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Macroeconomic Lessons from the Great Recession:

Evidence using Microeconomic Methods

By

Gabriel Isaac Chodorow-Reich

A dissertation submitted in partial satisfaction of the

requirements for the degree of

Doctor of Philosophy

in

Economics

in the

Graduate Division

of the

University of California, Berkeley

Committee in charge:

Professor Christina Romer, Chair

Professor Yuriy Gorodnichenko

Professor Atif Mian

Professor James Wilcox

Spring 2013

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Abstract

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This dissertation reflects two recent developments in the study of economics. The first concerns real world events. Beginning in 2007, the countries of Western Europe and the United States experienced a series of financial shocks and an economic downturn unprecedented in their experience since the Great Depression seventy years before. Dramatic changes in the economy raise new questions or renew old lines of inquiry. During the recession, many countries turned to discretionary fiscal stimulus packages. The size of these packages and the severity of the downturns make understanding their effectiveness of critical importance. The first chapter in this dissertation analyzes empirically the effects on employment of one particular component of the fiscal policy response in the United States. The Great Recession also renewed interest in the importance of credit to firms. The second chapter in this dissertation attempts to answer whether the withdrawal of credit following the bankruptcy filing of Lehman Brothers played a role in propagating the Great Recession.

The second development consists of the application of unit-level datasets and applied microeconomic methods to answer questions of macroeconomic importance. This research program recognizes that cross-sectional variation across individual units often exceeds the time-series variation of an aggregate. Moreover, cross-sectional studies can hold constant many macroeconomic variables that might otherwise confound the identification of a causal effect. The studies of fiscal policy and firm credit in chapters 1 and 2 apply microeconomic methods. A full answer to these questions, however, requires a correspondence between the cross-sectional analysis and the general equilibrium outcomes. In general equilibrium, spending in one geographic area may affect employment in another. Likewise, a firm not

directly affected by the withdrawal of bank credit may still change its employment in response to the adjustments of other firms. One approach to the problem of general equilibrium involves building a theoretical model that nests the cross-sectional analysis as well as the general equilibrium effects. The third chapter of this dissertation demonstrates this approach in the context of the effect of the supply of credit.

Acknowledgments

I would never have written this dissertation without Christy and David Romer. I like to think I would have found some other topic, but I would not have written what I did without their constant encouragement, advice, feedback, financial support, and detailed comments on innumerable drafts and presentations. And perhaps most important, I enjoyed every minute of my interactions with them. They were beyond generous with their time, including David who does not appear formally on my committee but nonetheless committed more resources to my development than most Chairs.

The research collected herein has aspects of macroeconomics, finance, and labor economics, and that leaves me in the debt of faculty in each of those fields. I had the great fortune of beginning graduate school the same year that Yuriy Gorodnichenko started as an assistant professor at Berkeley. I have learned from Yuriy in the classroom, in seminars, and in numerous sessions in his office. He accompanied me along every step of the research process. Maury Obstfeld, Pierre-Olivier Gourinchas, Brad DeLong, and Barry Eichengreen also contributed immeasurably to my training as a macroeconomist. In the field of finance, Ulrike Malmendier, Atif Mian, and Jim Wilcox taught me about firms and banks, and pushed my research to take seriously the institutional details of the loan market. Atif has also served as a tremendous role model for using microdata to study macroeconomic questions. In labor, my thinking about everything from research design to standard errors benefited from formal instruction and informal discussions with Pat Kline and Jesse Rothstein.

My graduate career would have been less interesting, less fun, and by any metric less successful without my macroeconomic classmates, Josh Hausman and Johannes Wieland. They made me look forward to coming into the office every day, and contributed enormously to my education. The same can be said of my first year study group and of all of my office-mates past and present.

The state fiscal relief chapter began while Laura, Zach, Gui and I were all staff economists at the Council of Economic Advisers. I am so glad it gave us the excuse to continue collaborating after our return to graduate school. The other chapters in this dissertation, not to mention anything I ever write in the future, have the benefit of what I learned about research from working with the "gang of four."

Going to graduate school in California and writing a dissertation using data at the Bureau of Labor Statistics in Washington, D.C. would not have been possible without contributions from a number of people. Jessica Helfand and Michael LoBue of the BLS flexibly accommodated my schedule and generously shared their wisdom about the Longitudinal Database whenever I had questions. Andy, Sasha, Elliot, Jenny, Kate and Rhett, and Joan and Jon all opened their homes to me on one or more occasions. Not only did staying with friends make the research trips possible on a graduate student's stipend, but they also made the trips far more enjoyable than they would otherwise have been.

Finally, I am extraordinarily lucky to have the example set by my parents as academics, both in the standard of their work and in their non-work lives. And, of course, to Abby. I cannot possibly enumerate the ways in which she has supported me, allowed me to do better research, and made me happy.

Introduction

This dissertation reflects two recent developments in the study of economics. The first concerns real world events. Beginning in 2007, the countries of Western Europe and the United States experienced a series of financial shocks and an economic downturn unprecedented in their experience since the Great Depression seventy years before. This "Great Recession" followed a twenty-five year period of unusual macroeconomic tranquility known as the Great Moderation.

Dramatic changes in the economy raise new questions or renew old lines of inquiry. At the recession's onset, many monetary authorities, including the Federal Reserve and the European Central Bank, had achieved their stated goals of low and stable rates of inflation. However, such low rates of inflation left these central banks with relatively little room to lower their policy interest rates once the downturn began. To offset the paucity of stimulus from monetary policy, many countries turned to discretionary fiscal stimulus packages. The size of these packages and the severity of the downturns make understanding their effectiveness of critical importance. The first chapter in this dissertation analyzes empirically the effects on employment of one particular component of the fiscal policy response in the United States.

The Great Recession also renewed interest in the importance of credit to firms. In the United States, the employment collapse beginning in the fall of 2008 came immediately after the banking panic that followed the bankruptcy of Lehman Brothers. Did the banking panic have a direct, causal effect on employment? The Bernanke (1983) analysis of the effects of the destruction of bank-specific intermediary capital in the Great Depression made economists aware that financial shocks *could* play a role in the propagation of business cycle downturns. The second chapter in this dissertation attempts to answer whether such shocks *did* play a role in propagating the Great Recession.

The second development consists of the application of unit-level datasets and applied microeconomic methods to answer questions of macroeconomic importance. This research program recognizes that cross-sectional variation across individual units often exceeds the time-series variation of an aggregate. Moreover, cross-sectional studies can hold constant many macroeconomic variables that might otherwise confound the identification of a causal effect. Advances in data collection and computing power make possible the application of cross-sectional methods to research questions traditionally confined to time-series analysis.

The empirical chapters of this dissertation apply microeconomic methods to help understand the importance of the credit channel and the effectiveness of fiscal policy. A full answer to these questions, however, requires a correspondence between the cross-sectional analysis and the general equilibrium outcomes. In general equilibrium, spending in one geographic area may affect employment in another. Likewise, a firm not directly affected by the withdrawal of bank credit may still change its employment in response to the adjustments of other firms. One approach to the problem of general equilibrium involves building a theoretical model that nests the cross-sectional analysis as well as the general equilibrium effects. The third chapter of this dissertation demonstrates this approach in the context of the effect of the supply of credit.

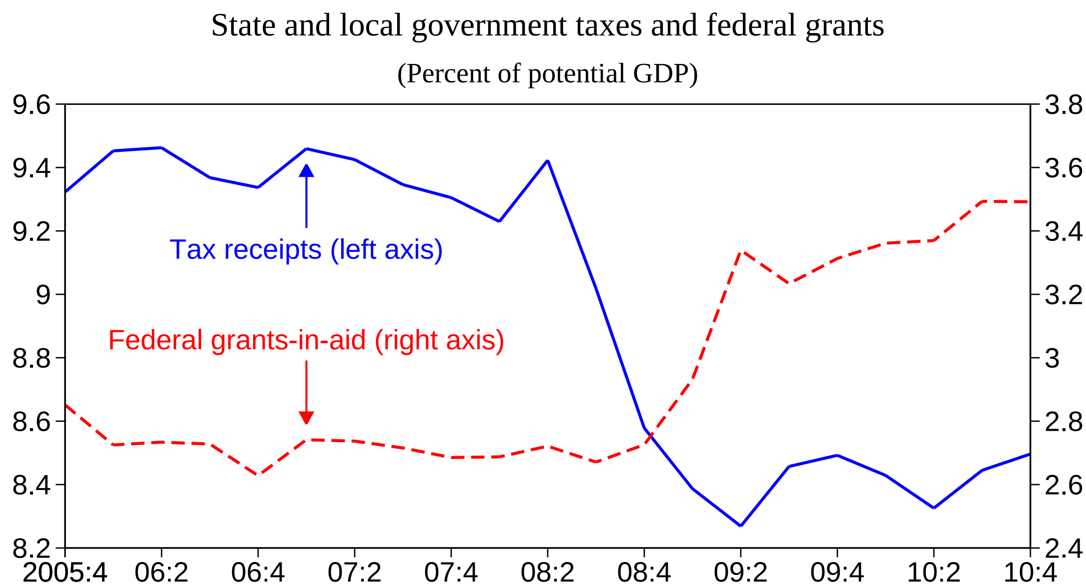
I now preview the chapters in greater detail.

State fiscal relief during recessions

The recession that began in the United States in December 2007 caused a dramatic decline in state and local tax receipts. Every state, with the exception of Vermont, has some form of a balanced budget requirement. Absent any increase in other revenue sources, the decline in tax receipts would thus require a decline in spending, generating a contractionary fiscal impulse that would exacerbate the recession already in place.

The American Recovery and Reinvestment Act (ARRA) came into law in February 2009 to provide a fiscal boost to an economy weakened by recession and without recourse to the usual monetary policy instruments. The single largest non-tax component of the Act sent $90 billion in aid to state governments to help offset the decline in tax receipts. Figure 1 displays the result. While tax receipts fell by more than a percentage point of potential GDP, the increase in aid from the federal government made up for a significant amount of the shortfall.

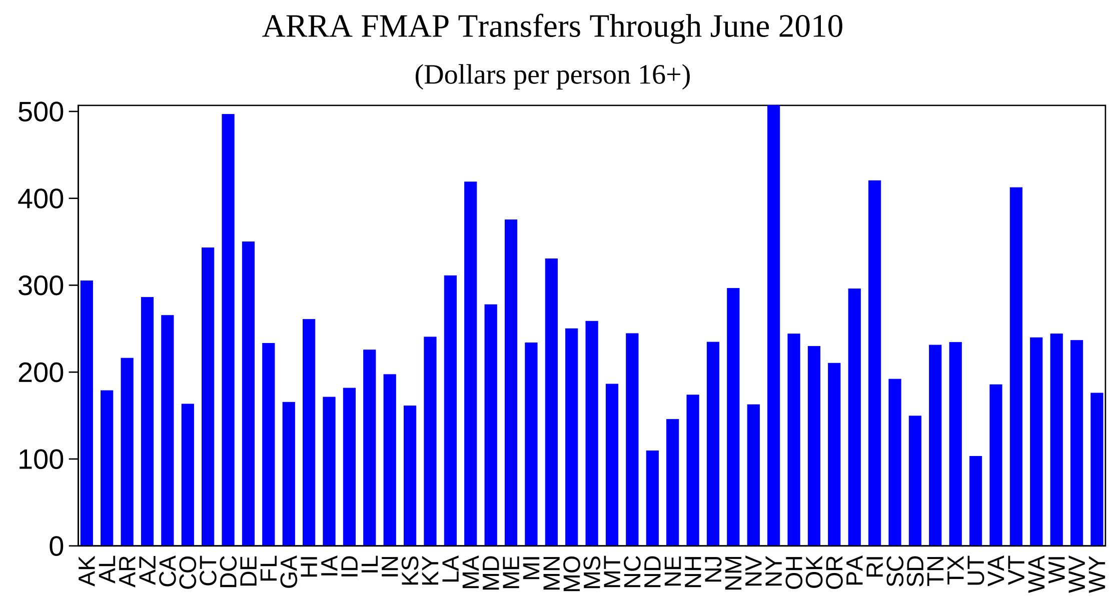
Figure 1:



Source: Bureau of Economic Analysis (NIPA table 3.3), CBO.

Did the state aid result in higher levels of employment and economic activity than would have otherwise occurred? In chapter 1, my coauthors Laura Feiveson, Zachary Liscow, and William Woolston and I provide an empirical answer to this question. The analysis exploits the design of the ARRA transfer program. The program increased the Federal Medical Assistance Percentages (FMAPs) for each state. The FMAPs determine the fraction of a state's Medicaid expenditure reimbursed by the federal government. The ARRA FMAP increases resulted in vastly different per capita dollar transfers in different states, depending on the relative size of the states' Medicaid programs. Since Medicaid is a mandatory spending program, every dollar reimbursed by the federal government releases a dollar of state funds for other use. Figure 2 summarizes this information by showing the per capita dollar amount transferred through the program through June 2010. While some states such as New York received as much as $500 per person 16+, other states received little more than $100 per person.

Figure 2:



Source: U.S. Recovery Accountability and Transparency Board.

The chapter uses the cross-sectional variation contained in figure 2. In particular, it compares employment outcomes at states that received more and less transfers through the ARRA FMAP increase. In order to remove sources of variation in transfer receipts potentially correlated with employment outcomes, the chapter presents an instrumental variables design that instruments for the variation in figure 2 using states' pre-recession Medicaid spending.

We find that the state fiscal relief had a large effect on employment. In our preferred specification, a state's receipt of a marginal $100,000 in ARRA FMAP transfers results in an additional 3.8 job-years. Some of this employment occurs in state and local government, but much of the effect appears in the private sector, consistent with both spillover effects and states using some of the additional funds to forgo tax increases.

These results suggest that counter-cyclical fiscal policy may have had an important role in ending the recession in the summer of 2009. In the study of fiscal multipliers more broadly, they are consistent with large multipliers under fixed interest rate conditions, and with federal transfers to state and local governments having a large effect.

Employment effects of credit market disruptions

The 2007-09 financial crisis had its proximate cause in the build-up of exposure to real estate and mortgage-backed securities in many of the country's largest financial institutions. When real estate prices fell and these assets lost value, many of these institutions found themselves dangerously under-capitalized. In addition, reliance on short-term wholesale financing made the financial system vulnerable to runs similar to those that afflicted commercial banks before the introduction of deposit insurance. The financial sector distress peaked in the fall of 2008 following the bankruptcy filing of the investment bank Lehman Brothers.

In the aggregate, new corporate lending by a sample of 43 of the largest banks in the United States reached a local trough in the fourth quarter of 2008, at a level roughly 70 percent below its previous peak. At the same time, the real economy experienced a decline of 4.3 million private sector jobs in just the six months following the Lehman bankruptcy. Did the decline in lending trace in part to the withdrawal of bank financing to nonfinancial firms? Or did the weak economy lead to a decline in loan demand, and this direction of causality explains all of the aggregate time-series evidence?

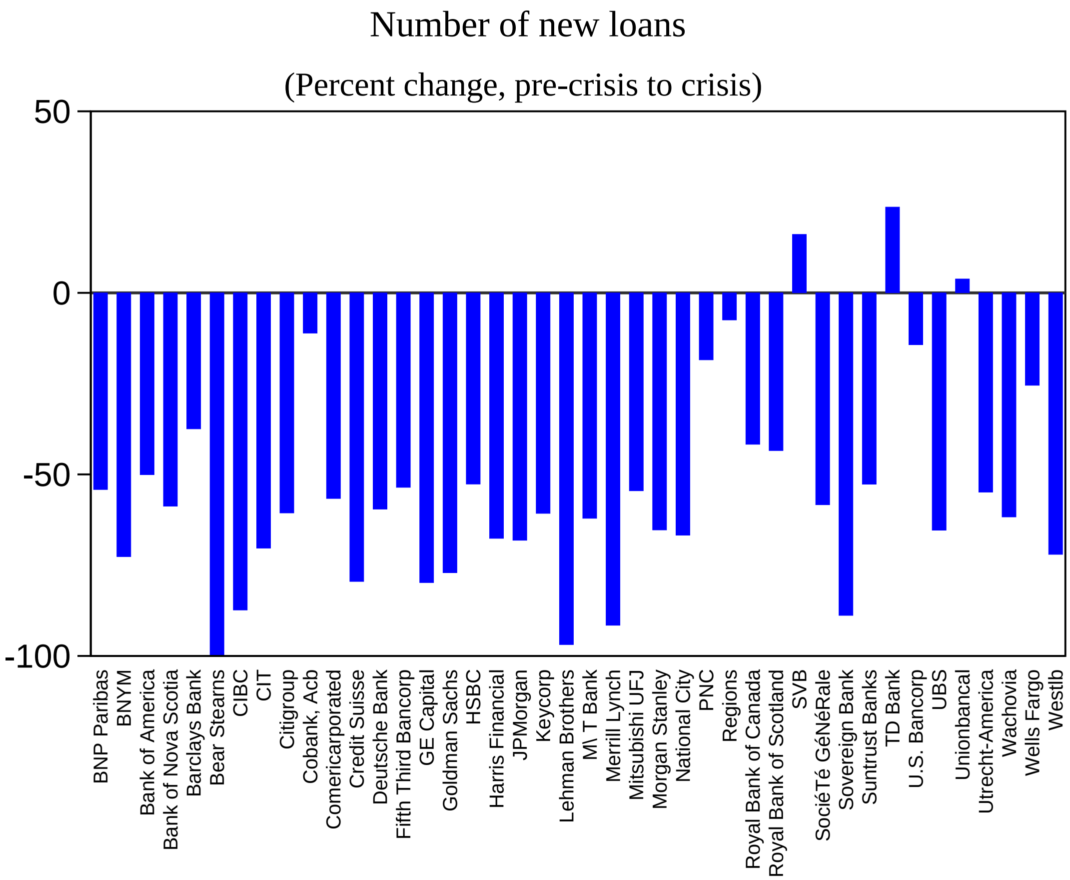
To answer this question, chapter 2 exploits cross-sectional variation in the health of different lenders. Figure 3 summarizes the variation. The figure shows the percent change in the annualized number of loans made by each bank between the periods October 2005 to June 2007 and October 2008 to June 2009. The origins of the 2008 crisis in real estate, mortgage-backed securities, and funding structure imply that the cross-sectional variation in banks' willingness to make corporate loans was plausibly orthogonal to banks' corporate loan portfolios. Indeed, the chapter shows that the cross-sectional variation in lending does not stem from segmentation of banks along dimensions such as industry, geography, or borrower riskiness.

A salient feature of the corporate loan market is that firms and lenders form relationships. This means that firms that borrowed *before* the crisis from banks that did less lending *during* the crisis would have greater difficulty obtaining bank financing than firms that had borrowed from healthier lenders. The research design investigates the link between credit market events and employment by comparing employment outcomes at firms that had pre-crisis relationships with different lenders. To provide additional robustness against the possibility that bank segmentation generates a spurious correlation, the chapter instruments for banks' change in lending using the exposure to Lehman Brothers, the exposure to mortgage-backed securities, and selected items from the banks' balance sheet and income statements.

I find that credit matters. From the chapter's abstract:

> Firms that had pre-crisis relationships with less healthy lenders had a lower likelihood of obtaining a loan following the Lehman bankruptcy, paid a higher interest rate if they did borrow, and reduced employment by more compared to pre-crisis clients of healthier lenders. Consistent with frictions deriving from asymmetric

Figure 3:



information, the effects vary by firm type. Lender health has an economically and statistically significant effect on employment at small and medium firms, but the data cannot reject the hypothesis of no effect at the largest or most transparent firms. Abstracting from general equilibrium effects, I find that the withdrawal of credit accounts for between one-third and one-half of the employment decline at small and medium firms in the sample in the year following the Lehman bankruptcy.

Cross-sectional estimation and general equilibrium

The cross-sectional econometric approach has the advantage of allowing for stronger statements of causality in many instances. However, when the question has macroeconomic importance, the approach has the disadvantage that the data do not immediately provide a quantitative estimate of the magnitude in general equilibrium. This shortcoming contrasts with time-series econometrics, where the construction of an impulse-response function provides the correct characterization of the causal effect of the shock studied on macroeconomic aggregates.

Economic theory can help to bridge the gap between the cross-sectional estimation and the general equilibrium effect by highlighting the relevant general equilibrium channels. In the realm of fiscal policy, empirical studies including the one presented in chapter 1 quickly led to new theoretical work, with notable contributions by Nakamura and Steinnson (2012) and Farhi and Werning (2012). (Indeed, none of this work has made it to publication as of this writing.) These papers have emphasized the degree of openness of states to inter-state trade and the level of risk-sharing across regions as important parameters in mapping the "transfer multiplier" estimated in chapter 1 into the closed economy government spending multiplier at the zero lower bound on interest rates.

The firm-level evidence of credit effects found in chapter 2 also raises the question of general equilibrium effects. As that chapter explains, "in general equilibrium some labor shifts from credit-constrained to unconstrained firms. Part of the reallocation of labor results from a shift in product demand, as relative prices at unconstrained firms fall or constrained firms ration their output. Further reallocation comes from the decline in employment at constrained firms causing a fall in the real product wage, which induces unconstrained firms to move down their labor demand curves." The firm-level analysis ignores this reallocation, which may cause it to overstate the effects of credit on aggregate employment. However, the chapter also points to a general equilibrium channel that may cause the firm-level analysis to understate the aggregate effects: the reduction in aggregate expenditure caused by the financial crisis. The fall in aggregate expenditure reduces labor demand at unconstrained firms.

The third chapter constructs a general equilibrium model to illustrate and investigate the relative importance of these two channels. The model contains households, firms, and a financial sector. In the model, a financial crisis generates an increase in firms' financing costs that varies according to the firm's pre-crisis lender, just as in the data studied in chapter 2. Likewise, the model gives rise to a cross-sectional relationship between firm-level employment growth and the health of the firm's pre-crisis lender. But the model also characterizes the general equilibrium channels that may create a wedge between the direct effects of the financial crisis and the aggregate effects on employment.

The general equilibrium spillovers in the model equal

$$\alpha_1 \hat{Y}_t - \alpha_2 \left[\left(\hat{p}_{1,t} - \hat{P}_t \right) + \left(\hat{w}_t - \hat{p}_{1,t} \right) \right],$$

where $\hat{Y}_t$ denotes the decline in aggregate expenditure; $\hat{p}_{1,t}$ the average change in the price charged by firms not directly affected by the withdrawal of credit; $\hat{P}_t$ the change in the aggregate price level; $\hat{w}_t$ the average wage; and $\alpha_1 > 0$ and $\alpha_2 > 0$ combine parameters into reduced-form elasticities. The first term of this expression constitutes the aggregate demand channel. The decline in aggregate demand also lowers demand at firms not directly affected by the credit crisis, causing them to reduce labor demand and hence amplifying the employment decline in general equilibrium. The first term in brackets, $\hat{p}_{1,t} - \hat{P}_t < 0$, gives the change in the (average) relative price at the firms not directly affected. The fall in the relative price shifts product demand to unconstrained firms, causing them to hire more labor and hence diminishing the employment effects in general equilibrium. The second term in brackets, $\hat{w}_t - \hat{p}_{1,t} < 0$, gives the change in the real product wage at the the unconstrained firms. The fall in the cost of labor relative to the price of their output induces unconstrained firms to move down their labor demand curves, again diminishing the employment effects in general equilibrium. The elasticity α_2 increases in the substitutibility of the goods produced by the constrained and unconstrained firms, but decreases in the degree of frictions to workers' switching firms.

The chapter provides a closed-form solution to the spillovers as a function of the average increase in borrowing costs. It also provides a calibration. For plausible parameter values, the general equilibrium channels come close to offsetting each other. This result suggests that in this case, the firm-level evidence from chapter 2 may provide a reasonable benchmark for the aggregate effects of credit disruptions.

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Does State Fiscal Relief During Recessions Increase Employment? Evidence from the American Recovery and Reinvestment Act

Gabriel Chodorow-Reich, Laura Feiveson, Zachary Liscow, and William Gui Woolston*

Abstract

The American Recovery and Reinvestment Act (ARRA) of 2009 included $88 billion of aid to state governments administered through the Medicaid reimbursement process. We examine the effect of these transfers on states' employment. Because state fiscal relief outlays are endogenous to a state's economic environment, OLS results are biased downward. We address this problem by using a state's pre-recession Medicaid spending level to instrument for ARRA state fiscal relief. In our preferred specification, a state's receipt of a marginal $100,000 in Medicaid outlays results in an additional 3.8 job-years, 3.2 of which are outside the government, health, and education sectors.

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Introduction

The federal government enacted the approximately $800 billion American Recovery and Reinvestment Act (ARRA) in February 2009 to provide a countercyclical impulse during the worst economic downturn in the United States in at least sixty years. At the same time, state governments, almost all of which have balanced budget requirements that restrict borrowing across fiscal years, had already begun to lay off employees, cut spending and transfer programs, and raise taxes. Rather than concentrate the stimulus in direct federal government purchases of output, the ARRA's authors chose to mitigate this sub-national contractionary fiscal impulse by routing roughly a third of the total through state and local governments. The largest of these programs was the increase in the federal match component of state Medicaid expenditures.

Countercyclical intergovernmental transfers to support sub-national budgets have occurred previously in the U.S. and in other countries around the world. Yet, this form of stimulus has received little attention in the academic literature, compared with the large number of studies of direct government purchases or tax reductions.[1] A priori, transfers could have a small or zero immediate impact on economic outcomes if states simply use them to bolster their rainy day funds, effectively shifting money between government accounts without affecting the overall stance of the general government sector. On the other hand, states may use the money to reduce tax increases or avert budget cuts, allowing the money to enter the economy more quickly than direct federal purchases that require project selection and approval. Reflecting this theoretical uncertainty, views on the effectiveness of state aid prior to the ARRA's passage ranged from then-House Minority Leader John Boehner, who predicted that "direct aid to the states is not going to do anything to stimulate our economy," to the Obama Administration, which predicted that the state relief would save or create more than 800,000 jobs in the fourth quarter of 2010.[2] Even well after the ARRA's passage, disagreement continued, with many Republicans and some economists claiming that no jobs had been created, while the White House continued claiming large job gains.[3]

This paper aims to fill the gap in our understanding of intergovernmental transfers by empirically assessing the impact of the ARRA's Medicaid match program. The program has a number of features that make it attractive for study. First, the total amount of money distributed through this program is large enough to plausibly generate a detectable effect on employment. Out of a total of $88 billion dedicated to an increase in the Medicaid matching funds, states had received $61.2 billion by June 30, 2010, the end of our period of study. Second, because state Medicaid programs operate on a mandatory basis, increasing the federal share of costs effectively transfers money into state budgets that states can then use for any purpose they choose – the money is fungible. Indeed, many states reported that they had allocated the money quickly to areas that otherwise would have undergone deeper budget cuts (Government Accountability Office 2009; National Association of State Budget Officers 2009b). Third, the level of additional money received by states as of June 2010 per person aged 16 or older (16+)

[1] There is a large literature on the extent to which federal grants crowd out local government spending which was spearheaded and summarized by Gramlich (1977).

[2] See http://www.msnbc.msn.com/id/28841300/ns/meet_the_press/t/meet-press-transcript-jan/ and Romer and Bernstein (2009).

[3] See http://www.factcheck.org/2010/09/did-the-stimulus-create-jobs/ for a list of quotes from Republicans claiming that the ARRA created no jobs. Also, a survey by the National Association for Business Economics showed that 69% of business economists they surveyed reported that the ARRA had no impact on employment (http://www.jsonline.com/business/82657582.html).

varied greatly, from a low of $103 in Utah to a high of $507 in DC, with an interquartile range of $114. This variation makes possible a cross-sectional econometric strategy. We focus our analysis on the effect on employment because the public debate on the effectiveness of the ARRA has centered largely on this outcome. Furthermore, high-quality monthly state level employment data makes it possible to obtain more precise estimates of fiscal multipliers than what is possible with the existing state-level income data.

The primary challenge to a cross-sectional study is that the amount of aid a state receives is endogenous to the state's economic conditions. Because states that were in worse economic shape received more aid, the OLS relationship between the level of state fiscal transfers and changes in employment understates the true effect of state fiscal relief. We address this concern by using an instrument that isolates the component of the Medicaid transfers unrelated to changes in economic circumstances. The ARRA increased the percentage of Medicaid expenditures that the federal government pays for all states by 6.2 percentage points and increased the match rate by more for states that experienced especially large increases in unemployment. Thus, the level of ARRA Medicaid transfers to each state is the result of four factors: the amount of Medicaid spending in the state prior to the recession; the change in the number of beneficiaries during the recession; the change in the average spending per beneficiary; and whether the state qualified for an additional match increase based on the change in the state's unemployment rate. The heart of our identification strategy lies in exploiting only the cross-sectional variation from the first of these factors, that is, the variation in ARRA Medicaid transfers that results from variation in Medicaid programs from *before* the recession.

Another set of reasons why a state may have both received more Medicaid funding and had different employment outcomes—omitted factors related to both state Medicaid program rules and economic changes—is not solved by the instrument. For example, more liberal coastal and Midwestern states both had larger downturns and have more generous Medicaid programs. We present several pieces of evidence that suggest that our results are not driven by underlying differences between high and low spending Medicaid states. First, to ensure that time-invariant differences between high and low Medicaid spending states are not driving our relationship, our empirical strategy considers changes, rather than levels, of employment. Second, in our baseline specification we exploit only differences in Medicaid spending within census divisions rather than between them, and include a number of variables that help predict how a state's employment would have changed absent the ARRA. Finally, we present falsification tests by running our baseline specification on pre-ARRA data and show that in the decade before the ARRA passed, states with high and low Medicaid spending experienced similar employment outcomes.

An important caveat to our analysis is that a cross-state approach forces us to ignore general equilibrium effects, which could alter our interpretation of the overall effect of stimulus spending on jobs and prevents us from tying down the aggregate fiscal policy multiplier. For example, spending in one state may increase demand in other states, which would lead us to under-state overall job increases.[4] On the other hand, investment could decrease across the country in response to increased government borrowing, though this effect is likely to have been especially muted during the low policy interest rate environment of 2009-10. Likewise, to the extent that people believe that their taxes will be raised in the future due to the increased government borrowing, spending may decrease throughout the country.

[4] Moretti (2010) notes that, through labor mobility, cross-state spillovers can also be negative. However, labor mobility is likely small over a period of time as short as that considered here.

With this caveat in mind, we find that the ARRA transfers to states had an economically large and statistically robust positive effect on employment. Assuming that employment does not persist beyond the time during which it is funded, our preferred specification suggests that a marginal $100,000 in Medicaid transfers resulted in 3.8 net job-years (i.e., one job that lasts for one year) of total employment through June 2010, of which 3.2 are outside the government, health, and education sectors. The effect is precisely estimated, and we can reject the null hypothesis that the spending had no effect on employment with a high degree of confidence. For this result to be economically plausible, states must have used the funds to avoid spending cuts or tax increases. Hence we also provide evidence that the transfers do not appear to have increased the states' end of year balances. In connecting our estimates to the implicit changes in government spending or taxes, our paper also adds to the recent literature on the employment effects of state spending (e.g., Shoag 2011; Wilson 2011; Suarez-Serrato and Wingender 2011; Clemens and Miran 2011), as well as the fiscal effects of government spending generally (e.g., Nakamura and Steinsson 2011).

The paper proceeds as follows. In Section I, we describe the institutional details of Medicaid grants and the ARRA stimulus package. Section II contains our econometric methodology and describes our baseline specification. In Section III, we describe our data. Sections IV and V present our main results and robustness checks, respectively. Section VI provides an interpretation of our results and relates them to the existing literature. Section VII discusses evidence of a budgetary transmission mechanism, and Section VIII concludes.

