Is Pocketbook Voting Sensitive to Policy?[†]

Christoffer H. Dausgaard[‡]

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People often vote with their pocketbooks. Is this an effective tool for holding governments accountable? A key unresolved question is whether voters reliably respond to the policy-induced component of their income changes or simply react to all income changes regardless of cause. Existing research cannot definitively answer this question due to methodological limitations: studies of single policies cannot distinguish voters responding to the income shock itself or to income changes regardless of their origin, while research on pocketbook attribution relies on potentially biased self-reports. To answer the question, I introduce a novel approach linking a large survey panel to policy microsimulation models that track how tax-and-transfer policies directly affect disposable incomes. This allows me to decompose respondents' total disposable income changes into policy-induced and residual components, providing a direct test of what income variation drives pocketbook voting. Applying this approach to the UK in the 2010s, a case of significant policy-driven income variation, I find that voters do not hold incumbents more accountable for policy-induced than residual income changes on average. Instead, they respond to total income changes, which prove to be a poor proxy for policy-induced income changes. These findings suggest that voters fail to reliably reward and punish incumbents for policies that affect their disposable incomes. Pocketbook voting may therefore be a weaker accountability mechanism than commonly assumed.

"… we did a lot for working people [but] we didn't tell people about it. We thought: just by legislating, people would know about it. Well, they don't!"

– Chuck Schumer, 'The Daily', 16 March 2025

How do voters hold democratic politicians accountable in a complex world? One widely documented shortcut is pocketbook voting: rewarding incumbents when personal finances improve and punishing them when they decline (Bechtel and Liesch 2020; Healy, Persson and Snowberg 2017; Tilley, Neundorf and Hobolt 2018). By just assessing personal income changes, voters can motivate the government to make policies they like without closely following politics or analyzing macroeconomic trends. This form of "rough justice" (Fiorina 1981, p.4) promises to discipline politicians by tying their survival to citizens' economic well-being.

Yet scholars disagree about which income changes voters respond to. Do they hold incumbents accountable for all income changes, regardless of their cause, as Fiorina (1981) famously argued? Or do they only respond to income changes that are 'policy-induced', i.e. caused by policy changes such as tax reforms, new spending programs, or changes to the welfare system? The first model – outcome-based pocketbook voting – is undoubtedly easier for voters who need only "calculate changes in their own welfare" (Fiorina 1981, p. 5) to decide the incumbent's fate. However, as Kramer (1983) and other scholars have pointed out, personal income changes are "contaminated" (p. 99) by idiosyncratic and exogenous variation unrelated to government policy, like workplace changes, life-cycle transitions, family circumstances, and health shocks. As a result, outcome-based pocketbook voting may frequently sanction incumbents for factors entirely beyond their control. The second model – policy-based pocketbook voting – involves the more demanding task of attributing responsibility for changes in economic outcomes, but results in a tighter link between policy effects and electoral sanctions (Achen and Bartels 2016; Duch and Stevenson 2008; Powell and Whitten 1993).

While recent research appears to support the policy-based model, existing research has not definitively resolved which model of pocketbook voting prevails. One strand of the economic voting literature examines whether voters are more responsive to income changes that they sub-

jectively attribute to government policy than to other income changes (e.g. Tilley, Neundorf and Hobolt 2018). However, these studies rely on self-reported attributions without objective benchmarks, leaving open the possibility of rationalized or biased attribution judgments (Feldman 1982; Larsen 2021). While citizens aspire to policy-based pocketbook voting, these studies do not show whether they achieve it.

Another strand – the policy feedback literature – focuses on actual behavioral responses to policies (e.g. Manacorda, Miguel and Vigorito 2011). But these are typically single-case studies of highly salient programs (Kogan 2021), which are somewhat atypical fiscal events and thus tell us little about how reliably voters respond to policy changes in general. More fundamentally, these studies lack the crucial counterfactual: how voters would have responded to comparable financial shocks had they not resulted from policy. The fact that voters respond to such policy-induced shocks is thus equally consistent with outcome-based pocketbook voting as it may reflect voters sanctioning incumbents for any income shocks regardless of their cause. In sum, there is little direct evidence on whether policy-induced or total income changes drive pocketbook voting.

This paper aims to adjudicate between the two models of pocketbook voting with a novel empirical approach. Moving beyond singular policies, I examine voters' responses to the net effects of all government policies that directly affect their disposable incomes using the advanced policy-microsimulation model, UKMOD. Essentially an advanced disposable income calculator, UKMOD allows me to decompose changes in the disposable incomes of 40,000 surveyed British households over a decade into (i) the net effect of changes to policies that directly affect incomes (i.e. taxes and transfers), and (ii) residual income fluctuations not directly caused by policy. I can thus capture how reliably voters respond to such 'policy-induced' income changes on average as well as any differential responsiveness to such changes compared to income changes in general. This provides a direct test of whether pocketbook voting is mostly policy-based or outcome-based.

Using this approach, I find limited support for policy-based pocketbook voting. With conventional survey measures of incumbent party support as outcomes, I find that voters sanction incumbents no more for policy-induced income changes than other income changes, instead re-

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sponding equally to all changes regardless of their origin. Subgroup analyses confirm that this pattern holds across the electorate, with no group displaying strong signs of policy-based pocketbook voting. These findings suggest that voters fail to reliably hold incumbents accountable for the direct income effects of their policies.

The income decomposition further reveals that such policy-induced changes account for only about a quarter of total income variation at the median. With voters responding equally to all income changes, pocketbook voting is primarily driven by this residual variation. Consequently, I estimate that outcome-based pocketbook voters fail to sanction incumbents in line with their direct policy-induced income gains or losses 42% of the time. Most pocketbook voting therefore represents "rough justice" based on total income changes, most of which bear little direct relation to government policy.

These findings have important implications for democratic accountability and party strategy. My results suggest that parties may struggle to secure electoral support simply by redistributing resources, as suggested in classical political economy models of strategic politicians (Dixit and Londregan 1996; Tufte 1978). This casts doubt on the effectiveness of party strategies like "deliverism" that seek to win voters through the delivery of tangible material gains (Bhargava, Shams and Hanbury 2024). The normative implications are more ambiguous. On the one hand, the disciplining power of pocketbook voting may be weaker than commonly assumed. On the other hand, voters' insensitivity to policy-induced income changes may reduce incumbents' incentives for tactical redistribution or "pork-barrel" politics, often seen as undermining accountability and broad-based economic growth (Ferejohn 1986; Maskin and Tirole 2019; Tufte 1978). Even then, my results do not eliminate the risk of electoral manipulation entirely. Despite my finding of a null average effect of policy-induced income changes on voter behavior, studies in the policy feedback literature suggest that certain highly visible policies still elicit electoral responses. Incumbents may thus retain some control over which policies they are held accountable for. This makes it crucial for future research to examine governments' capacity to selectively shape electoral accountability.

Pocketbook Voting and Incumbent Incentives

Recent studies suggest that pocketbook voting significantly shapes electoral behavior, influencing the electoral rewards and punishments incumbents can expect from their policies (Bechtel and Liesch 2020; Healy, Persson and Snowberg 2017; Tilley, Neundorf and Hobolt 2018). Yet, the relationship between pocketbook voting and incumbent incentives is not straightforward. When voters reward and punish incumbents for personal income changes, they incentivize incumbents to raise voters' incomes, especially for electorally pivotal groups. But for this mechanism to promote accountability, incumbents must expect to be rewarded specifically for policies that benefit voters and punished for those that harm them. Strong pocketbook voting alone does not guarantee this outcome because voters' incomes change for many reasons besides policy, like workplace changes, family circumstances, and life-cycle transitions. If voters respond equally to all income changes regardless of their cause, they may inadvertently punish incumbents whose policies actually helped them, or reward those whose policies hurt them. Under these conditions, even strong pocketbook voting fails to provide clear policy incentives.[1] The following section examines what features of pocketbook voting would overcome this problem.

A Model of Pocketbook Voting

Consider a simple pocketbook voting model of electoral accountability. In it, incumbent policies have a net effect on each voter's pocketbook that is either positive or negative. Voters, in turn, can choose to support or reject the incumbent at the ballot box (Ferejohn 1986; Key 1966). Following Kramer (1983), the net change in i's disposable income in period t can be defined as a sum of two jointly exhaustive components: policy-induced income changes, e.g. due to changes to tax brackets or new government benefits, and residual income changes, e.g. due to idiosyncratic changes or exogenous macro-level shocks. Ignoring other non-pocketbook considerations in vote choice for

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[1]To be sure, one cannot draw too specific conclusions about incumbent incentives based on individual-level voter behavior alone (Ashworth and de Mesquita 2014). This argument therefore avoids deriving precise implications for incumbent behavior. I return to this point in the discussion.

simplicity[2], we can model i's binary incumbent vote choice as a function of these components as follows:

$$\text{incumbent vote}_{it} = \beta_p \Delta y_{it}^{policy} + \beta_r \Delta y_{it}^{residual} \tag{1}$$

where the sum of Δy_{it}^{policy} and $\Delta y_{it}^{residual}$ is the total change in i's disposable income, and the β's are the weights voters put on each type of income change in deciding their vote.

Based on this equation, I define an electorate's *policy-sensitivity* as the share of voters whose incumbent support aligns with the sign on Δy_{it}^{policy} in a given election.[3] The policy-sensitivity of the electorate is what determines the strength of incumbent incentives: the more reliably voters sanction the incumbent in line with their personal gain or loss from their policies, the more reliably incumbents can expect electoral rewards for the effects of their policies on voters' incomes. For simplicity, I refer to cases where voters reward incumbents who helped them and punish those who hurt them as "congruent" sanctions, and the opposite as "incongruent" sanctions. What, then, determines how many voters deliver congruent sanctions? The model suggests two key factors: how much weight voters place on policy-induced versus other income changes ('attribution'), and how much of total income variation stems directly from policy versus other sources (the magnitude of residual variation).

The first and most obvious way for sanctions to be congruent is if voters act just on policy-induced income changes ($\beta_p > 0$) and ignore residual changes ($\beta_r = 0$). This would be a case of perfect attribution of responsibility as voters consistently sanction incumbents for policy changes, no matter how small. This is the policy-based model of pocketbook voting espoused by Kramer (1983) in his influential article arguing that pocketbook voting can yield strong and reliable electoral sanctions even if the overall relationship between income changes and incumbent support is weak. Other scholars are likewise optimistic that voters have the sophistication or at least useful

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[2]One can think of these as an added term, α_{it}, in Equation 1 representing the sum weight of alternative considerations.

[3]Equivalently, an individual's policy-sensitivity is this share within that individual across elections over time. I mostly focus on electorate-level policy-sensitivity because it is more intuitive.

heuristics to judge when policy is responsible for pocketbook changes and when it is not (Ashworth, de Mesquita and Friedenberg 2018; Fowler and Hall 2018; Fowler and Montagnes 2015, 2023; Tilley, Neundorf and Hobolt 2018). Identifying income changes that are directly caused by policy may be well within ordinary citizen competence. As they file their tax returns or get their monthly payslip, voters can directly keep track of changes in the amounts they pay in taxes or receive in benefits. Further, media coverage of economic policy-making can alert voters to changes that might affect their incomes (Ciobanu 2024; Mutz 1994) and voters might only need to know how their incomes are affected by major policies to approximate how they will benefit on average from the government's policy platform. It is also possible for voters to get attribution right without putting much effort into distinguishing policy-induced income changes at all. For instance, such changes may be more psychologically salient, having an outsized impact on voters' retrospective evaluations of their finances. One reason for this could be the element of surprise as income shocks from policy may generally be more unexpected than other disposable income changes that more often result from personal choices. Another possibility is that voters learn from similar others who follow politics more closely, using their pocketbook attributions as a shorthand for their own (Lupia and McCubbins 1998). However voters get pocketbook attributions right, policy-based pocketbook voting will by definition be highly policy-sensitive.

However, there are also reasons to doubt that voters reliably distinguish and attribute policy-induced income changes from other income changes. Indeed, by contrast to Kramer's model, Fiorina (1981) 's seminal theory of pocketbook voting involves no attribution. According to Fiorina's outcome-based model, voters exert "rough justice" (p.4) based on total income changes (i.e. $\beta_p = \beta_r$). Many scholars argue that distinguishing policy-induced income changes is a challenging task for the ordinary citizen (Achen and Bartels 2016; Feldman 1984; Gomez and Wilson 2001; Healy and Malhotra 2010; Healy, Malhotra and Mo 2010; Wolfers 2002) and many people do not routinely scrutinize their payslips nor possess the required financial literacy to fully understand them (Abeler and Jäger 2015; Bhargava and Manoli 2015; Peters 1991). As Tilley, Neundorf and Hobolt (2018) note, "Incumbent governments have little control over people's day-to-day finances,

and citizens are likely to be aware of this" (p. 557). Indeed, abandoning attribution may be a rational response to the complexity of the task. Following the logic of classical principal-agent models with unobserved agent effort, the principal just sanctions the agent for the total outcome when extracting the 'effort signal' is too costly (Holmström 1979).

