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João P. Sousa

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Department of Economics
University of Strathclyde, Glasgow

Taking the long view: using civil and military spending shocks from 1879 to 2018 to estimate the UK's government spending multiplier

João P. Sousa*

Fraser of Allander Institute, University of Strathclyde

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Abstract

I use a novel dataset compiled from archival research in the UK Parliament with 140 years (1879 to 2018) of in-year government spending shocks, which are unlikely to be anticipated due to the UK's idiosyncratic budget process — an assertion supported by statistical tests on the shocks. I find a multiplier of 0.44 on impact and 0.47 in the long run, along with some evidence of larger stimulative effects from civil spending shocks at short horizons relative to military spending shocks. This corroborates for the UK that the Second World War's effect on output was due to its size rather than composition. The findings also imply that a budget-balance-neutral transfer from civil to military spending may well reduce output in the short run. Effects on other macroeconomic variables support results from New Keynesian workhorse models, as well as negative consumption effects found in empirical studies using large military spending shocks. I also find evidence of larger multipliers in states of high slack as measured by unemployment considerably above the natural rate, but not for other measures of slack nor for broader measures of economic regimes such as levels of debt-to-GDP, openness to trade and exchange rate regimes.

JEL Classification: C32, C36, E62, H30

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*Corresponding address: joao.sousa@strath.ac.uk; Sir William Duncan Building, 130 Rottenrow, Glasgow, G4 0GE, Scotland, United Kingdom. Thanks to Pedro Gomes, Ron Smith, Yunus Aksoy, Morten Ravn, Ethan Ilzetzki, Jagjit Chadha, Stephen Millard, Charlie Bean, David Miles, Richard Hughes, Andy King, Stephen Farrington, Laura Gardiner, James Ebdon, Hannah Randolph, Ian McArtney and colleagues at the Office for Budget Responsibility, the National Institute for Economic and Social Research, the University of Strathclyde and the 2024 Portuguese Economic Journal Conference for helpful feedback. My thanks to the Office for Budget Responsibility (OBR) for providing funding for me to undertake this research. All views are my own, and do not necessarily reflect those of the OBR or their Budget Responsibility Committee. An earlier version of this work formed part of the thesis for the award of my PhD at Birkbeck, University of London. Any remaining errors are my responsibility.

1 Introduction

When deciding on the appropriate decisions for what fiscal policy to implement, it is imperative to consider what its effect will be on output. This effect is usually captured through a multiplier, but estimating that consistently is notoriously difficult. For example, fiscal aggregates such as public spending and tax revenues respond to economic conditions, making it hard to disentangle the effects of policy decisions from automatic stabilisers. Policy decisions are also not independent of how the economy is performing — governments look at how they expect variables such as GDP growth, employment and inflation to evolve in order to set their fiscal policy, and so observed macroeconomic outcomes may belie different ones that might have occurred in the absence of policy interventions. And that is to say nothing of the fact that policy announcements might be anticipated. This kind of endogeneity permeates fiscal policy announcements.

As such, much work has attempted to find exogenous, unanticipated shocks that can isolate the effects of government spending on output. Narrative approaches are an important source of such shocks, but these are costly to assemble and have generally been limited to the United States. There is also limited literature for fiscal multipliers focused specifically in the United Kingdom, especially on the spending side. Cloyne (2013) uses a narrative approach to identify tax shocks from archival data, but no analogue exists on the spending side. Studies tend to use data-driven identification strategies instead of the narrative approach of the canonical multipliers literature such as Ramey and Shapiro (1998), Gordon and Krenn (2010), C. D. Romer and D. H. Romer (2010) and Ramey (2011). Glocker, Sestieri, and Towbin (2019) note that "[c]onstructing a direct measure of spending shocks for the UK from variations in defense spending would require a sizeable archive work and the use of historical data (most of the action in defense spending occurred before the 1970s)." This paper sets out to provide such a historical data series and to discern what can be learned from it when assessing the effect of government spending on output in the UK.

The reason why this matters is that most of the literature has focused heavily on the United States historically, but studies using US data do not necessarily translate into other countries, not least because of the size of the domestic market and the anchor role of the US dollar across the dollar allowing more monetary freedom. But despite that, institutions such as the Office for Budget Responsibility (OBR) — the UK's official fiscal forecaster — are required to make assumptions about the effects of government spending, and often have to rely on US-based estimates to do so.

Furthermore, the UK's budget-setting process provides an ideal source of data for this kind of econometric estimation. Near or just after the beginning of the each financial year — which in the United Kingdom runs from 1 April to 31 March — the Chancellor of the Exchequer is required (and has been for nearly 150 years) to present their estimate of how much the Exchequer actually spent in the previous and how much it forecasts spending to be in the coming year, which given the way the UK's parliamentary system works, presents a way of obtaining a best estimate of the discrepancy between forecast and actual discretionary spending[1] - essentially an intra-year unanticipated spending shock. This allows me to compile a dataset of shocks from 1877-78 to 2018-19, a total of 142 years[2]

I can then combine these shocks with broader UK annual macroeconomic data, which I obtain from the UK's Office for National Statistics for 1946 onwards and which I splice with pre-1946 consensus estimates from Thomas and Dimsdale (2017). I estimate output elasticities at each horizon from years 0 to 4 after the shock, which I convert to multipliers using Y/G as a conversion ratio. I find evidence of statistically significantly positive multipliers of 0.44 on impact and 0.47 in the long run. I also use the breakdown of the shocks into military and civil components, with some evidence of civil spending being associated with larger multipliers than military spending at short horizons. I test the effect of the shocks on other macroeconomic variables, which show results that are broadly consistent with New Keynesian model results, although not necessarily all the empirical literature. The fall in private consumption spending in particular mirrors other studies that include large military shocks as their identification strategy.

The local projections framework I use allows me to test whether there is evidence of state-dependent effects of fiscal policy on output across a number of measures of states. I only find strong statistical evidence of higher multipliers in states of unemployment considerably above the natural rate, but not for other measures of slack — nor do I find results to suggest differences in multiplier across regimes such as high and low debt stocks, a more or less open economy or different exchange rate regimes.

The rest of the chapter is structured as follows: section 2 reviews the literature on UK multipliers and where this paper fits in terms of gaps; section 3 summarises the data used and the shocks, testing them to show that there is no strong evidence of anticipation; section 4 details the methodology employed; section 5 presents the results for the full shock, the breakdowns of military and civil spending, the effect on other macroeconomic variables, and state-dependent estimates; and section 6 assesses the policy implications of the findings; and section 7 concludes.

2 Literature review

The literature on fiscal multipliers is heavily focused on the United States. Take Ramey (2011) for example, which summarises significant contributions to the literature — the number of papers looking at the US far outweighs the few mentioned which look at other countries. Even in those cases, they tend to be done as panels rather than focusing on the specifics of a particular country. And that means they are more likely to be data-driven approaches, which are more practical for a panel (Ilzetzki,

[1] As detailed in the data section, I remove debt interest and social security spending because of their inherent correlation with macroeconomic conditions as automatic stabilisers.

[2] Although I have 142 years in the dataset, I have to drop two observations from the regression equations, as I use inflation as one of the control variables (which means dropping 1877-78) and I use one lag of each macroeconomic variable (which means dropping 1878-79).

Mendoza, and Végh 2013). Constructing a series such as the ones used by Ramey (2009) for another country would require a lot of archival work, let alone for a number of countries.

As such, it is perhaps unsurprising that the literature on fiscal multipliers for the United Kingdom is generally quite sparse. On the tax side, the seminal work of Cloyne (2013) employs a narrative approach by employing archival research to come up with discretionary tax changes on the basis of HM Treasury documents, and finding a multiplier of 0.6 on impact and 2.5 after three years.

But no analogue of Cloyne (2013) has been compiled on the spending side, for which even the limited number of studies conducted remains based on data-driven methods for identification rather than the narrative approach favoured by seminal multiplier papers such as Ramey and Shapiro (1998), Gordon and Krenn (2010), C. D. Romer and D. H. Romer (2010) and Ramey (2011).

The UK does feature as part of some multi-country studies, with a lot of interest focused on such studies in the aftermath of the 2007-08 financial crisis and responses by government across the world in its aftermath. Barrell, Holland, and Hurst (2012) find a government consumption multiplier of 0.74, but it is a simulation study using the National Institute Global Econometric Model (NiGEM) and not an econometric one. Cimadomo and Bénassy-Quéré (2012) find a spending multiplier for the UK of 0.28 on impact — in line with the 0.30 found by Perotti (2005) as well — though they find non-Keynesian effects in some parts of the time period they analyse (1971 to 2009). Baum, Poplawski-Ribeiro, and Weber (2012), using data from 1970 to 2011, find even lower spending multipliers for the UK, at 0.2 after a year and 0.1 after two — considerably lower than for other countries in their sample.

Recent UK-specific studies of note include Rafiq (2014), who uses data a time-varying framework with data from 1959 to 2009, and finds a multiplier of 0.93 after six months — considerably higher than that found for the UK in empirical studies in which other countries were present. Rafiq's Bayesian time-varying VAR framework also points to the multiplier in the UK being above 1 since the 1990s, with no evidence of long-term effects on the level of GDP. This is in contrast to the results of Shaheen and Turner (2020) results, which show negative spending multipliers across their sample, to a low of -0.42 after three years — a result not in line with most of the recent literature. They do find positive multipliers for non-boom conditions, but even then they are low (0.25 at most).

Glocker, Sestieri, and Towbin (2019) estimate the government spending multiplier for the UK to be 0.48 on average across states, although larger (1.21) when in recession and smaller (0.35) in non-recessionary periods. But they note that "[c]onstructing a direct measure of spending shocks for the UK from variations in defense spending would require a sizeable archive work and the use of historical data (most of the action in defense spending occurred before the 1970s)" and that it was beyond the scope of their paper. This paper sets out to provide such a historical data series and to determine what can be learned about historical data about the effects of government spending on UK output and other macroeconomic variables.

As for the effect of government spending on other macroeconomic variables, empirical results are mixed and not always in line with theoretical models. This is particularly well-documented in the case of private consumption, as discussed in detail by Ferrara et al. (2021). Theoretical models, including workhorse New Keynesian models, predict a fall in private real consumption as a result of a shock that increases government spending. This is corroborated by Ramey (2009) and Ramey (2012), but contradicted by other studies, including Blanchard and Perotti (2002), M. Ravn, Schmitt-Grohé, and Uribe (2006), Ferrara et al. (2021) and — focusing on UK multiplier estimates — Rafiq (2014).

Effects on inflation and prices are also contradictory between the baseline New Keynesian model and empirical evidence, as detailed in comprehensive literature reviews by Ferrara et al. (2021) and Jørgensen and S. H. Ravn (2022), and in Ramey (2019). New Keynesian models predict consistently an inflationary effect from an expansionary fiscal shock, including for increasing public spending, but the empirical evidence is mixed. Take the two recent studies mentioned above as an example: Jørgensen and S. H. Ravn (2022) find a negative effect of government spending on inflation across a number of specifications using a structural VAR (SVAR) with US data from 1951 to 2008; using a proxy SVAR for US data from 1964 to 2015, Ferrara et al. (2021) find instead a positive effect on inflation. These inconsistencies are reflected across a number of studies, as both the aforementioned papers detail. But there is also a large segment of the literature — including influential papers such as Mountford and Uhlig (2009) and Auerbach and Gorodnichenko (2012) — which find no significant lasting effect of fiscal policy on inflation.

The effect on interest rates found in the literature is also inconsistent with the standard New Keynesian framework, in which we would expect them to rise in response to an increase in government spending. Instead, empirical results generally find either no effect or a fall in interest rates (Murphy and Walsh 2022).

The effect of fiscal policy on employment is the more consensual across the literature. Recent work on the topic including Monacelli, Perotti, and Trigari (2010), Nakamura and Steinsson (2014), Suárez Serrato and Wingender (2016) and Dupor and Guerrero (2017) all find positive employment effects from government spending, although of different magnitudes.

A growing literature has in recent years focused on state-dependent effects of fiscal policy. One of the main areas of interest is the very Keynesian idea of larger effectiveness of loose fiscal policy during recessions. Again, most of this analysis has been done on US data. Gordon and Krenn (2010) estimate that in a state of high slack — such as during the Great Depression but before the supply constraints of the Second World War effort came through — multipliers were relatively high, at around 1.8. Auerbach and Gorodnichenko (2012) find similarly that multipliers are considerably larger in recessions than in expansions. However, neither Ramey and Zubairy (2018) nor Ghassibe and Zanetti (2022) find strong support for this — with the latter showing large multipliers are associated with demand-driven recessions, but not supply-driven ones.

Ilzetzki, Mendoza, and Végh (2013) use a broader suite of measures to test state dependence. They find that higher openness to trade is associated with lower multipliers, which supports the idea that fiscal stimulus in open economies has a higher degree leakage; that fixed exchange rates are associated with higher multipliers, in line with predictions from the Mundell-Fleming model; and that countries with higher debt-to-GDP ratios tend to have smaller multipliers.

