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STATE-RUN BANKS, MONEY GROWTH, AND THE REAL ECONOMY

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ABSTRACT

Aggregate credit and investment growth correlate with prior money growth more strongly in economies whose banking systems are more fully state-run. Within countries, individual state-run banks' lending correlates with prior money growth, while otherwise similar private-sector banks' lending does not. 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 higher correlation of state-run banks' lending with prior money growth if political pressure is stronger. These findings are consistent with a command-and-control channel of monetary stimulus transmission operating via state-run banks.

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1. Introduction

Money supply growth is thought to spur the real economy, at least in the short-run, by increasing bank lending, at least in part.[1] Banks' value-maximizing response to a monetary stimulus depends on expectations about ensuing real effects and incentives (Lucas 1972). A bank expecting no real effect would rationally ignore the stimulus; one expecting real effects might respond. State-run banks appear to respond to political, rather than economic incentives or signals (Saunders et al. 1990; Rajan and Zingales, 2003, 2004; Almeida and Wolfenzon 2006; Caprio et al. 2007; Perotti and Vorage, 2008; Laeven and Levine 2010; Morck et al. 2011).

The real effects of money growth appear to differ across countries. Bouis et al. (2013) conclude that "monetary policy stimulus did not show up in stronger growth" in OECD economies after the 2008 crises. In contrast, Deng et al. (2011) link China's 2008 monetary stimulus to substantial real effects, and associate this successful monetary stimulus with state-run banks obeying Party orders to boost lending.

Substantial parts of many countries' banking systems are state-run (LaPorta et al., 2002). If the Chinese example generalizes across countries, money growth might have more economically significant real effects where state-run banks are more important. However, generalization is not *a priori* obvious. Self-interest motivates bureaucrats (Wilson 1989); and Hood (2011) argues their career concerns induce blame-aversion. Blame-averse state-run bank managers, fearing responsibility for non-performing loans, might restrict credit especially tightly in downturns if they perceive faster money growth as indicating a worsening downturn. Most importantly, state-run bank managers' obedience to politicians induces inefficient politically-motivated loans (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; Coleman and Feler, 2015), perhaps regardless of monetary stimuli.

[1] See Gordon (2007) for an overview, and Caballero (2010) for a more critical perspective.

To explore these issues, we assemble bank-level panel data for the largest banks in each of 40 economies from 2001 through 2011. Following La Porta et al. (1999), we identify each bank's ultimate controlling shareholder, if any exists, each year. If this is a government entity, the bank is defined as state-run. We also build economy-level panel data gauging the importance of state control over an economy's banking sector by the credit-weighted fraction of banks that are state-run each year.

We find a one percentage point boost to money growth presaging a 0.26 percentage point higher increase in lending growth by a state-run bank than by a private-sector bank of similar size and liquidity in the same country. The same boost to money growth presages a 0.23 percentage point higher growth rate in aggregate bank lending and nearly a 0.8 percentage point higher growth rate in capital spending in a country whose banking system is fully state-run versus fully private-sector. Both findings survive batteries of robustness checks. The bank-level regressions are robust to including either bank and year or bank and economy-year fixed-effects; the economy-level regressions include economy fixed-effects. These findings are consistent with a command-and-control monetary policy transmission channel: state-run banks' lending growth may rise following increased money growth because political pressure encourages this.

A series of econometric results weigh against alternative explanations and in favour of this thesis. First, the bank-level results weigh against many familiar reverse causality scenarios (Mishkin 1996; Bovin et al. 2011). For example, shocks boosting demand for credit can boost lending, investment, and money growth if the central bank accommodates the increased demand. Were this to explain our bank-level findings, the exogenous shock would have to boost demand for loans from individual state-run banks, but not similarly sized and similarly liquid private-sector banks in the same country.[2] This restricts feasible

[2] Kashyap and Stein (2000), typically cited for showing that smaller less liquid US banks boost lending more after a monetary stimulus, is arguably more important for pioneering identification-by-disaggregation in economics. At the economy-level, bank lending correlates with money growth, but causality is unclear. By disaggregating to the bank-level, they show that their macro-level hypothesis is consistent with the bank-level heterogeneity they

alternative mechanisms to those with scope for such bank-level heterogeneity.