Besides ability, voters may not always like policy-induced income changes even when they are positive. This seems plausible, at least in certain cases. Many voters dislike the idea of being dependent on the welfare state, and policies that increase state transfers to their pocketbook may therefore invoke a negative response (Feldman 1982; Orbach 2006). Similarly, voters may be indifferent to or even dislike state support in certain cases, e.g., when it is seen as (insufficient) compensation for structural shocks (Gingrich 2019; Jares and Malhotra 2024; Kim and Gulotty 2024; Stutzmann 2025). Whether due to its cognitive cost or alternative preferences, voters' lack of attribution is not necessarily irrational.

How policy-sensitive is outcome-based pocketbook voting? Some scholars argue that without attribution, there is essentially no policy-sensitivity (Achen and Bartels 2016; Kramer 1983; Sances 2017; Wolfers 2002) while others disagree (Ferejohn 1986; Fiorina 1981; Key 1966). This disagreement can mostly be reduced to a disagreement over the assumed magnitude of non-policy income variation, $\Delta y_{it}^{residual}$, relative to Δy_{it}^{policy}. In the extreme case of $\Delta y_{it}^{residual} = 0$, voters' sanctions would be perfectly congruent even in Fiorina's no-attribution model. As such, attribution is not necessary for policy-sensitivity: without attribution, voters' sanctions may still be congruent if the residual variation is sufficiently small. The reason Kramer and others are sceptical of policy-sensitivity without attribution is their presumption that policy-induced income changes are relatively small. Thus, Kramer, for instance, makes an educated guess that income changes directly caused by policy make up 0-10% of total income variation on average (fn. 13, 1983).

I summarize these theorized relationships visually in Figure 1. It shows simulated policy-sensitivity across hypothetical pocketbook-voting electorates characterized by varying levels of attribution and residual variation. All electorates base their votes exclusively on their pocketbooks, varying only in the share of variation caused by policy and voters' responsiveness to it. The vertical

axis indicates the share of congruent incumbent sanctions achieved by each electorate going from chance (50%) to perfect (100%). Following Equation 1, each voter's income change is a product of two zero-centered, normally distributed income shocks, one from policy-induced changes and one from residual factors (see Appendix A for details.) The horizontal axis indicates the share of residual income variation, going from 0% to 100%, with the downward slopes indicating fewer congruent sanctions as this share increases. The five curves show varying degrees of attribution, between the extremes of Kramer's and Fiorina's theories, defined by the relative β-weights shown in Equation 1.
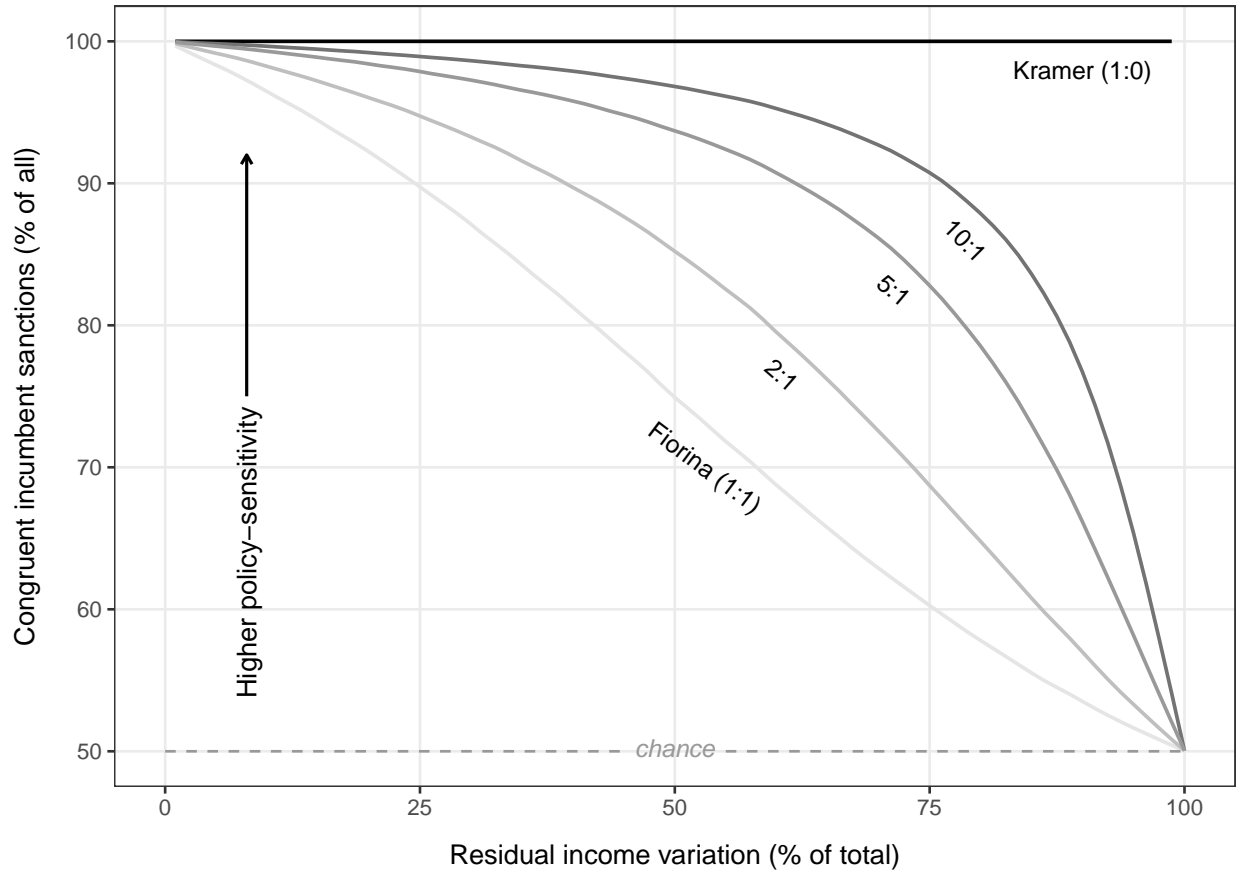

Figure 1: Policy-sensitivity across simulated electorates of pure pocketbook voters. Each electorate consists entirely of pocketbook voters with different combination of attribution $(\beta_p : \beta_r)$, as shown by gray curves ranging from none $(1 : 1)$ to perfect $(1 : 0)$, and magnitude of residual variation as indicated by the horizontal axis ranging from 0% to 100%. See Appendix A for technical details on the simulations.

As shown, Kramer's policy-based pocketbook voting always sanctions congruently regardless

of the level of residual variation. Fiorina's outcome-based pocketbook voting performs about as well when residual variation is limited. But as the residual increases in magnitude, the two models quickly diverge. At Kramer's estimated level of over 90% residual variation, even slight deviations from perfect attribution will result in sanctions that are scarcely congruent more often than chance. Still, as the darker curves with varying degrees of attribution show, voters need not engage in perfect attribution to largely sanction the incumbent congruently in many scenarios.

Summing up, the strength of pocketbook voting itself tells us little about its power to shape incumbent incentives. We must further know whether voters 'attribute' income changes, putting more weight on changes that are policy-induced (policy-based pocketbook voting), or whether they just respond to total income changes (outcome-based pocketbook voting). In the latter case, policy-sensitivity hinges on how reliably total income changes capture the effects of policy rather than residual income variation. Standard models of pocketbook accountability must therefore assume either a high degree of pocketbook attribution or a relatively small residual component. While the relative sizes of the two components has not been the subject of empirical research, much research has centered on attribution. I interrogate the research on this topic next.

Empirical Challenges to Inference in Existing Research

Voters' ability to attribute government responsibility for economic outcomes has been a lively topic since Kramer (1983) and Fiorina (1981)'s seminal theories. Most evidence has been interpreted as supporting voters' being competent at attribution, yet this evidence is indirect and cannot directly adjudicate between policy-based and outcome-based pocketbook voting.

First, one strand research on pocketbook voting finds that voters seem more responsive to income changes that they themselves attribute to government action than other income changes (Abramowitz, Lanoue and Ramesh 1988; Feldman 1982; Lau and Sears 1981; Marsh and Tilley 2010; Tilley, Neundorf and Hobolt 2018; although see Huber, Hill and Lenz 2012). In the perhaps most direct test of the question, Tilley, Neundorf and Hobolt (2018) find that income changes "have a greater impact when they can be [...] attributed directly to government policies, such as welfare payments and other government transfers" (Tilley, Neundorf and Hobolt 2018, p. 567).

However, these self-reported attributions may be "tainted by rationalization" (Kiewiet and Rivers 1984, p. 383) or self-serving biases (Larsen 2021). Without objective benchmarks, we cannot assess whether voters actually get attribution right. Indeed, other studies argue that voters are overly reluctant to link finances to politics, attributing only a very small share of their income to government (Brody and Sniderman 1977; Conover 1984; Feldman 1984). Thus, while this strand of research demonstrates that voters care about attributing income changes to policy, it has little to say about the accuracy of their attribution judgments.

The second category of evidence comes from studies of policy feedback. These studies show voters sometimes reward incumbents for targeted benefits (Kogan 2021; Manacorda, Miguel and Vigorito 2011; Pop-Eleches 2012; Rendleman and Yoder 2024; Zucco Jr. 2013), especially when policies are highly visible, salient, publicized and traceable to government (Hamel 2024; Mettler 2011; Shanks-Booth and Mettler 2019; Soss and Schram 2007). But these designs lack the crucial counterfactual: how would voters respond to comparable income shocks not caused by policy? related research on 'blind retrospection' suggests that voters also sometimes sanction incumbents for income shocks that cannot be tied directly to policy (Achen and Bartels 2016; Healy and Malhotra 2010; Healy, Malhotra and Mo 2010; Wolfers 2002; although see Fowler and Montagnes 2015, 2023; Graham et al. 2023). Without comparing voters' responses to both types of income shocks, these single case-designs are unable to adjudicate between policy-based pocketbook voting and outcome-based pocketbook voting. Moreover, these studies have mostly focused on rollouts of high-profile public spending programs (Kogan 2021) which are somewhat atypical fiscal events. It is therefore unclear how much can be inferred about the bulk of pocketbook variation in voters' daily lives, including the income effects of less visible policies like reforms of tax brackets or benefit eligibility criteria. Evidence of policy feedback is therefore consistent with voters rarely getting it 'right'.

Finally, the literature on clarity of responsibility finds that voters sanction incumbents more heavily for economic conditions when institutional arrangements and government characteristics make incumbent responsibility clearer (Duch and Stevenson 2008; Hobolt, Tilley and Banducci

2013; Powell and Whitten 1993). While these designs obtain the counterfactual by comparing more and less attributable income changes, and avoid relying on subjective attributions, they suffer from more fundamental problems of causal inference. As they rely on cross-country correlations, they are unable to rule out other country-level differences correlated with institutional characteristics driving the observed relationships (for an exception, see Larsen 2019). Insofar as they support policy-based pocketbook voting, the evidence is very indirect.

In sum, existing research provides mostly indirect evidence. While some studies suggest that voters care about attribution and there are examples where they get it right, they do not directly test whether pocketbook voting is driven by policy-induced or total income changes. In the next section, I describe a novel empirical approach that achieves this more directly. Moving beyond subjective attributions and income effects of single policies, I employ microsimulation models to calculate the net effects of all policies with direct income effects under the incumbent on individuals. Combining these data with survey data on vote choice, I can then test whether pocketbook voting is policy-based – driven by policy-induced income changes – or outcome-based – driven by total income changes. It additionally allows me to descriptively examine the share of policy-induced and residual variation in voters' pocketbooks. This is important because, as Figure 1 shows, their relative magnitudes are important for understanding how reliably outcome-based pocketbook voting results in congruent sanctions.

Methods and Data

My empirical approach involves first, estimating the share of policy-induced income changes in voters' pocketbooks, and second examining how reliably they sanction incumbents for them. For this, I turn to the policy microsimulation model, UKMOD. Developed by a team of University of Essex economists, UKMOD and its relatives are widely used for policy analysis by governments and researchers and modeled incomes have been extensively validated in economics (see Richiardi, Collado and Popova 2021). Despite this, they have barely been used in political science (Avram and Popova 2022; Elkjær and Mushövel 2023).

A microsimulation model is a script that applies a set of pre-defined policies to a set of individuals, with given market incomes and demographics, to calculate their post-policy disposable incomes. It includes government policies that directly affect citizens' disposable income, either through what they must pay (taxes, social insurance contributions) or receive (transfers, tax credits) from the state. I use UKMOD's coverage of such policies for the UK government from 2009-2019.

