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MANIPULATING CREDIT: GOVERNMENT POPULARITY AS DRIVER OF CREDIT CYCLES

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Abstract: This paper analyses the interaction between credit and political cycles, arguing that short-termist governments will seek to ride and amplify credit cycles for political gains. Specifically, it tests for the existence of political credit cycles not only before elections but throughout the term when executives seek to bolster support in periods of popularity drops. Compiling a unique database on government approval from opinion polls in 57 countries starting in 1980, it provides evidence that drops in popularity are systematically associated with larger future credit cycles, robust to a number of checks for confounding factors. Such credit manipulation appears to target credit to households specifically, is more prevalent in advanced, financialized, and indebted economies, and increases the likelihood of bad credit booms. Overall, this research points to the crucial importance of political cycles as drivers and sources of financial cycles and vulnerabilities.

Keywords: credit booms, financial stability, political business cycle, government popularity, electoral cycles, credit subsidies, homeownership

JEL classification: D72, E51, E58, G01, G18, I38, N20

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“When borrowers get used to easy money and credit, everyone’s mindset is affected, encouraging the adoption of what might be called “credit populism”. (...) Expansion, albeit imprudent, might well serve the purposes of EME and AE local political cycles.”
Luiz Awazu Pereira da Silva, Deputy General Manager of the BIS (2017)

“Politicians have looked for [new] ways to improve the lives of voters. And since the 1980’s, the seductive answer has been easier credit. Easy credit has large, positive, immediate, and widely distributed benefits, whereas all the costs lie in the future.”
Raghuram Rajan (2010)

Unsustainable credit growth on the back of low capital has been the prime cause of most systemic banking crises over the past 150 years and the top one predictor of such crises according to early warning models developed by a large literature in financial economics (Aikman et al., 2015; Drehmann & Juselius, 2014; Greenwood et al., 2020; Jordà et al., 2015; Schularick & Taylor, 2012): banking crises appear to be credit booms gone wrong, consistent with the early theories of Minsky (1986) and Kindleberger (1978). While credit expansion may be beneficial for the real economy through financial deepening processes, the costs of rapid expansions overall outweigh their benefits (Verner, 2019). Mitigating credit cycles thus became an “intermediate objective” of financial stability authorities (e.g. ESRB (2013)).

However, credit cycles are endogenous to deeper determinants, i.e. the specific circumstances under which unsustainable credit growth may take place. While a substantial body of the literature has looked at the global origin of crises and credit booms, highlighting the existence of a Global Financial Cycle and dynamics of surges and retrenchment of cross border capital flows (Bauerle Danzman et al., 2017; Jordà et al., 2019; Mendoza & Terrones, 2012; Rey, 2013), global factors appear to explain only a quarter of the variation in capital flows, the rest being domestic pull factors (Cerutti, Claessens, & Rose, 2017). Moreover, the non-random distribution of financial crises across countries point to the importance of understanding the unique configurations of local institutions and political processes leading to instability.

This paper analyses one aspect of such configurations, namely the interaction between political and credit cycles. This paper's starting insight is that domestic political cycles are crucial and overlooked determinants of (unsustainable) credit cycles, as short-termist governments seeks to expand credit for political gains. Promoting a debt-based system by incentivizing credit, notably mortgages, is one way for politicians to achieve a sentiment of increased living standards, at the expense of the longer-term build-up of financial vulnerabilities.

Specifically, I test whether, facing declining popularity, governments will seek to boost credit expansion, for instance by making use of a diverse policy toolkit ranging from monetary policy to credit market policies and financial regulation, all of which will bias actors’ incentives towards lending and borrowing. Thirty years ago, Dornbush and Edwards (1990) used the concept of “macroeconomic populism” to describe the short-termist political emphasis on growth and income distribution, at the expense of inflation and deficit risks which lead to macroeconomic collapse. This paper will seek to uncover the role of *credit* manipulation for short-term political gains, at the expense of encouraging the build-up of unsustainable credit bubbles in the medium-run.

Traditional research on political business cycles focuses on and assumes without much questioning that manipulation will simply happen in – or right before elections. A core contribution of this paper is to ask whether accountability mechanisms and hence manipulation extends outside of the election windows. A similar point is made by former Bank of England Deputy Governor Paul Tucker (2018) who notes, regarding the lack of compelling evidence on political monetary policy around elections, that researchers

might “have been looking at the wrong place (...), from what I saw in the late 1980’s and early 1990’s, the goal can be less concrete and more immediate: a surprise easing of policy would sometimes be targeted at improving near-term opinion poll ratings, political popularity being heavily path dependent”. This paper hence proposes a new channel to test political cycles beyond elections through changes in government popularity. To this aim, I construct a unique database on government popularity using national and international polling data for a set of 57 advanced and emerging countries, going back to the 1970’s for some countries (1950’s for the US and Germany), and available monthly for most countries.

Exploring the relationship between government popularity and credit cycles, I find strong evidence that declining popularity is systematically associated with larger future credit cycles, controlling for traditional determinants of credit booms, hence giving credence to the hypothesis of “political credit cycles”. Specifically, I find that a one standard deviation drop in government popularity leads to a 1-point increase in the change of credit to GDP the next year. These results are robust to a large set of checks and notably the use of interactive fixed effects models to control for potential unobservable country-specific trends which may act as confounding factors. These findings confirm recent evidence by Kern and Amri (2020) of political credit cycles in elections years but extends them beyond election years while digging further into the mechanisms at play. Going granular, I show that these governments seem to target credit to households rather than credit to corporates, consistent with the idea of the executive seeking direct popularity gains. In addition, advanced economies, and more generally financially developed economies that are more reliant on credit, are more prone to such political credit cycles. Finally, I show that such credit manipulation does not lead to a one-off benign increase in credit but may lead to economically large credit deviations in the medium run, increasing the likelihood of “bad” credit booms, i.e. booms followed by financial crises. This points to potentially important financial stability implications of such political credit cycles, echoing recent research by Herrera et al (2019) that shows that booming government popularity is an important predictor of financial crises in emerging economies.

Besides, I analyze potential amplifying or mitigating factors and circumstances: the main conclusion of several decades of research on political economic cycles is that these cycles are highly context-conditional and that incentives for engineering cycles will vary across policies, domestic and international political-economic and strategic contexts (Franzese & Jusko, 2009). As for country-specific context, I do not find convincing evidence of a systematic impact of partisanship or initial level of inequality in driving political credit cycles. As for potential policy constraints, I do not find any evidence of amplifying or mitigating effect of central bank independence, hence hinting at the fact that monetary policy may not be the prime tool and leverage for engineering credit expansion. I also test the role of fiscal constraints - i.e. is credit expansion a substitute for fiscal spending in countries with already too high government debt? - and actually find a possible complementary relationship: Countries with higher government debt to GDP are less likely to experience political credit cycles: government may actually use the fiscal lever to promote credit expansion, e.g. mortgage subsidies.

The rest of the paper is structured as follows: Section 1 presents a conceptual framework for political credit cycles and reviews early tests in the literature, identifying several gaps, both methodologically and conceptually, which this paper is trying to fill. Section 2 provides the empirical tests: it presents my newly constructed cross-country dataset on government popularity, the model, and tests for the existence of opportunistic political credit cycles. Section 3 looks at the potential amplifying and mitigating factors of credit populism, namely the level of inequality, the party in power, the independence of the central bank,

and a country's fiscal situation. Section 4 further analyses the economic significance of these political credit cycles with regards to financial stability. Section 5 concludes.

1. Political Credit Cycles: Conceptual Framework and Existing Work

A prolific body of literature, both theoretical and empirical, has looked since the 1970's at the political determinants of economic outcomes to demonstrate the existence of potential "political business cycles" (thereafter "PBC"), i.e. politicians seeking to remain in power will seek to manipulate macroeconomic aggregates². Two broad sets of models have emerged from this literature, the "opportunistic" PBC based on policy movements around elections and the "partisan" PBC based on policy shifts due to changes in the party in power. These two categories of PBC models have traditionally been applied to two main policy domains: monetary and fiscal policy (Drazen 2000). However, an important macroeconomic aggregate which has received only limited attention by the previous literature (with the welcome exception of Kern and Amri 2020) and may be the object of manipulation is credit. While the traditional PBC literature has focused on the business cycle (short-term) and real economy output variables like GDP, credit pertains to financial variables which display longer cycles.

1.1. Political Credit Cycles: Demand, Supply and Instruments

The notion that credit fluctuations are influenced by "political credit cycles", i.e. manipulation of credit cycles by governments for political motives, rests on three elements: First, there needs to be adequate "demand" for credit manipulation by voters. Second, politicians need to have sufficient incentives to provide this policy. And third, politicians need to have the capacity to manipulate credit availability. This section reviews these assumptions in turn.

A credible argument of "political credit cycles" first requires that voters care about credit. There is wide evidence that voters respond to economic outcomes, i.e. vote with their pocket (Downs, 1957; Duch & Stevenson, 2008; Fiorina, 1981). This literature has however traditionally looked at indicators like income, unemployment, and growth, and not so much the availability of credit. Yet, recent work has demonstrated the potency of the "credit constituency", which emerged in recent decades: governments failing to meet this demand are facing electoral losses when mortgage credit contracts (Antoniades & Calomiris, 2020) or reversely fare better electorally when interest rate expenditures are low (Brännlund, 2020).

On the one hand, credit has been used as a mean for further redistribution – thanks to credit, households may afford things that were previously beyond their means and "keep up with the Joneses" (Carr & Jayadev, 2015). Promoting a debt-based system by incentivizing credit is one way for politicians to achieve increased living standards in the short-run. In fact, there is evidence that the rapid rise in inequality led to popular pressures on politicians for promoting credit expansion (Bazillier & Hericourt, 2017; Kumhof et al., 2015; Rajan, 2010), mostly driven by middle income classes (Bazillier et al., 2017). In Anglo-Saxon countries, consumer credit to low income households is particularly developed, i.e. car loans, TV loans,

² See Alesina (1988) and Drazen (2000) for in depth review of theoretical and empirical work on the PBC.

and even short-term loans on consumption baskets (Dagdeviren et al., 2019). Crucially, credit is most of the time required to accede to homeownership, one of latest additions to the list of these “great societal expectations”, fueled by politicians’ promises³. On the other hand, wealth rather than income has been increasingly shaping households’ preferences in recent decades (Pagliari et al., 2018), not the least through house price increases (Ansell, 2014). For most households, wealth entirely consists of housing wealth (Causa and Woloszko 2019). When house prices rise, wealth rises too and homeowners may cash-in or self-insure against income loss. And indeed, the extreme expansion in credit that has been seen in the last 50 years was for a very large part loans collateralized on house prices (Jordà et al., 2015). In the long term, the credit cycle and the house cycle appear to be just two sides of the same coin.

As such, on the supply side, governments have often been embracing credit expansion and fueling credit cycles across the political spectrum. Indeed, it appears that both left and right parties have been competing to represent homeowners (Schelkle, 2012; Kohl, 2018). In the Netherlands, Sweden and Denmark, mortgage deregulation, if started under conservative governments, have largely been doubled down by following centre-left governments. More generally, wealth protection has increasingly been shaping preferences and priorities of voters (Chwioroth and Walter, 2019).

The long-term costs of credit cycles could in theory discourage politicians to fuel booms. All the more so as politically-motivated credit cycles may *ceteris paribus* be more dangerous than normal credit cycles: credit growth policies and incentivization may indeed induce market distortions by e.g. weakening screening and monitoring and increase moral hazard (See Calomiris and Haber 2014). Credit subsidies may increase risk-taking by lowering lending standards (Agarwal et al (2012)) and credit guarantees may similarly lead to traditional moral hazard issues.⁴ Why would people reward politicians if they may be harmed by such credit manipulation later on? Financial crises inflict great costs for societies and, as a consequence, for politicians.⁵ In the PBC literature, such discussion was linked to whether voters are rational or myopic, whether they adjust inflation expectations and thus punish politicians for inflating the economy (Drazen, 2000; Nordhaus, 1975). A fundamental difference with such literature relates to the time horizon: financial cycles are much longer than business cycles (6 quarters to 8 years for a standard business cycles and 10 to 20 years for the financial cycle (Drehmann et al., 2012)). In addition, financial cycles rarely end up in crises – financial crises happen on average every 40 years⁶, which is evidently much less frequent than inflation episodes or macroeconomic downturns. This contrasts with a politician’s horizon which is no longer than his term (generally 4 or 5 years per term) in democracies. Thus, politicians engineering credit booms on the back of the build-up of vulnerabilities should not care nor should they be affected by potential future crises, downplaying a potentially important caveat for our argument regarding the existence of political credit cycles.

Finally, beyond supply and demand, a conceptual framework for political credit cycles requires policymakers to have the capacity and toolkit to influence credit expansion. First, there should be no

³ See Mian & Sufi (2010, 2013) for a careful analysis of the political drift towards home ownership promotion in the US (expansion of Fanny and Freddy, subsidization of mortgage loans, lax regulation).

⁴ The impact of political credit cycles on financial vulnerabilities is tested in Section 4 below.

⁵ Incumbents in high veto player environment indeed lose on average 12% more in vote share in crisis-struck countries compared to crisis-free countries (Chwioroth & Walter, 2017).

