Unequal Political Business Cycles: Inequality, Policy Uncertainty and the Macroeconomy

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Abstract

This paper explores the existence of political cycles that are contingent on inequality. I claim that high inequality leads to high policy uncertainty during election years as pressures for redistribution increase at the same time that the wealthy become politically more powerful. This higher policy uncertainty harms the economy, resulting in a decline in GDP and the private components of aggregate demand. I explore empirically the presence of an unequal political business cycle (UPBC) using three different strategies. First, I implement a panel estimation in a group of 25 countries spanning four decades. Second, I use historical data for the US from 1947 to 2018 and 18 elections. In both cases I find evidence supporting the mechanism: GDP falls below its trend during an election only when inequality is sufficiently high. Similarly, elections are associated with a decline in the private components of aggregate demand and, for the US case, a spike in policy uncertainty, only when inequality is high. The third strategy runs estimations using microeconomic data from the PSID, which show that wealth-poor households decrease relatively more their expenditure rates during election years, evidence that is in line with elections impacting private consumption through policy uncertainty in times of high inequality.

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1 Introduction

Inequality has gained considerable academic as well as popular attention in the last years. The surge of interest has been pushed by a widening in income and wealth inequality in developed countries and the growing availability of micro data facilitating its economic and historical analysis. Despite this there are still few papers studying the connection between inequality and macroeconomic dynamics, particularly at the business cycles frequency.[1] In this paper I document empirically a specific connection using elections as an exogenous shock that triggers policy uncertainty depending on the level of inequality. This gives rise to unequal political business cycles (UPBC), which are characterized by a fall in GDP, private consumption and investment around elections, but only in times of high inequality.

In their classic paper Meltzer and Richard (1981) present a stylized model where rising inequality in democratic countries leads to votes for redistribution. Voters in countries depicting a large concentration of wealth and income may be pleased by politicians proposing higher levels of redistribution, hurting only the few that concentrate most of the resources in the economy. However rising inequality might boost the power of the rich, enabling them not only to counter the popular will but also to achieve even less redistribution through over representation in the political process (Benabou, 2000; Winters and Page, 2009; Gilens, 2012; Bartels, 2018; Epp and Borghetto, 2020).[2] Hence inequality moves away the desired policies of the different groups with political power, leading to a high degree of party polarization (Pontusson and Rueda, 2008; Garand, 2010; Grechyna, 2016; Duca and Saving, 2016; McCarty et al., 2016). At the same time pressures for redistribution on the one side, resistance to it on the other, and the failure to address the root issue, raise the gains for engaging in contentious politics, social discontent and unrest (Alesina and Perotti, 1996; Benabou, 1996), and intensify voters' distrust in established political parties and institutions (Benabou, 2020). Under these conditions inequality rises the odds of sharp swings in policies, makes political compromise more difficult, and undermines confidence in the ability of politicians to commit, heightening policy uncertainty, especially before general elections.[3]

Thereby inequality, through its consequences on socio-political instability, leads to higher policy uncertainty, which, in turn, affects the economy. The macroeconomic effects of policy uncertainty have been extensively studied. High uncertainty may have detrimental effects on economic activity due to adjustment costs in investments and hiring and firing decisions (Cukierman, 1980; Bernanke, 1983; Rodrik, 1991; Bloom, 2009), or due to the surge of precautionary savings by households (Ravn and Sterk, 2017; Den Haan et al., 2017; Bayer et al., 2019), among other channels, predictions that seem to be confirmed by empirical work (see e.g. Baker et al., 2016).[4] However causality from policy uncertainty to macro outcomes is difficult to identify because policy responds to economic conditions in a forward looking manner. In this dimension the paper can be understood as identifying an exogenous source of uncertainty, i.e. elections in times of high inequality, in order to measure its causal macroeconomic effects.[5]

The economic consequences of inequality due to socio-political instability have been studied in the economic growth literature. Among the first cross-country studies finding a negative relationship between inequality and long-run growth were Alesina and Rodrik (1994) and Persson and Tabellini (1994), with an interpretation that was in line with Meltzer and Richard (1981) hypothesis and the efficiency costs that redistribution inevitably would bring upon the economy.[6] However, in view of the lack of empirical evidence supporting a robust causal link between inequality and redistribution (Benabou, 1996), the focus changed, with Alesina and Perotti (1996) and Perotti (1996) claiming instead that rising socio-political instability was the main transmission mechanism.[7] Subsequent work confirmed not only the lack of a systematic response of redistributive policies to rising inequality, but also questioned the distortionary effects of these (see e.g. Berg et al., 2018, and references therein). In this paper I test the hypothesis that inequality hurts economic activity through socio-political instability, but in a different context. I explore short-run dynamics that are triggered by sharp increases in uncertainty due to decisive elections.[8] I exploit only within-country variation in inequality for a sample of mostly developed countries and find strong empirical support for it.[9]

According to this line of thought it is to be expected that in times of high inequality we should observe a recession around elections, everything else constant. The extensive literature on political business cycles (PBC) goes back to Nordhaus (1975) and Lindbeck (1976). Its focus has been on the incentives for an incumbent to appear competent ahead of elections and the political rewarding of preelectoral booms. Therefore it tries to explain the presence of economic expansions before elections, emphasizing the role of stabilization policies in generating them. Differently from this literature I focus on uncertainty and hence the occurrence of recessions around elections, with no specific role for stabilization policies. There is also a focus on the macroeconomic effects of policy switches due to partisan motivations, which lead to post-electoral business cycles. Unlike the UPBC, there may be a boom or a recession after the election depending on the identity of the winner party. In Azzimonti and Talbert (2014) these are generated by fiscal policy, while in Alesina (1987) and subsequent related work, are due to unexpected changes in monetary policy.[10] Among these the UPBC is most closely related to Azzimonti and Talbert (2014), since the post-electoral policy swings generating business cycles in their work generate the policy uncertainty causing the UPBC.

Canes-Wrone and Park (2012) and Julio and Yook (2012) provide evidence supporting an alternative view of PBC.[11] Julio and Yook (2012) find that corporate investment falls the year of an election in a sample of 48 countries between 1980 and 2005. Canes-Wrone and Park (2012) find a pre-electoral decline in private gross fixed capital formation using a panel of ten OECD countries between 1975 and 2006. These papers don't consider inequality as a conditioning variable, the focus of the present paper. Canes-Wrone and Park (2012) condition in political polarization, one of the different channels linking inequality and policy uncertainty in my framework. Accordingly I show

that political polarization is correlated with inequality, and that it can explain a fraction of the costs associated with the UPBC. However, polarization doesn't have any explanatory power beyond the one contained in inequality.[12] Moreover, not only investment is shown to be affected by elections in times of high inequality but also GDP, private consumption and policy uncertainty. Regarding policy uncertainty, although it is pointed out as the mechanism behind their results, Canes-Wrone and Park (2012) and Julio and Yook (2012) don't give direct evidence on it.[13] Regarding private consumption, this finding, together with a more detailed exploration using microeconomic data, allows the inclusion into the PBC literature of the mechanisms generated by household's precautionary savings, which extend the effects of policy uncertainty beyond those related to irreversible investments.[14]

Both macroeconomic and microeconomic data are used in this paper to test the UPBC hypothesis and the possible mechanisms behind it.

First I implement panel estimations with country and time-fixed effects for a group of 25 mostly advanced countries and four decades.[15] When not conditioning by inequality macroeconomic aggregates do not differ significantly from trend values during elections.[16] But results change when conditioning by inequality. In this case I find statistically significant differences between times of low and high inequality. When inequality is sufficiently high, GDP, private consumption and investment start falling below trends four or three quarters before the election takes place, and recover not before four quarters after it. Quantitatively the effect is large. A raise of one standard deviation in within-country inequality leads to a maximum fall of about 0.6% of quarterly GDP, which happens in the election quarter. Taking into account all of the quarters GDP is below its trend the total cost is around 1% of annual GDP. If a country goes from the 5th percentile of within-country inequality to the 95th percentile, the costs are three times as large, about 1.75% of quarterly GDP and 3% of annual GDP, respectively. I find that both private consumption and investment behave similarly, with falls that are proportional to their standard deviations, but there are no significant differences in the behavior of public spending. Results remain strong when dropping snap elections, and are robust to different measures of inequality, the inclusion of linear trends and changes in the sample. Results persist when restricting to advanced, parliamentary or richest countries, and the effects are still large and significant irrespective of the identity of the incumbent party or the one winning the election. Finally I show supporting evidence that political polarization and protests mediate the relationship between inequality and the socio-political instability that generates the fall in GDP.

Then I exploit recently constructed historical series of US policy uncertainty and wealth inequality spanning from 1947 to 2018, which includes 18 presidential elections. The findings support the existence of UPBC, not only in analyzing the behavior of macroeconomic aggregates but also policy uncertainty. In particular I find no significant unconditional effect of elections in these variables, but I do so when conditioning on wealth concentration. When inequality is relatively high

policy uncertainty starts to increase two quarters before an election and reaches a peak in the election quarter above two standard deviations. GDP starts to fall one quarter before the election and achieves its lowest level three quarters after the election. In this specification the effect is also sizeable. A raise of one standard deviation in inequality results in a maximum gap between quarterly GDP and its trend of 0.9% of quarterly GDP, and a total cost of 1.4% of annual GDP. Moving from an election when inequality was at the lowest level, observed in 1978, to the one with the highest level, observed in 2015, generates a total loss of more than 5.5% of annual GDP. In terms of macroeconomic aggregates I find that, similarly to the pattern found for GDP, both private consumption and investment fall around elections under high inequality. Again, the fall in each of these variables is proportional to its standard deviation. These results are not driven by the 2008 election, which coincided with the Great Recession and relatively high wealth inequality. Results are driven by medium and long-term changes in inequality, and they are robust to the inclusion of a trending effect in elections, lowering the likelihood that the estimations are capturing some alternative slow-moving explanatory variable. Although the effects are significant under different measures of wealth and income inequality, it is the fraction accumulated by the very rich, i.e. 1% of the population, the one that is more relevant in the results.

Results from these macroeconomic specifications are in line with the existence of a UPBC. One interesting result is the significant fall in private consumption, although it is not possible to conclude whether this is due to the direct effect of political uncertainty, or an indirect effect through income. To shed some light on this the third exercise I perform in this paper makes use of microeconomic data on consumption, disposable income and wealth to explore the existence of a precautionary response of households to elections when inequality is high. Using data from 2005 to 2017 I estimate panel fixed-effects regressions and find that, during election years, the change in the expenditure rate of relatively wealth poor agents falls significantly, with a magnitude that is as large as 70% of a standard deviation in the group of poorest households.[17] In contrast the estimations show no significant change in expenditure rates of wealth-rich agents. The finding that consumption falls more than income, and that this happens exclusively in wealth poor families, support the idea that the UPBC is generated by policy uncertainty, not only affecting firms but consumers as well.

The panel estimations are presented in Section 2, the US time-series estimations are presented in Section 3, and the US microeconomic evidence is presented in Section 4. Section 5 concludes.

2 Macroeconomic Evidence I: Panel Estimation

2.1 Panel Estimation: Data and Empirical Specification

I consider the following specification for exploring the dynamic effects of elections conditional on the level of inequality in a panel of countries:

$$\begin{aligned} x_{tj} &= \alpha_j + \delta_t + \beta_1 x_{t-1j} + \sum_{l=-Lg_e}^{Ld_e} \beta_{2l} e_{t+lj} + \sum_{l=-Lg_e}^{Ld_e} \beta_{3l}(e_{t+lj} \times (i_{t+lj} - \bar{i}_j)) \\ &+ \sum_{l=-Lg_e}^{Ld_e} \beta_{4l}(e_{t+lj} \times \bar{i}_j) + \beta_5 i_{tj} + \epsilon_{tj} \end{aligned} \tag{1}$$

where subindexes j and t denote country and quarter, respectively. The dependent variable x is quarterly GDP or a domestic demand component, seasonally adjusted and detrended using the HP filter. The specification contains both country and period fixed-effects, denoted by α_j and δ_t, respectively. The first lag of the dependent variable is included to better capture the dynamic effects of elections.[18] The variable e_{tj} is an indicator that takes the value of 1 when there is an election in period t and country j. Then, the coefficients β_{2l} capture the unconditional effect of elections on the variable of interest, from Lg_e periods before the election to Ld_e periods after the election. The following term in equation (1) captures the interaction with inequality. The variable i_{tj} is a measure of income inequality for period t and country j, and $\bar{i}_j$ is the country average for the entire period. Therefore the coefficients β_{3l} capture the additional effects of elections due to inequality, but only taking into account variations of inequality within countries.[19] Note that i varies with l so any short-run variation in inequality, which might be influenced by elections or the business cycle, is not taken into account when estimating β_{3l}. This can be seen more clearly considering that $(i_{t+lj} - \bar{i}_j)$ only influences the regressor corresponding to β_{3l} when there is an election in $t + l$ (hence e_{t+lj}=1). The last two independent variables are included to control for average inequality and inequality in periods of no elections. Finally ϵ_{tj} is the residual term.

