forecasts to factors in the yield curve to interpolate survey forecasts across maturity, horizon, and time. More precisely, I use consensus forecasts of nominal interest rates made by participants in the Blue Chip Financial Forecasts® survey (henceforth Blue Chip) as an empirical proxy for interest-rate expectations. I bootstrap both current and expected zero-coupon yield curves from Treasury constant maturity yields following Balduzzi, Marcus, and Zhao (2019). With these estimates in hand, I then utilize forecasts of the full term structure of interest rates to directly measure expectations of both future short-term interest rates *and* future excess bond returns across different maturities.

A long literature in finance has used present-value models of asset prices to decompose unexpected returns into news about cash flows and news about discount rates. An early approach, pioneered by Campbell and Shiller (1988a), Campbell and Shiller (1988b), Campbell (1991), and Campbell and Ammer (1993), obtains expectations of excess returns from a VAR and backs out cash flow news as a residual. These papers find that a substantial fraction of the variation in unexpected returns in both stocks and bonds is due to news about future expected returns (risk premia). In addition, Campbell and Ammer (1993) argue that news about future inflation rates explained a large fraction of unexpected bond returns from the 1950s-1980s.

These early papers embedded the full-information rational expectations assumption in order to project forward expectations of asset returns into the future. However, a more recent literature has suggested caution in drawing conclusions from model-based asset-price decompositions. In particular, measures of subjective expectations of asset prices, proxied by survey forecasts of investors and professional economists, have been increasingly utilized to study decompositions.[51] At a minimum, these papers suggest that the way in which expectations are modeled can crucially change interpretations of results from asset-price decompositions.

Unexpected bond returns must reflect unexpected news about future discount

[51]Recent papers have performed these decompositions for a range of asset classes: stocks (Chen, Da, and Zhao (2013), De la O and Myers (2018)), bonds (Cieslak (2018), Duffee (2018), Crump et al. (2018)), and currencies (Stavrakeva and Tang (2018)).

rates, as nominal cash flows are known in advance. Campbell and Ammer (1993) use bond-price identities to further decompose discount rate news of unexpected bond returns into news about future short-term real rates, news about future inflation, and news about future excess bond returns. Using a VAR of financial market variables, they find that the bulk of the variation in unexpected returns before the 1980s was due to news about future excess returns and inflation. However, these model-based decompositions might not accurately reflect beliefs of market participants, either due to omitted factors (Chen and Zhao (2009)) leading to model misspecification, or due to spurious assumptions of the statistical model used to construct expectations (Duffee (2018)).

A recent empirical literature has exploited information in survey forecasts to provide insights on asset price movements. Chen and Zhao (2009) argue that incorrectly modeling discount rate news can lead to overattributing its relative importance in return decompositions. In addition, a number of papers have found substantial deviations from rational expectations among investors and professional forecasters across different asset classes (Greenwood and Shleifer (2014), Piazzesi, Salomao, and Schneider (2015), Stavrakeva and Tang (2018), Cieslak (2018)). Survey forecasts have also been used to shed light on asset-price decompositions. Chen et al. (2013) find that news about expected future cash flows explain most the variance of excess stock returns. Balduzzi and Lan (2014) use survey forecasts to construct news and find that risk premium news explain a large fraction of the variance of returns. De la O and Myers (2018) find that news about cash flows (dividend growth rates) rather than discount rates can explain variation in unexpected stock returns. Duffee (2018) measures inflation news directly using survey forecasts, rather than via a VAR, and finds that it explains a small fraction of the variance of yield innovations. Crump et al. (2018) use all known survey forecasts and a model with time-varying means to construct expectations of future short-term real rates and future inflation rates. They find that the fraction of the variance of forward rates (both levels and changes) explained by expectations of future short-term real rates is high at short horizons, but decreases at long horizons. Buraschi et al. (2018) use the cross section of Blue Chip survey forecasts of interest rates to document large deviations from full-information rational expectations. Relative to this literature, this paper uses consensus survey forecasts of interest rates to perform unconditional variance decompositions of unexpected changes in excess bond returns.

3.2 Bond Return Decompositions

The yield on a zero-coupon bond maturing in n periods, $y_t^{(n)}$, can be decomposed into expectations of future nominal short-term rates and future excess returns (Campbell and Ammer (1993)):

$$y_t^{(n)} = \frac{1}{n}\sum_{i=1}^{n} y_{t+i-1}^{(1)} + \frac{1}{n}\sum_{n=1}^{n} ex_{t+i}^{(n-i+1)} \tag{7}$$

where t denotes date t, $y_t^{(1)}$ is the yield on a nominal one-period zero-coupon bond and $ex_{t+1}^{(n)}$ is the log return from holding a zero-coupon bond maturing in n periods from t to $t+1$ in excess of the risk-free rate. Taking expectations at time t and imposing the Law of Iterated Expectations gives:

$$y_t^{(n)} = \frac{1}{n}\sum_{i=1}^{n} \mathrm{E}_t[y_{t+i-1}^{(1)}] + \frac{1}{n}\sum_{i=1}^{n} \mathrm{E}_t[ex_{t+i}^{(n-i+1)}] \tag{8}$$

The excess return to holding a bond maturing in n periods from date t to $t+h$ equals:

$$ex_{t+h}^{(n)} = ny_t^{(n)} - (n-h)y_{t+h}^{(n-h)} - hy_t^{(h)} \tag{9}$$

Plugging in (7):

$$\begin{aligned}
ex_{t+h}^{(n)} &= n\left(\frac{1}{n}\sum_{i=1}^{n} \mathrm{E}_t[y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}]\right) - (n-h)\left(\frac{1}{n-h}\sum_{i=1+h}^{n} \mathrm{E}_{t+h}[y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}]\right) - hy_t^{(h)} \\
&= \sum_{i=1}^{h} \mathrm{E}_t[y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}] + \sum_{i=1+h}^{n}\left(\mathrm{E}_t[y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}] - \mathrm{E}_{t+h}[y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}]\right) - hy_t^{(h)} \\
&= -(\mathrm{E}_{t+h} - \mathrm{E}_t)\sum_{i=1+h}^{n}\left(y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}\right) + \mathrm{E}_t[ny_t^{(n)} - (n-h)y_{t+h}^{(n-h)} - hy_t^{(h)}]
\end{aligned} \tag{10}$$

Rearranging terms then gives:

$$ex_{t+h}^{(n)} - \mathrm{E}_t[ex_{t+h}^{(n)}] = -(\mathrm{E}_{t+h} - \mathrm{E}_t)\sum_{i=1+h}^{n}\left(y_{t+i-1}^{(1)} + ex_{t+i}^{(n-i+1)}\right) \tag{11}$$

Innovations in excess returns, $e_{t+h}^{(n)}$, can then be expressed as news about future nominal short-term interest rates and future excess bond returns:

$$e_{t+h}^{(n)} = \eta_{y,t+h}^{(n)} + \eta_{ex,t+h}^{(n)} \tag{12}$$

Under the "Expectations Hypothesis" of the term structure of interest rates, $\eta^{(n)}_{ex,t+h} = 0$. The unconditional variance of innovations in excess returns is given by:

$$\text{var}(e^{(n)}_{t+h}) = \text{cov}(\eta^{(n)}_{y,t+h}, e^{(n)}_{t+h}) + \text{cov}(\eta^{(n)}_{ex,t+h}, e^{(n)}_{t+h}) \tag{13}$$

where $\text{cov}(\eta^{(n)}_{ex,t+h}, e^{(n)}_{t+h}) = \text{cov}(e^{(n)}_{t+h} - \eta^{(n)}_{y,t+h}, e^{(n)}_{t+h})$. Scaling by the variance of return innovations gives:

$$1 = \frac{\text{cov}(\eta^{(n)}_{y,t+h}, e^{(n)}_{t+h})}{\text{var}(e^{(n)}_{t+h})} + \frac{\text{cov}(\eta^{(n)}_{ex,t+h}, e^{(n)}_{t+h})}{\text{var}(e^{(n)}_{t+h})} \tag{14}$$

Each of the terms on the right-hand side of (14) can be interpreted as coefficients from regressions of each component on excess return innovations:

$$\begin{aligned} \beta^{(n)}_{y}(h) &= \frac{\text{cov}(\eta^{(n)}_{y,t+h}, e^{(n)}_{t+h})}{\text{var}(e^{(n)}_{t+h})} \\ \beta^{(n)}_{ex}(h) &= \frac{\text{cov}(\eta^{(n)}_{ex,t+h}, e^{(n)}_{t+h})}{\text{var}(e^{(n)}_{t+h})} \end{aligned} \tag{15}$$

Campbell and Ammer (1993) propose an alternative formulation:

$$\text{var}(e^{(n)}_{t+h}) = \text{var}(\eta^{(n)}_{y,t+h}) + \text{var}(\eta^{(n)}_{ex,t+h}) + 2\text{cov}(\eta^{(n)}_{ex,t+h}, \eta^{(n)}_{y,t+h}) \tag{16}$$

Scaling by the variance of excess-return innovations gives:

$$1 = \frac{\text{var}(\eta^{(n)}_{y,t+h})}{\text{var}(e^{(n)}_{t+h})} + \frac{\text{var}(\eta^{(n)}_{ex,t+h})}{\text{var}(e^{(n)}_{t+h})} + \frac{2\text{cov}(\eta^{(n)}_{ex,t+h}, \eta^{(n)}_{y,t+h})}{\text{var}(e^{(n)}_{t+h})} \tag{17}$$

3.3 Methodology

3.3.1 *Constructing Zero-Coupon Yields*

Balduzzi et al. (2019) bootstrap the zero-coupon curve from prices of off-the-run Treasury securities by employing a first-order approximation of spot rates to yields.[52] In particular, they model the yield of a Treasury bond at time t maturing in τ_N periods, $y_c(t, \tau_N)$, as a polynomial of cash flows cf_n and maturity τ_n:

$$y_c(t, \tau_N) = \sum_{i=0}^{I} x_i(t) \sum_{n=1}^{N} \left(\frac{cf_n \tau_n}{\sum_{n=1}^{N} cf_n \tau_n} \right) \tau_n^i \tag{18}$$

They then extract factors at each date t, $\hat{x}_i(t)$, and fit zero-coupon yields across all maturities:

$$y_z(t, \tau_N) = \sum_{i=0}^{I} \hat{x}_i(t) \tau_N^i \tag{19}$$

Since participants in the Blue Chip survey forecast "constant maturity" Treasury yields, I follow their procedure to bootstrap a zero-coupon curve using these yields instead.[53] To be precise, I estimate equation 18 where cf_n is the coupon rate on an n-month constant maturity Treasury bond. As these yields are derived from on-the-run securities, I assume that they trade at par, and thus the coupon rate equals the bond yield. I use coupon rates to match forecasted maturities from the Blue Chip survey from 1988-2018 of the following maturities: 3-month, 6-month, 1-year, 2-year, 5-year, and 10-year bonds. From these regressions, I extract factors $\hat{x}_i^c(t)$ from a polynomial of

[52]Relative to Gurkaynak, Sack, and Wright (2006), their yields exhibit substantially less volatility and are less affected by outliers.

[53]These yields are modeled as a cubic spline with closing bids of Treasury on-the-run securities as inputs.

order 3 ($I = 3$), where the superscript c denotes that these are "current" factors. Plots of these factors are shown in Figure 22. For each order of the polynomial, I calculate root mean square errors (RMSE) for fitted yields relative to constant maturity yields. I choose a polynomial of order 3 in order to avoid issues of over-fitting and as the fits are marginally improved by increasing the order of the polynomial (see top panel of Table 22). With the current factors in hand, I obtain fitted values of zero-coupon yields from equation 19, $\hat{y}_z(t, \tau_N)$, across maturities $N = 1, ..., 120$. These extracted zero-coupon yields correspond closely with those obtained from Balduzzi et al. (2019), even though the model inputs differ (see Figure 23).

3.3.2 *Constructing Interest-Rate Expectations*

Each of the terms in equation 12 presents a challenge to measure when taken to the data. One approach, developed by Campbell and Ammer (1993), is to estimate a VAR of financial variables and project forward expectations to obtain news about each component in the identity: excess bond returns, real interest rates, and inflation rates.[54] They find that both inflation news and excess return news explain the bulk of the variation in unexpected returns. Duffee (2018) argues that their approach assumes cointegration of inflation rates and bond yields, leading to substantially larger attribution of inflation news than suggested by survey forecasts. His approach directly measures innovations in inflation rates from Blue Chip survey forecasts. As the focus of his paper is to measure the share of yield innovations explained by in-

[54]They use the Fisher relation to further decompose nominal interest rate news into news about inflation and real interest rates. Their VAR includes excess stock returns, the real interest rate, the change in nominal interest rate, the long-short yield spread, the dividend-price ratio, and the relative bill rate.

flation news, he obtains yield innovations from a statistical model in order to obtain conservative estimates of the inflation-variance ratio relative to those obtained via survey forecasts. He finds ratios of 10-20%, suggesting a significantly smaller role for inflation news than found in Campbell and Ammer (1993). A number of other papers have utilized survey forecasts to discipline the dynamics of a statistical model (D. H. Kim and Wright (2005), D. H. Kim and Orphanides (2012), Piazzesi et al. (2015), Crump et al. (2018)).

