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Safer ratios, riskier portfolios: Banks' response to government aid[☆]

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ABSTRACT

Using novel data on bank applications to the Troubled Asset Relief Program (TARP), we study the effect of government assistance on bank risk taking. Bailed-out banks initiate riskier loans and shift assets toward riskier securities after receiving government support. However, this shift in risk occurs mostly within the same asset class and, therefore, remains undetected by regulatory capital ratios, which indicate improved capitalization at bailed-out banks. Consequently, these banks appear safer according to regulatory ratios, but show an increase in volatility and default risk. These findings are robust to controlling for credit demand and account for selection of TARP recipients by exploiting banks' geography-based political connections as an instrument for bailout approvals.

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1. Introduction

The financial crisis of 2008–2009 resulted in an unprecedented liquidity shock to financial institutions in the U.S. (Gorton and Metrick, 2012) and abroad (Beltratti and Stulz, 2012). To stabilize the banking system, governments around the world initiated a wave of capital assistance to financial firms. Many economists and regulators argue that this wave altered the perception of the government safety net (Kashyap, Rajan, and Stein, 2008) and created a precedent that will have a profound effect on the future behavior of financial firms. At the forefront of this debate is the effect of the bailout on bank risk taking (Flannery, 2010), since risk taking, coupled with inadequate regulation (Levine, 2012), is often blamed for leading to the crisis in the first place. This debate has broad policy implications, since the relation between government intervention and bank risk taking is at the core of financial system design (Song and Thakor, 2011). This paper studies whether and how the recent bailout affected risk taking in credit origination and investment activities of U.S. banks.

Our empirical analysis exploits an economy-wide liquidity shock during the 2008–2009 financial crisis, which simultaneously affected an unusually large cross-section of firms and resulted in a bailout of hundreds of firms. In particular, we study the effect of the Capital Purchase Program (CPP), which invested $205 billion in U. S. financial institutions, becoming the first and largest initiative of the Troubled Asset Relief Program (TARP). Using hand-collected data on the status of bank applications for federal assistance, we observe both banks' decisions to apply for bailout funds and regulators' decisions to grant assistance to specific banks. This setting allows us to account for selection of bailed firms and to study the risk taking implications of both bailout approvals and bailout denials. Our risk analysis spans three channels of bank operations: (1) retail lending (mortgages), (2) corporate lending (syndicated loans), and (3) investment activities (financial assets).

Our empirical analysis begins with the retail credit market. By examining both approved and denied loan applications for nearly all residential mortgages in 2006–2010, our empirical strategy distinguishes the supply-side changes in bank credit origination from the demand-side changes in potential borrowers. In difference-in-difference tests, where the first difference is between banks that were granted and denied government assistance, and the second difference is from before to after the bailout, we find no significant effect of CPP on the volume of credit origination at approved banks, compared to their denied peers. We also find no significant change in the distribution of borrowers between approved and denied banks. Our main finding is that after being approved for federal assistance, banks shifted their credit origination toward riskier mortgages. This result holds whether we compare approved banks to denied banks, to non-applicant banks, or to all CPP-eligible banks. In economic terms, we find that relative to banks that were denied federal assistance, approved banks increased their origination rates on riskier mortgage applications (measured by the loan-to-income ratio) by 5.4 percentage points.

Our findings are qualitatively similar for large corporate loans. Using a similar difference-in-difference framework, we find a robust shift by approved banks toward higher-yield, riskier loans. After being approved for federal assistance, banks increased credit issuance to riskier firms, as measured by borrowers' cash flow volatility, interest coverage, and asset tangibility, and reduced credit issuance to safer firms. Altogether, our findings for both retail and corporate loans suggest that the bailout was associated with a shift toward higher-yield loans at approved banks rather than an expansion in credit volume.

We find a similar increase in risk taking by approved banks in their investment activities. After being approved for federal assistance, banks increased their investments in risky securities, such as non-agency mortgage-backed securities, and reduced their allocations to low-risk securities, such as Treasury bonds. For the average bank approved for federal assistance, the total weight of investment securities in bank assets increased by 9.7% after CPP relative to unapproved banks. Moreover, approved banks increased their allocations to risky securities, while, at the same time, reducing their allocations to lower-risk securities relative to unapproved banks. Overall, our analysis at the micro-level indicates a robust increase in risk taking in both lending and investment activities by banks approved for government assistance.

After providing micro-level evidence on the drivers of risk taking, we examine aggregate bank risk. First, we show that federal capital infusions improved capitalization levels of approved banks, with their average Tier-1 capital ratios increasing by approximately 160 basis points relative to unapproved banks. Second, we find that the reduction in leverage at approved banks was more than offset by their shift toward riskier assets. The net effect was a marked increase in the aggregate risk of approved banks compared to observably similar unapproved banks. This result holds robustly whether bank risk is measured by earnings volatility, stock volatility, market beta, or distance to default. For example, after the bailout, approved banks show a 20.9% increase in default risk (measured by the z-score) and a 15.3% increase in beta relative to unapproved banks.

We provide evidence that the shift in risk taking at approved banks is attributable to the treatment effect of government support rather than selection of approved firms. First, we explicitly control for proxies of the declared CPP selection criteria. We also capture any time-invariant heterogeneity between approved and unapproved banks via bank fixed effects. Second, we use propensity score matching of approved and unapproved banks based on firm fundamentals to allow for various functional forms of the relation between bank characteristics and risk. Finally, we use an instrumental variable approach, which relies on banks' geography-based political connections as an instrument for bailout approvals. In particular, we show that banks located in election districts of House members who served on key finance subcommittees during the development of CPP were more likely to be bailed out, while being virtually indistinguishable from unconnected banks based on other observable characteristics ex ante. We obtain similar results across these specifications.

We review three non-mutually exclusive explanations for the observed increase in risk at approved banks: (1) government intervention, (2) risk arbitrage, and (3) moral hazard. The first hypothesis—*government intervention*—posits that the increase in risk taking at approved banks is a consequence of government intervention in bank policies aimed at increasing capital flows into subprime mortgages and mortgage-backed securities. However, to the extent that bailed banks were subject to government regulations, these regulations sought to reduce rather than increase risk taking, for example, by limiting executive pay "to prevent excessive risk taking" and by restricting share repurchases and dividends to prevent asset substitution.

To investigate this hypothesis, we collect data on banks that applied for CPP, were approved, but did not receive CPP funds for various institutional reasons discussed in Section 5.2. We then compare risk taking by this subset of non-recipients to the banks that *did* receive the money and had similar size, financial condition, and performance at the time of CPP approval. We find a similar increase in risk taking across all banks approved for bailout funds, regardless of whether they received the money and were subject to the subsequent government regulation. As another test of the government intervention hypothesis, we examine changes in bank risk taking after the repayment of CPP capital. We find that the release from government oversight after the repayment of CPP funds has little effect on bank risk taking. Collectively, these results suggest that if government intervention played a role in banks' credit and investment policies, it was unlikely the primary driver of risk taking.

The second hypothesis—*risk arbitrage*—states that some risky assets, such as subprime mortgages and investment securities, were underpriced during the crisis, providing excess profit opportunities with low risk. In this case, CPP capital may have enabled approved banks to exploit these opportunities without an ex post increase in risk. In contrast, we find no evidence that an increase in risk taking at approved banks was followed by superior risk-adjusted returns, as proxied by alpha, the Sharpe ratio, or the information ratio. Rather, the shift toward higher-yield assets was associated with an increase in loan chargeoffs and, if anything, a slight decline in alphas at approved banks. Overall, while the extra capital likely played a role in approved banks' investment and lending decisions, these decisions reflected an increase in risk tolerance rather than low-risk arbitrage opportunities.

A third explanation—*moral hazard*—posits that a firm's approval for federal funds may signal its implicit government protection. According to this view, there is some ex ante probability that a given bank will be bailed out in case of distress. During a financial shock, the bank either receives government protection or is denied it. If there is some consistency in the regulators' treatment of banks across time, a bank's approval for government support signals an increase in the probability that this bank will be protected again in case of distress. Conversely, if a bank is denied government aid, the probability that this bank will be bailed out in the future goes down. This effect can be particularly significant in the short term, since the government will prefer to avoid the near-term distress of banks it has publicly declared to endorse. As an example of this continued government support, about 21% of CPP recipients were allowed to skip their dividends to the Treasury. Under this view, the bailout may encourage risk taking by protected banks by reducing investors' monitoring incentives and increasing moral hazard, as predicted in Acharya and Yorulmazer (2007) and Kashyap, Rajan, and Stein (2008), among others.

Our evidence suggests that moral hazard likely contributed to the increase in risk taking at approved banks. First, the finding that higher risk taking is associated with a signal of government support rather than with the capital injection itself is consistent with the effect of a revised probability of government protection in theoretical work (Mailath and Mester, 1994; Acharya and Yorulmazer, 2007). Second, the cross-sectional evidence aligns well with the predictions from models of moral hazard. In particular, the increase in risk taking is stronger at larger banks, banks that are closer to financial distress, and banks that received multiple signals of government forbearance in the form of skipped dividends. Finally, we find that approved banks increase their risk primarily by investing in assets with a high exposure to common macroeconomic risk, which is also reflected in an increase in banks' stock betas. If government protection is more likely in the case of a systematic rather than idiosyncratic shock to a firm, this evidence is consistent with a rational response of protected banks to a revised probability of future government support. This interpretation is also supported by the evaluation of CPP by its chief auditor, the Special Inspector General for the Troubled Asset Relief Program (SIGTARP).[1] It is also consistent with the views about a shift in bailed banks' risk tolerance expressed by prominent regulators in a testimony to Congress.[2]

Our article has important policy implications. First, one of the significant recent events was a negative revision of the outlook for long-term U.S. debt by Standard and Poor's, followed by a downgrade in August 2011 for the first time since the beginning of ratings in 1860. Among the reasons for a revised outlook cited by the rating agency were the increased risk of U.S. banks and a higher probability of another bailout.[3] Our paper identifies potential sources of the increased risk in the financial system and links them to the initial bailout policy and the predictions of academic theory. Second, earlier studies underscore the importance of bank capital for credit origination (Thakor, 1996) and economic growth (Levine, 2005). Our findings suggest an

[1] For example, in evaluating the consequences of government assistance on the financial sector, the SIGTARP report to Congress concludes that "To the extent that institutions were previously incentivized to take reckless risks through a 'heads, I win; tails, the Government will bail me out' mentality, the market is more convinced than ever that the Government will step in as necessary to save systemically significant institutions (SIGTARP, 2010, p. 6)."

[2] For example, in his testimony before the House Financial Services Committee on October 1, 2009, the former Fed Chairman, Paul Volker, stated: "What all this amounts to is an unintended and unanticipated extension of the official safety net…The obvious danger is that risk taking will be encouraged and efforts at prudential restraint will be resisted."

[3] Standard and Poor's Sovereign Credit Rating Report, "United States of America 'AAA/A-1+' Rating Affirmed; Outlook Revised To Negative", April 18, 2011, p. 4.

asymmetric response of banks to capital shocks. In particular, while previous research shows that a negative shock to bank capital forces a cut in lending (Berger and Bouwman, 2013), we find that a positive shock to capital need not result in credit expansion, but instead may lead to riskier lending and investments. Finally, though capital requirements are a key instrument in bank regulation (Bernanke and Lown, 1991), we show that banks' strategic response to this mechanism erodes its efficacy in monitoring bank risk.

2. Related literature

2.1. Theoretical motivation and main hypotheses

The government safety net has been long recognized as a cornerstone of the economic system. Its architecture includes social assistance programs, government insurance, and financial regulation. We adopt this broader perspective and begin with a review of theoretical work on government guarantees in general economic settings. We then proceed with a more specific discussion of government guarantees in financial regulation.

The early theoretical work on government guarantees has focused on social insurance programs such as social security and unemployment insurance. The classical studies in this area established the first predictions about the unintended effect of government guarantees on agents' incentives (Ehrenberg and Oaxaca 1976; Mortensen, 1977). In particular, government guarantees in the form of social insurance create moral hazard and perverse incentives for individuals and firms, imposing welfare costs. For firms, the moral hazard effect from government insurance results in riskier management of human capital (Feldstein, 1978; Topel, 1983; Burdett and Wright, 1989). For individuals, the implicit reliance on government insurance results in higher risk tolerance and reduced effort (Feldstein, 1989; Hansen and Imrohoroglu, 1992).[4]

In financial regulation, government guarantees were first studied in the context of deposit insurance. Using a contingent claim framework, Merton (1977) has shown that deposit insurance provides banks with a put option on the guarantor. Unless insurance premia perfectly adjust for risk, this option induces banks to take on more risk. In later work, Kanatas (1986) found that even if insurance premia are periodically adjusted for risk, banks have an incentive to show lower risk in assessment periods and increase risk between examination dates.

A related strand of theoretical work reached similar conclusions by studying another form of government insurance—loan guarantees. In particular, federal loan guarantees incentivize firms to make riskier investments and raise leverage (Chaney and Thakor, 1985), imposing large costs on the government in the form of higher liabilities (Sosin, 1980; Bulow and Rogoff, 1989; Hemming, 2006).

Perhaps the most extreme type of government guarantees is a bailout of distressed firms. A number of studies show analytically that this form of downside protection encourages risk taking by inducing moral hazard, both by individual banks (Mailath and Mester, 1994) and at the aggregate level (Acharya and Yorulmazer, 2007). This risk taking behavior has a destabilizing effect on the financial system (Acharya, Drechsler, and Schnabl, 2014). However, a contrasting theory argues that bailouts may reduce risk taking at protected banks. In particular, a bailout raises the value of a bank charter by reducing the refinancing costs and increasing the bank's long-term probability of survival. In turn, the higher charter value, which a bank would lose in case of failure, deters risk taking (Keeley, 1990). This disciplining effect of the charter is predicted to be amplified under conditions similar to those observed during the recent crisis. For example, when the bailout is discretionary and follows an adverse macroeconomic shock, the risk-reducing effect of the charter value may outweigh moral hazard, resulting in a lower equilibrium level of risk (Goodhart and Huang, 1999; Cordella and Yeyati, 2003).[5]

2.2. Empirical evidence

A recent wave of bailouts around the globe has enabled researchers to provide empirical evidence on various types of government intervention. In particular, government assistance in the United States and Germany has received the most attention in the literature and will be the primary focus of our discussion.

