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

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Abstract. Within countries, individual state-run banks' lending correlates with prior money growth; similar private-sector banks' lending does not. Aggregate credit and investment growth correlate with prior money growth more where banking systems are more state-run. Size and liquidity differences between state-run and private-sector banks do not drive these results; further tests discount broad classes of alternative explanations. Tests exploiting heterogeneity in political pressure on state-run banks associated with privatizations and elections suggest a command-and-control pseudo-monetary policy channel: changes in money growth, perhaps reflecting political pressure on the central bank, change banks' lending constraints; political pressure actually changes state-run banks' lending.

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Keywords: state run banks • monetary policy • loan growth • capital spending • monetary stimulus • banking

1. Introduction

Until recently, macroeconomics impugned money growth as a policy variable for stimulating economic growth, as opposed to targeting inflation (Goodfriend 2007, Mishkin 2011). Nonetheless, policy makers often countered downturns by increasing monetary growth, hoping to spur real growth, at least in the short-run, in part by encouraging bank lending and hence capital spending, both before (Rasche and Williams 2007) and especially after the 2008 crisis (Mishkin 2009, 2011; Caballero 2010; Claessens et al. 2010; Bernanke 2012). Outcomes are mixed. Bouis et al. (2013) conclude that "monetary policy stimulus did not show up in stronger growth" in Organisation for Economic Co-operation and Development economies. In contrast, China's monetary expansion led to a rise in state-run commercial banks' lending because their top managers were ordered to (Deng et al. 2015), rather than via any conventional monetary policy transmission channel. We follow that study in terming this command-and-control channel for transmitting increased money growth to the real economy *pseudo-monetary policy* and show that it seems to be operating in many countries.

State-run commercial banks are important in many countries (La Porta et al. 2002, Morck et al. 2011), so pseudo-monetary policy might work elsewhere. Prior work shows state-run bank lending to be driven by political pressure (La Porta et al. 2002, 2003; Sapienza 2004; Berger et al. 2005; Dinc 2005; Carvalho 2014; Coleman and Feler 2015). Consistent with pseudo-monetary policy occurring elsewhere, prior work also shows state-run bank lending to be less procyclical than lending by private-sector commercial banks, and even countercyclical in some cases (Brei and Schclarek 2013, Bertay et al. 2015, Coleman and Feler 2015). This cross-country study links these two strands of work to present evidence suggesting pseudo-monetary policy might have economically important effects in many economies.

Briefly summarized, our baseline economy-level panel (2001 to 2010, spanning 40 economies) regressions link increased money growth to no subsequent change in bank lending or investment growth in economies whose commercial banking sector contains no large state-run banks but to statistically and economically significantly larger increases in lending and investment in economies whose commercial banking sectors contain more state-run banks. Our baseline bank-level panel regressions show individual state-run banks significantly boosting lending after increased money growth but otherwise similar private-sector banks in the same economies at the same time not doing so. These results survive multiple robustness checks and support a pseudo-monetary policy transmission channel: monetary expansion letting state-run banks lend more by providing liquidity and political pressure making them do so.

Numerous robustness and identification tests support this interpretation and weigh against alternative causality scenarios. Simple macroeconomic reverse causality scenarios are refuted unless somehow altered to explain why only state-run bank lending responds. Size, liquidity, and other balance sheet differences between state-run and private-sector banks do not explain these findings. Large classes of latent factors are eliminated: bank fixed effects subsume all time-invariant bank-level and economy-level latent factors; economy-year fixed effects subsume all time-varying economy-level latent factors and their interactions with money growth (those interactions being economy-level time-varying latent factors too).

Three further sets of tests highlight pseudo-monetary policy as a parsimonious explanation of these patterns and weigh against alternative explanations. The first key set of tests clarifies the importance of political pressure in the mechanism inducing state-run banks to lend in concert with money growth. Faster money growth predicts increased state-run bank lending significantly more strongly in years immediately before free elections, when political pressure on state-run banks to lend more is plausibly stronger than in other years (see, e.g., Nordhaus 1975, Alesina et al. 1997, Dinc 2005, Micco et al. 2007). Private-sector bank lending shows no such pattern. Faster money growth predicts state-run banks' lending growth more strongly in economies whose central banks are less independent—that is, more subject to political pressure (Crowe and Meade 2008). No such pattern is evident for lending by private-sector banks, suggesting that political pressure on state-run banks is necessary. Faster money growth ceases to predict individual state-run banks' lending immediately after their privatizations, consistent with privatization shielding their lending from political pressure (Megginson 2005). Thus, the baseline results are stronger as political pressure on state-run banks to lend is stronger.

Another set of tests highlights the crucial role of money growth in this mechanism. Prior work shows state-run bank lending to be less procyclical than private-sector bank lending (Brei and Schclarek 2013, Bertay et al. 2015, Coleman and Feler 2015). If state-run bank clients' demand for credit were less sensitive to the business cycle and central banks boosted money growth as recessions began, money growth could seem to predict state-run bank lending being higher than private-sector bank lending. If so, including these variables directly would erode the importance of money growth; but this is not observed. Above and beyond such effects, state-run bank lending always significantly correlates with past money growth.

Yet another set of tests confirms state-run banks to be a critical cog in the mechanism underlying the baseline results. More interventionist governments, which might press nonfinancial state-owned enterprises to borrow and invest after money growth rises, might also have more state-run banks to supply such firms credit. However, faster money growth does not predict state-run bank lending more strongly where the reach of the state, measured in various ways, is larger. Indeed, faster money growth predicts faster growth in both credit to the *private sector* and capital spending by the *private sector* in economies with more fully state-run banking systems. Thus, the underlying mechanism cannot be not limited to state-run banks lending to state-owned enterprises.

The baseline results are parsimoniously explained by pseudo-monetary policy. If state-run banks and central banks are more important and more subject to political pressure, politicians can better order up money growth and state-run bank lending to boost aggregate lending and capital spending. Our results suggest pseudo-monetary policy can be an economically important contributor to the empirical relationship between money growth, bank lending, and investment where state banks constitute substantial fractions of national banking systems. Studies that fail to account for this may misconstrue the strength of traditional monetary policy transmission channels. Although our results suggest that state-run banks can be a policy tool for reducing the substantial social costs of business cycles (Lucas 1987, Imrohoroglu 2008, Coleman and Feler 2015, Morck et al. 2011), other work links state-run banks to socially costly capital misallocation (e.g., La Porta et al. 2002, 2003; Sapienza 2004; Berger et al. 2005; Dinc 2005; Deng et al. 2015; Morck et al. 2011; Carvalho 2014). Public policy makers may thus wish to weigh any short-run social benefits of pseudo-monetary policy against such long-run social costs.

2. Data

2.1. Defining State Control

Following La Porta et al. (2002), we identify each bank's *ultimate owners*, if any exist, each year as follows. First, a bank's *large shareholders* are defined as those with voting stakes of at least 5%. If a large shareholder is a corporation, its large shareholders, its large shareholders' large shareholders, and so on are identified until we reach a natural person, state organ, or entity without a controlling shareholder. This exercise using voting stakes is necessary because banks can be controlled indirectly, through chains of business group corporations or other control enhancement devices. The voting stakes of all 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*, $\delta_{i,j,t}$, set to 1 if bank i in country j is state-run in year t and to 0 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*, in year t using credit weights from year $t-1$.

2.2. Samples

The bank-level sample begins with a 2001 cross-section of classifications of the ultimate controlling shareholders of commercial banks from Morck et al. (2011, table 1). We determine the ultimate ownership of these banks for each year from 2001 through 2010.[1] 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. To be in the sample, a bank must have comparable financial statements for two consecutive years, for reasons explained below.

