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The political economy of fiscal procyclicality☆

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ABSTRACT

It is well-recognized that fiscal spending in developing countries tends to display significant procyclicality (increased spending during expansions and *vice versa*), in contravention of rational stabilization policy. Theoretical explanations have relied on either financial access or political-economic factors to justify this phenomenon. In this paper, we model the fiscal-output relationship as a dcc-garch process, and inquire whether debt or political economy constraints play a comparatively more important role in conditioning this correlation. Our evidence favors a positive effect from political economy, with weaker and more mixed results pertaining to financial access. Somewhat surprisingly, we also find that politics-induced procyclicality appears to be driven by advanced economies, and fiscal rules exacerbate procyclical tendencies.

1. Introduction

In December 2017, the United States Congress passed the Tax Cuts and Jobs Act. The law led to a swelling of the budget deficit by $780 billion in 2018 (and added another $2.3 trillion to the national debt), and famously occurred amid already solid late-cycle growth conditions. This example is not unique to the latest U.S. administration. In the midst of the first Obama term, a failure to extend the Great Recession-related stimulus package led to spending cutbacks even in a weak economy. And more recently in Europe, German policymakers—citing its infamous *schwarze Null* balanced-budget rule—remained reticent to calls for fiscal support in 2019, even as the economy was on the skids.

Fiscal procyclicality—the tendency to enact expansionary fiscal policy during a boom and *vice versa*—is by no means confined to advanced economies. Smaller economies as diverse as the Congo, Paraguay, the Philippines, and Saudi Arabia have, at numerous instances in their recent history, chosen to expand government expenditures even while enjoying already-strong economic performance (and contracted in the face of recession). Yet economic theory militates in favor of countercyclical—or at least acyclical—fiscal policy, which renders the relatively widespread nature of procyclicality somewhat of a puzzle.

The pervasiveness and persistence of fiscal procyclicality has led to the emergence of two major schools of thought on why we observe procyclicality in practice. The first stakes the argument on constraints to financial access faced by governments, due

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to reasons such as credit frictions (Aizenman et al., 2000), incomplete markets (Cuadra et al., 2010), or commitment difficulties (Bauducco and Caprioli, 2014). The second places the onus on political economy complications, such as electoral rules (Persson and Tabellini, 2004), political polarization (Ilzetzki, 2011), or corruption (Alesina et al., 2008). Yet despite the distinctiveness of these two competing explanations, there has been little effort at systematically comparing the relative contributions of each to the overall phenomenon of procyclicality.

In this paper, we take on the question of evaluating competing explanations for fiscal cyclicality.[1] Our approach first constructs measures of the time-varying relationship between fiscal spending and economic activity that is robust to a non-normal data generating process. In particular, we model the variations in the fiscal-output relationship as a multivariate generalized autoregressive conditional heteroskedasticity (garch) process exhibiting dynamic conditional correlation (dcc). We then jointly evaluate the relative importance of financial access and political economy explanations for procyclical behavior, at both the cross-sectional as well as panel levels.

Overall, our evidence favors a positive effect due to political economy—as proxied by a measure of political participation—for procyclical fiscal policy, consistent with theory. This effect is distinct from the more common phenomenon of *political* business cycles (which attributes economic fluctuations to political drivers), since business cycles *per se* tend to occur more infrequently than events on the political calendar. The effects of financial access—as proxied by the outstanding debt burden—are weaker and more mixed. We also find that the debt-related positive association for government consumption becomes negative with respect to government expenditure, suggesting that the public investment component of spending, if financially-unconstrained, could actually be countercyclical in nature, or that political influence operates via the transfer payments channel. Interestingly, secondary analyses also reveal that much of the politics-induced procyclicality in our full sample is driven by advanced, rather than developing, economies.

Existing empirical approaches often rely on estimates of cyclicality based either on discrete partitions of the data (e.g. correlations over a given time period), or on temporally-static coefficient estimates obtained from regressions of policy on a measure of cyclically-adjusted output. The problem with the first approach is that such partitions are arbitrary (and hence may be either artificially oversensitive or insensitive to temporal changes in correlations), while the concern with the second is that estimates may suffer from endogeneity (from reverse causality as income itself reacts to fiscal policy, omitted variables due to common unobserved confounders, or measurement error bias since the output gap is unobserved) along with imprecision (because heteroskedasticity resulting from the volatility of the business cycle can bias correlation estimates[2]). Taken together, these measurement issues cast doubt on whether the estimated procyclical relationship is genuine. In comparison, our dcc approach explicitly accounts for time-varying idiosyncratic changes in the evolution of the fiscal-output relationship, embeds all available historical information, while simultaneously allowing for heteroskedastic and leptokurtic features in the underlying data generating process.

Moreover, our reliance on a multivariate garch model means that we are able to directly accommodate the possibility that financial or political drivers alter our measure of procyclicality, without the need to secure plausible instrumental variables for either government spending or output. Although garch methods nevertheless inherit the identification problem associated with endogeneity, our approach allows us to adopt an agnostic stance on whether we have satisfied exclusion restrictions for potential candidate instruments, and instead focus on how the conditional correlation changes when these factors are introduced.

The question of whether fiscal policy tends to be procyclical has been directly taken on in a number of empirical papers. The majority of these papers find fairly strong evidence of procyclical behavior in developing economies (Alesina et al., 2008; Calderón et al., 2016; Frankel et al., 2013; Gavin and Perotti, 1997). In contrast, while some authors have argued that advanced economies tend to subscribe to countercyclical policies (Galí et al., 2003; Lane, 2003), others find little evidence of cyclicality in either direction (Bashar et al., 2017; Talvi and Végh, 2005). Moreover, even among OECD economies, certain conditions—notably political concentration—may increase the likelihood of running procyclical fiscal policies (Lane, 2003). Many of these analyses have been limited to a set of similar economies, for a comparatively brief time period. One of the advances we make in this paper is to expand our working sample to as many as 44 advanced and developing economies, with time spans for some nations extending to as long as 1800–2015. The extended temporal coverage is especially important, since it allows us to encapsulate many more occurrences of business cycles.

Importantly, many of these papers have sought to empirically account for how either financial imperfections or political confounders can explain procyclical fiscal choices. Most papers in the former category have found evidence that underscores the importance of financial access. Gavin and Perotti (1997), for example, demonstrate that reliance on emergency finance appears to spike during bad times, supporting the notion that borrowing constraints become more binding during contractions. Kaminsky et al. (2005) document that both fiscal policy as well as capital inflows are procyclical in emerging markets—consistent with the idea that reduced financing may be a relevant channel—while Aizenman et al. (2019) find that governments in more indebted countries (and thereby less able to obtain additional credit) tend to spend more in good times, and *vice versa*.

Studies in the latter group have likewise ascertained how political economy matters. Lane (2003) and Alesina et al. (2008), for instance, find that when political competition is greater, fiscal policy often ends up exacerbating the business cycle (competition in these respective papers are captured by measures of political constraints and political participation, respectively), while government corruption can further exacerbate the degree of procyclicality. Abbott et al. (2015) also find that the coincidence of political party

[1] Our definition of "fiscal" in this paper is, consistent with the literature, mainly focused on the variations in the spending side of the fiscal ledger, which we regard as the more relevant margin for typical applications of countercyclical stabilization policy. While revenue instruments—such as the tax rate—occasionally feature as discretionary fiscal policy, tax policy changes are rarer in practice, and consequently tend to be less amenable to analysis at business-cycle frequencies. Nevertheless, we also consider three fiscal balance measures in our robustness checks, which implicitly capture variations on the revenue side.

[2] Although *coefficient* estimates do not suffer from such bias, *correlations* require the use of second moments, which *are* biased in the presence of heteroskedasticity.

control at federal and state levels increases the likelihood that state legislatures accommodate rent seeking (Abbott et al., 2015). More generally, Calderón et al. (2016) show that higher levels of institutional quality better equip policymakers to resist pressures to engage in procyclical fiscal expenditures.

One common shortcoming among most of these "determinants" studies is that they seldom test for the relevance of the financial access and political economy channels side-by-side. Part of the reason is the difficulty in addressing endogeneity, which requires finding valid instruments. One example is Brückner and Gradstein (2014), who utilize weather shock instruments to resolve the endogeneity issues afflicting the government spending response.[3] In contrast, our approach here sidesteps this thorny problem by, first, deriving correlations from the *conditional* variance-covariance matrix of error terms (instead of from estimated coefficients, which may be more subject to simultaneity bias); and, second, by directly embedding our variables of interest into the multivariate system, and comparing the resulting changes in conditional correlations.

Fiscal procyclicality has also been touched on, albeit tangentially, in the literature on fiscal multipliers (see, for example, Auerbach and Gorodnichenko, 2012b,2012a; Candelon and Lieb, 2013; Favero et al., 2011). In most such instances, multipliers are conditional on, *inter alia*, the state of the business cycle. Consequently, the fact that recession (expansion)-phase multipliers are larger (smaller) indirectly suggests a diminished benefit to the pursuit of procyclical policy.[4] However, the unique conditions surrounding business cycle turning points caution against such generous interpretations against procyclicality on the basis of such indirect evidence.

2. Theoretical background

There is remarkable consistency in what economic theory claims for the behavior of fiscal policy over the business cycle. Neo-classical theory has long maintained, on the basis of intertemporal smoothing, that any shocks to the tax base should be offset by adjustments to fiscal balances to maintain expected constancy in tax rates (Barro, 1979).[5] The Keynesian prescription, likewise, implies that optimal fiscal policy should seek to return a post-shock economy to equilibrium via either automatic stabilizers or, if necessary, discretionary action (Blanchard et al., 2010). Either way, fiscal deficits (surpluses) would accompany economic expansions (contractions) in a countercyclical fashion.