In the U.S., several studies have focused on the causes and consequences of government assistance during the financial crisis. Veronesi and Zingales (2010) calculate the costs and benefits of the bailout from the perspective of big banks' stakeholders and conclude that these firms received large subsidies. Berger and Roman (2013) find that TARP recipients obtained competitive advantages, which allowed these banks to increase market share and market power. Bayazitova and Shivdasani (2012) study banks' incentives to participate in CPP and show that the bailout raised investor expectations of future regulatory interventions. Li (2013) examines the determinants of government assistance and provides evidence on asset growth at bailed banks. Duchin and Sosyura (2012) show that politically connected banks were more likely to receive CPP funds but earned lower returns for taxpayers.

Perhaps the closest to our article is a recent study by Black and Hazelwood (2013), which provides survey evidence on credit origination at 29 TARP banks and 28 non-TARP banks. The authors find that after the bailout, large and medium TARP banks shifted their lending toward riskier loans (as measured by the banks' own risk ratings), and attribute this result to moral hazard. This paper and ours provide complementary evidence from different angles: from commercial loans in their article to retail credit, syndicated loans, and portfolio investments in ours.

[4] More recent contributions derive similar conclusions and demonstrate the pernicious welfare effects resulting from perverse incentives introduced by government guarantees. See Fredriksson and Holmlund (2006) for a review of this work.

[5] Cheng and Milbradt (2012) further show that a bailout policy may play an important role in instilling confidence in creditor markets and preventing credit freezes.

In addition, by combining the study of banks' asset risk with the analysis of capital positions, we provide evidence on banks' aggregate risk. We find that the improvement in bailed banks' capital ratios was more than offset by an increase in their asset risk, resulting in a higher likelihood of default, compared to unapproved banks.

Outside of the U.S., research on government interventions in Germany has provided a valuable long-term perspective. Gropp, Grundl, and Guettler (2013) find that the removal of government guarantees for German savings banks leads to lower risk taking and conclude that such guarantees create moral hazard. Berger, Bouwman, Kick, and Schaeck (2012) study two types of regulatory interventions in Germany: disciplinary actions and mandatory capital support. The authors find that both types of interventions are generally associated with lower risk taking and liquidity creation at disciplined banks. Their evidence also yields two important conclusions: (1) the consequences of government interventions vary depending on the business cycle and have an effect mainly in non-crisis years, and (2) disciplinary actions against banks generate spillover effects on other banks, providing the latter with a competitive advantage.

The combination of prior evidence and our findings suggests a nuanced effect of government aid on bank risk taking. This effect appears to vary with the regulatory signal associated with capital infusions, the likelihood of regulatory forbearance, and the quality of program governance. Next, we briefly discuss these factors.

The first important factor is the type of the information signal (positive or negative) that accompanies government assistance. In the U.S., government capital injections were voluntary and targeted a large fraction of banks. In this setting, a bank's approval for federal funds implied that the regulators viewed it as sufficiently healthy and/or systemically important to receive a federal back-up (Paulson, 2008). In contrast, in Germany, capital injections were mandatory, targeted the weakest 7% of banks, and sent a strong negative signal that the bank was put on close watch by the regulators. Consistent with this interpretation, the negative signals from the regulators—mandatory injections in Germany and rejections of CPP applications in the U.S.—were kept confidential to avoid bank runs and were associated with a reduction in risk in both markets. In contrast, the positive signal of a federal back-up in the U.S. was associated with an increase in risk taking.

The second important factor is regulatory forbearance. Prior research shows that regulators are less likely to close weak banks during financial crises when the financial system is fragile (Acharya and Yorulmazer, 2007; Brown and Dinc, 2011). If these incentives reduce the threat of closure for bailed banks, government aid may be less effective during crises. Consistent with this view, Berger, Bouwman, Kick, and Schaeck (2012) find that government capital injections fail to restrict bank risk taking and have no effect on liquidity creation during crises, unlike in non-crisis years. Similarly, we show that government aid in the U.S. during the crisis had little effect on total credit supply and was associated with an increase rather than a reduction in risk taking. One caveat is that we study a relatively short time period, and our findings may be specific to programs initiated during crises.

The third important factor is the role of political interests in government intervention. Kane (1989, 1990) argues that regulators' political interests and short horizons weaken enforcement in government programs. More recently, Calomiris and Wallison (2009) find evidence of politically motivated regulatory forbearance during the recent crisis. Mian, Sufi, and Trebbi (2010) show political motivations in the adoption of TARP, which was initiated just before the elections. To the extent that such factors played a role in CPP, our evidence suggests that they may distort risk taking incentives. Under this view, our paper adds to research on economic distortions from government intervention in the financial sector (Sapienza, 2004; Khwaja and Mian, 2005; Berger and Roman, 2013) and in other settings (Faccio, Masulis, and McConnell, 2006; Cohen, Coval, and Malloy, 2011).

3. Data and summary statistics

3.1. Capital purchase program

The Emergency Economic Stabilization Act (EESA), signed into law on October 3, 2008, created TARP, a system of federal initiatives aimed at stabilizing the financial system. The first and largest of these initiatives was CPP. Initiated on October 14, 2008, this program invested $204.9 billion in 707 firms in 2008–2009.

To apply for CPP funds, a qualifying financial institution (QFI)—a domestic bank, bank holding company, savings association, or savings and loan holding company—submitted a two-page application (shown in Internet Appendix A.1) to its primary banking regulator: the Federal Reserve, the Federal Deposit Insurance Corporation (FDIC), the Office of the Comptroller of the Currency (OCC), or the Office of Thrift Supervision (OTS). Applications of bank holding companies were submitted both to the regulator overseeing the holding company's largest bank and to the Federal Reserve. If the initial application review by the banking regulator was successful, the application was forwarded to the Treasury, which made the final decision on the investment.

The review of CPP applicants was based on the regulators' *Camels* rating system, which evaluates six bank characteristics: *C*apital adequacy, *A*sset quality, *M*anagement, *E*arnings, *L*iquidity, and *S*ensitivity to market risk. The ratings in each category, ranging from 1 (best) to 5 (worst), are assigned based on financial ratios and onsite examinations. Our proxies for these assessment categories are shown in Appendix A.

In exchange for CPP capital, banks provided the Treasury with preferred stock, which pays quarterly dividends at an annual yield of 5% for the first five years and 9% thereafter. The investment amount in preferred shares was decided by the Treasury, subject to a minimum of 1% of a firm's risk-weighted assets (RWA) and a maximum of 3% of RWA or $25 billion, whichever was smaller. In addition, the Treasury obtained ten-year warrants for the common stock of public firms.[6]

3.2. Sample firms

To construct our sample, we begin with 600 publicly traded CPP-eligible firms that were active as of September 30, 2008, the quarter-end immediately preceding CPP. We focus on public firms because we can identify the status of their CPP applications from regulatory filings and because these firms account for the vast majority (92.8%) of CPP capital.

To identify CPP applicants and determine the status of each application, we read quarterly filings, annual reports, and proxy statements of all CPP-eligible public firms from 4Q 2008 to 4Q 2009. We also supplement these sources with a search of each firm's press releases for any mentioning of CPP or TARP and, in cases of missing data, we contact the firm's investor relations department for verification. Using this procedure, we are able to ascertain the application status of 538 of the 600 CPP-eligible public firms (89.7% of firms).

From the 538 firms with available data, we exclude the 17 large QFIs in our sample that were subject to stress tests under the Capital Assessment Plan (CAP).[7] This filter is motivated by several reasons. First, there is evidence that at least some of these firms were explicitly asked by the regulators to participate in CPP (Solomon and Enrich, 2008). Second, on February 10, 2009, the regulators announced that these firms would be required to participate in CAP. Under this plan, the said firms underwent formal assessment of capitalization levels, and nine of the 17 excluded QFIs were asked to raise $63.1 billion in equity capital.[8] Third, in contrast to CPP, the capital raised under CAP was in the form of common stock rather than preferred stock. Because of these distinctions of CAP firms, we follow a conservative approach and exclude them from our sample. Our results are qualitatively similar for these firms, as discussed in the robustness section.

Of the 521 firms in our final sample, 416 firms (79.8%) submitted CPP applications, and the remaining 105 firms disclosed their decisions not to apply to CPP. Among the 416 submitted applications, 329 applications (79.1%) were approved for funding. Finally, among the firms approved

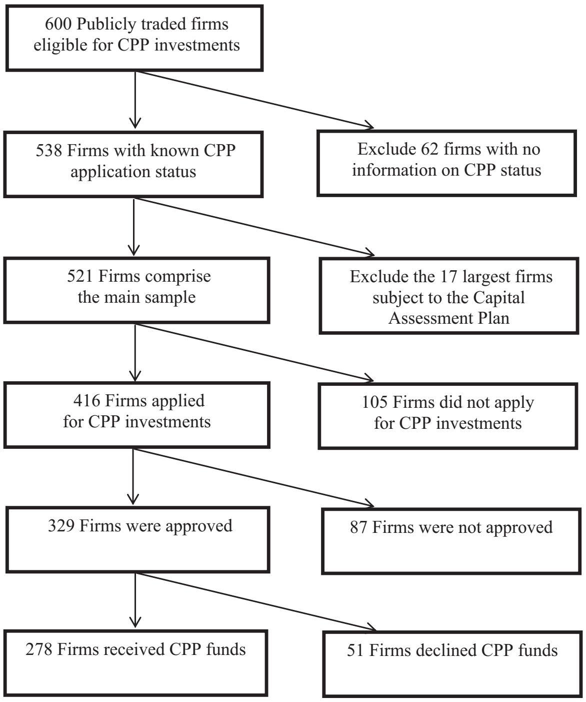

Fig. 1. Sample firms and their CPP applications. This figure illustrates the partitioning of firms based on the status of their CPP applications. The starting point for this partitioning is the universe of 600 publicly traded financial firms that were eligible for CPP assistance as of 3Q 2008.

for funding, 278 (84.5%) accepted the funds, while 51 firms (15.5%) declined the funds. Fig. 1 shows the partitioning of firms into these subgroups.

Fig. 2 illustrates the application timeline for the median CPP applicant in our sample. To reconstruct the key dates in the application process, we collect this information from firms' press releases, proxy filings, annual and quarterly reports, and records of shareholder meetings. Internet Appendix A.2 shows examples of firms' disclosures about their CPP applications. The median firm in our sample received a decision on its CPP application 19 calendar days after its submission. For the median firm whose application was approved, it took an additional 12 days to announce the firm's decision to accept or decline CPP funds. Finally, for the median firm that accepted CPP funds, it took an additional four days for the funds to be disbursed from the Treasury.[9] Overall, the vast majority (85.7%) of the QFIs in our sample received CPP funds by the end of January 2009.

The 278 public firms in our sample received $36.7 billion from CPP. Panel A in Table 1 shows that the average (median) amount of CPP investment was $132 ($30) million. Fig. 3 shows that the vast majority (77%) of CPP investments were made at the maximum

[6] The warrants were issued for such number of common shares that the aggregate market value of the covered common stock was equal to 15% of the investment in preferred stock.

[7] The excluded firms include Citigroup, JP Morgan, Bank of America (including Merrill Lynch), Goldman Sachs, Morgan Stanley, State Street, Bank of New York Mellon, Wells Fargo (including Wachovia), KeyCorp, Fifth Third Bancorp, Regions Corp., BB&T, Capital One, SunTrust, U.S. Bancorp, American Express, and PNC Financial Services. The two other firms subject to the Capital Assessment Plan (GMAC and MetLife) were not part of our original sample. In particular, GMAC, the financing arm of General Motors, received TARP funds through the Automotive Industry Financing Program (AIFP) rather than CPP. MetLife was excluded as an insurance firm with negligible (Internet) banking operations.

[8] The nine excluded QFIs required to raise capital include the following firms: Bank of America ($33.90 billion), Citigroup ($5.50 billion), Wells Fargo ($13.70 billion), Morgan Stanley ($1.80 billion), PNC Financial Services ($0.60 billion), SunTrust Banks ($2.20 billion), Regions Corp. ($2.50 billion), Fifth Third Bancorp ($1.10 billion), and KeyCorp ($1.80 billion).

[9] Our findings do not vary significantly with the time spent by an applicant firm in each stage of the CPP application process. In unreported robustness tests, we split the sample at the median value of the time interval spent by a firm in each stage of the application process: (1) time to receive a decision, (2) time to decide whether to accept CPP funds, and (3) time to receive the funds. Our conclusions about the effect of CPP approvals are very similar across these subsamples.

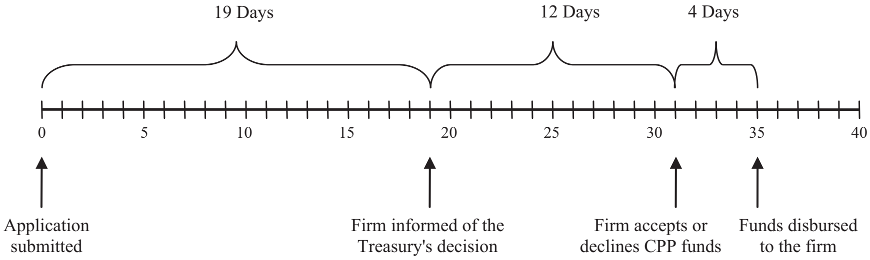

Fig. 2. CPP application timeline. This figure shows the median length of time in each stage of the CPP application process for our sample firms with available data. Time intervals are shown in calendar days relative to day zero, the application submission date. For firms with a missing application submission date, the application is assumed to have been submitted on the day of the application deadline for public firms, November 14, 2008. Time spent on the decision to accept or decline CPP funds is computed for approved CPP applicants. Time spent on the disbursement of CPP funds is computed for approved applicants that accepted the funds. The sample contains 416 publicly traded financial firms that submitted CPP applications.

amount stipulated by the program (3% of RWA). Because the investment amount was often hard-wired to a firm's RWA, we mostly do not focus on investment amounts.