We require economy-level data on monetary base growth, gross fixed capital spending, and other variables. Because of missing 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 1 lists the countries in our basic sample, together with summary statistics for key variables.

2.3. Money Growth Measure

Broadly speaking, monetary policy can be regulatory changes altering banks' ability to lend, market intervention altering key interest rates, or changes in money supply growth. We focus on money growth because regulatory changes are infrequent[2] and because neither regulatory changes nor interest rates are easily comparable across economies. In contrast, monetary aggregates change continuously and are readily comparable across countries. We further narrow our attention to monetary base growth because, among monetary aggregates, it directly overlaps least with the banking sector's balance sheet and is most consistently defined across countries.

Monthly base money growth is available for 40 countries in the International Financial Statistics (IFS) Database in the Central Bank Survey. For bank-level regressions, *money growth* for bank i in economy j and year t ($\Delta M_{i,j,t}$) is defined over 12-month intervals immediately before the start of the bank i's fiscal year as $\Delta M_{i,j,t} \equiv (M_{i,j,t} - M_{i,j,t-1})/M_{i,j,t-1}$. Thus, although base money growth is conceptually an economy-level variable, it can differ across banks in a given economy if their fiscal years differ. In economy-level tests, $\Delta M_{j,t}$ is calendar-year 12-month base money growth. These variables are winsorized at 10%.

2.4. Outcome Variables

In bank-level regressions, the dependent variable is the bank's annual real *growth in lending* in local currency, but including lending in all currencies, from BankScope, defined as $\Delta credit_{i,j,t+1} \equiv (credit_{i,j,t+1} - credit_{i,j,t})/credit_{i,j,t}$, where the subscripts i, j, and t index the bank, economy, and fiscal year, respectively. We define *credit* as gross lending where this is available because this measure is not mechanically affected by changes in discretionary loan loss provisions (Bushman and Williams 2012). However, if gross loans are unavailable, net loans are used. Real values are calculated by deflating nominal values using the economy's consumer price index.

To avoid artificially inflating the sample, only one financial statement is included each year for each *bvd* identifier number. The following procedure facilitates comparability across countries: first, consolidated statements are preferred over unconsolidated statements because lending by a bank group is arguably more important to the economy, and financial conglomerates might respond to monetary policy with internal capital market transactions that cancel out across the group (Campello 2002). Second, "audited" or "qualified" statements are preferred over "not audited" or "unqualified" statements. 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"). Despite these filters, a few extreme real growth rates in loans remain. We identify some as resulting from bank mergers and acquisitions. We drop 39 bank-year observations with real annual gross loan growth outside ±50% in the main sample but restore them for robustness tests.

We have controlling shareholder data for the largest banks in each economy. Although these banks are few in number in each country, they constitute 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 change in "domestic credit provided by banking sector" from the World Development Indicators (WDI) database, which provides domestic credit extended by the banking sector over gross domestic product (GDP). Our variable is this ratio times GDP in current local currency, deflated by the consumer price index. Aggregate real annual *growth in lending*, $\Delta credit_{j,t+1}$, for economy-year j,t is $\Delta credit_{j,t+1} \equiv (credit_{j,t+1} - credit_{j,t})/credit_{j,t}$. Aggregate credit growth thus includes lending by banks not

in our bank-level sample and nonbank financial institutions.

To explore the transmission of money growth via bank lending to economic growth, we focus on aggregate investment (Samuelson 1939). Gross fixed capital spending is from the International Monetary Fund's IFS database (National Accounts and Population line 93e). We use each economy's Producer Price Index (PPI) to deflate these data. An economy's real annual *growth in capital spending* is $\Delta capex_{j,t+1} \equiv (capex_{j,t+1} - capex_{j,t})/capex_{j,t}$, again measured over the year following that over which money growth is measured. All outcome variables are winsorized at 10%.

2.5. Summary Statistics

Table 1A provides a brief descriptions of the important variables. The online appendix provides more detailed descriptions and simple correlations between key variables. Table 1B provides means and standard deviations of money growth, growth in lending, and growth in capital spending by economy. On average, all countries experience monetary expansion and positive real gross loan growth. There is greater heterogeneity in real fixed capital growth: 22 register a positive average and eight a negative average. The table also shows which countries have more fully state-run versus private-sector commercial banking systems.

In the bank sample, loan growth correlates significantly positively with money growth and lagged liquidity. State-run banks are more liquid and smaller than private-sector banks. Bank liquidity and bank size are negatively correlated.

3. Baseline Results

3.1 Baseline Economy-Level Regressions

Our economy-level regressions tests whether changes in country j's year t base money growth, $\Delta M_{j,t}$, better predict changes in either its aggregate real credit growth, $\Delta credit_{j,t+1}$, or aggregate real capital investment growth, $\Delta capex_{j,t+1}$, if $f_{j,t}$, the state-run fraction of its banking system is greater. Our *baseline economy-level regression* specifications are

$$\Delta credit_{j,t+1} = a_1 f_{j,t} + a_2\ \Delta M_{j,t} + a_3\ \Delta M_{j,t} f_{j,t} + \sum_i d_j \lambda_j + e_{j,t}, \quad (1a)$$

$$\Delta capex_{j,t+1} = a_1 f_{j,t} + a_2\ \Delta M_{j,t} + a_3\ \Delta M_{j,t} f_{j,t} + \sum_i d_j \lambda_j + e_{j,t}, \quad (1b)$$

where economy fixed effects, denoted by λ_j, subsumes the intercept. Economy fixed effects also subsume omitted time-invariant economy characteristics. All economy-level regressions cluster by economy, with the Eurozone constituting one cluster.

Regressions 2.1 and 2.2 in Table 2 summarize these results. Regression 2.1, based on (1a), associates one percentage point higher base money growth the prior year with a statistically and economically significant 0.23 percentage point higher aggregate credit growth where the banking system is entirely state-run than where it is fully private-sector. Regression 2.2, based on (1b), links the same change in money growth to a statistically and economically significant 0.79 percentage point higher aggregate capital spending growth where the banking system is entirely state-run versus where it is entirely private-sector.

In both regressions, the money growth main effects attract negative coefficients. That in 2.2 is significant, implying that a rise in money growth anticipates reduced aggregate capital spending growth in economies whose banking systems are less than $0.27/0.79 = 34\%$ state-run. If central banks tend to boost money growth as slowdowns in capital spending impend, and Bouis et al. (2013) and others correctly conclude that a monetary stimulation is generally ineffective through conventional private-sector channels, this result could follow.

These results suggest macro-level monetary neutrality (Lucas 1972) might depend on the ownership structure of the country's banks. Money growth might be neutral in economies whose banking systems consist mainly of private-sector banks but effective in boosting lending and investment in proportion to the importance of state-run banks. Our findings thus suggest a possible route for reconciling mixed findings about the effectiveness of monetary policy after the recent financial crises (e.g., Bouis et al. 2013, Chakraborty et al. 2015, Deng et al. 2015).

3.2. Baseline Bank-Level Regressions Restrict Feasible Explanations

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

Our first set of additional tests utilizes identification by disaggregation, as introduced by Kashyap and Stein (2000, p. 408, 409). After exhaustively surveying estimation techniques for assessing the efficacy of monetary stimulus policies, they conclude that "to make further progress on this difficult identification problem, one has to examine lending behaviour at the individual bank-level" because different economy-level causality scenarios require that "the effect of monetary policy on lending should be more pronounced for some banks than for others." The issue at hand is amenable to this approach because, if state-run banks transmitted monetary growth more reliably than do private-sector banks, this would stand out in bank-level lending data.

