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THE INTERFACE OF POLITICS
HOW SOCIAL GROUPS
GUIDE POLITICAL CHOICE

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Summary

At the heart of liberal democratic theory lies the notion that governments derive their legitimacy from their ability to represent the interests of individual citizens. Elections serve as the mechanism for this representative claim: through voting, individuals express their preferences and ensure they are equally weighted in the formation of government. Yet this vision contains a fundamental tension. While democratic governments are supposed to represent individual interests, their policy-making does not concern individuals as such. Public policies by definition always affect aggregates of individuals who share policy-relevant traits, creating a gap between the personal experiences of voters and the aggregate-level decisions of government. For individual interests to be represented, voters must somehow map their personal interests onto political choices: they must solve what can be termed the “translation problem” between the personal and the political. This dissertation explores how voters navigate this fundamental challenge.

The dissertation’s main argument is that social groups function as a crucial interface between individual voters and the political system, helping voters solve the translation problem more effectively than relying on personal experience or national conditions alone. The central research question asks: *How do voters use social groups to evaluate policies and parties?* The theoretical framework developed here posits that both policy-making and voters’ political interests operate at the group level, making social groups a natural “interface” where citizens and governments interact. When politicians craft policy, they necessarily operate at the level of groups rather than individuals, targeting shared characteristics like age, income, or geography. Similarly, individual interests largely cluster into what we recognize as social groups, as people sharing sociodemographic characteristics tend to have overlapping interests across multiple dimensions. This clustered nature of both policy-making and interests creates conditions under which voters can learn the most about government alignment with their interests by relying on information about their social groups. The dissertation’s primary contribution is demonstrating empirically that group-based political reasoning represents a meaningful and prevalent response to the informational challenges voters

face, challenging prevailing accounts that characterize group-based behavior as purely emotional or tribal.

The dissertation is article-based and consists of a project frame (Chapter 1–4) and four empirical research articles (Chapter 5–8), of which three are solo-authored and one is co-authored. The research articles are self-contained and can be read without reading the project frame. Likewise, the project frame summarizes the entire project by elaborating on the overarching argument and how each article fits within it, discussing existing literature on group-based political behavior, developing a formal theorization of groups as the interface of politics, discussing cross-cutting methodological challenges, and summarizing key findings and their implications for research and the workings of democracy.

The first empirical paper, “You and Whose Economy?: Group-Based Retrospection in Economic Voting,” investigates whether voters evaluate incumbent performance based on the economic conditions of their social in-groups. Using panel data from the British Election Study and three pre-registered experiments in Denmark and the United States, the study finds that voters systematically assess incumbent performance based on how their social groups have fared economically, especially relative to the national economy. This group-based retrospective voting operates independently of both personal financial circumstances and national economic conditions, suggesting important limits to purely sociotropic models of economic voting. The effects are comparable in magnitude to traditional sociotropic voting, indicating that group-based considerations represent a fundamental dimension of electoral accountability.

The second paper, “Who (Else) Benefits?: Group-Based Responses to Distributive Policies,” challenges the conventional assumption that voters respond to targeted government policies primarily through pocketbook considerations. Examining COVID-era stimulus policies in Denmark and the United States, alongside three pre-registered experiments, the study demonstrates that voters’ responses to material benefits are shaped at least as much by perceived in-group benefit as personal economic gains. The effects vary significantly by group identity strength, with positive responses concentrated among groups with strong political identities. These findings help explain the mixed empirical record on distributive policy effects and suggest that the electoral returns to targeted spending depend critically on which groups benefit and how clearly this targeting is perceived.

The third paper, “Elite Rhetoric and the Running Tally of Party-Group Linkages,” co-authored with Frederik Hjorth, examines how citizens form and update perceptions of which political parties represent particular social groups. Using a novel automated approach with language models to measure group appeals in 1.6 million parliamentary speeches, the study connects party rhetoric to survey measures of perceived party-group linkages over three decades in the UK. The findings demonstrate that group linkages robustly track party elites’ rhetoric, especially for voters with a high media consumption, suggesting that voters maintain “running tallies” of party-group linkages that respond systematically to elite communication. This challenges views of group linkages as fixed reflections of social cleavages and supports the broader claim that group identities function as the primary interface through which voters interpret party behavior and political representation.

The fourth paper, “Is Pocketbook Voting Sensitive to Policy?,” addresses a key assumption underlying pocketbook voting as a mechanism of democratic accountability: that voters respond reliably to policy-induced income changes. Using a novel approach that links survey panel data on vote choice to policy microsimulation models, the study decomposes respondents’ disposable income changes into policy-induced and residual components across a decade of British fiscal policy. The analysis reveals that voters fail to privilege policy-induced income changes over other income fluctuations when evaluating incumbents, instead responding indiscriminately to total income changes. Moreover, policy-induced changes constitute only a small fraction of total income variation, meaning that even indiscriminate pocketbook voting aligns with actual policy effects only slightly more often than chance. These findings suggest significant limitations in voters’ ability to hold governments accountable through personal economic experience, reinforcing the importance of group-based heuristics as an alternative pathway for democratic accountability.

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PROJECT FRAME

Chapter 1

Introduction

At the heart of the liberal democratic ideal lies the notion of representation of citizens' interests. According to classical accounts, governments derive their legitimacy from their ability to represent the interests of individual citizens (Downs, 1957). Elections serve as the mechanism for this representative claim: by casting their vote, individuals express their preferences and ensure they are all equally weighted in the formation of government (Manin, Przeworski and Stokes, 1999). Through this act of "interest aggregation" (Achen and Bartels, 2016, p. 323) governments are said to represent, however marginally, the interests of each of citizen in the way they rule.

This vision of democracy retains deep normative appeal as evidenced by its enduring influence in political thought. But it also contains a deep tension. While democratic governments are supposed to represent individual interests, their actions are not directed towards individuals. As Kramer (1983) notes, in a modern mass democracy, "public policies by definition always affect aggregates of individuals" and "most individuals never receive, or expect to receive, purely personal favors or benefits from office holders" (p.106). This point may seem obvious, but it points to a gap in the logic of representation. For individual interests to be represented, voters must somehow map their personal interests onto political choices; at a basic level, it must be the case "that voters can judge which party offers more of what they want" (Bawn et al., 2012, p. 571). Yet, this "translation of private interests into political attitudes" (Mutz, 1994, p. 689) is far from straightforward. While individuals routinely and automatically evaluate personal events – such as a job loss, a promotion or an illness – as good or bad, it is far more challenging to assess how distant, aggregate-level political decisions and policies relate to their interests. For elections to serve as a meaningful vehicle of representation, an act of translation must occur between the personal – what voters personally care about and want from politics – and the political – the policies and actions of government.