Financial data on firms come from the Call Reports filed by all active FDIC-insured institutions. Panel A of Table 1 provides sample-wide summary statistics for the Camels proxies and other firm characteristics during our sample period, January 2006 to December 2010. The average (median) QFI is 67 (61) years old and has book assets of \$3.27 (\$1.45) billion. The Camels variable *Capital adequacy*, which reflects a bank's Tier-1 risk-based capital ratio, shows that the majority of banks are well capitalized. For example, the 50th percentile of the Tier-1 ratio in our sample is 10.7%, nearly double the threshold of 6% stipulated by the FDIC's definition of a well-capitalized institution. The variable *Asset quality* captures loan defaults and shows the negative of the ratio of nonperforming loans to total loans. The variable *Earnings*, measured as the return on equity (ROE), indicates that the average (median) bank in our sample has a quarterly ROE of 3.2% (6.5%). The variable *Management quality* is calculated as the negative of the annual number of disciplinary actions imposed on the bank holding company and its executives. In addition to serving as a management quality proxy, this variable controls for the effect of regulatory interventions on bank policies documented in Berger, Bouwman, Kick, and Schaeck (2012). The data on disciplinary orders, including the period when the order is in effect, are obtained from online databases of corrective orders of the four banking regulators.

3.3. Loan data

We collect mortgage application data from the Home Mortgage Disclosure Act (HMDA) Loan Application Registry, which covers about 90% of mortgage lending in the U.S. (Dell'Ariccia, Igan, and Laeven, 2012), except for loans issued by small rural banks.[10] Each observation is a mortgage application, which includes borrower characteristics (e.g., income, gender, and race), features of the requested loan (e.g., loan amount and loan type), and the bank's decision on the loan (e.g., loan originated, application denied, application withdrawn).

We aggregate financial institutions in HMDA at the bank holding company level and match them to our sample firms. We limit our sample to loan applications that were denied or approved, thus excluding observations with ambiguous statuses, such as incomplete files and withdrawn applications. To study new credit origination, we restrict our sample to new loans, excluding refinancing and purchases of existing loans. We also exclude loans that are sold upon origination because they have relatively little effect on the originating bank's risk.[11] In particular, such loans typically leave the originating bank's books within 39 days of issuance (Rosen, 2010).

Panel B of Table 1 shows summary statistics for our sample of mortgage applications. The median borrower earns \$73,000 per year and applies for a \$123,000 mortgage. About 64.3% of applications are approved. The data indicate significant variation in the loan-to-income ratio, a common measure of loan risk in the mortgage industry.[12] This ratio in our sample ranges from 0.85 at the 25th percentile to 2.78 at the 75th percentile.

Data on corporate loan facilities are collected from DealScan. This data set covers large corporate loans, the vast majority of which are syndicated (originated by several banks). DealScan reports loans at origination, allowing us to study new corporate credit and avoid contamination from the drawdowns of prior loan commitments. Each observation is a newly issued credit facility, which lists the originating bank(s), date of origination, loan amount, interest rate, and the corporate borrower. Panel B of Table 1 shows that the average (median) corporate loan amount in our DealScan sample is \$604 (\$300) million.

[10] A depository institution is required to report HMDA data if it has any branches in any metropolitan statistical area and meets the minimum threshold of asset size, which was equal to \$37 million in book assets as of 2008.

[11] In unreported tests, we study the effect of CPP approvals on the risk of securitized loans and find that this effect is qualitatively similar but economically smaller than the effect on originated-to-hold loans, which comprise our main sample.

[12] For example, the loan-to-income ratio is used by regulators in the assessment of mortgage risk in determining loan eligibility for federal loan modification programs, such as the Federal Home Affordable Modification Program (HAMP).

Table 1
Summary statistics.
This table reports summary statistics for our sample, which consists of 521 publicly traded firms eligible for participation in the Capital Purchase Program (CPP) with available data on program application status, excluding the firms subject to the Capital Assessment Plan (CAP). The sample period is 2006–2010, and the reported figures are sample-wide statistics. Panel A reports firm-level data. Financial data are from Call Reports, and CPP data are from the Treasury's Office of Financial Stability and firms' disclosures. Panel B reports loan-level data. Mortgage application data are from the Home Mortgage Disclosure Act (HMDA) Loan Application Registry. Corporate loan data are from DealScan. All variables are defined in Appendix A.

Variable	Mean	25th Percentile	Median	75th Percentile	Standard deviation
Panel A: Firm-level data					
CPP participation					
CPP application indicator	0.798	1.000	1.000	1.000	0.402
CPP approval indicator (if applied)	0.791	1.000	1.000	1.000	0.407
CPP investment indicator (if approved)	0.845	1.000	1.000	1.000	0.362
CPP investment amount ($000)	132,020	14,700	30,000	80,347	356,287
Firm characteristics					
Total assets ($000)	3,274,330	667,440	1,450,760	3,402,850	4,623,690
Age	67.0	21.0	61.0	107.0	48.6
Capital adequacy (%)	12.876	9.692	10.658	12.748	9.256
Asset quality (%)	−1.889	−2.274	−0.927	−0.264	3.166
Management quality	−0.313	−1.000	0.000	0.000	0.464
Earnings (%)	3.211	1.706	6.483	10.483	15.758
Liquidity (%)	3.993	2.231	3.028	4.207	4.217
Sensitivity to market risk (%)	14.681	5.382	11.029	19.865	12.534
Foreclosures (%)	0.397	0.033	0.148	0.411	1.086
Loan charge-offs (%)	0.824	0.069	0.269	0.924	1.705
Funding mix (%)	27.361	15.447	21.583	31.225	26.834
Exposure to regional econ. shocks (%)	−0.032	−0.619	0.303	0.740	1.109
Panel B: Loan-level data					
Mortgage application data					
Application approval indicator	0.643	0.000	1.000	1.000	0.479
Loan to income	2.000	0.851	1.778	2.778	1.515
Loan amount ($000)	179.1	59.0	123.0	238.0	165.9
Applicant income ($000 per year)	104.3	44.0	73.0	128.0	88.0
High yield	0.097	0.000	0.000	0.000	0.296
Corporate loan data					
Loan amount ($000)	604,000	150,000	300,000	700,000	941,000

4. Main results

4.1. Baseline evidence on retail lending

In this section, we study the effect of CPP on credit origination and risk taking in the mortgage market. We begin with a difference-in-difference model of credit origination, where the first difference is from before to after CPP, and the second difference is between approved banks and various control groups: denied banks, non-applicant banks, or all eligible banks. We continue with a matched sample analysis and instrumental variable regressions.

To isolate banks' active lending decisions from changes in credit demand, we estimate a linear model of loan approvals, where each observation is a mortgage application, and the outcome variable is a bank's decision to approve or deny the loan. This model, estimated over our sample period of 2006–2010, is specified as follows:

$$\begin{aligned} Y_{i,b,c,t} = {} & \beta_1\, Loan\ to\ income_{i,t} + \beta_2\, After\ \ CPP \times Approved\ \ bank_b \\ & + \beta_3\, After\ \ CPP \times Loan\ to\ income_{i,t} \\ & + \beta_4\, Approved\ \ bank_b \times Loan\ to\ income_{i,t} \\ & + \beta_5\, After\ \ CPP \times Approved\ \ bank_b \times Loan\ to\ income_{i,t} \\ & + A_b + B_c + C_t + \alpha X_{b,t} + \delta V_{c,t} + \gamma Z_i + \varepsilon_{i,b,c,t}. \end{aligned} \tag{1}$$

The dependent variable $Y_{i,b,c,t}$ is an indicator that equals one if a loan application by customer i at bank b in the local market c during year t is approved and zero otherwise. The independent variables related to CPP include the indicators *Approved bank* (which equals one for approved CPP applicants and zero for unapproved banks), *After CPP* (which equals one in 2009–2010 and zero otherwise), and their interaction terms. The primary measure of borrower risk is the loan-to-income ratio. This variable is available for both approved and denied applications and has been shown to be a good predictor of mortgage default (Campbell and Cocco, 2011). Our main variable of interest is the interaction term *After CPP* × *Approved bank* × *Loan to income*, which shows how the marginal effect of CPP on loan origination at approved banks (relative to unapproved banks) varies with borrower risk. In the robustness section, we also show evidence with an alternative measure of loan risk based on the loan yield spread.

The first set of control variables includes proxies for CPP selection criteria—the Camels scores—to account for differences in fundamentals between approved and denied firms. It is worth noting that our Camels proxies are imperfect measures of the true Camels scores because the former do not capture the content of onsite examinations. It is also possible that the regulators used other intangible or undeclared criteria in the selection process.

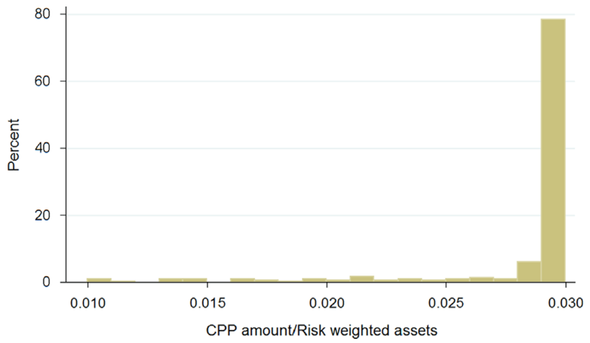

Fig. 3. Distribution of CPP investment amounts. This figure presents a histogram plot of the ratio of CPP investment amounts to risk-weighted assets (RWA) of recipient firms. According to CPP guidelines, the minimum CPP investment amount is equal to 1% of RWA, and the maximum amount is equal to 3% of RWA or $25 billion, whichever is smaller. The sample contains 278 publicly traded financial firms that received CPP investment funds.

To help control for this heterogeneity between approved and denied firms, we also include bank fixed effects, which capture all differences between the two groups that remain invariant during our five-year period.

The second set of control variables includes other time-variant bank characteristics ($X_{b,t}$), such as bank size, age, foreclosures, funding mix, and exposure to regional economic shocks. By including these variables, we control for the possibility that they vary systematically in time between approved and denied firms in a way that is correlated with risk but unrelated to CPP. As a proxy for a bank's funding mix, we use the ratio of deposit funding from purchased money to core deposits (Song and Thakor, 2007). We also construct an index of a bank's exposure to regional economic shocks. This index is computed as a weighted average of quarterly changes in the state-coincident macro indicators from the Federal Reserve Bank of Philadelphia across the states where a bank maintains branches, with the weights indicating the fraction of the bank's deposits in each state.[13]

Our third set of controls captures variation in borrower clientele and local credit market conditions. To account for cross-market variation, we include local market fixed effects, thus comparing credit origination between approved and denied banks within the same local market (Census tract). To account for heterogeneity in borrower clientele, we include fixed effects for borrowers' demographics (gender, race, and ethnicity (Z_i)) and proxies for quarterly changes in local economic conditions at the county level: (1) home vacancy rate from the U.S. Postal Service, (2) per capita income from the Bureau of Labor Statistics (BLS), and (3) unemployment from the BLS ($V_{c,t}$). To absorb common temporal shocks to treatment and control firms, all regressions include year fixed effects.

We estimate the regression model using an ordinary least squares (OLS) method and use standard errors clustered at the bank level to allow for within-bank correlation of residuals in loan approvals. Our choice of a linear rather than nonlinear model of loan approvals is motivated by two factors. First, nonlinear models tend to produce biased estimates in panel data sets with a short time series and many fixed effects, leading to an incidental parameters problem and inconsistent estimates.[14] Second, nonlinear fixed effects models generate biased estimates for interaction terms (Ai and Norton, 2003), the main coefficients of interest. Therefore, following the recommendation of the econometrics literature (Wooldridge, 2002) and the design of other recent studies on panel data sets of loan approvals (Puri, Rocholl, and Steffen, 2011), we estimate a linear model of loan approvals.

Column 1 of Table 2 shows baseline difference-in-difference evidence, which compares the volume and risk of credit origination between approved and denied banks from before to after CPP. Columns 2–3 compare approved banks to non-applicant firms and all CPP-eligible firms, respectively. The empirical results across the three columns show a significant shift in loan origination toward riskier borrowers at approved banks relative to any of the control groups, as indicated by the positive and significant coefficient on the interaction term *After CPP* × *Approved bank* × *Loan to income*. Across the first three columns, the coefficients on the triple interaction term are positive, statistically significant (p-values = 0.003–0.027), and comparable in magnitude (0.069–0.076), suggesting that the difference-in-difference increase in risk taking at approved banks is driven by the treatment group rather than specific to a given control group. The economic magnitudes are also nontrivial. Based on column 1, relative to banks that were denied federal assistance, approved banks increased their loan origination rates by 5.4 percentage points for riskier mortgage applications.[15] Importantly, the relative shift toward riskier borrowers by approved banks is observed only in the post-CPP period. In contrast, credit origination rates for riskier borrowers were statistically indistinguishable between the treatment and control groups before CPP, as indicated by the insignificant coefficients on the term *Approved bank* × *Loan to income*. Finally, the evidence shows no significant effect of CPP on the total credit supply by approved banks relative to any of the control groups, as indicated by the economically small and

[13] This index is constructed as in Bayazivota and Shivdasani (2012). The coincident indicators capture the economic conditions in a state by aggregating four state-level variables into one statistic: (1) nonfarm employment, (2) average hours worked in manufacturing, (3) unemployment, and (4) wage and salary disbursements deflated by the consumer price index.

[14] The incidental parameters problem, first noted in Neyman and Scott (1948) and discussed more recently in Lancaster (2000) and Greene (2004), arises because the number of fixed effects increases without bounds, but the amount of information available for their estimation is limited, particularly in large panel data sets with a short time series. As a result, both fixed effect estimates and coefficients on other variables tend to be biased in this setting.