In contrast to these papers, my focus is to accurately calculate news about expected future nominal interest rates, rather than inflation news, using a parsimonious extension of the model used to bootstrap the zero-coupon yield curve. The main identifying assumption of this approach is that this model accurately captures the cross-sectional fit of survey forecasts to factors of the yield curve on forecast dates across both maturities and forecast horizons. I assume that this relationship holds as well on non-forecast dates, and project on the current factors of the yield curve to obtain fitted survey forecasts on all dates.

Consensus (mean) forecasts of Treasury rates from the Blue Chip survey are used to proxy for interest-rate expectations. The Blue Chip is a closely followed survey of market participants produced each month since 1982.[55] Approximately 45 economists are asked their views on the future trajectory of a range of financial market variables. In particular, participants report their expectation of average Treasury "constant maturity" rates for short horizons (1-6 quarters ahead) at a monthly frequency and for long horizons (1-6 and 7-11 years ahead) semi-annually. In order to ensure consistent

[55]Cieslak (2018) documents significant coverage of the survey in minutes of FOMC meetings.

coverage of responses over time, I utilize forecasts from 1988-2018 of the following maturities: 3-month, 6-month, 1-year, 2-year, 5-year, and 10-year bonds. Full details of the coverage of Blue Chip interest-rate forecasts are given in Table 21. I follow the same procedure for bootstrapping the zero-coupon curve from current constant maturity yields, with the only difference being that I perform estimation with the information set available to forecasters on survey forecast dates, typically one week before the release date. The time-t forecast of a Treasury yield with maturity τ_N at horizon h is similarly modeled as a polynomial function of cash flows and maturity:

$$y_c^f(t, \tau_N, h) = \sum_{i=0}^{I} x_i^f(t, h) \sum_{n=1}^{N} \left(\frac{cf_n \tau_n}{\sum_{n=1}^{N} cf_n \tau_n} \right) \tau_n^i \tag{20}$$

From this procedure, I extract "forecast" factors at each date t, $\hat{x}_i^f(t, h)$, where the superscript f denotes that this is a forecast of a particular Treasury security's yield. These factors at the nearest horizon (1- to 3-months ahead) are plotted in Figure 24. Fitted forecasts from this routine almost perfectly fit actual forecasts across maturities and horizons (Figures 25 and 26). With the factors in hand, I then fit *forecasts* of zero-coupon yields across all maturities at forecast horizons on survey dates:

$$y_z^f(t, \tau_N, h) = \sum_{i=0}^{I} \hat{x}_i^f(t, h) \tau_N^i \tag{21}$$

Fitted values from this procedure, $\hat{y}_z^f(t, \tau_N, h)$, can be thought of as h-period ahead forecasts of zero-coupon yields made at date t for a bond of maturity τ_N. I follow a similar procedure of fitting Treasury forecasts by choosing a third-order polynomial in the estimation of equation 20 (see lower panel of Table 22). Figure 27 shows plots of fitted survey forecasts of zero-coupon yields on survey forecast dates. There is not much dispersion in short-term forecasts for all maturities (first two rows). However, long-term forecasts across horizons narrow at the end of monetary policy tightening cycles and widen during periods of monetary easing

As survey forecasts of interest rates are only available at a monthly frequency, I implement a simple interpolation procedure that relies on fitting survey forecasts to the *current* factors of the yield curve, $\hat{x}_i^c(t)$. This estimation procedure fits survey forecasts on forecast dates as a polynomial function of forecast horizon h and maturity τ_N.

$$\begin{aligned} y_z^f(t, \tau_N, h) &= \sum_{i=0}^{I} \left(\alpha_i(h,t) + \beta_i(h,t)\hat{x}_i^c(t)\right)\tau_N^i \\ &= \sum_{i=0}^{I} \alpha_i(h,t)\tau_N^i + \sum_{i=0}^{I} \beta_i(h,t)\hat{x}_i^c(t)\tau_N^i \end{aligned} \tag{22}$$

The coefficient of polynomial term i, $\beta_i(h,t)$, is modeled as a polynomial function of forecast horizon and a time trend. I allow for full flexibility of the polynomial by considering three separate specifications: the coefficients only vary by forecast horizon, $\beta_i^{(0)}(h)$; the coefficients vary by forecast horizon and there is time variation in the mean, $\beta_i^{(1)}(h,t)$; all coefficients vary by forecast horizon and time, $\beta_i^{(2)}(h,t)$. The routine proceeds by running stepwise fit regressions that progressively exclude regressors whose t-ratios are smaller than one in absolute value for each value P and K. I then select the regression specification that maximizes the adjusted R-squared statistic and choose orders P and K of each polynomial. The functional form for each case is given by:

$$\begin{aligned}
\beta_i^{(0)}(h) &= \sum_{p=0}^{P} \beta_{i,p} h^p \\
\beta_0^{(1)}(h,t) &= \sum_{k=1}^{K} \left(\sum_{p=1}^{P} \beta_{0,p} h^p \right) t^k \\
\beta_i^{(1)}(h,t) &= \sum_{p=1}^{P} \beta_{i,p} h^p \qquad \forall i \in \{1, ..., I\} \\
\beta_i^{(2)}(h,t) &= \sum_{k=1}^{K} \left(\sum_{p=0}^{P} \beta_{i,p} h^p \right) t^k
\end{aligned} \tag{23}$$

Table 23 gives results from estimation of equation 22 for these three cases allowing orders of both the horizon and time trend polynomials to vary. In all cases I use a third-order polynomial of the maturity τ_N ($I = 3$) in order to estimate the routine. The fit marginally improves beyond a second-order polynomial in terms of adjusted R-squares and RMSEs. I make the conservative choice of selecting fourth-order polynomials for both horizons and time trends ($P = 4$, $K = 4$) in order to avoid over-fitting. In the robustness section, I show that the main results hold for different orders of the polynomial. For each case, I calculate the sum of coefficients for all time-invariant ($\beta_i(h)$) and time-varying ($\beta_i(h,t)$) terms. I choose case 3 as the main specification for my analysis, as the sum of coefficients for all time-varying and time-invariant terms are significant at the 95th percentile. Results are robust to choosing one of the other two cases. I then project estimated loadings on the factors on all dates in order to construct expectations for each horizon h and maturity τ_N at a daily frequency. Figures 28 and 29 plot "actual" and fitted survey forecasts of zero-coupon yields on survey dates. The former forecasts are those obtained from fitting equation 20 while the latter are those estimated from 21.

3.4 Results

3.4.1 *Variance Decompositions*

This section presents results from the decomposition exercise. The objective of this analysis is to quantify the shares of the variance of excess return innovations explained by interest rate news relative to excess return news. I estimate equation 20 for separate maturities at horizons of 1-24 months ahead. News about future short-term interest rates are obtained from estimation of equation 22. I similarly obtain news about future yields and construct forecasts of excess bond returns at each date. Excess-return news are obtained as a residual. Results from the variance decompositions of excess-return innovations are shown in Figures 30 and 31, with darker lines corresponding to shorter-maturity bonds.

The first main result is that expectations of future short-term interest rates explain relatively more of the variation in unexpected excess bond returns for short-maturity bonds relative to long-maturity bonds across all forecast horizons. For example, at a one-month horizon, these shares are approximately 89%, 41%, and 11% for 6-month, 2-year, and 10-year bonds, respectively. Results for these estimates are all statistically significant at 99% confidence intervals. Table 24 includes coefficient estimates of $\beta_y^{(n)}(h)$ with Newey-West standard errors corrected for overlapping observations.

The second main result is that the share of news explained by future short-term interest rates increases with horizon for all maturities. The variance shares of unexpected excess returns on 6-month, 1-year, and 2-year bonds increases from 89%, 73%, and 41% at a one-month horizon to 98% at horizons just before maturity. For 10-year bonds, the variance shares explained by interest-rate news increases from 11% at a one-month horizon to over 33% after eighteen months. The majority of the variation in unexpected excess returns on 10-year bonds is due to news about future excess returns.

Figure 32 further illustrates these two main results graphically by depicting excess-return innovations (gray line) and interest-rate news (black line) over time. The pattern across all sub-figures is that interest-rate news tracks innovations more closely for 1- and 2-year bond yields than for 10-year bond yields at both horizons (1-month and 9-months ahead). However, the fluctuations in interest-rate news follow innovations relatively more closely at 9-month horizons.

The results from this exercise present new evidence on validity of the Expectations Hypothesis of interest rates. Using survey forecasts to proxy for interest-rate expectations, I show that the Expectations Hypothesis can explain relatively more of the variation in unexpected news about excess bond returns for short-maturity bonds than for long-maturity bonds. Stated differently, unexpected shocks to interest rates persist for short-maturity bonds, and thus explain most of the variation in returns. These results are broadly consistent with Cieslak (2018), who finds that unexpected excess returns explain most of realized excess bond returns for two-year Treasury bonds. Interestingly, while ER news matter relatively more for long-maturity bonds at short horizons, as the maturity of bonds shortens, IR news explains increasingly larger shares of the variance than ER news.

3.5 Extensions

3.5.1 *Comparison to Other Papers*

Results from the baseline regressions apply to the unconditional variance of unexpected excess bond returns. However, these results do not speak to potential heteroskedasticity (Balduzzi and Lan (2014)). In Tables 26-29, I present results for the relative shares of IR news and ER news across different sub-samples. The shares of IR news explains relatively more of the variance of unexpected excess returns in the post-1998 period. In addition, excluding the zero-lower bound (post-2008) increases all estimates, especially for long-maturity bonds. These results are indicative of IR news explaining innovations in excess bond returns during periods where monetary policy was less constrained.

3.5.2 *Comparison to Other Papers*

These results are broadly consistent with the findings of Crump et al. (2018), who show that the share of the variance of the *change in forward rates* due to interest-rate news is generally larger for short-maturity bonds, although the magnitudes fall significantly beyond two years. They also show that shares of interest rate news explain less of this variation at changes over longer horizons (1-month relative to 12-month changes). My empirical setting differs from their paper in that I construct *unexpected* changes in excess bond returns rather than *realized* changes in interest rates. This distinction is important, as I construct subjective forecasts of excess returns from surveys across all maturities and horizons, $\mathrm{E}_t[ex_{t+h}^{(n)}]$. The economic magnitudes of my estimates are generally larger, suggesting that interest-rate news plays a relatively larger role in explaining *unexpected* excess returns than realized changes.

Campbell and Ammer (1993) decompose the variance of excess-return innovations into the sum of the variances and covariances of individual components. Duffee (2018) updates their paper for the period March 1987-December 2013, broadly corresponding to the sample in this paper. He finds that inflation news explain the bulk of the variation in excess-return innovations of 10-year bond yields. I perform a similar decomposition and calculate variance shares of each component in equation 17 using fitted survey forecasts. Results from this exercise are shown in Table 30. In contrast to the Campbell and Ammer (1993) paper, I find that at a one-month horizon, news about future excess returns are the primary driver of excess-return innovations of 10-year bonds (87%). Duffee (2018) estimates a VAR of survey forecasts from the Survey of Professional Forecasters and calculates the variance shares of yield innovations ($y_t^{(n)} - \mathrm{E}_{t-1}[y_t^{(n)}]$). He finds that at short maturities the variance of news about real interest rates is the primary driver of innovations, but he is unable to distinguish relative shares for longer maturities. My estimates for 6-month and 1-year bonds are consistent with his findings, however I attribute more of the variation in innovations in excess returns on long-term bonds to excess-return news across all horizons.