Yet the routine violation of these standard implications in practice has led to efforts at formulating models that give rise to procyclicality in fiscal policy. These fall into two main families: Those that rely on imperfections in credit markets that inhibit countries from borrowing during downturns (or promote overspending during expansions) to smooth the cycle; and those that introduce political economy frictions that systematically push effected policy away from the socially optimal outcome.

The basic principle that undergirds models where financial access is the culprit behind procyclicality lies in the notion that governments always face some form of binding liquidity constraint (so expenditures would otherwise be greater in the absence of this constraint). During booms, the improved ease of financial access then leads to increased public borrowing and spending; during busts, funding becomes prohibitively expensive or evaporates entirely, which compels budgetary rationalization and prevents deficit-financed stimulus.

The mechanisms that govern the endogenous access to credit can vary. This could arise due to inefficiencies in tax collection and differences in creditor bargaining power (Aizenman et al., 2000), or because of an inability to commit to a risk-sharing arrangement with the rest of the world (Bauducco and Caprioli, 2014), or because imperfect enforcement affects the sovereign default risk premium faced by economies (Cuadra et al., 2010), especially when such repayment capacities are compromised by exchange rate volatility (Bi et al., 2016). Importantly, the accumulation of government liabilities may erode the financial depth of an economy by lowering aggregate liquidity and/or the valuation of the country's assets, due to crowding out (Caballero and Krishnamurthy, 2004). If so, the extent of financial access—and, consequently, the ability to conduct countercyclical fiscal policy—diminishes as the debt stock grows.

As compelling as the financial access argument may be, especially for developing economies, many have come to question why countries do not simply either self-insure through reserve accumulation, or why lenders do not extend credit to governments if they were certain that doing so would ultimately enable counteryclical policy that would help the economy exit recession (Alesina et al., 2008). This has led to political economy justifications for procyclicality.

Models where political economy feature as an explanation for procyclicality introduce political distortions of some form in order to justify deviations from the Ramsey optimum.[6] Such distortions mean that self-serving demands for public goods or tax relief tend to be myopic: rising during good times, and falling otherwise. Governments acting to satisfy these political pressures gives rise to

[3] While the use of instrumental variables is relatively common in the fiscal multiplier literature (e.g. Acconcia et al., 2014; Barro and Redlick, 2011; Ramey, 2011), its use in the fiscal cyclicality context is comparatively less common.

[4] Relatedly, papers that condition multipliers on debt (e.g. Eggertsson and Krugman, 2012; Huidrom et al., forthcoming; Ilzetzki et al., 2013) yield lower estimates when fiscal space is limited, which implies that the outstanding debt burden may itself alter the calculus behind countercyclical fiscal policy at the margin.

[5] And even in the absence of shocks, Ricardian equivalence would suggest that any increases in government expenditure would simply be offset by concomitant declines in private demand; at the extreme, such public expenditures may shift demand from producers' goods to consumers' goods and even prolong stagnation (Hayek, 1931). In this case, fiscal policy should at best be acyclical.

[6] Under the assumption, standard in public finance, that this first-best allocation is socially desirable.

procyclicality.[7]

Political distortions emerge from two main channels. Special interest pressures—either because a common-pool problem incites competition among politically influential groups over redistributive fiscal transfers (Tornell and Lane, 1999) or public investment funds (Park et al., 2005), or because tax revenues end up being directly appropriated by corrupt governments to fund political rent distribution in contrast to the socially-optimal fiscal policy (Alesina et al., 2008)—can influence the government in power, promoting expenditure excess. Alternatively, electoral competition may also induce overspending, especially when there is a high degree of political (Ilzetzki, 2011; Talvi and Végh, 2005) or social (Woo, 2009) polarization. Even constitutional rules governing elections may play a role in influencing the size and composition of government (Kantorowicz, 2017; Persson and Tabellini, 2004).[8] Whatever the channel, political turnover can condition the extent to which fiscal policy moves in a procyclical manner.

3. Empirical methodology

3.1. Measuring procyclicality

The conventional approach to measuring procyclicality in the literature has been to either compute the (static) unconditional correlation coefficient for a given country i for the time period between t and $t + n$:

$$\rho^u_{i,t,t+n} = \frac{\operatorname{cov}(G_i, Y_i)}{\sqrt{\sigma^2_{G_i}\sigma^2_{Y_i}}}, \tag{1}$$

where G and Y are measures of the government fiscal policy stance and the state of the business cycle, respectively; or to run regressions of

$$G_{i,t} = \alpha_i + \rho^r_{i,t,t+n} Y_{i,t} + \mathbf{X}'_{i,t}\boldsymbol{\beta}_i + \epsilon_{i,t}, \tag{2}$$

where $\mathbf{X}$ is a set of potential controls, and $\epsilon\%\mathcal{N}\left(0, \sigma^2_\epsilon\right)$ is an i.i.d. disturbance term. It is possible to obtain rolling (dynamic) correlations by running regressions for subsamples between t and $t + n$, $t + 1$ and $t + n + 1$, and so on.[9] In the absence of additional adjustments, the estimates $\widehat{\rho}^u$ and $\widehat{\rho}^r$ will typically be biased (Boyer et al., 1997; Forbes and Rigobon, 2002).[10]

Procyclicality is then assessed as a positive value—which, for (1), is strictly less than or equal to unity—although in practice there is probably difficult to determine whether a large, positive (but insignificant) coefficient is more reflective of procyclicality as compared to a positive, significant one that is smaller in magnitude. In this paper, we adopt a different strategy for computing correlations, premised on the conditional variance-covariance matrix that emerges from a garch model.

To obtain these conditional correlations, we first apply the dcc-garch model proposed by Engle (2002), represented by the system

$$\mathbf{Z}_{i,t} = \mathbf{X}'_{i,t}\boldsymbol{\Gamma} + \boldsymbol{\epsilon}_{i,t}, \tag{3a}$$

$$\boldsymbol{\epsilon}_{i,t} = \boldsymbol{\eta}^{1/2}_{i,t}\boldsymbol{\nu}_{i,t}, \tag{3b}$$

$$\boldsymbol{\eta}_{i,t} = \boldsymbol{\delta}^{1/2}_{i,t}\boldsymbol{\rho}^c_{i,t}\boldsymbol{\delta}^{1/2}_{i,t}, \tag{3c}$$

$$\boldsymbol{\rho}^c_{i,t} = \operatorname{diag}\left(\boldsymbol{\theta}_{i,t}\right)^{-1/2}\boldsymbol{\theta}_{i,t}\operatorname{diag}\left(\boldsymbol{\theta}_{i,t}\right)^{-1/2}, \tag{3d}$$

$$\boldsymbol{\theta}_{i,t} = \left(1 - \lambda_1 - \lambda_2\right)\boldsymbol{\rho}^c + \lambda_1\widetilde{\boldsymbol{\epsilon}}_{i,t-1}\widetilde{\boldsymbol{\epsilon}}'_{i,t-1} + \lambda_2\boldsymbol{\theta}_{i,t-1}, \tag{3e}$$

where $\mathbf{Z} = [G \;\; Y]$ is the 2×1 vector of dependent variables, $\boldsymbol{\eta}^{1/2}$ is a Cholesky factor of the time-varying conditional covariance matrix $\boldsymbol{\eta}$, $\boldsymbol{\delta}$ is a diagonal matrix of conditional variances in which each nonzero component evolves according to a univariate garch (1,1) model,[11] and $\boldsymbol{\rho}^c$ is a matrix of conditional quasicorrelations. $\boldsymbol{\nu}\%\mathcal{N}\left(\sigma^2_\nu\right)$ is a 2×1 vector of i.i.d. innovations, while $\widetilde{\boldsymbol{\epsilon}}\%(0,1)$ is a 2×1 vector of standardized errors.

The properties of the system (3) have been discussed extensively elsewhere (c.f. Aielli, 2013) and will not be reiterated here.

[7] There is a much older tradition that has explored political business cycles (Andrikopoulos et al., 2004; Castro and Martins, 2018; Potrafke, 2012). However, many of these only go part of the way toward explaining procyclicality, since there is no *ex ante* reason why business cycles—which are usually less frequent than elections and other regular fluctuations in political activity (such as turnovers in political appointments), especially in advanced economies—need to closely adhere to such political calendars. A spinoff literature on political budget cycles (Rogoff and Sibert, 1988; Shi and Svensson, 2006), while related, is also distinct, since papers in this vein typically examine spending patterns surrounding election events, but are generally less concerned with off-election periods.

[8] Although these two mechanisms feature in most political economy explanations of fiscal procyclicality, they are not the only ones that could matter, of course; Barseghyan et al. (2013), for instance, model legislative politics as a source of political distortion.

[9] An alternative (and more straightforward) approach, which we adopt in this paper, is to compute rolling correlations by repeating the exercise for the static correlation coefficient (1) with analogous slices into subsamples.

[10] Both are biased in the presence of heteroskedasticity, and if regressions are applied to $\widehat{\rho}^r$ without the Cochrane-Orcutt (or other serial correlation correction) procedure, inference will be biased further.

[11] Specifically, the variances of each diagonal element j evolve according to $\sigma^2_{D,j,i,t} = \varphi_{0,j,i} + \varphi_{1,i}\epsilon^2_{j,i,t-1} + \varphi_{2,j,i}\sigma^2_{D,j,i,t-1}$. φ_1 is known as the arch parameter, while φ_2 is known as the garch parameter.