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How do voters approach this fundamental translation problem? Three classical theories of voting behavior are worth examining as attempts to answer this question: the sociotropic heuristic, policy reasoning, and experiential voting. Apart from empirically describing political behavior, these theories constitute voters' best attempts to bridge the gap between the personal and the political. Thus, besides questions about empirical accuracy, there is a larger theoretical question of how far these strategies actually take voters in solving the translation problem. As I argue, each of them falls short in its own way.

On the sociotropic account, voters simply assume that what is good for the country is good for them, at least in the long run (Kinder and Kiewiet, 1981). As Kinder and Kiewiet (1981) note, this 'sociotropic' approach is not necessarily altruistic; it simply means voters base evaluations on national conditions rather than personal ones, and is aligned with theories that emphasize the importance of the 'competence' or 'quality' of government (Kinder and Kiewiet, 1981; Key, 1966; Ferejohn, 1986). Because this approach obviates the difficult act of translation altogether, it is not particularly demanding and relies on information about the country as a whole that is broadly available.

While the sociotropic approach may be a valid strategy when social interests are broadly aligned, this assumption is often unrealistic. Conflicting interests and competition over scarce resources constitute fundamental conditions of politics. Although positive-sum policies exist, many voters perceive politics in zero-sum terms (Davidai and Tepper, 2023; Chinoy et al., 2023; Stantcheva, 2022). When public policies produce winners and losers, a sociotropic perspective may mislead voters about how well the government serves them. In this sense, the sociotropic solution sidesteps the translation problem by denying the diversity of individual interests altogether. Given the inherently conflictual nature of societies, the sociotropic approach offers an incomplete solution to the translation problem.

A second possibility is, of course, that voters approach the translation problem head-on. Rather than assuming alignment between national and personal interests, voters may attempt to understand the political landscape by following political developments closely, tracking party positions, interpreting policy proposals and their implications, and understanding how the incumbent's decisions have personally affected them. This approach thus involves deducing the latent alignment between policies or parties and

their personal interests directly.

The main limitation of this ‘policy-reasoning’ approach is that it places extreme cognitive and informational demands on citizens. Most voters lack the time, motivation, and expertise to understand technical policy programs in detail and interpret or predict their personal consequences. There is thus a “presumption of this literature that it is difficult for individuals to assess the individual [effects] of policy changes” (Beiser-McGrath and Bernauer, 2023, p. 552). Empirically, policy-reasoning describes the behavior of only a small, highly politically engaged minority (De Vries and Hobolt, 2013; Lau and Sears, 1981). Additionally, the personal effects of past policies may not be so predictive of policy effects in the future. This returns to the idea that governments do not directly govern individuals: the fact that an individual voter benefits from some policy before an election does not imply that the incumbent will similarly favor them after the election (Drazen and Eslava, 2006). In sum, even for the highly politically engaged minority, policy-reasoning offers a costly and imperfect solution to the translation problem.

A third strategy reverses the direction: rather than reasoning deductively about the personal consequences of policies, voters may make inferences from their own lives to politics. On this ‘experiential’ account, individuals use their personal experience to infer how well the government serves their interests. Evaluating one’s personal conditions and wellbeing requires minimal cognitive effort or specialized knowledge, making this approach highly accessible (Fiorina, 1981; Key, 1966).

However, experiential voting also has significant limitations. Personal experience is ‘noisy’: life outcomes are shaped by many factors unrelated to government action. As a result, even when voters observe changes in their wellbeing, these changes may not be attributable to the incumbent’s performance (Ansolabehere, Meredith and Snowberg, 2014). As Kiewiet and Rivers (1984) illustrate this point: “suppose a distant relative dies, leaving a substantial inheritance. Does the lucky recipient attribute his or her good fortune to whoever happens to be in the White House at that moment?” (p. 381) Bridging the gap between personal wellbeing and politics requires filtering out this ‘noise’, but that reintroduces the translation problem with its cognitive demands (Gomez and Wilson, 2001). Voting based purely on personal experience may be simple, but it may also largely get it wrong.

In sum, these major approaches voters are thought to take to the translation problem

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fall short for different reasons. The sociotropic approach does not account for conflicting interests. The policy-reasoning approach makes unrealistic demands on citizens. And the experiential approach, while accessible, does not reliably connect interests to policy. Together, these limitations raise a fundamental puzzle: if the link between the personal and the political is so fragile, how can voters effectively have their interests represented?

This dissertation proposes an alternative solution: voters relying on their social groups as an intermediary. As government policy-making is ‘clustered’ by design, policies affect individuals by means of broadly shared characteristics like income levels, socio-demographic traits, and job features. By taking seriously that policies always affect clusters of individuals with common characteristics, I argue that voters can best overcome the translation problem by interpreting their political choices through the lens of social groups. By observing how parties and policies affect their groups, voters can learn how those parties and policies align with their own interests. This is similar to the benefit of aggregation from the sociotropic heuristic; yet concerns the smaller and more relevant unit of the social group within which interests are more strongly correlated than the nation as a whole. In the opposite direction, voters can observe conditions in their groups to make inferences about how the incumbent’s policies aligns with their interests. This is similar to experiential voting, but with a crucial advantage: whereas personal experience contains idiosyncratic noise, this noise is filtered out when voters aggregate personal experiences across their groups, providing a clearer signal of how parties and policies are aligned with ‘people like them’. Group interests thus serve as a proxy for personal interests, and this proxy can be both easier to interpret and more informative than direct personal experience.

Metaphorically, groups can thus be conceived as an ‘interface’ of politics. An interface is a “means or place of interaction between two systems” (Oxford English Dictionary, 2023) or a “place where two things come together and affect each other” (Cambridge English Dictionary, 2025). In computing, for instance, the “user interface” is the place where the computer’s information is presented to the user in a simplified way, and conversely where the user interacts with the computer through stylized commands. The interface is thus the location where two systems meet and where their interaction is simplified in a way that enables meaningful interaction. Similarly, social groups constitute the interface at which the political system and the electorate interact politically. Parties and policies interact with the electorate as groups of individuals with

Table 1.1: Overview of papers

Short title	Title	Chapter	Status
A: Retrospective Voting	<i>You and Whose Economy: Group-Based Retrospection in Economic Voting</i>	5	<i>Conditional Accept, American Journal of Political Science</i>
B: Distributive Policies	<i>Who (Else) Benefits? Group-Based Responses to Distributive Policies</i>	6	<i>Working paper</i>
C: Group Linkages	<i>Elite Rhetoric and the Running Tally of Group Linkages[†]</i>	7	<i>Conditional Accept, Journal of Politics</i>
D: Pocketbook Voting	<i>Is Pocketbook Voting Sensitive to Policy?</i>	8	<i>Working paper</i>

[†]Co-authored with Frederik Hjorth.

shared characteristics. Given this ‘clustered’ nature of policy-making, individuals can best make sense of policies and parties through the experiences and shared interests of the clusters they belong to. Groups thus offer a “place of interaction” that allows voters to translate between the personal and the political in a simplified yet meaningful way. I term this *the group interface of politics*.