⁶ Estimation based on Laeven and Valencia (2018)

strong institutional obstacles for politicians to engineer such booms, reflected in potential checks and balances from actors more concerned about the medium-run financial stability. Second, governments should have authority over the instruments enabling credit expansion. All in all, as Franzese and Jusko (2009) clearly put it, “election-motivated incumbents will prefer policies that are more targetable and timeable (by incumbents, to voters), manipulable (by incumbents), palpable (to voters), and attributable (by voters, to incumbents).”

Perhaps the easiest channel is to rely on state-owned banks or politically connected banks to increase credit in the short run⁷. Second, monetary policy appears an obvious tool for incentivizing credit growth, with lower policy rates leading to lower lending rates and credit expansion – depending importantly on the degree of legal and actual independence of the central bank. Fiscal policy may yet be another key tool, e.g. subsidies for mortgage credit. However, the crucial takeaway from the US-focused literature on the political economy of financialization and credit expansion is that politicians tried to stimulate private borrowing, notably through the dismantling of credit restrictions, *in lieu* of more aggressive fiscal redistribution, acting as a substitute to the Welfare State (Krippner, 2012; McCarty et al., 2013; Prasad, 2012). Finally, a wide range of other financial sectors policies may be used to manipulate credit directly or indirectly: macroprudential policies may be relaxed to boost credit – asset-based macroprudential tools e.g. loan to value ratios and debt to income ratios caps appear obvious candidates (Müller, 2019), capital based instruments like risk weights on loans may be relaxed, but also liability based tools – by allowing easier and cheaper access to funding, banks and other lending institutions would be able to expand their lending portfolio. Thus, while a careful empirical analysis of the channels of transmission of political credit cycles is left for future research, the executive appears to have a potentially wide range of policies for manipulating credit.

1.2. Political Credit Cycles: Existing work and Contributions of the present paper

It is thus surprising, in light of more than four decades of work on PBC and the rich macroeconomic and macro-sociological literature on financialization and credit expansion, that only limited attempts have been made to explicitly extend and develop a model and analysis of potential political financial cycles and notably political credit cycles (PCC) in light of the crucial role played by credit in financial dynamics.

A set of papers looked at the lending patterns of government-owned banks in election years relative to private banks, starting with Dinc (2005) and later confirmed by several country-specific studies: evidence of a systematic adjustment of lending by state-owned banks around elections compared to private banks has been found in Turkey (Bircan & Saka, 2018), Brazil (Carvalho, 2014), and Germany (Englmaier & Stowasser, 2017). In addition, these studies point to important consequences of these lending cycles, both for the real economy (Bircan & Saka, 2018)) and for financial vulnerabilities (reduced bank profitability and growth in non-performing loans (Englmaier & Stowasser, 2017)). While this literature tried to get closer to the precise microchannel through which political credit cycles may come at play (through state banks), a broader analysis is necessary as political motivated lending from state-owned banks is only one among many channels through which governments may induce credit expansion for political gains. In addition, this argument is specific to countries where the State retains important stake in the financial sector, a phenomenon mostly of emerging economies and becoming less frequent (Abiad et al., 2010;

⁷ The importance of such channel will significantly vary depending on the degree of state ownership of banks.

Denk & Gomes, 2017), or specific to countries where local banks are tightly linked with politicians.⁸ Overall, such strand of work still focuses on country-specific case studies and on only one specific channel of credit manipulation. One exception of recent and more direct cross-country tests of opportunistic political credit cycles is Kern and Amri (2020), which finds an increase in credit (both public and private) in election years, especially in developing countries and countries where governments own a substantial share of the domestic banking sector.

This paper attempts to deepen the current literature in several respects. First, one key objective of the paper is to test the existence of political credit cycles beyond election years, in line with recent research (surveyed below) pointing to the importance of accountability mechanisms beyond elections. Specifically, I point to continuous executive approval series as a promising avenue for analyzing the potential of macroeconomic manipulation by governments throughout political term. Such a continuous variable does not require assumptions on the timing of a policy change before an election and assumption about materialization of the change into voters' preferences. A PCC hypothesis in this context is that drops in government popularity will be associated with future credit expansion. The use of government approval data in turn provides another important benefit compared to elections, namely that it allows to extend the empirical analysis to non-democracies, while, in such countries, elections are not a credible mean to hold leaders accountable.

The second contribution of this paper is to deepen the analysis of the transmission mechanisms – using granular data on credit I am able to distinguish credit to households and corporate credit. I also deepen and extend efforts to identify favourable or discouraging factors and circumstances for such cycles. Thirdly and finally, I test the significance of such possible credit manipulation for medium-run financial stability, while little attempt had been provided in assessing the real economy implications of possible electoral manipulation of credit.⁹

2. Testing the Existence of Political Credit Cycles

This section presents our empirical strategy and baseline to test the above-mentioned questions. It starts by replicating standard PBC models around election years before turning to tests of political credit cycles beyond election years, introducing a new dataset compiled for this paper on government popularity.

2.1. Elections and credit cycles

First, I test a standard PBC model, similar to Kern and Amri (2020) who find evidence of increasing credit in election years.

⁸ Two recent work are worth noting in this respect, demonstrating political lending cycles beyond state-owned banks: Markgraf (2018) shows evidence of political cycles in bank manager appointments for formally independent Spanish saving banks. Even more relevant, Delatte, Matray and Pinardon Touati (2019) show evidence of political credit cycles from French banks with no formal political connections, with banks granting politicians election favors in order to access the market for loans to French local government entities.

⁹ “Election economics” type of tests has a hard time gauging the significance of these cycles: if there is indeed a one-off increase in credit in or pre-election, the credit expansion may well be short-lived and “benign”, i.e. with no significant medium-term impact in both GDP growth and financial stability dynamics.

Data on credit is taken from recent important data compilation efforts by the IMF – the Global Debt Dataset, which covers private and public debt for virtually the entire world (190 countries) dating back to the 1950s (Mbaye, Moreno Badia, et al., 2018). It substantially expands other datasets covering credit to the non-financial private sector in terms of time span but most significantly in terms of country coverage. In addition to the extensive coverage, an important advantage of the dataset for the sake of the present study is the split between household (HH) and non-financial corporate (NFC) debt. Indeed, in the context of this paper's argument that politicians engineer credit booms for popularity or electoral gains, political credit cycles should be stronger for credit to households than for credit to NFCs. Stylized facts are provided in Appendix A.

The credit variable used in the empirical analysis follows the literature on early warning systems reviewed above in detrending the above series to extract only its cyclical component: the credit to GDP gap. While I also use the simple credit to GDP growth, I believe the gap provides additional information as it reflects the build-up of “imbalances”, not explained by fundamentals (credit to GDP growth may reflect a structural financial deepening) and potentially destabilizing.

Figure 1 - Total credit to GDP gap and elections

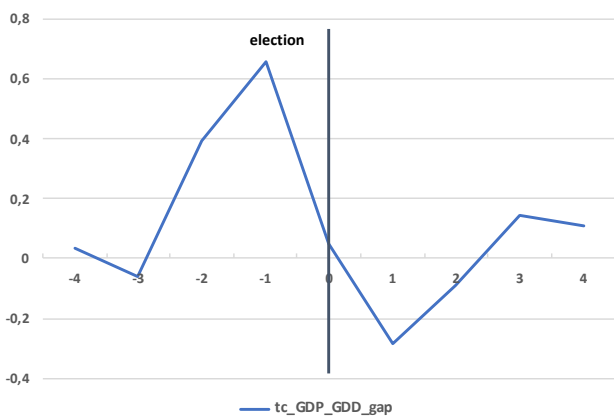


Figure 2 – Total credit to GDP growth and elections

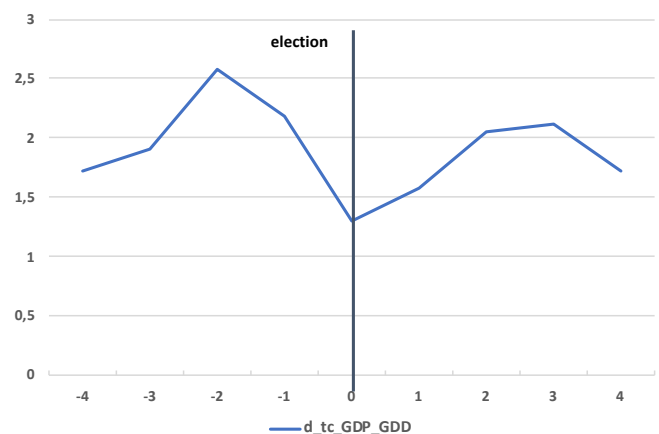


Figure 3 – Total credit to GDP gap and competitive elections

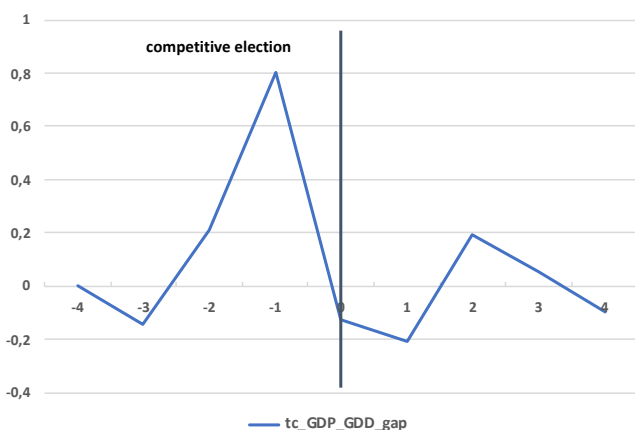
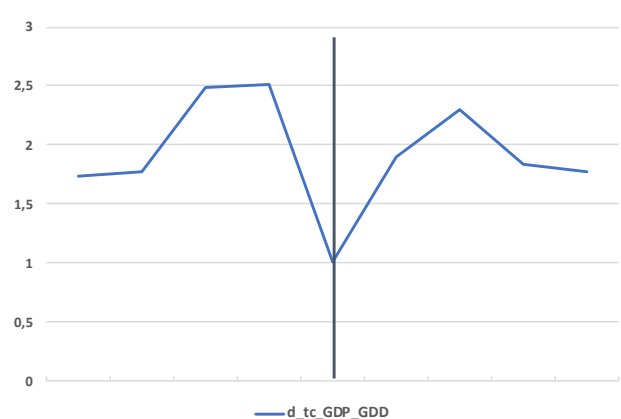


Figure 4 – Total credit to GDP growth and competitive elections



Note: average of sample [year-4; year+4], sample of democracies only (polity2 democracy score>6). Source: elections (Hyde & Marinov 2012, updated to 2015), credit (Global Debt Database); authors' calculations

Starting from simple plots of total credit to GDP gap and total credit growth in an 8 year-window around elections (Figure 1 and 2) and competitive elections (Figure 3 and 4), it appears rather striking that the credit cycle seems to peak right before elections.

I move on to test this more formally with a panel OLS econometric model, which will be my baseline and is standard in the literature on the determinants of credit cycles:

$$\textit{Credit to GDP gap}_{it} = \beta \textit{Election}_{it+k} + \Gamma X'_{it-1} + u_i + \mu_t + e_{it}$$

Where: *Credit to GDP gap*_{it} is total credit to GDP gap (credit to GDP series detrended using the HP filter with a smoothing parameter $\lambda=100$ as usual for series of annual frequency (Kern & Amri, 2020).

My variable of interest is an election dummy variable taking the value of one if an election is happening in that specific year. I try several leads of the election variable to test whether the credit gap is growing not only in election year, but also couple of years before as seems suggested by the above figures. The hypothesis of political credit cycles would predict a positive and significant β .

X'_{it-1} is a vector of controls that include time variant push and pull factors which follows the literature on the determinants of credit growth (Cerutti et al., 2015; Fendoğlu, 2017). To control for domestic macroeconomic variables, I add year on year growth of real GDP, to proxy the state of the domestic business cycle, and domestic interest rate to control for domestic monetary policy. Description and data sources of all variables used in the empirical analysis are provided in Table A 3 in Appendix A. The control variables are lagged by one year to reduce endogeneity concerns.

u_i represents country fixed effects intended to capture unobserved time invariant country-specific characteristics. Statistical tests also strongly support the inclusion of year fixed effects, here μ_t which will control for all time varying global factors affecting the credit to GDP gap. e_{it} is an error term. I use robust standard errors, clustered at the country level.

While recognizing that past level of credit growth is likely to partly determine current credit growth, I do not favor a dynamic estimation which would require using GMM methods as these estimation techniques are highly sensitive to parameter selection and other potential flaws¹⁰. I use a dynamic model with GMM in a robustness check (Appendix B).

¹⁰ In addition, the data structure is such that the panel dimension N (the number of countries) and the time dimension T (the number of years) are of similar order and reasonably large. The GMM methods are intended for datasets with large N and small T (Blundell and Bond 2000). When T is relatively large, as is the case in this data, there is an instrument proliferation problem which biases the GMM coefficient estimates towards the non-instrumented panel estimates and causes statistical tests for mis-specification to be weak (Roodman 2009).