Instead, we merely note three features that are useful for our application. First, the regression specification (3a) is multivariate, in that it treats both the fiscal stance G and cycle state Y as dependent, while also permitting the inclusion of additional independent variables in the matrix $\mathbf{X}$. Second, it is dynamic, in that not only the conditional covariance matrix $\boldsymbol{\delta}$ follows a univariate garch process, but the matrix $\boldsymbol{\rho}^c$ likewise evolves according to (3e). Third, the $\boldsymbol{\rho}^c$ matrix in (3e) turns out to be a weighted *average* of the unconditional mean of $\boldsymbol{\theta}$ and the unconditional covariance matrix of standardized errors $\widetilde{\boldsymbol{\epsilon}}$ (Aielli, 2013). Since this weighted expression is neither these two unconditional terms independently, the parameters of interest embedded in $\boldsymbol{\rho}^c$ are generally referred to as (conditional) quasicorrelations. These are calculated, for an element in row k of column l for the sample between t and $t+n$, as

$$\rho^c_{kl,i,t,t+n} = \frac{\theta_{kl,i}}{\sqrt{\theta_{kk,i}\theta_{ll,i}}} = \frac{\mathrm{cov}\,(G_i, Y_i)}{\sqrt{\sigma^2_{G_i}\sigma^2_{Y_i}}}. \tag{4}$$

Although (4) offers a conditional correlation sensitive to temporal dynamics, it remains a time-invariant representation of the procyclicality relationship. To obtain *dynamically evolving* measures of this correlation, we fit the estimated model (3) and obtain in-sample predictions of the conditional variance-covariance matrix, after which we can derive the time-varying conditional correlation:

$$\rho^d_{kl,i,t} = \frac{\theta_{kl,i,t}}{\sqrt{\theta_{kk,i,t}\theta_{ll,i,t}}} = \frac{\mathrm{cov}\left(G_{i,t}, Y_{i,t}\right)}{\sqrt{\sigma^2_{G_{i,t}}\sigma^2_{Y_{i,t}}}}. \tag{5}$$

The dynamic conditional estimate of correlations represented by $\hat{\rho}^c$ and $\hat{\rho}^d$ offer several distinct advantages relative to the standard approaches described earlier. First and foremost, real GDP movements over a sufficiently long time horizon are likely to suffer from heteroskedasticity in the underlying distribution; this in turn can lead to increased estimates of procyclicality, even if the true relationship between spending and output remain unaltered (Forbes and Rigobon, 2002). Second, this measure of procyclicality is not dependent on the idiosyncratic time frame. Deriving changes in correlation over arbitrary subsample splits can introduce the same sort of bias inherent in computing conditional correlations in the presence of heteroskedasticity (Boyer et al., 1997). Third, the time-varying correlations in (5) allow us to capture whether the fiscal-output relationship may have strengthened (or weakened) *over time*, rather than simply verify the presence of a continuous linkage, regardless of the state of the business cycle.

The system (3) is estimated via repeated maximum likelihood over a maximum of 16,000 iterations, using the Newton-Raphson algorithm.

3.2. Fiscal policy indicators

We construct our measures of fiscal policy along three main dimensions. The first is real government final *consumption* (G_c), and the second is real total *expenditure* (G_e). Fluctuations between the two are often due to gross public capital formation, and hence the latter series provides additional insight into government investment dynamics that are unavailable when considering consumption alone.[12] Both series are expressed in real terms using appropriate deflators, and constitute fiscal policy from the spending side of the government budget.

We obtain the cyclical component of these variables by passing each of these series through a Hodrick and Prescott (1997) (HP) filter[13] to extract the cyclical component, which we treat as a cyclically-adjusted spending indicator.[14] We repeat the same exercise for real GDP to derive our corresponding measure of the state of the business cycle.[15]

Our third approach introduces instead an indicator of fiscal policy that does not require calculating deviations from trend. This is the *primary expenditure* share of output (G_p). This measure nets our interest payments from expenditures—thereby better capturing the discretionary component of government spending—and divides it by GDP. Since this measure is already normalized by output—hence, increases in the primary expenditure share constitute real fiscal impulses—the corresponding business-cycle concept here is no longer deviations in the level of GDP, but rather real GDP growth, which is what we employ.

The extent of *fiscal procyclicality* is then captured by the degree to which either the cyclically-adjusted or GDP-share fiscal spending measures, when set against their corresponding business-cycle state measures such as the cyclical deviation of real GDP or its growth rate, yield high estimates of ρ^u, ρ^r, ρ^c, and/or ρ^d.

[12] The other major components of expenditure are transfer payments and interest expenses. The former tends to be less volatile, although it is a legitimate target for political-economic pressures. The latter tends to be both relatively stable and largely exogenous once the debt has been incurred.

[13] As the data are annual, we follow Ravn and Uhlig (2002) and adopt a smoothing parameter of 6.25.

[14] One known concern with the HP filter is that the endpoints for the filter tend to be suboptimal. There are several reasons to believe, however, that this is less an issue in our particular application. First, the poor calibration tends to be most problematic when drawing real-time inferences or rendering forecasts, while our use here is to detrend the respective series for subsequent secondary analysis. Second, the filter was applied to both spending and output; to the extent that there is an endpoint bias, it would apply with equal force to both measures, and hence give rise to a correspondingly diminished effect on the subsequent correlation calculations. Third, the filter is applied to only two of the three baseline metrics we consider. We nevertheless performed a sensitivity check by truncating the first and final three years of each series and recalculating the dynamic conditional correlations. The qualitative results we report do not change much as a result of this truncation, although the sample size is severely compromised, leading to noisier estimates. These results are available on request.

[15] While we have retained the standard approach in the literature for identifying the cyclical component of fiscal policy and output, other approaches are possible. Bashar et al. (2017), for example, impose an unobserved components model to disentangle the correlations in cycles from correlations in slopes of the relevant variables.

In our robustness checks, we also consider three different fiscal balance measures, namely the deficit/GDP ratio, the cyclical deviations of the fiscal balance, and the primary deficit/GDP. These alternative dependent variables are detailed in the data data appendix.

3.3. Econometric model

After obtaining our different measures of procyclicality, we subject these to a straightforward regression of our procyclicality measure on our two main determinants of interest:

$$\rho_{i,t}^{m} = \chi_0 + \chi_t + \chi_i + \mathbf{W}'\chi + \chi_P PolEc_{i,t-1} + \chi_F FinAcc_{i,t-1} + \varepsilon_{i,t}, \tag{6}$$

where $m \in \{u, r, c, d\}$ is one of our four candidate measures of procyclicality, and *PolEc* and *FinAcc* are proxy measures of political economy and financial access, respectively, lagged one period to alleviate the most egregious instances of simultaneity bias. In our baseline, we use political participation (the *Polity* index[16]) to capture political-economic influences, and the outstanding debt burden (public debt to GDP) to represent financial access. **W** represents a set of additional controls potentially related to the business cycle, while χ_0, χ_t, and χ_i represent a constant term, along with time and country fixed effects. $\varepsilon \% \mathcal{N}\left(0, \widehat{\boldsymbol{\Omega}}\right)$ is a matrix of variances that allows for two-way clustering of by country and time.[17,18]

Our controls are designed to address three other main demand-side channels by which other forms of stabilization policy (other than fiscal policy) may alter the fiscal-output relationship. These include monetary policy (proxied by the change in the money supply), exchange rate policy (captured by the change in the nominal exchange rate), and trade policy (approximated by the trade balance).[19]

χ_P and χ_F represent our coefficients of interest. *A priori*, the theories discussed in Section 2 suggest that greater participation would give rise to a stronger influence of politics on fiscal policy procyclicality (a positive coefficient). Theory also implies that a greater debt burden is likely to result in reduced financial access, and hence a greater tendency toward procyclical behavior (a positive coefficient).

3.4. Data matters

Our data are drawn from a number of distinct sources. The majority of the fiscal measures, as well as macro controls, are from the *World Development Indicators* (WDI), supplemented (especially for recent years) by the *World Economic Outlook* (WEO) database. Although primary expenditure data are available in the latter, we exploit the much longer temporal coverage available in the Mauro et al. (2015) dataset, along with the real growth series there. In general, G_c coverage is up to a maximum range of 1960–2015, G_e coverage is up to 1980–2015, and G_p coverage is up to 1862–2011.

The Polity index (as well as democracy indicator, used in robustness checks) are from latest version of the Marshall et al. (2002) Polity IV database, while the public debt data are from Abbas et al. (2011), which is also offers the longest temporal coverage.[20]

Most controls were also sourced from the WDI or WEO, although notably our corruption measure (used in our discussion) was drawn from the *International Country Risk Guide*, private credit was from the Macrohistory database (Jordà et al., 2017), and fiscal rules rely on the Fiscal Rules dataset first compiled by Schaechter et al. (2012).

In general, all the main explanatory variables and their controls were transformed with an inverse hyperbolic sine transformation prior to the regressions (this avoids the negative value-problem that plagues the more common logarithmic transform). Additional data and definitions, along with minor data cleaning procedures, are available in the data appendix.

[16] Although the polity measure captures only one dimension of political economy, it has the important advantage that it is available for many countries over a long time period. In Section 5.2, we expand our measures to include a proxy for special interest pressure (corruption), and we also consider a number of other alternative measures of political participation in our robustness checks.

[17] More formally, this is given by $\widehat{\boldsymbol{\Omega}} = \sum_{i=1}^{NT} \sum_{t=1}^{NT} \mathbf{I}_{i,t} \widetilde{\mathbf{W}}_i \widetilde{\mathbf{W}}_t' \widehat{\varepsilon}_i \widehat{\varepsilon}_t$, where $\mathbf{I}_{i,t}$ is an indicator that takes on unity when i, t share the same cluster, and zero otherwise; and $\widetilde{\mathbf{W}} = \left[\mathbf{W}\ PolEc\ FinAcc\right]$.

[18] Given the nature of the time-varying nature of the dependent variable, clustering by year is self-evident. Our decision to cluster at the country, instead of a higher level (e.g. region), is due to the likelihood that intra-country correlation is likely to still be present even after the inclusion of fixed effects (Cameron and Miller, 2015), because correlation estimates are constructed from the full set of country-level observations. We view this selection-based justification as the most defensible reason for clustering at the country level. Nevertheless, we also consider estimates with standard errors that are either clustered by year and region, or with Eicker-Huber-White-corrected robust standard errors. In either case, our qualitative conclusions are unchanged, with significance slightly weaker in the former case, and slightly stronger in the latter.