3.5.3 *Choice of Polynomials*

In this section, I explore whether the choice of polynomials affects the main results of the paper. In my baseline analysis, I utilize a polynomial of order 3 to bootstrap the

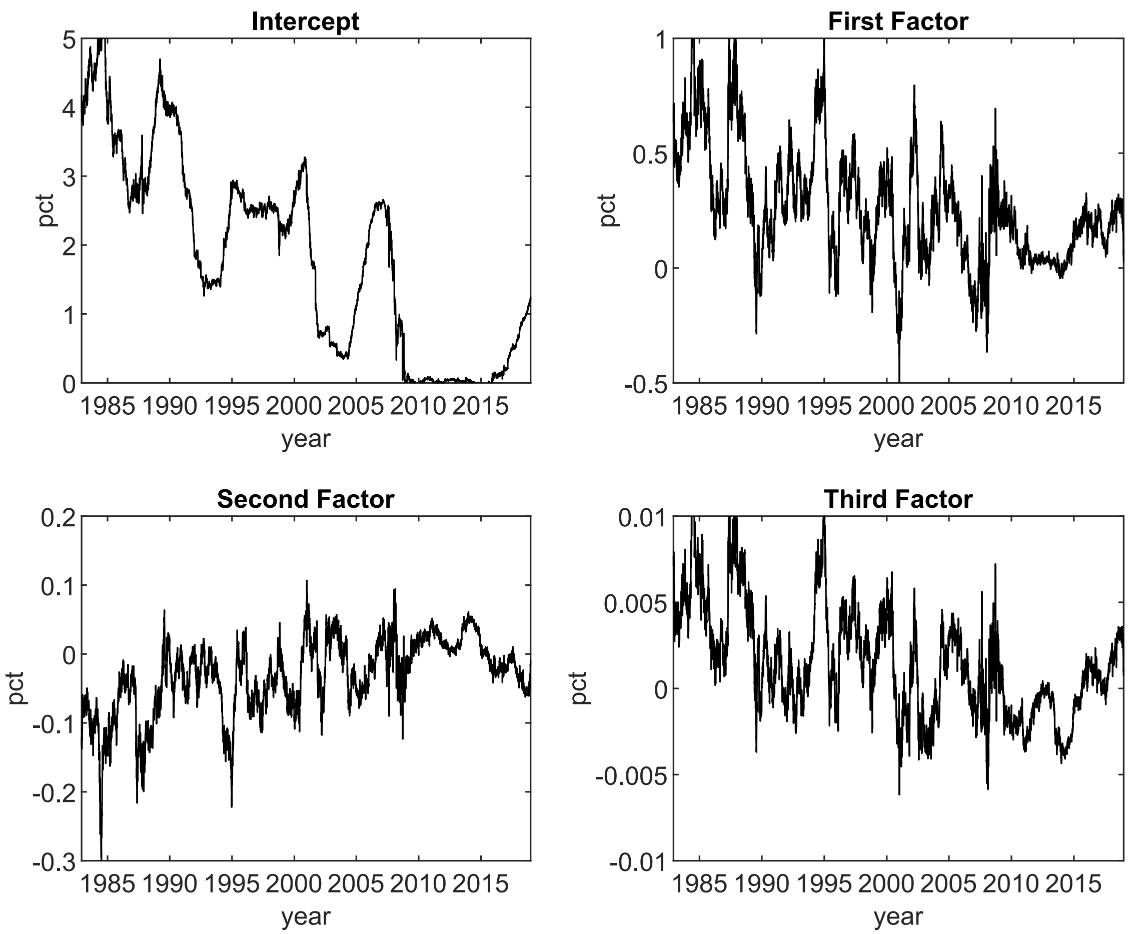
Treasury yield curve in order to reduce concerns of overfitting. The variance decomposition of excess-return innovations is robust to a fourth-order polynomial (Table A.3.1). A second choice is to model the relationship between the forecast factors and current factors. In my baseline analysis, I allow for a fully flexible polynomial of forecast horizon h and time trend t. In Table A.3.2 I show that results from a polynomial specification where the coefficients in equation 16 vary by forecast horizon, but there is only time variation in the mean, do not affect the main conclusions.

3.6 Conclusion

Understanding the drivers of the variation in unexpected returns has presented an empirical challenge to researchers in finance. This paper measures expectations of interest rates directly from survey forecasts, rather than rely on a statistical model embedded with the full-information rational expectations assumption. Relative to the existing literature, I document a large role for news about future nominal short-term interest rates in explaining the variation of unexpected bond returns. At short horizons, this news is relatively more important for short-maturity relative to long-maturity bonds. However, interest-rate news explains an increasingly larger fraction as the forecast horizon increases. These results contribute to an existing literature documenting the importance of subjective expectations in explaining asset-price movements.

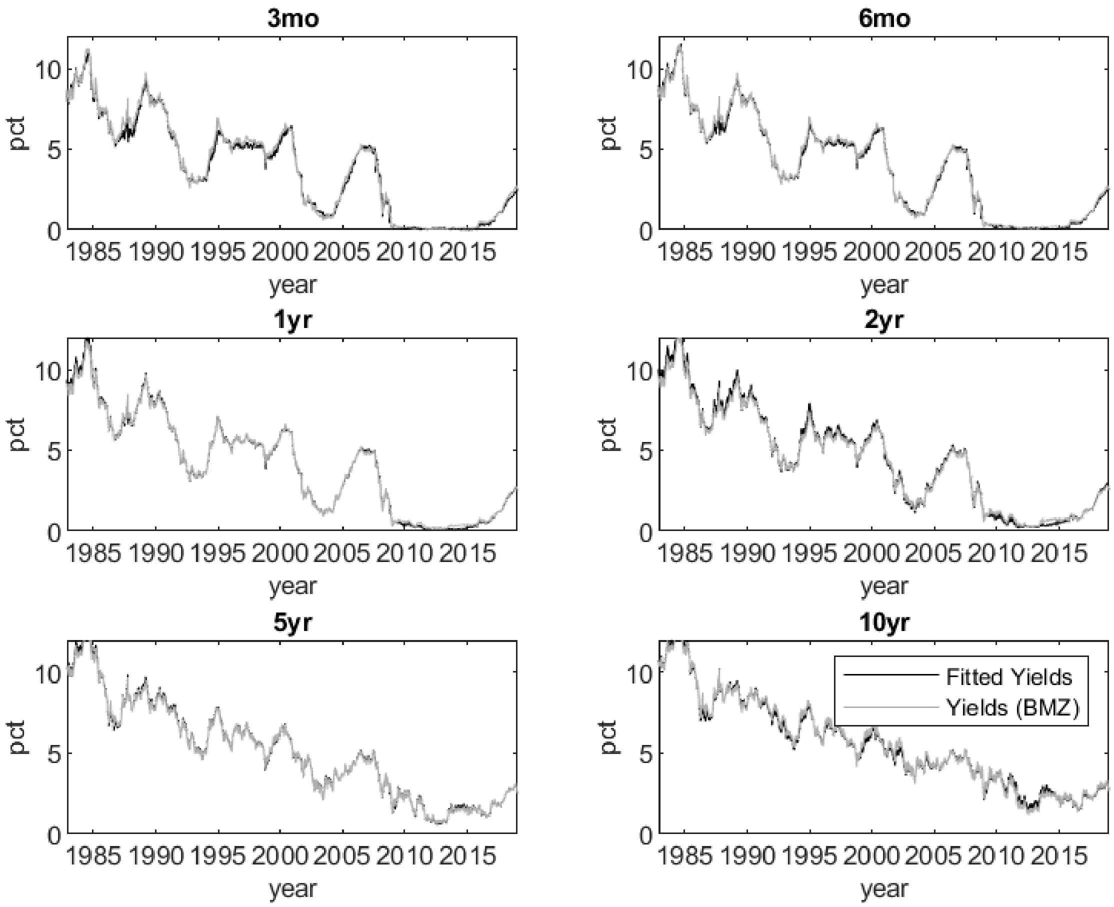
3.7 Figures

Figure 22: Treasury Yield Curve Current Factors



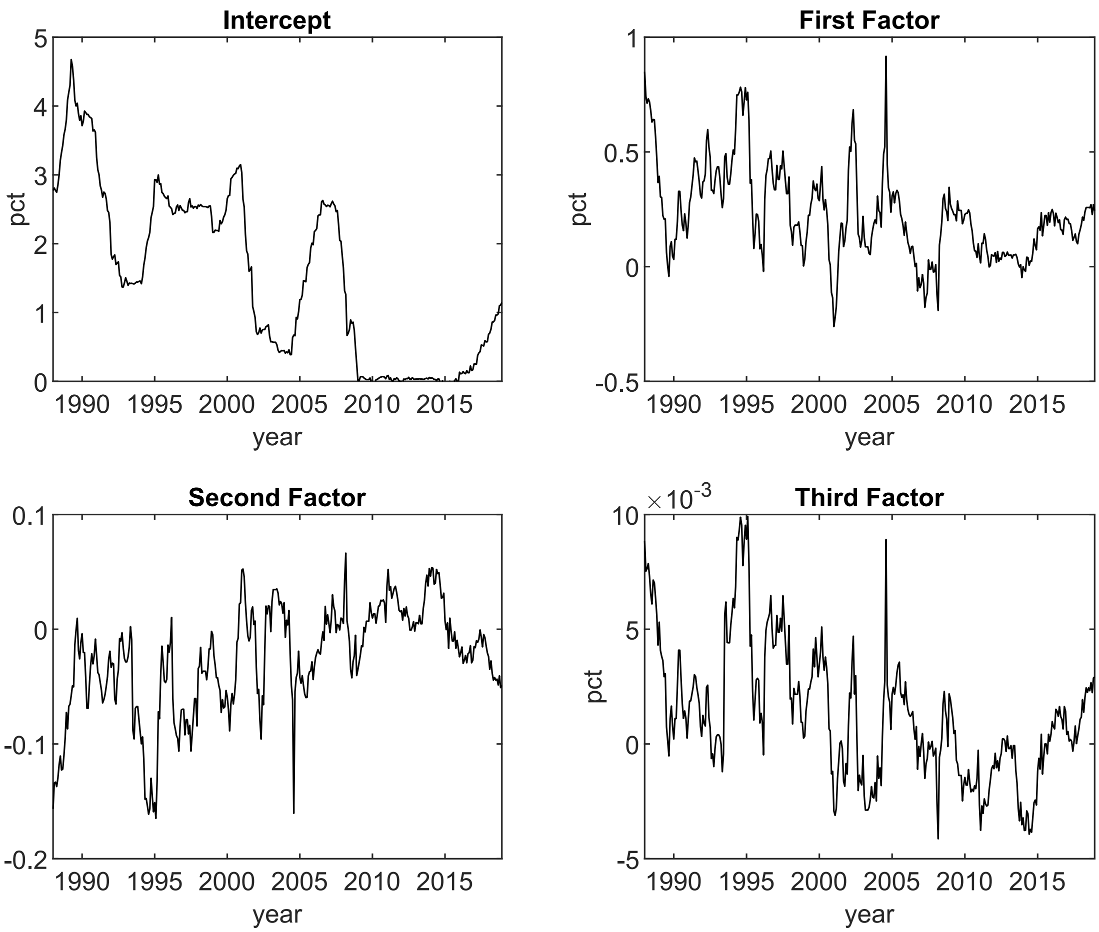
This figure shows factors extracted from the bootstrapping procedure of Treasury bond yields. Factors, $\hat{x}_i^c(t)$ are obtained by estimating equation 18.