Throughout this paper I will be careful about controlling for the Great Recession initiated in 2008. In the case of the panel estimations the time-effect is insufficient if countries were affected differently, or if the effect was different in countries going through electoral processes. To eliminate this possibility I don't consider any election taking place during the years 2008 and 2009 in the estimations, setting $e_{tj} = 0$ for that period. This leaves 12, from a total of 154 elections, out of the estimations.

For macroeconomic aggregates the source of the data is the The International Financial Statistics (IFS), from the International Monetary Fund.[20] For inequality I use data from the Luxembourg Income Study Database (LIS), which report inequality indicators from harmonized microdata collected from a cross-section of countries and various decades. Since they come from microeconomic surveys these indicators are available with a variable periodization, typically once every 3 or 5 years.

To obtain the value of inequality in each election year I interpolate the series annually.[21] LIS publishes different inequality indicators. I use the 90/10 percentiles ratio, as it is the one that better captures inequality at the extremes of the distribution. Since those at the top are the most influential in supporting parties opposing redistribution it should better capture the UPBC (Winters and Page, 2009). Below I show how results change when using the gini index. Data for elections comes from updates of Bormann and Golder (2013), complemented with data from internet sources for years that are not available. I consider only presidential elections in presidential systems and only parliamentary elections in parliamentary systems. I restrict the sample to elections in countries with high democracy scores. To do this I use the Polity IV dataset, and only include years with a polity index, which ranges from -10 to 10, above 5. Finally I keep countries with at least 10 years of consecutive observations.[22] Table 1 lists the countries included in the baseline estimations, the time period and the number of elections considered for each. Most of the countries are developed and parliamentary, the earliest data is from the 70s and spans from 10 to more than 40 years, and the number of elections per country ranges from 2 to 14.

2.2 Panel Estimation: Baseline Results

I first show the results using GDP as the dependent variable. Before conditioning on inequality I estimate equation (1) only with election dummies and fixed and time effects, to explore the existence of a PBC in the sample. Based on this specification the left-upper panel of Figure 1 shows the predicted path for GDP around an election quarter $t = 0$, together with bootstrapped 90% confidence bands.[23] As can be seen in the graph deviations from trend GDP are small (below 0.1%) and not statistically significant. Hence there is no evidence of PBC in the sample.

Next I turn to the estimation of UPBC introducing the interactions with income inequality specified in equation (1). Note that the existence of the UPBC implies that the coefficients on these interactions are negative and significant. I report the value and significance of these in the first column of Table 2. To save space I don't report the rest of the coefficients included in equation (1). I include seven different dummies, one for each of the five quarters before the election, the quarter of the election and the quarter after it.[24] The coefficients are negative and statistically significant, at least at the 95% confidence level, in every quarter during the year previous to the election. This evidence is in line with the existence of an UPBC: the path for GDP around elections under high inequality is below the path around elections under low inequality.

To get a better sense of the size of the effect I normalize the inequality variable $ii_{tj} - \bar{ii}_j$ in such a way that a value 1 corresponds to a benchmark of high inequality, and a value of 0 to a benchmark of low inequality. Note that these benchmarks are both set in terms of deviations from the country's average level of inequality, so they correspond to measures of within-country inequality.[25] As benchmarks I take the 95th and 5th percentile of the variable $ii_{tj} - \bar{ii}_j$ in the sample.[26] Therefore the coefficients in Table 2 can be interpreted as the GDP loss during the corresponding quarter,

when a country reaches the high inequality benchmark, relative to the level of GDP observed in the low inequality benchmark. However this number doesn't include the dynamics arising from the persistence in GDP. To include this dynamic effect I compute impulse-response functions for each of the benchmarks and obtain the maximum difference between the two, which I report in the eight row of column (1) in Table 2. Additionally I report in the ninth row the sum of the differences between the two impulse-response functions to obtain an estimation of the total loss during the entire election period. These rather extreme values for the high and low inequality benchmarks are picked to highlight the costs of high inequality, as some of the countries in the sample had achieved levels of inequality above the high inequality benchmark at the end of the sample, notably Spain and the US. To see what the estimations imply for smaller differences I additionally include in Table 2 the maximum and the accumulated effect when using the 25th and the 75th percentiles of within-country inequality in the tenth and eleventh rows, respectively. I also discuss below the size of the effects when considering differences in terms of standard deviations of within-country inequality.

These statistics show that, relative to the benchmark of low inequality, quarterly GDP falls a maximum of 1.76% during an election under high inequality. This is very close to the quarterly GDP standard deviation in the sample. The total loss in GDP is almost 12% of quarterly GDP, or around 3% of annual GDP. These estimations are statistically significant at a 99% level. When using the alternative benchmarks of 25th and 75th percentiles of within-country inequality, the effects decrease to about one third. Since the specification used is linear in inequality results are proportional to the change in inequality. Take for instance a variation in inequality equivalent to a one within-country standard deviation. This is 0.32. Then the difference would be one third of the difference reported for the benchmark: in a country where inequality increases by a one within-country standard deviation, quarterly GDP will fall a maximum of 0.58% relative to the initial situation, and the total lost would be 1% of annual GDP. Figure 2 shows the maximum effect for alternative differences in inequality, scaled by the within-country standard deviation.

The entire difference between the time paths under the two benchmarks, which where used to compute the mximum and total differences in Table 2 is depicted in the middle-upper panel of Figure 1. As decribed above, the left panel shows differences in time paths between periods with and without elections. Under Equation (1) this time path depends on inequality. It can be evaluated at the low and high inequality benchmark levels. The difference between the two is what I plot in the middle panel. This difference starts being negative 4 quarters before the election and remains like this for at least 7 quarters. The minimum value is achieved the quarter of the election. In the upper-right panel of Figure 1 the two time paths are shown separately. There we can see that under the low inequality benchmark GDP remains close to trend GDP, without significant deviations. Thus the results described above are due to the path under high inequality, which shows a persistent fall until the quarter of the election.

2.3 Panel Estimation: Demand Components

In this subsection I show results from estimating equation (1) using aggregate demand components as dependent variables. Specifically I explore the existence of an UPBC in private consumption, public expenditures and investment. Coefficient estimations are presented in columns (2) to (4) in Table 2. I keep the number of lags and leads of the election dummy used in the GDP estimations. Both private consumption and investment (columns 3 and 4) depict negative and significant coefficients. The size of the effect is large. When taking into account the dynamics of each component the maximum distance between the high and low inequality benchmarks are 1.96% and 6.35% of quarterly consumption and investment, respectively. When comparing these numbers to the standard deviations of each of the variables results are proportional to them, consumption falls a maximum of 1.09 standard deviations while investment does so a maximum of 1.11 standard deviations. These numbers are about 10% larger than for GDP. The total loss is 3.2% of annual consumption and almost 10.2% of annual investment. Public expenditures show a different pattern. There is only one significant coefficient, that is positive, four quarters before the election. Afterwards coefficients decrease in size and become negative after the quarter before the election, but the overall effect is not significant. This is a sign that fiscal policy is not behind the UPBC found in GDP and private demand components.

In Figure 1, below the paths for GDP, are shown those corresponding to each of the aggregate demand components. On the left I show first the time paths function when not conditioning in inequality, and it can be seen that none of the variables show significant deviations relative to their trend values. Then, in the middle, the differences between the high and low inequality benchmarks are plotted. As in the case of GDP, both consumption and investment (second and third rows, respectively) become negative and significant four quarters before the election. The lowest levels are achieved between the election quarter and one quarter before, and the difference remains negative and significant at least two quarters after the election. In both cases the results are due to a fall in times of high inequality, as can be seen in the right panels, which depict the impulse-responses for the two benchmarks separately. In the case of public expenditures (last row) we observe a positive and significant difference during the quarters before the election. But this pattern is explained by a lower spending in times of low inequality rather than higher spending under high inequality, as shown in the right panel. Spending under high inequality becomes negative and significant after the election, probably affected by the fall in GDP.

Hence we see that private consumption and investment decline around elections only when inequality is sufficiently high relative to the country's average. Although it is not possible to identify whether this is caused directly by inequality, or indirectly through the decline in income, these estimations may shed light on the mechanisms behind the UPBC. In particular the strong decline in consumption and investment may be the outcome of political uncertainty in line with the literature revised in the introduction. This decline in aggregate demand may lower GDP, and

this in turn may affect public expenditures.[27]

2.4 Panel Estimation: Sensitivity Analysis

In this subsection I modify the baseline specification in different dimensions to assess the robustness of the results just presented. For brevity I focus only in GDP. All of the results are reported in Table 3, together with the baseline estimation which is presented in column (1).

One concern with the results shown so far is the potential endogeneity of elections. Most of the countries in the sample have parliamentary systems that allow for snap elections. If the occurrence of these elections is correlated with the economic cycle then the estimations may be biased. Note however that in the UPBC, and unlike the traditional theories explaining the PBC, this correlation needs to be contingent in inequality. In order to bias the results, if snap elections are called when GDP is below (above) trend, then they have also to be called more frequently in times of higher (lower) than the average inequality. To be sure the result is not driven by this type of elections I estimate equation (1) including only electiones that were scheduled with at least one year in advance. In the rest of the elections I set $e_{jt} = 0$. Results are shown in column (2) of Table 3. Only 106 elections, out of 142, are included in this case. Results are very similar. Although the maximum fall is now larger (2.37% vs 1.76%), the overall effect is almost the same (11.1% vs 12%). Additionally in the third column of Table 3 I only keep countries that have never had a snap election during the period analyzed. These are only 10 countries, and only 43 elections are considered. Although the significance of some of the coefficients fall, as expected due to the smaller sample, the maximum and total effects are still similar than the baseline, and highly significant. Therefore these exercises show that it is unlikely that the results are driven by endogenous elections.

Next I use the gini index instead of the 90/10 income percentile ratio. Results are presented in column (4). Although results are still highly significant the magnitude of the effect is about 20% lower than in the benchmark. This suggest that inequality at the extremes is relevant for explaining the results. Then, to explore if filtering the dependent variable impacts the results I ran the baseline regression but using the growth rate of GDP. Results are presented in column (5). Under this alternative specification the drop in GDP is larger and more persistent, which may suggest that the effect of elections on GDP may have a longer duration than the length of the cycle assumed by the HP filter.[28] However, to be conservative and make results comparable with the macro literature, I keep the HP detrended series as the baseline. Next, in column (6) I introduce a linear trend interacting with the set of election dummies, in case results are rather capturing some time variation in PBC spuriously correlated with inequality. The size and significance of the results are mostly unchanged. The same happens when I consider elections during the years 2008 and 2009 (column 7), which are not included in the baseline estimations. Again results are unchanged. Finally, in columns (8) and (9) I restrict the sample to countries with at least 15 and 20 years of data, respectively. The total effect falls slightly, between 15% and 25%, and its significance remains

high, despite the reduction in the number of observations.

The next exercise is to estimate equation (1) for sub-groups of countries to explore potential characteristics behind the results. Again I restrict the analysis to GDP to save space. Results are presented in Table 4, again with the baseline specification appearing in column (1) for comparisons. First I redo the estimations keeping only countries with a parliamentary system. Results, presented in column (2), remain significant and very similar than in the baseline. Second I drop the 5 non-advanced economies included in the sample, as defined by the IMF, and results remain very similar, with a slight reduction in the total effect (column 3). Results remain unchanged as well when dropping the 5 transition economies included in the benchmark estimations (column 4). Since the sample is mainly composed by developed countries these alternative specifications only discard at most 5 countries. To make a larger adjustment in the last column I only keep the 15 richest countries in terms of average GDP per capita. Despite the reduction in the sample size results remain significant, although now the effect is smaller. The maximum fall is about 80%, and the total effect is 74%, relative to the baseline estimates.