This dissertation formulates a theory of the group interface of politics that demonstrates how voters can rely on social groups to solve the translation problem. It then demonstrates through four empirical papers how political behavior is shaped by social groups in line with the theory. While the logic of the argument applies broadly across areas of policy-making, I focus on the broad domain of the economy and economic policy as a major area in which the group interface operates. The economy is consistently one of the key areas on which elections are decided and provides a clear-cut case for distinguishing group interests from national or individual interests. The following research question encapsulates this broader aim of the thesis:

How do voters use social groups to evaluate policies and parties?

The main thrust of my answer to this question lies in the four research articles placed in Chapters 5-8, each providing empirical evidence on a separate part of my theory.

Chapter 1

Table 1.1 provides an overview of the four articles. Each article is self-contained and can be read independently of the others. The purpose of these introductory chapters is to develop the broader argument and contextualize the contribution of each article within it.

Summary of the Overarching Argument

The central argument of this dissertation is that social groups function as a fundamental interface between individual voters and the political system. The theory rests on the premise that both policy-making and voters' political interests operate at the group level. This makes social groups the natural point where citizens and governments interact.

When politicians craft policy, they necessarily operate at the level of groups rather than individuals. Practically, policies do not name specific individuals but define policy eligibility and targeting through shared characteristics: tax brackets apply to income ranges, social programs target demographic categories, and regulations affect entire industries or sectors. Similarly, policy-making intent must operate at this level as politicians design policies with aggregated constituencies in mind rather than unique individuals.

This clustered nature of policy-making has implications for how voters can meaningfully connect their personal interests to political outcomes. In principle, each voter possesses an almost unique bundle of interests stemming from their economic status, occupation, family structure, place of residence, transportation needs, recreational activities, health circumstances, and countless other characteristics. However, politicians cannot and do not govern with such idiosyncratic interest bundles in mind. Instead, they necessarily focus on broader interests that voters share with others. Thus, while policy effects may occasionally align perfectly with individual voters' granular preferences, such alignment represents coincidence rather than systematic political responsiveness. To make sense of how policies and parties serve their interests, voters must therefore focus on the interests they share with others.

Rather than being randomly distributed, individual interests largely cluster into what we recognize as "social groups" since interests correlate with sociodemographic characteristics. People in similar occupational categories, for example, consistently

share interests across multiple policy dimensions, forming meaningful constituencies with aligned preferences. The strength of this clustering varies across different characteristics, making some social groups far more politically cohesive, and thus relevant, than others.

The clustered nature of policy-making and interests fundamentally shapes how voters can evaluate government performance. Under these conditions, information about social groups becomes the most reliable way for voters to learn about the incumbent's alignment with their interests. Voters face a fundamental translation problem: inferring how well the government serves their interests from observable signals. They confront a choice between three imperfect information sources – national economic conditions, personal experiences, and the experiences of their social groups – each with distinct informational advantages and disadvantages. National conditions, while informative about the incumbent's governing, aggregate across conflicting interests and thus provide weak signals about how particular voters fare under incumbent policies. Personal experience, though more aligned with individual interests, suffers from idiosyncratic noise that obscures systematic patterns in government policy-making.

When interests are clustered, group-level outcomes offer a superior solution by filtering out individual-level noise through aggregation while remaining aligned with voters' clustered interests. Given that incumbent policy alignments necessarily occur at the group level rather than the individual level, signals more granular than group-level information add only noise without providing additional relevant information about the government. Under these structural conditions, social groups emerge as a valuable simplifying interface: to understand how policies and parties align with their interests, voters can look to how they align with the interests of their groups.

Beyond its informational value, group-level information is also highly accessible to ordinary voters. People can track how politics affects their social groups through two main channels: everyday social interactions and elite messaging. Groups are socially and geographically clustered in the sense that people tend to interact disproportionately with others who share their social characteristics (Kossinets and Watts, 2009; McPherson, Smith-Lovin and Cook, 2001; Fu et al., 2012). In turn, most people regularly receive information about how group members are faring and how they experience policies. Empirical research shows that voters often use such “socially sampled” information to form views about broader political and economic conditions (Alt et al.,

2022; Christensen, 2025; Books and Prysby, 1999; Larsen et al., 2019). At the same time, political elites often speak in group-based terms, and media coverage frequently frames policies by their impact on different groups (Thau, 2019; Huber, 2022; Huber and Haselmayer, 2024). These elite signals further enable voters to follow how parties and policies align with group interests, especially those that are politically salient.

The idea that group-based considerations shape political behavior is far from new. Nevertheless, the theory of groups as an informational interface departs in important respects from much of the existing literature on group-based political behavior. The next section discusses how.

Existing Literature: Why Voters Follow the Group

The notion that political behavior is fundamentally group-centric follows a long intellectual tradition in political science. Going back to Converse (1964)’s pioneering work, public opinion research has consistently found social group memberships to shape how voters act and think politically (Campbell et al., 1960; Miller, Wlezien and Hildreth, 1991; Bornschier et al., 2021; Claassen et al., 2021; Donnelly, 2021*b*). The connection between parties and groups has also been richly theorized, with such “group linkages” (Thau, 2019; Miller, Wlezien and Hildreth, 1991) constituting central components of party reputations (Stubager and Slothuus, 2013; Brewer, 2010; Petrocik, 1996). Similarly, research on policy feedback has shown how targeted policies rely on and partake in the social construction of social groups as e.g. disadvantaged or undeserving (Schneider and Ingram, 1993; Pierson, 1993; Mettler and Soss, 2004; Mettler and Sorelle, 2014; Elder and O’Brian, 2022; van Oorschot, 2006). When asked about their vote choices, voters frequently cite perceptions of parties’ group ties as one of their main reasons (Dalton, 2018; Campbell et al., 1960). In their influential book, Achen and Bartels (2016) forcefully conclude that “group ties and social identities are the most important bases of political commitments and behavior” (p. 319).

The overarching argument of the dissertation grows out of this rich literature, yet breaks with it in one crucial respect. Existing scholarship typically explains the vast evidence for group-based political behavior in terms of emotional attachments to social identities. This orientation draws heavily on Tajfel’s influential Social Identity Theory (Tajfel, 1974; Turner, 1975; Turner and Tajfel, 1986), which characterizes group-based

behavior as primarily driven by social competition over esteem and status. According to this framework, group identification creates intense psychological attachments to group interests that stem from individuals' fundamental need to "enhance their self-esteem" (Turner and Tajfel, 1986, p. 16).