I. Institutional Details of the ARRA and Medicaid Grants

The ARRA became law in February 2009 at an estimated 10-year cost of $787 billion. Through December 2010, it had distributed $609 billion.[5] As Cogan and Taylor (2010) point out, only $30 billion of this total got recorded in the national income accounts as federal government consumption or investment. A little more than half ($350 billion) went to individuals or business in the form of tax reductions or transfer payments. The rest, more than $200 billion, went through state and local governments, including $88 billion through the Medicaid match program designed especially to alleviate the strain on state budgets.[6] State fiscal relief had the added advantage of getting out the door quickly: in the first quarter of 2009, more than three-fourths of total ARRA outlays and tax expenditures took the form of Medicaid outlays.

Medicaid is a state-run program that provides health insurance for certain individuals and families with low incomes and resources. Both the eligibility requirements and the scope of the insurance coverage vary across states. The federal government reimburses states for between 50 and 83 percent of their Medicaid expenditures, as determined by the Federal Medical Assistance Percentages (FMAP). Many states require that local governments share in financing the non-federal portion of the program. Each federal fiscal year, states' FMAPs are recalculated based on the three-year average of each state's per capita personal income relative to the national average, with poorer states receiving higher reimbursement rates. Thus, states that have lower

[5] Data in this paragraph come from the Bureau of Economic Analysis Recovery Act data program at www.bea.gov/recovery.

[6] Another $38 billion went through the State Fiscal Stabilization Fund (SFSF), part of a $48.6 billion appropriation that apportioned the money according to a mix of population of persons aged 5-24 (61%) and total population (39%).

average incomes, more recipients of Medicaid per capita, or more generous benefits receive larger per capita matching funds from the federal government.

The ARRA made three changes to the baseline FMAP calculation for October 2008 through December 2010. First, the baseline FMAP could not decrease. Second, the FMAP was increased by 6.2 percentage points above the baseline for every state.[7] The additional match applied retroactively from passage in mid-February back to October 2008, making part of the transfer purely lump-sum. Finally, through December 2010, each state received a further increase in its FMAP based on the largest increase in its unemployment rate experienced between the trough three-month average since January 2006 and the most recently available 3-month average.[8] To qualify for the ARRA changes, states had to, at a minimum, maintain the eligibility standards, methodologies, and procedures of their Medicaid programs that existed on July 1, 2008. Program benefits could, however, change. The law also forbade states from increasing the share of the non-federally financed portion of Medicaid spending borne by local governments, in effect extending the fiscal relief to local governments as well.

There appear to have been two main rationales for the FMAP increases. First, unlike direct federal spending, state fiscal relief through changes to the FMAP could be implemented almost immediately; the first ARRA Medicaid reimbursements recorded by the Department of Health and Human Services occurred during the week ending on March 13, 2009, only a few weeks after the ARRA was signed into law. Second, the changes to FMAP were intended to boost the level of discretionary funds available to states, and not only to relieve Medicaid burdens. Because an increase in the FMAP reduces the state portion of mandatory payments, the additional funds are completely fungible – states can use them however they wish. Congress recognized the fungibility of the funds during the legislative debate. Indeed, the legislative text of the ARRA says that the first purpose of the section containing the FMAP increases is to "provide fiscal relief to States in a period of economic downturn." Section VII discusses the empirical evidence on how states used the extra FMAP funds.

Congress began discussions with state governors on a stimulus bill that would include significant aid to state governments as early as December 2008.[9] The House appropriation committee draft released on January 15, 2009 included an increase in the FMAP of 4.8 percentage points, and both the original House and Senate versions, passed on January 28 and February 10, respectively, had the same $88 billion allocated to Medicaid as the final bill. Hence our analysis should begin no later than December 2008 if state governments incorporated the likelihood of additional federal relief into their budget plans.

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[7] Under the ARRA, the 0.83 cap on FMAP was also removed.

[8] In the fourth quarter of 2008 and the first quarter of 2009, the extra amount was actually based on the largest increase between the trough 3-month average unemployment rate since January 2006 and the average unemployment rate from October 2008 to December 2008. In the third and fourth quarters of 2010, the calculation was based on the difference between the same trough average rate and the larger average of the two 3-consecutive month periods beginning with December 2009 and January 2010, respectively. Furthermore, there was a maintenance of status clause which legislated that any increase in FMAP made for a quarter on or after January 1, 2009, would be maintained through the second quarter of 2010.

[9] For example, House Speaker Nancy Pelosi met with a group of governors on December 1st to discuss the contours of a stimulus bill that would include state aid. See: Cowan, Richard. 2008. "House to Push $500 Billion Stimulus Bill." *Reuters*, December 1. Retrieved on August 10, 2010. http://www.reuters.com/article/idUSTRE4B05QP20081201..

II. Econometric Methodology and Baseline Specification

A. Instrumental Variables Motivation

We begin with a simple framework that relates state fiscal relief to total employment. The change in the ratio of employment to potential workers in a state, s, depends on the state fiscal relief that the state receives, a series of controls that capture differential trends, and a state-specific shock:

$$(1) \quad \frac{E_1^s - E_0^s}{N^s} = \beta_0 + \beta_1 \frac{Aid^s}{N^s} + \beta_2 Controls^{\,s} + \varepsilon^s$$

where E_i^s is the seasonally-adjusted employment in state s in period i, N^s is the 16+ population in state s, β_0 is a national-level shock, Aid^s is the state fiscal relief received by state s, $Controls^s$ are state level controls in state s, and ε^s is a state-level mean-zero shock.

If the state fiscal relief per potential worker, $\frac{Aid^s}{N^s}$, were uncorrelated with the error term, ε^s, then (1) could be estimated with bivariate OLS. However, this assumption is almost certainly not valid. The ARRA Medicaid transfers to each state reflect four factors: the amount of Medicaid spending in the state prior to the recession; the change in the number of beneficiaries during the recession; the change in the average spending per beneficiary; and whether the state qualified for the additional match increase based on the change in the state's unemployment rate. These last three factors, and especially the fourth, share the concern of reverse causality with respect to the outcome variable. Hence we use an instrument that restricts the cross-state variation to only that part of Medicaid transfers related to pre-recession Medicaid spending. Specifically, we implement a two-stage least squares estimation strategy, using 2007 Medicaid spending as an instrument for the FMAP transfers. We normalize all relevant variables by the number of individuals age 16+ in a state in 2008.

We also include a number of state-level controls that are potentially correlated with both 2007 Medicaid spending and changes in employment. These controls are detailed in Section III and include the lagged change in employment to capture pre-existing trends between high and low Medicaid spending states.

B. Other Aspects of the Baseline Specification

We focus on two primary outcome variables: change in seasonally adjusted total nonfarm employment and change in seasonally adjusted employment in the state and local government, health, and education sectors. We focus on total nonfarm employment because it is the most comprehensive measure of employment available in our primary data. We also consider government, health, and education workers since the direct effects of state spending are likely to be in these sectors, which contain state government employees, employees of local governments which may have received direct fiscal relief from lower required Medicaid payments and which depend heavily on state transfers for revenue, and employees of many of the private establishments that receive transfers or grants from state and local governments. To ensure that changes in federal employment are not driving our results, we exclude federal workers from this measure.

Although we show how our estimates evolve over time in Section IV, we focus on employment changes from December 2008 to July 2009 for our robustness checks and our summary statistics. We begin our period in December 2008 because, as described above, it is the last month before which the details of the ARRA, including the FMAP extension, became clear to the public. We end in July 2009 for three reasons. First, almost all states have fiscal years that run from July 1 to June 30.[10] Thus, employment through the middle of July reflects any changes to government employment that occurred at the beginning of the first full fiscal year after the ARRA was passed. Second, employees in education tend to remain on the payroll through the end of the school year, so July is the first month that would fully reflect changes in the number of jobs in education. This is important because of the large fraction of state and local government spending that goes to education.

Historic aggregate time series confirm that employment changes are especially large in July. In regressions reported in an online appendix, we compared the historical mean of the absolute value and square of state and local government employment changes for each month.[11] For both measures, the average July change was larger than that of every other month, and the difference was statistically significant for every month but September and October.

The third reason to end in July 2009 stems from efficiency considerations. For example, if the component of state employment orthogonal to our regressors is i.i.d. with variance σ^2 at a monthly frequency, then the residual variance in a regression with employment change taken over k months will equal $k\sigma^2$. That is, standard errors may increase with the duration of the employment change. This is confirmed in Section IV where we explore how the effect evolves over time. To generate precise estimates for the baseline specification, it is therefore preferable to restrict the time-window to be as short as possible.

The endogenous variable in our baseline specification is total FMAP outlays to a state through June 30, 2010, normalized by a state's 16+ population. This choice of endogenous variable is crucial to the interpretation of our results. If the state distribution of non-FMAP ARRA spending were correlated with the instrument, we would misestimate the true value of the coefficient on spending if we did not include the correlated component of spending in the endogenous variable. However, a regression of all non-FMAP ARRA outlays to states against the instrument (both normalized by 16+ population) and our baseline controls cannot reject the null that the instrument is uncorrelated with other spending (p-value = 0.413).[12]

Our final decision concerns the time covered by the endogenous variable. Since states tend to budget in yearly cycles, Medicaid transfers from the federal government received during a fiscal year could have an effect on employment at any point within that year. Borrowing restrictions make transferring funds across fiscal years difficult. With these facts in mind, we set the endogenous variable equal to the total FMAP transfers through June 2010, which corresponds to the end of fiscal year 2010 for nearly all states. We use this endogenous variable in all of our timing regressions which cover employment changes between December 2008 and each month through June 2010. Because the amount of Medicaid spending in a state exhibits a high degree of serial correlation, the precise end date barely affects the statistical significance of our results.

[10] All states other than Alabama, Michigan, New York, and Texas have fiscal years that begin on July 1. Alabama and Michigan's start on October 1 (as does the federal fiscal year), New York's fiscal year begins on April 1, and Texas's fiscal year begins on September 1. See National Association of State Budget Officers (2008a).

[11] Note that the employment data are seasonally adjusted, but only for levels, not higher-order moments.

[12] The ARRA state outlays are from Recovery.gov and exclude tax reductions.

III. Data and Summary Statistics

Outcome variables: Our primary outcome variables are derived from the seasonally adjusted state-level employment series available at a monthly frequency from the Current Employment Statistics (CES). [13] For each state for which the CES has data, we obtained monthly data from January 2000 to June 2010 on employment in total nonfarm, government, health, education, and education and health (a series that is reported separately and is available for a wider group of states than either the health or education series). The latest available vintage of CES data contains benchmarks to unemployment insurance (UI) records through September 2010, meaning that employment for each month is based on data from the UI program (adjusted for coverage using other CES sources) and therefore contains minimal sampling error. We normalize employment by a state's 16+ civilian non-institutional population as estimated by the Bureau of Labor Statistics from Census data.

Endogenous variables: Our primary endogenous variable is a state's total ARRA FMAP outlays as of June 30, 2010, normalized by a state's 16+ population. These data are available from recovery.gov (U.S. Recovery Accountability and Transparency Board 2009-2010).[14]

Instrument: The instrument is a state's Medicaid spending in fiscal year 2007, normalized by the 16+ population.[15,16] Figure 1 demonstrates the considerable cross-state variation in the instrument. To ease interpretation, the figure shows the instrument scaled by 6.2% because ARRA increased the FMAP by 6.2 percentage points, and inflated by 21/12 because from October 2008 (the month after which the FMAP increase was retroactively increased) through the end of June 2010 (the end of our sample), states received a cumulative 21 months of Medicaid reimbursements. Note that some states that are similar across many other dimensions have very different values; Medicaid spending is roughly twice as high in New York as in California, in Vermont as in New Hampshire, and in New Mexico as in Colorado.

Control variables: Our choice of control variables is motivated primarily by the threat to identification that states that received different amounts of Medicaid funding in 2007 were on different employment trends during the time period studied. Figure 2 shows on a map the value of the instrument, scaled as described above; states are grouped into six groups of spending per capita. One potential concern is there is substantial regional variation in Medicaid spending. For example, the map shows that New England has high Medicaid spending. Because the employment effects of the recession were distributed unevenly across regions, differences in

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[13] Because seasonal adjustment differs significantly across states, our baseline specification focuses on seasonally adjusted data. However, in Table 5, we present year-over-year changes in employment using non-seasonally adjusted employment changes from the QCEW.

[14] The agency Financial and Activity Reports available on Recovery.gov report outlays at the Treasury Account Financing Symbol (TAFS) level. The TAFS for FMAP is 750518. A payment to a state is recorded as an outlay when money is transferred from the U.S. Treasury to the state as reimbursement for a Medicaid payment the state has already made. Our data exclude about $3 billion provided through application of the ARRA FMAP increase to state contributions for prescription drug costs for full-benefit dual eligible individuals enrolled in Medicare Part D because the Financial and Activity Reports do not show a state-by-state breakdown of this spending during our period of study.

[15] Data on 2007 Medicaid spending by state are available from the Centers for Medicare and Medicaid Services (2008).

[16] Per capita Medicaid spending is highly correlated over time. For example, the correlation between our instrument using 2007 Medicaid spending per capita and 2001 Medicaid spending per capita is 0.95.

employment between high and low Medicaid spending states could reflect regional differences in underlying economic conditions rather than the effect of state fiscal relief. To address this concern, in our preferred specification, we include categorical variables for the nine census divisions, isolating the variation in the instrument that comes from within regions rather than between them.

In our preferred specification, we also control for pre-existing economic conditions using lagged employment change (from May to December 2008, the seven months prior to the beginning of our sample period). Adding this control is potentially important because empirically, employment changes are highly persistent. Moreover, while we cannot reject the null that our instrument is uncorrelated with employment changes from May to December 2008, the point estimate for this correlation is non-trivial in magnitude, raising the possibility that high and low Medicaid spending states might have been on different employment trends prior to the ARRA.[17] In Section V, we explore the robustness of our results to controlling for alternative measures of past economic conditions. In our baseline regression, we also control for GDP per potential worker and the employment manufacturing share.

To help address concerns about differential cyclicality of state spending related to the instrument through common political factors, we control for the 2007 share of workers in a union and the vote share for Senator Kerry in the 2004 presidential election. If cyclicality differs between states with different amounts of Medicaid spending (in ways not captured by a lag) because more liberal or unionized states have more Medicaid spending, as well as stronger safety nets and weaker balanced budget requirements, these controls would alleviate that concern. Finally, we control for the 2008 state population. Further details are in the appendix.

Table 1 presents summary statistics for the main variables used in the paper. All relevant variables are normalized by a state's 16+ population. The average total ARRA outlay through June 2010 was approximately $1,000 per person age 16+ (excluding tax benefits and spending not tracked at the state level). Of this, approximately one-quarter came through FMAP outlays, and more than one-third came through FMAP outlays plus the other large state fiscal relief program, the State Fiscal Stabilization Fund. There is considerable variation in both total ARRA and FMAP outlays across states, with the coefficients of variation at 0.32 and 0.36 respectively. During the period considered, average total nonfarm employment changes were sharply negative. However, there is also considerable cross-state variation in this pattern. For example, normalized employment changes were more than 5 times more negative for the state at the fifth percentile of the total employment change distribution (Indiana) than the state at the 95th percentile (Alaska). There is broadly similar variation in the change in employment in the government, health, and education sectors.

[17] The correlation between the change in per capita total nonfarm employment during the seven months prior to the beginning of our sample period (May and December 2008) and the instrument is 0.23 (p-value = 0.10). During this period, the correlation between the change in per capita government, health, and education employment and the instrument is -0.20 (p-value = 0.17). In contrast, during the main period of interest (December 2008 to July 2009), the correlation between the instrument and these outcome variables is larger and precisely estimated. For the change in employment, the correlation is 0.55 (p-value < 0.01), and for total nonfarm, the correlation is 0.40 for government, health, and education (p-value < 0.01).

IV. Baseline Results

A. First Stage

In Table 2, we present results from several first-stage regressions. The outcome variable is total FMAP outlays as of June 30, 2010, normalized by a states' 16+ population and measured in $100,000 increments.

To interpret Table 2, it is useful to divide the instrument coefficient by 0.062 to reflect the ARRA FMAP increase of 6.2 percentage points, and to further divide by 21/12 to adjust for the cumulative 21 months of Medicaid reimbursements through the end of June 2010 (the end of our sample), yielding a cumulative multiplicative scaling factor of 9.2. This scaled first stage coefficient would be 1 if the FMAP outlays simply represented 6.2% of Medicaid spending at 2007 rates. However, there are two reasons why we would expect the scaled coefficient to be larger than 1. First, FMAP ARRA outlays are based on current Medicaid spending, not 2007 spending. Due to the rapid growth in nominal Medicaid expenditures since 2007, if all states' Medicaid expenditures simply increased at the nominal national rate, we would expect a scaled coefficient substantially above 1.[18] Second, as described above, FMAP outlays also include FMAP increases for states that experienced sufficiently large changes in their unemployment rate. If high and low Medicaid spending states experienced identical changes in their unemployment rates, these FMAP expansions would mean that a larger number of dollars would flow to high Medicaid spending states, as a given FMAP increase translates into more dollars for these states. As a consequence, the average difference in Medicaid matching outlay for a high and low Medicaid spending state would be larger.

Model (1) presents a simple bivariate regression. The coefficient on our instrument is 0.18, and it is precisely estimated, with an F-statistic above 260. The instrument alone explains more than 80% of the variation in FMAP outlays. In Model (1), we can strongly reject the hypothesis that the scaled coefficient (0.18 divided by 0.062 and 21/12 = 1.68) is 1. Specifications (2) – (4) show that this positive and precisely estimated relationship between the instrument and our main endogenous variable is robust to including a large number of covariates. Model (2) includes our basic set of controls, including region fixed effects. Model (3) adds a control for lagged total employment change from May – December 2008, while Model (4) augments (2) with lagged change in government, health, and education employment over the same period. Overall, the first stage is very strong.

B. Baseline Results through July 2009

In this section, we present baseline results where the outcome variable is change in employment in a sector from December 2008 to July 2009. Table 3 presents baseline results for total employment. Models (1) – (3) report OLS regressions. The OLS regressions with controls (Models (2) and (3)) indicate a small positive correlation between a state's FMAP outlays and its change in total employment, although the effect is not statistically significant.

Models (4) – (6) present the baseline IV results. There is a precisely estimated positive relationship between instrumented FMAP outlays and a state's change in total employment. In

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[18] The Centers for Medicare and Medicaid Services (CMS) reports that in 2008, Medicaid spending increased 4.7%. CMS projected that Medicaid spending would increase 9.9% in 2009. See http://www.cms.gov/NationalHealthExpendData/25_NHE_Fact_Sheet.asp.

the bivariate IV regression [Model (4)], the coefficient on total FMAP outlays per person 16+ is 4.72. While the large difference between the IV and OLS estimates may appear surprising given the strength of the first stage, recall that the first-stage residual should be strongly negatively correlated with employment growth due to the unemployment triggers in the FMAP increase, biasing the OLS results downward.

Adding a wide variety of control variables [Model (5)] changes the estimate little. Including the lagged employment control [Model (6)] reduces the point estimate by approximately 40% but has little effect on the statistical significance of the result, as the standard error also shrinks. The fact that adding a control for lagged employment influences the point estimate suggests that high and low Medicaid spending states were on different employment trends prior to the ARRA, a hypothesis that we explore in the robustness section.

The coefficient in (6), the preferred specification, suggests that for every $100,000 in FMAP outlays per individual 16+ that a state received by June 30, 2010, that states' total employment increased by 2.83 per individual 16+ from December 2008 to July 2009. Section VI provides further discussion of how to interpret this magnitude.

Table 4 parallels the results from Table 3, using the change in government, health, and education employment as the outcome variable. The OLS coefficients [Models (1) – (3)] are positive, relatively small in magnitude, and not statistically significant. The IV results [Models (4) – (6)], in contrast, suggest a positive relationship between FMAP transfers and change in employment in these sectors. For the IV specifications, the control variables have very little influence on the point estimates, but they do substantially reduce the standard errors. The coefficient on (6) suggests that for every $100,000 in FMAP outlays per individual 16+ that a state received by June 30, 2010, that states' employment in the government, health, and education sectors increased by 1.17 per individual 16+ over the period considered.

The coefficients in Table 4 are less than half of the magnitude of those in Table 3, suggesting that the "indirect" employment gains in the non-government-related sectors were substantial. To see this more explicitly, we re-estimate our preferred specification, changing the dependent variable to be the change in total employment excluding the change in employment in the government, health, and education sectors. This regression yields a coefficient of 1.86 (95% CI: 0.32, 3.41).

C. Timing Results

The previous section presented results where the outcome variable was the change in employment from December 2008 until July 2009. This section explores how our estimates evolve as we change the month that marks the end of our sample. Specifically, we re-run the cross-sectional regression for changes in employment from December 2008 until every month from January 2009 to June 2010 and report the second stage coefficients on total FMAP outlays from our preferred specification with the full set of control variables. That is, we re-run the estimate from December 2008 to January 2009, December 2008 to February 2009, December 2008 to March 2009, etc. and report each of these 18 coefficients.

Figure 3 presents these results for total nonfarm employment. The solid line represents the point estimate, and the dashed lines indicate the 95% confidence interval. These timing results suggest three main patterns. First, while there appears to be a positive relationship between FMAP outlays and change in employment before July 2009, the relationship is small and not precisely measured. Second, starting in July 2009, the coefficient jumps in magnitude, varying from a low of 2.16 (September 2009) to a high of 4.44 (February 2010). Finally, as expected, the standard errors tend to widen over time, although all of the coefficients remain statistically significant at the 95% level.

Figure 4 parallels the results from Figure 3, using employment in the government, health, and education sectors. The broad patterns present in Figure 3 are also present in Figure 4. Again, the coefficient increases for July 2009, and the standard errors increase over time. However, the ratio of the standard errors to the point estimate is larger than for total employment. Comparing the magnitudes between the two timing figures shows that in all months, the estimates for total employment are larger than those for government employment, with the gap increasing through 2009 and peaking in early 2010. This pattern is consistent with the government employment results reflecting the relatively immediate direct effect of states and state-funded establishments not having to lay off workers, while the total employment results include the lagging induced effects of households responding to higher disposable income.

V. Robustness Checks and Extensions

A. Falsification Tests

Our identifying assumption is that, conditional on our control variables, states that had higher pre-recession Medicaid spending would not have experienced different employment outcomes from states that were lower spenders in the absence of the increase in FMAP. One way of assessing this assumption is to consider if the effects we estimate are larger than the relationship between Medicaid spending and employment growth that existed prior to the period of interest.

Figure 5 reports the second stage coefficients for placebo tests using data that begin in January 2000 and end in December 2008. To parallel our baseline specification, we consider seven-month changes in both total nonfarm employment and employment in government, health, and education. We then run our IV estimates on each overlapping seven-month period, for a total of 101 regressions. We rank the coefficients based on their magnitude and report the empirical CDF. For comparison, we also show the second stage estimate run on the baseline period, December 2008 to July 2009, with a vertical line.

The results show two key patterns. First, the estimates are centered around 0; the empirical median of the estimate is 0.00 for total nonfarm and 0.11 for government, health and education. That is, in the years before the ARRA was passed, there is little evidence to suggest that high and low Medicaid spending states experienced systematically different employment trends. Second, our baseline estimates of both total nonfarm and government, health, and education employment are large relative to the coefficients in the period before the ARRA. For total employment, our result is larger than all but seven of the 101 pre-ARRA estimates. For government, health, and education, our estimate is larger than all but three of the pre-ARRA estimates. Both pieces of evidence increase our confidence that the estimates reported above are capturing the effect of the ARRA rather than underlying differences between high and low Medicaid spending states.

B. Other Robustness Checks

Our baseline specification allows for the possibility that high and low Medicaid spending states were on different pre-existing employment trends by controlling for a linear lag of the change in employment. This subsection addresses the concern that a linear lag may not be a sufficient statistic for pre-existing employment trends. Specifically, we report results allowing for a more flexible pre-existing trend and using a state's pre-treatment industry composition and the change in employment by industry in other states to impute employment change during the treatment period, following Bartik (1991) and Blanchard and Katz (1992). The latter require detailed industry data from the QCEW, a dataset that is not available on a seasonally adjusted basis and that does not have representative coverage of the government sector.[19] We therefore present results for the change in total nonfarm employment, and for December 2008 to December 2009 in the specifications that use the imputed employment predictor.[20]

Model (1) of Table 5 shows the second stage coefficient when we re-run our baseline specification, replacing the linear lag of employment change with an autoregressive model estimated using 18 years of data prior to the sample period to forecast a state's employment change from December 2008 to July 2009.[21] The second stage coefficient is 2.89, essentially unchanged from the value of 2.83 in the specification with the linear lag presented in table 3. Models (2)-(3) add a quadratic and cube of the lagged employment change to account for the fact that the serial correlation in changes in per capita employment may be non-linear, again with essentially no effect on the coefficient of interest. The next three columns shift the end-month to December 2009 in order to accommodate our measure of imputed QCEW employment change based on pre-ARRA industrial composition. The appendix contains further details of this variable's construction. As a benchmark, column (4) gives the baseline result that appears in Figure 3. Column (5) adds the imputed employment change, with very little effect on the FMAP coefficient. Column (6) replaces the outcome variable with QCEW data and again finds essentially the same result.[22] In sum, the relationship between FMAP transfers and employment growth appears very robust to our alternative methods of generating an employment change counterfactual.[23]

VI. Discussion

A. Job-years

Our results indicate a positive and robust relationship between receiving FMAP transfers and relative employment outcomes. To interpret the magnitude of the estimates, we can translate the regression coefficients into the increase in job-years from $100,000 of marginal state fiscal

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[19] According to Bureau of Labor Statistics (2008)¸ 5% of total state and local government workers are not covered by the QCEW.

[20] We perform the imputed employment calculation at the four-digit level because of disclosure limitations that eliminate observations at higher levels of detail.

[21] Specifically, the logarithm of total employment was regressed against a time variable and nine monthly lags of itself. The coefficients were then used on data through December 2008 to forecast employment from January 2009 through July 2009. Note that this control variable is helpful if the patterns of employment changes over the 18 years prior to our sample period remained unchanged during our sample period. Because our period involves the most severe recession since World War II, this assumption may not be valid.

[22] The closeness of the coefficients in columns (5) and (6) reflects the benchmarking of the CES to the QCEW.

[23] In results reported in an online appendix, we also experimented with other possible control variables that might capture channels similar to those discussed in the text. These include the generosity of states' unemployment insurance systems and the presence of a Democratic governor in February 2009 as proxies for political factors, an index of budget restrictiveness from the Advisory Committee on Intergovernmental Relations to address the concern that the 2007 Medicaid spending levels might be correlated with state budget rules, and the degree of house price appreciation during the mid-2000s as a proxy for economic conditions. The results reported in Tables 3 and 4 are robust to the inclusion of these additional controls.

relief. This requires two assumptions. First, we assume that FMAP outlays received through June 2010 have no employment effects beyond June 2010. If instead the employment effects linger beyond June 2010, then our estimate of job-years is a lower bound. Second, we assume that transfers to states after June 2010 do not influence employment changes before June 2010. This assumption is likely to be valid (at least for state employment) if states are unable to shift money across fiscal years.

Under these assumptions, the increase in job-years from $100,000 of FMAP outlays can be calculated by taking the integral under the timing charts (Figures 3 and 4) and dividing by 12 to convert job-months to job-years. Our point estimates suggest that $100,000 of marginal state fiscal relief increases state employment by 3.8 job-years, 3.2 of which are outside the government, health, and education sectors. The associated p-value for this calculation is 0.018 for total employment, while the p-value for total employment excluding the government, health, and education sectors is 0.010. Dividing $100,000 by 3.8 job-years yields a cost per job of $26,000.

When considering the generalizability of the results, it is important to consider both the intended and apparently realized fungibility of the funds. As noted above, the text of the bill made clear that the funds were for general obligations, and states reported using them for this purpose. Indeed, results disaggregating the government, health, and education employment results suggest that only about a quarter of the increase in employment was in the health sector, with another quarter in education and the other half in state and local government.[24]

In the context of our broader understanding of the costs and benefits of fiscal stimulus, state fiscal relief, in particular, may be a particularly low-cost means of supporting employment during a recession. Furthermore, the jobs increases were rapid, perhaps because “shovel-ready” projects were often not necessary; in many cases, state and local governments only needed to avoid cuts.

B. *Comparison to the Literature*

This paper contributes to a literature which uses cross-state variation to estimate fiscal multipliers. We do this using the most recently-available evidence in a context in which the parameter being estimated has direct relevance to a policy question: how much is employment increased by state fiscal relief during a recession? Although estimated in quite different settings, Suarez-Serrato and Wingender (2011) and Shoag (2011) find estimates which are remarkably similar to our estimate of cost per job, at $30,000 and $35,000 per year respectively.[25]

While the political debate has focused on the effect of fiscal stimulus on employment, the academic literature more commonly estimates the government purchases multiplier for output. Also using cross-state variation, Nakamura and Steinsson (2011) find an open-economy government purchases multiplier of 1.5, and Shoag (2011) finds an output multiplier of 2.1. Our findings are consistent with this range. We roughly map our results to an output multiplier as follows: in 2008 average compensation in both the total economy and state and local government was $56,000 per employee. If total compensation equals the marginal product of labor and

[24] When using the change from December 2008 to July 2009 in state and local government employment as the dependent variable in our baseline regression, we estimate a coefficient of 0.65 (SE = 0.26) on the FMAP transfers, while changes in health and education employment yield coefficients of 0.21 (SE = 0.10) and 0.29 (SE = 0.11) respectively.

[25] See also Neumann et al. (2010) and Fishback and Kachanovskaya (2010) for studies using cross-sectional variation during the Great Depression.

workers affected by state fiscal relief have this same average compensation, this result would imply an output multiplier for a dollar of transfers of about 2.[26] Given that the results from this cross-state approach do not incorporate general equilibrium effects, cross-state multipliers, or the response of a monetary authority, we interpret this multiplier as only suggestive of the national multiplier of policy interest.[27]

A few other papers have also studied parts of the ARRA. Wilson (2011) and Feyrer and Sacerdote (2011) report costs per job of $114,312 and $170,000, respectively, but their numbers are not directly comparable to the 3.8 jobs per $100,000 reported above because they do not account for the timing of job creation, and they cover other portions of the stimulus.[28] Sahm et al. (2010) find a relatively modest impact from the Making Work Pay tax cut. Mian and Sufi (2010) find that the relatively small ($3 billion) "Cash for Clunkers" program (which was separate from the ARRA but implemented concurrently during the summer of 2009) had little net effect on purchases.[29]

VII. Mechanism

The ARRA transfers reached states in dire fiscal condition. During the 2009 fiscal year, 43 states faced budget gaps totaling more than $60 billion (National Conference of State Legislatures 2009). Almost all states have balanced budget requirements.[30] Thus, the large budget gaps necessitated that they take action by cutting expenditures, raising revenues, or drawing from their "rainy day" funds or end of year balances, which are used to smooth revenue across years.[31] Indeed, by December 2008, 22 states had made or announced cuts to their expenditures totaling $12 billion.[32] By July 2009, 42 states had made cuts to their expenditures totaling more than $30 billion, and 30 states had increased taxes or fees to boost their revenues.[33]

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[26] This calculation assumes that capital stays fixed. Data on average compensation per employee come from the Bureau of Economic Analysis GDP-by-Industry accounts. The output multiplier equals the jobs multiplier multiplied by value-added per job (equivalent to a worker‘s marginal product), or (3.8/$100,000)*$56,000=2.13.

[27] Ramey (2011) surveys the literature on national output multipliers. Our estimate is at the upper end of her preferred range, consistent with recent empirical work on state-dependent output multipliers that finds higher multipliers occur during depressed demand conditions such as prevailed during our period of study (Auerbach and Gorodnichenko forthcoming). Nakamura and Steinsson (2011) and Shoag (2011) explore the theoretical mapping from these estimates of local fiscal multipliers to the national multiplier in an open economy setting.