Overall the effects are robust to different specifications and different sub-samples. The most important variation occurs when using the gini index instead of the 90/10 income percentile ratio, which may be a sign that inequality at the extremes is more important, and when only including the two-thirds richest countries, which may respond better to inequality-driven political conflict

2.5 Panel Estimation: Transmission Mechanisms

I have shown so far evidence supporting the existence of a UPBC in a panel of mostly developed countries. In times of high inequality, and only then, GDP, private consumption and investment fall around elections. What is behind this pattern? As I have argued in the introduction these estimates are consistent with the commonly accepted idea in the long-run growth literature pointing out to the causal link between inequality and socio-political instability. When applying this logic to the short-run functioning of democracies it suggests that, through turning transfers of political power into decisive moments, inequality intensifies policy uncertainty around elections, exerting negative effects on economic activity. In general the patterns obtained so far are in line with this idea. Before the election policy uncertainty depress aggregate demand. If both a raise or a cut in taxes are likely in the future then firms stop investing. This is in contrast to the case when only one of these policies are expected. If only a cut in taxes is expected then firms increase their investment. Then, is it possible that the effect rather come from an expectation of a leftist turn in policies? This would be in line with the early hypothesis in the growth literature, which was subsequently abandoned as discussed in the introduction. In the present setting, although the response of consumption may go against this possibility, the pattern found before an election may be consistent with that idea.[29] However it is inconsistent with the patterns after the elections. If a raise in taxes is repeatedly expected then it should materialize, at least partially. But the results

show that the effects dissipate after the elections, which is consistent with both left and right-wing policies averaged out across countries and elections.[30] In this sense my results are complementary with polarized business cycles a la Azzimonti and Talbert (2014), specially if polarization is one of the political consequences of inequality. Notwithstanding their focus is on post-election partisan business cycles, while mine is on their negative implications before the elections.

To verify that the results are not fully dependent on the identity of the party in power or the party winning the election, I use data on their political orientation from the Database of Political Institutions (Cruz et al., 2020). They report data on the orientation of the executive's party with respect to economic policy, i.e. whether it is left, center or right-wing oriented. Table 5 shows the results, including in column (1) the baseline estimation for comparisons. Columns (2) and (3) show the results when differentiating by the orientation of the party in power before the election. Although we can observe a larger effect when a left-wing party is in power, the differences are relatively small and the effects are still large and very significant when a right-wing party is in power, although the estimation becomes less precise. Columns (4) and (5) include the cases when differentiating with respect to the orientation of the party winning the election. A similar pattern emerges, with some differences but the effect being large and highly significant in every case[31] An additional piece of evidence suggesting that the results found so far are due to policy uncertainty rather than to a leftist turn in policies is the lack of correlation between inequality and the identity of the party in power.[32]

If we want to go deeper in the exploration of the transmission mechanism surrounding UPBC, policy uncertainty would be a very important variable to analyze. To my knowledge there is no cross-country panel-data study using explicitly a measure of it, either as an explanatory or dependent variable, probably due to the lack of a good proxy that is comparable across countries. Recently Baker et al. (2016), together with other authors, have constructed a proxy of policy uncertainty using the news approach for a group of 26 countries, with only 14 of them appearing in my sample. When using this variable replacing GDP as a dependent variable I don't find significant effects though. Very likely the small sample size together with the noisy nature of the uncertainty proxy influence this result.[33] However in the next section I show significant results when only analyzing data for the US, the country for which Baker et al. (2016) build a much longer historical series of policy uncertainty.

I explore now the role of other variables that have an essential influence in the transmission mechanism, particularly on the link between inequality and socio-economic instability, and for which there are proxies available for a large group of countries. The first of these variables is political polarization, which has been widely analyzed in previous work. Canes-Wrone and Park (2012) is closely related to this paper since it shows drops in investment around elections when polarization is high, in a sample of 10 OECD countries between 1975 and 2006. I construct a proxy for this variable using data from the manifesto scaling by Lowe et al. (2011), who provide

estimates of parties' positions in electoral years. I follow Canes-Wrone and Park (2012) to construct the polarization proxy, using the variable "state involvement in the economy" and measuring it as the absolute difference between the major left and right parties.[34] The second variable proxies for social unrest. Since I work with a sample of democratic and mostly developed countries I use large but non-violent protests from The Mass Mobilization Data Project (Clark and Regan, 2016).[35] Since I claim that these variables mediate the effect of inequality on policy uncertainty and macro aggregates I proceed in two stages. First I test if they are correlated with income inequality. Second I explore whether these variables can explain a fraction of the results for the UPBC, replacing the income inequality variable in equation (1) by each of them, one at a time. It is important to remark that these exercises are only illustrative since these variables, that mediate the relationship between inequality and policy uncertainty, are endogenous, most likely in the second of these stages.

Table 6 displays the results from the first exercise. Time and country-fixed effects are included in both specifications.[36] The regression for political polarization appears in column (1). Since polarization is only recorded during electoral periods each observation corresponds to an election-country pair, which explains the relatively low number of observations. The estimated coefficient is positive and highly significant at the 99% level. This is consistent with previous work, such as McCarty et al. (2016), Garand (2010), Grechyna (2016), Duca and Saving (2016) and Pontusson and Rueda (2008), although none of them estimate this relationship using a panel of countries controlling for country fixed-effects. The estimated effect is large. Both variables are normalized by their within-country standard deviations, meaning that a difference of such a magnitude in inequality is associated with 1.45 within-country standard deviations of political polarization. Turning to protests, column (2) presents a relationship which is statistically significant as well, this time at a 95% confidence level.[37] The coefficient implies that an increase in inequality of one within-country standard deviation raises the probability of a large non-violent protest in 1.6%, or slightly above 6.5% in a year. This result is novel since, although theoretically inequality has always been considered as a determinant of protest, the empirical literature so far has produced ambiguous results (see Justino and Martorano, 2019, and references therein). I'm not aware of a panel estimation with time and fixed-effects finding a positive and statistically significant association between inequality and non-violent protests.

The results from the second exercise are presented in Table 7. This is, whether elections are characterized by a fall in economic activity only when either political polarization is high or protests are more common. I employ the same specification than in equation (1), only changing income inequality by each of these variables.[38] To save space I don't show the coefficients for each quarter, only de maximum and accumulated effects. In the upper panel I present the baseline case with inequality as the interaction variable for comparisons. The middle panel shows the results when using political polarization instead. Although at a lower level than in the baseline, the effects are statistically significant for GDP, private consumption and investment, the same variables for

which the effect is significant when using the interaction with inequality. Note that this result is stronger than the one found by Canes-Wrone and Park (2012), since they estimate significant results for investment only.[39] Comparing the effects when moving from the 5th percentile to the 95th percentile of within-country inequality and political polarization, the size of the accumulated effect is about one-half in the second case for each macroeconomic aggregate. The lower panel displays the results for protests. The pattern in this case is similar, although it looks like the effect on investment and GDP, which is between one-third and one-half the baseline effect of income inequality, it is stronger than for private consumption.[40] Finally I introduce political polarization and protests, both interacting with elections, as additional controls in the baseline estimations and results are practically unchanged. This implies that variations in these variables that are orthogonal to inequality are not relevant to explain the results.[41]

This analysis sheds light on the mechanisms behind the UPBC. Although not definite, it gives supporting evidence that political polarization and protests are symptoms of an underlying high income inequality, and that these variables might be mediating the relationship between inequality and socio-political instability, leading at the end to increasing policy uncertainty and a fall in macroeconomic activity around elections found in this paper.

3 Macroeconomic Evidence II: US Time-Series Estimations

In this section I test the existence of a UPBC in the US. I focus in this country for several reasons. First the data necessary for the estimation is available for a long period of time. Specifically I use data for 70 years and 18 presidential elections. An important advantage is that one of these series is an index of policy uncertainty, which can be used to better explore the mechanisms behind the UPBC.[42] There is also an expanded availability of inequality measures. There are measures of wealth as well as income inequality, accumulated by both the 1% and the 10% of the richest individuals. Moreover the US has a presidential system and periodical elections that have been taking place every four years without exceptions. It also has had the maximum democracy score since the beginning of the sample. An additional advantage is that inequality has had large and persistent swings during the sample period, an appropriate variation to test the UPBC.

3.1 Time-Series Estimation: Data and Empirical Specification

I slightly modify equation (1) to obtain the following specification for exploring the dynamic effects of elections conditional on the level of inequality in a time-series setting:

$$x_t = \beta_0 + \beta_1 x_{t-1} + \sum_{l=-Lg_e}^{Ld_e} \beta_{2l} e_{t+l} + \sum_{l=-Lg_e}^{Ld_e} \beta_{3l}(e_{t+l} \times i_{t+l}) + \beta_4 i_t + \epsilon_t \tag{2}$$

Because it is only one country there is no j index nor time effects, and inequality doesn't need to be adjusted by its sample average. As before the inequality indicator is kept constant around

elections to isolate the estimations from any potential causal effect of elections on inequality. Again the coefficients of interest are those in β_{2l} and β_{3l}. These, together with β_1 allow to study the dynamics of the variables of interest before and after elections.

Like in the panel estimations, I add to this equation dummy variables to ensure that results are not driven by the 2008 election, which coincided with a large drop in GDP in a time of relatively high inequality, but for reasons that may be not related to the election. I add Lg_e+1+Ld_e dummy variables, each taking the value 1 in only one of the same number of quarters considered in (2), but only around the 2008 election. I also add as an explanatory variable the growth rate of oil prices and its lags. Unlike the 2008 dummy variables, which reduce the size of the UPBC, oil prices don't affect much the results, but they allow for slightly more precise estimations. Besides this I try to keep the specification as parsimonious as possible, without including additional controls in the baseline estimations.

There are different measures of inequality that can be used in this analysis. For the baseline estimations I employ the fraction of wealth accrued by the top 1% of the population, which is published by WID. The top 1% may give a better indication of economic concentration, and hence of the potential over-representation of the richest in the political process, and wealth is more relevant to political power than income (Winters and Page, 2009). I show below results with alternative inequality measures. Figure 3 exhibit the series used in the estimations, from 1947 to 2016.[43] It is worth noticing that wealth concentration at the top has not behaved monotonically during the period analyzed. Raising shares are observed only since around 1980, when they bottomed out after a sustained decline that started around 1960. From the beginning of the sample until that year it remained relatively stable. This U-shaped pattern is relevant for the interpretation of the results since otherwise the estimations may be capturing some trending factors not necessarily related to inequality. Nevertheless I show below that results are not modified when including linear trends interacting with election dummies, and that they are reduced when only considering a binary variable indicating high or low inequality.

As dependent variables I employ, as in the panel estimations, GDP and aggregate demand components at a quarterly frequency. For these I use the real seasonally adjusted indexes published by BEA, which I detrend using the HP filter. Additionally I include the index of political uncertainty (PU) as a dependent variable, constructed by Baker et al. (2016). This reflects the frequency of articles in six major U.S. newspapers that contain ceratin combinations of terms related to PU. I use the cyclically adjusted component using the HP filter to clean for trending factors.[44]

3.2 Time-Series Estimation: Baseline Results

First I explore whether there are systematic differences in the behavior of GDP around elections, without taking into account the effect of wealth inequality. The left panel of Figure 4 shows the corresponding path for GDP together with 90% confidence bands. Period 0 is the election quarter,

and I present the predicted series starting two quarters before the elections and for a total of nine quarters. It can be seen that the impulse-response of GDP never takes values that differ significantly from trend. Therefore I don't find evidence of a PBC in the US in the last 70 years.

