Essays on Financial Intermediation

by

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Abstract

In Chapter 1, co-authored with Amir Kermani we ask the following question: can an increase in the supply of credit induce a boom and bust in house prices and real economic activity? This paper exploits the federal preemption of national banks from local anti-predatory laws to gauge the causal effect of the supply of credit on the real economy. Specifically, we exploit the heterogeneity in the market share of national banks across counties as of 2003, as well as heterogeneity in states anti-predatory laws to instrument for the outward shift in the supply of credit. We first show that if we compare counties in the top versus the bottom decile of presence of national banks in states with anti-predatory laws, the preemption regulation resulted in an 11% increase in annual loan issuance. Our estimates show that to such an increase in annual loan issuance correspond a 12% total increase in house prices and a 2% increase in employment in the non-tradable sectors, followed by a bust of similar magnitude in the subsequent years. Finally we show that the increase in the supply of credit reduced mortgages' delinquency rates during the boom years, but resulted in higher delinquency rates during the bust years.

In Chapter 2, co-authored with Marcin Kacperczyk we investigate the effect of the zero-bound interest rate policy on money market funds industry. We find that, as the Fed funds rate approaches zero bound, money funds display reaching for yield incentives in that they invest in riskier asset classes and hold less diversified portfolios. The reduction in interest rates also increases the likelihood of funds exiting the market and lowers expenses funds charge to investors. Consistent with the reputation concerns at stake, we find that funds affiliated with large financial institutions are more likely to exit the market while funds managed by independent asset management companies take on relatively more risk. Additional evidence from the Fed's forward guidance policy corroborates the findings.

In Chapter 3, co-authored with Marshall Van Allstyne we ask the following question: what drives workers to seek information from their peers? And how does communication affect employee performance? We address these questions using an original panel data set that includes all accesses to an information-sharing platform, together with performance measures of all loan officers at a major commercial bank. We show that low skill agents benefit the most from consuming others' information. Moreover, we provide evidence that job rotation destroys specialized human capital, such as soft information about local borrowers. Finally, by instrumenting the demand for information with the exogenous variation arising from differences in social norms among branches, we are able to assess the causal effect of information sharing on performance.

To my parents and sisters

Chapter 1
Credit-Induced Boom and Bust

1 Introduction

The Great Recession was preceded by a large expansion of credit and followed by a collapse in housing prices and consumption, which took more than three years to return to its level just prior to the recession. The resulting employment decline experienced during the Great Recession was greater than that of any recession of recent decades, with unemployment peaking at 10% in October 2009. What is the role of financial markets in generating these severe fluctuations? Specifically, does an outward shift in the credit supply during the expansionary phase of the business cycle explain the observed disruptions in the real economy?

This paper investigates how an increase in credit supply to riskier borrowers is responsible for the boom and bust cycle in housing prices and economic outcomes observed during the Great Recession. This question is important for understanding how financial markets affect the real side of the economy and how the supply of credit might amplify fluctuations. However, identifying the causal effect of credit is challenging because of omitted variables and reverse causality. The latter concern is especially important: counties that experience higher growth are going to increase their consumption and drive house prices up, but are also going to have higher demand for credit. As a result, house price and employment increases will be strongly correlated with the supply of credit, even if credit has no direct effect on house prices and consumption.

In this paper we attempt to estimate the causal effect of an increase in credit supply on economic outcomes by taking advantage of important changes to banking regulation in the U.S. during the early 2000s. In particular, starting in 1999 several states adopted anti-predatory laws (APL) that implemented several restrictions on the terms of mortgage loans to riskier borrowers, such as requiring verification of borrowers' repayment ability, as well as including limits on fees, rates and prepayment penalties. However, in 2004 the Bush administration through the Office of the Comptroller of the Currency (OCC), in an effort to increase home ownership, enacted a preemption rule, which barred the application

of state anti-predatory laws to national banks. In other words, national banks and their mortgage lending subsidiaries became exempt from state anti-predatory lending laws and state enforcement. In contrast, mortgage brokers and independent non-depository lenders, along with state-chartered depository institutions and their subsidiaries, were required to comply with the provisions in state anti-predatory lending laws.

This setting offers a great opportunity to exploit variation across states and across different types of lenders to investigate the role of shocks to the credit supply. Key to our identification strategy is the possibility to compare economic outcomes in states with and without APL, in particular before and after the OCC preemption rule was enacted, but taking advantage of the substantial heterogenous presence of national banks in different counties. Specifically, counties with a high fraction of loans originated by national banks in APL states before 2004 were subject to a positive credit supply shock after the OCC regulation: national banks were now able to grant loans to riskier borrowers in those counties with fewer limitations than the other financial institutions. However, states with APL might differ from states without APL, and counties with a higher presence of national banks might be subject to different shocks than counties with a predominant presence of local banks. To control for these differences, we compare counties within states with APL taking out the difference between counties with higher OCC lenders and counties with lower OCC lenders in non-APL states, that is, we employ a triple differences-in-difference estimator to gauge the effect of credit increase on the real economy. This allows us to sharply identify the effect of the preemption on the availability of lending to riskier borrowers, and then to use this as an instrument for the supply of credit during the period preceding the Great Recession.

There are four primary findings. First, we begin by showing that if we compare counties in the top versus the bottom decile of presence of national banks in states with anti-predatory laws the OCC preemption resulted in a 11% increase in annual loan issuance. To control for different county characteristics in all specifications we include county fixed-effects as well as year fixed-effects. We also include a number of different controls such as the county median

income and population, as well as the elasticity measure proposed by Saiz (2010) to control for the increase in the credit demand and in collateral values. This is important because it shows that our instrument is not capturing differences in the counties' propensity to experience house price increases, instead, our variation comes from the increase in the supply of credit. Further, when we restrict attention to subprime counties, defined as the counties with a higher than the median fraction of subprime borrowers in 2000, we show that the effect of the preemption on loan origination is about 50% larger. Interestingly, this corroborates our hypothesis that the preemption regulation significantly increased the availability of credit to riskier borrowers. To shed some lights on the time pattern of this effect, we investigate separately the boom period 2003-2005 and the bust period 2007-2009. We confirm that counties with stronger presence of OCC lenders experienced a more significant boom and bust in loan origination. This estimates constitute our first stage regression, as we can now instrument the supply of credit with the interaction between the presence of national banks in APL states and the post indicator for the period after 2004.

Second, using this as an instrument for the supply of credit, we estimate the effect of the credit supply on house prices. We find a large effect. A 10% increase in loan origination leads to a 3.5% increase in the house price growth rate, which resulted in a total increase of 12% in house prices during the boom period. Moreover, our interaction significantly predicts the bust in housing prices as well. Our estimate of the effect of the supply of credit on house price growth is robust to extensive controls for demographics and income differences across counties. Moreover, all specifications explicitly control for the elasticity of house prices. This means that absent the preemption regulation, a substantial fraction of the increase in house prices and the consequent collapse could have been avoided.

Third, we explore the effect of the increase in supply of credit on the employment in non tradable sectors (as defined by Mian and Sufi (2012)), in order to focus on the sectors that are mostly affected by the local demand. We find that employment rises significantly more in counties with a prominent presence of national banks in APL states, even controlling for

several county characteristics. Specifically, our IV estimates suggest that a 10% increase in loan origination leads to a 2% increase in employment in the non-tradable sectors. Consistent with our credit-induced fluctuation mechanism, we find that the effect doubles in counties with a higher fraction of subprime borrowers. Moreover, by restricting attention to the boom and bust period we find that the predicted increase in lending are associated with more pronounced boom and bust.

Finally, we provide evidence on the quality of loans originated by national banks in the boom period. Interestingly, we find that counties with a higher fraction of loans originated by OCC lenders in APL states experienced significantly lower delinquencies during the boom period, but at the same time a sharper increase in delinquencies during the bust period. In other words, if we compare counties in the top versus the bottom decile of presence of national banks in states with anti-predatory laws, the OCC preemption resulted in a 15% decrease in delinquencies during the boom period, and in 30% more delinquencies during the Great Recession. Interestingly, this shows that the increase in lending allowed households to avoid late payments during the boom years, but aggravated their financial situation during the bust, making them more fragile to the downturn. In this case as well we find that the results are even stronger when we restrict attention to subprime counties.

To check the robustness of our results and evaluate potential alternative mechanism we show several additional results. First, one potential concern with our results is that the presence of national banks might be correlated with the rise in securitization that occurred during the same period. To rule out this possibility, we compute for each county the fraction of loans securitized, and use this as a proxy for the banks' incentives to increase lending due to securitization. We show that all of our results are completely unaffected, which suggests that our instrument is not correlated with the increase in securitization experienced during the pre-crisis period. Second, to further control for potential unobserved heterogeneity across counties, we can restrict attention to the state borders. Since counties in the West coast are much larger than counties on the East coast, and the sample size of the counties close to the

state borders is small, we construct our main variables at the census tracts level. We can confirm our main result even restricting attention to the census tracts within five miles of the state borders. Third, we show that our analysis is robust even when we focus it only to states that *eventually* passed an anti-predatory law. In other words, if one concern is that states with APL are fundamentally different from non-APL states, this test shows that even without using this variation, but just the different timing of the adoption, our results hold. Finally, we also show that the main effects come from an increase in loans to households, while the lending to small businesses is not significantly affected by the preemption regulation.[1]

1.1 Related Literature

To the best of our knowledge, this is the first paper that is able to estimate the causal effect of an increase in credit supply on housing prices and real economic activity, showing that an outward shift in credit supply generates a distinct boom and bust pattern.

There is an emerging literature on the effects of the housing price booms on real economic activity that is related to this paper, the closest papers to ours are Mian and Sufi (2009), and Kermani (2012). First, Mian and Sufi (2009) show that Zip codes with a higher fraction of subprime borrowers experienced an unprecedented relative growth in mortgage credit. Kermani (2012), instead, provides theory and evidence that links the decline in consumption and housing wealth in many economic sub regions to the very increase in consumption and housing wealth in the area, and emphasizes that this cycle results naturally from the interplay between expanding credit, consumers keen on front-loading their consumption, and the endogenous relaxation of credit constraints. Our paper makes three significant advances relative to these contributions: (1) by exploiting an exogenous variation in the supply of

[1] We further prove the robustness of our results by showing that the predictive lending increases are not associated with an increase in employment in the tradable sectors. Furthermore, we can eliminate the states with the highest delinquency rates and most pronounced housing bubble, Arizona and Nevada, and show that our results are not driven by those states.

credit, we are able to estimate the role of the supply of credit on house prices controlling for local economic shocks; (2) the nature of our data allows us to track not only the effect of credit on house prices, but also on employment and delinquency rates; and (3) we find that the outward shift in the credit supply that followed the preemption regulation significantly predict both the boom *and* the bust in real economic activity.

Other related papers that study the interplay between credit, house prices and consumption include Mian et al. (2011), Mian et al. (2011), Greenstone and Mas (2012), and Adelino et al. (2012). Mian et al. (2011) exploit the difference between judicial and non-judicial foreclosure states as an instrument for foreclosures, and show that foreclosures lead to a significant decline in house prices and residential investment.[2] Mian et al. (2011) show that Zip codes with more levered households have a higher marginal propensity to consume out of housing wealth. The importance of the credit channel has recently been highlighted by Greenstone and Mas (2012), which assesses the role of the supply of credit from banks to small businesses in affecting the employment decline observed during the Great Recession. In contrast, we are able to instrument variations in lending with regulatory changes to show the effect of the increase in lending on the boom and bust experienced in several sectors of the economy. Finally, Mian and Sufi (2012) show that job losses in the non-tradable sector between 2007 and 2009 are significantly higher in high-leverage counties that experienced sharp demand declines, while Adelino et al. (2012) exploits changes in the conforming loan limit as an instrument to gauge the effect of lower cost of financing on house prices. We employ the same differentiation of Mian and Sufi (2012) between tradable and non tradable sectors to show that the increase in lending, boosted local demand which in turn increased

[2] Other related papers are Favara and Imbs (2010) and Kleiner and Todd (2007). Favara and Imbs (2010) employs the the passage of the Interstate Banking and Branching Efficiency Act (IBBEA) in 1994 to show that the deregulation triggered an increase in the demand for housing, that is, house prices rose because the supply of credit increased in deregulating states. In contrast, we identify an increase in credit supply due to the preemption rule of 2004, and its role in generating a boom and bust cycle on both house prices and employment. Kleiner and Todd (2007), instead, find that the requirement in many states that mortgage brokers maintain a minimum net worth is associated with fewer brokers, fewer subprime mortgages, higher foreclosure rates, and a greater percentage of high-interest-rate mortgages.

employment in the non tradable sectors.

This paper also contribute to the growing literature studying the effects of the decline in lending during the Great Recession. Ivashina and Scharfstein (2010), for instance, document that new loans to large borrowers fell by 79% between the second quarter of 2007 and the fourth quarter of 2008. They argue that it is in large part "supply-driven", because of the decline in banks' access to short-term debt following the failure of Lehman. Using Community Reinvestment Act data, Huang and Stephens (2011) and Berrospide and Edge (2010) show that multi-market banks' exposure to markets with housing busts affected the supply of small business loans within all MSAs. Goetz and Valdez (2010) find evidence that differences in liability structure of small U.S. commercial banks, particularly the use of "non-core" financing, affected lending patterns during the 2008 crisis. Dagher and Fu (2011) shows a positive correlation between the presence of non-bank mortgage originators and the increased foreclosure filing rates at the onset of the housing downturn.

Finally, Rajan and Ramcharan (2012) examine the farm land price boom (and bust) in the United States that preceded the Great Depression, and show that credit availability likely had a direct effect on inflating land prices. Moreover, areas with higher ex ante credit availability suffered a greater fall in land prices, and experienced higher bank failure rates. We show, instead, that the credit supplied by national banks during the expansionary phase of the business cycle is able to explain the large increase in housing price, employment and consumption and their subsequent collapse.

The remainder of the paper is organized as follows. Section 2 provides some background on the US credit market and its regulation. Section 3 provides details on the data sources. Section 4 explains the research design and how it is operationalized. Section 5 outlines the main results and interprets the findings. Section 6 discusses several robustness checks and Section 7 concludes.

2 Regulatory Framework

2.1 Dual banking system

In the United States, residential mortgage lenders are regulated by national and local regulatory agencies. Specifically, national banks, Federal thrifts, and their subsidiaries are supervised by the OCC or the OTS, respectively. In contrast, state banks and thrifts chartered at the state level are supervised by either the Federal Reserve System (FRS), the Federal Deposit Insurance Corporation (FDIC) or by their chartering state. Credit unions, instead, are supervised by the National Credit Union Administration (NCUA), while non-depository independent mortgage companies are regulated by the Department of Housing and Urban Development (HUD) and the Federal Trade Commission. One potential concern is the possibility for banks to switch regulatory agency.

The inconsistencies generated by this dual system have been the subject of a recent study by Agarwal et al. (2012). The authors show that federal regulators are significantly less lenient, downgrading supervisory ratings about twice as frequently as state supervisors. Moreover, under federal regulators, banks report higher nonperforming loans, more delinquent loans, higher regulatory capital ratios, and lower ROA.

Then, banks have the incentive to switch from Federal to state supervision, if allowed to do so. Rosen (2005) explores the switching in regulatory agencies between 1970 and 2003. He shows that most of these switches were in the early periods due to new banking policies, such as the lessening of prohibitions of interstate banking. He finds that the main reason for switching after the initial period is a merger with a bank chartered at a different level. However, he provides evidence that the banks who switch tend to be small banks with total assets less than one billion. These findings corroborates the validity of our identification strategy. However, the granularity of our data set allow us to track the banks that changed regulatory agencies in our sample, which gives us the opportunity to address any further

concerns related to this issue.

2.2 Anti-predatory laws

This dual banking system generated conflicting regulations when several states passed anti-predatory laws and the OCC issued a preemption rule for national banks. In 1994, Congress passed the Home Ownership and Equity Protection Act (HOEPA) which imposed substantive restrictions on lending terms and practices for mortgages with high prices, based on either the APR or the total points and fees imposed. This regulation aimed to address abusive practices in refinances and home equity loans with high interest rates or high fees.[3] However, very high thresholds used to classify mortgages as predatory or "high cost;" significantly reduced the applicability of these restrictions, in fact, these "high cost mortgages" only accounted for one percent of subprime residential mortgages, targeting the most abusive sector of the subprime mortgage market (Bostic et al. (2008)).

In subsequent years, many states adopted stronger anti-predatory lending regulations than federal law requires. Anti-predatory laws try to address different forms of unfair and deceptive practices such as lenders steering borrowers into a higher interest rate loan than they could qualify for, making a loan without considering the borrower's repayment ability, charging borrower exorbitant fees, or adding abusive subprime prepayment penalties, all of which might significantly increase the risk of foreclosure. The first comprehensive state law was passed in 1999 by North Carolina, and it aimed at preventing predatory mortgage lending in the subprime mortgage market. As of January 2007, 29 states and the District of Columbia had anti-predatory laws in effect.

The anti-predatory laws can potentially have different effects on the mortgage market outcomes. On the one hand, the laws might ration credit and increase the price of subprime

[3] Agarwal and Evanoff (2013), for instance, provide evidence of unscrupulous lender behavior — e.g., predatory lending — during the housing boom of the 2000s. They show that lenders steered higher-quality borrowers to affiliates that provided subprime-like loans, with borrowers being charged 40-60 bps higher APR.

loans. On the other hand, the regulation might be essential to allay consumer fears about dishonest lenders and ensure that creditors internalize the cost of any negative externalities from predatory loans, which might boost the demand for credit.

There is a strong body of evidence that has recently shown that anti-predatory laws had an important role in the subprime market. Ding et al. (2012), for instance, finds that anti-predatory laws are associated with a 43% reduction in prepayment penalties, and a 40% decrease in adjustable-rate mortgages. Moreover, they find that anti-predatory laws are also correlated with a significant reduction in the riskier borrowers' likelihood to default. These effects are even stronger for subprime regions, i.e. the ones with higher fraction of borrowers with FICO scores below 620.

Using 2004 HMDA data, Ho and Pennington-Cross (2006) find that subprime loans originated in states with APLs had lower APRs than loans in unregulated states. Further evidence is provided by Ho and Pennington-Cross (2008). They focus on border counties of adjacent states with and without anti-predatory laws to control for labor and housing markets characteristics, and using a legal index, they examine the effect of APLs on the probability of subprime applications, originations, and rejections. They find that stronger regulatory restrictions reduced the likelihood of origination and application. Similarly, Elliehausen et al. (2006) using a proprietary database of subprime loans originated by eight large lenders from 1999 to 2004, find that the presence of a law was associated with a decrease in total subprime originations. More recently, Agarwal et al. (2013) estimate the effect of an anti-predatory pilot policy in Chicago on mortgage default rates, which required "low-credit-quality" applicants and applicants for "risky" mortgages to submit their loan offers from state-licensed lenders for third-party review by HUD-certified financial counselors. They show that this policy significantly affected both the origination rates of risky mortgages and their characteristics.[4]

Finally, the anti-predatory laws had likely an important effect on lenders' securitization

[4]For a theoretical model of predatory lending see Bond et al. (2009).

incentives. In fact, the credit rating agencies clearly stated that after the APLs were enacted, they started requiring credit enhancement from lenders that could be in violation of state predatory laws: "To the extent that potential violations of APLs reduce the funds available to repay RMBS investors, the likelihood of such violations and the probable severity of the penalties must be included in Moody's overall assessment".[5]

We are going to follow this literature in considering only the states that passed anti-predatory laws pertaining purchase loans, and that were not just mini HOEPA implemented to prevent local regulation. Appendix A shows the states that passed an anti-predatory law and their implementation date.

2.3 Preemption Rule

On January 7, 2004 the OCC adopted sweeping regulations preempting a broad range of state laws attempting to regulate the "terms of credit" from applying to national banks' activities. The OCC determined that the preemption pertains to those laws that regulate loan terms, lending and deposit relationships and require a state license to lend. The final rule also provided for preemption when the law would "obstruct, impair, or condition a national bank's exercise of its lending, deposit-taking, or other powers granted to it under federal law", either directly or through operating subsidiaries. The new regulations effectively barred the application of all state laws to national banks, except where (i) Congress has expressly incorporated state-law standards in federal statutes or (ii) particular state laws have only an "incidental" effect on national banks. The OCC has said that state laws will be deemed to have a permissible, "incidental" effect only if such laws (i) are part of "the legal infrastructure that makes it practicable" for national banks to conduct their federally-authorized activities and (ii) "do not regulate the manner or content of the business of banking authorized for national banks," such as contracts, torts, criminal law, the right to collect debts, acquisition

[5] Available at https://www.moodys.com.

and transfer of property, taxation, and zoning.

Specifically, the OCC preempted all regulations pertaining the following:

- Loan-to-value ratios;
- The terms of credit, including schedule for repayment of principal and interest, amortization of loans, balance, payments due, minimum payments, or term to maturity of the loan, including the circumstances under which a loan may be called due and payable upon the passage of time or a specified event external to the loan;
- The aggregate amount of funds that may be loans upon the security of real property;
- Security property, including leaseholds;
- Access to, and use of, credit reports;
- Disclosure and advertising, including laws requiring specific statements, information, or other content to be included in credit application forms, credit solicitations, billing statements, credit contracts, or other credit-related documents;
- Processing, origination, servicing, sale or purchase of, or investment or participation in, mortgages;
- Rates of interest on mortgage loans;

This means that starting in 2004 the subprime mortgage market in states with anti-predatory laws was an unleveled playing field, as national banks were the only mortgage institutions able to provide credit to riskier borrowers without limitations on the terms of credit.

3 Data and Summary Statistics

We collect data on the flow of new mortgage loans originated every year from 1999 to 2011 through the "Home Mortgage Disclosure Act" (HMDA) data set at the loan application level. It records each applicant's final status (denied/approved/originated), purpose of borrowing (home purchase/refinancing/home improvement), loan amount, race, sex, income, and home ownership status. We aggregate HMDA data up to the county level and computed the fraction of loans originated by lenders regulated by the OCC. We augment this data set by obtaining information on the fraction of securitized loans by counties from Blackbox Logic. BlackBox is a private company that provides a comprehensive, dynamic dataset with information about twenty-one million privately securitized Subprime, Alt-A, and Prime loans originated after 1999. These loans account for about 90% of all privately securitized mortgages from that period.

Our county–level house price data from 1999 to 2011 come from Zillow.com which combines the underlying transactions data with a hedonic adjustment model that assigns values to homes based on characteristics of the home, specifically, it is a function of the size of the home, the number of bedrooms, and the number of bathrooms. To control for heterogeneity in the county propensity to experience housing bubbles we use the elasticity measure proposed by Saiz (2010) and largely adopted by the existing literature. To further complement our data about the financial conditions of the different counties, we employ The New York Fed Consumer Credit Panel which provides county level information on loan amounts, mortgage delinquency rates and the fraction of households with FICO scores below 620.

To study how the credit expansion affected employment, we extracted the employment data from the County Business Pattern, which allows us to differentiate between tradable and non tradable sectors (following the classification of Mian and Sufi (2012)). Finally, in order to control in our estimations for the local demand for credit, we also add county–level data on demographics, income, and business statistics through the Census.

4 Research Design

This paper's research design is based on the observation that the preemption regulation have significantly affected the availability of credit to subprime borrowers, especially in counties where the presence of national banks was already predominant. Our identification strategy exploits the heterogeneity in counties' exposure to national banks, under the testable assumption that riskier households can only incompletely substitute for the reduction in the supply of credit from their state-chartered bank affected by the APL. In other words, we believe that it is plausible that a lending supply shock to a subset of banks in a region can affect aggregate lending in that area since households cannot easily substitute across banks in different regions. This hypothesis will be tested directly in the first stage of estimation. Figure 1 shows the distribution of the fraction of loans originated by OCC lenders across the U.S. counties. It shows that indeed the importance of national banks in the mortgage market varies significantly across counties. This source of heterogeneity is critical to study how a change in the regulation affects the availability of credit in the different counties.

Specifically, our estimation methodology is a triple difference estimator (DDD). The reason why we are employing this empirical methodology is twofold. First, the potential problem with just using a difference-in-differences (DD) between counties with a higher fraction of OCC lenders relative to counties with a smaller fraction of OCC lenders might be contaminated by changes in the local mortgage market conditions, which might endogenously drive the presence of national banks. Second, a different DD analysis would be to use another state as the control group and use the counties with a higher fraction of OCC lenders from the non-APL state as the control group. However, this approach is problematic as well, as changes in the availability of credit in counties with a high fraction of national banks might be systematically different across states due to, say, income and wealth differences, rather than the preemption policy. Moreover, states that decided to enact an APL might have done that in response to the conditions of the local credit market.

A more robust analysis than either of the DD analyses described above can be obtained by using both a different state and a control group within the APL state. Specifically, we run the following regression

$$\begin{aligned} Log(\text{Loan Amount})_{i,t} &= \lambda_i + \eta_t + \beta_1 APL_{g,t} * Post_{2004} + \beta_2 OCC_{2003} * Post_{2004} \\ &\quad + \beta_3 OCC_{2003} * APL_{g,t} + \beta_4 APL_{g,t} * Post_{2004} * OCC_{2003} + X_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{1}$$

where i denotes the county, g the state, and t the year of origination of the loan. We measure the county $i's$ exposure to the preemption regulation with the fraction of loans originated by OCC lenders in 2003. $Post_{2004}$ is a dummy variable equal to 1 after 2004, when the preemption rule was enacted, whereas $APL_{g,t}$ is equal to 1 if the state has enacted an anti-predatory law in state g at time t. $X_{i,t}$ is a vector of controls at the county level such as population, income, and the elasticity of house prices. The coefficient of interest is β_4, the coefficient on the triple interaction.

The DDD estimate starts with the time change in averages for the counties with higher fraction of national banks in the APL state and then nets out the change in means for counties with a high fraction of OCC lenders in the non-APL state and the change in means for the counties with a low fraction of OCC lenders in the APL state. The objective is that this controls for two kinds of potentially confounding trends: ex ante differential incentives of lenders to supply credit in counties with high fraction of OCC lenders across states (that would have nothing to do with the preemption policy) and changes in the mortgage market of all counties in the APL state (possibly due to other state policies that affect everyone's propensity to lend, or state-specific changes in the economy that affect lenders' soundness).

We start our analysis by showing that the fraction of national banks is very persistent over time. Figure 2 shows the relation between the fraction of OCC lenders at time t and at $t-2$ for both states with and without anti-predatory laws. It shows that in both cases the correlation over time is above .9. This evidence reassures us that using the measure of

national banks as of 2003 is a strong predictor for their presence in the subsequent years, moreover, it also shows that the introduction of the 2004 preemption rule did not significantly affected the composition of the lenders, e.g. national banks did not suddenly increase their presence in states with APL after the OCC regulation.

Table 1 provides the summery statistics for our main variables and for the four different regions: counties with below and above the median presence of national banks, for states with and without anti-predatory laws. First thing to notice is that both for the level and the change in the elasticity of housing supply, population and median income there are no significant differences across the four regions. We then investigate for our main dependent variables, loan amounts, house prices and employment, the change from 2003 to 2005 and from 2008 to 2010. We find that there is a significant difference between our treatment and control group, which confirms our hypothesis that the preemption regulation differentially affected counties with a high compared with a low presence of OCC lenders in APL versus non-APL states. We then check for the existence of these differences in the pre-period, from 2001 up to 2003, and we find that there are none. This shows that there are no pre-trends, so our empirical strategy is valid.