Social Identity Theory was originally conceived as an alternative to 'realistic group conflict' theories (Campbell, 1965) that emphasized the role of material conflicts over scarce resources and "incompatible group goals" (p. 17). Tajfel and Turner's key insight was that real material conflict appeared neither necessary nor sufficient for intergroup conflict; sometimes even minimal self-categorization into opposing groups was enough. Famous social psychological experiments within this 'minimal group' paradigm have demonstrated how seemingly meaningless group memberships can shape attitudes and behavior, suggesting that group-based preferences can emerge from purely psychological foundations.

While Tajfel and Turner were careful not to completely dismiss the role of "objective interests" and acknowledged that distinguishing their account from material conflict was "nearly impossible in most natural social situations" (p. 23), their theory has profoundly shifted the study of group-based behavior in political science toward affective rather than interest-based explanations. This theoretical evolution has reached its culmination in influential contemporary work that characterizes group identities as fundamentally irrational forces in political life.

Achen and Bartels (2016), in the perhaps most prominent work on group-based political behavior in the post-Tajfel-Turner tradition, assert that group identities are no more than "affective tribal loyalties" (p. 325) and "emotional attachments that transcend thinking" (p. 228). They summarize their "group theory of politics" as "the powerful tendency of people to form groups, the ensuing construction of 'us' and 'them', and the powerful role of emotion rather than reason in directing group activity" (p. 215). This perspective has become increasingly dominant, with other influential scholars emphasizing how "identity can be linked to political attitudes far removed from group interests" (Jones, 2023, p. 511), describing group-based behavior as "deviations from self-interest" (Klor and Shayo, 2010, p. 270), and arguing that identity-driven voting emerges from "the connection between the group status and the self-concept rather than any objective facts" (Mason, 2018, p. 869). While these studies often provide compelling empirical evidence that voters prioritize group considerations over narrow

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policy issue preferences, the interpretation of such behavior as hollow tribal impulses devoid of political substance is rarely if ever pinned down.

This dissertation offers an alternative theoretical account of group-based political behavior: one rooted in the informational value of group ties rather than purely affective attachments. This approach aligns with recent scholarly work that questions the predominant emphasis on irrationality in group-based politics. As Fiorina (2018) observes, group identities may represent “one low-cost means (among others) of furthering your interests in a world where you have partial and ambiguous knowledge” (p. 50). Conover (1985) provided an early articulation of this perspective in her work on group interests in economic voting, emphasizing groups’ role in “linking the individual to politics” (p. 150). She succinctly captured the informational logic underlying group-based political reasoning: “information about the interests of salient groups will have more personal relevance for an individual than assessments of national well-being; at the same time, evaluations of group economic well-being may have more readily apparent political relevance than an individual’s own economic condition.” (p.150) In a similar vein, Ansolabehere, Meredith and Snowberg (2014) demonstrate formally that information from voters’ “micro-economies”, i.e. local social contexts, provides more informative predictions of their own future economic outcomes than either their current economic outcomes or national economic indicators. This finding suggests that group-based political reasoning may be more informationally rational than commonly assumed. Similarly, Donnelly (2021a) emphasizes how group interests help individuals navigate economic uncertainty, characterizing groups as “information-summarizing tools” (p. 19) that enable voters to predict how economic policies will affect them.

The common thread connecting these contributions is their demonstration of the informational value that groups provide to voters attempting to make sense of politics and their own interests. This dissertation follows in their footsteps, with particular focus on how voters use group-level information to learn about the incumbent and other parties. In my theoretical framework, group-level information proves valuable to voters because it reveals how policies and parties align with politically relevant interests they share with others. Rather than viewing group-based behavior as departures from self-interest, it suggests they may be complementary aspects of sophisticated political reasoning. Self-interest need not equate individualistic behavior; voters may follow their group to further their own interests.

It is important to clarify that my aim is not to demonstrate that group-based behavior is rational as such. The empirical papers show that voters pursue group interests in their voting behavior, irrespective of their immediate personal benefit. This implies a mechanism of democratic accountability at the level of group interests. The theoretical model presented in Chapter 2 shows that by pursuing group interests, voters can effectively pursue their own interest more effectively than had they focused narrowly on personal experience or national outcomes. While offering a rational explanation, it does not prove that voters pursue group interests *because* it is rational for them to do so, or even that the way they do it in practice is informationally optimal. Psychologically, voters' pursuit of group interests may still be driven by "tribal loyalties" (Achen and Bartels, 2016, p. 325) rather than personal interests, or be carried out in a manner that is not ultimately good for them. As such, my research does not directly adjudicate the 'rationality' of the group-based behavior I uncover. But settling this ultimate question of rationality is also not of primary importance, and may not even be possible. The most important task is uncovering what this group-based behavior looks like, what effects it has, and in turn what it means for our understanding of democracy and how it can be improved. I return to these broader implications of the theory and findings in Chapter 4.

Summary of Empirical Contributions

This thesis focuses empirically on voter behavior rather than elite behavior. While I take the clustered nature of policy-making as a theoretical premise of the argument, the central empirical question concerns how voters navigate in this political environment. The theoretical framework of groups as an interface of politics thus generates four specific predictions about voter behavior.

Group-based retrospective voting. Building on the extensive literature documenting voters' tendency to hold incumbents accountable for both national economic conditions and personal economic circumstances, the group interface theory predicts an additional mechanism of retrospective evaluation. Voters should systematically assess incumbent performance by evaluating economic outcomes for their relevant social groups, treating group-level economic performance as a meaningful, independent signal of government

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responsiveness to their interests. This group-based retrospective voting would operate alongside, rather than replacing, existing sociotropic and egotropic mechanisms.