[19] One important practical consideration directing our selection of these variables is to minimize sample attrition due to missing one or more of these additional controls. Thus, for example, we rely on the change in the money supply to proxy shocks from monetary policy, instead of changes to other possibly more common indicators such as the interest rate.

[20] One alternative to the use of public debt is *external* debt (owed to nonresidents of a country). The issue with relying on this series, and other variant debt measures more generally, is that doing so tends to decimate our sample size. External debt data, for instance, are available mainly from the 1990s, and for mostly developing economies, which would leave the working sample much smaller.

Table 1
Static and dynamic measures of fiscal procyclicality, 1810–2018 (maximum)[a].

	Static					
	Unconditional			Conditional		
	G_c	G_e	G_p	G_c	G_e	G_p
All	0.19	0.14	−0.06	0.20	0.10	0.66
Advanced	0.11	−0.04	−0.11	0.21	−0.04	0.63
Developing	0.22	0.23	−0.01	0.20	0.20	0.70
	Dynamic					
	Rolling			Conditional		
	G_c	G_e	G_p	G_c	G_e	G_p
All	0.17	0.11	−0.14	0.18	0.07	0.57
	(0.27)	(0.33)	(0.21)	(0.22)	(0.34)	(0.17)
Advanced	0.08	−0.06	−0.24	0.16	−0.10	0.57
	(0.28)	(0.36)	(0.14)	(0.23)	(0.44)	(0.11)
Developing	0.21	0.19	−0.04	0.19	0.17	0.56
	(0.25)	(0.29)	(0.22)	(0.22)	(0.22)	(0.23)

[a] Static correlations are the average across all countries for a given income group, and may be computed with different start/end years. Dynamic correlations are the average of within-country means across all countries for a given income group. Parentheses are standard deviations calculated from within-country distributions. G_c coverage is up to 1960–2018, G_e coverage is up to 1980–2018, and G_p coverage is up to 1810–2011.

4. Results

4.1. Preliminaries

Before proceeding to our main findings, it is useful to document the extent to which heteroskedasticity was an issue with the data. We perform two sets of tests: country-specific Breusch-Pagan/Cook-Weisberg tests, and panel-level LR tests, for regressions of each of the fiscal variables on output. For the full panel, the χ^2 statistics using any of the three fiscal variables are all highly significant. For the country cross-sections, the tests were frequently significant for G_c and G_e; only in the case of G_p did most of the tests turn out insignificant.[21] On balance, these tests reveal that heteroskedasticity is a likely problem in our data, which justifies our application of the garch model (this set of results are available on request).

4.2. Estimates of procyclicality

Table 1 reports our estimates of procyclicality. The top panel reports the two static forms (the unconditional Pearson's coefficient, ρ^u) and the conditional quasicorrelation between standardized errors (ρ^c). The bottom panel computes the time-varying rolling correlation, ρ^u, calculated over moving 10-year windows, along with the means of the predicted dynamic conditional correlation (ρ^d). In each instance, we report averages for the full sample, along with averages by income group.

For the world as a whole, fiscal policy appears to be procyclical, on average. Regardless of our choice of fiscal instrument or measurement approach, correlations tend to be positive; we take this to mean that the evidence is in favor of mild procyclicality, with correlations ranging from 0.1 to 0.2. The exception is the primary expenditure share vis-à-vis growth, which is consistently negative for the unconditional correlations of G_p (whether using a static or rolling measure), as compared to the conditional estimates, which are not just positive but quite large in magnitude.[22] While this exception may be surprising at first glance,[23] in most cases the positive conditional estimate is more consistent with the G_c and G_e variants from the same country (and other estimates of fiscal procyclicality in the literature), and hence strike us as more reasonable.[24] On balance, we view the plausibility, consistency, and stability of the estimates obtained from the dcc-garch model as evidence in favor of relying on such conditional correlations to more accurately pin down the true fiscal-output relationship.

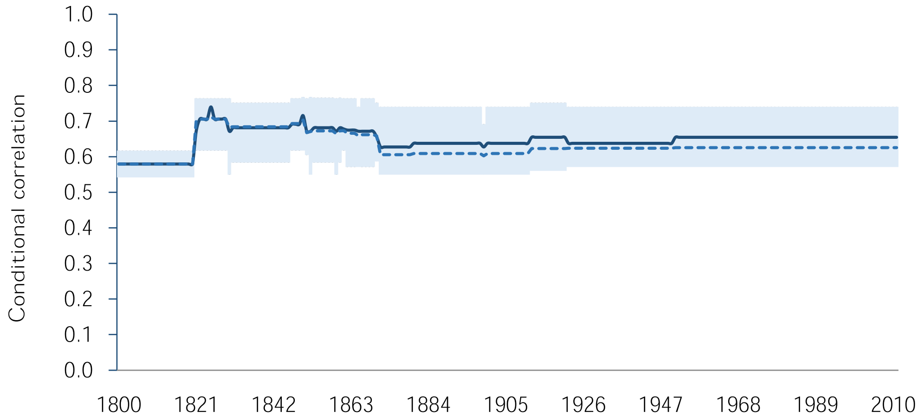
Advanced economies also tend to demonstrate less procyclical behavior, relative to developing ones. In half the cases, the calcu-

[21] One potential reason for this distinction is that G_p is measured as a share of GDP (rather than deviations from trend); consequently, the risk of bias arising from heteroskedasticity owing to fluctuations in the business cycle may be reduced. Nevertheless, given the importance of heteroskedasticity at the panel level (even for G_p), we continue to believe that garch corrections remain relevant for all indicators.

[22] While the magnitudes for the G_p correlation may be on the high side, for the analysis that follows, we are somewhat less interested in the actual *levels*—which are evidently sensitive to the specific metric used—but more in their *changes* over time, and especially when conditioned on other covariates.

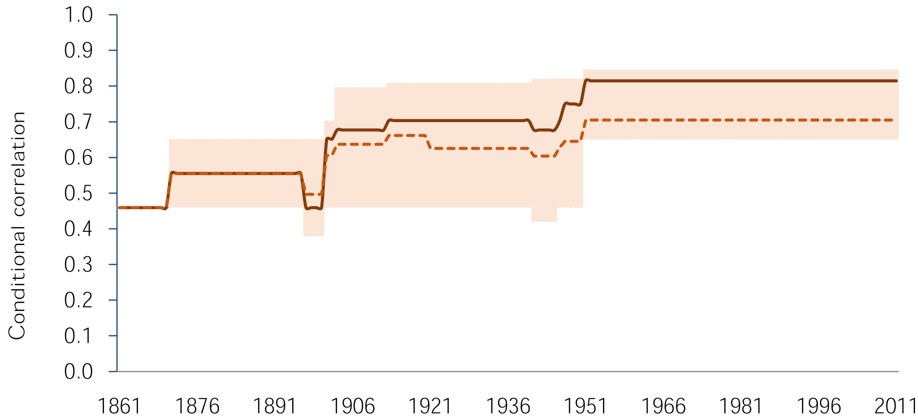
[23] This is especially the case since the country-specific heteroskedasticity tests, as reported in Section 4.1, tend to indicate little issue with heteroskedasticity for G_p. However, a few mitigating factors should be kept in mind. First, heteroskedasticity *is* an issue for the panel at large. Second, the positive conditional estimate is more consistent with the other measures of spending procyclicality. Third, conditional correlations are more sensitive to not only heteroskedasticity (which afflicts the standardized residuals in (3e)), but also temporality (via lags of the unconditional mean in (3e), as well as the time-varying conditional variance matrix (3c)). The upshot of this is that the conditional correlation embed all historical information in generating dynamic correlations, rather than just a partial history (as is the case for rolling correlations). Fourth, while rolling correlation estimates $\hat{\rho}^r$ *do* address changes over time, these tend to be swing sharply, changing from negative to positive over the course of several years.

[24] As an additional check on whether the conditional correlations are sensitive to the estimation methodology, we rerun estimates of $\hat{\rho}^c$ for G_p using two alternatives: a constant conditional correlation (ccc-garch) or a varying conditional correlation (vcc-garch) model. These alternative models differ from our baseline dcc-garch model in terms of the parameter restrictions imposed on the correlation matrix. The resulting conditional correlations, which are reported in the appendix, are not substantively different from the baseline dcc-garch estimates.



Source: Author's calculations.
Notes: Shaded area represents the upper (75th) and lower (25th) percentile, and solid (dashed) line represents the median (mean), in the distribution of the conditional correlations between primary expenditure and GDP growth, for any given year.

(a) Advanced



Source: Author's calculations.
Notes: Shaded area represents the upper (75th) and lower (25th) percentile, and solid (dashed) line represents the median (mean), in the distribution of the conditional correlations between primary expenditure and GDP growth, for any given year.

(b) Developing

Fig. 1. Distribution of static conditional correlations for advanced economies, 1800–2011 (left) and developing economies, 1861–2011 (right), between primary expenditure and GDP growth. Both groups exhibit procyclicality, on average, although developing economies do display systematically higher correlations since 1950.

lated correlation is actually negative; in most of the remaining instances where the coefficient is positive, it nevertheless tends to be lower than the comparable figure for developing countries (and in the two cases where it *is* higher, it is so by only a hair). This result is broadly consistent with the broader literature (Alesina et al., 2008; Calderón et al., 2016; Frankel et al., 2013).

It is also worth noting that among the dynamic calculations, the variability of correlations tend to be quite large, relative to the mean. This is not entirely surprising, and is reflective of the wide fluctuations in procyclical behavior both *between* countries (as demonstrated by the large standard deviations), as well as *within* them (not directly captured by the table, but this will be evident below).

The between-country variability in procyclicality is even more evident when we compare the distribution of static conditional correlations (ρ^c) by income group (Fig. 1). Even when we restrict the bounds to a 25th/75th percentile distribution, the within-group

Table 2
Cross-sectional regressions for fiscal procyclicality, 1800–1995 (maximum)[a].