Now I turn to the results when conditioning on wealth inequality. Column 1 of Table 8 shows the estimated values and significance levels of the coefficients on the inequality and election dummies interactions. The first negative and significant estimated coefficient is observed during the quarter of the election. Subsequently the values remain negative for at least three quarters after the election, with three of them significant at least at the 10% level. Hence the time-series evidence for the US shows the same pattern observed in the panel estimations, that GDP falls around the election when inequality is sufficiently high. The main difference in this case is that the fall in GDP seems to happen closer to the election quarter. The eight and ninth rows present the estimates of the maximum and total difference between the high and low inequality benchmarks. Both estimates are significant at the 99% level. Since in this case I work with only one country I choose the highest and lowest levels of inequality observed in the data to define the benchmarks. These values are 21% and 36%, recorded in 1978 and 2015, respectively. The estimates imply that the maximum difference in GDP between those two extremes during an election is 3.4% of quarterly GDP, and that the total difference when taking into account every period is about 23% of quarterly GDP, or more than 5.5% of annual GDP. As in the panel estimations the effect is linear in inequality. The standard deviation of inequality is a quarter of the difference used as benchmark. Thus an increase in wealth inequality of one standard deviation results in a total loss of about 1.4% of annual GDP, and a maximum drop of almost 0.9% of quarterly GDP. These estimates are larger than the ones found in the panel, even when expressed in terms of the quarterly standard deviation of GDP, which was 1.6% during the sample period.[45]

The upper-middle panel of Figure 4 shows the relative path for GDP around elections between times of high and low inequality. As expected from the results just presented, GDP differs significantly from trend around elections. GDP starts falling one quarter before the election, and becomes significantly different from zero on the election quarter, remaining as such for the following seven quarters. It reaches its lowest level in the third quarter after the election. The upper-right panel of Figure 4 shows the two impulse-responses separately. There we can see that part of the difference between periods of low and high inequality is due to a higher-than-trend level of GDP in times of low inequality, something not found in the panel estimations. Both the maximum loss in the high inequality benchmark, and the maximum gain in the low inequality benchmark, are about 2% of quarterly GDP. Hence the existence of a UPBC in the US, which is about the difference between times of low and high inequality, would have been hiding the existence of an office-seeking UPC.

As already noted an advantage of focusing in the US is the availability of a long series capturing political uncertainty, a key ingredient in the mechanisms behind the UPBC. For ease of interpretation I normalize the PU index by its standard deviation. As in the case of GDP I first

explore the existence of an unconditional change in the PU index during an election. This is shown in the lower-left panel of Figure 4. As it is the case with GDP, there is no significant effects for this variable either. It goes up the quarter of the election, but the effect is very small and not significant. However, when conditioning on wealth inequality results change. The second column of Table 8 shows the estimated coefficients. I adjust in this case the number of lags and leads for the election dummy because the significant coefficients appear before, which would be consistent with PU causing the fall in GDP. The first coefficient that is positive and significant is observed two quarters before the election, and the last one is observed in the quarter after the election. Again the effect is large. The peak of the difference between the high and low inequality benchmarks is around 3.6 standard deviations, and it is statistically significant at the 99% level.

The lower-middle panel of Figure 4 exhibits the difference between the path for the PU index around an election quarter in the high versus the low inequality benchmark. The difference starts to increase two quarters before the election and reaches its maximum level during the election quarter. It starts falling the following quarter, until it comes back to a level close to its average two quarters after the election. Note that the timing is consistent with PU causing the subsequent fall in GDP; PU starts reacting one or two quarters before GDP and reaches its normal level much faster.[46] The lower-right panel depicts the impulse-responses for the two benchmarks separately. Here we also observe a significant effect when inequality is low, and this contributes to the size found for the differential effect. When inequality is the highest the PU index reaches a peak of 2 standard deviations.[47]

3.3 Time-Series Estimation: Demand Components

In this section I assess how the UPBC affects the aggregate demand components in the US; private consumption, private fixed investment and public spending.[48]

Before describing the results I show in the left panel of Figure 5 the paths for these variables implied by estimating equation (2) without conditioning in inequality. As in all the cases I have revised, there in no evidence of a PBC in any of these variables. Results from estimating equation (2) with inequality are shown in the last three columns of Table 8. It can be seen that results are as expected for both private consumption and investment. The maximum drops are around 2.4% and 8.6%, respectively. This is equivalent to 1.9 and 1.8 standard deviations, respectively. The total cost is about 4,1% of annual consumption and 16.5% of annual investment. Unlike these private components of aggregate demand the coefficients for public expenditures change sign. In the quarter before the election the difference is positive and significant, but thereafter it becomes not significant, and after three quarters, negative. The corresponding patterns for each of the variables are depicted in the Figure 5. The figure shows consumption in the upper panel, investment in the middle, and government expenditures in the bottom panel. In the middle the difference between the impulse-response functions are shown, and in the right each of them are shown separately.

Therefore, in line with the UPBC predictions and the evidence from panel estimations, private demand declines during an election only if inequality is sufficiently high. The fall in private consumption is noteworthy. Its magnitude is large in comparison to its standard deviation.[49] This fall in consumption may be explained by the fall in GDP and the corresponding effect on disposable income. Alternatively it may be explained by the accumulation of precautionary savings by families facing more uncertainty about their income after taxes and subsidies in electoral periods under high inequality. In this case we would observe a fall in the expenditure rate. In the next section I explore this possibility using microeconomic data on consumption, income and wealth.

3.4 Time-Series Estimation: Sensitivity Analysis

In this subsection I perform different exercises to test the robustness of the time-series findings. To save space I restrict the analysis to GDP estimations. Table 9 shows the results, together with the baseline specification in column (1). First I use an alternative measure of wealth inequality; the fraction of total wealth accrued to the 10% richest fraction of the population. Column (2) shows the estimated coefficients. The response of GDP is statistically significant and qualitatively the same, although around 22% smaller. This is in line with the results using gini in the panel estimations, and may be indicating that wealth accumulation at the very top of the distribution is more relevant for UPBC. In the following two columns I switch to income. Column (3) shows the results when using the top 1% and column (4) when using the top 10%. In the first case the total response is 25% larger than the baseline, and when using the 10% is roughly the same. In spite of generating larger responses I prefer to keep wealth as the baseline inequality indicator because income is more volatile.[50]

The following variation of the baseline estimation is to use the HP trend of inequality as the variable interacting with elections. Since this eliminates movements at a business-cycle frequency it is a good exercise to confirm that results are really driven by movements in inequality at longer horizons. Results are shown in column (5) and we can see that they are very similar to the baseline. But if results come from low-frequency variations in inequality it may be more likely that other trending variables that spuriously correlates with inequality, are behind them. In column (6) I explore this case, considering only very low frequency changes in inequality. To do this I use for inequality a dummy variable that takes the value one when inequality is higher than its sample average, and zero otherwise. Hence there is still a U-shaped path for inequality, but variations at medium and high frequencies are not considered to identify the results. The effect of elections is much smaller in this case, about 45% of the baseline estimations.

The next exercise is to control by an interaction between elections and a linear trend, to capture any unobservable process that may make political business cycles less or more marked over time, in tandem with the evolution of inequality. Although wealth inequality has been increasing only since the mid-seventies, the estimations may still be capturing some trending effect not related

with inequality. Column (7) shows the results, which are still significant and quantitatively much stronger.[51]

Next I check the influence of each of the 18 elections in the results. To do this I introduce dummy variables, with the corresponding leads and lags, for each election, one at a time. Hence there are $Lg_e + Ld_e + 1$ additional coefficients in each estimation. Figure 6 depicts the maximum difference in GDP between the two inequality benchmarks. First I consider the baseline estimation, which has already a set of dummy variables for the year 2008. Results for this case are in blue. The solid line shows the baseline estimate and the circles show how this varies when using a set of dummies for the year marked in the horizontal axis. It can be seen that the 2012 election is the one that influences the most the results. Without that election the maximum drop in GDP goes from 3.4% to 3.8% of quarterly GDP, or 0.85% to 0.95% of annual GDP. In red I show the results from the same exercise but without the set of dummies for the 2008 election. The maximum loss is higher in about 0.7% of quarterly GDP. I don't control for the 2012 election in the baseline estimations because, unlike the 2008 election, it would have strengthen the result. It is worth noticing that all of the different estimates summarized in Figure 6 are statistically significant at the 99% confidence level. Considering or not this influential election have quantitative, but not qualitative, effects.

I don't show the results to save space but the same pattern is found when running the robustness analysis to the specification with PU as an explanatory variable. As in the case of GDP the effect is about 25% smaller when using the top 10% indicator for wealth inequality, and about 70% smaller when using the discrete measure of inequality that only takes into account low frequency changes. The results are robust to the inclusion of dummies for each one of the elections in the sample except, again, 2012. The maximum difference changes in 25% (still statistically significant at the 99% level), and in the opposite direction; the spike is smaller when not considering that election. Hence the 2012 election seems to be an election with better than expected macroeconomic performance, and higher than expected uncertainty levels.

As in the panel estimations it would be interesting to explore if the results change with the identity of the parties leaving or taking power. However, since in this case this exercise implies a substantial drop in degrees of freedom, results are sensitive to the inclusion of some particular elections. One robust result is that the effects are negative and highly significant in every case. It is also observed that power switches between the two parties are always more costly, and particularly so in the case when power transits from Republicans to Democrats. Interestingly in this last case most of the differences are observed two and three quarters after the election, which might be consistent with UPBC plus partisan post-election cycles as in Azzimonti and Talbert (2014).[52]

4 Microeconomic Evidence

In this section I use microeconomic data on consumption, income and wealth to explore more closely the channels behind the UPBC macroeconomic evidence reported so far. I focus in particular on

the claim that inequality, through its effect on policy uncertainty, leads to a surge of precautionary savings by households. Since consumption of wealth-poor agents is the most sensitive to changes in uncertainty, we should expect a relatively larger fall in the expenditure rate in this group during election years and high inequality. To test this hypothesis I make use of the Panel Study of Income Dynamics (PSID) which provides household level panel data on earnings, income, consumption expenditures and wealth for the U.S. Unfortunately data on consumption is available only for the last two decades, a period that is too short to perform the type of exercise done so far, where results are explicitly conditioned on the level of wealth inequality. Instead, and backed by the fact that inequality has reached its highest levels in the last years (Figure 3), I use data for 2005-2017 and interpret the results as if they were obtained under high inequality, i.e. as a time when the UPBC was active.

To test the hypothesis I estimate the following panel fixed-effects specification,

$$\Delta x_{g,t} = \gamma_0 + \gamma_1 e_t + \gamma_2 \ (e_t \times g) + \gamma_3 \ I_{t=2008} + \gamma_4 \ (I_{t=2008} \times g) + \eta_g + \nu_{g,t}. \tag{3}$$

In this equation the dependent variable $\Delta x_{g,t}$ is the change in the variable of interest x for the wealth group g from year t to year $t+2$. The timing is due to the fact that PSID data is given every two years. Hence I have 6 periods of data to use in the estimations, with 3 elections, 2008, 2012 and 2016. Every available period t I split the whole sample in 20 percentiles according to the net stock of wealth. For each individual I compute the change in x from t to $t+2$, and then the mean over group g, computed for period t, corresponds to $\Delta x_{g,t}$. The first explanatory variable in (3) is e_t, a dummy variable that takes the value 1 whenever there is an election between t and $t+2$. It turns out that elections have been held in years between those when PSID data becomes available, and hence $\Delta x_{g,t}$ is the change in variable x before and after the election when $e_t = 1$. Clearly this is not the best timing, and it can bias the results toward zero, but it is the only available option. Note that since e_t doesn't vary across groups it is not possible to include time-dummies in the specification. The second independent variable $e_t \times g$ is the election variable multiplied by the group indicator, and it captures differences across wealth groups. The hypothesis that relates the UPBC with precautionary savings implies $\gamma_2 > 0$. Finally $I_{t=2008}$ is a dummy variable for the 2008 election, which also appears interacted with g, to isolate the results from the effects of the Great Recession as it was done in the exercises using macroeconomic data. The variable η_g is the fixed-effect and $\nu_{g,t}$ is the residual term.

Estimation results are shown in Table 10. For ease of exposition I normalize the value of the dependent variable by its standard deviation, and show only the coefficients of interest, those associated with e_t. In the first column I use the percentage change in consumption as the dependent variable.[53] The coefficient on e_t shows that during election years there is a fall in consumption growth that is close to a quarter of a standard deviation, but the unconditional effect is not significant. The interaction with g has a negative coefficient. Although it is not significant as well, the two coefficients imply a significant negative effect for some of the groups. To see this I plot

the point effect, and its 90% confidence bands, for each of the 20 wealth groups in the left panel of Figure 7. It can be seen that the point effect is always negative, with an average size of a third of a standard deviation, and that it is significant between the 11 to 18 wealth quintiles. Next I use the percentage change in disposable income as the dependent variable in column 2 of Table 10. The unconditional effect is positive but not significant. The interaction is negative but it is not strong enough to make the overall effect negative and significant for some of the groups, and the average effect is close to zero (see the center panel of Figure 7).