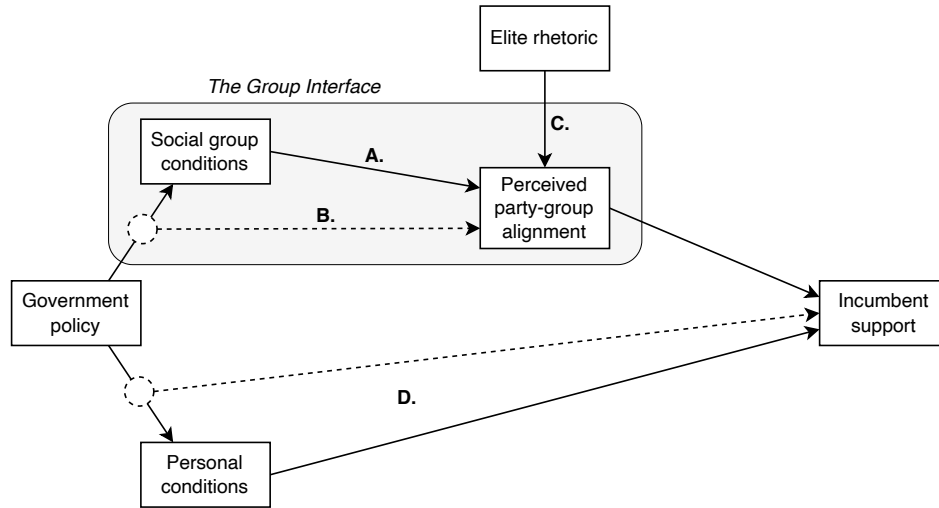
Group-based policy evaluation. The group interface theory predicts that voters will assess policies largely through their effects on relevant social groups rather than through personal experiences alone. This occurs because group-level policy effects provide more reliable signals about incumbents' alignment with voters' interests than individual-level outcomes, which may reflect idiosyncratic circumstances unrelated to systematic political choices. Voters should therefore respond strongly to policies that benefit or harm their social groups, even when this has no immediate implication for their personal gains or losses.

Keeping track of perceived party-group alignments. The group interface theory assigns a central role to information about interest alignments between parties and groups. Thus, voters should care about and keep track of how different parties are aligned with the interests of various social groups. To maintain their accuracy, voters should respond systematically to new information about such alignments, updating them on a running basis.

Limited use of personal conditions. The group interface theory positions social groups as central to political judgments. An implication of this is that personal conditions play a limited role in how voters translate their personal interests into political choices. Firstly, policy-reasoning, i.e. deducing the effects of policy on personal conditions, should be too demanding for voters to reliably engage in it. Secondly, experiential voting based on personal conditions should be too affected by idiosyncratic 'noise' to reliably sanction incumbents in line with individual interests.

I test these four behaviors in four empirical papers, which substantiate how social groups function as a fundamental interface of politics that connects individual voters to policies and parties. Figure 1.1 summarizes the overall theoretical model and the position of each paper within it.

The diagram shows the clustered nature of policy-making as government policies simultaneously affect individuals ('personal conditions') and social groups made up of individuals with shared interests ('social group conditions'). In turn, these both affect incumbent support. In line with the group interface logic, the effect of social group conditions on incumbent support goes through voters' perceptions of parties'

Figure 1.1. Overview of how each paper fits the broader argument of the dissertation.

alignments with their groups (‘perceived party-group alignment’). In addition, this effect works through two channels: voters may respond either to social group conditions *as such* (arrow A) or to *the effects* of policy on social group conditions (the dashed arrow B). This is the essence of the group interface: how clustered policy-making affects social groups, and how voters hold incumbents accountable for it.

Papers A and B examine exactly these two mechanisms. Paper A examines group-based retrospective voting: how voters’ retrospective evaluations of social group conditions affect incumbent support and perceived incumbent-group alignment. The main finding is that in-group economic evaluations are at least as important as sociotropic (nation-wide) economic evaluations in explaining incumbent support. The pattern is especially strong for group performance *relative* to the national economy, which provides a stronger information signal about the incumbent’s latent priorities. Paper B examines group-based policy evaluation: how voters respond to information about the effects of economic policies on their groups. It finds that voters like policies more when they benefit politically relevant groups they belong to, irrespective of their personal policy benefit, and this preference affects incumbent support.

While Papers A and B focus chiefly on the relationship between social group conditions and incumbent support, Paper C more closely examines how voters form and dynamically update their perceptions of party-group alignments that mediate this relationship (arrow C). Parties not in government cannot demonstrate their align-

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ment with groups through policy-making, but they can, among other things, signal their allegiances through rhetoric. In the paper, my co-author and I find that voters' perceptions of party-group alignments reliably track politicians' use of such 'group appeals' in public speech. This suggests that voters are attentive to the way parties talk about groups and indicates that these group-centered perceptions constitute important components of political reasoning.

Finally, Paper D studies voters responses to personal conditions (the two D arrows). As for social group conditions, personal conditions affect incumbent support through two channels: voters may either respond to the totality of their personal conditions (the full arrow) or to the effects of policy on their personal conditions (the dashed arrow). This mirrors two of the theories of voting behavior surveyed above, where experiential voting involves acting on experienced changes and the policy-reasoning approach involves separating out and responding only to the effects of policy. This paper examines both approaches empirically focusing on voters' responses to personal income changes, also known as "pocketbook voting". Consistent with the demandingness of the policy-reasoning approach, the paper finds that voters do not manage to separate out and act on 'policy-induced' income changes but instead act on total income changes, i.e. experiential voting. Further, it finds that when voting experientially, voters mostly sanction incumbents for 'noisy' income changes unrelated to their policy-making. This suggests that personal experience is not a reliable solution to the translation problem.

Jointly, these papers thus demonstrate how voters care about group interests, and how voting based on group interests rather than personal experience can help voters solve the translation problem.

Structure of the Dissertation

Before moving on to the four research articles, the following three chapters provide a discussion of issues, arguments and implications that relate to the dissertation as a whole. The purpose of these opening chapters is two-fold: to elaborate on the overarching argument and its connection to the individual research articles and to discuss important, cross-cutting issues in more detail. In Chapter 2, I provide a more detailed theoretization of the group interface of politics. I do so, first, by developing a formal model demonstrating the informational logic of group-based behavior with more

rigor, and, second, by discussing which social groups are most relevant to the group interface and how voters learn about them. In Chapter 3, I discuss key methodological issues in studying political behavior related to social groups and the economy. Finally, in Chapter 4, I summarize the empirical papers' key findings and draw conclusions about the dissertation and its implications as a whole.

Chapter 2

A Theory of The Group Interface of Politics

This chapter provides a formal theoretical foundation for the group interface argument advanced in this dissertation. First, I develop a simple theoretical model that formalizes the translation problem: voters' challenge of inferring incumbent alignment with their interests from observable signals. Using a stylized retrospective voting scenario, where voters evaluate recent incumbent performance, the model demonstrates conditions under which rational voters would rely on group-level outcomes rather than personal experience or national conditions to assess how well the incumbent serves their interests. I discuss how the model's insights extend beyond the retrospective voting context in various ways. In the chapter's second part, I build on these theoretical foundations to discuss which social groups are most relevant for the group interface and under what conditions. This section includes a discussion of how voters obtain information about group-level conditions and policy experiences.

