The Household Finance Analogy Does Not Drive Mass Support for Austerity

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Abstract

Austerity has been a feature of fiscal policy in many advanced industrialized economies and there is evidence that it is popularly accepted by electorates as necessary in many contexts. One very prominent explanation for this acceptance is that voters are opposed to government borrowing because they see it as analogous to household borrowing. This paper provides the first empirical evidence regarding the prevalence and consequence of the household finance analogy amongst voters by analyzing observational and experimental data from custom surveys fielded in the UK. We find that endorsement of the analogy is common and that analogy endorsement is indeed associated with more aversion to government borrowing. However, the evidence suggests that citizens who endorse the analogy are not applying their attitudes towards household borrowing to assess government borrowing any more than those who do not. Instead, those who are already opposed to government borrowing learn to endorse the household analogy as a justification from elite messages associating these attitudes.

1 Introduction

> The analogy between a household budget and a national economy is "seductive, because it's very easy for people to relate to", and it makes some sense when we're not in the grip of a macro-economic crisis. "But when we are, then individually rational behaviour adds up to a collectively disastrous result."
>
> (Paul Krugman, as quoted in Aitkenhead 2012)

> [T]he 'economy is like a household' idea ... has tremendous power, to the extent that I doubt we would have seen so much UK austerity without it
>
> (Wren-Lewis 2016)

In the aftermath of the Financial Crisis of 2007/8 and the associated Great Recession, the importance of reducing government debt and deficits has become a central conflict in economic policy debates (Blyth 2013; Farrell and Quiggin 2017). There is little consensus on the correct outcome among policy-makers and economists.[1] At the mass level too, while they may dislike (and punish) cuts to spending and increases in taxation (e.g. Bremer and Bürgisser 2019; Hübscher, Sattler, and Wagner 2018) there is plenty of evidence that voters are, on average, rather averse to government borrowing (e.g. Blinder and Holtz-Eakin 1983; Hansen 1998; A. Modigliani and F. Modigliani 1987; Peltzman 1992) – albeit by virtue of cues from political elites (Barnes and Hicks 2018; Bisgaard and Slothuus 2018).

There is a remarkable feature of the debate about the politics of government debt: the almost universal assumption that much of the power of the fiscally conservative side of the argument about fiscal consolidation can be attributed to analogical reasoning between the budget imperatives of households and those of governments (e.g. Krugman 2015; Wren-Lewis 2015). Voters are seen as hostile to counter-cyclical spending by governments on the basis of mistakenly attributing the constraints that households face to the government. When households hit economic hard times, they cut their expenditure and the same should apply to governments; so the analogical argument goes. Very explicitly, aversion to counter-cyclical

[1] E.g. compare Cochrane (2009) and Barro (2012) with Krugman (2015) and Wren-Lewis (2015).

spending by governments is seen to be, at least partially, *caused* by the widespread adoption and application of the analogy from household finances to government finances (e.g. Wren-Lewis 2016). In the view of many prominent scholars, the resulting political economy of public debt – that of "austerity" – has been "collectively disastrous" (Paul Krugman quoted in Aitkenhead 2012).

This idea of a pathological analogy, operating in the minds of both the policy elites and the mass electorates, is as neat as it is seductive. Despite this, we are aware of *no* extant empirical evidence that the claim, at least as it applies to voters in general, is actually true. In this paper, we provide the first such evidence; and find *no indication* that the analogy from household finances to government finances ('the household analogy') causes more hawkish attitudes regarding government borrowing. We do demonstrate, however, that there exists an observational correlation between endorsement of the household analogy and greater aversion to government borrowing. Finally, we reconcile these two apparently contradictory findings by providing evidence that is consistent with political elites communicating to voters in a way that associates the household analogy and debt aversion together, even though the former does not actually convince people of the latter – presumably because the elites believe the analogy *is* consequential.

Our findings are important for several reasons. Most notably, macroeconomists are at pains to point out that the financial constraints and imperatives of governments are radically different from those of households, and that failure to understand this can lead to very harmful policy choices. The textbook position is that fiscal stimulus is held to be an important tool for governments in the face of an economy in recession (The CORE Project 2015, ch. 13.8; Carlin and Soskice 2006, 194–197) – a position endorsed even by prominent deficit hawks (e.g. Alesina, Campante, and Tabellini 2008, 1006). Spending and borrowing more during a downturn is an important policy prescription – most notably when interest rates are at the zero lower bound – with the failure to follow it yielding lower economic output and higher unemployment. The recommendation for households facing worse financial circumstances is rather different. Subject to some degree of consumption smoothing, it is often perfectly sensible to reduce household expenditures when household incomes fall. Thus the analogy itself is macroeconomically dangerous, as it suggests a kind of popular 'paradox of thrift' in

public policy, whereby voters oppose government attempts to offset falling aggregate demand with increased government borrowing and expenditure.

In this light, it is critical to know whether adoption of the household analogy amongst voters is a cause of hawkish preferences on government borrowing. The answer has very obvious implications for ongoing debates regarding the political economy of macroeconomic management, generally, and of fiscal policy in particular (e.g. Schmidt and Thatcher 2014, 343; Blyth 2013; Crouch 2011, Chapter 5; Hopkin and Rosamond 2018; Farrell and Quiggin 2017; Raess and Pontusson 2015). If the household analogy is not so all pervasive amongst voters, Keynesian counter-cyclical spending may not face the political head-winds that we had previously thought. At least, it is useful to know that deficit-aversion is not the result of organic, erroneous analogical reasoning amongst voters.

Our findings also speak to the literature on mass reasoning about public policy using metaphors and analogies (Bougher 2012; Kalmoe 2014; Lau and Schlesinger 2005; Schlesinger and Lau 2000). Intriguingly, we show that an analogy can be relatively widely held, but have no (direct) causal effect on the attitude it is associated with.[2] The explanation that we advance for the combination of prevalence and causal irrelevance of the household analogy – that *elites* believe the analogy to be important – points to broader connections with the literature on elite communication – where we explicitly draw on the Receive–Accept–Sample model (Zaller 1992) – and framing (e.g. Chong and Druckman 2007). This is consistent with the broader political behaviour literature on how voters tend to form their opinions, as well as recent work specifically focused on the issue of deficits (Barnes and Hicks 2018; Bisgaard and Slothuus 2018).

More broadly, still, this paper contributes to an expanding effort to ensure that null findings are appropriately reported, circulated and understood (e.g. Esarey and Wu 2016; Franco, Malhotra, and Simonovits 2014; A. S. Gerber, Green, and Nickerson 2001; A. Gerber and Malhotra 2008). The need to publish such findings is especially important in situations where (a) there are strong prior beliefs about non-zero effects and (b) the results have important normative implications (e.g. Carnes and Lupu 2016). As we document, there is a strong

[2]To be clear, there is no implied claim that this is always the case with analogies, just that we must be as careful with our causal evidence here as with other types of explanation.

conventional wisdom about the importance of the household analogy, and it is thought to be extremely (negatively) consequential in political economy terms. The absence of evidence for an important and widely held expectation is surprising and significant.

2 The Household Analogy in Practice and Theory

The analogy between household and government finances has been made widely – especially by politicians of the right and center-right. In the USA, the then Speaker of the House of Representatives, John Boehner, argued that, "American families are tightening their belt, but they don't see government tightening its belt" (quoted in Krugman 2015). Likewise, right-leaning actors more broadly deploy the household analogy. For example, the Heritage Foundation in the USA use the idea of "the U.S. budget vs. the family budget" to explicitly, and negatively, frame government borrowing with reference to household finance.[3]

In the UK, the then Shadow Chancellor of the Exchequer, George Osborne, expressed concern with the Labour government "maxing out the nation's credit card" (Seager 2009). This echoed a long tradition emphasizing parallels between national and domestic budgets, running at least from the rhetoric of Margaret Thatcher's 1979 election campaign claim that, "Any woman who understands the problems of running a home will be nearer to understanding the problems of running a country." (quoted in Hooker 2014). Indeed, this kind of rhetoric is notably similar to the budget-balancing, financially prudent, 'Swabian housewife' of which Angela Merkel spoke approvingly in the aftermath of the Financial Crisis.

On the political left, the household economy metaphor is bemoaned in both popular and academic treatments. Academics query whether,

> the strength of neoliberalism may come from the seemingly common sense nature of neoliberal arguments. For example, appeals to the 'virtue' of sound finances using the metaphor of the household economy – extrapolating from the need to balance one's household budget to the need to do the same for the state budget – may resonate better with ordinary citizens than the Keynesian counterintuitive proposition to spend more at a time of high deficits and debts[.]

[3] At `https://www.federalbudgetinpictures.com/us-budget-vs-family-budget/`.

(Schmidt and Thatcher 2014, 343)

Meanwhile, left wing journalists lament that language and framing may be "duping" the public, with "[t]he same misleading metaphors . . . used again and again to talk about economic policy" (Williams 2018).

Whether simple common sense or simplistic error, the intuitive similarity between household and government spending is echoed in popular interventions in political debate. For example, Labour leader Ed Miliband was challenged in the 2015 election campaign on the basis that, "If I get to the end of the week and I can't afford to buy a pint it means I've overspent – I've got no money left. You got to the end of a 13-year government and you had spent, spent and spent."[4]

While the deployment of the analogy in the public sphere is pervasive, it is helpful to be more conceptually precise about what we mean by analogical reasoning in general? How is this applicable to political reasoning, and why do we focus on the household budget metaphor for public finance?

We have a fairly 'textbook' concept of analogy in mind. Analogies hold where similar relationships and systems of relationships hold across different objects. As such, analogies "capture parallels across different situations" (Gentner and Smith 2013). Perceiving these parallel features across different domains is a key feature of cognitive reasoning. Importantly, it also allows for learning when existing understandings of one object can be extended by the use of analogy to a new, unfamiliar one. This is a useful and powerful tool where the parallels are drawn appropriately; but it can lead to important inferential errors if the wrong parallels are drawn. The philosophical adequacy of arguments from analogy has a long history of debate – from Aristotle, via Mill's argument for other minds. But the presence of analogical reasoning in everyday psychology is much more straightforward. Applying familiar ideas to new domains which have some analogical similarity is ubiquitous in everyday language – e.g. consider "horsepower" as applied to cars, or "bandwidth" as applied to internet connectivity. Indeed, the ability to identify and use analogies appropriately is even used as a test of reasoning in itself in aptitude and IQ tests.

[4]BBC Question Time party leader debate, 30 April 2015. Also reported in e.g. Chorley and McTague (2015).

In political science more specifically, the content of analogies and more particularly metaphors employed by political elites in order to persuade has long been part of the study of political communication and rhetoric (see, for example Charteris-Black 2011). Other studies consider the types of metaphor deployed in media coverage of specific issues (Musolff 2004). But the focus of research explicitly concerned with how analogical reasoning acts as an independent variable has been more limited to the consideration of elite behaviour in general (but see Lau and Schlesinger 2005; Schlesinger and Lau 2000). As Bougher (2012) argues, this has also been predominantly focused on two specific domains: foreign policy (e.g. Khong 1992; Mintz and DeRouen, Jr 2010) and policy transfer.

Our particular interest is in mass attitudes regarding government borrowing: a policy which has little by way of *direct* personal effect on individuals. That is, as distinct from tax or benefit payments, for example, where people may have personal experience of payment or receipt, or other public policy choices which have tangible impact on voters' lives (e.g. choices on schools, healthcare, infrastructure and so on), macroeconomic policy choices remain abstract. The economic effects of fiscal policy choices are complicated, such that a huge amount of information would be required for voters to know how they, and indeed the country as a whole, would be affected by different choices. Under these conditions, the use of heuristic reasoning, rather than rational calculation, appears to be extremely likely.

There are other heuristics that voters may use – e.g. taking cues from party or media elites (see, for example Barnes and Hicks 2018). But many common heuristic devices are not easily available for reasoning about government budgets. For example, category based inference – simple reasoning based on stereotypes (Petersen 2009) – is hard to deploy since even stereotyped groups of winners and losers are hard to identify. What types of citizen would gain from an increase in government borrowing? The wealthy? The high income? The young? In contrast, adopting analogies to make inferences about the economy and economic policy seems like an obvious way for voters to reason about this highly abstract, impersonal and unfamiliar topic.

Indeed, qualitative work from the UK focused on exploring how people perceive, experience and understand 'the economy' finds that analogies and metaphors are ubiquitous. One common way of thinking about the economy is as a national container or "pot", with people

contributing or taking out, and with the overall goal of keeping the pot full (NEON et al. 2018, 28). Alternatively, the national *economy* as a whole is seen as analogous to a household: the most common way that participants in one recent study *defined* 'the economy' was as a personal or household budget (Ecnmy 2017).

If people use analogies to think about economics in general, the household budget analogy seems an even more powerful parallel for reasoning about the government budget. Given that the government budget is the same kind of object as a household budget (i.e. a 'budget'), that people already think of the national economy as a household budget, and that we know that people are more likely to retrieve analogies which have superficial object similarities, there are good theoretical reasons to expect the analogy of the household budget to be a widespread and powerful tool used to reason about, and shape preferences over, government budgets.