[28] Wilson's results for total job creation are closest to ours. This is not surprising, since his paper adopts our instrument, along with using simulated instruments for highway and education spending. The Feyrer-Sacerdote number corresponds only to "direct jobs" funded by the ARRA. Conley and Dupor (2011) find a positive effect of ARRA transfers on government employment, but no positive effect on employment outside of government.

[29] The Obama Administration (Council of Economic Advisers 2010), Congressional Budget Office (2010) and private forecasters and academics (Blinder and Zandi 2010) have all evaluated the ARRA using a multiplier model based on historical relationships between government spending, output and employment. These studies tend to find effects similar to or slightly smaller in magnitude than those in the current study for state fiscal relief. However, they are all calibrated models, whereas the current study uses empirical estimation. Council of Economic Advisers (2009) reported preliminary results of those in the current paper.

[30] All states, except for Vermont, have some version of balanced budget requirements as reported by the National Association of State Budget Officers (2008a). Poterba (1994) gives an overview of the varying requirements.

[31] From National Association of State Budget Officers (2008a). Kansas and Montana do not have budget stabilization (or "rainy day") funds. However, they, like other states, may use surpluses from the prior fiscal year to cushion any fiscal difficulty in the next.

[32] From National Association of State Budget Officers (2008b).

[33] Budget cuts from the National Association of State Budget Officers (2009a). The $32 billion figure refers to the expenditure cuts in fiscal year 2009 alone. Tax increases from Johnson, Nicholas, and Pennington (2009).

There are essentially only three ways in which states could use the ARRA state fiscal relief funds: to alleviate program cuts, to prevent or lower tax and fee increases, or to contribute to their end of year balances (which include their rainy day funds). As long as the states did not respond to the federal transfers by completely siphoning them to their end of year balances, the observed employment responses could come from multipliers on the states' spending or tax actions. The results in Section IV suggest that the ARRA funds were at least partially used to avoid program cuts, since a concentration of the employment effects appears to have occurred in sectors (government, health, and education) which are reliant on state funds. That total employment beyond those sectors is also affected positively by the federal fiscal relief suggests that there is a source of spillovers, arising from higher disposable income due to either the wages of the direct hires or lower net taxes because of fewer tax or fee increases.[34]

We can directly test the necessary condition that FMAP outlays affected spending or tax actions by regressing the change in end of year balances from 2008 to 2009 on instrumented FMAP outlays and controls. Models (1) – (3) of Table 6 summarize the results of these regressions. All else equal, if states that received more FMAP money decreased their balances less, we would expect a positive and significant coefficient on FMAP outlays, with the extreme case that if all of the money were saved we would expect a coefficient of 1. Instead, the estimates in (1) – (3) are small in magnitude, negative in all three of the specifications, and never significantly different from 0.[35] Furthermore, the models allow us to reject the null that half of transfers were saved by states at the 99% confidence level for two regressions and at the 95% confidence level for the third, confirming that at least some of the funds were used to slow either budget cuts or tax increases. Models (4) – (6) of Table 6 repeat the same exercise, using the change in end of the year balances from 2009 to 2010 as the dependent variable, and yield similar results.[36] In summary, although the regressions have wide standard errors, the point estimates provide no evidence to suggest that states are retaining the transferred money in the form of end of year balances or rainy day funds.[37]

To determine if states that received more transfers cut their budgets less, we ran specifications that parallel those in Table 6 where the outcome variable was the change in expenditure (normalized by a state's 16+ population) between 2008 and 2009 and between 2009 and 2010. Unfortunately, the results from this regression are quite noisy, and we can neither reject the null

[34] Several recent empirical studies have found a positive effect of lower taxes or higher transfers on economic outcomes (Johnson, Parker and Souleles 2006; Sahm, Shapiro and Slemrod 2009; Romer and Romer 2010).

[35] We exclude Alaska, a state that experienced a per 16+ population decline in its end of year funds that was more than ten times larger than that of the next largest states. When we include Alaska, we also cannot reject the null that the coefficient on total FMAP outlays per person is equal to 0 (p-value for the bivariate IV regression is 0.435 for changes from 2008 to 2009 and is 0.311 for changes from 2009 to 2010). In addition, because the National Association of State Budget Officers does not provide data on DC, we exclude it from our regressions.

[36] Poterba (1994) and Alt and Lowry (1994) examine how the states' balanced budget rules affect their responses to deficits and find that, in response to a positive deficit shock, states cut expenditures or raise taxes within either the current or the following fiscal year. This is consistent with the findings that a federal transfer (a negative deficit shock) would impact expenditures or taxes.

[37] These results contradict those of Cogan and Taylor (2010), who find using aggregate time-series data that ARRA Medicaid spending increased aggregate state net lending as measured in the National Income and Product Accounts. Given the unusual nature and length of the 2007-09 recession and its effect on state budgets, it is possible that aggregate time-series regressions misattribute the effect of the worsening recession and the eventual binding of state balanced budget requirements on net lending to the introduction of the FMAP expansion. Alternatively, it is possible that all states increased their saving in response to the FMAP transfers by the same dollar amount per capita, regardless of the amount of FMAP transfers actually received.

that all of the money was spent on reducing budget cuts (which would imply a coefficient of one) nor the null that none of the money was spent on reducing budget cuts (which would yield a coefficient of zero).[38] Results using changes in a state's revenue are similarly noisy, and thus do not provide conclusive evidence about the use of funds to reduce tax or fee increases. Further research into how states optimize over the margins of tax and spending when faced with an altered budget constraint would be a worthwhile area of future study.

VIII. Conclusion

This paper estimates the employment effects of a relatively unstudied form of government macroeconomic intervention that took center stage in the recent ARRA: fiscal relief to states during a downturn. We exploit cross-state variation in transfer receipts that comes from pre-recession differences in Medicaid spending. All else equal, states that spent more money on Medicaid before the recession received more money from the federal government. We confront the major threat to identification—that states that spent more money on Medicaid may be on differential employment trends from states that spent less—in several ways, including adding regional fixed effects and other control variables as well as conducting placebo tests. Our baseline specifications suggest that $100,000 of marginal spending increased employment by 3.8 job-years, 3.2 of which are outside the government, health, and education sectors.

The fact that state fiscal relief may be an effective tool to cushion employment losses in recessions raises two questions. First, if the employment effects of state fiscal relief are substantial, should the federal government play a larger role in providing revenue to states during recessions? When designing state fiscal relief, federal planners face a tradeoff between providing relief to states experiencing critical budget situations and minimizing perverse incentives for state policy makers. If states expect to receive federal aid during recessions, they may not save sufficiently during boom times. This moral hazard is compounded if federal aid targets states with larger budget shortfalls, which might be desirable because aid distributed using a non-need-based formula would likely produce smaller employment effects. An important area of future research is to determine the extent to which these tradeoffs limit the potential for state fiscal relief to be an effective tool for cushioning job losses during recessions.

Second, why are states unable to save money during economic booms and use this savings during recessions? Because most states have adopted balanced budget legislation, states cannot borrow money during recessions to smooth fluctuations. As a substitute, most states have a "rainy day" fund that allows them to avoid the requirement of literally balancing their budget every fiscal year. However, political economy considerations make saving difficult for democratic governments (Alesina and Tabellini 1990; Amador 2003), and most states have essentially no restrictions on when they must contribute to their rainy day fund.[39] For example,

[38] Fiscal year 2008 expenditure data and the enacted tax and fee data are from the National Association of State Budget Officers (2009b). Fiscal year 2009 and 2010 expenditure data are from the National Association of State Budget Officers (2010).

[39] The majority of states have requirements that they contribute to their rainy day funds only if the budget has a surplus. However, because states determine when they have a surplus by setting the level of taxes and spending, in practice such requirements impose few restrictions on states' contributions to their rainy day funds.

during the 1990s economic boom, states increased spending and cut taxes rather than contributing to their rainy day funds.[40]

To help solve these political economy problems, some states have considered adopting rules that would require the state to contribute to their rainy day fund during healthy economic times. For example, a state could be required to contribute to its fund when the unemployment rate in the state falls below a given threshold, and be permitted to tap into its fund when the unemployment rate rises to a sufficiently high level. These regulations have the advantage of constraining politicians, while helping to alleviate some of the fiscal strain induced by a recession. The evidence presented in this paper, though it concerns funding from the federal government, also informs the impact of additional state resources on state-level employment, and suggests that these and other rules may help states boost employment during recessions. Future research could focus on additional benefits, as well as costs, from state fiscal relief and state budgetary rules.

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[40] See Zahradnik and Ribeiro (2003) and the National Conference of State Legislatures (2004).

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FIGURES

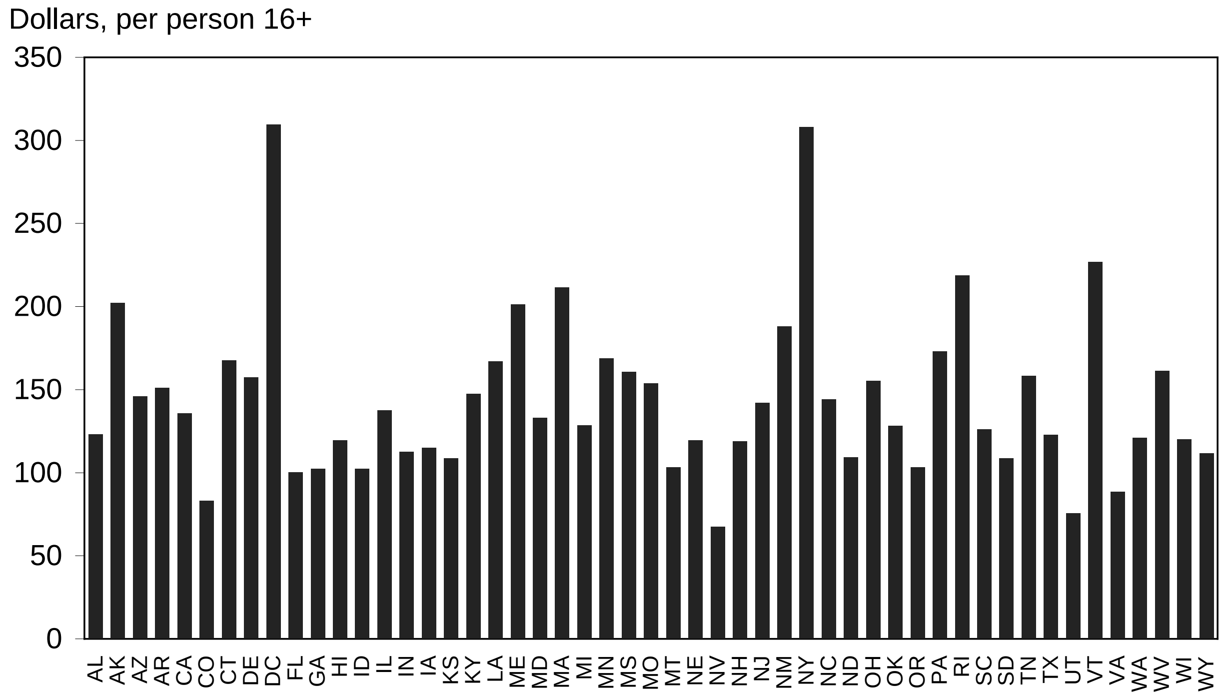

FIGURE 1. VALUE OF THE SCALED INSTRUMENT

Notes: The value of the scaled instrument is 0.062 * state's fiscal year 2007 Medicaid spending * 21/12. See text for full details. Data are from the Center for Medicaid Services, Data Compendium, Table VII.1.

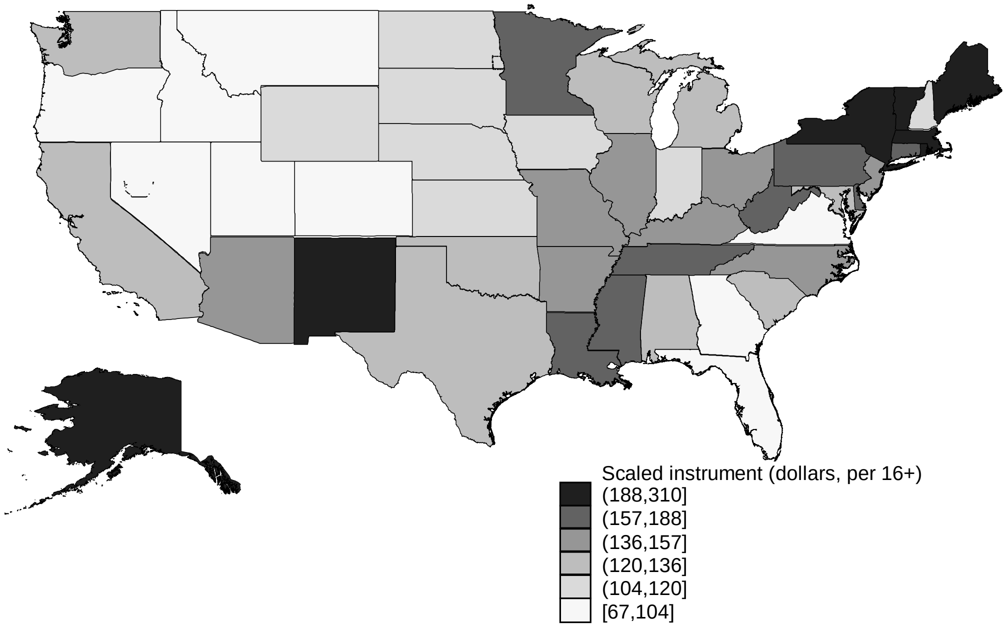

FIGURE 2. VALUE OF THE SCALED INSTRUMENT

Notes: The value of the scaled instrument is 0.062 * state's fiscal year 2007 Medicaid spending (per person 16+) * 21/12. See full text for details. Data are from the Center for Medicaid Services, Data Compendium, Table VII.1.

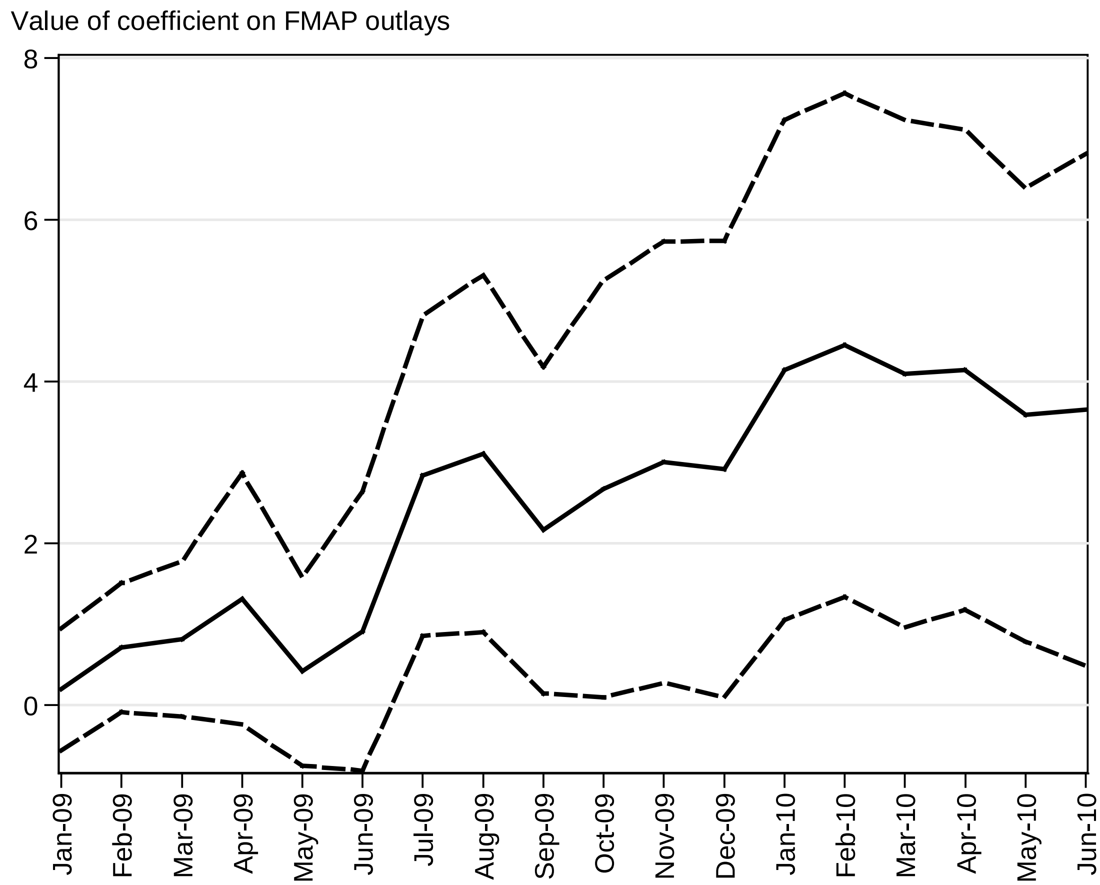

FIGURE 3. TOTAL NONFARM SECOND STAGE COEFFICIENTS

Notes: This chart displays the second stage coefficient for regressions where the outcome variable is the change in seasonally adjusted employment between December 2008 and the month indicated on the x-axis. The variable of interest is total FMAP outlays. Regressions include the full set of controls. The 95% confidence interval, derived from robust standard errors, is plotted in dashed lines.

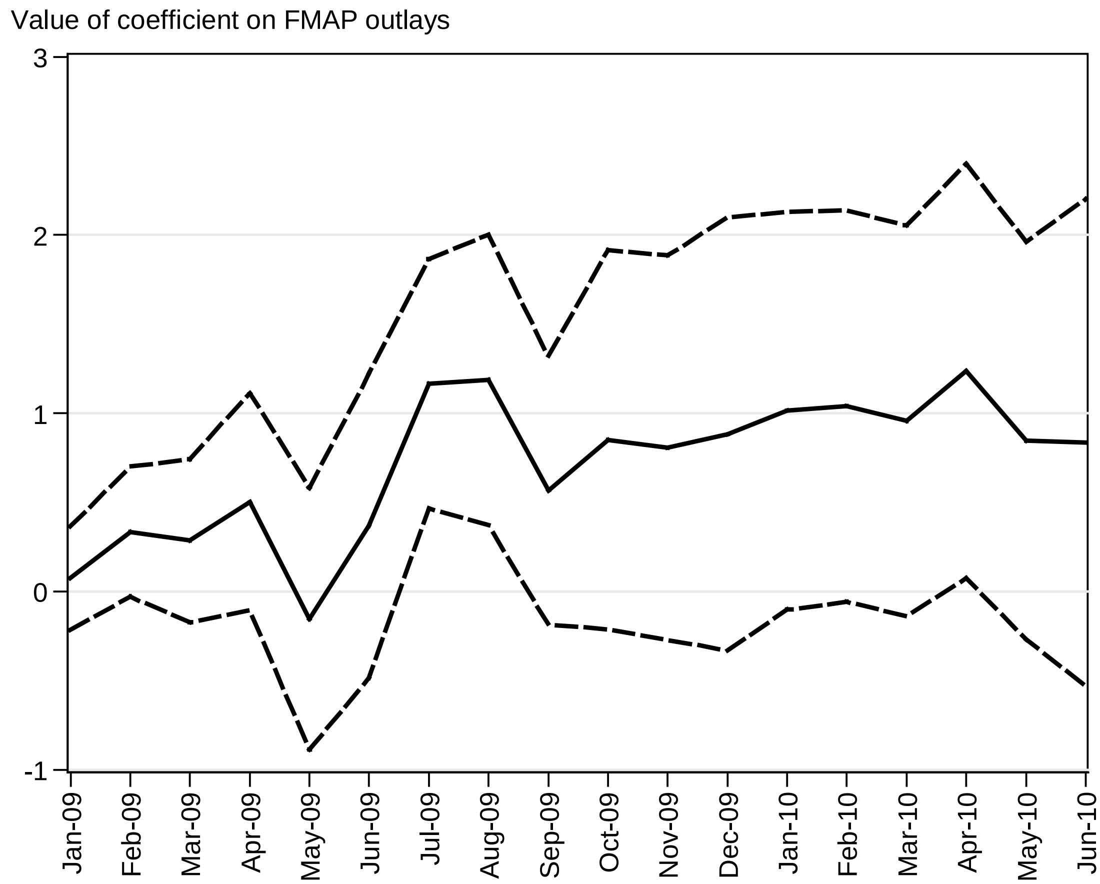

FIGURE 4. GOVERNMENT, HEALTH, AND EDUCATION SECOND STAGE COEFFICIENTS

Notes: This chart displays the second stage coefficient for regressions where the outcome variable is the change in seasonally adjusted employment between December 2008 and the month indicated on the x-axis. The variable of interest is total FMAP outlays. Regressions include the full set of controls. The 95% confidence interval, derived from robust standard errors, is plotted in dashed lines.

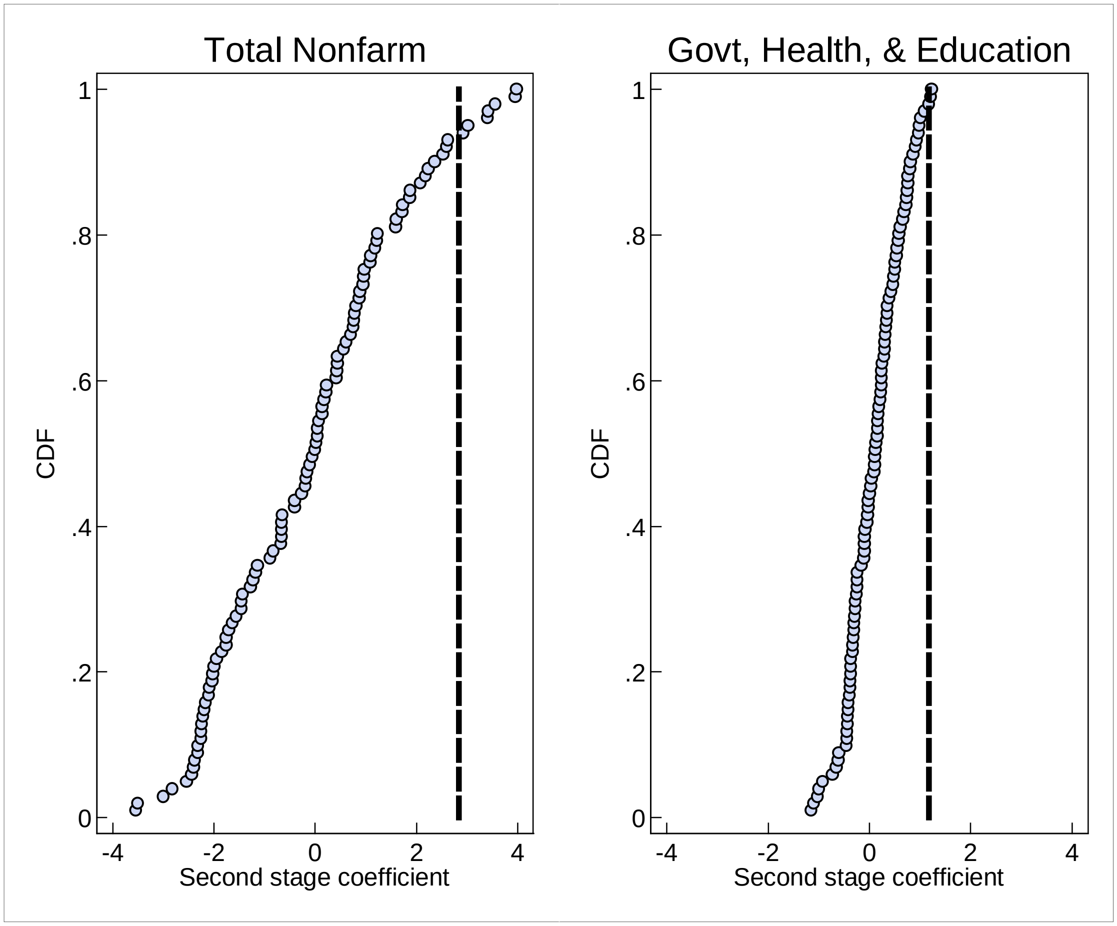

FIGURE 5. PLACEBO RESULTS

Notes: Plots results of second stage regressions, where the outcome variable is seasonally adjusted change in employment for each overlapping seven month period, starting in Jan 2000 and ending in Dec 2008. All regressions include the full set of control variables. Coefficient from Dec 2008 to July 2009 is indicated with the vertical line. Note that government excludes federal government employment.

TABLES

TABLE 1—SUMMARY STATISTICS

	Mean	Std. Dev.	Min.	Median	Max.
Outcome Variables, Per 1,000 People 16+					
Δ total nonfarm employment, Dec. 2008 → July 2009	-18.76	7.15	-38.84	-18.23	3.11
Δ govt, health, & education, Dec. 2008 → July 2009	0.97	2.06	-2.13	0.53	9.11
Payout Variables and Instrument, Per Person 16+					
Total ARRA outlays through June 2010	$1,002	$323	$586	$960	$2,940
Total FMAP outlays through June 2010	$250	$90	$103	$235	$507
Total FMAP and SFSF outlays through June 2010	$373	$88	$176	$358	$583
2007 Medicaid spending (instrument)	$1,328	$454	$624	$1,227	$2,854
Control Variables					
Employment in manufacturing, percent	11.03	4.28	1.40	11.00	20.30
Vote share Kerry (2004), percent	46.52	10.38	26.00	47.02	89.18
Union share, percent	11.16	5.49	3.00	10.40	25.20
GDP per person 16+ ($1,000)	49.20	17.20	31.91	46.28	154.89
Population 16 and older (millions)	4.60	5.13	0.41	3.32	27.85
Δ total nonfarm employment, May 2008 → Dec. 2008	-11.04	6.91	-33.42	-11.25	2.60
Δ govt, health, & education, May 2008 → Dec. 2008	1.73	1.27	-1.44	1.75	6.30

Notes: See text and data appendix for sources. Note that "government" excludes federal government employees. All employment data are seasonally adjusted and reported per 1,000 people 16+.

TABLE 2—FIRST STAGE REGRESSIONS

	(1)	(2)	(3)	(4)
2007 Medicaid spending (instrument)	0.18***	0.15***	0.16***	0.15***
	(0.01)	(0.01)	(0.01)	(0.01)
Region fixed effects?		X	X	X
Vote share Kerry (2004)		X	X	X
Union share		X	X	X
GDP per person 16+		X	X	X
Employment in manufacturing		X	X	X
State population		X	X	X
Lagged total employment change May 2008 to Dec 2008			X	
Lagged government, health, and education employment change May 2008 to Dec 2008				X
Observations	51	51	51	51
R-squared	0.84	0.93	0.93	0.93
Mean of dependent variable	250.23	250.23	250.23	250.23

Notes: The outcome variable for each regression is total FMAP outlays per individual 16+ in a state, through June 30, 2010. The variable is measured in $100,000 per person 16+. See text and data appendix for sources. Note that "government" excludes federal government employees. Robust standard errors are in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 3— TOTAL EMPLOYMENT BASELINE RESULTS

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per	2.94**	1.88	0.82	4.72***	4.61***	2.83***
person 16+ ($100,000)	(1.35)	(1.83)	(1.06)	(1.31)	(1.57)	(1.01)
Vote share Kerry (2004),		0.28	2.1		-0.79	1.14
percent/10,000		(2.02)	(1.57)		(1.59)	(1.14)
Union share,		-4.26	-2.93		-6.00**	-4.29**
percent/10,000		(3.60)	(2.17)		(2.91)	(2.01)
GDP per person 16+		0.01	-0.03		-0.01	-0.04
($1,000,000)		(0.07)	(0.06)		(0.06)	(0.05)
Employment in manuf.,		-10.05***	-6.61***		-9.75***	-6.83***
percent/10,000		(3.05)	(2.39)		(2.82)	(2.12)
State population 16+,		-0.43***	-0.33***		-0.46***	-0.36***
billions		(0.12)	(0.08)		(0.10)	(0.08)
Lagged total employment			0.42*			0.37**
ch. May to Dec 2008			(0.21)			(0.17)
Region fixed effects?		X	X		X	X
Observations	51	51	51	51	51	51
Mean of dep. var. * 1,000	-18.76	-18.76	-18.76	-18.76	-18.76	-18.76

Notes: The outcome variable for each regression is the seasonally adjusted change in total non-farm employment per individual 16+ in a state, from December 2008 to July 2009. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. Specifications (4) - (6) instrument total ARRA FMAP payouts with pre-recession Medicaid spending as described in the text. See text and data appendix for sources. Robust standard errors are in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 4— STATE AND LOCAL GOVERNMENT, HEALTH, AND EDUCATION

	OLS			IV		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per	0.43	0.34	0.30	0.99*	1.19***	1.17***
person 16+ ($100,000)	(0.53)	(0.44)	(0.40)	(0.54)	(0.37)	(0.36)
Vote share Kerry (2004)		-0.76*	-0.64		-1.10***	-1.01***
percent/10,000		(0.39)	(0.39)		(0.30)	(0.32)
Union share,		0.16	0.33		-0.38	-0.26
percent/10,000		(0.95)	(0.96)		0.76	0.8
GDP per person 16+		0.07***	0.07***		0.06***	0.06***
($1,000,000)		(0.02)	(0.02)		(0.02)	(0.02)
Employment in manuf.,		-1.93**	-1.84*		-1.84**	-1.77**
percent/10,000		(0.89)	(0.96)		(0.84)	(0.88)
State population 16+,		-0.11***	-0.10**		-0.12***	-0.11***
billions		(0.03)	(0.04)		(0.03)	(0.03)
Lagged total employment			0.18			0.14
ch. May to Dec 2008			(0.18)			(0.17)
Region fixed effects?		X	X		X	X
Observations	51	51	51	51	51	51
Mean of dep. var. * 1,000	0.97	0.97	0.97	0.97	0.97	0.97

Notes: The outcome variable for each regression is the seasonally adjusted change in total employment in state and local government, health, and education per individual 16+ in a state, from December 2008 to July 2009. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. Specifications (4) - (6) instrument total ARRA FMAP payouts with pre-recession Medicaid spending as described in the text. See text and data appendix for sources. Note that "government" excludes federal government employees. Robust standard errors are in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 5— TOTAL EMPLOYMENT ROBUSTNESS CHECKS

	Dec 2008 to July 2009			Dec 2008 to Dec 2009		
		CES		CES		QCEW
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per	2.89**	2.80***	2.79**	2.92**	2.74**	2.81**
person 16+ ($100,000)	(1.23)	(0.95)	(1.29)	(1.44)	(1.34)	(1.27)
Baseline controls	X	X	X	X	X	X
Forecasted emp ch, Dec 2008 to July 2009, CES	X					
Lagged total emp ch, July 2008 to Dec 2008, CES		X	X	X	X	X
Lagged total emp ch 2008, squared, July 2008 to Dec, CES		X	X			
Lagged total emp ch cubed, July 2008 to Dec 2008, CES			X			
Imputed emp ch, Dec 2008 to Dec 2009, QCEW					X	X
Observations	51	51	51	51	51	51
Mean of dependent variable * 1,000	-18.76	-18.76	-18.76	-21.81	-21.81	-22.17

Notes: Note: In (1) - (3), the outcome variable is the change in total employment from December 2008 to July 2009 from the CES. In (4) and (5), the outcome variable is the change in total employment from December 2008 to December 2009 from the CES. For (6), the the outcome variable is the change in total employment from December 2008 to December 2009 from the QCEW. The main variable of interest is total ARRA FMAP payouts through June 30, 2010. The construction of the instrument is described in the text. "Baseline controls" are vote share Kerry, union share, GDP per person 16+, employment in manufacturing, state population, and region fixed effects. Sources of control variables are detailed in the data appendix. See the text for the construction of forecasted and imputed employment change. Robust standard errors are in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

TABLE 6— TRANSMISSION MECHANISM

	Rainy Day Fund, change 2008 to 2009 (IV)			Rainy Day Fund, change 2009 to 2010 (IV)		
	(1)	(2)	(3)	(4)	(5)	(6)
Total FMAP payout per person 16+ ($100,000)	-0.26	0.01	-0.14	-0.04	0.08	0.04
	(0.18)	(0.23)	(0.21)	(0.09)	(0.18)	(0.17)
Region fixed effects?		X	X		X	X
Includes lagged employment?			X			X
Excludes Alaska?	X	X	X	X	X	X
Missing DC?	X	X	X	X	X	X
Observations	49	49	49	49	49	-17.84
Mean of dep. var. (*100,000)	-29.22	-29.22	-29.22	-17.84	-17.84	-17.84

Notes: The outcome variable for (1) - (3) is change in a state's rainy day fund, in $100,000, per person 16+, from fiscal year 2008 to fiscal year 2009. The outcome variable for (4) - (6) is the change in a state's rainy day fund, in $100,000, per person 16+, from fiscal year 2009 to fiscal year 2010. Data are from the National Association of State Budget Officers (NASBO) Fiscal Survey of the States. The fiscal 2008 rainy day fund data come from the Fall 2009 Fiscal Survey, and the fiscal 2009 and 2010 rainy day fund data come from the Spring 2010 Fiscal Survey. All specifications exclude DC due to missing data. They also drop Alaska, an outlier in terms of the change in the state rainy day fund. Robust standard errors are in parentheses.