Table 1A. Variable Names and Definitions

Variable	Definition
State-run bank indicator	1 if state organ is ultimate controlling shareholder; 0 otherwise. Bank-level annual panel dummy.
Fraction of banking system state-run	Economy-level annual panel of lagged credit-weighted fractions of banks with a state organ as ultimate controlling shareholder.
Growth in capital spending	Real growth rate in gross fixed capital spending. Economy-year annual panel. Source: IFS
Growth in lending (economy-level)	Real growth rates of domestic credit provided by banking sector. Economy-year annual panel. Source: WDI
Growth in lending (bank-level)	Real growth rates in gross loans, deflated by consumer price index. Bank-level annual panel. Source: BankScope
Money growth	Nominal monetary base annual growth. Economy-level calendar year annual panel for economy-level regressions; bank-level fiscal year annual panel for bank-level regressions. Source IFS.
Bank liquidity	Fiscal year-end ratio of government securities plus cash and amounts due from banks to total assets. Bank-year annual panel variable. Source: BankScope
Bank size	Fiscal year-end log total assets in U.S. dollars. Bank-year annual panel variable. Source: BankScope
Central bank independence	Higher values indicate more independent central bank. Economy-level cross section variable. Source: Crowe and Meade (2008)
Exchange rate depreciation	Change in local currency value of U.S. dollar over prior 12 months. Higher values indicate steeper depreciation. Economy-level annual panel variable. Source: IFS
Fiscal stimulus	Change in government spending minus tax receipts over GDP. Economy-level annual panel variable. See online appendix for details.
GDP growth	Growth rate in constant local currency GDP. Economy-level annual panel variable. Source: WDI
State-directed investment	Government investment as a share of total investment. Economy-level annual panel variable. Source Economic Freedom of the World Index.
State-controlled firms	Percent of firms that are state-owned enterprises. Economy-level cross sectional variable. Source: Faccio and Lang (2002), Claessens et al. (2000), and La Porta et al. (1999).
Output gap	Estimated potential GDP less actual GDP, all over potential GDP. Estimation uses Hodrick and Prescott (1997) filter on lagged GDP growth (Source: WDI) with smoothing parameter 6.25. Economy-level annual panel variable.
Transfers and subsidies	General government transfers and subsidies as a share of GDP. Economy-annual level panel. Source Economic Freedom of the World Index.
Election years	Dummy variable set to one if country holds free election the next year and to zero otherwise. See online appendix for details.
Change in capital regulatory index	Higher values indicate increased capital requirements. Economy-level annual panel variable. Source: Bank capital requirement stringency index in Barth et al. (2013).
Private sector loan growth	Growth in domestic credit to private sector by banks. Economy-level annual panel variable. Source: From WDI as percent of GDP, multiplied by current LCU GDP, deflated by CPI.
Private sector investment growth	Growth rate in gross fixed capital formation by private sector. Economy-level annual panel. Source: From WDI as percent of GDP, multiplied by current LCU GDP, deflated by PPI.

Our bank-level regressions test whether changes in base money growth, $\Delta M_{j,t}$, better predict changes in bank i's lending growth the next year, $\Delta credit_{i,j,t+1}$, if bank i is state-run versus private sector. These take the form

$$\Delta credit_{i,j,t+1} = a_1\delta_{i,j,t} + a_2\ \Delta M_{j,t} + a_3\ \Delta M_{j,t}\delta_{i,j,t} + \sum_i d_i\lambda_i + \sum_t d_t\lambda_t + e_{i,j,t} \quad (2a)$$

$$\Delta credit_{i,j,t+1} = a_1\delta_{i,j,t} + a_2\ \Delta M_{j,t} + a_3\ \Delta M_{j,t}\delta_{i,j,t} + \sum_i d_i\lambda_i + \sum_{j,t} d_{j,t}\lambda_{j,t} + e_{i,j,t}, \quad (2b)$$

with i, j, and t indexing banks, countries, and bank fiscal years, respectively. The state-run bank indicator variable, $\delta_{i,j,t}$, is 1 for state banks and 0 for private-sector banks. We include bank fixed effects denoted by λ_i.[3] We include year fixed effects, denoted λ_t in (2a) or economy-year fixed effects, denoted $\lambda_{j,t}$ in (2b). All bank-level regressions cluster by economy, with the Eurozone one cluster after the euro introduction

Regressions 2.3–2.5 subsume progressively finer fixed effects. Bank fixed effects in 2.3–2.5 subsume time-invariant bank factors, such as initial balance sheet characteristics. (No bank switches economy, and multinational banks are assigned distinct fixed effects in each economy.) Year fixed effects in 2.4 further subsume global time-varying latent factors, such as the state of the global economy. Finally, time-economy fixed effects subsume alternative explanations turning on economy-level latent factors, such as legal origin or cultural variables, as well as time-varying economy-level latent factors, such as unemployment rates, inflation rates, or other such time-varying country-level variables.

Note that the interactions of time-varying economy-level latent factors with money growth are themselves time-varying economy-level latent factors, so economy-year fixed effects also subsume all such

Table 1B. Descriptive Statistics of Main Variables

	Money growth		*Growth in lending*		*Growth in capital spending*		
Economy	Mean	Σ	Mean	σ	Mean	σ	*Fraction of banking system state-run*
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

Note. Economy-level means and standard deviations of economy-level money, lending, and capital spending growth rates, as well as mean fraction of banking system state-run.

interaction terms. Thus, 2.5 precludes money growth affecting credit demand differently under different economic conditions, governments with different attention to free markets, different trade conditions, and so on being relevant to explaining our baseline regressions

Regressions 2.3–2.5 summarize these tests. Regression 2.3, based on (2a), controls for bank fixed effects and links a 1 percentage point increase in money growth to a 0.30 percentage point statistically significantly larger increase in lending growth by a state-run bank than by a private-sector bank. This exposes an economically significant heterogeneity in the bank-level data: lending growth by individual state-run banks rises after an increase in money growth; lending by individual private-sector banks in the same economy does not. These results are preserved in progressive absorption of nuanced fixed effects: after money growth changes, state-run banks change their lending more than private sector banks do. The main-effect money growth coefficients in the bank-level regressions are insignificant, suggesting money growth predicts no growth in private-sector bank lending.

Finally, bank-level variation lets 2.3–2.5 refute broad classes of macro-level reverse causality scenarios in which another macroeconomic variable causes a general increase in money growth and credit demand. Such precluded scenarios include technology shocks, market

Table 2. Baseline Regression Results

Aggregation level	Economy	Economy	Bank	Bank	Bank	Bank	Bank	Bank
Explained variable: *growth in*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Lending*	*Lending*	*Lending*
Regression	2.1	2.2	2.3	2.4	2.5	2.6	2.7	2.8
Money growth	−0.06 (0.23)	−0.27 **(0.00)**	−0.16 **(0.01)**	−0.04 (0.55)		0.03 (0.93)	0.02 (0.95)	
Fraction of banking system state-run × money growth	0.23 **(0.06)**	0.79 **(0.00)**						
State-run bank indicator × money growth			0.30 **(0.00)**	0.22 **(0.01)**	0.20 **(0.04)**	0.32 **(0.00)**	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 (0.77)	−0.01 (0.81)	0.00 (0.98)
Bank size × money growth						−0.03 (0.39)	−0.01 (0.73)	−0.01 (0.51)
Bank liquidity × money growth						0.36 (0.38)	−0.02 (0.96)	−0.34 (0.34)
Bank size						−0.00 (0.82)	−0.03 (0.32)	−0.01 (0.69)
Bank liquidity						0.10 (0.23)	0.07 (0.39)	0.08 (0.36)
Fixed effects	Economy	Economy	Bank	Bank & year	Bank & economy × year	Bank	Bank & year	Bank & economy × year
Adjusted R^2	0.23	0.21	0.21	0.30	0.49	0.25	0.33	0.50
Observations	246	183	1,261	1,261	1,261	1,098	1,098	1,098

Notes. Economy-level panel is 2001–2010 spanning 40 economies for lending growth and 30 for capital spending growth, as listed in Table 1B. Bank-level panel is 2001–2010 spanning 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 1A. Numbers in parentheses are *p*-values, using economy-level clustering, eurozone economies considered one cluster after the introduction of the euro. Bold denotes significance at 10% or better.

expansions, regulatory reforms, or other such changes leading monetary authorities to boost money growth to accommodate anticipated increases in credit demand and investment. This is because the bank-level results limit feasible alternative causality scenarios to those that explain why only state-run banks' lending responds to changes in the macroeconomic variable.