The unique functionality of the model is its ability to compute disposable incomes under actual and counterfactual policy systems for a sample of survey respondents. By calculating respondents' disposable incomes under current and previous versions of the policy system, the difference reveals how policy changes have directly affected individuals' disposable incomes on net (Bargain and Callan 2010; Richiardi, Collado and Popova 2021). This has clear advantages over existing approaches to examining policy-sensitivity and attribution. Instead of asking people about their own perceptions of the impact of changing taxes or transfers on their disposable income (Tilley, Neundorf and Hobolt 2018), UKMOD calculates these impacts mechanically based on respondent characteristics. And rather than estimating voters' responses to the income effects of singular policies, like the roll-out of a new benefit, it estimates the total net effects of all changes to such policies for a given time period. Because it is linked with a panel survey, it further allows me estimate these relationships within individuals and essentially observe how the same individuals respond to policy-induced and residual income shocks.

I exploit this functionality of UKMOD to estimate the policy-sensitivity of pocketbook voting in three steps. First, I apply UKMOD to a large pre-linked panel dataset, the UK Household Longitudinal Survey (UKHLS). The UKHLS is an annual survey of a nationally representative sample of around 40,000 British households across 10 yearly waves from 2010-2019 (Reis and Tasseva 2020). It includes detailed income data and a wide range of demographic and labor market information and, importantly, a few political attitude variables.[4] The panel is one of several datasets

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[4]The UKHLS is based on proportionately stratified, equal probability (clustered) sample of residential addresses and includes booster samples to ensure representativity of immigrant and ethnic minority groups (Bronka, Popova and Richiardi 2023). Interviews are conducted physically or by phone for all waves, except for an online subsample in the final two waves. As the survey samples entire households, I restrict my sample to respondents of voting age.

pre-linked to UKMOD by other researchers, which involves the extensive work of matching its granular income variables and demographics to UKMOD's policy functions (Bronka, Popova and Richiardi 2023). Second, I estimate the yearly net effects of policy changes on panel respondents' disposable incomes using Bargain and Callan (2010)'s decomposition approach. This allows me to examine how much variation in citizens' disposable incomes is explained by the direct effects of policy versus residual factors. Third, I leverage vote intention questions in the UKHLS to answer the main question: do voters sanction incumbents harder for policy-induced income changes than other income changes? I do this by regressing incumbent support on both policy-induced and total income changes in a series of standard pocketbook voting models.

While the concept of policy-sensitivity refers broadly to any effects of government policy on voters' incomes, my operationalization focuses on the *direct* income effects of policy – namely, changes to taxes, tax credits, social insurance contributions, and government transfers. This excludes more diffuse or indirect income effects stemming from policies that shape labor and product markets, such as regulation or trade policy, as well as second-order and macroeconomic effects of fiscal policy.[5] There are both pragmatic and substantive reasons for this restriction. Indirect income effects are generally indeterminate at the individual level, both for voters and for researchers, and incorporating them would require strong model-based assumptions and introduce substantial uncertainty into my key measure of policy-induced income changes. In contrast, direct income effects are transparent, deterministic, and indisputably attributable to the national incumbent. They also dominate both classical theories of distributive politics – which emphasize the surgical precision and immediacy with which incumbents can target taxes and especially transfers – and the existing empirical literature on policy-responsive voting, which has primarily focused on direct benefits, tax credits, and taxes (e.g., Mettler 2011; Pop-Eleches 2012; Rendleman and Yoder 2024; Sances 2017; Zucco Jr. 2013). If voters are not responsive to these direct and most salient instruments of redistribution, then the scope for meaningful policy-sensitivity is likely limited overall. For

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[5]UKMOD is a static microsimulation model. This means that if, e.g., the top income tax rate is increased, the model only captures the effect on disposable incomes for those in the top tax bracket from having to pay more in taxes. Any second-order impacts on other citizens' incomes resulting from shifting macro-economic equilibria are ignored.

simplicity, references in the following to 'policy', 'policy-system', and 'policy-induced' income changes will thus refer exclusively to this class of policies and policy effects on income.

This section proceeds as follows. I first explain how I use UKMOD to measure disposable incomes and decompose disposable income changes. I then discuss the case of fiscal policy in the UK in the 2010's and finally outline my estimation strategy.

How UKMOD calculates disposable incomes

UKMOD calculates disposable incomes based on information about individuals and a policy system. Specifically, it applies a policy system from a given year, s_t, to a sample of individuals with characteristics, c_{it}, to compute their disposable incomes, y_{it}. Contrary to its name, it does not simulate or impute respondents' disposable incomes but mechanically calculates them based on detailed information about respondents' market incomes, tax rates, eligibility for benefits, and so on. In simple form, this calculation can be written as:

$$y_{it} = s_t(c_{it})$$

Here, the individual's characteristics, c_{it}, are the sum of two observed components: market incomes and policy-relevant demographic characteristics like the number of children in the household, employment status and age. The policy system, s_t also includes two components: a set of policy *rules* that are effectively functions that turn individual characteristics into disposable incomes, e.g. the marginal tax rate, and a set of nominal monetary *parameters* for these rules, which include e.g. the exact nominal cutoffs for the marginal tax rate. While the incumbent can intervene to change both of these aspects of the policy system, nominal parameters are regularly updated to match inflation as a matter of routine, also in the UK (Bargain and Callan 2010). The policy systems in the model include all policies in force in a given year with only a few exceptions.

UKMOD is extensively documented and validated against official data, and it closely emulates the policy simulation models used by UK government departments, sharing similar scope, assump-

tions, and results (Bronka, Popova and Richiardi 2023; Richiardi, Collado and Popova 2021).[6] The fact that benefits and taxes are calculated generally provides higher precision than survey self-reports, where there is e.g. a tendency for benefits to be underreported (Bronka, Popova and Richiardi 2023, p. 6). Recent political science research has likewise used UKMOD to calculate the income effects of policy at both the aggregate (Elkjær and Mushövel 2023) and individual level (Avram and Popova 2022). Nonetheless, three potential sources of measurement error merit discussion.

First, respondents' market incomes are derived from self-reported incomes in the UKHLS panel. While not as precise as administrative income records (Healy, Persson and Snowberg 2017), the UKHLS income data are unusually high in quality and granularity, and have been used in prior studies of pocketbook voting (Marsh and Tilley 2010). The dataset comprehensively captures income sources, including standard wages, earnings from casual jobs, private pensions, and passive income from investments and properties (Bronka, Popova and Richiardi 2023, p. 22). Respondents are instructed to consult their latest payslip when reporting, and income is recorded by source down to the nearest pound. This has allowed researchers to perform various cross-checks across items by comparing reported income totals with the sum of incomes components and the like (Bronka, Popova and Richiardi 2023). When benchmarked against official statistics, market incomes in the UKHLS align closely, with discrepancies largely confined to income under-reporting among top earners (Bronka, Popova and Richiardi 2023). To address this issue, I exclude top earners in robustness checks.

Second, although UKMOD simulates the vast majority of income-related policies, a few components are not modeled (Richiardi, Collado and Popova 2021). On the benefit side, the model captures nearly all non-contributory transfers – including social assistance, housing, family, and income-tested benefits – as well as basic state pensions. It excludes contributory pensions and disability benefits, which require administrative records or medical history not available in the survey.

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[6]UKMOD is a post-Brexit offshoot of the EUROMOD microsimulation model, customized for the UK context. It includes several improvements but remains largely identical in structure and function. See Richiardi, Collado and Popova (2021) for details.

On the tax side, UKMOD fully simulates personal income taxes and social insurance contributions but excludes capital and wealth taxes, which cannot be modeled from available data (though the local housing tax is included). These omissions are mostly excluded from simulated disposable incomes entirely, neither affecting the policy-induced component nor recorded total disposable income, and thus is not expected to bias comparisons between them.[7]

Finally, the model calculates entitlements and liabilities rather than actual take-up or compliance. This is similar to many studies in the policy feedback literature, that model benefit eligibility instead of benefit receipt. Although very similar, I therefore technically estimate intent-to-treat effects of policy: what voters stand to gain or lose under the law, rather than what they actually receive or pay. This aligns with the perspective of the incumbent, who controls the rules of eligibility but not their execution. Nonetheless, because take-up gaps are mostly an issue for small entitlements, I conduct robustness checks excluding individuals eligible for minimal benefits (Bronka, Popova and Richiardi 2023).

To validate that the model's estimated disposable incomes reflect respondents' subjective experiences, I use a survey item from the UKHLS tapping respondents' evaluations of their current financial situation: "How well would you say you yourself are managing financially these days?" with five options ranging from "living comfortably" to "finding it very difficult". Do these subjective evaluations track disposable incomes as calculated by UKMOD? To validate the linked data, Figure 2 plots the relationship between changes in subjective pocketbook evaluations (horizontal axis) as measured in the UKHLS and actual disposable income changes as calculated by UKMOD. As shown, there is a tight relationship between calculated income changes and subjective evaluations. Respondents reporting the same pocketbook evaluation two years in a row see a near-zero change in their disposable income, while each step change on the scale is associated with a substantial and consistent change in disposable income. In a linear model, a one-point increase on the scale is associated with a precisely estimated change in income of 68 £/month on average

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[7]A small number of non-simulated taxes and transfers are included in total disposable income but not in policy-induced income, meaning that they do not allow for counterfactual modeling. They are, however, marginal and their reported values closely track official statistics (Bronka, Popova and Richiardi 2023, p. 13).
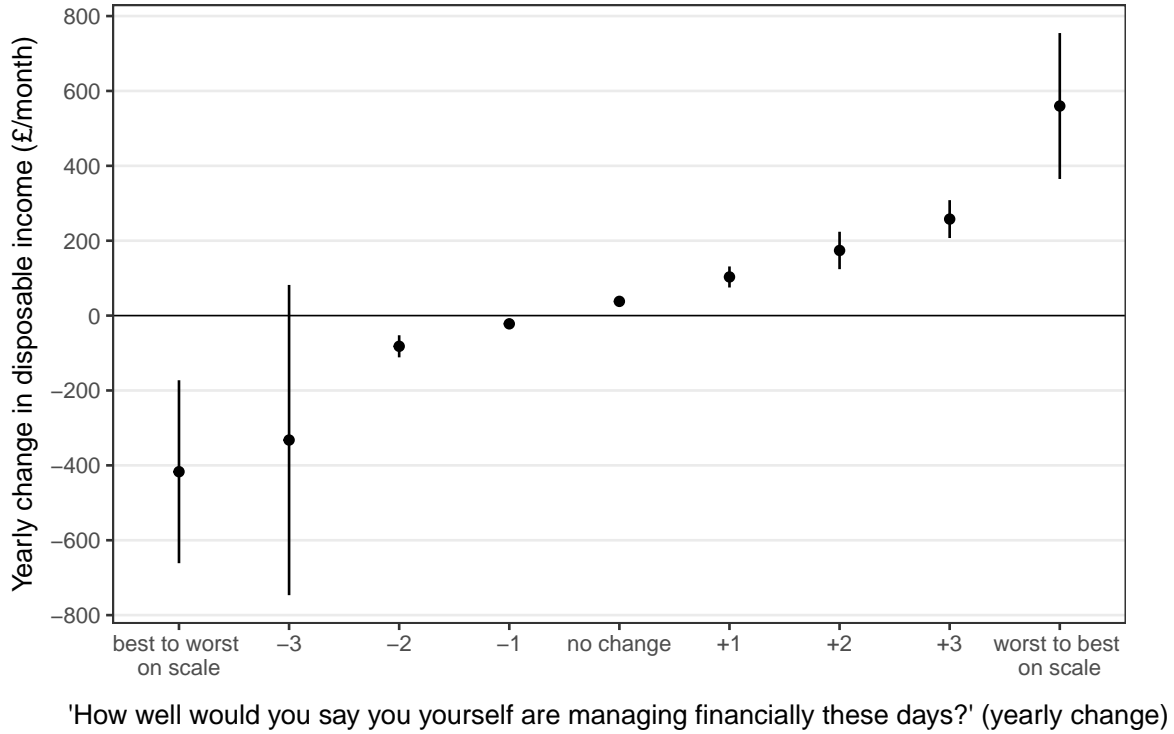
Figure 2: Average change in total disposable income by change in subjective financial evaluations across respondent-year observations.

($SE = 6.07$, $p < 0.001$). This provides reassuring validation that UKMOD's calculated disposable incomes mirror respondents' subjective experience.

Using UKMOD to calculate policy-induced income changes

The next step is the decomposition of calculated disposable income changes. As shown previously, UKMOD calculates disposable incomes by applying a policy system from a given year, s_t, to a sample of individuals with characteristics, c_{it}, to compute their disposable incomes, y_{it}. Given this model, the total change in i's disposable income between an initial period $t = 0$ and a final period $t = 1$ can be characterized as:

$$\Delta y_{i,0\rightarrow 1}^{total} = s_1(c_{i,1}) - s_0(c_{i,0})$$

17

The total disposable income change is thus the product of two simultaneous changes: changes in the policy system, $s_0 \rightarrow s_1$, and changes in the individual's market incomes and demographic characteristics, $c_{i0} \rightarrow c_{i1}$. These changes are confounded in observational data, as one cannot observe i's counterfactual income had the policy system, or their personal characteristics, not changed.