	Unconditional					
	G_c		G_e		G_p	
	(C1)	**(C2)**	**(C3)**	**(C4)**	**(C5)**	**(C6)**
Polity	−0.041	−0.005	−0.043	−0.031	−0.012	0.003
	(0.013)***	(0.012)	(0.025)*	(0.013)**	(0.012)	(0.020)
Debt	−0.143	−0.028	0.035	0.013	−0.049	−0.093
	(0.046)***	(0.028)	(0.066)	(0.029)	(0.029)*	(0.046)**
R^2	0.298	0.012	0.088	0.034	0.093	0.121
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
Errors	B/strapped	B/strapped	B/strapped	B/strapped	B/strapped	B/strapped
Obs.	39	148	32	160	43	54
	Conditional					
	G_c		G_e		G_p	
	(C7)	**(C8)**	**(C9)**	**(C10)**	**(C11)**	**(C12)**
Polity	−0.045	−0.041	−0.008	−0.039	−0.013	−0.024
	(0.022)**	(0.034)	(0.028)	(0.041)	(0.010)	(0.016)
Debt	−0.075	−0.010	0.066	0.061	0.023	0.031
	(0.068)	(0.054)	(0.077)	(0.065)	(0.028)	(0.039)
R^2	0.138	0.059	0.026	0.052	0.049	0.038
Estimation	OLS	OLS	OLS	OLS	OLS	OLS
Errors	B/strapped	B/strapped	B/strapped	B/strapped	B/strapped	B/strapped
Obs.	39	39	32	33	43	36

[a] The dependent variable is the static unconditional or conditional correlation between the cyclical components of economic activity and government spending listed in the first row of each panel. All other variables are expressed using the inverse hyperbolic sine transformation. A constant term was included in all regressions, but not reported. Bootstrapped standard errors, replicated over 16,000 iterations, are given in parentheses. Goodness-of-fit measures report the unadjusted R^2. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

interquartile range is more than twice as large among developing economies, especially in the first half of the 20th century. While this variation has narrowed since that time, it remains higher than that of advanced economies at virtually any time in their history.

Another important takeaway from this graphical representation of procyclicality is that we see little evidence of diminishing trends over time. Procyclicality in the advanced world has remained largely stable, while rising in the developing world. While this feature of the data may seem opposed to the claim made by some that procyclicality has diminished in the recent past (Frankel et al., 2013; Jalles, 2018), the divergence between the two measures of central tendency in our developing world subsample is actually consistent with this fact: that there has been *some* evidence of graduation (which accounts for the decline in mean relative to the median), but this occurs too infrequently to shift the median.

4.3. Cross-sectional regressions

As a first step, we consider regressions at the cross-section, using static measures of correlations, both unconditional ρ^u (Table 2, upper panel) and conditional, ρ^c (Table 2, lower panel). For each fiscal policy measure, we run a specification that uses only data from the initial year for each country (odd-numbered columns), and a specification that averages the observations over the full sample period (even-numbered columns).[25]

The main impression one receives from these regressions are that neither political economy nor financial access appears to be important for fiscal procyclicality. Many specifications—especially those where correlations are conditional—give rise to insignificant coefficients. And when coefficients are significant, they tend to produce signs that are inconsistent with theory and intuition.

For example, a higher initial level of political participation appears to be associated with less procyclicality (specification C1); while this *could* imply that greater participation inspires policymakers to adopt better stabilization policy, it is more likely reflecting the fact that higher-income economies are more likely to be democratic, and these same economies usually run less procyclical fiscal policies (due to omitted variable bias). Similarly, the possibility that a less financially-constrained economy chooses to exploit this enhanced access to reduce fiscal expenditures during booms *could* be the result of enlightened policymaking, or it may simply be a reflection of how economies able to sustain higher levels of debt are also those more likely to adopt countercyclical fiscal policies in the first place (an issue of selection).[26]

These issues could be addressed by the inclusion of additional controls, but are also easily remedied by applying the within

[25] Since we are working with a cross-section, we deviate from the model outlined in (6) and simply apply OLS, with bootstrapped standard errors given the relatively small size of the sample in most cases.

[26] A separate, technical concern is that the sample size varies between the unconditional and conditional correlations. We do not view this as a likely problem, since the results when using just observations from the initial year (odd-numbered columns), which do not suffer from this sample size change, are qualitatively similar and largely analogous to the results from the full sample period (even-numbered columns). To further verify that sample changes are not an issue, we reran the regression with the full sample restricted to only the countries in the initial-year sample. With the exception of the G_p measure, the results are qualitatively identical. For G_p, the sign change on the coefficient on debt between the unconditional and conditional correlations is indeterminate, since the coefficients on the latter are statistically insignificant.

estimator with appropriate fixed effects. Just as important, introducing the panel dimension will ensure the analysis takes into account *time-varying* correlations, which—as evident in Fig. 1—is a feature of the data. Accordingly, we discount this set of results and turn to our panel analysis.

4.4. Baseline panel analysis

Table 3 reports our baseline results. As before, we consider all three fiscal policy metrics, although we now rely on the respective dynamic conditional correlations (ρ^d) as our dependent variable. For each measure, we include specifications where we variously control for only country fixed effects, both country and time fixed effects, and fixed effects alongside additional controls.[27]

We offer several remarks about these results. First, the evidence suggests that political economy appears to matter relatively more than financial access, insofar as conditioning the dynamic correlation between fiscal policy and output is concerned. The coefficient on the polity variable is consistently positive, and frequently significant[28]; in contrast, the coefficient on debt switches signs when for government consumption versus expenditures (whether as a cyclical deviation or share of GDP).[29] The magnitudes suggest an elasticity of the political economy effect that ranges from 0.01 to 0.08 percent (among the significant coefficients).[30]

Second, the switch in signs for the debt constraint from positive for G_c to negative for G_e and G_p could imply that public investment—which is included in the latter two measures but not in the first—could well be countercyclical, especially if financial constraints are binding.[31] If we accept this to be the case, governments may choose to scale back on infrastructure or R&D expenditures (elements of government investment) during a boom, for fear of crowding out private sector efforts while simultaneously reserving capital for an actual downturn; conversely, during a recession, it is the loss of financing that inhibits the deployment of public investment as a means of stabilization policy. Government consumption may well be less sensitive to such considerations, and hence either exacerbates the cycle, or is acyclical, at best. However, the fact that polity tends to be statistically significant for these latter two measures could alternatively imply that political pressure for procyclical spending operates along the investment channel as well, a result that has some support in the literature (Gupta et al., 2016). Another possibility is that there could be politically-induced procyclicality from transfer payments (transfers are included in expenditures but not consumption), which is consistent with some empirical evidence (Manacorda et al., 2011).[32,33]

Third, the somewhat stronger results we obtain from primary expenditures vis-à-vis growth also speak to the possibility that recurring fiscal liabilities—in this case, interest payments—may not feature strongly in the spending decisions of governments. That is, netting out the contributions of such liabilities appears to heighten the sensitivity of the spending proxy to the effects of political economy and financial access. This could be because these conditioning variables tend to operate on the discretionary element of expenditures.

This tension between the different components government expenditure has implications for our understanding of how to model the key transmission channels theoretically, not least because many existing models of procyclical behavior tend to focus on electoral competition. Since this generally appeals to aspects of government consumption or transfer payments, such models could miss out on an equally important channel—public investment—by which political economy forces operate. The contrast between the cyclical patterns of expenditures versus consumption, or between recurrent versus discretionary spending, may also potentially reconcile why some empirical studies (e.g. Andrikopoulos et al., 2004) find that fiscal policy is stabilizing, while others find the opposite result (e.g. Castro and Martins, 2018), even when examining the same group of countries.

Although we have focused on government expenditure for this baseline (for reasons documented in the introduction), it is worth pointing out that the conditioning effect of polity and debt, while occasionally statistically significant, do appear to matter less when using fiscal *balance* measures instead as our dependent variable of interest (these are included as robustness checks, details of which

[27] We view the respective middle specifications for each as our baseline, because it controls for the two most important dimensions of unobserved heterogeneity (and correlated residuals along those same dimensions) for our particular sample, without sacrificing sample coverage.

[28] In some cases, even though the coefficient is insignificant at standard levels, they approach significance; for instance, for specifications P2 and P4, $p = 0.12$ and $p = 0.17$, respectively.

[29] One potential concern regarding the results in Table 3 is that the significance of the coefficients on specifications with controls (P3, P6, and P9) appear to be weaker. We believe that this is the consequence of a combination of sample attrition and the inclusion of the control for changes in the money supply. We systematically test the sensitivity of the results to controls by re-running the regressions using all possible combinations of controls, as well as substituting the change in the money supply with the interest rate. As shown in the appendix and discussed in greater detail there, the main takeaway from this exercise is that the coefficient on the polity variable tends to exhibit much more stability relative to debt, consistent with the conclusions we draw here.

[30] For a linear-arcsinh specification, it can be shown that the elasticity of ρ in response to a variable X, $\xi_{\rho X}$ is approximately $\hat{\xi}_{\rho X} \approx \frac{\hat{\beta}}{\rho}$, where $\hat{\beta}$ is the coefficient estimate of the linear-arcsinh model (Bellemare and Wichman, 2020).

[31] Although we are unable to definitively corroborate this claim, we do find evidence that public investment is countercyclical in the presence of financial constraints, while political economy pressures do not appear to be as important. These results, which are based on a more limited dataset, are reported and discussed in greater detail in the appendix.

[32] A third possibility is that this is a statistical artifact that results from sample coverage differences across the three measures. We attempt to rule out this contrivance by re-running the regressions using only the observations that overlap when each respective baseline specification (P2, P5, and P8) is considered. With one exception (the subsample for P8 leads to the coefficient on the polity measure flipping signs when G_c is the dependent variable), the results remain qualitatively unchanged from those reported in Table 3. These additional results are available on request.

