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Electoral Effects of Public Sector Austerity Efforts in the United Kingdom 1900-2015

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Abstract

This paper investigates empirically whether large expenditure cuts and revenue rises that were the result of deliberate political efforts towards being austere had an impact on electoral outcomes in the UK using data from 1900 to 2015. The main electoral outcomes considered are the change in ruling party ideology and the margin of victory faced by the incumbent at the general election, in terms of seats secured. The paper finds that large cuts in spending and large rises in revenue significantly increases the chance of a government changing. However, the loss in seats were significantly higher for the incumbent compared to the winning party only when large spending cuts were pursued rather than revenue increases during the incumbent's tenure in office. We also find that voters are sensitive to particular types of spending cuts, such as cuts in social security. These results are contrary to those in Alesina et.al (2013) using OECD data for 19 countries from 1975-2008, and several other papers in the empirical literature that found no significant correlation between fiscal adjustment and electoral outcomes.

Keywords: Fiscal austerity, electoral outcomes, United Kingdom

JEL Classification: H2, H3, H5

Electoral Effects of Public Sector Austerity Efforts in the United Kingdom 1900-2015

An older conventional wisdom, going back long before the 'median voter theorem' that became widely accepted in modern political science (Meltzer and Richards 1981), suggests that large discretionary cuts in government expenditure, tax increases and overall fiscal contraction may have dire consequences to the governments that implement such policies, at the elections that follow. The common, though often at least half-implicit, claim behind this view is that a mixture of party competition to woo median voters with tax cuts and extra services or benefits, together with 'retrospective voting' on the part of voters paying attention to parties' past record in office as well as their promises for the future (Fiorina 1981), gives median voters in marginal constituencies the motive and opportunity to punish parties in government that raise taxes and/or cut spending on services to those voters. And that kind of electoral logic, sometimes linked with claims about so-called 'political business cycles' in spending and taxation, is often said to underlie a pattern of long-term relative growth in public, and particularly welfare, spending in developed democracies (see for example Rose and Peters 1978) .

Empirical results, however, do not seem to support the clear-cut propositions of that older conventional wisdom. Empirical evidence for the pattern of redistributive public spending predicted by the median voter theorem is at best mixed (see for example Ansell and Samuels 2010). Evidence for clear-cut political business cycles has proved elusive as well (Drazen 2001). For example Kraemer (1997) found in a study of political business cycles in Latin American countries that voters did not reward 'loose' fiscal policies especially before elections. In a similar vein, Andrikapolous et. al (2004) did not find evidence to support the contention that incumbent national governments of the EU member states manipulated fiscal policy instruments to create political business cycles over the 1970-1998 period. In contrast, using data more frequent than those at annual intervals, Akhmedov and Zhuravskaya (2004) find strong evidence of opportunistic behaviour in Russia, a young democracy, over a decade while Alesina and Paradisi (2014) find evidence of political budget cycles for Italian cities, with municipalities choosing lower rate on a specific tax closer to elections. Even the idea that policies of general austerity lead inevitably to electoral defeat has been challenged. In particular, a study by Alberto Alesina and his colleagues (2013), using data for a sample of 19 OECD countries from 1975 to 2008, found that there was no empirical evidence to support the contention that governments that reduce budget deficits quickly were systematically voted out of office. They concluded from that general analysis that governments can reduce deficits decisively avoiding an electoral defeat. Similar results have been found in some other empirical studies, such as those of

Alesina, Perroti and Tavares (1998) who used OECD data and of Peltzman (1992) who found that US voters tended to behave as 'fiscal conservatives' by not systematically rewarding state Governors who increased spending.

It almost seems as if a new received view might be emerging on the basis of such studies, positing that spending retrenchment and tax rises are not automatic or fatal electoral toxins for governing parties. What should be noted, however, is that most empirical studies assume that a government's pursuing of austerity policy can be identified purely through outcome data- mainly changes to budget deficit as a proportion of GDP and sometimes spending and revenue as proportions of GDP. But changes to outcome data will not necessarily capture changes due to deliberate political effort.

For example, the literature surrounding fiscal consolidation identifies 'episodes' of consolidation by looking at the cyclically-adjusted primary balance (CAPB). The raw data is cyclically adjusted in order to capture the effect of deliberate policy decisions taken by the government that leads to changes in the budget deficit rather than those that are simply the automatic effect of business cycle fluctuations¹. But the use of cyclically adjusted data to proxy fiscal consolidation efforts can be problematic for four reasons.

The first relates to the concern that changes in government revenue and expenditure maybe correlated with other short-term developments (unrelated to business cycle effects) that are associated with economic activity rather than policy changes as discussed in Romer and Romer (2010), Milesi-Ferretti (2009), Morris and Schuknecht (2007), and Wolswijk (2007). For example, a stock market boom could improve the CAPB by increasing capital gains and cyclically adjusted tax revenues. Thus the cyclically adjusted figures may be misjudging the true extent and time period of consolidation efforts driven by *discretionary* fiscal policy.

A second issue relates to the sensitivity of automatic response to fiscal variables over time. The usual assumption in calculating CAPB is that this response (or elasticity) is constant over time. However in deep recessions the responsiveness can be higher, meaning that discretionary fiscal policy meant to reduce the deficit carried out during times of deep recession may not be captured by the standard method (Wolswijk 2007).

Third, deliberate efforts by the government to reduce spending and/or increase taxes will not always be reflected as a deficit reduction (cyclically adjusted or otherwise), especially if such measure were carried out at a time when GDP was falling due to adverse economic conditions as was the case of the UK in the 1920s. In this case deliberate fiscal

¹ But as noted by Alesina and Ardagna (2012:5), cyclical corrections are 'notoriously imperfect and arbitrary to some extent'.

squeeze efforts are not captured at all when using deficit changes as a starting point, and the focus should be on revenue and spending data separately and not together.

An alternative to simply using CAPB include the 'historical approach' to defining episodes as adopted by Guarjardo, Leigh and Pescatori (2011) and Romer and Romer (2010). Under this method historical documents such as contemporaneous policy documents and narrative records can be used to help identify the motive behind various fiscal policies as well as the size and timing of episodes of consolidation. This would allow to identify economic responses to *past* decisions and economic conditions rather than *prospective* conditions, and are therefore unlikely to be systematically correlated with other developments in economic activity, leading to econometric results that look for the causal effect of fiscal policy on macroeconomic outcomes more robust.

In this paper, we identify episodes of discretionary cuts in spending and increases in revenue (which we define as 'austerity efforts') use both outcome data and contemporaneous policy documents/ narrative records (budget speeches, parliamentary minutes, archived documents)² to identify episodes of *large* austerity efforts. The exact method will be discussed in the next section. Using narrative records help to identify the 'boundaries' of the squeeze more clearly as well as ascertain qualitatively the extent of political effort that went into planning and implementing such efforts.

We consider the relationship between austerity efforts and electoral outcomes by looking at a single country case, that of the UK, between 1900 and 2015 rather than looking at a number of country cases over a range of different times. The distinct advantage this affords is to see how a given political system behaves in a range of fiscal situations over time. This is particularly so in the case of the UK that is traditionally cited as the classic 'majoritarian' state, with a parliamentary system that tends to concentrate power in the central government and a first-past-the-post plurality electoral system for UK elections that tends to produce single-party majority governments much of the time, making it easier for voters to identify which party to blame for adverse economic outcomes (Lijphart 1999). So if the 'old conventional wisdom' about the relationship between fiscal adjustment and electoral outcomes applies anywhere in developed democracies, it ought to be observable in the UK.