For households, income and expenditure must be reasonably balanced over at least the medium term – at least in the absence of sizeable savings.[5] The laments of economists and commentators concerned about the inappropriate use of the household analogy to reason about budgets bemoan the inference that this too applies to governments, neglecting as it does the significant differences between households and governments: especially that the state 'lives' forever; and that the state can engage in monetary expansion.[6]

These general arguments provide face validity to the arguments of economists and political commentators that indeed the household budget analogy may be responsible for opposition to government borrowing: it is a plausible analogy, and the underlying household logic, paired with personal budget expectations, could explain a reticence to support public borrowing – at least if the household analogy is widely advocated.

That voters reason about governments' budgets by analogy to households', then, is firmly in the category of conventional wisdom: apparently widely accepted by scholars, policy-makers, and journalists. This conventional wisdom extends, too, to predictions about the direction of policy preferences. At the household level, budget orthodoxy in the form of

[5] In a survey experiment for a slightly different project, we primed people to think about household income drops by asking them by how much they would reduce their expenditure if their income dropped by 10%. The vast majority indicated they would reduce their expenditure.

[6] If it controls its currency.

a hard budget constraint is taken for granted. Thus, the implication is that reasoning by analogy in this way should cause more orthodox preferences over government budget balance.

The argument that the household analogy is – or was – a powerful driver of views about the government budget can also be found in prior scholarly work. In the UK in particular, Stanley (2014) provides qualitative evidence of powerful feelings of personal and household overspending as the "mood of the times" underpinning support for government budget consolidation. Stanley argues that the household analogy works by bringing together three elements – debt as an ambiguous moral obligation, a personal experience of 'living beyond one's means', and perceptions of an analogous profligacy in public spending – to underpin support for deficit reduction, despite (or perhaps even because of) the costs that austerity may entail. For our purposes, Stanley's work highlights a number of examples of participants making the analogy between household and government budgets more or less explicitly. For example, "Maddie" notes, "whether you go *from individuals, to nations*, to the globe. Debt is a problem" (our emphasis Stanley 2014, 904).

A common-sense approach thus makes for a plausible explanation of the popularity of budget consolidation. First, the household analogy is widespread; second, if one holds it, one infers that governments should balance their books. As noted above, this argument has been widely made – at least implicitly. We can formalize it in the following way.

Hypothesis 1 (Analogy-Balanced-Budget Correlation)

Individuals who make more use of the analogy from household/personal finances to government finances will be more likely to be more opposed to government borrowing.

However, the expectation that analogising will lead to deficit aversion comes from an implicit assumption about the *content* of the analogy, not just its prevalence in society. Explaining a hard budget constraint for the government with reference to the analogy to the household requires that households are thought to face fairly hard budget constraints themselves. Without this, the expectation of opposition to government borrowing (as related to the analogy) is not possible to sustain. While a fairly hard household budget constraint may be a common view, it is not the only view that individuals could hold about household finance. By contrast, they may have positive attitudes towards their own borrowing. Indeed, economists emphasise the logic and the empirical prevalence of consumption smoothing by

households, entailing borrowing in low-income periods. People may also view borrowing for productive investments as a good thing for households to do (as in the case of student loans, for example). If this were the case, then analogical reasoning from household finance to government finance should translate into a willingness to entertain greater levels of public borrowing. We can leverage the possibility for divergence in views of *household* borrowing to generate an additional observable implications that would follow from the idea that the analogy is an important tool for reasoning about the government budget. As such, we investigate a second hypothesis:

Hypothesis 2 (Moderation of Analogy-Balanced-Budget Correlation)
There should be a stronger association between analogy endorsement and aversion to government borrowing among those who hold more negative views on household borrowing.

The language we have used in Hypothesis 1 is correlational; deliberately so as we wish to make use of it when studying observational survey data (in Section 4.1). However, as the theoretical logic of this section suggests, and Hypothesis 2 implies, the argument about the importance of the household finance analogy is really that it *causes* attitudes about government borrowing to be more hawkish. Thus, there is a more explicitly causal companion hypothesis that we can state (and seek to test in Section 4.2 and Section 4.3).

Hypothesis 3 (Analogy-Balanced-Budget Causation)
The use of the analogy from household/personal finances to government finances will cause individuals to be more opposed to government borrowing.

This causal claim is, we think, implicit in the arguments about the 'power' of the analogy – in particular in the counterfactual form that we would not have seen as much austerity in the UK without it. The precise nature of this causal claim is worth unpacking in a little more depth, however. We interpret it as follows: if two people were identical except in their use of this particular kind of analogical reasoning, the one reliant on the analogy would have more anti-deficit views than the other. The issue with our observational data, then, is that these two kinds of attitudes are not independent, and the kinds of people who endorse the analogy are likely to differ in many (unobservable) ways which also make them more likely to oppose government borrowing. Moreover, we see reliance on the analogy to draw conclusions about

government borrowing as more of a 'continuous' characteristic than a binary one: people draw on many kinds of tools and reasons to reach their policy preferences, and the analogy is one among them which can be given more or less weight. As such, our experimental designs seek to make the analogy more salient to (a randomly selected subset of) respondents.

It is these three hypotheses that we test in Section 4, using observational survey data as well as data from a pair of survey experiments. Before proceeding to those tests, we use the next section to describe our data sources and approach to measuring out core conceptual variables.

3 Measuring Conceptual Variables

In this section, we describe the survey questions that we use to obtain measures of the core conceptual variables relating to support for balanced budgets and the household analogy. The questions described were deployed across three separate surveys, described in more detail in various sections below, through the period from June 2016 to March 2019.

We adopt two different survey questions to measure attitudes towards fiscal balance. To compare our original survey results with data across a wider set of countries and longer time period, we use one measure drawn directly from a long-running series of Eurobarometer surveys. This question asks respondents for their (dis)agreement with the following statement: "Measures to reduce the public deficit and debt in the UK are not a priority for now", where the response options cover "totally" and "tend to" agree and disagree.[7] Responses to this question are captured in a variable we denote as $DefPriority_i$. However, an issue with this question is that it clearly suffers from acquiesence bias – and it is also possible to (mis)interpret it as a factual question (about current policy) rather than a preference question.[8]

Consequently, we also deploy our own question wording for two of our three fielded surveys. Our preferred measure of attitudes towards fiscal balance is generated from responses to the question: "How appropriate do you think it is for the UK Government to spend more than its income in any given year?". The response options are: "Never appropriate"; "Rarely

[7] There was no "Don't know" response in the survey we fielded.

[8] Although we think that is relatively unlikely.

appropriate"; "Sometimes appropriate"; "Often appropriate"; "Always appropriate"; "Don't know". While the "how appropriate" set-up in the question could be answered with evaluative terms rather than the 'frequency' terms we use, this would raise the concern that people will acquiesce to the idea that the proposal is appropriate. The frequency ("how often") response options provide less of a normative steer (though we are not interested in frequency in itself). Most respondents will intuit the point of the question and give accurate answers: fiscal hawks or doves generally remain as such however we ask the question.

Regarding the household finance analogy, there is an inherent challenge in measuring its 'use' in that it is obviously impossible to observe the cognitive use of the concept. Instead, our approach is to ask survey respondents to indicate whether they endorse the value of the analogy for thinking about government finances. This is one step removed from 'use' per se, and requires us to assume that those who endorse the analogy will be much more likely to use it. This seems to us like a reasonable assumption, but it is possible that respondents may subconsciously use the analogy without realizing, and so without thinking it appropriate to endorse it. We cannot rule this out as we obviously have no way to observe the cognitive processes – either conscious or subconscious – of respondents. There is little that we can do to guard against this concern, but it follows that our results with respect to observed correlations between analogy endorsement and balanced budget attitudes are only able to capture conscious reasoning. However, proponents of the importance of the household finance analogy for fiscal policy attitudes do not tend to make the argument that the analogy operates only at the subconscious level, and if both conscious and unconscious reasoning is going on, the straightforward expectation is that the two at least be positively correlated.

Our measure of endorsement of the household analogy invites respondents to locate themselves on a six point scale anchored at each end by (opposite) declarative statements about the appropriateness of a household budget logic to reason about government budgets. We ask respondents the following question:

> People have different views about how we should think about government budget balance, that is, what the government spends compared to its income from taxes. How useful do you find ideas about **household** budgets for understanding gov-

ernment budgets? Please place yourself on this 6-point scale.

- "The way we think about household budgets shows us how we should think about government budgets."
- ...
- "The way we think about household budgets is irrelevant for how we should think about government budgets."
- 'Don't know"

We do not allow for a middle/neutral response as we wish to avoid a satisficing response that is difficult for us to classify as corresponding to analogizing or not (Johns 2005; Krosnick 1991; Van Vaerenbergh and Thomas 2013).[9] Again, we do not ask respondents to agree or disagree with any statement of our construction, to minimize acquiescence bias.

We define responses to this question for individuals indexed by i as $Analogy_i$. Further, having dropped "Don't know" responses, we define $Analogy_i^N$ as the numeric version on a 1–6 scale where higher values correspond to responses closer to the analogy endorsement statement.

As a measure of endorsement of the household finance analogy is a particularly novel feature of this paper, it is worth quickly assessing it, descriptively. How widespread is analogizing between household and government budgets? Our question was fielded to a sample of 3,810 YouGov respondents in the UK in March 2019, 1,298 of whom answered the analogy endorsement question before any other questions (of ours) relating to fiscal policy or personal borrowing. Figure 1 shows the distribution of the resulting $Analogy_i$ variable for this subset of respondents.[10]

Figure 1 shows the distribution of responses to our analogy question. In contrast to plausible expectations of wide acceptance of the household budget analogy, we find that there is a reasonably uniform distribution of responses – and, indeed, the modal response is actually the strongest rejection of the analogy. On balance, our data indicate that the household is endorsed by a sizeable subset of the population, but that it is also rejected by

[9] Respondents who really do not know whether they weakly agree or weakly disagree can, of course, respond with "Don't know".

[10] Excluding "Don't know" responses. The full survey made use of multiple random splits which determined the question ordering that respondents received – and which we make use of in Section 5.2.

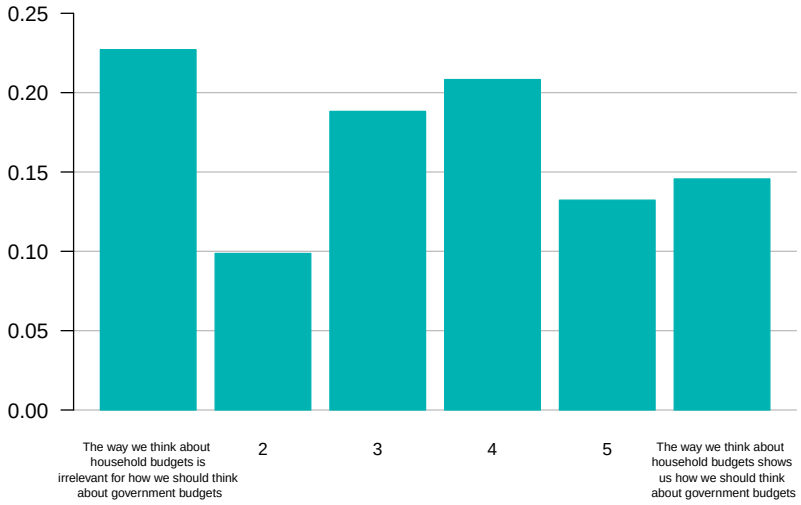


Figure 1: Distribution of responses to household analogy question.

about the same proportion.[11]

4 Does the Household Analogy Affect Balanced Budget Attitudes?

In this section, test the three hypotheses developed in Section 2. The evidence presented here is based on three customized surveys that we fielded in the UK with YouGov. We have pre-registered analysis plans (i.e. prior to data collection) for the evidence presented in 4.1 and 4.3.[12]

[11]Table A3 in the Supplementary Material shows the demographic and electoral correlates of endorsement of the household analogy. There is little association with demographic attributes, but more left-wing voters are generally less likely to endorse the analogy.

[12]The lack of such a pre-registered analysis plan for the survey in 4.2 was an oversight on our part, partly reflecting how strong our priors were that the household analogy effect would be present. Notably, we analyze the data in the standard/clean way that we approach the other experiments – i.e. evaluating the average treatment effects and avoiding searches for treatment heterogeneity. That is to say, we do not think we are open to the critique that we have in any way deviated from the analysis decisions that we, or any other reasonable analyst, would have taken prior to the data collection.

4.1 Observational Evidence

The analysis in this section is determined by the analysis plan that we pre-registered prior to data collection.[13] The data are drawn from a survey fielded with YouGov in the UK March 21–21 2019 as part of its omnibus daily polling service. We obtained a full sample for this survey of $N = 3,810$, but this sample was randomly split such that respondents saw the same questions, but in different orders.[14] For the analysis in this section, we use the full sample (subject to missing observations), but include (but do not report estimates for) dummy variables that indicate the question-order group that each respondent was assigned to.

Our first goal is to test Hypothesis 1. To do so, we use the $BalanceBudget_i^N$ and $Analogy_i^N$ variables from our survey data and estimate

$$BalanceBudget_i^N = \beta_0 + \beta_1 \cdot Analogy_i^N + \beta_X \cdot X + \beta_G \cdot Group_{G,i} + \epsilon_i \ , \tag{1}$$

where X is a set of control variables that are made available to us from the "profile" variables provided by YouGov, and $Group_{G,i}$ is a set of dummy variables indicating which experimental group each respondent is in. We present results from multiple specifications, where X moves from being an empty set, and then progressively including demographic covariates (e.g. age, household income, and education) and then political covariates (e.g. previous election vote choice, party identification, Brexit referendum vote choice, and newspaper readership).