Table 1: Elections and credit cycles

Dependent Variable	Total Gap	Total Gap	Total Gap	Total Gap	Total Gap
	1	2	3	4	5
Executive election	-0.610 0.37				
Executive election (t-1)		0.866**	1.103**		
		0.42	0.46		
Executive election (t-2)			0.620		
			0.42		
Executive election (t-3)			-0.088		
			0.45		
Competitive exec. elec. (t-1)				0.915*	
				0.47	
Competitive exec. elec. (t-1)					1.067*
					0.63
Interest rates (t-1)	-0.000***	-0.000***	-0.000***	-0.000***	-0.000***
	0.00	0.00	0.00	0.00	0.00
Real GDP growth (t-1)	-0.371***	-0.311***	-0.337***	-0.312***	-0.310***
	0.12	0.11	0.12	0.11	0.11
Crisis	5.489***	5.609***	5.488***	5.594***	5.611***
	1.26	1.32	1.35	1.33	1.32
Constant	0.575	-0.079	-0.474	0.099	0.080
	0.90	0.86	0.88	0.82	0.82
Year & Country FE	Yes	Yes	Yes	Yes	Yes
Observations	1,341	1,290	1,184	1,290	1,290
R-squared	0.193	0.190	0.196	0.189	0.189
Number of ifs_code	61	61	61	61	61

Note: Regressions ran using OLS with country and year fixed effects. The DV is the total credit to GDP gap. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

Table 1 displays a variety of tests of political credit cycles around executive election time, only in democracies (with a polity2 score superior to 6¹¹). While I do not find evidence of higher credit to GDP gap specifically in election year (col 1), I do find evidence of political credit cycles right before election (t-1) significant at the 5% level (col 2 and 3), which is still consistent with the hypothesis of political credit cycles¹². There is no evidence of higher credit at the 2nd and 3rd lag.

I then test whether political credit cycles are more prevalent for competitive elections (as expected in the PBC literature) using two alternative coding strategies used in the literature, namely when the share of the seats for the government party is lower than 60% following Prichard (2016) with the data from the DPI, as well as a measure based on the difference between the performance of the main government party and that of the main opposition party (see Efthyvoulou (2012)) is below the median of the sample – in this case 9.8%. Coefficients are still positive and significant (col 4 and 5) and further confirm that PCC mechanisms may be at play.

¹¹ The Polity2 score captures political regime authority spectrum on a 21-point scale ranging from -10 (hereditary monarchy) to +10 (consolidated democracy). Democracies are defined with scores from +6 to +10 (See Center for Systemic Peace (CSP), Polity IV dataset version 2015).

¹² Using dummies before instead of on election years is in line with Julio and Yook (2012) and Canes-Wrone and Park (2012) and helps account for the fact that if the election happens relatively at the beginning of the year, a positive credit in election years would capture something very different than pre-election credit engineering like optimism for a new government.

2.2. Moving beyond “election economics”: Government popularity and credit cycles

Having confirmed the existence of political credit cycles right before election years, this section now turns to the key contribution of this paper, which is to highlight that political credit cycles mechanisms may be at play beyond elections.

The PBC literature has solely focused on elections as their variable of interest (a dummy variable on whether there is an election at time t) and it is unsurprising that much of the recent work reviewed above has also kept this lens. However, such approach may be missing a large part of the policy and political dynamics by restricting the window of studies around elections as Tucker (2018) also points out. It is indeed not obvious that politicians would manipulate credit in (or right before) election years. Some of the reforms discussed in the context of encouraging credit expansion, especially in the context of the promotion of homeownership are part of important reform agendas which require significant political capital and whose sequence is planned along the political term. As Franzese and Jusko (2009) emphasizes, “post-electoral largesse is greater and more certain than pre-electoral, especially as newly seated governments are the most productive (honeymoons)”.

Thus, this paper favors a continuous variable as my main political cycle proxy: specifically, I use cross-country executive approval ratings data. Carlin et al (2012) notes that “surveys (...) are a largely underexploited opportunity to test current theories on the factors that best explain executive approval in a fully cross-national setting” and points to the crucial need to “study the evolution of citizen support for their leaders in nonelectoral periods”. Politicians care more and more about their continuous approval ratings along the political term, often described by the media as close to “obsession” and referred to by the expression “the tyranny of the polls”. Accountability is getting stronger and stronger between elections leading politicians to adjust their political agendas and policies accordingly.

Building a new cross-country dataset of government popularity

An important effort of the present paper is thus the compilation of a cross-country database of government popularity over time. I proxy government popularity by executive (presidential or government) approval data collected from opinion polls.

Within existing popularity datasets, Gallup World Poll is a unique cross-country dataset on the rate of approval in the executive leader, but the data is private and only available from 2005 which restricts the use of the data for panel regression exercises. The OECD has also built a Trust Database which compiles series of institutional trust for OECD countries (González & Smith, 2017), among which trust in government, but with small data coverage. An outstanding effort to compile national polling series for an important number of countries – although mostly in Latin America - is the Executive Approval Database project (Carlin et al 2016)¹³. As mentioned above, one of the unique works that analyse the link between popularity and financial vulnerability is Herrera et al (2019). They however use the subcomponent “government stability” from the political risk index of International Country Risk Group as proxy for government popularity, which I prefer not to use¹⁴.

¹³ <http://www.executiveapprovaldata.org/>

¹⁴ It appears preferable to build my own database of government popularity from polls because 1) population polls appear a more proximate proxy of government popularity than expert surveys (like the ICRG) can be for the test of this paper's

I leverage from three key sources for compiling my new dataset: first, on series collected through the Executive Approval Database project, selecting within each country the series that i) has the longest continuous range, ii) preferably has a monthly frequency, iii) is from credible research institute (e.g; IPSOS and GALLUP being some of the largest polling firms worldwide); second I extend the dataset with sources described in Herrera et al (2019); third, on regional public efforts to regularly survey the population, including asking about the satisfaction with the government: the Latinobarometro covers 19 countries and has data on government approval since 2002. The Eurobarometer is run biannually for all EU countries since 2000. Unfortunately, other regional efforts in Asia, Africa and Middle East are too recent or run in wave with gaps of several years and cannot be used in this context. I finally complete with a few selected national sources.

The final dataset covers 58 advanced and emerging countries as shown in Figure 5, going back to the 1970's for some countries and available monthly for most countries. The final sample is also globally representative, covering Europe, North, Central, and South America, as well as Asia. Appendix A provides a description of the dataset construction, sources, summary statistics and discusses empirical issues linked to government popularity data.

Figure 5 – Collected series on government popularity



argument, 2) only the third leg of the ICRG indicator is relevant as government popularity measure and may be blurred with the institutional subcomponents, 3) the control and dependent variable would not allow a coverage as wide as the full ICRG sample. I still test it as robustness check.

Political credit cycles beyond election years

Using the above described dataset, I adjust the baseline model introduced in Section 2.1 to test this paper's main hypothesis that declines in government popularity predict future credit cycles.

$$\{\textit{Credit to GDP gap}_{it}; \Delta\textit{Credit to GDP}_{it}\} = \beta d. \textit{Popularity}_{it-k} + \Gamma X'_{it-1} + u_i + \mu_t + e_{it}$$

*Popularity*_{it-k}, i.e. the change in government popularity, proxies the political cycle. I test different lags of that variable to account for effects, which may potentially take longer to materialize. A negative and significant β would be consistent with PCC dynamics.

The baseline model includes the lagged domestic interest rates, the lagged real GDP growth, a crisis dummy, and the score of democracy under several alternative specifications.

Unlike PCC tests in election years, these sets of regressions can use both autocracies and democracies in our regressions, hence allowing to use as much countries as possible from the government popularity dataset. There is indeed important evidence that governments in autocratic countries care about popularity as much, if not more than in liberal democracies: Guriev and Treisman (2016) indeed argue that the stakes of a popular movement against autocrats are higher; more than decade-long rules are at stake; and even the actual regime and institutions that autocrats have set may be at risk¹⁵. Regime stability, limiting social unrest, and keeping international credibility are all reasons for authoritarian leaders to care about popularity. Soaring popularity is often flagged by authoritarian rulers as a justification for their legitimacy. Similarly, the pressure of evaluation and promotion in bureaucratic autocracies such as China generated political cycles of tax break policies (Chen & Zhang, 2021). While Kern and Amri (2020) find PCC more likely in developing countries, there is no a priori reason it should be the case: the fact that advanced economies are more financialized and have much larger housing markets should in our framework call for higher societal demands for credit.

Turning to the results, and starting from the controls, domestic interest rates is negatively related with credit growth and the credit to GDP gap, which is expected. GDP growth is positively correlated with credit growth, as expected, and negatively correlated with the credit to GDP gap. Being in crisis year is not surprisingly associated with lower credit growth and a higher credit to GDP gap (crisis will likely happen around the peak of the credit to GDP gap, something empirically demonstrated by Drehmann, Borio and Tsatsaronis (2012)). The democracy score displays mostly negative coefficients: higher democracy scores over time in the same countries lead on average to lower credit gaps (the effect is not significant for credit growth). One interpretation could be that while trying to increase credit growth in the short term may be a reality of all political systems, countries with lower democracy score could run or ride larger and longer credit cycles (deviation from long term trend) unchecked. Democracies have both check and balances in place which may limit unsustainable credit cycles, and the fact that there is likely to be a change in the executive every 4 or 5 years should also decrease the likelihood of sustained and large deviation from trend. Restricting the sample to democracies (polity2 score>6) the effect of higher democracy scores and its significance on the credit gap drops – it does not matter whether a

¹⁵ Sinking popularity appears for example to have triggered the coup that ousted Mohamed Morsi in Egypt in 2013 (Younis (2013)).

country is “more democratic” if it is already a democracy. This gives further ground to this paper's interpretation.

The main hypothesis of this paper appears substantially validated in the baseline results: declining popularity leads to a higher deviation of credit from its long run trend. The coefficients are negative and statistically significant at the 1% level. This is also the case when the credit to GDP gap is replaced by the simple growth rate of the credit to GDP ratio. These results are robust to both country and time fixed effects. To provide an order of magnitude, and taking the regression of credit growth, which is intuitively easier to interpret than the credit to GDP gap, a one standard deviation drop in government popularity (around 10% drop) leads to an additional 1 point increase in the change of credit to GDP, roughly equal to the sample mean of credit growth.

The relationship between declining government popularity is furthermore robust to different combination of lags of the popularity variable¹⁶ (Columns 1 & 4 in Table 2 and 1-5 in Table A 4 in Appendix A), to the addition of various other control variables (Columns 6-14 in Table A 4): namely total capital inflows, the level of development (proxied by the logged real GDP per capita), capital account openness (using the Chinn-Ito index of capital account openness), central bank independence, and the index of financial reforms of Abiad et al (2010) extended by Denk and Gomes (2017). The burden of evidence in the case of drops in government popularity compared to elections the holding of which are supposed to be exogenous and thus allows identification is evidently stronger. Extensive additional robustness checks on reverse causality, confounding factors, dependent variable, symmetry and sample splits are thus provided in Appendix B. and confirm these general results.

¹⁶ I try 3 lags of government popularity (more than 3 year lags appears too much in light of a normal executive term (which is usually four or five years). The third lag is not significant. So the effect on credit appears to materialize with a delay of 2 lags. Government popularity change over 3 years appears particularly significant, potentially hinting to a cumulative effect.

Table 2 : Baseline results - Government popularity and credit cycles

Dependent variable	Total Gap	Total Gap	Total Gap	Credit growth	Credit growth	HH Gap	HH Gap	HH Gap	HH credit growth	NFC Gap	NFC credit growth	Total Gap	Total Gap
	1	2	3	4	5	6	7	8	9	10	11	EME	Adv
Δ Gov. popularity (t-1)	-0.068***	-0.051***		-0.087**	-0.087**	-0.022**	-0.027**		-0.014**	-0.028	-0.065*	-0.033	-0.099***
	0.02	0.02		0.04	0.04	0.01	0.01		0.01	0.02	0.04	0.02	0.03
Δ Gov. popularity (t-2)	-0.041			-0.008			-0.010					-0.036	-0.048
	0.03			0.03			0.01					0.04	0.03
Δ Gov. popularity (t-3)	-0.017			0.016			-0.010					0.011	-0.053
	0.02			0.02			0.01					0.03	0.03
Δ Gov. popularity (3Y sum)			-0.125***					-0.047*					
			0.04					0.02					
Interest rates (t-1)	-0.000***	-0.000***	-0.000***	-0.000	-0.000	0.000**	0.000**	0.000**	-0.002***	0.000	-0.006***	-0.000***	0.472*
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.26
Real GDP growth (t-1)	-0.439**	-0.381**	-0.449**	0.271	0.341	-0.226*	-0.247*	-0.251*	0.094	-0.218**	0.196	-0.161	-0.917***
	0.20	0.17	0.20	0.26	0.24	0.13	0.15	0.15	0.07	0.11	0.25	0.24	0.25
Crisis dummy	5.703***	6.915***	5.662***	-3.336	-2.795	1.364***	1.160***	1.139***	-0.352	5.129***	-0.457	4.429**	4.869**
	1.38	1.56	1.39	2.15	1.96	0.45	0.40	0.39	0.43	1.51	1.40	1.77	1.94
Democracy score	-1.155***	-1.169***	-1.176***	-0.311	-0.354	-0.794**	-0.768	-0.801	0.009	-0.522	-1.032	-1.056**	-3.578***
	0.28	0.24	0.27	0.36	0.36	0.39	0.50	0.50	0.22	0.61	0.99	0.44	1.01
Constant	11.345***	9.817***	11.710***	5.509	4.891	6.587*	6.902	7.259	1.156	5.001	10.543	6.122	31.788**
	3.03	2.31	2.95	3.40	3.40	3.50	4.43	4.45	2.07	5.59	9.29	3.98	12.72
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	683	760	683	682	758	647	581	581	639	646	638	282	401
R-squared	0.293	0.271	0.290	0.179	0.163	0.376	0.400	0.398	0.291	0.206	0.134	0.316	0.441
Number of co	43	43	43	43	43	37	37	37	37	37	37	18	25

Note: Regressions ran using OLS with country and year fixed effects. The DV proxying different dimensions of the credit cycle changes depending on the regression and is indicated in the first row. * p<0.10, ** p<0.05, *** p<0.01

I then leverage on a key split allowed by my credit series, namely between households and non-financial corporates (Table 2, Columns 6-11). The effect of government popularity on the credit to HH gap and credit to HH growth is negative and significant at the 5% level. In contrast, while the credit to NFCs variables also display a negative coefficient, only simple credit growth is significant, at the 10% level. These findings on political credit cycles are thus driven by household rather than corporate credit. This gives credence to this paper's conceptual framework as it seems intuitively more likely that governments that want to ride credit cycles to gain popularity will seek to get closer to the actual voters/constituents and target credit to households. Favoring credit to NFCs may materialize in future popularity gains too but only indirectly, a second order effect.