The central concern of this paper is to test whether there is a significant systematic relationship between large discretionary cuts in spending and rises in revenue and electoral outcomes. The latter is measured two ways: first by looking at whether or not there was a change in the party orientation of the Prime Minister compared to the previous year. and second by proxying the margin of victory faced by the incumbent at the general

² A more detailed discussion of such sources and evidence found in the individual chapters of Hood and Himaz (forthcoming).

election. The latter is measured by the gap in the proportion of seats secured by the incumbent at the general election compared to the closest competing party: A positive number and a larger gap will indicate a higher margin of victory enjoyed by the incumbent. This latter measure better captures the extent of voter retribution. The paper also looks at how responsive electoral outcomes were to government expenditure disaggregated as social security, health, education, defense, public order and transport- a sensitivity that is overlooked when aggregating various categories of expenditure together.

The paper is organised as follows. The next section describes the data used in the paper and looks descriptively at the incidence of large discretionary cuts in spending, rises in revenue and deficit reductions based on outcome data and narrative records, as well as the incidence of elections and changes in ruling party. Section 3 then analyses more systematically the impact of fiscal adjustment on electoral outcomes using regression techniques. A number of checks of robustness and further regressions are described to assess how far the results are consistent across a variety of specifications. In section 4 we turn to the ever-present issue of causal direction to explore whether 'reverse causality' (that is, the possibility that strong governments that carry out fiscal adjustments may be more likely to stay in power than weak ones) may be in play. We argue that our results are robust to this possibility, as far as they can be empirically tested using the data available. Section 5 provides conclusions, returning to the general issues of fiscal adjustment and electoral outcomes discussed above.

2. Data and Descriptive Statistics

Given the time-period covered by this analysis, the data is taken from various sources. For the period from 1900 to roughly the 1950s, most series such as total current expenditure, disaggregated current expenditure (on health, social security, defence, transport, public safety, education) and GDP were taken from Mitchell (1988). Data for the 1950 to 2015 period were taken from ONS and Treasury sources (see Data Appendix for more details). That means that almost all the series have one discontinuity around the 1950 period due to differences in sources, and the analysis has to be interpreted with such discontinuities in mind. We have not made any cyclical adjustments to the data given such corrections are 'notoriously imperfect and arbitrary to some extent' (Alesina and Ardagna 2012:5)³, and because the use of such corrections consistently for a data series that span over century, such as ours, will be difficult and almost impossible given data constraints.

Graph 1, below, shows how the UK government's overall current expenditure and revenue as a percentage GDP changed between 1900 and 2015. As can be seen, public expenditure

³ Alesina, A and S. Ardagna, (2012). "[The design of fiscal adjustments](#)," NBER Working Papers 18423, National Bureau of Economic Research, Inc.

peaked during the period of World War I (1914-1918) and II (from 1939 to about 1946) reaching a height of 70 per cent of GDP at its apogee. This peak was dominated by a sharp rise in defence expenditure, and the subsequent fall was also almost exclusively constituted by reductions in defence spending, with increases in every other major category of government spending.

GRAPH 1 HERE

Since we are concerned here with the impact of large discretionary cuts in spending and raises in revenue on electoral outcomes, we now move onto defining a 'large episode', 'discretionary' and 'electoral outcomes'. In the conventional literature on fiscal consolidation, episode identification usually computes thresholds based on the fiscal outcome measure (i.e., spending, revenue, deficit) as proportion of GDP. We abide by this convention to some extent and start off identifying episodes of spending cuts (revenue rise) as one where spending as a ratio of GDP fall (rose) by more than 1 percentage point on average for two or more consecutive years. Similarly, a deficit fall episode is indentified as one where the fiscal deficit (revenue *minus* spending) falls by more than 1 percentage point of GDP. Column 1 in Table 1 below presents episodes so defined using the 'ratio method'. The column shows that spending cut episodes were much shorter earlier on in the century, lasting 2-3 years while in the more recent decades are much longer. The depth of the cuts also seem much shallower in recent years than earlier on in the century.

The effort is 'discretionary' if our archival analysis and narrative records indicate that the austerity efforts were deliberate on the part of the government. The reasons can be various: The 'Geddes Axe' of the early 1920s were deliberate cuts triggered by tax rates that were deemed far too high. Notably, the Geddes cuts were not triggered by the presence of a large budget deficit. Indeed, the budget was almost balanced at the start of the episode due to following the McKenna Rule of balancing budgets on a year-on-year basis. In contrast the 2010 cuts in the wake on the financial crisis were largely a response to large government debts and deficits. Similarly, large increases in revenue identified through basic data analysis are deemed discretionary only if the historical records support this. Thus the 1926 increase in revenue by nearly 4% of GDP is not counted as a discretionary increase as it was associated with exchange rate changes rather than government discretion. Again increases in revenue could be for reasons other than fiscal consolidation, such as raising resources to finance a war⁴.

⁴ The data indicates a 3 percentage point rise in revenue in the early 1980s that was driven in part by the windfall North Sea oil revenue and in part tax revenues. Thus the discretionary aspect of this revenue rise is much less than the numbers hint, on their own.

The narrative record seems to indicate, however, variations in political effort and costs expended towards planning and implementing such efforts. On the lower end of the political effort spectrum on the revenue side are inertia strategies such as resistance to tax cuts, acceptance of taxes already in the pipeline. Requiring more political effort is the imposition of less visible and salient losses such as 'stealth taxes' (e.g., parking fines and other fees that are not counted as 'taxes')⁵. Fairly high costs are incurred with the imposition of visible and salient losses such as levying taxes on key voters or funders or breaking key election promises over taxes. On the spending side, examples of low, moderate and high political effort would be resistance to pressure for spending increases, stealth cuts (e.g., non-indexation of benefits) and the imposition of visible cuts in spending of high salience to key voters or funders (Hood and Heald (2014: 7)). The qualitative judgements made about how much political effort was exerted seems closely correlated to whether the austerity efforts were also accompanied by real changes in the respective variables (i.e., falls in spending and rises in revenue in constant price terms). The spending cuts and revenue increase episodes in the last three decades, for example, show changes in the relevant direction as a proportion of GDP, but not in constant price terms on average (see columns 2 and 3 of the table above). With spending cuts, this decision was deliberate. When the cuts of the Thatcher government in 1980s were pursued, the then Chancellor Nigel Lawson was careful to make sure that the cuts were made relative to GDP and not constant price terms (Hood and Himaz (forthcoming)). This pattern was followed in the 1990s cuts as well. Similarly in the most recent 2010s cuts, the ambitious plans set out in the 2010 June budget as soon as the coalition government came into power were restrained to follow the plans of the previous government budget, as GDP growth fell below expected levels. Thus the spending cut efforts earlier on in the century seemed politically harder than the austerity efforts later on in the century⁶.

Thus we define a 'large' spending cut (or revenue rise) episode as one that also indicated a percentage fall (rise) of 1% compared to the previous year in constant price terms, together with being identified as an episode under the ratio method.

We define 'electoral outcomes' as changes to the party-orientation of the Prime Minister as a result of electoral choices by the voters at general elections⁷. There were 29 general elections held in the UK during the 1900-2015 period, with the ruling party changing in 17 of

⁵ The significance of these stealth taxes have been increasing over the century with the most recent pre-budget autumn statement in 2015 outlining revenue increases amounting to nearly 1% of GNP via stealth taxes.

