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State-run Banks, Money Growth, and the Real Economy

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Abstract

Within countries, individual state-run banks' lending correlates with prior money growth, while otherwise similar private-sector banks' lending does not. Aggregate credit and investment growth correlate with prior money growth more strongly in economies whose banking systems are more fully state-run. Size and liquidity differences between state-run and private-sector banks do not drive these results, and further tests discount broad classes of alternative causality scenarios. Tests exploiting heterogeneity in likely political pressure on state-run banks associated with e.g. central bank independence, privatizations, and election years are consistent with a command-and-control channel of pseudo-monetary policy operating via state-run banks.

KEYWORDS: ownership structure, loan growth, capital spending, monetary stimulus, banking.

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1. Introduction

Until recently, macroeconomics impugned money growth as a policy variable for stimulating economic growth, as opposed to inflation targeting (Goodfriend 2007; Mishkin 2011). Nonetheless, policy-makers often countered economic downturns by increasing monetary growth to spur the real economy, at least in the short-run, in part by stimulating bank lending and thereby aggregate investment both before (Rasche and Williams 2007) and especially after the 2008 crisis (Mishkin 2009, 2011; Caballero 2010; Claessens *et al.* 2010; Bernanke 2012). However, a bank's responses to money growth depend on agents' expectations about ensuing real effects, constraints and incentives.¹ This study differs from previous work by building on established findings that political pressure, not profit maximization, drives state-run bank lending (La Porta *et al.* 2002, 2003; Sapienza 2004; Berger *et al.* 2005; Dinc 2005; Carvalho 2014; Coleman and Feler 2015). We extend this work by presenting evidence that state-run banks boost lending subsequent to increased money growth, while otherwise similar private-sector banks do not; that political pressure plays a key role in state-run banks' lending in these circumstances; and that their lending is accompanied by increased capital formation growth in the short run.

In economy-level panel data for 2001 through 2010, we find increased money growth presages no significant change in bank lending or investment growth in economies whose banking systems contain no large state-run banks. In contrast, faster money growth presages statistically and economically significantly larger increases in lending and investment in economies whose banking systems are more comprised of state-run banks. Identification follows from a combination of techniques.

As a first pass, we use identification-by-disaggregation (Kashyap and Stein 2000) – that is, we

¹ Fischer (1988) and Mishkin (1996) review this literature, which is too vast to summarize here. Caballero (2010) offers a critical perspective.

test firm-level data for patterns inconsistent with aggregate-level alternative causality scenarios. *Bank-level* difference-in-difference regressions using a panel comprised of the largest banks in 40 economies from 2001 to 2010, clustered by economy, show increases in money growth presaging increases in lending growth by individual state-run banks, but not by individual private-sector banks. These micro-level findings eliminate many macro-level reverse causality scenarios, such as those in which credit demand in general affects bank lending in general. The feasible set of alternative causality scenarios is narrowed to those in which individual state-run banks' lending tracks money growth, but lending by individual private-sector banks in the same economies at the same time does not.

Including fixed-effects and bank characteristics in these bank-level tests further narrows this feasible set. The above bank-level difference-in-difference findings remain after including bank and economy-year fixed-effects. Bank fixed-effects subsume all *time-invariant* bank-level and economy-level omitted variables, rebutting alternative causality scenarios turning on time-invariant latent factors such as individual banks' geographic client bases and historically predetermined variables, such as countries' legal origins. Economy-year fixed-effects subsume all time-varying economy-level omitted variables *and* their interactions with money growth and with the economy-level importance of state-run banks, both of which are also time-varying economy-level variables. These findings therefore rebut alternative explanations turning on state-run banks' lending varying as exchange rates, business cycle measures (Coleman and Feler, 2015), or any other time-varying economy-level latent factors.

Additional bank-level difference-in-difference tests letting lending by banks of different size and liquidity differentially track money growth (Kashyap and Stein 2000), leave state-run bank's' lending still significantly tracking money growth, and lending by private-sector banks of comparable size and liquidity not doing so. The set of economy-level feasible alternative causality scenarios is thus restricted to those with scope for individual state-run banks' lending to track money growth while lending by private-sector banks of similar size and liquidity in the same countries at the same time does not.

A second battery of identification tests directly explores the more prominent of these scenarios. Money growth might accompany or presage currency depreciations, and state-run banks might disproportionately lend to finance exports. If so, their lending might, without political pressure, increase more than that by private-sector banks subsequent to money growth increases. Or, money growth might accompany or presage fiscal expansion, and state-run banks might disproportionately lend to firms set to benefit from increased government spending, such as infrastructure construction firms. If so, state-run banks' lending might likewise increase more than lending by private-sector banks subsequent to an increase in money growth. Or, money growth might vary with the business cycle, which might also drive state bank lending (Bertay *et al.*, 2015; Coleman and Feler 2015). For example, if state-run banks' borrowers were differentially sensitive to booms and recessions, their lending might again seemingly track money growth without political pressure. In each of these alternative causality scenarios, money growth proxies for another variable, so its significance should fade once that variable enters directly. However, neither currency depreciation nor fiscal expansion, nor business cycle measures significantly presages lending growth by state-run banks but not by otherwise similar private-sector banks in the same country the same year. Only prior money growth does so. Absence of evidence cannot affirm a negative, but these tests provide circumstantial evidence that lets Ockham's razor further pare away the set of likely feasible alternatives.

A third battery of identification tests explores the extent to which the difference between state-run and otherwise similar private-sector banks' lending growth subsequent to increased money growth varies with measures of likely political pressure on state-run banks. One set of tests shows money growth ceasing to presage individual state-run banks' lending immediately after their privatizations, consistent with the link between state-run banks' lending and prior money growth vanishing after privatizations shield their lending from political pressure (Megginson 2005). Another set of difference-in-difference tests exploits previous work showing that state-run banks' lending becomes especially driven

by political pressure prior to elections (Nordhaus 1975; Alesina *et al.* 1997; Dinc 2005; Micco *et al.* 2007). These tests show that money growth presages state-run banks' lending significantly more strongly in years immediately prior to free elections than in other years, but private-sector bank lending shows no such pattern.

Further tests show that money growth presages state-run banks' lending growth more strongly in economies whose central banks are less independent (Crowe and Meade 2008). No such pattern is evident for lending by private-sector banks. This difference is consistent with politicized monetary policy facilitating more politicized lending by state-run banks, but not by private-sector banks; but weighs against alternative causality scenarios turning on state-run banks or their clients being simply being more sensitive to monetary policy. If that were so, the independence of the central bank conducting the monetary policy would not matter. Additional tests show money growth presaging growth in lending by state-run banks significantly more strongly in economies whose civil servants are more effective and more sensitive to political pressure. These findings reconcile seemingly discordant findings about state-run banks' lending amid downturns in different single country studies (Das *et al.* 2015; Deng *et al.* 2014).

Still other tests reveal state-run banks to be a critical cog in whatever political economy mechanism underlies the above findings. For example, the political pressure might be direct: governments pressing state-run banks to lend more when money growth makes this possible. Alternatively, the political pressure might be indirect: an interventionist state might press nonfinancial state-owned enterprises or politically connected firms to borrow more, and might incidentally also keep a large stable of state-run banks to supply them with credit. We find that measures of the general scope of state intervention do not explain variation in the correlation of prior money growth with aggregate lending or investment, while state control over the banking sector does so robustly. Still, we remain catholic as to the exact mechanism through which political influence affects state-run bank lending, though our findings affirm that state-run banks are a critical cog in it. We recognize that the command

and control mechanism our findings illuminate is beyond the traditional rubric of monetary policy, and tentatively suggest *pseudo-monetary policy* as more apt. We welcome further research into these issues and invite alternative explanations of our findings that also accord with our extensive battery of identification tests.

Although each individual identification test can be criticized, their combination makes a strong case for a politically-driven state-run bank channel through which policy-makers can influence aggregate credit and investment in the near term. Indeed, this channel may well be of paramount economic significance in that money growth presages growth in lending by state-run banks only: no such effect is evident in private-sector bank lending. These differences are economically significant. A one percentage point boost to money growth over the prior year presages a 0.26 percentage point increase in lending growth by a state-run bank, but no change in lending by a comparable private-sector bank. The same boost to money growth presages a 0.23 percentage point boost to aggregate bank lending and nearly a 0.8 percentage point boost to capital spending in a country whose banking system is fully state-run, but no change to either in an economy whose large banks are all private-sector firms.

Our results also survive a battery of standard robustness tests. Using drops in key policy interest rates as an alternative monetary stimulus measure generates results comparable to those in the tables, as do tests using alternative measures of loan growth. We also rule out several confounding factors by repeating our tests including or excluding foreign-controlled and global banks (Cetorelli and Goldberg 2012), distinguishing private-sector banks with versus without controlling shareholders (Caprio *et al.* 2007), and using consolidated or unconsolidated balance sheets to measure bank lending growth (Campello, 2002). None of these exercises alters the results shown in the tables

We conclude that state-run banks can be a policy tool for reducing the substantial social costs of business cycles (Lucas 1987; Imrohorglu 2008; Coleman and Feler 2015). This is consistent with previous work finding attenuated business cycle volatility in countries where state-run banks are more

important (Morck *et al.* 2011). However, state-run banks also impose substantial capital misallocation costs (e.g. La Porta *et al.* 2002, 2003; Sapienza 2004; Berger *et al.* 2005; Dinc 2005; Deng *et al.* 2011; Morck *et al.* 2011; Carvalho 2014). Public policy makers may thus wish to weight the short-run social benefits of state-run banks against their long-run social costs.

2. Data

2.1 Defining State-control

Following La Porta *et al.* (2002), we identify each bank's *ultimate owners*, if any exist, each year as follows. First, a bank's *large shareholders* are defined as those with voting stakes of at least five percent. If a large shareholder is a biological person or state organ, the name is recorded. If a bank's large shareholder is a corporation, its large shareholders, its large shareholders' large shareholders, and so on are identified until we reach a biological person, state organ, or entity without a controlling shareholder. This exercise using voting stakes is necessary because banks can be controlled indirectly, through chains of holding companies or business group corporations, and through super-voting shares and other control enhancement devices. The voting stakes of all identified ultimate owners are aggregated at each level of the chain by assuming family members act in concert and state organs obey a single authority.

We define a bank's *ultimate controlling owner* as the ultimate owner whose combined voting stake is largest if that stake totals at least 10 percent. If the ultimate controlling owner is a state organ, the bank is classified as *state-run*. If the ultimate owner is not state-run or if there is no ultimate owner, the bank is classified as *private-sector*. In bank-level tests, our primary variable is a *state-run indicator*, $\delta_{i,t}$, set to one if bank i is state-run in year t and to zero otherwise. In economy-level tests, the bank governance importance variables weigh each bank in each category by lagged total net credit. Thus, $f_{j,t}$ measures the credit-weighted fraction of economy j 's banking system that is *state-run*, as opposed to

private-sector banks in year t using credit weights from year $t-1$.

2.2 Samples

Our bank-level sample begins with a 2001 cross-section of ultimate controlling shareholder classifications from Morck *et al.* (2011, Table 1).² We determine the ultimate ownership of these banks for each year from 2001 through 2010.³ The result is a bank-level annual panel of ultimate controlling owner identities and stakes spanning 44 countries. The data for each bank begin in the year its ownership is first available. Ownership data are available for 79% of the sample in 2001; and for the rest after 2001. To be in the sample, a bank must have comparable financial statements for two consecutive years, for reasons explained below.

Our basic sample merges the list of economies containing these banks with the IMF's International Financial Statistics (IFS), Government Financial Statistics (GFS), and World Economic Outlook (WEO) databases; the World Bank's World Development Indicators (WDI) database; and Thomson-Reuters DataStream. This sample has economy-level data on monetary base growth, gross fixed capital spending and other variables. Because of missing GFS data on monetary base growth, our basic sample falls to 40 economies. Because fixed capital spending data are available only for 30 countries and interest rates for only 38, smaller samples are used in tests involving these variables. Table 2 lists the countries in our basic sample, together with summary statistics for key variables.