Next, I split further the sample between emerging and advanced economies, expecting PCC to be stronger in advanced economies (being more financially developed and having better functioning credit markets). This is indeed the case (Columns 10-11), a result that contrasts with the analysis of Herrera, et al (2019) and Kern and Amri (2020).

3. Amplification and Mitigation of Political Credit Cycles

The previous section has demonstrated that political credit cycles happen not only before election years but importantly throughout the political term when government popularity drops. In addition, such credit manipulation appears to target credit to households specifically, and is more prevalent in advanced financialized economies. However, motivations for credit manipulation may further depend on initial country specific circumstances: high inequality, type of party in power, type of ideology that underpins it. This section thus extends the analysis by testing amplifying or mitigating factors influencing the magnitude of political credit cycles. Copelovitch and Myren (2018) for instance argue that the likelihood of using credit expansion as a policy option depends on the macroeconomic constraints imposed by the Mundell trilemma. In particular, they demonstrate that credit growth will be higher under fixed exchange rate, high levels of capital account openness, during economic downturns and not upswings, and under left wing governments. It also argues that “credit growth policies” will be more likely when fiscal policy too is constrained. Kern and Amri (2020) also find that financial openness appears to mitigate the potential for political credit cycles, partly by putting hard constraints on both fiscal and monetary policies, thus eliminating options to manipulate credit.

To test such amplifying and mitigating factors on our political credit cycles, I use the same model as previously, interacting the government popularity variable with the other variable of interest (denoted Z here). The change in government popularity, the variable of interest, and the interaction variable are lagged by one year. Theoretically, this means testing whether the relationship between declining popularity at year t-1 and credit cycle at year t is conditional on the value of the variable of interest at year t-1.

$$\left. \begin{aligned} & \{ \text{Credit to GDP gap}_{it} \} \\ & \left\{ ; \Delta \text{Credit to GDP}_{it} \right\} \\ & = \beta \text{Popularity}_{it-1} + \partial Z_{it-1} + \phi \text{Popularity}_{it-1} * Z_{it-1} + \Gamma X'_{it-1} + u_i + \mu_t \\ & + e_{it} \end{aligned} \right\}$$

3.1. Political and macroeconomic context

Inequality

The first potential element influencing the magnitude of political credit cycle tested here is the level of inequality, and specifically whether countries with high inequality are more likely to experience larger political credit cycles. The existing literature showed evidence of a strong relationship between inequality and the level of credit to GDP (Kumhof et al (2015), Bazillier et al (2017; 2015), Ansell and Ahlquist (2017)). It may thus be expected that politicians to have even more incentives to manipulate credit in more unequal countries.

To do so, I add the Gini coefficient to the baseline model: the coefficient is non-significant and the results on government popularity are not altered (Table 3, column 1). Next, I allow for interaction between inequality and the change in government popularity, i.e. whether the potential for political credit cycles depends on the level of inequality of a country. I do not find any significant effect of the interacted variable (Column 2). Whether pre and post-tax Gini measures are used do not affect the results. While important research has demonstrated a long-run relationship between inequality and the level of credit, I do not find empirical support for a cyclical relationship – the impact of inequality on the credit cycles.¹⁷

¹⁷ However, as explained in e.g. Ansell (2014), preferences of households seem to be shifting from income to wealth at a time of financialized societies. One further research avenue would be to test the effect of wealth inequality instead of traditional income inequality variables like the Gini coefficients.

Table 3 – Interaction effects

Dep var: Total credit to GDP gap	Inequality		Partisanship						Central Bank Indep	
	1	2	3	4	5	6	7	8	9	10
Δ Gov. popularity (t-1)	-0.060***	-0.128	-0.048**	-0.054	-0.007	-0.062	-0.055	-0.054	-0.041**	-0.004
	0.02	0.14	0.02	0.05	0.04	0.05	0.04	0.04	0.02	0.03
Execrlc_dpi =1 (t-1)			-1.431							
			2.37							
Execrlc_dpi =2 (t-1)			1.431							
			2.33							
Execrlc_dpi =3 (t-1)			-1.098							
			2.28							
Net Gini coef (t-1)	0.509	0.506								
	0.61	0.60								
Net Gini coef * Δ Gov. popularity (t-1)		0.002								
		0.00								
Gov_party=2 (t-1)				0.065	0.476					
				1.73	1.71					
Gov_party=3 (t-1)				0.588	1.028					
				1.74	1.80					
Gov_party=4 (t-1)				0.508	0.922					
				1.92	2.06					
Gov_party=5 (t-1)				1.654	2.143					
				1.78	1.82					
gov_party_2 * Δ Gov. popularity (t-1)					-0.154					
					0.12					
gov_party_3 * Δ Gov. popularity (t-1)					-0.294*					
					0.15					
gov_party_4 * Δ Gov. popularity (t-1)					-0.191					
					0.14					
gov_party_5 * Δ Gov. popularity (t-1)					0.201					
					0.16					
Right-wing gov (t-1)						-0.002				
						0.01				
Right gov * Δ Gov. popularity (t-1)						-0.001				
						0.00				
Left-wing gov (t-1)							0.011			
							0.02			
Left gov * Δ Gov. popularity (t-1)							-0.001			
							0.00			
Center gov (t-1)								-0.021		
								0.02		
Center gov * Δ Gov. popularity (t-1)								0.001		
								0.00		
Central bank indep (t-1)									4.553	-0.785
									2.90	0.92
CBI * Δ Gov. popularity (t-1)									-0.029	0.04
									0.00	0.00
Interest rates (t-1)	-0.000***	-0.000***	-0.000***	-0.232***	-0.219***	-0.173***	-0.180***	-0.166***	-0.000***	0.000
	0.00	0.00	0.00	0.05	0.05	0.04	0.04	0.04	0.00	0.00
Real GDP growth (t-1)	-0.252	-0.254	-0.365**	-0.701**	-0.693**	-0.496*	-0.531**	-0.466*	-0.317**	-0.098*
	0.17	0.17	0.17	0.28	0.27	0.25	0.25	0.25	0.15	0.05
Crisis dummy	10.469**	10.455***	6.686***	5.515**	5.677**	5.899**	5.525**	5.579**	5.857***	1.284**
	3.90	3.88	1.48	2.36	2.46	2.37	2.39	2.23	1.57	0.51
Democracy score			-1.310***	-1.246	-1.719	-0.061	-0.223	-0.192	-1.218***	-0.886**
			0.24	1.92	1.89	2.14	2.17	2.04	0.24	0.34
Constant	-16.038	-15.983	11.874***	15.378	19.205	3.668	4.865	4.702	8.482***	7.580**
	19.32	19.26	2.74	18.29	18.04	19.77	20.61	19.22	2.81	2.95
Year and country FE	Y	Y	Y	Y	Y	Y	Y	Y	Y	Y
Observations	750	750	758	491	491	466	466	466	667	559
R-squared	0.186	0.186	0.277	0.359	0.377	0.331	0.332	0.335	0.255	0.343
Number of co	45	45	43	28	28	28	28	28	42	36

Note: Regressions ran using OLS with country and year fixed effects. The DV is the total credit to GDP gap. * p<0.10, ** p<0.05, *** p<0.01

Table 3 – Interaction effects (continued)

Dep var. :	Level of Government Debt				
	(1)	(2)	(3)	(4)	(5)
	tc_gdp_gap	tc_gdp_gap	HHc_gdp_gap	Δ tc_gdp	Δ HHc_gdp
Δ Gov. popularity (t-1)	-0.041**	-0.111***	-0.041***	-0.161**	-0.022*
	0.02	0.04	0.01	0.07	0.01
Interest rates (t-1)	-0.000***	-0.000***	0.000	-0.000*	-0.002***
	0.00	0.00	0.00	0.00	0.00
Real GDP growth (t-1)	-0.526***	-0.560***	-0.264*	0.053	0.044
	0.17	0.17	0.14	0.22	0.08
Crisis dummy	6.997***	6.935***	1.483***	-2.694	-0.357
	1.69	1.68	0.44	2.29	0.48
Democracy score	-1.267*	-1.292*	-0.840*	-1.229	0.016
	0.70	0.70	0.47	0.93	0.22
Level of central government debt (t-1)	-0.058**	-0.059**	-0.026***	-0.128***	-0.048*
	0.03	0.03	0.01	0.04	0.02
Level of cgov. debt * Δ Gov. popularity (t-1)		0.002***	0.000***	0.002	0.000*
		0.00	0.00	0.00	0.00
Constant	12.447*	12.922**	7.733*	16.224*	2.244
	6.31	6.37	4.28	8.83	2.24
Year and country FE	Y	Y	Y	Y	Y
Observations	708	708	628	706	620
R-squared	0.303	0.308	0.397	0.236	0.354
Number of co	41	41	36	41	36

Note: Regressions ran using OLS with country and year fixed effects. The DV changes across regressions with col 1-2 being the total credit to GDP gap, col 3 being the credit to household to GDP gap, col 4 being total credit to GDP growth, col 5 being household credit to GDP growth. * p<0.10, ** p<0.05, *** p<0.01

Partisanship

The second potential element influencing the magnitude of political business cycle tested here is partisanship in the tradition of partisan PBC models. In contrast to opportunistic PBC models, partisan models assume that left-wing and right-wing parties have different ideological positions on economic issues, based on different preferences of their popular base and as a consequence different macroeconomic objectives and preferences. A left-wing party should thus pursue a more expansionary monetary policy during its term.

With regards to political credit cycles, it may be expected that some party dynamics also be at play, besides or on top of opportunistic cycles. For instance, Ahlquist and Ansell (2017) argue that facing rising inequalities, governments will either choose to redistribute or incentivize credit depending on their political party, with countries with a long-term tradition of left-wing governments less likely to rely on surge in borrowing. Broz (2013) provides hints that right-wing governments would preside over financial booms: right wing governments fund credit expansions and asset-price appreciation with foreign borrowing and deregulate financial activities in line with their pro-market ideology, leading to financial crises. Right-wing parties may indeed be expected to be less supportive of financial regulation, usually adopting free market stances, less reliant on social welfare spending, usually more concerned about fiscal deficits, and hence may be expected to rely more on engineering private credit cycles. I thus expect credit growth to be higher under right-wing governments or political credit cycles to be more frequent under right-wing governments.

I try two different measures of partisanship, one from the Database on Political Institutions (1 to 3 scale) or from the Comparative Political Dataset (1 to 5 scale) from left to right. Results do not support this hypothesis: there is no systematic relationship between credit growth and the type of political party in

office (column 3-4). The interaction between popularity and partisanship also provides no convincing results, regardless of whether I use the composite measure of partisanship (gov_party – column 5) or whether I go granular and test the interaction of the actual share of cabinet seats from a specific party (right, left, and center - columns 6 to 8).

3.2. Political credit cycles and policy constraints

Monetary policy constraints: Central Bank Independence

I then test whether countries in which there is high central bank independence are less likely to run political credit cycles. As monetary policy is a potential avenue for credit manipulation, I would expect the interaction between central bank independence and government popularity growth to be negative.