⁶ This is true to some extent for revenue increases as well. The 1980s benefited from windfall revenue gains in terms of north sea oil taxation and privatisation proceeds that were not available to that extent during other periods.

⁷ In the case of the UK there were 10 instances between 1900 and 2015 where the prime minister changed without an election being held. In most of these cases the change was due to ill health or other reasons not directly due to unpopularity over fiscal squeeze.

these elections.⁸ In our dataset, this variable is defined as ‘change ruling,’ which is a dummy variable that equals 1 if there was a change in the ruling party at an election and zero otherwise.

When trying to model the impact of fiscal outcomes on consequent electoral outcomes, issues of timing are important. If voters are assumed to vote anticipatively, we might expect ‘anticipation effects’ of spending cutbacks and/or tax rises announced but not yet implemented to shape electoral outcomes. But if voters are assumed to vote primarily in a retrospective way, assigning electoral credit or blame on the basis of what has already happened to spending or taxes, it is the outcomes of fiscal adjustments that will primarily shape electoral outcomes. Since, as noted earlier, much of the conventional discussion about expected electoral punishment for fiscal adjustments rests on assumptions about retrospective voting, we focus here on the *implementation effect* rather than what can be called the *announcement effect* of fiscal adjustments.

We now move to an overall look at large fiscal adjustments and electoral outcomes in the UK using these definitions over the period 1900 to 2015.

As seen in Table 1, 9 periods indicate average spending cuts over 1 percentage point a year using the ratio method in the UK between 1900 and 2015. However, only 5 qualify as involving ‘large’ discretionary expenditure cuts, indicating both a fall in spending as a proportion of GDP and in real terms as well as being supported as involving deliberate political effort, as defined previously. These periods are 1919-21, 1923-25, 1933-35, 1946-49 and 1976-79.. During these periods of large cuts or two years soon after it, there were 71 general elections where the ruling party and/or its leader changed 5 times: Roughly 71 per cent of the time. That incidence of change is higher than the overall average of 55.1 per cent (i.e., the number of times the ruling party changed after a general election, regardless of whether there were fiscal adjustments or not⁹). If we consider only the three cases after 1980 where cuts were pursued but were not ‘large’ in terms of being a fall both in real terms as a proportion of GDP, we notice that government changed during 2 out of 5 elections (40 per cent of the time). From that broad-brush analysis, episodes of ‘large’

⁸ The general elections were held in the UK in 1906, 1910 (twice), 1918, 1922, 1923, 1924, 1929, 1931, 1935, 1945, 1950, 1951, 1955, 1959, 1964, 1966, 1970, 1974 (twice), 1979, 1983, 1979, 1983, 1987, 1992, 1997, 2001, 2005 and 2010. Although an election was held in 1900, it is not considered, as the regression results reported later in the paper do not include the year 1900 due to losing this data point when calculating changes to monetary variables.

⁹ As was noted earlier, there were 29 general elections during the 1901-2015 period with two elections held in 1910 and 1974. Due to the fact that we do not have fiscal and economic variable information for a part of the year, the econometric analysis later on in the paper considers only one data point for the years 2010 and 1974 taking into account the second rather than first election in both years,... Thus our paper considers only 27 elections and 15 changes in the ruling party rather than 29 elections and 16 changes. Hence an overall average of 55.5% rather than the actual 55.2%. Note also that we consider an election outcome reported as ‘party yes’ or ‘yes’ as being representative of a change in government.

expenditure cuts in the UK do indeed seem to have been followed by changes in ruling party and/or who led it to a greater extent than the sample average or periods during which 'softer' cuts were pursued.

TABLE 1 HERE

Table 2 below summarizes episodes of rises in government revenue based on the ratio method. There were 7 'large' episodes. These correspond to the 1916-21, 1931-32, 1941-45, 1960-61, 1964-69, 1973-75 and 1980-81 periods. There were 9 elections during this period out of which government changed 6 times or 67 per cent of the time which is higher than the average incidence of government change following an election which is 55%. As with large spending cuts, large revenue increases also seem to be associated with a higher incidence of government change.

TABLE 2 HERE

We can also look at how large deficit changes associate with the incidence of government change. An important point to note is that episodes of revenue and spending based austerity are not always reflected in episodes of deficit change. We compute budget deficit as the difference between revenue and spending and define, as described previously, a 'large' overall fiscal adjustment to mean an episode where the deficit to GDP ratio falls by more than 1.0 percentage points per year on average or 2 percentage points in a single year. Graph 2 summarizes what happened to the deficit to GDP ratio in the UK over the 115 years from 1900 to 2015.

GRAPH 2 HERE

Table 3 summarizes the eight 'large' fiscal adjustments in the UK over the period shown by Graph 2. As can be seen, the largest such adjustments took place in the aftermath of the two World Wars but there were no elections held during this period. The 1994-2000, 1968-69 and the ongoing 2010-2015 episodes also witnessed large adjustments in that sense. Some of the adjustments summarized in Table 3 were led by reductions in expenditure, such as the 2010-15 squeeze in deficits. Table 3 also indicates that the ruling party the ruling party or who led it changed roughly 58% per cent of the time as a result of elections (7 out of 12 times). This incidence is not too different to the overall average of 53.8 unlike the cases where we looked at episodes that were large spending or large revenue squeezes . . .

TABLE 3 HERE

3. Estimation and Results

Moving from the descriptive account given in the previous section, this section looks at the impact of expenditure, revenue and deficit change on electoral outcomes rather more systematically, and taking account of a wider range of economic and political factors. We first look at whether changes in the ruling party in the UK between 1900 and 2015 are correlated with changes in government expenditure to GDP, together with other major economic factors such as changes in GDP, unemployment and the rate of inflation. We also include a wider range of political variables in the analysis, namely, 'duration', indicating how many years an incumbent party has been in power (on the grounds that longer tenure makes it harder for incumbents to blame their predecessors for fiscal adjustments that impact on voters), whether the government is a coalition or not (exploring the idea suggested earlier that voters may find it more difficult to target blame for fiscal adjustments in coalition rather than single-party governments) and the size of governing party's or parties' majority in Parliament (proxied by the proportion of seats the incumbent party or parties hold in parliament), to explore the idea noted earlier that the degree of 'strength' of a party in government may affect its ability to implement fiscal adjustments without loss of office at subsequent elections. Since the years of World War I and II between 1914-1918 and 1939-1945 cause a very notable break in public spending we also included a dichotomous variable indicating whether or not the year concerned was a 'war year'. As noted earlier, Probit estimation is used and marginal effects are reported.

TABLE 4 HERE

The results are shown in Table 4. Column 1 indicates that any fall in public expenditure (CHEXP) seems to increase the probability of a government change, but this effect is not significant. Similarly, Column 2 indicates, inter alia, that changes in revenue do not have a significant impact on the chance of government change. Neither does an increase in government deficit (measured as expenditure minus revenue as a proportion of GDP), as seen in column 3. In column 4 we include an additional dummy variable 'LARGE_EXP' reflecting the fact that the year concerned was one where the discretionary spending cut was fall as a percentage of GDP and real terms as defined earlier. The coefficient on this variable indicates that the incidence of government change in a year of hard spending cuts is positive and significant. In column 5 we report results for the same regression in column 2, with the addition of the dichotomous variable 'LARGE_REV' that indicates revenue was increased both as a percentage of GDP (by more than 1 percentage point on average across the episode) and in constant price terms. Again the sign and significance on this variable indicates that when the revenue rise was substantial, it correlates with a higher incidence of

losing power at the subsequent election. We also ran the same regression as in column 3 with 'LARGE_DEF' added.. The coefficient on this variable is not significant (column 6).