To calculate the effect of the change in s, holding c constant, I proceed in two steps following Bargain and Callan (2010)'s decomposition approach. I first use UKMOD to calculate voters' counterfactual disposable income had there been no change in policy, i.e. $s_0(c_{i,1})$.[8] This is equivalent to i's disposable income in period $t = 1$ (with their market incomes and characteristics from this period, i.e. $c_{i,1}$) under the policy system of period $t = 0$ (i.e. s_0). In the second step, I calculate the difference between this counterfactual disposable income and their actual disposable income in period $t = 1$. This difference is the net effect of policy changes on their disposable income from $t = 0$ to $t = 1$. Consequently, the difference between the counterfactual income and their actual disposable income at $t = 0$ is the 'residual' income change that is caused by changes in their market incomes and other characteristics, holding the policy system constant (Bargain and Callan 2010). Formally, this decomposition can therefore be written as[9]:

$$\Delta y_{i,0\rightarrow 1}^{total} = \underbrace{\{s_1(c_{i,1}) - s_0(c_{i,1})\}}_{\Delta y_{i,0\rightarrow 1}^{policy}} + \underbrace{\{s_0(c_{i,1}) - s_0(c_{i,0})\}}_{\Delta y_{i,0\rightarrow 1}^{residual}}$$

To ensure that market incomes in period 1 are nominally comparable to the period 0 policy parameters, I apply a set of uprating factors that adjust monetary variables to the year of the policy system applied.[10] Uprating factors are likewise applied to adjust $s_1(c_{i,1})$ to nominal levels of pe-

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[8]Mathematically, the policy-induced income change could also be decomposed with respect to the counterfactual quantity $s_1(c_0)$. However, psychologically, this would not make much sense because the policy-induced change in that case, i.e. $s_1(c_0) - s_0(c_0)$, would correspond to a forward-looking effect of yet unrealized policy changes.

[9]This is a slight simplification of the decomposition in Bargain and Callan (2010) which includes uprating parameters and an inflation component, which they call "change in nominal levels" (p. 6) because they decompose nominal income changes. Since I am just interested in decomposing real income changes, I partial the inflation component out here for simplicity by applying an uprate factor that nominally adjusts market incomes to the year of the policy system applied.

[10]Note that the uprate factor is an estimate and not necessarily the actual set of nominal parameters as decided

riod 0 to ensure that y_i^{policy} does not capture the change in nominal levels between s_1 and s_0 due to inflation.[11] As a result, all estimated income changes are in real terms.[12]

A key feature of this decomposition is that not all changes in what citizens receive in transfers or pay in taxes count as policy-induced, only changes that result from a change in policy. Conceptually, as in the model, income changes caused by changes in personal characteristics, e.g., moving from one tax bracket to another or losing eligibility for a benefit due to a change in personal circumstances are not policy-induced although they are policy-related. In additional analyses, I nonetheless test whether voters respond more crudely to such overall changes in what they pay in taxes or receive in benefits.

Finally, I implement the decomposition with respect to *yearly* changes in disposable income. This has two advantages. The UK fiscal policy system is a largely unified, national system and changes to taxes and benefits are generally implemented once a year, in April, and announced only a couple of weeks before (with a few exceptions, see Appendix B for an overview) (Reis and Tasseva 2020). UKMOD tracks these fiscal years such that UKMOD's 2010 policy system was in force from April 2010 to March 2011 and so on (Reis and Tasseva 2020). Since changes to the system are discrete rather than continuous in time, survey panel respondents can be divided cleanly into fiscal year windows within which everyone was subject to the same policy system no matter the exact timing of their interview (see Appendix C). In addition, yearly income changes correspond

by the government. Following Bargain and Callan (2010)'s notation, if α^t is the chosen uprate factor for time t, and p^t is the set of nominal policy parameters for time t, $\alpha^1 p^0 \neq p^1$. There are many ways for governments to uprate tax-benefit parameters, but here I use price indexation, which is a neutral choice that Bargain and Callan (2010) also recommend. Either way, uprating is unlikely to make a substantial difference when intervals are as small as they are here (i.e. year-on-year) (Bargain and Callan 2010).

[11]This is what Bargain and Callan (2010) refers to as "base-weighting". It is much simpler but empirically similar to a full Shorrocks-Shapley decomposition that uses a combination of base- and end-weighting, as Bargain and Callan (2010) show.

[12]An advantage of this strategy is that the policy-induced component also captures a specific kind of "policy drift" related to inflation (Hacker 2004). As I uprate the income data by CPI, it will record policy parameters that do not follow the inflation rate as policy changes, like policies that freeze nominal benefit amounts or let benefit brackets rise more slowly than inflation ("bracket creep" (Paulus and Tasseva 2020)). As such, the model captures not only wholesale policy changes but also decisions to change policy parameters that governments can use to more subtly erode the real value of benefits.

closely to the time-frame on which pocketbook voting is usually studied and assumed to operate, namely with survey items asking respondents how their current financial situation compares to their situation 12 months ago.

The case of the UK in the 2010's

I study policy-sensitivity in economic voting in the United Kingdom from 2010-2019. This is the maximum window for which UKMOD could be linked with the UKHLS. It is also a good case for studying economic voting in general and the policy-sensitivity of pocketbook voting in particular. Economic voting in the UK has been intensely studied and an important recent study on pocketbook voting uses the same UKHLS survey data that I use here although for a different period (Tilley, Neundorf and Hobolt 2018). I can therefore estimate the policy-sensitivity of pocketbook voting in a context where pocketbook voting itself is well-established with the same survey data (Lewis-Beck, Nadeau and Elias 2008; Lewis-Beck, Nadeau and Foucault 2013). Further, the UK's fiscal policy system is relatively strongly redistributive (Hasell 2023) with substantial variation during the period of study (Elkjær and Mushövel 2023).

Further, the UK in the 2010's is a highly likely case for policy-based pocketbook voting to occur because of the clarity of responsibility for economic outcomes and substantial policy variation. Firstly, the UK represents a context where attribution of economic outcomes to government policy should be relatively straightforward. The Westminster parliamentary system typically produces single-party majority governments with concentrated executive power, creating clear lines of accountability for economic policy. Unlike federal systems or those with coalition governments, the UK features a unified national fiscal system where responsibility for taxes and benefits rests unambiguously with the central government. The governing party exercises strong control over both the legislative agenda and policy implementation, with limited institutional veto points that might disperse or obscure responsibility. During the period under study, these institutional features were largely intact. Apart from the 2010-2015 coalition with the Liberal Democrats, the Conservatives held sole power and implemented their fiscal agenda with minimal institutional constraints. This high clarity of responsibility context should, in theory, facilitate voters' ability to correctly attribute

income changes to government policy (Duch and Stevenson 2008; Powell and Whitten 1993).[13]

Second, the UK from 2010-2019 saw particularly high variation in policy stemming in part from the infamous austerity programs of successive Conservative governments. The benefit system in particular saw a large overhaul with the 'universal credit' scheme replacing six existing benefit schemes (including the Income Support, Tax Credits, the Housing Benefit, and the Job-seekers Allowance) in four phases from 2013-2017. This led to large shifts in who benefited from government programs, affecting an estimated 8 million households – or roughly one in three – of which 3.1 million households were on net entitled to more benefits and some 2.8 million entitled to less (BBC 2013). Importantly, despite the austerity policies reducing spending overall, roughly equal numbers of citizens thus saw decreases and increases in benefits. Taxes also changed substantially during this period, with income tax rates and brackets going up and down several times. The cutoff for the highest income tax decreased until 2015 before increasing towards 2020. The top marginal tax rate was lowered significantly by 10 points fom 2010-2013 while the personal allowance nearly doubled over the entire period. In sum, this period saw policy-induced income variation of an unusual magnitude. The high public salience of these policy changes should, all else equal, make policy-induced income changes more 'traceable' and thus make attribution easier (Hamel 2024).

Variables and estimation

Vote choice The main analysis estimates how voters sanction incumbents for disposable income changes depending on whether they are policy-induced or not. To measure incumbent sanctions, I use a survey measure of vote intention for the government party from the UKHLS. This is measured in a sequence of three items. The first asks whether the respondent supports a particular party, and if so which one. If they answer 'no', they are asked whether they feel closer to one party than others, and if so which one. If they again answer 'no', they are asked which party they would vote for if there were an election tomorrow. Following Tilley, Neundorf and Hobolt (2018), the

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[13]I exclude the coalition years in robustness checks to limit the analysis to periods where governmental responsibility was clearest.

incumbent support variable is then coded as an indicator for whether an incumbent party was mentioned across these three questions without differentiating them. Thus, any remaining 'none' or 'don't know' answers to the vote intention question are coded as *not* supporting the incumbent party. The resulting variable is widely used to measure vote intention in the British case and is strongly correlated with actual vote choice in election years (Tilley, Neundorf and Hobolt 2018).

Income variables I measure three types of disposable income variation: total income changes, policy-induced income changes, and residual income changes. All encode the individual-level income change from the last time the respondent took the survey – roughly a year prior – to the time of the interview. I use two versions of each. To account for the skewness of the income change data, the main version of the income change variables uses the inverse hyperbolic sine transformation. While an ordinary logarithmic transformation is useful for income levels, it is problematic for income changes which can be both positive, zero and negative. The inverse hyperbolic sine is a good alternative in such cases, as it approximates the natural logarithm across positive and negative values, allows for retaining zero-valued observations, and offers a similar interpretation in regressions (Bellemare and Wichman 2020; McKenzie 2023).

In addition, I use simple raw disposable income changes scaled to monthly income in £1,000 increments. A unit change in these variables is equivalent to a change in monthly disposable income of £1,000, or a change of £12,000 in yearly disposable income.

Estimation and identification To examine whether pocketbook voting is policy-based or outcome-based, I estimate a series of regressions of incumbent vote intention on decomposed income changes. Recall that the income variables capture changes in disposable income from last year's level to the current level. A classical pocketbook voting model would thus regress vote intention on total income change to estimate how incumbent support depends on recent changes in total income.

To test attribution, I therefore run two versions of this standard model: regressing vote intention on total income changes, and regressing vote intention on both total and policy-induced income changes (see Appendix E for model specifications). This approach is equivalent to in-

cluding the policy-induced and the residual components (Wooldridge 2016, p. 126), but is more easily interpretable (I report results for these alternative specifications in an appendix). This offers a direct comparison between the competing models of interest: Fiorina's outcome-based pocketbook voting driven by total income changes and Kramer's policy-induced pocketbook voting driven solely by policy-induced income changes. If the electorate is characterized by Kramer's perfect attribution, we would expect the coefficient on policy-induced changes to be positive and the coefficient on total income changes to be zero in a model including both variables. If the electorate is instead characterized by Fiorina's indiscriminate pocketbook voting, the coefficient on policy-induced changes should be similar to – and at least no greater than – the coefficient on total changes. In reality, attribution may be somewhere in between with different weights put on both.

The notion of incumbent sanctioning that I am testing is inherently causal: the question is how voters respond to different kinds of income changes. While the micro-simulation model captures the causal effects of policy on disposable incomes by construction, it does not guarantee causal identification of voters' responses to these income changes. There is a risk of reverse causality as governments may pad the pocketbooks of pre-existing supporters, and other factors correlated with vote choice may predict income changes. Following other recent research on pocketbook voting (Tilley, Neundorf and Hobolt 2018), I account for these time-varying sources of endogeneity using lagged dependent variable models with a set of controls. By controlling for lagged party choice, I mitigate reverse causality caused by endogenous policy targeting. To account for other economic predictors of income change, I further include fixed effects for economic status, dummies indicating job loss or gain, gender and age, as well as lagged disposable income and lagged pocketbook evaluations, although it is reassuring that these controls make little substantive difference to the results (see Appendix G). The resulting coefficient on policy-induced income change can thus be interpreted straightforwardly as the average difference in incumbent support between voters who experienced policy-induced income changes differing by one unit, holding constant their total income change as well as lagged party choice, economic status, demographics and lagged subjective financial evaluations. I further estimate a series of alternative specifications for robustness includ-

ing two-way fixed effects models and models that control for lagged policy-induced and residual income changes to account for temporal dependence. See Appendix E for all model specifications.

To account for the fact that policy-eligibility is partly determined at the household level, I cluster standard errors at the level of households (Abadie et al. 2023).[14] Note that I estimate the regressions at the level of household income changes although estimating them at the individual level generally makes no substantive difference to results (as shown in Appendix G).

Results

I now turn to results. Before presenting model estimates, I descriptively characterize the composition of disposable income changes in terms of the policy-induced and residual components. This in turn matters for the degree of attribution needed for pocketbook voting to be policy-sensitive.