3 The data

3.1 Compiling data for the shocks

Fiscal multipliers are difficult to estimate consistently — randomised experiments are impossible and the numerous confounding factors going on at any point in the macroeconomy make it hard to isolate the effect of a government fiscal policy impulse. Long historical time series go some way towards combating this issue, as they provide us with a larger pool of shocks from which to draw inferences (Ramey and Zubairy 2018), including large-scale wars: as Angus Deaton quipped in his response to Hall (1986), "nothing can be known *without* the wars." And while the use of long time series is not without its drawbacks — the size and scope of government has changed hugely since the 19th century, and consistent measurement becomes more of an issue the further back we go in time — I believe it to be a worthwhile endeavour.

To get around the difficulties in consistently estimating multipliers, one solution that has been employed frequently in the literature is to use a narrative approach to identify an exogenous shock, which is then used as an instrument for government spending in the reduced form equation. Ramey and Shapiro (1998) and C. D. Romer and D. H. Romer (2010) are two very famous uses of this approach, where they use dates for when it becomes anticipated that the US would enter into a war in the former and presidential and congressional records in the latter to identify the timing of shocks. Cloyne (2013) employs this kind of approach to UK data for tax changes.

My approach is similar to these in spirit. I have used records from the UK's Parliamentary Archives going back to 1877 to identify how much the government intended to spend in the forthcoming year and how much it estimates to have spent in the previous year. The difference between announced and actual spending is essentially an intra-year, unanticipated policy change in discretionary spending.

There is a lot in the last sentence of the previous paragraph, so it is worth outlining the rationale for arguing each of the points. First, I am only looking at discretionary spending, which means spending that the government has direct control over. That means that I have excluded two broad categories: debt interest, as it depends directly on the stock of debt outstanding and on market interest rates; and social security spending, as it depends directly on the state of the labour market. Secondly, because I use annual data, the shock occurs within the period, and so it is contemporaneous.

3.2 What do the shocks look like?

As both figure 1 and table 1 show, the shocks present a wide range of variation across time. This is not unexpected as they include the two World Wars — both their outbreak and their ending, neither of which would be predictable in terms of exact timing. The large positive shocks (i.e. spending above forecast) mean that the average shock is positive. The median, however, is pretty close to zero in all three cases (less than 0.1% of GDP).

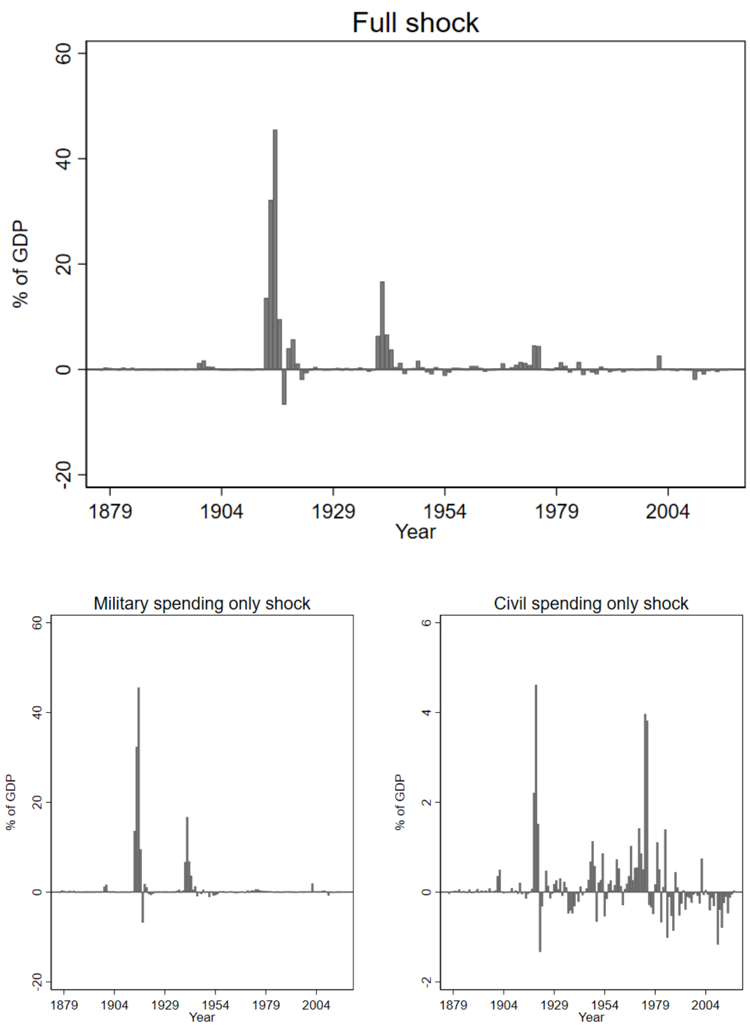

Figure 1: Discretionary spending shocks from 1879 to 2018 in the dataset.

Shocks can broadly be divided into two categories. One is the most

	Full shock	Military only	Civil only
Average	1.1289	0.9872	0.1416
Standard error	5.2046	5.1684	0.7584
Minimum	-6.7093	-6.7849	-1.3303
Median	0.0520	0.0229	0.0142
Maximum	45.5077	45.5490	4.6169
N	140	140	140

Table 1: Summary statistics of the shocks in the dataset (1879-2018) as a share of GDP.

obvious, which is large-scale wars against foreign powers. This includes but is not limited to the First and Second World Wars, which were both long and extremely expensive and — crucially for the identification strategy — much more so than initially anticipated. Table 2 shows the most notable shocks related to wars with foreign powers. The Second Boer War (1899-1902) is the first large event in the series, and broke out in the October of 1899 — right in the middle of the financial year — just four months after the failure of the Bloemfontein Conference. This resulted in additional military spending[3], which continued during the unexpectedly protracted conflict (Miller 2006). The lower than anticipated spending in 1902-03, on the other hand, reflects the end of the war on 31 May 1902, just two full months into the financial year.

The First World War contains by far the largest shocks in the series. The scale and expense of fighting the war was unprecedented, and manifested itself in very large increase of spending in-year. The immediate outbreak of war between Britain and Germany (and therefore the Central Powers) was midway through the financial year, on 4 August 1914, when Germany did not respond to the ultimatum of the Asquith Government over Belgian neutrality[4]. This was followed by an immediate vote of credit for additional military spending on 5 August, which would by no means be the last.

There is much contestation of how long governments expected the war in Europe to last (Hallifax 2010), but there is no doubt no one was prepared for the scale of the war that was to come. Spending over and above beginning of year estimates reached as high as 46% of GDP in 1916-17, with the financial burden of the war in Europe progressively shifting to the UK over successive war budgets (Allen 1917). But the pattern of expenditure also reflects the largely unexpectedly quick collapse of the German Army from May to November 1918 (Deist and Feuchtwanger 1996), which led to an underspending of around 7% of GDP in the 1918-19 financial year.

The Second World War's pattern of in-year additions to spending bears some resemblance to the First World War's, but is much less pronounced.

[3] Hansard record of House of Commons sittings of 18-20 October 1899

[4] Hansard record of the House of Commons sittings of 4-5 August 1914

There was an initial increase in spending in the aftermath of the declaration of war, approved by the House of Commons on 1 September 1939[5]. But the largest expense over and above that in the budget was in 1940-41, which includes the German invasion of France in May, the Battle of Britain and almost the whole of the Blitz.

The financing of the Second World War was much more planned than the First — see for example Keynes (1940) — and that fed into the lower magnitudes of expenditure shocks, as well as the experience of a protracted large scale war feeding into plans into the mid-1940s. But as with any other war, the exact timing of its end was unpredictable in advance, causing the same pattern of underspending in the final year (1945-46).

After the end of the Second World War, military spending has become much less important as a source of large shocks in expenditure relative to forecasts. This is not particularly surprising, as Britain's role in world affairs has diminished since then, although it was involved in a number of military interventions such as the Korean War in the 1950s and the proxy conflict in Afghanistan in the late 1970s and 1980s. The Iraq war is the sole exception to this lower level of importance, largely because of its timing: the United Nations inspections occurred in late 2002, with subsequent build-up of spending for the invasion launched on 20 March 2003, just 11 days before the end of the financial year. This was essentially a one-off occurrence — 2003-04 data shows pretty low errors in forecasts.

Event	Year	Full shock	*o/w military*	*o/w civil*
Second Boer War	1899-1900	1.20	*1.19*	*0.02*
	1900-01	1.69	*1.65*	*0.03*
	1901-02	0.58	*0.23*	*0.35*
	1902-03	0.48	*-0.16*	*0.50*
First World War	1914-15	13.61	*13.61*	*-0.01*
	1915-16	32.19	*32.33*	*-0.14*
	1916-17	45.51	*45.55*	*-0.04*
	1917-18	9.52	*9.52*	*0.00*
	1918-19	-6.71	*-6.78*	*0.08*
Second World War	1939-40	6.36	*6.68*	*-0.31*
	1940-41	16.70	*16.73*	*-0.04*
	1941-42	6.62	*6.84*	*-0.21*
	1942-43	3.78	*3.65*	*0.12*
	1943-44	0.44	*0.51*	*-0.07*
	1944-45	1.27	*1.28*	*-0.01*
	1945-46	-0.86	*-0.94*	*0.08*
Iraq War	2002-03	2.66	*1.92*	*0.75*
	2003-04	0.08	*0.14*	*-0.06*

Table 2: Notable shocks related to wars against foreign powers, expressed as percentage of GDP. Columns may not sum to totals due to rounding.

Outside wars with foreign powers, there are also some large shocks — mostly on the civil side, but not exclusively — that are worth noting. The post-First World War period is the earliest of note, and it is a combination of issues. Some of it was higher-than-budgeted for pay settlements for both the military and civil servants, including war bonuses and the dealing with

[5] Hansard record of the House of Commons sitting of 1 September 1939

the aftermath of the influenza pandemic.[6] But it also included additional military spending abroad — including the occupation of Istanbul and of the League of Nations mandates — and in Ireland as part of the war of independence. The end of the Irish War of Independence partway through 1921-22 also contributed to the fall in military spending in that year relative to the budget projections.

Post-Second World War, there were a number of large civil-led spending shocks. The first was immediately after the introduction of the National Health Service (NHS), whose costs immediately and severely overran in the first two years (Cutler 2003), and which contributed significantly to overspending relative to budget in 1948-49 and 1949-50. 1967-68 was the next large increase in expenditure relative to plans during the year, as the government tried to inject demand into the economy during the summer to improve growth with the aim of avoiding devaluing sterling (Newton 2010) — which it ended up having to do anyway on 18 November 1967.

This was followed by the most significant episode of loss of control over inflation and public spending in post-Second World War British history in the early to mid-1970s. The start of this episode predates the oil shock of 1973, though it no doubt was exacerbated by it. Inflation in Britain had been accelerating since 1968, and the government had resorted to an incomes policy to try to control it — essentially short-term limits on how much wages could rise by, imposed by the government and in the British case, agreed with trade unions and employer bodies. This, of course, is very different from today's consensus of the economics profession, which views inflation control as the job of monetary policy.

The incomes policy, in particular the 'stage three threshold payments' introduced in late 1973 that would become associated with the large loss of control of public spending in the face of high inflation. These threshold payments were to come into place if inflation rose above 7% — which proved to be the case, with average wages rising by nearly 15% between November 1973 and August 1974 (Ashenfelter and Layard 1983). The Harold Wilson government then abolished the Pay Board[7], with a subsequent increase in wages of around 25% in the twelve months to August 1975. It was during this time that public spending rose well above the plans laid out at the beginning of the financial years, exceeding them by 4.6% and 4.4% of GDP in 1974-75 and 1975-76, respectively, in advance of the 1976 IMF crisis.

There are two other shocks of note in the series. One is related to the coal miners' strike in 1984, which meant the Government needed to import coal instead of procuring it domestically, adding to expenditure by around 1.4% of GDP[8]. And the second is the introduction of an austerity programme part-way through 2010-11, as part of the 22 June 2010 emergency budget, which severely restricted spending within year — by nearly 2% of GDP, across both military and civil spending.

[6] Revised Financial Statement (1919-20), Cmd. 377.

[7] Hansard record of the House of Commons sitting of 18 July 1974.

[8] Supply Estimates 1984-85, Supplementary Estimates (Classes I-XVIII), H.C. 7 (1984-85). Also see Hansard record of the 1985-86 budget statement, House of Commons sitting of 19 March 1985

Event	Year	Full shock	*o/w military*	*o/w civil*
Post-First World War	1919-20	4.02	*1.81*	*2.21*
adjustment: pay pressures, post-war	1920-21	5.70	*1.08*	*4.62*
occupations and war in Ireland	1921-22	1.13	*-0.39*	*1.52*
NHS costs overrun	1948-49	1.65	*0.52*	*1.13*
immediately after introduction	1949-50	0.43	*-0.16*	*0.58*
Currency crisis and devaluation	1967-68	1.15	*0.13*	*1.03*
Loss of control over inflation	1971-72	1.43	*0.01*	*1.42*
	1972-73	1.23	*0.37*	*0.86*
	1973-74	0.82	*0.31*	*0.50*
	1974-75	4.58	*0.61*	*3.97*
	1975-76	4.43	*0.62*	*3.82*
Coal miners' strike	1984-85	1.42	*0.03*	*1.40*
Austerity introduced mid-year	2010-11	-1.94	*-0.77*	*-1.17*

Table 3: Notable shocks not related to wars against foreign powers, expressed as percentage of GDP. Columns may not sum to totals due to rounding.