To test the robustness of these results we re-estimate the regressions in columns 1-6 just for the post world war 2 period (after 1950), and find that the key difference is that hard revenue squeezes cease to be significant as do hard deficit reduction episodes, unlike episodes of hard spending cuts which continue to result in voter punishment of the incumbent (unreported). None of the economic variables such as the change in unemployment, GDP or inflation are significant in any of the regressions in the table. However, confirming the idea that long-lived governments find it harder to blame their predecessors, the longer has a party stayed in power the higher is the probability of government change as seen by the significantly positive coefficient in the DURAT variable. The results also show that if the incumbent occupied a larger proportion of seats, suggesting being a popular government at the time of being elected and perhaps a strong one, there is a reduced chance of a change in the ruling party. The 'war year' effect is also marked, but that is because there were no General Elections between 1935 and 1945 as a result of normal election terms being suspended during World War II and similarly during World War I.

We now move onto seeing if *average* economic performance and fiscal aggregates *over the term of government* (from one general election to the other) had an impact on the chance of government change as defined earlier. There were 29 General Elections in the UK between 1900 and 2015 (excluding the 1900 election which we don't use as we calculate averages over the tenure). As noted earlier, two elections were held in 2010 and 1974 but we account for one election in each of these years due to the fact that we only have annual data for economic variables. Thus we have 27 data points. The explanatory variables are the same as the base line specification, with the key difference being that they are the average for the tenure in government that immediately precedes the elections. The results, reported in columns 1-5 of Table 5, are similar to what we have already observed: None of those average fiscal variables seem to correlate significantly with the chance of government change. However, if the tenure was one that exhibited on average a hard spending squeeze, there was a higher chance of losing office, significant at the 10 per cent level. As before, if the government had a larger majority there was a significantly lower chance of losing office while longer the government stayed in power, significantly higher was the chance of losing office.

We then changed the specification of the dependent variable (i.e., the electoral outcome variable) to reflect the margin of victory (or defeat) experienced by the incumbent based on the difference seats secured at the election compared to the closest competitor. The results are reported in Table 5, columns 6-10. Again we notice that although the correlations between the fiscal outcome variables such as average change in expenditure or revenue over the tenure and distance in seats is not statistically significant, if the tenure was

one that had a hard expenditure cut, it correlates significantly with a lower number of seats secured by the incumbent compared with the closest competitor. This is statistically significant at the 10 per cent level¹⁰.

TABLE 5 HERE

To test for robustness of our basic specification as explained earlier, we run different versions of that specification. This includes re-estimating all of the regressions using after including changes to US GDP growth variable to proxy world economic performance (results unreported), to explore the possibility that the electoral impact of fiscal adjustments might vary according to the buoyancy or otherwise of the global economy, with cutting in a global recession likely to have more severe effects on voters than cuts imposed in more benign economic conditions. Plausible as such claims might seem, this analysis indicated that this variable was not significant and did not change the sign, size or significance of any of the other coefficients. We also re-estimated all the above regressions using average values for the two years immediately preceding the elections, to see if any of the observed effects were stronger, due to voter sensitivity to expenditure cuts and revenue (or tax) increases increase closer to general elections. The results remain the same as in the baseline case, with no strong evidence to suggest increased voter sensitivity to fiscal squeeze closer to elections.

We now move onto estimating whether electoral outcomes are more responsive to particular types of expenditure. We therefore consider average changes across the tenure to expenditure on social security, health, education, defence, public order and transport. As column 1 in Table 6 indicates, increases in social security expenditure is significantly positively correlated with a smaller chance of government change, at the 5 per cent level.

¹⁰ The variable 'distance seats' is a reflection of how strong the position of the incumbent is, compared to the main opposition at the election. This is proxied by the gap in seats between the incumbent and its closest competitor. If the government was a coalition, the comparison is between the party that secured the majority of seats within the coalition and the closest competing party. If the distance in seats is positive, this reflects the fact that the incumbent won the election and secured the extra proportion of seats, reported in the relevant cell. The only exception is the 1923 election where the distance is positive but the incumbent lost the election. At the 1923 election the incumbent Conservative party secured 41% of the seats compared to its closest competitor, the Labour party, that secured 31% of the seats. But Labour formed a minority government, propped up by the liberals for key votes.

TABLE 6 HERE

We also look at whether average economic performance and disaggregated fiscal spending over the term of government (from one general election to the other) had an impact on the chance of government change as defined earlier. The results are provided in Table 7 where columns 1-6 use change in ruling party as the dependent variable and columns 7-12 use distance in seat as the dependent variable. Column 1 indicates that increases in social security reduces the chance of government change. Columns 8 and 9 suggest that voters may be responsive to health and education expenditure cuts. Although we cannot conclude that there is overwhelming evidence to support that voters are sensitive to particular types of expenditure cuts, the results do suggest the possibility, especially in terms of social security expenditure and change of government.

TABLE 7 HERE

Altogether, the results show that when fiscal outcomes such as expenditure and revenue are aggregated, then changes to those variables are not associated significantly with electoral outcomes, similar to the conclusions of Alesina et al (2013) and Alesina et al (1998). However, electoral outcomes seem responsive to large cuts in expenditure that are cuts both as a percentage of GDP and in real terms. Moreover, when expenditure is disaggregated into its various components, we find some evidence for the UK to suggest that voters may punish governments for cutting particular types of spending such as those on social security..

4. Accounting for Reverse Causality

The reason that even large fiscal adjustments do not seem to have an impact on the chances of a change government may simply be due to the fact that 'stronger' governments that implement fiscal adjustment are more likely to be re-elected than weaker governments that implement fiscal adjustment. To take a hypothetical example, if any given fiscal adjustment might be expected to cause such displeasure amongst 10 per cent of the supporters of an incumbent party that they switch political allegiance, such a policy being implemented by a party that has won government with a landslide majority of 75 per cent or so of the voters will still leave the incumbent with a comfortable majority, in sharp contrast to the position of a party that has scraped into government with bare plurality of votes if large fiscal

adjustments causes 10 per cent of their supporters to switch sides. Thus there is a possibility that when a strong party in government (in the sense of having a large majority) implements a cut it has a higher chance of being re-elected than a party in government with a smaller majority. As Alesina et al (2013) point out, the argument here is not necessarily that 'strong' governments are more likely to implement unpopular fiscal policy but simply that stronger governments are more likely to be re-elected in spite of unpopular policy.

To test such a proposition requires finding a convincing proxy for the strength of parties in government before elections, and that is not a simple task. Alesina et al (2013) suggest that a coalition government may be 'weaker' than a single-party government, since the former is more likely to have internal disagreements than the latter. But that seems to confuse 'strength' in the sense of size of majority (as in the hypothetical example given earlier) with strength in the sense of cohesiveness, and it could be objected that the parties forming a coalition government may be more able to dodge blame at the polls for large fiscal adjustments than single parties in government, since in a coalition the parties may find they have opportunities to blame each other for decisions taken in government, thereby making it harder for voters to decide who to punish. Indeed, the fact that the UK's 115 year history from 1900 to 2015 does not offer many instances of coalition government at national level¹¹ might even be a factor in explaining why the data analysed here points to different conclusions to those reached by the Alesina studies. Nevertheless, we include this variable in our analysis along with what might be considered a more straightforward indicator of 'strength' in the sense discussed earlier, namely the proportion of Parliamentary seats secured by the incumbent party as a proportion of total seats.