The dependent variable is an ordinal variable with five levels. This suggests that estimation using an ordered logit model is appropriate, and our pre-analysis plan commits us to preferring results from models of this sort over straight OLS *if the inferences differ*. In fact, the inferences do not differ across the models,[15] and so we choose to present the results from OLS models as they are easier to interpret.

[13] Reference removed for anonymity.

[14] Full details of the design of this experiment are in the pre-analysis plan and Table A2 in the Supplementary Material.

[15] Compare Table A4 and Table A5 in the Supplementary Material.

Hypothesis 1 implies that the following should be true:

$$\beta_1 > 0 \ , \tag{2}$$

and this is the test that we conduct.

Figure 2 provides good support for the hypothesis, showing that the coefficient on $Analogy_i^N$ is positive, statistically significant, and rather stable across all specifications that we use. Compared to every other coefficient in the model, it is rather precisely estimated.

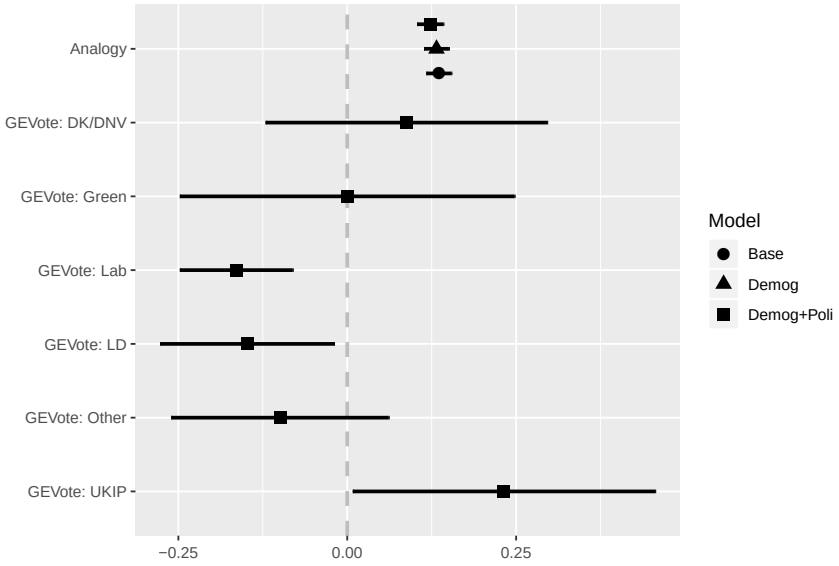


Figure 2: Coefficient plot for a set of models explaining $BalanceBudget_i^N$, including $Analogy_i^N$ as the core predictor of interest. 'Base' model includes only $Analogy_i^N$ and treatment group dummies. 'Demog' model adds controls for age, gender, social grade, housing tenure, and household income. 'Demog+Poli' model further adds controls for 2017 general election vote choice, Brexit referendum vote choice, and newspaper readership. Based on estimates from Table A4 in the Supplementary Material.

For concreteness, the smallest estimated coefficient (0.13) implies that those who most strongly reject the household analogy are on average 0.63 lower in their endorsement of budget balance than those at the top – equivalent to 0.7 of a standard deviation on $BalanceBudget_i$. This compares to a -0.2 difference between Conservative and Labour voters. In this sense, the substantive magnitude of the association between $BalanceBudget_i^N$ and $Analogy_i^N$ seems to be rather large.

On its face, this appears to be good evidence in favor of the theory that the household

Label	Question	Not at all important	Not very important	Important	Very Important	Don't Know
$HHBorrowLongRun$	It allows for purchases that are beneficial in the long run, for example in the form of business or student loans, or mortgages.	•	•	•	•	•
$HHBorrowHardTimes$	It allows people to get through temporary hard times.	•	•	•	•	•
$HHBorrowDifficulties$	It seems like a good idea in the short run, but leads to difficulties later on.	•	•	•	•	•
$HHBorrowUnsustainable$	It encourages spending at unsustainable levels.	•	•	•	•	•
$HHBorrowHardAvoid$	Whatever the underlying reasons or consequences, it is better to avoid borrowing if possible.	•	•	•	•	•

Table 1: Questions to capture attitudes about household borrowing.

finance analogy leads opposition to government borrowing. However, as we discussed above when deriving Hypothesis 2, such a claim implies that the relationship between $BalanceBudget_i^N$ and $Analogy_i^N$ should be moderated by the content of the analogy itself – i.e. by the view that individuals have about whether it is good or bad for households to borrow. Our next step, then, is to test Hypothesis 2 using data from a further set of questions that we fielded that enable us to measure individual attitudes about household borrowing.

Specifically, we asked our survey respondents to answer the following question:[16]

> In Britain today, people borrow money for many different reasons. For example, people may borrow to invest for the future, to increase their personal spending, or to make ends meet when times are hard. How important should each of the following considerations be when households are thinking about borrowing money?

Following our pre-analysis plan, we use the responses to these questions – converted to numerical form (indicated with the N superscript) such that "Very Important" evaluates to 4 and "Not at all important" evaluates to 1 – to construct two measures of attitudes to household borrowing. The first measure is simply $HHBorrowAvoid_i^N$, which respondents

[16]One may wonder if asking these questions about abstract "households", rather than the respondent's own household, yields a poor measure of views about household borrowin. In a pilot version of this study, we fielded a version that explicitly asked respondents about their own (positive or negative) experiences with borrowing, and obtained extremely similar inferences regarding Hypothesis 2 as we do with the question wording we report here.

provide directly in their response to the final question shown in Table 1. The second is defined as:

$$\begin{aligned} HHBorrowBad_i^N &= HHBorrowDifficulties_i^N + HHBorrowUnsustainable_i^N \\ &\quad -HHBorrowLongRun_i^N - HHBorrowHardTimes_i^N \ , \end{aligned} \tag{3}$$

i.e. formed as an index in which we add together the negative responses regarding household borrowing and subtract the positive responses. Both $HHBorrowAvoid_i^N$ and $HHBorrowBad_i^N$ are scaled such that higher values correspond with greater aversion to household borrowing. We then estimate models of the following form:

$$\begin{aligned} BalanceBudget_i^N &= \alpha_0 + \alpha_1 \cdot Analogy_i^N + \alpha_2 \cdot HHBorrowBad_i^N \\ &\quad +\alpha_3 \cdot Analogy_i^N \cdot HHBorrowBad_i^N \\ &\quad +\alpha_X \cdot X + \xi_i \ . \end{aligned} \tag{4}$$

We do likewise for the $HHBorrowAvoid_i^N$ moderator variable, as well. The test for Hypothesis 2 is:

$$\alpha_3 > 0 \ . \tag{5}$$

In the pre-analysis plan, we developed a further pair of related hypotheses that are even more stringent than Equation (5), but we do not dwell on these here because, as will soon become apparent, the less stringent hypothesis very clearly fails.[17]

The full results for each of the $HHBorrowAvoid_i^N$ and $HHBorrowBad_i^N$ models are presented in Table A6 in the Supplementary Material. Here, we focus on the core inferen-

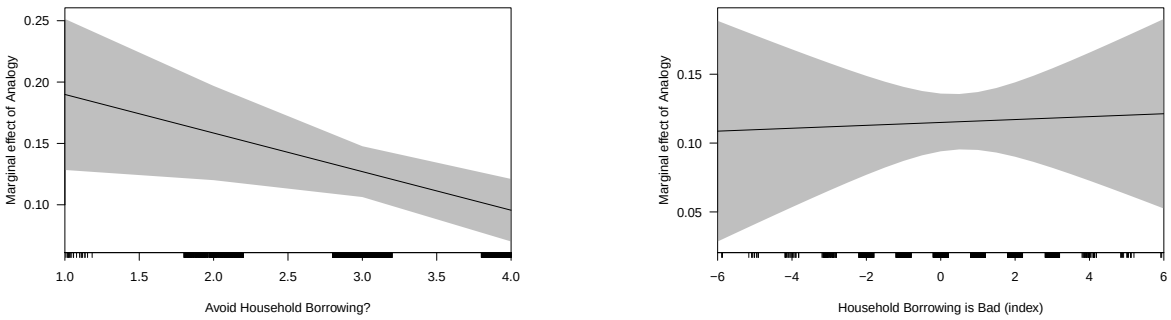
[17]The stronger claim is that for individuals that use the analogy, those with positive views about household/personal borrowing will be more supportive of government borrowing than those with negative views of household/personal borrowing. There should be no such difference between individuals that do not use the analogy. Formally, if we define notation such that $\overline{HHBorrowBad_i^N}$ indicates values of $HHBorrowBad_i^N$ that are high and $\underline{HHBorrowBad_i^N}$ are low. If there is a causal effect of the analogy on balanced budget attitudes, then, for those who endorse the analogy, we should observe:

$$\alpha_1 + \alpha_3 \cdot \overline{HHBorrowBad_i^N} > 0 \tag{6}$$

$$\alpha_1 + \alpha_3 \cdot \underline{HHBorrowBad_i^N} < 0 \tag{7}$$

tial aspects of the results. Regarding the explicit hypothesis tests, we obtain estimates of: $\alpha_1^{BorrowAvoid} = -0.031$ ($p = 0.014$) and $\alpha_1^{BorrowBad} = 0.001$ ($p = 0.863$). That is to say, the hypothesis finds no support, no matter which measure of household borrowing attitudes we use as a moderator for analogy endorsement.

In Figure 3, we visualize the results in this regard to make things even clearer. It shows that, if anything, the evidence is more consistent with the the effect of analogy endorsement on hostility to government borrowing being *weaker* when hostility to household borrowing is higher. This is exactly the opposite of what we would expect if the the household finance analogy was operating causally in the way that is typically suggested. As such, we conclude from examination of our observational data that the evidence does not support this typical view.



(a) Household borrowing is best avoided. (b) An index of 'Household borrowing is bad'.

Figure 3: Marginal effects of $Analogy_i^N$ on $BalanceBudget_i^N$, conditional on attitudes about household borrowing. Based on estimates from Table A6 in the Supplementary Material.

4.2 Experimental Evidence: Priming Personal Austerity

There are well-known deficiencies in using observational data to try to establish the (non-)existence of causal effects. Given that, one response to our findings in the previous section might simply be to dismiss them as lacking credibility. If one has strong prior beliefs in the existence of a strong effect from houshold analogizing to hawkish government borrowing attitudes, one may not update those priors very much in the face of observational evidence to the contrary. In this section, and the next, we develop empirical tests of the explicitly causal Hypothesis 3. We seek to establish whether it is possible to detect any such causal effect.

Our first experiment had a very simple design: we sought to prime a random subset of our respondents with the idea that faced with a drop in incomes, households would need to think about curbing expenditure in order to balance their budgets. Meanwhile, the control group is left unprimed. All respondents – treated or control – then answered the Eurobarometer survey question as the measure of their aversion to government borrowing.[18] The idea is that, to the extent that respondents use the household analogy, priming the idea of austerity (cutting spending in response to an income drop) at the household level should affect views on government borrowing given the presence of the analogy.In order to try to avoid the issue that arises if respondents analogise from a favorable view of personal borrowing, our priming treatment leans on the budget constraint aspect of personal finances in bad times. As such, we refer to this as the "personal austerity" prime.

The specific question wording is, "If your monthly income were to unexpectedly fall by 10%, how do you think this would affect your monthly expenditure, if at all?", with response options of: "Reduce monthly spending by 10% or more"; "Reduce monthly spending by less than 10%"; "No change"; "Increase monthly spending by less than 10%"; "Increase monthly spending by 10% or more"; "Don't know".[19] Table 2 sets out the full design of the experiment. It was fielded to 3400 UK respondents July 2016 by YouGov.

Group	Step 1	Step 2	Probability	$\approx N$
A	–	*DefPriority*	50%	1,700
B	*PersonalAusterity*	*DefPriority*	50%	1,700

Table 2: Design for the priming personal austerity experiment.

The results of this experiment are summarized in the coefficient plot presented in Figure 4.[20] It shows the estimated treatment effect of having been assigned to the *PersonalAusterity* question. The estimates are based on OLS applied to an interval-scale version of the $DefPriority_i$ question, ranging 1–4.[21] The base specification simply has a dummy for the treatment (as well as a constant), while the second specification includes (pre-treatment)

[18] As detailed in Section 3.

[19] To reiterate, the question wording for the dependent variable asks for (dis)agreement with the following statement: "Measures to reduce the public deficit and debt in the UK are not a priority for now".

[20] The full results are presented in Table A9 in the Supplementary Material, as is evidence that the randomization procedure yielded good balance across observables in Table A7 and Table A8.

[21] The inferences are unchanged if we switch to an ordered logit model.

controls for gross household income, party-id, and past general election vote.[22] We find *no* evidence to support the existence of a treatment effect that is distinguishable from zero. Moreover, this does not seem to be because we have an unduly noisy estimate of the treatment effect: as shown, the (95%) confidence intervals are rather narrow. The figure is plotted on a scale of −1–1 in order to correspond with unit changes in $DefPriority_i$. We can also compare the estimated treatment effects and associated confidence intervals to the variation in the dependent variable. From the data, $StdDev(DefPriority_i) = 0.83$, and so on this metric, too, we feel comfortable inferring that the estimated treatment effects really are, essentially, zero.