² Morck *et al.* (2011) explain the construction of this initial cross-section in detail. They begin with a cross-section of data kindly provided by Caprio *et al.* (2007) for publicly listed banks, and expand this to include every country's ten largest banks, listed or unlisted, as ranked by 2001 assets in *The Banker* (2001), and whose ownership can be identified from BankScope or other sources.

³ This approach provides a stable panel, but omits new large banks and includes banks grown smaller over time. An alternative approach, using the largest few banks each year in each country, must contend with survival bias or banks entering the sample due, for example, to mergers.

2.3. Monetary Stimulus Variable

Broadly speaking, a monetary stimulus can be a regulatory change increasing banks' ability to lend, a market intervention that lowers key interest rates, or an increase in the money supply. We focus on the last because regulatory changes are infrequent and because neither regulatory changes nor interest rates are easily comparable across economies. Moreover, the importance of any given regulatory change depends on multiple regulatory, legal, and other economy-specific considerations. In contrast, monetary aggregates change continuously and are readily comparable across countries. We further narrow our attention to monetary base growth because, among available monetary aggregates, this has the least direct overlap with the banking sector's balance sheet and the most consistent definition across countries. Nonetheless, we revisit changes in interest rates as an alternative monetary stimulus variable in robustness checks below.⁴

Monthly monetary base growth is available for 40 countries in the IFS Database in the Central Bank Survey (IFS country tables, section 10, line 14). Monetary base is defined as currency in circulation (line 14a) plus central bank liabilities to other depository corporations (line 14c) plus central banks liabilities to other sectors (line 14d). For the bank-level regressions, *money growth* in economy j and year t ($\Delta M_{i,j,t}$) is defined over twelve-month intervals immediately prior to the beginning of bank i 's fiscal year as $\Delta M_{i,j,t} \equiv (M_{i,j,t} - M_{i,j,t-1})/M_{i,j,t-1}$.

Thus, although the growth rate in the monetary base is conceptually an economy-level variable, it can differ across banks in a given economy if their fiscal years differ. For example, a bank with a fiscal year beginning on January 1st has a prior money growth rate calculated from the end of December to the end of December. In contrast, a bank whose fiscal year begins in March 1st has its money growth rate

⁴ More radical monetary stimuli, such as regulatory changes and market interventions may well coincide with rapid money growth, so our results could possibly reflect, in part at least, other dimensions of monetary policy also acting disproportionately through state-run banks.

calculated from the end of February in the previous calendar year through the end of February in the current calendar year. In economy-level tests, $\Delta M_{j,t}$ is calendar-year 12-month money growth. Prior six-month money growth, defined over the second halves of each of these periods, is used as an alternative measure in robustness tests⁵. These variables are winsorized at 10% to limit the influence of outliers.

2.4 Outcome Variables

The outcome variables capture real growth in bank-level lending, economy-level lending, and economy-level fixed capital spending. These data are winsorized at 10% to limit outlier influence.

2.4.1 Bank-level loan growth

In bank-level regressions, the dependent variable is annual real growth in a bank's gross loans, calculated in local currency but including lending in all currencies, from BankScope, and is defined as $\Delta credit_{i,j,t+1} \equiv (credit_{i,j,t+1} - credit_{i,j,t})/credit_{i,j,t}$, where the subscripts i, j , and t index the bank, economy, and fiscal year, respectively. To measure *credit*, we use gross loan growth where available because this measure is not mechanically affected by changes in discretionary loan loss provisions.⁶ However, if gross loans are unavailable, net loans are used. Real values are calculated by deflating nominal values using the economy's CPI index.

BankScope sometimes provides multiple accounting statements for a bank in one year. For example, BankScope provides separate financial statements for Jyske Bank A/S (Group) and Jyske Bank

⁵ Seasonal adjustment is conventional where variables are constructed across disparate subsets of months. Where seasonally adjusted monetary base data are available from the IMF, these are used. Otherwise, we use five-year rolling regressions of money growth on month dummies to remove seasonal effects. The 12-month money growth rates, in contrast, largely avoid concerns about seasonality affecting the results.

⁶ Bushman and Williams (2012), (2015) argue that U.S GAAP and IFRS allow discretion in loan provisioning and discretionary loan provisioning practices, and that this varies across countries.

A/S in Denmark, but under the same *bvd* identifier number. To avoid artificially inflating the sample, only one financial statement is included each year for each *bvd* identifier number. For better comparability across countries, the following procedure is applied. First, consolidated statements are preferred over unconsolidated statements if both are available. This is because overall lending by a bank group is arguably more important to the economy as a whole than is lending by one of its member firms. Indeed, financial conglomerates, especially conglomerates with banks across countries, might respond to a monetary stimulus with internal capital market transactions that cancel out across the group as a whole (Campello, 2002). However, unconsolidated statements are used in robustness tests. Second, “audited” or “qualified” statements are preferred over “not audited” or “unqualified” statements if both are available. Finally, statements based on international accounting standards (codes IFRS, IFRS-NFC or IAS) are preferred over statements using local accounting systems (designated “local GAAP” or “regulatory”) if both are available.

Despite these filters, a few extreme real growth rates in loans remain. We identify some as resulting from bank mergers and acquisitions. In these cases, BankScope either discontinues data for one of the merged banks and continues data for the merged entity under the other’s identification code or discontinues data for both and starts recording data for a new bank. The former procedure can generate extreme loan growth rates. Spot checking the data reveals M&As responsible for most extreme observations. We therefore drop 39 bank-year observations with real annual gross loan growth outside plus or minus 50% in the main sample, but restore them for robustness tests.

2.4.2 Economy-level Aggregate Loan Growth

We have controlling shareholder data for the largest banks in each economy, and use these in our bank-level loan growth tests. While these banks are few in number in each country, this sample constitutes a large fraction of each economy’s banking sector (See Morck *et al.* (2011) for details). We therefore

anticipate that our bank-level results can provide useful insights into economy-level questions.

Economy-level gross lending is the change in “domestic credit provided by banking sector” from WDI dataset. The WDI provides domestic credit extended by the banking sector divided by GDP; so we obtain our variable by multiplying this ratio by GDP in current local currency. Each economy’s CPI index is then used to deflate these nominal aggregates. Finally, aggregate real annual loan growth, $\Delta credit_{j,t+1}$, is calculated for each economy-year observation j,t as in [2]. This is a broad measure of banking sector credit growth, which also includes lending by other banks and non-bank financial institutions.

2.4.3 Economy-level Fixed Capital Spending Growth

We look for the possible real effect of money growth in gross fixed capital spending growth. This is because aggregate investment is by far the most volatile and pro-cyclical component of aggregate demand, and plays a key role in the accelerator effect (Samuelson, 1939), whereby firms boost investment when they expect higher future aggregate demand. Thus, to explore the transmission of a monetary stimulus via bank lending to aggregate demand, we focus on aggregate investment.

We take gross fixed capital spending from the IMF’s International Financial Statistics database: National Accounts and Population line 93e. Gross fixed capital spending is the total value of fixed asset acquisitions, less disposals plus certain additions to the value of non-produced assets (such as subsoil assets or major improvements in the quantity, quality, or productivity of land). We use each economy’s PPI index to deflate these data. The economy’s real annual growth rate in gross fixed capital spending is $\Delta capex_{j,t+1} \equiv (capex_{j,t+1} - capex_{j,t})/capex_{j,t}$, again measured over the year following that over which money growth is measured. Capital spending data are quarterly; however, we measure capital spending growth over a full year because capital investment is unlikely to be an instantaneous response.

2.5 Control Variables

All economy-level regressions control for economy fixed-effects. The bank-level regressions include bank fixed-effects, and most also include either year or economy-year fixed-effects. Additional variables are introduced below when used. Table 1 provides details as to their sources and construction.

2.6 Summary Statistics

Table 2 provides means and standard deviations of the main variables. On average all countries experience monetary expansion and positive real gross loan growth. There is greater heterogeneity in real fixed capital growth: among the countries for which we have data, twenty two register a positive average and eight a negative average.

Table 3 displays pairwise cross-country correlation coefficients of loan growth, money growth, fractions of banks state-run, and key controls. In estimating correlations with bank-level variables, we use economy-level means. Loan growth correlates significantly positively with money growth and lagged liquidity. State-run banks are more liquid and smaller than private-sector banks. Bank liquidity and bank size are negatively correlated.

3. Empirical Methodology and Results

3.1 Baseline economy-level regressions

Our economy-level regressions use differences in country j 's year t money growth, $\Delta M_{j,t}$, to explain differences in either its aggregate real credit growth, $\Delta credit_{j,t+1}$, or its aggregate real capital investment growth, $\Delta capex_{j,t+1}$, in year $t + 1$. These regressions take the form

$$[1a] \quad \Delta credit_{j,t+1} = a + b \Delta M_{j,t} + controls + e_{j,t},$$

$$[1b] \quad \Delta capex_{j,t+1} = a + b \Delta M_{j,t} + controls + e_{j,t},$$

To explore differences in this relationship associated with $f_{j,t}$, the state-run fraction of the country's banking system, we test for significant differences in the coefficient b associated with state control over banks. A varying parameter specification operationalizes this by replacing the regression parameter b in [1a] and [1b] with the linear function

$$[2] \quad b(f_{j,t}) \equiv b_0 + b_1 f_{j,t}$$

Substituting [2] into [1], controlling for the main-effect of the state-run banking measure, $f_{j,t}$, and economy fixed-effects, denoted λ_j , yields our *baseline economy-level regression* specifications

$$[3a] \quad \Delta credit_{j,t+1} = a_1 f_{j,t} + (b_0 + b_1 f_{j,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t},$$

$$[3b] \quad \Delta capex_{j,t+1} = a_1 f_{j,t} + (b_0 + b_1 f_{j,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t},$$

in which the economy fixed-effects subsume a , the intercept in [1]. These regressions test for whether differences in money growth presage differences in aggregate credit or investment growth differently where the banking system is more state-run. All country-level regressions cluster residuals $e_{j,t}$ by economy, with Eurozone countries one cluster for this purpose only. If increases in money growth presage greater increases in credit and capital growth in countries with higher fractions of state-run banks, the estimated coefficients b_1 would be significantly positive.

Regressions 4.1 and 4.2 in Table 4 summarize these results. Regressions 4.1, based on [3a] associates a one percentage point higher money growth rate over the prior twelve months with a statistically and economically significant 0.23 percentage point higher aggregate credit growth in an economy whose banking system is entirely state-run than in an economy whose banking system is fully private-sector. Regression 4.2, based on [3b] links the same increase in money growth to a statistically and economically significant 0.79 percentage point higher aggregate capital spending growth where the banking system is entirely state-run versus where it is entirely private-sector. In both regressions, the money growth main-effects, representing a fully private-sector banking system, are statistically indistinguishable from zero. That is, money growth presages no statistically detectable increase in

aggregate credit growth or aggregate capital spending growth in economies whose banking systems are entirely private-sector.

If monetary expansion is ineffective in boosting lending by large private-sector banks, but effective in boosting lending by similar state-controlled banks, monetary neutrality might depend on the ownership structure of the country's banks. Money growth might be neutral in economies whose banking systems consist mainly of large private-sector banks, but effective in boosting lending and investment in proportion to the importance of state-run banks. Our findings thus suggest one possible route for reconciling mixed findings about the effectiveness of monetary policy after the recent financial crises (e.g., Deng *et al.*, 2011; Bouis *et al.*, 2013; Chakraborty, Goldstein and MacKinlay, 2015).

Obviously, these economy-level regressions demonstrate only correlations. Inferring that a more fully state-run banking system more effectively transmits money growth into real credit and capital spending growth requires additional tests, which the remainder of this section and the next section develop.

3.2 Economic implications of bank-level heterogeneity

Our first set of additional tests utilize identification by disaggregation, as introduced by Kashyap and Stein (2000). After exhaustively surveying estimation techniques, they conclude that “to make further progress on this difficult identification problem, one has to examine lending behaviour at the individual bank-level” because different economy-level causality scenarios require that “the effect of monetary policy on lending should be more pronounced for some banks than for others.” The issue at hand is amenable to this approach because, if state-run banks transmitted monetary growth more reliably than do private-sector banks, this would stand out in bank-level lending data.