To capture any specific effect on the chance of change in government that is associated with stable governments implementing large fiscal adjustments as defined earlier, we therefore re-estimated the baseline regressions including two interaction terms, namely the fiscal variable interacted with the coalition variable and the fiscal variable interacted with the strength-of-majority variable. The results (unreported) indicate that neither of these variables is significant, suggesting that the political strength of the government that implements a large fiscal adjustment is not associated with a significantly higher or lower chance of an electorally-induced change of government as we defined it earlier.

5. Conclusion and discussion

Contrary to several cross-national studies, these empirical results based on UK data from 1900-2015 provide some evidence to support the contention that large discretionary

¹¹ . Apart from the Lloyd George coalition up to 1922, the National (but increasingly Conservative) government from 1931 to 1940, the World War II coalition, the period 1977-8 when there was a pact between the then Labour government and the Liberal party that fell short of a coalition and the Conservative-Liberal Democratic coalition in 2010.

expenditure cuts may well have a significant impact on the chances of a change in the ruling party or who leads it as a result of electoral choices by voters in general elections. This result is robust to the various specifications we have employed in this paper, albeit at significance levels of 10 or 5 per cent. This analysis thus suggests (consistent with retrospective voting ideas) that the *implementation* effects of large spending cuts are associated with reduced chances of incumbent retention of office. This result is corroborated by the fact that large expenditure cuts seem to result in the incumbent facing a significant loss in seats at the general election compared to its closest competitor. The analysis also suggests that voters are more sensitive to cuts in social security and health expenditure with this increasing the likelihood that governments are voted out of office and facing a tougher competition at general elections. Overall, therefore, there is some evidence, for a correlation between large expenditure cutbacks and electoral retribution in the UK suggests that the older conventional wisdom about the electoral effects of budget-cutting may have something to be said for it after all, and provides some justification for why governments are often reluctant to whole-heartedly pursue expenditure-led policies of fiscal adjustment.

This analysis also raises a number of questions that merit further consideration. One is that of apparent 'UK exceptionalism'. Is the sensitivity of electoral outcomes to large austerity efforts due to UK's peculiar political features as noted in the introduction such as its odd first-past-the-post electoral system and relatively low incidence of coalitions at central government level (such that electoral blame for pain imposed through fiscal adjustment is easier for voters to attribute)? Or might we see similar results in other countries over the longer time-period considered here?

Second, as explained earlier, the focus of the analysis here, consistent with ideas about retrospective voting, is on the relationship between the *implementation* effects of fiscal adjustments and electoral outcomes. But the announcement effects of fiscal adjustments on electoral outcomes also merit attention, and that is a matter for future research.

A third issue arising from this analysis, as noted earlier, is why revenue increases seem to be less electorally dangerous to political incumbents than expenditure cuts from this analysis, since revenue (and overall deficit) changes do not have a significant impact on the chances of a change in government. The answer seems to be that revenue raises are not necessarily electorally toxic because they can occur without necessarily squeezing voters, for instance through limited-time gains from sources such as North Sea Oil taxation, privatisation, taxes on the financial sector all common in the 1980s and to some extent in later periods in the UK. What is perhaps more toxic are increases to the marginal rate of tax faced by the median voter. This again is a matter for future research.

Fourth, this study raises some open questions for the analysis of blame-avoidance over fiscal adjustment. For example, the finding here that the longer a party has been in office, the greater are the chances of a change in government is consistent with the idea that blame is

easier to attribute when parties can less plausibly blame their predecessors in office for fiscal adjustments that affect voters adversely. Second, while this study finds that large expenditure cuts are associated with significantly increased risk of electoral retribution in the case of the UK over the period considered here, the effects are far from overwhelming, suggesting that blame can indeed be deflected sometimes, and raising intriguing questions as to when and how blame avoidance outcomes can result.

Data Appendix

National Accounts data

CHEXP: Change in public expenditures: percentage point change of TME to GDP

CHREV: Change in public revenue: percentage point change of total revenue to GDP. Public revenues are computed as government current receipts.

CHDEF: Change in public deficit calculated as CHREV minus CHEXP

dDEFLATOR: Inflation calculated as changes to the GDP deflator taken from www.measuringworth.com; 2008=100

dGDP: Rate of growth of real GDP, percent. Calculated as the percentage change in real GDP at 2008 prices. Nominal GDP figures are deflated using GDP deflator figures. dUNR: Growth of the unemployment rate, percent: $[(UNR_t/UNR_{t-1})-1]*100$.

LARGE_EXP: A dichotomous variable indicating a hard cut in spending. The variable equals 1 if CHEXP fell on average by at least one percentage point every consecutive year as well as falling in constant price terms. For a single year to count as a hard spending cut, CHEXP <-2 should hold as well as spending falling in real terms. The identified episodes should also be discussed as deliberate efforts of spending cuts in the historical narrative as evidenced by policy documents, budget speeches and parliamentary memos of the time.

LARGE_REV: A dichotomous variable indicating a hard increase in revenue. The variable equals 1 if CHREV increased on average by at least one percentage point every consecutive year as well as rising in constant price terms. For a single year to count as a hard revenue rise, CHREV > 2 should hold as well as revenue rising in real terms. The identified episodes should also be discussed as deliberate efforts of revenue increases in the historical narrative as evidenced by policy documents, budget speeches and parliamentary memos of the time.

LARGE_DEF: A dichotomous variable indicating a hard increase in deficit. The variable equals 1 if CHDEF increased in total by at least 1.5 percentage point as well as rising in constant price terms. The identified episodes should also be discussed as deliberate efforts of deficit reduction in the historical narrative as evidenced by policy documents, budget speeches and parliamentary memos of the time.

Political Variables

Change _ Ruling: Change in ruling party or party orientation (left, right, mixed) based on qualitative assessment. Dummy variable equal to 1 if there is a change between current year and next.

Distance seats: The gap in seats won at the general election by the incumbent and its closest competitor. If the government was a coalition, the comparison is between the party that

secured the majority of seats within the coalition and the closest competing party. If the distance in seats is positive, this reflects the fact that the incumbent won the election and secured the extra proportion of seats, reported in the relevant cell. The only exceptions are 1923 and 2015. In the 1923 election the distance is positive but the incumbent lost the election: The incumbent Conservative party secured 41% of the seats compared to its closest competitor, the Labour party, that secured 31% of the seats. But Labour formed a minority government, propped up by the liberals for key votes. In the 2015 election the incumbent party was a Conservative-Liberal coalition. The Prime Minister came from the Conservative party. At the election, the conservatives gained 330 seats compared to the 232 they had gained at the previous election while the Liberals lost securing just 8 seats in 2015 compared to the 57 seats they had secured in 2010. The result was that the government changed from being a Conservative-Liberal coalition to Conservative only, with the incumbent Prime Minister continuing for another term. According to our definition of distance in seats, this means that although there was a change in ruling party, the gain in seats (as a proportion of total seats) for the Conservatives was 0.15 compared to its closest competitor, the Labour party. The incumbent at the 1931 election is considered to be the Labour party (that implemented the austerity policies) and not the National Government formed 2 months before the election.

Duration: The number of years in integers that a cabinet has been in power up to the current year. It is set to 1 the year after the election.

Majority: The proportion of seats held in parliament by the incumbent party.