[33] A final possibility is that multicollinearity may be present—perhaps because the polity and debt variables exhibit comovement—and this violation of standard assumptions leads to the changed signs. We believe that this is unlikely, given the low correlation between the two variables ($\rho = 0.07$). Nevertheless, as an additional check, we enter these two variables separately to ascertain that the sign switch is not because of collinearity issues. Doing so does not generally alter the signs nor the significance of the coefficients on the variables of interest (results are available on request).

Table 3
Panel regressions for fiscal procyclicality, 1801–2016 (unbalanced)[a].

		G_c			G_e			G_p	
	(P1)	**(P2)**	**(P3)**	**(P4)**	**(P5)**	**(P6)**	**(P7)**	**(P8)**	**(P9)**
Polity	−0.001	0.011	0.009	0.010	0.016	0.016	0.025	0.010	0.018
	(0.007)	(0.007)	(0.009)	(0.007)	(0.008)**	(0.009)*	(0.005)***	(0.005)*	(0.010)*
Debt	0.050	0.042	0.021	−0.008	−0.016	−0.029	−0.046	−0.034	−0.015
	(0.019)**	(0.029)	(0.040)	(0.024)	(0.028)	(0.031)	(0.018)**	(0.011)***	(0.037)
Trade balance			−0.013			−0.006			−0.016
			(0.009)			(0.013)			(0.011)
Δ money supply			0.022			−0.026			−0.010
			(0.060)			(0.096)			(0.065)
Δ exchange rate			0.000			0.028			−0.004
			(0.000)			(0.011)**			(0.002)**
Fixed effects:									
Time?	No	Yes	Yes	No	Yes	Yes	No	Yes	Yes
Country?	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (adj.)	0.469	0.455	0.432	0.704	0.708	0.633	0.303	0.443	0.479
R^2 (within)	0.010	0.013	0.009	0.002	0.005	0.010	0.050	0.018	0.039
Estimation	FE	FE	FE	FE	FE	FE	FE	FE	FE
Errors	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
Ctry (yr)	44 (57)	44 (57)	37 (55)	41 (37)	41 (37)	33 (37)	40 (211)	40 (211)	29 (50)
Obs.	1874	1874	1491	1081	1081	762	3222	3222	1198

[a] The dependent variable is the dynamic conditional correlation between the cyclical components of economic activity and government spending listed in the first row. All other variables are expressed using the inverse hyperbolic sine transformation (except for change variables, which are expressed as percentage changes) and lagged one period. A constant term was included in all regressions, but not reported. Standard errors, clustered over country and year, are given in parentheses. Goodness-of-fit measures report the R^2 and within R^2. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

are reported in the appendix). One possible explanation for this is that procyclicality has a tendency to operate on the spending margin.[34] In particular, if governments exercise the (rational) policy of countercyclicality on the revenue side—subject to them facing no constraints on the revenue front—this choice could mute the effects of procyclical spending.

Finally, we recognize that these results are obtained from a relatively narrow set of economies (between 29 and 44), and may not extend to a more diverse sample. While we have made an effort to ascertain that our results are not unduly affected by sample size fluctuations, the compromise we make by relying on time-series methods to generate our procyclical measures means that external validity questions inevitably arise.[35] Even so, we view the incorporation of many more cycles into the working sample that we do have as an important advance.

4.5. Endogeneity concerns

Notice that, in contrast to existing approaches in the literature, the formulation in (6) only accounts for endogeneity by way of controlling for unobserved heterogeneity. Endogeneity may of course arise instead from reverse causality. The lagged implementation of the *PolEc* and *FinAcc* proxies should go some way toward alleviating immediate concerns regarding simultaneity bias, especially given the slow-evolving nature of both variables, which are stocks rather than flows.

Still, one may hold residual doubt about this form of endogeneity. One strategy, commonly employed in the literature, is to deploy instrumental variable techniques. This is challenging in our context, because even if we *were* able to secure instruments that convincingly satisfy the exclusion restriction, it is unlikely that these instruments would be sufficiently long-dated, given the temporal coverage of our sample.

These caveats aside, it is possible to make some progress in terms of understanding the extent to which either political economy or financial access influences procyclicality. After all, the correlations ρ_c and ρ_d are not derived from simply taking, at face value, the average effect of output fluctuations on fiscal policy, after controlling for political participation or the debt constraint. Rather, we can instead ask whether politics or finance affects the variance-covariance matrix derived from the error terms in (3e). This distinction is important, because it means that we can observe how the calculated correlations change after embedding our variables of interest directly into the multivariate dcc-garch specification.

The strategy we employ here is to alternately[36] include either polity or debt as a dependent variable in (3), then calculate the changes in the dynamic correlation ρ^d relative to that without the added variable. Two-sided t tests of the difference-in-means are then computed, by country. These are reported in Table 4, along with the total share of the sample for which the difference-in-means are significant.[37]

The results largely corroborate the qualitative conclusions from our panel analysis. Including either political economy or financial access into the model results in significant changes in the dynamic conditional correlations obtained between 50 and 100 percent of the time. The significant divergences appear more frequently when polity is included, with some exceptions, mainly when using the primary expenditure (although the difference in significant shares is much smaller than those for total expenditure). In addition, the divergences were often *in*significant for government expenditure.

The income group-disaggregated results also suggest that financial access might be a more significant driver of procyclicality in developing countries insofar as government consumption is concerned, while political economy factors feature more in its effects on government expenditure. Overall, the results lend some modest additional support to the notion that political economy appears to matter more than financial access in governing fiscal procyclicality.

These divergences are well-captured visually. Fig. 2 graphs the predicted dynamic conditional correlations between government consumption and output, together with the further inclusion of either polity or debt as an additional dependent variable, for Finland and Greece.[38] For Finland, it is clear that significant separation in the occurs after taking into account the effects of financial access, which underscores the value of directly conditioning on our variables of interest; in contrast, there is little difference after accounting for either politics or finance.

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[34] There are theoretical and empirical reasons that support this claim. Theoretically, both government transfers and consumption tend to be easier to alter than tax rate changes, since the former typically only require budget-line adjustments which—while dependent on their magnitude and existing fiscal rules—may be independently executed by the fiscal authority or executive. In contrast, revenue-side changes almost always require legislative approval as well. Empirically, both taxes and revenue are countercylical or acyclical in our sample. The correlation between the tax rate (as a share of profit) and the cyclical component of GDP is extremely low, always insignificant, and almost uniformly negative; in the government consumption and expenditure subsamples, for example, these are $\rho(\tau, Y) = -0.020, p = 0.65$ and $\rho'(\tau, Y) = -0.023, p = 0.62$, respectively, and in the primary expenditure subsample, this is $\rho''(R, Y) = -0.038, p = 0.03$. These low correlations are largely independent of development status.

[35] In addition to the sample restrictions we document in footnote 32, we also re-ran the regressions for the first two specifications of each spending measure set, using the restricted subsample from the final specification, to check if the findings using the larger sample held. In almost all cases, the results were qualitatively unchanged (for G_p, however, the coefficients on debt fell out of significance). These results are also available on request.

[36] We do not do so simultaneously because we run into severe degree-of-freedom problems that prohibit convergence in the estimation.

[37] In principle, it may be possible to replicate this approach using fixed effect panels, by comparing the coefficient estimates before and after including the additional variable of interest, via a Hausman test. However, doing so could mask important period-to-period changes in the relationship (a coefficient that changes significantly with each incremental year may nevertheless average no change over the full sample period). Second, while the two-step method is an indirect way of evaluating effect changes due to an added variable, our method accounts for how this additional variable alters the fiscal-output relationship directly, since our conditional (quasi) correlations are, by construction, derived from residuals.

[38] For additional context, we also provide equivalent charts for Chile and Nicaragua, which illustrate mid-period switches and trends in procyclicality, in the appendix.

Table 4
Difference in means for dynamic conditional correlations after controlling for polity or debt, 1862–2015 (unbalanced)[a].

	Political economy					
	G_c		G_e		G_p	
	Diff.	Sig. (%)	Diff.	Sig. (%)	Diff.	Sig. (%)
All	−0.008	59	−0.028	57	−0.380	90
Advanced	−0.013	60	−0.016	60	−0.467	88
Developing	−0.002	58	−0.039	50	−0.292	100
	Financial access					
	G_c		G_e		G_p	
	Diff.	Sig. (%)	Diff.	Sig. (%)	Diff.	Sig. (%)
All	−0.038	59	−0.057	29	−0.441	100
Advanced	−0.068	50	0.022	40	−0.352	100
Developing	−0.009	67	−0.136	0	−0.529	100

[a] Differences are the mean changes in dynamic conditional correlations within each country, averaged across all countries for a given income group, and may be computed with different start/end years. Significant shares indicate the number of countries where the *t*-test for differences in means are significant at the 5 percent level. G_c coverage is up to 1960–2015, G_e coverage is up to 1980–2015, and G_p coverage is up to 1862–2011.

4.6. Robustness

In the appendix, we consider three sets of robustness checks that rely on different measures of the key variables of interest or fiscal policy measure. First, we use a more narrowly-defined measure for political participation (democracy instead of polity); second, we use an alternative estimate of the debt constraint (fiscal space instead of debt); and third, we use various measures of fiscal balance (the fiscal deficit or the primary balance). While these different measures may, arguably, be more nuanced proxies for the effects we seek, we find that the results are broadly unchanged, relative to our baseline.

5. Discussion

One fascinating result in Section 4 is how procyclicality responds differently to political economy effects versus financial access constraints. More specifically, the former tends to be uniform in its (positive) influence, whereas the latter is more ambiguous, depending on the fiscal policy measure in question. We have speculated that this could be due to differential responses in government consumption as opposed to investment. In this section, we probe whether this result is due to income—a quintessential distinction drawn in the literature—or whether additional political or financial channels may be responsible.