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. If state-run banks were smaller or less liquid than private-sector banks, our state-run indicator might merely proxy for such lending constraints. In fact, simple correlations show state-run banks smaller but more liquid than private-sector banks. To explore this, we modify the bank-level regressions in (2) to let bank size and bank liquidity join the state-run bank indicator in modulating how changes in money growth predict changes in bank-credit growth.

Regressions 2.6 and 2.8 display these results. If state-run bank size or liquidity drove our results, the interactions of bank size and liquidity with money growth would be significant and leave the interaction of the state-run bank indicator with money growth insignificant. This is not observed. The interactions with size and liquidity are insignificant, and that with the state-run indicator remains significant—indeed its point estimate barely budges.[4]

We therefore conclude that state-run bank's lending is significantly more related to prior money growth than is lending by a private-sector bank of similar size and liquidity in the same country at the same time under similar conditions. Indeed, state-run bank lending alone changes after money growth changes; private-sector bank lending does not. This suggests a mechanism distinct from that modelled by Kashyap and Stein (2000), such as state-run banks obeying politicians' orders to lend more after money growth loosens their lending constraints.

3.3. Robustness of Economy- and Bank-Level Baseline Regressions

We designate the economy-level Regressions 2.1 and 2.2 and the bank-level Regressions 2.6 and 2.7 in Table 2 our

baseline results. These survive a battery of robustness checks, which are described in more detail in the online appendix. In describing these, we say the results are *qualitatively similar* if we see an identical pattern of signs and significance and comparable point estimates.

The baseline results survive a range of robustness checks. Measuring monetary expansion by interest rate drops, rather than base money growth, over the prior 12 months generates qualitatively similar results. Additional tests reject significantly different effects in monetary contractions versus expansions and in developed versus developing economies. More radical monetary stimuli, such as regulatory changes, may well coincide with especially rapid money growth, so our results might possibly reflect, in part at least, other dimensions of monetary policy also acting disproportionately through state-run banks. Additional tests revisit this below. Measuring aggregate lending by summing the gross credit extended by all banks in BankScope data set in each economy each year also generates qualitatively similar results.

Reclassifying banks using a 5% control threshold yields qualitatively similar results. Our data include only commercial banks. Including non–deposit-taking state-run development banks as state-run banks also generates qualitatively similar results. Partitioning private-sector banks into widely held and controlled banks (Caprio et al. 2007, Laeven and Levine 2009, Morck et al. 2011) also leads to qualitatively similar results and reveals no significant differences by type of private-sector bank. Our data include banks with global operations and exclude foreign-owned subsidiaries. Treating global banks as a new category yields qualitatively similar results, as does including foreign-controlled subsidiaries as a new category. The coefficients on the foreign bank and global banks main effects and interactions with money growth are uniformly insignificant.

Money growth and bank-level loan growth are winsorized at 10%, and observations with absolute value of loan growth above 50% are dropped. Winsorizing at 5%, not winsorizing, and retaining the extreme values all yield qualitatively similar results. Cook's D statistics show no economy consistently over the $4/n$ threshold in any regressions.

Economy-level and bank-level panel regressions cluster by economy, with Eurozone countries as one cluster. Petersen (2009) recommends this as a conservative approach using panel data of this sort; two-way clustering, by economy and by year, yields qualitatively similar results. Not clustering or clustering only by year yields uniformly better p-values. Dropping all fixed effects yields qualitatively similar results with lower p-values, as does rerunning the bank-level regressions with economy fixed effects instead of bank fixed effects.

Bank-level characteristics other than size and liquidity might also interact with money growth in bank lending decisions. Rerunning our baseline bank-level regressions including total deposits and total equity, both scaled by total assets, and their interactions with money growth does not affect our main results. We use consolidated data for banks that report both consolidated and unconsolidated figures. Using unconsolidated data yields qualitatively similar results.

Finally, the negative significant coefficient of money growth in baseline Regression 2.2 could be misleading if the true main effect is nonlinear and attenuates if money growth is near zero. Allowing for such nonlinearity (using logs, piecewise linear, or linear-quadratic terms) yields qualitatively similar results and the nonlinear terms are insignificant.

4. Identification of the Mechanism

This section considers alternative explanations of our baseline economy-level and bank-level regressions. By showing that faster money growth predicts faster lending growth by state-run banks than by private-sector banks of comparable size and liquidity in the same economies in the same years, the bank-level regressions narrow the field to alternative explanations with scope for this bank-level heterogeneity. Two classes of alternative explanations are considered: (1) explanations turning on variables other than money growth differentially affecting state-run and private sector lending; and (2) explanations turning on other measures of state power making state-run bank lending differ from private sector bank lending.

4.1. Money Growth Seems Crucial

We posit that a boost in money growth precedes a boost in state-run bank lending because faster money growth lets banks lend more and political pressure makes state-run banks lend more. This subsection considers the possibility that differences in state-run banks' lending might be tracking changes in some other time-varying economic policy or economy characteristic, $p_{j,t}$, rather than changes in money growth. For such a variable other than money growth to explain our baseline bank-level findings, it must be correlated with money growth and must lead to different behavior changes in state-run versus private-sector banks. One way this might arise is if state-run banks' borrowers respond differently to some variable that correlates with money growth.

To test for this, we consider economy-level regressions of the form (1) and bank level regressions of the form (2) but allow for interaction of the control variable $p_{j,t}$ with $f_{j,t}$ in country-level regressions and with $\delta_{i,j,t}$ in bank-level regressions. These regressions essentially run horse races to see which best explains the left-hand side variables: interactions of state-run banking

Table 3. Fiscal and Exchange Rate Policy, Business Cycle Sensitivity, and Banking Regulations