*** Significant at the 1 percent level.

** Significant at the 5 percent level.

* Significant at the 10 percent level.

APPENDIX

Sources and Descriptions of Baseline Control Variables

- Region effects: We include dummy variables for each of the 9 census defined divisions. Definitions are given at http://www.census.gov/geo/www/us_regdiv.pdf.

- Lagged change in employment: We control for the lagged value of the outcome variable. Specifically, if the outcome variable is change in total (government, health, and education) employment from month *m* to month *m'*, the lagged change in employment will be the change in total (government, health, and education) employment from month *m-7* to month *m*. In our baseline specification where the outcome variable is change in employment from December 2008 to July 2009, the lagged change in employment is the change in employment from May 2008 to December 2008. This 7 month lag was chosen to follow the 7 month period used for the outcome variable in our baseline specification.

- GDP per potential worker: We use a state's 2008 GDP, normalized by a state's 16+ population, from the Bureau of Economic Analysis.[41]

- Employment manufacturing share: From the Census Bureau, we also control for the share of the civilian employed population 16 years and older that is in the manufacturing sector. Data are from the American Community Survey and are averaged over 2005 – 2007 to reduce measurement error.

- State 2008 population and 2008 16+ population. Data are from the US Census.

- 2004 Kerry share. The state's share of voters who cast ballots for Senator Kerry in the 2004 United States presidential election.

- Union share. Share of workers in a union, from the Bureau of Labor Statistics.[42]

Description of Imputed Employment

As a robustness check, we control for a measure of imputed employment that uses information on the change in employment in each industry in the rest of the country and the initial industry distribution in each state to impute an expected employment change. Specifically, for state j and industry k define the percent change in employment in industry k in all other states as:

$$\text{(A1)} \qquad \%\Delta E^{-j,k} = \frac{\sum_{s\neq j} E_{\mathrm{t}}^{s,k}}{\sum_{s\neq j} E_{\mathrm{t}-1}^{s,k}} - 1$$

The imputed employment change for industry k in state j is then:

$$\text{(A2)} \qquad \Delta\hat{E}^{j,k} = E_{\mathrm{t}-1}^{j,k} \cdot \%\Delta E^{-j,k}$$

[41] See http://www.bea.gov/newsreleases/regional/gdp_state/2009/pdf/gsp0609.pdf.
[42] From http://www.bls.gov/news.release/pdf/union2.pdf.

The total imputed employment change for state j is the sum over all industries:

$$\text{(A3)} \qquad \Delta\hat{E}^{j} = \sum_k \Delta\hat{E}^{j,k}$$

We implement (A1)-(A3) using the QCEW December 2008 and December 2009 state-level flat files.[43] The QCEW provides employment by NAICS code and ownership status (private, federal government, state government, and local government). The QCEW suppresses output for state-industry-ownership rows where the number or concentration of firms does not surpass a minimum disclosure threshold. Letting $o \in \{private, federal, state, local\}$ define ownership status, we set $E_{t-1}^{j,k,o} = 0$ for any state-industry-ownership row with suppressed output. In practice, missing the disclosure threshold correlates well with small size, so this assumption is quite mild. Nonetheless, we define industries using four digit rather than six digit NAICS codes in order to minimize disclosure limitations. Using six digit industries yields very similar results. Since we are interested in industry variation, we collapse the QCEW data on ownership status before implementing (A1)-(A3).

[43] The files can be downloaded from ftp://ftp.bls.gov/pub/special.requests/cew/yyyy/state for yyyy={2008,2009}.

The Employment Effects of Credit Market Disruptions: Firm-level Evidence from the 2008-09 Financial Crisis

Gabriel Chodorow-Reich*

Abstract

This paper investigates the effect of bank lending frictions on employment outcomes. I construct a new dataset that combines information on banking relationships and employment at two thousand nonfinancial firms during the 2008-09 crisis. The paper first verifies empirically the importance of banking relationships, which imply a cost to borrowers that switch lenders. I then use the dispersion in lender health following the Lehman crisis as a source of exogenous variation in the availability of credit to borrowers. I find that credit matters. Firms that had pre-crisis relationships with less healthy lenders had a lower likelihood of obtaining a loan following the Lehman bankruptcy, paid a higher interest rate if they did borrow, and reduced employment by more compared to pre-crisis clients of healthier lenders. Consistent with frictions deriving from asymmetric information, the effects vary by firm type. Lender health has an economically and statistically significant effect on employment at small and medium firms, but the data cannot reject the hypothesis of no effect at the largest or most transparent firms. Abstracting from general equilibrium effects, I find that the withdrawal of credit accounts for between one-third and one-half of the employment decline at small and medium firms in the sample in the year following the Lehman bankruptcy.

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1. Introduction

Does the health of banks on Wall Street affect economic outcomes on Main Street? In the wake of the 2008-09 financial crisis, this question has generated substantial interest among politicians, the popular press, and the public at large. The renewed interest partly reflects the deeply unpopular government support for financial institutions, which policymakers defended by arguing that not providing such support would have dire implications for jobs in sectors far removed from banking (see e.g. Bernanke 2008). Notwithstanding the policy interventions, bank lending to nonfinancial firms in the United States contracted significantly during the crisis, and the economy experienced the sharpest employment decline in sixty years.

This paper investigates the link between credit market frictions and employment. I construct a new dataset that merges the Dealscan syndicated loan database, which contains the borrowing history of both public and private firms that have accessed the syndicated loan market, with confidential employment data from the Bureau of Labor Statistics Longitudinal Database. The merged dataset contains information on employment outcomes and banking relationships at two thousand nonfinancial firms, ranging in size from fewer than fifty employees to more than ten thousand. I then compare employment outcomes at firms that had borrowed before the crisis from relatively healthy financial institutions with otherwise similar firms that had borrowed from lenders more adversely affected during the crisis.

The paper's methodological approach relies on two key facts established below. First, bank-borrower relationships are sticky. This means that firms that borrowed *before* the crisis from banks that did less lending *during* the crisis would have greater difficulty obtaining bank financing than firms that had borrowed from healthier lenders. Second, the origins of the 2008 crisis lay outside of the corporate loan sector, implying that the cross-sectional variation in banks' willingness to make corporate loans was plausibly orthogonal to the characteristics of each banks' pre-crisis borrowers.

To further clarify the logic of the exercise, consider the examples of U.S. Bankcorp and Credit Suisse, both active lenders in the syndicated market in the United States. During the financial crisis, Credit Suisse suffered large losses from exposure to mortgage-backed securities, and its stock price declined by 60 percent during 2007-08. U.S. Bankcorp had relatively little exposure to mortgage-backed securities, experienced one of the smallest stock price declines among major banks during the crisis, and in July 2009 became among the first banks to fully repay its TARP commitment to the U.S. Treasury. In the nine months between the bankruptcy of Lehman Brothers and the end of the recession, Credit Suisse reduced its lending in the syndicated market by about 79 percent relative to before the crisis, whereas U.S. Bankcorp reduced lending by only 14 percent. As a result, firms with pre-crisis lending syndicates where U.S. Bankcorp had a lead role were nearly four times as likely to receive a loan during the crisis as firms with syndicates where Credit Suisse had a lead role.

To establish the causal effect of credit supply in the general case, I first document the importance of borrower-lender relationships in the syndicated loan market. In a syndicated loan, the "lead arrangers" set up the loan deal with the borrower and provide most of the financing, and recruit other "participant" lenders to provide the remainder of the funds. Among borrowers that have previously accessed the syndicated market, the empirical stickiness in borrower-lender relationships exceeds by a factor of seven what one would predict based only on lenders' market shares, indicating frictions to switching lenders.

I next discuss measures of bank health in order to isolate the effect of credit supply. Unfortunately, banks' internal cost of funds are not directly observable. Instead, I measure the relative health of a firm's lenders using the amount of lending to other borrowers during the crisis by the firm's pre-crisis syndicate. The validity of this measure relies on unobserved components of borrower demand not varying in the cross-section of lenders. Three pieces of evidence lend support to this assumption. First, the sample appears well-balanced on observable characteristics, including industry and county employment of the borrowers. Second, using the Khwaja and Mian (2008) within-firm estimator, I show that the estimated effect of lender health on the likelihood of borrowing does not change in specifications with and without borrower fixed effects. This result directly addresses the concern of unobserved borrower characteristics within the subsample of firms that obtain a new loan during the crisis. Third, placebo regressions corresponding to employment changes near the end of the last business cycle expansion and during the 2001 recession indicate that employment at firms that had pre-crisis relationships with less healthy lenders did not differ systematically from employment at other firms during the placebo periods.

I also report results that instrument for banks' overall lending. The instrument set exploits the fact that the financial crisis originated outside of the nonfinancial corporate sector. The first instrument follows Ivashina and Scharfstein (2010) in measuring exposure to Lehman Brothers through the syndicated market. The second captures exposure to mortgage backed securities through the loading of each bank's stock return on the ABX index. The third contains selected items from banks' balance sheet and income statement items chosen to avoid concerns of reverse causality coming through the corporate loan portfolio. Each of the proposed instruments has predictive power for the change in lending by the bank. In these specifications, the identification assumption becomes that less healthy banks as measured by the particular instrument did not also lend to corporate borrowers drawn from a different distribution of borrower health.

With the bank health measures in hand, I turn to the consequences of the sharp contraction in credit supply following the collapse of Lehman Brothers. Pre-crisis clients of banks in worse financial condition had a fifty percent lower likelihood of receiving a new loan or a positive modification in the nine months following the Lehman failure. Moreover, among banks that did obtain a loan, interest spreads increased more. These findings resemble those of Ivashina and Scharfstein (2010) and Santos (2011) using similar data, but differ from those papers by focusing on borrower outcomes rather than bank outcomes, thereby establishing that borrowers of weaker banks could not simply switch to healthier banks during the crisis.

I then present evidence that the effects in the loan market translated into effects on real outcomes for the borrowers. In the year following the Lehman Bankruptcy, employment at pre-crisis clients of lenders at the 10th percentile of bank health fell by roughly four to five percentage points more than at clients of lenders at the 90th percentile. The estimated magnitude changes little whether or not the bank's overall change in lending is instrumented using the measures described above. Moreover, the instruments separately yield quantitatively similar results despite relatively weak cross-correlations within the instrument set.

The data also suggest that the importance of the credit supply channel varied by firm type. A partial equilibrium aggregation exercise indicates that the credit channel can explain between one-third and one-half of the employment decline at small and medium-sized firms in the sample (fewer than one thousand employees) in the year following Lehman. In contrast, the data cannot reject that the relative availability of bank credit supply had no effect on relative employment outcomes at the largest firms, or at firms with access to the bond market. The finding of differential effects at large and small firms can serve as a specification check for the validity of the research design. It also may help to explain the course of the U.S. recession. In the aggregate, employment declined by 30 percent more at small firms than large firms in the year following the Lehman bankruptcy, such that two-thirds of the aggregate decline occurred at firms with fewer than one thousand employees.

The employment shortfall at firms that had borrowed from less healthy lenders can inform an estimate of the aggregate effects of the financial frictions under certain conditions. In general equilibrium, some demand shifts from the more credit-constrained to the less constrained firms, inducing an increase in labor demand at the less constrained firms. In the opposite direction, the reduction in aggregate expenditure caused by the financial crisis lowers labor demand at the less constrained firms. The data do not inform on the direction of the general equilibrium effects. Instead, in an online appendix I calibrate a general equilibrium model which suggests that under plausible conditions they may be quantitatively small. In that case, the partial equilibrium aggregation exercise also gives an estimate of the aggregate effect of the financial frictions.

The paper relates to a number of strands of literature in both macroeconomics and finance. Bernanke (1983) renewed interest in the concept of a credit channel that translates shocks to lending institutions into outcomes in the real economy through the destruction of bank-specific intermediary capital. A "small versus large" literature has looked for evidence of the credit channel using the insight that due to greater asymmetric information or smaller buffer savings, smaller, less transparent borrowers should exhibit greater sensitivity to credit supply constraints (Gertler and Gilchrist 1994; Duygan-Bump, Levkov and Montoriol-Garriga 2011). In parallel, a "natural experiment" literature has studied shocks that induce variation in the cross-section of credit availability.[1]

The natural experiment papers robustly find contractions in lending at affected banks. However, firms facing a withdrawal of credit from one financing source may be able to substitute with financing from an alternative source (Becker and Ivashina 2010; Adrian, Colla and Shin 2013). Data limitations have made it difficult to show that the shocks affect real borrower economic outcomes. Two exceptions, Gan (2007) and Almeida et al. (2012), find contractions in investment at affected borrowers, but, in violation of the logic of the small versus large literature, necessarily restrict attention to firms that have regulatory filings with borrower-level information.[2] Conversely, Peek and Rosengren (2000) and Ashcraft (2005)

[1]Slovin, Sushka and Polonchek (1993) study the share price response of borrowers of Continental Illinois Bank around its failure and rescue in 1984. Peek and Rosengren (1997), Peek and Rosengren (2000), Gan (2007), and Amiti and Weinstein (2011) explore consequences of the bursting of the Japanese real estate bubble. Khwaja and Mian (2008), Chava and Purnanandam (2011), and Lin and Paravisini (2013) use variation relating to the response to the 1998 Pakistani nuclear tests, the 1998 Russian crisis, and the 2002 WorldCom bankruptcy, respectively. Papers that use variation generated by the 2007-09 crisis include Albertazzi and Marchetti (2011), Aiyar (2012), De Haas and Van Horen (2012), Almeida et al. (2012), Ivashina and Scharfstein (2010), and Santos (2011).

[2]Slovin, Sushka and Polonchek (1993) and Lin and Paravisini (2013) link lending supply shocks to borrower stock price and financial outcomes. They face the same data restrictions that exclude non-public firms or firms not in the Compustat database. Amiti and Weinstein (2011) examine the effect on exports

find that local areas affected by banking distress have reduced economic activity, but cannot trace the effects to the firm level.[3] A key innovation of the current paper is to build a dataset that contains financial information and employment outcomes at a wide range of U.S. firms. The finding of heterogeneous treatment effects then provides a bridge between the small versus large and natural experiment literatures.[4] To my knowledge this is also the first natural experiment paper to examine effects on employment, a variable of key economic and popular interest.[5]

The outline of the paper is as follows. Section 2 reviews the theoretical reasons for the formation of banking relationships, explains the key features of the syndicated loan market, and presents empirical evidence for the presence of frictions to switching lenders. Section 3 describes the data sources and the construction of the merged loan-employment dataset. Section 4 details the identification strategy, provides relevant background on the 2008-09 crisis, and describes the measures of bank health. Sections 5 and 6 report the paper's key empirical results, demonstrating the effect of lending supply on loan market outcomes and employment outcomes, respectively. Section 7 relates the firm-level results to an estimate of the total decline in employment in the sample due to financial frictions. Section 8 concludes.

2. Relationship lending

The econometric approach in this paper requires that borrowers and lenders form relationships. Otherwise, pre-crisis clients of banks that restricted lending during the crisis could costlessly switch to borrowing from less constrained banks, with no reason to expect differential outcomes at pre-crisis borrowers of different banks. These relationships may result from adverse selection in the market for borrowers that switch lenders (Sharpe 1990); a signaling equilibrium where lending to the same borrower helps to overcome the moral hazard problem inherent to lead lenders recruiting participants (Sufi 2007*b*; Holmstrom and Tirole 1997); or a decline in ex-ante (due diligence) or ex-post (costly state verification) monitoring costs for repeat borrowers (Williamson 1987; Montoriol-Garriga and Wang).[6]

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at Japanese firms, but only include firms listed on a stock exchange. A parallel corporate finance literature that studies the investment sensitivity of firm cash flows has had the same firm-size limitations (Fazzari, Hubbard and Petersen 1988; Kaplan and Zingales 1997).

[3]Benmelech, Bergman and Seru (2011) also use the Peek and Rosengren experiment but analyze the effect on MSA-level unemployment rates.

[4]In a very different institutional setting, Khwaja and Mian (2008) find a greater ability for larger borrowers to substitute financing following the Pakistani nuclear shock.

[5]In contemporaneous work, Greenstone and Mas (2012) show that the withdrawal of lending to small firms has a significant effect on county employment during the Great Recession.

[6]In the Holmstrom and Tirole (1997) model, borrowers without enough "skin in the game" can choose a technology with lower probability of success in exchange for capturing private benefit. The borrower's incentive compatibility constraint puts a limit on the fraction of output that can be claimed by lenders, yielding a maximum incentive compatible interest rate. In the costly state verification model, lenders trade off the extra income from charging a higher interest rate with the increased probability that the borrower will be unable to repay all of the principal and interest and will declare bankruptcy. Deadweight loss in bankruptcy implies a maximum interest rate the lender is willing to charge. In that case, even borrowers with positive loan demand at an interest rate above their lender's internal cost of funds may find themselves unable to borrow, and the likelihood of getting rationed out of the market rises for clients of banks with larger increases in their internal cost of funds. These models therefore have the implication that some of the

The theories just listed share a common assumption of asymmetric information. This generates testable hypotheses for the heterogeneity of treatment effects. First, the benefit to using a previous lender, or conversely the lemons cost to switching lenders, should decline with the transparency of the borrower. Second, if the per-dollar monitoring cost falls with the size of the loan, then the cost of asymmetric information falls with borrower size. Thus, the health of the relationship lenders should matter more to less transparent, smaller borrowers.

2.1. The syndicated loan market

In a typical syndicated loan deal, the lead arranger comes to preliminary agreement on the terms of the loan with the borrower, performs the due diligence, and recruits other participant lenders to provide some of the financing. Both the lead lender and the participants sign the loan contract with the borrower. The modal deal contains one lead arranger and one participant; however, about one out of three deals contain at least five participant lenders, and one-quarter of loans contain multiple lead arrangers. Besides handling the borrower relationship, the lead arranger also retains a larger share of the loan than participants, with the lead share usually fifty to one hundred percent larger than each participant's share. Traditional deposit-taking banks and investment banks act as lead arrangers, while participants also include hedge funds and pension funds.

The syndicated loan market accounts for nearly half of all commercial and industrial lending in the United States, and two-thirds of lending with a maturity greater than 365 days.[7] The market serves both publicly-traded and private firms, with about half of the firms private. The median borrower in my sample (described in further detail in section 3) had sales of about $500 million (in 2005 dollars), and had 620 employees in 2008. However, the 10th percentiles of sales and employment were $60 million and 77 employees. For comparison, 71 percent of private sector employees in the United States work at firms with at least 50 employees.

2.2. Evidence of banking relationships in the syndicated market

Whether borrowers use their previous lenders when accessing the market for a new loan provides a direct test of the presence of banking relationships.[8] Table 1 reports this likeli-

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loan market adjustment may occur on the extensive margin of access to the market at all.

[7]Beginning in November 2012, the Federal Reserve Survey of Terms of Business Lending began reporting separately on loans made under participation or syndication. By value, and averaging over the November 2012 and February 2013 releases, these loans constituted 40.8 percent of all commercial and industrial lending during the survey's reference week; 53.3 percent of lending with maturity of 31 to 365 days; 65.6 percent of lending with maturity greater than 365 days; and 56.0 percent of lending of any maturity by large domestic banks. See http://www.federalreserve.gov/releases/e2/default.htm.

[8]Previous work has documented a number of features of the market that indicate the presence of asymmetric information. Dennis and Mullineaux (2000) and Sufi (2007) show that the number of participants in the syndicate increases with the transparency of the borrower. They interpret this finding as indicating that lead arrangers retain a larger portion of loans to less transparent borrowers as a signal of the quality of their private information about the borrower. Sufi (2007) also shows that lead arrangers tend to recruit participants that have formed part of a previous syndicate for the borrower, again consistent with the existence of private information about borrowers that banks can learn over time. Bharath et al. (2007) explore the persistence of banking relationships in a regression framework.

hood. In column 1, the table reports results from the regression

$$\begin{aligned} Lead_{b,i} &= \alpha_b + \gamma_1 \left[Previous\ lead_{b,i}\right] + \gamma_2 \left[Previous\ participant_{b,i}\right] \\ &+ \gamma_3 \left[Previous\ lead_{b,i}\ X\ Public\left(Unrated\right)\right] + \gamma_4 \left[Previous\ lead_{b,i}\ X\ Rated\right] + \epsilon_{b,i}, \end{aligned} \tag{1}$$

where $Lead_{b,i} = 1$ if bank b serves as the lead bank for borrower i, and $Previous\ lead_{b,i} = 1$ if bank b served as the lead bank for i's previous loan. The estimated value of γ_1 is 0.71. In words, even after controlling for a bank's average market share (α_b), a bank that served as the prior lead lender of a private borrower (the omitted category) has a seventy-one percentage point greater likelihood of serving as the new lead lender. Both γ_3 and γ_4 are negative, indicating a lower repeat borrowing propensity among publicly-traded borrowers without and with a credit rating, respectively. The higher repeat-borrowing propensity among privately-held borrowers fits with the stickiness deriving from asymmetric information. Finally, $\gamma_2 > 0$, suggesting that previous participants also have a higher likelihood of becoming the lead lender. The small magnitude of γ_2 reflects the fact that it corresponds to the unconditional likelihood that a previous participant becomes the lead lender; the likelihood conditional on the lead lender disappearing is much larger.

Although equation (1) controls for overall lender market share, it does not account for the possibility that some banks may concentrate in lending to particular types of firms. If so, the repeat borrowing propensity could reflect bank specialization rather than true state dependence. Column 2 therefore adds a large number of fixed effects that effectively control for a lender's market share separately by borrower industry, state, year, incorporation status, riskiness, and size. The inclusion of the fixed effects adds remarkably little explanatory power to the regression, and has only minor effect on the estimated γs. Repeat borrowing appears to reflect the formation of relationships rather than bank specialization.

Finally, columns 3 and 4 repeat the exercise for participant banks rather than lead banks. The results suggest persistence among participants as well, with a previous participant having a roughly fifty percentage point greater likelihood of serving as a participant on a new loan.

3. Data

A principal innovation of this paper is to link datasets of loans and employment in order to observe employment outcomes of borrowers of different banks.

The loan market data come from the Thomson Reuters Dealscan database. Dealscan collects loan-level information on syndicated loans from SEC filings, company statements, and media reports, and attempts to process the universe of such loans.[9] The data include the identities of the borrower and lenders present at origination, the terms of the loan, and the purpose of the loan (working capital, leverage buyout, etc.). The sample I use begins

[9] Public companies must report any new bank loan to the SEC by filing a form 8-K or as an attachment to their quarterly or annual filing. Thomson Reuters publishes a quarterly set of "League Tables" using the Dealscan data, which rank lenders according to their level of activity in the syndicated market over the prior period. The public ranking of lenders gives banks an incentive to report to Dealscan loans that Dealscan might otherwise miss. For about ten percent of loans Dealscan reports a single lead lender and zero participants. In some of these cases, Dealscan does not observe which lenders serve as syndicate participants. In others, the loan is an add-on to a previous loan facility. Loans with a single lead arranger and zero participants are if anything slightly larger than other loans in the dataset.

Table 1: Banking relationship regressions

	Lender chosen as lead		Lender chosen as participant	
	(1)	(2)	(3)	(4)
Explanatory variables:				
Previous lead	0.71**	0.67**	0.022**	−0.023**
	(0.011)	(0.012)	(0.0040)	(0.0045)
Previous participant	0.029**	0.020**	0.50**	0.46**
	(0.0014)	(0.0015)	(0.011)	(0.011)
Previous lead X Public (Unrated)	−0.052**	−0.043*		
	(0.016)	(0.017)		
Previous lead X Public (Rated)	−0.058**	−0.086**		
	(0.014)	(0.016)		
Previous participant X Public (Unrated)			0.039*	0.033+
			(0.018)	(0.018)
Previous participant X Public (Rated)			0.012	−0.038*
			(0.014)	(0.015)
Lender FE	Yes	Yes	Yes	Yes
2-digit SIC X lender FE	No	Yes	No	Yes
State X lender FE	No	Yes	No	Yes
Year X lender FE	No	Yes	No	Yes
Public/private X lender FE	No	Yes	No	Yes
All in drawn quartile X lender FE	No	Yes	No	Yes
Sales quartile X lender FE	No	Yes	No	Yes
R^2	0.480	0.504	0.285	0.334
Borrower clusters	3,253	3,253	3,253	3,253
Observations	349,008	349,008	349,008	349,008

The dependent variable is an indicator for whether the lender serves in the role indicated in the table header. For each loan in which the borrower has previous accessed the syndicated market, the dataset contains one observation for each potential lender, where a potential lender is a lender active in the syndicated loan market in that year. The variables Previous lead and Previous participant equal one if the lender served as the lead or as a participant on the borrower's previous loan, respectively. The sample covers 2001 to June 2009 and excludes loans to borrowers in finance, insurance, or real estate, and for which the purpose of the loan is not working capital or general corporate purposes. Estimation is via OLS. Standard errors in parentheses and clustered by borrower. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

from all loans made to non-FIRE U.S. businesses with the primary purpose of the loan listed as "working capital" or "corporate purposes." To restrict the sample to firms likely to have an active relationship with a lender during the crisis, I keep only borrowers with either at least one loan signed in or after 2004, or with a loan open in October 2007 or later.[10]

[10]Dealscan contains 11,740 unique U.S. borrowers that either obtained a syndicated loan between 2004 and August 2008 or obtained a loan prior to 2004 that matured after October 2007. Of these, about two-thirds (7,885) report the primary purpose of the loan as "working capital" or "corporate purposes." (The next most common purpose is for a corporate takeover.) Eliminating borrowers in finance or real estate (FIRE), defined as SIC codes 6011-6799, further reduces the sample to 6,569. Finally, I remove borrowers

To obtain lender financial information, I merge the Dealscan lenders at the holding company level with data from the Federal Reserve FR Y-9C Consolidated Financial Statements for Bank Holding Companies (for lenders where the highest level parent is either a domestic financial holding company or a domestic bank holding company), and with data from Bankscope (for foreign holding companies and investment banks). I merge the lenders with data from CRSP to obtain stock price information. To keep the sample reasonably well balanced, I eliminate loans where the lead lender made only a small number of loans during the sample period.[11] This last restriction reduces the sample size by about five percent.

Employment data come from the confidential Bureau of Labor Statistics Longitudinal Database (LDB). The LDB builds from unemployment insurance records at state workforce agencies. The database follows establishments longitudinally and contains monthly employment and quarterly payroll (wages and salaries) at every private sector establishment in the United States. An establishment is a single physical place of work. Within each quarter, the LDB reports the employer identification number (EIN) of the tax-filing unit to which the establishment belongs. I refer to a group of establishments reporting under the same EIN as a firm.[12] Employment corresponds to the total number of employees on payroll in the pay period containing the 12th day of the third month of the quarter, consistent with the standard U.S. statistical agency definition.

The LDB does not share a common identifier with the Dealscan data. For about one-quarter of the sample, I use a linking table between Dealscan and Compustat as described in Sudheer and Roberts (2008), and then use the EIN reported in Compustat to link to the LDB.[13] Another twelve percent of the firms in the Dealscan sample have exact matches in the LDB along the dimensions of firm name, city, and zip code. In the remaining cases, I perform a "fuzzy merge" using geographic and industry identifiers along with a bigram string comparator score of the firm name as reported in each dataset.[14] The final merged sample contains just over 2000 firms, or roughly half of the original Dealscan dataset.[15]

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with missing industry, state, or public/private status and winsorize the top one percent by sales, leaving a sample of 4,791 borrowers.

[11] For purposes of constructing their league tables, Thomson Reuters identifies one or more lead arrangers for each loan based on the descriptive role of each lender, and I adopt their classification.

[12] This definition of a firm derives from tax purposes and reporting conventions, and does not always correspond to the economic definition of control, nor to the scope of activities controlled by the loan recipient in the Dealscan data. If, however, shared tax liability maps into shared internal capital markets, then EIN is the correct ownership level for matching firms to their borrowing history in Dealscan. Otherwise, failure to identify all EINs with common ownership would lead to measurement error in the growth rates derived from the LDB, raising the standard errors in the regressions reported in section 6. More problematic, some states allow establishments that use professional payroll firms to report the EIN of the payroll firm rather than the establishment's owner, and I have to drop these firms from the sample. I hand check any firms with a symmetric growth rate (defined below) greater in absolute value than 0.9. In many cases the extreme growth rates result from the use of multiple EINs by a single controlling economic unit. I either combine the EINs into a single new firm or drop the firm from the sample if I cannot identify all of the relevant EINs.

[13] The matching file is available on request from Michael Roberts. I conducted an assessment of each match using the information on firm sales reported in Dealscan as well as industry and geographic identifiers, resulting in a slightly different set of matches than in the Roberts file.

[14] A bigram string comparator computes the fraction of consecutive character matches between two strings. I implement the fuzzy merge using the stata ado file reclink written by Michael Blasnik. I also did a manual review of all of the matches to ensure accuracy.

[15] The unmatched firms fall into several categories. The merged sample does not include any firms with

Panel A of table 2 gives summary statistics for the full sample of borrowers, the sample limited to loans with at least one lead lender among the forty-three most active, and the merged Dealscan-LDB sample. Each borrower appears in the sample exactly once, and the summary statistics correspond to the borrower's last pre-crisis loan. Both the sample limited to loans with the most active lenders and the merged Dealscan-LDB sample look quite similar to the full sample along the observable dimensions of loan size and borrowers' sales.[16] The sample (unweighted) average of the symmetric growth rate (defined in section 6) of employment from 2008:3-2009:3 is -9 percent. Aggregate employment in the sample declined by 5.8 percent (not shown), almost exactly equal to the 5.7 percent employment decline in the entire U.S. private sector.

The industry distribution of the firms broadly reflects that of the whole private sector, with a Spearman rank correlation of 0.49 between employment shares (in NAICS 3-digit industries) in the sample and the whole population. Employment in the sample over-weights most heavily in retail trade and manufacturing, and under-weights in health care and administrative and support services.[17] Even among the smallest firms (fewer than 250 employees), the size distribution roughly resembles that of all private sector firms between 50 and 249 employees, with a Spearman correlation of 0.37.