Shares of policy-induced and residual changes in voters' pocketbooks

The first question of interest is descriptive: how much pocketbook variation is generally attributable to policy changes? To answer this, I first consider overall variation in the two decomposed income change variables, namely respondent-year disposable income changes that are policy-induced or not. Figure 3 shows these distributions, truncated at +/- 600 £/month. Unsurprisingly, there is far more residual variation ($SD = 2518$ £/month) than variation from policy ($SD = 1376$ £/month). When ordered by absolute magnitude, i.e. ignoring the sign of the change, the median residual pocketbook change is 172 £/month while the median policy-induced change is just 26 £/month. Both distributions have a numerically similar left-skew, consistent with a growing national economy throughout the period. Importantly for the subsequent analyses, the two components are virtually uncorrelated at the individual-wave level with Pearson correlation coefficient of -0.02, mitigating concerns about multicollinearity.

Just how large is the residual across voters on average? Recall the simulations in Figure 1 showing the importance of the residual for policy-sensitivity: the larger the residual relative to

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[14]UKHLS only provides unique household identifiers within and not across waves. I therefore construct longitudinal household identifiers based on respondents' first wave in the panel, if they are already in a household, or the first wave in which they joined an existing household.
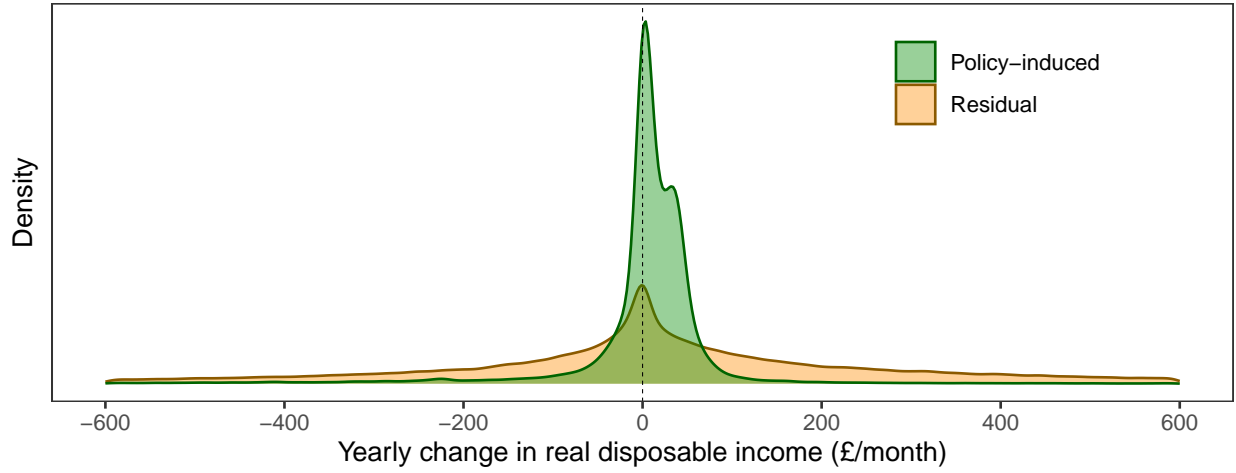
Figure 3: Histogram of the distributions of policy-induced and residual disposable income changes at the respondent-year level in the UK from 2010-2019. Truncated at +/- 600 £/month.

policy-induced income variation, the more correct attributions voters need to make congruent incumbent sanctions. Figure 4 shows the relative share of policy-induced income changes across respondent-year observations, i.e. the magnitude of policy-induced variation as a share of total (policy-induced plus residual) variation.[15] As shown, the median share of policy-induced income change is just 16% (mean = 29%) putting it in the lower end of the scale in Figure 1. In just around 24% of observed total income changes is the policy-induced change larger than the residual change.

With a residual of this size, attribution is almost a necessity for voters to reliably deliver congruent sanctions. To see this, consider how often voters, given the observed distributions of policy-induced and residual income changes, would congruently sanction the incumbent under Fiorina-style outcome-based pocketbook voting. To do this, I compute how often positive total income changes coincide with positive policy-induced income changes, and vice versa (see Appendix F). Given the observed income changes, classical outcome-based pocketbook voting would lead voters to incongruently punish the incumbent 23% of the time and incongruently reward the incumbent

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[15]I measure the relative share of policy-induced variation in voters' pocketbooks descriptively at the respondent-wave level by summing the absolute value of each component and calculating the policy-induced change as a percentage of this total: $\frac{|\Delta y_{it}^{policy}|}{|\Delta y_{it}^{policy}|+|\Delta y_{it}^{residual}|}$. The numerator is the absolute policy-induced change in disposable income and the denominator is the sum of this absolute change and the absolute residual income change.

19% of the time. In total, voters' sanctions would be congruent in just 58% of all cases, performing just marginally better than chance.
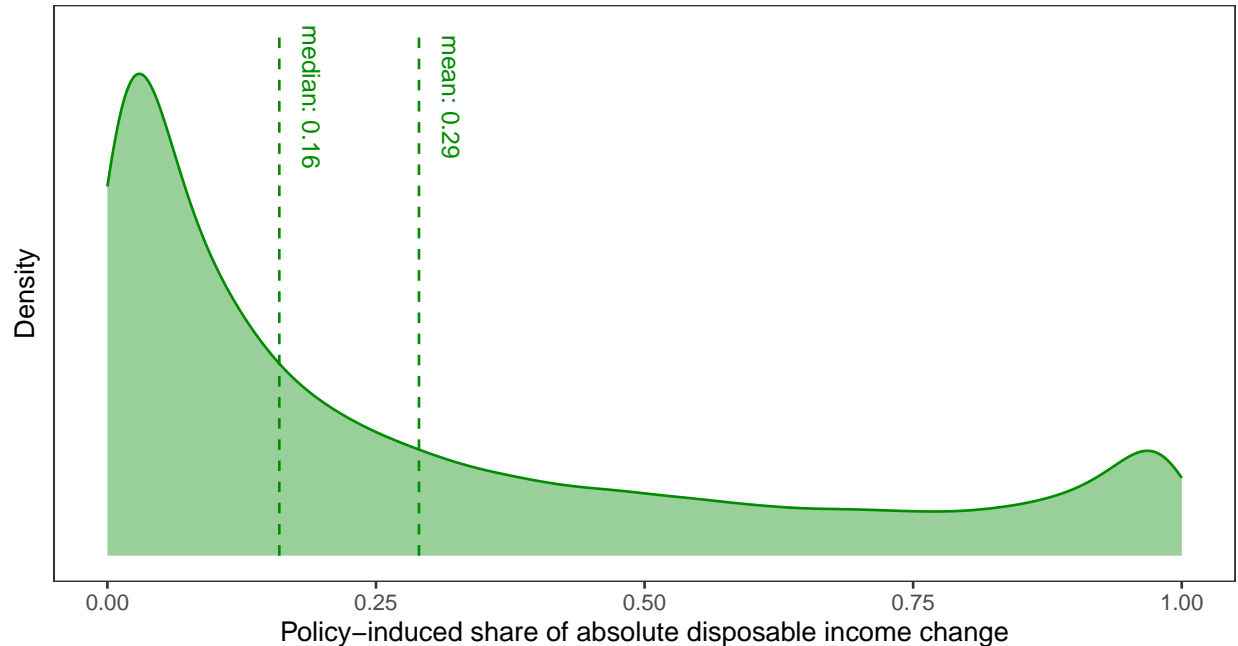

Figure 4: Shares of policy-induced income changes at respondent-year level, as proportion of absolute disposable income change.

In sum, citizens' disposable incomes vary substantially from year to year, and for most people, only a modest share of this variation is caused directly by policy changes. Importantly, these results likely represent a high watermark for policy-induced income changes due to the large changes to the UK fiscal system during the decade under study. The key takeaway is that total disposable income changes are not a reliable indicator for the direct effects of income policies. In turn, Fiorina-style pocketbook voting does not consistently sanction incumbents for the direct income effects of their policies. To be sufficiently policy-sensitive, voters must therefore respond more strongly to policy-induced income changes than other changes. The next section examines whether they do.
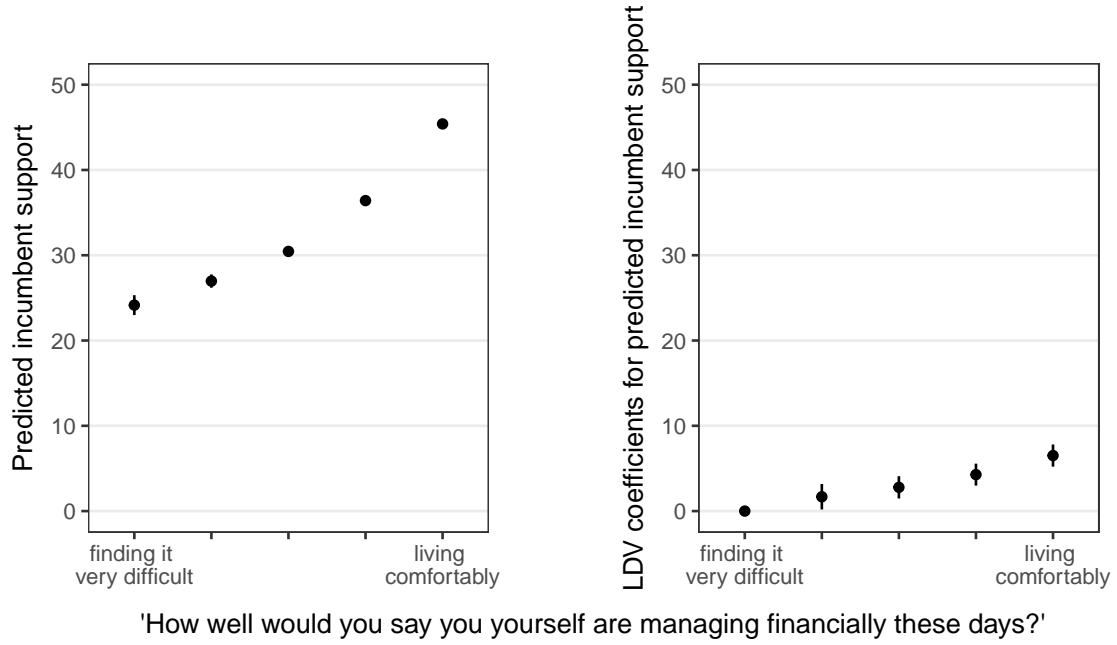
Figure 5: Regressions of lagged dependent variable models of incumbent support (0-100) with household-level clustered errors. Pocketbook evaluations are rescaled to a 0-100 scale. All income changes are yearly changes in monthly disposable household income (£), in real terms.

How policy-induced pocketbook changes affect incumbent support

Next, I turn to model estimates. Before presenting the decomposed pocketbook voting models, I first validate that the basic pocketbook voting relationship holds in my data. Figure 5 shows the relationship between pocketbook evaluations and incumbent support that is typically reported in studies on pocketbook voting. Here, I use a lagged dependent variable specification similar to that in Equation 4. More rarely, existing studies estimate the relationship between actual income changes and incumbent support; I also show this alongside the main results in Table 1 and more extensively in Appendix D. Both analyses demonstrate that the basic relationships hold in my data, with voters sanctioning incumbents for actual and perceived changes in their financial situation.

The critical question is whether this relationship reflects genuine policy attribution. If voters are more sensitive to policy-induced changes than to other income changes, we should observe a positive coefficient on Δy^{policy} that exceeds the coefficient on Δy^{total}. I test this by regressing

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Table 1: Effects of policy-induced and total disposable income changes on incumbent support.

	Income changes (£1000/mo)		Income changes (asinh)	
	Total	Decomposed	Total	Decomposed
Δy^{total}	0.15 (0.06)**	0.15 (0.06)**	0.08 (0.03)**	0.08 (0.03)**
Δy^{policy}		−0.27 (0.07)***		−0.05 (0.03)+
N	70798	70798	70798	70798
Std.Errors	household	household	household	household
Lagged DV	✓	✓	✓	✓
Controls	✓	✓	✓	✓

Note. Regression table of lagged dependent variable models with household-level clustered errors. The outcome is incumbent support (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms. * p<.05, ** p<.01, *** p<.001.

incumbent support on decomposed disposable income changes using lagged dependent variable models.

Table 1 shows the results. As shown, there is little sign of policy attribution in pocketbook voting. The coefficient on total income changes is consistently positive and significant across specifications, barely changing when policy-induced changes are included separately. However, the coefficient on policy-induced income changes is never positive and even reaches statistical significance with a negative sign in one specification.

Following Rainey (2014), I calculate 90% confidence intervals for the coefficients on policy-induced income changes to test whether I can rule out negligible positive effects. The intervals confirm that I can exclude any meaningful positive responsiveness to policy-induced income changes in these models[16]. These results prove robust across various alternative specifications, including two-way fixed effects instead of lagged dependent variables, adding wave fixed effects to the main models, removing controls, including dummies for extreme income changes, using quadratic terms, including lagged income change variables, excluding the highest earners and individuals

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[16]The 90% confidence intervals are [-0.389, -0.153] for the model in column 2 and [-0.094, -0.004] for the model in column 4.

eligible for very small benefits, and using individual-level rather than household-level income changes (see Appendix G). I further find no significant differences in these relationships by the party in power (see Appendix H. In substantive terms, these results imply that voters do not reward incumbents more for policy-driven income gains than for income gains from other sources.