Figure 23: Zero-Coupon Yields

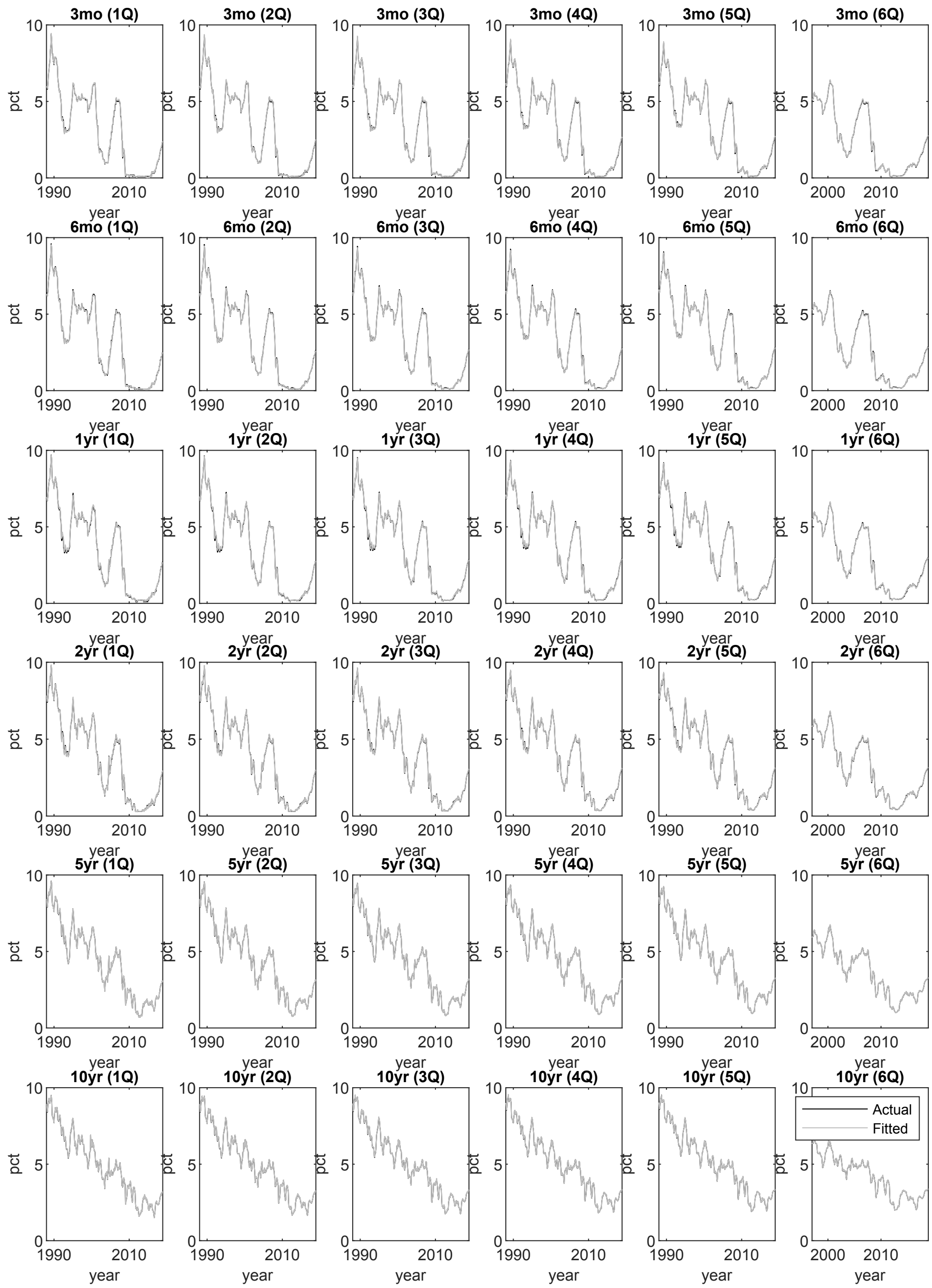


This figure shows zero-coupon yields fitted from the bootstrapping procedure in this paper (black line). Yields, $\hat{y}_z(t, \tau_N)$ are obtained from equation 19. The gray line contains zero-coupon yields from Balduzzi et al. (2019).

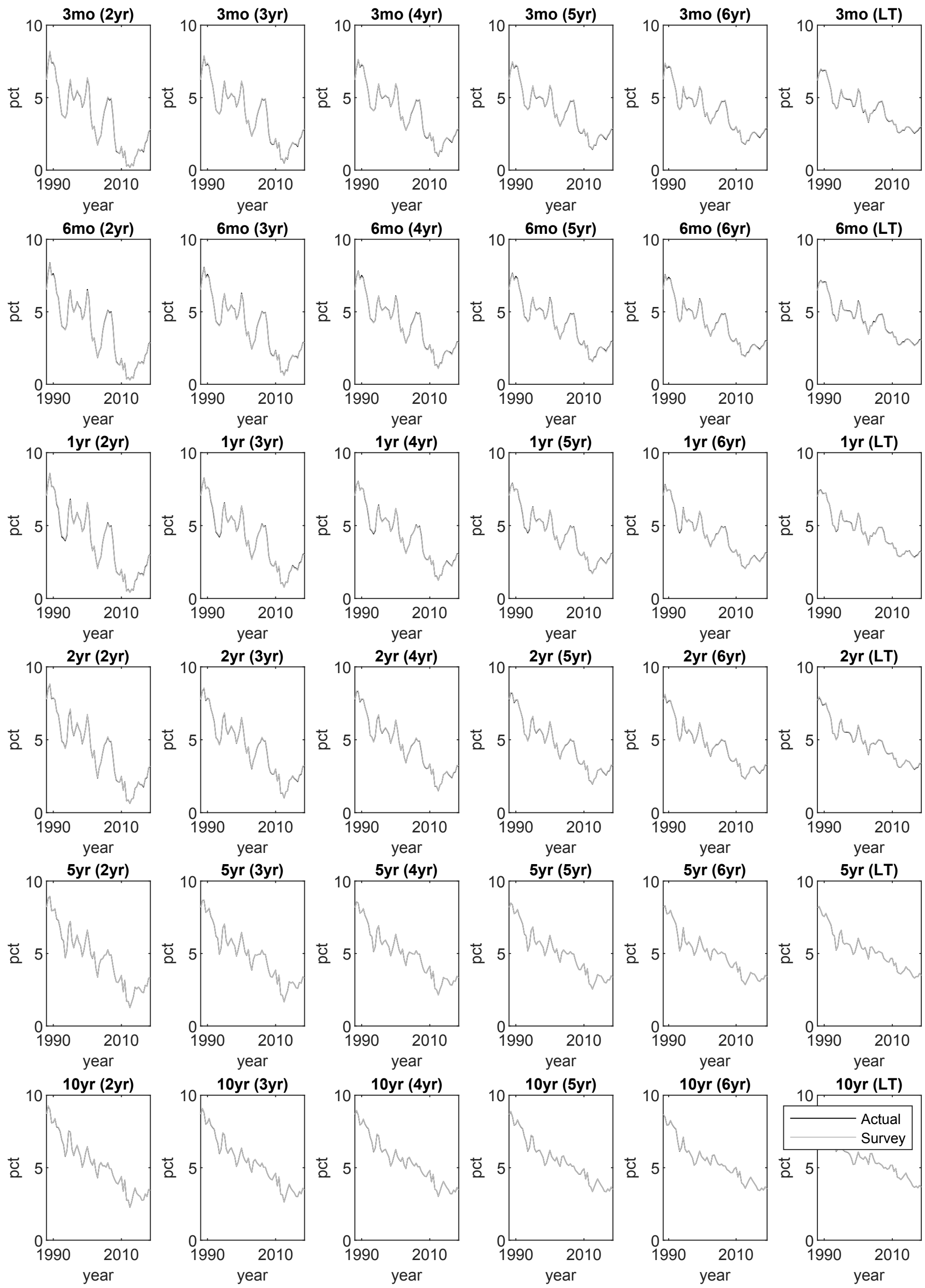
Figure 24: Treasury Yield Curve "Forecast" Factors



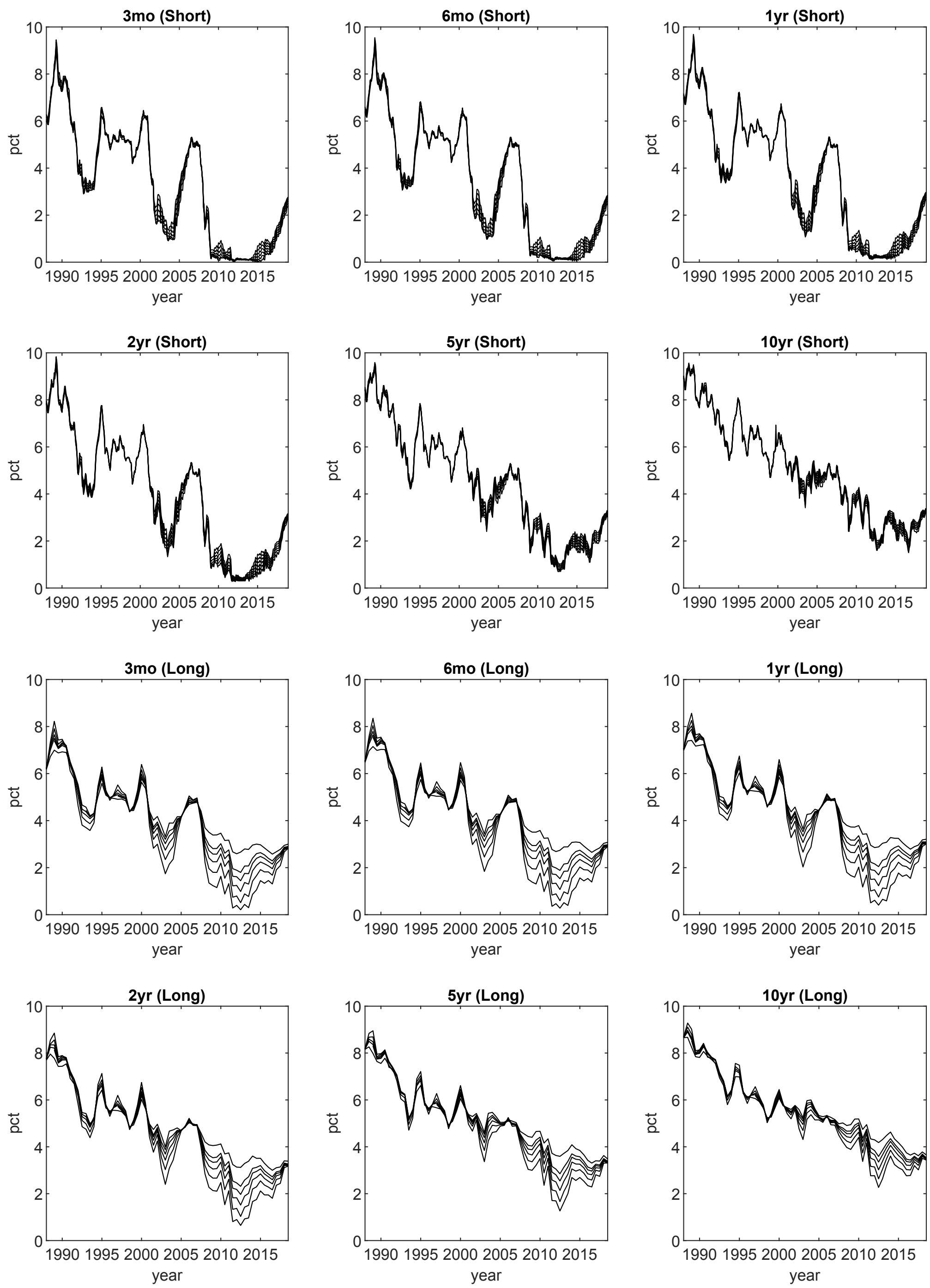
This figure shows factors extracted from the bootstrapping procedure of survey forecasts. Factors, $\hat{x}_i^f(t)$ are obtained by estimating equation 20.



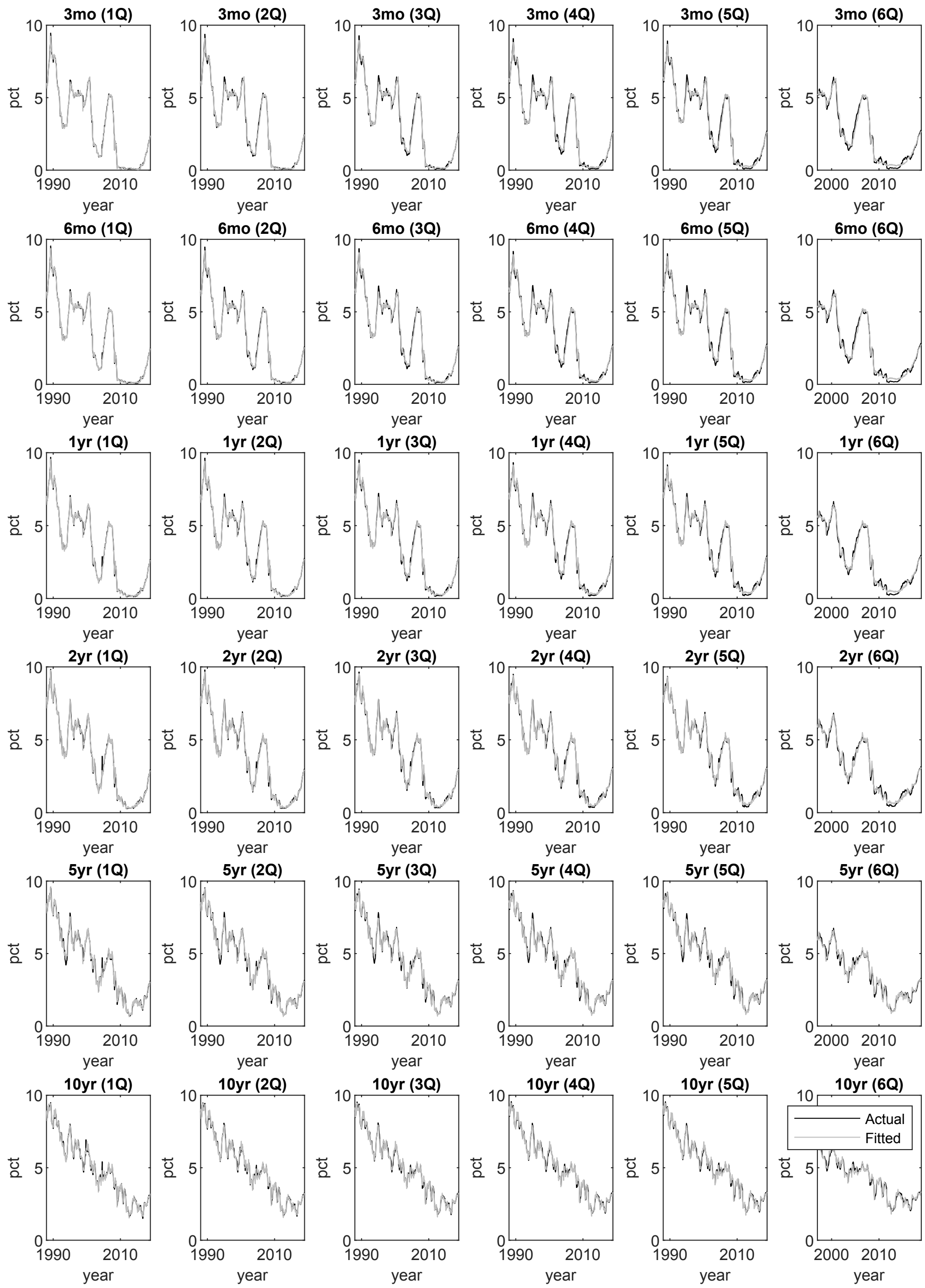
This figure shows short-term forecasts of Treasury bond yields from fitted values of survey forecasts (i.e. the left- and right-hand sides of equation 20 post-estimation). Each row represents a particular Treasury maturity, while columns correspond to forecast horizons. The black lines are actual survey forecasts, while the gray lines are fitted survey forecasts.