3.3 Are the shocks truly unanticipated?

This is the most important question, and in this case the UK's unique budget framework makes a more compelling case than other jurisdictions for that to be the case. The budget process is fairly well established in the UK. The Chancellor of the Exchequer must present a financial statement and budget report to the House of Commons each year in a session presided by the Chairman of Ways and Means[9]. In the Westminster system (often described as an elective dictatorship, or executive-dominant), there is no divided government — the government must have support in money bills (the budget) as a pre-condition for being in power or else it falls. The government also has the power to set the agenda of the Commons, effectively stopping any other source of money bills (Tsebelis 2009). In the period in my sample, not a single budget failed to pass the House of Commons, and the only one (1909) which did fall in the Lords precipitated the 1911 Act which removed the Lords' veto over money bills — and was passed into law immediately after a general election. Party discipline, enforced by the whips' office, is also very strong and has been since the formation of modern political parties (1830-1860); and the combination of the House of Lords' convention not to oppose manifesto commitments and the Parliament Acts 1911 and 1949 means that the Government is effectively able to pass any legislation it brings forward.

The budget process itself also makes it less likely that the government

[9] This is a holdover from before 1967, when the Committee of Ways and Means was abolished and full responsibility for all fiscal matters was formally transferred to the Chancellor of the Exchequer. Prior to that point, the Chancellor presented a financial statement with Government policy outlined, but formally any member of Parliament (MP) could put forward proposals for taxation and spending, although in practice the government's majority rendered this ineffective. The Chairman of Ways and Means is now the principal Deputy Speaker of the House of Commons. Since 1718, every Chancellor of the Exchequer has sat in the House of Commons rather than the Lords (with the exception of four brief interim periods), and the Commons' primacy over the Lords in money bills was formally put into law in the Parliament Act 1911.

will ignore information in its forecasting of spending for its own benefit. Because the passage of the government's budget is guaranteed by the government's very existence, there are no negotiations in public between parties. The Treasury's position as announced in the budget is taken as given, and then used for debt issuance and cash management. Therefore, I would argue that any minuscule gains from gaming the forecasting system are effectively ignorable, especially in-year.

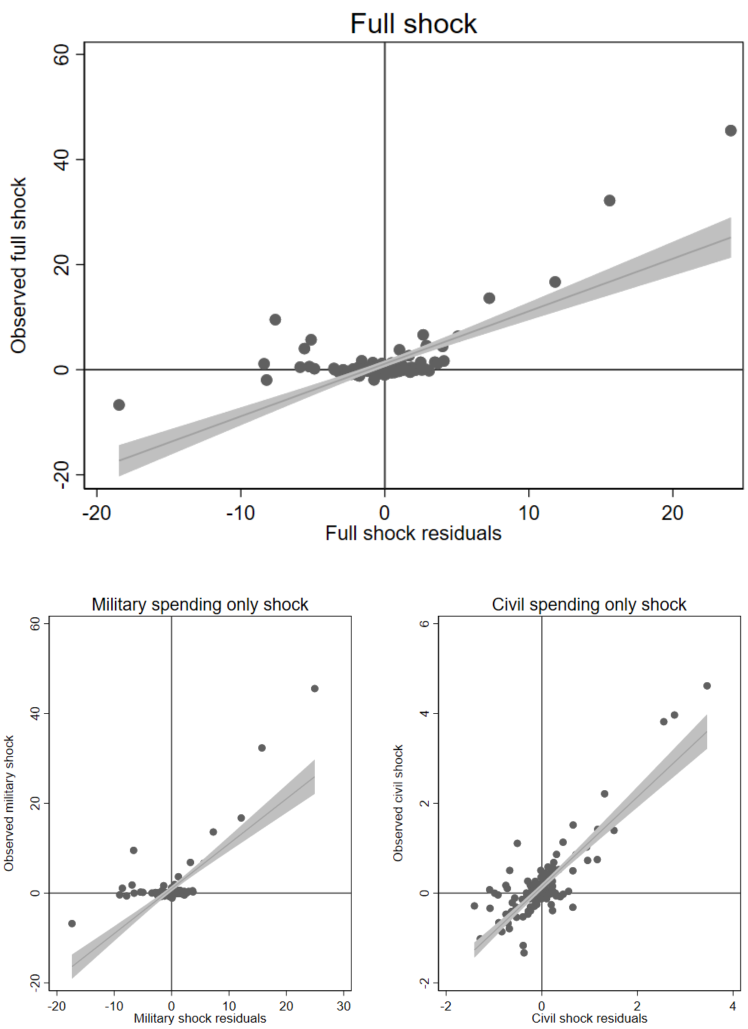

Figure 2: Mincer-Zarnowitz regressions of the civil and military spending shocks. Lines are the fitted Mincer-Zarnowitz regressions with 95% confidence intervals around them and represent the null hypothesis that the intercept is zero and the slope one — which I fail to reject.

Since at least 1877, the Chancellor has clearly laid out the latest estimate for expenditure for the year just gone, as well as their forecast for how much they expect to be spent in the year ahead[10]. It is these forecasts which I take from the financial statements and budget reports in the archives to compile my series of shocks, and which I am also able to break down into military and civil spending components.

Testing of the shocks using the Mincer and Zarnowitz (1969) regressions supports the claim that they are exogenous. The idea behind using the Mincer-Zarnowitz method is that a shock should be unforecastable with all information available at the time at which they occur. In practice, this means using the same autoregressive structure — known at the time of the shock by definition — as in the main specification to try and forecast the shocks, and then estimating the residuals. If the shocks are unforecastable, we should not be able to reject the joint hypothesis that when regressing the shocks on their residuals, the intercept is 0 and the coefficient on the residuals is 1. Figure 2 shows the scatter plot of the Mincer-Zarnowitz regressions for the full shock and the military and civil spending breakdowns. In all three cases I fail to reject the null hypothesis, lending further weight to the argument for using them as an instrument in this specification.[11]

3.4 Other data

For macroeconomic variables, I combine two sources. The first one is the UK's Office for National Statistics (ONS), which compiles and publishes the UK's national accounts and has data on most series post-Second World War (1946 onwards). For pre-1946 data, I have spliced the ONS series with the series published by Thomas and Dimsdale (2017) as part of the Bank of England's *Millennium of macroeconomic data for the UK* project. This includes GDP, central government spending[12], household consumption, inflation as measured through the GDP deflator, the employment rate and the Bank of England's policy interest rate[13].

[10] For almost all this time, the budget report was laid in the Commons in either March or April for the forthcoming financial year. More recently, this has been brought forward to the Autumn, but there is a Spring Statement in March, alongside which the Office for Budget Responsibility presents its latest forecasts, and that is what I use for the last few years of my dataset.

[11] See the appendix for detailed results of the Mincer-Zarnowitz regressions.

[12] This is the central government contribution to total managed expenditure (TME) for post-war observations and as consistent as possible with that definition before. TME is a public sector-wide metric, so it includes general government, public corporations and the Bank of England since nationalisation, but excludes public sector-owned commercial banks, and it is the most widely used metric of public sector expenditure used in the UK.

[13] Given the secular decline in real and nominal interest rates, I use a Kálmán filter to estimate the wedge between the fitted interest rate and the actual interest rate at each point in time.

4 Methodology

4.1 Econometric specification

I use annual data from 140 financial years: 1879-80 to 2018-19[14] and apply a local projections with instrumental variables (LP-IV) approach, based initially on Jordà (2005) and following the syntheses of Stock and Watson (2018) and Jordà and Taylor (2025). I estimate the regression equations in logs at each horizon and which is a direct estimation[15] of the cumulative impulse response function (IRF), so that for each $h \epsilon \{0, 1, 2, 3, 4\}$ I estimate the following equation:[16]

$$\sum_{j=0}^{h} \ln y_{t+j} = \phi_h L z_{t-1} + \beta_h \sum_{j=0}^{h} \ln g_{t+j} + \varepsilon_{t+h} \tag{1}$$

where h is the horizon at which the cumulative IRF is estimated; Lz_{t-1} is a one lag operator of macroeconomic variables (output, government spending, policy interest rate, inflation, consumption, the employment rate and the debt-to-GDP ratio); g is government spending; and ε is an error term. I then convert the estimate of β_h, which is an output elasticity with respect to government spending, into a multiplier estimate using the sample average of Y/G:

$$\hat{\gamma}_h = \hat{\beta}_h \times \frac{\bar{y}}{\bar{g}} = \hat{\beta}_h \times \frac{\sum_{t=0}^{T} y_t}{\sum_{t=0}^{T} g_t} \tag{2}$$

For the remaining macroeconomic variables for which I estimate impulse responses (consumption, policy interest rate, inflation and employment rate) I estimate a similar equation at each horizon, but with the variable in question on the left-hand side.

Plagborg-Møller and Wolf (2021) show that the IRFs estimated using local projections are similar to those estimated using a VAR. However, the local projection framework makes it easier to estimate non-linear effects, which are of interest to me. To do so, I estimate the follow equation at each horizon h for each states A (when $I = 1$) and B (when $I = 0$):

$$\begin{aligned} \sum_{j=0}^{h} \ln y_{t+j} &= I_{t-1} \left[\phi_{A,h} L z_t + \beta_{A,h} \sum_{j=0}^{h} \ln g_{t+j} \right] \\ &+ (1 - I_{t-1}) \left[\phi_{B,h} L z_t + \beta_{B,h} \sum_{j=0}^{h} \ln g_{t+j} \right] + \nu_{t+h} \end{aligned} \tag{3}$$

[14] Although I have 142 years in the dataset, I drop two observations from the regression equations, as I use lagged inflation as one of the control variables (which means dropping 1877-78) and I use one lag of each macroeconomic variable (which means dropping 1878-79). UK financial years throughout the whole period start on 1 April, so that financial year 1879 runs from 1 April 1879 to 31 March 1880 and so on. The choice of financial years over calendar years is to take into account the Treasury budget cycle, and therefore ensure that shocks are being assigned to the correct financial year.

[15] See the appendix for discussion of the properties of this method of estimation.

[16] This horizon is roughly equivalent to the standard 20 quarters usually reported as "long run" in multiplier papers — for example, see Ilzetzki, Mendoza, and Végh (2013) and Ramey and Zubairy (2018).

These output elasticity estimates can then be converted into multipliers in a similar way as in equation (2):

$$\hat{\gamma}_{A,h} = \hat{\beta}_{A,h} \times \frac{\bar{y}_A}{\bar{g}_A} = \hat{\beta}_{A,h} \times \frac{\sum_{t=0}^{T} 1_{\{I_{t-1}=1\}} y_t}{\sum_{t=0}^{T} 1_{\{I_{t-1}=1\}} g_t} \tag{4}$$

$$\hat{\gamma}_{B,h} = \hat{\beta}_{A,h} \times \frac{\sum_{t=0}^{T} 1_{\{I_{t-1}=0\}} y_t}{\sum_{t=0}^{T} 1_{\{I_{t-1}=0\}} g_t} \tag{5}$$

I then test for a number of different states, including high and low slack — measured by the unemployment rate, the wedge between the unemployment rate and an estimate of the natural rate of unemployment, and a measure of the output gap — and regimes such as high and low debt-to-GDP ratios, high and low openness to trade, and fixed and flexible exchange rates.

I settle on a 1-lag structure for all the specifications. As I use annual data, this is similar to the 4-lag structure for quarterly data often used in these procedures — see Ilzetzki, Mendoza, and Végh (2013) and Ramey and Zubairy (2018), for example. This is also partly due to the number of observations I have available and the fact that I control for a number of variables. This is a criticism that can be levelled at this kind of analysis, of course. Going back this far in time — which I think can give us valuable insight — means using data from a time when quarterly national accounts were not available. It would be possible to interpolate data on a quarterly basis for before then — and some studies such as Ramey and Zubairy do so — but my judgement falls on the side of not introducing potentially further patterns into the data on the basis of a statistical procedure. And given that my identification strategy is well aligned with annual data, I land on the side of using fewer observations and lags, but without interpolation.

4.2 Instrument strength and inference

Because of the endogeneity problem highlighted above, I use an instrumental variables approach and estimate equations (1) and (3) using two-stage least squares (2SLS). I instrument government spending using the shocks I compiled from the Parliamentary Archives, and estimate results using the full shock, with results for military and civil spending as robustness checks. I also compare the effects on other macroeconomic variables (consumption, policy interest rate, inflation and employment rate) with those in the theoretical and empirical literatures, and test whether this dataset supports recent empirical findings on state-dependence of multipliers in a number of contexts.