[15] The risk of mortgage applications is measured symmetrically around the median loan-to-income ratio (1.778). Specifically, increasing the loan-to-income ratio from 10% below the median (40th percentile = 1.415) to 10% above the median (60th percentile = 2.122) implies an increase of $0.076 \times (2.122 - 1.415) = 0.054$ or 5.4 percentage points in the post-CPP mortgage origination rate for riskier borrowers by approved banks relative to denied banks.

Table 2
Credit origination and risk taking in the mortgage market.

This table reports regression estimates from a linear probability model explaining the relation between a bank's approval for CPP funds and a bank's mortgage origination decisions across borrowers of different risk. The dependent variable is an indicator that equals one if a loan was approved and zero if it was denied. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. *Loan to income* is the loan amount requested in a mortgage application divided by the applicant's annual income. Columns 1, 4, and 7 compare approved CPP applicants to denied applicants; columns 2, 5, and 8 compare approved CPP applicants to other eligible firms that did not apply for CPP funds; columns 3, 6, and 9 compare approved CPP applicants to all CPP-eligible firms. In columns 1–6, *Approved bank* is an indicator that equals one if a bank applied for CPP funds and was approved for funding. In columns 7–9, *Approved bank* is instrumented as the predicted likelihood that a bank is approved for CPP funds, conditional on applying, from a regression of CPP approvals on a bank's geography-based representation on the House Financial Services Committee (please see Appendix C for details). The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. Columns 4–6 refer to matched sample analysis, constructed as follows. In the matched sample, for each firm that applied for but was denied CPP funds (column 4) or for each eligible firm that did not apply for CPP funds (column 5) or for any eligible firm that was not approved for CPP funds (column 6), we match the closest approved bank based on propensity scores estimated from a regression that predicts the likelihood of CPP approval, using a bank's Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size (please see Appendix B for matched samples). All variables are defined in Appendix A. The individual loan application data come from the Home Mortgage Disclosure Act (HMDA) Loan Application Registry and cover the period 2006–2010. All regressions include bank-level controls, housing market controls, borrower demographic controls (gender, race, and ethnicity), year fixed effects, bank fixed effects, and regional market fixed effects. *Bank-level controls* include the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. *Housing market controls* include home vacancy rates, per capita income, and unemployment, which are measured at the county level. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Specification	Full sample			Matched sample			Instrument		
Treatment	Approved vs. unapproved applicants	Approved vs. non-applicants	Approved vs. all CPP-eligible firms	Approved vs. unapproved applicants	Approved vs. non-applicants	Approved vs. all CPP-eligible firms	Approved vs. unapproved applicants	Approved vs. non-applicants	Approved vs. all CPP-eligible firms
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Loan to income	−0.029***	−0.028***	−0.028***	−0.033***	−0.034***	−0.032***	−0.027***	−0.021***	−0.030***
	[<0.001]	[0.002]	[<0.001]	[<0.001]	[0.002]	[0.001]	[<0.001]	[0.003]	[<0.001]
After CPP × Approved bank	−0.023	0.015	−0.024	−0.046	0.006	0.011	−0.015	0.014	−0.019
	[0.500]	[0.493]	[0.209]	[0.545]	[0.469]	[0.582]	[0.521]	[0.600]	[0.467]
After CPP × Loan to income	0.007	0.038	−0.023	−0.039	0.040	−0.059	−0.067	0.035	−0.030
	[0.928]	[0.491]	[0.792]	[0.461]	[0.516]	[0.457]	[0.260]	[0.583]	[0.366]
Approved bank × Loan to income	−0.012	−0.013	−0.022	−0.007	−0.011*	−0.009	−0.023	−0.011	−0.011
	[0.388]	[0.192]	[0.382]	[0.413]	[0.092]	[0.428]	[0.447]	[0.104]	[0.311]
After CPP × Approved bank × Loan to income	0.076***	0.069**	0.071***	0.062***	0.073*	0.064***	0.080***	0.065**	0.075***
	[0.003]	[0.027]	[0.005]	[0.005]	[0.087]	[0.003]	[0.009]	[0.024]	[0.008]
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing market controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional market fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	686,106	715,793	895,132	115,176	238,105	768,746	686,106	715,793	895,132
Adjusted R^2	0.287	0.230	0.298	0.216	0.238	0.250	0.286	0.225	0.283

statistically insignificant coefficients (p-values = 0.21–0.50) on the interaction term *After CPP* × *Approved bank*.

4.2. Matched sample analysis

So far, we have used control variables to separate the effect of CPP approval from the proxies of CPP selection criteria. This specification assumes a linear relation between bank characteristics and measures of risk. In this subsection, we relax this assumption by constructing matched samples of approved and denied banks.

Using a one-to-one matching method without replacement, for each of the 87 denied CPP applicants, we select one approved bank that is closest to the denied bank according to observable characteristics. In particular, the matching bank selected is the bank with the closest propensity score, estimated from a linear regression of CPP approvals on a bank's Camels proxies, foreclosures, size, age, exposure to regional shocks, and funding mix. This procedure results in a matched sample of 174 firms, where the treatment and control groups are statistically indistinguishable according to the Camels proxies and other bank characteristics (columns 1–4 of Appendix Table B.1).

In columns 4–6 of Table 2, we estimate regressions of mortgage origination using matched samples of approved and unapproved firms by comparing approved and denied CPP applicants (column 4), approved CPP applicants and non-applicants (column 5), and approved CPP applicants and all CPP-eligible firms (column 6). The results are similar to those in baseline tests. In particular, in difference-in-difference terms, we find that after CPP, approved banks shift their credit origination toward riskier loans across all control groups. This result is shown by the positive coefficients on the interaction term *After CPP* × *Approved bank* × *Loan to income* across columns 4–6, with magnitudes comparable to those observed earlier (0.062–0.073), but slightly higher p-values (0.003–0.087) in a smaller sample. A similar calculation shows that, based on column 4, approved banks increased their origination rates on riskier mortgages by 4.4 percentage points. As before, we find no effect of CPP on total credit volume, as shown by the insignificant coefficients on the term *After CPP* × *Approved bank*.

Overall, our main finding is that after CPP, approved banks tilted their credit origination toward higher-yield loans by loosening credit standards for riskier borrowers. This pattern is consistent with a strategy aimed at originating higher-yield assets without causing a decline in regulatory ratios, which typically do not distinguish between higher-yield and lower-yield mortgages.[16]

4.3. Instrumental variable analysis

In this section, we use an instrumental variable (IV) approach to account for selection of CPP firms and demonstrate that the reported effect of CPP approval on bank credit policies may have a causal interpretation.

As an instrument for CPP approval, we propose a firm's geographic location in the election district of a House member serving on key finance committees involved in drafting and amending TARP. We consider a firm to be connected to a politician if it is headquartered in his or her election district. We consider a politician to be connected to TARP if he or she served on the Subcommittee on Financial Institutions or the Subcommittee on Capital Markets of the House Financial Services Committee in October 2008. These subcommittees played a direct role in the development of EESA and were charged with preparing voting recommendations for Congress on authorizing and expanding TARP. This role of the subcommittees fostered close interaction between committee members, banking regulators, and the Treasury. Duchin and Sosyura (2012) provide examples of this interaction. Members of these subcommittees have been shown to arrange meetings between banks and the Treasury, write letters to banking regulators, and even write provisions into EESA aimed at helping particular firms.

We define our instrument as an indicator, *Finance committee representation*, which takes on the value of one if a firm is headquartered in a district of a House member who served on either of the two key subcommittees in October 2008 and zero otherwise. This representation is dispersed across 30 states.[17] In our sample, 19.1% of CPP applicants have this political connection. Appendix C provides details on the instrumental variable.

Column 1 in Appendix Table C.1 shows the results of a first stage OLS regression explaining CPP approvals using *Finance committee representation*, Camels proxies, bank size, age, foreclosures, funding mix, and exposure to regional economic shocks. In the first stage regression, the instrument has a positive and significant effect on CPP approvals. In particular, the F-test in the first stage model is highly significant (F-statistic = 14.56, p-value < 0.001), confirming the strength of the instrument. Also, Shea's (1997) partial R-squared from the first stage regression exceeds the suggested hurdle rate of 10%, with a value of 14.4%. These statistics indicate that the instrument is relevant in explaining the variation of our model's potentially endogenous regressors.

Next, we consider whether the proposed instrument likely satisfies the exclusion restriction. We begin by providing a brief discussion of the appointment of House members to committees. The first important factor in committee assignments is the fraction of House seats won by each party in the most recent elections, which affects the ratio of seats allocated to the party on each congressional committee. For example, in the 110th Congress (2007–08), the Subcommittee on Capital Markets consisted of 26 Democrats and 23 Republicans, but in the 112th Congress (2009–10), it included 30 Democrats and 20 Republicans. The second factor in committee assignments is the pool of elected House members and their committee preferences. In particular, each House member can serve on no more

[16] For example, a Tier-1 risk-based capital ratio is computed by dividing a bank's capital by risk-weighted assets. According to regulatory requirements, both low-yield and high-yield mortgages are assigned the same risk weight of 0.5.

[17] States that were represented on both subcommittees in 2008 include: CA, CO, DE, FL, GA, IL, KS, KY, MN, NC, NH, NJ, NY, OH, PA, SC, TN, TX, and WV. This list excludes ex-officio positions.

than two standing committees and four subcommittees of those committees. There are also additional constraints on committees imposed by each party.[18] Overall, committee members are determined separately by each party in a process that considers the number of seats negotiated by the party, the constraints on committee memberships, and individual members' preferences.

Since the distribution of House seats and the pool of House members are determined in nationwide elections, these factors are likely outside of the control of a given firm. Further, since committee assignments are reevaluated every two years, there is turnover in committee representation. For example, among the districts represented on finance subcommittees in 2008 (the basis of our instrument), nearly one-half of districts (47.0%) experienced turnover (were represented in some years but not others) during our five-year sample. As another summary measure of turnover, two-thirds of districts (66.7%) represented in 2008 were no longer represented on either of the key subcommittees by 2013.

These factors, combined with a relatively sudden adoption of the bailout program, make it reasonable to conjecture that a firm's geography-based committee representation is not directly related to a firm's risk taking and credit origination, except through the effect of *Finance committee representation* on CPP approvals. Appendix C shows several falsification tests that support this conjecture. First, we compare the pre-bailout characteristics of firms that were represented on the 2008 finance subcommittees with those of firms that were not represented (columns 2–4). Across various firm fundamentals, including the Camels proxies, we find that the two groups were statistically indistinguishable before the bailout (3Q 2008). Column 5 confirms these univariate conclusions in a multivariate setting. Second, in columns 6–8, we compare risk taking by connected and unconnected firms *before* the bailout, relying on the identifying assumption that TARP was unexpected by the average firm. We find no significant differences in risk exposure between firms with and without committee representation *before CPP* across both accounting-based measures of risk (*z*-score, which measures distance to default) and market-based measures of risk (stock volatility and stock beta). These tests demonstrate that the instrumental variable was not directly related to risk taking, absent a bailout (before 3Q 2008).

In columns 7–9 of Table 2, we reestimate our main results in IV regressions. In particular, we replace the binary indicator *Approved bank* with the predicted likelihood that a firm is approved for CPP based on the first stage regression in Appendix C. We obtain the same conclusions as in non-instrumented tests. Across all IV regressions in Table 2, the coefficients on the triple interaction term are positive and statistically significant (p-values $=0.008$–0.024), confirming the effect of CPP approvals on riskier lending.

Overall, we obtain similar results in three empirical models: (1) baseline difference-in-difference tests, (2) matched samples, and (3) IV regressions. We find that CPP approvals were associated with a shift in banks' mortgage origination toward riskier, higher-yield loans, but had little effect on the total volume of new credit.

5. Additional evidence and possible explanations

In this section, we provide cross-sectional evidence on bank risk taking, examine several explanations for our results, and discuss robustness tests. Throughout the rest of the paper, we estimate our tests using the three methods discussed above: baseline difference-in-difference model, matched samples, and IV regressions. Our conclusions are similar across these tests. For brevity, we report baseline difference-in-difference results in the paper and offer evidence from the two other methods in the Internet Appendix.

5.1. *Cross-sectional evidence*

In Table 3, we reestimate the baseline regression of loan approvals in subsamples of banks partitioned on several characteristics: size, capitalization, organizational form, exposure to the crisis, and compliance with CPP dividend schedule. In Internet Appendix Table 1, we repeat the analyses in matched samples and IV regressions.

First, we examine how our results vary with bank size. Prior research suggests that there are significant differences in the credit policies of large and small banks. For example, Berger, Miller, Petersen, Rajan, and Stein (2005) show that large banks have different balance sheet compositions, borrower clienteles, and lending practices than small banks. Yet it is less clear how these differences affect risk. On the one hand, bank size may be positively related to risk taking because large banks can diversify their assets and absorb more risk (Saunders, Strock, and Travlos, 1990). Also, to the extent that bank size captures market power in lending, this power can also lead to riskier loan portfolios (Boyd and De Nicolo, 2005; Berger, Klapper, and Turk-Ariss, 2009). On the other hand, market power increases franchise value, which deters risk taking (Keeley, 1990; Demsetz, Saidenberg, and Strahan, 1996). While the general relation between bank size and risk taking is debated, we know even less about the differences in risk taking between large and small banks in response to federal aid.

In columns 1 and 2 of Table 3, we split our sample at the median value of book assets ($1.45 billion) and reestimate our main difference-in-difference model of loan approvals. First, our main finding of higher risk taking by approved banks (relative to denied banks) after CPP holds for both larger and smaller banks, as shown by the positive and significant coefficient on the triple interaction term *After CPP* × *Approved bank* × *Loan to income*. Second, the increase in risk taking after CPP by approved banks relative to denied banks is much stronger, both statistically and economically, at larger banks. In Internet Appendix Table 2, we use a higher threshold of bank size in the split sample and find that the post-CPP increase in risk taking at large banks is even more pronounced when we impose a more restrictive definition of large banks.