5.1. Contrasting procyclicality in advanced versus developing economies

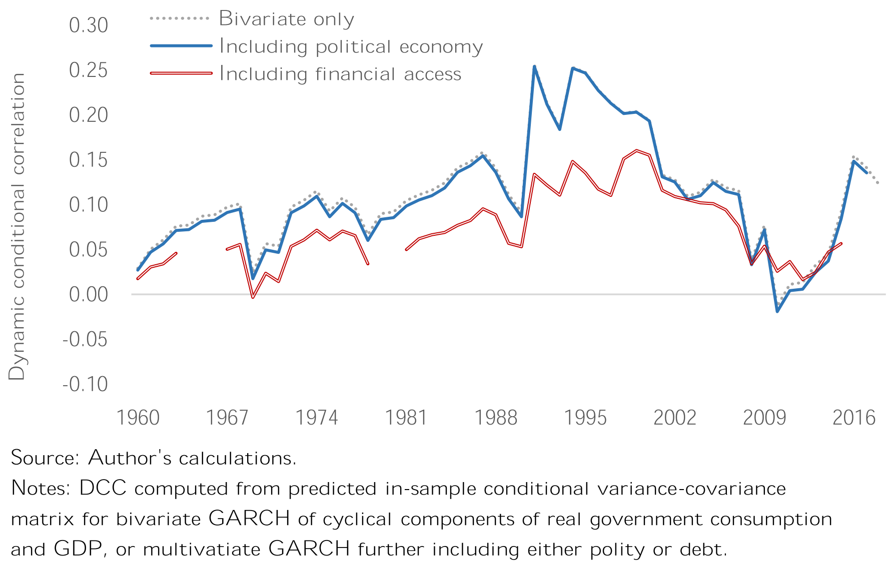
Given the pervasiveness of the advanced/developing distinction in the literature, we would be remiss not to consider our main results in the context of a sample split in that manner. One important point to underscore is that we are not analyzing whether procyclicality is higher or lower for each group; rather, the subsample split only allows us to evaluate whether polity and/or debt are more pronounced in their effects on procyclicality *within* each income group. As discussed in Section 4.2, procyclicality is clearly higher in developing countries. Hence the contribution from the estimated coefficients to the *level* of the conditional correlations are not directly comparable across the two groups. With that qualification in mind, Table 5 replicates the specification with both country and time fixed effects (P2, P5, and P8), by income group.[39]

There seems to be stronger evidence that political economy matters more in advanced economies (while estimates are uniformly positive, they tend to be measured with more noise for developing countries). This is surprising, since one typically expects political economy influences to matter more in developing nations. In contrast, there is just one case where financial access enters with a (highly) significant coefficient, although as was the case with government expenditure before, it carries a negative sign. Overall, while splitting our sample into the two income groups does little to alter the qualitative takeaways from our baseline results, the comparative importance of political economy among advanced economies comes across as somewhat of a paradox.[40]

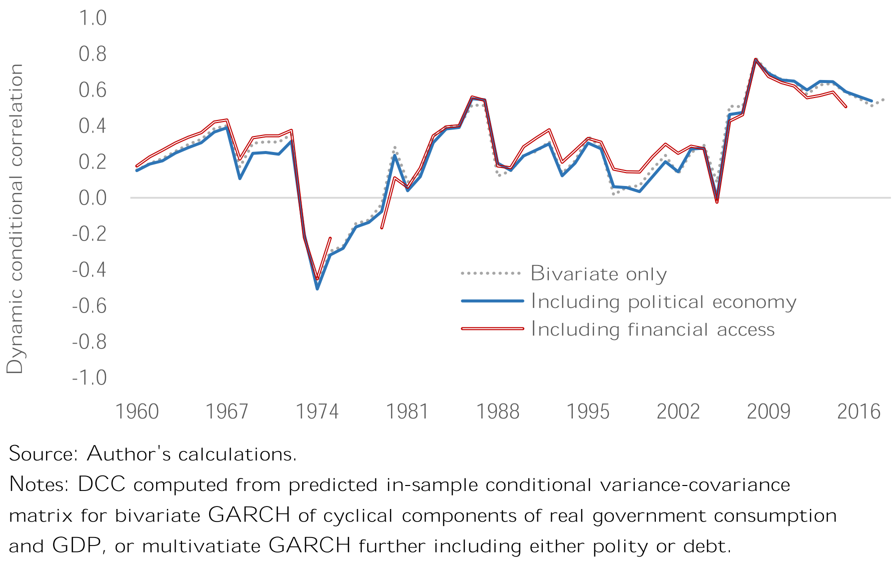
Yet it need not be. This result serves to partially validate theories of the political business cycle, albeit with some additional nuance. If political economy forces operate on fiscal policy mainly in high-income, democratic nations, then such pressures likely originate around the electoral cycle. This would then imply a comparatively tighter relationship between political and economic cycles in more democratic nations than one might expect from the full-sample results. While this is the case in our data, there is another possibility: since elections tend to occur at a higher frequency than business cycle movements, our results could also imply that such political influences could alternatively be giving rise to mini-cycles of activity around the broader business cycle. Such

[39] The observation count for the subsamples do not precisely match those for the full sample due to the need to drop singletons in the subsamples, which is a finite-sample corrections necessary in multi-way fixed effects models that also include multi-way clustering. Keeping these observations will lead to an underestimation of standard errors (Cameron et al., 2011).

[40] It is important to underscore the fact that the more significant coefficient on the political economy variable for fiscal procyclicality in advanced economies *does not imply* that procyclicality, *per se*, is pronounced in such economies (in fact, it is the opposite, as shown in Section 4.2).



(a) Finland



(b) Greece

Fig. 2. Dynamic conditional correlations for Finland (top) and Greece (bottom), 1960–2018, between the cyclical components of government consumption and GDP. In Greece, there is little difference in correlations after accounting for either political economy or financial access in a multivariate garch model, whereas significant separation in correlations occurs for Finland when financial access is included.

mini-cycles could still be procyclical, but need not be as tightly linked to just the frequency of the longer-duration business cycle.

Relatedly, the fiscal multiplier literature has repeatedly found that recessionary states tend to give rise to larger multipliers (Auerbach and Gorodnichenko, 2012b,2012a; Candelon and Lieb, 2013). We extend this line of reasoning to consider whether *procyclicality* responds more in a contraction. We restrict the sample to just the trough[41] and the year thereafter, and repeat the analysis, by income group. Despite the much smaller sample sizes involved, our qualitative findings are unchanged. However, the

[41] Trough turning points were identified algorithmically using the Bry and Boschan (1971) procedure.

Table 5
Panel regressions for fiscal procyclicality by income group subsample[a].

	Advanced			Developing		
	G_c (S1)	G_e (S2)	G_p (S3)	G_c (S4)	G_e (S5)	G_p (S6)
Polity	0.019	0.033	0.005	0.003	0.022	0.011
	(0.007)**	(0.012)**	(0.004)	(0.013)	(0.009)**	(0.013)
Debt	0.050	−0.046	−0.037	0.018	−0.011	0.010
	(0.034)	(0.045)	(0.009)***	(0.041)	(0.035)	(0.045)
Fixed effects:						
Time?	Yes	Yes	Yes	Yes	Yes	Yes
Country?	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (adj.)	0.481	0.775	0.442	0.428	0.472	0.498
R^2 (within)	0.022	0.008	0.020	0.001	0.009	0.007
Estimation	FE	FE	FE	FE	FE	FE
Errors	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
Ctry (yr)	20 (57)	17 (37)	23 (211)	24 (57)	24 (35)	17 (72)
Obs.	889	501	2396	985	578	779

[a] The dependent variable is the dynamic conditional correlation between the cyclical components of economic activity and government spending listed in the first row. All other variables are expressed using the inverse hyperbolic sine transformation and lagged one period. A constant term was included in all regressions, but not reported. Standard errors, clustered over country and year, are given in parentheses. Goodness-of-fit measures report the adjusted R^2 and within R^2. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

magnitude of the coefficients did increase relative to the full sample. This provides some limited validation that, much like fiscal multipliers, procyclicality appears to be more pronounced in a recessionary context (these results are available on request).

Moving on to developing countries, it is natural to question whether there is quantitative evidence of graduation from procyclicality among developing countries (Frankel et al., 2013). In Section 4.2, we have already offered some aggregate evidence that this phenomenon may not be all that widespread. Here, we examine the argument more systematically, by examining whether there has been any palpable change in procyclicality before and after the year 2000. Indeed, we do find some evidence of graduation, but only in a limited fashion. The positive coefficient for G_c falls in advanced economies and *even turns negative* in developing ones. And while a similar reversal occurs for G_p in high-income countries, we otherwise find little systematic evidence of widespread graduation. This underscores the fact that graduands from procyclicality still remain few and far between (these results are available on request).

5.2. Additional channels for political economy and financial access

Having exhausted these avenues with respect to different slices of the data, we move to adding additional variables that could offer some leverage to improving our understanding of the different channels by which political economy and financial access may operate. For the former, we now include corruption alongside polity; for the latter, we supplement debt with private credit.[42]

Corruption allows us to potentially separate the pressures emanating from electoral competition versus those due to special interest lobbying. Lobbying, whether through campaign contributions or via information delivery, can easily alter the trajectory of fiscal policy choices (Grossman and Helpman, 2001; Hillman, 2019). Lobbying pressures are typically most effective in the presence of corruption; including the level corruption measure into (6) thereby allows us to evaluate whether special interest politics matters, and entering corruption as an interaction permits the assessment of the extent to which the effects of political participation on procyclicality may be further conditioned by lobbying activity.

Since our focus on financial constraints is restricted to public debt, interacting this with its private counterpart allows us to evaluate if the *total* burden of debt is what matters for procyclicality (as opposed to just the public share). There is evidence that the growth rate of total national debt is what matters for growth (Lim, 2019), and this could well be the case for fiscal procyclicality. If crowding out is present, this could potentially mitigate an unchecked expansion of public-sector liabilities.