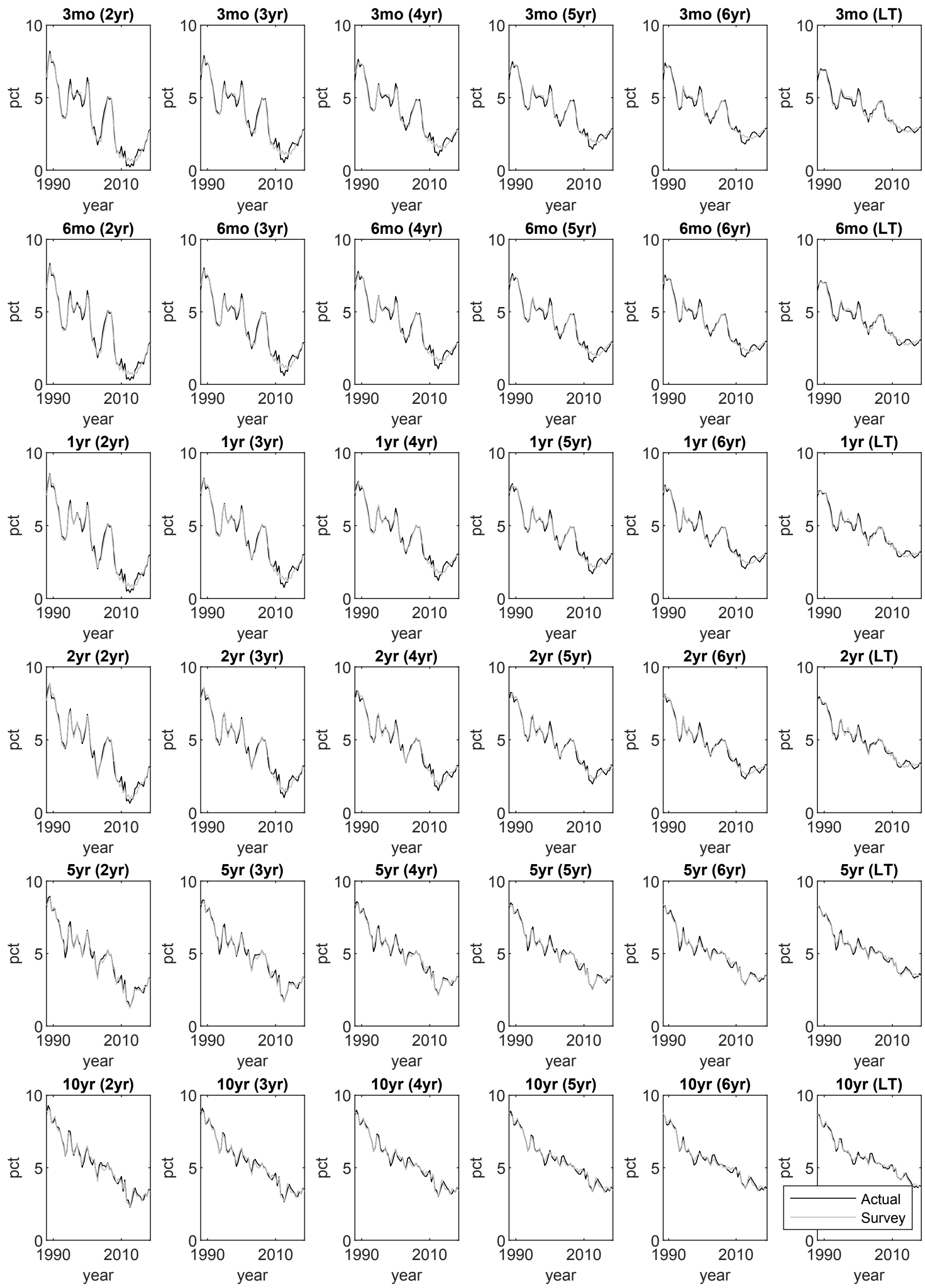
This figure shows long-term forecasts of Treasury bond yields from fitted values of survey forecasts (i.e. the left- and right-hand sides of equation 20 post-estimation). Each row represents a particular Treasury maturity, while columns correspond to forecast horizons. The black lines are actual survey forecasts, while the gray lines are fitted survey forecasts.



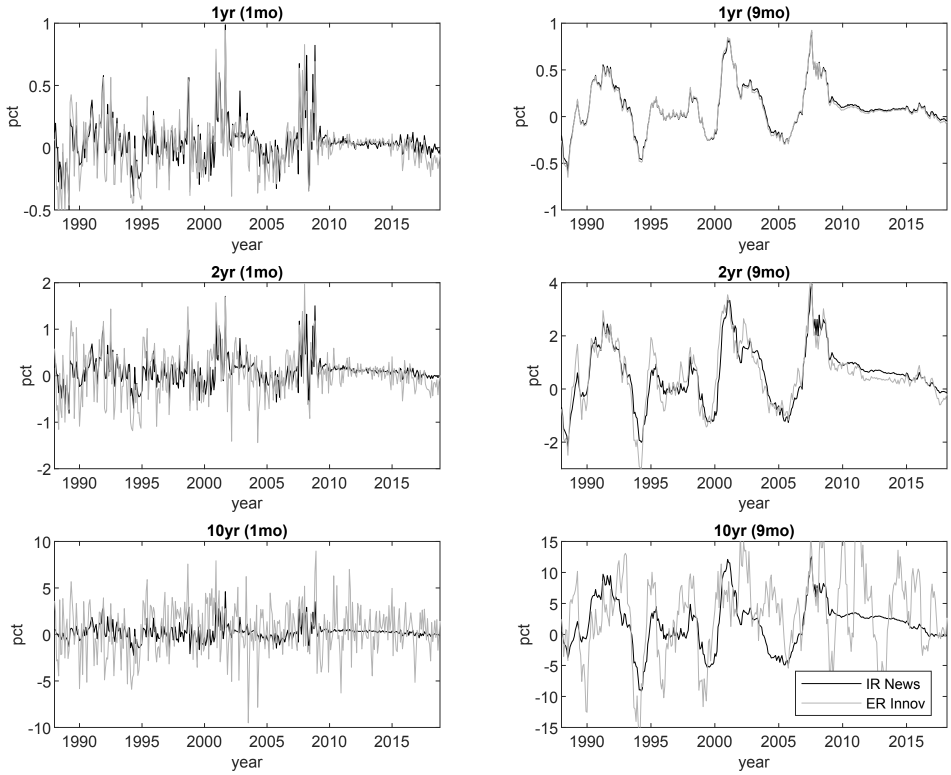
This figure shows fitted values of survey forecasts of the zero-coupon yield curve for the following bond maturities: 3-month, 6-month, 1-year, 2-year, 5-year, and 10-year. The first two rows plot short-term forecasts (1-6 quarters), while the last two rows plot long-term forecasts (2-6 years and 11 years ahead).



This figure shows short-term forecasts of Treasury zero-coupon yields from fitted values of survey forecasts (i.e. the left- and right-hand sides of equation 22 post-estimation). Each row represents a particular Treasury maturity, while columns correspond to forecast horizons. The black lines are actual survey forecasts, while the gray lines are fitted survey forecasts.

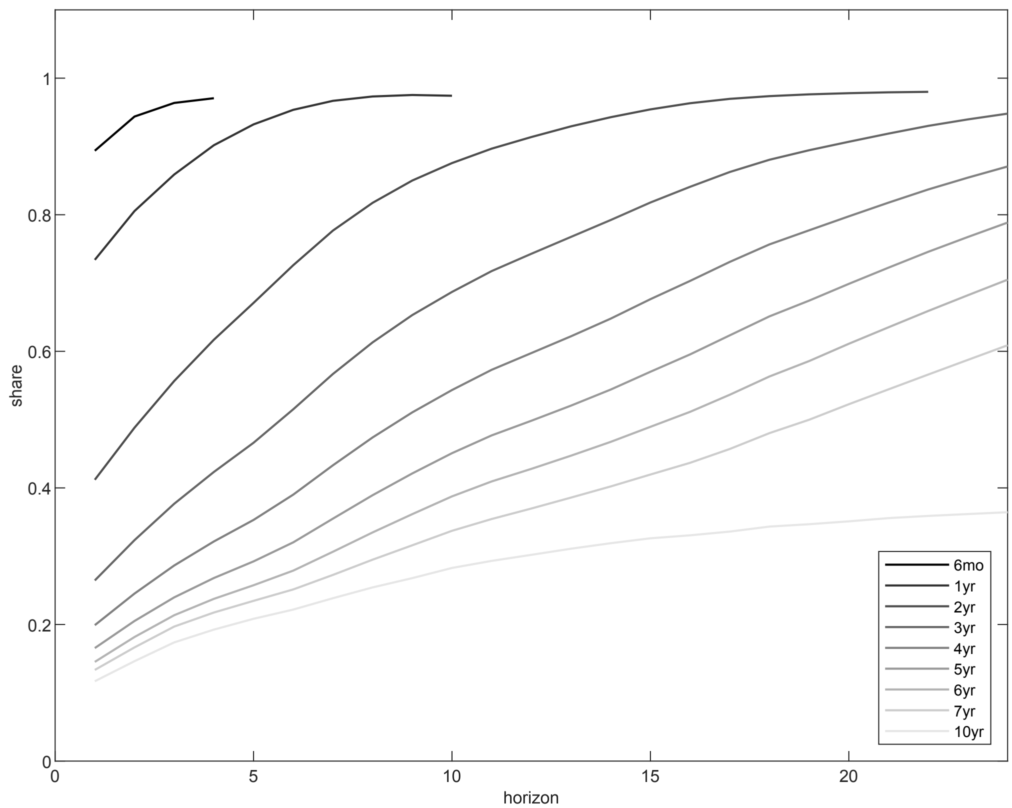


This figure shows long-term forecasts of Treasury zero-coupon yields from fitted values of survey forecasts (i.e. the left- and right-hand sides of equation 22 post-estimation). Each row represents a particular Treasury maturity, while columns correspond to forecast horizons. The black lines are actual survey forecasts, while the gray lines are fitted survey forecasts.