[18] For example, the Democratic Party, but not the Republican Party, considers the House Financial Services Committee to be an exclusive committee, and the Democratic members of that committee cannot serve on other committees.

Table 3
Cross-sectional evidence.

This table reports regression estimates from a linear probability model explaining the relation between a bank's approval for CPP funds and a bank's mortgage origination rates across borrowers of different risk. The dependent variable is an indicator that equals one if a loan was approved and zero if it was denied. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. *Approved bank* is an indicator that equals one if a bank applied for CPP funds and was approved, and zero if it applied but was not approved. The sample excludes the firms subject to stress tests. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. *Loan to income* is the loan amount requested in a mortgage application divided by the applicant's annual income. All variables are defined in Appendix A. The individual loan application data come from the Home Mortgage Disclosure Act (HMDA) Loan Application Registry and cover the period 2006–2010. All regressions include bank-level controls, housing market controls, borrower demographic controls (gender, race, and ethnicity), year fixed effects, bank fixed effects, and regional market fixed effects, which are not shown to conserve space. *Bank-level controls* include the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. *Housing market controls* include home vacancy rates, per capita income, and unemployment, which are measured at the county level. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Sort criterion	Size		Equity capital ratio		Exposure to economic shocks		Organizational structure		Regulatory compliance	
Subsample	Small	Large	Low	High	Weak	Strong	Standalone bank	Bank holding company	Missed CPP dividends	Paid all CPP dividends
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Loan to income	−0.025***	−0.028***	−0.033***	−0.028***	−0.023***	−0.026***	−0.032***	−0.033***	−0.024***	−0.032***
	[0.005]	[0.003]	[<0.001]	[0.002]	[<0.001]	[0.001]	[0.002]	[0.002]	[0.001]	[0.004]
After CPP × Approved bank	−0.038	−0.030	−0.023	−0.006	−0.029	−0.020	−0.052	−0.049	−0.031	−0.054
	[0.323]	[0.482]	[0.303]	[0.527]	[0.545]	[0.222]	[0.435]	[0.334]	[0.440]	[0.684]
After CPP × Loan to income	−0.055	−0.012	−0.090	−0.052	−0.029	−0.086	−0.082	−0.053	−0.054	−0.040
	[0.437]	[0.233]	[0.313]	[0.475]	[0.355]	[0.300]	[0.129]	[0.426]	[0.260]	[0.286]
Approved bank × Loan to income	−0.023	−0.007	−0.012	−0.014	−0.014	−0.039	−0.026	−0.024	−0.026	0.008
	[0.430]	[0.562]	[0.354]	[0.642]	[0.395]	[0.249]	[0.405]	[0.537]	[0.546]	[0.800]
After CPP × Approved bank × Loan to income	0.026**	0.076***	0.079***	0.042**	0.033***	0.078***	0.052*	0.094***	0.091***	0.056***
	[0.031]	[0.010]	[0.007]	[0.033]	[0.008]	[0.004]	[0.095]	[0.007]	[0.008]	[0.001]
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Housing market controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Borrower demographic controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Regional market fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	192,315	493,791	337,564	348,542	341,912	344,194	181,475	504,631	211,935	531,352
Adjusted R^2	0.237	0.300	0.339	0.245	0.250	0.307	0.308	0.322	0.296	0.250

Our findings on bank risk taking are consistent with Black and Hazelwood (2013). Using survey data, the authors study changes in the internal risk ratings of originated loans at 29 TARP banks and 28 non-TARP banks. They find that all but the smallest TARP banks increased their risk after CPP infusions, and this increase in risk was the strongest at larger banks. The similarity of our evidence suggests two inferences. First, our measures of loan risk result in similar conclusions to those based on the banks' own risk ratings. Second, the increase in risk at the majority of CPP banks was likely a conscientious decision since it was noted in their internal risk assessments.

Next, we study how the effect of CPP approval varies with bank capitalization. On the one hand, higher capitalization may decrease risk taking by reducing asset substitution (Morrison and White, 2005) and improving monitoring incentives (Holmstrom and Tirole, 1997; Mehran and Thakor, 2011). On the other hand, higher capitalization may push banks to shift capital into riskier portfolios unless this risk shifting is constrained by the regulators (Koehn and Santomero, 1980). Further, if higher capitalization increases banks' likelihood of survival, banks may increase risk because they estimate a lower probability of regulatory closure (Calem and Robb, 1999).

In columns 3 and 4 of Table 3, we split our sample at the median equity capital ratio (10.1%) and test whether the effect of CPP approval differs between high- and low-capitalization banks. We find that our main conclusions hold in both subsamples. In particular, after CPP, both subsamples of approved banks increased origination rates on riskier loans (relative to denied banks), as indicated by the positive and significant coefficients on the triple interaction term. However, the increase in risk was significantly stronger for low-capitalization banks. For example, the point estimate on the triple interaction term is almost twice as large for low-capitalization banks as for high-capitalization banks (0.079 and 0.042, respectively).

Consistent with our main results, we find no significant increase in lending in both subsamples partitioned on capitalization. These findings are related to Li (2013), who studies the effect of TARP on credit supply and finds a modest increase (6.4%) in loan supply among poorly capitalized TARP banks. The difference in our results in this subsample could be attributed to methodological distinctions. First, since loan demand is unobservable in Li's study, it may account for some of the increase in the book value of bank loan portfolios. Second, while we focus on public banks to infer their application status from disclosure, Li does not require this information and studies both public and private banks. Under this interpretation, it is possible that the stimulatory effect of CPP on credit supply was confined to small private banks with low capitalization.

Next, we examine whether our results differ between banks with high and low exposure to regional economic shocks, as measured by an index of macroeconomic indicators across the states where a bank maintains active branches. Columns 5 and 6 of Table 3 show the results in the subsamples of banks with below- and above-median value of their state macro index (median index value = 0.303%). The results indicate that the post-CPP increase in risk taking was more pronounced at banks with a high exposure to regional economic shocks, with the point estimate on the triple interaction term about twice as large for this subsample (and statistically different). In Internet Appendix Table 2, we obtain qualitatively similar results when we split the sample at the zero value of the index, comparing between banks exposed to states with economic contraction versus growth.

Our next cross-sectional tests study how the effect of CPP varies with a firm's organizational structure. In our sample, 79.6% of approved firms are holding companies, and 20.4% are standalone banks. On the one hand, holding companies operate in multiple geographic markets, and theory predicts that better-diversified firms have more capacity to take on risk (Lewellen, 1971). On the other hand, changes in government policies in other settings have been shown to have stronger effects on standalone banks (Campello, 2002).

In columns 7 and 8, we show the results of our difference-in-difference tests of loan approvals for the subsamples of standalone banks and bank holding companies. While the increase in risk taking is observed in both subsamples, the economic effect is stronger for bank holding companies. This result is consistent with the view that revenue diversification allows a firm to vary its risk exposure more significantly over time. In Internet Appendix Table 2, we present evidence that supports this interpretation. We construct two measures that capture a firm's diversification across geographic markets: the Herfindahl index of the dispersion of a firm's deposits across U.S. states and a binary indicator for a firm's international diversification (operation of bank branches outside the U.S.). We find that the post-CPP increase in risk taking at approved firms was stronger for better-diversified firms.

In our final cross-sectional tests, we examine how the effect of CPP varies with a firm's compliance with program conditions. This analysis is motivated by previous research, which suggests that regulatory forbearance in government intervention may encourage risk taking (Acharya and Yorulmazer, 2007; Calomiris and Wallison, 2009). Because CPP was adopted hastily in a crisis, the program's regulatory mechanisms were fairly loose. First, CPP recipients were not required to trace the deployment of federal capital or disclose its use. Second, there was a lack of the enforcement tools necessary to guarantee the timely payment of CPP dividends or recover taxpayer investment in case of a firm's insolvency. For example, by the end of 2010, nearly 21% of publicly traded CPP recipients (57 firms) skipped $203 million in CPP dividend payments, with the median delinquent firm missing three of the eight required payments. If some firms are allowed to skip the required dividends, they may infer regulatory forbearance and continued government back-up.

In the last two columns of Table 3, using data from the Treasury's Office of Financial Stability, we estimate our baseline regressions in split samples, where CPP recipients are distinguished as firms that missed CPP dividends (column 9) and those that complied with all dividend payments (column 10). We find an economically larger increase in risk taking in the post-CPP period among the dividend-skipping CPP recipients, as shown by the magnitudes of the triple interaction term of interest (0.091 vs. 0.056).

In summary, our conclusions about the effect of CPP on risk taking and credit origination hold in various subsamples. In economic terms, the increase in risk taking in response to CPP approval was stronger at larger and better-diversified firms with greater capacities to absorb risk. The increase in risk was also stronger at weakly capitalized banks and banks exposed to harder-hit states, which were arguably closer to financial distress. Finally, the increase in risk was also stronger at firms that received signals of regulatory forbearance.

5.2. Possible explanations

In this section, we evaluate three non-mutually exclusive explanations for the increase in risk taking at approved banks relative to denied banks: (1) government intervention, (2) risk arbitrage, and (3) moral hazard.

The first hypothesis—*government intervention*—posits that the increase in risk taking at approved banks is a consequence of government intervention in bank policies aimed at increasing lending to riskier borrowers. As our first test of this hypothesis, we collect data on banks that applied for CPP, were approved, but did not receive CPP funds for various institutional reasons. To identify these banks, we search banks' press releases, proxy statements, financial reports (8K and 10Q), records of shareholder meetings, and news announcements in Factiva for any mentioning of CPP. We identify 51 such firms in our sample. We then read these press releases and news articles to understand the reasons for a bank's decision to decline CPP funds. Examples of the reasons stated by the declining banks include restrictions on the issuance of preferred stock in the firm's articles of incorporation, sufficient capitalization levels, and restrictions associated with CPP participation. Internet Appendix A.3 provides sample disclosures of banks that elaborate on these reasons.

While all approved banks received the signal of government support, only the banks that received federal capital were subject to possible government intervention. If this intervention caused riskier lending, we should observe an increase in risk only for approved banks that received the funds. In contrast, column 1 of Table 4, Panel A shows that the increase in risk was similar across all approved banks, regardless of whether they received the funds and were subject to CPP regulations. This can be seen from the coefficient on the interaction term *After CPP* × *Approved bank* × *Loan to income* (in this column, *Approved bank* is defined as an indicator that equals one for approved banks that accepted CPP funds and zero for approved banks that declined the funds), which shows that the change in risk was indistinguishable between the two groups of approved banks. Internet Appendix Tables 2 and 3 provide corroborating evidence. In Internet Appendix Table 2, we find that the post-CPP increase in risk at approved banks relative to denied banks was similar in magnitude for banks that received large versus small CPP amounts. In Internet Appendix Table 3, we replicate the results obtained in column 1 of Table 4, Panel A, using matched samples, where each approved bank that did not receive CPP funds is matched to the most fundamentally similar approved bank that received the funds. The matching process is discussed in Appendix B, and summary statistics for the matched sample appear in columns 5–8 of Appendix Table B.1.

As another test of the government intervention hypothesis, we compare CPP banks that repaid their funds and exited the program with CPP banks that remained under government supervision. One caveat is that after a bank repays its CPP capital, it may take time to observe changes in its risk taking behavior because its existing portfolio of outstanding loans is likely to generate temporal stickiness in the overall measures of bank risk. To mitigate this concern, as in our earlier tests, we focus on the origination of new loans, which allows us to separate forward-looking changes in a bank's risk tolerance from the temporal persistence in the risk of its outstanding loans. We obtain data on CPP repayments from the Treasury's Office of Financial Stability and present the results of this analysis in column 2 of Table 4, Panel A. In this column, *Approved bank* is an indicator that equals one for approved banks that repaid CPP funds and zero for approved banks that did not repay.

We find that the increase in risk was similar for CPP banks that repaid and did not repay their funds, as shown by the insignificant coefficient on the triple interaction term in the comparison of these groups. In Internet Appendix Table 3, we find similar results in matched samples, where each bank that repaid CPP funds is matched to the most fundamentally similar bank that did not repay the funds. The matching process is described in Appendix B, and summary statistics for this matched sample appear in columns 9–12 of Appendix Table B.1 Overall, both tests of the government intervention hypothesis yield similar conclusions. To the extent that government intervention affected banks' lending, it appears unlikely to have been the main driver of risk taking.

We also consider the possibility that the effect of government intervention may operate indirectly via regulatory constraints imposed on CPP participants, the most significant of which concerned executive compensation.[19] So far, two pieces of evidence suggest that these regulations were unlikely to cause higher risk taking. First, CPP regulations of executive compensation sought to *reduce* rather than increase risk, carrying an explicit mandate to prevent excessive risk taking. Second, the finding that the post-CPP increase in risk was similar for approved banks that accepted the funds (and were subject to CPP regulations) and approved banks that declined the funds (and

[19] Restrictions on the executive compensation of TARP recipients were initially imposed by Section 111 of the Economic Emergency Stabilization Act (EESA) in October 2008 and subsequently expanded by Section 7001 of the American Recovery and Reinvestment Act (ARRA) in February 2009. These restrictions, which apply to a recipient firm until it repays its TARP investment, imposed limits on the level and tax deductibility of executive pay, introduced claw-back provisions, and prohibited retention payments and golden parachutes. According to Section 111(b) of EESA, the objective of these restrictions was "to exclude incentives for senior executive officers of a financial institution to take unnecessary and excessive risks." The details of these regulations, as well as the full text of EESA and ARRA, are available at: http://www.treasury.gov/initiatives/financial-stability/TARP-Programs/executive-comp/Pages/default.aspx.

Table 4
Alternative hypotheses.