Aggregation level	Economy		Bank		Economy		Bank		Economy		Bank		Economy		Bank	
Explained variable: *growth in*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*	*Lending*	*Lending*
Regression	3.1	3.2	3.3	3.4	3.5	3.6	3.7	3.8	3.9	3.10	3.11	3.12	3.13	3.14	3.15	3.16
Money growth	–0.07 (0.12)	-0.24 **(0.00)**	0.00 (0.99)		–0.04 (0.44)	-0.20 **(0.00)**	–0.02 (0.95)		–0.05 (0.24)	–0.11 **(0.01)**	0.05 (0.90)		–0.06 (0.21)	-0.29 **(0.00)**	–1.12 (0.73)	
State-run[a] × *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)**	0.23 **(0.08)**	0.99 **(0.00)**	0.23 **(0.00)**	0.25 **(0.05)**
Bank size × *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)			0.00 (0.94)	–0.02 (0.39)
Bank liquidity × *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)			0.30 (0.45)	–0.21 (0.54)
State-run[b]	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)	0.02 (0.41)	0.08 **(0.10)**	0.00 (0.94)	–0.01 (0.89)
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)			–0.02 (0.36)	–0.01 (0.75)
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)			0.02 (0.84)	0.04 (0.63)
Additional control		Fiscal stimulus			Exchange rate depreciation					Output gap			Change in capital regulatory index			
State-run[b] × *additional control*	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)	–0.00 (0.90)	0.06 (0.11)	–0.01 (0.89)	–0.58 **(0.03)**
Additional control	–0.05 (0.90)	0.06 (0.89)	–0.12 (0.75)		-0.09 **(0.01)**	–0.10 (0.12)	–0.03 (0.35)		–0.28 (0.35)	–1.67 **(0.00)**	0.14 (0.75)		–0.00 (0.84)	–0.01 (0.23)	0.00 (1.00)	
Fixed effects	Economy	Economy	Bank & year	Bank & economy × year	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	0.09	0.22	0.34	0.50
Observations	246	182	954	954	246	188	1,072	1,072	246	188	1,098	1,098	223	172	1,029	1,029

Notes. Regressions explore variables for which money growth might proxy. Variables are described in Table 1A. Values in parentheses are *p*-values, clustering by economy, with Eurozone economies one cluster after the euro introduction. Bold denotes significance at 10% or better.

[a]State-run is the fraction of banking system state-run in economy-level regressions and state-run bank indicator in bank-level regressions.

with money growth or interactions of state-run banking with the suspected omitted variable, $p_{j,t}$. If these additional terms in (1) and (2) leave a_3 insignificant—or even just substantially reduced in magnitude—the alternative explanation merits attention.

One possibility is heterogeneous borrower responses to a *fiscal stimulus*. State-run banks' borrowers might be disproportionately sensitive to a fiscal stimulus. For example, a fiscal stimulus via infrastructure spending might boost credit demand at state-run banks if infrastructure firms were disproportionately among their clients. If the central bank accommodated this by letting money growth rise, letting actual lending subsequently rise, our baseline results could ensue, but increased borrowing from state-run banks and increased investment by their borrowers would be causing money growth, rather than the converse. Regressions 3.1–3.4 in Table 3 explore this by augmenting the four baseline regressions with a fiscal policy measure and its interactions with the relevant state-run bank measure. Including these terms leaves the baseline results qualitatively unchanged and the additional terms insignificant.

A second possibility is heterogeneous borrower responses to *currency depreciation*, which can accompany money growth (Fleming 1962, Mundell 1963). If currency depreciation boosted demand for export-related loans and state-run banks disproportionately provided these, state-run bank lending might rise as the exchange rate fell, and our baseline results could ensue. Regressions 3.5–3.8 thus include the prior year's *exchange rate depreciation* (percent change in local currency units per U.S. dollar, positive values implying local currency depreciation) and its interaction with the relevant state-run bank variable. The baseline results are unaffected, inconsistent with money growth proxying for currency depreciation. The bank-level Regressions 3.7 and 3.8 link faster money growth, but not steeper currency depreciation, to faster lending growth by individual state-run banks than by otherwise similar individual private-sector banks in the same country at the same time. The economy-level results 3.5 and 3.6 show that controlling for economy-level effects associated with currency depreciation does not disturb the baseline economy-level results regarding state-control over banks and money growth.

A third possibility is that state-run banks' borrowers might be disproportionately insulated from the business cycle. For example, if state-run banks' clients were disproportionately regulated utilities or in other recession-proof industries, credit demand at state-run banks could be less procyclical than at private-sector banks. Countercyclical monetary policy might then leave faster money growth spuriously predicting state-run bank lending exceeding private-sector bank lending. Regressions 3.9–3.12 explore this by augmenting the baseline regressions with prior year's *output gap* and its interactions with the relevant state-run bank variable. Output gap, the log of potential GDP (Hodrick and Prescott 1997) over actual GDP, rises in recession and falls in booms. Regression 3.10 is consistent with prior work showing aggregate investment falling significantly less in downturns if the banking system is more state-run (Micco and Panizza 2006, Morck et al. 2011, Lin et al. 2013, Coleman and Feler 2015). More relevant to our hypothesis, all four baseline results are qualitatively unchanged after allowing for this effect.

Yet another possibility is state-run and private-sector bank lending differentially tracking changes in the *stringency of banking regulations*. If politicians or central bankers loosen banking regulations, banks can lend more. If political pressure then leads state-run banks to actually boost lending, the central bank might accommodate this by boosting money growth. Regressions 3.13–3.16 explore this by including the annual change in Barth, Caprio and Levine's (2013) capital regulation stringency index[5] and its interaction with the relevant state-run bank variable. Consistent with this effect, Regression 3.16 shows state-run banks boosting lending significantly more than do private-sector banks after capital regulations are relaxed. However, the baseline results are unchanged and therefore are unlikely to be driven by regulatory changes.

Overall, money growth seems to be crucial across all specifications. The interaction of money growth with the fractional importance of state-run banks remains positive and significant, 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.

4.2. State-Run Banks Seem Crucial

State-run banks might be more prevalent where state power is broader and deeper. 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, and its central bank to accommodate this. State-run banks would then be only one cog in a far-reaching apparatus of state intervention reflected in our baseline results. If so, our baseline results would be stronger where governments are more generally interventionist.

To explore this, Table 4 lets measures of the reach of the state, denoted $q_{j,t}$, modulate the interactions between money growth and the state-run bank variables. This introduces triple interactions, $q_{j,t} \times f_{j,t} \times \Delta M_{j,t}$ in the economy-level regressions (1) and $q_{j,t} \times \delta_{i,j,t} \times \Delta M_{i,j,t}$ in the bank-level regressions (2), as well as the