One class of feasible alternative explanations posits state-run and private-sector bank lending to different clienteles, whose loan demand moves differentially with money growth or something correlated with it. For example, money growth might correlate with fiscal expansion. If fiscal measures, such as infrastructure construction, disproportionately flowed through nonfinancial state-owned enterprise or politically connected firms, which disproportionately borrowed from state-run banks, our results might ensue. Or, if money growth correlates with exchange rate depreciation and demand for export financing; our results might ensue if state-run banks disproportionately financed exporters or their foreign buyers. Or, state-run banks' clients' credit demand might be less pro-cyclical.[3] If an activist central bank implemented countercyclical money growth, state-run bank's clients might again demand relatively more loans as money growth picked up. However, regressions letting measures of fiscal expansion, currency depreciation, and the business cycle compete with state control over banks to modulate the link between money growth and bank lending, economy lending, and economy capital spending do not dent the explanatory power of state control over banks.

Another class of feasible alternative explanations is that state-run banks might be more prominent where state intervention is broader in general. A more interventionist state wanting to stimulate the economy might order its many state-owned enterprises and politically-linked firms to invest, its state-run banks to lend them money, and its central bank to expand the money supply to accommodate this. In such an economy, we would observe state-run banks lending on increased money growth. However, state-run banks would be only one cog in a large apparatus of state intervention. If so, state-control over banks might matter more where the state is more interventionist. Further tests show our

detect, while its main alternatives are not. The micro-level findings identify the preferable explanation of the macro-level findings.

[3] In many countries, state-run banks' lending is less pro-cyclical than private-sector banks' lending (Micco and Ugo 2006; Lin et al. 2013; Coleman and Feler 2015).

results to be largely independent of general state intervention, as gauged by the scope of government transfers and subsidies, state-directed investment, and politically connected firms.

One very specific dimension of state power, politicization of the central bank (Crowe and Meade 2008), might affect our findings. A more independent central bank's monetary growth policies might be less synchronized with political leaders' priorities, and thus with their pressure on state-run banks. Consistent with this, increased money growth presages a larger rise in state-run banks' lending relative to private-sector banks' lending where the central banks is less independent. This result reinforces the hypothesis that money growth, if coordinated with political pressure on state-run banks to lend more, presages increased aggregate lending and capital spending.

Additional tests exploit other sources of heterogeneity in more general political pressure. First, state-run bank lending abruptly ceases correlating with prior money growth upon their privatizations. If privatization distances banks from political pressure (Megginson 2005), this supports our hypothesis. Second, in countries with free elections, state-run banks' lending is two to five times more correlated with money growth in election years than in other years, consistent with political pressure intensifying then (Dinc, 2005).

Overall, these findings are consistent with a command-and-control channel for monetary stimulus transmission; that is, with state-run banks obeying politicians' orders to expand credit. The findings thus suggest that state-run banks might be a policy tool for reducing social costs of business cycles (Lucas 1987; Imrohoroglu 2008). However, state control of banks clearly also correlates with socially costly long-run capital misallocation (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). Therefore, state-run banks are not *prima facia* welfare improving. However, a public policy trade-off may exist: state-controlled banks may render a monetary stimulus more effective in the short-run, but impose long-term capital misallocation 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 corporation, its large shareholders, its large shareholders' large shareholders, and so on are identified until we reach a biological person 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%. 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 bank*s in year t-1.

2.2 Samples

Our bank-level sample begins with a 2001 cross-section of ultimate controlling shareholder classifications

from Morck, Yavuz and Yeung (2011, Table 1).[4] We determine the ultimate ownership of these banks for each year from 2001 through 2010.[5] 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 as elaborated 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.

2.3. Monetary Stimulus Variable

Broadly speaking, a monetary stimulus can be a regulatory change increasing banks' ability to lend, a market intervention lowing key interest rates, or an increase in the money supply. We focus on the last

[4] Morck, Yavuz and Yeung (2011) explain the construction of the initial cross-section dataset 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) whose ownership can be identified from BankScope or other sources. The final panel consists of between one and fourteen banks across 44 countries.