This table presents additional evidence on risk taking and risk-adjusted performance for various subsets of CPP applicants. Panel A examines the government intervention hypothesis by comparing several categories of approved banks. Column 1 of Panel A compares mortgage origination and risk taking at CPP-approved banks that accepted the funds (the indicator variable *Approved bank* equals one) and CPP-approved banks that declined the funds (the indicator *Approved bank* equals zero). Column 2 of Panel A compares credit origination and risk taking at CPP-approved banks that repaid the funds (the indicator variable *Approved bank* equals one) and CPP-approved banks that did not repay the funds (the indicator variable *Approved bank* equals zero). In Panel A, the unit of observation is one loan application, and the dependent variable is an indicator that equals one if a loan was approved and zero if it was denied. Panel B examines the risk arbitrage hypothesis by comparing measures of after-CPP performance of approved and denied CPP applicants. The unit of observation is a bank-quarter. In column 1, the dependent variable is net loan charge-offs, expressed as a fraction of total loans. In columns 2–5, the dependent variables are measures of risk-adjusted performance: the Sharpe ratio, the information ratio, and one- and three-factor alphas, respectively. All dependent variables in Panel B are expressed in percentage points (multiplied by 100) to facilitate the interpretation of regression coefficients. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. All variables are defined in Appendix A. In Panel A, the regressions include bank-level controls, housing market controls, borrower demographic controls (gender, race, and ethnicity), year fixed effects, bank fixed effects, and regional market fixed effects. In Panel B, the regressions include bank level controls, year fixed effects, and bank fixed effects. *Bank-level controls* include the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. *Housing market controls* include the home vacancy rate, per capita income, and unemployment, which are measured at the county level. Reported p-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Panel A: Government intervention

Treatment	Approved banks that accepted vs. declined CPP funds	Approved banks that repaid vs. did not repay CPP funds
Column	(1)	(2)
Loan to income	−0.027***	−0.034***
	[0.001]	[0.002]
After CPP × Approved bank	−0.048	−0.011
	[0.772]	[0.784]
After CPP × Loan to income	−0.025	−0.028
	[0.235]	[0.158]
Approved bank × Loan to income	−0.037	−0.005
	[0.367]	[0.576]
After CPP × Approved bank × Loan to income	0.021	0.015
	[0.301]	[0.371]
Bank-level controls	Yes	Yes
Housing market controls	Yes	Yes
Borrower demographic controls	Yes	Yes
Year fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Regional market fixed effects	Yes	Yes
Observations	572,617	503,903
Adjusted R^2	0.208	0.236

Panel B: Risk arbitrage

Dependent variable	Loan charge-offs	Sharpe ratio	Information ratio	One-factor alpha	Three-factor alpha
Column	(1)	(2)	(3)	(4)	(5)
After CPP × Approved bank	0.069***	0.037	−0.010	−0.162*	−0.174**
	[0.003]	[0.671]	[0.920]	[0.054]	[0.047]
Bank-level controls	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes
Observations	7,946	7,946	7,946	7,946	7,946
Adjusted R^2	0.601	0.507	0.421	0.193	0.223

were exempt from CPP regulations) suggests that the possible differences in executive compensation between these groups are unlikely to have been the main driver of higher risk taking.

In Internet Appendix Tables 4 and 5, we present formal evidence on the effect of CPP approvals on firm governance. We reach two main conclusions. First, we do not find an asymmetric pattern in the dynamics of executive compensation, management turnover, or board turnover from before to after CPP, whether we compare approved and denied CPP applicants or approved applicants that accepted the funds and approved applicants that declined the funds. Second, the risk-increasing effect of CPP approvals holds with similar economic magnitudes after controlling for changes in banks' compensation, management turnover, and board turnover. Overall, it appears unlikely that government intervention alone can explain the observed changes in bank risk taking.

The second hypothesis—*risk arbitrage*—conjectures that some risky assets, such as mortgages and investment securities, were underpriced during the crisis, providing excess profit opportunities with low risk. In this case, CPP funds may have allowed banks to exploit these opportunities without an ex post increase in risk.

Our first evidence on this hypothesis comes from post-CPP changes in loan charge-offs at approved banks relative to their denied peers. One limitation is that we have a relatively short post-CPP horizon, though previous work shows that a large fraction of mortgage defaults is concentrated during the first two years of a loan's life, the time horizon of choice in recent work on loan performance (Demyanyk and Van Hemert, 2011; Rajan, Seru, and Vig, 2014). Another caveat is that our data do not allow us to trace the performance of each loan. Consequently, the observed loan charge-offs reflect losses on loans approved both before and after CPP. However, to the extent that denied CPP applicants had a lower quality of loan portfolios (Bayazitova and Shivdasani, 2012), this effect would bias our tests towards finding an increase in loan charge-offs at *denied* banks relative to approved banks. In contrast, our findings indicate the opposite pattern. In column 1 of Table 4, Panel B, using a difference-in-difference regression framework, where the dependent variable is the ratio of loan charge-offs to total loans, we find a significant post-CPP increase in loan charge-offs at approved banks relative to their denied peers, as indicated by the positive and significant interaction term *After CPP* × *Approved bank*. In economic terms, after CPP, the net charge-offs at approved banks increased by 6.9 bps (8.4%) more than at denied banks.

As a second test of the risk arbitrage hypothesis, we compare risk-adjusted performance of approved and denied banks after CPP based on their Sharpe ratios, information ratios, and one- and three-factor alphas, whose definitions appear in Appendix A. This approach views each bank as a portfolio of assets and evaluates its risk-adjusted performance by relying on the identifying assumption that profits from individual loans and arbitrage trades are eventually revealed in stock prices over our sample period. In Table 4, Panel B, we find no evidence that the increase in risk taking at approved banks was followed by superior risk-adjusted returns relative to denied banks based on any of the proxies. If anything, the evidence suggests a small decrease in alphas (16–17 bps) for approved banks relative to denied banks after CPP. Overall, approved banks' shift toward riskier assets appears to have reflected an increase in risk tolerance rather than capital allocation to arbitrage opportunities.

The third explanation—*moral hazard*—posits that a firm's approval for CPP provides a signal of implicit government protection of that firm in case of distress. Because the government has incentives to prevent the failure of firms it has declared to support, the bailout may encourage risk taking by protected banks.

Several empirical results suggest that moral hazard likely contributed to the observed shift in bank risk. First, the finding that higher risk taking is associated with the certification of government support, rather than with the capital injection itself, is consistent with the effect of a revised probability of government support (Mailath and Mester, 1994; Acharya and Yorulmazer, 2007). Second, the cross-sectional evidence on risk shifting aligns well with the predictions in the models of moral hazard. In particular, the increase in bailed banks' risk taking was stronger at larger banks and banks that were closer to financial distress. Proximity to distress increases the value of the put option from implicit government protection and may contribute to risk taking, since such a firm has less to lose before it reaches the critical capitalization level that triggers government aid. Third, the increase in risk taking was stronger at firms that experienced regulatory forbearance by skipping dividends owed to the Treasury, a result predicted in previous work on the moral hazard effect of forbearance in government aid (Kane, 1990; Acharya and Yorulmazer, 2007). Finally, the evidence hints at a strategic aspect in approved banks' risk taking. In particular, approved banks increased their risk mostly *within* the regulated asset classes (thus reducing the effect on regulatory ratios) and invested in asset classes exposed to common macroeconomic risk.[20] If government protection is more likely in case of a systematic rather than idiosyncratic shock to a firm, this pattern would be consistent with a strategic response to a revised probability of government support.

Overall, the increase in risk taking at government-protected banks was likely associated with a combination of factors, including an increase in available capital, possible government guidelines, and reaching for yield in credit origination. Though it is difficult to assess the relative impact of these incentives, our evidence suggests that moral hazard from a revised probability of government support was likely a contributing factor. Consistent with this interpretation, the following robustness section provides corroborating evidence indicating that CPP approvals were followed by a significant increase in the average loan-to-income ratio and the fraction of subprime loans in originated credit at approved banks relative to unapproved banks. [21]

5.3. Robustness

This section evaluates the robustness of our results along three dimensions. First, we examine alternative time periods and subsamples of CPP firms. Second, we study the effect of changes in CPP conditions imposed by the American Recovery and Reinvestment Act. Finally, we provide direct evidence on loan demand and introduce an alternative measure of loan risk. We obtain similar conclusions in these tests. These results are discussed in Internet Appendix B and shown in Internet Appendix Tables 6–9.

[20] We refer to banks' investments in higher-yield mortgages, an asset class whose performance has been shown to be highly sensitive to common macroeconomic risk factors (Hayre, Saraf, Young, and Chen, 2008; Mayer, Pence, and Sherlund, 2009).

[21] Following the HMDA definition, we define subprime loans (dummy *High yield*) as loans for which the interest rate exceeds the yield on the Treasury of comparable maturity by at least 300 (500) bps for first-lien (second-lien) mortgages.

6. Extensions

In this section, we extend our analysis by studying the effect of CPP on two other channels of bank operations: (1) corporate credit and (2) portfolio investments. While we believe that the richness of data in the mortgage market provides the cleanest empirical setting, we offer these additional tests as complementary evidence.

6.1. Corporate lending

We study the effect of CPP on corporate credit by investigating the origination of large syndicated loans by approved and denied banks before and after CPP. In this analysis, three caveats are in order. First, in contrast to the mortgage market, in the corporate credit market we do not observe loan applications. Therefore, to control for credit demand at the level of each borrowing firm, we focus on within-borrower variation in credit supplied by approved and denied banks. Second, we make an assumption about the role of each bank in the syndicate by using the following criterion to distinguish lead managers from regular participants. Following Ivashina (2009), we define the lead manager as the bank that serves as an administrative agent for the loan facility. For observations in which the administrative agent is not indicated, the lead manager is defined as the bank whose syndicate status is indicated by one of the following roles: lead arranger, lead bank, lead manager, book runner, agent, or arranger. After identifying the lead manager, we consider all other banks to be syndicate participants. For observations in which the share of the lead manager and each participant is stated, we use the actual shares of credit provided. For observations in which the exact shares are missing, we use the median share of the lead manager in our sample as a proxy for the lead manager's share and assign all regular participants an equal share of the remaining facility. Third, our tests focus only on the main treatment and control groups (approved and denied CPP applicants, respectively) because more refined comparisons, such as those between approved firms that accepted the funds and approved firms that declined the funds, are precluded by sparse coverage of smaller banks on DealScan.

In Panel A of Table 5, we examine corporate credit issuances by approved and denied CPP applicants. The unit of observation is a corporate loan facility-lender pair, and the dependent variable is the fraction of credit supplied in a given loan facility by approved banks relative to all CPP applicants with a known application status, excluding banks subject to stress tests. As in our tests of retail credit, the main independent variables include CPP indicators (*After CPP* and *Approved bank*) and their interaction terms. The key variable of interest is the triple interaction term *After CPP* $\times$ *Approved bank* $\times$ *Borrower risk*, interpreted as in previous tests. The measures of borrower risk include *Cash flow volatility*, *Intangible assets*, and *Interest coverage*, which have been shown to be correlated with default risk (Blume, Lim, and MacKinlay, 1998; Tang and Yan, 2010; Douglas, Huang, and Vetzal, 2012). All tests include time-varying bank-level controls, bank fixed effects, and year fixed effects, as well as controls for the loan facility type (term loans vs. credit lines).

Across all columns, each of which corresponds to one of the three measures of risk, we find a positive and statistically significant coefficient on the triple interaction term of interest. These findings indicate that the fraction of CPP-approved banks in riskier loans has increased after CPP (and, correspondingly, the fraction of denied banks in these riskier loans has declined). As before, we do not find a significant difference in the total volume of credit originated by approved and denied firms, consistent with evidence from the mortgage market.

6.2. Loan yields and loan commitments

As an additional test of the effect of CPP on the risk of originated credit in the retail and corporate markets, we provide evidence on the average yield of loan portfolios at approved and denied banks. To the extent that approved banks shifted their credit origination toward higher-risk loans after CPP, this effect should result in an increase in the average loan yield at approved banks relative to their denied peers. We test this prediction by estimating a difference-in-difference regression, where the dependent variable is the average loan yield, as proxied by the ratio of interest income on loans and leases to the end-of-period book value of loans and leases. Each observation is the average loan yield at a given bank in a given quarter. The independent variables include the interaction term *After CPP* $\times$ *Approved bank*, bank-level controls, bank fixed effects, and year fixed effects.

The evidence on loan yields at approved and denied banks is presented in column 1 of Table 5, Panel B. The main variable of interest is the interaction term *After CPP* $\times$ *Approved bank*, which captures the marginal change in the average loan yield between approved and denied banks from before to after CPP. The coefficient on this term is positive, significant, and economically large. Based on the point estimate in column 1, CPP approvals were followed by a 90 bps increase in the average loan yield at approved banks relative to denied banks. These results corroborate the micro evidence in the retail and corporate credit markets and provide an aggregate, market-based measure of an increase in credit risk at approved banks. As mentioned earlier, these conclusions are verified in matched samples and IV regressions in Internet Appendix Table 10.

We conclude our analysis of the effect of CPP on credit origination with a study of loan commitments, the main source of off-balance sheet financing, which plays a significant role in liquidity creation (Kashyap, Rajan, and Stein, 2002; Berger and Bouwman, 2009). We use the same difference-in-difference regression framework as in the analysis of loan yields, except the dependent variable now is the amount of a bank's end-of-period loan commitments. The results of estimation are shown in column 2 of Table 5, Panel B. The coefficient on the interaction term *After CPP* $\times$ *Approved bank* is insignificant and economically small, indicating no significant change

Table 5
Corporate loans, loan commitments, and loan yields.