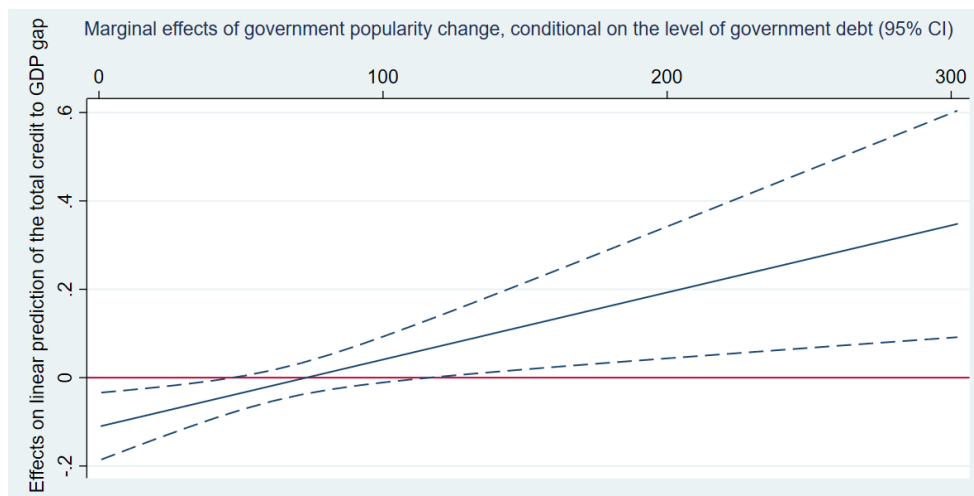
Central bank independence has no significant direct effect on the size of the credit cycles. The sign is negative as expected but not statistically significant (column 1). Turning to interaction effect with political credit cycles, I do not find any effect for the interaction between government popularity and central bank independence, in line with evidence in Kern and Amri (2020): while this paper does not yet try to identify the channel of transmission, i.e. the tools and policies used to engineer political credit cycles, this provides a preliminary suggestion that interest rates and monetary policy may not be the prime leverage. Another interpretation could be that central bank independence does not matter much in practice for political credit cycles – either because financial stability is not part of the monetary policy mandate and as a result the central bank, independent or not will not seek to mitigate credit cycles; alternatively it may be that *de facto* independence is much lower than suggested by its legal institutional features and so even central banks which are *de jure* independent may still bow to political requests of further credit provision.

Fiscal policy constraints: Level of Government Debt

As discussed previously, there may also be important theoretical relationships between political credit cycles and the fiscal situation of a country. Indeed, the above reviewed literature on the political economy of credit expansion highlights that credit expansion is favored in lieu of fiscal spending – that is, as the possibility to extend the welfare-state dried out in the context of constrained fiscal situations, government favored credit expansion. Credit expansion is thus seen as a substitute to welfare spending. I thus test the relationship between the government debt level on the likelihood of political credit cycles, tentatively expecting political credit cycles to be stronger in fiscally constrained countries.

The level of government debt is negatively and significantly associated with future credit growth, this separately from the government popularity effect. When level of government debt is high while government popularity is declining, the potential for political credit cycle is mitigated: the overall effect of the change in government popularity on the total credit to GDP gap becomes: $-0.111 + 0.002 * \text{level of debt in the past period}$. The conditional marginal effects are highlighted in Figure 6 below. With government debt to GDP of above 100%, the effect of a change in government popularity on credit turns positive.

Figure 6 – Marginal effects of the change in government popularity conditional on the growth in government spending



These results seem to go against the macro-sociological literature that political credit cycles happen when government debt is too high, and fiscal spending cannot be run, so governments rely on private sector credit expansion (See e.g. Prasad (2012), and Lepers (2021) for a review). On the opposite, instead of being substitute, they appear to be complementary: high government debt mitigates political credit cycles.

A deeper look of the “credit policies” available to policymakers indeed paradoxically notes that many such policies actually entail immediate or future fiscal costs to countries: mortgage subsidies, first-home buyers grant, higher tax deductibility of mortgages, direct loan guarantees or through GSE (government sponsored entities) are all linked to the current or future fiscal space. In terms of significance, the size and impact of such credit subsidies are potentially very large: e.g. mortgage guarantees and subsidies in the US in 2010 have been estimated to equal the size of the post-crisis fiscal stimulus of the American Recovery and Reinvestment Act (Lucas, 2016). Implicit subsidies from bailout expectations also would imply high fiscal costs. There is indeed recent evidence that excess private debt systematically turns into higher public debt (Mbaye, Chae, et al., 2018). Beyond the possible taking on of private debt by the public once in excess, fiscal spending may actually be one leverage/instrument through which credit is incentivized (See Lepers (2021) for a discussion).

4. Benign one-off credit increase or destabilizing manipulation?

This final section seeks to analyze a little further the economic significance of our political credit cycles. As may be the case for the traditional PBC literature, it could be that the impact of political cycles on economic variables is benign in the medium, e.g. if it quickly reverses after elections. I thus seek to test here whether the credit manipulation uncovered in the baseline does not leads to a one-off benign increase in credit or to economically large credit deviations in the medium run, with potential financial stability implications. Indeed, the traditional political economy time inconsistency issue comes into play: with a political term of 5 years on average, short-sighted policymakers will take the benefits of expanded credit growth and popularity gains at the expense of the potential build-up of long run financial vulnerabilities. The result that political credit cycles are mainly found with regards to household credit,

while not surprising from a political economy point of view, appears also worrisome from a financial stability point of view as recent evidence demonstrate that household credit (and specifically mortgage credit) is more dangerous than corporate credit and associated with more severe post crisis recessions (Bezemer & Zhang, 2019; Müller & Verner, 2021).

Looking at the potential to sustain medium-run credit booms over the whole political term may provide more insights in this regard than the simple impact of political credit cycles in election year and allow to contribute to a recent literature looking of the political determinants of crises. From a structural perspective, Lipsy (2018) demonstrates that in the long-run, democracies are more likely to experience financial crises than non-democracies. From a more cyclical lens, and contributing to the literature on early warning systems of crises, Herrera et al (2019) argues that “political booms”, measured by the growth in governments’ popularity predict financial crises above and beyond other early warning indicators, however only in the case of emerging economies. I depart on purpose from crisis regression work, which is associated with some caveats leading recent research on financial stability to build instead continuous variables of vulnerabilities (i.e. Duprey et al 2015, Gandrud and Hallerberg 2017) or model-free, intuitive, early warning and monitoring frameworks (Aikman et al., 2017; Bengtsson et al., 2018; Lepers & Sánchez Serrano, 2020). I also do not seek to assess the precise predictive power of political factors in leading to crises but rather to identify the channels through which vulnerabilities build up – here the credit channel.

I run the same baseline specification replacing the dependent variable by a dummy variable taking the value of 1 if the country is experiencing a credit boom, and a value of 0 if it is in normal times. While the measurement of credit booms has seen various propositions in the literature (Bakker et al., 2012; Cerutti et al., 2015; Gorton & Ordoñez, 2019; Mendoza & Terrones, 2012), I follow the simple approach of Fendoğlu (2017), namely the country is said to be experiencing a credit boom (taking the value of 1) if either of the following two conditions are met: (i) the credit to GDP gap exceeds 1.5 times its country specific standard deviation, and the annual credit to GDP growth exceeds 10%, or (ii) the annual change in the credit to GDP ratio exceeds 20%. I compute such credit boom dummies for both total credit and credit to households. Summary statistics of the credit boom series can be found in Table 4, a credit boom happens on average 6.5% of the time and 4.8% of the time for credit to household.

I further create a ‘bad boom’ dummy variable (following Bakker et al 2012, and Gorton and Ordonez 2019), which takes the value of one if the credit boom is following by a crisis within 3 years (with crisis dates defined by Laeven and Valencia).

Table 4 – Total credit boom and credit to households boom – Summary statistics

tc_GDD_boom				HH_GDD_boom			
_Fendoglu	Freq.	Percent	Cum.	_Fendoglu	Freq.	Percent	Cum.
0	4,717	93.46	93.46	0	1,711	95.21	95.21
1	330	6.54	100.00	1	86	4.79	100.00
Total	5,047	100.00		Total	1,797	100.00	

To run this specification, I make a few adjustments to my model: I drop the crisis dummy, I ran a probit model instead of panel OLS, which I try with and without country dummies. The year dummies are not

supported statistically so I drop them and replace them with the log of the VIX to still control for global factors.

Table 5 – Likelihood of credit booms

	(1)	(2)	(3)	(4)	(5)	(6)
Dep var:	Total credit boom	Total credit bad boom	Total credit bad boom	Household credit boom	Household credit bad boom	Household credit bad boom
Δ Gov. popularity (t-1)	-0.009 0.01			-0.017** 0.01	-0.026* 0.01	
Δ Gov. popularity (3Y av.)		-0.006 0.02	-0.014 0.02			-0.095*** 0.03
Interest rates (t-1)	-0.000* 0.00	-0.000*** 0.00	-0.000*** 0.00	0.001** 0.00	-0.000* 0.00	0.000 0.00
Real GDP growth (t-1)	0.010 0.03	-0.012 0.03	-0.021 0.03	-0.016 0.03	0.002 0.04	0.002 0.05
Crisis dummy	0.540* 0.29	0.756** 0.33	0.504 0.42	0.301 0.55	-2.818** 1.15	-5.501** 2.61
Democracy score	0.101 0.07	0.114 0.08	-0.007 0.06	-0.259*** 0.08	-0.234*** 0.05	-0.273*** 0.09
Constant	-4.295*** 1.21	-5.053*** 1.35	-3.450** 1.54	-0.812 1.86	7.079** 3.43	14.095* 7.36
Observations	720	661	661	613	720	661

Note: Regressions ran using probit. The DV alternatively takes different dimensions of credit cycles and is displayed in the first row. * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$

The effect is overall negative: a declining popularity in the previous year increases the likelihood of a credit boom in the subsequent year (Table 5). However, the effect is not significant for total credit boom, only significant in credit to households' boom. Still, the coefficient on the credit to household boom is significant at the 5% significance level, and the impact of declining popularity on bad household boom is even higher (and significant at the 1% level). While these results should be seen as preliminary and further research may be needed on the consequences on political credit cycles, they do point to a potentially destabilizing effect of political credit cycles and the risks from potential politically induced distortions to credit markets in the context of declining government popularity.

5. Conclusions & Avenues for Future Research

This paper analyses the interaction between credit and political cycles, arguing that governments will seek to ride and amplify credit cycles for political gains. Having first confirmed the existence of political credit cycles right before election years, I move beyond the election window by constructing a unique database on government popularity based on opinion polls for 57 countries starting in the 1980's and provide robust evidence that declining popularity is systematically associated with larger future credit cycles. Going granular, I find that such "credit populism" seem to target credit to household specifically rather than credit to corporates, going closer to voters. It is more prevalent in advanced economies and economies which are financially developed. I also provide hints that it increases the likelihood of unsustainable cycles, with potentially dangerous longer-term financial stability implications. These results contribute to the wide literature on the determinants of credit booms (Bakker et al., 2012; Fendoğlu, 2017; Gorton & Ordoñez, 2019; Mendoza & Terrones, 2012) and financial crises (Aikman et al., 2015;

Behn et al., 2013; Jordà et al., 2015; Schularick & Taylor, 2012) by emphasizing the crucial role of domestic political factors, and notably cyclical ones, and not simply time-invariant structural ones like democracy (Lipscy 2018). It also provides renewed evidence of the importance of popular demand (besides or in complement to private pressures)¹⁸ in driving financial dynamics.

Analyzing potential amplifying and mitigating factors, I find little evidence for partisanship, inequality or central bank independence, while indebted countries are less likely to manipulate credit. These latter two results provide preliminary insights on the policy tools which may be used in manipulating credit: as it could be reasonably expected that an independent central bank would constrain the use of interest rates for political motives, the absence of significant result may indicate that interest rates may not be the prime policy tool. In addition, and somewhat counterintuitively in light of the important literature on credit expansion in lieu of welfare spending, I find that highly indebted countries are less likely to run political credit cycles, acting as a constraint, and pointing that the fact credit subsidies may be an important manipulation tool. These findings resonate with the broader political economy and macro-sociology literature on domestic credit expansion, adding a cyclical perspective to this literature.

The construction of a cross-country dataset on executive approval adds to the literature on understanding government approval and to recent efforts aimed at cross-country analysis (Carlin et al., 2012, 2015; Guriev & Treisman, 2016; Herrera et al., 2019). On a methodological note, the use of continuous popularity data promoted in this paper instead of election dummy for political business cycle type of models appear in my view an overlooked and promising research avenue by enabling an analysis of the potential of macroeconomic manipulation by governments at various phases of the political term. Beyond credit cycles, it may be used in various aspects of the wider literature on political business cycles cycles (Alesina & Roubini, 1992; Drazen, 2000; Hibbs et al., 1977; Nordhaus, 1975), and notably applied to fiscal and monetary policy analysis.

I see two direct and important avenues for future research out of these results. First, there is a clear need to dig deeper into the policy toolkit used for manipulating credit. Existing research, and the present paper is no exception, has not reached this step yet basically due to the non-existence of policy data on tools that may be “credit-enhancing” or “credit-reducing”. I see important work to be done in clearly specifying a taxonomy of possible tools, and collecting data on policy changes across countries and over time. More specifically with regards to the results on fiscal policy, more research is needed to understand the relationship between fiscal policy and credit expansion, with the perspective that fiscal policy may be a leverage for - and not a substitute to – credit expansion (See Lepers (2021) for an attempt).

Second, the conclusion that politicians may manipulate credit at the expense of longer-term financial instability, asks the questions of the role of macroprudential authorities, a relatively recent invention, in this context and their relationship with the executive branch. Macroprudential authorities are specifically created to limit systemic risk and increase resilience. While the appropriate role and effectiveness of such authorities is still being debated, their institutional design is even more crucial in the context of political credit cycles. Future research should analyze such institutional features specifically in light of this interaction. Political credit cycles may indeed be one argument in favor of granting more independence

¹⁸ While a large literature focuses on crony capitalism and regulatory capture, an emerging literature reasserts the importance of popular demand and citizens perceptions in driving financial policy and regulation. Chwioroth and Walter (2019) most recently argued for a median voter explanation to the increased recourse to bank bailout by governments.

from the political branch to the macroprudential authority. As the price stability mandate of central banks may clash with politicians' emphasis on growth and employment, independent macroprudential authorities may clash with governments encouraging credit expansion at the expense of crises ...