Our bank-level tests derive from a simple bank-level specification paralleling the economy-level relationship [1],

$$[4] \quad \Delta credit_{i,t+1} = a + b\Delta M_{j(i),t} + controls + e_{i,t},$$

with i, j and t indexing banks, countries, and time, respectively. Each observation is a bank-fiscal year, and all bank-level regressions cluster residuals $e_{i,t}$ by economy, with Eurozone countries one cluster for this purpose only.

At the bank-level, the proportionality relationship [2] is replaced by a dichotomy: do individual state-run banks' extend more credit than otherwise similar private-sector banks following increased money growth? This results in simple difference-in-difference bank-level regressions in which a state-run bank dummy, $\delta_{i,t}$, alters the magnitude of the coefficient b in [4]. That is, the fixed parameter b is replaced by

$$[5] \quad b(\delta_{i,t}) \equiv b_0 + b_1\delta_{i,t}$$

Substituting [5] into [4], and controlling for the main-effect of the state-run bank dummy and bank fixed-effects, λ_i , yields the bank-level difference-in-difference regression

$$[6] \quad \Delta credit_{i,t+1} = a_1\delta_{i,t} + (b_0 + b_1\delta_{i,t})\Delta M_{j(i),t} + \sum_i d_i \lambda_i + e_{i,j,t}.$$

This tests for differences between state-run and private-sector banks in how differences in lending track differences in money growth.

Augmenting the list of controls with year fixed-effects, denoted λ_t in [7a] or with economy-year fixed-effects, denoted $\lambda_{j,t}$ in [7b] yields the bank-level difference-in-difference specifications

$$[7a] \quad \Delta credit_{i,t+1} = a_1\delta_{i,t} + (b_0 + b_1\delta_{i,t})\Delta M_{j(i),t} + \sum_i d_i \lambda_i + \sum_t d_t \lambda_t + e_{i,j,t}$$

and

$$[7b] \quad \Delta credit_{i,t+1} = a_1\delta_{i,t} + (b_1\delta_{i,t})\Delta M_{j(i),t} + \sum_i d_i \lambda_i + \sum_{j,t} d_{j,t} \lambda_{j,t} + e_{i,j,t},$$

Note that bank fixed-effects subsume both the intercept a and economy fixed-effects. In [7b], economy-year fixed-effects subsume the main-effect of money growth because $\Delta M_{j(i),t}$ varies at the economy-year-level.

The remainder of Table 4 summarizes these difference-in-difference findings. Because

regression 4.3, based on [6], controls for bank fixed-effects, it links a one percentage point increase in money growth to a 0.30 percentage point larger increase in lending growth by a state-run bank than by a private-sector bank in the same economy. In contrast, the -0.16 coefficient on money growth and 0.30 coefficient on money growth interacted with the state-run bank dummy indicate that, when money growth rises by one percentage point, private-sector banks actually cut their lending by 0.16 percentage points while state-run banks in the same country boost their lending growth by $0.30 - 0.16 = 0.14$ percentage points, which is statistically significant ($p = 0.07$). This exposes an economically significant heterogeneity in the bank-level data: lending growth by individual state-run banks rises following an increase money growth; lending by individual private-sector banks in the same economy does not.

Regression 4.4, which follows specification [7a], including year fixed-effects in addition to bank fixed-effects, paints a similar picture. A unit increase in money growth presages the typical state-run bank increasing its lending growth by a statistically significant 0.22 percentage points. Regression 4.5, based on [7b], confirms that the difference in lending growth between state-run versus private-sector banks subsequent to an increase in money growth remains significant after controlling for omitted variables that vary at the country-year-level, as well as the interactions of any such variables with money growth, all of which the economy-year interaction fixed-effects subsume.⁷

Regressions 4.3, 4.4 and 4.5 substantially narrow the scope for alternative explanations of the economy-level findings in several ways. First, they render unlikely macro-level reverse causality explanations of our baseline economy-level results positing monetary authorities boosting money growth to accommodate anticipated increases in credit demand and investment due to a technology shock, market expansion, regulatory reform, or other exogenous factor. That state-run banks might

⁷ The main-effect coefficient on money growth is not economically meaningful in 4.4 or 4.6 because the inclusion of year and bank fixed-effect leaves its estimation dependent solely on the 4% of bank-year observations in which banks' fiscal year-ends differ from the calendar year-end. Excluding these observations does not affect our results; nor does allowing them to determine main-effect coefficients.

boost lending in response to such developments, while private-sector banks stand aloof, seems *a priori* implausible in standard models of such phenomena. The bank-level results thus limit the feasible set of alternative causality scenarios to those with scope for the bank-level heterogeneity we observe – that is, to alternative explanations that let prior money growth correlate with growth in individual state-run banks’ lending, but not with lending by individual private-sector banks in the same county at the same time.

Second, the fixed-effects in the Table 4 regressions further restrict the scope for alternative explanations. Economy fixed-effects in 4.1 and 4.2 subsume omitted time-invariant economy characteristics, as do bank fixed-effects in 4.3 and 4.4. This is because no bank switches economy in our data, and multinational banks are assigned distinct fixed-effects in each economy, leaving economy dummies linear combinations of bank dummies. This precludes alternative explanations turning on time-invariant differences between economies, such as legal origin, cultural differences, and the like. Regression 4.5 has farther-reaching implications because it includes bank and economy-year fixed-effects. The latter subsume all time-varying economy-level omitted variables. For example, if money growth had different implications for economies with different legal origin, cultural factors, or other such variable denoted $z_{j,t}$, economy-year fixed-effects subsume this because the interaction $\Delta M_{j,t} z_{j,t}$ varies only at the economy-year-level.

3.3 Baseline bank-level regressions: controlling for size and liquidity

One mechanism permitting bank-level heterogeneity is that in Kashyap and Stein (2000). They envision lending-constrained small illiquid banks responding to money growth, which relaxes those constraints, but large liquid banks, already lending optimally, not responding. Finding evidence of precisely this heterogeneity in bank-level regressions, they argue that their bank-level findings identify a bank credit monetary policy transmission channel at the economy-level. If state-run banks were smaller or less

liquid than private-sector banks, our state-run indicator might also proxy for such lending constraints. In fact, the simple correlations in Table 3 show state-run banks smaller but more liquid than private-sector banks.

To explore this, we modify the bank-level regressions to let bank size and liquidity, denoted $x_{k,i,t}$ for $k \in \{size, liquidity\}$, alongside the state-run bank dummy, modulate the link between differences in money growth and differences in bank-credit growth. This is operationalized by replacing b in [4] with

$$[8] \quad b(\delta_{i,t}, \{x_{k,i,t}\}) \equiv b_0 + b_1 \delta_{i,t} + \sum_k \gamma_k x_{k,i,t},$$

rather than with [5]. Also augmenting the list of control variables in [7] with the $x_{k,i,t}$ yields another set of bank-level difference-in-difference regressions, which we refer to below as our *baseline bank-level regressions*,

$$[9a] \quad \Delta credit_{i,t+1} = a_1 \delta_{i,t} + (b_0 + b_1 \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_t d_t \lambda_t + e_{i,j,t}.$$

$$[9b] \quad \Delta credit_{i,t+1} = a_1 \delta_{i,t} + (b_0 + b_1 \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_{j,t} d_{j,t} \lambda_{j,t} + e_{i,j,t}.$$

These explore how a unit increase in money growth presages different increases in lending growth by state-run versus private-sector banks of comparable size and liquidity within each country. Note that the bank fixed-effects, denoted λ_i in [9a] and [9b], control for all bank-level and country-level time-invariant latent factors and the country-year interaction fixed-effects, denoted $\lambda_{j,t}$, in [9b], control for all time-varying latent variables and their interactions with money growth. As above, residuals are clustered by economy, the Eurozone being a single economy for this purpose.

The last two columns in Table 4, 4.6 and 4.7, summarize these results. Bank size is the log of total assets the prior year; bank liquidity is the sum of government securities, cash and funds due from

other banks, all over total assets.⁸ If state-run bank size or liquidity drove our results, their interactions with money growth would be significant and leave the interaction of state control with money growth insignificant. This is not observed. The interactions of size and liquidity with money growth are insignificant, and that of state-control with money growth remains significant – indeed its point estimate barely budges.

These findings do not necessarily contradict Kashyap and Stein (2000). Their analysis exploits the unusual structure of the U.S. banking system: the thousands of very small independent banks that persist as a legacy of its Depression era regulations restricting banks to a single branch (Calomiris and Haber, 2014). Our analysis uses only the largest, and presumably most liquid, banks in each economy. The comparatively limited variation in bank size and liquidity in our sample understandably makes the Kashyap and Stein (2000) effect difficult to find. For our purposes, this is helpful because it also makes that effect less likely to interfere with our primary task.⁹

We therefore conclude that the Table 4 findings are unlikely to be an artefact of state-run and private-sector banks being of systematically different size or liquidity. Rather, state-run bank's lending is significantly more related to prior money growth than is lending by a private-sector bank of similar size and liquidity in the same country at the same time under similar conditions. This suggests a mechanism distinct from that modelled by Kashyap and Stein (2000), such as state-run banks obeying politicians' orders to lend more.

3.4 Robustness of baseline regressions

⁸ Kashyap and Stein (2000) measure bank size using a large bank indicator (set to one for banks among the largest percent in their economies, zero otherwise) and measure liquidity as cash plus all securities and funds due from other banks, all over total assets. Results qualitatively similar to those in regressions 4.6 and 4.7 ensue after substituting either or both of these alternative measures.

⁹ For further work on the Kashyap and Stein (2000) hypothesis, see e.g. Ivashina and Scharfstein (2010).

Table 4 survives a battery of robustness checks. In describing these, we say the results are *qualitatively similar* if we see an identical pattern of signs and significance and comparable point estimates. Where results are not qualitatively similar, we provide details.

Table 4 is robust to alternative measures of money growth. Using the prior 6 months monetary base growth instead of the prior 12 months generates qualitatively similar results with only one exception: in bank-level regressions, the interaction of size with money growth is significantly negative. This robustness check reproduces in international data Kashyap and Stein's (2000) finding that less liquid U.S. banks respond more robustly to money growth.

An important robustness check is to use an alternative measure of the strength of a monetary stimulus, the decline in the economy's key policy interest rate. This approach is more problematic than using base money growth, which is readily comparable across economies, because different central banks use different benchmark interest rates. The IMF IFS data provide a "monetary policy related interest rate" for 22 economies in our sample. If this is missing, we substitute the "discount rate", "lending rate" or "money market rate", in that order, as available in IFS. This yields interest rate data for 38 economies. We define "interest rate drop" as minus one times the change in this rate over either the prior 6 or prior 12 months. Gauging interest rate drops over either period generate qualitatively similar results, except that b_1 in analogues to 4.5 and 4.7 loses statistical significance (p -levels range from 0.20 to 0.30). Analogues of 4.3 link a one percentage point interest rate drop over the prior 6 or 12 months to state-run banks boosting lending by 2.85% or 1.59%, respectively, more than do their private-sector peers. Both differences-in-differences are statistically significant.

The bank-level regressions survive further robustness checks. Only 5 of the banks in this sample switch between being state-run and being private-sector during our sample window, so the main-effect of the state-run bank dummy in 4.3 through 4.7 may be poorly estimated. Rerunning these without this main-effect yields qualitatively similar results to those in the table. Dropping the five banks whose state-

run status changes during the sample window also generates qualitatively similar results.

Money growth and bank-level loan growth are winsorized at 10%, and observations with absolute value of loan growth above 50% are dropped. Winsorizing at 5%, not winsorizing, and retaining the extreme values all yield qualitatively similar results. We use consolidated data for banks that report both consolidated and unconsolidated figures; using unconsolidated data yields qualitatively similar results. Most tables cluster by economy, with Euro-zone countries as one cluster, which Petersen (2009) recommends as the most conservative approach to data of this sort. Two-way clustering by economy and year yields qualitatively similar results. Not clustering or clustering only by year yields uniformly lower p-levels. Dropping all fixed-effects yields qualitatively similar results with lower p-levels, as does rerunning the bank-level regressions with economy fixed-effects instead of bank fixed-effects.