We can now start presenting our estimation results. Table 2 reports the result of regressing the mortgages originated in different counties for purchase a house on the interaction between $APL_{g,t}$, the Post indicator and an indicator OCC which is equal to one if the originator of the loan is regulated by the OCC, and is then exempt from complying with the anti-predatory laws, and zero otherwise. Column 1 looks at the level and show that there is a significant increase in loan originated by national banks in APL states after 2004. In columns 2 and 3 we investigate the effects on the lending growth controlling for county fixed effects, and county times agency fixed effects respectively. In both specifications, we find that national banks located in states with APL increased their lending significantly. These results suggest that lenders regulated by the OCC significantly increased their lending after the preemption regulation in states with anti-predatory laws.

Table 3, instead, shows the results of (1) estimated on different subsamples. In column (1) we restrict attention to the boom period 2003-2005 and run a cross-sectional regression with the log of the change in loan origination between 2003 and 2005 being our dependent variable. We control for the change in median income and population over the same period and for the elasticity of house prices. We find that our coefficient of interest is positive and both statistically and economically significant. This means that counties in APL states with a higher fraction of national banks have experienced a larger expansion of credit than other counties. Columns (2)-(4) estimate, instead, the same regression but on the yearly changes of loan amounts, controlling in turn for year and county fixed-effects, log of the median income and population, and for the elasticity of housing prices and its interaction with the Post indicator. We consistently find that the presence of national banks in APL states is associated with larger increases in loan origination.

Since the preemption regulation affects mainly the subprime market, in column (5) we restrict attention to the counties with FICO scores below 620 in 2000 above the median of 24%. Consistently with the hypothesis that after the preemption rule the national banks had the opportunity to significantly expand their supply of credit to riskier borrowers, we find that the coefficient is about 50% larger than when we consider the whole sample of counties. Finally, in column (6) we examine the bust period, 2007 through 2010, where the dependent variable is the change in loan origination in that period, and find that the same counties that increased lending during the expansionary phase of the business cycle are the ones where lending is cut the most during the bust period. We find that if we compare counties in the top versus the bottom decile of presence of national banks in states with anti-predatory laws the OCC preemption resulted in a 11% increase in annual loan issuance.

This results show that the preemption rule had a significant effect on the credit supply of national banks in APL states. To further check that the differential effects of the expansion of credit across counties are not driven by differential trends among the counties, Figure 3

depicts the time-series coefficients of the following regressions:

$$Log(\text{Loan Amount})_{i,t} = \sum_{\tau \neq t_0} \beta_\tau APL_{g,t} * Post_{2004} * OCC_{2003} \mathbf{1}_{(\tau=t)} + \gamma_t + \phi_i + \Gamma_{i,t} + \varepsilon_{g,t},$$

where $\mathbf{1}_{(\tau=t)}$ is a dummy variable equal to 1 for year t, and $\Gamma_{i,t}$ contains all the other main effects. We have normalized the coefficient β_{2004} corresponding to the preemption rule to zero. This event study shows that in the pre-period there was no difference in credit supply among counties with different fraction of OCC lenders that might explain our results. In other words, the treatment group (counties with a higher fraction of OCC lenders) and control group (lower fraction) were on parallel trends in the pre-period.

5 Main Results

In this section we present the main results of the paper by looking at the effect of the predicted change in the supply of credit on house prices, employment, and delinquency rates.

5.1 The Effect of Credit Expansion on House Prices

To precisely estimate the effect of the credit expansion on house prices, controlling for different characteristics of the counties, we present in Table 4 the results from the following reduced form

$$\begin{aligned} House\ Prices\ Growth_{i,t} &= \lambda_i + \eta_t + \beta_1 APL_{g,t} * Post_{2004} + \beta_2 OCC_{2003} * Post_{2004} \\ &\quad + \beta_3 OCC_{2003} * APL_{g,t} + \beta_4 APL_{g,t} * Post_{2004} * OCC_{2003} + X_{i,t} + \varepsilon_{i,t}, \end{aligned} \tag{2}$$

with β_4 being our coefficient of interest. In columns (1)-(3) we start controlling for year and county fixed-effects and then add the change in median income and population and the elasticity measure times Post as additional controls. In all three specifications the coefficient

is positive and significant. This shows that predicted increase in credit supply are indeed associated with an increase in house prices. As in the existing literature, we also find that in counties with more elastic supply of houses we find that the house prices increases less than in other counties. House prices growth is also negatively correlated with the introduction of the APL, as this would reduce the amount of lending to subprime borrowers, while changes in income and population are positively associated with house price growth.

Interestingly, we confirm in column (4) that the effect on house prices is even larger for counties with a larger fraction of subprime borrowers. This corroborates the hypothesis that a large fraction of the house appreciation is due to the increase in credit available to riskier borrowers, who would not have had the possibility to purchase a house otherwise.

Since we are ultimately interested in how a change in the credit supply affects the house growth, we can explicitly instrument the credit supply with our main interaction, moreover, we have already shown that our instrument is uncorrelated with other county characteristics. In column (5) we estimate the effect of an increase in loan amounts using two stage least squares as follows:

$$\text{House Growth}_{i,t} = \widehat{Loan\ Amount}_{i,t} + \lambda_i + \gamma_t + X_{i,t} + \varepsilon_{i,t}$$

where the predicted increase in loan amount $\widehat{Loan\ Amount}$ is estimates using the first stage regression 1. We find that the effect is insignificantly larger, as the coefficient increase by about 40 percent. This IV estimation allows us to argue that a 10 percent increase in the credit supply results in 3.5 percent increase in house prices growth over the 2003-2006 period. This leads to a total increase of house prices by 12%.

As an additional check we assessed the issue of possible weakness of our instrumental variable. We generally observe F statistics above Stock (2008) weak identification critical value of ten, rejecting the hypothesis that the IV is weak. We also verified that all our results were robust to weak instruments by employing the approach in Moreira (2009), which

produces tests and confidence sets with correct size when instruments are arbitrarily weak for the just-identified case of a single endogenous variable.

5.2 The Effect of Credit on Employment

In this section, we estimate the effect of the outward shift of the credit supply on aggregate employment. We should expect that job losses in the non-tradable sector will be more correlated with the local demand and with the household indebtedness, than job losses in the tradable sector. The underlying mechanism that we aim to identify is the following: an increase in the availability of credit will boost the local demand and consumption, which in turn will raise employment.

Table 5 shows the main results on employment in the non-tradable sector. In column (1) we investigate the change in employment during the boom years 2003-2005. We find that counties with a higher fraction of national banks experienced a greater increase in employment during those years. We then look in column (2) at the effect on employment controlling for year and county fixed-effects, and the coefficient is still positive and significant. We check the robustness of our results by controlling for various county characteristics in columns (2)-(4). The coefficient remains positive and both statistically and economically significant.

In column (5) we restrict attention to subprime counties and find that the coefficient doubles in magnitude. This result suggests that a 10% increase in annual loan issuance induces a 3% increase in employment in counties with riskier borrowers. In column (6) we instrument the increase in credit supply and find that the coefficient is 35% larger than the OLS estimates. The instrumental variables estimate implies that a ten percent increase in loan issuance is associated with a 2% increase in employment in the non-tradable sector. These results together show that the credit boom experienced during the early 2000s can account for a large fraction of the increase in employment pre-crisis, but also for their subsequent

collapse.

5.3 The Effect of Credit on Delinquency Rates

In the previous sections we have documented that counties that are more exposed to the preemption regulation, because of a higher fraction of national banks, experienced larger boom and bust in house prices and employment. In this section we provide evidence that one of the mechanism that contributed towards aggravating the fluctuations in the counties with a higher fraction of national banks is the increased fragility of borrowers that increased their leverage during the boom period, and in turn their propensity to default at the onset of the downturn.

We formally test this hypothesis in Table 6. In column (1) we show the results for the cross section of counties, and find that the delinquency rates were significantly lower during the 2003-2006 period in counties with a higher fraction of national banks in APL states, even controlling for changes in population and income. In column (2)-(4) we estimate a similar reduced form controlling for various characteristics of the county. As expected, we find that income is negatively correlated with delinquency rates, similarly more elastic counties are the ones with lower default rates. The main coefficient of interest is positive and significant in all the specifications. The effect is also economically large as a 10% increase in annual loan issuance predicts a reduction of 10% in defaults.

Finally, column (6) analyze the period 2007-2010 and shows that predicted increases in lending are associated with a significant increase in delinquency rates. This suggests that all the debt accumulated during the boom made the households more vulnerable to defaults in the recession. The effect is even more significant than for the boom period, as if we compare counties in the top versus the bottom decile of presence of national banks in states with anti-predatory laws the OCC preemption resulted in a 30% increase in delinquencies. This is consistent with the idea that riskier borrowers were able to maintain their level of

indebtedness without defaulting thanks to the amount of credit available during the booms, but were adversely affected in the subsequent years, which led them to default with higher frequency.

One potential explanation for the increase in loan issuance shown in Table 3 is that national banks were more able to identify the higher quality borrowers than the other lenders, such as independent mortgage lenders or local banks. However, if this was true than we should have expected to find that the households who borrowed from national banks were less and not more likely to default at the onset of the crisis, but Table 6 rules out this possibility.

6 Robustness

In this section, we further test the validity of our identification strategy and examine several alternative hypothesis which could potentially explain our results.

6.1 Securitization

One potential concern with the results presented in the previous sections is that the presence of national banks might be correlated with the rise in securitization that occurred during the same period. Alternatively, given the credit rating concerns' about potential violation of the state anti-predatory laws, the inaction of the OCC preemption rule might have also increase the national banks' possibility to securitize loans, even if independent mortgage lenders rather than national banks are usually recognized as the key players in the securitization market. In other words, we try address the following question: can our result be explained by the rise in the securitization rather than by an outward shift in the credit supply?

To control for such concern, we collected data from BlackBox Logic which is the largest provider of data on securitized loans. The database covers 90 percent of the entire universe of securitized loans, and we aggregated this data at the county level. This gives us a reliable

measure of securitized loans that varies at the county level.

Table 7 presents the main estimation of the paper (1), but adds as an additional control this measure of securitization. We find that all of our results are robust to such inclusion: both the magnitude and the statistical significance is unchanged. This suggests that our instrument is not picking up variation in the mortgage originators' incentives to securitize loans. Our instrument is then capturing a different source of variation that works through the national banks' lending incentives, which contributed to the credit boom experienced in the 2003-2006 period.

6.2 Evidence from States borders

We have controlled for a number of county characteristics, however, in order to control for potential unobserved heterogeneity across counties which could potentially bias our results, we can restrict attention to the state borders. Since counties in the West coast are much larger than counties on the East coast, and the sample size of the counties close to the state borders is small, we construct our main variables at the census tracts level. This allow us to have a very homogeneous sample as census tracts are very similar in terms of size across the whole U.S. and a much larger sample size.

We consider only census tracts pairs in different states whose minimum distance is about 10 miles. We have a sample of 4600 census tracts for the results on loan amounts, while we have house price data for only 540 census tracts close to state borders. The reason why we do not have more data on house prices for more census tracts is because many census tracts at the border are rural area, whose house prices indexes are not available. In order to run our triple-difference estimator we compute the fraction of loans originated by national banks in 2003 in each census tract.

Table 8 shows the results of the same regression as in 1 but by including border times time fixed effects and census tracts fixed effects. This allows us to control for any trend

specific to the border and for unobserved and time-invariant heterogeneity across census tracts. Interestingly, we find that our main interaction coefficient is still highly significant and its magnitude is just slightly lower than the one in Table 3. This further validates our triple difference estimator, because in principle if the assumptions underlying it are valid, the magnitude should not depend upon the sample we consider.

6.3 Focusing on States with APL

Since the implementation of anti-predatory laws is not random, one of our main concerns is to control for heterogeneity between states that decided to enact such a law, and the ones that did not. Our triple-difference methodology is partly motivated by such a concern, moreover, the previous results show that even when we restrict attention to the state-borders we find similar effects. However, we can run an additional robustness check: we can focus our analysis to states that *eventually* passed an anti-predatory law. In other words, our treatment group includes the states that passed an anti-predatory law between 2000 and 2004, and the control group is the set of states that implemented these regulations in the subsequent years.

If the main concern is that states with APL are fundamentally different from non-APL states, this test should show that even without using this variation, but just the different timing of the adoption, our results hold. Obviously, we are not saying that the timing is exogenous, but we do believe that this additional test might still be useful in showing that the variation we are employing is not coming from heterogeneity across states, but from the preemption rule and its effects on the national banks' credit supply.

Table 9 shows the results for the four dependent variables of interest. In all specifications, we control for time and county fixed effects. Column 1 shows that the effect on loan amounts is both economically and statistically significant. Column 2 analyze the impact of our main interaction variable on house prices, and it shows that counties with a higher fraction of

national banks experience a larger house prices increase compared to counties with a lower fraction of national banks, within states that passed at some point an anti-predatory law. Column 3 investigates a similar specification for the employment in the non-tradable sector. The sign and the magnitude are very similar to the main specifications, but the coefficient is not significant. Finally, column 4 analyzes the pattern of delinquency rates, and it shows that very similar results to our main specification hold for this restricted sample.

6.4 Other Banking Activities

The preemption regulation changed the competition landscape of the mortgage market, as national banks by having an advantage in the subprime segment could have employed the additional profits and invest it locally. This in turn could have significantly affected the local economy through different channels than the credit supply to households one. For instance, one possibility is that the national banks could have started increase their lending to small businesses with the proceeds of the loans to riskier borrowers during the boom. The relaxation of the credit constraints for the local businesses could explain the increase in employment and potentially in house prices.

To evaluate this possibility, we have collected information about national banks' lending to small businesses using the Community Reinvestment Act (CRA) disclosure data from the Federal Financial Institutions Examination Council (FFIEC). The CRA requires banks with assets above the $1 billion threshold to report small business lending each year. Greenstone and Mas (2012) estimate that in 2007 CRA eligible banks accounted for approximately 86% of all loans under $1 million. FFIEC provides data by bank, county, and year. We aggregated the data by type of lender to compute the total amount lent by national banks to small businesses during our sample period.

Table 10 shows our main regression (1). Column (1) analyze the effect of CRA lending on the loan issuance, while Columns (2)-(4) are devoted to the house price growth, the

employment in the non tradable sector and on the delinquency rate. We find that CRA lending does not impact neither the significance nor the magnitude of our coefficient of interest, namely the interaction between APL, the Post indicator and the fraction of loans to households by OCC lenders in 2003. This evidence helps out in disentangling the direct credit supply effect from the indirect spillover that might have contributed to the county growth. Moreover, these results are in line with Figure 2, as they suggest that the preemption regulation did not significantly affected the mortgage market in other dimension other than the lending to riskier households.

7 Conclusion

In this paper we have exploited important changes in banking regulation which had differential effects on states that enacted anti-predatory laws versus the ones without such laws, and on counties with a different presence of national banks. This provides us with a novel identification strategy that allows us to investigate the role of the supply of credit on the boom and bust in house prices and real economic activity experienced by the U.S.

We uncover four main findings. First, counties that are more affected by the new regulation, that is, the one with stronger presence of national banks in APL states, are the ones where there is a significantly higher origination of loans, an increase of 11% per year. Second, house prices rise significantly more in these same counties, but they also experience a more significant drop during the bust periods. Third, we provide evidence that this increase in the supply of credit had a significant effect on the real economic activity, as employment in the non-tradable sector increases are associated with the predicted increases in lending to riskier borrowers. Forth, we also provide evidence that such a credit boom led to an increase in delinquency rates at the onset of the housing downturn.

These results shed novel lights on the effect of a credit boom on the real economy, how it spreads across the U.S. during the 2002-2006 period, and show that an outward shift in the

supply of credit to riskier households may lead to an exacerbation of economic fluctuations.

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Table 1
Summary Statistics

The table reports descriptive statistics for the main variables employed in our analysis. Loan Amount is computed using HDMA data, and denotes the value of mortgages to purchase a home by mortgage lenders in the period 2000-2011. Data on Population and Income are from the Census. House prices are from Zillow.com and are aggregated at the county level. The Fraction of OCC lenders in 2003 is the share of loans originated by all the mortgage lenders regulated by The Office of the Comptroller of the Currency (OCC) as of 2003, and is computed using data from HDMA. We compute the averages for these variables for counties with above and below the median fraction of loans originated by OCC lenders, and for both states with and without anti-predatory laws. the Column Diff-in-Diff is our baseline estimator and tests for significant differences between the treatment and the control group. Asterisks denote significance levels (***=1%, **=5%, *=10%).

		States without Anti-Predatory Law		States with Anti-Predatory Law		Diff-in-Diff	
		Below Median	Above Median	Below Median	Above Median		
	Fraction of OCC lenders in 2003	0.219	0.349	0.230	0.345	-0.015	
	Elasticity of housing supply	-1.568	-2.083	-1.575	-1.775	0.315	
	Log Population in 2003	10.58	10.66	10.70	10.73	-0.05	
	Log Median Income in 2003	12.51	12.09	13.26	12.60	-0.24	
Change from 2003-2005	Median Income	0.0727	0.0549	0.103	0.0835	-0.0017	
	Population	0.0306	0.0220	0.0212	0.0171	0.0045	
	Fraction of Loans Securitized	0.204	0.162	0.238	0.194	-0.002	
	Loan amounts	0.710	0.443	0.455	0.428	0.24	***
	House prices	0.450	0.250	0.359	0.289	0.13	*
	Employment in non-tradables	0.0725	0.0471	0.0508	0.0441	0.0187	*
Change from 2008-2010	Median Income	-0.00495	-0.0108	-0.00595	-0.00928	0.00252	
	Population	0.00583	0.00728	0.00583	0.00724	-4E-05	
	Loan amounts	-0.265	-0.202	-0.179	-0.210	-0.094	***
	House prices	-0.170	-0.0744	-0.112	-0.0929	-0.0765	**
	Employment in non-tradables	-0.0567	-0.0409	-0.0403	-0.0481	-0.0236	**
Change from 2001-2003 (Pre-trends)	Median Income	0.0302	0.0257	0.0146	0.00803	-0.00207	
	Population	0.00803	0.0102	0.0178	0.0150	-0.00497	
	Loan amounts	0.379	0.280	0.372	0.286	0.013	
	House prices	0.207	0.154	0.275	0.150	-0.072	
	Employment in non-tradables	0.0450	0.0196	0.0454	0.0138	-0.0062	

Table 2

Preemption of National Banks and the Amount of Loans Issued Under Each Regulatory Agency

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated loans under each regulatory agency to the preemtion of national banks were weights equal to population of county. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated for each regulatory agency at county level for each year. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "OCC" is equal to one if the regulating agency is OCC. The results reported in columns 1 to 3 are for years 2000 to 2006. Robust standard errors, clustered at county level, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3
	Log of loan amount	*Loan Amounts / Loan Amounts in 2000*	
APL x Post x OCC	0.09***	0.55***	0.54***
	(0.03)	(0.10)	(0.10)
APL	-0.01	-0.10	
	(0.02)	(0.07)	
APL x OCC	-0.01	-0.11**	-0.10*
	(0.02)	(0.06)	(0.05)
APL x Post	-0.10***	-0.33***	
	(0.02)	(0.09)	
OCC		-0.09***	-0.09***
		(0.02)	(0.02)
Post	0.42***	1.21***	
	(0.02)	(0.08)	
OCC x Post	-0.07***	-0.63***	-0.63***
	(0.02)	(0.08)	(0.08)
Constant	11.52***	1.81***	1.94***
	(0.01)	(0.02)	(0.02)
Time Fixed Effects	Yes	Yes	Yes
County Fixed Effects		Yes	
County-Agency Fixed Effects	Yes		
County-Year Fixed Effetcs			Yes
Observations	90,957	89,170	89,170
R-squared	0.98	0.16	0.14

Table 3
Preemption of National Banks and Boom-Bust in Loan Origination

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated purchase loans to the preemtion of national banks with weights equal to the population of each county. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated at county level for each year. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results in columns 2 to 5 are for years 2000 to 2006. Subprime counties are defined as counties with the fraction of subprime borrowers above the median. Robust standard errors , clustered at county level for columns 2 to 5, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6
	Change in Loan Amount in 2003-2005	*Log of Loan amount*				*Change in Loan Amount in 2008-2010*
			Full Sample		*Subprime counties*	
APL X Post X Fraction OCC	1.468***	0.568***	0.513***	0.864***	1.197***	-0.376*
	(0.367)	(0.150)	(0.123)	(0.225)	(0.305)	(0.218)
APL	-0.600***	0.0418	0.0267	0.0339	0.135*	0.152**
	(0.122)	(0.0350)	(0.0364)	(0.0561)	(0.0705)	(0.0729)
APL X Post		-0.214***	-0.212***	-0.304***	-0.378***	
		(0.0520)	(0.0425)	(0.0692)	(0.0920)	
APL X Fraction OCC		-0.210**	-0.127	-0.175	-0.530**	
		(0.0971)	(0.101)	(0.175)	(0.241)	
Post X Fraction OCC		-0.667***	-0.479***	-0.630***	-0.614***	
		(0.117)	(0.0924)	(0.168)	(0.230)	
Fraction OCC	-1.052***					0.499***
	(0.276)					(0.143)
Elasticity	-0.0517***					-0.0225**
	(0.0119)					(0.0112)
Elasticity X Post				-0.0435***	-0.0686***	
				(0.00850)	(0.0111)	
Log(Median Income)			1.540***	1.514***	1.380***	
			(0.144)	(0.171)	(0.241)	
Log(Population)			1.208***	1.281***	1.320***	
			(0.155)	(0.175)	(0.222)	
Change in Median Income	2.104***					
	(0.271)					
Change in Population	3.158***					
	(0.481)					
Year Fixed Effect		Yes	Yes	Yes	Yes	
County Fixed Effect		Yes	Yes	Yes	Yes	
Observations	770	15,533	15,533	5,390	2,758	770
R-squared	0.371	0.027	0.151	0.214	0.276	0.079
Number of counties	770	2,219	2,219	770	394	770

Table 4
Preemption of National Banks and Boom-Bust in House Prices

The table reports coefficient estimates of weighted least square regressions relating house prices to the preemtion of national banks and the increase in the supply of loans induced by the preemption where the weights are given by the population of each county. House prices are from Zillow.com. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results in columns 2 to 6 are for years 2000 to 2006. In column 6, "APL X Post X Fraction OCC" is used as an instrument for the log of loan amounts. Subprime counties are defined as counties with the fraction of subprime borrowers above the median. Robust standard errors , clustered at county level for columns 2 to 5, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6	7
	Change in House Prices in 2003-2005	*House Prices Growth*					*Change in House Prices in 2008-2010*
			Full Sample		*Subprime counties*	*IV estimate*	
APL X Post X Fraction OCC	0.814**	0.357***	0.330***	0.330**	0.467**		-0.504**
	(0.370)	(0.108)	(0.106)	(0.140)	(0.188)		(0.210)
Instrumented Log of Loan Amounts						0.364***	
						(0.139)	
APL	-0.336***	0.0177	0.0249	0.0241	0.0710	0.0104	0.170**
	(0.115)	(0.0352)	(0.0361)	(0.0489)	(0.0435)	(0.0318)	(0.0680)
APL X Post		-0.124***	-0.120***	-0.114**	-0.170***	0.00250	
		(0.0347)	(0.0346)	(0.0496)	(0.0575)	(0.0232)	
APL X Fraction OCC		-0.117	-0.133	-0.145	-0.185	-0.0897	
		(0.102)	(0.106)	(0.141)	(0.138)	(0.0878)	
Post X Fraction OCC		-0.208***	-0.174***	-0.147	-0.262*	0.186	
		(0.0623)	(0.0605)	(0.0905)	(0.146)	(0.119)	
Fraction OCC	-0.472						0.517***
	(0.290)						(0.186)
Elasticity	-0.0547***						0.0271***
	(0.0172)						(0.00666)
Elasticity X Post				-0.0108*	-0.0170***	0.0103	
				(0.00634)	(0.00543)	(0.0136)	
Log(Median Income)			0.310*	0.340*	-0.0133		
			(0.166)	(0.185)	(0.152)		
Log(Population)			0.493***	0.541***	0.670***		
			(0.117)	(0.132)	(0.177)		
Change in Median Income	2.795***						-0.169
	(0.736)						(0.235)
Change in Population	1.643***						0.484
	(0.324)						(0.449)
Year Fixed Effect		Yes	Yes	Yes	Yes	Yes	
County Fixed Effect		Yes	Yes	Yes	Yes	Yes	
Observations	459	4,057	4,057	2,754	1,261	2,754	478
R-squared	0.476	0.023	0.042	0.046	0.077	-0.045	0.155
Number of counties	459	693	693	472	216	472	478

Table 5
Preemption of National Banks and Boom-Bust in Employment in Non-Tradable Sector

The table reports coefficient estimates of WLS regressions relating employment in non-tradable sector to the preemtion of national banks and the increase in the supply of loans induced by the preemption, with weights equal to the population of each county. Employment data comes from County Business Pattern and non-tradable sectors are definied according to Main and Sufi (2013). "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results in columns 2 to 6 are for years 2000 to 2006. In column 6, "APL X Post X Fraction OCC" is used as an instrument for the log of loan amounts. Subprime counties are defined as counties with the fraction of subprime borrowers above the median. Robust standard errors , clustered at county level for columns 2 to 5, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6	7
	Change in Employment in Non-Tradable Sector in 2003-2005	*Employment in Non-Tradable Sector*					*Change in Employment in Non-Tradable in 2008-2010*
			Full Sample		*Subprime counties*	*IV estimate*	
APL X Post X Fraction OCC	0.207**	0.216***	0.165***	0.169**	0.326***		-0.205**
	(0.0817)	(0.0734)	(0.0610)	(0.0750)	(0.109)		(0.0966)
Instrumented Log of Loan Amounts						0.198**	
						(0.0782)	
APL	-0.0718***	0.0523***	0.0330**	0.0231	0.0281	0.0155	0.0616**
	(0.0242)	(0.0175)	(0.0163)	(0.0196)	(0.0275)	(0.0161)	(0.0308)
APL X Post		-0.0731***	-0.0619***	-0.0667***	-0.114***	-0.00692	
		(0.0234)	(0.0186)	(0.0223)	(0.0302)	(0.00652)	
APL X Fraction OCC		-0.169***	-0.0902*	-0.0590	-0.0956	-0.0243	
		(0.0545)	(0.0493)	(0.0613)	(0.0888)	(0.0548)	
Post X Fraction OCC		-0.201***	-0.108**	-0.0830	-0.201**	0.0440	
		(0.0594)	(0.0463)	(0.0598)	(0.0813)	(0.0422)	
Fraction OCC	-0.0991						0.0342
	(0.0644)						(0.0554)
Elasticity	-0.00207						0.00739***
	(0.00284)						(0.00268)
Elasticity X Post				-0.00495*	-0.00668	0.00286	
				(0.00274)	(0.00458)	(0.00406)	
Log(Median Income)			0.289***	0.289***	0.259***	-0.0108	
			(0.0422)	(0.0443)	(0.0724)	(0.123)	
Log(Population)			0.893***	0.958***	0.966***	0.689***	
			(0.0653)	(0.0688)	(0.108)	(0.133)	
Change in Median Income	0.122**						0.115
	(0.0488)						(0.116)
Change in Population	1.041***						0.297
	(0.111)						(0.215)
Year Fixed Effect		Yes	Yes	Yes	Yes	Yes	
County Fixed Effect		Yes	Yes	Yes	Yes	Yes	
Observations	532	5,362	5,362	3,721	1,767	3,721	538
R-squared	0.224	0.015	0.229	0.285	0.313	0.194	0.045
Number of counties	532	790	790	541	259	541	538