The results using the change in the expenditure rate are shown in column 3 of Table 10. It is worth to notice that this variable is the group-average of the change in individual expenditure rates, and not the change in the ratio of the group-average consumption and disposable income. It closely captures the reaction of precautionary savings of individual agents. In this case there is a negative and significant effect of elections. The coefficient implies a fall in three quarters of a standard deviation in the expenditure rate in a year of election relative to a year without it. As expected the coefficient on the interaction between election year and group is positive, meaning that the fall is greater for wealth-poor groups. Although not significant by itself, it counteracts the significant unconditional effect, implying that the overall effect stops being significant for agents in the 12 and 7 quintiles and above, for 90% and 95% significance levels, respectively (see Figure 7).

Overall the effects are in line with the existence of a precautionary reaction of consumption in election years. Although the estimations are imprecise, consumption seems to fall similarly across wealth groups. More precise estimations are obtained for the expenditure rate, which is closely associated with the precautionary motive. The response of this variable during election years is negative and significant, but only for wealth-poor agents, precisely those from which a rise in precautionary savings is to be expected.

5 Conclusions

In this paper I document a specific mechanism by which inequality affects macroeconomic dynamics at the business cycles frequency. This mechanism is novel and manifests itself trough policy uncertainty triggered by presidential elections. Inequality and the lack of a redistributive response by democratic institutions generates political polarization, social discontent, unrest and distrust in established political parties and institutions, among other features, which in turn increase the likelihood of sharp swings in policies and weaken the ability of politicians to commit. In this context elections turn into decisive events where post-election policies become extremely uncertain. The paper complements and extends the works by Azzimonti and Talbert (2014) and Canes-Wrone and Park (2012). The first presents a theoretical analysis of post-election partisan cycles, the ones that give rise to policy uncertainty in this paper. The second presents evidence linking political polarization to falls in investment during elections.

Using data for a panel of 25 mostly developed and highly democratic countries, and for a

long period of time for the US, I find evidence supporting the existence of UPBC, i.e. a fall in GDP and private aggregate demand components, and a raise in policy uncertainty, around presidential elections only in times of relatively high inequality. The effects are large and robust to different specifications. The macroeconomic estimates show a strong effect on private consumption, a demand component that has not been associated directly with elections by previous literature. The behavior of private consumption might be explained by the fall in income. However it may also be explained by a direct effect of policy uncertainty. To explore this in detail I use US microeconomic data and find evidence of a fall in expenditure rates for relatively wealth-poor agents during elections. This is consistent with the hypothesis that policy uncertainty, triggered by elections under high inequality, generates an increase in household's precautionary savings, depressing at least in part aggregate consumption.

Different findings assert the role of policy uncertainty and socio-political instability behind the results. The effects dissipate some quarters after the elections, the UPBC is strongly significant independently of the identity of the incumbent party or the one wining the election, and there is no association between the identity of the party and inequality. Moreover results are always stronger when using indicators capturing inequality at the extremes of the distribution, in line with the hypothesis that the very rich are the ones gaining political influence and blocking redistribution, and political polarization and protests are shown to be variables that mediate the relationship between inequality and macroeconomic variables. All of this reduces the likelihood that the results are due to a leftist turn in policies, as it would be the case under the framework derived from Meltzer and Richard (1981) and the early literature on growth and inequality (Alesina and Rodrik, 1994; Persson and Tabellini, 1994). Therefore this paper not only contributes to the literatures on the costs of inequality, the PBC and the macroeconomic effects of policy uncertainty, but also to the literature on inequality and economic growth since it presents evidence on the main mechanism studied by it. The explicit role of policy uncertainty in this paper is novel in this context since the growth literature hasn't considered it directly, contrary to common interpretation (Benabou, 1996). Although the empirical framework is built to capture short-run effects, the costs can be thought as permanent, not only because there is no recovery from the recession, but also because it is to be expected that inequality keeps policy uncertainty at high levels even during non electoral periods (Baker et al., 2014).

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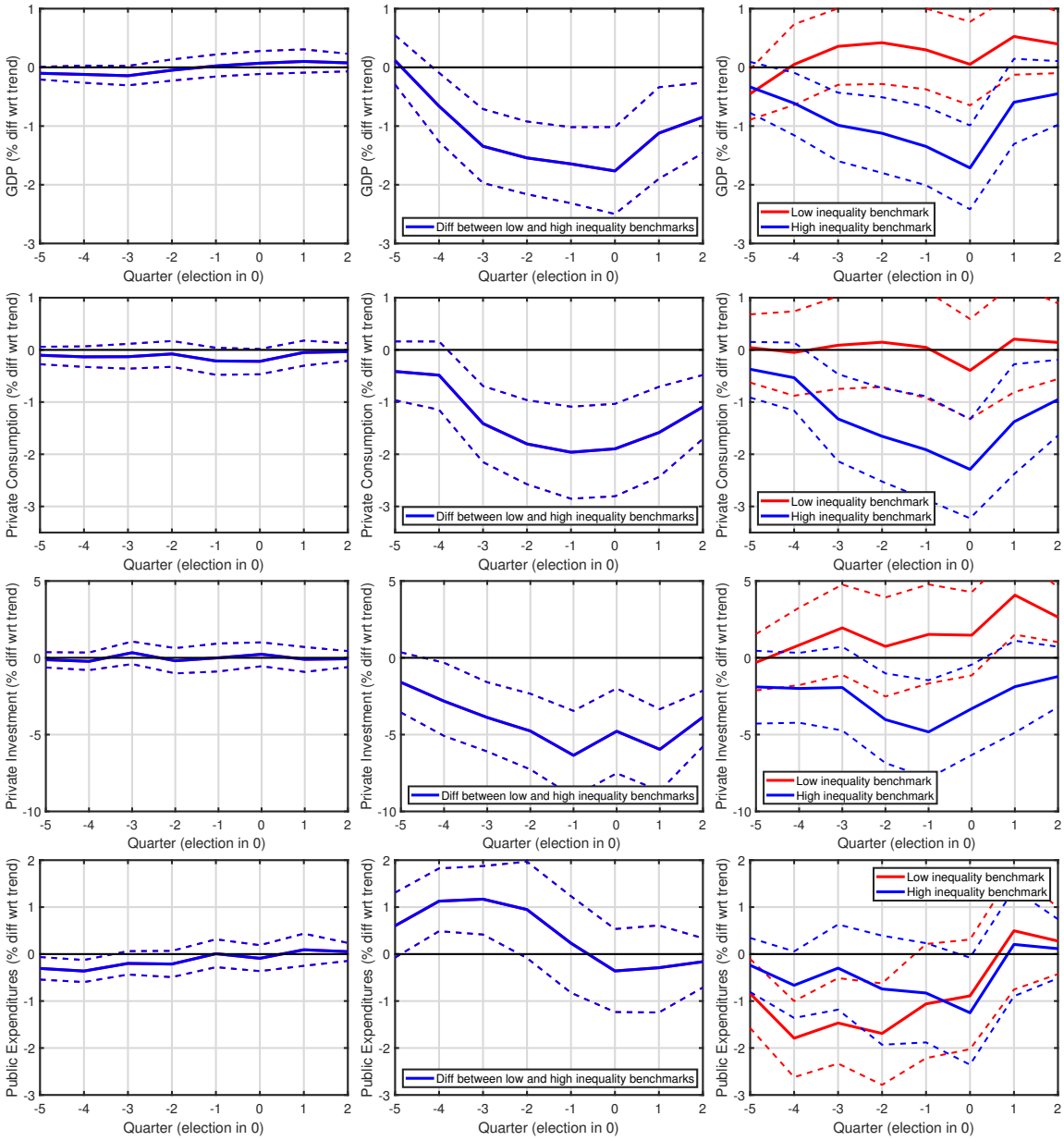
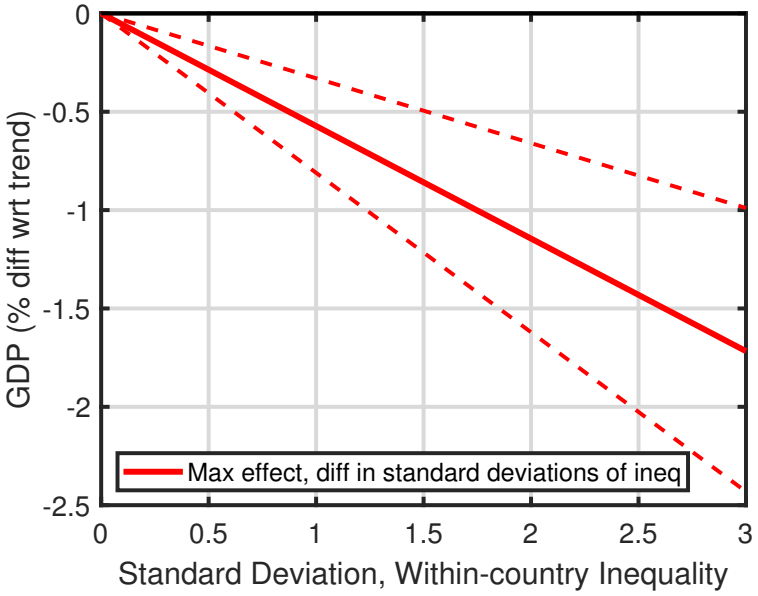


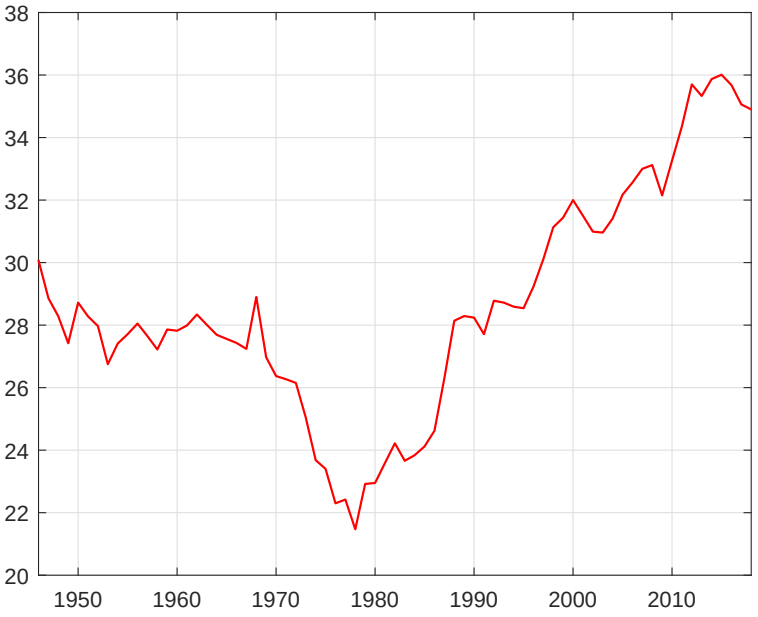
Note: the figure shows the impulse-response functions derived from the estimation of equation (1) for GDP (first row), private consumption (second row), private investment (third row) and public expenditures (fourth row). The left column depicts the impulse-response functions without conditioning in inequality. The center column depicts the difference in impulse-response functions between the high and low inequality benchmarks. The right column depicts the impulse-response functions for the high (blue) and low (red) inequality benchmarks separately. Period 0 is the quarter of the election. Dashed lines are 90% confidence intervals computed using the Wild Bootstrap.