The salience of non-policy income changes

Voters are not only equally responsive to the two income components, as in Fiorina's model. If anything, they are even *less* responsive to policy-induced income changes than other income changes (see also coefficients on residual income changes in Appendix G). What might explain this? One possibility is that non-policy changes are simply more psychologically salient, dominating voters' overall assessments of their financial wellbeing despite potentially caring more about policy effects in principle. To test this, I examine how the two types of income changes affect subjective financial evaluations, i.e. voters' own changing assessments of their personal finances. This outcome is measured as the difference in reported financial wellbeing between the current wave and the last wave, i.e. over the duration of the measured income change. Here, I include policy-induced and residual income changes, rather than total income changes, to directly compare the psychological salience of the two types of income variation (the model specifications are otherwise identical). If the attribution failure reflects differential salience rather than indifference to policy effects, we should see residual income changes having larger effects on subjective evaluations.

The results shown in Table 2 support this interpretation. Voters' subjective pocketbook evaluations respond significantly more to residual than policy-induced income changes. While policy-induced income changes do show a positive coefficient in one model, its magnitude is less than one-third that on residual changes. This suggests that residual changes, for whatever reason, are more psychologically salient in voters' evaluations of their financial wellbeing, potentially explaining why they drive voting behavior despite being less directly tied to policy.

Still, one puzzling feature of the results is that the point estimate on policy-induced income changes is negative, reaching statistical significance in one model. This suggests voters may actually be more likely to punish than reward the incumbent for policy-induced income *increase*

Table 2: Regressions of pocketbook evaluations on decomposed disposable income changes.

	Income changes (£1000/mo)	Income changes (asinh)
	LDV	LDV
Δy^{policy}	-0.02 (0.04)	0.08 (0.02)***
$\Delta y^{residual}$	0.14 (0.06)*	0.28 (0.02)***
N	85 903	85 903
Std.Errors	household	household
Lagged DV	✓	✓
Controls	✓	✓

Note. Regression table of lagged dependent variable models with household-level clustered errors. The outcome is pocketbook evaluations (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms, split by their causal origin. * $p<.05$, ** $p<.01$, *** $p<.001$.

and vice versa. This pattern is unlikely to result from unobserved correlates of policy-induced income changes. Besides the lagged dependent variables (and fixed effects in robustness checks), the models control for various time-varying confounders including economic status, job loss and gain, lagged disposable income, and lagged pocketbook evaluations. Why, then, might voters respond negatively to policies resulting in them either receiving or keeping more income from the state? As discussed earlier, it is not inconceivable that voters punish incumbents for increasing what they might perceive as their reliance on welfare. It could also be a backlash to perceptions of insufficient income compensation, e.g., from the repercussions of the financial crisis. Still, this negative coefficient is not sufficiently robust to warrant any firm conclusions.

Examining more crude forms of attribution

Overall, these results suggest voters are not particularly apt at distinguishing and responding to income changes from policy. Yet, even if voters cannot identify the specific causal mechanisms behind income changes, they might engage in cruder forms of attribution based on the *source* of income changes. Instead of distinguishing income changes caused by changes in policy, they may just detect changes in the amounts they receive in transfers or pay in taxes. For instance, they

might reward incumbents when their tax burden decreases or benefit payments increase, regardless of whether these changes stem from policy reforms or changes in personal circumstances (like moving tax brackets or becoming eligible for existing benefits). Such crude attribution would represent a more pragmatic, if imperfect, approach to disciplining incumbents and would suggest at least some recognition of government involvement in their finances.

If voters engage in such crude attribution driven by the source of income change rather than its cause, we would expect voters to respond more strongly to income changes due to changes in what they pay in taxes (Δy^{taxes}) or receive in transfers ($\Delta y^{transfers}$) compared to changes in market incomes (Δy^{market}). To examine this, I estimate models of incumbent support on income changes decomposed into these three types, using the main specification from before. Table 3 shows the results.[17]

As shown, there is little evidence of even crude attribution. Voters respond consistently to market income changes while showing no systematically stronger response to tax or transfer-related changes. The coefficients on tax and transfer changes are positive but neither larger nor robustly statistically significant. This is consistent with a relatively even responsiveness to income changes regardless of their source. Thus, voters treat a pay rise and a tax cut equivalently when evaluating incumbents with seemingly little recognition of the government's differential role in these income sources.

To further probe the main result, Appendix I reports heterogeneous effects by age, political interest, lagged party support, respondent-level average disposable income, economic status, and the valence of policy-induced income change. The lack of attribution generalizes across all subgroups, with no group apearing to respond more strongly to policy-induced pocketbook changes. This suggests that the main result reflects a general lack of attribution rather than mixed responsiveness among subgroups canceling out in the electorate-level effect. Notably, even highly politically interested voters, who are most likely to follow policy developments, show no greater

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[17]Note that to facilitate interpretation, the coefficients on 'taxes' are flipped such that a positive unit change is an increase in disposable income due to taxes (i.e. a *reduction* in taxes paid).

Table 3: Regressions of incumbent support on disposable income changes, by income type.

	Income changes (£1000/mo)	Income changes (asinh)
	LDV	LDV
Δy^{market}	0.59 (0.26)*	0.08 (0.04)*
$\Delta y^{transfers}$	0.06 (0.16)	0.09 (0.04)*
Δy^{taxes}	1.18 (0.57)*	0.01 (0.05)
N	74 714	74 714
Std.Errors	household	household
Lagged DV	✓	✓
Controls	✓	✓

Note. Regression table of lagged dependent variable models with household-level clustered errors. The outcome is incumbent support (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms, split by their type. * $p<.05$, ** $p<.01$, *** $p<.001$.

policy-sensitivity to policy-induced effects. This strengthens confidence that the results reflect fundamental attribution challenges rather than simple inattention.

Discussion and Conclusion

A prominent model of pocketbook accountability holds that by sanctioning incumbents for personal wellbeing changes, voters can effectively hold them accountable for their economic performance (Fiorina 1978; Key 1966; Kramer 1983). Recent work suggests that pocketbook voting may be even stronger than previously thought, making personal financial well-being a key driver of electoral outcomes (Bechtel and Liesch 2020; Healy, Persson and Snowberg 2017; Tilley, Neundorf and Hobolt 2018). The model, however, depends on an assumption of what I term 'policy-sensitivity': that voters, by voting with their pocketbooks, reliably respond to the effects of incumbent policy. If pocketbook voting is not policy-sensitive, and voters instead vote on the basis of total income changes, it does not provide reliable sanctions for incumbent behavior and in turn fails to hold incumbents accountable.

In this paper, I have argued that the assumption of policy-sensitivity is unlikely to hold. By

using microsimulation models to decompose income changes into policy-induced and residual components, I provide a novel method for estimating how reliably voters distinguish direct income effects of policy from other income fluctuations. This approach offers a unique opportunity to estimate how well voters sanction incumbents for direct policy effects *on average*, moving beyond single-case studies of isolated policies to assess the broader relationship between income-related policy and voter behavior.

I find that the overall relationship between policy-induced income changes and voter behavior is weak. For most voters, the impact of policy changes on disposable income is heavily over-shadowed by other sources of income variation, with only a small fraction of income changes directly attributable to changes in policy. Furthermore, voters generally fail to accurately connect income changes to the policies that caused them, with little evidence of reliable attribution across the electorate. Subgroup analyses show that this lack of policy-sensitivity holds consistently across different demographic groups, even for the highly politically interested. Consistent with Fiorina's classical pocketbook voting model, these results suggest that voters reward and punish incumbents for total income changes – regardless of their relation to policy – rather than for the policy-induced income changes that would more effectively motivate incumbents.

Some limitations to the analysis remain. First, I focus on policy changes that have direct effects on disposable incomes. Yet, electoral accountability, broadly speaking, is about more than *direct* income effects, with e.g. the indirect effects of trade policy, industrial policy, and public goods provision notably being outside the scope of my analysis. This is partially driven by the fact that the pocketbook effects of such policies is fraught with uncertainty. More importantly, voters are unlikely to perform much better in the domain of indirect income effects. As such, my focus on direct income effects is a more likely case to observe attribution. Still, I cannot firmly conclude that my results generalize to other policy domains.

Second, I exploit a unique opportunity to link survey measures of incumbent support and microsimulated income changes in present-day UK. In many ways, this happens to be a good case to study policy-sensitivity of pocketbook voting given the dramatic transformations of the UK's

fiscal system at the time, generating lots of useful variation in the independent variables. Still, it naturally raises the question of how my findings generalize to other contexts. I provide evidence that my findings are fairly robust across subgroups in the electorate, suggesting that the lack of attribution is pervasive and not a feature of the composition of the British electorate. I further find no indication that attribution was any different under the Labour government. Still, I mostly study a period of welfare state retrenchment under successive center-right governments, which may be special in other ways although, given the extent to which the government's 'austerity' policy program was politicized, I would not expect this to be a case of *lower* public responsiveness to policy effects. It is important to note, however, that my results are not inconsistent with negative electoral responses to austerity policies as such. The UK Conservatives' austerity policies encompassed cuts to various public services and through fiscal multipliers in local economies may have affected voting behavior via their effects on social groups or communities rather than individual pocketbooks (Fetzer 2019; Gaikwad, Genovese and Tingley 2022). More research should examine such group-level retrospective voting behavior.

Third, this paper has focused on estimating the overall relationship between income policy and pocketbook behavior and only secondarily on psychological mechanisms. To understand exactly how and when voters attribute income changes differently, narrower case studies and experiments are typically more useful. Existing work already provides important insights on this, and my results are not inconsistent with any of these psychological theories *per se*. My results suggest that the lack of attribution is in some part due to non-policy income changes being more psychologically salient in voters' summary evaluations, for whatever reason. Occasional backlash to perceived increased state dependence or to perceived insufficient compensation for macro-level shocks may also play a role. Future studies should examine these questions further. Nonetheless, if I had a richer survey dataset than the UKHLS available for linking with UKMOD, it would have allowed me to get more definitive evidence for the psychological mechanisms at play.

These caveats notwithstanding, my results have several important implications. First and foremost, they extend and cast new light on existing empirical work on pocketbook attribution. Several

studies suggest that citizens seem more responsive to financial changes that they subjectively attribute to government action (Abramowitz, Lanoue and Ramesh 1988; Feldman 1982; Lau and Sears 1981; Marsh and Tilley 2010; Tilley, Neundorf and Hobolt 2018). Likewise, research on policy feedback has in some cases found voters to respond to economic policies that affect them (Kogan 2021; Manacorda, Miguel and Vigorito 2011; Pop-Eleches 2012; Rendleman and Yoder 2024; Zucco Jr. 2013). Despite this, I find that voters do not *on average* sanction incumbents for policy-induced income changes. This suggests that although voters evidently try to get attribution right, and sometimes do manage to get it right, they generally get it wrong. When voters occasionally get it right, it is not because they are good at attribution, but rather because their "rough justice" (Fiorina 1981, p.4) happens to deliver congruent sanctions in rare cases where the policy 'signal' is perhaps sufficiently strong or where the policy change is sufficiently publicized by the government. These isolated successes, while important for understanding specific policies, thus mask a broader pattern of attribution failure.

Second, the study demonstrates the value of linking microsimulated disposable incomes to individual-level attitudes. Rarely used in political science (Avram and Popova 2022; Elkjær and Mushövel 2023), these models offer a uniquely detailed account of how policy changes shape voters' financial situations while covering wholesale changes to the fiscal system. By leveraging them to estimate the net impact of an incumbent's full policy record, this paper provides a novel approach to studying pocketbook voting and policy feedback. Future research can build on this approach to analyze electoral reactions to specific policy changes, shedding further light on how voters respond to specific types of pocketbook changes.

Finally, by estimating the extent of policy-sensitivity in pocketbook voting, this study speaks to broader debates on electoral politics and democratic accountability. One key implication concerns changes in party coalitions. If voters are largely unresponsive to policy-induced income changes, parties may struggle to secure electoral support simply by enacting material benefits. This casts doubt on the effectiveness of strategies like the US Democratic party's so-called 'deliverism' approach in the 2024 Presidential Election, which sought to win voters organically by delivering tan-

gible material gains (Bhargava, Shams and Hanbury 2024). My findings suggest that its apparent failure is less an anomaly than a reflection of a broader pattern: in a world of low policy-sensitivity, material benefits do not automatically translate into reliable political rewards. This has important implications for how parties approach governance and campaigning. Rather than assuming that good policy outcomes will speak for themselves, parties may need to invest heavily in communication strategies that make policy effects visible and attributable. Alternatively, they might focus on highly salient, easily attributable policies even if these are not the most economically efficient approaches.