Table 6
Preemption of National Banks and Decline and Subsequent Increase in Mortgages Delinquencies

The table reports coefficient estimates of weighted least square regressions relating the percentage of delinquent mortgages to the preemtion of national banks with weights equal to the population of each county. Delinquency is defined as at least 90 days late payments and comes from Federal Reserve Bank of New York Consumer Credit Panel. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results in columns 2 to 5 are for years 2000 to 2006. Subprime counties are defined as counties with the fraction of subprime borrowers above the median. Robust standard errors , clustered at county level for columns 2 to 5, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4	5	6
	Change in Delinquency Rates in 2003-2005	*Delinquency Rates*				*Change in Delinquency Rates in 2008-2010*
		Full Sample			*Subprime counties*	
APL X Post X Fraction OCC	-1.402***	-1.001***	-0.971***	-1.794***	-2.278***	0.823*
	(0.399)	(0.321)	(0.306)	(0.457)	(0.705)	(0.453)
APL	0.574***	-0.0370	-0.0294	-0.106	-0.182	-0.317**
	(0.122)	(0.0941)	(0.0953)	(0.133)	(0.200)	(0.141)
APL X Post		0.447***	0.455***	0.693***	0.854***	
		(0.104)	(0.0991)	(0.142)	(0.210)	
APL X Fraction OCC		0.0251	-0.0362	0.197	0.372	
		(0.280)	(0.283)	(0.413)	(0.688)	
Post X Fraction OCC		0.932***	0.730***	0.926***	0.857*	
		(0.194)	(0.181)	(0.287)	(0.473)	
Fraction OCC	-1.408***					-0.411
	(0.283)					(0.305)
Elasticity	-1.194***					-0.0638***
	(0.377)					(0.0199)
Elasticity X Post				0.0564***	0.107***	
				(0.0194)	(0.0312)	
Log(Median Income)			-1.897***	-2.004***	-2.068***	
			(0.316)	(0.398)	(0.713)	
Log(Population)			-0.737*	-0.857*	-0.633	
			(0.384)	(0.445)	(0.679)	
Change in Median Income	2.104***					-2.161**
	(0.271)					(0.864)
Change in Population	3.158***					0.0667
	(0.481)					(1.164)
Year Fixed Effect		Yes	Yes	Yes	Yes	
County Fixed Effect		Yes	Yes	Yes	Yes	
Observations	768	15,533	15,533	5,390	2,758	769
R-squared	0.111	0.008	0.023	0.072	0.093	0.011
Number of counties	768	2,219	2,219	770	394	769

Table 7
Robustness Test I: Securitization

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated purchase loans, house prices, employment in non-tradable sector, and deliqneuency rates to the preemption of national banks with weights equal to the population of each county, controlling for the fraction of loans that in each county were securitized. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated at county level for each year. House prices are from Zillow.com. Employment data comes from County Business Pattern and non-tradable sectors are definied according to Main and Sufi (2013). Delinquency is defined as at least 90 days late payments and comes from Federal Reserve Bank of New York Consumer Credit Panel. Fraction of Securitized loans come from BlackBox Logic, which covers 90% of the securitization market. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results are for years 2000 to 2006. Subprime counties are defined as counties with the fraction of subprime borrowers above the median. Robust standard errors , clustered at county level for columns 1 to 4, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	1	2	3	4
	Log of Loan amount	House Prices Growth	Employment in Non-Tradable Sector	Delinquency Rates
APL X Post X Fraction OCC	0.809***	0.269**	0.165**	-1.701***
	(0.199)	(0.133)	(0.07)	(0.431)
APL X Post	-0.305***	-0.0922**	-0.0647***	0.703***
	(0.0616)	(0.0466)	(0.0207)	(0.132)
APL X Fraction OCC	-0.0882	-0.0424	-0.0517	0.0428
	(0.137)	(0.139)	(0.0491)	(0.415)
Post X Fraction OCC	-0.567***	-0.154*	-0.0657	0.910***
	(0.143)	(0.0833)	(0.0522)	(0.273)
APL	0.00343	-0.0142	0.0118	-0.11
	(0.0423)	(0.0481)	(0.0137)	(0.13)
Fraction of Securitized Loans	0.632***	-0.0549	0.182***	-0.390**
	(0.0894)	(0.115)	(0.0305)	(0.192)
Log(Median Income)	1.232***		0.205***	-1.818***
	(0.154)		(0.0467)	(0.419)
Log(Population)	1.294***		0.959***	-0.879*
	(0.16)		(0.0619)	(0.45)
Elasticity X Post	-0.0155*	-0.0136	0.00298	0.0373*
	(0.008)	(0.0111)	(0.00245)	(0.0198)
Change in Median Income		0.347		
		(0.211)		
Change in Population		0.505***		
		(0.126)		
Year Fixed Effect	Yes	Yes	Yes	Yes
County Fixed Effect	Yes	Yes	Yes	Yes
Observations	5,322	2,733	3,706	5,322
R-squared	0.254	0.05	0.314	0.079

Table 8
Robustness Test II: State Borders

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated purchase loans, and house prices to the preemption of national banks, with weights equal to the population of the census tract. We restrict attention to tracts within 10 miles from state borders. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated at census tract level for each year. House prices are from Zillow.com. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003 at the census tract level. The results in columns 1 and 2 are for years 2003 to 2005, while the results in columns 3 and 4 are for the changes between years 2007 and 2009. Robust standard errors , clustered at county level for columns 1 to 4, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

VARIABLES	1 *Change in Loan Amount in 2003-2005*	2 *Change in House Prices*	3 *Change in Loan Amount in 2007-2009*	4 *Change in House Prices in 2008-2010*
APL X Post X Fraction OCC	0.236**	0.237***	-0.280**	-0.0434*
	(0.0968)	(0.0439)	(0.116)	(0.0247)
APL	-0.0763**	-0.0807***	-0.00766	-0.0154*
	(0.0313)	(0.0134)	(0.0386)	(0.00860)
APL X Post				
APL X Fraction OCC				
Post X Fraction OCC				
Log(Median Income)				
Fraction OCC	0.0244	-0.00605	0.272***	0.0318
	(0.0580)	(0.0176)	(0.0733)	(0.0227)
Change in County Median Income	0.490***	0.640***		
	(0.146)	(0.0904)		
Constant	0.341***	0.202***	-0.671***	-0.105***
	(0.0226)	(0.0108)	(0.0251)	(0.00755)
Observations	11,567	7,517	11,377	7,451
R-squared	0.114	0.368	0.158	0.398

Table 9
Robustness Test III: Only APL States

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated purchase loans, house prices, employment in non-tradable sector, and deliqneuency rates to the preemption of national banks with weights equal to the population of each county, restricting attention only to the states that at some point in time decided to implement an anti-predatory law. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated at county level for each year. House prices are from Zillow.com. Employment data comes from County Business Pattern and non-tradable sectors are definied according to Main and Sufi (2013). Delinquency is defined as at least 90 days late payments and comes from Federal Reserve Bank of New York Consumer Credit Panel. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). The results are for years 2000 to 2006. Robust standard errors , clustered at county level for columns 1 to 4, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)
	Log of Loan amount	House Prices Growth	Employment in Non-Tradable Sector	Delinquency Rates
APL X Post X Fraction OCC	0.779***	0.374***	-0.0203	-2.206***
	(0.280)	(0.140)	(0.0681)	(0.661)
APL	0.0326	0.0129	0.0303*	-0.113
	(0.0508)	(0.0471)	(0.0165)	(0.129)
APL X Post	-0.173**	-0.0234	0.00967	0.486***
	(0.0771)	(0.0361)	(0.0184)	(0.184)
APL X Fraction OCC	-0.175	-0.102	-0.0863*	0.245
	(0.166)	(0.138)	(0.0511)	(0.410)
Log(Median Income)	1.679***	0.0868	0.323***	-2.491***
	(0.199)	(0.172)	(0.0468)	(0.473)
Log(Population)	1.148***	0.302***	0.871***	-0.501
	(0.233)	(0.103)	(0.0647)	(0.562)
Elasticity X Post	-0.0328***	-0.00906	-0.00343	0.0311
	(0.0112)	(0.0105)	(0.00334)	(0.0239)
Post X Fraction OCC	-0.567**	-0.176*	0.136**	1.348**
	(0.273)	(0.107)	(0.0674)	(0.611)
Year Fixed Effect	Yes	Yes	Yes	Yes
County Fixed Effect	Yes	Yes	Yes	Yes
Observations	2,842	1,514	2,838	2,842
R-squared	0.238	0.019	0.339	0.068
Number of fips	406	260	406	406

Table 10
Robustness Test IV: CRA Lending

The table reports coefficient estimates of weighted least square regressions relating the amount of newly originated purchase loans, house prices, employment in non-tradable sector, and deliqneuency rates to the preemption of national banks with weights equal to the population of each county and controlling for the lending to small businesses by national banks. Loan amounts is based on HMDA and is the amount of loans originated for purchainsg a house aggregated at county level for each year. House prices are from Zillow.com. Employment data comes from County Business Pattern and non-tradable sectors are definied according to Main and Sufi (2013). Delinquency is defined as at least 90 days late payments and comes from Federal Reserve Bank of New York Consumer Credit Panel. "APL" is equal to one if the state has passed anti-predatory law and zero otherwise. "Post" is a dummy equal to one for years after 2004. "Fraction OCC" is the fraction of OCC lenders in 2003. "Elasticity" is a measure of elasticity of housing supply provided by Saiz (2010). CRA_OCC is the Log of loan amounts to small businesses by OCC lenders with assets above $1 billion. The results are for years 2000 to 2006. Robust standard errors , clustered at county level for columns 1 to 4, are below the coefficients in paranthesis. Asterisks denote significance levels (***=1%, **=5%, *=10%).

	(1)	(2)	(3)	(4)
	Log of Loan amount	House Prices Growth	Employment Non-Tradable Sector	Delinquency Rates
APL	0.0271	0.0166	0.0365**	-0.115
	(0.0539)	(0.0499)	(0.0174)	(0.117)
APL X Post X Fraction OCC	0.782***	0.311**	0.186***	-1.851***
	(0.220)	(0.133)	(0.0580)	(0.463)
APL X Post	-0.278***	-0.101**	-0.0646***	0.718***
	(0.0671)	(0.0445)	(0.0178)	(0.142)
APL X Fraction OCC	-0.108	-0.130	-0.119**	0.217
	(0.173)	(0.142)	(0.0530)	(0.361)
Log(Median Income)	1.302***	-0.0494	0.256***	-1.371***
	(0.171)	(0.130)	(0.0365)	(0.361)
Log(Population)	1.371***	0.412***	1.001***	-1.127**
	(0.195)	(0.0986)	(0.0741)	(0.517)
Elasticity X Post	-0.0395***	-0.0213***	-0.00400	0.0377**
	(0.00866)	(0.00622)	(0.00246)	(0.0182)
Post X Fraction OCC	-0.583***	-0.144	-0.0598	1.160***
	(0.167)	(0.101)	(0.0466)	(0.298)
CRA_OCC	0.00645	-0.0192	0.00175	0.0249
	(0.0164)	(0.0165)	(0.00435)	(0.0376)
Year Fixed Effect	Yes	Yes	Yes	Yes
County Fixed Effect	Yes	Yes	Yes	Yes
Observations	4,502	2,696	4,499	4,502
R-squared	0.174	0.042	0.311	0.058
Number of fips	764	469	764	764

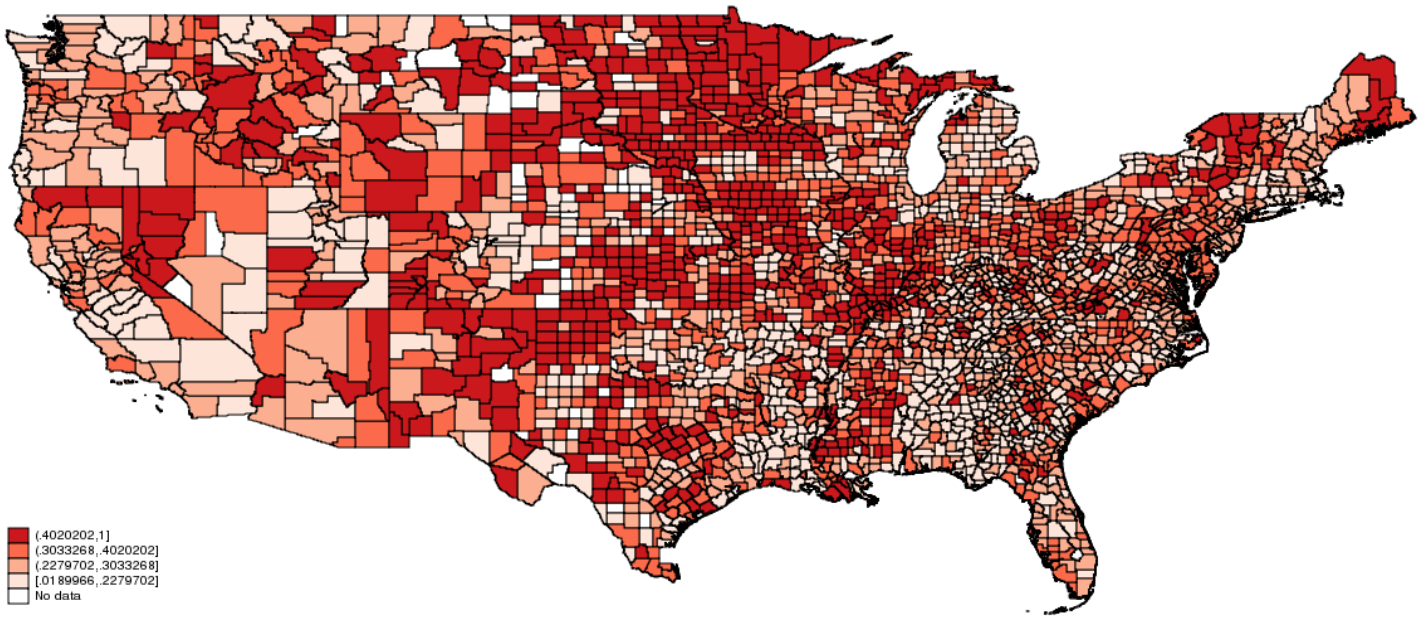

Figure 1- Fraction of Lending Done by National Banks in 2003 for Each County

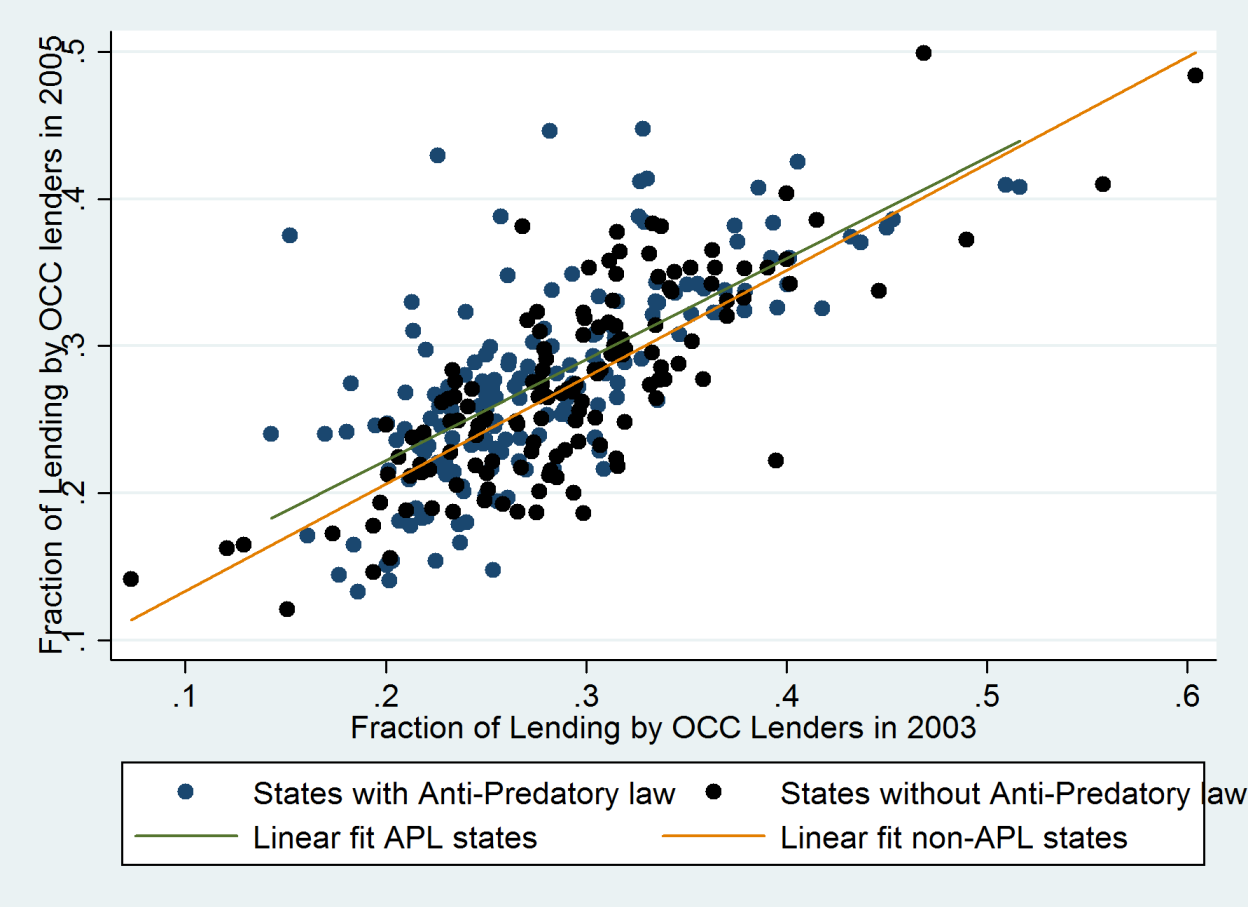

Figure 2 – The Relation between the Fraction of Lending Done by National Banks in 2003 and in 2005 for Each County

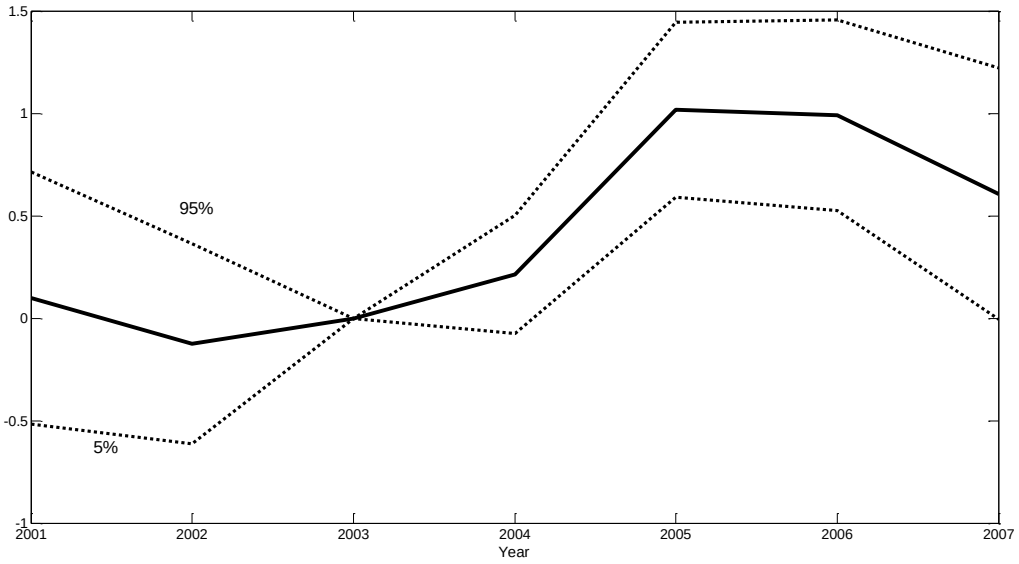

Figure 3- Time Series Coefficient for $\beta(\tau)$ in Equation (1).

Note: Coefficient for 2003 is normalized to zero.

Chapter 2
The Unintended Consequences of the Zero-Bound Policy

1 Introduction

In the aftermath of the financial crisis of 2007-2008, the Federal Reserve took an unprecedented decision to lower short-term nominal interest rates to zero, a policy commonly called a zero-bound policy. Although this decision had a positive intention of stimulating a faltering economic growth and boosting employment in the U.S. economy, it has also produced an adverse shock to competitiveness of money market funds. Since money market funds invest in instruments that offer rates that are close to the Fed funds rate, their gross profit margins collapsed nearly to zero and many fund investors were facing investment opportunities with guaranteed negative returns after paying their fund expenses. In this paper, we analyze the consequences of the interest rate policy for the behavior of money market funds.

The traditional business model of money market funds used to rely on the idea that such funds offer relatively low returns for the provision of ultimate safety. While this idea has been somewhat shattered with the collapse of the Reserve Primary Fund and the run on money market funds in September 2008, until then, money market funds offered positive rates to investors, even after taking into consideration fund expenses. The consequence of the unprecedented change in the interest rate to levels close to 0% has been that returns on traditional money market instruments, such as Treasuries, repos, or deposits declined to similarly low levels. As a result, any fund investing in such assets would guarantee negative net of expense returns to investors. Hence, it has become obvious that this business model cannot be sustained as money would flow out of funds with negative returns.

Such dire situation posed a dilemma for money market funds. On the one hand, they could accept the situation and keep their risk profile as is. This, however, would force them to first reduce or even waive their fees, and in the end, if the low-rates situation persist, to exit the market. On the other hand, funds could reach for yield by shifting their risk into securities with higher interest rates, thus accepting higher risk in their portfolios.

Money funds might increase the return offered to fund investors by means of reducing

expenses charged to investors, especially if they are part of larger financial institutions which can be willing to subsidize the operation of the money funds. However, funds which struggle to generate positive returns to their investors can also find it optimal to increase risk in their portfolios, which would then lead to an increase in fund returns and potentially fund flows. As has been documented in Kacperczyk and Schnabl (2013), money market funds face a typical positive flow-performance relationship. Given that fund compensation is an increasing function of flows such strategy would potentially improve the profitability of the funds themselves. Moreover, boosting fund net returns above zero would likely prevent the fund from ultimate distress and exit from the market. The cost of increasing risk, however, would be a higher chance of being run on in the event of distress in the money market industry. The consequence of such runs would be distress of individual funds themselves, which could generate high costs either in terms of the necessity to bail out the fund or through the significant loss of reputation for the fund organization and other related business centered on fund sponsor.

In this paper, we provide empirical evidence of the equilibrium response of money funds to the low interest rates environment. We exploit both a time-series and cross-sectional variation in the data to identify the effect of monetary policy on money market funds' strategies. Specifically, we first look at the effect of the Fed funds rate on the funds return, expenses, probability to exit from the industry, and the funds' incentives to take risk. We show that there exists a strong discontinuity in the effect of the Fed funds rate on these variables, because the Fed interest rate policy has no significant effect when the rate is above one percent, but it becomes very significant both economically and statistically in a low interest rate regime. In particular, we find that a reduction in interest rate leads to a significant increase in risk-taking incentives, a reduction in charged expenses, an increase in fund subsidies, and an increased probability to exit from the market.

Moreover, consistent with the differential incentives shaped by reputational concerns, we find that the reaching for yield phenomenon is particularly pronounced for independent

funds, that is, funds that are not affiliated with a commercial bank, an investment bank, or an insurance company. In contrast, rather than ramping up risk, affiliated funds are more likely to take the path of exiting the market altogether. At the same time, we do not find significant differences across fund types in terms of their expense policy.

To avoid any contamination of our results from other concurrent macro events and further sharpen our empirical identification, we next turn to evidence from event studies related to an unprecedented zero-bound interest rate policy introduced by the Fed in the aftermath of the financial crisis. In particular, we study the money fund response to four FOMC announcements, which signaled that interest rates would be kept low in the future. Those include the original decision in December 2008 to lower Fed funds rate to zero, and subsequent four decisions of forward guidance policies that provided more details on how long the bound would be maintained. Important, these decisions were unlikely endogenous with respect to the behavior of money funds. Within these event windows, we compare the money funds' risk choices, exit decisions, and expense ratios both in the cross-section and in the time series.

We find that in the period of three to six months after these announcements, there is a significant increase in the probability of exit from the fund industry, fund risk taking increases, as measured by the funds' spread and by the fraction of portfolio invested in riskier asset classes, and expenses charged by money funds go down and the fund subsidies go up. Interestingly, while we do not find any variation in the expenses incurred by these funds over time, the expenses charged are significantly reduced during a zero interest rate period. This result suggests that money funds were actively trying to maintain their net returns positive as a way of keeping their business alive and did not simply witness a period of lower operating costs altogether. We also find significant differences in the cross-section of funds consistent with our previous findings. Following the FOMC decisions, funds that are affiliated with an independent sponsor take on relatively more risk but are less likely to leave the money fund industry. At the same time we do not observe significant differences

in the cost policy across the two groups. These results are consistent with the explanation in which reputational concerns shape up the strategic decisions of money funds.

Altogether, our results suggest that the unconventional monetary policy implemented during the financial crisis by the Fed might have produced unintended consequences regarding the fragility of an important part of financial markets, the market for short-term financing and shadow banking. Specifically, low interest rates can decrease rather than increase the liquidity available for banks, as one of the main sources of such wholesale funding, the money market funds, are incentivized to invest in riskier securities when they are not forced to exit the market altogether.

Furthermore, the results highlight one important channel for monetary policy that has been completely overlooked by the academic literature, but that is extremely relevant for practitioners and policy makers. For instance, in August 2009 Fitch released a report about U.S. money market funds stating that "Over the longer term, more conservative portfolio composition, combined with the current low interest rate environment, may result in fund closures, fund consolidation, and/or a resurgent appetite for credit and liquidity risk."

In the same spirit, a recent article on the Financial Times summarized the risks associated with the zero interest rates as follows:

"The risks are that as rates will plunge to zero or negative, money market funds and their investors would panic as their sources of yield disappeared, and that banks will follow Bank of New York Mellon's lead last year and consider the possibility of charging fees on deposits. Money market funds would likely be subsidized for a time by their sponsors, but that can't be counted on to the extent that it was before the crisis. Were this to pass, we couldn't with any certainty predict the consequences—but given the panic that ensued when Reserve Primary broke the buck, it's worth taking none of this lightly."

The rest of the paper proceeds as follows. In Section II, we discuss the related literature. Section III provides further details about the institutional setting of money market funds. The empirical design and data, as well as the empirical results are discussed in Section IV

and Section V, respectively. Section VI concludes.

1.1 Related Literature

Several papers have studied the role of money market funds during the financial crisis, the European debt crisis, or outside the crisis. These include Christoffersen (2001), Christoffersen and Musto (2002), Baba, McCauley, and Ramaswamy (2009), Duygan-Bump, Parkinson, Rosengren, Suarez, and Willen (2010), McCabe (2010), Adrian, Kimbrough, and Marchioni (2011), Kacperczyk and Schnabl (2013), Strahan and Tanyeri (2012), Wermers (2012), Chernenko and Sunderam (2012), and Di Maggio (2012).