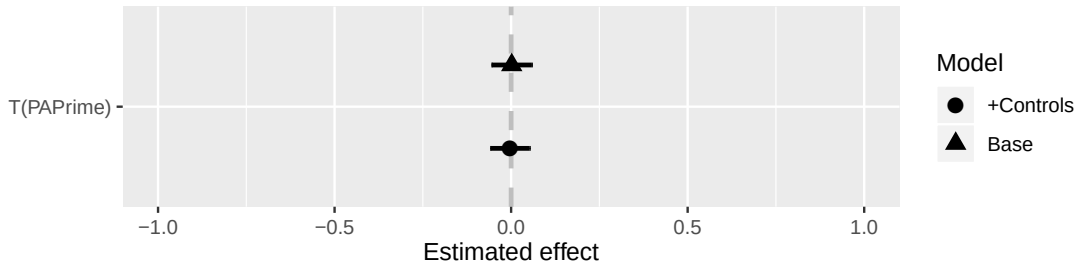


Figure 4: Estimated treatment effect of the "personal austerity" prime ($T(PersonalAusterity)$) on balanced budget attitudes, measured using $DefPriority_i$.

One explanation for these null results could be that the personal austerity prime is not working to make people focus on a binding household budget, for example if it reminded people just how little they would in fact adjust their spending if their incomes fell. We can assess the extent to which this is true by examining the responses that we obtained to our personal austerity priming question. Figure 5 plots the (weighted) distribution of these respondents and shows that respondents do lean strongly towards reductions in their own spending when faced with a hypothetical reduction in their incomes. We take this as evidence that the prime does raise a real awareness of household budget constraints.

[22] All controls are entered using dummies for all-but-one of the factor levels, allowing full flexibility on functional form.

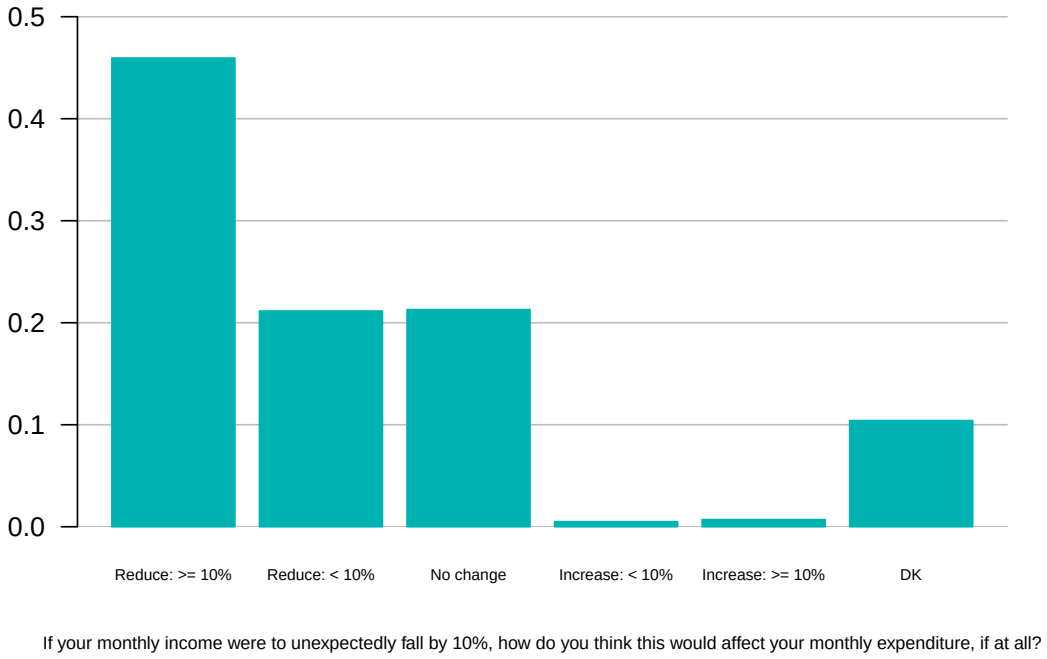


Figure 5: Distribution of responses to the personal austerity priming question.

4.3 Experimental Evidence: Analogical Reason

One possible objection to the previous experiment is that it is only indirect in its approach to studying the household finance analogy. On the one hand, this does mean that this experiment does not rely on the explicit and conscious endorsement of the analogy for its inferential power. On the other, without explicit invocation of analogical logic from household finances to government finances, respondents may simply not join the dots between the two, and the idea that voters *spontaneously* use the analogy to reason about the government budget is a stronger one than advocates of the idea need hold. That is, if policy-makers, commentators and the media *provide* the analogy, and voters then use it, the spontaneous test above would fail to capture the mechanism that is really of interest. If that is the case, a more appropriate research design is to randomly assign survey respondents to receive different arguments/justifications for a particular position with respect to government borrowing, where the arguments differ in the extent to which they invoke the household finance analogy. We describe the results from fielding just such a design in this section.

Specifically, in our next experiment, we sought to more directly manipulate respondents' attitudes about balanced budgets by randomly providing them with one of a set of three reasons why such a policy is desirable. One of the reasons explicitly makes use of analogical reasoning, repeatedly drawing a comparison with household finance. Another makes only implicit reference to the household analogy, by using terms that have a clear parallel with household/personal finance. The third has no direct relation with analogical reasoning. Our interest is then in comparing support for balanced budgets across these three treatment conditions against a control group that saw no reasons to favour the policy. The question is: are household-analogical arguments capable of shifting respondents' views on balanced budgets?

Our dependent variable in this experiment is based on the $BalanceBudget_i^N$ question that we used in Section 4.1, asking respondents to rate how appropriate it is for the government to spend more than its income in a year.

The control group only sees the question text. Immediately preceding this text (presented in the same paragraph), the treatment groups see one of the following:

1. *Analogical Reason*

   "A common way of thinking about government finances is that governments should not spend more than they earn in any given year because borrowing leads to major financial difficulties. Just like households, governments have creditors who worry about repayment. Just like households, governments have a credit score that may be downgraded. Just like households, governments borrowing too much can see their debts spiral out of control."

2. *Less Analogical Reason*

   "A common way of thinking about government finances is that governments should not spend more than they earn in any given year because borrowing leads to major financial difficulties. Governments have creditors who worry about repayment. Governments have a credit rating that may be downgraded. Governments borrowing too much can see their debts spiral out of control."

3. *Reason*

   "A common way of thinking about government finances is that governments should not

spend more than they earn in any given year because borrowing leads to major financial difficulties."

Finally, immediately after answer the *Balance Budget* question, respondents answer the *Analogy* endorsement question. Note that our goal was not to directly manipulate explicit endorsement of the household analogy per se. Indeed, part of the logic for designing the experiment in the way that we did – i.e. with reasons to support balanced budgets having the analogy embedded directly within them – was that it is possible that people may implicitly use the analogy, without realizing, such that their explicit endorsement of the analogy (or not) may not be dispositive of the connection between cognitive household analogizing and attitudes regarding balanced budgets. Nonetheless, we fielded the *Analogy* instrument to provide additional evidence that can speak to the question of whether our treatments may operate through the household analogy.

Table 3 summarizes the design for this experiment. Once again, we have a relatively large sample, in this case yielding ≈ 567 in each treatment condition. Power calculations reported in the pre-analysis plan indicate that we are adequately powered to detect treatment effects that are even "small" in Cohen's-d terms (**Anonymized**). The survey was fielded by YouGov in the middle of August 2018, immediately after registration of the pre-analysis plan.

Group	Step 1	Step 2	Step 3	Probability	$\approx N$ Respondents
A	–	*Balance Budget*	*Analogy*	25%	567
B	*Reason*	*Balance Budget*	*Analogy*	25%	567
C	*Analogical Reason*	*Balance Budget*	*Analogy*	25%	567
D	*Less Analogical Reason*	*Balance Budget*	*Analogy*	25%	567

Table 3: Design for the Analogical Reason experiment.

The main results of the experiment are presented in Figure 6, which shows estimates (based on OLS) of the the three "reason" treatment effects.[23] In short, we find that none of our treatments has a discernible effect on attitudes towards balanced budgets.[24] The confidence intervals around our point estimates are somewhat larger than those for the "Priming Personal

[23]Following the pre-analysis plan, we do not provide covariate adjusted estimates (**Anonymized**). The full results are presented in Table A12 in the supplementary material, as is evidence that the randomization procedure yielded good balance across observables in Table A10 and Table A11.

[24]Table A12 in the Supplementary Material shows that there were also no discernible treatment effects on $Analogy_i$. We report this here as it constitutes part of our pre-analysis plan, but we note that, as per the pre-analysis plan (p.6), we do not consider a zero treatment effect on $Analogy_i$ to be inferentially consequential.

Austerity" experiment, although they are hardly wide on the (plotted) scale of unit changes in $BalanceBudget_i$ or compared to the standard deviation in that variable (0.91). Moreover, there is very little in the way of evidence of a *positive* effect from any of the treatments; if anything, the opposite. Most notably for our purposes, providing a reason to support balanced budgets that explicitly makes use of the household analogy appears to have no effect on expressed views about balanced budgets. This constitutes our main finding from the experiment.

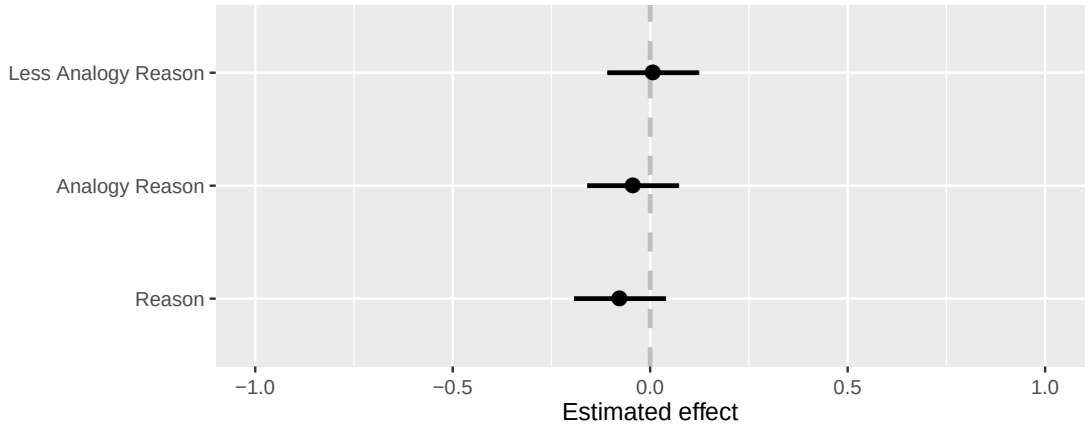


Figure 6: Estimated effects of the various "reason" treatments on balanced budget attitudes, measured using $BalanceBudget_i$.

One potential objection to our conclusion is that this experiment has shown no ability to manipulate balanced budget attitudes at all. Could it be that these attitudes are so firmly held by respondents that they are resistant to any experimental manipulation? If so, our null findings hardly speak to the question of whether household analogical reasoning does – or perhaps did – influence balanced budget attitudes. We feel reasonably comfortable claiming a substantively genuine null finding in this case because other similarly structured experimental treatments have had effects on balanced budget attitudes (Barnes and Hicks 2018).[25] Thus, we infer that the explanation for null findings for this particular experiment is that the reasons provided to respondents – including very explicit household-analogical reasons – are not convincing to respondents, as opposed to the attitudes being too sticky to move.

[25]One potential qualifier to this view is that those other experiments have only managed to move respondents in the direction of less concern about borrowing, not more concern.

5 Explaining the Observational Correlation Between Analogy and Budget Balance Attitudes

The evidence from the previous section all weighs against the idea that the use or prevalence of the household finance analogy causes mass attitudes regarding government borrowing to be more hawkish. However, Section 4.1 showed evidence that analogy endorsement and debt hawkishness are positively *correlated*, and in a way that is very robust to the inclusion of various covariates. This poses a puzzle. If there is no causal relationship between the two, why would there be a correlation? Answering this question goes somewhat beyond the intended scope of this paper. However, we can offer an argument and evidence that goes some way to supporting it.

Our argument is that, consistent with the very strong prior beliefs about the importance of the household finance analogy amongst scholars and journalists, political elites *believe* the analogy to be consequential and so deliver their messages to voters in such a way as to package up the analogy *together* with indications of the preferences that voters should hold about government borrowing. That is to say, voters acquire the two ideas – or "considerations" (Zaller 1992, Chapter 3) – at the same time, and are therefore able to associate them together, even though the analogical argument is *not persuading* voters about how much government should borrow. In this final substantive section, we develop two approaches to probe whether this argument is empirically accurate.

5.1 The Politically Attentive Drive the Analogy–Hawk Correlation

One implication of the argument that voters acquire, from elite messages, both the analogy and hawkish attitudes is that the correlation between analogy endorsement and hawkish attitudes should be much stronger for those who are paying attention to elite messages. Those who are not receiving or accepting the elite messages may well provide responses to our survey questions about government borrowing and analogy endorsement, but they will have no particular reason to think of them as related; rather they would be more of a random draw of possible responses from the set of probably quite disconnected considerations that

such respondents hold and have accessible (Zaller 1992).