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APPENDIX A. Data description and stylized facts

A new database on government popularity

This section provides a detailed description of the dataset construction, sources, summary statistics as well as a discussion of empirical issues linked to government popularity data. Collected series by country are described in Table A1. Summary statistics for the full sample, for advanced and for emerging economies are provided in Table A2, showing that there is no major difference in the volatility and mean of the two groups of countries.

Table A 1 Government Popularity Dataset – Coverage and sources

Country	Coverage	D/M/Q/Y	Government/President	Source
Argentina	1984-2015	M	Gov	Ipsos Mora y Araujo
Australia	1985-2015	M	Pres/PM	Newspoll
Austria	2001-2017	H	Gov	Eurobarometer
Belgium	2001-2017	H	Gov	Eurobarometer
Bolivia	2001-2018	M	Pres/PM	IPSOS Apoyo
Brazil	1987-2010	M	Pres/PM	Fonte: Datafolha
Bulgaria	1990-2013	M	PM	NCIOM
Canada	1985-2009	Q	Pres/PM	http://www.queensu.ca/cora/trends
Chile	2002-2017	Y	Pres/PM	Latinobarometer
Colombia	1994-2018	M	Pres/PM	Gallup Colombia - Desempeno
Costa Rica	1978-2016	Q	Pres/PM	CID Gallup
Croatia	2004-2017	H	Gov	Eurobarometer
Cyprus	2004-2017	H	Gov	Eurobarometer
Czech Republic	2001-2018	H	Gov	CVVM
Denmark	2001-2017	H	Gov	Eurobarometer
Ecuador	1979-2011	Y	Pres/PM	Cedatos
El Salvador	1986-2017	M	Pres/PM	Gallup
Estonia	2004-2017	H	Gov	Eurobarometer
Finland	2001-2017	H	Gov	Eurobarometer
France	1978-2018	M	Pres/PM	TNS Sofres
Germany	1953-2018	M	PM	IfD-Allensbach & ARD-DeutschlandTREND/Infratest dimap
Greece	2001-2017	H	Gov	Eurobarometer
Guatemala	1987-2018	Q	Pres/PM	Gallup
Honduras	1986-2018	M	Pres/PM	Gallup
HongKong	1992-2018	M	Gov	https://www.hkpopop.hku.hk/english/popexpress/trust/trusthkgov/poll/datatables.html
Hungary	1998-2014	H	Gov	IPSOS

Iceland	1992-2016	M	Gov	Gallup
Ireland	2001-2017	M	Gov	IPSOS MRBI Ireland
Italy	2001-2017	H	Gov	Eurobarometer
Japan	1998-2018	M	Gov	NHK
Korea	1988-2018	Q	Pres/PM	Gallup Korea
Latvia	2004-2017	H	Gov	Eurobarometer
Lithuania	2004-2017	H	Gov	Eurobarometer
Luxembourg	2001-2017	H	Gov	Eurobarometer
Malaysia	2007-2018	M	Pres/PM	Merdeka
Mexico	1997-2016	M	Pres/PM	BCG Beltran Juarez y Asociados (Gobernar)
Malta	2004-2017	H	Gov	Eurobarometer
Netherlands	2001-2017	H	Gov	Eurobarometer
New Zealand	1999-2016	M	Pres/PM	https://thespinoff.co.nz/politics/23-03-2017/a-statistical-analysis-of-john-keys-legacy/
Nicaragua	1989-2017	Q	Pres/PM	CID Gallup
Panama	2002-2017	Y	Pres/PM	Latinobarometer
Paraguay	2002-2017	Y	Pres/PM	Latinobarometer
Peru	1983-2018	M	Pres/PM	IPSOS Apoyo y mercado
Philippines	1986-2018	M	Pres/PM	SWS
Poland	1993-2018	M	Pres/PM	https://cbos.pl/EN/trends/trends.php?trend_parametr=stosunek_do_rzadu
Portugal	1986-2018	M	PM	Euroexpansao
Rep Dom	2004-2017	Y	Pres/PM	Latinobarometer
Romania	2004-2017	H	Gov	Eurobarometer
Russia	1990-2018	M	Pres/PM	Levada & D.Treisman
Slovakia	2004-2017	H	Gov	Eurobarometer
Slovenia	1999-2016	M	Gov	http://www.ninamedia.si/arhiv.php
Spain	1986-2018	Q	Pres/PM	http://www.analisis.cis.es/cisdb.jsp
Sweden	2001-2017	H	Gov	Eurobarometer
Turkey	2004-2017	H	Gov	Eurobarometer
UK	1977-2016	M	Pres/PM	Ipsos Mori
Uruguay	1988-2018	M	Pres/PM	Equipos Consultores
US	1953-2017	D	Pres/PM	https://www.presidency.ucsb.edu/statistics/data/presidential-job-approval
Venezuela	1989-2015	Q	Pres/PM	Consultores 21

Table A 2 Government Popularity Dataset – Summary statistics

Variable	Obs	Mean	Std. Dev.	Min	Max
Full sample	1251	42.67334	16.30873	5	86
EME	609	42.64646	18.41211	5	86
AE	642	42.69883	14.03985	6.746853	76.64783

There are several issues with collecting data on government popularity. The first regards the fact that data are often only nationally available, so researchers must collect it country by country, with potential language barriers and with potential issues of data comparability when merging different countries. In this case, cross-country data comparability issues are relatively minor. One issue may be the substance of the poll itself: indeed, some of the countries in the sample are about trust in government, trust in the executive leader, approval of the work of the government, or of the executive leader. Trust may be different than approval rating – one could indeed believe that trust in government would be more stable than approval ratings. Similarly, satisfaction of the government may not in theory necessarily correlate with satisfaction of the prime minister or president. In practice however, the question on trust/approval ratings are mostly standardized and substitutes across the various surveys and polls. González and Smith (2017) highlighted the reliability of compiling the datasets by showing a correlation of above 0.8 across the various surveys they merge with regards to their question on trust/satisfaction in government for the same country. I do additional tests by analyzing correlations between different series for the same country. For the same country, there appears to be high correlation between trust in government, trust in president, and presidential approval, and presidential competence data series.¹⁹ Another related concern is potential discrepancies across countries in the variation/min-max of the series. My empirical specification uses changes in popularity, not its level, and uses country fixed effects, which would take care of cross-country differences in measurement of popularity.

The second issue is that frequent government approval data is usually collected by polling companies, usually hired by media companies – the polls should not be one-off but the same question has to be asked to people regularly over time. The data is private and the time series are not often shared publicly (help from several people in polling institutes is gratefully acknowledged in the appendix).

The third is the reliability of polling data in certain countries. In autocracies, it may be argued that popularity does not matter: I believe instead that this is on the contrary one more benefit of using popularity data instead of election data – as Guriev and Treisman (2016) argues, high ratings may be even more important for authoritarian rules than for democrats: the stakes are higher as institutions themselves are in play in authoritarian regimes, “public acclaim substitutes for procedural legitimacy or sanitize undemocratic acts”. The second concern is that popularity series in autocracies are meaningless, either

¹⁹ France Eurobarometer series on trust in government has a 0.86 correlation with national TNS Sofres approval ratings series. Uruguay Latinobarometro series has a 0.96 correlation with national Equipos Consultores series. Slovenia series have a 0.87 correlation; Ecuador 0.72, Spain 0.92 ... The composite index of 5 aspects of confidence in the Argentinian government from the Universidad di Tella – the most different measure of my dataset from traditional approval ratings series has a 0.68 correlation with the Latinobarometro satisfaction series. The Executive Approval Database collects multiple series per country, which also surprisingly appear broadly synchronized.

because the polling institute is not independent from political influences. None of the countries are classified as full autocracies (according to the Polity IV definition), where the reliance of polls would undoubtedly be questionable. As for countries which are “anocracies”, I rely on regional efforts like the Latinobarometer, Eurobarometer or for Gallup World Poll which should provide unbiased polls, or on sources for which I researched the credibility of the polling institute and ensure through the summary statistics that there is sufficient variability in the data. Still, people in anocracies may self-censor even to an independent institute, I thus control for the level of democracy in the empirical specification and test as robustness checks if a single country is driving the results.

Looking at the dataset as a whole, two interesting stylized facts emerge from the data. First, government popularity seems to have gone through a small but structural decline in the past decades. This may relate to the current debate on the rise of populism and the large research on the disenchantment with democracy that seems to peak today (Foster & Frieden, 2017; van der Meer, 2017). Second and most importantly, popularity appears cyclical, which may not be intuitively surprising but important to confirm with panel data. As new government, new party, or new personality takes over power, it starts off with high popularity – the so-called “honeymoon”²⁰, before the population gets disappointed vis a vis the action of the government and popularity continues to decline until the next election where new hopes arise from the campaign and new faces.²¹ This is striking in Figure A1 which averages plots the average of all countries before and after election years. Popularity steadily decline pre-election, jumps in election year as a new president arrives, holds steady up or goes up to the end of the first year before dropping. Both stylized facts are evident from the long series of the United States (Figure A3): popularity is structurally declining and moves in cycles peaking in election years and declining thereafter. Finally, I plot government popularity data and financial crises. Popularity drops as soon as the financial crisis start, and further in the first year of the crisis, before somewhat recovering (Figure A2). This is consistent with findings on electoral consequences of financial crises (Chwieroth & Walter, 2019).

Figure A1 – Election and government popularity

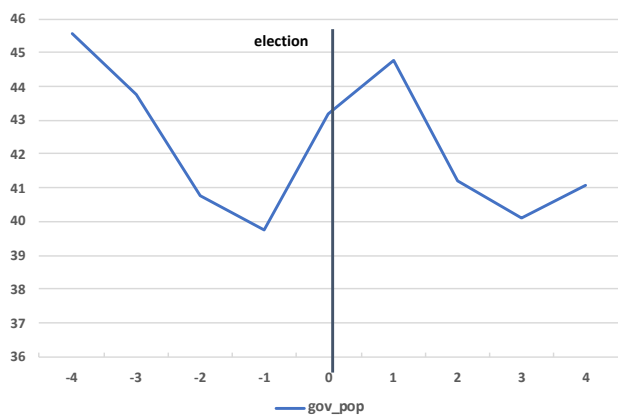
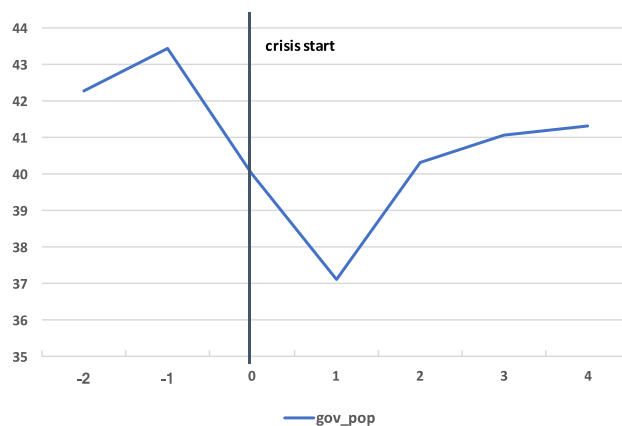


Figure A2 – Crises and government popularity



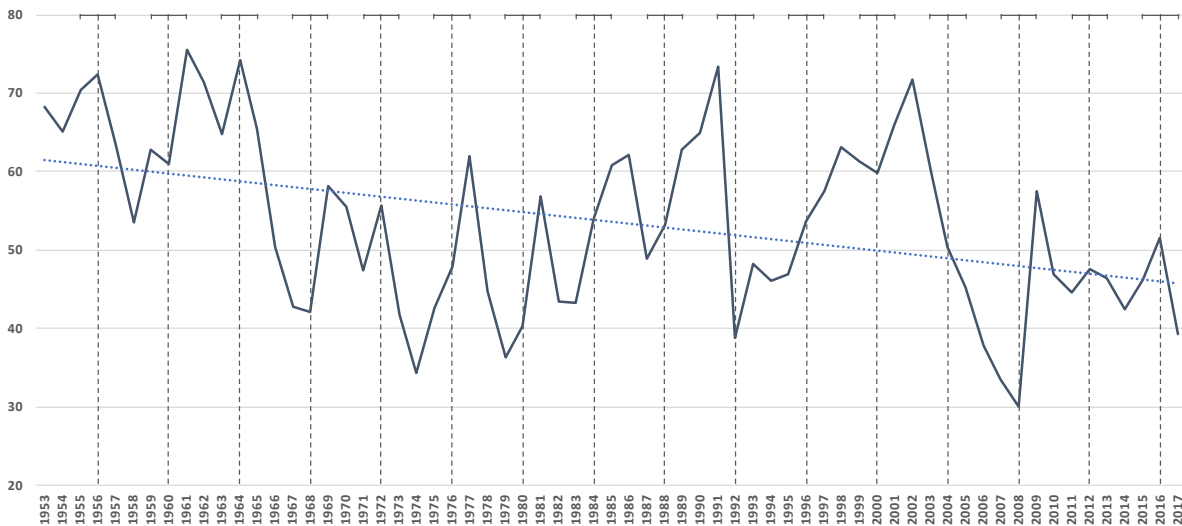
Note: average of sample [year-4; year+4], sample of democracies only (polity2 democracy score>6). In election year, the popularity of the previous and next leader is averaged.