To the best of our knowledge ours is the first paper to examine the role of monetary policy in affecting the risk-taking incentives of money fund managers. In this regard, the closest papers to ours are Kacperczyk and Schnabl (2013), Strahan and Tanyeri (2012), and Di Maggio (2012) all of which investigate the risk-taking incentives of money market funds before and after the collapse of Lehman Brothers, and during the European debt crisis, respectively. The key difference between these papers and ours is that we examine the role of the monetary policy to quantify the incentives to reach for yields, to adjust fund expense policy, and to moderate the entry and exit dynamics of the money fund industry.

More broadly, our paper sheds new light on the incentives for asset managers to reach for yield, which has been identified as one of the core factors contributing to the buildup of credit that preceded the financial crisis (Rajan (2010), Yellen (2011), and Stein (2013)). There might be several explanations of the reaching-for-yield phenomenon. It could be driven by competition among fund managers, or it could be a reflection of different preferences for risk or a desire to offset constraints imposed by regulation. The contribution of this paper is to provide a setting in which the incentives to reach for yield are on the one hand limited by strict regulation, yet on the other hand they are significantly affected by changes in interest rates and expectations about future changes.

Our paper is also related to studies on the importance of monetary policy for asset prices. Several papers investigate the response of the Treasury rates and asset prices to the FOMC announcements (see among others, Krishnamurthy and Vissing-Jorgenson (2011), Gürkaynaka, Sack, and Swanson (2005)) and of the term structure of interest rates to monetary policy surprises. We contribute to this literature by examining the role of monetary policy in shaping fund managers' incentives to reach for yield.

Finally, our paper has important implications for the vast literature on conducting the zero interest rate monetary policy (for summary, see Woodford (2003), and Bernanke, Reinhart, and Sack (2004)). In this regard, we highlight an important instability outcome that might be triggered by conducting the zero-bound policy.

2 The Institutional Setting: Money Market Funds

Money market funds (MMFs) are important intermediaries between investors who want low-risk, liquid investments and banks and corporations that have short-term borrowing needs. The funds are key buyers of short-term debt issued by banks and corporations: commercial paper, bank certificates, and repurchase agreements, with an aggregate volume of $1.8 trillion. Given the importance of short-term credit markets to both investors and businesses, any disruption represents a potential threat to financial stability. MMFs have recently drawn attention of academics as illustrated by a strand of literature exploring their behavior during the financial crisis in 2007-2009 (Kacperczyk and Schnabl (2013) and Gorton and Metrick (2012)) and the more recent Sovereign debt crisis (Chernenko and Sunderam (2012) and Ivashina, Scharfstein, and Stein (2012)). We contribute to this literature by showing the impact of the zero interest rates on the funds' trading, cost, and exit strategies.

Money market funds emerged in the 1970s as an alternative to bank deposits. At that time, bank deposits were highly regulated and paid lower interest rates than did money market instruments, which made money funds attractive to investors as they paid higher

interest for taking on comparable risks. Even though the regulation of bank deposits was eventually abolished, the size of the fund industry grew steadily over time up to $2.4 trillion at the beginning of 2007 (see Federal Reserve Flow of Funds Data).

An important characteristic of money funds is that, contrary to bank deposits, investments in the funds are not insured by the government. But, contrary to regular mutual funds, money funds seek to preserve the value of their assets at $1 per share. They do so by using historical cost accounting, rather than market value pricing, to assess the value of their holdings. This allows them to sell demand deposits that are considered almost as safe as bank deposits. The downside of this approach is that it exposes them to runs. If the market value of a fund's holdings is expected to drop below its amortized cost, investors tend to redeem their shares, which can exacerbate the market value drop due to forced liquidation at re-sale prices. Also, funds may suffer losses on their investments because of changes in interest rates or individual securities' defaults.

In the United States money market funds' holdings are regulated by Rule 2a-7 of the Investment Company Act of 1940. The funds are prohibited from purchasing long-term assets such as mortgage-backed securities, corporate bonds, or equity and can only hold short-term assets; and even these short-term liabilities must be of high quality. As an additional requirement, to enhance diversification, the funds cannot hold more than 5% of their assets in the securities of any individual issuer with the highest rating and not more than 1% in the securities of any other issuer.

In January 2009, after a tumultuous year for money market funds, the SEC voted to amend the 2a-7 rules to strengthen money market funds. The new rules seek to limit the risk and improve on fund disclosure. For instance, funds are now required to have enhanced reserves of cash and readily liquidated securities to meet redemption requests and they can invest only 3 percent (down from 5 percent) of total assets in tier-2 securities, the term on which is limited to a maximum maturity of 45 days.

Under the new rules, starting in November 2010 money market funds have make monthly

disclosure of detailed data, including each fund's holdings and shadow net asset value (NAV). This information becomes available to the public after 60 days. The new N-MFP form on which it is filed constitutes one the main sources of data for the present study.

3 Research Design and Data

3.1 Research Design

In our empirical tests we aim to identify the role of the interest rate policy for money funds' risk-taking behavior, the expense policy, and the likelihood to exit from the market. To this end, we entertain two empirical strategies. In our first test, we examine the consequences of the changes in the Fed funds rate over the longer period of 2005-2013. We restrict our attention to the period of January 2005–December 2013 in order to have a relatively balanced panel around the time in which the zero-bound policy was introduced. This period includes two distinct interest rate regimes: A regime in which the rate is higher than zero percent (2005-2008) and a regime with zero interest rates (2009-2013). As Figure 1 indicates, in the first regime the interest rate had been gradually going up from 2% at the beginning of 2005 to 5.25% in the middle of 2007 and then subsequently going down to 0-0.25% by the end of 2008. The second regime has been manifested by a continuous zero interest rate policy (in fact, the rate has been cut to zero on December 16, 2008).

Our identification in this test comes from the various changes in interest rates over the sample period. In addition, we examine the differences in fund behavior in periods of high rates and low rates. In particular, we are interested to learn whether approaching zero bound alters fund incentives in a significant way relative to other periods. To this end, we study fund responses separately in periods with interest rates higher than 1% and in periods with rates equal to or less than 1%.

In our second test, we refine our empirical strategy by looking only inside the zero interest-rate regime. Our analysis of that sub-period allows us to keep the level of interest rates constant at zero and further explore the importance of the additional communication from the Federal Reserve regarding the duration of the zero-rate policy. In our context, the duration of zero rate policy is crucial as it directly determines how long the money market fund business is subject to profit stress. In particular, one could imagine that short-lasting policy would have different equilibrium implications as money funds could withstand temporary headwinds by taking short-term losses. The situation differs when the pressure is held for longer time.

In our analysis, we concentrate on the money fund behavior around events related to FOMC meetings during which at least one of the following outcomes occurred: (1) a change in the interest rates, (2) forward guidance announcement. Table 1 provides a short description of the events in a chronological order. The first event date is December 16, 2008, which is the date of the meeting at which the Fed funds rate was cut to 0-0.25%, while the other four event dates capture the meetings in which the Fed gave its forward guidance regarding the duration of the zero-rate regime. Specifically, on March 18, 2009, the Fed announced that the rates will be zero for "an extended period of time", while on August 9, 2011, January 25, 2012, and September 13, 2012 the Fed stated that the rates will remain at zero until 2012, 2014, and 2015, respectively.

Our event-study analysis requires constructing reasonable windows around event dates. Given that various money funds decisions can be adapted with different speed we consider two horizons: a short-term horizon of three months after the event and a long-term horizon of six months after the event. In both cases, the pre-event window is set at one month as we want to ensure that no pre-event trends drive the patterns in our data. Our empirical strategy is to compare the average fund behavior around the event date.

In both empirical tests, we also exploit cross-sectional differences across money market funds. In particular, we distinguish between funds whose sponsors are affiliated with a

large financial institution, such as commercial bank, investment bank, or insurance company and funds whose sponsors are affiliated with an independent asset management company. We believe the two groups might exhibit distinct responses to the events of interest rate changes. For example, the bank-affiliated funds might exhibit weaker incentives to reach for yield than independent funds to limit the probability of the bad outcome in which the bank would be forced to invest resources to save the fund. Independent funds, in turn, have stronger incentives to reach for yield in order to provide investors with higher returns, which should compensate the investors for giving up the implicit insurance of the bank. Moreover, bank-affiliated funds might have reputation at stake in which case they might prefer to exit the less risky yet unprofitable fund industry rather than improve its profitability by ramping up risk.

3.2 Data

We collect data from four sources. First, we obtain data on the universe of taxable money market funds from iMoneyNet, which cover the period from January 2005 to December 2013 and include weekly fund-level data on yields, expense ratios (charged and incurred), average maturity, holdings by instrument type, and fund sponsor. Second, we complement the data with information from the CRSP Mutual Fund Database, especially assets under management of the fund sponsor. Third, we use COMPUSTAT and companies' websites for information on fund sponsor characteristics. Similar data, though for a different time period, have been already used and additionally discussed in Kacperczyk and Schnabl (2013). Fourth, we gather detailed information about Fed funds rate changes and the forward guidance policy from the Federal Reserve Board website.

We conduct our analysis at the fund portfolio level. We therefore aggregate all share classes by fund and type of investor (retail, institutional). We compute fund characteristics (e.g., expense ratio) as the weighted average with assets per share class as weights. Some

funds offer both retail and institutional share classes. Institutional shares are generally larger; hence, we classify a fund as institutional if it offers at least one institutional class and as retail if it does not offer institutional share classes.

Table 2 provides the summary statistics of the data. In columns (1) and (2), we provide information about mean and standard deviation of various fund and sponsor characteristics in the entire sample period. Our sample includes 349 different fund portfolios. The average fund size in our sample equals approximately $8.3 billion. The average portfolio maturity is 40 days and the average fund age equals 15.8 years. The average Fed funds rate in our sample equals 182 basis points while the average gross fund return equals 231 basis points. Out of the abnormal profit of 48 basis points 38 basis points account for expenses, which leaves about 10 basis points accruing to fund investors. Notably, our sample is quite balanced with respect to sponsor type as 59% of funds have bank-affiliated sponsors and 41% are sponsored by independent asset management companies.

In the subsequent four columns, we compare sample properties between the high-rate and low-rate regimes. This sample split is based on the median interest rate equal to 1% and reflects our view of what we consider a period of profit stress. Comparing the two sub-samples we note a number of interesting patterns. First, the spread during the low-rate period is 25% lower than that in the high-rate period; also, the nominal gross return was almost ten times larger in the high-rate period. This suggests that money market funds faced greater challenges in obtaining high returns in a low interest rate environment. Second, if we look at the expenses charged they are significantly lower in the low-rate period, with a drop from 50 to 28 basis points, while the expenses incurred remain almost the same. This suggests that while the costs were not affected by the monetary policy, the stress imposed on the profit margin reduced the possibility for the fund to charge fees to the investors. In other words, funds were more likely to offer subsidies to their fund investors. Third, while fund flows are positive during the earlier period, they become negative in the low interest rate environment. This is consistent with the idea that investors have become less willing to

make investments in money market funds as their returns became less attractive. Finally, we observe a significant decline of more than 50 in the number of funds over the two periods: from 326 to 274 funds in the second period, which constitutes a significant exit from this sector.

In the last four columns of Table 2 we focus only on the period of low interest rates and report separate summary statistics for two major groups of funds: bank affiliated and independent. Bank-affiliated funds are defined as funds sponsored by a commercial bank, an investment bank, or an insurance company. Bank-affiliated funds are on average smaller with the difference of about $3 billion. They are also less risky as their spreads are lower; they invest in shorter maturity assets as well as in safer assets as repos and Treasuries. They also charge slightly lower expenses, and they face higher outflows, consistent with the flow-performance relationship observed in other studies (Chevalier and Ellison (1997), and Kacperczyk and Schnabl (2013)).

3.3 Empirical Results

In this section, we present the main results of the paper. First, we illustrate the importance interest rates play in generating fund returns and illustrate the link between fund returns and subsequent fund flows. Next, we look at the effects on risk taking, fund exit, and expenses of changes in the Fed funds rate near the zero interest rate bound and compare them to any other changes in interest rates before the zero-bound policy. Finally, we zero in on the effects within the low interest rate policy and examine similar behavior around the FOMC interest rate and forward guidance announcements.

4 The Effect of the Fed Funds Rate Changes

4.1 Flow-Performance Relationship

We begin our analysis by identifying the link between interest rate and gross fund return. To this end, we estimate the regression model of fund returns (Fund Return) on the Fed funds rate over the entire sample period. In this regression model, we control for other determinants of fund returns possibly correlated with the level of interest rates, such as the natural logarithm of fund size (Log(Fund Size)), the natural logarithm of fund family size (Log(Family Size)), the level of expenses charged by funds (Expenses), the natural logarithm of fund age (Age), the percentage change in fund assets accounted for capital appreciation (Fund Flow), the standard deviation of fund flows (Fund Flow Volatility), and an indicator variable for the fund that is institutional (Institutional). Further, we account for any time-invariant fund and sponsor characteristics by introducing fund-fixed and sponsor-fixed effects. To address a potential concern that interest rates do not vary across fund observations within given week and thus Fed funds rate might simply proxy for the time trends in the data we also include year-fixed effects. Finally, we cluster standard errors at the time dimension to address the possibility that interest rates are identical across fund observations. We report the results in Table 3.

Our results show that fund performance is higher in periods of higher interest rates. The effect is statistically and economically highly significant. Specifically, a one-standard-deviation increase in the interest rate leads to an increase of 128-194 basis points in the fund return, more than 60% change in terms of one standard deviation of fund returns. This result underscores the importance of interest rate regimes for generating fund performance.

In the next test, we show that generating superior performance has important implications for fund flows and hence for fund manager compensation. To this end, we estimate the standard flow-performance relationship. Kacperczyk and Schnabl (2013) demonstrate the

presence of such a relationship in the pre-Lehman period for the subsample of institutional money funds. Here we examine this relationship for all prime funds over the longer time period and separately for the high-rate and low-rate regimes. Our set of controls mimics that used in Table 3. However, given the nature of the data in some specifications we account for the finer week-level variation by introducing week-fixed effects. In all regressions we cluster standard errors at the fund sponsor level. Table 4 reports the results.

In column (1) we report the results for the full sample of money funds. We confirm the findings from earlier studies that investors exhibit strong sensitivity to fund past returns. The coefficient of Fund Return is positive and highly statistically significant. It is also economically significant: A one-standard-deviation increase in Fund Return results in a fund flow of about 1.9% per week, which is approximately 40% of the standard deviation of fund flows in the data. In terms of fund size, it means over the year a fund with a one-standard-deviation higher return than the average would almost double its size relative to the average fund.

In column (2), we also show that fund flows are positively correlated with the Fed funds rate, which is a direct consequence of our earlier findings in Table 3. In column (3), we further show that the flow-performance relationship does not depend on the level of interest rates, as the coefficient of the interaction term Fund Return*Fed Rate is statistically insignificant. This result however is obtained without properly accounting for week-fixed effects and does not account for potential nonlinearities in which interest rates affect the sensitivity. To this end, in columns (4) and (5) we further provide estimates of the flow-performance relationship separately for the period with high interest rates and low interest rates.

At first glance, it seems that the flow-performance relationship is stronger in periods of lower interest rates as the coefficient of Fund Return is about 30% larger in that sample. Hence, we conclude that the changes in interest rates altered considerably the payoff per unit of extra performance. What has also changed is the level of fund performance which made investors less attracted to money funds and triggered significant fund responses in terms of

their exit, risk-taking, and expense strategies. We now discuss each of these adjustments.

4.2 Adjustments along Exit, Risk-Taking, and Expense Strategies

Since interest rate environment directly affects profit opportunities it is becoming increasingly more difficult for funds to operate profitably as the interest rates approach zero rate bound. Consequently, they might need to adjust along various margins, such as their exit strategies, their risk, and expense policy.

We begin by analyzing the effect of the Fed rate on the number of active funds and on the probability of their exit. To this end, we estimate the regression model for each of the two dependent variables on the level of Fed funds rate using a full sample of funds and two sub-samples based on the level of interest rates. Apart from the standard controls we used before, all regressions include year/month-fixed effects and sponsor-fixed effects. We cluster standard errors at the week level. Table 5 reports the results.

Overall, we find little effect of higher interest rates on exiting or attrition in this industry in the full sample as highlighted by columns (1) and (4). However, significant differences emerge when we compare results across two interest-rate regimes. While we again find little effect of interest rate changes on exit strategies in the high-rate regime, as evidenced in columns (2) and (5), we find that the changes in interest rates from 1% towards zero have a very important implications for fund exit, as demonstrated in columns (3) and (6). In particular, we find that by reducing the Fed rate from 1% to 0% the number of funds in our sample decreases by about 11.3, and the probability of exiting from the industry increases by 7.2%. Both effects are highly significant both statistically and economically. Notably, in this specification, we include very fine year/month-fixed effects, which makes it highly unlikely that we are picking up the time trend, or the effect of some unobserved macro shock.

We next turn to the funds' incentives to take risk. We use four different measures of risk, similar to those used in Kacperczyk and Schnabl (2013). Spread is the difference between

Fund Return and the rate on Treasury bill; Holdings Risk is a long-short portfolio invested long in the riskiest asset class (bank obligations) and short in the safest asset class (Repos and U.S. Treasuries and Agency assets); Maturity Risk is the weighted average maturity of the fund; Concentration is a Herfindhal index of the portfolio holdings in risky assets, such as commercial paper, asset-backed commercial paper, floating-rate notes, and bank obligations. Higher values of each measure indicate a greater degree of risk taking. We estimate the regression model in which the dependent variables are various risk measures and the main independent variable is Fed Rate. As before, all regressions include year/month-fixed effects and sponsor-fixed effects, and standard errors are clustered at the week dimension. We report the results in Table 6. Given our interest in the effects of low-rate policy we only report results for the low-rate regime; the high-rate regime results are significantly weaker and omitted for brevity.

We find a statistically significant positive effect of reducing Fed Rate on the level of risk for three out of four risk measures. The effect is negative, but statistically insignificant for Maturity Risk. In terms of economic values, a reduction in the Fed Rate from 1% to 0% increases Spread by almost 95 basis points Holdings Risk by 6.8 percent, and Concentration by 2.4%. These are sizable effects, especially for the money funds with returns close to zero.

Our last dimension of adjustment is the expense policy. It is apparent that in the wake of low interest rates and thus low returns fund companies might want to maintain their client relationship by reducing the fees charged to their investors, effectively increasing these investors' net returns. We want to contrast this behavior with the expenses that are truly incurred by the funds which are more difficult to adjust. In addition, we measure the degree of subsidies that funds offer to their investors by taking the difference between incurred and charged expenses. We estimate the regression model in which Charged Expenses, Incurred Expenses, and Subsidy are our dependent variables and Fed Rate is our main independent variable. All other controls are the same as before. However, in contrast to previous regressions, we cluster standard errors at the sponsor level since fund expenses seem to be quite

persistent over time and thus this dimension of dependence produces more conservative standard errors. Table 7 reports the results from the estimation for the full sample and the two sub-samples of high and low rates.

The results, in columns (1), (4), and (7), indicate a general negative effect of interest rates on fund expenses, both charged and incurred, and a positive effect on fund subsidies. However, this result is largely driven by the high-rate regime (columns (2), (5), and (8)), that is, fund expenses are generally lower when interest rates are higher, perhaps because funds have generally higher profit margins and do not need to charge much for their service. However, the opposite is true when interest rates approach zero bound: Lower rates reduce the expenses charged by the funds. As the Fed Rate goes down from 1% to 0% funds charge 7.6 basis points less for their service, as presented in column (3). This reduction occurs despite the fact that fund incurred expenses are generally unaffected by the interest rate change, as demonstrated in column (6). This asymmetric response in expenses is equivalent to an increase in fund subsidies offered to investors. As presented in column (9), a decrease in Fed Rate from 1% to 0% increases fund subsidy by an economically large 6.6 basis points. This effect is statistically and economically highly significant.

4.3 Evidence from the Cross-Section of Funds

So far, we based our results on the time-series identification strategy, that is, we compared changes in fund behavior across different levels of the Fed funds rate. In this section, we further buttress our identification strategy using cross-sectional variation in the incentives to respond to profit margin squeeze across different fund sponsors. As we have demonstrated, fund sponsors might want to respond to margin deterioration either by changing their exit, their risk, or expenses strategies. However, these responses also might depend on other concerns. In particular, fund sponsors with greater reputation concerns at stake or greater ability to provide funding in case of the run might want to internalize these negative spillovers

by either taking less risk or leaving the industry altogether. They might also entertain different pricing strategies. In our sample, we postulate that the important dimension of differences is whether the fund is sponsored by a financial institution (with large concerns and greater ability to pay) or is sponsored by an independent asset management company (usually with less reputation concern and smaller ability to pay).

We explore these differences empirically using a difference-in-differences regression approach. In particular, we want to compare exit, risk taking, and expenses strategies for funds with different sponsor type across High-Rate and Low-Rate regimes. To this end, we define two indicator variables: Independent Sponsor equal to one if the sponsor is an independent company and zero if it is a financial institution; Low Rate equal to one if the Fed Rate is at most 1% and zero if the rate is above 1%. The differential effect of the change in the interest rate regime across two fund types will be measured by the coefficient of the interaction term Independent Sponsor* Low Rate.

In Table 8, we present the results for the number of funds and exit strategies. All regressions include year/month-fixed and sponsor-fixed effects. We find a positive and statistically significant effect of the interaction term for the number of funds: There are relatively more funds sponsored by independent companies in the low-rate regime. At the same time, the results indicate little evidence of differences across fund types in terms of their exit strategies.

Table 9 reports the results for the risk measures. In all regressions, we include week-fixed effects; thus eliminating any possible effects due to time trends. Consistent with our hypothesis that independent funds might have less at stake in terms of their risk taking, we find that such funds, on average, take on more risk when the monetary policy shifts to lower interest rate regime. This effect is statistically and economically large for all four measures of risk. Specifically, relative to affiliated funds, the spread of independent funds increases by 2.4 basis points, holdings risk increases by 7.3 basis points, funds maturity risk increases by one third of the standard deviation, or equivalently by 4.5 days on average, and concentration increases by 3.8 percentage points. In all regressions, the coefficient of Independent Sponsor

is not statistically different from zero, which suggests the two groups of funds are similar to each other in high-rate periods.

We entertain similar tests for two measures of expenses and subsidy and report the results in Table 10. Although independent sponsors are on average less likely to subsidize their funds, we find no statistically significant differential effect between the independent funds and the bank-affiliated funds in terms of expense strategies. These results suggest that even though funds, in general, lower their expenses in the low-rate regime they do not execute this strategy in economically distinct ways.

4.4 Fund Strategies around the FOMC Announcements

Up to this point we have analyzed the effect of Fed funds rate on different outcome variables over different sample periods, also taking advantage of the cross-sectional differences across fund sponsors. In this section, we turn into our second empirical strategy in which we evaluate fund behavior, both in the time series and in the cross-section around the important decisions taken by the FOMC during the low interest rate period. As we have argued before there are five events in which the Fed has signaled important information regarding the level and duration of the zero interest rate policy. We study the changes in fund behavior before and after such events, also conditional on fund sponsor type. Our main variable in all tests is Event which is an indicator variable equal to one for the period after the event date and zero beforehand.

In our analysis, we consider two types of windows: We first look at the interval between one month before and three months after the event, then we investigate the effect over a longer time interval as we compare the outcome over the six months after the event compared to the month before the event. We restrict attention to one month before the event to avoid any contamination with other potential events. We analyze these two types of windows because there are strategies that the funds can immediately alter, such as the riskiness of

their portfolio or expense policy, but also other strategies for which we might not observe any effect for an extended period of time, such as the exit from the market.

We begin with the analysis of the exit strategy. Panel A of Table 11 reports the results. We find that, on average, three funds drop after the event both in the shorter and longer time window. Similarly, we find that the probability of exiting the industry increases significantly in both horizons following the event. We have reported the results for all the events together, but we find consistent results for each event separately. Moreover, later events are more important than early ones, perhaps the forward guidance policy extended the low-rate regime into a longer future with more certainty. In sum, our results indicate that there is indeed tension in the industry generated by the monetary policy.

Panel B shows a similar event study analysis but conditional on the sponsor type. The incremental effect of change with respect to sponsor type is measured by the coefficient of the interaction term Event*Independent Sponsor. We find that the independent funds are more likely to stay and less likely to exit because these are the funds which are less wary of any reputational concerns due to runs. Instead, these funds offset their profits' squeeze by increasing their risk taking to attract more investors. The result is particularly strong for the longer six-month window, which is consistent with our premise that adjustments, such as exit might take longer to materialize.

Next, we perform a similar analysis for the risk-taking incentives. The results are reported in Panel A and Panel B of Table 12. We find that as a result of strengthening the policy of the zero interest rate, three out of four measures of fund riskiness increase. The only one that in fact goes down is Maturity Risk. But this is likely driven by the provision in the Dodd-Frank Act which implemented a significantly higher lower bound for the fraction of assets maturing within the next seven days that money market funds need to hold. The results in the cross-section of funds, reported in Panel B, generally paint a picture that funds sponsored by independent asset management companies take on more risk following the change in the interest rate policy. This result is true again for the same three measures

of risk as before. Overall, these results are consistent with our earlier finding that lowering interest rates makes such funds reach for yield relatively more.

Finally, we investigate changes in the expense policy. The results are reported in Panel A (for the time series) and Panel B (for the cross-section) of Table 13. Consistent with the previous findings, we find that the expenses charged (fund subsidies) get reduced (increased) following the FOMC events. These effects are particularly strong for the longer, six-month window, which might reflect some stickiness with which fund companies generally respond in terms of their expense policies. Again, we find no differences across fund types in terms of their expense policies. Notably, many funds often waive their expenses to investors, especially in the low-rate regimes, another dimension of adjustment which can be used to alleviate the pressure due to low interest rate policy.

To the extent that the FOMC events trigger attrition in the fund industry the worry is that our results for risk taking might be mechanically different by differences in the sample selection before and after the events. To the extent that safer funds were more likely to exit we could observe an increase in average risk taking even though individual funds might not change their policies at all. We address this concern by focusing on a subset of funds that are present in both periods around the event date. In Table 14, we present the results from the estimation of regression models similar to those in Table 12. The results we find are qualitatively and quantitatively very similar to those reported before; thus, our results are unlikely to be driven by the differential selection within the event window.

In sum, our results corroborate our earlier findings that money funds respond to pressures in their business due to unusually low interest rates either by increasing the riskiness of their portfolios, lowering (increasing) the expenses (subsidies) they charge to their investors, or leaving the fund industry altogether. The strength of these effects tends to vary with the fund sponsor, especially when it comes to dimensions of risk and exit.

5 Conclusion

This paper investigates the consequences of the zero interest rate policy on the money market funds' behavior. The monetary policy has a direct effect on money market funds as these primarily invest in asset classes whose returns are linked to the Fed funds rate. This constitutes a great setting to explore the unintended consequences that monetary policy may have on financial markets.

We uncover novel empirical evidence showing that the low interest rate regime leads fund managers to increase the risk of their portfolios in order to generate positive returns. The effect is more pronounced for funds sponsored by asset management companies. Almost all funds significantly reduced their expenses charged, even if the incurred expenses did not vary much over time, as an attempt to deliver non-negative net returns to their investors. These subsidies amounted to an economically large value of $27 million per average fund and about $7.3 billion for all funds. We also show that the funds that are not successful in retaining their investors' base, or are worried about possible negative reputation spillovers, are more likely to leave this market as a response to the cuts in interest rates.