Table 6 produces these results. The direct effects of lobbying are positive (based on the statistically significant coefficient on corruption in column I3), which indicates that, *ceteris paribus*, special interest pressures facilitate procyclical fiscal choices. However, the negative coefficient on the interaction term implies that when such pressures are *greater*, they could mitigate the tendency of policymakers to veer toward procyclicality. Alternatively, more robust electoral competition can prevent special interest groups from channeling expenditures in a procyclical direction.[43] This suggests that the two could be institutional *substitutes*, and hence may (paradoxically) offset each other in their effort to influence the distribution of government spending. That said, even at the highest levels of special interest lobbying, the total effect of electoral competition on fiscal procyclicality never turns significantly negative.[44]

[42] We are hardly the first to consider the inclusion of these additional variables, which have appeared in the literature under various pretexts. For corruption, see Calderón et al. (2016) or Alesina et al. (2008); and for private credit, see Furceri and Jalles (2019).

[43] Either outcome is possible because fiscal procyclicality does *not* require that spending be directed toward the same beneficiaries; hence, special interest groups may benefit from procyclical spending in some cases, but the broader electorate may benefit in others.

[44] This result is easily verified visually, and a figure that does so is supplied in the appendix. The appendix also discusses the marginal effects of each, when evaluated at the respective means.

Table 6
Conditioning on additional political economy and financial access channels[a].

	Political economy			Financial access		
	G_c (I1)	G_e (I2)	G_p (I3)	G_c (I4)	G_e (I5)	G_p (I6)
Polity	−0.035	0.064	0.085	0.019	−0.173	0.003
	(0.032)	(0.028)**	(0.046)*	(0.008)**	(0.924)	(0.006)
Corruption	−0.063	−0.051	0.212			
	(0.061)	(0.060)	(0.074)***			
Polity × corruption	0.020	−0.036	−0.044			
	(0.020)	(0.016)**	(0.023)*			
Debt	0.016	−0.032	−0.076	0.471	−1.257	−0.047
	(0.043)	(0.027)	(0.035)**	(0.148)***	(0.986)	(0.067)
Pte credit				0.503	−1.119	−0.016
				(0.199)**	(0.959)	(0.075)
Debt × pte credit				−0.093	0.232	0.002
				(0.036)**	(0.189)	(0.016)
Fixed effects:						
Time?	Yes	Yes	Yes	Yes	Yes	Yes
Country?	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (adj.)	0.463	0.713	0.521	0.507	0.560	0.432
R^2 (within)	0.005	0.018	0.110	0.028	0.025	0.014
Estimation	FE	FE	FE	FE	FE	FE
Errors	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
Ctry (yr)	41 (32)	34 (32)	40 (27)	11 (57)	8 (37)	16 (141)
Obs.	1172	876	1023	598	262	1699

[a] The dependent variable is the dynamic conditional correlation between the cyclical components of economic activity and government spending listed in the first row. All other variables are expressed using the inverse hyperbolic sine transformation and lagged one period. A constant term was included in all regressions, but not reported. Standard errors, clustered over country and year, are given in parentheses. Goodness-of-fit measures report the adjusted R^2 and within R^2. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

Governments also appear to condition their procyclical choices on the pervasiveness of private debt. While more financially developed economies enable governments to pursue procyclical policy (as evidenced by the statistically significant sign on the credit variable in column I4), the negative and significant interaction term indicates that governments do restrain themselves—and exercise countercyclical fiscal policy—when the private sector is already heavily extended. Or, conversely, the private sector may internalize a Ricardian equivalence-type argument and reduce its borrowing when it observes that public-sector indebted is high. In either case, the two forms of debt appear to be substitutes, at least at the margin, in terms of their conditional effect on procyclicality.[45]

5.3. *Are fiscal rules a panacea?*

A relatively recent literature has emerged that has argued for the promise of fiscal rules as a potential solution to procyclicality (Heinemann et al., 2018). Analogous to the rules-versus-discretion debate in monetary policy, the premise here is that such constraints can curb governmental tendencies toward procyclical action. The resistance to such claims lies in the fact that the nature of fiscal policy—being a function of societal choices regarding resource allocation—means that it may be far less amenable to technocratic management than its monetary counterpart.

In this subsection, we briefly explore how fiscal rules may alter the conclusions within our framework of analysis. Table 7 includes a numerical score for the prevalence of different types of fiscal rules (first three columns), and the interaction of these rules with our two main regressors of interest (latter three columns).[46]

Unfortunately, the results presented in Table 7 do not lend support to the claim that the imposition of fiscal rules may be able to resolve the problem of procyclicality. For starters, the presence of fiscal rules, *ipso facto*, is associated with *greater* procyclicality, not less. And although this sign reverses when rules are further conditioned on both polity and debt, the conditional effect of rules on either turns out to further exacerbate, rather than diminish, the independent effects of these factors (the coefficient on the interaction term is positive when statistically significant).[47]

[45] As was the case previously, at no level of private credit does the total effect of debt on procyclicality turn significantly negative; a figure that verifies this fact visually is likewise provided in the appendix.

[46] The usual caveat here is that our sample suffers from significant attrition once we include this variable. We have also considered specifications that include the additional short-term controls from our panel baseline—at even greater detriment to our sample size—and the qualitative results remain essentially unaltered.

[47] One possibility is that these results are being driven by the countries within the European Monetary Union (EMU), which face a common-pool problem that exacerbates the temptation to engage in public deficit financing, thereby necessitating fiscal rules to ensure discipline (Detken et al., 2004). In this case, our results may be reflecting selection rather than causality. We consider whether the EMU countries are driving our results in two ways. First, we include an indicator variable for EMU economies (this sample retains non-EMU and EMU economies prior to entry), and examine whether the coefficient on this EMU effect is positive and significant. Second, we restrict our analysis to only EMU economies, and ask whether the conditional effect of the EMU and fiscal rules is positive and significant. In both instances, however, the EMU effect turns out to be statistically insignificant. These results are reported in the appendix.

Table 7
Accounting for fiscal rules[a].

	Rules only			Conditioned on rules		
	G_c (F1)	G_e (F2)	G_p (F3)	G_c (F4)	G_e (F5)	G_p (F6)
Fiscal rules	0.031	0.046	0.020	−0.043	−0.136	−0.049
	(0.024)	(0.014)***	(0.014)	(0.155)	(0.091)	(0.155)
Polity	0.017	0.026	0.031	0.016	0.024	−0.002
	(0.017)	(0.010)**	(0.031)	(0.018)	(0.010)**	(0.017)
Debt	0.035	−0.027	−0.106	0.018	−0.084	−0.100
	(0.075)	(0.040)	(0.057)*	(0.069)	(0.043)*	(0.051)*
Rules × polity				−0.002	0.012	0.065
				(0.009)	(0.003)***	(0.034)*
Rules × debt				0.018	0.033	−0.027
				(0.033)	(0.019)*	(0.028)
Fixed effects:						
Time?	Yes	Yes	Yes	Yes	Yes	Yes
Country?	Yes	Yes	Yes	Yes	Yes	Yes
R^2 (adj.)	0.471	0.734	0.359	0.470	0.735	0.371
R^2 (within)	0.014	0.041	0.054	0.015	0.050	0.075
Estimation	FE	FE	FE	FE	FE	FE
Errors	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered
Ctry (yr)	28 (29)	25 (29)	29 (26)	28 (29)	25 (29)	29 (26)
Obs.	760	604	712	760	604	712

[a] The dependent variable is the dynamic conditional correlation between the cyclical components of government spending and economic activity listed in the first row. All other variables are expressed using the inverse hyperbolic sine transformation and lagged one period. A constant term was included in all regressions, but not reported. Standard errors, clustered over country and year, are given in parentheses. Goodness-of-fit measures report the adjusted R^2 and within R^2. * indicates significance at 10 percent level, ** indicates significance at 5 percent level, and *** indicates significance at 1 percent level.

Although perhaps unexpected, these results should not be all that surprising. Others have found that compliance with fiscal rules tends to be tepid (Caselli and Reynaud, 2020; Reuter, 2015).[48] Moreover, there are potential empirical issues that we do not resolve that take us beyond the scope of this paper; for example, it is possible that different rules have different effects (Guerguil et al., 2017), and this is washed out by our aggregate approach. Alternatively, the endogeneity of fiscal rules means that a more careful approach is required to identify causal effects (Heinemann et al., 2018). Still, the fact that we uncover a positive effects of rules—in terms of both levels and interactions—indicates that a naive reliance on the presence of rules alone would be insufficient to deliver the sort of countercyclical actions that rational policymaking would call for.

6. Conclusion

This paper has offered an alternative approach to measuring the procyclicality of fiscal policy, using a dcc-garch model. This novel approach allows us to directly embed key explanatory factors—political economy and financial access—into the computation of dynamic conditional correlations between government spending and output.

We find that fiscal procyclicality owes more to political economy drivers, especially electoral pressures in advanced economies. In contrast, while we occasionally find that constraints due to limited financial access may matter, the evidence in favor of this explanation is more mixed. Moreover, the effects of political participation and the burden of public debt may be conditioned by other channels. In particular, we find that special interest pressures may serve as a substitute for electoral competition and hence condition the procyclicality of fiscal expenditures, just as high levels of private debt may end up offsetting the tendency toward excess public spending. Importantly, we are able to tease out these relationships despite a fairly wide variation in spatial and temporal coverage, which speaks to the overall strength of the results and lends credibility to our methodology.

Future work can exploit our time-series approach to expand the analysis to include additional, structural factors—such as social capital or productivity—that could further condition the cyclicality of fiscal policy. We leave such work, along with theoretical justifications of the mechanisms at play, to future research.

Declaration of competing interest

The author has declared all funding sources in the title footnote, and has no conflicts of interest to report.

Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.ejpoleco.2020.101930.

[48] The same paper finds, however, that *in spite of* selective compliance, rules nevertheless appear to alter the behavior of fiscal policy.

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