[5] 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.

because regulatory changes are infrequent and 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. [6]

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 $\Delta M_{i,j,t} \equiv \left(M_{i,j,t} - M_{i,j,t-1}\right)/M_{i,j,t-1}$.

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 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 calculated for each calendar year using the 12-month money growth depending on the specification. Prior six-month money growth, defined over the second halves of each of these periods, is used as an alternative measure in robustness checks[7]. These

[6] Nonetheless, we revisit changes in interest rates as an alternative monetary stimulus variable in robustness checks below. 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.

[7] 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

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 in local currency, from BankScope, defined as $\Delta credit_{i,j,\mathrm{t}+1} \equiv \left(credit_{i,j,t+1} - credit_{i,j,t}\right)/credit_{i,j,t}$, where the subscripts i, j, 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.[8] 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 A/S from 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 subsidiaries. Indeed, financial

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.

[8] Bushman and Williams (2012), (2013) argue that U.S GAAP and IFRS allow scope for discretion in loan provisioning and discretionary loan provisioning practices, and that this varies across countries.

conglomerates 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" and "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&A 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, 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 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 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 $\Delta capex_{j,t+1} \equiv \left(capex_{j,t+1} - capex_{j,t}\right)/capex_{j,t}$, 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 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 Loan growth is also significantly positively correlated with lagged liquidity. State-run banks are more liquid and smaller than private-sector banks. However in the entire sample, bank liquidity and bank size are negatively correlated.

3. Empirical Methodology and Results

3.1 Baseline economy-level regressions

Our economy-level regressions associate country *j*'s year *t* money growth, $\Delta M_{j,t}$,with 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] $\Delta credit_{j,t+1} = a + b\,\Delta M_{j,t} + controls + e_{j,t}$,

or

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

Their objective is to see if the coefficient b varies significantly with $f_{j,t}$, the state-run fraction of the country's banking system. A varying parameter regression captures this by replacing the regression parameter b in [1a] and [1b] with the linear function

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

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

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

in which the economy fixed-effects subsume *a*, the intercept in [1]. All country-level regressions cluster residuals $e_{j,t}$ by economy, with Eurozone countries one cluster for this purpose only. If monetary growth translates more effectively into 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 regressions. 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. Regressions 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.

Obviously, these regressions demonstrate only correlation. Inferring that a more fully state-run banking system more effectively transmits money growth into real credit and capital spending growth requires considerably more work.

3.2 Economic implications of bank-level heterogeneity

One approach to this problem is identification by disaggregation, pioneered by Kashyap and Stein (2000). After exhaustively surveying estimation techniques in macroeconomics, 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 transmit 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] $\Delta credit_{i,t+1} = a + b\Delta M_{j(i),t} + controls\ + e_{i,t}$,

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? We model this by letting a state-run bank dummy, $\delta_{i,t}$, alter the magnitude of the coefficient b in [4]. That is, the fixed parameter b is replaced by

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

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 regression

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

Augmenting the list of controls with year fixed-effects, denoted λ_t, or with economy-year fixed-effects, denoted $\lambda_{j,t}$, yields the alternative bank-level specifications

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

and

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

In all three alternative specifications, 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 $\Delta M_{j(i),\mathrm{t}}$ varies only at the economy-year level.

The remainder of Table 4 summarizes these tests. Because regression 4.3, based on [6], controls for the bank fixed-effects, it links a one percentage point boost in money growth to a 0.30 percentage point greater increase in lending by a state-run bank than by a private-sector bank in the same economy. This highlights an economically significant heterogeneity in the bank-level data: 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 cut their lending by 0.16 percentage points while state-run banks in the same country at the same time boost their lending by 0.30 – 0.16 = 0.14 percentage points, which is statistically significant (p = 0.07).