Inference using 2SLS estimates in an instrumental variables (IV) setting is asymptotically valid provided instruments are relevant and strong. Instrument strength is a concern in macroeconomics and in the narrative approach in particular, and many applications fail to clear the rule of thumb of a first-stage F-statistic above 10 suggested by Staiger and Stock (1997): see Ramey (2016); Ramey (2019); and José L Montiel Olea, Stock, and Watson (2021). José Luis Montiel Olea and Pflueger (2013) showed that a heteroscedasticity and autocorrelation robust (HAR) effective F-statistic has larger critical values — for the one instrument, just-identified case, the critical value for a worst case bias of 10% relative to the OLS estimate is 23.1085, well above the Staiger and Stock rule of thumb.

Work by José L Montiel Olea, Stock, and Watson (2021) has provided a foundation for how to conduct inference in the case of weak instruments, building on work by Davidson and MacKinnon (2014) and on the recommendations of Lazarus, Lewis, Stock, and Watson (2018). As my model is specified on a just-identified basis, I follow their approach of always using Anderson-Rubin (AR) confidence sets — that is, inverting the Anderson and Rubin (1949) test — regardless of the first-stage F-statistic rather than the 2SLS-based standard errors to compute confidence intervals. The non-parametric AR approach means that confidence sets will be identical to 2SLS ones with strong instruments, while computing sets that are still valid in the presence of weak instruments.[17]

5 Results

5.1 Multiplier estimates

Figure 3 shows the multiplier estimates for the full shock — the cumulative IRF of output in response to a 1% of GDP increase in government spending. Table 4 details the results at each horizon up to 4 years after impact.

The multiplier estimate using the full shock is 0.44 on impact and then rises slightly, before dropping to a long-run value of 0.47. As an estimate, this is very much in line with results for the US (Ramey 2019). Relative to UK-specific estimates, it is higher than most, though not all the literature, and more in line with Glocker, Sestieri, and Towbin (2019). All estimates are statistically significant at the 1% level, save for year 4, which is significant at the 5% level.

Having the breakdown of the shock between military and civil spending allows the possibility of looking at whether each has a significantly different effect on output. As figure 4 shows, while the full shock and the military spending shock on its own are for the most part strong instruments (using the Kleibergen-Paap F-statistic as a measure), the civil spending shock by itself does not clear the critical values.

Nevertheless, using AR confidence sets allows me to say something about the relative size of the multipliers for the two shocks, even in the presence of a weak instrument. Figure 5 plots the IRFs for both shocks. The military spending only shock is similar to the full shock, which is unsurprising given that military spending drives most of the large shocks

[17] I additionally opt for the quadratic spectral (QS) kernel to compute standard errors on which AR confidence sets are calculated, as Lazarus, Lewis, and Stock (2021) show that the QS kernel achieves the size-power frontier. The decision to not use the Newey-West default kernel (Bartlett) nor its default bandwidth calculation is based on Müller (2014) and Lazarus, Lewis, Stock, and Watson (2018), which document substantial evidence that doing so would lead to wrongly rejecting the null hypothesis too often, which in this case would mean finding statistically significant estimates too often.

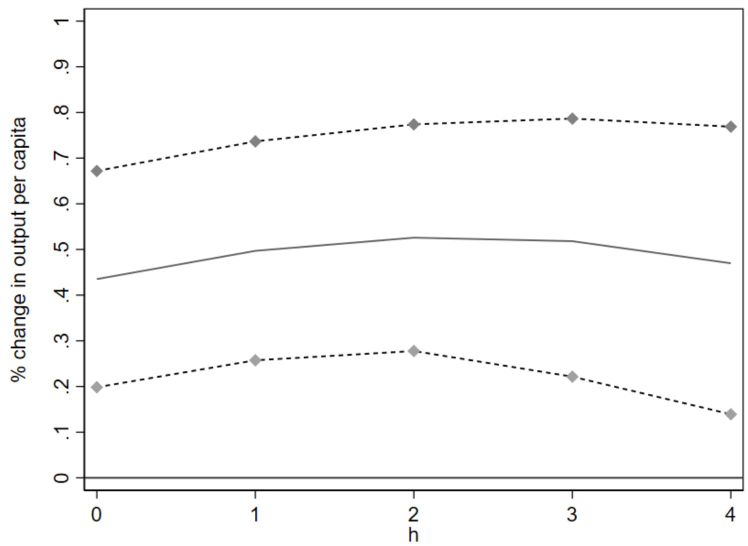

Figure 3: Cumulative IRFs of output per capita in response to a 1% of GDP increase in government spending per capita. Dashed lines represent the bounds of the 90% Anderson-Rubin confidence set, calculated using HAR standard errors using the QS kernel.

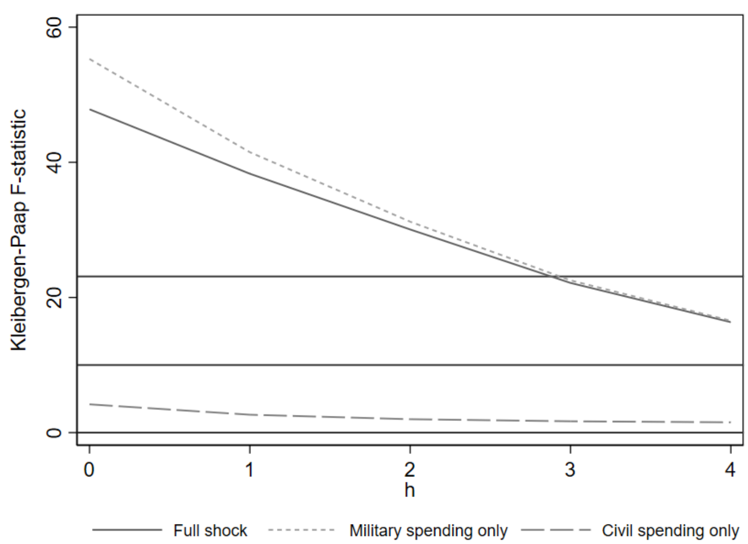

Figure 4: Kleibergen-Paap F-statistic at each horizon for each of the shocks.

(see figure 1). But the civil spending only shock — although underpowered and only significant at the 5% level on impact and at the 10% level after one year — has a much larger point estimate, and is statistically sig-

	Impact (Year 0)	Year 1	Year 2	Year 3	Year 4
Output elasticity	0.0717*** (0.0247)	0.0816*** (0.0249)	0.0860*** (0.0257)	0.0844*** (0.0277)	0.0762** (0.0307)
Y/G	6.0667	6.0905	6.1148	6.1396	6.1653
Multiplier estimate	0.4352***	0.4970***	0.5258***	0.5158***	0.4698**
90% AR confidence set	[0.1984, 0.6719]	[0.2574, 0.7366]	[0.2777, 0.7739]	[0.2215, 0.7865]	[0.1392, 0.7689]
Kleibergen-Paap F-stat	47.8438	38.3113	30.0628	22.1509	16.3427
N	140	139	138	137	136
Significance: *** - 1%; ** - 5%; * - 10%					

Table 4: LP-IV estimates of the fiscal multiplier using the full shock. Values in brackets are heteroscedasticity and autocorrelation robust (HAR) standard errors estimated using the QS kernel. 90% confidence sets are calculated based on the inverted Anderson-Rubin test, in line with Davidson and MacKinnon (2014) and José L Montiel Olea, Stock, and Watson (2021). Statistical significance is calculated based the whole Anderson-Rubin confidence sets being the same side of zero on the real line at the 90% (for the 10% significance level), 95% (for the 5% significance level) and 99% (for the 1% significance level) confidence levels.

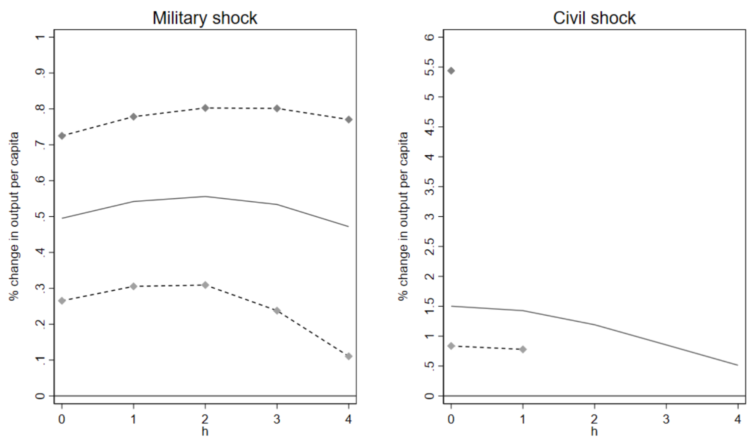

Figure 5: Cumulative IRFs of output per capita in response to a 1% of GDP increase in government spending per capita using the military spending only and civil spending only shocks. Dashed lines represent the bounds of the 90% Anderson-Rubin confidence set, calculated using HAR standard errors using the QS kernel.

nificantly higher than the estimate for the military spending only shock at those short horizons.

5.2 Effect on other macroeconomic variables

The LP-IV framework allows me to estimate IRFs for other macroeconomic variables, much in the same way as one would in a VAR. Figure 6 shows the results for consumption, the policy interest rate, the employment rate and inflation of a 1% change in government spending.

Consumption falls in response to an increase in government spending, corroborating the insights from the New Keynesian models and the results from Ramey (2009) and Ramey (2012) for the US. This result might be related to the fact that, much like Ramey's work, this paper's results are in large part driven by military spending shocks. But using the civil spending only shock also produces negative effects on consumption — significant at short horizons — which might indicate broader crowding-out effects of government spending.

Interest rates are a policy decision, and can respond differently to similar fiscal policy shocks, depending on the monetary policy setting. Accordingly, I find no evidence that monetary policy systematically accommodates or counteracts the fiscal shocks. Employment rate effects are strongly positive and statistically significant, in line with the literature, and are not only long-lasting but increasing over time. The estimates for the effect on inflation are positive and statistically significant — corroborating the findings of Ferrara et al. (2021), as well as the predictions of New Keynesian models.

5.3 State-dependent multipliers

I use the framework presented in equations (3)-(5) to estimate different fiscal multipliers across states of the economy, which can then be tested to ascertain whether they are statistically different from one another.

Table 5 summarises multiplier estimates across states of high and low slack using different measures. The output gap measure is based on a polynomial trend of potential output, for which I define high slack as an output gap of more than 0.5% below estimated potential GDP. The unemployment rate-based measure defines a state of high slack as one with an unemployment rate above the average of the whole period. And the NAIRU-based measure uses a Kálmán filter to estimate the non-accelerating inflation rate of unemployment, and defines high slack as

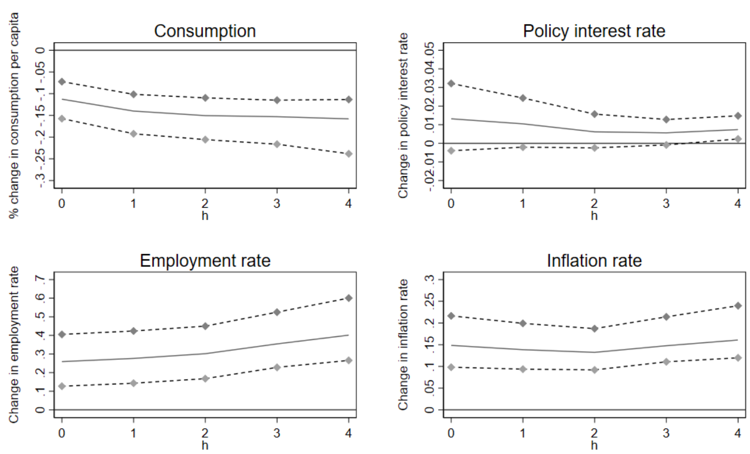

Figure 6: Cumulative IRFs of various macroeconomic variables in response to a 1% increase in government spending per capita using the full shock. Red dashed lines represent the bounds of the 90% Anderson-Rubin confidence set, calculated using HAR standard errors using the QS kernel.

Multiplier	Years after impact 0	1	2	3	4
	Actual employment rate				
High	0.45	0.67**	0.89***	1.05***	1.22***
Low	0.46***	0.54***	0.60***	0.60***	0.54***
Difference	-0.01	0.12	0.29	0.45	0.68
	Unemployment rate relative to estimated NAIRU				
High	2.11***	3.22***	3.72***	3.54***	3.01***
Low	0.39***	0.51***	0.58***	0.62***	0.60***
Difference	1.72***	2.71***	3.14***	2.92**	2.41**
	Output gap estimate				
Below -0.5%	1.34**	2.28**	2.57**	2.51**	2.09**
Above -0.5%	0.43***	0.53***	0.58***	0.59**	0.56**
Difference	0.90	1.75	1.99	1.92	1.53
Significance: *** - 1%; ** - 5%; * - 10%					

Table 5: Summary of multiplier estimates across states of slack. Significance calculations reflect the Anderson-Rubin confidence sets for single estimates and tests of restrictions on coefficients using HAR standard errors.

being an unemployment rate more than 0.5 percentage points higher than the estimated NAIRU at any given point.

In all cases except the NAIRU-based measure, the difference between the multiplier estimates in states of high and low slack is not statistically significant at conventional levels. Results are robust to different choices of thresholds and to methods of estimating potential output.

If we define high slack as unemployment being more than 0.5 percentage points above the estimated NAIRU, the results suggest significantly larger multipliers at high slack (1.2 on impact and as high as 2.1 after two years) than in low slack states (0.2 on impact and no higher than 0.3 at any horizon). The significant difference between states is sensitive to thresholds, but is present above 0.4 percentage points.