Coalition: Dummy variable equalling one if the government was a coalition. Zero otherwise.

War year: A dummy variable equalling one if the year was during the first or second world wars where there are noticeable breaks in the data (1914-1918; 1939-1949)

Data sources: Data sources: Public spending data and GDP 1900-1949: Mitchell (1988), chapter XI; 1950 onwards: Office for National Statistics (ONS) and HM Treasury data as compiled by the Institute for Fiscal Studies downloaded on 1 April 2015 from http://www.ifs.org.uk/uploads/publications/ff/lr_spending_Autumn_Statement_2014.xls. Public revenue data: 1900-1949: Mitchell (1988), chapter XI, 1950 onwards: ONS Public Sector Finances Supplementary data February 2014, released March 2014. <http://www.ons.gov.uk/ons/rel/psa/public-sector-finances---supplementary-table/february-2014/index.html>. GDP deflator figures taken from www.measuringworth.com. Unemployment: Data from Hendry (2001); ONS, Annual Budget Speeches, reports and parliamentary memos as documented in Hood and Himaz (forthcoming) Butler and Butler (1988), Section IV; <http://www.election.demon.co.uk/geresults.html>.

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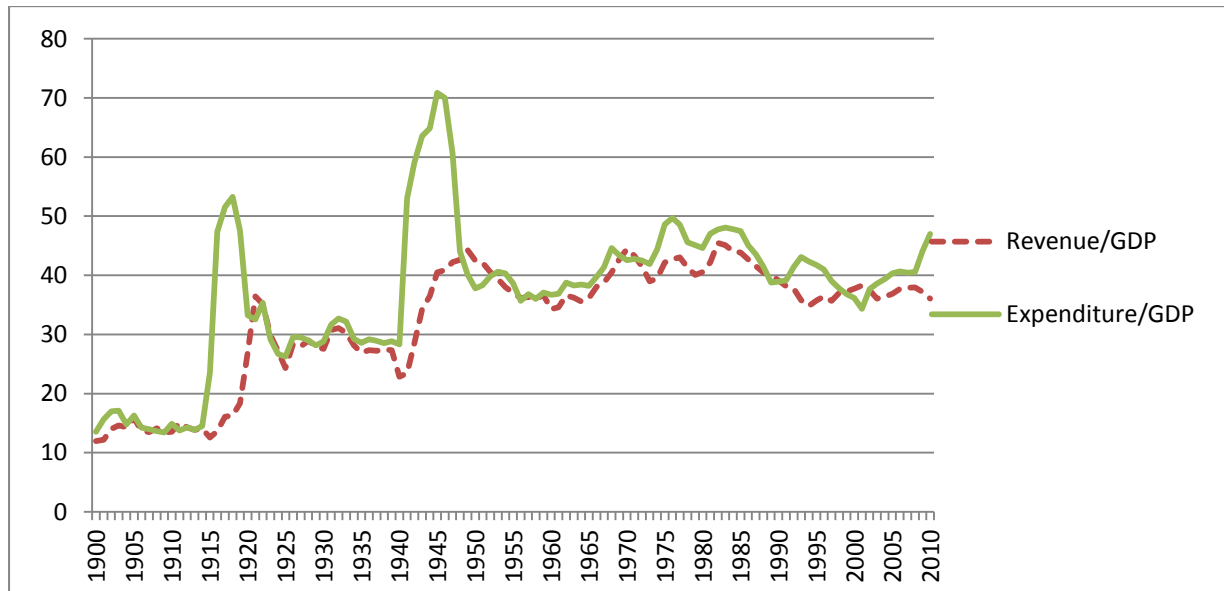
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Graph 1: UK Fiscal Outcome Data, 1900 to 2015



Source: Authors' calculations, based on data sources outlined in the data appendix.

Table 1: Episodes of Large Expenditure Cutbacks from 1900 to 2015 ¹

Episode (ratio method)¹	Length of episode	Average annual % point change of EXP/GDP	Average annual % change in constant prices	Lost office? (election year)	Difference in seat share (election year)²
1919-21	3	- 6.8	- 21.8	Partly yes (1922)	-0.32 (1922)
1923-25	3	- 3.0	- 5.6	Yes (1923) Yes(1924)	0.1(1923), -0.42(1924)
1933-35	3	- 1.3	- 1.0	No (1935)	0.38 (1935)
1946-49	4	- 8.3	- 13.5	No (1950) Yes (1951)	0.03(1950) -0.04(1951)
1968-69	2	-1.3	0.5	-	-
1976-78	3	- 1.5	- 1.2	Yes (1979)	-0.11 (1979)
1983-88	6	- 1.6	0.3	No(1983) No(1987)	0.29(1983), 0.23(1987)
1993-2000	8	- 1.1	0.1	Yes (1997) No (2001)	-0.38(1997) 0.37(2001)
2010-15 ⁴	6	- 1.3	0.3	Partly yes (2015)	0.15 (2015)

Notes:

1. This analysis only includes elections occurring during the squeeze episode or up to two years subsequently, so the elections of 1929, 1992 and 2005 are not considered.
2. The ratio method refers to the calculation of austerity episodes based on spending as a proportion of GDP. If the percentage point difference of this ratio is greater than -1 on average for two consecutive or more, then that period is counted as an episode of spending squeeze, provided the historical narrative supports the fact that the cuts were deliberate on the part of the government. See text for more details.
3. Difference in seat share is calculated as the difference in parliamentary seats (as a proportion of total seats) between the incumbent party and the closest competing party - usually the opposition. See data appendix for further details.
4. Episode still ongoing, at the time of writing.

Source: Authors' calculations, based on data as outlined in the data appendix

Table 2: Episodes of Large Revenue Increases from 1900 to 2015¹

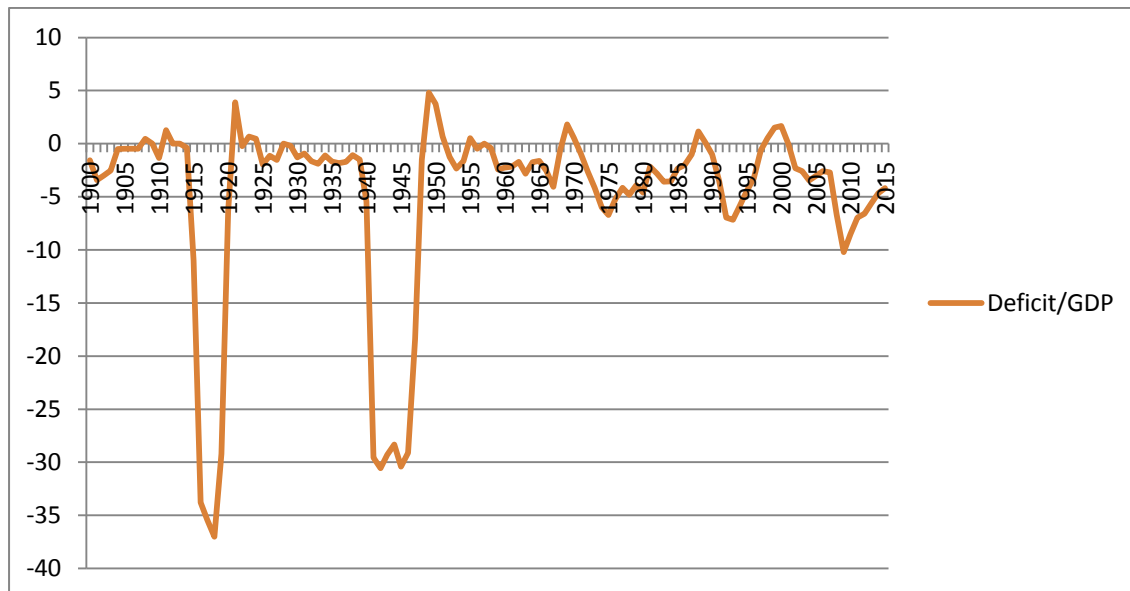
Episode (ratio method)²	Length of episode	Average annual % point change of REV/GDP	Average annual % change in constant prices	Lost office? (election year)	Difference in seat share (election year)³
1916-21	6	3.9	14.0	Mostly No (1918) Partly Yes (1922)	0.28 (1918) -0.32 (1922)
1931-32	2	1.8	4	Partly Yes (1931)	-0.69 (1931)
1941-45	5	3.5	11.7	Partly Yes (1945)	-0.31(1945)
1960-61	2	1.1	6.7	-	
1964-69	6	1.4	6.2	Yes (1964) No (1966) Yes (1970)	-0.02 (1964) 0.18 (1966) -0.07(1970)
1973-75	3	1.3	5.2	Yes (1974 Feb) No (1974 Oct)	-0.01 (1974 Feb) 0.03 (1974 Oct)
1980-81	2	2.5	2.9	-	-