Source: Crisis data from Laeven and Valencia (2018)

²⁰This phenomenon has been outlined by the academic literature, both theoretical and empirical, starting from the seminal work of Mueller (1985). Stimson (1976) explains it by regular expectation/disillusionment cycles among the less well-informed segments of the public, tied to the four-year election calendar in the case of the United States.

²¹ From the point of view of the empirical model of this paper, I note that potential structural global trends would be captured by time fixed effects.

Figure A3 – United States presidential job approval & elections

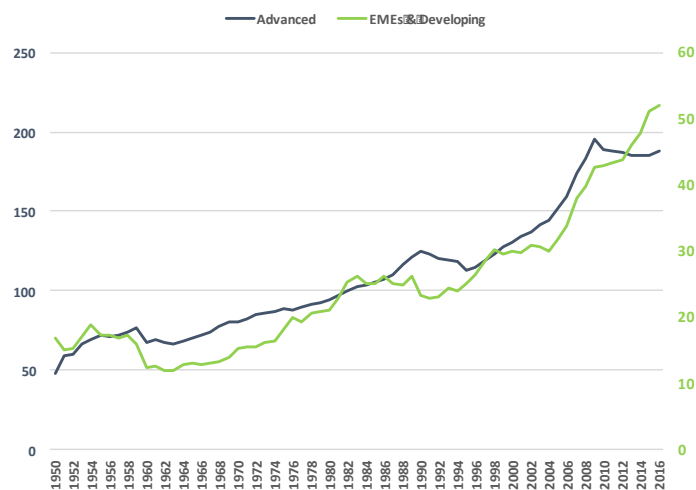


Source: the American Presidency Project, Author's calculations

Credit data

This section provides a short discussion of key stylized facts from my credit dataset. Figure A4 shows how exceptional the expansion of credit has been in both advanced and emerging economies over the few recent decades, called the “financial hockey stick” (Jordà et al., 2016), as well as a notable retrenchment in advanced economies since the crisis.

Figure A4: Total private non-financial debt to GDP

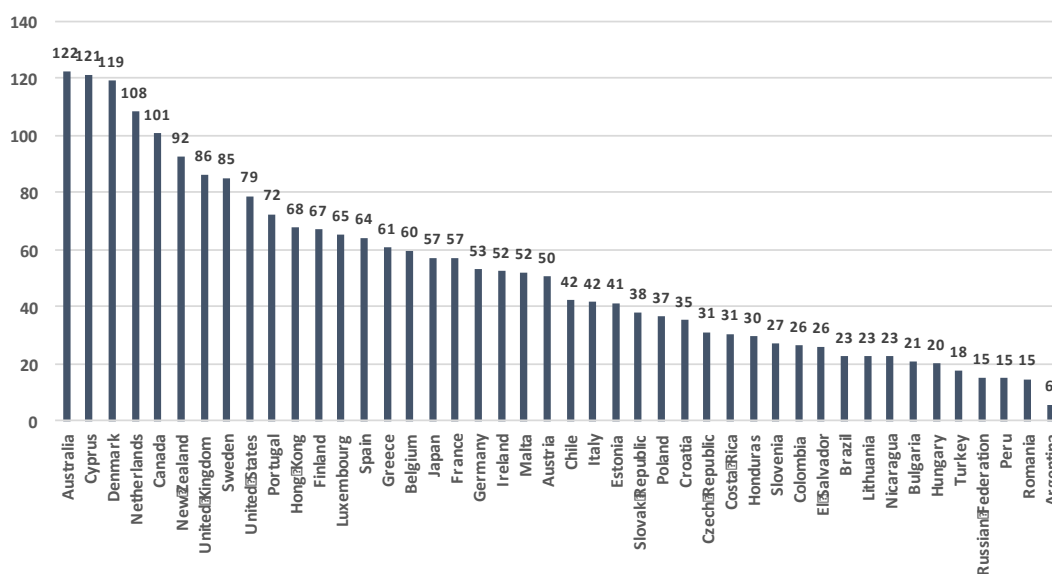


Note: unbalanced sample, 145 countries in 2016
 Source: Global Debt Database

This aggregated picture hides important differences in the financial markets of countries, with substantial heterogeneity in the credit to GDP ratios of countries, most striking among countries with similar level of development. Some countries simply do not rely as much on credit markets as others (Figure A5).

Such heterogeneity cannot be fully explained by the traditional dichotomy between market v. bank-based systems (ESRB, 2014; Fuller, 2015), as households are not able to access market financing; nor by the traditional varieties of capitalism (Hall & Soskice, 2001). There is notably some evidence that the share of household credit relative to corporate credit is higher in more urban societies, in countries with smaller manufacturing sectors and more market-based financial systems (Beck et al., 2012). It also importantly depends on differences in the structure of housing markets. Indeed, mortgages have generally been estimated to account for the golden share of household debt (Causa et al., 2019), while it may be less in developing countries (recently collected data by Müller (2018) shows that this share is actually significantly lower in developing countries amounting today around 40% only –15% being credit card debt and 10% car loans, a much higher value than in advanced economies). For instance, the rental market is less developed in Sweden which has a very high HH debt to GDP ratio, while in Germany mortgage credit is not as prevalent with an active rental market. Countries in the former Soviet Union have historically high level of homeownership without the need to rely on the weakly developed mortgage markets, reflected in much lower HH debt to GDP ratios. Implications from these stylized facts for the sake of my argument are that credit is very much a product of policy choices and structural historical patterns and not simply a question of level of development and hence political credit cycles may be a reality of certain countries and not others.

Figure A5: Household debt to GDP (2016)



Source: Global Debt Database

Table A 3 Data sources

Variables		
Indicators	Description	Data source
Credit to GDP gap	Total credit to GDP Credit to households to GDP Credit to non financial corporates to GDP Detrended with HP filter, $\lambda=100$	Mbaye, Moreno Badia and Chae (2018)
Credit to GDP gap 2	Domestic private credit Total credit to GDP Bank credit to GDP Detrended with HP filter, $\lambda=100$	World Bank Global Development Database
Partisanship	Right (1); Left (3); Center (2); No information (0); No executive (NA) Party orientation with respect to economic policy	Database of Political Institutions
Partisanship 2	Parliamentary seat share of (left/right/center) in government. Weighted by the number of days in office in a given year. <i>Or:</i> Cabinet composition (Schmidt-Index): (1) hegemony of right-wing (and centre) parties ($gov_left1=0$), (2) dominance of right-wing (and centre) parties ($0 < gov_left1 \leq 33.33$), (3) balance of power between left and right ($33.33 < gov_left1 < 66.67$), (4) dominance of social-democratic and other left parties ($66.67 \leq gov_left1 < 100$), (5) hegemony of social-democratic and other left parties ($gov_left1=100$).	Comparative Political Dataset
ICRG Government Stability	Index	International Country Risk Group
Democracy	Democracy score from -10 to +10	Polity IV project - Marshall, Jaggers and Gurr (2011)
Real GDP growth		IMF WEO
Real GDP per capita	Logged	World bank WDI
VIX	Logged	FRED
Total Capital inflows		IMF BoP
Central Bank Independence		Bodea & Hicks (2015)
Capital account openness	Index of capital account openness, normalized from 0 to 1	Chinn & Ito (2016)
Election	Dummy=1 in election year Executive election Any election (legislative + executive)	Hyde & Marinov(2012), extended
Inequality	Gini coefficient Gini_market: pre-tax inequality Gini_net: post-tax inequality	Standardized World Income Inequality Database, Solt (2019)
Crisis	Systemic Banking Crisis dummy	Laeven and Valencia (2018)
Growth in fiscal spending	General government final consumption expenditure (% of GDP) – growth year on year	World Bank World Development Indicators
Government Debt	Government debt to GDP	Mbaye, Moreno Badia and Chae (2018)
Interest rates	Money market rates (%)	IMF IFS

Financial reforms	Index of stock of financial reforms, with higher values noting more reforms	Abiad et al (2010) extended by Gomes et al (2017)
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Table A 4 - Baseline selection & Additional controls

Dep. Var: Total credit to GDP gap	1 L1	2 L2	3 L3	4 2lag	5 3lag	6 IR	7 inflows	8 GDPgrowth	9 GDPpc	10 Kopenness	11 CBI	12 crisis	13 demo	14 reforms
Δ Gov. popularity (t-1)	-0.084*** 0.02			-0.095*** 0.02	-0.112*** 0.02	-0.113*** 0.03	-0.107*** 0.03	-0.088*** 0.02	-0.102*** 0.03	-0.082*** 0.02	-0.061*** 0.01	-0.077*** 0.02	-0.072*** 0.02	-0.080*** 0.03
Δ Gov. popularity (t-2)		-0.074*** 0.02		-0.082*** 0.02	-0.082*** 0.02	-0.072*** 0.03	-0.075** 0.03	-0.048* 0.03	-0.058** 0.02	-0.046 0.03	-0.032 0.03	-0.039 0.03	-0.045* 0.03	-0.032 0.03
Δ Gov. popularity (t-3)			-0.005 0.02		-0.029 0.02	-0.024 0.03	-0.013 0.03	-0.012 0.03	0.005 0.04	-0.010 0.03	-0.015 0.02	0.008 0.04	-0.026 0.02	-0.037 0.04
Interest rates (t-1)						-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00
Total capital inflows (t-1)							0.06 0.03							
Real GDP growth (t-1)								-0.676*** 0.21		-0.666*** 0.21	-0.444** 0.19	-0.378* 0.20	-0.604*** 0.21	-0.895*** 0.21
real GDP per cap. (log)									1.775 3.80					
Capital account openness										-2.758 8.91				
Central bank indep.											-1.714 4.47			
Crisis dummy												10.294** 4.79		
Democracy score													-1.090*** 0.29	-2.196 1.52
Financial reforms (t-1)														9.192* 4.60
Constant	-0.888** 0.36	-0.568 1.13	1.033 1.27	-1.084 0.89	0.514 1.14	0.183 1.29	0.327 1.54	2.639* 1.34	-16.769 37.18	4.284 6.08	3.012 2.17	1.816 1.41	11.298*** 3.16	17.869 13.64
Country & Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1,067	1,022	978	1,009	953	731	656	731	731	721	569	731	683	486
R-squared	0.127	0.123	0.118	0.131	0.133	0.132	0.123	0.148	0.011	0.142	0.207	0.187	0.258	0.349
Number of co	56	56	56	56	56	45	45	45	45	44	43	45	43	29

APPENDIX B. Robustness checks to the baseline

While I have demonstrated that this paper's results are robust to different lag specifications, time and country fixed effects, and a various set of additional controls, I run a further battery of robustness checks: i) I use different credit series from the World Bank Global Financial Development Database, and a different proxy for government popularity; ii) I check for non-linearity in the relationship depending on the initial level of growth in credit to GDP; iii) I use a GMM dynamic model that accounts for the potential persistence of the credit series and partly accounts for potential endogeneity issues; iv) I test the potential symmetry or asymmetry in the relationship between credit and government popularity; v) I drop countries one by one to test whether the effect is not driven by a potential outlier: the negative and significant coefficient of government popularity holds for all regressions; vi) I finally discuss the potential for reverse causality and further test the potential for confounding factors using the interactive fixed effects method developed by Bai (2009).

Alternative dependent variable and popularity proxy

Replacing my government popularity data by the ICRG index of government stability, capturing among other elements popularity of government and used in Herrera, Ordoñez and Trebesch (2019), I also find a negative relationship between credit and change in the index but not statistically significant (Table B1, Column 1-3). This may either be due to the fact that the ICRG index is not a good proxy for popularity as it captures more than popularity and consisting in expert judgment; it may also be due to the fact that the country sample is much wider for the ICRG, adding developed countries for which credit markets are non-existent²².

I then replace the baseline credit series by the World Bank Development Database series of domestic private credit to GDP (Table B1, Column 4-8), I find similar significant results with regards to the credit to GDP gap, while the simple change in the ratio is also negative but not significant. Further using the World Bank database to split between credit extended by banks vs. total credit (banks and non-banks), I find similar results, albeit with lower coefficient, highlighting that political credit cycles may potentially leverage on both bank and non-bank financial institutions.

²² Indeed, restricting the country sample to the government popularity baseline country sample, the lag 2 of the ICRG variable is significant at the 5% level and the first lag at the 15% level with regards to the credit to GDP gap. It is still not significant for simple credit growth.