Regression 4.4, which follows specification [7a], including year fixed-effects in addition to bank fixed-effects, paints a similar picture. When money growth raises by one percentage point, the typical private-sector bank cuts its lending by an insignificant 0.04 percentage points. This is perhaps unsurprising because the inclusion of year fixed-effects means this coefficient is estimated using banks whose fiscal year-end is not the calendar year-end. A unit boost to money growth presages the typical state-run bank increasing lending by 0.18 = 0.22 – 0.04 percentage points. This is statistically significant (p = 0.03).

Regression 4.5, based on [7b], confirms the differential correlation of lending with money growth for state-run versus private-sector banks after controlling for omitted variables that vary at the country-year level, all of which its economy-year interaction fixed-effects subsume. [9]

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 generic reverse causality explanations

[9] Because a few banks have idiosyncratic fiscal year-ends and we measure money growth relative to banks fiscal years, our data contain limited bank-level variation in $\Delta M_{j(i),t} y_{k,j,t}$. However, this variation is driven by very few observations (about 4% of bank-year observations), and is economically problematic. Excluding these observations does not affect our results; nor does allowing them to determine main effect coefficients.

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 boost lending in response to such developments, while private-sector banks stand aloof, seems *a priori* implausible given 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 private-sector banks.

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

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

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

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 specifications, which we refer to below as our *baseline bank-level regressions*,

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

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

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 regressions. Bank size is the log of its total assets the prior year; bank liquidity is the sum of its government securities, cash and funds due from other banks, all over total assets.[10] 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

[10] Kashap and Stein (2000) measure bank size using a large bank indicator (set to one for banks among the largest 5% 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.

barely budges.

Our data are ill-suited to exploring the Kashyap and Stein (2000) effect further. Their analysis exploits the unusual structure of the American 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 also the 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.[11]

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 a similar economy. 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

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.

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

banks respond more robustly to money growth.

An alternative measure of the strength of a monetary stimulus is 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 drop over either period generate qualitatively similar results, except that b_1 in analogues to regression 4.5 and 4.7 loses statistical significance (*p*-levels range from 0.20 to 0.30). Analogues of regression 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 private-sector banks in the same economies. Both figures are statistically significant.

The bank-level regressions are subjected to further robustness checks. Only 5 banks 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 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 Peterson (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 is 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 those with a biological person or family as an ultimate controlling shareholder and widely held 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 main effect and 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.

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 help 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 customer bases

One set of alternative explanations posits state-run banks' lending varying not with money growth, but with other factors correlated with money growth. Stepped up money growth might coincide with an expansionary fiscal policy, 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 would have to react differently to them.

This sub-section considers the possibility that state-run banks might be altering their lending in step with some other time-varying economic policy or economy characteristic, $p_{j,t}$, rather than money growth. To accommodate the possibility, we consider economy-level regressions of the form

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

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

and bank-level regressions of the form

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

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

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 missing 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 various likely candidates for the 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. If the central bank accommodated this by letting money growth 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 implausible. 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 the Table 4 analogues.

These results are consistent with a unit rise in money growth presaging increased lending by state-run banks, but not by private-sector banks, 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 $p_{j,t}$ = *exchange rate depreciation* (percent change in local currency units per U.S. dollar, positive values imply 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 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. A falling exchange rate might accompany more dramatically weakening aggregate demand in economies with more state-run banking systems, for example.

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 state-run banks than by private-sector banks. The economy level results show that allowing for effects associated with currency depreciation leaves the baseline economy-level results qualitatively unchanged.

4.1.3 Heterogeneous borrower 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, attaining larger values 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; 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 $p_{j,t}$, modulate the link between money growth and bank-credit growth. In the economy-level regressions, this entails replacing the parameter b in [1] with the expression

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

The second and third terms on the righ-hand side of [12] let the strength of the link between money growth, on the one hand, and aggregate credit or capital spending growth, on the other, 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] $$\Delta credit_{j,t+1} = a_1 f_{j,t} + a_2 p_{j,t} + \left(b_0 + b_1 f_{j,t} + b_2 p_{j,t} + b_3 p_{j,t} f_{j,t}\right)\Delta M_{j,t} + \sum_i d_j \lambda_j + e_{j,t}$$

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

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

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

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

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

In these regressions, the coefficients b_1, b_2, and b_3 each have an economic relevance to our arguments. 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 stated-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 general state intervention 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. This is consistent with a broader mechanism of state power over both banks and other firms working to transmit higher money growth into higher real economic growth. However, the bank-level and economy-level results as a whole suggest that state-run banks are likely a critical cog in any broader command and control system.