On the other hand, for those who pay lots of attention to political/media elites, there will be a much stronger association between the analogy and borrowing attitudes because they are (ex hypothesi) commonly part of the same pair of considerations. In sum, we should expect the positive association between analogy endorsement and balanced budget attitudes to be disproportionately driven by those who are more likely to receive (and accept) political messages from elites. To be clear: we do not expect differential reactions to elite messages from high attention respondents (compared to low), but rather we expect that high attention respondents are more likely to have been exposed to the relevant messages. This echoes the finding that it is the most politically engaged citizens who tend to draw on partisan heuristics, as it is (only) these citizens who have any knowledge of the relevant positions (Dancey and Sheagley 2013).

The data from our March 2019 survey allow us to assess whether the evidence is consistent with this view.[26] One of the 'profile' variables collected by YouGov on their panel members provides an indication of their level of political engagement. Specifically, respondents are asked, "How much attention do you generally pay to politics?", and given a 0–10 scale on which to respond. Let us define this variable as $PoliticalAttention_i^N$.

An obvious test of our argument that the politically-attentive drive the analogy–hawkish association is to re-estimate the models underlying Figure 2, while allowing for $PoliticalAttention_i^N$ to moderate the relationship via an interaction effect. The full results from estimating just such a set of models are presented in Table A13 in the Supplementary Material. Here, we focus on the core aspect of the model relating to the interaction effect. Figure 7 plots the marginal effect of $Analogy_i^N$ on $BalanceBudget_i^N$, conditional on $PoliticalAttention_i^N$. It shows a very clear positive and statistically significant upward slow to the marginal effect, such that those who report paying little or no attention to politics are found to have no correlation between $Analogy_i^N$ and $BalanceBudget_i^N$, whilst those who are most attentive have a rather high correlation.[27]

[26] N.b. the analysis in this section is not subject to a pre-registered analysis plan.

[27] This is not just an artifact of the assumption of linear moderation (Hainmueller, Mummolo, and Xu 2019), as can be seen in Figure A3 in the Supplementary Material, where we allow the moderation to operate by splitting $PoliticalAttention_i^N$ into low, middle, and high categories with independent moderating effects.

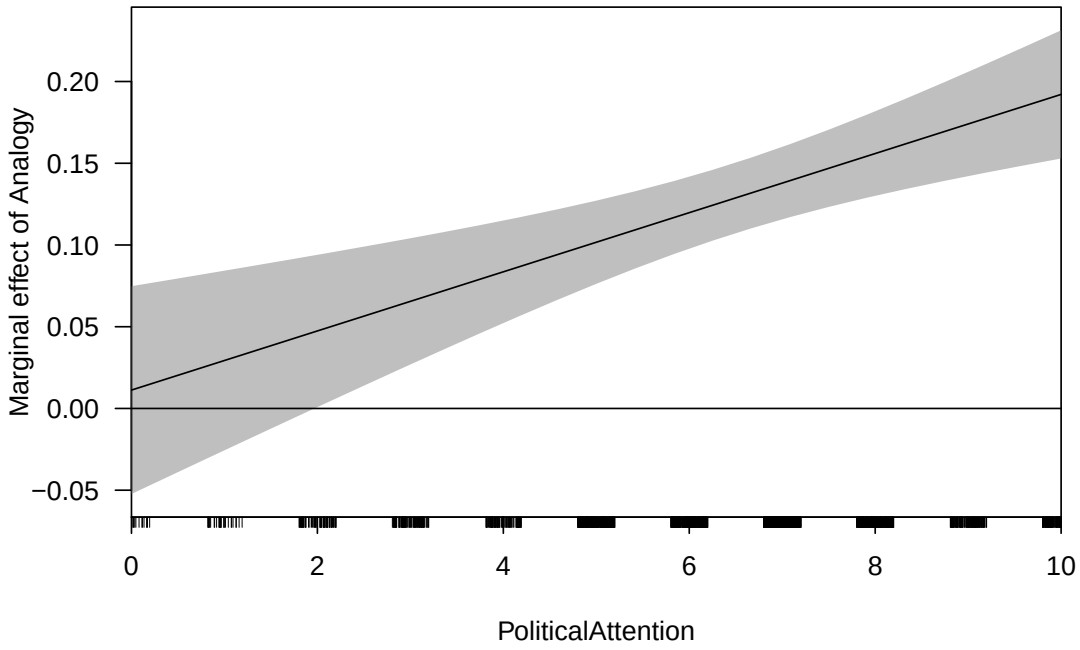


Figure 7: Estimated marginal effect of $Analogy_i$ on $BalanceBudget_it$, conditional on self-reported political attention ($PolitialAttention_i$). Figure based on estimates in Table A13 in the Supplementary Material.

5.2 Balanced Budget Attitudes Cause Analogy Endorsement

One way of reconciling the observational correlation between endorsement of the analogy and balanced budget attitudes and the absence of evidence for the causation from analogizing to deficit attitudes that is also consistent with an elite-driven view is that the causal content of the correlation in fact goes the other way. That is, voters develop views on the importance of deficit reduction (at least in part from elite cues), and then use the analogy as a justification of their (anti-deficit) positions. Under this view, voters are more meaningfully committed to a view about budget balance than they are about the household analogy – not least because the former has a substantive importance of its own.

This points to another way of studying the relationship between analogy endorsement and hawkish attitudes. We can assess whether (randomly) adjusting the order in which survey respondents answer our analogy endorsement and balanced budget questions affects the expressed responses to each question, or the relationship between them. Respondents who answer the balanced budget question before answering the analogy question are being given an opportunity to explain or justify their first response. Since respondents tend to express a fairly hostile view to government borrowing,[28] on average answering the balanced budget question first would lead to a higher rate of endorsement of the analogy. If it is the case that balanced budget attitudes come first, analogy endorsement is a rationalisation of balanced budget attitudes, and analogy endorsement itself does not *cause* balanced budget attitudes, then we should see no such question ordering effect on responses to the balanced budget question.

In this section, we discuss an aspect of our March 2019 survey that allows us to assess the propositions set out in the preceding text.[29] We exploit the randomization of respondents into treatment groups that answered the balanced budget question either before or after the analogy endorsement question. This sets up an easy test of the empirical implications that we just set out. Table 4 summarizes design of this experiment.

Figure 8 shows the estimated effects of the question ordering on the $BalanceBudget_i$ and

[28] As seen in Figure A1 in the Supplementary Material.

[29] The analysis of the unmoderated treatment effects in this section is determined by the analysis plan that we pre-registered prior to data collection.The remaining analyses are not subject to a pre-registered analysis plan.

Group	Step 1	Step 2	Probability	$\approx N$ Respondents
A	*Analogy*	*Balance Budget*	50%	1,000
B	*Balance Budget*	*Analogy*	50%	1,000
				2,000

Table 4: Design for the Balanced Budget Attitudes Cause Anology Endorsement experiment.

$Analogy_i$ dependent variables, respectively.[30] Consistent with our argument, we find that balanced budget attitudes are not influenced by whether respondents have been asked the extent to which they endorse the household analogy first. However, we do find that analogy endorsement is influenced by whether respondents have previously indicated their balanced budget preference.

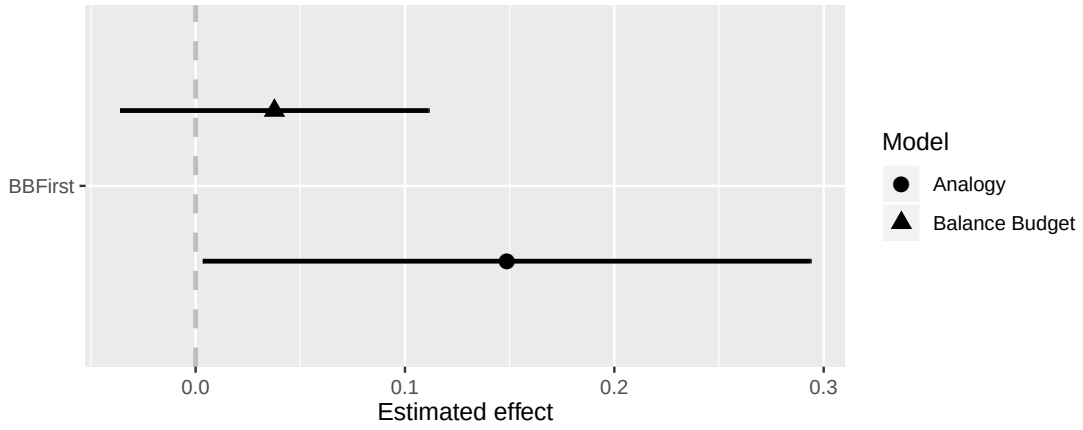


Figure 8: Estimated effect of answering the *Balance Budget* question first on balanced budget attitudes ($BalanceBudget_i$) and household analogy endorsement ($Analogy_i$).

How consequential is this reverse causation effect? Table 5 shows the results of models that use $Analogy_i$ to predict $BalanceBudget_i$, but where we allow the relationship between the two to be moderated by the balance-budget-first treatment indicator ($BBFirst_i$).[31] We present three specifications: a parsimonious one with just these variables; one that includes some demographic controls, and; one that further includes political attitudes/behaviours as controls. The table shows that, irrespective of the use of control variables, or not, the observed (partial) correlation between $BalanceBudget_i$ and $Analogy_i$ is much lower when

[30]The full results are presented in Table A17 in the Supplementary Material, as is evidence that the randomization procedure yielded good balance across observables in Table A15 and Table A16.

[31]We estimate models with balanced budget attitudes as the dependent variable so that the results are directly comparable with those in Section 4.1, rather than to imply a reversal of the reverse causation.

the balanced budget question is asked after to the *Analogy* question. To focus on model 2, the coefficient for $Analogy_i$ when its underlying question is asked first is around 48% of the size of the coefficient when it is asked after the balanced budget question.

Table 5: Estimated effect of answering the balanced budget question first on balanced budget attitudes ($BalanceBudget_{i,t}$).

	BalanceBudget		
	Model 1	**Model 2**	**Model 3**
Analogy	0.085***	0.085***	0.082***
	(0.016)	(0.016)	(0.016)
$BBFirst$	−0.317***	−0.285***	−0.249***
	(0.085)	(0.085)	(0.087)
$Analogy \times BBFirst$	0.101***	0.093***	0.087***
	(0.022)	(0.022)	(0.023)
Constant	3.114***	3.308***	3.309***
	(0.059)	(0.090)	(0.113)
Age	No	Yes	Yes
Household Income	No	Yes	Yes
Gender	No	Yes	Yes
Vote Choice (2017)	No	No	Yes
Brexit Vote Choice	No	No	Yes
Party-id	No	No	No
N	2081	2081	1795
R-squared	0.075	0.097	0.146

***p < .01; **p < .05; *p < .1

We can push the analysis of the relationship between analogy endorsement and balanced budget attitudes further, however. Reverting to $Analogy_i$ as the dependent variable – in keeping with our claim about the direction of causation in this section – there are at least two reasons to think that the $BBFirst_i$ treatment effect on $Analogy_i$ will not be equal across respondents who hold different preferences with respect to government borrowing.

First, we have suggested that voters are likely to acquire their balanced budget attitudes and analogy endorsement considerations from the same elite messages.[32] Given the strong perception that scholars, policy-makers, and journalists hold that the household finance analogy causes more hawkish borrowing attitudes, it follows that respondents who hold more hawkish attitudes are more likely to have received elite messages that deploy the household analogy – because those messages that sought to make respondents less hawkish are unlikely to have deployed the analogy. As such, more hawkish respondents should be

[32]From Barnes and Hicks (2018), we know that elite messages are important determinants of balanced budget attitudes, at least.

more likely to associate the borrowing and analogy considerations in their heads, and so be more likely to endorse the analogy after having been primed by the balanced budget question.

Second, respondents may find it cognitively appealing to give an analogy endorsement response that they believe provides a justification for their balanced budget preference. From Section 4, we know that this justification is not causal on balanced budget attitudes (on average), but respondents may nonetheless value a post hoc rationalization. Even if all respondents were equally exposed to the household analogy before they entered our survey, to the extent that they believe that the analogy justifies a more hawkish borrowing position, those with less hawkish borrowing views would be less inclined to endorse the analogy as it would appear to contradict their balanced budget attitude. On the other hand, those who are hawkish may find it to be a convenient post hoc rationalization for their borrowing position.

If either (or both) of these arguments are correct, we should expect to see that the $BBFirst_i$ treatment effect on $Analogy_i$ should be stronger for those who hold more hawkish borrowing attitudes. To be clear, while we can estimate whether this is the case, we cannot distinguish between the two mechanisms. Moreover, the implied moderation effect is only observationally identified, in the sense that we do not experimentally manipulate balanced budget attitudes. Nonetheless, we believe analysis of this sort is of value.

To probe these arguments, we estimate models in which $Analogy_i$ is the dependent variable and $BalanceBudget_i$, $BBFirst_i$, and their interaction terms are the main explanatory covariates. To make things fully flexible and avoid unwarranted assumptions of linearity in the moderation effect (Hainmueller, Mummolo, and Xu 2019), we estimate the interaction effect factorially. Figure 9 shows the most pertinent results for our purposes.[33] Consistent with our arguments, it shows that the treatment effects are indeed minimal (and not distinguishable from zero) for deficit doves and largest for deficit hawks.

6 Conclusions

We noted at the start of this paper that a positive interpretation of the evidence here is that we are not in such a political economy quandary with regard to mass attitudes about

[33]It is based on a model with additional covariates for household income, gender, and age. Full results presented in Table A18 in the Supplementary Material.