These results are not particularly strong — they represent some evidence of higher multipliers in specific definitions of high slack states, but otherwise display no statistical evidence of state-dependent differences. In that sense, they are weaker than the results of Gordon and Krenn (2010) and Auerbach and Gorodnichenko (2012) — which find strong evidence large multipliers in times of recession — but stronger than those of Ramey and Zubairy (2018) and Ghassibe and Zanetti (2022), with the caveat that all those are US-focused.

Table 6 looks at the suite of indicators of regimes for differences in multipliers used by Ilzetzki, Mendoza, and Végh (2013). Unlike that paper, I find no statistically significant difference between multipliers across each of the states. Unlike other states of interest (e.g. the zero-lower bound, which was only really hit in the UK after the 2007-08 crisis), the first-stage F-statistics do not indicate that the shocks are particularly underpowered in any of the states.

In all three cases (high/low debt, open/closed economy and flexible/fixed exchange rates), multiplier estimates for one of the main states are not statistically different from zero. That is the case for low debt states, higher openness states and fixed exchange rates. High debt (over

Multiplier	Years after impact 0	1	2	3	4
	Debt-to-GDP ratio				
High	0.36***	0.39***	0.40***	0.36***	0.28***
Low	0.29	0.33	0.32	0.34	0.33
Difference	0.07	0.06	0.08	0.02	-0.05
	Openness to trade				
High	0.13	0.21	0.25	0.30	0.31
Low	0.46**	0.50***	0.50***	0.44**	0.32*
Difference	-0.33	-0.29	-0.26	-0.15	-0.01
	Exchange rate regime				
Flexible	0.28***	0.33***	0.36***	0.37***	0.35***
Fixed	-0.01	0.05	0.01	-0.04	-0.08
Difference	0.30	0.28	0.35	0.41	0.43
Significance: *** - 1%; ** - 5%; * - 10%					

Table 6: Summary of multiplier estimates across regimes. Significance calculations reflect the Anderson-Rubin confidence sets for single estimates and tests of restrictions on coefficients using HAR standard errors.

60% of GDP), lower openness (below the historic average of 36% of GDP) and flexible exchange rates, on the other hand, are all associated with positive estimates that are statistically different from zero. The test of difference in coefficients however does not provide enough evidence to reject the null hypothesis that the two parameters are the same — weaker results than those found by Ilzetzki et al., although that is a cross-country study which is likely to have more variation in regimes than one country across time.

6 Policy implications

There are several important implications from these findings, both in terms of how we interpret historical events and how we might assess the impact of policy in the coming years. Of particular importance are the findings regarding the larger pound-for-pound effect of civil spending on output relative to military spending.

Looking back, one of the main conclusions is that the large increases in UK military spending during the First and Second World Wars had a large positive impact on output because of their very size — and not because of intrinsically higher output effects of spending on national defence. This is an analogous conclusion for the UK to that found by Ramey and Zubairy (2018) for the US: a large part of why the Great Depression came to an end was due to the size of the fiscal stimulus.

This is not to state in any way whether going to war is a good or bad idea, or whether devoting more resources to national defence should or should not be pursued. The UK's decision to enter the Second World War was, after all, a political and national imperative rather than a macroeconomic policy decision. But the implication is that wars have economic costs too, and the resources that needed to be used for defence could have had higher output stimulative effect had they not been required for the war.

Take the 1940-41 financial year, for example — the first full year of Britain's involvement in the Second World War. The in-year increase in military spending was equivalent to around 17% of GDP, and using the military shock multiplier I estimate that it raised output by 8.3%. But the output increase for the same stimulus on the civil side would have been 25%, or around three times as large as the military stimulus.

Of course, it is logical that the requisition of such an astonishing level of resources was only made possible by the threat faced by Britain at that time. It would have been unthinkable to increase spending by anywhere near that amount for non-defence reasons.

But with the UK Government announcing in 2025 that it will increase defence spending initially to 2.5% of GDP, and subsequenetial to 3%,[18] the forward-looking implications of these results are even more relevant. This is a much smaller increase in spending, and one where the threat is less imminent — and therefore the trade-off between civil and military expenditures more acute and relevant too.

The implication is that if the increase in military spending comes at the expense of civil spending as announced, one should expect this to *reduce* output in the short run relative to the counterfactual, as civil spending has a larger positive effect on GDP. The composition of spending therefore matters for how it affects output.

Again, this statement says nothing about whether this increase in defence spending is desirable or not. But my results suggest that based on Britain's historical record, we should not expect such a transfer to increase GDP and provide at least an immediate solution to the UK's continued low growth in the 21st century, but rather the opposite.

7 Conclusion

Fiscal multipliers are notoriously tricky to estimate, and one of the main issues is how to identify exogenous shocks that allow the establishment of causality. Narrative approaches from seminal papers such as Ramey and Shapiro (1998), Gordon and Krenn (2010), C. D. Romer and D. H. Romer (2010) and Ramey (2011) have increased our understanding of fiscal multipliers for US data, but data for other countries is costly to assemble and in many cases impractical. Cloyne (2013) used an archival source to conduct a narrative study focused on UK tax policy, but no analogue exists on the spending side. This paper seeks to provide that spending dataset and to ascertain what can be learned from a long time series approach to estimating spending multipliers for the UK.

In this paper, I have used a novel dataset which I have compiled from archival research in the UK Parliament, and which allows me to create a 140-year series of in-year, unanticipated discretionary spending shocks, which exploit the UK's idiosyncratic budget process and therefore are unlikely to be anticipated. This is further supported by running Mincer-

[18] Hansard record of the House of Commons sitting of 25 February 2025, volume 762: Defence and Security, statement by the Prime Minister.

Zarnowitz regressions, which fail to reject the hypothesis that the shocks are unanticipated.

Using a local projections method, I find the multiplier to be statistically significantly greater than zero at all horizons, with an estimate of 0.44 on impact and 0.47 in the long run. This is not dissimilar to results for the US, although higher than the majority of the estimates for the UK in the literature. I also find some evidence of a larger multiplier associated with civil spending shocks than with military spending shocks at short horizons. The implication is that historical government spending shocks such as the Second World War had a large effect on output because of their sheer size, rather than because of a particularly large pound-for-pound stimulative effect. This also implies that a transfer of spending from civil to military streams, as has been announced in Britain recently, might actually reduce output — at least in the short run.

I find that the effect of a positive spending shock on other macroeconomic variables broadly reflects results from New Keynesian models (lower private consumption, higher employment and higher inflation, although I find no significant effect on the policy interest rate) — which reflect some but not all the empirical literature. The fall in consumption in particular corroborates other studies which use large military spending shocks as their identification strategy — see Ramey (2009) and Ramey (2011).

In terms of state-dependent effects, I only find strong statistical evidence of higher multipliers in states with unemployment above the natural rate by large amounts — a finding that is robust for a number of thresholds. I find no evidence of other state-dependent differences, be they other measures of slack or broader economic conditions (high/low debt, open/closed economy or flexible/fixed exchange rates). The results for states of slack are less categorical than those found by Gordon and Krenn (2010) and Auerbach and Gorodnichenko (2012), but somewhat stronger those of Ramey and Zubairy (2018) and Ghassibe and Zanetti (2022) — although all those studies are US-focused. The results for the broader suite of economic regimes are weaker than those found by Ilzetzki, Mendoza, and Végh (2013), although that study uses cross-country data, and therefore may include more variation than available in following one country across time.

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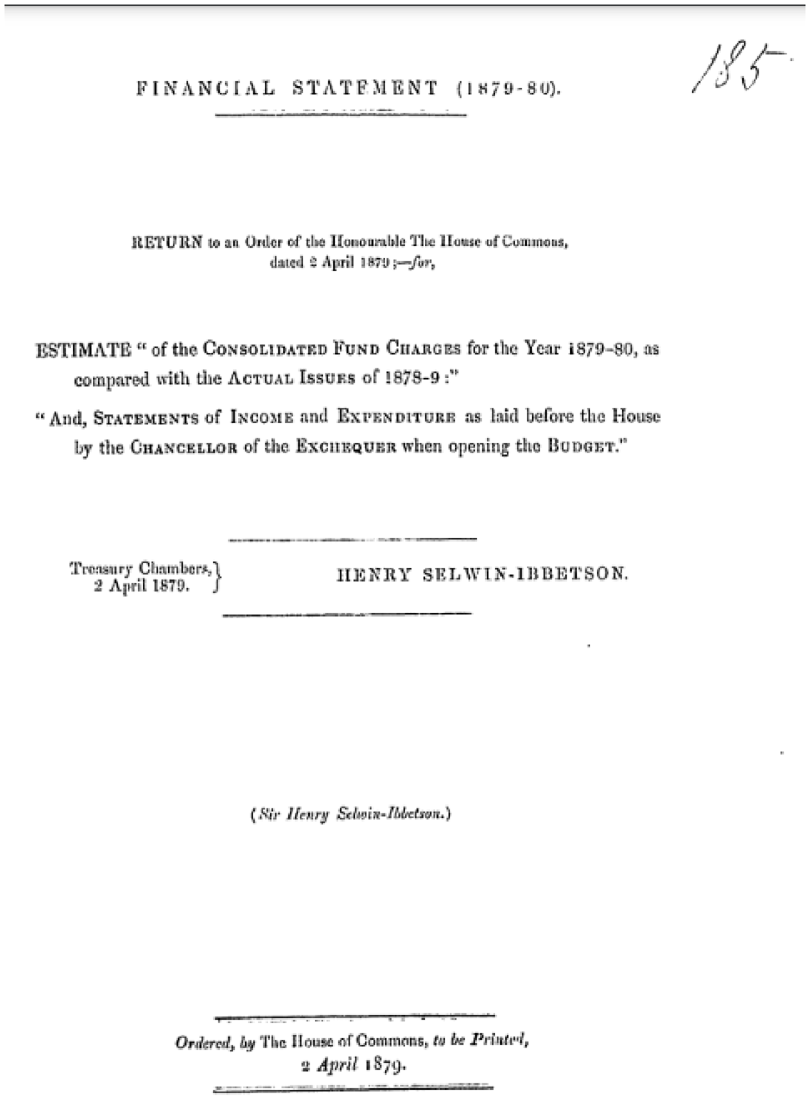
A Examples of documents used for archival research



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FINANCIAL STATEMENT (1879-80).

RETURN to an Order of the Honourable The House of Commons,
dated 2 April 1879;—*for*,

ESTIMATE "of the CONSOLIDATED FUND CHARGES for the Year 1879-80, as compared with the ACTUAL ISSUES of 1878-9:"

"And, STATEMENTS of INCOME and EXPENDITURE as laid before the House by the CHANCELLOR of the EXCHEQUER when opening the BUDGET."

Treasury Chambers, 2 April 1879. HENRY SELWIN-IBBETSON.

(*Sir Henry Selwin-Ibbetson.*)

Ordered, by The House of Commons, *to be Printed*,
2 *April* 1879.

Figure 7: Cover of the Financial Statement 1879-80, the first used for the estimation process. This is illustrative of all covers running until Budget 1997. Until then, the statement of income and expenditure for the Government was always signed and presented to the House of Commons by the Financial Secretary to the Treasury, rather than the Chancellor of the Exchequer, although of course all estimates were signed off by the Chancellor.

EXPENDITURE.

ESTIMATE for 1879-80, coupled with ACTUAL EXCHEQUER ISSUES in 1878-9.

	Estimate for 1879-80.	Actual Exchequer Issues in 1878-9.
	£.	£.
Permanent Charge of Debt	28,000,000	28,000,000
Interest on Local Loans	440,000	314,932
Interest on Supply, Exchequer Bonds and Loan to India	220,000	129,375
Charge of Suez Loan	200,000	199,876
Other Consolidated Fund Charges	1,760,000	1,624,422
	30,620,000	30,268,605
Army	15,645,700	17,653,473
Home Charges of Forces in India	1,100,000	1,080,000
Navy	10,586,894	11,962,316
Vote of Credit (South African War)	- -	1,500,000
Abyssinian Expedition	- -	17,865
Civil Services	15,084,851	14,974,760
Customs and Inland Revenue	2,865,383	2,791,169
Post Office	3,308,825	3,273,000
Telegraph Service	1,115,195	1,109,000
Packet Service	766,725	777,600
TOTAL - - - £.	81,153,573	85,407,788

NET DECREASE of Estimated Expenditure in 1879-80 - - - £. 4,254,215.

Figure 8: Table from the Financial Statement 1879-80 detailing the forecast at the beginning of the year for the 1879-80 financial year, as well as actual spending (labelled 'actual Exchequer issues') for the previous financial year of 1878-79. This is the type of table that is present in Financial Statements all the way until the introduction of spending controls in 1988, under the original 'planning total' guise. These are the longest standing breakdowns, and therefore the ones I use for military and civil spending purposes. The debt charge and any subsequent forms of interest payments are not used in the calculations for the shocks, which instead focus only on discretionary spending.