Figure 1: Unequal Political Business Cycles, Panel Estimates



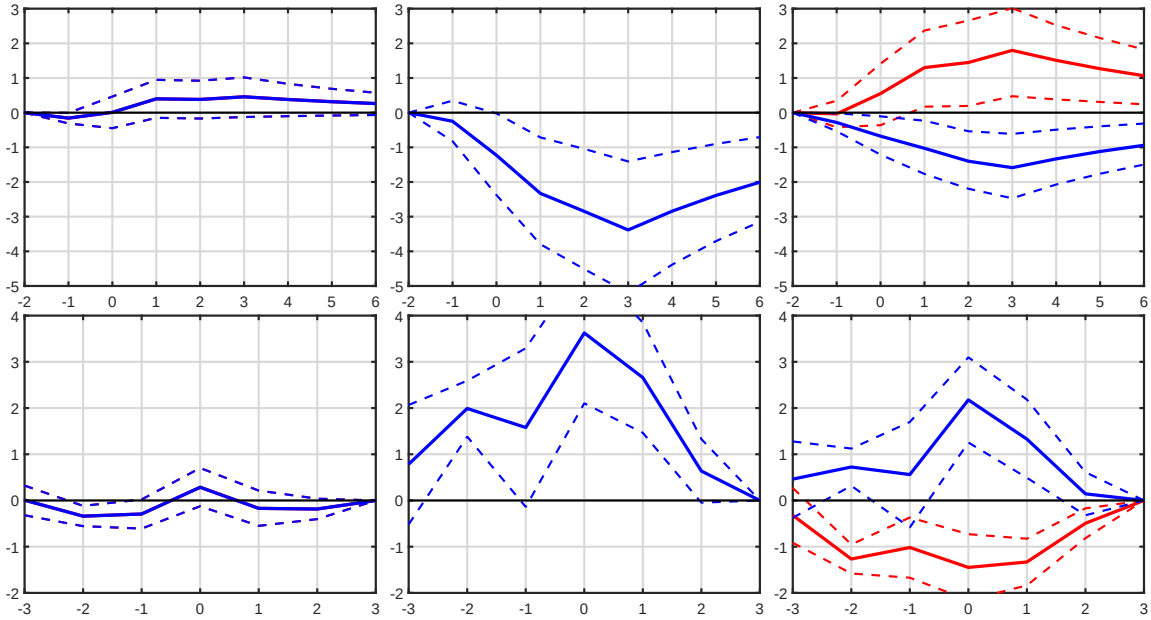
Note: maximum effect of elections between economies with different levels of inequality, from estimating Equation (1), by within-country inequality standard deviation. Dashed lines are 90% confidence intervals computed using the Wild Bootstrap.

Figure 2: Maximum Effect on GDP by Inequality Differences, Panel Estimations



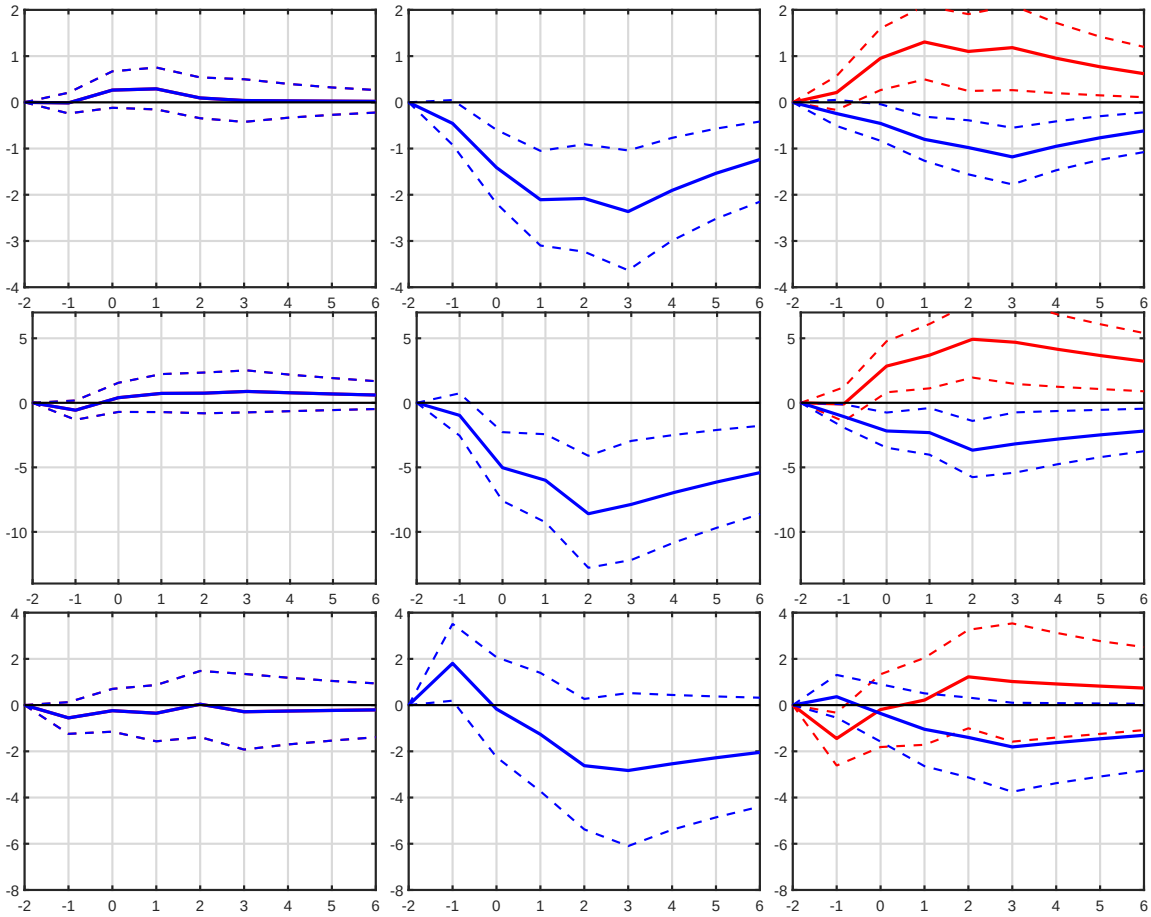
Note: fraction of total wealth held by the richest 1% of the population. Source: WID.

Figure 3: Wealth Inequality in the US, 1945-2016.



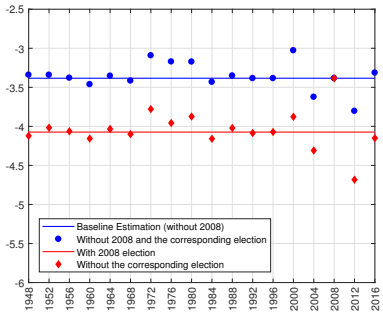
Note: the figure shows the impulse-response functions derived from the estimation of equation (2) for GDP (first row) and the PU index (second row). The left column depicts the impulse-response functions without conditioning in inequality. The center column depicts the difference in impulse-response functions between the high and low inequality benchmarks. The right column depicts the impulse-response functions for the high (blue) and low (red) inequality benchmarks separately. Period 0 is the quarter of the election. Dashed lines are 90% confidence intervals computed using the Wild Bootstrap.

Figure 4: Unequal Political Business Cycles, US Time-series Estimations



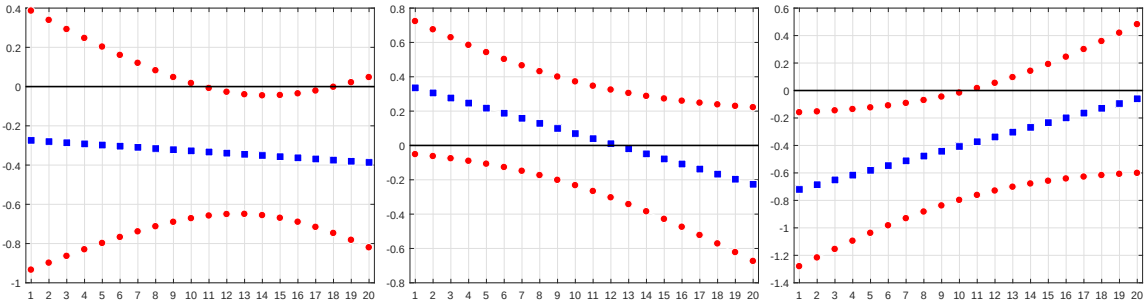
Note: the figure shows the impulse-response functions derived from the estimation of equation (2) for private consumption (first row), investment (second row) and public expenditures (third row). The left column depicts the impulse-response functions without conditioning in inequality. The center column depicts the difference in impulse-response functions between the high and low inequality benchmarks. The right column depicts the impulse-response functions for the high (blue) and low (red) inequality benchmarks separately. Period 0 is the quarter of the election. Dashed lines are 90% confidence intervals computed using the Wild Bootstrap.

Figure 5: Unequal Political Business Cycles, US Time-series Estimations, Demand Components



Note: maximum difference between the high and low inequality benchmarks of the impulse-response functions derived from the estimation of equation (2). The solid blue line is the baseline estimate. Blue circles show the estimates when controlling for the election marked in the horizontal-axis. The solid red line is the estimate when not controlling for the 2008 election. Red diamonds show the estimates when controlling for the election marked in the horizontal-axis but not for the 2008 election.

Figure 6: Political Business Cycles, US Time-series Estimations, Sensitivity Analysis



Note: effect of elections on consumption (left), disposable income (center) and expenditure rate (right) for each of the 20 wealth quintiles, from estimating Equation (3). In blue the point estimate and in red the 90% confidence interval. The dependent variable is normalized by its standard deviation.

Figure 7: Elections and Consumption, Microeconomic Evidence

	Country	Years	Elections		Country	Years	Elections
1	Australia	1981 - 2013	12	14	Israel	1990 - 2015	8
2	Austria	1996 - 2012	4	15	Italy	1995 - 2013	5
3	Canada	1971 - 2012	13	16	Luxembourg	1995 - 2012	3
4	Switzerland	1982 - 2012	8	17	Netherlands	1996 - 2012	6
5	Czech Republic	1996 - 2012	4	18	Norway	1979 - 2012	8
6	Germany	1991 - 2014	6	19	Slovenia	1997 - 2011	4
7	Spain	1995 - 2012	5	20	Sweden	1993 - 2004	3
8	Estonia	2000 - 2012	3	21	United States	1974 - 2015	10
9	Finland	1990 - 2012	6	22	Chile	1996 - 2014	4
10	France	1978 - 2009	8	23	Hungary	1995 - 2014	5
11	United Kingdom	1969 - 2015	12	24	Mexico	1997 - 2011	2
12	Greece	1995 - 2012	6	25	Poland	1995 - 2015	6
13	Ireland	1995 - 2009	3				

Note: countries included in the panel baseline estimation. Years are the initial and final year the country appears in the sample and Elections is the number of elections included for each country in the sample.

Table 1: Sample in Panel Estimations

	GDP	Private Consumption	Investment	Public Expenditures
	(1)	(2)	(3)	(4)
$\beta_{3,-5}$	$0,12$	$-0,41$	$-1,59$	$0,60$
$\beta_{3,-4}$	$-0,75^{***}$	$-0,20$	$-1,79$	$0,79^{**}$
$\beta_{3,-3}$	$-0,84^{***}$	$-1,08^{***}$	$-2,06^{**}$	$0,54$
$\beta_{3,-2}$	$-0,53^{***}$	$-0,83^{***}$	$-2,25^{**}$	$0,29$
$\beta_{3,-1}$	$-0,48^{**}$	$-0,72^{**}$	$-3,26^{***}$	$-0,30$
$\beta_{3,0}$	$-0,52$	$-0,54^{*}$	$-0,67$	$-0,49$
$\beta_{3,1}$	$0,21$	$-0,28$	$-2,86^{***}$	$-0,09$
Max effect (95th-5th)	$-1,76^{***}$	$-1,96^{***}$	$-6,35^{***}$	$-0,36$
Total effect (95th-5th)	$-11,1^{***}$	$-12,9^{***}$	$-40,8^{***}$	$3,06$
Max effect (75th-25th)	$-0,52^{***}$	$-0,57^{***}$	$-1,86^{***}$	$-0,11$
Total effect (75th-25th)	$-3,24^{***}$	$-3,77^{***}$	$-11,9^{***}$	$0,90$
R^2	0,80	0,60	0,62	0,46
Obs	2302	2274	2274	2274
Elections	142	141	141	141
Countries	25	25	25	25

Note: equation (1) estimation results. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (1) for high and low inequality benchmarks (5th and 95th within-inequality percentiles). The ninth row shows the total difference between these two impulse-response functions. The tenth and eleventh rows show maximum and total differences for the alternative benchmarks of 25th and 75th within-country inequality. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 2: Panel Estimations