Notes:

1. This analysis only includes elections occurring during the squeeze episode or up to two years subsequently.
2. The ratio method refers to the calculation of austerity episodes based on revenue as a proportion of GDP. If the percentage point difference of this ratio is greater than 1 on average for two consecutive or more, then that period is counted as an episode of revenue based austerity, provided the historical narrative supports the fact that there were deliberate efforts to increase revenue on the part of the government. See text for more details. The mid and latter 1980s, the mid 1990s and the post 2010 period indicate increases in revenue in real terms but not as a ratio of GDP. These periods, therefore, are not identified under the ratio method as being episodes of revenue rise. The historical records support this by indicating that the political effort at revenue increases over the period compared to spending cuts were low to moderate.
3. Difference in seat share is calculated as the difference in parliamentary seats (as a proportion of total seats) between the incumbent party and the closest competing party - usually the opposition. See data appendix for further details.

Source: Authors' calculations, based on data sources as outlined in the data appendix

Graph 2: UK Budget Deficit Data, 1900 to 2015



Source: Authors' calculations, based on data sources as outlined in the data appendix

Table 3: Episodes of deficit reductions 1900 to 2015

Episode (ratio method) ²	Number of years	Average annual % point fall in DEFECIT/GD P	% of deficit fall due to change in spending	Number of General Elections during the period	Change in ruling party after elections
1904	1	2.02	113.8	Yes(1906)	-0.32 (1906)
1911	1	2.64	43.8	-	-
1919-1921	3	13.5	47.5	Partly yes (1922)	-0.32 (1922)
1928	1	1.5	33.32	Yes(1929)	-0.04 (1929)
1943-1949	7	5.34	21.6	Partly Yes (1945)	-0.31 (1945)
				No (1950) Yes (1951)	0.03(1950) -0.04(1951)
1954-1955	2	1.4	183.8	No (1955)	0.10 (1955)
1968-1969	2	3.02	48.14	-	-
1976-79	3	1.1	178.43	Yes (1979)	-0.11 (1979)
1981	1	2.43	-18.97	-	-
1985-1988	5	1.12	242.19	No(1983) No(1987)	0.29(1983), 0.23(1987)
1994-2000	7	1.23	60.62	Yes (1997) No (2001)	-0.38(1997) 0.37(2001)
2010-2015*	6	1.0	120.64	Partly yes (2015)	0.15 (2015)

2. Episode defined as those where budget deficits fell by more than 1 per cent of GDP.

* Squeeze episode ongoing at the time of writing

Source: Authors' calculations, based on data sources as outlined in the data appendix

Table 4: Impact of Economic and Political variables on the Chance of a Change in Ruling Party (Probit Marginal Effects)

Dependent variable: Change in party orientation

	(1)	(2)	(3)	(4)	(5)	(6)
CHEXP	0.00 (0.26)			0.00 (0.90)		
CHREV		-0.01 (-0.46)			-0.01 (-1.56)	
CHDEF			0.00 (0.62)			-0.00 (-0.00)
LARGE_EXP				0.15* (1.90)		
LARGE_REV					0.25** (2.27)	
LARGE_DEF						-0.03 (-1.00)
dUNR	-0.06 (-1.39)	-0.06 (-1.14)	-0.06 (-1.28)	-0.06** (-2.10)	-0.05 (-0.99)	-0.06 (-1.25)
ddeflator	-0.44 (-1.30)	-0.38 (-1.19)	-0.42 (-1.21)	-0.51 (-1.44)	-0.26 (-1.38)	-0.37 (-1.15)
dGDP	-0.37 (-0.87)	-0.53 (-1.09)	-0.43 (-0.99)	-0.41 (-1.04)	-0.38 (-0.91)	-0.34 (-0.81)
duration	0.05*** (3.29)	0.05*** (3.65)	0.05*** (3.37)	0.05*** (3.20)	0.04*** (3.64)	0.05*** (3.45)
prop_seats	-0.78*** (-3.72)	-0.75*** (-3.58)	-0.76*** (-3.68)	-0.71*** (-3.83)	-0.47*** (-3.23)	-0.75*** (-3.74)
coalition	-0.00 (-0.02)	-0.01 (-0.17)	0.00 (0.01)	-0.02 (-0.81)	-0.01 (-0.23)	0.01 (0.21)
waryears	-0.09** (-2.55)	-0.08*** (-2.80)	-0.09*** (-2.98)	-0.08** (-2.48)	-0.07*** (-2.71)	-0.08*** (-2.97)
Observations	115	115	115	115	115	

Source: Author estimations based on data sources outlined in the data appendix

Table 5: Impact of Economic and Political variables (average across government tenure) on electoral outcomes

Dependent variable: Columns 1-6: Change in party orientation of Prime Minister; Columns 7-12: Distance in seats secured