Preview of the Model

The model developed in this chapter formalizes the translation problem – voters’ challenge of inferring how well the incumbent serves their interests from observable signals – and illustrates how it varies across different social structures. Crucially, it demonstrates that relying on group-level outcomes can be rational under plausible assumptions about the structure of interests and policy-making in society.

In the model’s stylized version of the translation problem, voters try to assess how the incumbent’s policies will affect them in a future period based on noisy information about policy outcomes in the current period. The model is thus retrospective in the vein of Fiorina (1981), in that voters observe past outcomes under the incumbent to predict how they will fare in the future if the incumbent is re-elected.¹ While the model focuses on voters’ information acquisition rather than their actual vote choice, this simplification is inconsequential: once voters obtain their preferred signal about the incumbent’s alignment with their interests, the voting decision follows mechanically (voting for the incumbent if the estimated alignment is positive, against if negative). The translation problem concerns which information source best reveals this alignment, not the subsequent voting decision itself. As I discuss in a final section, it can easily be extended to cover forward-looking information about policies and parties beyond the incumbent.

Importantly, the model is theoretical rather than behavioral. As such, its aim is to show how voters can use their groups to solve the translation problem under certain conditions, not to show that they necessarily do so, nor that they would necessarily rely on groups for this reason. In its modeling of rational agents, it makes a normative rather than a positive argument about the value of alternative information sources.

The model compares four information sources voters might use: i) national economic conditions (the sociotropic heuristic), ii) personal wellbeing (experiential voting),

¹As Fiorina emphasizes, it is not that voters use their retrospective judgments to predict future *policies*, as suggested in Downs (1957)’s retrospective voting model. Instead, they focus on predicting their own outcomes, regardless of the specific policy instruments used. Fiorina traces this aspect of his theory back to Key (1966), who argues that the mass public should be viewed as prioritizing the ends of government policy over the means. In Fiorina’s words, “the citizen simply knows that the Democrats have done well by him/her in the past, and therefore assumes they are more likely to do so in the future. How they do so is a matter for the party leaders to decide” [emphasis in original] (1981, p. 197). I adopt this perspective in my operationalization of the latent alignment of interests between the incumbent and the individual by focusing on future welfare under the incumbent.

iii) the effect of policy on their wellbeing (policy-reasoning), and iv) the economic conditions of social in-groups (the group interface). In a homogeneous society with common interests, national conditions provide the most accurate estimate of the incumbent's systematic policy effects. Since any deviations from the average policy effect are pure noise, they contain no information about how the individual will fare under the incumbent in the future. Voters can thus effectively ignore their personal experience and rely on sociotropic signals to estimate incumbent 'competence' or 'quality'. This scenario is, of course, unrealistic: there must be some systematic signal in how policy effects are distributed among voters.

Adding Kramer (1983)'s more realistic assumption that policy-making and interests are clustered alters voters' calculus fundamentally. When interests cluster within social groups and policy-making targets these clusters, group-level information becomes more informative than either personal experience or national conditions for most voters. Group-level signals aggregate away idiosyncratic noise while capturing both common effects and group-specific effects, whereas national signals systematically miss the latter component. A key insight of the model is that given policy alignments between the incumbent and voters occur at the group level, more granular signals than those at the group level only add noise without providing additional relevant information.

These theoretical results integrate major approaches to the translation problem into one framework and shows how their attractiveness depends critically on the underlying structure of conflict in society. The analysis thus provides a rational foundation for group-based political behavior, showing that voters' reliance on social groups can represent an effective response to the information environment without invoking any social psychological mechanisms rooted in identity-based esteem or status.

The Formal Model

The Translation Problem

Consider an electorate with n voters, labeled $i = 1, \dots, n$, and a sitting incumbent. Each voter i faces two periods: 'current' and 'future'. These periods can be thought of as government terms separated by an election between them. In the current period, the voter (and the rest of the electorate) is exposed to the incumbent's policy platform

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which has a net effect on her welfare. At the end of the period, voter i chooses whether to vote for or against the incumbent in the election: this is the decision that I model. After the election, in the future period, she is exposed to the incumbent's future policy platform with a further, unknown net effect on her welfare.

We can denote the policy-induced effects on voter i 's welfare as p_i and p_i^{future} . The key insight is that the policy effect p_i contains both systematic components related to the incumbent's underlying alignment with voter i 's interests, as well as idiosyncratic components that don't reflect this deeper alignment. Voters care about learning the systematic alignment component, denoted generally as ϕ , because this predicts how the incumbent will treat them in the future period (p_i^{future}). However, ϕ is not directly observable. This creates the translation problem: voters are forced to extract information about the incumbent's true alignment from the noisy signals available to them.

The specific form of the alignment parameter ϕ depends on the structure of interests in society. In some cases, all voters share the same alignment with the incumbent, making ϕ a common parameter. In other cases, alignment varies systematically across groups, making ϕ group-specific. Regardless of its specific form, ϕ represents the latent incumbent trait that voters want to learn to predict their future welfare. In the next sections, I define a set of information signals voters can choose from and show how their informational value depends on assumptions about what the incumbent's alignment with voters' interests looks like.

Information Signals

To approach the translation problem, voters can obtain information to learn about the incumbent's alignment with their interests, ϕ . In the model, voter i can 'buy' an information signal, S_p , where the p -subscript indicates the various types of signals available. For simplicity, voters are assumed to acquire a single signal with a fixed cost prior to voting. While it is true that real-world voters typically seek and assess information about incumbents on a running basis, allowing for a mix of information, this aspect is less relevant for my argument regarding the relative attractiveness of different types of information signals. The model demonstrates which type of information voters would rely on if constrained to choose only one option.

Each signal varies in its informational value about the incumbent's alignment, but also comes with a cost, k , representing the time, cognitive effort and specialized knowledge required to obtain and interpret the signal. These S_p 's are the pieces of information advanced by each of the major approaches to the translation problem surveyed above, and the translation problem can thus be reduced to the voter's decision of which signal to buy before making her vote choice. The sociotropic approach involves information about the change in average national wellbeing under the incumbent (S_{nation}). The policy-reasoning approach involves information about the effects of the policy platform on the individual herself (S_{self}). Finally, the experiential approach involves information about changes in the individual's own wellbeing under the incumbent ($S_{\text{wellbeing}}$). These can be defined as:

$$\begin{aligned}
 S_{\text{self}} &= p_i \\
 S_{\text{wellbeing}} &= p_i + \underbrace{\eta_i}_{\text{non-policy noise}} \\
 S_{\text{nation}} &= \frac{1}{n} \sum_{j=1}^n S_{\text{wellbeing},j} = \frac{1}{n} \sum_{j=1}^n (p_j + \eta_j)
 \end{aligned} \tag{2.1}$$

As shown, S_{self} is just the individual policy platform effect, p_i , as defined earlier. $S_{\text{wellbeing}}$ is the individual policy effect plus an idiosyncratic noise term, η_i , that contains all individual variation in wellbeing unexplained by the incumbent's policy platform. S_{nation} is then the aggregate of $S_{\text{wellbeing}}$ across all citizens, representing changes in observable national conditions (as summarized by measures like GDP, the unemployment rate and so on).