	Baseline	Only fixed elections	Only countries with fixed elections	Gini Index	GDP Growth	Linear trend	With 2008-2009 elections	15 years of observations	20 years of observations
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
$\beta_{3,-5}$	0.12	0.15	-0.24	0.12	-0.11	0.13	0.13	0.15	0.27
$\beta_{3,-4}$	-0.75^{***}	-1.06^{***}	-1.42^{***}	-0.55^{**}	-0.99^{***}	-0.79^{***}	-0.79^{***}	-0.76^{***}	-0.60^{**}
$\beta_{3,-3}$	-0.84^{***}	-1.09^{***}	-0.31	-0.64^{**}	-0.88^{***}	-0.92^{***}	-0.83^{***}	-0.78^{***}	-0.76^{**}
$\beta_{3,-2}$	-0.53^{***}	-0.62^{***}	-0.88^{***}	-0.55^{***}	-0.34^{*}	-0.50^{**}	-0.60^{***}	-0.50^{***}	-0.64^{***}
$\beta_{3,-1}$	-0.48^{**}	-0.64^{**}	-0.45	-0.41^{*}	-0.21	-0.61^{**}	-0.47^{**}	-0.37^{*}	-0.24
$\beta_{3,0}$	-0.52	-0.76^{**}	-0.09	-0.37	-0.19	-0.59^{*}	-0.44	-0.51	-0.79^{*}
$\beta_{3,1}$	0.21	0.39	-0.43^{*}	0.12	0.59^{*}	0.20	0.27	0.20	0.32
Max effect	-1.76^{***}	-2.37^{***}	-2.09^{***}	-1.43^{***}	-0.99^{***}	-1.96^{***}	-1.73^{***}	-1.56^{***}	-1.65^{***}
Total effect	-11.1^{***}	-12.0^{***}	-16.3^{***}	-9.1^{***}	-2.1^{***}	-12.2^{***}	-10.9^{***}	-9.6^{***}	-8.2^{**}
R^2	0.80	0.81	0.86	0.80	0.34	0.81	0.81	0.80	0.79
Obs	2302	2302	772	2302	2287	2302	2302	2032	1398
Elections	142	106	43	142	140	142	154	128	92
Countries	25	25	10	25	25	25	25	20	11

Note: equation (1) estimation results using GDP as dependent variable. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (1) for high and low inequality benchmarks. The ninth row shows the total difference between these two impulse-response functions. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 3: Panel Estimations: Robustness Analysis

	Baseline	Only parliamentary	Only Advanced Economies	No Transition Economies	Richest 15 Economies
	(1)	(2)	(3)	(4)	(5)
$\beta_{3,-5}$	$0,12$	$0,44$	$0,31$	$0,11$	$-0,29$
$\beta_{3,-4}$	$-0,75^{***}$	$-0,61^{**}$	$-0,43$	$-0,69^{***}$	$0,02$
$\beta_{3,-3}$	$-0,84^{***}$	$-0,93^{***}$	$-0,81^{***}$	$-0,87^{***}$	$-0,58^{*}$
$\beta_{3,-2}$	$-0,53^{***}$	$-0,57^{**}$	$-0,41^{*}$	$-0,51^{***}$	$-0,56^{*}$
$\beta_{3,-1}$	$-0,48^{**}$	$-0,67^{**}$	$-0,55^{**}$	$-0,44^{*}$	$-0,69^{*}$
$\beta_{3,0}$	$-0,52$	$-0,73^{*}$	$-0,63$	$-0,51$	$-0,23$
$\beta_{3,1}$	$0,21$	$0,47$	$0,37$	$0,26$	$-0,23$
Max effect	$-1,76^{***}$	$-2,06^{***}$	$-1,72^{***}$	$-1,59^{***}$	$-1,42^{**}$
Total effect	$-11,1^{***}$	$-10,4^{**}$	$-8,8^{**}$	$-9,5^{***}$	$-8,3^{**}$
R^2	0,80	0,80	0,81	0,79	0,80
Obs	2302	2004	2010	1968	1594
Elections	142	128	126	121	100
Countries	25	22	21	20	15

Note: equation (1) estimation results using GDP as dependent variable and restricting the sample according to the group defined at the top of the table. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (1) for high and low inequality benchmarks. The ninth row shows the total difference between these two impulse-response functions. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 4: Panel Estimations: Subgroups of Countries

	Baseline	Incumbent Left	Incumbent Right	Winner Left	Winner Right
	(1)	(2)	(3)	(4)	(4)
$\beta_{3,-5}$	$0,12$	$-0,15$	$-0,42^{**}$	$0,69^{*}$	$0,52$
$\beta_{3,-4}$	$-0,75^{***}$	$-1,11^{***}$	$1,47^{**}$	$-0,64^{**}$	$-1,24^{***}$
$\beta_{3,-3}$	$-0,84^{***}$	$-0,59^{**}$	$-1,43^{*}$	$-1,23^{***}$	$-1,23^{***}$
$\beta_{3,-2}$	$-0,53^{***}$	$-0,50$	$-0,16$	$-0,62^{**}$	$-1,05^{***}$
$\beta_{3,-1}$	$-0,48^{**}$	$-0,57^{***}$	$-2,49^{*}$	$-0,22$	$-0,53^{***}$
$\beta_{3,0}$	$-0,52$	$-0,65$	$0,28$	$-0,95^{**}$	$-0,63$
$\beta_{3,1}$	$0,21$	$-0,04$	$0,58$	$0,85^{**}$	$0,45$
Max effect	$-1,76^{***}$	$-1,80^{***}$	$-2,90^{**}$	$-1,87^{***}$	$-2,34^{***}$
Total effect	$-11,1^{***}$	$-12,0^{***}$	$-7,1$	$-7,0^{*}$	$-12,2^{***}$
R^2	0,80	0,78	0,78	0,78	0,78
Obs	2302	1984	1984	1984	1994
Elections	142	119	119	119	119
Countries	25	22	22	22	22

Note: equation (1) estimation results using GDP as dependent variable and restricting the sample according to the criteria defined at the top of the table. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (1) for high and low inequality benchmarks. The ninth row shows the total difference between these two impulse-response functions. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 5: Panel Estimations: Partisan Differences

	Political Polarization	Protests
	(1)	(2)
Income Inequality	1.45^{***}	0.007^{**}
	0.33	0.004
Obs	104	1805
Countries	17	23

Note: regression results using Political Polarization (column 1) and Protests (column 2) as dependent variables, and Income Inequality as explanatory variable. Political Polarization is only available for election quarters. Political polarization and income inequality are normalized by their within-country standard deviation. See the text for the construction of each dependent variable. Time and country fixed effects are included in both specifications. The years 2008-2009 are not included in the estimations. A lag of the dependent variable is included in column 2. Robust standard errors (below the coefficients) are clustered at a country level. ***,**, and * indicate significance at 1%, 5%, and 10% levels.

Table 6: Panel Estimations: Transmission Mechanisms, Step 1

	GDP	Private Consumption	Investment	Public Expenditures
	(1)	(2)	(3)	(4)
A. Income Inequality				
Max effect	$-1,76^{***}$	$-1,96^{***}$	$-6,35^{***}$	$-0,36$
Total effect	$-11,1^{***}$	$-12,9^{***}$	$-40,8^{***}$	$3,1$
Obs	2302	2274	2274	2274
Countries	25	25	25	25
B. Political Polarization				
Max effect	$-0,85^{**}$	$-1,15^{**}$	$-3,85^{**}$	$-1,17^{*}$
Total effect	$-4,9^{**}$	$-6,3^{*}$	$-18,0^{**}$	$-1,2$
Obs	1529	1521	1521	1521
Countries	17	17	17	17
C. Protests				
Max effect	$-0,80^{**}$	$-0,84^{**}$	$-4,27^{**}$	$-1,35$
Total effect	$-4,9^{**}$	$-3,8$	$-21,0^{**}$	$-3,2$
Obs	1732	1732	1732	1732
Countries	23	23	23	23

Note: results from estimating equation (1) using Political Polarization (middle panel) and Protests (lower panel) instead of income inequality. See the text for the construction of each dependent variable. The first panel shows the baseline results for comparisons. Only the maximum and total difference between the impulse-response functions for high and low inequality benchmarks are shown. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 7: Panel Estimations: Transmission Mechanisms, Step 2

	GDP	Policy Uncertainty	Private Consumption	Investment	Public Expenditures
	(1)	(2)	(3)	(4)	(5)
$\beta_{3,-3}$		0.78			
$\beta_{3,-2}$		1.99^{***}			
$\beta_{3,-1}$	-0.25	1.58	-0.46	-0.97	1.81^{*}
$\beta_{3,0}$	-1.02^{*}	3.62^{***}	-1.04^{***}	-4.17^{***}	-1.79^{**}
$\beta_{3,1}$	-1.30^{**}	2.66^{***}	-0.97^{***}	-1.56	-1.11
$\beta_{3,2}$	-0.89	0.64	-0.38	-3.30^{***}	-1.49^{**}
$\beta_{3,3}$	-0.99^{*}		-0.69^{*}	-0.29	-0.48
Max effect	-3.38^{***}	3.62^{***}	-2.36^{***}	-8.59^{***}	-2.83
Total effect	-23.4^{***}	11.3^{***}	-16.5^{***}	-65.8^{***}	-19.4
R^2	0.74	0.21	0.68	0.82	0.82
Obs	287	287	287	287	287
Elections	18	18	18	18	18

Note: equation (2) estimation results. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (2) for high and low inequality benchmarks. The ninth row shows the total difference between these two impulse-response functions. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 8: Time-series Estimations

	Baseline	Top 10% wealth	Top 1% income	Top 10% income	HP Trend Inequality	Inequality high-low	Linear trend
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
$\beta_{3,-1}$	-0.25	-0.22	-0.53^{*}	-0.36	-0.36	-0.17	-0.42
$\beta_{3,0}$	-1.02^{*}	-1.55^{**}	-0.79	-0.91	-0.57	-0.48	-1.35
$\beta_{3,1}$	-1.30^{**}	-0.47	-2.00^{***}	-1.68^{***}	-1.77^{***}	-0.59	-1.85^{*}
$\beta_{3,2}$	-0.89	-0.83^{*}	-1.29^{**}	-0.90^{**}	-1.07^{**}	-0.27	-1.61^{*}
$\beta_{3,3}$	-0.99^{*}	-0.59	-1.20^{**}	-0.83^{*}	-0.92^{*}	-0.46	-1.61^{***}
Max effect	-3.38^{***}	-2.65^{**}	-4.37^{***}	-3.46^{***}	-3.57^{***}	-1.48^{**}	-5.28^{***}
Total effect	-23.4^{***}	-19.5^{**}	-29.7^{***}	-24.3^{***}	-24.3^{***}	-10.6^{**}	-35.9^{***}
R^2	0.74	0.74	0.75	0.75	0.75	0.75	0.75
Obs	287	287	287	287	287	287	287
Elections	18	18	18	18	18	18	18

Note: equation (2) estimation results using GDP as dependent variable. Only the coefficients on the interaction between elections and inequality are shown (first seven rows). The eighth row shows the maximum difference between the impulse-response functions derived from the estimation of equation (2) for high and low inequality benchmarks. The ninth row shows the total difference between these two impulse-response functions. ***,**, and * indicate significance at 1%, 5%, and 10% levels computed using the Wild bootstrap.

Table 9: Time-series Estimations: Sensitivity Analysis

	$\Delta\ c_{g,t}$	$\Delta\ y_{g,t}$	$\Delta\ (c/y)_{g,t}$
e_t	-0.27	0.37	-0.76 **
	0.43	0.25	0.36
$e_t \times g$	-0.01 †	-0.03	0.03 ‡
	0.03	0.02	0.03
Obs	120	120	120

Note: results from estimating Equation (3). The dependent variable is normalized by its standard deviation. †: makes the overall effect significant at the 90% confidence level for quintiles 11-18 (see Figure 7). ‡: makes the overall effect not significant for quintiles 11-20 and 7-20 for 90% and 95% significance levels, respectively (see Figure 7). Coefficients on 2008 election dummies and fixed effects included but not shown. Robust standard errors, below the estimated coefficients, are clustered at wealth groups. ***,**, and * indicate significance at 1%, 5%, and 10% levels.

Table 10: Elections and Consumption, Microeconomic Evidence

Notes

[1]The relation between inequality and business cycles has been analyzed in the context of heterogeneous agents models. The causal effect operates through differences in marginal propensities to consume and work between wealthy and poor individuals (see e.g. Kumhof et al., 2015; Athreya and Schwartzman, 2017; Auclert and Rognlie, 2018). In this paper the causal effect works through politics.

[2]More generally Galor et al. (2009), Engerman and Sokoloff (2002) and Acemoglu et al. (2005) among others, analyze how inequality on either land, wealth or political power, was a barrier for the historical development of human capital and growth enhancing institutions precisely because of the opposition to reform by the most favored groups.