Table 4. Baseline Regressions Adapted to Consider the Reach of the State

Aggregation level	Economy		Bank		Economy		Bank		Economy		Bank		Private sector only	
Explained variable: *growth in*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*	*Lending*	*Lending*	*Lending*	*Capital spending*
Regression	4.1	4.2	4.3	4.4	4.5	4.6	4.7	4.8	4.9	4.10	4.11	4.12	4.13	4.14
Money growth	−0.06	−0.17	−0.06		−0.15	−0.05	0.10		−0.08	−0.21	−0.06		−0.12	−0.21
	(-0.69)	(0.44)	(0.88)		(0.24)	(0.70)	(0.81)		(0.20)	**(0.00)**	(0.87)		**(0.00)**	**(0.00)**
State-run[a] × *money growth*	0.27	0.94	0.29	0.31	0.55	0.31	0.33	0.33	0.29	0.55	0.39	0.27	0.17	0.52
	(0.29)	**(0.07)**	**(0.10)**	(0.14)	**(0.00)**	(0.27)	**(0.01)**	**(0.03)**	**(0.08)**	**(0.09)**	**(0.00)**	**(0.07)**	**(0.08)**	**(0.00)**
Bank size × *money growth*			−0.00	−0.01			−0.02	−0.01			0.01	0.02		
			(0.85)	(0.69)			(0.64)	(0.71)			(0.85)	(0.47)		
Bank liquidity × *money growth*			0.11	0.09			0.09	0.09			−0.11	−0.54		
			(0.80)	(0.32)			(0.82)	(0.33)			(0.85)	(0.44)		
State-run[b]	0.02	0.15	−0.01	−0.00	0.02	0.15	0.01	0.02	0.05	0.15	−0.01	0.01	0.06	−0.00
	(0.47)	**(0.03)**	(0.80)	(0.98)	(0.53)	**(0.05)**	(0.64)	(0.64)	(0.15)	**(0.05)**	(0.44)	(0.37)	**(0.03)**	(0.98)
Bank size			−0.03	−0.02			−0.02	−0.02			−0.00	0.01		
			(0.33)	(0.29)			(0.35)	(0.36)			(0.90)	(0.42)		
Bank liquidity			0.08	−0.34			0.07	−0.33			0.11	0.16		
			(0.80)	(0.35)			(0.82)	(0.36)			(0.43)	(0.37)		
Additional control	Transfers & subsidies				State-directed investment				State-controlled firms					
State-run[b] × *additional control* × *money growth*	0.00	−0.02	−0.00	−0.00	−0.01	0.02	−0.00	−0.00	0.01	0.00	−0.01	0.00		
	(0.77)	(0.56)	(0.69)	(0.73)	(0.20)	**(0.02)**	(0.36)	(0.63)	(0.21)	(0.89)	(0.33)	(0.84)		
Additional control × *money growth*	0.00	−0.01	0.00		0.00	−0.01	−0.00		−0.00	−0.01	−0.01			
	(0.87)	(0.65)	(0.98)		(0.65)	(0.11)	(0.77)		(0.92)	(0.20)	**(0.06)**			
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	Economy	Economy
Adjusted R^2	0.24	0.21	0.34	0.50	0.25	0.21	0.34	0.50	0.06	0.10	0.31	0.48	0.26	0.21
Observations	232	183	1,072	1,072	230	180	1,056	1,056	172	150	733	733	246	184

Notes. Regressions explore how measures of the reach of the state modulate difference between state-run and private-sector banks. Variables are described in Table 1A. *p*-values in parentheses cluster by economy, with Eurozone economies one cluster after the euro introduction. Bold denotes significance at 10% or better.

[a]State-run is the fraction of banking system state-run in economy-level regressions and state-run bank indicator in bank-level regressions.

reach-of-the-state measure and its interaction with money growth in both.

Our reach-of-the-state measures include *transfers and subsidies* as a fraction of GDP to capture general state intervention, *state-directed investment*, defined as government investment over total investment, to capture the state's direct control over investment, and the percentage of *state-controlled firms*, to capture direct state ownership of business.[6]

The point estimates on the interaction of money growth with the state-run bank variables change little from those in the baseline regressions, though the p-values fall below significance thresholds in 4.1, 4.4, and 4.6. In 4.1 and 4.4 the additional terms are insignificant, and the regression R^2s change little, suggesting the additional variables introduce collinearity without improving the fit. Regression 4.6, shows aggregate capital spending changing more after money growth changes if both state-run banks and state-directed investment are more prevalent.

A further robustness check (not shown) gauges the reach of the state by the fraction of large firms, by market capitalization, Faccio (2006) designates *politically connected*. If politically connected private-sector firms disproportionately responded to state directives to borrow, perhaps because they anticipate bailouts in unpropitious states (Khwaja and Mian 2005, Faccio 2006), and borrowed from state-run banks, our baseline results might ensue. However, these added terms are insignificant and our baseline results are unaffected.

In summary, state-run banks, rather than more general measures of the reach of state power, drive the baseline regression results. This suggests a crucial role for state-run banks lending to the private sector, rather than to state-owned or politically connected firms.

The rightmost two regressions in Table 4 explore this further. These resemble the baseline economy-level regressions, but 4.7 explains growth in lending *to the private-sector only* and 4.8 explains capital spending *by the private sector only*. Aggregate lending to the private sector and aggregate capital spending by the private sector both rise more after a boost to money growth if state-run banks are more important. Because private-sector banks do not boost lending after increases in money growth on average, these findings are inconsistent with state-run banks boosting lending only to other state-run firms.

4.3. Political Pressure Seems Crucial

The previous sections considered feasible alternative explanations and excluded or substantially restricted each. This section presents evidence consistent with political pressure on state-run banks explaining the differences in state-run and private-sector banks' lending change subsequent to a change in money growth. The difference between the lending growth of an individual state bank and that by an otherwise similar private-sector bank subsequent to a unit change in money growth varying with political pressure is difficult to reconcile with alternative causality scenarios.

A more politically sensitive central bank might let politicians order up faster money growth to encourage more lending. In contrast, an independent central bank might adjust money growth with little regard for current political priorities. If so, our baseline findings would be stronger if the central bank is more politically sensitive. Regressions 5.1–5.4 in Table 5 let central bank independence modulate the interaction of money growth with the state-run bank variables. Independence is gauged using Crowe and Meade's (2008) 0 to 1 variable, one indicating maximal independence.[7] Regressions 5.1 and 5.2 show greater central bank independence attenuating the baseline aggregate lending result but not the aggregate capital spending result. Regressions 5.3 and 5.4 reveal an interaction effect at the bank level: if the central bank is independent, state-run banks' lending growth is insignificantly different from that of private-sector banks after increased money growth, with p-values of 0.71 and 0.85 using the parameters and covariance matrices from 5.3 and 5.4, respectively. If the central bank is not independent, and so subject to political pressure, state-run banks' lending growth is significantly correlated with prior money growth, but private-sector banks' lending is not.

Politicians may press harder for lending growth upon a monetary expansion if elections loom closer (Nordhaus 1975, Alesina et al. 1997, Dinc 2005). If so, our baseline results might be stronger during election campaigns than at other times. We therefore test for differences between years immediately before free elections (defined using an election dummy set to 1 if the country has a free election the subsequent year and to 0 otherwise) and other years in the difference between state-run and private-sector banks' lending growth after a unit increase in money growth. To the extent that election cycles are an exogenous source of heterogeneity in political pressure on state-run banks, these tests further contribute to identification.

Regressions 5.5–5.8 summarize these tests. Regression 5.5 shows that, in years preceding free elections, a 1 percentage point increase in money growth predicts 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 5.6 shows that, in nonelection years, the same unit increase in monetary growth predicts a significant 1.34 percentage point higher

Table 5. Political Pressure

Aggregation level	Economy		Bank		Economy		Bank	
Explained variable: *growth in*	*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
Money growth	0.06	−0.38	−0.41		0.01	−0.12	0.04	
	(0.61)	**(0.00)**	(0.15)		(0.87)	(0.15)	(0.88)	
State-run[a] × *money growth*	−0.05	0.94	0.74	0.78	0.01	1.34	0.16	0.26
	(0.87)	**(0.06)**	**(0.00)**	**(0.01)**	(0.91)	**(0.02)**	**(0.00)**	**(0.00)**
Bank size × *money growth*			−0.01	−0.02			−0.01	−0.03
			(0.71)	(0.22)			(0.58)	(0.14)
Bank liquidity × *money growth*			−0.14	−0.35			−0.10	−0.54
			(0.68)	(0.34)			(0.70)	(0.19)
State-run[a]	0.01	0.17	−0.04	−0.04	0.03	0.24	0.00	−0.00
	(0.61)	**(0.02)**	(0.20)	(0.56)	(0.44)	**(0.04)**	(0.94)	(1.00)
Bank size			−0.02	−0.00			−0.04	−0.02
			(0.42)	(0.86)			(0.24)	(0.60)
Bank liquidity			0.15	0.08			0.15	0.17
			(0.68)	(0.34)			(0.15)	(0.15)
Additional control	Central bank independence				Election years			
State-run[b] × *additional control* × *money growth*	0.48	−0.22	−0.80	−0.80	0.36	0.58	0.13	0.27
	(0.32)	(0.72)	**(0.02)**	(0.06)	(0.04)	**(0.02)**	(0.53)	**(0.07)**
Additional control × *money growth*	−0.19	0.18	0.74		−0.11	−0.12	0.02	
	(0.26)	(0.34)	**(0.00)**		(0.18)	(0.09)	(0.79)	
Additional control					0.01	0.00		
					(0.63)	(0.80)		
Fixed effects	Economy	Economy	Bank & year	Bank & economy × year	Economy	Economy	Bank & year	Bank & economy × year
Adjusted R^2	0.25	0.21	0.36	0.51	0.07	0.21	0.30	0.48
Observations	213	179	1000	1,000	239	183	1,063	1,063

Notes. Regressions explore how measures of political pressure proxies modulate difference between state-run and private-sector banks. Variables are described in Table 1A. *p*-values in parentheses cluster by economy, with Eurozone economies one cluster after the euro introduction. Bold denotes significance at 10% or better.