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Figure 1: Fed Funds Rate: 2005-2013

The figure presents the evolution of Fed funds rate over the period 2005-2013.

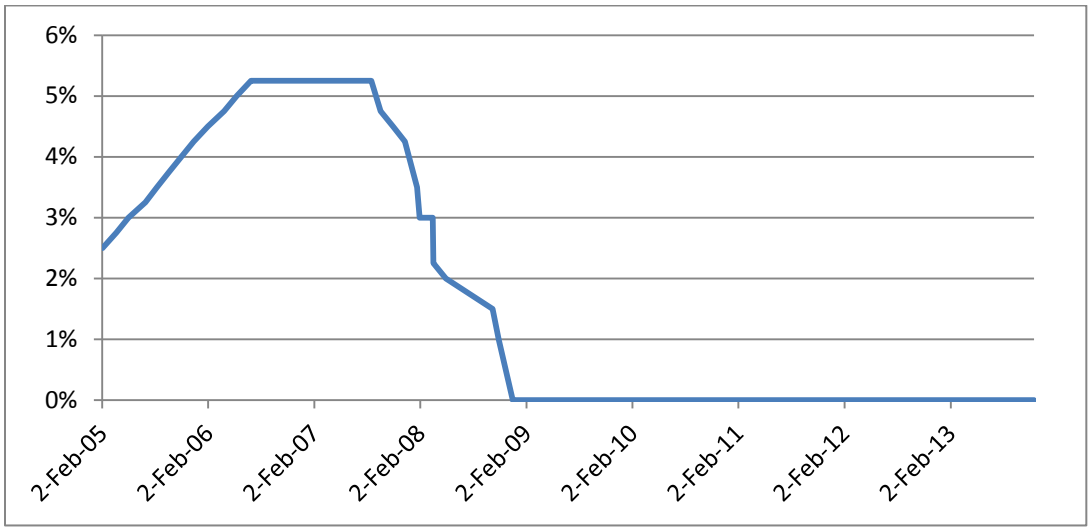

Table 1: Zero Interest Rate Policy Events (ZIRP)

We report the dates of FOMC meetings in which the Fed decided to change the Fed funds rate or provided policy guidance about the prevailing zero interest rate policy.

Date	Event
December 16, 2008	Fed funds rate reduced to 0-0.25
March 18, 2009	Zero rates for extended time period
August 9, 2011	Zero rates at least until 2013
January 25, 2012	Zero rates at least until 2014
September 13, 2012	Zero rates at least until 2015

Table 2: Summary Statistics

The sample is all prime money market funds. The data span the period January 2005-December 2013. The first two columns provide the results for the entire sample over the full period. The subsequent two columns (High Rate) restrict the sample to the period of high interest rates (Fed funds rate greater than 1%). In the next two columns (Low Rate), we restrict the sample to the period of low interest rates (Fed funds rate between 0 and 1%). The following four columns focus on the low-rate regime and summarize the data of funds whose sponsors are affiliated with a financial institution (Affiliated) and for funds whose sponsors are independent asset management companies (Independent).

Variable	Unconditional		High Rate		Low Rate		Low Rate: Affiliated		Low Rate: Independent	
	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.	Mean	St. dev.
Fed Rate (in %)	1.83	2.02	3.65	1.31	0.06	0.15	0.06	0.15	0.06	0.15
Fund Return (in bps)	231.07	206.28	419.13	114.58	46.95	53.23	46.41	52.78	47.72	53.84
Spread (in bps)	47.24	53.84	53.85	61.78	40.74	43.74	40.14	43.14	41.60	44.56
Holdings Risk (in %)	-8.30	27.98	-5.13	26.19	-11.37	29.29	-14.34	27.32	-7.17	31.38
Maturity (in days)	40.29	13.05	39.55	12.85	41.00	13.21	39.12	12.77	43.66	13.36
Concentration (in %)	27.63	17.52	31.91	18.43	23.50	15.52	23.37	16.36	23.69	14.25
Expenses Charged (in bps)	38.54	25.38	49.69	27.98	27.63	16.37	27.16	16.60	28.29	16.02
Expenses Incurred (in bps)	56.99	32.38	58.36	32.62	55.65	32.10	53.74	28.30	58.34	36.63
Subsidy	18.34	27.33	8.50	20.50	27.97	29.65	26.53	25.58	30.01	34.48
Fund Size	8303	21651	7083	17173	9481	25176	8102	24211	11430	26356
Family Size	151599	253903	122320	204508	182160	293725	113790	118291	279344	415854
Age (in years)	15.80	7.86	13.40	7.04	18.13	7.91	17.28	7.87	19.33	7.81
Fund Flow (in %)	0.05	4.78	0.26	5.24	-0.16	4.28	-0.19	4.64	-0.12	3.70
Fund Flow Volatility (in %)	3.66	3.86	4.12	4.27	3.21	3.37	3.57	3.52	2.70	3.08
Independent Sponsor (in %)	40.92	49.17	40.37	49.07	41.44	49.26	0	0	100	0
Bank Affiliated Sponsor (in %)	59.08	49.17	59.63	49.07	58.56	49.26	100	0	0	0
Sponsor Equity	10117	17052	9955	16742	10275	17345	16580	19918	1374	5492
U.S. Treasuries & Agency	0.10	0.16	0.07	0.13	0.13	0.18	0.13	0.16	0.15	0.20
Repurchase Agreements	0.13	0.15	0.11	0.15	0.14	0.16	0.16	0.16	0.13	0.15
Bank Deposits	0.02	0.06	0.02	0.06	0.03	0.07	0.03	0.06	0.02	0.07
Bank Obligations	0.15	0.14	0.13	0.13	0.17	0.15	0.14	0.14	0.20	0.15
Floating-Rate Notes	0.18	0.17	0.20	0.17	0.17	0.17	0.17	0.18	0.16	0.15
Commercial Paper	0.30	0.22	0.34	0.24	0.26	0.19	0.26	0.19	0.26	0.19
Asset-Backed Commercial Paper	0.11	0.14	0.13	0.15	0.10	0.12	0.11	0.12	0.08	0.11
Institutional Funds (in %)	45.94	46.76	45.73	46.71	46.14	46.81	47.63	46.81	44.02	46.72
Number of Funds	349		326		274		159		115	

Table 3: Fund Returns and Fed Funds Rate

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variable is *Fund Gross Return* computed as the annualized return. *Fed Rate* is the annualized Fed funds rate. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow growth, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level. Column (1) includes year-fixed effects, column (2) includes fund-fixed effects, column (3) includes sponsor-fixed effects, and column (4) includes year-fixed and sponsor-fixed effects. Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Fund Gross Return			
Fed Rate	62.291***	93.025***	94.370***	62.291***
	(5.087)	(1.012)	(0.963)	(5.087)
Log(Fund Size)	1.260***	16.683***	5.170***	1.260***
	(0.061)	(1.292)	(0.361)	(0.061)
Log(Family Size)	1.038***	3.556***	5.754***	1.038***
	(0.095)	(0.779)	(1.034)	(0.095)
Expenses	20.641***	118.308***	86.409***	20.641***
	(1.957)	(8.642)	(6.641)	(1.957)
Log(Age)	0.453	-5.946***	-8.508***	0.453
	(0.331)	(1.382)	(1.358)	(0.331)
Fund Flow	21.525***	10.017	15.402*	21.525***
	(5.265)	(8.677)	(9.193)	(5.265)
Fund Flow Volatility	-32.930***	104.429***	98.761***	-32.930***
	(4.181)	(13.735)	(13.816)	(4.181)
Institutional	4.842***	-9.791***	13.391***	4.842***
	(0.353)	(1.198)	(1.004)	(0.353)
Constant	87.400***	-116.116***	-42.169***	87.400***
	(9.498)	(15.848)	(12.965)	(9.498)
Year-Fixed Effects	Yes	No	No	Yes
Fund-Fixed Effects	No	Yes	No	No
Sponsor-Fixed Effects	No	No	Yes	Yes
Observations	98,496	98,496	98,496	98,496

Table 4: The Flow-Performance Relationship

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variable is *Fund Flow*, computed as the percentage change in total net assets from time *t* to time *t+1*, adjusted for market appreciation. *Fed Rate* is the annualized Fed funds rate. *Fund Return* is the annualized fund return. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include sponsor-fixed effects. *High Rate* restricts the sample to the period of high interest rates (Fed funds rate greater than 1%). *Low Rate* restricts the sample to the period of low interest rates (Fed funds rate between 0 and 1%). Columns (1), (4), and (5) additionally include week-fixed effects. Standard errors are clustered at the fund sponsor level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

	(1)	(2)	(3)	(4)	(5)
VARIABLES		Full Sample		High Rate	Low Rate
Fund Return	0.009***		0.003***	0.007***	0.009***
	(0.001)		(0.001)	(0.002)	(0.002)
Fed Rate		0.001***	-0.002*		
		(0.000)	(0.001)		
Fed Rate*Fund Return			0.000		
			(0.000)		
Log(Fund Size)	-0.000***	-0.000**	-0.001***	-0.001***	-0.000
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Log(Family Size)	0.002***	0.002***	0.001***	0.001	0.002***
	(0.000)	(0.000)	(0.000)	(0.001)	(0.001)
Expenses	-0.003**	-0.001	-0.003**	-0.003**	-0.007*
	(0.001)	(0.001)	(0.001)	(0.001)	(0.004)
Age	-0.001	-0.001*	-0.001	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Fund Flow Volatility	0.072***	0.073***	0.071***	0.092***	0.042*
	(0.014)	(0.015)	(0.015)	(0.020)	(0.022)
Institutional	-0.000	0.000	-0.000	-0.001	-0.000
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Constant	-0.018***	-0.013**	-0.011**	-0.002	-0.022***
	(0.005)	(0.005)	(0.005)	(0.009)	(0.008)
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes
Week-Fixed Effects	Yes	No	No	Yes	Yes
Observations	98,948	98,974	98,948	50,572	48,376

Table 5: Fund Exit and Fed Funds Rate

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variables are *Number of Funds*, defined as the number of funds in a given period, and *Exit*, defined as an indicator variable equal to one if the fund exits the fund industry in week *t*. *Fed Rate* is the annualized Fed funds rate. Control variables include the annualized fund return, the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year/month-fixed and sponsor-fixed effects. *High Rate* restricts the sample to the period of high interest rates (Fed funds rate greater than 1%). *Low Rate* restricts the sample to the period of low interest rates (Fed funds rate between 0 and 1%). Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)
	Number of Funds			Exit		
	Full Sample	High Rate	Low Rate	Full Sample	High Rate	Low Rate
Fed Rate	0.654	0.515	11.310***	0.002	0.004	-0.072***
	(0.478)	(0.697)	(3.063)	(0.002)	(0.002)	(0.016)
Fund Return	-0.061	-0.248**	0.174**	-0.014***	-0.020***	-0.001
	(0.061)	(0.101)	(0.070)	(0.003)	(0.004)	(0.001)
Log(Fund Size)	-0.001	-0.000	-0.002***	-0.001***	-0.002***	-0.001***
	(0.001)	(0.001)	(0.001)	(0.000)	(0.000)	(0.000)
Log(Family Size)	-0.001	0.005**	-0.000	-0.001*	-0.000	-0.001
	(0.002)	(0.002)	(0.001)	(0.001)	(0.000)	(0.001)
Expenses	-0.015*	-0.017*	-0.058**	-0.014***	-0.023***	-0.011**
	(0.008)	(0.009)	(0.028)	(0.002)	(0.004)	(0.005)
Age	0.003**	0.005*	0.000	0.001*	0.000	0.001**
	(0.001)	(0.003)	(0.001)	(0.000)	(0.000)	(0.001)
Fund Flow	0.038	0.022	0.087	-0.029***	-0.018***	-0.041***
	(0.081)	(0.107)	(0.113)	(0.008)	(0.007)	(0.015)
Fund Flow Volatility	-0.017	-0.093***	0.034	0.037***	-0.005	0.076***
	(0.020)	(0.025)	(0.026)	(0.010)	(0.010)	(0.019)
Institutional	-0.004**	-0.006**	-0.004**	-0.004***	-0.008***	-0.002**
	(0.001)	(0.003)	(0.002)	(0.001)	(0.001)	(0.001)
Constant	290.883***	291.512***	175.000***	0.054***	0.069***	0.018*
	(0.927)	(1.337)	(0.014)	(0.011)	(0.013)	(0.010)
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	98,792	50,334	48,458	98,792	50,334	48,458

Table 6: Fund Risk and Fed Funds Rate (Low-Rate Regime)

The sample is all U.S. prime money market funds. The dependent variables are: the weekly annualized spread (*Spread*), the fraction of assets held in risky assets, net of the riskless assets (*Holdings Risk*), average portfolio maturity (*Maturity Risk*), and portfolio concentration, defined as a Herfindahl Index of asset classes (*Concentration*). *Fed Rate* is the annualized Fed funds rate. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year/month-fixed and sponsor-fixed effects. The sample is restricted to the period of low interest rates (Fed funds rate between 0 and 1%). Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

	(1)	(2)	(3)	(4)
VARIABLES	Spread	Holdings Risk	Maturity Risk	Concentration
Fed Rate	-94.917**	-6.770**	0.924	-0.024*
	(46.443)	(3.070)	(4.121)	(0.014)
Log(Fund Size)	0.400***	1.690***	0.208***	0.003***
	(0.099)	(0.042)	(0.026)	(0.000)
Log(Family Size)	-2.371***	0.694***	-0.395	0.009***
	(0.604)	(0.260)	(0.290)	(0.002)
Expenses	1.996***	0.243	0.969***	0.009***
	(0.439)	(0.265)	(0.193)	(0.001)
Fund Flow	-37.591***	-6.721**	-12.627***	0.216***
	(2.455)	(2.798)	(1.413)	(0.012)
Fund Flow Volatility	4.346***	-3.275***	11.074***	-0.030***
	(0.737)	(1.160)	(0.502)	(0.006)
Institutional	-15.650***	-18.582***	-15.326***	0.024***
	(1.479)	(1.299)	(1.049)	(0.007)
Constant	-0.070	2.586***	-1.076***	0.002
	(0.140)	(0.171)	(0.129)	(0.001)
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes
Observations	45,322	46,065	46,054	46,065

Table 7: Fund Expenses and Fed Funds Rate

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variables are *Charged Expenses*, defined as percentage expense rate charged by a fund, *Incurred Expenses*, defined as percentage expense rate incurred by a fund, *Subsidy*, defined as the difference between incurred and charged expenses. *Fed Rate* is the annualized Fed funds rate. Control variables include the annualized fund return, the natural logarithm of fund assets, the natural logarithm of family assets, the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year-fixed and sponsor-fixed effects. *High Rate* restricts the sample to the period of high interest rates (Fed funds rate greater than 1%). *Low Rate* restricts the sample to the period of low interest rates (Fed funds rate between 0 and 1%). Standard errors are clustered at the sponsor level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

VARIABLES	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Charged Expenses			Incurred Expenses			Subsidy		
	Full Sample	High Rate	Low Rate	Full Sample	High Rate	Low Rate	Full Sample	High Rate	Low Rate
Fed Rate	-0.006***	-0.005***	0.076***	-0.002	0.003**	0.009	0.004***	0.007***	-0.066***
	(0.001)	(0.001)	(0.026)	(0.002)	(0.001)	(0.011)	(0.001)	(0.001)	(0.021)
Log(Fund Size)	-0.020***	-0.036***	-0.007***	-0.052***	-0.057***	-0.046***	-0.031***	-0.020***	-0.039***
	(0.001)	(0.000)	(0.000)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Family Size)	0.021***	0.012***	0.007***	0.010***	0.010***	0.010***	-0.010***	-0.000	0.004
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)
Age	0.009***	0.000	0.013***	0.013***	-0.010***	0.027***	0.005	-0.011***	0.015***
	(0.002)	(0.001)	(0.002)	(0.002)	(0.001)	(0.003)	(0.003)	(0.001)	(0.003)
Fund Flow	0.009	0.018	0.015	0.003	0.031**	-0.038	0.007	0.029***	-0.045
	(0.013)	(0.012)	(0.023)	(0.018)	(0.014)	(0.037)	(0.019)	(0.010)	(0.040)
Fund Flow Volatility	-0.946***	-1.104***	-0.524***	-0.631***	-0.734***	-0.593***	0.290***	0.345***	-0.089
	(0.023)	(0.024)	(0.032)	(0.035)	(0.023)	(0.089)	(0.037)	(0.015)	(0.096)
Institutional	-0.206***	-0.323***	-0.099***	-0.318***	-0.336***	-0.317***	-0.111***	-0.012***	-0.217***
	(0.006)	(0.002)	(0.006)	(0.001)	(0.002)	(0.002)	(0.005)	(0.001)	(0.006)
Constant	0.542***	0.848***	0.439***	0.982***	1.122***	0.789***	0.417***	0.249***	0.626***
	(0.019)	(0.013)	(0.022)	(0.023)	(0.018)	(0.035)	(0.027)	(0.013)	(0.038)
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	98,792	50,334	48,458	98,484	50,186	48,298	98,484	50,186	48,298

Table 8: Fund Exit and Fed Funds Rate: Conditioning on Sponsor Type

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variables are *Number of Funds*, defined as the number of funds in a given period, and *Exit*, defined as an indicator variable equal to one if the fund exits the fund industry in week *t*. *Low Rate* is an indicator variable equal to one if the Fed funds rate is less than or equal 1%, and zero otherwise. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the annualized fund return, the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year/month-fixed and sponsor-fixed effects. Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

	(1)	(2)
VARIABLES	Number of Funds	Exit
Low Rate	-0.623	-0.009
	(0.581)	(0.008)
Independent Sponsor*Low Rate	0.003**	0.000
	(0.001)	(0.001)
Fund Return	-0.046	-0.013***
	(0.060)	(0.003)
Log(Fund Size)	-0.001	-0.001***
	(0.001)	(0.000)
Log(Family Size)	-0.001	-0.001*
	(0.002)	(0.001)
Expenses	-0.017**	-0.014***
	(0.007)	(0.002)
Age	0.003**	0.001*
	(0.001)	(0.000)
Fund Flow	0.033	-0.029***
	(0.082)	(0.008)
Fund Flow Volatility	-0.013	0.037***
	(0.020)	(0.010)
Institutional	-0.004***	-0.004***
	(0.001)	(0.001)
Constant	292.123***	0.058***
	(0.113)	(0.010)
Year-Month-Fixed Effects	Yes	Yes
Sponsor-Fixed Effect	Yes	Yes
Observations	98,795	98,795

Table 9: Fund Risk and Fed Funds Rate: Conditioning on Sponsor Type

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variables are: the weekly annualized spread (*Spread*), the fraction of assets held in risky assets, net of the riskless assets (*Holdings Risk*), average portfolio maturity (*Maturity Risk*), and portfolio concentration, defined as a Herfindahl Index of asset classes (*Concentration*). *Low Rate* is an indicator variable equal to one if the Fed funds rate is less than or equal 1%, and zero otherwise. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include week-fixed effects. Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

VARIABLES	(1) Spread	(2) Holdings Risk	(3) Maturity Risk	(4) Concentration
Independent Sponsor	-0.239	-0.130	-0.904	-0.026
	(0.893)	(2.535)	(1.042)	(0.020)
Independent Sponsor*Low Rate	2.449**	7.328**	4.519***	0.038**
	(1.224)	(2.905)	(1.170)	(0.017)
Log(Fund Size)	0.847***	3.029***	0.540*	-0.009**
	(0.277)	(0.634)	(0.314)	(0.005)
Log(Family Size)	0.867**	2.776***	0.427**	-0.013***
	(0.361)	(0.746)	(0.209)	(0.005)
Expenses	5.652**	10.423**	0.565	0.022
	(2.426)	(4.892)	(1.978)	(0.036)
Age	2.624*	2.216	3.655***	0.055**
	(1.548)	(2.943)	(1.338)	(0.026)
Fund Flow	0.522	-40.494*	9.170	-0.255**
	(7.537)	(21.799)	(8.570)	(0.111)
Fund Flow Volatility	-31.457**	-85.261**	-54.163***	-0.583***
	(13.800)	(36.820)	(16.366)	(0.194)
Institutional	2.469*	4.215	-0.298	-0.024
	(1.327)	(2.963)	(1.045)	(0.020)
Constant	16.236**	-75.077***	15.104*	0.232*
	(8.237)	(15.476)	(8.017)	(0.128)
Week-Fixed Effect	Yes	Yes	Yes	Yes
Observations	94,521	95,264	95,253	95,264

Table 10: Fund Expenses and Fed Funds Rate: Conditioning on Sponsor Type

The sample is all U.S. prime money market funds over the period January 2005-December 2013. The dependent variables are *Charged Expenses*, defined as percentage expense rate charged by a fund, *Incurred Expenses*, defined as percentage expense rate incurred by a fund, *Subsidy*, defined as the difference between incurred and charged expenses. *Low Rate* is an indicator variable equal to one if the Fed funds rate is less than or equal 1%, and zero otherwise. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the annualized fund return, the natural logarithm of fund assets, the natural logarithm of family assets, the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include week-fixed effects. Standard errors are clustered at the sponsor level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

	(1)	(2)	(3)
VARIABLES	Charged Expenses	Incurred Expenses	Subsidy
Independent Sponsor	0.012	0.062	0.049*
	(0.027)	(0.040)	(0.027)
Independent Sponsor*Low Rate	-0.013	-0.031	-0.018
	(0.024)	(0.022)	(0.030)
Log(Fund Size)	-0.017***	-0.048***	-0.030***
	(0.005)	(0.010)	(0.007)
Log(Family Size)	0.002	0.008	0.006
	(0.005)	(0.010)	(0.007)
Age	0.023	-0.005	-0.027
	(0.015)	(0.024)	(0.020)
Fund Flow	0.005	-0.009	0.001
	(0.018)	(0.035)	(0.033)
Fund Flow Volatility	-1.006***	-0.640***	0.341*
	(0.156)	(0.240)	(0.175)
Institutional	-0.190***	-0.291***	-0.099***
	(0.020)	(0.030)	(0.019)
Constant	0.505***	1.008***	0.496***
	(0.097)	(0.168)	(0.135)
Week-Fixed Effects	Yes	Yes	Yes
Observations	98,795	98,484	98,484

Table 11: Fund Exit and ZIRP Shocks

The sample is all U.S. prime money market funds. The dependent variables are *Number of Funds*, defined as the number of funds in a given period, and *Exit*, defined as an indicator variable equal to one if the fund exits the fund industry in week *t*. The estimation window includes one month before and three months (in columns (1) and (2)) and six months (in columns (3) and (4)) after the event dates defined in Table 1. *Event* is an indicator variable equal to one for the period after the event date and zero for the period before the event date. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the annualized fund return, the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year-fixed and sponsor-fixed effects. Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

Panel A: Fund Exit and Monetary Shocks

	(1)	(2)	(3)	(4)
VARIABLES	Number of Funds	Exit	Number of Funds	Exit
	3 Months Ahead		6 Months Ahead	
Event	-3.248***	0.002**	-2.789***	0.002**
	(0.809)	(0.001)	(0.764)	(0.001)
Fund Return	3.899***	-0.001	6.569***	-0.002
	(0.690)	(0.002)	(0.862)	(0.002)
Log(Fund Size)	-0.012*	-0.001**	-0.023***	-0.001***
	(0.007)	(0.000)	(0.008)	(0.000)
Log(Family Size)	-0.171	-0.003	-0.146	-0.003*
	(0.130)	(0.002)	(0.103)	(0.002)
Expenses	-0.801***	-0.014***	-0.298	-0.019***
	(0.260)	(0.005)	(0.438)	(0.005)
Age	0.298**	0.001*	0.280**	0.002**
	(0.129)	(0.001)	(0.134)	(0.001)
Fund Flow	-1.515	-0.065**	-1.110	-0.055**
	(1.129)	(0.026)	(0.903)	(0.022)
Fund Flow Volatility	3.734***	0.068**	4.881***	0.081***
	(0.607)	(0.030)	(0.611)	(0.030)
Institutional	-0.070*	-0.002**	0.047	-0.003***
	(0.040)	(0.001)	(0.059)	(0.001)
Constant	237.789***	0.042	181.502***	0.039**
	(2.280)	(0.028)	(1.581)	(0.019)
Year-Fixed Effects	Yes	Yes	Yes	Yes
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes
Observations	18,568	18,568	25,914	25,914

Panel B: Conditioning on Sponsor Type and Monetary Shocks

VARIABLES	(1) Number of Funds	(2) Exit	(3) Number of Funds	(4) Exit
	3 Months Ahead		6 Months Ahead	
Event	-3.280***	0.002***	-2.862***	0.002***
	(0.803)	(0.001)	(0.767)	(0.001)
Event*Independent Sponsor	0.077	-0.002	0.178**	-0.003**
	(0.067)	(0.002)	(0.081)	(0.001)
Fund Return	3.900***	-0.001	6.574***	-0.002
	(0.689)	(0.002)	(0.861)	(0.002)
Log(Fund Size)	-0.012*	-0.001**	-0.023***	-0.001***
	(0.007)	(0.000)	(0.008)	(0.000)
Log(Family Size)	-0.172	-0.003	-0.150	-0.003*
	(0.130)	(0.002)	(0.103)	(0.002)
Expenses	-0.802***	-0.013***	-0.309	-0.019***
	(0.260)	(0.005)	(0.436)	(0.005)
Age	0.298**	0.001*	0.281**	0.002**
	(0.129)	(0.001)	(0.134)	(0.001)
Fund Flow	-1.515	-0.065**	-1.106	-0.055**
	(1.130)	(0.026)	(0.905)	(0.022)
Fund Flow Volatility	3.727***	0.068**	4.888***	0.081***
	(0.605)	(0.030)	(0.611)	(0.030)
Institutional	-0.070*	-0.002**	0.045	-0.003***
	(0.040)	(0.001)	(0.059)	(0.001)
Constant	237.790***	0.042	181.543***	0.038*
	(2.283)	(0.027)	(1.587)	(0.019)
Year-Fixed Effects	Yes	Yes	Yes	Yes
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes
Observations	18,568	18,568	25,914	25,914

Table 12: Fund Risk and ZIRP Shocks

The sample is all U.S. prime money market funds. The dependent variables are: the weekly annualized spread (*Spread*), the fraction of assets held in risky assets, net of the riskless assets (*Holdings Risk*), average portfolio maturity (*Maturity Risk*), and portfolio concentration, defined as a Herfindahl Index of asset classes (*Concentration*). The estimation window includes one month before and three months (in columns (1) -(4)) and six months (in columns (5)-(8)) after the event dates defined in Table 1. *Event* is an indicator variable equal to one for the period after the event date and zero for the period before the event date. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year/month-fixed and sponsor-fixed effects (in Panel A) and year/month-fixed effects (in Panel B). Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