Service.	Estimate, 1914-15 (November 1914).	Estimate, 1915-16.	Estimate for 1915-16, more (+) or less (—) than 1914-15.
I. Consolidated Fund Services.			
National Debt Services :	£	£	£
Inside the Fixed Debt Charge :—			
Interest and Management	17,580,000	24,500,000	+ 3,750,000
Repayment of Capital	3,170,000		
	20,750,000	24,500,000	+ 3,750,000
Outside the Fixed Debt charge :—			
Interest and Management	3,443,000	‡ 15,750,000	+12,307,000
	24,193,000	40,250,000	+16,057,000
Road Improvement Fund	1,545,000	1,431,000	— 114,000
Payments to Local Taxation Accounts, &c.	9,885,000	9,406,000	— 479,000
Other Consolidated Fund Services	1,706,000	1,697,000	— 9,000
Total Consolidated Fund Services	37,329,000	52,784,000	+15,455,000
II. Supply Services.			
Army (including Ordnance Factories)	28,885,000	§ *15,000*	*—28,870,000*
Navy	51,550,000	§ *17,000*	*—51,533,000*
Civil Services*	58,885,000	59,018,000	+ 133,000
Customs and Excise, and Inland Revenue Departments	4,741,000	4,788,000	+ 47,000
Post Office Services	26,227,000	26,836,000	+ 609,000
Total Supply Services	170,288,000	*90,674,000*	*—79,614,000*
Votes of Credit	325,000,000	*250,000,000*	*—75,000,000*
Total	532,617,000	393,458,000	—139,159,000

Figure 9: Table from the Financial Statement 1915-16, during a period which saw some of the largest in-year policy changes due to the First World War. There are two things of particular interest. One is the large amount of the votes of credit, which allowed the government to plan in additional spending as it came without needing to vote it through the estimates process and apportioning it to different funding streams. This was very common in UK history in times of war, and gave the government flexibility in financing those wars. The second interesting point is the forecast for a much reduced cost of the war in 1915-16 compared with 1914-15. In fact, 1915-16 would be the second largest in-year increase in spending relative to forecast, totalling over 30% of GDP, surpassed only by the 46% of GDP increase the following year.

	1915-16.			
	Total Expenditure provided for in the Budget as revised in September 1915.	Additional Expenditure for which Supplementary Estimates were presented.	Total Estimated Expenditure.	Amount issued to meet Total Expenditure
I. CONSOLIDATED FUND SERVICES.	£	£	£	£
National Debt Services :				
Inside the Fixed Debt Charge - -	22,055,000	—	22,055,000	20,338,000
Outside the Fixed Debt Charge - -	45,030,000	—	45,030,000	39,911,000
	67,085,000	—	67,085,000	60,249,000
Road Improvement Fund - - - -	525,000	—	525,000	694,000
Payments to Local Taxation Accounts, &c.	9,600,000	—	9,600,000	9,757,000
Other Consolidated Fund Services - -	1,800,000	—	1,800,000	2,788,000
Total Consolidated Fund Services -	79,010,000	—	79,010,000	73,488,000
II. SUPPLY SERVICES.				
Army (including Ordnance Factories) -	16,000	—	16,000	15,000
Navy - - - - - - - -	17,000	—	17,000	7,000
Ministry of Munitions (including Ordnance Factories).	—	2,000	2,000	2,000
Civil Services - - - - - - -	59,039,000	196,000	59,235,000	54,718,000
Customs and Excise and Inland Revenue -	4,788,000	—	4,788,000	4,603,000
Post Office Services - - - - -	26,836,000	—	26,836,000	26,673,000
Total Supply Services - - -	90,696,000	198,000	90,894,000	86,018,000
Votes of Credit - - - - -	1,420,000,000	—	1,420,000,000	1,399,652,000
GRAND TOTAL - - -	1,589,706,000	198,000	1,589,904,000	1,559,158,000

Figure 10: Table from the Financial Statement 1916-17, showing the actual expenditure in 1915-16. The duration of the war and its toll on the public finances was such that a revised Budget had to be issued in September 1915. Votes of credit became almost the whole of the government's financing in terms of parliamentary procedure, and I have apportioned them fully to military expenditure as they were war related. When compared with the previous table, it becomes clear that government spending was over four times the allocated amounts at the beginning of the financial year — a scale of intra-year shock that no other war has come close to.

1948–49

TABLE I.—Comparison of Exchequer Issues with Estimated Expenditure

1947–48 Exchequer Issues	—	1948–49 Total Expenditure provided for in the Budget	1948–49 Supplementary and Excess Votes subsequently granted	1948–49 Total Estimated Expenditure	1948–49 Exchequer Issues
	Ordinary Expenditure				
£000	CONSOLIDATED FUND SERVICES	£000	£000	£000	£000
502,626	Interest and Management of National Debt	500,000	—	500,000	477,176*†
24,314	Payments to Northern Ireland Exchequer	26,000	—	26,000	32,294‡
7,291	Other Consolidated Fund Services	8,000	—	8,000	9,501
534,231	Total	534,000	—	534,000	518,971
	SUPPLY SERVICES				
383,600	Army Votes	305,000	55,000	360,000	346,700
194,300	Navy Votes	153,000	15,500	168,500	162,700
181,900	Air Votes	173,000	16,400	189,400	186,900
94,050	Ministry of Supply (Defence)	61,000	*2,000*	59,000	56,300
331	Ministry of Defence ...	632	—	632	550
854,181	Total Defence	692,632	84,900	777,532	753,150
1,768,729	Civil Votes (excluding Ministry of Supply (Defence))	1,708,613	224,309	1,932,922	1,844,815
29,963	Customs and Excise, Inland Revenue and balance of Post Office Votes ...	40,434	150	40,584	35,846
2,652,873	Total Supply Services ...	2,441,679	309,359	2,751,038	2,633,811
3,187,104	**Total Ordinary Expenditure**	**2,975,679**	**309,359**	**3,285,038**	**3,152,782**
22,374	**Sinking Funds**	—	—	—	**22,824†**
3,209,478		**2,975,679**	**309,359**	**3,285,038**	**3,175,606**
	Self-Balancing Expenditure				
143,300	Post Office Expenditure corresponding to Revenue ...	150,200	—	150,200	152,700
23,183	Excess Profits Tax, Post-war refunds (part deducted for tax)	16,000	—	16,000	8,751
166,483	**Total Self-Balancing Expenditure**	**166,200**	—	**166,200**	**161,451**

Figure 11: Table from the Financial Statement 1949-50 detailing comparisons between initial estimates and actual spending for 1948-49. That year saw a large shock in civil spending due in no small part to overspending on the newly created health service.

B List of variables used

Variable	ONS code 1946-2018	Thomas and Dimsdale (2017) reference (Sheet.Column) 1878-1945	Notes
Population	UKPOP	A18.C until 1920 A18.D subsequently	Spliced series. Follows political boundaries
Nominal GDP	YBHA	A9.P	Spliced series
Real GDP	ABMI	A8.U	Monetary series calculated from A8.U index for splicing
GDP deflator	n/a	n/a	Calculated from nominal and real GDP
Employment	MGRZ	A50.C	Spliced series. Simplified assumption of employment divided by population to obtain employment rate
Official Bank Rate	n/a	n/a	Obtained from Bank of England (official history since 1694)
Unemployment	MGSC	A50.G	Used to calculate unemployment rate
Consumption	ABPF + ABNU	A12.F	Households + non-profits serving households. Spliced series
National debt	HF6W	A29.AJ	Spliced series
Central government managed expenditure	ECOD	A27.C	Spliced series
Exports	IKBL	A35.G	Spliced series
Imports	IKBK	A35.M	Spliced series

Table 7: List of variables used in the estimation process.

C Mincer-Zarnowitz regression results

Table 8 shows the regression coefficients estimated using the Mincer-Zarnowitz regressions, as well as the testing of the joint hypothesis that the coefficient of the residual of the shock is 1 and the constant is 0. In none of the three cases is this rejected, as discussed in the chapter, which lends further confidence to the shocks being unanticipated. y is output per capita, g is government spending per capita, r is the policy interest rate (Bank Rate and its predecessors), π is the inflation rate as measured by the GDP deflator, e is the employment rate, c is consumption per capita and d is the debt-to-GDP ratio. The residuals used in the bottom panel are obtained by subtracting the fitted values of the top panel from the observed values of the shocks.

	Full shock	Military spending shock	Civil spending shock
$\ln y_{t-1}$	-0.6021***	-0.5988***	-0.0033
	(0.0835)	(0.0869)	(0.0129)
$\ln g_{t-1}$	0.1495***	0.1502***	-0.0007
	(0.0199)	(0.0208)	(0.0031)
r_{t-1}	0.8212**	0.6155	0.2057***
	(0.3981)	(0.3987)	(0.0635)
π_{t-1}	-0.1351	-0.2049	0.0699***
	(0.1263)	(0.1318)	(0.0195)
e_{t-1}	0.0592**	0.0633**	-0.0041
	(0.0258)	(0.0271)	(0.0043)
$\ln c_{t-1}$	0.3281***	0.3241***	0.0039
	(0.0653)	(0.0680)	(0.0101)
d_{t-1}	-0.0947***	-0.0966***	0.0019
	(0.0135)	(0.0141)	(0.0021)
Constant	134.8807***	133.2610***	1.6197
	(19.5060)	(20.3385)	(3.0930)
N	140	140	140
F-stat	10.34	9.05	5.73
Residual of shock	1.0000***	1.0000***	1.0000***
	(0.1017)	(0.1007)	(0.0642)
Constant	1.1289	0.9872	0.1416*
	(0.7433)	(0.7399)	(0.0787)
N	140	140	140
F-stat	95.33	97.30	239.05
Joint hypothesis test	2.31	1.78	3.23
p-value	0.3156	0.4105	0.1984
Significance: *** - 1%; ** - 5%; * - 10%			

Table 8: Mincer-Zarnowitz regression outputs and testing of joint hypothesis that the coefficient on the residual of the shock is 1 and the constant is 0. Numbers in brackets are HAR standard errors, calculated using the QS kernel.

D Regression results for the linear case

Table 9 shows the results of the cointegration tests between $\sum_h \ln g_{t+j}$ and the relevant left-hand side variables at different horizons. As the results show, there is strong evidence of a common stochastic trend between output and government spending, as well as between consumption and government spending, such that we can confidently reject the null hypothesis that a linear combination of them is I(1) using the first step in the Engle-Granger procedure.

	Horizon h				
Cointegration with $\sum_h \ln g_{t+j}$	0	1	2	3	4
$\sum_h \ln y_{t+j}$					
Test statistic	-3.121	-3.603	-3.971	-4.152	-4.202
p-value	0.025	0.006	0.002	<0.001	<0.001
$\sum_h \ln c_{t+j}$					
Test statistic	-3.523	-4.064	-4.461	-4.552	4.491
p-value	0.007	0.001	<0.001	<0.001	<0.001
$\sum_h r_{t+j}$					
Test statistic	-11.077	-12.251	-11.815	-7.959	-6.046
p-value	<0.001	<0.001	<0.001	<0.001	<0.001
$\sum_h \pi_{t+j}$					
Test statistic	-5.104	-5.652	-5.925	-5.559	-5.374
p-value	<0.001	<0.001	<0.001	<0.001	<0.001
$\sum_h e_{t+j}$					
Test statistic	-3.708	-4.537	-4.858	-4.988	-4.956
p-value	0.004	<0.001	<0.001	<0.001	<0.001

Table 9: Results from the first step of the Engle-Grange procedure, the augmented Dickey-Fuller test on the residuals of the two variables. Null hypothesis is the presence of an I(1) process, and therefore rejection means evidence of a joint I(0) process — in other words, cointegration.

The specification in equation (1) essentially amounts to an instrumental variables-augmented implementation of an autoregressive distributive lag (ARDL) model, a procedure that is robust to cointegrated variables by including contemporaneous variables on the right-hand side (Pesaran, Shin, et al. 1995). I choose this implementation rather than an error correction model (ECM) because my interest in less in the short-run dynamics of the variables — something an ECM is more suited to — and more in what the cumulative and long-run effect is on output and consumption. Implementing a deterministic trend, an error correction term or running a canonical ARDL model all yield very similar results to what is essentially an analogous implementation to the stochastic trend model in Blanchard and Perotti (2002), who also found little difference between implementing and not implementing the error correction term. Running the PSS bounds test (Pesaran, Shin, and Smith 2001) after an ARDL model strongly supports the use of contemporaneous and lagged levels in the regression.