The merged Dealscan-LDB sample contains roughly twice as many firms as would merging Dealscan with Compustat after applying my sample filters. Crucially, while the Dealscan-LDB sample has about the same number of large (1000 or more employee) firms as a Dealscan-Compustat sample, it has more than five times as many small and medium firms.[18] Still, even among firms with more than 50 employees, the Dealscan-LDB sample over-weights large firms relative to the distribution in the whole private sector, where firms with between 50 and 1000 employees vastly outnumber those with more than 1000.[19]

headquarters in eight states that did not grant access to their microdata. Some firms closed or merged prior to the financial crisis, and do not appear in the LDB with the same ownership structure as in Dealscan. A few firms that moved their headquarters across states or changed their name in the interval between their last Dealscan loan and the financial crisis may also be missing. As discussed in footnote 12, the merged sample also omits firms with establishments that use a professional employer organization to handle their payroll. Finally, firms that operate under multiple names may have generated too low a bigram string score to qualify as a match. The summary statistics presented in the next paragraph suggest that on balance the match attrition caused by these factors had limited effect on the composition of the sample.

[16]The difference between the samples described as "All lenders" and "Top 43 lenders" also reflects the removal of less than five percent of firms that are excluded from the probit regressions reported in section 5. These firms are in industries in which no firm in the sample obtained a loan or positive modification during the crisis.

[17]The under-weighting of administrative and support services (NAICS 561) partly reflects the sample construction. The category includes professional employer organizations (NAICS 561330) and in many states the establishments for which they report payroll, the latter which I drop from the sample for the reasons described in footnote 12.

[18]See footnote 47 for a description of the Dealscan-Compustat sample.

[19]The sample contains roughly twice as many firms in the 50-999 as in the 1000+ size class. In 2008 in the whole private sector, that ratio was roughly 24:1.

Table 2: Sample summary statistics

	N	Mean	SD	p10	p50	p90
	Panel A: Firm variables					
Loan size (millions of 2005 dollars)						
All lenders	4,791	287	530	23	119	693
Top 43 lenders	4,391	302	542	26	129	720
Merged Dealscan-LDB	2,040	305	544	27	131	703
Sales at close (millions of 2005 dollars)						
All lenders	3,954	1,836	4,059	53	433	4,661
Top 43 lenders	3,623	1,928	4,149	60	478	4,869
Merged Dealscan-LDB	1,721	2,024	4,310	68	551	4,813
Employment growth rate, 2008:3-2009:3	2,040	-0.09	0.23	-0.29	-0.06	0.08
2008 employment level	2,040	2,985	9,993	77	620	6,128
	Panel B: Bank variables					
%Δ number of loans	43	-52.4	29.3	-87.4	-58.8	-7.5
Lehman cosyndication exposure (%)	42	1.15	1.20	0.34	0.72	1.91
ABX exposure	40	1.16	0.46	0.71	1.07	1.77
2007-08 trading revenue/assets (%)	42	-0.08	0.62	-0.72	0.01	0.39
2007-08 real estate net charge-offs/assets (%)	21	0.24	0.23	0.04	0.18	0.49
2007 deposits/assets (%)	43	42.2	25.4	3.0	47.4	68.2

Notes: The sample includes non-FIRE U.S. borrowers that obtained a loan for working capital or corporate purposes and with valid state, industry, and public/private status identifiers, excluding the top 1% by sales. Statistics for loan size and borrower sales correspond to information as of the last loan obtained by each borrower prior to September 2008. Rows indicated by Top 43 lenders include only borrowers whose last pre-crisis loan included one of the most active 43 lenders as a lead arranger. Rows indicated by Merged Dealscan-LDB and the employment statistics include only borrowers matched to the LDB. The variable %Δ loans equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007.

4. Identification

4.1. Theory

To determine the effect of credit availability on employment, one needs to isolate a measure of loan supply.

Formally, write the growth rate $g^y_{i,s}$ of employment y at firm i that had a pre-crisis relationship with lending syndicate s as a function of a vector of (omitting time subscripts) loan characteristics $L_{i,s}$; observable characteristics of the firm X_i; unobservable characteristics U_i; and an unobserved idiosyncratic component uncorrelated with X_i or U_i:

$$g^y_{i,s} = f\left(L_{i,s}, X_i, U_i, \epsilon_i\right). \qquad (2)$$

For the moment, suppose $L_{i,s}$ consists only of an indicator variable for whether the firm receives a loan, and that this depends on firm characteristics, the internal cost of funds at the pre-crisis lending syndicate R_s, and an idiosyncratic disturbance η_i uncorrelated with ϵ_i and U_i:

$$L_{i,s} = h\left(R_s, X_i, U_i, \eta_i\right). \tag{3}$$

Under the assumptions (i) $U_i \perp\!\!\!\perp R_s$ (where $\perp\!\!\!\perp$ denotes statistical independence) and (ii) separability of $f(\cdot)$ between its first two and second two arguments, equations (2) and (3) could be estimated using the generalized method of moments, with the moment condition $E\left[\left\{g^y_{i,s} - f\left(L_{i,s}, X_i, 0, 0\right)\right\} R_s\right] = 0$. In economic terms, assumption (i) states that the health of banks must be uncorrelated with the unobserved characteristics of their borrowers that affect either loan market or employment outcomes. I will sometimes refer to this assumption below as "as good as random" matching of banks and borrowers conditional on observables. With these assumptions, the system (2)-(3) would identify the causal effect of the extensive margin loan market outcome on employment.

Three complications prevent direct adoption of the setup described above. First, R_s is not directly observed. Instead, one needs an observable measure of loan supply M_s such that $Corr\left(M_s, R_s\right) \neq 0$ and $U_i \perp\!\!\!\perp M_s$. Second, in practice modifications of existing loans may have as important an effect on the availability of credit as the signing of a new loan, and as described below the data appear to systematically under-report that outcome. Third, employment may depend not only on success in obtaining a loan, but also on the interest spread, size, length, covenants, and other characteristics of the loan obtained, as well as on expectations of future credit availability if firms face costs to adjusting their labor input. In other words, $h(\cdot)$ is a vector-valued function, and the system (2)-(3) is under-identified for determining the effect of any particular component of $L_{i,s}$ on employment. These considerations do not however invalidate study of the reduced-form impact of lender health on employment. Substituting the arguments of (3) into (2), and replacing R_s with the observed measure M_s:

$$g^y_{i,s} = g\left(M_s, X_i, U_i, \epsilon_i, \eta_i\right). \tag{4}$$

Consistent estimation of $\frac{\partial g}{\partial M_s}$ follows from the orthogonality condition $U_i \perp\!\!\!\perp M_s$.

4.2. Origins of the 2008-09 crisis

The 2008-09 financial crisis began outside of the corporate loan sector. This makes the period particularly amenable to a study of the effects of loan supply precisely because it enhances the plausibility of bank health shocks orthogonal to the corporate loan portfolio.

The first signs of distress in financial markets came in June 2007, with the rescue by the investment bank Bear Stearns of a subsidiary hedge fund that had invested heavily in subprime mortgages. A month and a half later, the French bank BNP Paribas announced the freezing of three investment funds based on an inability to value the funds' subprime assets. The announcement sparked a rise in the interest rate at which banks lend to each other in the interbank market (see Figure 1).[20] Concerns mounted as a wave of bank writedowns on

[20] The Treasury-Eurodollar ("TED") spread became a widely watched indicator of financial distress during the crisis. Gertler and Kiyotaki (2011) provide theoretical justification for why stress in the interbank market matters in an economy with lending relationships of the type described in section 2.

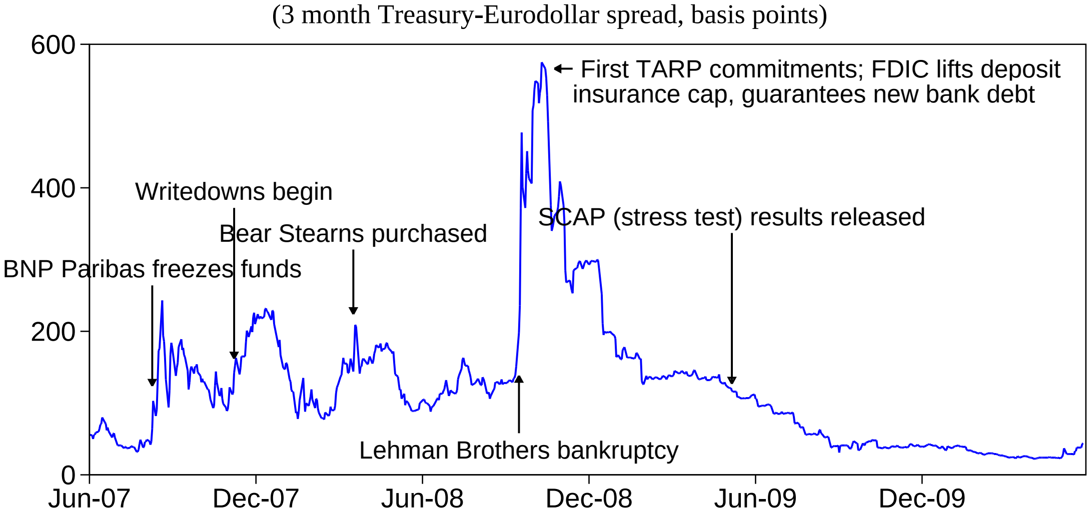
their subprime portfolios ensued. The panic reached a brief crescendo in March 2008, when the withdrawal of short-term financing to Bear Stearns forced its sale to J.P.Morgan.

Financial conditions stabilized somewhat over the summer, but then deteriorated sharply in September 2008. On September 10 Lehman Brothers reported a $3.9 billion loss for the third quarter of its fiscal year.[21] Five days later, unable to find a buyer and unable to obtain short-term financing, Lehman Brothers filed for bankruptcy. The cost of interbank lending spiked immediately. A cascade of market and policy events followed, including an $85 billion loan from the New York Federal Reserve Bank to the insurer AIG; the announcement by the money market fund Reserve Management Corporation that its net asset value had fallen below par, prompting widespread withdrawals from other money market funds; the forced sales of the investment bank Merrill Lynch and the commercial bank Wachovia; and direct capital injections by the federal government into major financial institutions through the TARP, to name a few. The stress in the interbank lending market began to ameliorate during the fall and winter of 2008, but remained elevated until the summer of 2009.

Figure 1:

Stress in the interbank lending market

(3 month Treasury-Eurodollar spread, basis points)



Source: Federal Reserve Board of Governors (H.15 Release).

The major institutional failures during the crisis - the Bear Stearns and BNP Paribas funds, Bear Stearns itself, Lehman Brothers, and AIG - all resulted from exposure to real estate and mortgage securities and funding structure. The economics literature has studied the cross-section of bank health outcomes more systematically. I am not aware of any

[21]The problems at Lehman also stemmed from outside its corporate loan portfolio. The weakness on the asset side of Lehman's balance sheet traced in part to an active decision by the bank's management at the beginning of 2006 to expand its own account investment in commercial real estate assets, compounded by a decision one year later to adopt a "counter-cyclical growth strategy" of increasing market share while other banks sought to reduce their real estate exposure (Valukas 2010, p. 59-80). On the liability side, Lehman relied heavily on short-term wholesale financing, leaving the firm vulnerable to a "repo run" (Gorton and Metrick 2012).

paper that has implicated the performance of the corporate loan portfolio. Instead, the literature has highlighted exposure to specific failing institutions (Ivashina and Scharfstein 2010); exposure to the real estate market and toxic assets (Santos 2011; Erel, Nadauld and Stulz 2011); and liability structure (Fahlenbrach, Prilmeier and Stulz 2012; Ivashina and Scharfstein 2010; Gorton and Metrick 2012).

As banks absorbed asset writedowns and reduced funding availability, the internal cost of funds would rise. In standard models, the bank's first order condition for new lending equates the (properly discounted) expected return on the loan with the shadow value of the marginal dollar – the internal cost of funds. This relationship between new lending and the internal cost of funds provides the link between the financial market distress and the reduction in lending. Indeed, Cornett et al. (2011) conduct a comprehensive analysis of bank outcomes using regulatory data on a large set of commercial banks from the FDIC Call reports, and find that variation in bank balance sheets and funding sources well-explain the cross-sectional distribution of loan origination during the crisis.

4.3. Measuring loan supply

How then to measure the health of different banks? A broad measure uses the quantity of lending at each bank to proxy for the shadow price. Specifically, to measure credit availability to borrower i during the crisis, I use lending during the crisis by i's pre-crisis syndicate to all other borrowers.[22] Since lenders retain a larger share of loans in which they have a lead role, I weight each loan by the share retained by the lender.[23] If a bank's total lending reflects its internal cost of funds, then this measure will satisfy the condition $Corr\left(M_s, R_s\right) \neq 0$, making it relevant for loan outcomes of borrower i. It will satisfy the exclusion restriction $Corr\left(M_s, U_i\right) = 0$ if the unobserved characteristics of pre-crisis borrowers of syndicate s that influence loan outcomes are uncorrelated.

Formally, let $L_{b,j,t}$ equal one if bank b makes a loan to borrower j in period t, and $\alpha_{b,j,t}$

[22] For mergers that occur prior to the onset of the financial crisis, I treat borrowers of the acquired company as if they had borrowed from the acquirer. This amounts to assuming that the loan desks of the acquiring and target banks had become fully integrated. For the mergers that occurred during the crisis, I keep the acquiring and target lenders separate. However, in measuring the crisis lending of each firm, I recode as borrowing from the target if a pre-crisis client of the target obtains a crisis loan from the acquirer. For example, Wachovia and Wells Fargo are separate lenders, but a firm that borrowed from Wachovia before the crisis and from Wells Fargo during the crisis gets recoded as having borrowed from Wachovia during the crisis. This treatment allows the data to determine the benefits to borrowers of a "shotgun marriage."

[23] In most cases Dealscan does not report the actual loan shares, so I instead use as weights the average share retained by lead lenders and participants in deals with the same syndicate structure. For example, in deals with one lead arranger and one participant on average the lead arranger retains 60 percent of the loan and the participant the rest, so I use 0.6 and 0.4 as weights in all deals with one lead arranger and one participant. The absence of actual shares also explains the use of the number of loans rather than the dollar value, as the fat right tail in the distribution of loan size means that the dollar value may compound measurement error stemming from the need to impute loan shares. At the bank level, the correlation coefficient between the two measures equals 0.87 (0.91 when weighted by the number of pre-crisis borrowers). All of the results in the paper would look similar using the dollar value of lending instead. However, the dollar value correlates less strongly with the other health variables described below, consistent with the dollar value containing greater measurement error.

bank b's imputed share of the total commitment. First define:

$$\Delta L_{-i,b} \;=\; \frac{\sum_{j\neq i}\alpha_{b,j,crisis}L_{b,j,crisis}}{0.5\sum_{j\neq i}\alpha_{b,j,normal}L_{b,j,normal}}, \tag{5}$$

where *crisis* equals the nine month period from October 2008 to June 2009, and *normal* equals the eighteen month period containing October 2005 to June 2006 and October 2006 to June 2007. In words, $\Delta L_{-i,b}$ measures the quantity of loans made by bank b to all borrowers other than firm i relative to before the crisis. $\Delta L_{-i,b}$ uses all loan packages in which b participates as either a lead lender or participant, but gives greater weight to packages where it served as a lead lender.

The measure of loan supply to each borrower uses a weighted average over all members of the last pre-crisis loan syndicate. Let a tilde denote this measure:

$$\Delta\tilde{L}_{i,s} \;=\; \sum_{b\in s}\alpha_{b,i,last}\Delta L_{-i,b}. \tag{6}$$

This measure gives a simple, transparent, and consistent way of classifying banks. It circumvents the difficulty of determining the correct level of ownership for ascribing bank health indicators, since it applies directly to the relevant lending entity. And, it should have a tight relationship with the unobserved internal cost of funds R_s since it relates directly to the loan portfolio. However, it relies on the relatively strong identification condition that the *cross-sectional* variation in bank lending reflects only supply factors or observed characteristics of the borrowers – in other words, that unobserved characteristics of borrowers that affect loan demand are not correlated at the lender level. The origins of the 2008-09 described above, and the fact that previous literature has explained the cross-sectional variation in bank health from factors outside of the corporate loan portfolio, make this identification condition at least plausible. Empirical support comes from the balancing of the sample on observed borrower characteristics reported in section 4.4, and an exercise exploring unobserved characteristics reported in section 4.5. Nonetheless, in what follows I also instrument for this measure using indicators of lender health that partially relax this identification condition. With these measures, the identification assumption becomes that less healthy banks as measured by the particular instrument did not also lend to corporate borrowers drawn from a different distribution of borrower health.[24]

The first proposed instrument follows Ivashina and Scharfstein (2010) in constructing exposure to Lehman Brothers through the fraction of a bank's syndication portfolio where Lehman Brothers had a lead role (*Lehman exposure*). Ivashina and Scharfstein show that this measure correlates negatively with new lending. They argue that firms that had credit lines where Lehman had a lead role drew down their lines by more as a precautionary measure

[24] I use these other indicators as instruments for $\Delta\tilde{L}_{i,b}$, rather than inserting them directly into equation (4), to facilitate comparison of magnitudes. In most cases the instrumental variables estimates will exceed the OLS. In general, loan demand can reflect either a healthy firm wanting to expand or an unhealthy firm needing to cushion a fall in revenue. This suggests ambiguity in the direction of any possible bias with OLS. Moreover, if the loan share imputation procedure introduces measurement error into $\Delta\tilde{L}_{i,b}$, then the instrumental variables estimates may correct the attenuation bias in the OLS results. This would also cause the instrumental variables estimates to exceed the OLS.

following the disappearance of their main lender, and this led to a draining of liquidity from the other syndicate members.

The next measure captures banks' exposure to toxic mortgage-backed securities (*ABX exposure*). For the foreign-owned or investment banks in the sample that do not file standardized FR Y-9C reports to the Federal Reserve, it is essentially impossible to obtain the exposure directly from the balance sheet on a consistent basis. Instead, I infer banks' exposure from the correlation of their daily stock return with the return on the ABX AAA 2006-H1 index. This index follows the price of residential mortgage-backed securities issued during the second half of 2005 and with a AAA rating at issuance. The loading of a bank's stock return on the ABX index thus gives a measure of the bank's exposure to the underlying components or similar securities. The AAA index includes securities that banks would have viewed as completely safe upon acquisition. Indeed, the index remained roughly at par until the fall of 2007, but then fell by ten percent in October and November of that year. By 2009 the index had fallen another third; however, I compute the loadings only over the period October 2007 to December 2007 to avoid reverse causality if movements in the ABX sometimes reflected fire-sale of securities by distressed banks around the period of the Bear Stearns and Lehman Brothers collapses.[25]

Finally, I measure lender health using a number of bank balance sheet and income statement variables not directly affected by the corporate loan portfolio (*Bank statement items*). These include trading account losses (where much of the subprime writedowns occurred), real estate charge-offs, and the ratio of deposits to liabilities (a proxy for funding stability).

For each measure, I construct the weighted average over the members of the borrower's last pre-crisis syndicate, defined by the last loan obtained with a start date before September 2008. The weights are the (imputed) loan commitment shares. Panel B of table 2 reports summary statistics for the bank change in lending and each of the proposed instruments. The full sample contains 43 banks; the Lehman exposure measure excludes Lehman Brothers; the MBS exposure excludes three banks without publicly-traded equity; the trading revenue excludes Bear Stearns because it covers the period through the end of 2008; and the real estate charge-offs omit banks not regulated by the Federal Reserve or FDIC.

Table 3 presents correlations of each of the three proposed instruments, at the bank level, with the change in lending. The table reports regressions weighted by the number of pre-crisis borrowers of each bank, and the right hand side variables have been normalized to have unit variance. Column 1 replicates the finding in Ivashina and Scharfstein (2010) that banks that had participated in a higher fraction of syndicates where Lehman had a lead lending role reduced new lending by more. Column 2 indicates a strong relationship with the loading of the bank's stock return on the ABX AAA index covering the latter half of 2005. Finally, column 3 indicates that the bank statement items also predict the change in lending.[26] Importantly, the three proposed instruments do not correlate strongly with each

[25]The results in the paper are robust to using a longer window, to using the partial loading after conditioning on the three Fama-French factors, to using a later vintage of the ABX, or to using the loading on the index of the lowest rated BBB- securities. I am grateful to Stijn Van Nieuwerburgh for the suggestion of using the correlation with the ABX to measure exposure to toxic assets.

[26]In results not shown, I also find an absence of evidence of reverse causality between corporate loan portfolios and the overall health of the bank. I can split bank net income into net charge-offs on commercial and industrial (C&I) loans and other net income only for the subset of banks in the sample regulated

other, with a (weighted) correlation coefficient of 0.29 between the Lehman exposure and the ABX exposure, 0.53 between the Lehman exposure and the fitted values of the bank statement items, and 0.34 between the ABX exposure and the bank statement items.[27]

Table 3: Determinants of bank lending

	Change in lending during the crisis		
	(1)	(2)	(3)
Explanatory variables:			
Lehman cosyndication exposure	-0.14^{**}		
	(0.049)		
ABX exposure		-0.11^{*}	
		(0.041)	
2007-08 trading revenue/assets			0.046
			(0.040)
Real estate charge-offs flag			0.012
			(0.050)
2007-08 real estate net charge-offs/assets			-0.092^{+}
			(0.051)
2007 Bank Deposits/Assets			0.19^{**}
			(0.059)
Joint test p-value	0.008	0.013	0.002
R^2	0.16	0.15	0.35
Observations	42	40	42

Notes: The dependent variable is the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, with each loan scaled by the importance of the lender in the loan syndicate as described in section 4.3 of the text. Observations weighted by number of pre-crisis borrowers. The explanatory variables have been normalized to have unit variance. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

4.4. Observed characteristics of borrowers

The identification assumption requires orthogonality between bank health and *unobserved* firm characteristics that affect loan demand or employment. The empirical exercises below

by the Federal Reserve or FDIC. A regression of the change in lending on only C&I charge-offs yields an insignificant coefficient and of the "wrong" sign. Similarly, a bivariate regression of the bank's stock return on C&I charge-offs yields an insignificant coefficient and an R^2 of 0.05, while adding net income excluding C&I charge-offs to the regression produces a highly significant coefficient (t-statistic of 5) and raises the R^2 to 0.61. Finally, recognizing that stock markets are forward-looking, a regression of the 2007-08 stock return on C&I charge-offs in 2009 also yields a coefficient insignificantly different from zero.

[27] A previous version of this paper also considered the change in lending by each bank to borrowers accessing the syndicated market for the first time. The strength of the correlation of this measure with the overall change in lending produced results extremely close to the OLS results reported below. This version does not report these results to conserve space, but they are available upon request.

therefore control for a rich set of observed nonfinancial borrower characteristics. In particular: industry fixed effects remove the possibility of spurious results due to banks that specialize in particular industries doing poorly on the measures described in section (4.3); state fixed effects and the total employment change in a borrower's county control for spatial clustering of banks and borrowers; loan-year fixed effects remove any confound if the timing of firms' borrowing is endogenous as in Mian and Santos (2011); the interest spread over the base rate (usually LIBOR) charged on the last pre-crisis loan proxies for ex-ante borrower riskiness; borrower's sales, fixed effects separating borrowers into non-public and without access to the bond market, public and without access to the bond market, and borrowers with access to the bond market,[28] and indicator variables for whether the borrower had used multiple lead lenders pre-crisis and whether the last pre-crisis loan had multiple lead arrangers all proxy for both transparency and access to outside funds; and indicator variables for whether the last pre-crisis loan was a credit line or term loan and whether the borrower had any loan reported in Dealscan coming due during the crisis proxy for loan demand.

Each of these variables may influence borrower outcomes regardless of pre-crisis relationship, making one motivation for including them a reduction in the residual variance of the dependent variable. As well, stability of the coefficient of interest with and without control variables helps to address the concern that borrowers of different pre-crisis banks also differed along unobserved dimensions. To that end, the main tables below also report bivariate specifications for comparison.

Table 4 displays summary statistics for the control variables after splitting borrowers into four quantiles of the change in lending to other borrowers measure. The first two rows show the average employment change in a borrower's industry and county, respectively, using the national Quarterly Census of Employment and Wages. Clients of lenders in the top (healthiest) quartile belonged to industries that experienced an average employment decline of 8.9 percent in the year following the Lehman bankruptcy, compared to an average decline of 8.6 percent in the industries of clients of lenders in the bottom quartile. Clients of lenders in the top and bottom quartile operated in counties that had essentially identical average employment changes.[29]

The fact that lenders identified as healthier did not lend before the crisis to firms in counties or industries that had systematically better crisis employment outcomes indicates that bank specialization by industry or geography cannot explain the borrower outcomes even in a bivariate OLS context. Importantly, the absence of differences between firms with relationships with healthy and unhealthy lenders masks significant variation in employment outcomes across counties and industries. Instead, it reflects strong balancing of the sample

[28] I classify firms as having access to the bond market if they have a credit rating from either Moody's or Standard and Poor's as reported in Dealscan, or if they have ever issued public debt. For the latter, I merge Dealscan with the Mergent FISD bond database, using a procedure similar to that described in section 3 to merge Dealscan with the Bureau of Labor Statistics Longitudinal Database.

[29] The county-level measure uses the change in employment in each county in which a firm operates establishments, averaged to the firm level using establishment employment shares as weights. The changes reported in table 4 weight the percent employment change in each industry (county) by the number of firms in the quantile operating in that industry (county). The unconditional average employment decline by industry using the industry weights is 8.5 percent, while the decline by county using county weights is 5.6 percent. The difference reflects the distribution of firms across industries and geography, and crucially the fact that the weights do not account for the total employment in each industry or county.

along these observable dimensions. A similar pattern emerges for the remaining covariates described above, with the differences across lender quantiles small in magnitude.

Table 4: Balancing of covariates in the sample

	Quantile of lender health				Memo: standard
	1	2	3	4	deviation
Mean employment change in:					
Borrower's industry, 2008:3-2009:3	-0.086	-0.081	-0.085	-0.089	0.083
Borrower's county, 2008:3-2009:3	-0.056	-0.056	-0.056	-0.056	0.009
Share with bond market access	0.455	0.540	0.458	0.236	0.494
Share private, no bond market access	0.418	0.331	0.363	0.525	0.492
Share public, no bond market access	0.127	0.129	0.179	0.239	0.374
Mean all in drawn spread	266	155	156	199	133
Median sales at close ($2005 billions)	0.366	0.837	0.701	0.285	4.146
Mean year of last pre-crisis loan	2005.83	2005.98	2006.03	2006.05	1.50
Share with loan due during crisis	0.193	0.188	0.183	0.205	0.394

Notes: The table splits the sample into four quantiles based on the change in the annualized number of loans made by the borrower's last pre-crisis syndicate between the periods October 2005 to June 2007 and October 2008 to June 2009. Employment change by borrower industry computed at the 4-digit SIC level using 6-digit NAICS employment levels from the Quarterly Census of Wages and Employment and an SIC-NAICS concordance table available from the Bureau of Labor Statistics. Employment change by borrower county computed by averaging the employment change in all counties in which a firm operates establishments using establishment employment shares as weights. The last column reports the standard deviation of the variable summarized in each row.

4.5. Unobserved characteristics of borrowers

An exercise using the sample of borrowers that obtained a loan during the crisis can help to address whether unobserved characteristics of borrowers correlate at the lender level. The first step of this exercise asks whether lenders that reduced overall lending by more also reduced lending by more to the *same borrower* as compared to other lenders. Following Khwaja and Mian (2008), column 1 of table 5 implements this test by regressing the change in lending in a borrower-lender pair on the loan supply measure and a full set of borrower fixed effects. The fixed effects then absorb any borrower characteristics that might influence loan outcomes. Inclusion of borrower fixed effects necessitates that every borrower have multiple lenders. The sample therefore includes one observation for each lead lender and participant in the pre-crisis syndicate. The dependent variable equals the log change in the dollar value of lending by that syndicate member from the pre-crisis loan to the crisis:

$$[\ln(1 + \alpha_{b,i,crisis} V_{i,crisis}) - \ln(\alpha_{b,i,last} V_{i,last})] = \delta\,[\Delta L_{-i,b}] + FE_i + \nu_{i,b},$$

where $\alpha_{b,i,t}$ denotes bank b 's imputed share of the loan to borrower i in period t ; $V_{i,t}$ denotes the dollar value of the loan; $t \in \{crisis, last\, pre - crisis\, loan\}$; $\Delta L_{-i,b}$ denotes the change in lending by bank b to all other borrowers; and $\nu_{i,b}$ an idiosyncratic error term.[30] Column 1 of table 5 reports the results. The positive coefficient indicates that a firm that had both healthy and unhealthy members in its pre-crisis syndicate borrowed more during the crisis from the healthier member.

Table 5: Testing for unobserved characteristics of borrowers

	Δ log(lending in borrower-lender pair)	
	(1)	(2)
Explanatory variables:		
$\%\Delta$ loans to other borrowers ($\Delta\tilde{L}_i$)	1.05**	1.07**
	(0.33)	(0.32)
1-digit SIC, loan year FE	No	Yes
Bond market access/Public/Private FE	No	Yes
Additional Dealscan controls	No	Yes
Borrower FE	Yes	No
R^2	0.423	0.088
Borrowers	432	432
Banks	43	43
Observations	2,005	2,005

Notes: The sample contains only borrowers that signed a new loan between October 2008 and June 2009. The sample contains one observation per member of the borrower's last pre-crisis syndicate. The dependent variable is the log change in the dollar amount of lending from that lender to the borrower. The variable $\Delta\tilde{L}_i$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. Estimation is via OLS. Standard errors in parentheses and clustered by the pre-crisis lender (column 1) or twoway-clustered on pre-crisis lender and borrower (column 2). +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

The difference in the point estimates between regressions including and excluding the fixed effects captures the amount of bias induced by not-as-good-as-random matching of borrowers and lenders. To facilitate this comparison, the second column of table 5 reports the corresponding specification without the borrower fixed effects but with the full set of controls discussed in section 4.4. Intuitively, the difference between the right hand side variables in the specifications reported in columns 1 and 2 captures exactly the unobserved characteristics of the borrowers. If the unobserved characteristics were correlated with the lending measure, then the point estimate would change to reflect the omitted variables. Instead, the point estimate is essentially identical across columns. This provides direct validation of as-good-as-random matching within the set of firms that obtained a loan during the crisis.

[30]The addition of 1 in the first log term accounts for the fact that some pre-crisis syndicate members do not appear in the crisis syndicate, in which case $\alpha_{b,i,crisis} = 0$. Other suitable growth rate measures that can handle exit, such as a symmetric growth rate or the conventional percent change, yield similar results.

5. Loan market outcomes

This section presents results for the effect of the banking relationship on loan outcomes. Heuristically, these results correspond to the first-stage of an instrumental variables design, where the second stage outcome is employment. For the measure of credit availability to be relevant to firm employment, it should also predict outcomes in the loan market during the financial crisis.

5.1. Timing

Before discussing the loan market results, I comment briefly on the timing. Figure 2 shows the dollar value of new lending by the forty-three most active banks in the syndicated market. The market expanded rapidly during the mid-2000s, but began to contract during the fourth quarter of 2007.[31] New lending troughed in the fourth quarter of 2008, coincident with the peak of stress in the interbank market. Lending started its rebound somewhat after the interbank market stabilized, with a slow recovery in volume beginning at the end of 2009.[32]

The narrative timeline, the timepath of interbank lending spreads in figure 1, and the timing of the trough in new bank lending all point to a division in the crisis between the periods before and after the Lehman bankruptcy. The acuity of both financial distress and employment losses following the Lehman bankruptcy suggest that financial frictions may have had especially great influence on employment outcomes during that period. Indeed, of the 8.8 million private sector jobs lost in the United States from the peak in January 2008 to the local trough in February 2010, fully half came in just the six month period ending in March 2009, and three-quarters in the year after the Lehman bankruptcy. Finally, the Lehman cosyndication exposure measure of bank health applies only to the period after the Lehman bankruptcy. The loan market results therefore focus on the period immediately following the Lehman bankruptcy, defined as October 2008 to June 2009.[33] The choice of June 2009 as a terminal month reflects both the timing of the U.S. recession, which ended that month, and the timing of the return to normalcy in the interbank lending market.