Panel A: Fund Risk and Monetary Shocks

VARIABLES	(1) Spread	(2) Holdings Risk	(3) Maturity Risk	(4) Concentration	(5) Spread	(6) Holdings Risk	(7) Maturity Risk	(8) Concentration
	3 Months Ahead				6 Months Ahead			
Event	34.329***	0.954***	-1.216**	0.005***	21.341**	0.879***	-1.347***	0.005***
	(12.113)	(0.323)	(0.503)	(0.002)	(10.471)	(0.271)	(0.386)	(0.001)
Log(Fund Size)	0.311***	1.672***	0.244***	0.002***	0.251***	1.645***	0.248***	0.002***
	(0.087)	(0.051)	(0.043)	(0.000)	(0.073)	(0.048)	(0.034)	(0.000)
Log(Family Size)	-7.141***	0.632	-0.065	0.013***	-4.828***	0.435	0.472	0.011***
	(1.528)	(0.524)	(0.516)	(0.004)	(1.177)	(0.395)	(0.423)	(0.003)
Expenses	3.950***	2.043***	1.898***	0.015***	3.140***	1.296***	1.659***	0.012***
	(0.914)	(0.335)	(0.289)	(0.002)	(0.697)	(0.313)	(0.246)	(0.002)
Age	-63.297***	-5.511	-14.730***	0.173***	-52.901***	-2.220	-10.630***	0.197***
	(5.856)	(3.417)	(2.111)	(0.021)	(4.554)	(3.474)	(2.152)	(0.018)
Fund Flow	2.211*	0.002	10.502***	-0.041***	1.566	-0.739	9.684***	-0.037***
	(1.286)	(1.955)	(0.854)	(0.012)	(1.043)	(1.641)	(0.681)	(0.010)
Institutional	-23.283***	-13.671***	-22.086***	0.002	-21.940***	-19.000***	-21.336***	-0.011
	(3.680)	(2.358)	(1.544)	(0.008)	(2.649)	(2.182)	(1.330)	(0.007)
Constant	-0.208	3.497***	-0.732***	0.010***	-0.459**	3.350***	-0.738***	0.008***
	(0.235)	(0.250)	(0.152)	(0.001)	(0.193)	(0.268)	(0.189)	(0.001)
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,830	16,882	16,879	16,882	23,462	23,527	23,524	23,527

Panel B: Conditioning on Sponsor Type and Monetary Shocks

VARIABLES	(1) Spread	(2) Holdings Risk	(3) Maturity Risk	(4) Concentration	(5) Spread	(6) Holdings Risk	(7) Maturity Risk	(8) Concentration
	3 Months Ahead				6 Months Ahead			
Independent Sponsor	-3.837	6.642**	4.768***	-0.000	-0.776	5.681	5.107***	-0.011
	(2.878)	(3.309)	(1.366)	(0.020)	(2.940)	(3.492)	(1.484)	(0.021)
Independent Sponsor*Event	9.215***	1.821***	-1.010**	0.008**	4.646***	3.405**	-1.897***	0.026***
	(1.239)	(0.634)	(0.460)	(0.004)	(1.668)	(1.533)	(0.709)	(0.007)
Log(Fund Size)	1.358**	3.102***	0.559	-0.009*	1.121**	3.168***	0.503	-0.008
	(0.601)	(0.889)	(0.444)	(0.005)	(0.514)	(0.871)	(0.440)	(0.005)
Log(Family Size)	1.132*	3.542***	0.347	-0.012**	0.936*	3.336***	0.427	-0.013**
	(0.663)	(0.939)	(0.304)	(0.005)	(0.559)	(0.904)	(0.293)	(0.005)
Expenses	15.013**	15.387**	-2.012	0.046	13.661***	15.591**	-2.208	0.052
	(5.818)	(6.780)	(3.046)	(0.041)	(4.966)	(6.588)	(2.981)	(0.040)
Age	11.336***	4.921	7.683***	0.085***	9.207***	3.969	7.367***	0.087***
	(3.351)	(4.337)	(2.137)	(0.032)	(2.932)	(4.171)	(2.164)	(0.032)
Fund Flow	3.399	-42.839	15.279	-0.248*	3.629	-45.113*	16.330	-0.247*
	(15.427)	(26.417)	(9.695)	(0.129)	(12.685)	(25.876)	(10.039)	(0.127)
Fund Flow Volatility	-61.994**	-53.305	-60.787***	-0.397*	-52.152**	-57.933	-60.876***	-0.403**
	(25.558)	(43.026)	(18.732)	(0.209)	(21.349)	(42.253)	(18.776)	(0.204)
Institutional	6.432**	8.657**	-0.656	-0.006	5.285**	8.297**	-0.567	-0.006
	(2.908)	(3.824)	(1.505)	(0.021)	(2.481)	(3.668)	(1.472)	(0.021)
Constant	-34.986*	-111.568***	-3.997	-0.012	-25.810	-104.344***	-2.431	-0.027
	(18.869)	(23.029)	(12.725)	(0.160)	(16.505)	(22.463)	(12.892)	(0.159)
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,830	16,882	16,879	16,882	23,462	23,527	23,524	23,527

Table 13: Fund Expenses and ZIRP Shocks

The sample is all U.S. prime money market funds. The dependent variables are *Charged Expenses*, defined as percentage expense rate charged by a fund, *Incurred Expenses*, defined as percentage expense rate incurred by a fund, *Subsidy*, defined as the difference between incurred and charged expenses. The estimation window includes one month before and three months (in columns (1)-(3)) and six months (in columns (4)-(6)) after the event dates defined in Table 1. *Event* is an indicator variable equal to one for the period after the event date and zero for the period before the event date. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year-fixed and sponsor-fixed effects (in Panel A) and week-fixed effects (in Panel B). Standard errors are clustered at the sponsor level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

Panel A: Fund Expenses and Monetary Shocks

VARIABLES	(1) Charged	(2) Incurred	(3) Subsidy	(4) Charged	(5) Incurred	(6) Subsidy
	3 months ahead			6 months ahead		
Event	-0.001	-0.002**	-0.002	-0.014**	-0.003***	0.010**
	(0.002)	(0.001)	(0.002)	(0.006)	(0.001)	(0.005)
Log(Fund Size)	-0.006***	-0.045***	-0.039***	-0.005***	-0.044***	-0.038***
	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)	(0.001)
Log(Family Size)	0.014***	0.031***	0.020***	0.006**	0.028***	0.025***
	(0.003)	(0.003)	(0.003)	(0.002)	(0.002)	(0.003)
Age	0.010***	0.026***	0.016***	0.012***	0.031***	0.019***
	(0.003)	(0.005)	(0.004)	(0.003)	(0.004)	(0.004)
Fund Flow	-0.028	-0.006	0.029	-0.024	-0.005	0.028
	(0.028)	(0.032)	(0.037)	(0.023)	(0.026)	(0.029)
Fund Flow Volatility	-0.699***	-0.804***	-0.127*	-0.641***	-0.833***	-0.218***
	(0.057)	(0.044)	(0.074)	(0.044)	(0.035)	(0.061)
Institutional	-0.133***	-0.318***	-0.184***	-0.120***	-0.317***	-0.196***
	(0.012)	(0.002)	(0.011)	(0.009)	(0.002)	(0.009)
Constant	0.445***	0.648***	0.167***	0.197***	0.566***	0.337***
	(0.034)	(0.038)	(0.042)	(0.034)	(0.032)	(0.039)
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Year-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,568	18,506	18,506	25,914	25,829	25,829

Panel B: Conditioning on Sponsor Type and Monetary Shocks

VARIABLES	(1) Charged	(2) Incurred	(3) Subsidy	(4) Charged	(5) Incurred	(6) Subsidy
		3 months ahead			6 months ahead	
Independent Sponsor	-0.003	0.030	0.032	-0.006	0.029	0.034
	(0.020)	(0.039)	(0.027)	(0.022)	(0.039)	(0.027)
Independent Sponsor*Event	0.005	0.008	0.003	0.011	0.011	0.000
	(0.006)	(0.005)	(0.007)	(0.012)	(0.010)	(0.013)
Log(Fund Size)	-0.006	-0.047***	-0.041***	-0.006	-0.047***	-0.040***
	(0.004)	(0.011)	(0.008)	(0.004)	(0.011)	(0.008)
Log(Family Size)	0.003	0.004	0.001	0.004	0.005	0.000
	(0.005)	(0.012)	(0.010)	(0.005)	(0.012)	(0.010)
Age	0.015	-0.002	-0.016	0.016	0.002	-0.014
	(0.013)	(0.026)	(0.022)	(0.012)	(0.026)	(0.023)
Fund Flow	-0.022	0.003	0.035	-0.012	0.004	0.032
	(0.033)	(0.046)	(0.047)	(0.029)	(0.038)	(0.039)
Fund Flow Volatility	-0.789***	-0.592*	0.166	-0.751***	-0.599**	0.115
	(0.149)	(0.299)	(0.240)	(0.134)	(0.287)	(0.242)
Institutional	-0.115***	-0.290***	-0.175***	-0.102***	-0.291***	-0.188***
	(0.016)	(0.033)	(0.024)	(0.015)	(0.033)	(0.025)
Constant	0.327***	1.020***	0.687***	0.287***	0.996***	0.701***
	(0.081)	(0.188)	(0.152)	(0.074)	(0.194)	(0.162)
Week-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes
Observations	18,568	18,506	18,506	25,914	25,829	25,829

Table 14: Fund Risk and ZIRP Shocks—Conditioning on Survival

The sample is all U.S. prime money market funds. The dependent variables are: the weekly annualized spread (*Spread*), the fraction of assets held in risky assets, net of the riskless assets (*Holdings Risk*), average portfolio maturity (*Maturity Risk*), and portfolio concentration, defined as a Herfindahl Index of asset classes (*Concentration*). The estimation window includes one month before and three months (in columns (1)-(4)) and six months (in columns (5)-(8)) after the event dates defined in Table 1. *Event* is an indicator variable equal to one for the period after the event date and zero for the period before the event date. We restrict our estimation to the sample of funds that are present in both periods before and after the event. *Independent Sponsor* is an indicator variable equal one if the fund sponsor is an independent asset management company, and zero otherwise. Control variables include the natural logarithm of fund assets, the natural logarithm of family assets, expense ratio (charged), the natural logarithm of fund age, fund flow computed as a percentage change in total net assets from time *t* to time *t+1* adjusted for market appreciation, standard deviation of fund flow, and an indicator variable equal to one if the fund is offered to institutional investors and zero otherwise. All regressions are at the weekly level and include year/month-fixed and sponsor-fixed effects (in Panel A) and year/month-fixed effects (in Panel B). Standard errors are clustered at the week level. ***, **, * represent 1%, 5%, and 10% significance, respectively.

Panel A: Fund Risk and Monetary Shocks

VARIABLES	(1) Spread	(2) Holdings Risk	(3) Maturity Risk	(4) Concentration	(5) Spread	(6) Holdings Risk	(7) Maturity Risk	(8) Concentration
	3 Months Ahead				6 Months Ahead			
Event	34.329***	0.954***	-1.216**	0.005***	21.341**	0.879***	-1.347***	0.005***
	(12.113)	(0.323)	(0.503)	(0.002)	(10.471)	(0.271)	(0.386)	(0.001)
Log(Fund Size)	0.311***	1.672***	0.244***	0.002***	0.251***	1.645***	0.248***	0.002***
	(0.087)	(0.051)	(0.043)	(0.000)	(0.073)	(0.048)	(0.034)	(0.000)
Log(Family Size)	-7.141***	0.632	-0.065	0.013***	-4.828***	0.435	0.472	0.011***
	(1.528)	(0.524)	(0.516)	(0.004)	(1.177)	(0.395)	(0.423)	(0.003)
Expenses	3.950***	2.043***	1.898***	0.015***	3.140***	1.296***	1.659***	0.012***
	(0.914)	(0.335)	(0.289)	(0.002)	(0.697)	(0.313)	(0.246)	(0.002)
Age	-63.297***	-5.511	-14.730***	0.173***	-52.901***	-2.220	-10.630***	0.197***
	(5.856)	(3.417)	(2.111)	(0.021)	(4.554)	(3.474)	(2.152)	(0.018)
Fund Flow	2.211*	0.002	10.502***	-0.041***	1.566	-0.739	9.684***	-0.037***
	(1.286)	(1.955)	(0.854)	(0.012)	(1.043)	(1.641)	(0.681)	(0.010)
Institutional	-23.283***	-13.671***	-22.086***	0.002	-21.940***	-19.000***	-21.336***	-0.011
	(3.680)	(2.358)	(1.544)	(0.008)	(2.649)	(2.182)	(1.330)	(0.007)
Constant	-0.208	3.497***	-0.732***	0.010***	-0.459**	3.350***	-0.738***	0.008***
	(0.235)	(0.250)	(0.152)	(0.001)	(0.193)	(0.268)	(0.189)	(0.001)
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Sponsor-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,830	16,882	16,879	16,882	23,462	23,527	23,524	23,527

Panel B: Conditioning on Sponsor Type and Monetary Shocks

VARIABLES	(1) Spread	(2) Holdings Risk	(3) Maturity Risk	(4) Concentration	(5) Spread	(6) Holdings Risk	(7) Maturity Risk	(8) Concentration
	3 Months Ahead				6 Months Ahead			
Independent Sponsor	-3.748	6.178*	4.633***	-0.008	-0.979	5.382	5.016***	-0.016
	(2.837)	(3.305)	(1.355)	(0.023)	(2.893)	(3.476)	(1.469)	(0.022)
Independent Sponsor*Event	9.167***	2.507***	-0.823*	0.020*	4.972***	3.865**	-1.776**	0.034***
	(1.212)	(0.801)	(0.473)	(0.011)	(1.631)	(1.556)	(0.702)	(0.010)
Log(Fund Size)	1.367**	3.104***	0.558	-0.009*	1.124**	3.171***	0.501	-0.008
	(0.601)	(0.889)	(0.444)	(0.005)	(0.514)	(0.871)	(0.440)	(0.005)
Log(Family Size)	1.132*	3.541***	0.346	-0.012**	0.935*	3.336***	0.427	-0.013**
	(0.664)	(0.939)	(0.304)	(0.005)	(0.560)	(0.904)	(0.293)	(0.005)
Expenses	15.085**	15.406**	-2.019	0.046	13.689***	15.613**	-2.217	0.052
	(5.819)	(6.778)	(3.047)	(0.041)	(4.965)	(6.586)	(2.982)	(0.040)
Age	11.204***	4.882	7.694***	0.085***	9.156***	3.929	7.385***	0.087***
	(3.367)	(4.339)	(2.138)	(0.032)	(2.936)	(4.172)	(2.165)	(0.032)
Fund Flow	3.334	-42.871	15.281	-0.248*	3.592	-45.149*	16.336	-0.248*
	(15.402)	(26.403)	(9.696)	(0.129)	(12.675)	(25.859)	(10.040)	(0.127)
Fund Flow Volatility	-61.740**	-53.231	-60.808***	-0.396*	-52.045**	-57.844	-60.907***	-0.402*
	(25.537)	(43.025)	(18.733)	(0.209)	(21.338)	(42.252)	(18.777)	(0.204)
Institutional	6.473**	8.666**	-0.660	-0.006	5.298**	8.305**	-0.573	-0.006
	(2.908)	(3.823)	(1.505)	(0.021)	(2.480)	(3.667)	(1.472)	(0.021)
Constant	-34.490*	-111.411***	-4.036	-0.011	-25.613	-104.192***	-2.501	-0.026
	(18.921)	(23.038)	(12.728)	(0.160)	(16.520)	(22.468)	(12.896)	(0.159)
Year/Month-Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	16,830	16,882	16,879	16,882	23,462	23,527	23,524	23,527

Chapter 3
Information Sharing, Social Norms and Performance

1 Introduction

A recurrent economic problem in organizations is how to use available knowledge efficiently. Information, however, is often dispersed among agents, which prevents optimal decisions if communication is absent. One important consequence is that organizations operating in markets requiring specialized knowledge can underperform. Credit, insurance, and financial markets are but a few examples. Agents might, in fact, decline a loan to a reliable client or fail to identify the appropriate risk class for a new client. Instead, when communication is available, agents can acquire information from co-workers in the organization. As Hayek (1945) pointed out: "it is a problem of the utilization of knowledge which is not given to anyone in its totality."

In the same spirit, Arrow's 1974 seminal work on the limits of organization argues that one key activity of any organization is internal communication. He has also pointed out that the trade-off between the cost of communication and the benefit of communication lies at the core of the agenda of organizations.

The effects of improved communication on productivity are in fact not obvious ex ante. On the one hand, low-skill agents may ask others for direction, thus communication might improve performance through a *learning effect*. On the other hand, high-skill agents might spend a larger fraction of their time helping others, which could negatively affect their performance, via a *substitution effect* as they swap information provision tasks for work completion tasks. Furthermore, even when production and use of information are not explicitly rewarded, a strategic motive might drive agents away from efficiency. Consider a high-skill worker facing the opportunity to share his knowledge with others. He might use this as an instrument to signal his ability in order to increase his chances of promotion. Alternatively he might prefer to hoard his knowledge to prevent a competitor from overtaking him on the ladder to promotion. Similarly, a low-skill worker might be reluctant to ask others for information required to complete his tasks out of fear of revealing his low ability. There exists then an internal job market *signaling effect* on behalf of high ability and an *adverse selection effect* on behalf of low ability workers,

Table 1: Competing Hypotheses

Hypothesis	*Information Consumption*	*Hypothesis*	*Information Production*
↑ : Learning	People who ask questions gain skill & advice and are *more* productive.	↑ : Signaling	People who answer questions are higher skill and *more* productive.
↓ : Adv. Select.	People who ask questions are lower skill and *less* productive.	↓ : Substitution	People who answer questions are distracted and *less* productive.

which influences agents' decisions. These effects are summarized in Table 1 where diagonal entries represent causal effects and off-diagonal entries represent selection effects. Finally, it is challenging to empirically disentangle the effects of communication versus those of innate ability regarding an agent's performance and demand for information.

The effect of knowledge sharing has recently attracted scholars' attention as one of main factors driving consumption and saving decisions (Moretti (2011) and Duflo and Saez (2003)), the creation of new technologies (Jaffe et al. (1993)) and adoption of technology (Foster and Rosenzweig (1995); Kremer and Miguel (2007); Conley and Udry (2010)). Our study extends this literature by empirically investigating how information sharing and communication affect loan officers' productivity within a large commercial bank. Moreover, by doing so we are also able to understand what drives the officers' demand for information. We analyze these issues within the corporate division of a global bank primarily located in Japan. Starting in 2003, the bank implemented an online platform to allow employees to access information from headquarters and to share their private information with each other. Every employee gained online access to documents provided by headquarters as well as the ability to pose questions of and provide answers to other employees. Adopting this new technology allowed for more efficient communication, both vertically, between headquarters and bank branches, and horizontally among loan officers.

Information provided by bank headquarters mainly concerns legal and taxation issues, new financial instruments and services provided by the organization, and general policy guidelines as well as details about the most successful management practices to adopt.

The information exchanged by loan officers is more often related to clients, for example, account management, credit worthiness, deal closure, collecting debts, and avoiding defaults. This provides us with a natural environment in which to understand how communication, and information sharing generally, affects productivity.

We exploit two unique properties of our dataset. First, we have very detailed data on the performance of each loan officer inside the bank for the two-year period 2006-2008. We observe the individual-level targets set by bank headquarters and the results achieved by each officer along a number of different dimensions, such as gross profit, loan volume and revenues. Since these measures are expressed in yen and easily quantifiable, the performance of the agents on these dimensions constitutes our *objective* performance measure. However, we also collected the branch manager's evaluations of each loan officer's performance on softer more informal dimensions, such as his or her contributions to branch operations and customer service. These evaluations can be interpreted as *subjective* performance measures because they are not directly related to the attainment of a pre-determined target. Moreover, as reported by the bankers these evaluations try to correct for luck and to reward officers' efforts.

Second, we are able to exploit a source of exogenous variation coming from an anti-corruption law that requires loan officers to switch branches every two to five years. This allows us to use an instrumental variable procedure in order to assess the causal effect of information sharing on employees' productivity. Moreover, our results are not contaminated by incentive considerations, in fact, there is no material incentive to use the new technology. Hence, if acquiring knowledge from others became an important part of the loan officer's job, it is exclusively due to the reliability and the productivity benefits of access to available information. Furthermore, the banking sector in Japan is very different from that in the United States. In particular, the incentive system implemented by this bank provides loan officers with only de minimus end-of-year bonuses, so incentives are almost exclusively constituted by the possibility of promotion.

This paper provides two main sets of results. First, exploiting the longitudinal dimension of our data, we are able to identify a significant positive effect of information sharing

on performance. Moreover, we also highlight what is the main mechanism through which communication affects performance. We provide evidence that low-skill agents benefit the most from acquiring information from others. The magnitude of the effect is large and significant. In fact, controlling for unobserved heterogeneity over time, between branches and among officers, a standard deviation increase in information access increases performance by more than ten percent. This supports the idea that agents might under-perform because they are not aware of more successful management practices implemented elsewhere or lack the information necessary to fill the gap (Bloom et al. (2011)) and might acquire soft information from other loan officers to improve their knowledge of the local credit market conditions (Stein (2002)). We can also conclude that asking questions and providing answers to others is mainly driven by innate ability, as captured by individual fixed effects. Intuitively, low-ability agents, in an attempt to improve their performance, are more likely to ask questions of their peers.

Second, we exploit the source of exogenous variation by restricting attention to agents who switched branches. We can study how job rotation affects (1) the demand for information and (2) the officers' performance. We find that they tend to increase their demand for information immediately after switching jobs. However, this effect declines over time. That job rotation increases information demand supports the hypothesis that learning is a major factor affecting performance and that mostly *soft* rather than *hard* information is shared with peers. Intuitively, when a loan officer is forced to switch branches, he does not possess, for example, the knowledge of the local market conditions to assess the reliability of new clients. Without the ability to communicate with more experienced officers, whom he also has trouble identifying, an officer's productivity can suffer.

Restricting our attention to "switchers," gives us the opportunity to investigate their performances in the new branches. We find strong evidence that switching negatively affects their performance and, after the switch, they perform on average significantly worse than before. This result is of independent interest as it suggests specialized human capital is destroyed when they move to a different branch. This is surprising because we are considering the same worker within the same organization, controlling for regional and

branch differences. This finding contributes to the labor literature on the accumulation and destruction of specialized human capital and the effect of worker displacement.[1] As we discuss in section 6, this presents us with an opportunity to quantify and bound the costs associated with the anti-corruption law.

Finally, we further exploit the mandatory switching of loan officers across branches in order to assess the causal effect of communication and information sharing on performance. Motivated by branch variation in usage of the information-sharing platform, we construct an instrument based upon the attitudes of a branch towards the new technology. For each officer i, our instrument is the amount of information accessed in the previous branch excluding officer i. That is, if specific officer A works in a branch where problems are usually resolved within the branch and without attempting to find the solutions elsewhere in the organization, then when officer A moves to a different branch, he will tend to communicate less with other officers in other branches. In contrast, if officer A works for a branch where access to information provided by others is encouraged, he will tend to communicate more across the organization when in the new branch. In other words, our instrument is based on the idea that social norms tend to be developed within branches and have an enduring effect on loan officers' behaviors.

We find even stronger results than those found with the estimation of the longitudinal model. This provides further evidence in favor of the hypothesis that there exists substitution between the loan officers' abilities and the amount of information to which they have access. Moreover, this suggests that in contrast to the o-ring theory formulated by Kremer (1993), which predicts that the productivity of each worker is increasing in the skill level of his co-workers, we find an *asymmetric* effect. While we observe that the opportunity to share information with high-skill workers increases the low-skill workers' productivity, we do not observe any negative effect on the high-skill workers' performance.

The significance of our instrument suggests that agents take into account their peers' behavior, when deciding how to cope with their daily tasks. For example, suppose loan

[1] Seminal papers in this strand of the literature include Hamermesh (1987), Ruhm (1991), and Jacobson and Sullivan (1993). For an early survey see Kletzer (1998).

officer A is facing a taxation issue with one of his corporate clients. Our results show that, not only will he be more inclined to ask questions if he has underperformed in the past (i.e. indicating a low-ability agent), but he will be more inclined to do so if his peers behave in the same way. That is, there exists a *complementarity* in information sharing: the higher the number of officers who consult their colleagues, the higher is the incentive for each of them to continue sharing information. This might be explained by the absence of competition and the lower risk of being identified as a low-ability agent which is based on having provided information.[2] Moreover, we show that it is more likely for officer A to meet his targets and improve his performance, by solving his taxation issue with the help of others, than by taking an uninformed decision.

The paper is organized as follows. The next section places this paper in relation to existing literature. Section 2 discusses the institutional background, and describes our data. Section 3 explains our approach and the methodology we employ to estimate the effect of information sharing on performance. Section 4 presents the first set of results for the effect of communication on performance, and the effect of switching on information demand and productivity. Section 5 presents the main results of our instrumental variable estimates, and discuss additional evidence supporting the validity of our instrument. Section 6 analyzes the relevance of our results for three different issues: theory of tournaments, cost of regulation, and relational banking. Section 7 summarizes and concludes.

1.1 Related Literature

Since Marschak and Radner (1972) pioneering work, team theory has theoretically investigated issues similar to our own and the literature has developed around the idea that information flows, and not just incentives, drive agents' behaviors inside an organization. [3] More recently, Garicano (2000) presents a theoretical model of hierarchical organization

[2] For an interesting overview of the economic literature on corporate culture see Hermalin (2007).

[3] Sah and Stiglitz (1986), for example, is an early attempt to compare decision-making in different organizational forms when agents possess heterogeneous information. Other important contributions in this literature include Bolton and Dewatripont (1994) and Van Zandt (1999) which highlight the

of expertise, which is applicable to our setting. Decisions involve problem solving and thus acquiring the relevant knowledge for each decision. There exists a trade-off between information acquisition costs and communication costs because agents can directly acquire information at a cost or elicit the relevant information from others at a higher level in the organization. The latter is costly because agents at the higher levels need to spend time solving problems faced by others, i.e. what we call the *substitution effect*. Our empirical understanding of these issues lags far behind, as data on information flows within firms is seldom collected and it is hard to disentangle the effect of information sharing on individual productivity from other confounding factors. This paper addresses some of these issues as we had the opportunity to collect individual-level data on performance and information flows within a large commercial bank.[4]

The behavior of loan officers has been the object of few recent studies. Hertzberg et al. (2010) show that a rotation policy that routinely reassigns loan officers to borrowers of an Argentinian commercial bank affects the officers' reporting behavior, while Agarwal and Ben-David (2012) run a controlled field experiment within a bank which shows an increase in defaults when the incentive structure of a subset of small business loan officers was altered from fixed salary to volume-based pay. While they focus on the loan officers' agency problems, we analyze how communication among officers might significantly improve their productivity. The role of organizational design and loan officers' behavior has been highlighted by Berger et al. (2005), they find that large banks are less willing to engage in information-intensive loans for which soft information is more important. Similarly, Liberti and Mian (2009) finds that greater hierarchical/geographical distance between the information collecting agent and the loan approving officer leads to less reliance on subjective information and more on objective information, while Agarwal and Hauswald (2010) find that borrower proximity facilitates the collection of soft information. We find that loan officer's performance decreases when they are forced to switch

importance of hierarchies to diminish the costs related to processing information that flows through the network of contacts.

[4] Bloom et al. (2009) employ an international data set in order to investigate the effect of information technology and communication on worker autonomy, plant manager autonomy, and span of control. We complement their analysis by focusing on the loan officers' performance and their demand for information.

branch, which suggests that the acquisition of soft information about local businesses is one of the main factors driving loan officers' productivity.