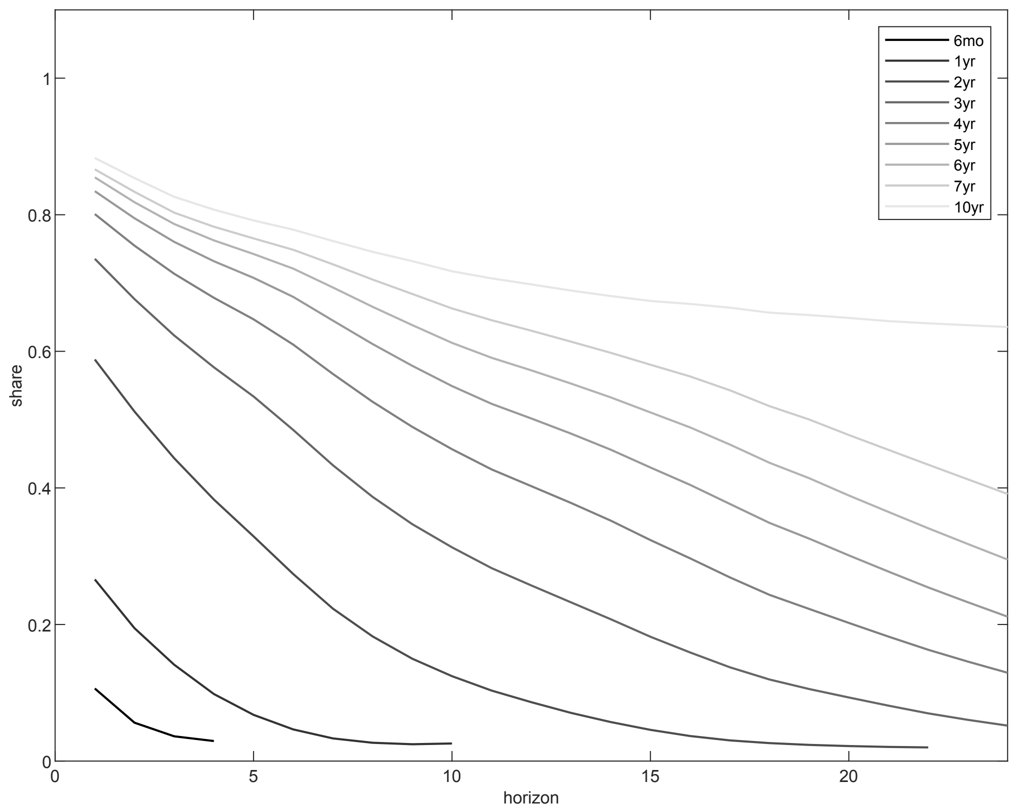
This figure plots excess-return innovations (gray line) and interest-rate news (black line) for 1-year, 2-year, and 10-year bond yields at 1-month and 9-month horizons.

Figure 31: Shares of Variance of Excess-Return Innovations Explained by Interest-



This figure shows estimates of $\beta_y^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news.

Figure 32: Shares of Variance of Excess-Return Innovations Explained by Excess-



This figure shows estimates of $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to excess-return news.

3.8 Tables

Table 21: Coverage of Blue Chip Interest-Rate Forecasts (1983-2018)

Horizons	Frequency	Maturity	
		3	6, 12, 24, 60, 120
1Q, 3Q, 5Q	Monthly	11/1982-12/2018	1/1988-12/2018
2Q, 4Q	Monthly	4/1983-12/2018	1/1988-12/2018
6Q	Monthly	1/1997-12/2018	1/1988-12/2018
2Y, 3Y, 4Y, 5Y, 6Y	Semi-Annually	10/1983-12/2018	3/1988-12/2018
Average 7-11Y	Semi-Annually	3/1986-12/2018	3/1988-12/2018

This table provides a breakdown of the availability of interest-rate forecasts in the Blue Chip Financial Forecasts® survey from 1983-2018. In order to obtain a consistent series over time, I exclude forecasts of the interest rates on 3-year, 7-year, 20-year, 30-year, and average "long-term" bonds. In addition, the first releases in November and December 1982 contained a more detailed coverage of bonds across maturities. However, interest-rate forecasts for these bonds were not included in subsequent releases consistently until 1988.

Table 22: RMSEs of Treasury Current Yield Curve and Forecasts (1988-2018)

Root Mean Square Errors (%)					
	Polynomial Order				
	1	2	3	4	5
Zero-Coupon Curve N = 54,084	0.2437	0.1461	0.1190	0.1116	0.1006
Survey Forecasts N = 14,952	0.1843	0.0832	0.0297	0.0150	0.0000

The top panel of this table gives root mean square errors from fits of equation 18. Treasury constant maturity yields are fitted to polynomials of order 1-5 as a function of coupon rates and maturity between Dec 1982 and Dec 2018. The bottom panel of this table gives root mean square errors from fits of equation 20. Forecasts of Treasury constant maturity yields are fitted to polynomials of order 1-5 as a function of coupon rates and maturity between 1988 and 2018 for each horizon.

Table 23: Fits of Zero-Coupon Yield Forecasts (1988-2018)

Case 1			Case 2								
				k = 1		k = 2		k = 3		k = 4	
p	Adj R2	RMSE	p	Adj R2	RMSE	Adj R2	RMSE	Adj R2	RMSE	Adj R2	RMSE
1	0.9866	0.0027	1	0.9879	0.0026	0.9881	0.0026	0.9886	0.0025	0.9895	0.0024
2	0.9891	0.0024	2	0.9906	0.0023	0.9908	0.0023	0.9914	0.0022	0.9924	0.0020
3	0.9892	0.0024	3	0.9907	0.0023	0.9909	0.0022	0.9915	0.0022	0.9926	0.0020
4	0.9893	0.0024	4	0.9908	0.0022	0.9910	0.0022	0.9917	0.0021	0.9928	0.0020
5	0.9894	0.0024	5	0.9908	0.0022	0.9911	0.0022	0.9917	0.0021	0.9928	0.0020
6	0.9894	0.0024	6	0.9909	0.0022	0.9911	0.0022	0.9917	0.0021	0.9928	0.0020
			Case 3								
				k = 1		k = 2		k = 3		k = 4	
			p	Adj R2	RMSE	Adj R2	RMSE	Adj R2	RMSE	Adj R2	RMSE
			1	0.9884	0.0025	0.9890	0.0025	0.9897	0.0024	0.9903	0.0023
			2	0.9911	0.0022	0.9918	0.0021	0.9926	0.0020	0.9933	0.0019
			3	0.9913	0.0022	0.9920	0.0021	0.9929	0.0020	0.9936	0.0019
			4	0.9914	0.0022	0.9921	0.0021	0.9930	0.0020	0.9938	0.0019
			5	0.9914	0.0022	0.9922	0.0021	0.9931	0.0020	0.9938	0.0019
			6	0.9915	0.0022	0.9922	0.0021	0.9931	0.0020	0.9939	0.0018

This table presents adjusted R-square and root mean square errors from fitting equation 22 for each case given in 23. Given below are the polynomial specifications for the loadings on factors β estimated in equation 22. P and K denote orders of the horizon and time trend polynomials, respectively.

Case 1: $\beta_i^{(0)}(h) = \sum_{p=0}^{P} \beta_{i,p} h^p$

Case 2: $\beta_0^{(1)}(h,t) = \sum_{k=1}^{K} \left(\sum_{p=1}^{P} \beta_{0,p} h^p \right) t^k, \beta_i^{(1)}(h,t) = \sum_{p=1}^{P} \beta_{i,p} h^p i \qquad \forall \in \{1, ..., I\}$

Case 3: $\beta_i^{(2)}(h,t) = \sum_{k=1}^{K} \left(\sum_{p=0}^{P} \beta_{i,p} h^p \right) t^k$

Table 24: Summary Statistics

Horizon (months)		Means					Standard Deviations				
		Maturity (months)					Maturity (months)				
		6	12	24	60	120	6	12	24	60	120
1	IR News	0.02	0.04	0.09	0.18	0.22	0.09	0.18	0.31	0.48	0.84
	ER News	-0.01	-0.03	-0.05	-0.25	0.21	0.03	0.10	0.38	1.27	2.47
	ER Innov	0.00	0.01	0.04	-0.07	0.43	0.09	0.21	0.51	1.44	2.65
3	IR News	0.03	0.10	0.25	0.52	0.63	0.11	0.31	0.63	1.04	1.76
	ER News	-0.01	-0.02	-0.04	-0.13	0.54	0.02	0.11	0.55	2.08	3.93
	ER Innov	0.02	0.08	0.21	0.40	1.17	0.12	0.35	0.89	2.49	4.35
6	IR News		0.13	0.43	1.01	1.19		0.36	0.95	1.74	2.91
	ER News		-0.02	-0.03	0.07	1.06		0.06	0.56	2.65	5.24
	ER Innov		0.12	0.40	1.08	2.25		0.38	1.13	3.34	5.84
9	IR News		0.10	0.55	1.46	1.69		0.25	1.10	2.31	3.87
	ER News		-0.01	-0.02	0.26	1.56		0.02	0.45	2.74	5.92
	ER Innov		0.09	0.53	1.73	3.25		0.26	1.19	3.70	6.58
12	IR News			0.60	1.88	2.14			1.10	2.79	4.67
	ER News			-0.01	0.42	2.04			0.32	2.80	6.52
	ER Innov			0.58	2.30	4.18			1.16	4.06	7.24
15	IR News			0.57	2.25	2.55			0.97	3.12	5.26
	ER News			-0.01	0.57	2.51			0.19	2.69	6.95
	ER Innov			0.56	2.83	5.07			1.00	4.22	7.69
18	IR News			0.47	2.59	2.93			0.72	3.34	5.68
	ER News			-0.01	0.69	2.97			0.08	2.41	7.19
	ER Innov			0.45	3.28	5.90			0.74	4.21	7.87
21	IR News			0.28	2.85	3.25			0.39	3.47	5.98
	ER News			-0.01	0.75	3.37			0.02	2.07	7.37
	ER Innov			0.26	3.60	6.62			0.40	4.18	8.00
24	IR News				3.05	3.53				3.51	6.17
	ER News				0.76	3.72				1.69	7.44
	ER Innov				3.81	7.25				4.05	7.99

This table presents summary statistics of changes in expectations of future interest rates (IR news), future excess returns (ER news), and excess-return innovations (ER Innov). The first set of column for each variable contains the mean and the second contain its standard deviation. All statistics are given as annualized percents.

Table 25: Variance Decomposition of Excess-Return Innovations

Horizon	Share of Interest-Rate News					Share of Excess-Return News				
	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.89	0.73	0.41	0.17	0.12	0.11	0.27	0.59	0.83	0.88
	(0.04)	(0.05)	(0.04)	(0.03)	(0.02)	(0.04)	(0.05)	(0.04)	(0.03)	(0.02)
2	0.94	0.81	0.49	0.21	0.15	0.06	0.19	0.51	0.79	0.85
	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)	(0.02)	(0.04)	(0.04)	(0.03)	(0.03)
3	0.96	0.86	0.56	0.24	0.17	0.04	0.14	0.44	0.76	0.83
	(0.01)	(0.03)	(0.04)	(0.04)	(0.04)	(0.01)	(0.03)	(0.04)	(0.04)	(0.04)
4	0.97	0.90	0.62	0.27	0.19	0.03	0.10	0.38	0.73	0.81
	(0.01)	(0.03)	(0.04)	(0.04)	(0.05)	(0.01)	(0.03)	(0.04)	(0.04)	(0.05)
5		0.93	0.67	0.29	0.21		0.07	0.33	0.71	0.79
		(0.02)	(0.04)	(0.04)	(0.05)		(0.02)	(0.04)	(0.04)	(0.05)
6		0.95	0.73	0.32	0.22		0.05	0.27	0.68	0.78
		(0.01)	(0.05)	(0.04)	(0.06)		(0.01)	(0.05)	(0.04)	(0.06)
9		0.98	0.85	0.42	0.27		0.02	0.15	0.58	0.73
		(0.01)	(0.04)	(0.05)	(0.07)		(0.01)	(0.04)	(0.05)	(0.07)
12			0.91	0.50	0.30			0.09	0.50	0.70
			(0.04)	(0.06)	(0.09)			(0.04)	(0.06)	(0.09)
15			0.95	0.57	0.33			0.05	0.43	0.67
			(0.02)	(0.06)	(0.10)			(0.02)	(0.06)	(0.10)
18			0.97	0.65	0.34			0.03	0.35	0.66
			(0.01)	(0.06)	(0.11)			(0.01)	(0.06)	(0.11)
21			0.98	0.72	0.36			0.02	0.28	0.64
			(0.01)	(0.05)	(0.12)			(0.01)	(0.05)	(0.12)
24				0.79	0.36				0.21	0.64
				(0.05)	(0.12)				(0.05)	(0.12)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table 26: Variance Decomposition of Excess-Return Innovations (Pre-1998)