Table B1 - Alternative credit series, alternative proxy for government popularity

Dependent Variable:	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Total Gap	Total Gap	Credit growth	WB Cgrowth	WB Cgrowth	WB Bank Cgrowth	WB Gap	WB Bank Gap
Δ Gov. popularity (t-1)				-0.041 0.05	-0.043 0.04	-0.022 0.02	-0.074** 0.03	-0.037** 0.02
Δ Gov. popularity (t-2)				0.034 0.05				
Δ Gov. popularity (t-3)				0.097 0.07				
Δ ICRG gov. stab. score (t-1)	-0.212 0.17	-0.179 0.15	-0.042 0.17					
Δ ICRG gov. stab. score (t-2)	-0.234 0.19							
Δ ICRG gov. stab. score (t-3)	0.057 0.16							
Interest rates (t-1)	-0.000*** 0.00	-0.000*** 0.00	-0.000*** 0.00	-0.000 0.00	0.000* 0.00	0.000* 0.00	-0.000** 0.00	-0.000** 0.00
Real GDP growth (t-1)	-0.224*** 0.06	-0.237*** 0.06	0.305** 0.13	0.474* 0.26	0.357 0.33	0.178 0.16	-0.614** 0.30	-0.307** 0.15
Crisis dummy	6.447*** 1.25	6.426*** 1.24	-2.024* 1.15	-3.758** 1.81	-8.919** 3.52	-4.459** 1.76	9.028*** 2.86	4.514*** 1.43
Democracy score	-0.070 0.14	-0.066 0.12	-0.065 0.09	-0.892 0.54	-0.997** 0.49	-0.499** 0.24	-1.856*** 0.40	-0.928*** 0.20
Constant	1.690 1.70	1.032 1.67	2.388 1.64	13.364*** 4.74	10.097** 4.51	5.048** 2.25	11.383** 4.76	5.692** 2.38
Country and Year FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1.598	1.672	1.66	630	694	694	704	704
R-squared	0.161	0.157	0.085	0.185	0.181	0.181	0.236	0.236
Number of co	74	75	75	45	45	45	45	45

Reverse causality

Reverse causality issues may originate from the fact that government popularity and credit may be associated both ways: government popularity may be associated with subsequent higher credit; but credit itself will influence popularity. I note first that the empirical model partly addresses this issue by using different lags of the regressors. Second, the previous result may intuitively reduce endogeneity concerns: I find that a fall in popularity is associated with increased credit growth subsequently. A reverse causality concern would highlight that a fall in credit would cause higher growth in popularity which does not make sense. However, there may still be issues of simultaneous effect at play. Third, I find that the results are robust to GMM estimation, which is also designed to partly address endogeneity issues.

The use of GMM may appear intuitive not only for endogeneity reasons but also as it is likely that the dependent variable is highly correlated by its lagged value. In these circumstances, it is necessary to include the lagged value of the dependent variable as regressor to avoid omitted variable bias. However, a dynamic model with fixed effects may suffer from Nickell bias (Nickell 1981) with inconsistent within-estimators as the demeaned lagged dependant variable will be correlated with the error term in the case of large N and small T. I follow the literature in using the Arellano-Bond GMM estimator to correct for the Nickell bias (GMM methods have been used in the literature on the determinants of credit growth in Cerutti, Claessens and Laeven (2017; 2017) while Kuttner and Shim (2016) and Lepers and Mehigan (2019) note that with quarterly data T is large enough for the Nickell bias to be benign). I use the one-

step system GMM with robust standard errors. Besides the lagged dependent variable, I treat the lagged government popularity, the lagged interest rate, the lagged GDP growth, and the crisis dummy as endogenous regressor, with democracy and the year dummies treated as exogenous. I use a limited set of instruments in the estimation, namely, one to three lags. The instrument lag choice yields AR(2) p-values above the 5% threshold. I do not use higher lags to avoid instrument proliferation. The models are valid and the results remain consistent in sign, significance and range to what I find in earlier results, for both total credit, and the household/NFC split (Table B2). Hence, I believe that reserve causality issues should not affect my results.

Table B2 - Baseline regressions with GMM estimation

Dependent variable:	Total Gap 1	Total Gap 2	Total Gap 3	Total Gap 4	Total Gap 5	HH Gap 6	NFC Gap 7	Credit growth 8
Total credit to GDP gap (t-1)		0.324**	0.374***	0.448***	0.379***			
		0.12	0.12	0.10	0.11			
Credit to HHs to GDP gap (t-1)						0.799***		
						0.02		
Credit to NFCs to GDP gap (t-1)							0.364***	
							0.10	
Δ Credit to GDP (t-1)								0.079
								0.12
Δ Gov. popularity (t-1)	-0.068***	-0.076***	-0.080***	-0.072***	-0.063***	-0.013*	-0.035	-0.094**
	0.02	0.02	0.02	0.02	0.02	0.01	0.02	0.04
Δ Gov. popularity (t-2)	-0.041	-0.029	-0.034					
	0.03	0.02	0.03					
Δ Gov. popularity (t-3)	-0.017	-0.020	-0.025					
	0.02	0.02	0.02					
Interest rates (t-1)	-0.000***	-0.000***	-0.000***	0.000	-0.000***	-0.001***	-0.003***	-0.000
	0.00	0.00	0.00	0.00	0.00	0.00	0.00	0.00
Real GDP growth (t-1)	-0.439**	-0.216	-0.180	-0.132	-0.213*	0.003	-0.116	0.338*
	0.20	0.21	0.17	0.14	0.12	0.05	0.13	0.18
Crisis dummy	5.703***	2.699*	2.656*	2.114	4.284***	0.301	2.508***	-0.338
	1.38	1.51	1.46	1.42	1.57	0.21	0.90	1.19
Democracy score	-1.155***	-0.925***	-0.233	-0.381**	-0.956***	-0.067*	-0.254**	0.438
	0.28	0.27	0.15	0.16	0.32	0.04	0.10	0.29
Constant	11.345***	8.813***	2.751	13.720		1.085	4.199	-3.784
	3.03	2.76	1.72	23.01		1.02	3.08	3.35
Observations	683	682	682	758	710	639	638	755
R-squared	0.293	0.379						
Number of co	43	43	43	43	43	37	37	43
AR(1)			0.008	0.025	0.019	0.006	0.064	0.013
AR(2)			0.383	0.342	0.231	0.051	0.457	0.112

Confounding factors

As for potential confounding factors, the time and country fixed effects should control for a wide range of variables that could be correlated with government popularity and I try many additional time-varying country-specific controls. One evident way of ensuring the absence of OMV would imply potential instrument variable technics but in practice I did not find adequate instrument for change in popularity.²³

²³ The IV approach would presuppose finding an instrument which is highly correlated with changes in government popularity while not being correlated with the error term of the baseline empirical model, e.g regressions on credit. While some studies could provide a basis to select an appropriate candidate for the instrument (e.g. Guriev & Treisman (2016) or Murtin et al. (2018)), which have highlighted the importance of economic situation, media freedom, perceptions of immigration and corruption as determinants of government popularity, in practice such instruments are difficult to apply to the sample mainly for data availability reasons (both time and country-wise). The fact that I study changes in government popularity rather than

Thus, I rely on panel interactive fixed effects technics developed by Bai (2009), which have become more and more used as a way to check for confounding factors (Kejriwal et al., 2019). Interactive fixed effects are well suited for large N, large T panel dataset with unobservable multiple interactive effects which may be correlated with drop in popularity.²⁴ Results are displayed in Table B3. Coefficients on government popularity change remains negative and significant under this specification, and robust to using one or two common factors (column 2) and still having time and country fixed effects in the model. This is the case for the total credit gap, household credit gap (5) or simple change in the credit to GDP ratio (3-4). My results are thus robust to unobservable trends specific to each country, i.e. country-specific/heterogenous effects of a mix of global trends. This significantly reduced the potential for confounding factors and thus increase the confidence in the mechanism tested here.

Table B3 - Baseline regressions with interactive fixed effects

Dependent variable:	Total gap	Total gap	Credit growth	Credit growth	HH credit gap
Δ Gov. popularity (3Ysum)	-0.111** 0.05	-0.091** 0.04			-0.060** 0.03
Δ Gov. popularity (t-1)			-0.038** 0.02	-0.039* 0.02	
Interest rates (t-1)	-0.000*** 0.00	-0.000 0.00	-0.000*** 0.00	0.000 0.00	0.000* 0.00
Real GDP growth (t-1)	-0.462*** 0.15	-0.271** 0.11	0.014 0.24	0.257** 0.10	-0.008 0.05
Crisis dummy	4.316*** 1.17	2.950** 1.14	-1.935 2.12	-0.400 1.45	0.885 0.60
Democracy score	-1.200** 0.48	-1.014** 0.47	-0.447 0.47	-0.334* 0.17	-1.308** 0.49
Constant	12.141*** 4.43	10.136** 4.20	6.544 4.47	4.491*** 1.46	12.261** 4.59
Year & Country FE	Yes	Yes	Yes	Yes	Yes
# of factors	1	2	1	2	1
Observations	683	683	758	758	581

Increasing government popularity leading to future decline in credit cycles?

While the results appear very robust to multiple checks, a question may be asked about the symmetry of the relationship between government popularity and credit booms. While I do not see any intuitive or theoretical reason to back the idea that growing popularity would have a systematic negative impact on future credit growth, my baseline results allow for such possibility.

I adjust the baseline model to allow for non-linearity, i.e. to allow the effect of popularity on credit to be different whether the change in popularity is negative or positive.

*Credit to GDP gap*_{it}

$$= \beta_1 d. pop_{it-1} * \{d. pop_{it-1} > 0\} + \beta_2 d. pop_{it-1} * \{d. pop_{it-1} < 0\} + \Gamma X'_{it-1} + u_i + \mu_t + e_{it}$$

level makes the finding of instrument even more difficult as it requires sufficient movement in the instrument series. I find that change in corruption level or terrorist attacks do not explain well change in popularity.

²⁴ I use the regife stata package. The algorithm converges when increasing the maximum number of iterations to 300 000.

I interact the lagged change in popularity with a dummy taking the value of 1 if this change is positive and 0 otherwise, and add the same mirrored interaction term to the equation if the change is negative. The positive dummy takes two forms: one is created on the full sample; the second one is created on a sample stripped out of election year data. Indeed, the boom in popularity in election years are outlier observations for the sake of this test: they are discontinuities in the series insofar as they reflect a change of government and the high expectations associated with it. I keep time and country fixed effects.

Table B4 – Testing for negative and positive change in government popularity

Dep Variable:	1 Total Gap	2 Total Gap	3 Total Gap	4 Total Gap
Δ Gov. popularity * Dum_neg (t-1)	-0.179*** 0.06	-0.162** 0.08	-0.176** 0.07	-0.071+ 0.04
Δ Gov. popularity * Dum_pos_noelection (t-1)			-0.009 0.05	-0.040 0.04
Δ Gov. popularity * Dum_pos (t-1)		-0.032 0.03		
Interest rates (t-1)				-0.000*** 0.00
Real GDP growth (t-1)				-0.382** 0.18
Crisis dummy				6.878*** 1.55
Democracy score				-1.195*** 0.23
Constant	-1.743** 0.66	-1.529* 0.79	-1.705** 0.77	9.850*** 2.27
Country & Year FE	Y	Y	Y	Y
Observations	1,067	1,067	1,067	760
R-squared	0.129	0.129	0.129	0.270
Number of co	56	56	56	43

As Table B4 shows, coefficients are never significant on the interaction with the popularity increase dummy, while the coefficients on the interaction with the popularity drop dummy is significant all throughout, with larger coefficients. This confirms that the results are driven by declining popularity leading to amplified credit cycles and not the other way around.

Different effect in countries which are heavily credit-based and others?

As outlined in the section describing the credit data, the potential for political credit cycles may be expected to be different across countries depending on the importance that credit has for the type of financial system. This is especially relevant for household debt: in some countries, households do not rely much on credit. As I have highlighted in a previous section that credit to household appears to be an important part of the political credit cycle story, I split the sample by quartile of the credit to household to GDP distribution and run the baseline regression separately, i.e. on each quartile (with thresholds at 10, 29 and 56% of GDP).

The change in government popularity still remains negative and significant for the three quartiles of higher level of credit to GDP in the distribution, while the lowest quartile appears insignificant (Table B5, Column 1-4): political credit cycles for households do not seem relevant when credit to GDP has not reached a certain level. This is not surprising and further backs the idea that political credit cycles may be

a phenomenon of relatively advanced, financially developed economies, as found earlier. I get similar result by splitting the distribution in thirds (Table B5, Column 5-7).

Table B5 – The importance of the credit to GDP distribution

Dependent variable:	HH Gap 4th quart.	HH Gap 3rd quart.	HH Gap 2nd quart.	HH Gap 1st quart.	HH Gap 3rd third	HH Gap 2nd third	HH Gap 1st third
Δ Gov. popularity (t-1)	-0.043* 0.02	-0.031*** 0.01	-0.036* 0.02	0.001 0.01	-0.041** 0.02	-0.046** 0.02	0.001 0.01
Interest rates (t-1)	0.497 0.3	0.147 0.1	-0.005*** 0	0.005 0.01	-0.115 0.33	0.001*** 0	-0.000** 0
Real GDP growth (t-1)	-0.620* 0.33	0.097* 0.06	0.037 0.08	-0.117** 0.05	-0.527** 0.24	0.003 0.1	-0.106*** 0.02
Crisis dummy	2.162*** 0.72	-0.494 0.58	-0.065 0.35	-0.694 0.39	1.699*** 0.55	-0.225 1.01	-0.704 0.41
Democracy score	-1.784** 0.71	-2.181*** 0.46	-1.507*** 0.47	0.417 0.36	-2.008** 0.9	-1.793*** 0.41	0.486** 0.22
Constant	18.898** 8.22	15.437*** 3.89	9.585** 4.08	-3.28 2.55	22.644 14.72	14.759*** 3.34	-4.706** 1.66
Year & Country FE	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	264	165	148	70	333	186	128
R-squared	0.487	0.604	0.604	0.55	0.463	0.611	0.514
Number of co	19	22	17	8	20	22	14