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 $p_{j(i),t}\,\delta_{i,t}\,\Delta M_{j(i),t}$.[12] 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. Supporting Evidence for a Command and Control Channel

The previous section considered feasible alternative explanations, and rejected or substantially limited each. This section considers other approaches to detecting a command and control channel whereby state-run banks transmit money growth into increased lending and capital spending.

5.1 Central bank independence

Some countries' central banks are more independent from their governments, so their monetary policies might be less synchronized with political leaders' desires to boost economic growth. If so, our results might be stronger where central banks are less independent.[13]

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

[12] 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, if present, subsume the interactions of all of the interventionism measures with money growth.

[13] 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 banks to support its monetary policy.

regressions letting central bank independence modulate difference between state-run and private-sector banks' behaviour following increased money growth. These 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. Replacing central bank independence with broad measures of the political sensitivity and effectiveness of the civil service in the bank-level regressions reveals individual state-run banks boosting lending more if civil servants are more effective and more responsive to political pressure.[14] However, the corresponding coefficients in

[14] 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 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.

the analogous exercise using economy-level data are insignificant.

5.2 Privatizations

When state-run banks are privatized, political pressure on their lending decision may diminish. 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 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 privatizations dates.

These caveats in mind, we examine how the differential responsiveness of state-run banks to monetary growth changes after their privatizations.[15] 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 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. Privatization often occurs in stages, and state-run banks are often 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

[15] We do not investigate bank nationalizations because these occur disproportionately amid financial crises.

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 set of financial 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. Regression 8.1 explains real lending 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 of the specifications show a bank's lending ceasing (the sums of the appropriate coefficients are always insignificant) to covary 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 less 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 private-sector banks' responses are comparatively muted. 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' responsiveness to monetary growth becomes significantly muted 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 borrowers. Subject to this caveat, the Table bolsters the case for a command and control channel of monetary stimulus transmission.

5.3 Election Cycles

Politicians may press harder for monetary expansion as elections loom closer (Nordhaus 1975, Alesina et al. 1997). If so, our baseline results might be stronger during election campaigns than at other times. We therefore use impending 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) accompanied by positive monetary growth as a source of heterogeneity in political pressure on state-run banks. 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 most 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

Taken as a whole, these empirical findings are consistent with a command and control channel for the transmission of a monetary stimulus: that is, with the government stimulating the economy by ordering state-run banks to expand credit when the central bank expands the money supply. Across countries, money growth correlates more strongly with credit and investment growth where state-run banks are more prevalent. Across banks, money growth correlates with higher lending growth by state-run banks, but not by private-sector banks.

A simple explanation of these findings, consistent with much prior work on state-run banks, is

that state-run banks' objective functions assign large weights to obeying politicians' instructions while private-sector banks' objective functions do not (La Porta et al. 2002).[16] These considerations suggest that governments can make a monetary expansion more effective by also ordering state-run banks to lend more.

Further results substantially narrow the range of feasible alternative explanations of our findings. First, the bank-level findings eliminate many simple reverse causality scenarios. For example, a real shock boosting loan demand and the central bank loosening money passively has homogenous implications across banks absent further suppositions. Feasible alternative explanations must let individual state-run banks' lending rise more than does individual private-sector banks' lending after an increase in money growth. Second, controlling for bank size and liquidity and their interaction with money growth in the bank-level regressions leaves the baseline bank-level results unaffected. Hence our findings are unlikely to be an artefact of state-run and private-sector banks being of systematically different size or liquidity with differing access to alternative sources of funds. Third, the baseline bank-level regressions survive the further inclusion of economy-year fixed-effects, which capture any latent factors with variation at the economy-year level, such as other government policies and their interactions with money growth. Feasible alternative explanations must therefore let state-run banks' lending correlate more positively with money growth than does the lending of private-sector banks of similar size and liquidity in similar economic conditions.