Our economy-level credit growth variable may be excessively broad because it encompasses all credit, rather than just credit extended by banks. We therefore construct an alternative aggregate bank credit growth measure by adding up the gross credit extended by all banks covered by the BankScope dataset in each economy each year, and constructing a real growth rate in this aggregate. This measure can be criticized for relying on the incomplete or time varying coverage of banks in BankScope and for omitting non-bank financial institutions of many sorts. Repeating our tests with this alternative measure of economy-level bank credit growth also generates qualitatively similar results.

Some studies (Caprio *et al.* 2007; Laeven and Levine 2009; Morck *et al.* 2011) partition private-sector banks into those that are widely held and those that have a controlling shareholder. To explore this, we define controlling shareholder banks as private-sector banks with a biological person or family as an ultimate controlling shareholder and widely held private-sector banks as those with no ultimate controlling shareholder. Rerunning the regressions partitioning banks into state-run, widely held, and controlling shareholder banks generates results qualitatively similar to the tables. Higher money growth presages faster growth in state-run bank lending, but not in lending by either type of private-sector

bank. Higher money growth presages increased growth in aggregate lending and investment in economies where state-run banks are more predominant, but not where either class of private-sector bank is more predominant.

Our data contain no foreign subsidiaries of other countries' state-run banks, but some private-sector banks in some economies are subsidiaries of foreign banks. These may have better access to international money markets than do purely domestic banks, and thus might potentially be less affected by a domestic monetary stimulus. Also, the importance of foreign banks might correlate with overall openness. Using our data on controlling owners for each bank each year, we set a new dummy variable to one if the bank is foreign-controlled that year and to zero otherwise. We then recalculate a new set of credit-weighted economy-level bank governance variables denoting the importance of state-run banks, domestic private-sector banks, and foreign-controlled banks. We rerun the baseline bank-level regressions, first dropping all foreign-controlled banks, and then controlling for the foreign-controlled bank dummy's interaction with money growth. We rerun the baseline economy-level regressions analogously – first using the revised importance of state-run banks measure and then controlling for the main-effect and interaction with money growth of the foreign-controlled bank importance measure. These exercises generate results qualitatively similar to the baseline regressions, and the coefficients on the foreign-bank main-effects and interactions are uniformly insignificant.

Another possibility is that domestic banks may be global banks that have subsidiaries abroad and as a result their loan growth may be less affected by domestic monetary stimulus. This is especially a concern given that we use consolidated financial statements however as mentioned above using unconsolidated statements yield similar results. Regardless, we identified banks that control other banks in other countries and labelled them as global banks. Dropping these banks yields similar results in baseline regressions. Keeping these banks but recalculating our main variables denoting the importance of state-run banks, domestic private-sector banks, and global banks also yields qualitatively similar

results to the baseline regressions.

We included bank size and liquidity as possible moderators of effect of money growth on bank lending following Kashyap and Stein (2000) results. There are other bank-level variables that may interact with money growth in affecting bank lending decisions. We also use lagged total deposits/ total assets, and lagged total equity/ total assets and their interactions with money growth as additional control variables in Table 4. We have tried all combinations and these additional control variables together with existing controls. The coefficients of these additional variables are never significant and the main variable of interest, the interaction of money growth with the state-run bank dummy, always attracts a positive significant coefficient.

4. Feasible Alternative Explanations

This section considers alternative explanations of our baseline findings with scope for the bank-level heterogeneity evident in regressions 4.3 through 4.7. Each discussion includes analogues of the baseline bank-level regressions 4.6 and 4.7 alongside analogues of the baseline economy-level regressions 4.1 and 4.2. In each case, the bank-level regressions further limit the range of feasible alternative explanations and the economy-level regressions speak to economic significance. Analogues of the bank-level regressions 4.3, 4.4 and 4.5 are not shown because, in every case, they generate results qualitatively similar to the analogues of 4.6 and 4.7.

4.1 Heterogeneity in bank's borrower bases

One class of alternative explanations posits state-run and private-sector banks lending to different clienteles, whose loan demand moves differently with money growth, or with something correlated with money growth. The latter might include expansionary fiscal policy, a depreciating currency, or even just

the phase of the business cycle. For such things, rather than money growth, to explain our baseline findings, state-run banks and private-sector banks, or their respective borrower clienteles, would have to react differently to them. We rule out such alternative explanations using several strategies. This section shows that our results are unaffected by the inclusion of other important variables correlated with money growth, and their interactions with state control over banks. Additional tests that weigh in favour of a state-run bank channel and against such alternative mechanisms are presented in section 5.

This sub-section considers the possibility that differences in state-run banks' lending might be tracking differences in some other time-varying economic policy or economy characteristic, $p_{j,t}$, rather than differences in money growth. To accommodate such possibilities, we consider economy-level regressions of the form

$$[10a] \quad \Delta credit_{j,t+1} = a_1 f_{j,t} + (\beta_0 + \beta_1 f_{j,t}) p_{j,t} + (b_0 + b_1 f_{j,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t},$$

$$[10b] \quad \Delta capex_{j,t+1} = a_1 f_{j,t} + (\beta_0 + \beta_1 f_{j,t}) p_{j,t} + (b_0 + b_1 f_{j,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t},$$

and bank-level regressions of the form

$$[11a] \quad \Delta credit_{i,j,t+1} = a_1 \delta_{i,t} + (\beta_0 + \beta_1 \delta_{i,t}) p_{j(i),t} + (b_0 + b_1 \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_t d_t \lambda_t + e_{i,j,t}.$$

$$[11b] \quad \Delta credit_{i,j,t+1} = a_1 \delta_{i,t} + (\beta_0 + \beta_1 \delta_{i,t}) p_{j(i),t} + (b_1 \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_{j,t} d_{j,t} \lambda_{j,t} + e_{i,j,t}.$$

These regressions essentially run horse-races to see which best explains the left-hand side variables: interactions of state-run banking with money growth or interactions of state-run banking with the suspected omitted variable, $p_{j,t}$. If including the additional terms in [10] and [11] leaves b_1 insignificant – or even just substantially reduced in magnitude – the alternative explanation merits attention. However, if including the additional terms leaves b_1 significantly positive and little changed in magnitude, our baseline results merit further investigation. The following subsections consider horse-races against plausible candidates for such a missing variable.

4.1.1 Heterogeneous borrower responses to a fiscal stimulus

If state-run banks' borrowers were more sensitive than private-sector banks' borrowers to a fiscal stimulus, demand for credit from state-run banks could rise faster after a fiscal stimulus than does demand for credit from private-sector banks. If the central bank accommodated this by letting money growth rise, letting actual lending subsequently rise, our baseline results could ensue, but increased borrowing from state-run banks and increased investment by their borrowers would be causing money growth, rather than the converse.

Such scenarios are not *a priori* excludable. For example, a fiscal stimulus might entail spending on infrastructure. If infrastructure construction firms were disproportionately state-run bank clients, their capital expenditure plans could disproportionately boost demand for credit at state-run banks. Likewise, a fiscal stimulus effected by partially subsidizing investment by nonfinancial state-owned enterprises or politically-connected private-sector firms, which borrowed the remaining capital costs from state-run banks, could disproportionately boost demand for credit at state-run banks.

Table 5 explores this alternative explanation. Regressions 5.1 and 5.2 re-estimate the baseline economy-level regressions 4.1, 4.2 but as in specifications [10a] and [10b]. Regressions 5.3 and 5.4 re-estimate the baseline bank-level regressions 4.6 and 4.7, but as in specification [11]. In all four regressions, b_1 remains positive and significant, its magnitude little changed from its Table 4 analogues.

These results are consistent with a unit rise in money growth presaging increased lending by state-run banks, but not by otherwise similar private-sector banks in the same country at the same time, and presaging a larger increase in aggregate lending and aggregate investment in economies whose banking systems are more fully state-run. A unit increase in fiscal stimulus presages none of these things.

4.1.2 Heterogeneous borrower responses to currency depreciation

Exchange rate drops can accompany changes in money growth (Fleming 1962; Mundell 1963). An exchange rate drop can spur both exports and inward foreign direct investment, and might thus spur lending associated with either or both.

This scenario also has room for bank-level heterogeneity. First, some countries charge state-run banks with export promotion – that is, with lending to exporters or exporters’ foreign customers. Conceivably, a lower exchange rate could spur demand for export-related loans, and if state-run banks disproportionately provided these, their lending might rise disproportionately as the exchange rate falls. Second, currency depreciation can attract inward foreign direct investment (Froot and Stein, 1991). If state-run banks’ clients disproportionately formed joint ventures or partnerships with foreign entrants, we might observe state-run banks’ lending rising disproportionately as the currency drops. In either case, if money growth rises as the currency drops, our baseline results might ensue.

Regressions 5.5 to 5.8 therefore repeat the exercise above, but using $p_{j,t} = \text{exchange rate depreciation}$ (percent change in local currency units per U.S. dollar, positive values implying local currency depreciation). The objective of these regressions is to let exchange rate depreciation compete with money growth to explain the differences associated with state control over banks in the baseline regressions.

In both economy-level regressions, β_1 is significantly positive; in both bank-level regressions, it is insignificantly negative and the exchange rate depreciation main-effect is significantly negative. The economy-level regressions are thus consistent with faster lending growth after an exchange rate drop where more banks are state-run; but the bank-level regressions implicate something other than differential lending behaviour by the two types of banks.

More important to the issues at hand, b_1 remains positive and significant across all four regressions, its magnitudes little changed from Table 4. The bank-level results are consistent with faster

money growth, but not steeper exchange rate drops, presaging faster lending growth by individual state-run banks than by otherwise similar individual private-sector banks in the same country at the same time. The economy-level results show that controlling for economy-level effects associated with currency depreciation does not disturb the baseline economy-level results regarding state-control over banks and money growth.

4.1.3 Heterogeneous borrower or state-run bank response over the business cycle

State-run banks' borrowers might be less sensitive than private-sector banks' borrowers to the business cycle. For example, if state-run banks' clients were disproportionately regulated utilities or other recession-proof industries, credit demand at state-run banks might be substantially less procyclical than at private-sector banks. If activist monetary authorities ran countercyclical monetary policies, increased money growth might spuriously presage state-run banks' lending rising above private-sector banks' lending.

To explore this, we take $p_{j,t}$ as the *output gap*, the economy's potential GDP, estimated using the filter developed by Hodrick and Prescott (1997), minus its actual GDP, all as a fraction of the former. Output gap measures the business cycle, growing larger when the economy is deeper in recession and smaller when the economy is more prosperous.

Regressions 5.9 through 5.12 repeat the exercise above letting the interaction with money growth with output gap compete for explanatory power against the interaction of money growth with state-run bank measure. Neither output gap nor its interaction with state-run bank measure or dummy is significant in explaining either economy-level credit growth (5.9) or bank-level credit growth (5.11 and 5.12). Regression 5.10 shows aggregate investment to be significantly negatively correlated with the output gap (i.e. pro-cyclical), but significantly less so if more of the banking system is state-controlled. Regression 5.10 is consistent with investment falling off in business cycle downturns, but to a lesser

degree in economies whose banking systems are more state-run, consistent with prior work (Micco and Ugo 2006; Morck *et al.* 2011; Lin *et al.* 2013; Coleman and Feler 2015) linking more state control over banks to lower business cycle amplitudes.

More importantly to our hypothesis, the interaction of money growth with the fractional importance of state-run banks remains positive and significant in explaining subsequent aggregate credit and investment growth, as does the interaction of money growth with the state-run bank dummy in the regressions explaining bank-level lending growth. That is, the baseline results are unlikely to be an artefact of state-run banks' clients' demand for credit being systematically less procyclical than that of private-sector banks' clients.

4.2 The Reach of the State

State-run banks might be more prevalent where state power is broader and deeper in general. A highly interventionist government might direct its ministries, nonfinancial state-owned enterprises or politically-dependent private-sector firms to borrow and invest more, its state-run banks to lend more, which the central bank might then accommodate. If so, state-run banks are only one cog in a far-reaching apparatus of state intervention; and our baseline results regarding state-run banks should be more pronounced in economies where the overall apparatus is larger. That is, the state-run bank dummy or fractional importance variable would then matter more in economies whose governments are more generally highly interventionist.