Table 10 summarises the regression results for the linear case when using the full shock as instrument for $\sum_h \ln g_{t+j}$. These are the outputs of the estimation of equation (1) across each horizon h, with the coeffi-

$\sum_h \ln y_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.0717***	0.0816***	0.0860***	0.0844***	0.0762**
	(0.0247)	(0.0249)	(0.0257)	(0.0277)	(0.0307)
$\ln y_{t-1}$	1.1084***	2.2648***	3.4289***	4.6398***	5.9678***
	(0.0461)	(0.1369)	(0.2668)	(0.4225)	(0.5910)
$\ln g_{t-1}$	-0.0816***	-0.1882***	-0.2976***	-0.4025***	-0.5034***
	(0.0283)	(0.0595)	(0.0948)	(0.1353)	(0.1792)
r_{t-1}	-0.3592*	-1.0817**	-1.6637*	-2.1073	-2.6349
	(0.2034)	(0.5389)	(0.9288)	(1.2948)	(1.6062)
π_{t-1}	-0.0533	-0.0850	-0.0951	-0.0910	-0.0418
	(0.0620)	(0.1695)	(0.3078)	(0.4629)	(0.6297)
e_{t-1}	-0.0443***	-0.1349***	-0.2628***	-0.4118***	-0.5706***
	(0.0134)	(0.0410)	(0.0820)	(0.1329)	(0.1899)
$\ln c_{t-1}$	-0.0794**	-0.1821*	-0.2759	-0.3851	-0.5467
	(0.0352)	(0.1053)	(0.2067)	(0.3308)	(0.4687)
d_{t-1}	0.0129*	0.0411*	0.0823**	0.1394**	0.2203**
	(0.0074)	(0.0214)	(0.0411)	(0.0646)	(0.0904)
Constant	-2.3889	-4.0403	-3.9737	-15.8448	-60.7634
	(10.2600)	(30.5216)	(60.1374)	(96.7004)	(138.9287)
N	140	139	138	137	136
Kleibergen-Paap F-stat	47.844	38.311	30.063	22.151	16.343
AIC	601.044	858.973	1009.820	1108.692	1178.854
SBIC	627.519	885.384	1036.166	1134.972	1205.068
Significance: *** - 1%; ** - 5%; * - 10%					

Table 10: Regression estimates for output using the full shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

cient on $\sum_h \ln g_{t+j}$ being the output elasticity with respect to government spending of interest. Tables 11 to 14 show the effects on consumption, policy interest rates, the employment rate and inflation, whereas tables 15 and 16 show the results for the military and civil spending shocks, respectively.

$\sum_h \ln c_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	-0.1127***	-0.1401***	-0.1505***	-0.1530***	-0.1580***
	(0.0256)	(0.0273)	(0.0289)	(0.0305)	(0.0358)
$\ln y_{t-1}$	0.1837***	0.5705***	1.1380***	1.8626***	2.4797***
	(0.0455)	(0.1411)	(0.2720)	(0.4334)	(0.6045)
$\ln g_{t-1}$	0.0976***	0.2199***	0.3141***	0.3766***	0.4318**
	(0.0299)	(0.0663)	(0.1061)	(0.1446)	(0.1987)
r_{t-1}	-0.1392	-0.4059	-0.6970	-1.0653	-1.5230
	(0.1975)	(0.5190)	(0.8842)	(1.3487)	(1.5984)
π_{t-1}	-0.1594**	-0.3871**	-0.6076*	-0.8227	-1.0463
	(0.0647)	(0.1883)	(0.3541)	(0.5138)	(0.6297)
e_{t-1}	-0.0313**	-0.0858**	-0.1693**	-0.2917**	-0.4494**
	(0.0131)	(0.0411)	(0.0840)	(0.1423)	(0.1984)
$\ln c_{t-1}$	0.8483***	1.5481***	2.1303***	2.6010***	2.9608***
	(0.0347)	(0.1086)	(0.2124)	(0.3415)	(0.4841)
d_{t-1}	0.0068	0.0250	0.0611	0.1123*	0.1788*
	(0.0076)	(0.0227)	(0.0437)	(0.0678)	(0.0970)
Constant	-8.9235	-40.1515	-96.5602	-165.8469	-251.9462*
	(9.9859)	(30.3955)	(60.4976)	(102.3041)	(144.7959)
N	140	139	138	137	136
Kleibergen-Paap F-stat	47.780	38.260	30.180	24.686	16.453
AIC	609.023	880.428	1032.279	1130.411	1207.363
SBIC	635.498	906.838	1058.624	1156.691	1233.577
Significance: *** - 1%; ** - 5%; * - 10%					

Table 11: Regression estimates for consumption using the full shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

$\sum_h r_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.0132	0.0105	0.0062	0.0056	0.0074**
	(0.0109)	(0.0079)	(0.0055)	(0.0041)	(0.0036)
$\ln y_{t-1}$	0.0341***	0.0857***	0.1169***	0.1256***	0.1231**
	(0.0120)	(0.0275)	(0.0383)	(0.0467)	(0.0572)
$\ln g_{t-1}$	-0.0175	-0.0283*	-0.0256	-0.0280*	-0.0353*
	(0.0114)	(0.0163)	(0.0167)	(0.0168)	(0.0185)
r_{t-1}	0.2701**	-0.0768	-0.3763*	-0.5531***	-0.4595**
	(0.1176)	(0.2067)	(0.2159)	(0.1900)	(0.1833)
π_{t-1}	0.0031	-0.0080	-0.0116	0.0198	0.0244
	(0.0245)	(0.0492)	(0.0608)	(0.0656)	(0.0714)
e_{t-1}	-0.0049	-0.0146*	-0.0238*	-0.0303*	-0.0355*
	(0.0036)	(0.0088)	(0.0128)	(0.0162)	(0.0201)
$\ln c_{t-1}$	-0.0278***	-0.0767***	-0.1109***	-0.1245***	-0.1359***
	(0.0097)	(0.0220)	(0.0306)	(0.0374)	(0.0458)
d_{t-1}	0.0031	0.0056	0.0057	0.0060	0.0043
	(0.0021)	(0.0045)	(0.0062)	(0.0075)	(0.0091)
Constant	-1.8729	-0.0589	5.1406	10.4163	19.5396
	(3.1549)	(7.1885)	(10.1351)	(12.4143)	(15.0495)
N	140	139	138	137	136
Kleibergen-Paap F-stat	47.780	38.260	29.983	22.070	16.270
AIC	401.059	538.740	563.543	564.907	578.276
SBIC	427.534	565.150	589.889	591.187	604.490
Significance: *** - 1%; ** - 5%; * - 10%					

Table 12: Regression estimates for the policy interest rate using the full shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

$\sum_h e_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.2592***	0.2764***	0.3017***	0.3543***	0.4014***
	(0.0838)	(0.0844)	(0.0849)	(0.0893)	(0.0960)
$\ln y_{t-1}$	0.1355	0.2499	0.1117	-0.3012	-0.8926
	(0.1210)	(0.3453)	(0.6462)	(1.0341)	(1.4906)
$\ln g_{t-1}$	-02767***	-0.5517***	-0.8255***	-1.1694***	-1.5092***
	(0.0911)	(0.1819)	(0.2686)	(0.3637)	(0.4771)
r_{t-1}	-0.1259	-1.5701	-4.0579	-6.6950*	-9.331*
	(0.6868)	(1.7552)	(2.9036)	(3.9968)	(4.9784)
π_{t-1}	-0.0875	-0.3892	-0.7076	-1.1236	-1.7435
	(0.1989)	(0.5436)	(0.9621)	(1.4230)	(1.9188)
e_{t-1}	0.9143***	1.6968***	2.3599***	2.9075***	3.3901***
	(0.0380)	(0.1119)	(0.2141)	(0.3358)	(0.4944)
$\ln c_{t-1}$	-0.0810	-0.1696	-0.0671	0.1955	0.5123
	(0.0956)	(0.2748)	(0.5160)	(0.8257)	(1.1871)
d_{t-1}	0.0279	0.0613	0.0945	0.1210	0.1269
	(0.0205)	(0.0567)	(0.1045)	(0.1650)	(0.2373)
Constant	-4.1587	48.6952	173.512	392.6746	695.1702
	(29.4783)	(85.7425)	(165.3337)	(267.9368)	(393.7849)
N	140	139	138	137	136
Kleibergen-Paap F-stat	47.780	38.260	29.983	22.257	16.702
AIC	953.810	1182.876	1319.055	1409.737	1474.250
SBIC	962.285	1209.286	1345.400	1436.017	1500.464
Significance: *** - 1%; ** - 5%; * - 10%					

Table 13: Regression estimates for the employment rate using the full shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

$\sum_h \pi_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.1485***	0.1386***	0.1324***	0.1477***	0.1608***
	(0.0356)	(0.0317)	(0.0286)	(0.0297)	(0.0328)
$\ln y_{t-1}$	0.0720	0.1881	0.2956	0.3944	0.5104
	(0.0502)	(0.1405)	(0.2386)	(0.3742)	(0.5363)
$\ln g_{t-1}$	-0.1346***	-0.2150***	-0.2677***	-0.3739***	-0.4863***
	(0.0386)	(0.0705)	(0.0954)	(0.1307)	(0.1744)
r_{t-1}	0.4347	0.7558	0.6836	0.5646	0.4726
	(0.3095)	(0.7036)	(0.9696)	(1.2336)	(1.4814)
π_{t-1}	0.7156***	1.0108***	1.1238***	1.1720**	1.1514*
	(0.0832)	(0.2081)	(0.3347)	(0.4883)	(0.6638)
e_{t-1}	-0.0419***	-0.1144***	-0.1897**	-0.2679**	-0.3320*
	(0.0154)	(0.0440)	(0.0778)	(0.1232)	(0.1778)
$\ln c_{t-1}$	-0.0873**	-0.2766**	-0.4932***	-0.7385**	-1.0329**
	(0.0397)	(0.1103)	(0.1884)	(0.29734)	(0.4303)
d_{t-1}	0.0021	-0.0128	-0.0396	-0.0709	-0.1097
	(0.0085)	(0.0227)	(0.03824)	(0.0592)	(0.0848)
Constant	18.3787	75.1869**	151.7024***	246.6820***	352.0389***
	(12.2049)	(33.3398)	(58.0202)	(91.3697)	(132.7762)
N	140	139	138	137	136
Kleibergen-Paap F-stat	47.862	39.408	29.983	22.070	16.270
AIC	721.759	928.500	1026.705	1109.318	1178.127
SBIC	748.234	954.911	1053.050	1135.597	1204.341
Significance: *** - 1%; ** - 5%; * - 10%					

Table 14: Regression estimates for the inflation rate using the full shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

$\sum_h \ln y_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.0816***	0.0890***	0.0909***	0.0869***	0.0765**
	(0.0240)	(0.0246)	(0.0256)	(0.0276)	(0.0307)
$\ln y_{t-1}$	1.1120***	2.2695***	3.4312***	4.6391***	5.9672***
	(0.0454)	(0.1363)	(0.2664)	(0.4225)	(0.5916)
$\ln g_{t-1}$	-0.0923***	-0.2032***	-0.3112***	-0.4106***	-0.5045***
	(0.0275)	(0.0588)	(0.0945)	(0.1352)	(0.1792)
r_{t-1}	-0.3723*	-1.1085**	-1.6936*	-2.1279	-2.6384
	(0.2045)	(0.5426)	(0.9338)	(1.2997)	(1.6121)
π_{t-1}	-0.0473	-0.0759	-0.0877	-0.0879	-0.0416
	(0.0614)	(0.1691)	(0.3078)	(0.4629)	(0.6296)
e_{t-1}	-0.0455***	-0.1375***	-0.2658***	-0.4140***	-0.5710***
	(0.0133)	(0.0409)	(0.0820)	(0.1328)	(0.1896)
$\ln c_{t-1}$	-0.0813**	-0.1856*	-0.2759	-0.3876	-0.5471
	(0.0348)	(0.1048)	(0.2067)	(0.3303)	(0.4680)
d_{t-1}	0.0141*	0.0426*	0.0833**	0.1395**	0.2202**
	(0.0073)	(0.0214)	(0.0411)	(0.0646)	(0.0904)
Constant	-3.0243	-4.0614	-2.5397	-13.6872	-60.1836
	(10.1632)	(30.4694)	(60.1489)	(96.6211)	(138.5870)
N	140	139	138	137	136
Kleibergen-Paap F-stat	55.287	41.498	31.214	22.531	16.553
AIC	600.408	859.012	1010.012	1108.659	1178.794
SBIC	626.883	885.422	1036.357	1134.939	1205.008
Significance: *** - 1%; ** - 5%; * - 10%					

Table 15: Regression estimates for output using the military spending shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

$\sum_h \ln y_{t+j}$	Horizon h 0	1	2	3	4
$\sum_h \ln g_{t+j}$	0.2474*	0.2343*	0.1948	0.1390	0.0832
	(0.1015)	(0.1163)	(0.1116)	(0.0965)	(0.0891)
$\ln y_{t-1}$	1.1719***	2.3607***	3.4789***	4.6233***	5.9563***
	(0.0791)	(0.2151)	(0.3469)	(0.4530)	(0.6173)
$\ln g_{t-1}$	-0.2716**	-0.4996**	-0.5973*	-0.5765*	-0.5270
	(0.1106)	(0.2417)	(0.3192)	(0.3290)	(0.3347)
r_{t-1}	-0.5927*	-1.6359*	-2.3231*	-2.5512	-2.7044
	(0.3422)	(0.9135)	(1.3863)	(1.6480)	(1.9130)
π_{t-1}	0.0537	0.10354	0.0694	-0.02406	-0.0392
	(0.1148)	(0.3009)	(0.4591)	(0.5408)	(0.6300)
e_{t-1}	-0.0662***	-0.1880**	-0.3295***	-0.4581***	-0.5776***
	(0.0247)	(0.0736)	(0.1238)	(0.1621)	(0.2018)
$\ln c_{t-1}$	-0.1125*	-0.2553	-0.3589	-0.4387	-0.5547
	(0.0577)	(0.1660)	(0.2761)	(0.3560)	(0.4656)
d_{t-1}	0.0330**	0.0733*	0.1040*	0.1408**	0.2181**
	(0.0158)	(0.0398)	(0.0584)	(0.0703)	(0.0940)
Constant	-13.6892	-4.4765	27.6496	30.6183	-49.2032
	(17.2534)	(46.5171)	(84.1165)	(128.177)	(190.8629)
N	140	139	138	137	136
Kleibergen-Paap F-stat	4.186	2.649	1.984	1.682	1.531
AIC	720.713	975.062	1086.889	1132.167	1178.106
SBIC	747.188	1001.472	1113.234	1158.447	1204.320
Significance: *** - 1%; ** - 5%; * - 10%					

Table 16: Regression estimates for output using the civil spending shock at each horizon h. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

E Regression results with the full shock and different states of slack

Table 17 shows the regression results for high and low slack states as defined by whether the observed unemployment rate is more or less than 0.5 percentages above the estimated NAIRU (non-accelerating inflation rate of unemployment, also called the natural rate of unemployment) — the latter being estimated using a Kálmán filter. This is the only state-dependent estimate for which I estimate significant differences in the coefficients on the cumulative change in government spending. Details of the remaining regression outputs are available on request.