[a]State-run is the fraction of banking system state-run in economy-level regressions and state-run bank indicator in bank-level regressions.

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 1.34 + 0.58 = 1.92 percentage points.

Regressions 5.7 and 5.8 perform analogous exercises using bank-level data. Regression 5.8, which controls for bank and economy-year fixed effects, shows the same unit boost to money growth in nonelection 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 0.26 + 0.27 = 0.53 percentage points. Regression 5.7, 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 2.6, and reveals a positive but insignificant added difference in election years.

This evidence is predominantly (that is, except for 5.7) consistent with state-run banks more effectively transmitting money growth into increased credit and investment during election years. In other words, state-run banks respond to monetary growth more strongly when political pressure to do so is likely stronger.

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

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

We begin with a large sample of bank privatizations provided by Megginson (2005) and augment these with more recent transactions from the Privatization Barometer and World Bank privatization transactions databases.[8] Following Boubakri et al. (2005), we consider the date when more than 10% of the bank becomes privately owned. If residual state ownership implies continued political pressure on lending decisions, this should induce attenuation bias. We merge these data with money growth and unconsolidated BankScope data (because consolidation could include different related firms before and after privatization). The sample includes only privatized banks for which data are available in the years immediately before and after the privatization year.

Table 6 summarizes these event study tests. Regression 6.1 explains real lending growth with money growth, an *after privatization* dummy, and the interaction of the two, all controlling for bank fixed effects. Regressions 6.2 and 6.3 augment this with bank size and liquidity and their interactions with money growth, with 6.3 using stepwise estimation to introduce the additional controls, given possible multicollinearity in the small sample. All 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 1 percentage point increase in money growth the prior year to a bit less than a percentage point lower loan growth after privatization than before privatization.

The main effect of money growth on loan growth is positive and significant, except in 6.2 where the full set of control variables are included. The significant coefficients indicate that a 1 percentage point boost to money growth over the prior year predicts a 0.65 percentage point boost to state-run banks' lending growth before their privatizations. This affirms our baseline findings that state-run banks' lending responds significantly to monetary growth, whereas otherwise comparable private-sector banks' does not. The sum of the regression coefficients for money growth 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. The timing of this change around privatizations is consistent with the end of state-control reducing this correlation. Subject to the caveats discussed above, the table bolsters the case for state-run banks, but not private-sector banks, being part of the mechanism underlying pseudo-monetary policy.

These tests combine to implicate political pressure. If political pressure is stronger, faster money growth predicts state-run banks' lending growth outpacing private-sector banks' lending growth by a larger margin.

4.4. Consistency with Other Studies

Country-level studies of lending by state-run banks reach conflicting conclusions. Deng et al. (2015) report China's state-run banks boosting lending after money growth increased amid the 2008 financial crisis. In contrast, Das et al. (2015) find no such effect in India. India's civil service is shown elsewhere to be highly ineffective and unresponsive to central direction (Das 2005, Bertrand et al. 2007, Niehaus and Sukhtankar 2013); although, more recently, Agarwal et al. (2017b) found India's publicly owned state banks to be effective

Table 6. Bank Privatizations

Estimation method	OLS	OLS	Stepwise
Regression	6.1	6.2	6.3
Money growth	0.67	1.43	0.64
	(0.02)	(0.54)	**(0.00)**
After privatization dummy × money growth	–0.89	–1.06	–0.94
	(0.02)	**(0.00)**	**(0.01)**
Bank size × money growth		–0.08	Drops
		(0.69)	
Bank liquidity × money growth		–0.23	Drops
		(0.92)	
After privatization dummy	0.12	0.11	0.10
	(0.12)	**(0.06)**	(0.27)
Bank size		–0.02	drops
		(0.93)	
Bank liquidity		–1.26	–1.28
		(0.00)	**(0.00)**
Fixed effects	Bank	Bank	Bank
Adjusted R^2	0.64	0.77	0.77
No. of observations	36	36	36
No. of banks	18	18	18

Notes. Explained variable is bank-level *loan growth*, defined as the bank's year-on-year growth rate in real gross loans. Sample includes 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. Numbers in parentheses are *p*-values. Bold denotes significance at 10% or better.

in administering a financial inclusion program in 2015–2016. Although China is not in our sample, its civil servants are shown elsewhere to be both highly effective (Burns 2004) and highly responsive to political direction (MacGregor 2010).

These conflicting studies suggest that our baseline findings might differ with civil service characteristics. To explore this, we run regressions (not shown) analogous to those exploring central bank independence but replacing that variable with the product of measures of the political sensitivity and effectiveness of the civil service.[9] The baseline results are preserved, but the bank-level regressions show individual state-run banks boosting lending by a significantly greater margin where civil servants are rated both more effective and more sensitive. The pseudo-monetary policy transmission channel we posit might thus work more effectively where government officials are more effective and centrally disciplined. However, we are reluctant to press this interpretation because no corresponding significant differences are evident in the economy-level regressions. We welcome additional research into these issues.

Our findings must be qualified in that our sample includes only large banks. We may miss monetary growth via smaller banks (Kashyap and Stein 2000) or through household finance decisions (Agarwal et al. 2015, Agarwal et al. 2017a, Di Maggio et al. 2017). The latter literature explores how low interest rate shocks, debt relief programs, and reductions in debt servicing costs due to loan modifications affect household consumption, default, and debt repayment decisions of households (especially more indebted ones) and thus the broader economy. Our findings complement this literature in that both highlight transmission depending crucially on bank-level incentives, organizational capital, and decision-making freedom.

4.5. Discussion of Identification

The above constitutes a series of identification tests that successively pare away alternative causality scenarios to leave pseudo-monetary policy the most plausible and parsimonious explanation. Our firm-level regressions preclude macroeconomic reverse causality scenarios (e.g., credit demand shifts causing money growth and bank lending) that cannot explain only state-run bank lending responding. Bank fixed effects preclude causality scenarios driven by time-invariant bank-level (e.g., historical bank characteristics) or economy-level (e.g., legal origin) latent factors. Economy-year fixed effects preclude alternative causality scenarios driven by time-varying economy-level

latent factors (e.g., importance of state-owned enterprises, etc.) or their interactions with money growth (these interactions are also time-varying economy-level variables).

Such latent factors could only have traction if they have potential to affect state-run and private-sector banks differently. Examples of variables with such potential include exchange rate depreciations, fiscal expansions, or business cycle variables, which might increase credit demand at state-run banks relative to private-sector banks if state-run banks disproportionately financed exports, infrastructure projects, or relatively recession-proof government-connected sectors, respectively. Tests directly incorporating these and other such variables continue to show changes in money growth presaging changes in state-run bank lending.