To explore this, we augment the baseline regressions in yet another way. We let a measure of the reach of the state, again denoted $p_{j,t}$, also modulate the link between differences in money growth and differences in bank-credit growth. In the economy-level regressions, this entails replacing the parameter b in [1] with the expression

$$[12] \quad b(f_{j,t}) \equiv b_0 + b_1 f_{j,t} + b_2 p_{j,t} + b_3 p_{j,t} f_{j,t}$$

The second and third terms on the right-hand side of [12] let the strength of the link between money growth and either aggregate credit or capital spending growth vary with the importance of state-run banking, $f_{j,t}$, and with the reach of the state, $p_{j,t}$. The final term lets the link between money growth and aggregate credit or capital spending growth vary more strongly with the importance of state-run banks if the reach of the state is greater too. Including economy fixed-effects and the main-effect of $p_{j,t}$ yields the augmented economy-level specification

$$[13a] \quad \Delta credit_{j,t+1} = a_1 f_{j,t} + a_2 p_{j,t} + (b_0 + b_1 f_{j,t} + b_2 p_{j,t} + b_3 p_{j,t} f_{j,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t}$$

$$[13b] \quad \Delta cap_{j,t+1} = a_1 f_{j,t} + a_2 p_{j,t} + (b_0 + b_1 f_{i,t} + b_2 p_{j,t} + b_3 p_{j,t} f_{i,t}) \Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t}$$

In the bank-level regressions, the analogous exercise replaces b in [4] with the expression

$$[14] \quad b(\delta_{i,t}, \{x_{k,i,t}\}, p_{j(i),t}) \equiv b_0 + b_1 \delta_{i,t} + b_2 p_{j(i),t} + b_3 p_{j(i),t} \delta_{i,t} + \sum_k \gamma_k x_{k,i,t},$$

Substituting [14] into [4] and including the same controls as in [6], along with the main-effect of $p_{j,t}$ yields the augmented bank-level regressions

$$[15a] \quad \Delta credit_{i,j,t+1} = a_1 \delta_{i,t} + (b_0 + b_1 \delta_{i,t} + b_2 p_{j(i),t} + b_3 p_{j(i),t} \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_t d_t \lambda_t + e_{i,j,t}$$

$$[15b] \quad \Delta credit_{i,j,t+1} = a_1 \delta_{i,t} + (b_1 \delta_{i,t} + b_2 p_{j(i),t} + b_3 p_{j(i),t} \delta_{i,t} + \sum_k \gamma_k x_{k,i,t}) \Delta M_{j(i),t} + \sum_k c_k x_{k,i,t} + \sum_i d_i \lambda_i + \sum_{j,t} d_{j,t} \lambda_{j,t} + e_{i,j,t}$$

In these regressions, the coefficients b_1 , b_2 , and b_3 each have an economic interpretation. Collecting terms and simplifying reveals [13] and [15] to include triple interaction terms: b_3 being the coefficient of $p_{j,t} \times f_{j,t} \times \Delta M_{j,t}$ in the economy-level regressions and of $p_{j(i),t} \times \delta_{i,t} \times \Delta M_{j(i),t}$ in the bank-level regressions. A significantly positive b_3 suggests that, after money growth rises, state-run banks' lending rises by more than private-sector banks' lending does where the reach of the state is greater. Thus, a significantly positive b_2 suggests that the general reach of the state matters and a significantly positive b_3 suggests that state control over banks matters more where the general reach of the state is greater. If b_1 , b_2 and b_3 were all significantly positive, the general reach of the state might

still be the predominant factor. But if b_1 remains significantly positive, its magnitude little changed from Table 4, with b_2 and b_3 insignificant, state-run banks alone, not the reach of the state more generally, are implicated.

We set $p_{j,t}$ equal to each of three measures of the reach of state power in turn. The first, government *transfers and subsidies* as a fraction of GDP, we take as a proxy for the size and importance of government in the economy. The second, *state-directed investment*, is government investment as a share of total investment. We interpret this as a proxy for the state's scope for directing its agencies, including state-owned enterprises, to demand more credit and to invest more. The third is the fraction of large businesses, by total market capitalization, run by people with *political connections* to top government officials, as calculated by Faccio (2006). We interpret this as a proxy for the strength of business-government connections, and posit that more politically connected private-sector firms might be especially responsive to government directives to borrow and invest more – perhaps because they anticipate bailouts in unpropitious states (Mian and Khwaja 2005; Faccio *et al.* 2006). This measure is a cross-section only.

Table 6 summarizes the results. The coefficient b_1 remains positive and significant, its values little changed, across all the bank-level regressions, save that in 6.4 its p-level rises to 0.14. Both b_2 and b_3 are uniformly insignificant across all the bank-level regressions. Overall, these findings are consistent with higher money growth presaging higher lending by state-run banks and with other dimensions of a state's reach in the economy being largely irrelevant.

The economy-level baseline results are preserved except in two regressions. Regression 6.1, which lets government transfers and subsidies enter via [12], shows aggregate lending no longer growing significantly faster following increased money growth where banking is more thoroughly state-run. However, the transfers and subsidies variable, its interaction with money growth, and its triple interaction are also all insignificant. Second, regression 6.6 shows aggregate capital spending rising more

following increased money growth only where both state-run banks and state-directed investment in general are more prevalent.

These findings do not exclude a broader mechanism of state power over both banks and other firms transmitting higher money growth into higher real economic growth. However, they implicate state-run banks as a critical cog in any such broader mechanism.

4.3 Robustness Discussion

The regressions in Tables 5 and 6 survive a battery of robustness checks. Using the growth rate in government consumption, rather than the deficit, to measure fiscal stance yields qualitatively similar results to 5.1 through 5.4. Qualitatively similar results also arise using real GDP growth, rather than output gap, to track business cycles. Table 5 presents a separate set of regressions for each additional variable. A single set of regressions including all three additional variables as well as all three of their interactions with the state-run bank dummy or importance measure yields qualitatively similar results to the baseline regressions.

Table 6 is robust to using alternative measures of the reach of the state. Total government consumption over GDP and the Fraser Institutes world index of economic freedom (an inverse measure) generate qualitatively similar results. Table 6 presents a separate set of regressions for each measure of state interventionism. A single set of regressions including all three measures of state power and all of their double and triple interactions leaves the coefficients of the state-run bank measures and their interactions with money growth insignificant; however, the point estimates are essentially unchanged. The additional variables and interactions are highly collinear, so we estimate principal components of the three state intervention measures. Three principal components are significant, and sets of regressions including all three principal components along with their double and triple interactions yield qualitatively similar results, except in those explaining economy-level loan growth.

The regressions in Tables 5 and 6 have different forms because they ask different questions. Those in Table 5 ask whether state-run banks' lending varies with money growth *or* with something else – specifically, fiscal stance, currency depreciation, or the business cycle. Exploring these requires regressions that run horse races of money growth and its interactions with the state-control measures against these alternative variables and their interactions with the state-control measures. Table 6, in contrast, asks whether state-run banks are critical or a generally interventionist state is needed. Table 6 thus runs regressions controlling for the state interventionism measures' main-effects, interactions with money growth, and triple interactions. The last let money growth matter more if state interventionism is greater *and* the bank is state-run or state-run banks are more important.

An overarching set of robustness checks runs regressions that include all interactions present in both tables. That is, for each additional variable $p_{j,t}$, we augment the baseline economy-level regressions with $p_{j,t}$, $p_{j,t} \Delta M_{j,t}$, $p_{j,t} f_{j,t}$, and $p_{j,t} f_{j,t} \Delta M_{j,t}$ and augment the baseline bank-level regressions with $p_{j(i),t}$, $p_{j(i),t} \Delta M_{j(i),t}$, $p_{j(i),t} \delta_{i,t}$, and $p_{j(i),t} \delta_{i,t} \Delta M_{j(i),t}$.¹⁰ The results in Tables 5 and 6 all survive, and the additional coefficients are almost always insignificant. Notably, the failure of 6.6 to preserve the baseline result is mitigated: the 6.6 analogue assigns b_1 a point estimate of 0.43 ($p = 0.13$).

5. Corroborating Evidence of a State-Run Bank Channel

The previous section considered feasible alternative explanations, and excluded or substantially restricted each. This section assists in identification by presenting additional direct evidence consistent with variation in likely political pressure on state-run banks explaining the magnitude of the differences (between state-run and private-sector banks) in the difference in bank lending subsequent to a unit

¹⁰ Including all double interactions is not possible where the reach of state power is measured by the importance of politically connected firms, as this variable has no time series variation. Its interactions with money growth and with the fractional importance of state-run banks therefore drop out. Economy-year fixed-effects subsume the interactions of all of the interventionism measures with money growth.

difference in money growth. These tests further narrow the field of feasible alternative hypotheses to those with scope for variables gauging likely political pressure on state-run banks to matter. The difference between the lending growth of an individual state bank and that by an otherwise similar private-sector bank subsequent to a unit change in money growth varying with political pressure is difficult to reconcile with alternative causality scenarios.

5.1 Central bank independence

The central banks of some countries are less independent - that is, more politicized – than those of other countries. A more politically sensitive central bank would let politicians order up faster money growth to encourage more lending.¹¹ In contrast, an independent central bank might adjust money growth with little regard for current political priorities. If political pressure heightens state-run banks' responsiveness to money growth, such asynchronies would weaken our finding in economies with more independent central banks. This test thus further narrows the set of feasible alternative causality scenarios by ruling out those turning on state-run banks' clients credit demand being more sensitive to money growth (than is private-sector banks' clients' credit demand) for reasons unrelated to political pressure and not controlled for in our tests. If state banks clients were simply more sensitive to money growth, they would be so regardless of whether or not the central bank is independent.

To explore this, we gauge central bank independence using the measure of Crowe and Meade (2008), which ranges from zero to one, one indicating maximal independence. Table 7 presents

¹¹ Some caveats may apply to these tests. First, even highly independent central banks cooperate with other government branches (Acharya, 2015). Second, as Stiglitz (2012) argues in a speech to the Reserve Bank of India: “[The crisis] has shown that one of the central principles advocated by Western central bankers – the desirability of central bank independence – was questionable at best ... There is no such thing as truly independent institutions. All public institutions are accountable, and the only question is to whom.” Third, an independent central bank might still effectively press state-run banks (more than private-sector banks) to support its monetary policy.

regressions letting differences in central bank independence modulate difference between how state-run and private-sector banks' lending growth tracks differences in money growth. These tests take the same form as those in Table 6, replacing the parameter b in [1] with the expression [8], in which $p_{i,t}$ is central bank independence.

The economy-level regressions 7.1 through 7.2 show that aggregate lending result from Table 4 loses significance, while the aggregate capital spending result survives. The bank-level regressions 7.3 and 7.4 show a clear interaction effect: where the central bank is maximally independent, state-run banks' lending growth is insignificantly different from that of private-sector banks following increased money growth, with p-levels of 0.71 and 0.85 using the parameters and covariance matrices from 7.3 and 7.4, respectively. Where the central bank is minimally independent, that is maximally subject to political pressure, state-run banks' lending growth is significantly correlated with prior money growth while private-sector banks' lending is not.

The Table 7 results also survive a battery of robustness checks. Table 7 uses the Crowe and Meade central bank independence measure; using Alpanda and Honig's (2010) central bank *de facto* independence index instead to classify central banks as independent yields qualitatively similar results. Augmenting the Table 7 regressions by including the interaction of central bank independence with the state-controlled bank dummy or fractional importance generates qualitatively similar results. Including only those interactions (the specification used in Table 5) restores the baseline results.