[31] The initial fall in lending does not seem to reflect expectations of a large decline in real activity; for example, in June 2008 members of the Federal Reserve Open Market Committee forecast that the unemployment rate would remain roughly unchanged (at around 5.5 percent) over the coming year.

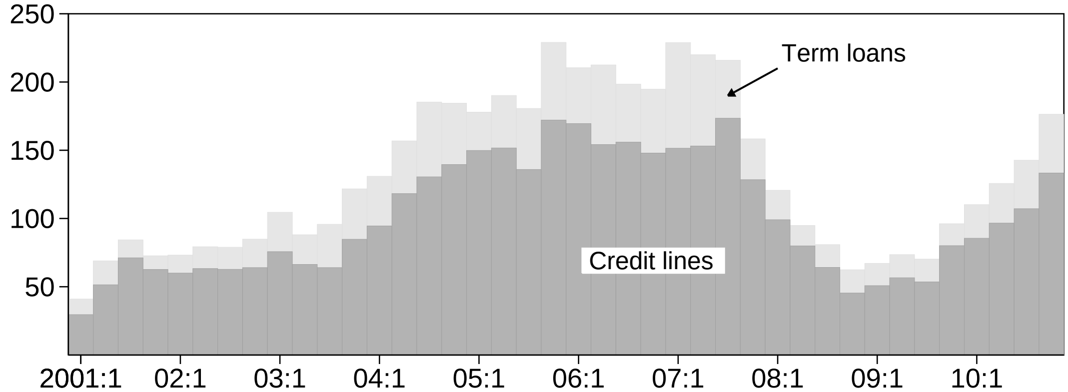
[32] Much discussion has centered on the build-up of liquid assets at nonfinancial corporations following the crisis, with the implication that borrowing constraints could not matter in an environment where corporations had large amounts of cash or cash equivalents at their disposal. Leaving aside issues of aggregation - the measure of cash typically cited comes from the Flow of Funds and represents the entire nonfinancial sector, and may therefore have less relevance for the small and medium firms identified as most affected in the empirical results below - the timing of the cash build-up merits mention. In nominal terms, liquid assets (Flow of Funds code FL104001005Q) actually declined during the recession, and only in 2009:3 surpassed their pre-recession level. Liquid assets did rise substantially beginning in the latter part of 2009, but this build-up partly reflects a return to the pre-crisis trend, as well as firms' increasing their optimal buffer stock of savings in response to the credit crunch. The time-series of liquid asset holdings does not necessarily indicate that borrowing constraints did not bind in equilibrium.

[33] I set the post-Lehman period to begin in October 2008 because the loan date reported in Dealscan corresponds to the start of the loan facility, which may lag the signing of the loan agreement and will certainly lag the beginning of the loan processing.

Figure 2:

Aggregate new lending from top 43 lenders

(Billions of 2005 dollars, seasonally-adjusted at quarterly rate)



Source: Dealscan.

Notes: The figure shows the face value of new loans to non-FIRE borrowers for working capital or general corporate purposes in which one of the forty-three most active lenders had a lead role. Values seasonally-adjusted by author using Census-X12.

5.2. Loan market extensive margin results

The first outcome concerns whether the firm signed a new loan during the crisis period, or received a favorable modification of an existing loan. Most loans get renegotiated over the course of the contract (Roberts 2012). I define a favorable modification as either increasing the size of the loan, or extending the maturity or changing non-pricing terms (typically relaxing financial covenants) without reducing the loan size.[34] The loan modification margin may affect many more firms than those that actually wanted to sign a new loan during the crisis.[35] Unfortunately, no comprehensive dataset exists of loan modifications. The Dealscan loan modification module contains information on four times as many modifications of loans of publicly-traded firms as privately held firms during the crisis. This difference may reflect a reporting bias, since the publicly-traded firms must report loan modifications in their regulatory filings while private firms have no reporting obligation. If so, the findings reported below may understate the effect of lender health on this outcome.

Columns 1 and 2 of table 6 estimate the specification:

$$P\left(Borrow_{i,s}=1\right) \;=\; G\left(\pi_0+\pi_1\Delta\tilde{L}_{i,s}+\gamma X_i+\eta_{i,s}\right), \tag{7}$$

[34] This definition implicitly assumes that a modification that doesn't change size or pricing constitutes a favorable change of terms to the borrower, such as the change in the definition of a covenant to avoid technical default. The definition classifies 61 percent of firms that receive a modification as receiving a favorable modification. Of the remainder, 28 percent received a reduction in their available commitment, and an additional 5 percent had an increase in pricing.

[35] In many cases, a renegotiation stems from the violation or imminent violation of a loan covenant, which gives the lender the option of waiving the violation or reducing the loan amount and increasing the interest rate. Indeed, Sufi (2007a) estimates that eight percent of publicly traded firms are in violation of a covenant in any quarter, a number which likely understates the level during a recession.

where $Borrow_{i,s}$ is an indicator variable for whether borrower i of pre-crisis syndicate s obtained a loan or favorable modification (from any lender) during the crisis period of October 2008-June 2009; $\Delta \tilde{L}_{i,s}$ is the measure of the change in the number of loans to other borrowers of the pre-crisis syndicate as defined in section 4.3; X_i contains the additional control variables defined in section 4.4; and $\eta_{i,s}$ is an error term. The estimation uses the full Dealscan sample (i.e. including firms not matched to the LDB) and is done via probit. The table reports the marginal coefficients scaled by 100 and after normalizing $\Delta \tilde{L}_{i,s}$ to have unit variance; hence the coefficient on $\Delta \tilde{L}_{i,s}$ has the interpretation of the percentage point increase in the likelihood of a positive loan market outcome from having a relationship with a bank one standard deviation above the mean. The variance-covariance matrix of $\eta_{i,s}$ allows for arbitrary correlation among borrowers with the same lead arranger.

The coefficient on $\Delta \tilde{L}_{i,s}$ indicates a strong, positive effect of loan supply on the likelihood of obtaining a loan during the crisis. Column 1 estimates the bivariate specification, and column 2 includes the full set of controls. With the full set of controls, a one standard deviation increase in loan growth from the members of the pre-crisis syndicate implies a two percentage point increase in the likelihood of signing a new loan or receiving a favorable modification. Changing the loan supply of a borrower at the 10th percentile of the distribution to that of a borrower at the 90th percentile would increase the likelihood by about five percentage points. The predicted likelihood at the mean of all other covariates for a borrower of a syndicate at the 10th percentile was 9.1 percent (not shown), yielding an increase of roughly fifty percent in the likelihood of obtaining a loan when going from the 10th percentile syndicate to the 90th.

The inclusion of the controls changes the point estimate little, but reduces the residual variance such that the standard error falls by a third. The coefficient on pre-crisis loan spread is negative and significant, suggesting that riskier borrowers had greater difficulty obtaining a loan during the crisis. Similarly, the positive coefficient on sales implies that smaller borrowers had a lower likelihood of a positive loan market outcome. Finally, borrowers with a loan coming due during the crisis had a higher likelihood of obtaining a loan.[36]

Each of the next columns reports an instrumental variables specification. The magnitude of the bank health variable $\Delta \tilde{L}_{i,s}$ changes little using the ABX exposure or the bank statement items.[37] The magnitude rises using the Lehman cosyndication measure. The larger magnitude (but not the statistical significance) reflects the influence of borrowers from just four lenders that each have exposure to Lehman more than 70 percent higher than any other lender, with the fitted coefficient falling by nearly half excluding these borrowers.[38] Thus these three imperfectly correlated instrument sets all generate coefficients on the endogenous variable of a similar magnitude. The last column provides more formal statistical diagnos-

[36]Splitting the dependent variable into the signing of a new loan and a positive modification indicates predictive power of bank health on both dimensions. For example, the column 2 coefficient of 2.00 divides into roughly 60 percent from the signing of a new loan, and 40 percent from modifications, with each estimate on its own significant at the 1 percent level. Besides conserving space, combining the measures also reflects the finding of Roberts (2012) that Dealscan does not always properly classify a new contract as a new loan or as a modification of an existing loan.

[37]For the columns including the ABX measure, the regressions exclude borrowers of the three lead lenders without stock price information.

[38]The lenders are Goldman Sachs, Morgan Stanley, Bear Stearns, and Royal Bank of Canada. Excluding borrowers of these lenders, the fitted coefficient falls to 2.5, with a standard error of 1.1.

Table 6: The effect of bank health on the likelihood of obtaining a loan

	Firm obtains a new loan or positive modification					
	Probit		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	2.19**	2.00**	3.65**	2.33*	2.28**	2.32**
	(0.79)	(0.53)	(1.28)	(1.12)	(0.64)	(0.63)
2-digit SIC, state, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			14.0	8.2	18.2	19.8
J-statistic p-value			.	.	.	0.206
$E[borrow]$	0.134	0.134	0.134	0.134	0.134	0.134
$E[\widehat{borrow}: \Delta\tilde{L}_{p90} - \Delta\tilde{L}_{p10}]$	0.052	0.048	0.087	0.055	0.054	0.055
Lead lender 1 clusters	43	43	43	40	43	40
Lead lender 2 clusters	43	43	43	40	43	40
Observations	4,391	4,391	4,391	4,354	4,391	4,354

Notes: The dependent variable is an indicator for whether the borrower signed a new loan or received a favorable modification to an existing loan between October 2008 and June 2009. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for reporting real estate charge-offs. The last column includes all of the instruments. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. In columns 1 and 2 estimation is via probit and the table reports marginal coefficients. In columns 3-6 $\Delta\tilde{L}_{i,s}$ is instrumented using the variable indicated in the column heading and estimation is via 2SLS. Borrower-level covariates are as of the last pre-crisis loan taken by each borrower. Variables sales and all in drawn contain imputed value of 0 if missing and regression contains an indicator variable for whether imputation occurs. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

tics, grouping all of the instruments together. The F-statistic in the last column is 19.8, above the Stock and Yogo (2005) criterion for 5 percent maximal relative bias. Moreover, the Hansen J-statistic cannot reject exogeneity of all of the instruments. The estimated coefficient in the last column is again very similar in magnitude to the OLS coefficient.

5.3. Loan market intensive margin results

Table 7 shows the effect of loan supply on the loan interest rate among borrowers that obtained a new loan during the crisis period. Following Santos (2011) and Hubbard, Kuttner and Palia (2002), the sample contains crisis loan facilities matched to the last pre-crisis facility of a similar type.[39] Unlike the existing literature, the sample includes some borrowers who switched lenders during the crisis, since I am concerned with borrower outcomes rather than bank outcomes. The control variables mirror those described above, except that the much smaller sample size requires substituting 1 digit for 2 digit SIC industry fixed effects and removing the geography fixed effects.

The results indicate that pre-crisis borrowers of healthier banks received a lower interest rate if they borrowed during the crisis. Among all matched crisis borrowers, the average interest spread increased by about 130 basis points. In the OLS estimation, borrowers with pre-crisis relationships at the 10th percentile of crisis lending had increases of 33 basis points more than borrowers at the 90th percentile, and the coefficients are precisely estimated. As with the extensive margin results, the additional controls have minor effect on the point estimate. The instrumental variables specifications suggest even larger effects on borrowing costs, with an interdecile difference of 48 basis points grouping all of the instruments together, but one cannot reject equality with the OLS estimates.

6. Employment outcomes

A number of channels may link loan market outcomes to employment. For firms that use working capital to finance payroll or other inputs into production, the relevant measure of marginal cost in the pricing decision incorporates the interest cost of the borrowing (Chari, Christiano and Eichenbaum 1995). A higher price of borrowing therefore acts like a cost-push shock, which for a firm facing a downward-sloping product demand curve implies a lower quantity of output and lower labor demand. At the extreme, firms may decide to forgo any working capital and only finance production inputs out of retained earnings, or may face credit rationing. A firm that does not borrow at all because of the health of its lender would have to cut payroll to levels consistent with its cash holdings. More generally, any firm that uses the borrowing rate to discount future profits and that does not hire its labor on the spot market each period (due to adjustment costs or search costs or some other friction) will demand less labor as soon as the borrowing rate rises. Finally, firms that do not currently need to borrow but face uncertain future shocks may optimally decrease payroll as a precautionary motive if they believe their ability to secure a loan in the future has diminished due to the health of their lender.

6.1. Employment growth rate definition

As has become standard in the literature using establishment-level employment micro-data (see e.g. Davis, Haltiwanger and Schuh 1996), define the growth of employment y at

[39] Specifically, both loans in a matched pair must either be term loans or credit lines. If the borrower obtained multiple facilities of the same type as part of the last pre-crisis loan package, the match uses the facility closest in size and maturity.

Table 7: The effect of bank health on interest rate spreads

	Change in interest rate spread					
	OLS		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	-14.6^{**}	-12.2^{**}	-23.1^{*}	-20.0	-17.2^{*}	-17.6^{**}
	(5.26)	(4.15)	(11.2)	(13.3)	(7.63)	(6.68)
1-digit SIC, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			60.5	7.8	14.3	14.5
J-statistic p-value	.	.	.	.	.	0.967
$E[\Delta Spread]$	130.6	130.6	130.6	130.7	130.6	130.7
$E[\widehat{Spread}\text{: } \Delta\tilde{L}_{p90} - \Delta\tilde{L}_{p10}]$	-39.7	-33.0	-62.8	-54.3	-46.6	-47.7
Lead lender 1 clusters	34	34	34	32	34	32
Lead lender 2 clusters	30	30	30	28	30	28
Observations	350	350	350	346	350	346

Notes: The dependent variable is the interest spread, in basis points, charged to a firm on a loan starting between October 2008 and June 2009, less the interest spread charged to the same firm on its last loan of the same type (credit line or term loan) obtained prior to September 15, 2008. The regressions exclude loan pairs with an increase of >400 basis points. See the text for further details of the sample construction. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for reporting real estate charge-offs. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

establishment e belonging to firm i between periods $t-k$ and t using the symmetric growth rate:

$$g^{y}_{e,i,t-k,t} = \frac{y_{e,i,t} - y_{e,i,t-k}}{0.5\left[y_{e,i,t} + y_{e,i,t-k}\right]}. \tag{8}$$

The growth rate definition in (8) is a second order approximation of the log difference growth rate around zero; it is bounded in the range $[-2, 2]$; and it can accommodate both entry and exit. The latter two features help particularly to limit the influence of outliers.

The analogous growth rate at the firm level simply aggregates all establishments belonging to the firm *in a given quarter*, which equivalently yields a weighted average of the establishment-level growth rates:

$$\begin{aligned} g^{y}_{i,t-k,t} &= \frac{\sum_{e\in i_{t-k}} y_{e,i,t} - \sum_{e\in i_{t-k}} y_{e,i,t-k}}{0.5\left[\sum_{e\in i_{t-k}} y_{e,i,t} + \sum_{e\in i_{t-k}} y_{e,i,t-k}\right]}, \\ &= \sum_{e\in i_{t-k}} \omega^{y}_{e,i,t-k,t} g^{y}_{e,i,t-k,t} \end{aligned} \tag{9}$$

where

$$\omega^{y}_{e,i,t-k,t} = \frac{y_{e,i,t} + y_{e,i,t-k}}{\sum_{e\in i_{t-k}} \left[y_{e,i,t} + y_{e,i,t-k}\right]}.$$

Grouping establishments according to their ownership in a given quarter, rather than computing the growth rate based on the level of employment reported across all establishments of a firm in each quarter, removes the influence of mergers or divestments on employment change. That is, this grouping convention defines employment changes as occurring only when a job appears or disappears, and not when firm employment changes due only to changes in ownership.[40] The decision to group firms by their period $t-k$ owner rather than their period t owner stems from the design of the natural experiment. Firms received a "treatment" beginning in September 2008. A potential outcome for a firm is divestment of selected establishments. Since divested establishments received the same loan supply treatment as retained establishments, I group them according to their beginning of period ownership. In practice the distinction matters little to the results reported below.

6.2. Employment specifications

The empirical specification mirrors that in (7), where as before $\Delta\tilde{L}_{i,s}$ denotes the change in the number of loans made during the crisis to other borrowers of $i's$ pre-crisis syndicate (see section 4.3):

$$g^{y}_{i,s,t-k,t} = \beta_0 + \beta_1 \Delta\tilde{L}_{i,s} + \gamma X_i + \epsilon_{i,s,t-k,t}. \tag{10}$$

The covariance matrix of $\epsilon_{i,s,t-k,t}$ again allows for arbitrary correlation across firms with the same lead lender.

As above, I present results for a bivariate specification, and with additional firm-level controls. Along with the variables described in section 4.4, the firm-level controls in X_i contain the following variables drawn from the LDB data: a lag of the dependent variable, computed over the two-year period just prior to the onset of any financial turmoil, 2005:2-2007:2; the average employment change in the counties where the firm operates, using establishment employment shares as weights; fixed effects separating the firms into three size bin classes of 1-249 employees (small), 250-999 (medium), and 1000+ (large); and fixed effects for three

[40] It also helps to mitigate against the problems caused by the possible use of multiple EINs reporting under the same ownership structure in the LDB data, since otherwise a change in reporting structure would reveal itself as large swings in employment as whole establishments changed EINs, even if no actual job creation or destruction occurred. See footnote 12 for further discussion.

age bin classes corresponding to firm birth in the 2000s (young), 1990s (mid), and pre-1990 (old).[41] Each of these variables may affect labor demand independent of the pre-crisis banking relationship. Including them should therefore reduce the residual variance, while stability of $\hat{\beta}_1$ with and without the control variables provides support for the validity of the loan supply measure. In particular, the lag of the dependent variable helps to address the concern that some banks may have lent to borrowers on different long-term employment trajectories. The county employment change absorbs local demand shocks, while recent research has highlighted the role of young firms in job creation (Haltiwanger, Jarmin and Miranda 2013).

Economic theory predicts that less transparent firms and firms without access to alternative forms of financing would exhibit greater sensitivity to banking frictions. Indeed, this theory makes an unconditional prediction – firms with access to the bond market should perform better during banking crises than firms without access. While a proper test of this prediction would have to account for the fact that bond market access is not randomly assigned, it is nonetheless useful to check the raw correlation. Table 8 shows the result in the baseline sample. Firms without access to the bond market had employment growth rates about three percentage points lower than firms with access, and this difference is highly statistically significant.[42] Moreover, adding a number of firm covariates to the regression, including size, age, industry, and county employment change, slightly increases the point estimate. These findings are suggestive of banking frictions mattering during this period, and are also consistent with the results in Adrian, Colla and Shin (2013) and Becker and Ivashina (2010) that firms with access did in fact substitute toward the bond market.

The financial frictions literature has also put special emphasis on smaller firms (Gertler and Gilchrist 1994), which may be more vulnerable to credit shocks due to lower transparency, non-convex monitoring costs, or fewer pledgable assets. These considerations motivate regressions including interactions for the three size bins:

$$\begin{aligned} g^y_{i,s,t-k,t} &= \beta_0 + \beta_{1,small}\left[\Delta\tilde{L}_{i,s} * Small\right] + \beta_{1,med}\left[\Delta\tilde{L}_{i,s} * Medium\right] + \beta_{1,large}\left[\Delta\tilde{L}_{i,s} * Large\right] \\ &+ \gamma X_i + \epsilon_{i,s,t-k,t}; \end{aligned} \tag{11}$$

as well as allowing the treatment effect to differ by whether the firm has access to public debt markets:

$$\begin{aligned} g^y_{i,s,t-k,t} &= \beta_0 + \beta_{1,bond\,access}\left[\Delta\tilde{L}_{i,s} * bond\,market\,access\right] \\ &+ \beta_{1,no\,access}\left[\Delta\tilde{L}_{i,s} * no\,access\right] + \gamma X_i + \epsilon_{i,s,t-k,t}. \end{aligned} \tag{12}$$

6.3. Main employment results

Table 9 shows results for the change in employment over the period 2008:3-2009:3. The

[41] Although the large size bin contains more than double the employment of each of the small and medium bins, the number of firms in each size bin in the merged Dealscan-LDB sample is roughly equal. I define size classes by the 2007:2 employment level to mitigate against mean reversion in employment. Firm age corresponds to the age of the oldest establishment belonging to the firm.

[42] As explained in footnote 28, I classify firms as having access to the bond market if they have a credit rating from either Moody's or Standard and Poors, or if they have ever issued public debt.

Table 8: Bond market access and employment

	Employment growth rate 2008:3-2009:3	
	(1)	(2)
Explanatory variables:		
No bond market access	-2.65^{**}	-3.15^{**}
	(0.98)	(1.10)
2-digit SIC and state FE	No	Yes
Firm size bin FE	No	Yes
Firm age bin FE	No	Yes
Lagged employment growth	No	Yes
County employment growth	No	Yes
R^2	0.003	0.172
Observations	2,040	2,040

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. Firms that do not have access to the bond market do not have a credit rating from either Moody's or Standard and Poors, and have never issued public debt. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Eicker-White standard errors in parentheses. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

one year period obviates the need to control for seasonality. Once again the table reports coefficients scaled by 100 and with $\Delta\tilde{L}_{i,s}$ normalized to have unit variance, so that the coefficients have the interpretation of the percentage point change in employment growth from moving one standard deviation in pre-crisis syndicate health.

In the OLS specification with the full set of controls, the credit supply measure has a large and statistically significant effect on employment. The magnitude falls slightly in the bivariate specification, but rises after instrumenting. In all columns the statistical significance reaches at least the five percent threshold.[43] In this subsample of Dealscan firms matched to their BLS counterparts, the first stage F-statistic with all of the instruments equals 23.1, again above the Stock and Yogo criterion for 5 percent maximal bias, and the J-statistic again cannot reject exogeneity of the instrument set. Of note, the county employment change enters with a coefficient close to one, suggesting that local demand conditions had an important effect on firm outcomes. However, this control variable by itself does not affect the lender health coefficient, which reflects the geographic balancing of firms with relationships with healthier and less healthy lenders.

The implied economic magnitude is substantial. Borrowing from the 10th rather than the 90th percentile of lenders results in an additional decline in employment of four percentage points in the OLS specification and 5.5 percentage points with all of the instruments. For comparison, the average firm-level employment decline in the sample equaled 9.2 percent, while employment in the total nonfarm private sector declined 5.7 percent over the period.

[43]Technically the loading on the ABX AAA index constitutes a generated regressor. The standard errors reported in the tables do not account for this to maintain comparability across columns. A bootstrap standard error based on a bootstrapped sample of trading days to compute pseudo-loadings and of firm observations accounting for the clustering indicates no bias in the standard errors reported in the paper.

Table 9: The effect of lender credit supply on employment

	Employment growth rate 2008:3-2009:3					
	OLS		$\Delta\tilde{L}_{i,s}$ instrumented using			
			Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)	(6)
Explanatory variables:						
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	1.17*	1.67**	2.49*	3.17*	2.13*	2.38**
	(0.58)	(0.61)	(1.00)	(1.35)	(0.88)	(0.77)
Lagged employment growth		0.0033	0.0039	0.0045	0.0036	0.0039
		(0.019)	(0.019)	(0.019)	(0.019)	(0.019)
Emp. change in firm's county		0.89*	0.85+	0.86+	0.87+	0.89+
		(0.43)	(0.46)	(0.48)	(0.45)	(0.46)
2-digit SIC, state, loan year FE	No	Yes	Yes	Yes	Yes	Yes
Firm size bin FE	No	Yes	Yes	Yes	Yes	Yes
Firm age bin FE	No	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	No	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	No	Yes	Yes	Yes	Yes	Yes
First stage F-statistic			15.5	8.5	18.5	23.1
J statistic p-value			.	.	.	0.190
$E[g_j^y]$	-9.2	-9.2	-9.2	-9.3	-9.2	-9.3
$E[\hat{g}_j^y\text{: } \Delta\tilde{L}_{p90} - \Delta\tilde{L}_{p10}]$	2.7	3.9	5.8	7.4	5.0	5.5
Lead lender 1 clusters	43	43	43	40	43	40
Lead lender 2 clusters	43	43	43	40	43	40
Observations	2,040	2,040	2,040	2,015	2,040	2,015

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. The variable Lehman cosyndication exposure equals the fraction of the bank's syndication portfolio where Lehman Brothers had a lead role in the loan deal. The variable ABX exposure equals the loading of the bank's stock return on the ABX AAA 2006-H1 index between October 2007 and December 2007. The balance sheet and income statement items include the ratio of deposits to assets at the end of 2007; the ratio of trading revenue over 2007-08 to assets; the ratio of net real estate charge-offs over 2007-08 to assets; and an indicator for report real estate charge-offs. For each firm, the bank-level measures are averaged over the members of the firm's last pre-crisis loan syndicate, with weights given according to each bank's role. In columns 1 and 2 estimation is via OLS. In columns 3-6 $\Delta\tilde{L}_{i,s}$ is instrumented using the variable indicated in the column heading. Borrower-level covariates are as of the last pre-crisis loan taken by each borrower. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Variables sales and all in drawn contain imputed value of 0 if missing and regression contains an indicator variable for whether imputation occurs. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

Table 10 presents the results allowing for heterogeneous treatment effects. The table reports only results for the $\Delta\tilde{L}_{i,s}$ measure, but reduced form regressions of the size and bond access bins interacted with the instruments yield similar results. Column 1 indicates that the credit supply measure has a large and precisely estimated effect on employment at small and medium firms. In contrast, the data cannot reject no effect of the measure on employment at the largest firms, and the point estimate is about one-quarter of the size. The finding that banking relationships matter more to smaller borrowers provides strong evidence of asymmetric information in lending markets. The results also comport with those in Duygan-Bump, Levkov and Montoriol-Garriga (2011), who find that employment during the recession fell by more in financially-dependent industries, but only at small and mid-sized firms, and Krueger (2010), who finds a relatively greater increase in layoffs at small establishments beginning around late 2008.[44]

The concentration of loan supply effects at small and medium firms may also help to explain why employment fell more at these firms during the post-Lehman banking crisis period. Figure 3 shows quarterly employment changes during the recession and recovery by firm size class, using the published tabulations of the LDB (the Business Employment Dynamics).[45] Prior to the Lehman bankruptcy, employment at the largest firms fell slightly faster than at firms in the 50-249 and 250-999 size classes. The relationship reverses dramatically thereafter. During the heart of the banking crisis in the fourth quarter of 2008 and the first quarter of 2009, smaller firms reduced employment by much more than those in the largest size class. Indeed, a monotonic relationship between the severity of losses and firm size class obtains during this period. The differential then disappears beginning in the summer of 2009, coincident with the timing of the stabilization of interbank lending markets. Integrating over the entire two-year period of employment losses in the United States, about two-thirds of the decline in employment came at firms with fewer than 1000 employees.

Column 2 of table 10 indicates that credit supply has an economically large effect on and significant explanatory power for the change in employment at firms without access to the bond market, but a much smaller effect on and no significant explanatory power for the change at firms with access. While these results may stem in part from the positive correlation between bond market access and transparency, they also likely reflect the substitution toward bond financing by firms with bond market access and diminished access to bank credit. Indeed, in the subsample of the firms in Dealscan that have ever accessed the bond market, a probit regression of whether the firm issued any public debt between October 2008 and June 2009 on the syndicate health measure $\Delta\tilde{L}_{i,s}$ and the standard set of controls yields a negative coefficient with a t-statistic of 2.6. That is, firms attached to weaker lenders did in fact compensate in part by issuing more public debt.[46]

The last column of table 10 investigates the relative importance of size and bond market

[44]Krueger uses unpublished JOLTS data that identify size class from establishment rather than firm size.

[45]The employment changes in Figure 3 and discussed in this paragraph use a dynamic sizing methodology. This means that a firm that begins the quarter with 1010 employees and ends the quarter with 980 employees will contribute to a decline in employment of 10 in the 1000+ class (going from 1010 to 1000) and of 20 (going from 1000 to 980) in the 250-999 class.

[46]Becker and Ivashina (2010) and Adrian, Colla and Shin (2013) show that within a single firm bond issuance moves counter-cyclically while bank finance moves pro-cyclically. The result reported in the text provides evidence of financing substitution in the cross-section of firms as well.

Table 10: The effect of lender credit supply on employment with heterogeneous treatment effects

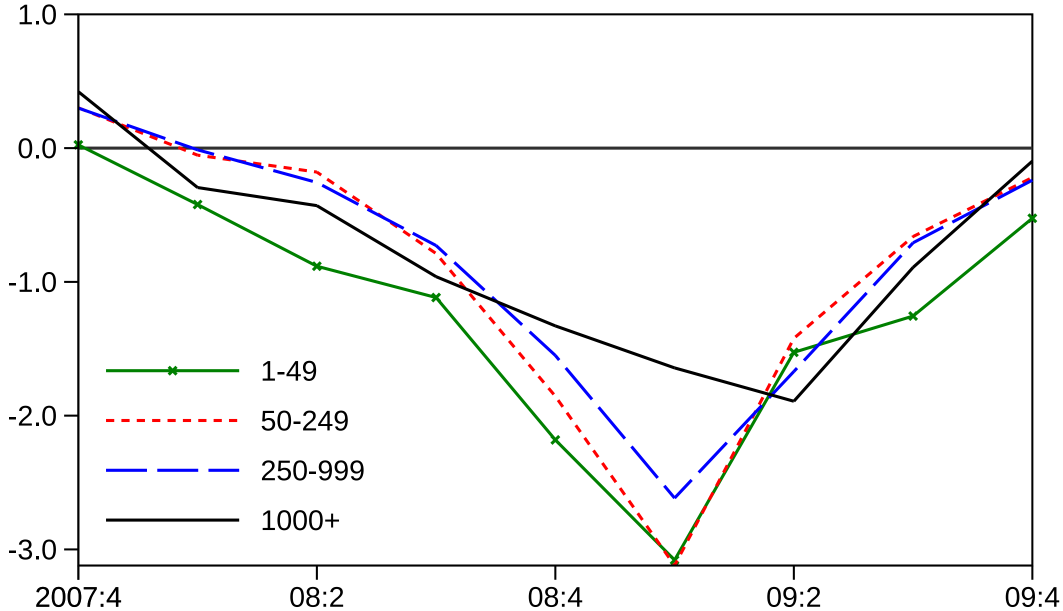
	Employment growth rate 2008:3-2009:3		
	(1)	(2)	(3)
Explanatory variables:			
$\Delta\tilde{L}_{i,s}$ * Large	0.54		
	(0.97)		
$\Delta\tilde{L}_{i,s}$ * Medium	1.84^{+}		
	(0.97)		
$\Delta\tilde{L}_{i,s}$ * Small	2.16^{**}		
	(0.79)		
$\Delta\tilde{L}_{i,s}$ * Bond market access		1.04	
		(1.00)	
$\Delta\tilde{L}_{i,s}$ * No access		2.01^{**}	
		(0.60)	
$\Delta\tilde{L}_{i,s}$ * Bond access & large			0.23
			(1.15)
$\Delta\tilde{L}_{i,s}$ * Bond access & small/medium			1.47
			(1.06)
$\Delta\tilde{L}_{i,s}$ * No access & large			0.79
			(1.21)
$\Delta\tilde{L}_{i,s}$ * No access & small/medium			2.26^{**}
			(0.58)
Lagged employment growth	Yes	Yes	Yes
Emp. change in firm's county	Yes	Yes	Yes
2-digit SIC, state, loan year FE	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes
Observations(Access & large)	483	483	483
Observations(Access & small/medium)	434	434	434
Observations(No access & large)	315	315	315
Observations(No access & small/medium)	808	808	808
Observations	2,040	2,040	2,040

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods October 2005 to June 2007 and October 2008 to June 2009, and has been normalized to have unit variance. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Bond market access is equal to 1 if the firm has any bonds listed in the Mergent FISD database or if the firm has a credit rating. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

Figure 3:

Employment losses by firm size

(Quarterly change in private sector employment, percent)



Source: Bureau of Labor Statistics (Business Employment Dynamics).