Low policy-sensitivity also has implications for strategic incumbent behavior. Pocketbook voting plays a central role in theories of the political economy of elections, where it is often seen as a driver of economic mismanagement and weakened democratic control (Ferejohn 1986; Maskin and Tirole 2019; Tufte 1978). Compared to sociotropic voting, which rewards overall economic performance, pocketbook voting alters incumbent incentives by making electoral support more contingent on the targeted redistribution of resources rather than fostering broad-based growth (Battaglini and Coate 2007, 2008; Maskin and Tirole 2019). While such tactical redistribution or 'pork-barrel' politics is often thought to undermine accountability (Ferejohn 1986; Lizzeri and Persico 2005; Tufte 1978) it is unlikely to be effective if voters are largely indifferent to policy-induced income changes. This does not eliminate all electoral incentives for redistributive policies, but it fundamentally alters them. Rather than facing direct electoral rewards for beneficial policies, incumbents may primarily fear the political costs of highly visible policy debates themselves. To the extent that incumbents fear increasing taxes or reducing benefits – which they seem to do (Fuest et al. 2024) – it may be not the retrospective sanctioning but the political costs of highly visible policy debates and the opportunity they bring for opposition mobilization (Fastenrath and Marx 2025). In addition, it is possible that voters may respond more to community- or group-level economic conditions than their personal economic conditions, which could generate similar tactical incentives to policy-based pocketbook voting (Dausgaard forthcoming; Drazen and Eslava 2006). Future theoretical work should examine the incumbent incentives that arise from policy-

insensitive pocketbook voting with modest policy-induced income variation. In particular, it might mean that incumbents can expect the highest electoral returns by targeting voters with low and relatively stable incomes.

Finally, my results do not eliminate the possibility of electoral manipulation. While my results suggest that voters do not consistently *constrain* incumbents based on policy-driven income changes, they do not rule out the possibility that incumbents can still exploit pocketbook motives under specific conditions. Indeed, findings from the policy feedback literature suggest that certain highly visible policies can sometimes shape voter behavior in meaningful ways. This suggests that incumbents may have some degree of control over which policies voters sanction them for, making it a critical question for future research to examine how governments might strategically influence the policies for which they are held accountable.

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Appendix

A Simulations of policy-sensitivity

Figure 1 is generated by simulating the policy-induced and residual income variation as two random variables that I assume to be i) normally distributed, ii) zero-centered, and iii) uncorrelated:

$$
\begin{aligned}
\text{policy} &\sim \mathcal{N}(0, \delta_s) \\
\text{residual} &\sim \mathcal{N}(0, \delta_n)
\end{aligned}
\tag{2}
$$

The magnitude of residual income variation, or 'residual strength' is then defined as:

$$
\text{residual strength} = \frac{\delta_n}{\delta_s + \delta_n}
\tag{3}
$$

I then simulate electorates for values of residual strength between 1 and 100, which goes on the x-axis. To obtain the curves, I specify five decision rules that determine how voters cast a binary vote either for or against the incumbent. At either extreme is Kramer and Fiorina, with the other three rules falling somewhere in between. They are all specified using Equation 1 by varying the β weights as follows:

$$
\text{Kramer } (\beta_s > 0, \beta_n = 0) \quad
\begin{cases}
1, & \text{if policy} > 0 \\
0, & \text{otherwise}
\end{cases}
$$

$$
\text{Fiorina } (\beta_s = \beta_n > 0) \quad
\begin{cases}
1, & \text{if policy} + \text{residual} > 0 \\
0, & \text{otherwise}
\end{cases}
$$

$$
\beta_s = 2 \times \beta_n > 0 \quad
\begin{cases}
1, & \text{if } 2 \times \text{policy} + \text{residual} > 0 \\
0, & \text{otherwise}
\end{cases}
$$

$$
\beta_s = 5 \times \beta_n > 0 \quad
\begin{cases}
1, & \text{if } 5 \times \text{policy} + \text{residual} > 0 \\
0, & \text{otherwise}
\end{cases}
$$

$$
\beta_s = 10 \times \beta_n > 0 \quad
\begin{cases}
1, & \text{if } 10 \times \text{policy} + \text{residual} > 0 \\
0, & \text{otherwise}
\end{cases}
$$

I then calculate, under each of these rules, the share of voters who make congruent incumbent sanctions, i.e. vote (do not vote) for incumbents whose policies have benefited (harmed) them on net, i.e. those whose incumbent vote decision aligns with the sign on their policy-induced component.

1

B Timing of budget statements

The budget of the United Kingdom is an annual budget set by the treasury for the following financial year. Budgets are usually set once a year and last for the duration of the financial year running from 1 April to 31 March. Budgets have historically been announced in March less than a month before the start of the new financial year. This practice changed in 2017 where budgets were released 4-5 months earlier, in October or November. Thus, for the 2010-2019 period under study, budgets were announced shortly before implemented policy changes in 2010-2017, and announced in the preceding Autumn for the 2018 and 2019 policy changes.

One exception to the pattern was in 2015, where an additional budget was announced in July, four months after the March budget, due to the change of government. Due to the odd timing of this July budget, some measures took immediate effect on the day of announcement, including changes to carried interest taxation for fund managers and modifications to Controlled Foreign Company rules (HM Treasury 2015; Osborne 2015). However, many of the headline policies were scheduled for implementation at the start of the following financial year in April 2016, including the introduction of the National Living Wage at £7.20 for workers aged 25 and above, the replacement of the Dividend Tax Credit with a £5,000 tax-free Dividend Allowance, and the increase in the Employment Allowance from £2,000 to £3,000 (HM Treasury 2015; House of Commons Library 2015). Other significant measures had even longer implementation timescales, such as the planned reductions in corporation tax rates in 2017 and the phased introduction of restrictions on landlord tax relief starting in 2017 (HM Treasury 2015).
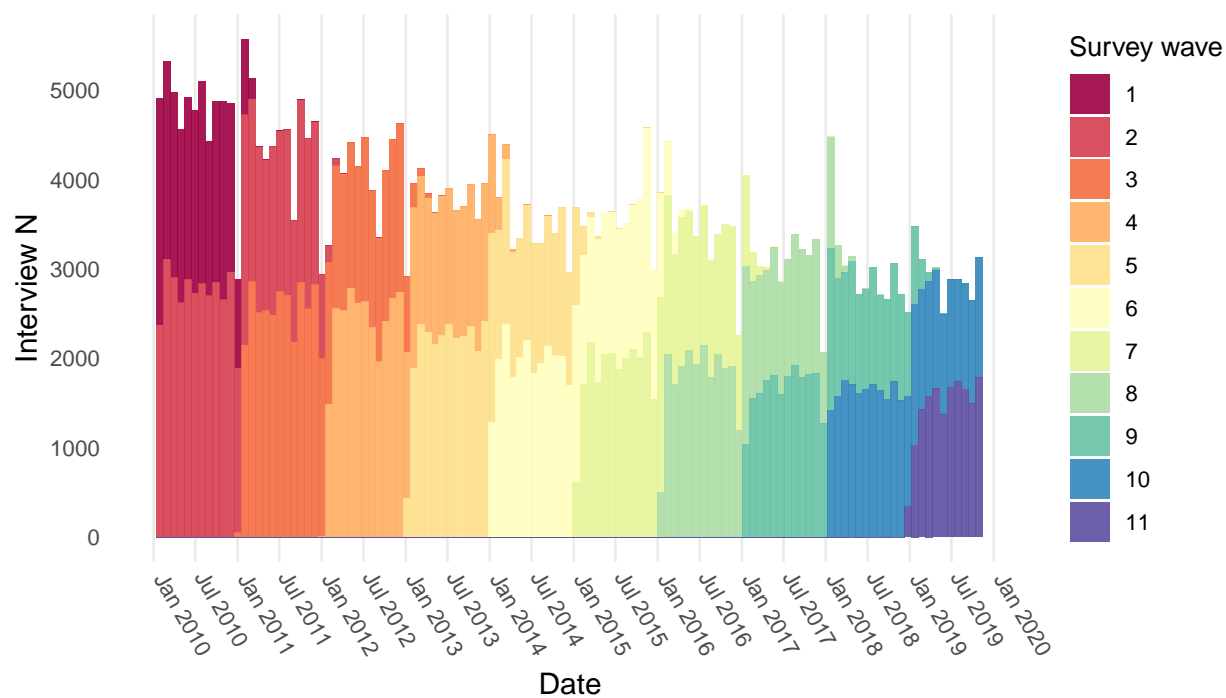
C   Timing of UKHLS survey waves

Figure C1: How survey respondents are distributed across waves of the UKHLS.

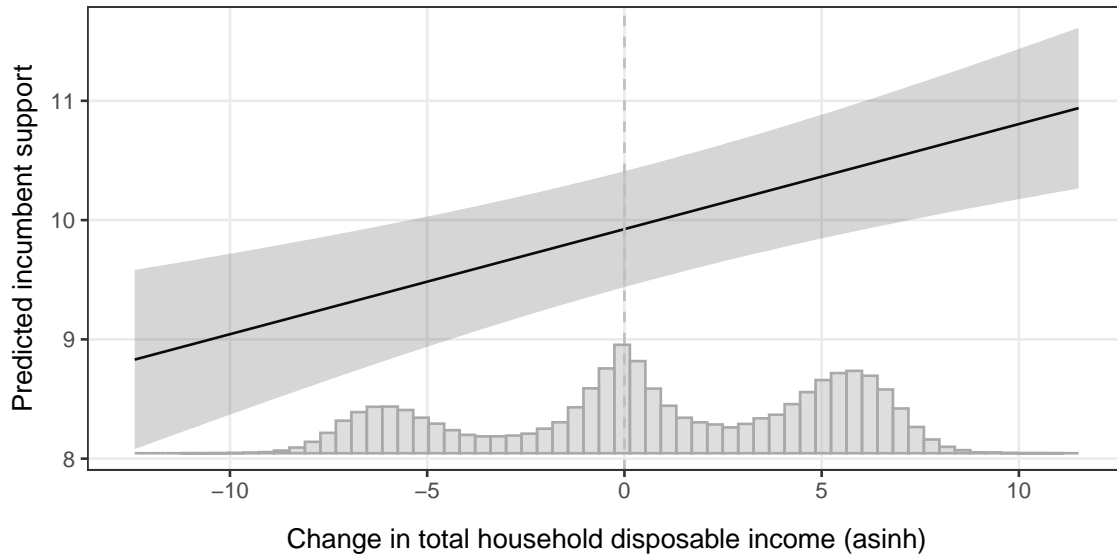# D Validation of standard pocketbook voting relationships

Figure D1: Regression of incumbent support (0-100) on yearly changes in total household disposable income (inverse hyperbolic sine transformation) with overlaid histogram of the distribution of these income changes. The coefficient on income change is significant at the 0.001 level. LDV specification based on Equation 4 with household-clustered standard errors.

Table D1: Estimates from regressing incumbent support on pocketbook evaluations and calculated total income changes.

	1	2	3
Pocketbook evaluations	0.08 (0.00)***		
Total income change (£1000/mo)		0.20 (0.08)**	
Total income change (asinh)			0.09 (0.03)***
N	158 680	128 000	128 000
Std.Errors	household	household	household
Lagged DV	✓	✓	✓
Controls	✓	✓	✓

Note. Regression table of lagged dependent variable models of incumbent support (0-100) with household-level clustered errors. Pocketbook evaluations are rescaled to a 0-100 scale. All income changes are yearly changes in monthly disposable household income (£), in real terms. Income changes are either raw or transformed using the inverse hyperbolic sine. * $p<.05$, ** $p<.01$, *** $p<.001$.

E   Model specifications

To estimate how decomposed pocketbook changes affect incumbent support, I estimate two kinds of models: lagged dependent variable models (Equation 4) and two-way fixed effects models (Equation 5) as shown below.

$$v_{it} = \beta_p \Delta y_{it}^{policy} + \beta_t \Delta y_{it}^{total} + v_{it-1} + \mu_{it} + \varepsilon_{it} \tag{4}$$

$$v_{it} = \beta_p \Delta y_{it}^{policy} + \beta_t \Delta y_{it}^{total} + \mu_{it} + \alpha_i + \eta_t + \varepsilon_{it} \tag{5}$$

In these models, v_{it} is incumbent support for individual i in fiscal year t. Δy_{it}^{policy} is the change in the individual's average monthly disposable income (£) from the last fiscal year to the current fiscal year caused by policy changes, and Δy_{it}^{total} is the total income change. η_t and α_i are wave and individual fixed effects, respectively. μ_{it} is a vector of control variables that includes lagged disposable income, employment status, a job loss and a job gain dummy, lagged subjective pocketbook evaluations (and age and gender for the LDV models). To account for the fact that policy-eligibility is partly determined at the household level, I cluster standard errors at the level of households (Abadie et al. 2023).[18] Note that I estimate the regressions at the level of household income changes although estimating them at the individual level generally makes no substantive difference to results (as shown in Appendix G).