The informational value of each signal depends on how accurately it allows voters to estimate the incumbent's alignment parameter ϕ that predicts their future welfare if the incumbent were re-elected. A straightforward measure of signal quality is the expected squared error when using the signal to estimate ϕ . This captures how closely the signal approximates the true value of ϕ . For a signal S_p trying to estimate target parameter ϕ , we define the mean squared error as:

$$\text{MSE}(S_p) = \mathbb{E}[(S_p - \phi)^2] \tag{2.2}$$

where the expectation is taken over the distribution of voters who might use this signal. Since individual voters don't know their specific realizations of noise terms (ε_i, η_i),

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they must evaluate signals based on average performance across the population. Thus, $\text{MSE}(S_p)$ represents the expected error for a randomly selected voter using this signal. Some voters will get better or worse signals than this average: for instance, those with small ε_i get more accurate individual signals than those with large deviations. However, not knowing their personal realization of the noise terms, voters are forced to choose based on expected performance.

To facilitate interpretation, we define informational value as the inverse of mean squared error:

$$\text{Info}(S_p) = \frac{1}{\text{MSE}(S_p)} \quad (2.3)$$

Higher values of $\text{Info}(S_p)$ thus indicate more informative signals. A signal that perfectly reveals ϕ would have MSE approaching zero and thus very high informational value, while a noisy signal would have large MSE and low informational value.

The final part of the model concerns the cost of each signal, denoted as k_{nation} , k_{self} , and $k_{\text{wellbeing}}$. As discussed earlier, the difficulty of figuring out the total effects of an incumbent's policy platform on oneself is at the heart of the translation problem. In other words, S_{self} is very costly. By contrast, observing S_{nation} is far easier in part because information on the national economy is widely reported by experts and in the media, and is an issue of near-constant political salience. The least costly signal by far, however, is just observing one's personal wellbeing in lieu of figuring out the policy platform effect or gathering external information. As $S_{\text{wellbeing}}$ can be obtained by casual observation, it is by far the cheapest signal. This results in the following hierarchy of signal costs:

$$k_{\text{wellbeing}} < k_{\text{nation}} < k_{\text{self}} \quad (2.4)$$

Decision Rule

With these moving parts in place, the final step of the model is the voter's decision rule. Each voter pays cost k_p to see signal S_p with a given information value $\text{Info}(S_p)$. She thus chooses the S_p that maximizes:

$$\text{Value}(S_p) = \underbrace{\text{Info}(S_p)}_{\text{how informative } S_p \text{ is of } \phi} - \underbrace{\lambda k_p}_{\text{cognitive cost}} \quad (2.5)$$

where $\lambda > 0$ is a scaling parameter that converts unit changes in the variance of their

estimated μ into the same units as cognitive cost, reflecting the relative weight the individual puts on cost vs information (assumed to be uniform for simplicity).

The Translation Problem Under Homogeneous Interests

Which signal should voters buy? We begin with the benchmark case where the electorate has homogeneous interests. In this scenario, all voters have the same underlying alignment with the incumbent. The incumbent's policies can be characterized by a single parameter μ that captures how well they serve the common interest. This can be thought of as incumbent 'quality' or 'competence'. Here, the alignment parameter is thus $\phi = \mu$ for all voters.

On the assumption of common interests, the policy effects for the current and future period can be understood as:

$$\begin{aligned} p_i &= \mu + \varepsilon_i \\ p_i^{\text{future}} &= \mu + \varepsilon_i^{\text{future}} \end{aligned} \tag{2.6}$$

where μ is the incumbent's effect on the national average of wellbeing, and ε_i is an idiosyncratic, zero-centered noise term capturing individual-level deviations from this average. Thus, in this scenario, the incumbent makes policies with some net effect on average national wellbeing (μ) but this effect is distributed unevenly across individuals. Note that μ is constant across the current and future time periods, reflecting the simplifying assumption that the incumbent's quality or competence is a perfectly stable characteristic. We relax this assumption in an extension below, but the stability of μ does not alter the relative informativeness of the signals considered here.

Importantly, by contrast, ε_i varies between the current and future period. In other words, current deviations from μ are entirely idiosyncratic and randomly distributed and thus not predictive of deviations from μ in the future. This is the critical feature of this scenario that makes interests homogeneous. If ε_i and $\varepsilon_i^{\text{future}}$ were correlated, the incumbent would be systematically more or less aligned with certain individuals, implying that interests are not homogeneous. When ε_i and $\varepsilon_i^{\text{future}}$ are independent shocks, only the common parameter, μ , is informative about their future welfare under the incumbent.

With all voters wanting to learn the same parameter μ , this becomes a straightforward-

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ward signal extraction problem. Inserting the formula for p_i from Equation 2.6 into the signal definitions from Equation 2.1, the information signals are defined as:

$$\begin{aligned} S_{\text{self}} &= \mu + \varepsilon_i \\ S_{\text{wellbeing}} &= \mu + \varepsilon_i + \eta_i \\ S_{\text{nation}} &= \mu + \frac{1}{n} \sum_{j=1}^n \eta_j \end{aligned} \tag{2.7}$$

Note that in S_{nation} , the ε_j terms cancel out when aggregated since they represent deviations from the mean by definition.

To evaluate these signals, we calculate how accurately each estimates the target parameter μ . The mean squared errors are:

$$\begin{aligned} \text{MSE}(S_{\text{self}}) &= \mathbb{E}[(\mu + \varepsilon_i - \mu)^2] = \text{Var}(\varepsilon) \\ \text{MSE}(S_{\text{wellbeing}}) &= \mathbb{E}[(\mu + \varepsilon_i + \eta_i - \mu)^2] = \text{Var}(\varepsilon) + \text{Var}(\eta) \\ \text{MSE}(S_{\text{nation}}) &= \mathbb{E}[(\mu + \frac{1}{n} \sum \eta_j - \mu)^2] = \frac{\text{Var}(\eta)}{n} \end{aligned} \tag{2.8}$$

The information values are therefore:

$$\begin{aligned} \text{Info}(S_{\text{self}}) &= \frac{1}{\text{Var}(\varepsilon)} \\ \text{Info}(S_{\text{wellbeing}}) &= \frac{1}{\text{Var}(\varepsilon) + \text{Var}(\eta)} \\ \text{Info}(S_{\text{nation}}) &= \frac{n}{\text{Var}(\eta)} \end{aligned} \tag{2.9}$$

Given these definitions, the informational ranking is clear: $\text{Info}(S_{\text{nation}}) > \text{Info}(S_{\text{self}}) > \text{Info}(S_{\text{wellbeing}})$. Because S_{nation} averages n draws of η_j , the idiosyncratic noise shrinks by a factor $1/n$. In nations with millions of citizens, this noise term essentially disappears, making S_{nation} nearly perfectly informative about μ as its MSE approaches zero as n grows large. By contrast, individual-level signals contain irreducible noise: S_{self} includes the distributional noise ε_i that makes individual experiences deviate from the average, while $S_{\text{wellbeing}}$ adds additional non-policy noise η_i on top of this.