Notes: The figure shows the percent change in employment by firm size class. The numerator is the change in employment using a dynamic sizing methodology. The denominator is the average level of employment during the two quarters. The BLS only reports employment levels for the first quarter of each year; the denominators for intervening quarters are linearly interpolated.

access. The column reports coefficients from allowing the treatment effect to differ among four firm-type bins, with each bin the interaction of access to the bond market and firm size divided between large (>999 employees) and small/medium. As expected from the results just discussed, the point estimate of the treatment at large firms with bond market access is essentially zero, while the effect at small and medium firms without bond market access is economically large and highly statistically significant. While the magnitudes of the standard errors limit the potential inference, the coefficients at both large firms without bond market access and small and medium firms with access are larger than the coefficient at large firms with bond market access. It appears that both size and bond market access have separate effects on the importance of lender health to employment outcomes.

As a final point, the heterogeneous treatment results serve as a specification check for the validity of the loan supply measure. For unobserved borrower characteristics to explain the results, it would have to either be the case that only small and medium borrowers matched selectively with banks along the unobserved dimensions, while the largest borrowers associated with lenders randomly, or that the largest borrowers faced no frictions to switching lenders during the crisis.[47] Similarly, borrowers with bond market access would have to have matched differently from borrowers without such access.

6.4. Other time periods

The effect of credit supply on employment over two other periods can help shed light both on the mechanism at work and the course of the 2007-09 recession. The first period covers the beginning of the recession in December 2007 up to the Lehman bankruptcy. As shown in figure 2, the aggregate volume of new lending to firms began to decline in the fourth quarter of 2007, and troubles in the banking sector had begun to appear by that time as well. Still, the severity of the credit crunch, as measured by the volume of lending or the stress in the interbank market, remained well below what obtained following the Lehman bankruptcy. For that reason, one might expect that credit availability explains a smaller share of the decline in employment during this period.

The top panel of table 11 reports the results for the pre-Lehman period. For these specifications only, the loan supply measure reflects the change in lending during the pre-Lehman period relative to before the crisis, rather than the change in lending post-Lehman as used elsewhere in the paper.[48] The panel also omits results for the Lehman cosyndication exposure instrument and the bank balance sheet items, since both capture aspects of bank health that post-date the employment period. (The next subsection will report placebo regressions.) The results indicate an effect of credit supply on employment in the pre-Lehman period, but of about one-third of the magnitude relative to the post-Lehman results reported in columns 2 and 4 of table 9.

Whether credit frictions can have long-lasting effects on real outcomes has important implications for the ability of such frictions to explain events at business cycle frequencies. In the context of the 2007-09 recession and recovery, credit market conditions appear to have stabilized by the summer of 2009, but employment did not begin to recover until the beginning of 2010. The timing thus suggests that either something else held back hiring in the aggregate economy, affected firms still had difficulty obtaining credit despite the apparent calm, or propagation mechanisms prolonged the effect of the initial shock.

The bottom panel of table 11 offer suggestive evidence by extending the baseline period to 2008:3-10:3. Recalling that the OLS coefficient on loan supply for the 2008:3-09:3 period

[47] A potential caveat to the heterogeneity of treatment effects by size stems from the measurement error problem raised in footnote 12. Larger firms are more likely to split their reporting into multiple EINs, which could increase the measurement error in the dependent variable for this group of firms. To check this possibility, I re-ran the specification but using employment as reported in Compustat rather than the LDB. The merged Dealscan-Compustat sample contains many fewer firms in the small and medium size classes, so I combine those into a single class. I also restrict the sample to companies that end their fiscal year in December (about 71 percent of the Compustat firms), and compute the employment change over the smallest possible window that encompasses Sep-08 to Sep-09, namely Dec-07 to Dec-09. The resulting sample has 850 large (>999) firms and 238 small and medium firms. The results look quite similar to those in table 10, with a t-statistic of -0.85 for the effect of $\Delta\tilde{L}_{i,s}$ on the employment change at large firms, and a t-statistic of 3.63 for the effect at small and medium firms. Measurement error does not appear to explain the absence of loan supply effects at large firms.

[48] The R^2 of the two measures is 0.72. Since the matching of borrowers and lenders for the pre-Lehman period relies on loans obtained prior to December 2007, the sample omits borrowers who obtained their first loan after that date.

Table 11: The effect of lender credit supply on employment pre-Lehman and in the medium-run

	Employment growth rate				
	OLS	$\Delta\tilde{L}_{i,s}$ instrumented using			
		Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)
	Panel A 2007:4-2008:3				
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	0.55^{+}		1.26		
	(0.31)		(0.81)		
Lagged employment growth	0.052^{**}		0.053^{**}		
	(0.015)		(0.015)		
Emp. change in firm's county	0.59^{+}		0.52		
	(0.32)		(0.32)		
First stage F-statistic			9.4		
Observations	1,895		1,872		
	Panel B 2008:3-2010:3				
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	1.94^{**}	3.40^{**}	5.18^{**}	2.14^{*}	2.67^{**}
	(0.63)	(1.26)	(1.94)	(1.00)	(0.90)
Lagged employment growth	0.049^{**}	0.051^{**}	0.052^{**}	0.050^{**}	0.050^{**}
	(0.018)	(0.018)	(0.017)	(0.017)	(0.017)
Emp. change in firm's county	-0.17	-0.21	-0.25	-0.17	-0.19
	(0.49)	(0.52)	(0.52)	(0.50)	(0.50)
First stage F-statistic		15.4	8.4	18.5	23.0
Observations	2,013	2,013	1,988	2,013	1,988
2-digit SIC, state, loan year FE	Yes	Yes	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. The variable $\Delta\tilde{L}_{i,s}$ equals the change in the annualized number of loans made by the bank between the periods December 2004 to August 2006 and December 2007 to August 2008 (panel A), or between the periods October 2005 to June 2007 and October 2008 to June 2009 (panel B), and has been normalized to have unit variance. Firms divided into size bin classes of 1-250, 250-999, and 1000+, and age bins for birth in the 2000s, 1990s, or earlier. Additional Dealscan controls: Multiple lead lenders indicator; Loan due during crisis indicator; Credit line indicator; Log sales at close; All in drawn spread; Credit line*all in drawn. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

equaled 1.67, the coefficient of 1.94 in the first column indicates that firms forced to shed additional employment post-Lehman did not make up any of the difference in the year following. A similar result obtains for the instrumental variables specifications. Credit frictions appear to have long-lasting effects. In regressions not shown, I also find that these firms had no differential likelihood of obtaining a loan during the second year post-Lehman.[49]

A number of mechanisms could potentially explain the persistence of the effects. If firms use recessions to purge excess labor or "innovate to survive" under tighter financial constraints, then being forced to cut employment more during the crisis could have long-lasting effects, even if the borrowing constraint weakens subsequently. Likewise, restricting output due to financing constraints may cause customers to switch suppliers, which could persist beyond the period during which the constraint binds. Aghion, Farhi and Kharroubi (2012) model a precautionary saving channel, in which affected borrowers reduce inputs in order to hoard liquidity in case of a future shock. Finally, excluding firms from the sample that went bankrupt reduces the coefficients in the 2008:3-10:3 period by roughly 20 percent.

6.5. The effect of loan market outcomes on employment

Having presented evidence that lender health affects both loan market and employment outcomes of borrowers, a natural extension would measure the employment consequences of an adverse loan market outcome. This would involve a 2SLS design, with the first stage the loan market outcome regressed on one of the lender health measures listed in the header to table 9, and the second stage employment regressed on the loan market outcome.

This procedure suggests an economically large effect of credit availability on employment. For example, grouping the Lehman exposure, ABX exposure, and bank balance sheet and income statement items together as the instrument set, the second stage coefficient for the extensive margin outcome studied in table 6 equals 42.0, with a standard error of 23.4 (not shown). The coefficient has the interpretation that a firm that did not receive a new loan or positive modification would have a decline in employment of 42 percent using a symmetric growth rate, which corresponds to a 53 percent decline relative to the initial level. The effect appears even larger using the instruments separately or using the bank lending to other borrowers to instrument for the loan market outcome, and in four out of the five specifications the data reject the null of no effect with at least ten percent significance.

Section 4.1 suggested two problems with this approach that may cause the exercise to overstate the true effect of losing access to credit. First, the single source of exogenous variation in loan outcomes – variation in lenders' internal cost of funds – and multiple endogenous variables (did the firm receive a loan, the interest rate conditional on receiving a loan, etc.) render the system under-identified. In other words, the estimated effect of not receiving a loan also encompasses the effect of other outcomes such as receiving a higher interest rate. Indeed, the J test of the exclusion restriction of the grouped instruments rejects exogeneity at the ten percent level. Second, and as discussed in section 5.2, the endogenous variable appears to potentially exclude a number of loan modifications to private

[49]The implications of the loan market outcomes are not obvious. Firms that could not obtain a loan during the year after Lehman may have had pent-up loan demand, suggesting a higher likelihood of obtaining a loan once markets stabilized. On the other hand, serial correlation in lender health could mean that these firms remained attached to weaker lenders.

borrowers. Such incomplete information would bias the first stage toward zero, leading to an overestimate of the second stage coefficient. Importantly, while these concerns implicate the consistency of the second stage coefficients of employment on loan market outcomes, they do not invalidate the estimates of employment directly on bank health measures reported in the rest of this section.

6.6. Employment placebo regressions

The effect of the crisis bank health measures on employment during pre-crisis periods can serve as a specification check of the validity of the health measures. Table 12 reports results for two pre-crisis periods that use the same sample as that of table 9, and assign the same loan supply measures to each borrower. Thus the only difference in specification is the period covered by the dependent variable.[50]

The first period covers the end of the previous business cycle expansion up until the beginning of the turmoil in financial markets, 2005:2-07:2. A finding of a positive relationship in this period would raise the concern that banks with higher crisis lending had pre-crisis borrowers that had higher secular employment growth trends, while a negative finding could simply indicate that the borrowers had abnormal cyclical patterns. However, the top panel shows that the loan supply measures have essentially no predictive power during this period.

The second placebo period covers the end of the previous U.S. recession, from 2001:3-02:3. The 2001-02 period has a number of superficial similarities to the 2008-09 period, including the gap between the period start and the NBER business cycle peak; the economic shock during the first month of the period (in the former case the events of September 11th); and the timing of the last month of the period as near but not yet at the local minimum in aggregate employment. On the other hand, the 2001 recession did not have any obvious origin in credit market events. The concern, raised in a different context in Mian and Santos (2011), that some banks lend to more countercyclical borrowers and that this tendency explains the results above, would predict a positive coefficient of 2008-09 lending on 2001-02 employment. Instead, the point estimates in four of the five columns of the bottom panel are negative, and none is statistically significant.

Firms attached to worse lenders and that had worse employment outcomes during 2008-09 do not appear different from other firms during pre-crisis periods.

7. Aggregate implications

With some additional assumptions, the firm-level results in section 6 can help to inform about the aggregate effect of the credit frictions during the 2008-09 period.

7.1. Aggregate effects in the sample

The first step involves estimating the total effect of bank lending frictions on employment

[50] This statement requires two caveats. First, the placebo samples are slightly smaller, as a few of the borrowers either did not appear in the LDB during the placebo period or had an identification change that prevents linking them to the original sample. Second, the lagged dependent variable included in the regressions corresponds to the period from 3.25 to 1.25 years prior to the beginning of the placebo period, mirroring the time difference used in the main regressions.

Table 12: The effect of lender credit supply on employment in two placebo periods

	Employment growth rate				
	OLS	$\Delta\tilde{L}_{i,s}$ instrumented using			
		Lehman exposure	ABX exposure	Bank statement items	All
	(1)	(2)	(3)	(4)	(5)
	Panel A 2005:2-2007:2				
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	-0.19	-0.67	-1.57	1.63	0.92
	(0.74)	(1.63)	(1.72)	(1.24)	(1.15)
Lagged employment growth	0.028^{+}	0.027^{+}	0.028^{+}	0.028^{+}	0.028^{+}
	(0.014)	(0.014)	(0.014)	(0.015)	(0.015)
Emp. change in firm's county	0.80	0.80	0.78	0.79	0.77
	(0.49)	(0.49)	(0.50)	(0.48)	(0.49)
First stage F-statistic		15.6	8.8	18.9	23.8
Observations	1,879	1,879	1,854	1,879	1,854
	Panel B 2001:3-2002:3				
Explanatory variables:					
%Δ loans to other firms ($\Delta\tilde{L}_{i,s}$)	-0.80	-0.74	1.30	-0.93	-0.72
	(0.59)	(1.44)	(1.89)	(0.93)	(0.85)
Lagged employment growth	0.024	0.024	0.024	0.024	0.024
	(0.020)	(0.020)	(0.020)	(0.020)	(0.020)
Emp. change in firm's county	1.53^{**}	1.53^{**}	1.62^{**}	1.53^{**}	1.59^{**}
	(0.51)	(0.50)	(0.51)	(0.51)	(0.50)
First stage F-statistic		16.5	7.7	17.8	26.3
Observations	1,675	1,675	1,653	1,675	1,653
2-digit SIC, state, loan year FE	Yes	Yes	Yes	Yes	Yes
Firm size & age bin FE	Yes	Yes	Yes	Yes	Yes
Bond access/Public/Private FE	Yes	Yes	Yes	Yes	Yes
Additional Dealscan controls	Yes	Yes	Yes	Yes	Yes

Notes: The dependent variable is the symmetric growth rate g_j^y of employment. All right hand side variables exactly equal those used to produce columns 2-6 of table 9, except the lagged employment growth rate which instead uses the period from 3.25 to 1.25 years prior to the beginning of the placebo period, and the county employment change which is contemporaneous to the placebo period. Standard errors in parentheses and twoway clustered on the lead lenders in the borrower's last pre-crisis loan syndicate. +,*,** indicate significance at the 0.1, 0.05, 0.01 levels respectively.

in the sample. In the counterfactual exercise, every borrower faces a pre-crisis syndicate that changed its lending supply by the same amount as the most liberal syndicate. The estimate of effects in the sample depends on two assumptions:

A1 (partial equilibrium): the total employment effects equal the sum of the direct employment effects measured at each firm.

A2 (unconstrained at the top): the most liberal syndicate did not shift its lending supply function during the crisis.

The widespread distress in financial markets after the Lehman bankruptcy suggests that A2 may be quite conservative. If the syndicate identified as the most liberal also contracted its lending supply function, the estimates below will understate the true level of employment effects in the sample.

To begin, define the counterfactual growth rate if firm i in size-class c had borrowed from the τ'th percentile of lenders:

$$\begin{aligned} g^y_{i,s,t-k,t}\left(Q_\tau\right) &= E\left[g^y_{i,s,t-k,t}|\Delta\tilde{L}_{i,s} = \Delta\tilde{L}_{Q_\tau}\right] \\ &= \hat{g}^y_{i,s,t-k,t} + \hat{\beta}_{1,c}\left[\Delta\tilde{L}_{Q_\tau} - \Delta\tilde{L}_{i,s}\right]. \end{aligned} \tag{13}$$

$\Delta\tilde{L}_{Q_\tau}$ denotes the loan measure for the borrower at the τ'th percentile of the distribution, while $\hat{g}^y_{i,s,t-k,t}$ denotes the fitted value from the regression. Let T denote the mapping from symmetric growth rates to the end-period level, holding the initial level fixed:[51]

$$T\left[x\right] = \frac{1+0.5x}{1-0.5x}y_{i,t-k}. \tag{14}$$

Using equation (14), define the counterfactual period t employment level at the τ'th percentile as $y_{i,t}\left(Q_\tau\right) = T\left[g^y_{i,s,t-k,t}\left(Q_\tau\right)\right]$, and similarly the fitted value employment level as $\hat{y}_{i,t} = T\left[\hat{g}^y_{i,b,t-k,t}\right]$.[52] Identifying the most liberal syndicate as that at the τ'th percentile of the distribution, the total sample employment losses due to frictions are then:

$$\text{Total losses due to frictions} = \sum_{\Delta\tilde{L}_{i,s}\leq\Delta\tilde{L}_{Q_\tau}} \left[y_{i,t}\left(Q_\tau\right) - \hat{y}_{i,t}\right].$$

The fraction of the sample net employment change due to frictions equals:

$$\frac{\sum_{\Delta\tilde{L}_{i,s}\leq\Delta\tilde{L}_{Q_\tau}}\left[y_{i,t}\left(Q_\tau\right) - \hat{y}_{i,t}\right]}{\sum_i\left[y_{i,t-k} - y_{i,t}\right]}. \tag{15}$$

Table 13 reports the results. Since the estimated coefficient for large firms is quantitatively small and not significant, I impose that $\beta_{1,large} = 0$, and use the coefficients reported in column 1 of table 10 for the marginal effect at small and medium firms.

[51] That is, $T\left[g^y_{t-k,t}\right]$ solves:

$$g^y_{t-k,t} = \frac{T\left[g^y_{t-k,t}\right] - y_{i,t-k}}{0.5\left[T\left[g^y_{t-k,t}\right] + y_{i,t-k}\right]}.$$

[52] OLS imposes that $\sum_i g^y_{i,s,t-k,t} = \sum_i \hat{g}^y_{i,s,t-k,t}$. However, both the non-linearity of $T\left[.\right]$ and the non-degenerate size distribution of $y_{i,t-k}$ imply that $\sum_i y_{it} \neq \sum_i \hat{y}_{it}$ unless by chance.

Table 13: Total effect of credit availability at small and medium firms in the sample

	2008:3-2009:3 (Percent)
Total employment decline	7.0
Share of losses due to credit availability, $\tau = 90$	34.4
Share of losses due to credit availability, $\tau = 95$	47.3

Notes: The table reports the fraction of employment losses due to credit availability at small and medium firms, as described in the text. τ refers to the percentile of the lending syndicate identified as the most liberal syndicate.

The table reports results using either the 90th or 95th percentile syndicate as the unconstrained lender. Total employment at firms in the sample with fewer than 1000 employees declined by seven percent from 2008:3 to 2009:3.[53] The exercise indicate that the shift in loan supply can account for between one-third and one-half of these losses, depending on the percentile used to identify the most liberal syndicate.

7.2. Aggregate effects in the population

Extrapolating the results in table 13 to inform an estimate for the whole economy presents additional challenges. The first concerns external validity. On the one hand, the sample excludes very small firms - recall that the tenth percentile firm by employment has 77 employees. In the population, about one-third of private sector employment occurs at firms at least that small. If these firms exhibit even greater sensitive to their lenders' health, then the estimated magnitude of effects in the sample could understate the average in the population.

Counter to that, the sample by construction contains firms dependent on external financing. The requirement that firms have a lending relationship by itself may not generate too much sample selection bias. For example, the 2003 Federal Reserve Survey of Small Business Finances (the most recent available) finds that more than eighty percent of firms with between 100 and 499 employees have a credit line (Mach and Wolken 2006). Still, the criterion that the loan be syndicated does restrict the sample to those firms with the largest bank dependence, and this selection may be most severe for smaller firms.

General equilibrium effects pose a second challenge to extrapolating to the whole economy. In partial equilibrium, financially-constrained firms facing a downward-sloping product demand curve reduce production and raise prices. This results in lower labor input at constrained firms relative to unconstrained firms, consistent with the cross-sectional empirical evidence presented above. The aggregate effect of the frictions, however, also depends on whether the unconstrained firms adjust their labor input.

General equilibrium analysis suggests opposing channels that may lead unconstrained firms also to adjust their labor input. First, some labor shifts from constrained to unconstrained firms. Part of the reallocation of labor results from a shift in product demand, as

[53]For comparison, summing over the four quarters 2008:4-2009:3 the employment declines at firms with between 50 and 999 employees in the quarter, and dividing by the employment level in 2008:3, yields a decline of 6.7 percent in the entire U.S. economy.

relative prices at unconstrained firms fall or constrained firms ration their output. Further reallocation comes from the decline in employment at constrained firms causing a fall in the real product wage, which induces unconstrained firms to move down their labor demand curves. The magnitude of the labor reallocation depends on the substitutibility of both the goods produced at the different firms and the labor used in production. Second, the financial shock generates a reduction in aggregate expenditure. The fall in aggregate expenditure reduces labor demand at unconstrained firms.

An online appendix presents a formal general equilibrium model that illustrates these channels and fully characterizes the relationship between the empirically-estimated relative employment outcomes and the aggregate effects that obtain in general equilibrium. For plausible parameter values, the general equilibrium effects in the model either magnify the effects from the partial equilibrium exercise or have at most a modest attenuating effect. This result suggests that the magnitudes in table 10 may provide a reasonable benchmark for the aggregate effect of the frictions. Nonetheless, it has some sensitivity to the model's assumptions and parameter choices, indicating that general equilibrium effects provide a second source of uncertainty in moving from the estimated effects in the sample to the whole economy.

8. Conclusion

This paper has shown that banking relationships matter. In particular, it has linked the health of a firm's lenders to its employment outcomes. The relationship appears economically important both at the level of the firm and for aggregate fluctuations. At the level of the firm, the predicted change in employment varies by as much as five percentage points depending on the health of its lenders. In the aggregate, these frictions can account for between one-third and one-half of the decline in employment at small and medium firms in the year following the collapse of Lehman Brothers.

These results have implications for explanations of the severity of the 2007-09 recession. In a series of papers, Mian and Sufi (2010; 2011; Mian, Rao and Sufi 2011) argue for the importance of what they term the "deleveraging-aggregate demand hypothesis," which explains the recession by a reduction in consumption demand by households trying to reduce debt burdens following the collapse of house prices (see also Eggertsson and Krugman 2012). The findings here provide direct evidence for a complementary channel that highlights the role of financial frictions in restricting the availability of credit to firms (Gilchrist and Zakrajsek 2012; Hall 2011; Stock and Watson 2012). The frictions channel can potentially explain the acceleration of the downturn following Lehman Brothers, as well as the added severity during that period at smaller firms. It may also provide a partial explanation for the unusual rise in unemployment relative to the fall in output (Daly and Hobjin 2010), if the lack of credit caused firms to purge excess labor more than they otherwise would.[54]

If financial frictions can help to explain why the downturn accelerated in the fall of 2008, they face a challenge in explaining the persistence of the slump, since credit markets appear to have stabilized well before the aggregate economy returned to normal (Hall 2010).

[54] Uncertainty and structural mismatch have also been suggested as explanations for the depth of the recession. Of course, the channels are not mutually exclusive.

The result that at the level of the firm employment losses due to frictions do not appear to have dissipated at all two years later is intriguing in this regard. Better understanding of the mechanisms that generate such persistence at the microeconomic level could lead to improved macroeconomic insight and would provide one fruitful avenue for future research.

Finally, the paper has documented that the importance of banking relationships varies by firm type. Consistent with theories where banking relationships form due to the presence of asymmetric information about borrowers, the pre-crisis relationship appears to have essentially no effect on crisis outcomes for the largest and most transparent borrowers, and substantial effects for smaller borrowers and those without access to public debt markets. Data constraints have in the past limited analysis of credit frictions and cash-flow sensitivity to the effect at large, transparent firms. Future research should continue to look for ways to study the effects at smaller firms as well.

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Cross-sectional Estimation and General Equilibrium

Gabriel Chodorow-Reich

1. Introduction

A crucial question for cross-sectional empirical studies of questions of macroeconomic importance is what their results imply about the aggregate. Writing out a fully-specified general equilibrium model can help to fill this gap. This chapter presents just such a model for the empirical exercise conducted in chapter 2.

2. General equilibrium model of heterogeneous bank shocks

The aggregation exercise in chapter 2 assumes that the credit frictions have no effect on employment at firms borrowing from healthy lenders. The cross-sectional econometric approach requires this assumption, since it implicitly measures the employment shortfall at borrowers of less healthy syndicates relative to employment at borrowers of healthier syndicates. In general equilibrium, however, two opposing channels lead unconstrained firms also to adjust their labor input. First, some demand shifts from constrained to unconstrained firms. The shift may occur because relative prices at unconstrained firms fall, or because constrained firms ration their output. The magnitude of the labor reallocation depends on the substitutibility of both the goods produced at the different firms and the labor used in production. Second, the financial shock generates a reduction in aggregate expenditure. The fall in aggregate expenditure reduces labor demand at unconstrained firms.

This chapter presents a stylized general equilibrium model that fully characterizes the relationship between the empirically-estimated relative employment outcomes and the aggregate effects that obtain in general equilibrium. The model contains three sectors: a household that consumes and supplies labor; monopolistically competitive firms that use labor to produce a differentiated good; and financial sector firms that supply credit lines to goods producers. Banking relationships enter the model through the assumption that each financial sector firm operates on an "island" with a subset of the goods producers, such that producers can only obtain financing from the financial sector firm on their island. A financial crisis causes an increase in the interest rate charged on the credit lines.

The model delivers simple closed-form expressions that illustrate the general equilibrium channels. An illustrative calibration using parameters consistent with macroeconomic fluctuations suggests that the general equilibrium effects either magnify the effects from the partial equilibrium exercise or have at most a modest attenuating effect.

2.1. Household

A household consists of a continuum of individuals who supply labor to firms and pool consumption risk. The household's objective function takes the form:

$$U = E_t \sum_{\tau=t}^{\infty} \beta^{\tau-t} u\left(C_\tau, L_\tau\right), \tag{1}$$

and its flow budget constraint is

$$P_t C_t + B_t = w_t L_t + \left(1 + i_{t-1}\right) B_{t-1} + T_t. \tag{2}$$

The consumption bundle C_t is a Dixit-Stiglitz constant elasticity of substitution (CES) aggregation of differentiated varieties:

$$C_t = \left[\int_0^1 \int_0^1 \xi_{j,s,t}^{\frac{1}{\sigma}} c_{j,s,t}^{\frac{\sigma-1}{\sigma}} dj ds\right]^{\frac{\sigma}{\sigma-1}}, \tag{3}$$

where $c_{j,s,t}$ is the consumption of variety j produced by a firm operating on island s; σ is the elasticity of substitution across varieties; and $\xi_{j,s,t}$ is a variety-specific taste-shock. Variety j on island s is assumed to also be differentiated from all varieties produced on island s'. The bundle price P_t is the cost of purchasing one unit of C_t:

$$P_t = \left[\int_0^1 \int_0^1 \xi_{j,s,t} p_{j,s,t}^{1-\sigma} dj ds\right]^{\frac{1}{1-\sigma}}, \tag{4}$$

where $p_{j,s,t}$ is the price of variety j produced on island s.

L_t is a CES aggregation of the labor supplied to different firms, with elasticity of substitution ν:

$$L_t = \left[\int_0^1 \int_0^1 L_{j,s,t}^{\frac{\nu+1}{\nu}} dj ds\right]^{\frac{\nu}{\nu+1}}, \tag{5}$$

where $L_{j,s,t}$ is the labor supplied to firm j on island s. w_t is the composite wage defined as the compensation earned by the household from optimally allocating $L_t = 1$ unit of labor taking the firm wage distribution as given, and satisfies

$$w_t = \left[\int_0^1 \int_0^1 w_{j,s,t}^{1+\nu} dj ds\right]^{\frac{1}{1+\nu}}, \tag{6}$$

where $w_{j,s,t}$ is the wage paid by firm j on island s.

As $\nu \to \infty$, labor becomes a homogenous input and, from the first-order condition (7) below, the real wage must equate across firms. As $\nu \to 0$, it becomes very costly to reallocate labor input and large real wage differentials may obtain. Thus ν determines how easily labor can shift among firms.[1] This makes it similar to other frictions such as search costs or labor adjustment costs that would also delay the reallocation of labor from constrained to unconstrained firms.

Finally, B_t denotes purchases of a riskless bond at time t, i_t is the nominal interest rate, $\beta < 1$ is the discount factor, and T_t contains dividend payouts from the goods sector and the financial sector and rebates from the financial sector as described further below.

The household's first order conditions are:

$$-\left[\frac{L_{j,s,t}}{L_t}\right]^{\frac{1}{\nu}} \frac{u_{L_t}}{u_{C_t}} = \frac{w_{j,s,t}}{P_t} \ \forall j, s, \tag{7}$$

$$u_{c_t} = E_t\left[\beta\left(1+i_t\right)\frac{P_t}{P_{t+1}} u_{c_{t+1}}\right], \tag{8}$$

$$c_{j,s,t} = \xi_{j,s,t}\left(\frac{p_{j,s,t}}{P_t}\right)^{-\sigma} C_t \ \forall j, s. \tag{9}$$

2.2. Goods producers

The production-side of the economy consists of firms that operate on a unit square, with $s \subseteq [0,1]$ indexing the first dimension (islands) and $j \subseteq [0,1]$ the second dimension (firms on an island). Firm j operating on island s produces output using the production technology

$$y_{j,s,t} = a_{j,s,t} l_{j,s,t}^{1-\gamma} \tag{10}$$

[1] With utility separable over consumption and labor and a constant Frisch elasticity $\eta = \nu$, this setup collapses to the firm-specific labor input model. In general, it allows the elasticity of labor supply to an individual firm to differ from the intertemporal labor supply decision.

and subject to the demand curve (9). The firm distributes all profits to the household each period.[2]

Firms exit with exogenous probability δ each period and are replaced by a measure δ of new firms. Exit occurs after production but before paying labor. The revenue of exiting firms rebates to the household lump sum. The possibility of exit forces firms to maintain a credit line sufficient to cover their wage bill. A credit line costs $r_{s,t}$ for each dollar of coverage, so that a firm with wage bill $w_{j,s,t}l_{j,s,t}$ must pay the lender $r_{s,t}w_{j,s,t}l_{j,s,t}$. Since the firm enters the period with no retained earnings, it pays the lender after realizing revenues. Firms maximize expected profits, given by

$$\Pi_{j,s,t} = [1-\delta]\left[p_{j,s,t}y_{j,s,t} - (1+r_{s,t})\, w_{j,s,t}l_{j,s,t}\right] + \delta\,[0]\,. \tag{11}$$

The profit-maximizing price equals a markup over marginal cost:

$$p_{j,s,t} = \mathcal{M}\,(1+r_{s,t})\,\frac{w_{j,s,t}}{(1-\gamma)\,a_{j,s,t}l_{j,s,t}^{-\gamma}}, \tag{12}$$

where $\mathcal{M} \equiv \frac{\sigma}{\sigma-1}$ is the markup.[3][4] Together, (9)-(12) and the market-clearing condition $c_{j,s,t} = y_{j,s,t}$ yield the following equation for firm labor demand:

$$l_{j,s,t} = \left[\frac{\xi_{j,s,t}}{a_{j,s,t}}\left(\frac{\mathcal{M}\,(1+r_{s,t})\,\frac{w_{j,s,t}}{(1-\gamma)a_{j,s,t}}}{P_t}\right)^{-\sigma} C_t\right]^{\frac{1}{1+\gamma(\sigma-1)}}. \tag{13}$$

The joint distribution of $(\xi_{j,s,t}, a_{j,s,t})$ is the same on every island and time-invariant. Thus the only difference across islands comes from the financial sector, and not from technology or taste. The assumption of time-invariance could be relaxed without changing any of the important results.

[2] I leave unmodeled the agency problem that gives rise to this dividend distribution policy. For example, it would arise if firms' managers could abscond with any undistributed profits at the end of the period.

[3] Note that each firm acts as a price-taker in the labor market despite using a differentiated labor input. Following Woodford (2003), one may justify this assumption by having a continuum of "industries" on each island, where each industry consists of a continuum of firms receiving identical technology and taste shocks and hiring from the same pool of workers. All firms in an industry then hire the same amount of labor and set the same wage. However, individual firms do not have monopsony power in wage-setting.