[3]Hare and Poole (2014) show that political polarization has led to greater oscillation in policy outcomes in the US. See Baker et al. (2014), Azzimonti and Talbert (2014), Canes-Wrone and Park (2012) and Frye (2001) for the link between political polarization and policy uncertainty. Alesina and Perotti (1996) and Perotti (1996) highlights policy uncertainty coming from socio-political instability as one of the main channels through which inequality affects growth. Dustmann et al. (2017) show that distrust correlates with support of populist governments, whose policy proposals involve very uncertain future consequences since these are disregarded by them (Guiso et al., 2017).

[4]The papers studying the effects of precautionary savings focus on job rather than policy uncertainty as a determinant of aggregate fluctuations through its effect on household's consumption decisions. In the case of investment, Hassan et al. (2019) focus specifically in firms facing political risk, which retrench investment and hiring, while Caldara et al. (2020) study trade policy risks. Frye (2001) emphasizes the incentives to invest abroad or lobbying the government when investors perceive high policy uncertainty.

[5]It is well known that the exogeneity of elections depends on the political system. Results are robust in this paper to the exclusion of elections that were not planned in advance. In the case of inequality, and its potential two-way correlation with GDP, the estimations below don't rely on variation in inequality at the business-cycle frequency.

[6]See also Bertola (1993) and Perotti (1993) for early theoretical contributions.

[7]The literature has also consider alternative mechanisms such as financial market imperfections and fertility decisions, but these are little related with pre-electoral short-run dynamics. Closer is the mechanism linking growth and political polarization, which in this setting is largely influenced by inequality, where polarization, together with political instability, lead the government to overspend and increase tax distortions, affecting investment and economic activity (Persson and Svensson, 1989; Alesina and Tabellini, 1990; Azzimonti, 2011).

[8]Uncertainty is not part of the socio-economic channel studied in the growth literature, which is, according to Benabou (1996), contrary to common interpretation.

[9]Since the focus here is in short-run dynamics it is not straightforward to extrapolate the results to long-run growth. However, since I don't find a recovery from the recession generated by the UPBC the implied costs are permanent. Furthermore, as shown by Baker et al. (2014), high inequality keeps policy uncertainty at high levels not only around elections, meaning that the negative consequences of inequality trough the channel analyzed in this paper should persist in non-electoral periods as well.

[10]See Alesina (1988); Franzese Jr (2002); Persson and Tabellini (2002); Drazen (2004) for reviews of the literature on office-seeking and partisan PBC. There is also a related literature that explores the different performance of Republican and Democrat governments in the US (see e.g. Blinder and Watson, 2016; Pastor and Veronesi, 2020).

[11]See also related work by Julio (2016), Jens (2017), and Frye (2001).

[12]These exercises are just illustrative since political polarization is endogenous not only to macroeconomic conditions (see Jens, 2017; Garro, 2020), but also to inequality (Hare and Poole, 2014; McCarty et al., 2016). I also explore the role of non-violent protests as an additional link between inequality and UPBC, finding similar results.

[13]The result that policy uncertainty spikes in elections under high inequality comes from a time-series estimation for the US, not from a panel estimation. In this last case I pursue alternative exercises to shed light on the transmission

mechanism.

[14]An effect on precautionary savings is also found by Julio and Yook (2012), but for the case of firms. Using microeconomic data they show an increase in cash holdings, together with a reduction in corporate investment, the year before elections.

[15]Because results rely on the interaction between election dummies and inequality, a country-fixed effect is not enough to clean for any country-specific invariant component influencing the results. More unequal countries may have additional features that only manifest during elections, making macro variables more sensitive to them at that moment. The specification used in this paper fixes this problem in a simple way, by interacting election dummies with deviations of inequality from country averages, exploiting in this way only variation within countries.

[16]The sample is composed mainly of developed countries. Therefore this result doesn't necessarily contradict those found by Julio and Yook (2012), who find that the effect of elections on corporate investment is less pronounced in countries with stronger check and balances and relatively more stable governments.

[17]The 2008 election is also excluded from this result.

[18]As explained below I restrict the sample to countries with at least 40 quarters of data. The average number of quarters per country is more than 90. This makes unlikely the existence of the incidental parameter problem. I show below that results remain significant when restricting the sample to countries with at least 60 and 80 quarters of data, cases in which the average number of quarters per country rises to 100 and 130, respectively. When using local projections results are similar.

[19]The inclusion of a fixed-effect would typically make this adjustment unnecessary. It is not the case here because the focus is on the interaction term $e_{tl} \times i_{tl}$, which has an average very close to zero.

[20]I use seasonally adjusted real GDP (codes NGDPRSAXDC, NGDPRKSAXDC, NGDPRKSAIX and NGDPRCHSAXDC). In the case of demand components I use seasonally adjusted final consumption expenditure from private sector (code NCPSAXDC), final consumption expenditure from the public sector (NCGGSAXDC) and gross fixed capital formation (code NFISAXDC), all deflated by the seasonally adjusted GDP deflator (code NGDPDSAIX). The only modifications I make is to adjust base year changes in the data for Canada and Israel, as well as dropping data for Israel previous to 1990, a period of high inflation rates.

[21]To obtain the value in periods without elections, a variable that is used as a control, I interpolate annually and then assume there is no variation within a year.

[22]Because of the slow-moving nature of inequality it is important to consider long periods of time for each country. In the sensitivity analysis I show results with even more stringent restrictions.

[23]Here and in the rest of the paper I use the wild bootstrap to compute confidence intervals and to assess the significance of the estimated coefficients.

[24]These periods are chosen to have at least one coefficient before and after the election, and one non-significant coefficient at each extreme. Results don't change much when using an alternative configuration of these dummies.

[25]Hence it may be the case that an individual country never achieves any of these levels, or may be the case that a country with a low level of inequality on average, achieves at some point the high inequality benchmark.

[26]I only consider developed countries when computing the percentiles to avoid the influence of potentially high deviations in developing countries. It turns out however that differences are not so large. In the baseline case, where I use the 90/10 income ratio, the percentiles would be about 30% lower and higher when including developing countries. In the case of the gini coefficient, for which I show results below, the difference is close to zero.

[27]In the last part of the paper I use microeconomic data to show that the consumption-income ratio falls during elections in wealth-poor households, which suggest an independent effect of inequality on consumption through uncertainty.

[28]The table shows maximum and total effects for the growth rates. When transforming this to GDP levels the maximum effect is about 2.5%, and GDP stays 2% below its initial level one quarter after de election.

[29]The response in consumption is ambiguous because of the distributive effect of tax cuts. In contrast, the effect of

policy uncertainty is negative since it affects disproportionately wealth-poor agents, and these are the ones harmed by tax cuts. Unfortunately the macro evidence is too coarse to disentangle these different channels. In the last section I use micro data and show that in the US wealth-poor households reduce their consumption-income ratios around elections.

[30]The fact that the effects are stronger when using top income inequality instead of the gini index also support this idea since the former is more closely associated to the over-representation of the rich in politics.

[31]Partisan cycles arising from inflation expectation deviations predict a positive response of GDP with the election of a left-wing government. The results are consistent with partisan cycles arising from fiscal policies, as in Azzimonti and Talbert (2014), which, as already mentioned, generate policy uncertainty before the election.

[32]This comes from a panel regression with the party orientation as a dependent (dummy) variable and the ratio p90/10 as the explanatory variable, including both year and fixed-effects.

[33] It is not clear how comparable the series are across countries. The dispersion of the index is about three time as large in the UK, and twice as large in Canada and France, than in developing countries like Chile, Mexico and India, and some southern European countries like Greece and Italy. Although I find significant effects under some specifications, the results are very sensitive to the inclusion of particular countries, so I prefer not to present the results as evidence of UPBC.

[34]Canes-Wrone and Park (2012) choose the identity of the major parties beforehand. Since I have a larger sample I pick the two parties most voted in an election and compute the absolute difference between the two (as in Pontusson and Rueda, 2008). I only use election-country pairs where the second most voted party gets at least 25% of the vote and where the median position is between the two. Canes-Wrone and Park (2012) use a dummy for higher (lower) than median polarization, while I use the continuous variable.

[35]The project codes gatherings of 50 or more people to make a demand of the government since 1990 using newspaper sources. Among other features it is coded a categorical indicator of participation and whether protesters engaged in violence against the state. I use the log of one plus the number of protests in a quarter, where protesters didn't engage in violence and where participants were above 10,000, the maximum threshold reported.

[36]To be consistent with the UPBC estimations I also ignore here the years 2008-2009.

[37]In this specification I include a lag of the dependent variable on the right hand side.

[38]In this case I allow lags and leads, Lg_e and Ld_e, to be different than in the baseline depending on the specification. Also, because of its nature, I allow the proxy for protests to vary during the quarters surrounding an election.

[39]They find a positive effect on GDP excluding investment, which they interpret as evidence of a opportunistic PBC, covered by the effects of uncertainty. The results I find for demand don't show this, which may be due to the different specifications and sample sizes.

[40]In both cases the baseline estimation produce larger effects when restricting to the smaller samples used in panels B and C. Hence the relative effects of these alternative variables are even smaller, specially for investment.

[41]Note however that these variables are "bad controls" in the sense that they capture part of the transmission mechanism from inequality to economic outcomes (see Angrist and Pischke, 2008), and thereby results are not straightforward to interpret.

[42]As already mentioned, this index is also available for some of the countries appearing in the panel estimations, but results in that case are not statistically significant. This may be due to the small sample size or the difficulty of comparing the index across countries (see footnote 33). In the case of the US there is a quarterly variable available for the whole sample.

[43]Although these series are available from 1913 I only consider the postwar period.

[44]The index shows an upward drift since the 1960s. According to Baker et al. (2016) this may have been caused by rising political polarization or the growing economic role for government.

[45]In the panel estimations the standard deviation of inequality in the US is 15% larger than the average. Then, according to the panel estimations, a change in one standard deviation would mean a maximum and total loss of

0.67% and 1.15%, respectively. These numbers are the ones should be compared to the 0.9% and 1.4% found here.

[46]Unlike GDP the variable for PU is just a proxy. Actual PU may be more persistent that what its proxy shows.

[47]An alternative indicator that may be used is the partisan conflict index (PCI), constructed by Azzimonti (2018), which measures lawmakers disagreement about policy. Unfortunately the quarterly series is only available since 1981. This not only reduces the sample considerably, but also leaves exclusively the period of rising inequality in the estimations, which complicates the interpretation of the results as they may be due to any other trending variable. In any case, when using the detrended PCI as a dependent variable in equation (2) I find two quarters of statistically significant values in the impulse-response function for the differential effect between the high and low inequality benchmarks. The first is negative and happens the quarter before the election. The second is positive and happens the quarter of the election. It persists to the following quarter but it looses its significance.

[48]The source for all these variables is BEA, and all of them are seasonally adjusted. For public spending I use Federal Government Consumption Expenditures and Gross Investment.

[49]I don't show the results but when differentiating between durable and nondurable consumption, both fall around elections under high inequality, but the response of the first is much stronger, with a maximum fall that is more than twice larger than the fall in total consumption.

[50]Indeed the difference is significantly reduced when leaving the election of 1948, the first in the sample, out of the estimations. The measures of income inequality are very volatile around that election. I show below how the baseline results change when leaving each of the elections out of the estimations. The 1948 election is not relevant in that case.

[51]The trend has positive coefficients meaning that over time GDP has been reacting more positively to elections. Then larger coefficients on inequality counteract this effect in the last decades.

[52]Similarly than in the panel estimations an additional exercise that can be done is to obtain proxies for variables capturing the transmission mechanism of the UPBC, and see how they relate to inequality, and if they can explain part of the estimated fall in GDP. Two suitable options are the long series of Congress' political polarization build by McCarty et al. (2016), and the also long (annual) partisan conflict index constructed by Azzimonti (2018). Unfortunately, for the period analyzed here, these two variables exhibit a marked drift and hence results might be capturing other trending variables. However, after adjusting these series by linear trends results are in line with what I found in the panel estimations; they can explain a fraction of the UPBC when replacing inequality by any of them in equation (2).

[53]For consumption I use food, housing, transportation, clothing, trips, and other recreation expenditures. A narrower measure that only includes food, housing and transportation is available since 1999. I use the first one as it is more representative but results are similar when using this alternative measure.