$\sum_h \ln y_{t+j}$	Horizon h 0	1	2	3	4
$I_{t-1} \sum_h \ln g_{t+j}$	0.3373***	0.5150***	0.5953***	0.5652***	0.4819***
	(0.0919)	(0.1383)	(0.1662)	(0.1643)	(0.1492)
$(1 - I_{t-1}) \sum_h \ln g_{t+j}$	0.0654**	0.0842***	0.0957***	0.1009***	0.0981***
	(0.0246)	(0.0266)	(0.0273)	(0.0283)	(0.0293)
I_{t-1}	68.5526**	347.5754***	753.4731***	1106.8980***	1352.4500***
	(33.3170)	(123.8984)	(250.5096)	(364.6650)	(459.7882)
$I_{t-1} \ln y_{t-1}$	0.95568***	1.3834***	1.4164	1.6836	2.4599
	(0.1261)	(0.4448)	(0.9140)	(1.3485)	(1.7132)
$(1 - I_{t-1}) \ln y_{t-1}$	1.0653***	2.1915***	3.3467***	4.5710***	5.8900***
	(0.0444)	(0.1293)	(0.2444)	(0.3787)	(0.5268)
$I_{t-1} \ln g_{t-1}$	-0.2661***	-0.6989***	-1.0889***	-1.2665***	-1.2426***
	(0.0672)	(0.1729)	(0.2847)	(0.3530)	(0.3768)
$(1 - I_{t-1}) \ln g_{t-1}$	-0.0732***	-0.1925***	-0.3283***	-0.4705***	-0.6089***
	(0.0298)	(0.0654)	(0.0990)	(0.1317)	(0.1638)
$I_{t-1} r_{t-1}$	-0.0360	-0.8635	-2.0477	-3.3558	-3.5415
	(0.4226)	(1.2044)	(2.1218)	(2.9343)	(3.3660)
$(1 - I_{t-1}) r_{t-1}$	-0.8293***	-2.2190***	-3.2086***	-3.7167**	-4.0676**
	(0.2270)	(0.6495)	(1.1478)	(1.5771)	(1.8715)
$I_{t-1} \pi_{t-1}$	-0.1805**	-0.3976*	-0.5769	-0.7368	-1.0098
	(0.0787)	(0.2104)	(0.3819)	(0.5457)	(0.6787)
$(1 - I_{t-1}) \pi_{t-1}$	0.0560	0.1565	0.2246	0.2316	0.2623
	(0.0623)	(0.1807)	(0.3520)	(0.5258)	(0.6744)
$I_{t-1} e_{t-1}$	0.0432	0.1770*	0.3202*	0.3854	0.3145
	(0.0280)	(0.0936)	(0.1849)	(0.2736)	(0.3384)
$(1 - I_{t-1}) e_{t-1}$	-0.0593***	-0.1725***	-0.3226***	-0.4887***	-0.6537***
	(0.0130)	(0.0402)	(0.0796)	(0.1276)	(0.1771)
$I_{t-1} \ln c_{t-1}$	-0.1151	-0.1108	0.0784	0.1961	0.1106
	(0.0730)	(0.2265)	(0.4561)	(0.6798)	(0.8825)
$(1 - I_{t-1}) \ln c_{t-1}$	-0.0355	-0.0951	-0.1602	-0.2650	-0.4132
	(0.0326)	(0.0973)	(0.1917)	(0.3050)	(0.4329)
$I_{t-1} d_{t-1}$	-0.0391**	-0.1766***	-0.3716***	-0.5250***	-0.6073**
	(0.0169)	(0.0632)	(0.1325)	(0.1988)	(0.2567)
$(1 - I_{t-1}) d_{t-1}$	0.0165**	0.0548***	0.1123***	0.1865***	0.2814***
	(0.0068)	(0.0195)	(0.0366)	(0.0566)	(0.0794)
Constant	3.7502	2.4678	-4.3250	-2.2284	-78.6029
	(9.6087)	(28.5691)	(55.9154)	(88.8015)	(124.8896)
Difference in coefficients	0.2719	0.4308	0.4995	0.4643	0.3838
Test statistic	8.67	9.74	9.09	8.07	6.70
p-value	0.003	0.002	0.003	0.005	0.010
N	140	139	138	137	136
Significance: *** - 1%; ** - 5%; * - 10%					

Table 17: Regression estimates for output using the full spending shock at each horizon h for different states of slack. I represents high slack (unemployment rate more than 0.5 percentage points higher than the estimated NAIRU) and $(1 - I)$ represents low slack. Numbers in brackets are HAR standard errors, calculated using the QS kernel. Significance calculated for the endogenous variable on the basis of the inverted Anderson-Rubin confidence test and using the t-test otherwise.

F Shocks used in the first stage regression

Table 18 shows the raw values of the shocks used for the estimation process. These are in cash values (£ million) and in nominal terms, and so have been subsequently divided by GDP for normalisation.

Year	Full shock	Military	Civil	Year	Full shock	Military	Civil
1879-80	3.337	3.089	0.247	1949-50	52.395	-19.213	71.608
1880-81	1.103	0.727	0.376	1950-51	-68.696	17.564	-86.260
1881-82	0.046	-0.274	0.321	1951-52	-130.921	-161.762	30.841
1882-83	4.681	3.861	0.820	1952-53	68.612	26.520	42.092
1883-84	0.701	0.775	-0.074	1953-54	13.433	-132.262	145.695
1884-85	3.971	3.641	0.330	1954-55	-215.466	-118.644	-96.822
1885-86	-2.009	-1.999	-0.010	1955-56	-118.657	-89.300	-29.357
1886-87	0.200	0.459	-0.259	1956-57	64.614	26.400	38.214
1887-88	0.432	-0.378	0.811	1957-58	67.902	9.416	58.486
1888-89	-1.155	-0.855	-0.300	1958-59	57.398	49.200	8.198
1889-90	-0.082	0.182	-0.264	1959-60	11.933	-26.435	38.368
1890-91	0.525	0.170	0.355	1960-61	171.839	-21.830	193.669
1891-92	0.632	-0.351	0.983	1961-62	181.000	33.000	148.000
1892-93	-0.071	-0.027	-0.044	1962-63	84.000	45.000	39.000
1893-94	0.645	0.144	0.501	1963-64	-139.000	-46.000	-93.000
1894-95	0.200	0.004	0.196	1964-65	-69.000	-92.000	23.000
1895-96	2.001	1.499	0.502	1965-66	6.000	-64.000	70.000
1896-97	0.792	0.561	0.231	1966-67	85.000	-57.000	142.000
1897-98	1.576	0.201	1.375	1967-68	493.000	55.000	438.000
1898-99	1.281	1.069	0.212	1968-69	85.000	-39.000	124.000
1899-1900	22.725	22.388	0.337	1969-70	216.000	-62.000	278.000
1900-01	32.869	32.207	0.662	1970-71	517.000	200.000	317.000
1901-02	11.371	4.436	6.935	1971-72	929.000	8.000	921.000
1902-03	9.443	-0.310	9.753	1972-73	901.000	272.000	629.000
1903-04	3.477	3.196	0.281	1973-74	691.000	265.000	426.000
1904-05	-0.279	0.266	-0.545	1974-75	4,498.000	602.000	3,896.000
1905-06	-1.249	-1.052	-0.197	1975-76	5,343.000	744.000	4,599.000
1906-07	-2.698	-2.466	-0.232	1976-77	142.000	549.000	-407.000
1907-08	-0.886	-0.923	0.037	1977-78	-106.000	456.000	-562.000
1908-09	1.087	-0.750	1.837	1978-79	-415.000	533.000	-948.000
1909-10	0.374	0.465	-0.091	1979-80	942.000	545.000	397.000
1910-11	0.248	-0.529	0.777	1980-81	3,593.000	632.000	2,961.000
1911-12	-2.407	-1.576	-0.831	1981-82	2,000.000	500.000	1,500.000
1912-13	5.522	0.491	5.031	1982-83	-1,900.000	300.000	-2,200.000
1913-14	1.498	2.635	-1.137	1983-84	100.000	-300.000	400.000
1914-15	356.797	357.001	-0.204	1984-85	5,500.000	100.000	5,400.000
1915-16	1,001.701	1,006.186	-4.485	1985-86	-4,400.000	-100.000	-4,300.000
1916-17	1,672.150	1,673.665	-1.515	1986-87	-800.000	-300.000	-500.000
1917-18	427.725	427.800	-0.075	1987-88	-2,900.000	-200.000	-2,700.000
1918-19	-348.082	-352.000	3.918	1988-89	-5,100.000	-200.000	-4,900.000
1919-20	225.463	101.328	124.135	1989-90	3,300.000	500.000	2,800.000
1920-21	325.488	61.799	263.689	1990-91	1,600.000	900.000	700.000
1921-22	52.129	-18.025	70.154	1991-92	-3,600.000	100.000	-3,700.000
1922-23	-83.666	-27.079	-56.587	1992-93	-2,200.000	-300.000	-1,900.000
1923-24	-29.283	-16.211	-13.072	1993-94	200.000	-100.000	300.000
1924-25	-0.212	-0.426	0.214	1994-95	-4,200.000	-1,000.000	-3,200.000
1925-26	19.487	-1.136	20.623	1995-96	-1,400.000	-500.000	-900.000
1926-27	6.089	0.130	5.959	1996-97	-500.000	700.000	-1,200.000
1927-28	-3.847	2.325	-6.172	1997-98	-2,220.000	40.000	-2,260.000
1928-29	-2.581	-1.130	-1.451	1998-99	-220.000	310.000	-530.000
1929-30	8.496	0.390	8.106	1999-2000	300.000	500.000	-200.000
1930-31	11.941	0.435	11.506	2000-01	-100.000	800.000	-900.000
1931-32	0.184	-2.355	2.539	2001-02	-2,200.000	700.000	-2,900.000
1932-33	11.106	-1.334	12.440	2002-03	32,100.000	23,100.000	9,000.000
1933-34	-4.486	-1.074	-3.412	2003-04	1,000.000	1,800.000	-800.000
1934-35	10.236	0.159	10.077	2004-05	1,900.000	1,300.000	600.000
1935-36	17.610	12.699	4.911	2005-06	-300.000	500.000	-800.000
1936-37	4.633	27.821	-23.188	2006-07	-4,700.000	1,300.000	-6,000.000
1937-38	-22.333	-1.018	-21.315	2007-08	2,400.000	4,400.000	-2,000.000
1938-39	-6.583	18.941	-25.524	2008-09	100.000	5,000.000	-4,900.000
1939-40	383.013	401.963	-18.950	2009-10	100.000	500.000	-400.000
1940-41	1,217.323	1,220.001	-2.678	2010-11	-31,400.000	-12,500.000	-18,900.000
1941-42	566.681	585.000	-18.319	2011-12	-6,200.000	400.000	-6,600.000
1942-43	351.601	340.000	11.601	2012-13	-16,700.000	-3,000.000	-13,700.000
1943-44	43.409	50.000	-6.591	2013-14	-5,800.000	-1,500.000	-4,300.000
1944-45	124.219	125.000	-0.781	2014-15	-1,700.000	200.000	-1,900.000
1945-46	-82.229	-90.000	7.771	2015-16	-9,300.000	-200.000	-9,100.000
1946-47	13.555	-13.225	26.780	2016-17	-2,000.000	500.000	-2,500.000
1947-48	27.506	-45.150	72.656	2017-18	-1,000.000	0.000	-1,000.000
1948-49	192.132	60.518	131.614	2018-19	1,600.000	900.000	700.000

Table 18: Annual values of the in-year discretionary spending shocks — full, military only and civil only — used in the first stage of the regressions. Values are in £ million in current prices.