[4] From equation (12), the credit line requirement enters the model isomorphic to a payroll-in-advance constraint that would require the firm to obtain intraperiod working capital (see e.g. Chari, Christiano and Eichenbaum 1995). An important theoretical difference between the working capital and credit line models is that the former requires a supply of intraperiod liquidity in the model whereas the latter does not. Most syndicated loans take the form of credit lines.

2.3. Financial sector

Each of a continuum of financial firms operates on a single island. Financial firms provide credit lines to the goods producers on their island, using the payments from surviving firms to cover the losses they incur from making wage payments to the workers of exiting firms. In addition, each financial firm diverts a fraction $\zeta_{s,t}$ of the payments earned from surviving firms to the household sector. $\zeta_{s,t}$ introduces variation in bank health into the model without requiring an explicit treatment of nonperforming assets. In an extended model, it might reflect writedowns on mortgages that are liabilities of the household sector. The distribution of $\zeta_{s,t}$ across islands is exogenous, reflecting the assumption that the distress in the financial sector during 2007-09 originated in areas other than corporate lending.

A financial firm that diverts $\zeta_{s,t}$ to the household sector lends to the nonfinancial business sector at rate $r_{s,t}$. Without loss of generality, let $r_s < r_{s'}$ if $s > s'$, so that a higher index denotes a lower interest rate. New financial firms can enter an island but must pay the same earnings penalty $\zeta_{s,t}$ as the incumbent firm. In equilibrium no new firms enter but incumbent firms earn zero profits, giving the condition

$$r_{s,t} = \frac{\delta}{1-\delta} + \zeta_{s,t}. \tag{14}$$

2.4. Equilibrium

An equilibrium consists of quantities $(\{c_{j,s,t}\}, \{l_{j,s,t}\})$ and prices $(\{p_{j,s,t}\}, \{w_{j,s,t}\})$ subject to the household's first-order conditions (7)-(9), the firms' optimal pricing decision (12) and labor demand (13), the financial sector firms' lending rate condition (14), the exogenous driving forces $(\{a_{j,s,t}\}, \{\xi_{j,s,t}\}, \{\zeta_{s,t}\})$, and the market-clearing conditions

$$\begin{aligned} l_{j,s,t} &= L_{j,s,t} \, \forall j, s, && (15) \\ y_{j,s,t} &= c_{j,s,t} \, \forall j, s, && (16) \\ B_t &= 0. && (17) \end{aligned}$$

2.5. Relation to empirical work

Let $\hat{x} = \frac{dx}{\bar{x}}$, where $\bar{x}$ is the value in a zero-inflation non-stochastic steady-state. Substituting for the firm real consumption wage $\frac{w_{j,s,t}}{P_t}$ in (13) using the intraperiod labor-consumption tradeoff (7), imposing market clearing in the labor market, and using the aggregate relationship $\hat{C}_t = [1-\gamma]\hat{L}_t$ as well as the economy-wide counterpart to (7) yields the equilibrium employment function

$$\hat{l}_{j,s,t} = \kappa \left[1 + \frac{\sigma}{(1-\gamma)\,\nu}\right] \hat{C}_t - \kappa\sigma \left[\hat{w}_t - \hat{P}_t\right] - \kappa\sigma\hat{r}_{s,t} + \kappa \left[\hat{\xi}_{j,s,t} + (\sigma - 1)\,\hat{a}_{j,s,t}\right], \tag{18}$$

where $\kappa \equiv \frac{\nu}{(1-\gamma)\nu+\sigma(1+\gamma\nu)} \subseteq [0,1]$.

In steady-state, the fraction $\zeta_{s,t}$ diverted to households equals zero at all financial firms, and $r_{s,t} = \bar{r} = \frac{\delta}{1-\delta}$. A "financial crisis" occurs at time t_0, resulting in $\zeta_{s,t} \geq 0\ \forall s$. Let $m_{s,t} = \chi\hat{r}_{s,t}$ denote an observed variable, with χ an unknown scalar.

Grouping terms in (18) yields an estimation equation corresponding to the regression specification in chapter 2:

$$\hat{l}_{j,s,t} = \beta_0 + \beta_1 m_{s,t} + \epsilon_{j,s,t}, \tag{19}$$

where:

$\beta_0 = \alpha_1\hat{C}_t - \alpha_2\left[\hat{w}_t - \hat{P}_t\right]$ depends on aggregate output and the real wage, with $\alpha_1 = \kappa\left[1 + \frac{\sigma}{\nu(1-\gamma)}\right] > 0$ and $\alpha_2 = \kappa\sigma > 0$ the semi-elasticities;

$\beta_1 = -\frac{\kappa\sigma}{\chi}$;

$\epsilon_{j,s,t} = \kappa\left[\hat{\xi}_{j,s,t} + (\sigma-1)\,\hat{a}_{j,s,t}\right]$ is a composite of the firm-level idiosyncratic shocks and has mean zero by assumption.

Consider first the partial equilibrium exercise of cumulating the employment shortfall relative to firms that borrow from the bank on island 1. Using the approximation that the level employment deviation $l_{j,s,t} - \bar{l} \approxeq \hat{l}_{j,s,t} * \bar{l}$, where $\bar{l}$ is steady-state employment,

$$\begin{aligned} Shortfall^{PE} &= \bar{l}\int_0^1 \hat{l}_{j,1,t}dj - \bar{l}\int_0^1\int_0^1 \hat{l}_{j,s,t}djds \\ &= \bar{l}\int_0^1 \beta_1\left(m_{1,t} - m_{s,t}\right)ds > 0. \end{aligned} \tag{20}$$

This is the model counterpart to the aggregation exercise implemented in chapter 2. Note that while employment falls as a result of the financial crisis, $Shortfall^{PE}$ is defined to be positive.

Next consider the general equilibrium exercise of cumulating the employment shortfall relative to if all firms could borrow at the steady-state intraperiod lending rate $\bar{r}$. Further assume that the bank on island 1 provides credit lines at cost $\bar{r}$, so that $m_{1,t} = 0$. Then:

$$\begin{aligned} Shortfall^{GE} &= -\bar{l}\int_0^1\int_0^1 \hat{l}_{j,s,t}djds \\ &= Shortfall^{PE} - \bar{l}\beta_0 \\ &= Shortfall^{PE} - \bar{l}\left(\hat{l}_{1,t}\right), \end{aligned} \tag{21}$$

where $\hat{l}_{1,t}$ is the average employment deviation at firms on island 1. Equation (21) states that the difference between the partial equilibrium exercise and the general equilibrium deviation of aggregate employment from an economy where all firms can borrow at $\bar{r}$ is given by the response of employment at unconstrained firms.

Finally, define the percent difference between the partial equilibrium and the general equilibrium employment losses as

$$\begin{aligned} Difference &\equiv -\frac{Shortfall^{GE} - Shortfall^{PE}}{Shortfall^{PE}} \\ &= -\frac{\beta_0}{\beta_1 \tilde{m}_t}, \end{aligned} \tag{22}$$

where $\tilde{m}_t = \int_0^1 m_{s,t} ds$ denotes the average level of bank health. As constructed, a negative value of $Difference$ means that the decline in employment in general equilibrium exceeds the partial equilibrium decline.

The sign of $Difference$ depends on the average deviation of employment at unconstrained firms, β_0, since $\beta_1 \tilde{m}_t = -\kappa\sigma\bar{r} < 0$. The first term of β_0, $\alpha_1 \hat{C}_t < 0$, constitutes the aggregate demand channel, whereby the decline in aggregate consumption in response to the financial shock lowers demand at unconstrained firms. The second term, $\alpha_2 \left[\hat{w}_t - \hat{P}_t\right]$, reflects the movement of workers from constrained to unconstrained firms. Writing $\left[\hat{w}_t - \hat{P}_t\right] = \left[\left(\hat{p}_{1,t} - \hat{P}_t\right) + (\hat{w}_t - \hat{p}_{1,t})\right]$, the reallocation of workers has two components. The first, $\hat{p}_{1,t} - \hat{P}_t < 0$, is the change in the (average) relative price at unconstrained firms. The fall in the relative price shifts product demand to unconstrained firms. The second term, $\hat{w}_t - \hat{p}_{1,t} < 0$, is the change in the economy-wide average wage deflated by the product price at unconstrained firms. The fall in the cost of labor relative to the price of their output induces unconstrained firms to move down their labor demand curves. The elasticity α_2 increases in the substitutibility of the goods produced by the constrained and unconstrained firms (σ), but decreases in the degree of frictions to workers' switching firms ($\frac{1}{\nu}$).[5]

It is worth emphasizing that the regression equation (19) and the correction (22) apply to more general settings. Sections 4 and 5 offer two examples, deriving the corresponding expressions in models allowing for sticky prices and where some firms completely lose access to credit markets, respectively. The general equilibrium logic does not depend on the particulars of the modeling assumptions.

[5] In particular, $\frac{\partial \kappa\sigma}{\partial \sigma} = [1-\gamma]\,\kappa^2 > 0$, and $\frac{\partial \kappa\sigma}{\partial \frac{1}{\nu}} = -\sigma^2\kappa^2 < 0$.

2.6. Solution

The parsimony of the model setup allows for a closed-form solution of β_0 in terms of the model's primitives. Letting $\tilde{r}_t \equiv \int_0^1 \hat{r}_{s,t} ds$ denote the average percent increase in the cost of a credit line, section 3 proves

$$\beta_0 = \kappa \left[\sigma - \Upsilon^{-1}\right] \tilde{r}_t, \tag{23}$$

where Υ denotes the real rigidity in the model, defined formally as in Ball and Romer (1990) as the elasticity of a firm's optimal relative price with respect to changes in aggregate expenditure. Section (3) provides an expression for Υ in terms of primitive parameters of the model. The sign of β_0 thus depends on the relative magnitude of the elasticity of substitution across goods and the model's real rigidity.

Labor demand at unconstrained firms depends on the economy's real rigidity for the same reason that real rigidity increases the response of output to monetary shocks in sticky price models. With large real rigidity, that is, Υ small, firms resist changes in their relative prices in response to changes in aggregate conditions. In a sticky price monetary model, this resistance leads to smaller individual price changes, lower inflation, and higher output following a monetary shock. In the present environment, greater real rigidity implies that the optimal relative price at unconstrained firms falls by less in response to a given decline in aggregate demand. This has two effects. First, product demand and employment shift to unconstrained firms only to the extent that they actually lower their relative prices. Second, in equilibrium prices at unconstrained firms must fall relative to prices at constrained firms experiencing higher marginal cost. It follows that with large real rigidity, output must contract even more to induce the unconstrained firms to lower their relative prices, in which case the general equilibrium employment decline exceeds the partial equilibrium decline.

2.7. Calibration

I consider calibrations of β_0 for the case of preferences separable over consumption and labor input (SEP) and allowing for complementarity of the form suggested by Greenwood, Hercowitz and Huffman (1988) (GHH):

$$u\left(C,L\right) = \begin{cases} \frac{\left[C-\phi\left(\frac{\varepsilon}{1+\varepsilon}\right)L^{1+\frac{1}{\varepsilon}}\right]^{1-\frac{1}{\theta}}}{1-\frac{1}{\theta}} & \text{GHH} \\ \frac{C^{1-\frac{1}{\rho}}}{1-\frac{1}{\rho}} - \frac{L^{1+\frac{1}{\varepsilon}}}{1+\frac{1}{\varepsilon}} & \text{SEP.} \end{cases}$$

One can show that with these sets of preferences,

$$\beta_0^{GHH} = \left[\kappa\sigma - \left(\gamma + \frac{1}{\varepsilon}\right)^{-1}\right]\tilde{r}_t, \tag{24}$$

$$\beta_0^{SEP} = \left[\kappa\sigma - \left(\gamma + [1-\gamma]\frac{1}{\rho} + \frac{1}{\varepsilon}\right)^{-1}\right]\tilde{r}_t. \tag{25}$$

The choice of parameters reflects a compromise between elasticities that match microeconomic studies and those found necessary to generate plausible macroeconomic fluctuations. In all rows I set the elasticity of substitution across goods σ to 6.5, the elasticity of output to labor $1-\gamma$ to $2/3$, and the Frisch elasticity of labor supply ε to 2. The values of σ and γ are relatively uncontroversial. Hall (2009) argues that a Frisch elasticity of 2 best captures the slope of the aggregate labor supply curve in a model without explicit treatment of the extensive margin, and is consistent with an elasticity along the intensive margin of hours per person of about 0.7. For the case of separable preferences, I report results for an intertemporal elasticity of substitution ρ of 1 and 3, with $\rho = 3$ the preferred parametrization. Rotemberg and Woodford (1997) and Woodford (2003) argue forcefully that in calibrating models where all output takes the form of nondurable consumption goods, one should nonetheless use an intertemporal elasticity of substitution higher than that typically estimated for nondurable consumption to compensate for the absence of the more interest-elastic categories of consumer durables and investment. The value of 3 is still only half of what Rotemberg and Woodford estimate to match U.S. business cycle fluctuations, and not far above recent estimates by Gruber (2006) and Nakamura et al. (2011).

The only parameter not commonly found in macroeconomic models is ν. From a microeconomic perspective, ν is the elasticity of labor supply to an individual firm. Ashenfelter, Farber and Ransom (2010) and Manning (2011) provide recent surveys of studies that estimate this elasticity. Many of the the studies find a very low elasticity, between 0.1 and 2 in the short-run and between 2 and 4 in the long-run. Webber (2011) conducts an analysis of the universe of U.S. firms and finds an average elasticity of 1.08, with 90 percent of firms exhibiting an elasticity below 1.75. From a macroeconomic perspective, ν governs the degree of strategic complementarity in price-setting across firms through its influence on κ. Woodford (2003) advocates for a value of Υ of between 0.1 and 0.15, the latter which implies a value of ν between 1 and 2.5. Accordingly, I report results for $\nu = 1, 2, 3$.[6] For comparison,

[6] Manning (2011) raises a number of concerns with the empirical methodologies that may bias the microeconomic estimates down. Nonetheless and as discussed further below, the model abstracts from a number of features that would increase real rigidity, suggesting that estimates at the low end of the microeconomic literature may still be appropriate for macroeconomic purposes.

I also report the case of $\nu = 1000$, corresponding to the frictionless labor market.

Table 1: Calibrated percent difference between partial equilibrium and general equilibrium effects

ρ	ν	GHH	SEP
1	1	−0.72	0.04
1	2	−0.12	0.38
1	3	0.08	0.49
1	1000	0.48	0.71
3	1	−0.72	−0.36
3	2	−0.12	0.11
3	3	0.08	0.27
3	1000	0.48	0.59

Notes: Each cell of the table reports the percent difference between the employment change in partial equilibrium and general equilibrium, given by the formula $-\frac{\beta_0}{\beta_1 \bar{m}_t}$, for the values of the cross-sectional labor supply elasticity ν and intertemporal elasticity of substitution ρ reported in the first two columns. A negative entry indicates that the general equilibrium employment decline exceeds the partial equilibrium decline. The column labeled GHH reports values using the Greenwood, Hercowitz Huffman (1988) preference structure. The column labeled SEP reports values for preferences separable over consumption and labor input. In all cells, $\sigma = 6.5$ is the elasticity of substitution across goods, $\gamma = 1/3$ is one minus the elasticity of output with respect to labor, and $\varepsilon = 2$ is the Frisch elasticity of labor supply.

The table indicates that for the preferred set of parameter values, with $\rho = 3$ and $\nu \in \{1, 2, 3\}$, the model's general equilibrium channels either amplify the partial equilibrium employment decline or have a modest attenuating effect. Consumption-hours complementarity magnifies the fall in aggregate demand, so that for a given set of parameter values GHH preferences always produce lower employment at unconstrained firms than separable preferences (recall that a negative entry in the table indicates that the general equilibrium employment decline exceeds the partial equilibrium decline). With $\nu = 1$, the partial equilibrium decline understates the general equilibrium decline under both preference specifications. With $\nu = 2$, corresponding to the rough midpoint of microeconomic estimates, the general equilibrium decline is within 12 percent of the partial equilibrium decline. Finally, a value of $\nu = 3$ implies a small overestimate of the aggregate effects in partial equilibrium, with the adjustment between 8 and 27 percent depending on the preference structure.

The calibration with separable preferences has some sensitivity to the intertemporal elasticity ρ.[7] Many macroeconomic papers set $\rho = 1$, consistent with a balanced growth path. In that case, and with separable preferences and ν at the upper limit of the preferred range, the partial equilibrium decline exceeds the general equilibrium decline by 49 percent. However, with $\rho = 1$ and no other frictions, the model requires extremely large shocks (from

[7] The calibration with GHH preferences is invariant to ρ because of the absence of wealth effects in the labor supply decision.

any source) to cause a large movement in output.[8] The smaller general equilibrium decline then occurs because the financial shock generates an implausibly small fall in aggregate output as households smooth consumption. It is in this sense that Rotemberg and Woodford (1997) argue that a model without investment or durable goods needs a higher value of ρ to generate realistic fluctuations in economic activity (see also Barsky, House and Kimball (2007)). Furthermore, it may be appropriate to assume less consumption smoothing in an environment where credit frictions have increased. Finally, a smaller ρ does not necessarily imply a large difference between partial and general equilibrium; with $\rho = \nu = 1$, the difference is 4 percent.

On the other hand, adding a number of features to the model specification would further push the general equilibrium channels toward amplifying the direct effects. For example, variable elasticity preferences that contain positive comovement between a firm's market share and its optimal markup would increase real rigidity and lower employment at unconstrained firms (Kimball 1995).[9] Likewise, accounting for the input-output structure of the economy would lower employment demand if labor and intermediate inputs are not substitutes in production (Basu 1995). Intuitively, if unconstrained firms use the output of constrained firms in their production process, then their marginal cost also rises as a result of the financial shock. Real wage rigidity reduces the incentive for unconstrained firms to poach labor from constrained firms, which in the extreme case results in a decline in employment at unconstrained firms for any calibration or preference specification.[10] Finally, the relative price component of the reallocation channel would diminish to the extent that constrained firms dis-hoard labor rather than reduce output.

In sum, the partial equilibrium effects in chapter 2 do not appear to substantially overstate the importance of the credit supply channel in general equilibrium.

[8] With $\rho = 1$ and separable preferences, output falls by 0.44 percent in response to a uniform 1 percent increase in the cost of a credit line. For comparison, with $\rho = 1$ a 1 percent fall in aggregate technology corresponds to a fall in output of 0.38 percent.

[9] Formally, the model with variable elasticity of demand behaves exactly as that described above, except that κ generalizes to $\kappa = \frac{\nu}{\nu(1-\gamma)(1+\sigma\epsilon_{\mathcal{M},c})+\sigma(1+\gamma\nu)}$, where σ now denotes the elasticity of demand in the symmetric steady-state and $\epsilon_{\mathcal{M},c} \geq 0$ denotes the elasticity of the markup with respect to market share at the symmetric steady-state.

[10] With a fixed real wage and labor demand-determined, the reallocation channel shuts down completely leaving only the aggregate demand channel to act in general equilibrium. The formal result follows from replacing the labor-consumption tradeoff (7) with the assumption $\frac{w_t}{P_t} = \omega_0$ in section 4 and taking the limit of the solution as $\alpha \to 0$.

3. Solution of the model

Log-linearizing equations (7) and (8) gives:

$$\hat{w}_{j,s,t} - \hat{P}_t = \left[\frac{1}{\eta} - \frac{1}{\nu} - \omega_{CL}\right]\hat{L}_t + \left[\frac{1}{\nu}\right]\hat{L}_{j,s,t} + \left[\frac{1}{\rho} - \omega_{LC}\right]\hat{C}_t, \tag{26}$$

$$-\frac{1}{\rho}\hat{C}_t + \omega_{CL}\hat{L}_t = E_t\left[\hat{i}_t - \pi_{t+1} - \frac{1}{\rho}\hat{C}_{t+1} + \omega_{CL}\hat{L}_{t+1}\right], \tag{27}$$

where:

$\rho = -\left(C\frac{u_{CC}}{u_C}\right)^{-1}$; $\eta = \left(L\frac{u_{LL}}{u_L}\right)^{-1}$; $\omega_{LC} = -C\frac{u_{LC}}{u_L}$; $\omega_{CL} = L\frac{u_{CL}}{u_C}$; and $\pi_t = \hat{P}_t - \hat{P}_{t-1}$. In addition, an aggregate version of (26) obtains:

$$\hat{w}_t - \hat{P}_t = \left[\frac{1}{\eta} - \omega_{CL}\right]\hat{L}_t + \left[\frac{1}{\rho} - \omega_{LC}\right]\hat{C}_t. \tag{28}$$

Substituting (28) into the firm labor equilibrium condition production (18), using the production function (10), integrating, and solving gives an expression for the quantity of labor in terms of model primitives and the exogenous shocks:

$$\hat{L}_t = -\left[\gamma + (1-\gamma)\left(\frac{1}{\rho} - \omega_{LC}\right) + \left(\frac{1}{\eta} - \omega_{CL}\right)\right]^{-1}\tilde{r}_t. \tag{29}$$

From equation (29), the aggregate labor response does not depend on the mobility of labor across firms, governed by ν. This independence is an artifact of using first-order methods to solve the model. At the first order, only the average marginal cost increase matters to aggregate variables, and in particular the dispersion of the cost shock does not affect aggregate quantities. Higher order terms of L_t include higher moments of the distribution of $r_{s,t}$ and do depend on ν.

The fact that Υ measures the model's real rigidity follows from log-linearizing (12) to write the firm's optimal relative price as

$$\begin{aligned}\hat{p}_{j,s,t} - \hat{P}_t &= \left[\hat{w}_{j,s,t} - \hat{P}_t\right] + \gamma\hat{l}_{j,s,t} + \hat{r}_{s,t} - \hat{a}_{j,s,t} \\ &= \left[\gamma + \frac{1}{\nu}\right]\hat{L}_{j,s,t} + \left[\frac{1}{\eta} - \frac{1}{\nu} - \omega_{CL}\right]\hat{L}_t + \left[\frac{1}{\rho} - \omega_{LC}\right]\hat{C}_t,\end{aligned}$$

where the second line substitutes the labor supply relationship (26) and drops the shock terms $\hat{r}_{s,t} - \hat{a}_{j,s,t}$ which have no effect on the subsequent calculations. Replacing $\hat{L}_{j,s,t}$ with the equilibrium employment level and rearranging terms gives

$$\hat{p}_{j,s,t} - \hat{P}_t = \kappa \left\{ \gamma + \left[(1-\gamma)\left(\frac{1}{\rho} - \omega_{LC}\right) + \left(\frac{1}{\eta} - \omega_{CL}\right) \right] \right\} \hat{C}_t.$$

By definition, $\Upsilon \equiv \kappa \left\{ \gamma + \left[(1-\gamma)\left(\frac{1}{\rho} - \omega_{LC}\right) + \left(\frac{1}{\eta} - \omega_{CL}\right) \right] \right\}$ measures the elasticity of the optimal relative price with respect to aggregate demand.

Using labor-consumption tradeoff (28), the aggregate relationship $\hat{C}_t = [1-\gamma]\,\hat{L}_t$, and equation (29) in the definition of β_0, rearranging, and using the definition of Υ gives the expression in equation (23) in the text.

4. Model with sticky prices

In the sticky price model, a firm can only change its price with Calvo probability $(1-\alpha)$ each period. Firms discount profits using the household's stochastic discount factor. Assume that the financing shock disappears with probability μ each period. The central bank's reaction function takes the form

$$\hat{i}_t = \phi_\pi \pi_t + \phi_Y \hat{Y}_t. \tag{30}$$

Note that $\hat{Y}_t$ and $\hat{i}_t$ in (30) measure deviations from the no financial crisis state, rather than from the natural rate of output and interest that would adjust for the cost-push shock.

The New Keynesian Phillips curve and IS curve complete the model:

$$\pi_t = \lambda\theta E_t \sum_{\tau=t}^{\infty} \beta^{\tau-t}\hat{Y}_\tau + \frac{\lambda}{1-\beta\mu}\tilde{r}_t \tag{31}$$

$$\left[1 - \frac{\rho\omega_{CL}}{1-\gamma}\right]\hat{Y}_t = \left[1 - \frac{\rho\omega_{CL}}{1-\gamma}\right] E_t\left[\hat{Y}_{t+1}\right] - \rho E_t\left[\hat{i}_t - \pi_{t+1}\right], \tag{32}$$

where $\lambda \equiv \frac{(1-\alpha)(1-\alpha\beta)(1-\gamma)\kappa}{\alpha} > 0$, and $\theta \equiv \left[\frac{1}{\rho} - \omega_{LC} + \frac{\gamma + \frac{1}{\eta} - \omega_{CL}}{1-\gamma}\right]$ is the elasticity of average real marginal cost (net of the financial shock) with respect to output.

The solution is via the method of undetermined coefficients, with

$$\begin{aligned} \hat{Y}_t &= \Theta_Y \tilde{r}_t \\ \pi_t &= \Theta_\pi \tilde{r}_t \\ \hat{i}_t &= \Theta_i \tilde{r}_t. \end{aligned}$$

Solving,

$$\Theta_Y = \frac{\frac{\rho\lambda[\mu-\phi_\pi]}{1-\beta\mu}}{[1-\mu]\left[1-\frac{\rho\omega_{CL}}{1-\gamma}\right]-\frac{\rho\lambda\theta[\mu-\phi_\pi]}{1-\beta\mu}+\rho\phi_Y} \tag{33}$$

$$\Theta_i = \phi_\pi\left[\frac{\lambda\theta\Theta_Y+\lambda}{1-\beta\mu}\right]+\phi_Y\Theta_Y \tag{34}$$

$$\Theta_\pi = \frac{\lambda\theta\Theta_Y+\lambda}{1-\beta\mu}. \tag{35}$$

Labor demand remains governed by equation (13), which in log deviation form implies

$$\hat{l}_{j,s,t} = [1-\gamma]^{-1}\Theta_Y\tilde{r}_t - \sigma[1-\gamma]^{-1}\left[\hat{p}_{j,s,t}-\hat{P}_t\right]+[1-\gamma]^{-1}\left[\hat{\xi}_{j,s,t}-\hat{a}_{j,s,t}\right]. \tag{36}$$

To characterize the regression model, it remains to specify $\left[\hat{p}_{j,s,t}-\hat{P}_t\right]$. Profit maximization implies that a firm resetting its price in period $T \geq t_0$ during which the financial shock continues to bind sets a relative price (ignoring the idiosyncratic shock terms) of

$$\hat{p}^{\star}_{j,s,T}-\hat{P}_T = \frac{\alpha\lambda\theta\Theta_Y}{[1-\alpha][1-\alpha\beta\mu]}\tilde{r}_T+\frac{\alpha\beta\mu}{1-\alpha\beta\mu}\Theta_\pi\tilde{r}_T+\frac{\alpha\lambda}{[1-\alpha][1-\alpha\beta\mu]}\hat{r}_{s,T}. \tag{37}$$

It simplifies the subsequent algebra to set $T = t_0$, so that a measure α of firms have prices set before the financial shock hits and the remaining firms all reset their price in the current period anticipating that the financial crisis will persist with probability μ. In that case, the relative price of a firm not resetting its price, again ignoring the idiosyncratic terms, is

$$\begin{aligned}\hat{p}^{no\,reset}_{j,s,t}-\hat{P}_t &= -\pi_t \\ &= -\Theta_\pi\tilde{r}_t.\end{aligned} \tag{38}$$

Using (37) and (38) in (36), taking a conditional expectation over $\hat{r}_{s,t}$, and simplifying terms yields

$$E\left[\hat{l}_{j,s,t}|\hat{r}_{s,t}\right] = \beta_0^{sticky}+\beta_1^{sticky}m_{s,t}, \tag{39}$$

where $\beta_0^{sticky} = \frac{1}{1-\gamma}\left[\Theta_Y+\frac{\alpha\sigma\lambda}{1-\alpha\beta\mu}\right]\tilde{r}_t$ and $\beta_1^{sticky} = -\frac{\alpha\sigma\lambda}{[1-\gamma][1-\alpha\beta\mu]\chi}$. By the Gauss-Markov theorem, an OLS regression of $\hat{l}_{j,s,t}$ on $m_{s,t}$ provides the best linear unbiased estimator of $\hat{l}_{j,s,t}|\hat{r}_{s,t}$, implying that β_0^{sticky} and β_1^{sticky} give expressions for the OLS intercept and slope coefficient.

Equation (22) for the difference between the employment effects in partial and general equilibrium continues to hold in this model. Accordingly,

$$Difference^{sticky} = 1 - \left[\frac{\alpha\beta\mu - 1}{\alpha\sigma\lambda}\right]\Theta_Y. \tag{40}$$

5. Model with some firms excluded from credit markets

This section describes a model that relaxes the assumption that all firms borrow during the financial crisis. To capture that outcome, the model departs from the assumptions of firm exit and credit line requirements used in the text. Instead, a fraction $1-\zeta_{s,t}$ of firms on island s have access to credit markets and can set their payroll at an optimal level. Firms that cannot borrow set their employment to l_j^c, with the superscript indicating "constrained" and the distribution of l_j^c exogenously given. At the firm level, the option to borrow takes the form of a Bernoulli random variable independent of other firm-level idiosyncratic shocks and the constrained employment level. It simplifies the notation without detracting from the argument to assume that firms that borrow face an interest rate of zero and that there is a common wage across firms.

Let $p^\star_{j,s,t}$ denote the price of an unconstrained firm:

$$p^\star_{j,s,t} \;\; = \;\; \mathcal{M}\frac{w_t}{a_{j,s,t}}. \tag{41}$$

Labor demand at unconstrained firms satisfies:

$$l^\star_{j,s,t} \;\; = \;\; \left[\frac{\xi_{j,s,t}}{a_{j,s,t}}\left(\frac{\mathcal{M}\frac{w_t}{(1-\gamma)a_{j,s,t}}}{P_t}\right)^{-\sigma} C_t\right]^{\frac{1}{1+\gamma(\sigma-1)}}. \tag{42}$$

Let $\bar{l}_{t-1}$ denote the employment level at all firms in the $t-1$ steady-state, and $\hat{l}_{j,s,t} = \frac{l_{j,s,t}}{\bar{l}_{t-1}} - 1$ the growth rate of employment. Then taking a conditional expectation of the growth rate over the health measure $\zeta_{s,t}$ yields

$$E\left[\hat{l}_{j,s,t}|\zeta_{s,t}\right] = \bar{\hat{l}}^\star_t + \left[\bar{\hat{l}}^c_t - \bar{\hat{l}}^\star_t\right]\zeta_{s,t}, \tag{43}$$

where $\bar{\hat{x}}_t$ denotes the time t unconditional average value of $\hat{x}_{j,s,t}$. By the Gauss-Markov theorem, (43) implies that a regression of $\hat{l}_{j,s,t}$ on $\zeta_{j,s,t}$ will have an intercept $\beta_0 = \bar{\hat{l}}^\star_t$ and a slope coefficient $\beta_1 = \left[\bar{\hat{l}}^c_t - \bar{\hat{l}}^\star_t\right]$.

The partial equilibrium employment shortfall is:

$$\begin{aligned} Shortfall^{PE} &= \bar{l}_{t-1}\left[\bar{\hat{l}}^{c}_{t} - \bar{\hat{l}}^{\star}_{t}\right]\int_{0}^{1}\zeta_{s,t}ds \\ &= \beta_1 \bar{l}_{t-1}\int_{0}^{1}\zeta_{s,t}ds. \end{aligned} \tag{44}$$

The partial equilibrium employment gap is the difference between average employment at constrained and unconstrained firms, multiplied by the share of firms in the economy that are constrained.

The difference between the partial and general equilibrium effects is

$$Difference = -\frac{\beta_0}{\beta_1 \int_0^1 \zeta_{s,t}ds}. \tag{45}$$

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