In these models, β_p and β_t capture how voters' incumbent support changes as a function of policy-induced vs total income changes, holding the other constant. Attribution is thus estimated directly as the size of β_p relative to β_t (and this is equivalent to estimating models with $\Delta y_{it}^{residual}$ instead of Δy_{it}^{total}; also shown in Appendix G). If the electorate is characterized by policy-based pocketbook voting with perfect attribution, β_p is positive while β_t is zero, and the difference between them is significant. If there is no attribution, as in Fiorina's model, there should be no significant difference between β_p and β_t, regardless of their levels.

Substantive interpretations of these coefficients are a little complex as they do not capture the change in incumbent support associated with income changes, but rather with changes in the magnitude of income changes. The coefficients on Δy_{it}^{policy} and Δy_{it}^{total} in the TWFE model in Equation 5 do not capture the change in incumbent support associated with income changes, but rather with changes in the magnitude of income changes. To begin with the simplest case of a model with only unit fixed effects and absolute income changes of £1000 increments, β_s captures the average percentage point increase in incumbent support associated with a £1000 difference in how much an individual's monthly disposable income changed from $t-1$ to t. So for instance, it would capture the difference between someone whose monthly disposable income changed by £5000 one year and £6000 another. A positive coefficient thus implies that voters are more supportive of the incumbent when their yearly income changes are greater than average.

Similarly, the coefficients on income changes in the LDV model capture the change in incumbent support associated with a change in the magnitude of the income change from one year to the next. Similar to both, if voters attribute correctly, they should be more sensitive to these changes

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[18]UKHLS only provides unique household identifiers within and not across waves. I therefore construct longitudinal household identifiers based on respondents' first wave in the panel, if they are already in a household, or the first wave in which they joined an existing household.

when they come from policy. In the more complex model specifications in Equations 5 and 4 with wave fixed effects, time-varying controls, and income transformations, the estimates change to become how voters respond to income changes that are greater than the population average, conditional on controls, and unit change becomes proportional rather than absolute.

F Fiorina-style pocketbook voting under observed policy-induced income variation

To calculate how many voters (or rather respondent-wave observations) would correctly sanction the incumbent under Fiorina's decision rule, I calculate the valence of the policy-induced component and the valence of the total income change. Straightforwardly, congruent sanctions occur when voters vote for the incumbent in cases where the valence of the policy-induced change is positive, and vote for the opposition otherwise. However, Fiorina's decision rule says to vote for the incumbent when the valence of the *total* income change is positive.

Table F1: Cross-tabulation of total income change valence and policy-induced change valence.

	Policy-induced change ≤ 0	Policy-induced change > 0
Punishers	0.18	0.23
Rewarders	0.19	0.40

The question is how often positive total income changes coincide with positive changes to the policy-induced component. This comes down to the relative size of the policy-induced component and the correlation between the policy-induced and residual components. I calculate it by simply cross-tabulating the valence of the policy-induced component change (positive vs non-positive; in columns) and the valence of the total income change (positive vs non-positive; in rows). This is shown in Table F1. Congruent sanctions occur in the upper left and lower right cells; incongruent sanctions occur in the upper right and lower left cells. The sum of the two cells is 58%. As shown, voters would incorrectly punish the incumbent 23% of the time and incorrectly reward the incumbent 19% of the time.

G Robustness to varying model specifications

Table G1 shows results of regressing incumbent support on decomposed disposable income changes across a variety models that deviate from the main specifications in Table 1. Model 1 and 2 show results for fully decomposed models, i.e. including residual income variation instead of total income variation. Mirroring the main results, Model 1 uses raw income changes (£ 1000/mo) while Model 2 uses the inverse hyperbolic sine transformation. Model 3 is a pooled cross-sectional model pooling all observations without unit or wave FE's, lagged dependent variables or controls. Model 4 and Model 5 show versions of the main estimates in Table 1 using unit fixed effects or two-way fixed effects (with controls). Model 6 shows the main specification but without control variables. Model 7 shows the main specification but with income changes at the individual rather than household level.

Table G1: Incumbent support regressed on decomposed disposable income changes, various model specifications.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Δy^{policy}	−0.27 (0.07)***	−0.04 (0.03)	−0.45 (0.04)***	−0.08 (0.03)**	−0.06 (0.03)*	−0.11 (0.03)***	−0.05 (0.02)*
$\Delta y^{residual}$	0.16 (0.06)**	0.11 (0.03)***					
Δy^{total}			0.07 (0.03)**	0.02 (0.02)	0.01 (0.02)	0.07 (0.03)**	0.07 (0.02)***
N	70798	70798	153214	148709	153214	71757	70677
Std.Errors	household	household	household	household	household	household	household
Wave FE					✓		
Unit FE				✓	✓		
Lagged DV	✓	✓				✓	✓
Controls	✓	✓		✓	✓	✓	✓
Income lvl.	household	household	household	household	household	household	individual

> *Note.* Regression table of various model specifications with household-level clustered errors. The outcome is incumbent support (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms, transformed using the inverse hyperbolic sine, and split by their causal origin. * p<.05, ** p<.01, *** p<.001.

Table G2 shows results for further alternative specifications. They all use the main model specification with lagged dependent variables and controls, only adding variables or excluding observations. All models but Model 2 use the inverse hyperbolic sine transformation (Model 2 uses raw income changes). Model 1 includes dummies for the most extreme 10 percent of positive and negative income changes (as in Tilley, Neundorf and Hobolt (2018)). Model 2 includes squared terms. Model 3 includes lagged versions of the total and policy-induced income variables. Model 4 excludes the top decile of income earners, i.e. those who have an average gross income in the top decile in the sample. Model 5 excludes individuals who receive transfers in the lowest quartile in size in any year, for whom the risk of benefit non-take-up is greatest.

Table G2: Incumbent support regressed on decomposed disposable income changes, various model specifications.

	Incumbent Support (0-100)				
	Model 1	Model 2	Model 3	Model 4	Model 5
Δy^{policy}	−0.18 (0.09)+	−0.33 (0.10)***	−0.31 (0.08)***	−0.35 (0.11)**	−0.22 (0.11)*
Δy^{total}	0.12 (0.05)*	0.27 (0.10)**	0.16 (0.08)+	0.11 (0.24)	0.27 (0.23)
$(\Delta y^{policy})^2$		0.00 (0.00)			
$(\Delta y^{total})^2$		0.00 (0.00)**			
N	70798	70798	45390	44125	16701
Std.Errors	household	household	household	household	household
Lagged DV	✓	✓	✓	✓	✓
Controls	✓	✓	✓	✓	✓

Note. Regression table of lagged dependent variable models with household-level clustered errors. The outcome is incumbent support (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms. Models 1-3 use the full sample. Models 4-5 use restricted samples. + $p<.10$, * $p<.05$, ** $p<.01$, *** $p<.001$.

H   Effects by governing period

Table H1 shows results split by the three governing periods covered by the data: Labour (2010), Conservatives and Liberal Democrats (2010-2015), and Conservatives (2015-2019). With Labour only in power in the 2010 wave, the modeling approach is restricted to a simple cross-sectional model relying on between-individual variation with some controls. Moreover, this model includes only Δy^{policy} since $\Delta y^{residual}$ requires recorded market incomes and demographics from the previous survey wave (although note leaving out $\Delta y^{residual}$ makes little substantive difference to the results). As these differences in modeling makes it difficult to compare with the main models directly, I specify a similar model for all three governing periods in the first three columns. While there are small differences, with the coalition years seeing a significant negative effect, the Labour years are not substantively different and in no period is there a significant positive coefficient. I also estimate the LDV model from the main specification in the two rightmost columns for the coalition and Conservative governments and find them to be similar, suggesting that the party in power makes little difference.

Table H1: Incumbent support regressed on decomposed disposable income changes, by governing party.

	Between-models (policy-induced only)			Within-models (LDV)	
	Labour	Center-right coalition	Conservatives	Center-right coalition	Conservatives
Δy^{policy}	-0.25 (0.32)	-0.20 (0.02)***	0.07 (0.06)	-0.04 (0.03)	-0.06 (0.08)
$\Delta y^{residual}$				0.12 (0.03)***	0.01 (0.05)
N	2606	155 257	58 017	58 675	12 123
Wave FE				✓	✓
Unit FE					
Lagged DV				✓	✓
Controls	✓	✓	✓	✓	✓

Note. Regression table of simple multiple regression models and lagged dependent variable models with household-level clustered errors. The models are subset by the governing party, with Labour for the 2010 wave, Center-right coalition for 2011-2015 and Conservatives for 2016-2019. The outcome is incumbent support (0-100). All income changes are yearly changes in monthly disposable household income (£), in real terms, transformed using the inverse hyperbolic sine, and split by their causal origin. * $p<.05$, ** $p<.01$, *** $p<.001$.
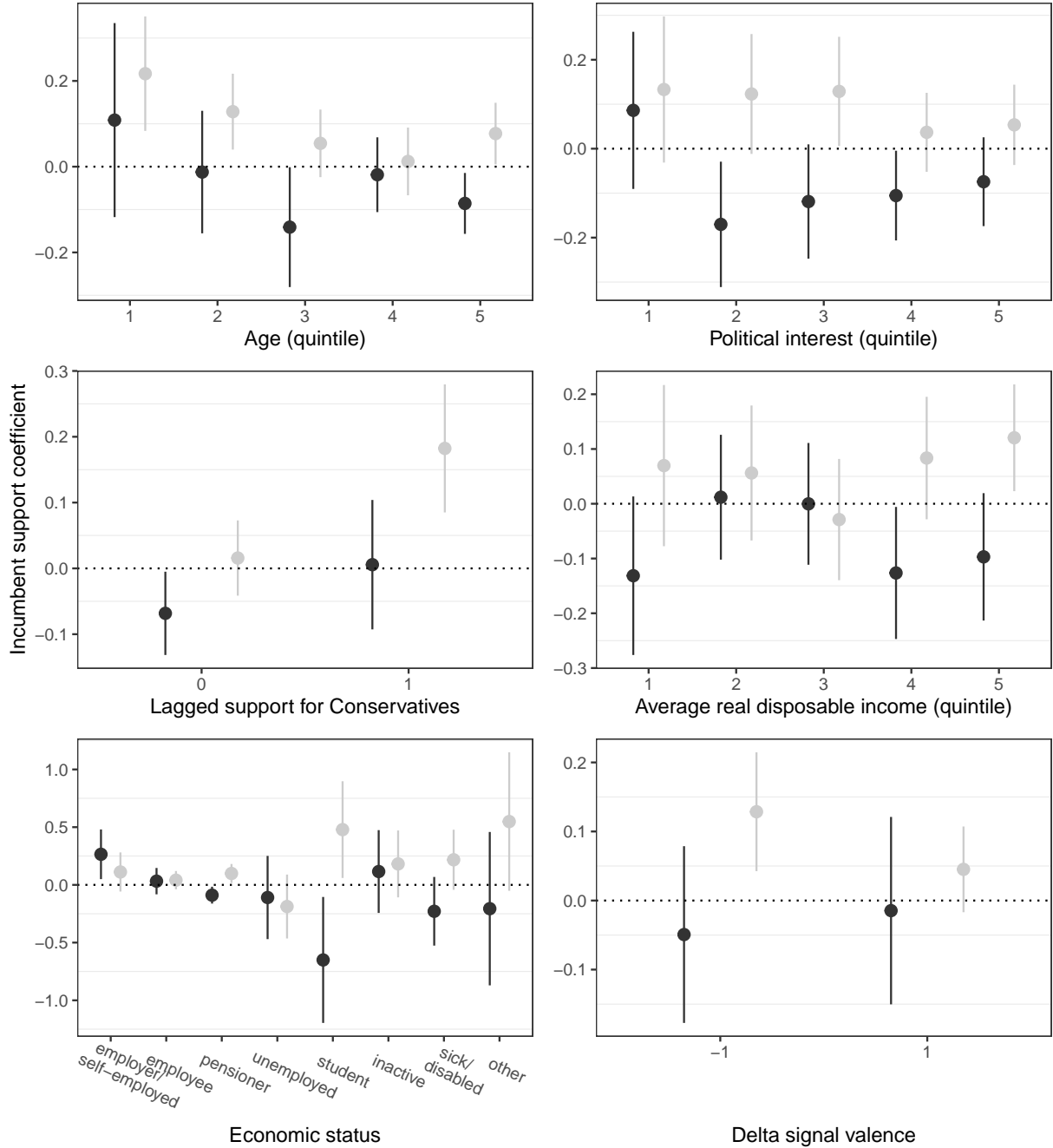
I Heterogeneous effects

Figure I1: Coefficients from LDV regression of incumbent support on total disposable income changes (inverse hyperbolic sine transformation) based on the main model specification in Equation 4. Black estimates are coefficients on Δy^{policy}, grey estimates are coefficients on Δy^{total}. 'Delta policy valence' captures whether the policy-induced income shock for a given observation is positive or negative.