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.85	0.66	0.40	0.21	0.16	0.15	0.34	0.60	0.79	0.84
	(0.05)	(0.04)	(0.04)	(0.04)	(0.02)	(0.05)	(0.04)	(0.04)	(0.04)	(0.02)
2	0.92	0.73	0.44	0.25	0.21	0.08	0.27	0.56	0.75	0.79
	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)	(0.03)	(0.04)	(0.04)	(0.04)	(0.03)
3	0.95	0.79	0.49	0.28	0.25	0.05	0.21	0.51	0.72	0.75
	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)	(0.03)	(0.04)	(0.05)	(0.04)	(0.04)
4	0.98	0.84	0.54	0.30	0.27	0.02	0.16	0.46	0.70	0.73
	(0.03)	(0.04)	(0.06)	(0.05)	(0.05)	(0.03)	(0.04)	(0.06)	(0.05)	(0.05)
5		0.88	0.59	0.32	0.28		0.12	0.41	0.68	0.72
		(0.04)	(0.07)	(0.05)	(0.05)		(0.04)	(0.07)	(0.05)	(0.05)
6		0.92	0.64	0.34	0.30		0.08	0.36	0.66	0.70
		(0.03)	(0.07)	(0.06)	(0.06)		(0.03)	(0.07)	(0.06)	(0.06)
9		0.97	0.77	0.41	0.34		0.03	0.23	0.59	0.66
		(0.02)	(0.08)	(0.08)	(0.08)		(0.02)	(0.08)	(0.08)	(0.08)
12			0.85	0.47	0.38			0.15	0.53	0.62
			(0.07)	(0.10)	(0.09)			(0.07)	(0.10)	(0.09)
15			0.92	0.52	0.42			0.08	0.48	0.58
			(0.05)	(0.11)	(0.10)			(0.05)	(0.11)	(0.10)
18			0.97	0.61	0.48			0.03	0.39	0.52
			(0.02)	(0.12)	(0.12)			(0.02)	(0.12)	(0.12)
21			0.99	0.71	0.55			0.01	0.29	0.45
			(0.01)	(0.11)	(0.12)			(0.01)	(0.11)	(0.12)
24				0.81	0.63				0.19	0.37
				(0.09)	(0.12)				(0.09)	(0.12)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table 27: Variance Decomposition of Excess-Return Innovations (Post-1998)

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.94	0.80	0.42	0.13	0.09	0.06	0.20	0.58	0.87	0.91
	(0.03)	(0.06)	(0.06)	(0.04)	(0.03)	(0.03)	(0.06)	(0.06)	(0.04)	(0.03)
2	0.97	0.88	0.53	0.16	0.10	0.03	0.12	0.47	0.84	0.90
	(0.02)	(0.03)	(0.06)	(0.05)	(0.04)	(0.02)	(0.03)	(0.06)	(0.05)	(0.04)
3	0.97	0.92	0.62	0.19	0.13	0.03	0.08	0.38	0.81	0.87
	(0.01)	(0.02)	(0.06)	(0.06)	(0.05)	(0.01)	(0.02)	(0.06)	(0.06)	(0.05)
4	0.97	0.95	0.68	0.22	0.14	0.03	0.05	0.32	0.78	0.86
	(0.01)	(0.01)	(0.05)	(0.06)	(0.06)	(0.01)	(0.01)	(0.05)	(0.06)	(0.06)
5		0.97	0.74	0.25	0.16		0.03	0.26	0.75	0.84
		(0.01)	(0.04)	(0.06)	(0.07)		(0.01)	(0.04)	(0.06)	(0.07)
6		0.98	0.79	0.28	0.18		0.02	0.21	0.72	0.82
		(0.01)	(0.04)	(0.06)	(0.07)		(0.01)	(0.04)	(0.06)	(0.07)
9		0.97	0.90	0.40	0.23		0.03	0.10	0.60	0.77
		(0.01)	(0.04)	(0.09)	(0.10)		(0.01)	(0.04)	(0.09)	(0.10)
12			0.95	0.49	0.26			0.05	0.51	0.74
			(0.04)	(0.10)	(0.12)			(0.04)	(0.10)	(0.12)
15			0.97	0.58	0.29			0.03	0.42	0.71
			(0.03)	(0.11)	(0.14)			(0.03)	(0.11)	(0.14)
18			0.97	0.66	0.30			0.03	0.34	0.70
			(0.02)	(0.10)	(0.15)			(0.02)	(0.10)	(0.15)
21			0.97	0.72	0.29			0.03	0.28	0.71
			(0.01)	(0.10)	(0.15)			(0.01)	(0.10)	(0.15)
24				0.76	0.29				0.24	0.71
				(0.09)	(0.15)				(0.09)	(0.15)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table 28: Variance Decomposition of Excess-Return Innovations (Post-1998 (no ZLB))

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.96	0.81	0.43	0.14	0.11	0.04	0.19	0.57	0.86	0.89
	(0.02)	(0.05)	(0.07)	(0.05)	(0.04)	(0.02)	(0.05)	(0.07)	(0.05)	(0.04)
2	0.97	0.89	0.52	0.16	0.13	0.03	0.11	0.48	0.84	0.87
	(0.01)	(0.04)	(0.07)	(0.06)	(0.06)	(0.01)	(0.04)	(0.07)	(0.06)	(0.06)
3	0.98	0.93	0.61	0.21	0.18	0.02	0.07	0.39	0.79	0.82
	(0.01)	(0.03)	(0.07)	(0.07)	(0.08)	(0.01)	(0.03)	(0.07)	(0.07)	(0.08)
4	0.97	0.96	0.69	0.26	0.25	0.03	0.04	0.31	0.74	0.75
	(0.01)	(0.02)	(0.06)	(0.07)	(0.09)	(0.01)	(0.02)	(0.06)	(0.07)	(0.09)
5		0.98	0.75	0.29	0.29		0.02	0.25	0.71	0.71
		(0.01)	(0.05)	(0.06)	(0.10)		(0.01)	(0.05)	(0.06)	(0.10)
6		0.99	0.81	0.34	0.35		0.01	0.19	0.66	0.65
		(0.01)	(0.05)	(0.07)	(0.11)		(0.01)	(0.05)	(0.07)	(0.11)
9		0.98	0.93	0.48	0.51		0.02	0.07	0.52	0.49
		(0.01)	(0.04)	(0.09)	(0.11)		(0.01)	(0.04)	(0.09)	(0.11)
12			0.97	0.56	0.59			0.03	0.44	0.41
			(0.04)	(0.11)	(0.13)			(0.04)	(0.11)	(0.13)
15			0.98	0.64	0.68			0.02	0.36	0.32
			(0.02)	(0.10)	(0.12)			(0.02)	(0.10)	(0.12)
18			0.98	0.72	0.71			0.02	0.28	0.29
			(0.02)	(0.09)	(0.11)			(0.02)	(0.09)	(0.11)
21			0.97	0.77	0.74			0.03	0.23	0.26
			(0.01)	(0.09)	(0.13)			(0.01)	(0.09)	(0.13)
24				0.82	0.78				0.18	0.22
				(0.09)	(0.18)				(0.09)	(0.18)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table 29: Variance Decomposition of Excess-Return Innovations (no ZLB)

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.90	0.73	0.41	0.17	0.14	0.10	0.27	0.59	0.83	0.86
	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)	(0.03)	(0.04)	(0.04)	(0.03)	(0.03)
2	0.95	0.80	0.48	0.21	0.17	0.05	0.20	0.52	0.79	0.83
	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)	(0.02)	(0.04)	(0.04)	(0.04)	(0.04)
3	0.96	0.85	0.54	0.25	0.22	0.04	0.15	0.46	0.75	0.78
	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)	(0.02)	(0.03)	(0.04)	(0.04)	(0.04)
4	0.97	0.90	0.61	0.28	0.26	0.03	0.10	0.39	0.72	0.74
	(0.01)	(0.03)	(0.05)	(0.03)	(0.05)	(0.01)	(0.03)	(0.05)	(0.03)	(0.05)
5		0.93	0.67	0.31	0.28		0.07	0.33	0.69	0.72
		(0.02)	(0.05)	(0.03)	(0.05)		(0.02)	(0.05)	(0.03)	(0.05)
6		0.96	0.73	0.34	0.31		0.04	0.27	0.66	0.69
		(0.02)	(0.05)	(0.04)	(0.06)		(0.02)	(0.05)	(0.04)	(0.06)
9		0.98	0.85	0.44	0.38		0.02	0.15	0.56	0.62
		(0.01)	(0.05)	(0.05)	(0.07)		(0.01)	(0.05)	(0.05)	(0.07)
12			0.91	0.50	0.44			0.09	0.50	0.56
			(0.04)	(0.06)	(0.09)			(0.04)	(0.06)	(0.09)
15			0.95	0.56	0.48			0.05	0.44	0.52
			(0.03)	(0.07)	(0.10)			(0.03)	(0.07)	(0.10)
18			0.97	0.64	0.52			0.03	0.36	0.48
			(0.01)	(0.07)	(0.11)			(0.01)	(0.07)	(0.11)
21			0.98	0.71	0.56			0.02	0.29	0.44
			(0.01)	(0.06)	(0.13)			(0.01)	(0.06)	(0.13)
24				0.78	0.60				0.22	0.40
				(0.06)	(0.14)				(0.06)	(0.14)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table 30: Alternative Variance Decomposition of Excess-Return Innovations

Share of Variance of ER Innovations	Horizon (months)	Maturity (months) 6	12	24	60	120
Var(ER News)	1	0.09	0.25	0.56	0.78	0.87
		(0.02)	(0.04)	(0.10)	(0.10)	(0.09)
Var(IR News)		0.88	0.73	0.38	0.11	0.10
		(0.22)	(0.18)	(0.10)	(0.02)	(0.02)
2Cov(IR News, ER News)		0.02	0.03	0.06	0.11	0.04
		(0.04)	(0.07)	(0.07)	(0.05)	(0.04)
Var(ER News)	3	0.02	0.10	0.39	0.69	0.82
		(0.00)	(0.02)	(0.08)	(0.09)	(0.10)
Var(IR News)		0.94	0.82	0.50	0.17	0.16
		(0.25)	(0.21)	(0.12)	(0.04)	(0.04)
2Cov(IR News, ER News)		0.04	0.08	0.11	0.13	0.02
		(0.03)	(0.05)	(0.08)	(0.08)	(0.07)
Var(ER News)	6		0.03	0.25	0.63	0.81
			(0.01)	(0.05)	(0.09)	(0.12)
Var(IR News)			0.94	0.70	0.27	0.25
			(0.24)	(0.17)	(0.06)	(0.06)
2Cov(IR News, ER News)			0.04	0.05	0.10	-0.06
			(0.03)	(0.07)	(0.09)	(0.10)
Var(ER News)	9		0.00	0.14	0.55	0.81
			(0.00)	(0.03)	(0.09)	(0.11)
Var(IR News)			0.96	0.84	0.39	0.35
			(0.25)	(0.20)	(0.08)	(0.08)
2Cov(IR News, ER News)			0.04	0.01	0.06	-0.16
			(0.02)	(0.07)	(0.11)	(0.13)
Var(ER News)	12			0.08	0.48	0.81
				(0.02)	(0.10)	(0.13)
Var(IR News)				0.91	0.47	0.42
				(0.21)	(0.09)	(0.10)
2Cov(IR News, ER News)				0.02	0.06	-0.23
				(0.06)	(0.12)	(0.15)
Var(ER News)	24				0.17	0.87
					(0.03)	(0.19)
Var(IR News)					0.76	0.60
					(0.12)	(0.13)
2Cov(IR News, ER News)					0.08	-0.46
					(0.09)	(0.24)

This table presents estimates of each of the shares from equation 17.

3.9 Appendix

3.9.1 *Tables*

Table B.1: Variance Decomposition of Excess-Return Innovations (4th-Order Polynomial for Bootstrapping Procedure of the Current Zero-Coupon Yield Curve)

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.88	0.66	0.41	0.15	0.09	0.12	0.34	0.59	0.85	0.91
	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)
3	0.96	0.81	0.55	0.23	0.12	0.04	0.19	0.45	0.77	0.88
	(0.01)	(0.03)	(0.04)	(0.03)	(0.02)	(0.01)	(0.03)	(0.04)	(0.03)	(0.02)
6		0.93	0.70	0.31	0.12		0.07	0.30	0.69	0.88
		(0.02)	(0.04)	(0.04)	(0.03)		(0.02)	(0.04)	(0.04)	(0.03)
9		0.97	0.82	0.41	0.12		0.03	0.18	0.59	0.88
		(0.01)	(0.04)	(0.05)	(0.03)		(0.01)	(0.04)	(0.05)	(0.03)
12			0.88	0.50	0.14			0.12	0.50	0.86
			(0.04)	(0.06)	(0.04)			(0.04)	(0.06)	(0.04)
15			0.93	0.56	0.16			0.07	0.44	0.84
			(0.03)	(0.06)	(0.04)			(0.03)	(0.06)	(0.04)
18			0.96	0.64	0.19			0.04	0.36	0.81
			(0.02)	(0.06)	(0.04)			(0.02)	(0.06)	(0.04)
21			0.98	0.72	0.23			0.02	0.28	0.77
			(0.01)	(0.06)	(0.04)			(0.01)	(0.06)	(0.04)
24				0.79	0.26				0.21	0.74
				(0.06)	(0.04)				(0.06)	(0.04)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

Table B.2: Variance Decomposition of Excess-Return Innovations (Case 2: Time-Varying Means and Time-Invariant Factor Loadings in Fitted Zero-Coupon Forecasts)

	Share of Interest-Rate News					Share of Excess-Return News				
Horizon	Maturity (months)					Maturity (months)				
(months)	6	12	24	60	120	6	12	24	60	120
1	0.89	0.73	0.41	0.16	0.11	0.11	0.27	0.59	0.84	0.89
	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)	(0.02)	(0.03)	(0.03)	(0.02)	(0.01)
3	0.96	0.86	0.55	0.24	0.17	0.04	0.14	0.45	0.76	0.83
	(0.01)	(0.03)	(0.04)	(0.03)	(0.03)	(0.01)	(0.03)	(0.04)	(0.03)	(0.03)
6		0.95	0.72	0.32	0.22		0.05	0.28	0.68	0.78
		(0.01)	(0.04)	(0.04)	(0.04)		(0.01)	(0.04)	(0.04)	(0.04)
9		0.98	0.85	0.42	0.27		0.03	0.15	0.58	0.73
		(0.01)	(0.04)	(0.05)	(0.06)		(0.01)	(0.04)	(0.05)	(0.06)
12			0.91	0.49	0.30			0.09	0.51	0.70
			(0.03)	(0.05)	(0.08)			(0.03)	(0.05)	(0.08)
15			0.95	0.57	0.33			0.05	0.43	0.67
			(0.02)	(0.05)	(0.09)			(0.02)	(0.05)	(0.09)
18			0.97	0.64	0.35			0.03	0.36	0.65
			(0.01)	(0.05)	(0.10)			(0.01)	(0.05)	(0.10)
21			0.98	0.72	0.36			0.02	0.28	0.64
			(0.01)	(0.05)	(0.10)			(0.01)	(0.05)	(0.10)
24				0.78	0.37				0.22	0.63
				(0.05)	(0.11)				(0.05)	(0.11)

This table presents estimates of $\beta_y^{(n)}(h)$ and $\beta_{ex}^{(n)}(h)$ from equation 15, the shares of variance of excess-return innovations due to interest-rate news and excess-return news, respectively. Newey-West corrected standard errors are given below coefficient estimates to account for overlapping observations.