Alternative causality scenarios in which state-run banks are more important in countries with more generally interventionist governments are also considered. For example, state-run bank lending might rise because demand for credit by nonfinancial state-owned enterprises rises after a boost to money growth. Again, our results are not affected by directly incorporating a series of variables measuring the reach of the state into our tests. Rather, additional tests show faster money growth predicts faster credit to the *private sector* and faster capital spending by the *private sector* in economies with more predominantly state-run banking systems.

Finally, a series of tests show our results to be stronger where political pressure is stronger on state-run banks to boost lending after a boost to money growth. Our baseline results are stronger when money growth precedes an election and where the central bank is less independent, letting money growth and state-run bank lending better respond to political pressure together. Additional tests on a sample of privatized banks show their lending moving in step with money growth until their privatizations and then entirely ceasing to do so after their privatizations.

Although no single one of these identification tests is "bulletproof," their combined results weigh strongly against alternative causality scenarios and in favor of a pseudo-monetary policy effect. Faster money growth, perhaps reflecting political pressure on the central bank, *lets* banks lend more; political pressure *makes* state-run banks lend more. Thus, faster money growth predicts both faster loan growth by state-run banks and faster aggregate credit and capital spending growth by economies whose banking sectors are more state-run.

5. Conclusions

A command-and-control channel may connect money growth to the real economy via state-run banks. Money growth changes, perhaps reflecting political pressure on the central bank. State-run banks then change their lending because politicians order them to. Because this mechanism entails a monetary expansion being transmitted to the real economy via increased bank lending to the private sector, it is properly considered a variant of monetary policy. However, because it operates via political pressure it differs fundamentally from standard monetary policy transmission channels. We therefore term this phenomenon pseudo-monetary policy.

We find that pseudo-monetary policy is statistically and economically significant in many economies. At the bank level, faster money growth precedes faster loan growth by state-run banks than by private-sector banks. At the economy level, faster money growth precedes faster bank credit growth and capital investment growth by greater margins (both in total and in the private sector alone) in economies whose banking systems are more fully state-run but does not precede either in economies whose large banks are entirely private sector. A sequence of identification tests leaves pseudo-monetary policy the most parsimonious and plausible explanation of these findings.

The seeming efficacy of pseudo-monetary policy suggests that differences between state-run and private-sector banks are important in this context. One key difference is in their respective objective functions. Private-sector banks are in business to maximize firm value; state-run banks must also obey politicians (La Porta et al. 2002, 2003; Sapienza 2004; Dinc 2005; Khwaja and Mian 2005; Deng et al. 2015; Morck et al. 2011).

State-run bank senior executives, whether career civil servants or political appointees, must attend to political priorities to advance their careers. India's state-run commercial banks have always been run by political appointees, though the bank of Baroda made headlines in 2015 by appointing P.S. Jayakumar of Citibank its chief executive officer (CEO) (Bandyopadhyay 2018).[10] China's state-run bank top executives, always career Party cadres, are compensated annually for bank accounting performance, but their careers are in the hands of the Communist Party of China, via its Organization Department. Their harmonious implementation of Party policies critically affects their being promoted, demoted, or reassigned to a new position (Deng et al. 2015). The tiny Bank of North Dakota, the only state-run commercial bank in the United States, has generally been run by ex-politicians and political advisors, though its current CEO, Eric Hardmeyer, rose up through the ranks internally. In each case, responding to political directives plausibly enters their utility.

We posit that pseudo-monetary policy operates effectively because state-run banks' directors and officers are public servants, whose careers depend on implementing politicians' formal policy directives and

informal "jawboning" requests (Klitgaard 1988). Money growth suffices to let bank lending expand; political pressure necessitates that they do so. This straightforward mechanism contrasts with conventional monetary policy transmission channels, surveyed by Mishkin (1996), which contain chains of causality whereby money growth ultimately may affect how private-sector banks' lending decisions affect their valuations or other objective functions.

Before the 2008 financial crisis, many macroeconomics researchers had concluded (Rasche and Williams 2007, p. 490) that "the case for consistently effective short-run monetary stabilization policies is problematic" and relegated central bankers to inflation targeting (Goodfriend 2007, Mishkin 2011). Macroeconomists increasingly and firmly held supposition that countries need only "keep inflation within a tight range through control of a short-term interest rate, and everything else will take care of itself" (Borio 2012, p. 191). The various channels through which monetary policy might buffer recessions seemed to work poorly if at all.

Yet politicians and central bankers never completely abandon the monetary stimulation option. The U.S. Federal Reserve Open Markets Committee justified a monetary expansions after the 1987 market crash "to cushion the effects on prospective economic growth," to counter the "Y2K" scare about widespread computer failures in January 1, 2000, and after the "9/11" terrorist attacks to counter "heightened uncertainty and concerns about a deterioration in business conditions both here and abroad damping economic activity" (Rasche and Williams 2007). As the 2008 financial crisis unfolded the academic consensus weakened (Caballero 2010, Mishkin 2011) and central bankers oversaw unprecedented monetary expansions (Mishkin 2009, Claessens et al. 2010, Bernanke 2012), even as benchmark interest rates fell into the zero-lower bound zone, where even neo-Keynesians thought monetary policy ineffective (Tobin 1947, Abbassi and Linzert 2012). Regardless of the theories and empirical evidence, politicians and central bankers (voluntarily or pressed) felt they needed to "do something"; and monetary expansion was "something," so they did it.

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

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Endnotes

[1] This approach avoids survival bias but omits emerging large banks.

[2] The capital requirement stringency index of Barth et al. (2013) is unchanged from one year to the next for 81% of our bank-year observations.

[3] If the state-run bank indicator is constant through time for all banks, the state-run bank dummy is perfectly collinear with the bank fixed effects. If no bank has different fiscal year end, main effect of money growth is subsumed by country-year fixed effects. Because five banks switch status and only 4% of banks have fiscal year ends different from those of other banks in the same economy, we do not attach economic significance to coefficient of a_1 and refrain from estimating a_2 in (2b). Including or excluding these banks does not alter the baseline results.

[4] These findings do not necessarily contradict Kashyap and Stein (2000), whose tests exploits the unusual structure of the U.S. banking system: the thousands of very small independent banks (Calomiris and Haber 2014). Our analysis uses only the largest, and presumably most liquid, banks in each economy. Our comparatively limited variation in bank size and liquidity makes the Kashyap and Stein (2000) effect difficult to find. For our purposes, this helps because it also makes that effect less likely to interfere with our primary task.

[5] This change is zero (no change) in 81% of our bank-year observations, but regulatory loosening does accompany monetary expansions: its pooled simple correlation with money growth is −0.08 ($p = 0.01$), though economy fixed effects reduce the coefficient to an insignificant 0.01 ($p = 0.39$).

[6] The last merges data from Faccio and Lang (2002), Claessens et al. (2000), and La Porta et al. (1999).

[7] Qualitatively similar results ensue using Alpanda and Honig's (2010) central bank *de facto independence index*.

[8] Our panel data cover the largest banks in each economy, of which only five are privatized in the years covered.

[9] A country's civil service is called *effective* if its government effectiveness index (Kaufmann et al. 2010) exceeds its sample median. A country's civil service is called *sensitive* to political pressure if the average response to two survey questions (Q8.b and Q8.e) in the Quality of Government Expert Survey Data set (Teorell et al. 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.

[10] Canara Bank also appointed a CEO with private-sector experience in 2015, though Rakesh Sharma had worked at the State Bank of India for three decades before moving to the private sector.

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