The surviving set of feasible alternative explanations contains two broad subsets. One posits state-run banks and private-sector banks, or their clients, responding differently to some other variable that correlates with money growth. For example, money growth might correlate with expansionary fiscal

[16] In contrast, increased money growth presages no statistically discernible change in otherwise similar private-sector banks' lending, or in aggregate credit and investment growth in economies with predominantly private-sector banking systems. Our sample includes only the largest and most liquid banks in each economy, so our findings cannot preclude money growth affecting lending by small or illiquid private-sector banks (Kashyap and Stein 2000).

policy, exchange rate depreciation or the business cycle. State-run banks might disproportionately lend to firms whose credit demand is relatively high during fiscal expansions, exchange rate depreciations, or recessions, for which money growth merely proxies. In regressions that let either faster money growth or one or more of these alternative variables interact with the state-run bank dummy or importance measure, only the interaction with money growth is significant, affirming the primacy of money growth as the variable of interest.

A second class of feasible alternative explanations accepts the key role of money growth, but posits a variable other than state-control over banks actually driving the results. For example, more interventionist governments might apply many levers to boost aggregate investment, and ordering state-run banks to lend more might be only one such lever. However, tests using measures of the general scope of state interventionism generally refute such arguments, underscoring state control over banks as the key variable of interest.

Tentatively rejecting both classes of alternative explanations, we muster further evidence consistent with greater political pressure on state-run banks explaining why faster money growth presages their lending rising more than lending by private-sector banks. To this end, we exploit heterogeneity in the likely strength of the political pressure being applied. First, state-run banks boost lending growth more after an increase in money growth in countries with more politicized (i.e. less independent) central banks. Second, after state-run banks are privatized, and thus plausibly more insulated from political pressure, their lending abruptly becomes uncorrelated with prior money growth. Third, the link between money growth and subsequent increases in lending and investment we detect for state-run banks is significantly stronger during election periods, when political pressure is plausibly more intense. Finally, our results survive extensive batteries of robustness checks.

The results above, taken as a whole and in conjunction with prior work on state-run banks, are consistent with the simple and intuitive hypothesis that, if they can, politicians augment the efficacy of

money growth in boosting credit growth and capital spending by commanding banks they control to lend more. However, state-run banks are shown elsewhere to allocate capital extremely inefficiently (La Porta et al. 2002; La Porta, López-de-Silanes and Zamarripa 2003; Sapienza 2004; Dinc 2005, Deng et al 2011; Mian and Khwaja 2011; Morck et al. 2011). Our results are therefore not evidence of state-run banks increasing social welfare. Rather, together with the extant literature, they suggest a public policy trade-off, with state-run banks potentially having short-run benefits as conduits for transmitting money growth into the real economy, but long-term costs of capital misallocation.

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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 an 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 lonas 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 loan(t+1) - gross loan(t)]/gross loan(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 rate during the last 6-12 months of the previous year, [monetary base (t)-monetary base(t-1)]/monetary base(t-1) 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 positive and higher value implies 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/Deficit data are from DataStream (DS Mnemonic=..govbala), and are supplemented with IMF GFS data on either net operating balances or net lending. These variables can be calculated on accounting or cash bases, and at for the government overall, the 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 from (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 et al., 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 rate. Money growth and annual loan growth rates 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.

	Money growth		*Loan Growth*		*Capital Spending Growth*		**%State**
	Mean	***σ***	***Mean***	***σ***	***Mean***	***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.