The diffusion of information technologies (IT) has been associated with an increase in productivity in organizations (see for example the survey by Brynjolfsson and Hitt (2000)). However, empirically identifying the effect of IT on productivity and ascertaining the channel through which it affects performance have proved elusive. Recently, Paravisini and Schoar (2012) shows that by decreasing monitoring costs, IT reduces loan officers' agency costs. Our results complement their study by showing that IT increases loan officers' productivity by lowering their costs of acquiring soft information from their peers.

Finally, this paper is also related to the recent strand of the literature that studies the differences in productivity performance between firms and plants within sectors and across countries.[5] Existing works in different fields have linked productivity levels to a number of features of technology, demand, human capital and market structure. However, as noted by Bloom and Reenen (2007) and Bloom and Reenen (2010), to create persistent performance differences the advantageous inner workings must be difficult to imitate, and this suggests that part of the performance variations across similar enterprises might be due to other softer and more informal aspects of organizations. We contribute to this literature by identifying another factor which significantly affects individual performance, that is, the possibility of acquiring knowledge from others.

2 Empirical models and results

The foregoing discussion suggests that the officers' performances might be affected by several different factors such as branch characteristics and their access to information. Because the estimation strategy is affected by data availability, this section begins with a description of the institutional background and the data.

[5] See Gibbons and Henderson (2010) and Gibbons (2010) for surveys of the literature on performance differences across similar enterprises.

2.1 The Setting

We analyze the behavior of loan officers, also called "relationship managers," in the corporate banking division of a major Japanese bank (the Bank) during the two-year period 2006-2008. Located across more than two hundred branches throughout Japan, the officers' primary tasks are to grant and manage loans to local enterprises. Their performances are assessed every six months and are measured by the percentage of the targets met during the same time. The officers' performance can be affected by two main factors. First, there exist regional differences between branches, such as the local demand for loans and the profitability of local enterprises. Second, there is some heterogeneity in loan tasks, in fact, while some officers only deal with the public administration, others need to re-structure more profitable loans or solicit loans from new clients. However, we shall take into account these sources of heterogeneity among officers.

As explained in the introduction, one of the main differences between a U.S. bank and the Japanese bank we are analyzing is the incentive system. While end-of-year bonuses are extensively adopted in the U.S. banking sector, the Bank rewards its loan officers by means of promotion. We observe about two hundred instances of promotion in our sample, and in untabulated results we also show suggestive evidence that the likelihood of being promoted is positively associated with the loan officers information production (e.g. number of answers posted) and negatively affected by how much information officers acquire (e.g. number of questions posted). This evidence, albeit not conclusive, suggests that a signaling motive might be present.

By law, the Bank implements a switching rule as a way to prevent bribery and graft among loan officers. This regulation obliges loan officers to change branches every two to five years, which allows us to disentangle the effect of an individual branch's working environment from the officer's ability on productivity. We shall show that headquarters do not relocate officers based upon their past performance.

In this environment, information sharing among loan officers has several effects. First, allowing officers to share information lets them better assess the riskiness of client enter-

prises, or work on more projects at the same time. Second, it might allow the low-ability workers to bridge the gap separating them from the most productive ones. Third, high-skill workers might be required to devote a larger fraction of their time helping their colleagues, which could reduce their performance. Fourth, anticipating that this can increase their chances of being promoted, officers might share their knowledge with others in order to signal their expertise in a particular field. Our main contribution is to identify and disentangle the different effects that communication has on productivity.

We now discuss the features of this work environment that allow us to assess whether information sharing shapes individual performance.

2.2 Data Characteristics

We collected data on performance, communication and information sharing from the corporate banking division of a major Japanese bank. Our primary data source is the bank's personnel records. These include all loan officers, approximately 2800 people, located across hundreds of branches in Japan. Branches vary in size and primary type of business, mainly due to location. In general, metropolitan branches have more loan officers – between 30 and 100 – and larger enterprises as customers, while those located in suburban areas have fewer officers, about 10, and smaller businesses as customers. Our data span October 2006 through September 2008. Prior to this study, only in the local branch employing individual loan officers kept their performance data. Information asymmetry made it difficult for loan officers to observe each other's productivity across branches, likely increasing dispersion of individual output. Since the Bank had a major merger in October 2005, we focus our attention on the stable, second year of the new bank to avoid having merger activity influence results via changes in officers' performances. The strengths of the data lie in their fine-grained level of detail and the possibility of tracking each officer's performance over time.

Dependent Variable. Loan officers are reviewed semi-annually to assess their performance. In order to account for branch location and task differences, we control for the six

main groups to which each officer may belong. These groups are: large existing account, small existing account, loan restructuring, public sector, new strategic account, and new non-strategic account. The main differences among groups depend on the different clientele. For example, officers working in the public sector group exclusively deal with public administration, while officers in the restructuring group try to renegotiate underperforming loans. Bank headquarters sets the targets for managers in these groups and to each loan officer the head of the branch assigns a score of up to 80 percentage points based on his quantitative measures and up to 20 percentage points based on qualitative measures in the categories of Table 2.

Table 2: Output Metrics

Quantitative	*Qualitative*
revenue	
individual loan profit	customer service
liquid deposit profit	loan reinforcement
loan volume	contribution to branch operations
reduced delinquencies	contribution to organization operations
reduced estimated losses	
bank gross profit	

The dual sets of metrics help to increase accuracy relative to stochastic environmental shocks. As reported by bank executives, the branch managers often assign qualitative scores as a reward for major effort that did not yield results or discount low effort that did. The weighted performance of loan officers along dimensions in the first column constitutes our objective performance measure, and weighted performance along the second column constitutes our subjective performance measure. Results are robust to using either column.

Our data include all targets, objective results, and subjective scores for each loan officer in each group and for every branch. That is, we have all performance assessments between 2006 and 2008 for the corporate division of the Bank. We believe that the richness of our data and the fact that we need not rely on wage data to extrapolate observed performance make it highly suitable for study of productivity differences across

officers.

In most of the analysis below, we focus our investigation on one dependent variable: total performance. This is the total score assigned to officers, representing the weighted sum of performance in each of the different categories and including the individual subjective score. It also represents the performance metric used by the bank.

Independent Variables. The main variables of interest capture how loan officers use the internal platform to share and gather information. We collected data on all accesses to the information platform by each officer during the period of interest. Our data include (i) the number of documents consulted by each officer, (ii) the number of questions posted, and (iii) the number of answers provided, down to the second of access and across each term.

Information provided by bank headquarters mainly concerns legal and taxation issues, new financial instruments and services provided by the organization, and general policy guidelines as well as details about the most successful management practices to adopt. The information exchanged by loan officers is more often related to clients, for example, account management, credit worthiness, deal closure, collecting debts, and avoiding defaults.

We also have information on the number of years the officer has worked for the Bank, captured by the variable "tenure," and whether he came directly from school with no prior experience (or transferred from another bank), captured by the dummy variable "college."

2.3 Descriptives

Table 3 reports descriptive statistics for our variables of interest, and two things are worth noting. First, loan officers perform significantly differently, in fact, the mean of our main measure of performance is 52, but the standard deviation is 21. Even if we focus on the employees of the same bank, within the same region and with homogeneous tasks, we still find that their performance is heterogeneous. Second, loan officers seem to make

great use of the available information within the organization. On average they access 569 documents, post 77 questions and provide 250 answers during a six-month period. Moreover, the standard deviations of all three variables are quite high, ranging from 164 to 282. This will be relevant to interpret estimated coefficients.

Our main hypothesis is that officers can access the information generated within the Bank in order to improve their performance. In particular, we expect low performers to ask more questions and provide fewer answers.

Figure 1 shows that the kernel density of our total performance measure. Note that performance for below-the-median number of answers (above-the-median number of questions) is to the left of above-the-median number of answers (below-the-median number of questions). The loan officers who help others more often, answering their questions, on average perform better than the others. In contrast, the loan officers that ask more questions are associated with lower performance. Table 4 confirms this intuition showing the officers' performance for those who have shared information more or less than the median officer. The first column shows that there is no significant difference in performance between officers who had access to greater or fewer numbers of documents than the median. The second column, instead, shows that there exists a positive correlation between the number of answers provided and performance. The loan officers who provide a higher number of answers perform significantly better than the others. The third column confirms this result showing that those who ask more questions, above the median, perform significantly worse than the others. These results suggest that information sharing is correlated with performance and with the officers' innate ability.

In the remainder of the paper, we present formal evidence to shed light on whether these descriptive results are robust to controlling for other determinants of performance. In doing so, we make precise the underlying identifying assumptions required to interpret when this evidence is causal and present evidence in support of these identifying assumptions.

3 Information Sharing and Worker Performance

In this section we explain our approach and the methodology we employ to estimate the effect of communication and information sharing on performance.

3.1 Longitudinal Specifications

The analysis proceeds in two stages. First, we estimate the effect of information sharing on loan officers employing a longitudinal model that allows us to control for unobserved heterogeneity between branches and loan officers. Next, we take advantage of an anti-corruption law that requires officers to switch branches every few years to identify the causal effect of information sharing on performance.

To identify whether information sharing affects officer performance, we estimate the following panel-data regression:

$$y_{i,j,k,t} = \alpha_1 I^a_{i,j,k,t} + \alpha_2 I^g_{i,j,k,t} + \alpha_3 I^p_{i,j,k,t} + \lambda_t + \eta_k + \gamma_j + \delta_i + \beta_1 T_i + \beta_2 C_i + \varepsilon_{i,j,k,t} \quad (1)$$

where $y_{i,j,k,t}$ is officer i's log performance in branch j in group k and during term t. The main variables of interest are I^a, I^g, and I^p which capture the number of documents consulted, questions posted and answers provided by officer i in branch j. The time fixed effects λ_t account for unobserved shocks that might have affected both the officers' performance and their demand for information, such as those arising during a financial crisis. The group fixed effects η_k capture permanent productivity differences across regions and tasks, such as those arising from the different clientele and heterogeneity of loan types. The branch fixed effects γ_j allows us to control for permanent productivity differences across branches, such as those arising from a more profitable location or a better head manager of the branch. Finally, individual fixed effects δ_i provides the possibility of controlling for innate ability or motivation. We also include the tenure of the loan officer T_i, when we do not include officers' fixed effects and the dummy C_i which is equal to 1

if the loan officer joined the bank right after college and equal to 0 if he had previous experience in the banking sector.

We also note that information sharing and performance are unlikely to be identically and independently distributed within a branch. We therefore adopt a conservative strategy when estimating standard errors and allow the disturbance ε_{ijkt} to be clustered by officer throughout.[6]

3.2 Instrumental Variable Specification

To better assess the causal effect of communication and information sharing on performance, we exploit the mandatory switching of loan officers across branches. Since there is variation in usage of the information sharing platform across branches, we can construct an instrument based upon the attitude of a given branch toward the new technology. For each loan officer i, we construct an instrument Z_{-i} which is the amount of information accessed in the previous branch excluding officer i. The choice of this instrument is motivated by the idea that if officer A worked in a branch where problems are usually resolved within the branch, without attempting to find solutions elsewhere in the organization, then even when officer A moves to a different branch, he will have been trained to communicate less with other loan officers. In contrast, if officer A worked for a branch where the access to information provided by others is encouraged, he will carry that attitude into the new branch. We construct similar instruments for each of our three endogenous variables of interests: the number of documents accessed, the number of questions posted, and the number of answers provided.

Formally, the first stage for each endogenous variable $e \in \{a, g, p\}$ is represented by:

$$I^e_{i,j,k,t} = \alpha_1 Z^a_{-i,j,k,t} + \alpha_2 Z^g_{-i,j,k,t} + \alpha_3 Z^p_{-i,j,k,t} + \lambda_t + \eta_k + \gamma_j + \beta_1 T_i + \beta_2 C_i + \eta_{i,j,k,t}$$

[6] Clustering the disturbance terms by branch leads to the standard errors on the parameters of interest being considerably smaller than those we report.

while the second stage is

$$y_{i,j,k,t} = \alpha_1 \widehat{I}^a_{i,j,k,t} + \alpha_2 \widehat{I}^g_{i,j,k,t} + \alpha_3 \widehat{I}^p_{i,j,k,t} + \lambda_t + \eta_k + \gamma_j + \beta_1 T_i + \beta_2 C_i + \varepsilon_{i,j,k,t}$$

where we have employed three instruments for the three endogenous variables. The validity of this instrumental-variable procedure relies on the relevance of our instruments and their exogeneity. First, we shall show that our methodology is not affected by the "weak instrument" problem, in fact, the coefficients in the first stage regressions for each endogenous variable are highly significant, and the F-Test is always above 10, the standard threshold for weak instruments. Second, we have constructed our instruments for officer i, excluding officer i from the computation of the information accessed in his branch. This should reduce the correlation between the instrument and officer i's innate ability. However, since our instruments rely on variations in social norms across branches we are afraid that officer i might have contributed to the branch's culture in the past, which could bias our estimates. We address this concern by restricting attention to larger branches (with more than 50 officers), for which this possibility is, at least, less likely.

Furthermore, to show that the exclusion restrictions hold, we shall show that the assignment of officers to new branches is not affected by their own past performance. In particular, we show that the timing of their move across branches is not a function of their productivity (e.g. officers are not rewarded by being moved to branches in larger cities). Moreover, we also show that where the officers are going to be transferred to is not influenced by the productivity of the branch looking to locate an additional officer, that is, we find no evidence of *cherry picking*, e.g. the officers with the highest performance being moved to the most (or the least) productive branches.

4 Baseline Results

Table 5 presents estimates of our baseline specification (1). The results show that the pattern of unconditional differences in worker performance by information sharing is

robust to conditioning on a rich set of determinants of officer performance. It presents estimates for the main parameter of interest showing that the number of documents and the number of answers are positively correlated with individual performance, while the number of questions is instead negatively correlated with their performance.

Furthermore, tenure significantly affects performance, which can be the result of a learning process within the Bank. At the same time, joining the bank right after college, without any previous experience, is positively correlated with performance. This result can be interpreted as a result of the greater effort exerted by new employees.

The main concern with these results is that information sharing and the effect on performance might be driven by other factors, such as a market downturn, a greater need for information for a specific local market, or a result of a better performing branch. In order to control for all this unobserved heterogeneity, as shown by column (4), we control for time, group and branch fixed effects. Except for the effect of the number of answers, the other coefficients are still economically and statistically significant.

In column (5) we further control for the interaction of time and branch fixed effects, which shows that the results are robust to this more restrictive specification. These results suggest that when officers increase the number of documents accessed, this has a positive and significant effect on the productivity of the average worker, whereas increasing the number of questions has a negative impact on his performance. The magnitude of these effects implies that when officers increase their information access by one standard deviation it increases their performance by eleven percent. Similarly, an increase in the number of questions is associated with a reduction in performance of about five percent.

A concern with these results is that the estimation might be picking up heterogeneous effects that are unrelated to information sharing, in particular one of the main factors for which we cannot directly control: officer's ability. It is plausible that innate ability has a significant effect, which would create a spurious correlation between information sharing and performance. For example, it is likely that a loan officer who is able to close a higher number of deals and identify the most profitable ones will post fewer questions. Then, observing a negative correlation between the number of questions and the offi-

cers' performance might just be driven by heterogeneous innate ability or differences in training.

Column (6) of Table 5 therefore provides evidence on the effects of communication and information sharing on the productivity of the *same* worker. We exploit the longitudinal nature of our data and control for individual fixed effects. In accord with the descriptive evidence presented in the previous section, once we control for the individual fixed effects as in column (5), the number of questions is not significant anymore. However, we find an even stronger effect for the number of documents. This correlation suggests that performance could be significantly affected by the number of documents consulted by the loan officers, even controlling for individual ability. As highlighted in the introduction, we interpret this as evidence of the possibility that loan officers know more successful management practices implemented elsewhere in the bank. We instrument this explicitly in Section 5

4.1 Who Benefits the Most from Information?

To explore whether the effects of information sharing are heterogeneous across loan officers, we use quantile regression methods to estimate the conditional distribution of the log of performance of loan officer i in branch j, and group k during the term t, $y_{i,j,k,t}$, at different quantiles, θ. We therefore estimate the following specification:

$$Quant_{\theta}\left(y_{i,j,k,t}|\cdot\right) = \alpha_{1\theta} I^{a}_{i,j,k,t} + \alpha_{2\theta} I^{g}_{i,j,k,t} + \alpha_{3\theta} I^{p}_{i,j,k,t} + \beta_{\theta} X_{i,j,k,t} + \varepsilon_{i,j,k,t} \tag{2}$$

All variables are as previously defined, and bootstrapped standard errors, based on 1000 replications, are calculated throughout. The effect of information access, gathering, and production on officers' performance at the θth conditional quantile of log performance is measured by the vector α_{θ}.

Table 6 reports the estimates of α_{θ} from the specification above at various quantiles, controlling for tenure and experience as well as time, group and branch fixed effects. Two points are of note. First, the effect of information access is zero for the top two

quantiles, and is positive and significant at the bottom three quantiles. Second, the effect of information gathering as measured by the log of the number of questions posted is negative and significant for all quantiles. In particular, a ten percent increase in the number of documents predicts a performance increase of at least twenty percent, whereas the same increase in the number of questions posted predicts a decrease of more than twenty-five percent.

The data suggest that information access increases the performance of loan officers in the left tail of the productivity distribution, while it has no significant effect on officers' performance in the right tail of the distribution. These results provide evidence that information transfers might help the low performing officers learn from the most productive officers without disrupting the higher performers. That is, the learning effect dominates the substitution effect.

This result has two implications. First, it suggests that in a distributed decision *polyarchy* (see Sah and Stiglitz (1986)), like the setting investigated here, the agents benefit from communicating with each other due to the replicability of their decisions. For example, two loan officers serving different clients might improve their productivity by sharing information, because the profitability of these loans is affected by common factors such as the credit market conditions and the available financial products. Second, since we do not observe any negative effects on high-skill agents' productivity, we can conclude that these white-collar workers do not suffer from the "information overload" problem identified by Van Zandt (2004). This is probably due to the digital platform's ability to disseminate reusable information without having high performers re-enter answers to the same questions.

4.2 Soft Information and Productivity

Up to now we have found evidence that communication and information sharing might help the low-performing officers acquire the necessary knowledge to improve their performance. If this is true, we should then expect officers to significantly increase their

access to the available information when they switch branches. A loan officer might, for example, start working in a different environment, with different existing customers, and a different local credit market, which should have a significant impact on his demand for information. The switchers might then require information about the reliability of the customers and the conditions offered by competitors. We observe 618 loan officers who switched branches, as prescribed by the anti-corruption law, after two years of experience in the same branch.

As a first step, we investigate whether or not Bank headquarters relocates officers to different branches based upon their performance. On the one hand, it might be that in order to improve the productivity of a branch with below-average productivity, the Bank might find it optimal to allocate the best officers to the branches that need to improve their productivity. On the other hand, the high-performing officers might be rewarded by being allocated to the "best" branches. Figure 2 favors the latter case. It displays the slightly positive relation between the mean officers' performance before the switch and the productivity of the branch where they work after the switch. This means that the Bank does not employ the switching rule to strategically locate officers across branches to improve branch productivity.

Table 7 reports coefficient estimates on job rotation relative to document consumption – indicator variable "switch" is equal to one when a loan officer moves from one branch to another. As highlighted by columns (1) and (2) the results are consistent with the learning hypothesis. Even controlling for time, group, branch and individual fixed effects, the coefficient is positive and both statistically and economically significant. This suggests that switching might be an important determinant of the demand for information. Intuitively, the less-experienced officers would try to acquire a greater amount of information, as shown by the coefficients on tenure, negative but insignificant, and the coefficient on college, which is instead positive and significant.

However, if the demand for information is driven by a temporary need driven by the new environment, we should observe a diminishing effect of switching over time. Columns (3) and (4) investigate this issue, presenting the estimate for another indicator variable,

"after switch," which equals one for all terms after the switch. The effect is still positive, but no longer significant. This confirms the hypothesis that switching has only a short-run impact on the demand for information.

Since we have identified a significant effect of switching on communication, it is now natural to investigate the level of costs associated with the application of this anti-corruption law. In particular, we can investigate if switching has a positive or negative effect on the loan officers' performance. On the one hand, switching might result in the officers exerting more effort during the first few months of the new appointment to signal their ability. On the other, loan officers might have acquired some specific knowledge about the type of firms and market conditions in the previous branch, which suggests that after the switching they would need more time to learn work practices in other environments.

Table 8 presents evidence that strongly supports the latter hypothesis. As shown by columns (3) and (4), switching has a negative and significant effect on performance. Moreover, this effect is even stronger for longer-tenured officers, as suggested by the negative coefficient on the interaction term between the indicator variable and the officers' tenure.[7] This means that even if loan officers might tend to work more when they are forced to change branch, the overall impact on their performance is negative. As in the case of the demand for information we should expect a decreasing effect of switching on the officers' performance over time. Column (5) shows the coefficient estimates on the indicator variable that accounts for all the time after the change of branch. Although still significantly negative, its magnitude is diminished.[8]

The last two columns (6) and (7) assure that these results are robust to the inclusion of individual fixed effects. Overall this evidence suggests that implementing a switching rule as a way to prevent bribery and corruption can impose high costs. In particular our

[7] Notice that this suggests that there is important soft information that get lost in the rotation across branches. In fact, if the experience accumulated in one branch is very useful in another, then we should find that the shorter-tenured officers are more affected by the rotation, while this is not the case.

[8] The negative effect of relocation on performance can be driven by adjustment costs borne by the loan officers, who have to adapt themselves to different branch social norms. However, at least part of this is captured by the inclusion of the branch fixed effects, which should control for unobserved cultural heterogeneity.

estimates suggest that some specialized human capital is destroyed when a loan officer switches from one branch to another. In contrast to the existing literature on worker displacement (which investigates the effect of layoffs on earnings), we are able to estimate the effect of turnover directly on performance. Moreover, we have the advantage of analyzing a sample of white-collar workers that switch locations within the *same* organization, and with the same tasks. This guarantees that the negative shock to officers' performance is not driven by the relocation to a different firm or to a job that requires another set of qualifications.

Finally, a different way in which we can explain our results in table 8, is that incoming officers get worse projects than incumbents. One plausible rationale is the following. One can think that officers who worked in outstanding branches get tougher cases to handle. Although, we do not find the assumption that the performance of the various branches is common knowledge among loan officers satisfying, we can exclude this possibility with a different argument. In table 8 we show that the negative effect on performance declines over time. However, if the fact that the new officer has previously worked in a more demanding environment motivates the type of projects assigned to him, he should always get tougher projects. It would be suboptimal for the new branch to stop employing his superior ability after few months. Moreover, in section 5.1 we further rule out this hypothesis by analyzing the aggregate productivity of the branch before and after the new loan officer joined the branch.

5 Instrumental Variable Estimates

Up to now, the evidence presented strongly suggests the existence of a significant effect of communication on officers' performance. Specifically, there is substitution between the demand for information and the innate ability of officers. Moreover, low performers or officers who just switched to a new branch significantly increase their demand for information. We can now address a natural endogeneity problem that can arise in our context. The loan officer who is facing a market contraction, for example, can *decide* to

acquire more information in order to improve his performance. Although in the previous estimates we have accounted for a variety of unobserved shocks, with the inclusion of a set of fixed effects, we now try to address this endogeneity issue in order to understand whether we can interpret the results as causal or not.

Since we do not have data before adoption of the new technology, which allowed for information to be shared, we are not able to run a natural experiment to understand the effect of information sharing on officers' performance. Nevertheless, we are able to address this issue by employing the anti-corruption law as a source of exogenous variation. For each officer i who switched from branch j at time t to branch j' at time $t+1$, we use the number of documents (as well as questions and answers) accessed in the branch j as an instrument for the number of documents (and the number of questions and answers) that officer i had access to in branch j' at time $t+1$. That is, we exploit the variation in branch attitudes toward information sharing to determine the effect of communication on performance. Then, we shall use the cross-sectional variation among switchers to identify the effect of communication on performance.

Table 9 presents the first stage estimate for each one of the endogenous variables. The first column reports the coefficient estimates of our instrument for the number of documents, which shows that both the coefficient (positive and statistically significant at the one percent level) and the F-test (above 10) strongly suggest that our instruments are not weak. Columns (2) and (3) present the first stage instruments for the number of questions and the number of answers. As for the documents, our instruments seem to significantly affect the demand for information. Intuitively, for all three variables of interest, tenure has a negative and significant effect, which confirms that even restricting attention only to the switchers; the more experienced people demand less information. Given the small sample of switchers we are not able to control for branch fixed effects, but we include both time and group fixed effects.

Table 10 presents the ordinary least-square estimates restricted to the switchers sample and the instrumental-variable estimates. The coefficient on the number of documents is negative and not significant for all the OLS estimates while positive and highly signifi-

cant for the IV estimates. This can be interpreted as evidence in favor of our substitution hypothesis, that is, high-ability officers tend to seek out less information than their colleagues. The magnitude is also interesting, in fact; the most conservative specification in column (6) suggests an effect of about ten percent on performance. This means that incentivizing the usage of the information produced by others within the same organization might actually result in a significant improvement in productivity.

Interestingly, Black and Lynch (1996) found that a 10% rise in average education, roughly one year of schooling, led to an 8.5% productivity increase in manufacturing and a 12.7% increase in non-manufacturing. The ten percent gain we find in banking therefore appears comparable to just under one year of education.

Our second variable of interest, the number of questions, has a negative effect on performance in both the OLS and IV estimates. This is consistent with the previous results, and suggests that even the exogenous variation in the number of questions negatively affects performance. The magnitude is higher for our IV estimates than in the OLS results, ranging from five percent to almost twenty percent. This confirms the substitution between officers' ability and the number of questions posted.

Finally, in contrast to the panel estimates presented above, the number of answers has a significant, positive effect on performance. These estimates show that the exogenous variation in the number of answers has an impact on performance. However, the coefficients on both the number of questions and answers should be interpreted carefully because, based on the panel analysis of the previous section, we know that these might not be robust to the inclusion of individual fixed effects.

5.1 Discussion of the Exclusion Restrictions

The main concern with our instrument is that it might fail the exclusion restrictions. There are three potential issues with our instrument: loan officer i might have an effect on the previous branch's social norm; the assignment of loan officers to branches is not random, and the endogeneity of loan portfolios assignment to incoming officers. We

devote this section to discuss how we can address each one of these concerns within our empirical framework.

First, one might imagine that even if we do not include officer i's demand for information in the construction of our instrument, he might have had an effect on the cultural attitude of the branch regarding information sharing. However, this effect should be more pronounced for small branches than for larger branches. Table 11 shows that even when we restrict attention to the subsample of branches with more than 50 loan officers, we find the same results. That is, the number of documents consulted and the number of answers provided positively affect performance, while the impact of the number of questions is negative. As expected given the lower number of observations, the estimates are significant only at the five- and ten-percent level in the most conservative specification shown in column (6). However, both the magnitudes and the signs are consistent with the previous results.

Second, we have already shown in figure 2 that there is very small evidence of *cherry picking*, e.g. the officers with the highest performance being moved to the most productive branches. Even if this were the case, then we should expect the estimated effect of switching on performance to be biased downward. This makes our result, that performance is significantly affected after the rotation took place, even more striking. Furthermore, we can test whether the timing of the officers' transfer across branches is a function of their productivity, e.g. officers that underperform are more likely to be relocated in a new branch. Table 12 shows that the likelihood of officer i being rotated is not significantly affected by his past performance. Then, there is no evidence of the headquarters strategically timing the rotation of the loan officers.