Rerunning these bank-level difference-in-difference regressions, but replacing central bank independence with the product of measures of the political sensitivity and effectiveness of the civil service, reveals individual state-run banks boosting lending more where civil servants are more effective and more responsive to political pressure.¹² This finding reconciles seemingly discordant results in prior

¹² A country's civil service is effective if its government effectiveness index (Kaufmann, Kraay and Mastruzzi, 2010) exceeds its sample median. A country's civil service is sensitive to political pressure if the average response to

studies using data from individual countries. Das et al (2015) find state-run banks relatively sluggish in responding to changes in cash reserve ratios in India (save in rural areas). In contrast, Deng *et al.* (2014) report state-run banks boosting lending after money growth increased amid the 2008 financial crisis in China. Das *et al.* (2015) as argue that India's cumbersome rules and regulations explain state-run banks' sluggish responses. Consistent with this, our data rank India's civil service as very ineffective. Other work characterizes India's civil service as profoundly undisciplined and unresponsive to central government directives (Das 2005; Mathur 2014). Although China is not in our civil service characteristics data, the behaviour of state-run banks in China (Deng *et al.* (2014) is consistent with studies showing its civil service to be both highly effective (Burns 20004) and highly politicized (MacGregor 2010).

Overall, independence of the central bank and effectiveness of civil servants modulate state-run banks' response to monetary policy. These findings are consistent with a command and control channel operating better in countries where civil servants are more obedient and effective.

5.2 Privatizations

We propose that faster money growth presages boosts in state-run bank lending, but not private-sector bank lending, because civil servants in state-run banks are subject to political pressure. If little else about the banks changes upon their privatizations, we have a clean natural experiment. However, other things may well change too. For example, if the privatized bank's loan portfolio changes, changes in its lending behaviour might merely reflect its new borrowers' different credit needs. Still, the exercise is potentially useful because persistent factors such as geographical focus (Berger *et al.* 2005) and switching costs (Rajan 1992) plausibly deter borrowers from changing banks. Also, privatizations can be

two survey questions (Q8.b and Q8.e) in the Quality of Government Expert Survey Dataset (Teorell, Dahlström and Dahlberg, 2011) exceeds its sample median. The two questions ask experts to evaluate how fully public sector employees strive to implement (1) the ideology of the party/parties in power, and (2) the policies of the top political leadership.

drawn-out processes, so borrowers wanting to switch and banks wanting to offload their nonperforming loans onto the state (Berger *et al.* 2005) can do so well ahead of their actual privatization dates.

These caveats in mind, we examine how the differential responsiveness of state-run banks to monetary growth changes after their privatizations.¹³ If political pressure makes a state-run bank more responsive to money growth, any differential responsiveness would disappear upon its privatization. If state-run banks instead merely had different sorts of borrowers, and their loan portfolios changed little with their privatizations, no such change in responsiveness would be evident.

We begin with a large sample of bank privatizations provided by Megginson (2005) and augment these data with more recent transactions from the Privatization Barometer and World Bank privatization transactions databases. This is necessary because our main bank-level panel includes only the largest banks in each country, and only 5 of these are privatized during our observation window. Privatization often occurs in stages, and state-run banks are sometimes incompletely privatized. We follow the literature and consider the date of the first privatization transaction (Boubakri *et al.* 2005) when more than 10% of the bank is transferred to private owners. If residual state ownership implies continued political pressure on lending decisions, this should work against our finding differences in the responsiveness of their lending to money growth after privatizations. We then merge these data with our BankScope and monetary base data. In this case, we only consider unconsolidated statements, because consolidation could be done with different sets of related firms before and after privatization. The sample includes only privatized banks for which data are available in the years both immediately before, $t = -1$, and immediately after, $t = +1$, the privatization year, $t = 0$. Therefore our tests include two years of loan growth data for each privatization.

Table 8 summarizes the results of these event study tests. Regression 8.1 explains real lending

¹³ We do not investigate bank nationalizations because these occur disproportionately amid financial crises.

growth with money growth, an *after privatization* dummy, and the interaction of the two, all controlling for bank fixed-effects. Regressions 8.2 and 8.3 augment this with bank size and liquidity and their interactions with money growth, with 8.3 using a stepwise regression to introduce the additional controls, given possible multicollinearity issues in the small sample. All the specifications show a bank's lending ceasing (the sums of the appropriate coefficients are always insignificant) to co-vary with money growth after its privatization. The point estimates range from -0.89 to -1.06, linking a one percentage point increase in money growth the prior year to a bit less than a percentage point lower loan growth after privatization than before privatization.

A few other results in the table merit note. First, the main-effect of money growth on loan growth is positive and significant, except in 8.2 where the full set of control variables and interactions is included. The significant coefficients indicate that a one percentage point boost to money growth over the prior year presages a 0.65 percentage point boost to state-run banks' lending growth prior to their privatizations. This affirms our baseline findings that state-run banks' lending responds significantly to monetary growth; while otherwise comparable private-sector banks' does not. Second, the sum of the regression coefficients for the money growth rate and the cross term ranges from -0.30 to 0.37, and is always insignificant. Thus, after privatization, a banks' credit growth does not track money growth. Third, the main-effect of the *after privatization* dummy, though significant in regression 8.2 becomes insignificant in regression 8.3 when control variables are introduced using the stepwise method. This suggests that privatized banks' lending does not pick up significantly immediately after their privatizations. Loan growth might indicate new clients with different characteristics or old clients borrowing more. Even were the former the main explanation, our short observation window arguably mitigates this concern.

In summary, privatized banks' lending no longer tracks monetary growth after their privatizations. The magnitude of this change is consistent with our baseline findings that state-run

banks' lending correlates with monetary growth significantly more than does private-sector banks' lending. The timing of this change around privatizations is consistent with the end of state-control reducing this correlation sensitivity. Of course, this interpretation of Table 8 depends on privatization not corresponding to an abrupt change in the bank's borrower clientele. Subject to this caveat, the Table bolsters the case for a state-run bank stimulus transmission channel.

5.3 Elections

Politicians may press harder for lending growth upon a monetary expansion if elections loom closer (Nordhaus 1975, Alesina *et al.* 1997, Dinc 2005). If so, our baseline results might be stronger during election campaigns than at other times. We therefore test for differences between years immediately prior to free elections (defined using an election dummy set to one if the country has a free election the subsequent year and to zero otherwise in Panel D of Table 1) and other years in the difference between state-run and private-sector banks' lending growth following a unit increase in money growth. To the extent that election cycles are an exogenous source of heterogeneity in political pressure on state-run banks, these tests further contribute to identification. Table 9 summarizes these tests, which are regressions of the forms [13] and [15].

Regression 9.1 shows that, in years preceding free elections, a one percentage point increase in money growth presages aggregate loan growth rising by 0.36 percentage points more in an economy whose banking system is entirely state-run than in an economy with an entirely private-sector banking system. This difference is significant ($p = 0.04$). Indeed, outside election years, the aggregate loan growth result loses both economic and statistical significance. Regression 9.2, shows that, in non-election years, the same unit increase in monetary growth presages a significant 1.34 percentage point higher boost to capital spending growth if the banking system is fully state-run than if it is fully private-sector. In election years, this difference rises significantly ($p = 0.02$) to 1.92 percentage points ($1.34 +$

0.58).

Regressions 9.3 and 9.4 perform analogous exercises using bank-level data. Regression 9.4, which controls for bank and economy-year fixed-effects, shows the same unit boost to money growth in non-election years presaging a significant 0.26 percentage point larger boost to a state-run bank's lending than to lending by an otherwise similar private-sector bank in similar economic conditions. In election years, this difference rises significantly ($p = 0.07$) to 0.53 percentage points ($0.26 + 0.27$). Regression 9.3 which controls for bank and year fixed-effects, preserves the significance of the baseline result, albeit with a reduced coefficient of only 0.16 versus 0.25 in regression 4.6, and reveals a positive but insignificant added difference in election years.

This evidence is predominantly (that is, except for regression 9.3) consistent with state-run banks more effectively transmitting money growth into increased credit and investment during election years. In other words, state-run banks respond to monetary growth more strongly when political pressure to do so is likely stronger.

6. Conclusions

Prior to the 2008 financial crisis, macroeconomists and central bankers had largely come to a consensus (Goodfriend 2007). Rasche and Williams (2007), after a thorough literature review, conclude (p. 490) that "the case for consistently effective short-run monetary stabilization policies is problematic." Central bankers thus focused on low-level inflation targets, and the so-called Great Moderation in many OECD countries' business cycle amplitudes from the mid-1980s through 2006 validated Taylor's (2016) supposition that "the real world will take care of itself."

However, the idea that monetary expansion could counter real economy downturns never completely faded. Rasche and Williams (2007) note that the Federal Reserve Open Markets Committee justified a monetary expansion after the 1987 stock market crash "to cushion the effects on prospective

economic growth.” Expansionary monetary policies were also rolled out to counter the “Y2K” scare about widespread computer failures in January 1st 2000, and again after the “9/11” terrorist attacks to counter “heightened uncertainty and concerns about a deterioration in business conditions both here and abroad damping economic activity.” As the 2008 financial crisis unfolded, the consensus unravelled (Caballero 2010; Mishkin 2011). The central banks of affected countries undertook unprecedented monetary stimulus policies (Mishkin 2009; Claessens *et al.* 2010; Bernanke 2012), even as their benchmark interest rates fell into the zero-lower bound zone, where even neo-Keynesian macroeconomists saw monetary policy becoming ineffective (Tobin 1947; Abbassi and Linzert 2012). Thus, despite unresolved major theoretical and empirical questions about its effectiveness, monetary expansion seems a preferred macroeconomic policy variable, perhaps because it imposes fewer political and structural constraints on policy-makers than do alternative potential policy variables.

We examine whether the effectiveness of monetary policy varies with state control over economies’ banking systems and over individual banks. The findings above show that faster money growth precedes faster loan growth by state-run banks, but not by private-sector banks. At the economy-level, money growth precedes faster bank credit growth and capital investment growth by greater margins in economies whose banking systems are more fully composed of state-run banks, but does not precede faster growth in either bank credit or capital investment in economies whose large banks are entirely private-sector. A series of identification tests successively pare away alternative causality scenarios to leave a command and control channel for the transmission of a macro-stimulus policy as the most plausible explanation of these findings.

The precise operation of this channel is relegated to future research. One obvious possibility is government officials “jawboning” state-run banks (Shleifer and Vishny 1994) to lend more in response to increased money growth, or merely pressuring state-run banks to boost lending during a downturn irrespective of monetary growth, as Coleman and Feler (2015) find in Brazil. Another is government

officials “jawboning” politically connected firms or non-financial state-owned enterprises to demand more credit, which state-run banks are then pressed to supply. Either such mechanism is consistent with the substantial empirical literature on state-run banks’ lending responding to political pressure, which often overrides bank-value maximization (La Porta *et al.* 2002, 2003; Sapienza 2004; Dinc 2005, Deng *et al.* 2011; Mian and Khwaja 2011; Morck *et al.* 2011).¹⁴ Regardless of the precise mechanism, our findings demonstrate that its operation involves an increase in money growth, the existence of state-run banks, and scope for political pressure affecting state-run banks’ lending decisions. All are all essential cogs in the mechanism. We therefore tentatively posit the existence of a command and control channel for the transmission of a monetary stimulus; though its dissimilarity to standard channels of monetary policy transmission might argue for a more qualified term, such as a pseudo-monetary policy stimulus (Deng *et al.* 2014).

The ability of political leaders to effect a short-run economic stimulus by ordering state-run banks to lend more may present a dangerous political temptation. State-run banks’ lending constitutes less efficient capital allocation than does lending by private-sector banks (La Porta, *et al.* 2002, 2003; Morck *et al.* 2011), and inefficient capital allocation imposes long-run barriers to economic growth (Levine and King 1993; Rajan and Zingales 1998; Wurgler 2000). A social welfare trade-off thus plausibly exists, with more state-run banks mitigating short-run welfare losses from business cycles but aggravating long-run costs of capital misallocation. Because myopia can distort self-interested politicians’ priorities (Nordhaus 1975; Alesina *et al.* 1997; Dinc 2005; Micco *et al.* 2007), government policy might compromise social welfare by making excessive use of such a command and control stimulus channel. .

¹⁴ Recent work seeks to resolve such ambiguities using loan-level application and credit data (e.g. Puri *et al.* 2011; Jiménez *et al.* 2014). Unfortunately, comparable data for enough countries and years to be of use in this context are not presently available.