This table provides evidence on the relation between a bank's approval for CPP and its corporate lending, loan commitments, and loan yields. Panel A reports regression estimates from loan-level data explaining the relation between a bank's approval for CPP and corporate lending. In Panel A, the dependent variable is the fraction of credit supplied in a given loan facility by each bank. The unit of observation is the new credit originated by a given bank to a given borrower in a given loan facility. Data on corporate loans are obtained from DealScan and cover the period 2006–2010. In Panel A, we use three measures of borrowers' risk. *Cash flow volatility* is the volatility of earnings, net of taxes and interest and scaled by total assets, over the previous three years. *Intangible assets* is the ratio of intangible assets to total book assets. *Interest coverage* is the inverse of the interest coverage ratio, calculated as interest expense divided by earnings before interest and taxes. Panel B reports regression estimates from panel regressions explaining bank loan commitments and yields on loan portfolios. In Panel B, the unit of observation is a bank-quarter. In column 1, the dependent variable is *Yield on loan portfolios*, measured as interest and fee income from loans and leases divided by total loans and leases. In column 2, the dependent variable is loan commitments scaled by total assets. The quarterly data are from the Call Reports for 2006–2010. *Loan facility controls* include loan type (e.g., term loans, credit lines) and loan maturity in years. *Bank-level controls* include the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. All variables are defined in Appendix A. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the borrower level in Panel A and at the bank level in Panel B. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Panel A: Corporate loans

Risk measure Column	Cash flow volatility (1)	Intangible assets (2)	Interest coverage (3)
Borrower risk	−0.239	−0.245	−0.238
	[0.352]	[0.352]	[0.352]
After CPP × Approved bank	−0.080	−0.146	−0.100
	[0.507]	[0.330]	[0.498]
After CPP × Borrower risk	−0.066**	−0.076	−0.049
	[0.022]	[0.253]	[0.181]
Approved bank × Borrower risk	0.023	0.038	0.031*
	[0.136]	[0.298]	[0.061]
After CPP × Approved bank × Borrower risk	0.057**	0.041**	0.014*
	[0.026]	[0.039]	[0.066]
Loan facility controls	Yes	Yes	Yes
Bank-level controls	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Observations	5,957	5,957	5,957
Adjusted R^2	0.638	0.648	0.632

Panel B: Loan commitments and loan yields

Dependent variable Column	Yield on loan portfolios (1)	Loan commitments (2)
After CPP × Approved bank	0.009*	0.013
	[0.080]	[0.386]
Bank-level controls	Yes	Yes
Year fixed effects	Yes	Yes
Bank fixed effects	Yes	Yes
Observations	7,946	7,946
Adjusted R^2	0.124	0.802

in loan commitments between approved and denied banks from before to after CPP.

6.3. Security investments

The evidence so far suggests that banks increased the risk of their loan portfolios after being approved for CPP funds. If this strategy reflects a general increase in risk taking by approved banks, we may observe a similar tilt toward riskier assets in banks' portfolio investments. The advantage of this analysis is that the risk of financial assets is often more transparent and can be estimated based on market information.

In our analysis of portfolio investments, we study whether banks increased their allocations to riskier securities relative to other assets after being approved for CPP. We examine total investment in securities, the average interest yield, and the breakdown of securities into safer and riskier classes. To provide a simple and transparent classification, we define ‘lower-risk securities’ as Treasuries and securities issued by state and political subdivisions, and ‘riskier securities’ as equity products, mortgage-backed securities (excluding agency obligations), and other domestic and foreign debt securities. For completeness, we scale security investments both by total assets and total security holdings.

Table 6 presents difference-in-difference analysis of portfolio investments between approved and denied

Table 6
Banks' investment securities.

This table reports regressions explaining banks' portfolio investments in various security classes scaled by total assets or by total securities. Quarterly data on bank security investments are obtained from Call Reports and cover the period 2006–2010. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. *Approved bank* is an indicator that equals one if a bank applied for CPP funds and was approved, and zero if it applied but was not approved. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. *Riskier securities* comprise mortgage-backed securities (excluding government-sponsored agency obligations), other domestic and foreign debt securities, and investments in mutual funds and equity products. *Lower-risk securities* include U. S. Treasury securities and securities issued by states and political subdivisions. *Long-term debt securities* comprise securities with the remaining maturity greater than five years. The ratios of interest income from securities to assets and securities are expressed in percentage points (multiplied by 100) to facilitate the interpretation of regression coefficients. All regressions include year fixed effects, bank fixed effects, and bank-level controls. *Bank-level controls* comprise the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. All variables are defined in Appendix A. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Dependent variable	Total securities/ assets	Riskier securities/ assets	Riskier securities/ securities	Lower-risk securities/ assets	Lower-risk securities/ securities	Int. income from securities/ assets	Int. income from securities/ securities	Long-term debt securities/ assets	Long-term debt securities/ securities
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
After CPP × Approved bank	0.097**	0.043*	0.160**	−0.008***	−0.044***	0.076**	0.739***	−0.002	0.079**
	[0.045]	[0.086]	[0.011]	[0.009]	[0.002]	[0.017]	[0.008]	[0.138]	[0.030]
Bank-level controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	7,946	7,946	7,946	7,946	7,946	7,946	7,946	7,946	7,946
Adjusted R^2	0.885	0.865	0.820	0.888	0.817	0.593	0.560	0.853	0.793

Table 7
Capitalization.

This table provides evidence on the relation between CPP approvals and bank capital ratios. Panel A provides univariate evidence on the dynamics of bank capital ratios around CPP investments for various subsets of CPP-eligible firms: non-applicants, rejected firms, approved firms that received capital funding, and approved firms that declined capital funding. For each subset of firms, the table reports the average of three capitalization ratios at the start of our sample period (Q1 2006), before CPP (Q3 2008), after most CPP investments (Q1 2009), and at the end of our sample period (Q4 2010). The three capitalization ratios include: (1) Tier 1 risk-based capital ratio, (2) total risk-based capital ratio, and (3) equity capital ratio. Panel B reports difference-in-difference regressions explaining the three bank capitalization ratios. Quarterly financial data are obtained from Call Reports and cover the period 2006–2010. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. In Panel B, *Approved bank* is an indicator that equals one if the applicant bank was approved for CPP and zero if it was denied. The capitalization ratios are expressed in percentage points (multiplied by 100) to facilitate the interpretation of regression coefficients. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. *Bank-level controls* include the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. All regressions include bank and year fixed effects. All variables are defined in Appendix A. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Panel A: *Univariate evidence*

Capitalization measure	Tier-1 risk-based capital ratio					Total risk-based capital ratio					Equity capital ratio				
Period	Q1 2006	Q3 2008	Q1 2009	Q4 2010	Diff (3)–(2)	Q1 2006	Q3 2008	Q1 2009	Q4 2010	Diff (8)–(7)	Q1 2006	Q3 2008	Q1 2009	Q4 2010	Diff (13)–(12)
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)	(13)	(14)	(15)
Non-applicants	15.531	14.847	14.239	15.831	−0.609	16.504	15.857	15.305	16.965	−0.552	11.150	11.279	10.743	11.342	−0.536
					[0.263]					[0.295]					[0.141]
Rejected firms	12.457	11.384	10.946	12.324	−0.439	13.532	12.643	12.266	13.650	−0.377	9.817	9.242	8.478	8.671	−0.765
					[0.330]					[0.377]					[0.719]
Approved firms that received funding	11.423	10.736	11.619	12.450	0.883***	12.662	12.058	13.013	13.888	0.955***	9.803	10.009	10.288	10.047	0.279***
					[0.001]					[0.001]					[0.001]
Approved firms that declined funding	12.519	11.925	11.775	12.965	−0.150	13.556	12.933	12.841	14.134	−0.092	9.434	9.479	9.336	9.905	−0.143
					[0.683]					[0.801]					[0.716]

Panel B: *Regression evidence*

Dependent variable	Tier-1 risk-based capital ratio	Total risk-based capital ratio	Equity capital ratio
Model	(1)	(2)	(3)
After CPP × Approved bank	1.566***	1.494***	1.451***
	[< 0.001]	[< 0.001]	[< 0.001]
Bank-level controls	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes
Observations	7,946	7,946	7,946
Adjusted R^2	0.730	0.721	0.687

Table 8
Overall bank risk.

This table reports regression evidence on the relation between CPP approvals and bank risk. Bank quarterly data are obtained from Call Reports and cover the period 2006–2010. *After CPP* is an indicator that equals one in 2009–2010 and zero in 2006–2008. *Approved bank* is an indicator that equals one if the applicant bank was approved for CPP and zero if it was denied. The variables *After CPP* and *Approved bank* drop out of the regression due to the inclusion of year and bank fixed effects, respectively. *ROA volatility* is calculated as the quarterly standard deviation of ROA over the trailing four quarters. *Z-score* is a measure of a firm's distance to default, computed as the sum of the return on assets (ROA) and the equity capital ratio divided by the standard deviation of ROA. Lower *z*-scores indicate a higher risk of default. *Betas* are calculated based on the market model (with the CRSP value-weighted index as the market proxy), using daily returns over a one-year horizon. *Stock return volatility* is calculated from daily returns over a one-year horizon. All regressions include year fixed effects, bank fixed effects, and bank-level controls. *Bank-level controls* comprise the Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. All variables are defined in Appendix A. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Risk measure Column	ROA volatility (1)	*Z*-score (2)	Beta (3)	Stock return volatility (4)
After CPP × Approved bank	0.006*** [0.007]	−10.390*** [0.002]	0.113** [0.036]	0.018** [0.030]
Bank-level controls	Yes	Yes	Yes	Yes
Year fixed effects	Yes	Yes	Yes	Yes
Bank fixed effects	Yes	Yes	Yes	Yes
Observations	7,946	7,946	7,946	7,946
Adjusted R^2	0.750	0.637	0.642	0.581

banks from before to after CPP, and Internet Appendix Table 11 shows this evidence in matched samples and IV regressions. Column 1 in Table 6 indicates that approved banks increased their allocations to investment securities after CPP. For the average approved bank, the weight of investment securities in bank assets increased by 9.7% after CPP, relative to denied banks.[22] Moreover, approved banks increased their allocations to riskier securities by 4.3% (column 2), while, at the same time, reducing their allocations to lower-risk securities relative to denied banks. Columns 6 and 7 show that after CPP, approved banks tilted their portfolios to higher-yield assets relative to denied banks. In particular, after CPP, the average interest yield on investment portfolios at approved banks increased by 74 bps relative to denied banks. Similar evidence emerges from the analysis of the average maturity of assets, indicating an increase in allocation to long-term securities as a fraction of portfolio investments at approved banks relative to denied banks (column 9). Overall, the analysis of investment portfolios suggests that approved banks, compared to denied banks with similar fundamentals, actively increased their risk after CPP by investing in riskier asset classes and tilting portfolios to higher-yield securities.

7. Bank risk

In this section, we study how changes in bank credit policies and portfolio investments after CPP affected aggregate bank risk. Since, broadly defined, the two primary sources of bank risk are leverage and asset composition, we first examine the effect of CPP on capital ratios and continue with evidence on aggregate risk.

7.1. Leverage and capital ratios

We begin with descriptive evidence on capital ratios around CPP investments for various subsets of CPP-eligible firms: non-applicants, rejected firms, approved firms that received funding, and approved firms that declined funding. For each group, Table 7, Panel A shows the dynamics of three capitalization ratios (defined in Appendix A) around CPP: (1) Tier-1 risk-based capital ratio, (2) total risk-based capital ratio, and (3) equity capital ratio. Across all ratios, approved firms that received funding experienced an increase in capitalization, which ranges from 28 bps (equity capital ratio) to 96 bps (total risk-based capital ratio) and is significant at the 1% level. The point estimates also show that rejected firms, as well as approved firms that declined funding, experienced a decline in capitalization around CPP infusions, but this decline is statistically indistinguishable from zero at conventional levels. As expected, non-applicant firms show no significant change in capitalization around CPP.

We continue with regression evidence on changes in capitalization at approved and denied banks. In Panel B of Table 7, we report the results of difference-in-difference regressions where the dependent variable is one of the bank capital ratios, and the independent variables include the interaction term *After CPP* × *Approved bank*, bank-level controls, and bank and year fixed effects. The results suggest that after CPP, approved banks improved their capitalization ratios relative to denied banks. These results are significant at the 1% level and have sizable economic magnitudes. Based on column 1, after CPP, the Tier-1 risk-based capital ratio at approved banks increased by 157 bps relative to denied banks, consistent with an infusion of government aid. In Internet Appendix Table 12, we reach qualitatively similar conclusions in matched samples and IV specifications and provide additional analysis of capital ratios at banks that

[22] In unreported robustness tests, we study security investments and aggregate lending separately for approved banks that accepted the funds and approved banks that declined the funds. We find that the increase in security investments is driven by the extra CPP capital allocated to banks that accepted the funds. This capital allowed CPP recipients to increase their portfolio investments, while maintaining approximately the same level of aggregate lending.

received small and large CPP investments. As expected, we find that banks that received larger capital infusions experienced a greater increase in capital ratios.

7.2. Overall risk

In our final analysis, we examine the aggregate effect of changes in banks' leverage and asset composition on overall bank risk. First, we focus on the *z*-score, a measure of a bank's distance to insolvency, which aggregates the effects of leverage and asset composition. Defined in Appendix A, this score approximates the inverse of the default probability, with higher *z*-scores reflecting a lower chance of default.[23]

We complement the accounting-based *z*-score with market-based estimates of bank risk: stock volatility and beta, both of which reflect the combined effect of changes in leverage and asset composition. We compute stock return volatility using daily returns over a one-year horizon. To compute beta, we assume the market model and use daily returns over a one-year horizon. Our results are similar if we use market beta from a two-factor model, which is often assumed to describe the return generating process for banks.[24]

In Table 8, we report the results of panel regressions of bank risk, where the dependent variables include the volatility of the return on assets (ROA), *z*-score, market beta, and stock volatility. The evidence across the columns indicates a significant increase in each of the aggregate measures of risk at approved banks. This result suggests that the improvement in capital ratios at approved banks relative to denied banks was more than offset by an increase in the riskiness of the asset mix at approved banks. The net effect was a marked increase in total risk (stock volatility), market risk (beta), and likelihood of default (inverse of the *z*-score) at approved banks relative to denied banks. The overall effect on bank risk is economically large. For example, after the bailout, approved banks show a 20.9% increase in default risk and a 15.3% increase in beta relative to denied banks with similar characteristics. In Internet Appendix Table 13, we repeat the analyses in matched samples and IV regressions and reach similar conclusions. One explanation for the increase in aggregate risk combined with a relative decline in leverage could be a strategic response of banks to regulatory capital requirements, such as a strategy designed to increase the profitability of assets, while improving capitalization levels monitored by the regulators.