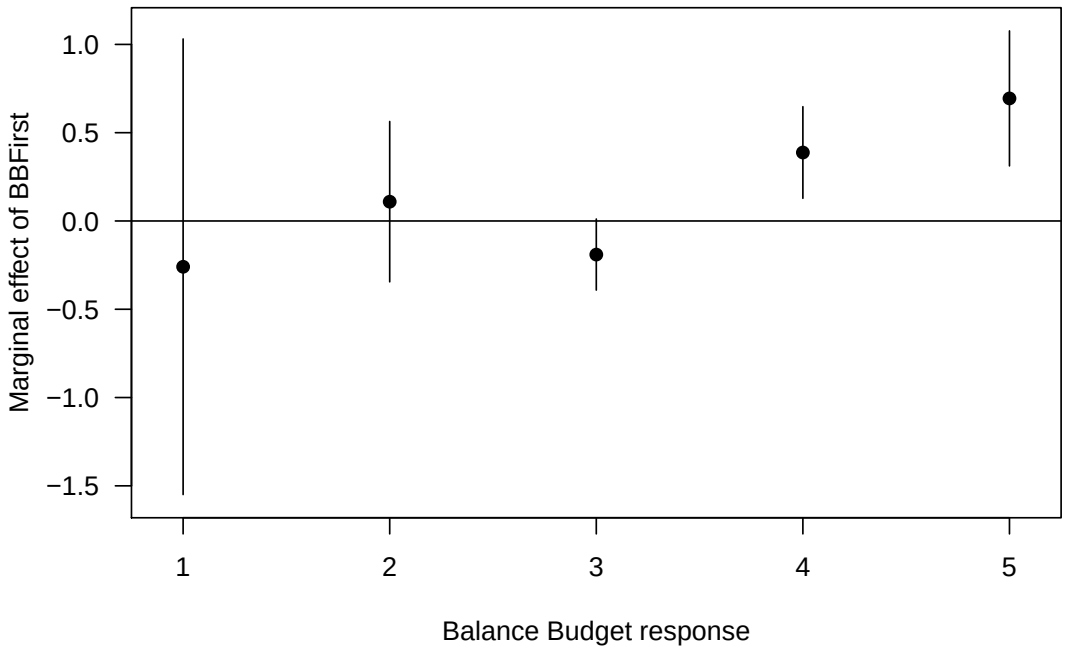


Figure 9: Estimated effect of answering the *Balance Budget* question first on analogy endorsement ($Analogy_i$), conditional on factor-levels of balanced budget attitudes ($BalanceBudget_i$).

government borrowing – in the sense that the household analogy does *not* seem to drive mass hawkishness. Certainly, this paper should provide some reassurance to scholars and practitioners in the area the political economy of macroeconomic policy. However, the good news in this regard yields its own new set of inter-related puzzles. Despite finding no causal link from the analogy to opposition to government borrowing, we nonetheless continue to see evidence that voters are, on average, rather hawkish. Thus, while the explanation may not be the analogy, it does not follow that the problem of a mismatch between mass attitudes and good economic policy has been erased; far from it.

Our findings, do, however, point the way towards an important avenue for explaining this mismatch. Consistent with other work in this area (Barnes and Hicks 2018; Bisgaard and Slothuus 2018), the evidence presented here is consistent with an extremely important role for political and media elites in the determination of mass attitudes regarding government borrowing. In one sense, this could be a back door way in which the household analogy could be seen to be causal on mass attitudes. To the extent that it is causal on elite attitudes, and

that those elites then cause mass attitudes, the analogy *could* be consequential. Quite why the analogy would only fool elites and not masses is not clear to us, but we leave it as an open possibility.

More broadly, to the extent that elites are responsible for mass attitudes, the 'fixes' in regard to the political economy of government debt are most plausibly located there. Whether such fixes really are possible amidst current constellations of partisan orientations and incentives across many countries is, however, more open to doubt. Thus, while we believe it is imperative to understand mass attitudes regarding (especially salient) policies, the evidence here suggests that understanding *elite* politics and preference formation is likely to be particularly important. For that reason, the continuing development of elite-oriented studies in this regard is very welcome (e.g. Bremer and McDaniel 2019; Dellepiane-Avellaneda 2015; Farrell and Quiggin 2017; Flickenschild and Afonso 2019; Helgadóttir 2016; Wren-Lewis 2015).

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Supplementary Material for: "The Household Finance Analogy Does Not Drive Mass Support for Austerity"

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Supplementary Material for:
"The Household Finance Analogy Does Not Drive Mass Support for Austerity"

A Survey Designs

Variable	Question	Response Options
$PersonalAusterity_{i,t}$	"If your monthly income were to unexpectedly fall by 10%, how do you think this would affect your monthly expenditure, if at all?"	"Reduce monthly spending by 10% or more"; "Reduce monthly spending by less than 10%"; "No change"; "Increase monthly spending by less than 10%"; "Increase monthly spending by 10% or more"; "Don't know"
$DefPriority_{i,t}$	"Measures to reduce the public deficit and debt in the UK are not a priority for now."	"Totally agree"; "Tend to agree"; "Tend to disagree"; "Totally disagree"
$ReduceDef_{i,t}$	"How necessary do you think it is for the UK Government to eliminate the deficit over the next 3 years — that is close the gap between what the government spends and what it raises in taxes?"	"It is completely necessary"; "It is important but not absolutely necessary"; "It is not necessary but it would be desirable"; "It is completely unnecessary"; "Don't know"

Table A1: Question wording for the variables that we use from the British Election Study panel data set.

Group	Step 1	Step 2	Step 3	Probability	Target N
A	*Balance Budget*	*Analogy (Clean)*	*Household Borrowing View*	2/6	1000
B	*Analogy (Clean)*	*Balance Budget*	*Household Borrowing View*	2/6	1000
C	*Household Borrowing View*	*Balance Budget*	*Analogy (Clean)*	1/6	500
D	*Household Borrowing View*	*Analogy (Clean)*	*Balance Budget*	1/6	500

Table A2: The design of the survey fielded by YouGov in March 2019. The data from this survey are used in Section 4.1 and Section 5.2.

B Extra Results for: "Observational Evidence"

Table A3: Correlates of endorsing the household finance analogy. All models are estimated by OLS and include controls for dummy variables for experimental split (not shown). Omitted categories are: party vote: Conservative.

	Analogy	
	Model 1	**Model 2**
AgeStd	0.044	−0.030
	(0.036)	(0.041)
Gender: Male	−0.160***	−0.194***
	(0.061)	(0.065)
SocialGrade: B	0.010	0.088
	(0.107)	(0.111)
SocialGrade: C1	0.066	0.111
	(0.100)	(0.104)
SocialGrade: C2	−0.025	0.028
	(0.111)	(0.117)
SocialGrade: D	0.093	0.159
	(0.129)	(0.139)
SocialGrade: E	0.025	0.037
	(0.130)	(0.139)
Housing: Other	−0.142	−0.254*
	(0.124)	(0.142)
Housing: Renter	−0.192**	−0.209**
	(0.078)	(0.085)
HHInc: >£100k	−0.310*	−0.284
	(0.173)	(0.181)
HHInc: £25k-50k	−0.016	−0.016
	(0.082)	(0.088)
HHInc: £50k-100k	−0.121	−0.083
	(0.104)	(0.111)
HHInc: DK/NA	−0.121	−0.114
	(0.089)	(0.096)
GEVote: DK/DNV		0.204
		(0.202)
GEVote: Green		−0.765***
		(0.245)
GEVote: Lab		−0.545***
		(0.079)
GEVote: LD		−0.210*
		(0.127)
GEVote: Other		−0.592***
		(0.156)
GEVote: UKIP		−0.748***
		(0.225)
EUVote: DK/DNV		−0.041
		(0.162)
EUVote: Leave		0.235***
		(0.074)
Constant	3.655***	3.758***
	(0.119)	(0.140)
N	3184	2732
R-squared	0.010	0.050

***p < .01; **p < .05; *p < .1

Table A4: Correlates of balanced budget attitudes. All models are estimated by OLS. Omitted categories are: party vote: Conservative; newspaper: None/Other.

	BalanceBudget		
	Model 1	**Model 2**	**Model 3**
Analogy	0.136^{***}	0.132^{***}	0.123^{***}
	(0.009)	(0.009)	(0.010)
TreatmentGroup: B	-0.029	-0.028	-0.040
	(0.038)	(0.038)	(0.039)
TreatmentGroup: C	-0.071	-0.066	-0.043
	(0.047)	(0.047)	(0.048)
TreatmentGroup: D	-0.050	-0.040	-0.019
	(0.047)	(0.047)	(0.048)
AgeStd		-0.008	-0.042^{**}
		(0.019)	(0.021)
Gender: Male		-0.153^{***}	-0.171^{***}
		(0.031)	(0.033)
SocialGrade: B		0.131^{**}	0.081
		(0.055)	(0.056)
SocialGrade: C1		0.121^{**}	0.055
		(0.051)	(0.053)
SocialGrade: C2		0.282^{***}	0.183^{***}
		(0.057)	(0.059)
SocialGrade: D		0.341^{***}	0.200^{***}
		(0.066)	(0.071)
SocialGrade: E		0.227^{***}	0.223^{***}
		(0.067)	(0.070)
Housing: Other		-0.044	-0.071
		(0.065)	(0.072)
Housing: Renter		0.001	-0.015
		(0.040)	(0.043)
HHInc: >£100k		-0.206^{**}	-0.143
		(0.088)	(0.091)
HHInc: £25k-50k		-0.021	-0.028
		(0.042)	(0.044)
HHInc: £50k-100k		-0.062	-0.048
		(0.053)	(0.056)
HHInc: DK/NA		0.031	0.010
		(0.046)	(0.048)
GEVote: DK/DNV			0.088
			(0.104)
GEVote: Green			-0.0001
			(0.124)
GEVote: Lab			-0.164^{***}
			(0.042)
GEVote: LD			-0.148^{**}
			(0.064)
GEVote: Other			-0.100
			(0.080)
GEVote: UKIP			0.232^{**}
			(0.112)
EUVote: DK/DNV			0.114
			(0.082)
EUVote: Leave			0.194^{***}
			(0.038)
‘Reads: FT‘			-0.054
			(0.094)
‘Reads: Guardian‘			-0.212^{***}
			(0.052)
‘Reads: Telegraph‘			0.083
			(0.054)
‘Reads: Times‘			-0.086^{*}
			(0.049)
‘Reads: Sun‘			-0.107
			(0.081)
‘Reads: Mail‘			0.036
			(0.042)
‘Reads: Mirror‘			-0.020
			(0.069)
‘Reads: Express‘			0.070
			(0.063)
‘Reads: Star‘			-0.018
			(0.041)
‘Reads: Record‘			-0.030
			(0.112)
Constant	2.974^{***}	2.909^{***}	3.009^{***}
	(0.042)	(0.070)	(0.081)
N	3084	3084	2660
R-squared	0.066	0.093	0.154

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table A5: Correlates of balanced budget attitudes. All models are estimated as ordered logistic regressions. Omitted categories are: party vote: Conservative; newspaper: None/Other.

	BalanceBudget_f		
	Model 1	**Model 2**	**Model 3**
Analogy	0.315***	0.312***	0.306***
	(0.021)	(0.021)	(0.024)
TreatmentGroup: B	−0.107	−0.099	−0.129
	(0.082)	(0.082)	(0.090)
TreatmentGroup: C	−0.166	−0.161	−0.106
	(0.102)	(0.102)	(0.111)
TreatmentGroup: D	−0.156	−0.140	−0.100
	(0.102)	(0.102)	(0.112)
AgeStd		−0.033	−0.111**
		(0.041)	(0.049)
Gender: Male		−0.367***	−0.425***
		(0.069)	(0.076)
SocialGrade: B		0.301**	0.208
		(0.120)	(0.129)
SocialGrade: C1		0.288***	0.152
		(0.112)	(0.122)
SocialGrade: C2		0.627***	0.422***
		(0.125)	(0.138)
SocialGrade: D		0.762***	0.485***
		(0.146)	(0.164)
SocialGrade: E		0.540***	0.545***
		(0.148)	(0.163)
Housing: Other		−0.088	−0.131
		(0.145)	(0.170)
Housing: Renter		0.018	−0.002
		(0.089)	(0.098)
HHInc: >£100k		−0.449**	−0.300
		(0.195)	(0.211)
HHInc: £25k-50k		−0.044	−0.040
		(0.093)	(0.103)
HHInc: £50k-100k		−0.133	−0.078
		(0.117)	(0.129)
HHInc: DK/NA		0.087	0.056
		(0.101)	(0.112)
GEVote: DK/DNV			0.198
			(0.232)
GEVote: Green			0.049
			(0.280)
GEVote: Lab			−0.385***
			(0.097)
GEVote: LD			−0.318**
			(0.148)
GEVote: Other			−0.206
			(0.192)
GEVote: UKIP			0.567**
			(0.259)
EUVote: DK/DNV			0.283
			(0.191)
EUVote: Leave			0.463***
			(0.088)
‘Reads: FT‘			−0.177
			(0.219)
‘Reads: Guardian‘			−0.552***
			(0.122)
‘Reads: Telegraph‘			0.201
			(0.124)
‘Reads: Times‘			−0.180
			(0.113)
‘Reads: Sun‘			−0.260
			(0.186)
‘Reads: Mail‘			0.091
			(0.096)
‘Reads: Mirror‘			0.063
			(0.160)
‘Reads: Express‘			0.140
			(0.145)
‘Reads: Star‘			−0.032
			(0.095)
‘Reads: Record‘			−0.072
			(0.266)
N	3084	3084	2660

***p < .01; **p < .05; *p < .1

Table A6: Correlates of balanced budget attitudes. All models are estimated by OLS and include controls for dummy variables for experimental split (not shown). Omitted categories are: party vote: Conservative; newspaper: None.