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
avgCHEXP	0.04			0.07			0.02			-0.00		
	(0.85)			(1.00)			(0.65)			(-0.14)		
avgREV		0.11			-0.05			0.01			0.03	
		(0.97)			(-0.27)			(0.24)			(0.35)	
avgDEF			0.02			0.01			0.02			0.02
			(0.38)			(0.24)			(0.69)			(0.64)
avgLARGEEXP				1.04*						-0.61***		
				(1.73)						(-3.05)		
avgLARGEREV					0.69						-0.09	
					(1.12)						(-0.39)	
avgLARGEDEF						-0.11						0.01
						(-0.40)						(0.05)
avgUNR	-0.64	-1.25**	-0.61	-0.74	-1.08*	-0.68	-0.09	-0.19	-0.02	0.04	-0.19	-0.02
	(-1.13)	(-1.98)	(-0.84)	(-1.31)	(-1.67)	(-0.98)	(-0.40)	(-0.64)	(-0.08)	(0.23)	(-0.63)	(-0.07)
Avgddeflator	0.20	0.95	0.40	0.41	0.79	0.59	0.43	0.61	0.40	-0.31	0.72	0.39
	(0.08)	(0.35)	(0.15)	(0.15)	(0.22)	(0.23)	(0.35)	(0.48)	(0.30)	(-0.27)	(0.50)	(0.30)
avgdGDP	-12.11	-15.12	-12.62	-12.65	-18.03*	-12.98	-2.50	-3.30	-2.47	-1.19	-3.03	-2.47
	(-1.19)	(-1.56)	(-1.21)	(-1.26)	(-1.78)	(-1.26)	(-0.54)	(-0.68)	(-0.48)	(-0.30)	(-0.59)	(-0.47)
Duration	0.13	0.11	0.14	0.15	0.18	0.15	-0.10*	-0.10*	-0.09*	-0.13*	-0.11	-0.09*
	(1.33)	(0.98)	(1.39)	(1.46)	(1.14)	(1.38)	(-1.93)	(-1.81)	(-1.82)	(-1.92)	(-1.48)	(-1.75)
prop_seats	-3.95**	-4.11**	-3.81**	-3.96**	-4.22**	-3.98**	0.84	0.88	0.81	0.55	0.93	0.82
	(-2.27)	(-2.52)	(-2.26)	(-2.07)	(-2.37)	(-2.17)	(0.82)	(0.84)	(0.79)	(0.76)	(0.83)	(0.71)
Coalition	0.41	0.48	0.35	-0.23	0.29	0.35	-0.18	-0.19	-0.19	0.11	-0.16	-0.19
	(1.27)	(1.43)	(1.19)	(-0.59)	(0.68)	(1.19)	(-1.15)	(-1.22)	(-1.25)	(0.74)	(-0.95)	(-1.23)
Waryears	-0.71*	-0.73*	-0.70*	-0.68	-0.73*	-0.70	0.55	0.61	0.54	0.49	0.64	0.54

	(-1.73)	(-1.90)	(-1.66)	(-1.48)	(-1.75)	(-1.64)	(1.45)	(1.64)	(1.42)	(1.40)	(1.62)	(1.38)
Constant							0.04	0.05	0.02	0.32	0.07	0.01
							(0.08)	(0.09)	(0.03)	(0.89)	(0.12)	(0.02)
Observations	27	27	27	27	27		27	27	27	27	27	

Note: All socioeconomic explanatory variables reflect average values for the entire term of government (i.e., from one general election to the other.) The political variables refer to characteristics of the incumbent that implemented the squeeze. The coefficients reported are marginal effects using probit analysis in columns 1-6 and OLS regression results for columns 7-12. Robust z-statistics in parentheses *** p<0.01, ** p<0.05, * p<0.1

Source: Author estimations based on data sources outlined in the data appendix

Table 6: The responsiveness of disaggregated expenditure categories to a change in ruling party

Dependent variable: change in ruling party

	(1)	(2)	(3)	(5)	(6)
dSOCSEC	-0.08** (-2.00)				
dHEALTH		-0.11 (-1.32)			
dEDUC			-0.00 (-0.09)		
dPUBORDER				-0.09 (-0.55)	
dTRANSPORT					0.02 (0.60)
dUNR	-0.04 (-0.93)	-0.04 (-1.02)	-0.06 (-1.43)	-0.05 (-1.24)	-0.09* (-1.96)
ddeflator	-0.25 (-0.91)	-0.47 (-1.56)	-0.41 (-1.26)	-0.38 (-1.25)	-0.47* (-1.81)
dGDP	-0.85* (-1.95)	-0.67* (-1.68)	-0.28 (-0.68)	-0.36 (-0.86)	-0.24 (-0.87)
duration	0.04*** (2.95)	0.05*** (3.32)	0.05*** (3.27)	0.05*** (3.24)	0.04*** (2.86)
prop_seats	-0.61*** (-3.74)	-0.73*** (-3.82)	-0.76*** (-3.60)	-0.73*** (-3.52)	-0.68*** (-4.17)
coalition	-0.01 (-0.19)	-0.01 (-0.28)	0.01 (0.23)	0.00 (0.10)	-0.02 (-0.58)
waryears	-0.08*** (-2.60)	-0.08*** (-2.81)	-0.09*** (-2.80)	-0.09*** (-2.81)	-0.06** (-2.13)
Observations	115	113	113	113	98

Robust z-statistics in parentheses*** p<0.01, ** p<0.05, * p<0.1

Source: Author estimations based on data sources outlined in the data appendix

Table 7: The responsiveness of disaggregated expenditure categories (averages over the tenure of government) to a change in ruling party
 Dependent variable: change in ruling party (columns 1-5), distance seat (columns 6-10)

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
avgSOCSEC	-1.46** (-2.24)					0.10 (0.33)				
avgHEALTH		-1.47 (-1.55)					0.76 (1.69)			
avgEDUC			-1.12 (-1.35)					0.80* (2.05)		
avgPUBORD				1.79 (0.53)					1.97 (1.10)	
avgTRANS					-0.02 (-0.03)					-0.04 (-0.23)
avgUNR	0.29 (0.35)	-1.07* (-1.67)	-0.38 (-0.55)	-0.00 (-0.01)	-1.30* (-1.74)	-0.24 (-0.66)	0.10 (0.52)	-0.34 (-0.93)	-0.18 (-0.52)	0.02 (0.07)
avgdeflator	6.00 (1.53)	1.98 (0.62)	0.91 (0.25)	-2.75 (-0.78)	2.40 (0.62)	0.25 (0.18)	-0.17 (-0.17)	0.50 (0.51)	-0.97 (-0.75)	-0.16 (-0.12)
avgdGDP	-13.62 (-1.22)	-23.48* (-1.80)	-13.36 (-1.20)	5.32 (0.60)	-23.74* (-1.78)	-3.55 (-0.68)	3.14 (0.93)	-2.38 (-0.44)	-0.60 (-0.14)	-1.43 (-0.21)
Duration	0.16 (1.25)	0.19* (1.94)	0.19** (2.02)	0.14 (1.28)	0.06 (0.55)	-0.10* (-1.89)	-0.14** (-2.85)	-0.14*** (-4.10)	-0.12* (-2.00)	-0.11 (-1.22)
prop_seats	-2.48 (-1.20)	-4.13** (-2.48)	-4.21** (-2.53)	-4.11* (-1.84)	-3.15* (-1.70)	0.75 (0.74)	1.19 (1.10)	1.26 (1.28)	0.97 (0.87)	1.20 (0.84)
Coalition	0.06 (0.19)	0.42 (1.52)	0.23 (0.57)	0.29 (0.77)	0.39 (1.07)	-0.18 (-1.05)	-0.33* (-1.92)	-0.16 (-0.95)	-0.24 (-1.43)	-0.37** (-2.52)
Waryears	-0.72 (-1.50)	-0.70** (-2.29)	-0.67 (-1.63)	-0.63 (-1.12)	-0.67 (-1.29)	0.62 (1.68)	1.13** (2.88)	0.82** (2.40)	0.84* (1.94)	0.89 (1.61)
Constant						0.13 (0.20)	-0.18 (-0.30)	-0.05 (-0.08)	0.03 (0.04)	-0.12 (-0.18)
Observations	27	26	26	26	24	27	26	26	26	24
R-squared						0.28	0.39	0.43	0.35	0.26

Note: All socioeconomic explanatory variables reflect average values for the entire term of government (i.e., from one general election to the other.) The political variables refer to characteristics of the incumbent that implemented the squeeze. The coefficients reported are marginal effects using probit analysis in columns 1-6 and OLS regression results for columns 7-12. Robust z-statistics in parentheses *** p<0.01, ** p<0.05, * p<0.1

Source: Author estimations based on data sources outlined in the data appendix