In summary, we find that banks approved for CPP shifted their credit origination toward riskier borrowers and tilted their portfolio investments toward riskier securities. This strategy was associated with an increase in systematic risk and the probability of distress.

[23] This relation was first formalized in Roy (1952). For a recent discussion, please see Laeven and Levine (2009).

[24] The two-factor model is based on market risk and interest rate risk, with the latter factor proxied by daily changes in the Treasury rate (Flannery and James, 1984; Saunders, Strock and Travlos, 1990).

8. Conclusion

This paper has investigated the effect of government assistance on bank risk taking. While we do not find a significant effect of government assistance on the aggregate credit supply, our results suggest a considerable effect on the risk of originated loans. After being approved for federal funds, CPP participants issue riskier loans and increase capital allocations to riskier, higher-yield securities, as compared to banks that were denied federal funds. A fraction of CPP funding is also used to improve capital positions. Yet, despite an improvement in capitalization ratios, the net effect is a significant increase in systematic risk and the probability of distress at approved banks. Overall, our evidence is broadly consistent with the theories that predict an increase in risk taking incentives as a result of government protection. From a policy perspective, our findings show that any capital provisions should establish clear investment guidelines and tracking mechanisms for capital deployment.

Appendix A. Variable definitions

A.1. Bank-level variables

Camels proxies

Capital adequacy = Tier-1 risk-based capital ratio, measured by the ratio of Tier-1 capital to risk-weighted assets.

Asset quality = negative of noncurrent loans and leases scaled by total loans and leases.

Management quality = negative of the number of corrective actions that were taken against bank executives by the corresponding banking regulator (Fed, OTS, FDIC, and OCC) each year.

Earnings = return on equity (ROE), measured by the ratio of quarterly net income to total equity capital.

Liquidity = cash divided by deposits.

Sensitivity to market risk = sensitivity to interest rate risk, measured by the ratio of the absolute difference between short-term assets and short-term liabilities to earning assets.

Capital ratios

Tier-1 risk-based capital ratio = Tier-1 capital divided by risk-weighted assets.

Total risk-based capital ratio = total risk-based capital divided by risk-weighted assets.

Equity capital ratio = equity capital divided by total assets.

Bank fundamentals

Size = natural logarithm of book assets.

Age = age in years since the year an institution was established.

Exposure to regional economic shocks = weighted average of quarterly changes in the state-coincident macro indicators from the Federal Reserve Bank of Philadelphia across all states in which a given bank maintains active branches. The weights represent the fraction of the bank's deposits held in the branches in a given state.

Foreclosures=backward-looking measure of loan quality and exposure to the crisis, measured as the value of foreclosed assets divided by net loans and leases.

Loan charge-offs=ratio of net loan charge-offs to total loans.

Funding mix=ratio of deposit funding from purchased money to core deposits.

Investment portfolios

Lower-risk securities=U.S. Treasury securities and securities issued by states and political subdivisions.

Riskier securities=mortgage-backed securities (excluding government-sponsored agency obligations), other domestic and foreign debt securities, and investments in mutual funds and equity products.

Long-term debt securities=debt securities with the remaining maturity greater than five years.

Bank risk

ROA volatility=standard deviation of quarterly ROA over the trailing year.

Z-score=ROA plus capital asset ratio divided by the standard deviation of ROA.

Beta=market beta computed from daily returns over a one-year horizon, with the Center for Research in Security Prices (CRSP) value-weighted index used as the market proxy.

Stock return volatility=volatility of daily stock returns computed over a one-year horizon.

Sharpe ratio=annualized excess daily stock return divided by the annualized standard deviation of excess daily stock returns estimated over the trailing one year.

Information ratio=annualized market-adjusted daily stock return divided by the annualized standard deviation of market-adjusted daily stock returns estimated over the trailing one year; CRSP value-weighted index is used as the market proxy.

One-factor alpha=annualized alpha estimated from a regression of daily excess stock returns on the daily market excess return estimated over the trailing one year.

Three-factor alpha=annualized alpha estimated from a regression of daily excess stock returns on the daily market excess return, the HML (high minus low) factor, and the SMB (small minus big) factor estimated over the trailing year.

A.2. CPP and financial regulation

CPP application indicator=indicator that equals one if a firm applied for CPP funds.

CPP investment indicator=indicator that equals one if a firm received (conditional on being approved for) CPP funds.

Large (*Small*) *CPP investment*=CPP investment above (below) 2.6% of a firm's risk-weighted assets, respectively.

After CPP=indicator that equals one in 2009–2010 and zero in 2006–2008.

Approved bank (specifications without instrumental variable)=indicator that equals one if a firm's CPP application was approved.

Approved bank (instrumental variable specifications)= predicted likelihood that a firm's CPP application is approved based on the regression of CPP approvals on a firm's *Finance committee representation*, as defined below.

Finance committee representation=indicator that equals one if a firm is headquartered in a district of a House member who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee in October 2008.

A.3. Credit origination and credit risk

Retail lending

Application approval=indicator that equals one if a mortgage application was approved.

Loan to income=loan amount requested in a mortgage application divided by the applicant's annual income.

High yield=indicator that equals one if the interest rate on the mortgage exceeds the yield on the Treasury of comparable maturity by at least 300 (500) basis points for first-lien (second-lien) loans.

Local credit markets

Home vacancy rate=ratio of vacant residential addresses, as determined by the United States Postal Service, to the total number of residential addresses in the county.

Per capita income=total personal income of county residents divided by county population.

Unemployment rate=percent of unemployed county residents in the total county workforce.

Corporate lending

Fraction of approved banks per loan=ratio of the number of CPP-approved banks in the loan facility to the total number of creditors in the loan facility.

Cash flow volatility=volatility of earnings, net of taxes and interest and scaled by total assets, over the trailing three years.

Intangible assets=ratio of intangible assets to total book assets.

Interest coverage=inverse of the interest coverage ratio, calculated as interest expense divided by earnings before interest and taxes.

Overall credit activity

Yield on loan portfolios=interest and fee income from loans and leases divided by total loans and leases.

Loan commitments=total unused loan commitments scaled by total assets.

Appendix B. Matched samples

Table B.1
Descriptive statistics for matched samples.

This table provides details on three sets of matched samples in our study: (1) approved vs. denied CPP applicants, (2) approved banks that accepted vs. declined CPP funds, and (3) approved banks that repaid vs. did not repay CPP funds. The samples are constructed from publicly traded CPP-eligible firms with known application status by using the following one-to-one matching procedure. In the matched sample of approved vs. denied CPP applicants, for each bank that was not approved for CPP, we match the closest approved bank based on propensity scores estimated from a regression that predicts the likelihood of CPP approval based on a bank's Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. In the matched sample of approved CPP applicants that accepted vs. declined funds, for each bank that declined funding, we match the closest CPP recipient based on propensity scores estimated from a regression that predicts the likelihood of declining funding based on a bank's Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. In the matched sample of approved CPP applicants that repaid vs. did not repay CPP funds, for each bank that repaid CPP funds, we match the closest CPP recipient that did not repay the funds based on propensity scores estimated from a regression that predicts the likelihood of repaying CPP funds based on a bank's Camels proxies, foreclosures, funding mix, exposure to regional economic shocks, age, and size. All variables are defined in Appendix A. Reported *p*-values correspond to the test of the difference of means. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Matched samples	Approved vs. denied CPP applicants				Approved banks that accepted vs. declined CPP funds				Approved banks that repaid vs. did not repay CPP funds			
	Denied	Approved	Difference	*p*-value	Declined funding	Accepted funding	Difference	*p*-value	Repaid	Did not repay	Difference	*p*-value
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Capital adequacy (%)	11.548	12.013	0.464	0.451	12.050	11.622	−0.428	0.541	13.152	11.014	−2.138	0.116
Asset quality (%)	−1.938	−2.063	−0.125	0.754	−1.625	−1.818	−0.192	0.194	−1.725	−1.803	−0.078	0.528
Management quality	−0.310	−0.284	0.026	0.346	−0.294	−0.317	−0.022	0.752	−0.286	−0.274	0.012	0.614
Earnings (%)	3.285	3.502	0.217	0.676	3.467	2.520	−0.947	0.744	3.914	3.392	−0.522	0.478
Liquidity (%)	4.061	3.783	−0.278	0.446	4.026	3.860	−0.166	0.504	4.153	3.753	−0.400	0.582
Sensitivity to market risk (%)	11.508	9.969	−1.540	0.270	14.571	12.964	−1.607	0.320	17.624	15.798	−1.826	0.496
Foreclosures (%)	0.315	0.304	−0.012	0.716	0.301	0.390	0.089	0.398	0.199	0.238	0.039	0.499
Funding mix (%)	27.789	26.600	−1.189	0.806	27.201	28.824	1.623	0.292	26.746	28.629	1.883	0.129
Exposure to regional shocks (%)	−0.046	−0.042	0.004	0.674	−0.044	−0.039	0.005	0.240	−0.041	−0.045	−0.004*	0.080
Loan charge-offs (%)	0.511	0.503	−0.008	0.655	0.488	0.514	0.026	0.688	0.486	0.543	0.057	0.120
Age	68.402	69.471	1.069	0.396	73.158	67.817	−5.341	0.303	70.338	63.020	−7.318***	0.008
Size (log assets)	13.922	13.402	−0.520	0.137	13.911	14.295	0.384**	0.032	14.763	14.057	−0.706*	0.062

Appendix C. Instrumental variable

Table C.1
First stage instrumental variable regression and falsification tests.

This table shows evidence on the instrumental variable *Finance committee representation.* This instrument is defined as an indicator that equals one if a firm is headquartered in a district of a House member who served on the Capital Markets Subcommittee or the Financial Institutions Subcommittee of the House Financial Services Committee during the adoption of TARP in October 2008. Column 1 reports the first stage linear regression explaining CPP approvals with the instrumental variable *Finance committee representation*, while controlling for other bank-level characteristics. The dependent variable is an indicator that equals one if a firm's CPP application was approved and zero if it was denied. Columns 2–3 compare financial characteristics immediately before the bailout (3Q 2008) between banks that were not represented on key finance committees in October 2008 (column 2) and banks that were represented (column 3). Column 4 shows the difference-of-means test between these two groups of banks. Column 5 reports multivariate correlations between the instrumental variable and bank-level characteristics immediately before the bailout (3Q 2008). Columns 6–8 report results of falsification tests, which examine the relation between the instrumental variable and firm risk taking *in the absence* of the bailout (1Q 2006 to 3Q 2008). Measures of risk taking in the falsification tests in columns 6–8 include *Z-score, Beta,* and *Stock return volatility.* All variables are defined in Appendix A. Reported *p*-values [in brackets] are based on standard errors that are heteroskedasticity consistent and clustered at the bank level. Significance levels at 10%, 5%, and 1% are indicated by *, ** and ***, respectively.

Test	First stage regression	Univariate difference-in-means			Falsification tests			
Dependent variable	CPP approval	Finance committee representation?			Finance committee representation	*Z*-score	Beta	Stock return volatility
		No	Yes	Difference				
Column	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Finance committee representation	0.119***					−2.171	0.123	−0.055
	[0.006]					[0.776]	[0.387]	[0.347]
Capital adequacy	−0.004	11.505	12.103	0.598	0.003	0.066	−0.025	−0.014*
	[0.195]			[0.540]	[0.306]	[0.770]	[0.777]	[0.072]
Asset quality	−0.101	−1.917	−1.855	0.063	0.092	7.050*	0.026	0.002
	[0.163]			[0.233]	[0.268]	[0.097]	[0.566]	[0.913]
Management quality	0.018	−0.323	−0.280	0.043	0.074	−0.525	−0.015	−0.004
	[0.307]			[0.422]	[0.138]	[0.391]	[0.668]	[0.583]
Earnings	0.044***	3.245	3.250	0.004	−0.012	3.549	−0.060	0.002
	[< 0.001]			[0.778]	[0.283]	[0.528]	[0.375]	[0.581]
Liquidity	−0.002	3.987	3.592	−0.395	−0.002	0.216	0.021	−0.005
	[0.770]			[0.439]	[0.777]	[0.700]	[0.410]	[0.171]
Sensitivity to market risk	0.002	12.682	13.446	0.764	0.000	−0.649	−0.007	0.006*
	[0.225]			[0.538]	[0.822]	[0.166]	[0.636]	[0.094]
Foreclosures	0.001	0.384	0.360	−0.024	0.000	−0.671*	0.005	0.011*
	[0.509]			[0.695]	[0.451]	[0.075]	[0.153]	[0.054]
Funding mix	0.043	26.979	27.826	0.847	0.034	0.042	0.003	0.008
	[0.436]			[0.294]	[0.603]	[0.759]	[0.985]	[0.623]
Exposure to regional shocks	−0.174	−0.034	−0.031	0.003	7.318	−0.890	0.271***	0.002***
	[0.285]			[0.225]	[0.523]	[0.654]	[0.002]	[< 0.001]
Age	−0.140	66.402	68.960	2.558	0.000	−0.828	0.223*	0.000*
	[0.622]			[0.647]	[0.855]	[0.566]	[0.074]	[0.084]
Size	0.025	14.102	14.254	0.152	0.010	−1.484	0.387**	−0.004
	[0.110]			[0.314]	[0.612]	[0.197]	[0.041]	[0.539]
Observations	416				416	4,448	4,448	4,448
Adjusted R^2	0.242				0.026	0.061	0.319	0.062
F-statistic [*p*-value]	14.564 [< 0.001]							
Shea's (1997) partial R^2	0.144							

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