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Table I: Variable definitions and sources

Panel A: Variables reflecting state control over banks

- State-run** Bank-year panel dummy set to 1 if the bank has a state organ as ultimate controlling shareholder. Control is imputed to the largest blockholder whose voting control, direct and indirect, sum to at least 10%. Indirect control is inferred using the “weakest link” method (La Porta *et al.* 1999).
- % State-run** Economy-year panel lagged credit-weighted fraction of banks ultimately state-run

Panel B: Main monetary policy and outcome measures

- Capital Spending Growth** Economy-year panel of annual real growth rate of gross fixed capital spending, $(capex_{t+1} - capex_t)/capex_t$ and winsorized at 10% level over the full panel. Gross fixed capital spending is seasonally adjusted total value of producers’ acquisitions, less disposals, of fixed assets plus certain additions to the value of non-produced assets (e.g. subsoil assets or major improvements in the quantity, quality, or productivity of land), deflated by the producer price index. We take seasonally adjusted values from either the reporting country or the IMF, if available; and otherwise run a rolling regression for 5 prior years of gross fixed capital spending on quarter dummies to calculate seasonal adjusted values. Source: IMF International Financial Statistics (IFS) Database: National Accounts and Population, Gross Fixed Capital Spending (line 93e).
- Economy Loan Growth** Economy-year panel of real growth rates of domestic credit provided by banking sector. Aggregate loans are defined as domestic credit provided by banking sector over GDP from WDI times GDP in current local currency. Each country’s CPI index is used to deflate nominal aggregates. The growth rate is winsorized at 10% level within the entire panel. Source: WDI.
- Loan Growth** Bank-year panel of real growth rates in gross loans, i.e. $(gross\ loans_{t+1} - gross\ loans_t) / gross\ loans_t$, deflated using the producer price index and winsorized at 10% within the entire panel. If gross loans are missing net loans are used. Source: BankScope.
- Money growth** Economy-year panel of nominal monetary base growth during the last 6-12 months of the prior year, $(monetary\ base_t - monetary\ base_{t-1}) / monetary\ base_t$ winsorized at the 10% level within entire panel. Seasonally adjusted values are used if last 6 months monetary based growth is used. If seasonally adjusted values are not available in the dataset seasonal adjustment is made as for Capex Growth by using month dummies. Euro-zone countries are considered one economy in calculating this variable after adoption of the euro. Source: IMF International Financial Statistics (IFS) Database, Central Bank Survey, section 10, country table line 14.

Panel C: Control variables

- Central bank independence** Crowe and Meade’s (2008) independence index reflecting appointment procedures for head of central bank, resolution of conflict between central bank and executive branch, existence of explicit policy target, and rules limiting lending to government. The index is cross-sectional.
- Exchange Rate Depreciation** Percent change in the exchange rate measured as local currency in US dollar, over the prior 12 months. A more positive value implies more local currency depreciation against the U.S. dollar. Source: IMF Financial Statistics.
- Fiscal stimulus** Economy-level panel of changes in fiscal balance during the prior 12 month, as a fraction of the prior years’ year-end nominal GDP. Sources: Government surplus or deficit data are from DataStream (DS Mnemonic =.govbala), and supplemented with IMF GFS data on either net operating balances or net lending. These variables are calculated on accounting or cash bases and

for the overall government, central government, or budgetary central government; and we take data as available in those orders of priority. Net operating balances (line anob) are revenue (a1) less expenses (a2). Revenues includes taxes, social contributions, grants and other revenues; expenses include compensation of employees, use of goods and services, consumption of fixed capital, interest, subsidies, grants, social benefits and other expenses (GFSM manual 2001). Net cash inflow from operating activities (ccio) is cash receipts (c1) less payments for (c2) operating activities. Net lending/borrowing (anlb) is net operating balance (anob) less net acquisition of nonfinancial assets (a31). The cash equivalent, the cash surplus/deficit (ccsd), is net cash inflow from operating activities (ccio) less net cash outflow from investments in nonfinancial assets (c31).

Foreign-controlled	Bank-year panel dummy set to 1 if the bank is a subsidiary of a foreign bank. Constructed analogously to State.
Fraction of Connected Firms	Politically connected firms as percent of market capitalization. Source: Faccio (2006).
GDP Growth	GDP growth in constant local currency lagged by one year. GDP growth is calculated as $[GDP(t) - GDP(t-1)]/GDP(t-1)$. Source: World Bank national accounts, OECD National Accounts.
State-directed Investment	Economy-level panel data of lagged annual government investment as a share of total investment. Source Economic Freedom of the World Index.
Liquidity	Bank-year panel variable equal to the bank's previous year-end ratio of government securities plus cash and due from banks to total assets. Source: BankScope.
Output gap	Potential GDP less actual GDP, as a percent of potential GDP, all lagged one year. Potential GDP is estimated using the filter developed by Hodrick and Prescott (1997) using past annual GDP growth, with the smoothing parameter of 6.25 they suggest for annual GDP data.
Size	Bank-year panel variable equal to prior fiscal year-end log total assets in USD. Source: BankScope
Transfers and subsidies	Economy-level panel data of lagged annual measure of general government transfers and subsidies as a share of GDP. Source Economic Freedom of the World Index.
Election years	Dummy variable set to one if the country will hold a free election the next year and to zero otherwise. Election dates (presidential elections for presidential systems and parliamentary elections for parliamentary and assembly-elected presidential systems are from the International Institute for Democracy and Electoral Assistance (IDEA) database. The system in effect in each country each year is from the World Bank Database of Political Institutions (Beck <i>et al.</i> , 2001). The variable is zero for elections (or countries) classified as "not free" by Freedom House.

Table II: Economy-level descriptive statistics of main variables

Economy-level means and standard deviations of key variables for 2001 – 2011. Money growth is prior 12 month monetary base growth. Money growth and annual loan growth are economy-level means of bank-level data. Money growth, loan growth and capex growth are winsorized at 10%. Variables are defined in Table 1.

	<i>Money growth</i>		<i>Loan Growth</i>		<i>Capital Spending Growth</i>		<i>%State</i>
	<i>Mean</i>	σ	<i>Mean</i>	σ	<i>Mean</i>	Σ	
Argentina	0.212	0.083	0.041	0.102	0.061	0.095	57
Austria	0.112	0.090	0.070	0.096	-0.007	0.045	0
Brazil	0.063	0.061	0.097	0.117	0.026	0.023	43
Canada	0.037	0.017	0.049	0.080	0.033	0.064	0
Colombia	0.181	0.031	0.112	0.084	0.078	0.073	13
Denmark	0.088	0.087	0.094	0.113	-0.007	0.060	0
Egypt	0.207	0.117	0.010	0.121	0.065	0.129	94
Finland	0.144	0.096	0.085	0.142	NA	NA	0
France	0.112	0.086	0.084	0.101	0.023	0.038	12
Germany	0.107	0.089	0.041	0.108	-0.013	0.042	25
Greece	0.162	0.083	0.139	0.112	0.015	0.105	79
Hong Kong	0.112	0.089	0.078	0.098	NA	NA	3
India	0.155	0.062	0.172	0.073	NA	NA	100
Indonesia	0.156	0.075	0.140	0.105	0.084	0.048	93
Ireland	0.141	0.074	0.145	0.115	0.004	0.081	0
Israel	0.013	0.032	0.011	0.068	-0.002	0.054	56
Italy	0.148	0.077	0.085	0.085	-0.011	0.059	0
Japan	0.049	0.062	0.000	0.052	-0.031	0.040	20
Jordan	0.093	0.074	0.099	0.109	NA	NA	7
Kenya	0.102	0.037	0.076	0.094	NA	NA	73
Korea	0.084	0.093	0.087	0.087	0.033	0.058	53
Malaysia	0.076	0.038	0.069	0.052	0.025	0.075	6
Mexico	0.141	0.038	0.061	0.144	0.043	0.075	0
Netherlands	0.145	0.079	0.039	0.076	0.004	0.076	26
Norway	0.172	0.097	0.079	0.040	0.030	0.107	59
Pakistan	0.123	0.051	0.126	0.115	NA	NA	93
Peru	0.127	0.084	0.074	0.142	0.082	0.062	12
Philippines	0.175	0.100	0.049	0.131	0.002	0.074	6
Portugal	0.132	0.091	0.086	0.073	NA	NA	10
Singapore	0.094	0.076	0.048	0.064	NA	NA	42
South Africa	0.152	0.013	0.087	0.127	0.090	0.083	0
Spain	0.134	0.086	0.118	0.114	0.029	0.074	10
Sri Lanka	0.137	0.030	0.048	0.106	NA	NA	58
Sweden	0.048	0.087	0.072	0.075	0.023	0.073	0
Switzerland	0.046	0.065	0.039	0.085	0.015	0.031	29
Thailand	0.087	0.053	0.017	0.078	0.021	0.070	51
Turkey	0.251	0.027	0.146	0.103	0.035	0.093	22
United Kingdom	0.174	0.103	0.046	0.112	-0.001	0.099	0
United States	0.074	0.080	0.041	0.120	-0.021	0.046	0
Venezuela	0.224	0.112	0.070	0.179	0.061	0.095	0

Table III. Simple correlations

Sample is annual data for the 40 economies listed in Table 1 from 2001 to 2011. We first collapse panel data at the economy-level and then calculate pairwise correlations of economy-level averages. Numbers in the second row are p-levels. Boldface indicates significance at 10% or better. Money growth rate is over the twelve months prior to the year in question. Variables are as defined in Table 1.

	<i>Bank Loan Growth</i>	<i>Money growth</i>	<i>State Run</i>	<i>Bank Size</i>	<i>Bank Liquidity</i>	<i>Fiscal Stimulus</i>	<i>Output Gap</i>
<i>Money growth</i>	0.27 (0.09)						
<i>State Run</i>	0.06 (0.71)	0.19 (0.23)					
<i>Bank Size</i>	-0.19 (0.24)	-0.03 (0.84)	-0.51 (0.00)				
<i>Bank Liquidity</i>	0.29 (0.06)	0.39 (0.01)	0.56 (0.00)	-0.39 (0.01)			
<i>Fiscal Policy</i>	0.05 (0.79)	0.15 (0.34)	-0.16 (0.34)	0.27 (0.11)	-0.02 (0.91)		
<i>Output Gap</i>	-0.08 (0.62)	-0.01 (0.95)	-0.25 (0.12)	0.23 (0.16)	-0.36 (0.02)	0.04 (0.79)	
<i>Exchange Rate</i>	-0.11 (0.51)	0.44 (0.01)	0.20 (0.22)	-0.07 (0.66)	0.24 (0.14)	0.10 (0.55)	-0.35 (0.03)

Table IV. Baseline regression results

Economy-level sample is 40 economies for lending growth and 30 economies for capital spending growth, as listed in Table 2. Bank-level sample is 288 large banks in those economies. Money growth rate is change in monetary base over beginning of period monetary base, measured over the prior 12 months. Variables are as in Table 1. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface, using economy-level clustering, euro-zone economies considered one cluster after the introduction of the euro.

Aggregation level	economy	economy	bank	bank	bank	bank	bank
Left-hand side variable: growth in	lending	capital spending	lending	lending	lending	lending	lending
Regression	4.1	4.2	4.3	4.4	4.5	4.6	4.7
Money growth	-0.06 (0.23)	-0.27 (0.00)	-0.16 (0.01)	-0.04 (0.55)		0.02 (0.95)	
Fraction of banking system state-run x money growth	0.23 (0.06)	0.79 (0.00)					
State-run bank indicator x money growth			0.30 (0.00)	0.22 (0.01)	0.20 (0.04)	0.25 (0.00)	0.26 (0.01)
Fraction of banking system state-run	0.02 (0.32)	0.16 (0.04)					
State-run bank indicator			-0.01 (0.68)	-0.00 (0.93)	-0.00 (1.00)	-0.01 (0.81)	0.00 (0.98)
Bank size x money growth						-0.01 (0.73)	-0.01 (0.51)
Bank liquidity x money growth						-0.02 (0.96)	-0.34 (0.34)
Bank size						-0.03 (0.32)	-0.01 (0.69)
Bank liquidity						0.07 (0.39)	0.08 (0.36)
Fixed-effects	economy	economy	bank	bank & year	bank & economy ⊗ year	bank & year	bank & economy ⊗ year
Adjusted R^2	0.23	0.21	0.21	0.30	0.49	0.33	0.50
Observations	246	183	1,261	1,261	1,261	1,098	1,098
Cluster	economy	economy	economy	economy	economy	economy	economy

Table V. Fiscal and exchange rate policy, and business cycle sensitivity

Money growth rate is change in monetary base over beginning of period monetary base, measured over the prior 12 months. Variables are as in Table 1. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface, with economy-level clustering, Euro-zone countries considered one cluster after the introduction of the euro.