	BalanceBudget	
	Model 1	**Model 2**
Analogy	0.221***	0.115***
	(0.044)	(0.011)
HHB_Avoid	0.311***	
	(0.048)	
HHB_Bad		0.065***
		(0.024)
AgeStd	−0.051**	−0.064***
	(0.021)	(0.022)
Gender: Male	−0.166***	−0.182***
	(0.033)	(0.034)
SocialGrade: B	0.101*	0.107*
	(0.055)	(0.058)
SocialGrade: C1	0.081	0.084
	(0.052)	(0.054)
SocialGrade: C2	0.179***	0.200***
	(0.058)	(0.062)
SocialGrade: D	0.227***	0.232***
	(0.070)	(0.074)
SocialGrade: E	0.249***	0.240***
	(0.069)	(0.073)
Housing: Other	−0.049	−0.096
	(0.072)	(0.077)
Housing: Renter	0.001	−0.031
	(0.043)	(0.045)
HHInc: >£100k	−0.113	−0.082
	(0.091)	(0.097)
HHInc: £25k-50k	0.0004	−0.020
	(0.044)	(0.046)
HHInc: £50k-100k	−0.002	−0.047
	(0.056)	(0.058)
HHInc: DK/NA	0.033	−0.013
	(0.048)	(0.052)
GEVote: DK/DNV	0.093	0.043
	(0.107)	(0.115)
GEVote: Green	−0.031	−0.012
	(0.122)	(0.124)
GEVote: Lab	−0.183***	−0.161***
	(0.040)	(0.042)
GEVote: LD	−0.166***	−0.150**
	(0.063)	(0.066)
GEVote: Other	−0.025	−0.100
	(0.079)	(0.084)
GEVote: UKIP	0.259**	0.257**
	(0.112)	(0.117)
EUVote: DK/DNV	0.136	0.194**
	(0.083)	(0.089)
EUVote: Leave	0.205***	0.237***
	(0.037)	(0.039)
Analogy:HHB_Avoid	−0.031**	
	(0.013)	
Analogy:HHB_Bad		0.001
		(0.006)
Constant	1.904***	2.915***
	(0.174)	(0.079)
N	2560	2359
R-squared	0.173	0.148

***p < .01; **p < .05; *p < .1

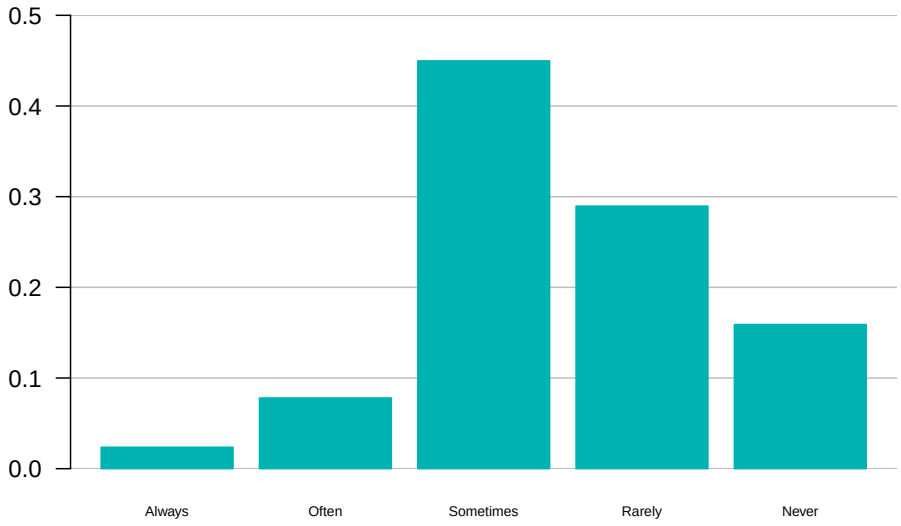

Figure A1: Distribution of $BalanceBudget_i$.

C Extra Results for: "Experimental Evidence: Priming Personal Austerity"

Table A7: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	PAPrime	p-value
Vote2015: Lab	0.250	0.245	0.255	0.501
	0.007	0.010	0.011	
Vote2015: LD	0.068	0.060	0.076	0.054
	0.004	0.006	0.006	
Vote2015: SNP	0.036	0.041	0.032	0.132
	0.003	0.005	0.004	
Vote2015: PC	0.004	0.005	0.004	0.582
	0.001	0.002	0.001	
Vote2015: UKIP	0.100	0.096	0.104	0.429
	0.005	0.007	0.007	
Vote2015: Green	0.041	0.045	0.038	0.281
	0.003	0.005	0.005	
Vote2015: BNP	0.001	0	0.001	0.097
	0.0004	0	0.001	
Vote2015: Other	0.019	0.022	0.017	0.306
	0.002	0.004	0.003	
Vote2015: DK	0.050	0.052	0.048	0.603
	0.004	0.005	0.005	
Vote2015: NA	0.149	0.156	0.142	0.233
	0.006	0.009	0.008	
PId: Con	0.261	0.260	0.263	0.880
	0.008	0.011	0.011	
PId: Lab	0.270	0.277	0.263	0.349
	0.008	0.011	0.011	
PId: LD	0.071	0.066	0.077	0.201
	0.004	0.006	0.006	
PId: SNP	0.026	0.027	0.025	0.722
	0.003	0.004	0.004	
PId: PC	0.003	0.003	0.003	0.991
	0.001	0.001	0.001	
PId: UKIP	0.068	0.065	0.072	0.447
	0.004	0.006	0.006	
PId: Green	0.038	0.038	0.038	0.896
	0.003	0.005	0.005	
PId: BNP	0.002	0.001	0.002	0.414
	0.001	0.001	0.001	
PId: Other	0.016	0.018	0.015	0.480
	0.002	0.003	0.003	
PId: DK	0.073	0.075	0.072	0.699
	0.004	0.006	0.006	

Table A8: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	PAPrime	p-value
Gender: Male	0.455	0.465	0.445	0.249
	0.009	0.012	0.012	
HHInc: £5k-10k	0.049	0.047	0.050	0.667
	0.004	0.005	0.005	
HHInc: £10k-15k	0.066	0.062	0.070	0.327
	0.004	0.006	0.006	
HHInc: £15k-20k	0.065	0.064	0.066	0.823
	0.004	0.006	0.006	
HHInc: £20k-25k	0.076	0.071	0.082	0.218
	0.005	0.006	0.007	
HHInc: £25k-30k	0.067	0.064	0.070	0.443
	0.004	0.006	0.006	
HHInc: £30k-35k	0.053	0.057	0.050	0.374
	0.004	0.006	0.005	
HHInc: £35k-40k	0.051	0.057	0.046	0.171
	0.004	0.006	0.005	
HHInc: £40k-45k	0.051	0.043	0.060	0.027
	0.004	0.005	0.006	
HHInc: £45k-50k	0.036	0.033	0.039	0.377
	0.003	0.004	0.005	
HHInc: £50k-60k	0.054	0.058	0.050	0.267
	0.004	0.006	0.005	
HHInc: £60k-70k	0.031	0.030	0.033	0.515
	0.003	0.004	0.004	
HHInc: £70k-100k	0.044	0.042	0.045	0.645
	0.003	0.005	0.005	
HHInc: £100k-150k	0.017	0.019	0.015	0.411
	0.002	0.003	0.003	
HHInc: >£150k	0.009	0.009	0.009	0.841
	0.002	0.002	0.002	
HHInc: DK	0.061	0.063	0.058	0.530
	0.004	0.006	0.006	
HHInc: Declined	0.179	0.188	0.169	0.138
	0.007	0.010	0.009	
HHInc: NA	0.070	0.074	0.067	0.427
	0.004	0.006	0.006	

Table A9: Estimated effect of the "personal austerity" prime on balanced budget attitudes.

	$DefPriority$	
	Model 1	**Model 2**
$T(PersonalAusterity)$	0.002	−0.003
	(0.029)	(0.028)
Constant	2.417***	2.442***
	(0.020)	(0.113)
Vote Choice (2015)	No	Yes
Party-id (2015)	No	Yes
Household Income	No	Yes
N	3400	3400
R-squared	0.00000	0.063

***p < .01; **p < .05; *p < .1

D Extra Results for: "Experimental Evidence: Analogical Reason"

Table A10: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	B	C	D	p-value
Vote2017: Lab	0.375	0.403	0.371	0.359	0.364	0.508
	0.011	0.023	0.023	0.023	0.022	
Vote2017: LD	0.081	0.089	0.089	0.074	0.073	0.674
	0.006	0.013	0.014	0.012	0.012	
Vote2017: SNP	0.035	0.037	0.041	0.033	0.030	0.823
	0.004	0.009	0.010	0.009	0.008	
Vote2017: PC	0.006	0.011	0.007	0.004	0.002	0.353
	0.002	0.005	0.004	0.003	0.002	
Vote2017: UKIP	0.023	0.020	0.016	0.029	0.028	0.486
	0.004	0.006	0.006	0.008	0.008	
Vote2017: Green	0.018	0.015	0.016	0.020	0.019	0.929
	0.003	0.006	0.006	0.007	0.006	
Vote2017: N/A	0.012	0.013	0.014	0.009	0.011	0.902
	0.003	0.005	0.006	0.004	0.005	
Vote2017: Other	0.029	0.026	0.018	0.038	0.032	0.321
	0.004	0.007	0.006	0.009	0.008	
Vote2017: DK	0	0	0	0	0	
	0	0	0	0	0	
PId: Con	0.239	0.227	0.247	0.223	0.261	0.590
	0.011	0.022	0.023	0.022	0.023	
PId: Lab	0.252	0.246	0.228	0.256	0.278	0.477
	0.012	0.023	0.022	0.023	0.024	
PId: LD	0.087	0.105	0.081	0.082	0.081	0.617
	0.007	0.016	0.015	0.015	0.014	
PId: SNP/PC	0.028	0.037	0.031	0.014	0.031	0.242
	0.004	0.010	0.009	0.006	0.009	
PId: Other	0.094	0.071	0.101	0.104	0.100	0.363
	0.008	0.014	0.016	0.016	0.016	
PId: DK	0.065	0.071	0.059	0.070	0.061	0.881
	0.007	0.014	0.013	0.014	0.013	

Table A11: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	B	C	D	p-value
Gender: Male	0.574	0.551	0.601	0.590	0.554	0.221
	0.011	0.021	0.021	0.021	0.021	
HHInc: £5k-10k	0.042	0.047	0.059	0.038	0.024	0.036
	0.004	0.010	0.011	0.009	0.007	
HHInc: £10k-15k	0.072	0.065	0.095	0.072	0.057	0.124
	0.006	0.011	0.013	0.012	0.010	
HHInc: £15k-20k	0.077	0.067	0.087	0.078	0.077	0.708
	0.006	0.011	0.013	0.012	0.012	
HHInc: £20k-25k	0.091	0.097	0.089	0.074	0.104	0.351
	0.006	0.013	0.013	0.012	0.014	
HHInc: £25k-30k	0.076	0.077	0.071	0.076	0.079	0.968
	0.006	0.012	0.012	0.012	0.012	
HHInc: £30k-35k	0.070	0.067	0.061	0.070	0.081	0.654
	0.006	0.011	0.011	0.011	0.012	
HHInc: £35k-40k	0.053	0.053	0.059	0.052	0.047	0.884
	0.005	0.010	0.011	0.010	0.009	
HHInc: £40k-45k	0.055	0.069	0.042	0.056	0.051	0.322
	0.005	0.011	0.009	0.010	0.010	
HHInc: £45k-50k	0.053	0.047	0.048	0.060	0.055	0.782
	0.005	0.010	0.010	0.011	0.010	
HHInc: £50k-60k	0.054	0.049	0.051	0.062	0.055	0.810
	0.005	0.010	0.010	0.011	0.010	
HHInc: £60k-70k	0.031	0.032	0.028	0.032	0.031	0.982
	0.004	0.008	0.007	0.008	0.008	
HHInc: £70k-100k	0.046	0.055	0.046	0.050	0.033	0.396
	0.005	0.010	0.009	0.010	0.008	
HHInc: £100k-150k	0.020	0.016	0.018	0.020	0.026	0.750
	0.003	0.006	0.006	0.006	0.007	
HHInc: >£150k	0.008	0.008	0.008	0.006	0.008	0.973
	0.002	0.004	0.004	0.003	0.004	
HHInc: DK	0.054	0.049	0.053	0.044	0.071	0.262
	0.005	0.010	0.010	0.009	0.011	
HHInc: Declined	0.179	0.181	0.158	0.199	0.177	0.404
	0.009	0.017	0.016	0.018	0.017	

Table A12: Estimated effects of the various "reason" treatments on balanced budget attitudes (*BalanceBudget*) and household analogy endorsement ($Analogy_{i,t}$).