3.9.2 *Survey Forecasts of Yields*

Forecasts of future Treasury yields are available at a monthly frequency for short horizons and semi-annually for longer horizons (Table 1). In addition, the period over which future interest rates are forecast varies between horizons and releases. While available at a monthly frequency, forecasted end-dates are fixed at the quarterly or yearly level. In order to construct consistent estimates of the average interest rate between the forecast date and the forecasted horizon, I rely on the following interpolation scheme. First, I make one key assumption about the very shortest horizon forecasts (1-3 months), namely that the average forecast is evenly distributed between months. For example, a 1-quarter ahead forecast (between January and March) of 3% made on February 1 would assume that the average interest rate for February and March would be 3%, giving a 2-month ahead forecast of the Treasury bill rate. To be precise, I denote $y_{t,t+1}$ as the average interest rate realized between month t and $t+1$.[56] The very shortest horizon forecasts provided by the Blue Chip survey cover the period finishing in the current quarter:

$$
\begin{aligned}
y^{BC}_{t,t+1} &= \frac{1}{3}\left(y_{t-2,t-1} + y_{t-1,t} + \mathrm{E}_t[y_{t,t+1}]\right) \text{ for Mar, Jun, Sep, Dec} \\
y^{BC}_{t,t+2} &= \frac{1}{3}\left(y_{t-1,t} + \mathrm{E}_t[y_{t,t+1} + y_{t+1,t+2}]\right) \text{ for Feb, May, Aug, Nov} \\
y^{BC}_{t,t+3} &= \frac{1}{3}\left(\mathrm{E}_t[y_{t,t+1} + y_{t+1,t+2} + y_{t+2,t+3}]\right) \text{ for Jan, Apr, Jul, Oct}
\end{aligned}
$$

For forecasts occurring in March, June, September, and December, the 1-quarter ahead forecast provided by Blue Chip is to the average of the previous two months'

[56] For example, if t corresponds with January, then this would be the average interest rate over the month of January.

realized values and the 1-month ahead expectation of interest rates. For forecasts occurring in February, May, August, and November, the 1-quarter ahead forecast provided by Blue Chip is the average of the previous month's realized value and the 2-month ahead expectation of interest rates. For January, April, July, and October the forecasted number provided by the survey is exactly the expectation over the current quarter. I denote $y^{EH}_{t,t+k}$ as the expectation of the average level of interest rates made at time t for the period t to $t+k$, which corresponds theoretically to the "Expectations Hypothesis" component of nominal yields. Given this notation, I can formally state the assumption as $y^{EH}_{t,t+k} = y^{BC}_{t,t+k}$. This assumption will hold exactly when $k = 3$ and not when $k = 1, 2$.

Quarterly forecasts between two- and six-quarters ahead are formed based on the average level of interest rates prevailing in that quarter. Formally, denote the k-month ahead forecast made at $t+k-3$ as $y^{BC}_{t+k-3,t+k}$, which is the average expected level of interest rates in the quarter starting $(k-3)$-months ahead:

$$y^{BC}_{t+k-3,t+k} = \frac{1}{3}\left(\mathrm{E}_t\left[\sum_{j=k-2}^{k} y_{t+j-1,t+j}\right]\right)$$

To construct forecasts from t to $t+k$, I simply average over forecasts for different horizons. For example, in order to construct a 5-month ahead forecast on February 1, I take the average of the 2-month ahead forecast and the "2-quarter ahead" forecast provided by BCFF, which would give the average expected level of interest rates between April and June. Thus, for horizons between four and eighteen months (two-to six-quarters ahead), I can construct $y^{EH}_{t,t+k}$:

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}}\left(\frac{1}{3}y^{EH}_{t,t+1} + \sum_{j=1}^{\frac{k-1}{3}} y^{BC}_{t+3j-2,t+3j+1}\right) \text{ for Mar, Jun, Sep, Dec, } k \in 4, 7, 10, 13, 16$$
$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}}\left(\frac{2}{3}y^{EH}_{t,t+2} + \sum_{j=1}^{\frac{k-2}{3}} y^{BC}_{t+3j-1,t+3j+2}\right) \text{ for Feb, May, Aug, Nov, } k \in 5, 8, 11, 14, 17$$
$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}}\left(y^{EH}_{t,t+3} + \sum_{j=1}^{\frac{k-3}{3}} y^{BC}_{t+3j,t+3j+3}\right) \text{ for Jan, Apr, Jul, Oct, } k \in 6, 9, 12, 15, 18$$

In order to construct forecasts at horizons longer than six-quarters ahead, I utilize yearly and long-term forecasts. Yearly forecasts between one- and six-years ahead are formed based on the average level of interest rates prevailing in that year. Formally, denote the k-month ahead forecast made at $t + k - 12$ as $y^{BC}_{t+k-12,t+k}$, which is the average expected level of interest rates in the year starting $(k - 12)$-months ahead:

$$y^{BC}_{t+k-12,t+k} = \frac{1}{12}\left(\mathrm{E}_t\left[\sum_{j=k-11}^{k} y_{t+j-1,t+j}\right]\right)$$

Forecasts are only available at these horizons on a regular basis in March (1984-1996), June (1997-2018), October (1983-1995), and December (1996-2018), due to the fact that the date on which longer-term forecasts were made changed in 1996. In addition, there is one instance in which these forecasts were made in January (2003) and November (1985). Due to this varying coverage of forecasts, I present below precise construction of average expected rates ($y^{EH}_{t,t+k}$) for horizons between two- and six-years ahead ($k \in [19, 79]$):

for Jan 2003 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{j=1}^{3} y^{BC}_{t+3j,t+3j+3} + \sum_{m=1}^{\frac{k-12}{12}} 4y^{BC}_{t+12m,t+12m+12} \right), k \in 24, 36, 48, 60, 72$$

for Mar 1984-1996 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{3} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{\frac{k-10}{12}} 4y^{BC}_{t+12m-2,t+12m+10} \right), k \in 22, 34, 46, 58, 70$$

for Jun 1997-2004, 2006-2007 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{2} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{\frac{k-7}{12}} 4y^{BC}_{t+12m-5,t+12m+7} \right), k \in 19, 31, 43, 56, 67$$

for Jun 2005, 2008-2018 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{4} y^{BC}_{t+3j-2,t+3j+1} + 2y^{BC}_{13,16} + \sum_{m=2}^{\frac{k-7}{12}} 4y^{BC}_{t+12m-5,t+12m+7} \right), k \in 31, 43, 56, 67, 79$$

for Oct 1983,1989-1991,1993,1995 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{j=1}^{4} y^{BC}_{t+3j,t+3j+3} + \sum_{m=1}^{\frac{k-15}{12}} 4y^{BC}_{t+12m+3,t+12m+15} \right), k \in 27, 39, 51, 63, 75$$

for Oct 1984,1986-1988,1992,1994 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{m=1}^{\frac{k-3}{12}} 4y^{BC}_{t+12m-9,t+12m+3} \right), k \in 27, 39, 51, 63$$

for Nov 1985 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{2}{3} y^{EH}_{t,t+2} + \sum_{m=1}^{\frac{k-2}{12}} 4y^{BC}_{t+12m-10,t+12m+2} \right), k \in 26, 38, 50, 62$$

for Dec 1997-2001,2003-2018 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{4} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{\frac{k-13}{12}} 4y^{BC}_{t+12m+1,t+12m+13} \right), k \in 25, 37, 49, 61, 73$$

for Dec 1996 :

The one assumption I make in constructing June forecasts in 2005 and 2008-2018 is that the expected average interest rate 13-16 months in the future equals the expected average interest rate 16-19 months ahead, i.e. that $y^{BC}_{13,16} = y^{BC}_{16,19}$. I make this assumption since forecasts over the latter horizon are unavailable on those dates.

Long-term forecasts of interest rates are formed based on the expectation of the average level of interest rates prevailing over a five-year period in the future. The Blue Chip survey gives semi-annual estimates of expectations of average interest rates either 6-10 or 7-11 years ahead.[57] Formally, denote the k-month ahead forecast made at $t+k-60$ as $y^{BC}_{t+k-60,t+k}$, which is the average expected level of interest rates in the five-year period starting $(k-60)$-months ahead:

$$y^{BC}_{t+k-60,t+k} = \frac{1}{60}\left(\mathrm{E}_t\left[\sum_{j=k-59}^{k} y_{t+j-1,t+j}\right]\right)$$

Forecasts are only available at long horizons on a regular basis in March (1986-1996), June (1997-2018), October (1986-1995), and December (1996-2018), due to the fact that the date on which long-term forecasts were made changed in 1996. In addition, there is one instance in which these forecasts were made in January (2003). Due to this varying coverage of forecasts, I present below precise construction of average expected

[57]While Blue Chip provides forecasts at horizons of 1-5 and 2-6 years ahead, I do not use this information since it is already embedded in the yearly forecasts on those dates.

rates ($y^{EH}_{t,t+k}$) for horizons between ten- and eleven-years ahead ($k \in [121, 139]$):

for Jan 2003 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{j=1}^{3} y^{BC}_{t+3j,t+3j+3} + \sum_{m=1}^{5} 4y^{BC}_{t+12m,t+12m+12} + 20y_{t+72,t+132} \right), k \in 132$$

for Mar 1986-1996 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{3} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{5} 4y^{BC}_{t+12m-2,t+12m+10} + 20y_{t+70,t+130} \right), k \in 130$$

for Jun 1997-2004, 2006-2007 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{2} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{5} 4y^{BC}_{t+12m-5,t+12m+7} + 20y_{t+67,t+127} \right), k \in 127$$

for Jun 2005, 2008-2018 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{4} y^{BC}_{t+3j-2,t+3j+1} + 2y^{BC}_{13,16} + \sum_{m=2}^{6} 4y^{BC}_{t+12m-5,t+12m+7} + 20y_{t+79,t+139} \right), k \in 139$$

for Oct 1983,1989-1991,1993,1995 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{j=1}^{4} y^{BC}_{t+3j,t+3j+3} + \sum_{m=1}^{5} 4y^{BC}_{t+12m+3,t+12m+15} + 20y_{t+75,t+135} \right), k \in 135$$

for Oct 1984,1986-1988,1992,1994 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(y^{EH}_{t,t+3} + \sum_{m=1}^{5} 4y^{BC}_{t+12m-9,t+12m+3} + 20y_{t+63,t+123} \right), k \in 123$$

for Dec 1997-2001,2003-2018 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+1} + \sum_{j=1}^{4} y^{BC}_{t+3j-2,t+3j+1} + \sum_{m=1}^{5} 4y^{BC}_{t+12m+1,t+12m+13} + 20y_{t+73,t+133} \right), k \in 133$$

for Dec 1996 :

$$y^{EH}_{t,t+k} = \frac{1}{\frac{k}{3}} \left(\frac{1}{3} y^{EH}_{t,t+} + \sum_{m=1}^{5} 4y^{BC}_{t+12m-11,t+12m+1} + 20y_{t+61,t+121} \right), k \in 121$$

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