Third, one problem with the interpretation of our results might come from the possibility of incoming officers being assigned less desirable loan portfolios. We can address this concern by investigating the aggregate branch productivity before and after a new officer joined the branch. The test we perform is motivated by the following idea: it is reasonable to assume that every branch has a certain flow of bad loans (which can fluctuate over time) determined, for instance, by the business cycle and the credit worthiness of

the local firms, which ultimately determines the aggregate productivity of the branch. To analyze if this bad loans are assigned to the incoming officers or not, we can analyze the branch aggregate productivity before and after a new officer joins the branch. If there is no drop in the branch productivity after the new officer joined the branch, then it is very likely that the poor-performing loans are merely given to the incoming officer. This might explain why loan officers tend to underperform in the new branch and try to compensate this adverse loans assignment by increasing their demand for information. However, if there is a significant drop in productivity after the officer joined the branch, then this suggests that indeed the incoming officer's performance has been adversely affected by the rotation, which would suggest that specific human capital, such as the knowledge of the local businesses and market conditions, has been destroyed.

The latter hypothesis is supported by the evidence presented in Table 13. It shows that the productivity of the branches involved in the rotation is significantly decreased, once the incoming managers join the new office. Specifically, after controlling for time and branch fixed effects, the branch productivity decreases on average by 7 percent as shown in column (3). This reassures us that even if different officers get assigned different firms, projects and loan applications to evaluate, there is no evidence of selection bias in our estimates. In other words, the incoming loan officers reduced performance decrease the productivity of the whole branch.

6 Implications

We devote the next section to implications of our empirical findings for three strands of literature. In the first section, we consider theoretical and empirical results from tournament research that bear on information sharing incentives and thus group productivity. We then analyze human capital inefficiencies generated by the mandatory-transfer regulation. Finally, we interpret our findings in the context of existing studies on relational banking.

6.1 Information Sharing and Tournaments

If information sharing affects productivity, an organizational design question arises as to how promotion incentives interact with sharing incentives. Tournament theory, as modeled by Lazear and Rosen (1981), models promotions as a *relative* game, that is, prizes depend on relative rather than absolute performance. The compensation at one level of the firm, in addition to motivating individuals at that level, motivates those at lower levels.

These basic ideas of tournament theory have been extended in numerous ways.[9] In particular, Dye (1984) and Lazear (1989) consider how the potential for collusion, sabotage, or other forms of non-cooperative behavior counter the incentive value generated by promotions and tournaments. Chan (1996) suggests that handicapping insiders in the tournament, with respect to external hires, can help mitigate the possibility of influence activity or sabotage. Then, the existing theoretical literature has recognized the costs and inefficiencies generated by the implementation of a promotion-based incentive system. Prendergast (1999) discusses the possibility that incentives from promotion methods give rise to dysfunctional behavioral responses and that companies adjust management compensation to address some of the negative responses from promotion incentives.

Empirically, many studies have confirmed these predictions from tournament theory. Using a survey of Australian firms, for example, Drago and Garvey (1998) show that individuals are less helpful and work harder when promotion incentives are strong. This seems to suggest that workers incentivized via promotions are less willing to cooperate with each other, because the effort to help others may reduce their own probability of being promoted.

In contrast to these observations, we find that loan officers intensively cooperate with each other, sharing their knowledge and in doing so they indirectly improve their colleagues' performance. This result seems to suggest that it is possible to reconcile competitive promotion incentives with cooperation in knowledge-sharing. We attribute

[9] See Gibbons and Waldman (1999) for a survey of this literature.

this novel result to the relative weight placed on tenure in Japanese banking and to the dual role of posting questions and providing answers. On the one hand, sharing information with competitors can increase their chances of promotion via increased performance. On the other hand, helping others signals skill and is recognized and rewarded by the organization.

6.2 The Cost of Anti-Corruption Regulation

Corruption is recognized as a driving factor in persistent poverty in less-developed countries but also a source of inefficiency and rent extraction in developed countries. The World Bank ranks the fight against corruption as a top priority for poverty reduction. Much theoretical work, since Becker and Stigler (1974) and Tirole (1986), focused on understanding the incentives and the constraints within corruptible bureaucracies.[10]

Existing evidence, however, on anti-corruption policies shows that corruption is fought and defeated with very simple tools. For example, Klitgaard (1991) describes successful cases of corruption elimination, such as in the Hong Kong Police Force and the Singapore Excise Department. The main factors were better monitoring and replacing individual bad actors. Similarly, Olken (2007) analyzes a randomized field experiment on reducing corruption in Indonesia suggesting that traditional top-down monitoring can play an important role in reducing corruption, even in a highly corrupt environment.

This gives rise to a more fundamental question: if these levers for eliminating corruption are within the choice set of governments, why are they not more often implemented? A possible answer is suggested by Acemoglu and Verdier (2000). They identify a trade-off between market failures and government failures. That is, since preventing all corruption is excessively costly, the second-best intervention may involve tolerating a certain fraction of bureaucratic corruption. Then, government failures may indicate an unavoidable price of dealing with market failures.

In our setting we do not directly observe corruption, but we are able to quantify the cost associated with the anti-corruption regulation. If the implementation of the rotation

[10] See Banerjee and Mullainathan (2009) for a recent survey of this literature.

rule has been efficient, the social cost of enduring corruption should be higher than the banking inefficiencies generated by the remedial regulation. Then, we can infer the costs of corruption by analyzing the productivity loss associated with the officers' relocation. As a first step, we collected loan officers' performance in dollar terms. On average each officer generated almost four million dollars in bank gross profit every six months. Then, as is shown by Table 8, and holding other factors constant, switching reduces performance by more than 10%. This translates to a reduction in profits of 939,200 dollars a year for each officer transferred.

From this, we can conclude that in two years the Bank passed up more than 200 million dollars due to the adoption of this anti-corruption law. Since our data span only two years, we cannot analyze potential long term productivity gains that might arise from exposure to multiple branches. Yet, if corruption regulation has been optimally implemented, this estimate would constitute a reasonable bound on the amount of corruption avoided due to regulatory intervention.

6.3 Relational Banking

Boot (2000) defines relationship banking as the provision of financial services by a financial intermediary on the basis of long-term investment in obtaining firm-specific information through multiple interactions with diverse financial services. Banks are interested in relationship-based banking mainly for two reasons. First, the cost of information gathering is reduced by learning through repeated transactions. Second, financial contracts are typically incomplete: banks and customers can build commitment and reputation through repeated transactions across services. This banking model has been the predominant one in Japan.[11]

The possibility for a bank to build a long-term relationship with a client enables collection of soft information that is otherwise unavailable. The bond markets and the rating agencies collect financial disclosures, accounting reports, and default histories which can

[11] See Hoshi et al. (1990) and Hoshi et al. (1991) for an empirical analysis of the the role of banks in Japan.

be considered hard information. Banks collect information on the client's ability and his honesty, which cannot be easily communicated to others Petersen and Rajan (1994).

Stein (2002) argues that larger, more hierarchical banks, where the decision maker is further from the information collector, are more likely to rely on hard information, because such organizations are expected to be less efficient at making relationship loans. Information in a large bank is potentially collected by one individual or group and a decision made by another. Thus the decisions must be made on information that is easy to transmit across physical or organizational distances. Consistent with this intuition, Berger et al. (2005) find that larger banks are more likely to lend to more customers at a greater distance and communicate with the borrower more impersonally, i.e. by mail or phone as opposed to face-to-face.

Our paper contributes to this literature in two ways. First, we show that information sharing among relationship managers allows a large bank, like the one analyzed here, to build long-term relationships with clients by reducing the costs of communicating the soft information collected. The platform used by the Bank allows officers to store the relevant information about diverse clients and to effectively communicate this to their colleagues. Second, we highlight the negative impact that breaking the relationship between borrowers and loan officers has on the productivity of the latter, by showing that loans officers significantly underperform when they are relocated to a new branch.

7 Concluding Remarks

To address the question of whether access to information produced and gathered within the same organization affects the performance of loan officers, we examined two years of micro data from a major Japanese bank. Data include all accesses to an information-sharing platform, objective and subjective performance measures, and all job rotations among more than 2,800 loan officers. Exogenous legal requirements, aimed at curbing corruption by compulsory office rotation, permit analysis of loan officer performance in different settings.

We find that a standard deviation increase in the number of shared documents predicts an 11% rise in output, in specifications with time, location and individual fixed effects. This is comparable to just under one year of education among non-manufacturing workers Black and Lynch (1996). Quantile regression estimates suggest that communication and information sharing greatly benefits the low-performance officers. In fact, questions are more salient among workers of lesser ability and, when productivity gains exist, they appear strongly on the left tail of the distribution, at the 10^{th} and 25^{th} percentiles, but do not appear on the right tail, at the 75^{th} and 90^{th} percentiles.

We also observe 618 instances of loan officers switching branches as a result of an anti-corruption law. This exogenous shock provides an opportunity to observe the same knowledge worker in different contexts. With this instrument, and controlling for unobserved heterogeneity over time and branch, a standard deviation increase in shared document consumption boosts productivity by at least 10%. These results appear to be causal and enrich the growing literature on loan officers' behavior by highlighting the importance of sharing soft information among officers, rather than directly acquiring it from borrowers, in improving their productivity.

The difference between OLS and IV specifications provides evidence of the substitution hypothesis: high-ability officers demand less information than low-ability officers, while low-ability officers can compensate for low independent performance by consuming information provided by others. We also find that switching jobs significantly reduces overall performance, possibly indicating destruction of job specific human capital such as soft information about local businesses. The anti-corruption law should therefore avoid economic losses from graft of at least this magnitude. Interestingly, officers of all abilities increase their demand for information on switching jobs, which suggests that the possibility to gather information within the Bank is an important instrument in increase performance in the new environment.

A range of potential extensions is left for future research. It is important to understand how information sharing and communication are related to the incentive system in place. One could address this question by developing a similar analysis on micro data

from a major U.S. bank, which should clarify if end-of-year bonuses and an incentive system heavily based upon performance, affects the information shared between white collar workers. Moreover, it would be interesting to understand whether it is possible to increase the flow of information within the organization by explicitly relating information consumption to monetary incentives. Finally, we are also interested in the robustness of our results with respect to the possibility of anonymously seeking and providing information. On one hand, this could reduce embarrassment costs in requesting information and increase the provision of novel but controversial ideas. On the other hand, the quality of information could decrease due to lower signaling and reputation-building effects. This would enable tests of how incentives and reputation interact with organizational theories of the firm.

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Table 3: Summary Statistics

	Mean	Median	Min	Max	Std. Dev.
Total Performance	52	48	8	110	21
Number Documents	569	522	102	1618	282
Number Questions	77	45	4	585	123
Number Answers	250	238	4	813	164
Tenure	10	11	0	32	5

Notes: entries are the summary statistics for our main variables of interest. "Total Performance" is the sum of the objective and the subjective performance measures. "Tenure" is the number of years managers worked for the Bank. On average each manager obtains a score of 52 out of 100, downloads 569 documents, post 77 questions, and provides 250 answers over a six-month period. Overall there are 2451 manager-branch-term observations.

Table 4: Managers Performance by Information Sharing

	Number Documents	Number Answers	Number Questions
Below the Median	52.603	50.356	53.919
	(0.291)	(0.294)	(0.308)
Above the Median	52.025	54.438	50.727
	(0.306)	(0.306)	(0.287)
Difference	0.577	4.085***	-3.192***
	(0.422)	(0.420)	(0.421)

Notes: *** denotes significance at 1%, ** at 5%, and * at 10%. The standard errors clustered by manager are reported in parenthesis. Performance is measured as the total score assigned to a manager in a given branch.

Table 5: Panel Model – Fixed Effects Estimates

Log(Tot. Performance)	(1)	(2)	(3)	(4)	(5)	(6)
Log(Number Documents)	0.0493***	0.0562***	0.0226**	0.0300***	0.0216**	0.0374**
	(0.010)	(0.011)	(0.010)	(0.011)	(0.011)	(0.018)
Log(Number Questions)	-0.0505***	-0.0335***	-0.0246***	-0.0217***	-0.0195***	-0.0104
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)
Log(Number Answers)	0.0508***	0.00848	0.00533	0.00337	0.00475	-0.0062
	(0.003)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Log(Tenure)	0.0632***	0.0674***	0.105***	0.100***	0.1000***	
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)	
College	0.108***	0.113***	0.105***	0.0924***	0.0891***	
	(0.028)	(0.028)	(0.026)	(0.026)	(0.025)	
Time Fixed Effects		YES	YES	YES	YES	YES
Group Fixed Effects			YES	YES	YES	YES
Branch Fixed Effects				YES	YES	YES
Time * Branch Fixed Effects					YES	
Individual Fixed Effects						YES
Observations	9,805	9,805	9,805	9,805	9,805	9,805
R-squared	0.0629	0.0582	0.1567	0.3049	0.467	0.157

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the log of total performance. "College" is a dummy variable equal to one if the manager joined the Bank directly after college. The time period is 2006-2008. The estimation method in all columns is OLS. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, group and branch fixed effects. As additional controls Column (5) includes time dummies interacted with branch dummies, while Column (6) includes individual fixed effects.

Table 6: Quantile Regression Estimates

Log(Total Performance)	10th	25th	50th	75th	90th
Log(Number Documents)	0.0391**	0.0409***	0.0250*	0.002	-0.012
	(0.017)	(0.012)	(0.014)	(0.013)	(0.010)
Log(Number Questions)	-0.0207***	-0.0251***	-0.0372***	-0.0361***	-0.0164***
	(0.007)	(0.008)	(0.007)	(0.006)	(0.006)
Log(Number Answers)	0.004	0.008	0.013	0.0209**	0.006
	(0.010)	(0.012)	(0.012)	(0.009)	(0.007)
Observations	9,805	9,805	9,805	9,805	9,805

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the log of total performance. All specifications control for time, group, and branch fixed effects.

Table 7: Effect of Switching on Information Access

Log(Number Documents)	(1)	(2)	(3)	(4)
Switch	0.0199**	0.0218**		
	(0.009)	(0.009)		
After Switch			0.0125	0.0150*
			(0.008)	(0.008)
Log(Tenure)	-0.0218		-0.0217	
	(0.014)		(0.014)	
College	0.278***		0.279***	
	(0.042)		(0.042)	
Time Fixed Effects	YES	YES	YES	YES
Group Fixed Effects	YES	YES	YES	YES
Branch Fixed Effects	YES	YES	YES	YES
Individual Fixed Effects		YES		YES
R-squared	0.3342	0.347	0.334	0.346
Observations	10,055	10,055	10,055	10,055

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the log number of documents downloaded in a six-month period. The estimation method in all columns is OLS. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, group and branch fixed effects. As additional controls Column (2) and (4) include individual fixed effects. "Switch" is a dummy variable equal to one only in the first term after the manager's relocation to another branch. "After Switch" equals one for all the terms after the transfer. "College" is a dummy variable equal to one if the manager joined the Bank directly after college.

Table 8: Effect of Switching on Performance

Log(Total Performance)	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Switch	-0.111***	-0.0317	-0.118***	-0.0461*		-0.108***	
	(0.012)	(0.027)	(0.012)	(0.027)		(0.013)	
Switch*Tenure		-0.00778***		-0.00726***			
		(0.002)		(0.002)			
After Switch					-0.0889***		-0.0915***
					(0.010)		(0.012)
Log(Number Documents)	0.0470***	0.0469***	0.0287***	0.0283***	0.0274**	0.0387**	0.0348**
	(0.010)	(0.010)	(0.010)	(0.010)	(0.010)	(0.018)	(0.018)
Log(Number Questions)	-0.0515***	-0.0516***	-0.0210***	-0.0210***	-0.0205***	-0.00851	-0.0078
	(0.005)	(0.005)	(0.005)	(0.005)	(0.005)	(0.007)	(0.007)
Log(Number Answers)	0.0569***	0.0571***	0.0059	0.00618	0.00476	-0.00394	-0.0052
	(0.003)	(0.003)	(0.006)	(0.006)	(0.006)	(0.007)	(0.007)
Log(Tenure)	0.0636***	0.0700***	0.0994***	0.105***	0.0994***		
	(0.009)	(0.009)	(0.008)	(0.008)	(0.008)		
College	0.106***	0.104***	0.0915***	0.0907***	0.0897***		
	(0.027)	(0.028)	(0.026)	(0.026)	(0.026)		
Time Fixed Effects			YES	YES	YES	YES	YES
Group Fixed Effects			YES	YES	YES	YES	YES
Branch Fixed Effects			YES	YES	YES	YES	YES
Manager Fixed Effects						YES	YES
Observations	9,805	9,805	9,805	9,805	9,805	9,805	9,805
R-squared	0.0698	0.0701	0.3078	0.308	0.3054	0.166	0.165

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variable in all columns is the log of total performance. The estimation method in all columns is OLS. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, group and branch fixed effects. As additional controls Column (6) and (7) include individual fixed effects. "Switch" is a dummy variable equal to one only in the first term after the manager's relocation to another branch. An interaction term between "Switch" and "Tenure" is included in columns (2) and (4). "After Switch" equals one for all the terms after the transfer. "College" is a dummy variable equal to one if the manager joined the Bank directly after college.

Table 9: First Stages

	Documents	Answers	Questions
Documents Prev Branch	7.398***	-0.654*	-0.214
	(0.744)	(0.362)	(0.248)
Answers Prev Branch	-14.439***	4.190***	-1.637**
	(2.282)	(1.112)	(0.757)
Questions Prev Branch	8.420***	2.271**	7.437***
	(2.157)	(1.052)	(0.718)
Tenure	-10.419***	-4.132***	-2.688***
	(2.622)	(1.278)	(0.873)
College	54.021	19.242	-2.409
	(69.580)	(33.915)	(23.161)
F-Test	50.289	16.774	49.343
Time Fixed effects	YES	YES	YES
Group Fixed Effects	YES	YES	YES
Observations	618	618	618

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. The dependent variables are the number of documents, answers and questions in a six-month period after the relocation. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, and group fixed effects. "Documents Prev Branch" is the average number of documents consulted within the branch before the relocation. Similarly for "Answers Prev Branch" and "Questions Prev Branch". "College" is a dummy variable equal to one if the manager joined the Bank directly after college. Overall we observe in our sample 618 managers switching branch.

Table 10: IV Estimates

Total Performance	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Number Documents	-0.00123	0.0197**	-0.00138	0.0225***	-0.00347	0.0194**
	(0.003)	(0.008)	(0.003)	(0.008)	(0.003)	(0.008)
Number Questions	-0.0306***	-0.133**	-0.0287***	-0.104***	-0.0169**	-0.0908***
	(0.008)	(0.061)	(0.008)	(0.034)	(0.008)	(0.033)
Number Answers	0.0175***	0.112	0.0148**	0.0727**	0.0114*	0.0674**
	(0.007)	(0.080)	(0.007)	(0.036)	(0.007)	(0.034)
Tenure	0.387**	0.871**	0.385**	0.689***	0.603***	0.819***
	(0.172)	(0.382)	(0.174)	(0.242)	(0.183)	(0.223)
College	-3.736	-7.538	-3.714	-6.748	-3.414	-6.015
	(4.809)	(6.436)	(4.806)	(5.74)	(4.839)	(5.544)
Time Fixed Effects			YES	YES	YES	YES
Group Fixed Effects					YES	YES
Observations	618	618	618	618	618	618

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. In all columns the dependent variable is the manager's total performance. "College" is a dummy variable equal to one if the manager joined the Bank directly after college. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, and group fixed effects. The estimation method in columns (1), (3), and (5) is OLS. Columns (2), (4), and (6) are estimated using 2SLS. In both cases, we restrict attention to our subsample of 618 managers switching branch.

Notice that, albeit significant, we do *not* interpret causally the effect of the "number of questions" and the "number of answers" on performance. In fact, as shown by the results in column (6) of table 5, these effects would disappear if individual fixed effects were added to the specification.

Table 11: Robustness I: IV Estimates for Large Branches

Total Performance	(1) OLS	(2) IV	(3) OLS	(4) IV	(5) OLS	(6) IV
Number Documents	0.007	0.026	0.006	0.0129*	0.004	0.0156**
	(0.005)	(0.042)	(0.005)	(0.007)	(0.005)	(0.007)
Number Questions	-0.0455***	0.224	-0.0384***	-0.0422**	-0.0199**	-0.0377*
	(0.009)	(0.683)	(0.009)	(0.019)	(0.010)	(0.021)
Number Answers	0.0316***	-0.387	0.019	0.020	0.011	0.0392*
	(0.011)	(1.049)	(0.013)	(0.025)	(0.013)	(0.021)
Tenure	0.243	-0.039	0.211	0.269	0.603**	0.695**
	(0.272)	(1.100)	(0.278)	(0.269)	(0.282)	(0.286)
College	-25.25***	-13.650	-22.65***	-24.11***	-19.94***	-23.44***
	(7.129)	(33.150)	(6.421)	(6.669)	(7.420)	(8.608)
Time Fixed Effects			YES	YES	YES	YES
Group Fixed Effects					YES	YES
Observations	240	240	240	240	240	240

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. In all columns the dependent variable is the manager's total performance. Large branches are those with more than 50 employees. "College" is a dummy variable equal to one if the manager joined the Bank directly after college. Standard errors in brackets under coefficients in all columns are clustered by individual (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns include a full set of time, and group fixed effects. The estimation method in columns (1), (3), and (5) is OLS. Columns (2), (4), and (6) are estimated using 2SLS. In both cases, we restrict attention to our subsample of 240 managers switching branch.

Notice that, albeit significant, we do *not* interpret causally the effect of the "number of questions" on performance. In fact, as shown by the results in table 5, this effect would disappear if individual fixed effects were added to the specification.

Table 12: Robustness II: Probability of Being Relocated and Past Performance

Switch	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Tenure	-0.00916***		-0.0129***	-0.0128***	-0.0139***	-0.0217***	-0.0314***
	(0.0031)		(0.0034)	(0.0035)	(0.0035)	(0.0037)	(0.0040)
Lag Productivity		-0.00159*	-0.0013	-0.0013	-0.0007	0.0002	0.0004
		(0.0008)	(0.0008)	(0.0008)	(0.0009)	(0.0009)	(0.0009)
College				-0.0706	-0.0512	-0.0134	-0.0167
				(0.0992)	(0.0999)	(0.1010)	(0.1080)
Time Fixed Effects					YES	YES	YES
Group Fixed Effects						YES	YES
Branch Fixed Effects							YES
Observations	10,060	7,108	7,108	7,108	7,108	7,108	6,993

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. All columns estimated by probit maximum likelihood. The dependent variable in all columns is a dummy variable equal to one if the loan officer is relocated to another branch at time t. "Lag Productivity" is the managers' total performance recorded at t-1. All columns include "Tenure" as control variable. "College" is a dummy variable equal to one if the manager joined the Bank directly after college. Additional controls include time, group, and branch fixed effects.

Table 13: Robustness III: Branch Aggregate Productivity

Branch Productivity)	(1)	(2)	(3)
Switch	-75.85***	-83.43***	-7.174***
	(11.47)	(11.28)	(1.854)
Time Fixed Effects		YES	YES
Branch Fixed Effects			YES
Observations	1129	1129	1129
R-squared	0.004	0.029	0.881

Notes: * significant at 10%; ** significant at 5%; *** significant at 1%. In all columns the dependent variable is the branch aggregate quantitative productivity. "Switch" is a dummy variable equal to one after an incoming manager joins the branch. The estimation method in all columns is OLS. Standard errors in brackets under coefficients in all columns are clustered by branch (i.e. robust to heteroskedasticity and autocorrelation of unknown form). Columns (2) include time fixed effects, while Column (3) include time and branch fixed effects.

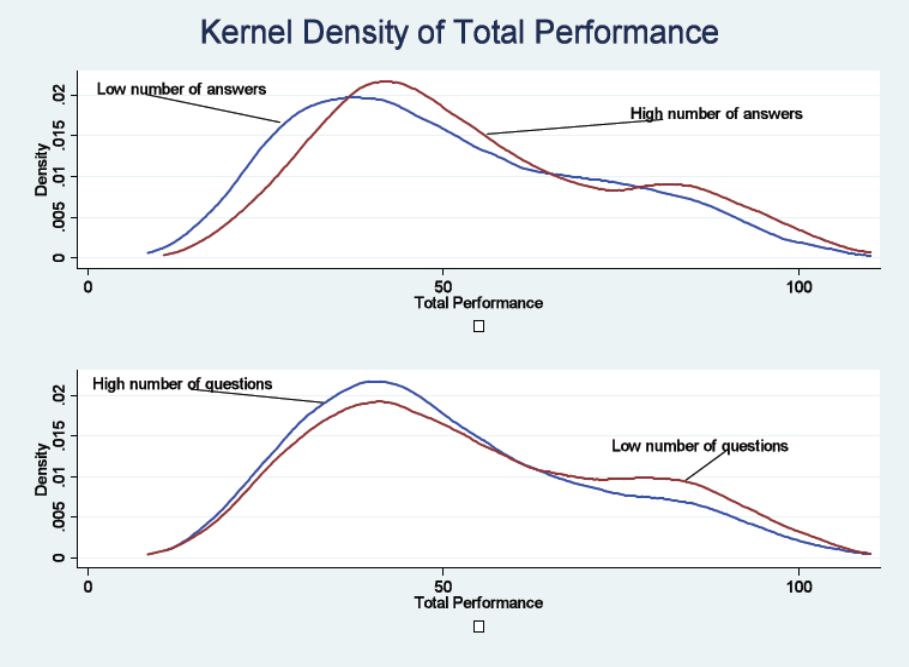

Figure 1: The plot displays the kernel density estimation of Total Performance for managers that provided an above (below) the median number of questions and answers. The density estimates are calculated using an Epanechnikov kernel.

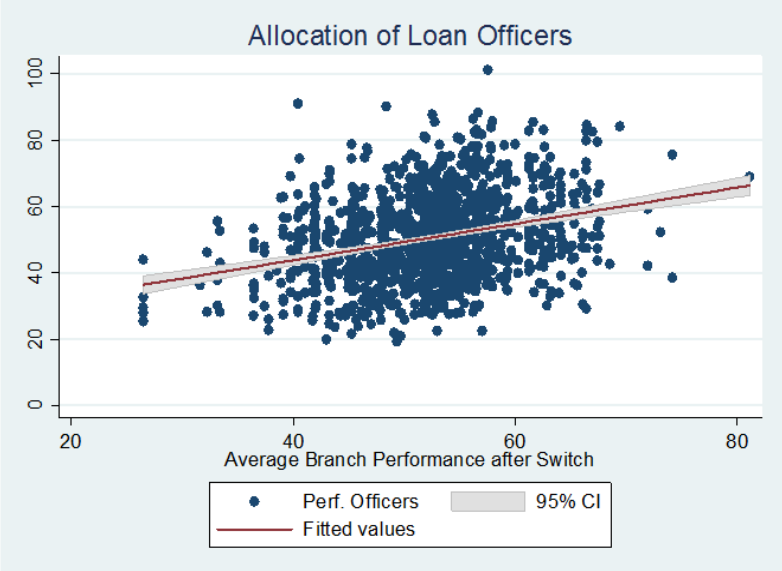

Figure 2: The plot displays the relationship between the officers' average performance in the term before the relocation to another branch, and the average productivity of the branch to where they are relocated. The fitted values and the 95% confidence interval are reported.