	Bank Loan Growth	***Money growth***	***State Run***	***Bank Size***	***Bank Liquidity***	***Fiscal Stimulus***	***Output Gap***
Money growth	**0.27** **(0.09)**						
State Run	0.06 (0.71)	0.19 (0.23)					
Bank Size	-0.19 (0.24)	-0.03 (0.84)	**-0.51** **(0.00)**				
Bank Liquidity	**0.29** **(0.06)**	**0.39** **(0.01)**	**0.56** **(0.00)**	**-0.39** **(0.01)**			
Fiscal Policy	0.05 (0.79)	0.15 (0.34)	-0.16 (0.34)	0.27 (0.11)	-0.02 (0.91)		
Output Gap	-0.08 (0.62)	-0.01 (0.95)	-0.25 (0.12)	0.23 (0.16)	**-0.36** **(0.02)**	0.04 (0.79)	
Exchange Rate	-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 countries for lending growth and 30 countries 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 and Euro-zone countries 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 economy	183 economy	1,261 economy	1,261 economy	1,261 economy	1,098 economy	1,098 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.

Aggregation level	**economy**		**bank**		**economy**		**Bank**		**economy**		**Bank**	
Left-hand side variable: growth in	**lending**	**capital spending**	**lending**	**lending**	**lending**	**capital spending**	**lending**	**lending**	**lending**	**capital 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.

Aggregation level	**economy**		**Bank**		**economy**		**Bank**		**economy**		**bank**	
Left-hand side variable: growth in	**lending**	**capital spending**	**lending**	**lending**	**lending**	**capital spending**	**lending**	**lending**	**lending**	**capital 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	-0.17	-0.06		-0.15	-0.05	0.10		**-0.09**	**-0.26**	0.15	
	(-0.69)	(0.44)	(0.88)		(0.24)	(0.70)	(0.81)		**(0.0.1)**	**(0.00)**	(0.72)	
State-run[a] x money growth	0.27	**0.94**	**0.29**	0.31	**0.55**	0.31	**0.33**	**0.33**	**0.44**	**0.60**	**0.28**	**0.17**
	(0.29)	**(0.07)**	**(0.10)**	(0.14)	**(0.00)**	(0.27)	**(0.01)**	**(0.03)**	**(0.00)**	**(0.00)**	**(0.00)**	**(0.08)**
Bank size x money growth			-0.00	-0.01			-0.02	-0.01			-0.02	-0.00
			(0.85)	(0.69)			(0.64)	(0.71)			(0.64)	(0.87)
Bank liquidity x money growth			0.11	0.09			0.09	0.09			-0.29	0.12
			(0.80)	(0.32)			(0.82)	(0.33)			(0.45)	(0.29)
State-run[a]	0.02	**0.15**	-0.01	-0.00	0.02	**0.15**	0.01	0.02	0.01	**0.15**	-0.06	-0.05
	(0.47)	**(0.03)**	(0.80)	(0.98)	(0.53)	**(0.05)**	(0.64)	(0.64)	(0.73)	**(0.04)**	(0.11)	(0.45)
Bank size			-0.03	-0.02			-0.02	-0.02			-0.02	-0.02
			(0.33)	(0.29)			(0.35)	(0.36)			(0.46)	(0.28)
Bank liquidity			0.08	-0.34			0.07	-0.33			**0.18**	-0.61
			(0.80)	(0.35)			(0.82)	(0.36)			**(0.03)**	(0.20)
Additional Control	***government transfers & subsidies***				***State-directed investment***				***politically-connected firms***			
State-run[a] x additional control x money growth	0.00	-0.02	-0.00	-0.00	-0.01	**0.02**	-0.00	-0.00	-0.01	0.01	-0.00	0.01
	(0.77)	(0.56)	(0.69)	(0.73)	(0.20)	**(0.02)**	(0.36)	(0.63)	(0.50)	(0.37)	(0.59)	(0.46)
Additional control x money growth	0.00	-0.01	0.00		0.00	-0.01	-0.00		-0.00	0.00	0.00	
	(0.87)	(0.65)	(0.98)		(0.65)	(0.11)	(0.77)		(0.97)	(0.65)	(0.63)	
Additional control	0.00	-0.00	-0.00		0.00	0.00	0.00					
	(0.94)	(0.93)	(0.46)		(0.84)	(0.96)	(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.

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. 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	**lending**	**capital spending**	**lending**	**lending**
Regression	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