	BalanceBudget	*Analogy*
	Model 1	**Model 2**
Reason	−0.077	−0.137
	(0.057)	(0.108)
Analogy Reason	−0.044	0.016
	(0.057)	(0.108)
Less Analogy Reason	0.007	−0.155
	(0.057)	(0.107)
Constant	2.432^{***}	3.678^{***}
	(0.040)	(0.075)
N	2017	1995
R-squared	0.001	0.002

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

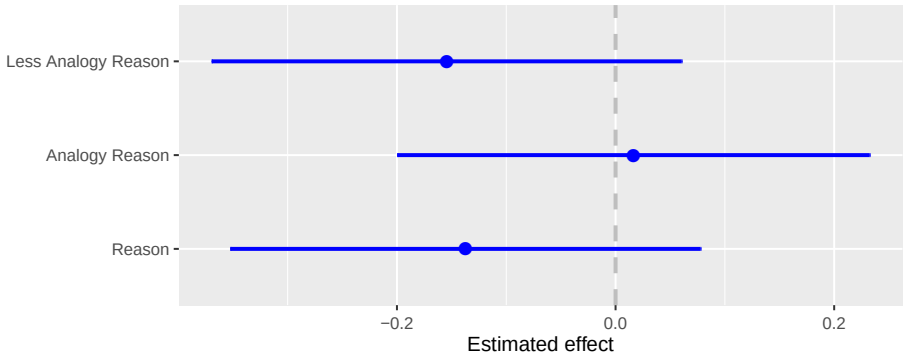

Figure A2: Estimated effects of the various "reason" treatments on household analogy endorsement, measured using $Analogy_{i,t}$.

E Extra Results for: "The Politically Attentive Drive the Analogy–Hawk Correlation"

Table A13: Estimated relationship between *BalanceBudget* and *Analogy*, as moderated by political attention.

	BalanceBudget		
	Model 1	**Model 2**	**Model 3**
Analogy	0.011	0.011	0.018
	(0.033)	(0.032)	(0.037)
PoliticalAttention	-0.128^{***}	-0.116^{***}	-0.101^{***}
	(0.018)	(0.018)	(0.021)
Analogy × *PoliticalAttention*	0.018^{***}	0.018^{***}	0.016^{***}
	(0.005)	(0.005)	(0.005)
Constant	3.800^{***}	3.853^{***}	3.846^{***}
	(0.127)	(0.142)	(0.173)
Age	No	Yes	Yes
Household Income	No	Yes	Yes
Gender	No	Yes	Yes
Vote Choice (2017)	No	No	Yes
Brexit Vote Choice	No	No	Yes
N	2081	2081	1795
R-squared	0.097	0.113	0.154

$^{***}p < .01$; $^{**}p < .05$; $^{*}p < .1$

Table A14: Estimated relationship between *BalanceBudget* and *Analogy*, as moderated by political attention.

	BalanceBudget		
	Model 1	**Model 2**	**Model 3**
Analogy	0.051	0.051	0.054
	(0.033)	(0.032)	(0.040)
$PoliticalAttention = Low$	-0.466^{***}	-0.431^{***}	-0.288^{*}
	(0.142)	(0.141)	(0.168)
$PoliticalAttention = Mid$	-0.836^{***}	-0.757^{***}	-0.567^{***}
	(0.151)	(0.151)	(0.175)
$PoliticalAttention = High$	0.076^{**}	0.073^{**}	0.066
	(0.036)	(0.036)	(0.042)
$Analogy \times PoliticalAttention = X$	0.115^{***}	0.113^{***}	0.092^{**}
	(0.039)	(0.038)	(0.044)
Constant	3.499^{***}	3.589^{***}	3.522^{***}
	(0.131)	(0.145)	(0.184)
Age	No	Yes	Yes
Household Income	No	Yes	Yes
Gender	No	Yes	Yes
Vote Choice (2017)	No	No	Yes
Brexit Vote Choice	No	No	Yes
N	2081	2081	1795
R-squared	0.092	0.108	0.151

***p < .01; **p < .05; *p < .1

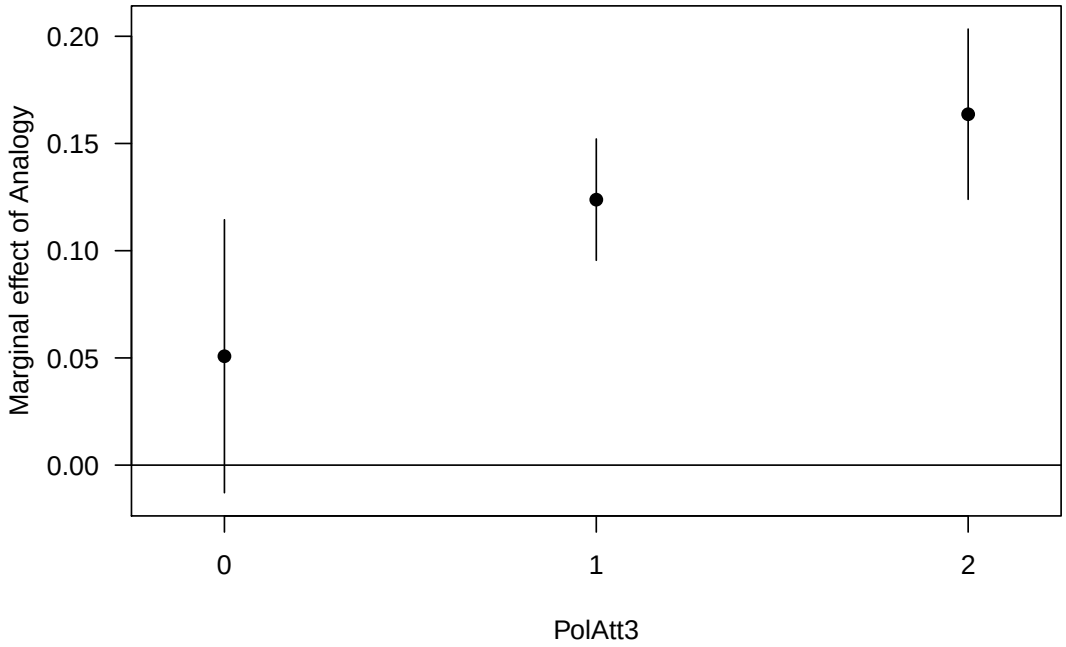

Figure A3: Estimated marginal effect of $Analogy_i$ on $BalanceBudget_it$, conditional on self-reported political attention ($PolitialAttention_i$). Figure based on estimates in Table A14 in the Supplementary Material.

F Extra Results for: "Balanced Budget Attitudes Cause Analogy Endorsement"

Table A15: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	B	C	D	p-value
Vote2017: Lab	0.375	0.403	0.371	0.359	0.364	0.508
	0.011	0.023	0.023	0.023	0.022	
Vote2017: LD	0.081	0.089	0.089	0.074	0.073	0.674
	0.006	0.013	0.014	0.012	0.012	
Vote2017: SNP	0.035	0.037	0.041	0.033	0.030	0.823
	0.004	0.009	0.010	0.009	0.008	
Vote2017: PC	0.006	0.011	0.007	0.004	0.002	0.353
	0.002	0.005	0.004	0.003	0.002	
Vote2017: UKIP	0.023	0.020	0.016	0.029	0.028	0.486
	0.004	0.006	0.006	0.008	0.008	
Vote2017: Green	0.018	0.015	0.016	0.020	0.019	0.929
	0.003	0.006	0.006	0.007	0.006	
Vote2017: N/A	0.012	0.013	0.014	0.009	0.011	0.902
	0.003	0.005	0.006	0.004	0.005	
Vote2017: Other	0.029	0.026	0.018	0.038	0.032	0.321
	0.004	0.007	0.006	0.009	0.008	
Vote2017: DK	0	0	0	0	0	
	0	0	0	0	0	
PId: Con	0.239	0.227	0.247	0.223	0.261	0.590
	0.011	0.022	0.023	0.022	0.023	
PId: Lab	0.252	0.246	0.228	0.256	0.278	0.477
	0.012	0.023	0.022	0.023	0.024	
PId: LD	0.087	0.105	0.081	0.082	0.081	0.617
	0.007	0.016	0.015	0.015	0.014	
PId: SNP/PC	0.028	0.037	0.031	0.014	0.031	0.242
	0.004	0.010	0.009	0.006	0.009	
PId: Other	0.094	0.071	0.101	0.104	0.100	0.363
	0.008	0.014	0.016	0.016	0.016	
PId: DK	0.065	0.071	0.059	0.070	0.061	0.881
	0.007	0.014	0.013	0.014	0.013	

Table A16: Summary statistics: means (odd rows) and standard errors of the means (even rows). First column: full sample. Middle columns: each of the experimental treatment conditions. Final column: test of whether we can reject the null that treatment dummies do not belong in a bare model predicting each respective row variable — such that low p-values indicate a worrying lack of balance across treatment conditions.

Variable	All	Control	B	C	D	p-value
Gender: Male	0.574	0.551	0.601	0.590	0.554	0.221
	0.011	0.021	0.021	0.021	0.021	
HHInc: £5k-10k	0.042	0.047	0.059	0.038	0.024	0.036
	0.004	0.010	0.011	0.009	0.007	
HHInc: £10k-15k	0.072	0.065	0.095	0.072	0.057	0.124
	0.006	0.011	0.013	0.012	0.010	
HHInc: £15k-20k	0.077	0.067	0.087	0.078	0.077	0.708
	0.006	0.011	0.013	0.012	0.012	
HHInc: £20k-25k	0.091	0.097	0.089	0.074	0.104	0.351
	0.006	0.013	0.013	0.012	0.014	
HHInc: £25k-30k	0.076	0.077	0.071	0.076	0.079	0.968
	0.006	0.012	0.012	0.012	0.012	
HHInc: £30k-35k	0.070	0.067	0.061	0.070	0.081	0.654
	0.006	0.011	0.011	0.011	0.012	
HHInc: £35k-40k	0.053	0.053	0.059	0.052	0.047	0.884
	0.005	0.010	0.011	0.010	0.009	
HHInc: £40k-45k	0.055	0.069	0.042	0.056	0.051	0.322
	0.005	0.011	0.009	0.010	0.010	
HHInc: £45k-50k	0.053	0.047	0.048	0.060	0.055	0.782
	0.005	0.010	0.010	0.011	0.010	
HHInc: £50k-60k	0.054	0.049	0.051	0.062	0.055	0.810
	0.005	0.010	0.010	0.011	0.010	
HHInc: £60k-70k	0.031	0.032	0.028	0.032	0.031	0.982
	0.004	0.008	0.007	0.008	0.008	
HHInc: £70k-100k	0.046	0.055	0.046	0.050	0.033	0.396
	0.005	0.010	0.009	0.010	0.008	
HHInc: £100k-150k	0.020	0.016	0.018	0.020	0.026	0.750
	0.003	0.006	0.006	0.006	0.007	
HHInc: >£150k	0.008	0.008	0.008	0.006	0.008	0.973
	0.002	0.004	0.004	0.003	0.004	
HHInc: DK	0.054	0.049	0.053	0.044	0.071	0.262
	0.005	0.010	0.010	0.009	0.011	
HHInc: Declined	0.179	0.181	0.158	0.199	0.177	0.404
	0.009	0.017	0.016	0.018	0.017	

Table A17: Estimated effect of answering the balanced budget question first on balanced budget attitudes ($BalanceBudget_{i,t}$) and analogy endorsement ($Analogy_{i,t}$).

	BalanceBudget	*Analogy*
	Model 1	**Model 2**
Balance Budget First	0.038	0.149**
	(0.037)	(0.073)
Constant	3.438***	3.342***
	(0.026)	(0.051)
N	2356	2142
R-squared	0.0004	0.002

***p < .01; **p < .05; *p < .1

Table A18: Estimated effect of answering the balanced budget question first on analogy endorsement ($Analogy_{i,t}$).

	Analogy		
	Model 1	**Model 2**	**Model 3**
$BalanceBudget = 2$	0.078	0.077	−0.107
	(0.589)	(0.591)	(0.623)
$BalanceBudget = 3$	0.793	0.790	0.601
	(0.573)	(0.575)	(0.607)
$BalanceBudget = 4$	1.171**	1.165**	0.934
	(0.577)	(0.578)	(0.611)
$BalanceBudget = 5$	1.094*	1.092*	0.896
	(0.586)	(0.588)	(0.623)
$BBFirst$	−0.292	−0.259	−0.587
	(0.657)	(0.658)	(0.700)
$BalanceBudget = 2 \times BBFirst$	0.403	0.369	0.659
	(0.696)	(0.698)	(0.741)
$BalanceBudget = 3 \times BBFirst$	0.104	0.069	0.361
	(0.665)	(0.666)	(0.708)
$BalanceBudget = 4 \times BBFirst$	0.681	0.647	0.988
	(0.670)	(0.672)	(0.713)
$BalanceBudget = 5 \times BBFirst$	0.986	0.953	1.323*
	(0.685)	(0.687)	(0.734)
Constant	2.500***	2.414***	2.987***
	(0.569)	(0.581)	(0.631)
Age	No	Yes	Yes
Household Income	No	Yes	Yes
Gender	No	Yes	Yes
Vote Choice (2017)	No	No	Yes
Brexit Vote Choice	No	No	Yes
Party-id	No	No	No
N	2081	2081	1795
R-squared	0.082	0.084	0.119

***p < .01; **p < .05; *p < .1