Aggregation level	economy		bank		economy		Bank		economy		Bank	
Left-hand side variable:	capital				capital				capital			
growth in	lending	spending	lending	lending	lending	spending	lending	lending	lending	spending	Lending	lending
Regression	5.1	5.2	5.3	5.4	5.5	5.6	5.7	5.8	5.9	5.10	5.11	5.12
Money growth	-0.07 (0.12)	-0.24 (0.00)	0.00 (0.99)	n/a	-0.04 (0.44)	-0.20 (0.00)	-0.02 (0.95)	n/a	-0.05 (0.24)	-0.11 (0.01)	0.05 (0.90)	n/a
State-run ^a x money growth	0.33 (0.00)	0.86 (0.00)	0.25 (0.00)	0.22 (0.03)	0.22 (0.10)	0.63 (0.00)	0.25 (0.00)	0.27 (0.00)	0.22 (0.01)	0.63 (0.00)	0.23 (0.00)	0.26 (0.01)
Bank size x money growth			0.00 (0.89)	-0.01 (0.60)			-0.01 (0.84)	-0.02 (0.31)			-0.01 (0.7)	-0.01 (0.51)
Bank liquidity x money growth			-0.05 (0.91)	-0.47 (0.2)			0.01 (0.97)	-0.37 (0.28)			-0.07 (0.87)	-0.34 (0.35)
State-run ^a	0.02 (0.38)	0.16 (0.01)	-0.05 (0.12)	-0.04 (0.55)	0.03 (0.24)	0.17 (0.01)	-0.01 (0.80)	0.00 (1.00)	0.03 (0.29)	0.24 (0.00)	0.00 (0.99)	0.00 (0.97)
Bank size			-0.02 (0.52)	0 (0.96)			-0.02 (0.44)	-0.01 (0.68)			-0.02 (0.36)	-0.01 (0.68)
Bank liquidity			0.12 (0.14)	0.09 (0.41)			0.07 (0.39)	0.09 (0.33)			0.06 (0.44)	0.08 (0.36)
Additional control:		fiscal stimulus			exchange rate depreciation					output gap		
State-run ^a x additional control variable	0.81 (0.56)	2.70 (0.14)	0.62 (0.51)	0.06 (0.92)	0.10 (0.06)	0.19 (0.06)	-0.03 (0.19)	-0.04 (0.08)	0.98 (0.11)	1.18 (0.02)	0.42 (0.40)	0.04 (0.94)
Additional control variable	-0.05 (0.90)	0.06 (0.89)	-0.12 (0.75)	n/a	-0.09 (0.01)	-0.10 (0.12)	-0.03 (0.35)	n/a	-0.28 (0.35)	-1.67 (0.00)	0.14 (0.75)	n/a
Fixed-effects	economy	economy	bank & year	bank & economy ⊗ year	economy	economy	bank & year	bank & economy ⊗ year	economy	economy	bank & year	bank & economy ⊗ year
Adjusted R ²	0.23	0.22	0.34	0.50	0.25	0.21	0.34	0.50	0.25	0.35	0.33	0.50
Observations	246	182	954	954	246	188	1,072	1,072	246	188	1,098	1,098

a. State-run is the fraction of the banking system under state control in economy-level regressions and the state-run bank dummy in bank-level regressions

Table VI. The reach of the state

Money growth rate is change in monetary base over beginning of period monetary base, measured over the prior 12 months. Variables are as in Table 1. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface, with economy-level clustering Euro-zone countries considered one cluster after the introduction of the euro.

Aggregation level	economy		Bank		economy		Bank		economy		bank	
Left-hand side variable:	capital				capital				capital			
growth in	lending	spending	lending	lending	lending	spending	lending	lending	lending	spending	lending	lending
Regression	6.1	6.2	6.3	6.4	6.5	6.6	6.7	6.8	6.9	6.10	6.11	6.12
Money growth	-0.06 (-0.69)	-0.17 (0.44)	-0.06 (0.88)		-0.15 (0.24)	-0.05 (0.70)	0.10 (0.81)		-0.09 (0.01)	-0.26 (0.00)	0.15 (0.72)	
State-run ^a x money growth	0.27 (0.29)	0.94 (0.07)	0.29 (0.10)	0.31 (0.14)	0.55 (0.00)	0.31 (0.27)	0.33 (0.01)	0.33 (0.03)	0.44 (0.00)	0.60 (0.00)	0.28 (0.00)	0.17 (0.08)
Bank size x money growth			-0.00 (0.85)	-0.01 (0.69)			-0.02 (0.64)	-0.01 (0.71)			-0.02 (0.64)	-0.00 (0.87)
Bank liquidity x money growth			0.11 (0.80)	0.09 (0.32)			0.09 (0.82)	0.09 (0.33)			-0.29 (0.45)	0.12 (0.29)
State-run ^a	0.02 (0.47)	0.15 (0.03)	-0.01 (0.80)	-0.00 (0.98)	0.02 (0.53)	0.15 (0.05)	0.01 (0.64)	0.02 (0.64)	0.01 (0.73)	0.15 (0.04)	-0.06 (0.11)	-0.05 (0.45)
Bank size			-0.03 (0.33)	-0.02 (0.29)			-0.02 (0.35)	-0.02 (0.36)			-0.02 (0.46)	-0.02 (0.28)
Bank liquidity			0.08 (0.80)	-0.34 (0.35)			0.07 (0.82)	-0.33 (0.36)			0.18 (0.03)	-0.61 (0.20)
Additional Control	government transfers & subsidies				State-directed investment				politically-connected firms			
State-run ^a x additional control x money growth	0.00 (0.77)	-0.02 (0.56)	-0.00 (0.69)	-0.00 (0.73)	-0.01 (0.20)	0.02 (0.02)	-0.00 (0.36)	-0.00 (0.63)	-0.01 (0.50)	0.01 (0.37)	-0.00 (0.59)	0.01 (0.46)
Additional control x money growth	0.00 (0.87)	-0.01 (0.65)	0.00 (0.98)		0.00 (0.65)	-0.01 (0.11)	-0.00 (0.77)		-0.00 (0.97)	0.00 (0.65)	0.00 (0.63)	
Additional control	0.00 (0.94)	-0.00 (0.93)	-0.00 (0.46)		0.00 (0.84)	0.00 (0.96)	0.00 (0.06)					
Fixed-effects	economy	economy	bank & year	bank & economy ⊗ year	economy	economy	bank & year	bank & economy ⊗ year	economy	economy	bank & year	bank & economy ⊗ year
Adjusted R ²	0.24	0.21	0.34	0.50	0.25	0.21	0.34	0.50	0.20	0.18	0.36	0.49
Observations	232	183	1,072	1,072	230	180	1,056	1,056	215	172	913	913

a. State-run is the fraction of the banking system under state control in economy-level regressions and the state-run bank dummy in bank-level regressions

Table VII. Heterogeneity in political control over central banks

Money growth rate is change in monetary base over beginning of period monetary base, measured over the prior 12 months. Variables are as in Table 1. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface, with economy-level clustering Euro-zone countries considered one cluster after the introduction of the euro.

Aggregation level	economy	economy	bank	bank
Left-hand side variable: growth in	lending	capital spending	lending	lending
Regression	7.1	7.2	7.3	7.4
Money growth	0.06 (0.61)	-0.38 (0.00)	-0.41 (0.15)	
State-run x money growth	-0.05 (0.87)	0.94 (0.06)	0.74 (0.00)	0.78 (0.01)
Bank size x money growth			-0.01 (0.71)	-0.02 (0.22)
Bank liquidity x money growth			-0.14 (0.68)	-0.35 (0.34)
State-run	0.01 (0.61)	0.17 (0.02)	-0.04 (0.20)	-0.04 (0.56)
Bank size			-0.02 (0.42)	-0.00 (0.86)
Bank liquidity			0.15 (0.68)	0.08 (0.34)
State-run ^a x central bank independence x money growth	0.48 (0.32)	-0.22 (0.72)	-0.80 (0.02)	-0.80 (0.06)
Central bank independence x money growth	-0.19 (0.26)	0.18 (0.34)	0.74 (0.00)	
Fixed-effects	economy	economy	bank & year	bank & economy ⊗ year
Adjusted R^2	0.25	0.21	0.36	0.51
Observations	213	179	1000	1,000

^a. State-run is the fraction of the banking system under state control in economy-level regressions and the state-run bank dummy in bank-level regressions

Table VIII. Privatizations

Left-hand side variable is bank-level *loan growth*, defined as the bank's year-on-year growth rate in real gross loans. Sample include observations within 1 year of the privatization year (exactly two observations per privatization: $t = -1, +1$). The sample includes only banks with at least one observation both before and after the privatization year. Money growth is for the prior 12 months. Regression in column 3 is a stepwise regression, where additional control variables are included with forward selection at 10% probability. All regressions include bank fixed-effects and residuals are clustered by economy and Euro-zone countries considered one economy after introduction of the euro. Variables are defined in Table 1. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface.

	OLS	OLS	Stepwise
	8.1	8.2	8.3
Money growth rate	0.67 (0.02)	1.43 (0.54)	0.64 (0.00)
After privatization dummy X Money growth rate	-0.89 (0.02)	-1.06 (0.00)	-0.94 (0.01)
Bank size X Money growth rate		-0.08 (0.69)	drops
Bank liquidity X Money growth rate		-0.23 (0.92)	drops
After privatization dummy	0.12 (0.12)	0.11 (0.06)	0.10 (0.27)
Bank size		-0.02 (0.93)	drops
Bank liquidity		-1.26 (0.00)	-1.28 (0.00)
Fixed-effects	Bank	Bank	Bank
Adjusted R ²	0.64	0.77	0.77
Number of Observations	36	36	36
Number of Banks	18	18	18

Table IX. Election cycles

Economy-level sample is the 40 countries for lending growth and 30 countries for capital spending growth listed in Table 2. Bank-level sample is 288 large banks in those economies. Money growth rate is change in monetary base over beginning of period monetary base, measured over the prior 12 months. Definitions of election years and other variables are given in Table 1. Variables are not winsorized. Residuals are clustered by economy, with the Euro-zone considered one economy after the introduction of the euro. Numbers in parentheses are p-values with coefficients significant at 10% or better in boldface.

Aggregation level	economy	economy	bank	bank
Left-hand side variable: growth in		capital spending		
Regression	lending		lending	lending
	9.1	9.2	9.3	9.4
Money growth	0.01 (0.87)	-0.12 (0.15)	0.04 (0.88)	
State-run x money growth	0.01 (0.91)	1.34 (0.02)	0.16 (0.00)	0.26 (0.00)
Bank size x money growth			-0.01 (0.58)	-0.03 (0.14)
Bank liquidity x money growth			-0.10 (0.70)	-0.54 (0.19)
State-run	0.03 (0.44)	0.24 (0.04)	0.00 (0.94)	-0.00 (1.00)
Bank size			-0.04 (0.24)	-0.02 (0.60)
Bank liquidity			0.15 (0.15)	0.17 (0.15)
State-run ^a x election year x money growth	0.36 (0.04)	0.58 (0.02)	0.13 (0.53)	0.27 (0.07)
Election year x money growth	-0.11 (0.18)	-0.12 (0.09)	0.02 (0.79)	
Election year	0.01 (0.63)	0.00 (0.80)		
Fixed-effects	economy	economy	bank & year	bank & economy ⊗ year
Adjusted R^2	0.07	0.21	0.30	0.48
Observations	239	183	1,063	1,063

^a. State-run is the fraction of the banking system under state control in economy-level regressions and the state-run bank dummy in bank-level regressions