Adding the assumption that $k_{\text{nation}} < k_{\text{self}}$ (national information is cheaper to obtain than detailed policy analysis), rational voters choose S_{nation} . In other words, when

interests are homogeneous, voters can effectively further their own interests, and solve the translation problem, through sociotropic voting, just as argued in the first major strand of the literature. To be sure, this does not entirely exclude individual-level signals from rational consideration since $S_{\text{wellbeing}}$ still has a cost advantage over S_{nation} . If voters place sufficiently high weight on minimizing cognitive costs relative to accuracy (i.e. if the scaling parameter, λ , is sufficiently high) the cost differential could outweigh its informational disadvantage. Importantly, though, this would lead voters to choose the cheapest signal, $S_{\text{wellbeing}}$, and not S_{self} .

This result in favor of sociotropic voting depends critically on the assumption that what is in the interest of the nation is in the interest of the individual. In this scenario, any differences in policy effects across individuals cannot be ascribed to incumbent intent or systematic alignments with conflicting interests but is essentially random. However, this characterization poorly reflects policy-making in the real world where such distributional effects of policy are not just the norm but can be assumed to reflect something about the incumbent's latent alignment with conflicting interests in society.

Importantly, though, these interest alignments do not occur at the level of individuals but rather at the level of shared characteristics. This suggests that policy effects should be systematically related to such clusters of shared characteristics rather than purely random or related to individuals directly. The next section examines what happens when interests systematically vary at the level of such clusters, or *social groups*.

The Translation Problem under Clustered Interests

In this scenario, the incumbent's policies affect voters through two distinct channels: a common component that affects all citizens similarly (reflecting overall competence or quality), and cluster-specific components that systematically favor or disadvantage particular social groups. This dual structure better captures the reality of policy-making, where governments both pursue broad economic management that affects everyone and make distributional choices that create winners and losers along group lines.

To formalize this, assume the electorate is divided into two equally sized clusters (i.e. social groups), A and B , of size $m = n/2$. Each cluster experiences both the common effect of incumbent policies and a cluster-specific deviation from this effect. Assuming

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voter i belongs to cluster c , the policy effects for the current and future period can be expressed as:

$$\begin{aligned} p_i &= \mu + \gamma_c + \varepsilon_i \\ p_i^{\text{future}} &= \mu + \gamma_c + \varepsilon_i^{\text{future}} \end{aligned} \tag{2.10}$$

where μ represents the incumbent's common effect on all voters (capturing overall competence or quality as before), γ_c represents the stable cluster-specific deviation from this common effect, and ε_i represents individual-level noise that is independent from one period to the next. We assume $\gamma_A + \gamma_B = 0$, meaning that cluster-specific effects sum to zero, i.e. what one group gains, the other loses in relative terms.² Thus, one can think of γ_c as taking out some of the idiosyncratic variation in ε_i and making it vary systematically at the group-level.

This formulation captures the clustered nature of policy-making in modern democracies: incumbents make policies that have both positive-sum components (economic growth, public goods provision) and zero-sum components (distributional choices, targeted benefits). The parameter μ captures the former while γ_c captures the latter. Importantly, both components are stable characteristics of the incumbent that persist across periods, reflecting their underlying competence and their alignment with different group interests respectively.

To understand the relative importance of these two components, we define a clustering parameter based on their magnitudes:

$$\theta = \frac{|\gamma_c|}{|\mu| + |\gamma_c|} \tag{2.11}$$

This parameter captures the share of the total systematic policy effect attributable to group-specific targeting. When $\theta = 0$, all systematic policy variation comes from common effects affecting everyone equally. This is the special case of the 'homogeneous interests' scenario considered in the previous section. When $\theta = 1$, there is no common component and all systematic variation is group-specific and zero-sum, i.e. a pure spoils system. For intermediate values, both components contribute to policy outcomes. θ is an objective feature of the incumbent's policy platform, measurable from actual

²If cluster effects did not sum to zero, we can redefine μ to include the average cluster effect, ensuring that γ_c captures only the zero-sum distributional component. This does not affect the analysis.

policy effects and varies exogenously across incumbents and contexts.

Under this clustered structure, voters in cluster c want to learn $\phi = \mu + \gamma_c$, that is, the total systematic effect combining both common and cluster-specific components, to predict their future welfare $p_i^{\text{future}} = \mu + \gamma_c + \varepsilon_i^{\text{future}}$. This fundamentally changes the value of different information signals. The signals become:

$$\begin{aligned} S_{\text{self}} &= \mu + \gamma_c + \varepsilon_i \\ S_{\text{wellbeing}} &= \mu + \gamma_c + \varepsilon_i + \eta_i \\ S_{\text{nation}} &= \mu + \frac{1}{n} \sum_{j=1}^n \eta_j \end{aligned} \tag{2.12}$$

Notice that in S_{nation} , the cluster-specific effects γ_c cancel out when aggregating across both clusters (since $m \cdot \gamma_A + m \cdot \gamma_B = m(\gamma_A + \gamma_B) = 0$). This is a key insight: national-level signals can reveal the common component μ but provide no information about cluster-specific alignments.

In addition to the three signals from before, voters can observe an additional signal under clustered interests. Similarly to observing changes in the national average wellbeing, voter i can also observe changes in the average wellbeing of their own cluster c , defined as:

$$S_{\text{cluster}} = \frac{1}{m} \sum_{j \in c} (p_j + \eta_j) = \mu + \gamma_c + \frac{1}{m} \sum_{j \in c} \eta_j \tag{2.13}$$

In contrast to S_{nation} , this signal captures both the common component μ and the cluster-specific component γ_c , making it potentially more informative for voters trying to estimate their total systematic effect. The cost of this signal, k_{cluster} is likely to be relatively low since group members can easily observe conditions in their groups through everyday interactions in their social networks. I discuss this point further in the section on extensions. For now, it is simply assumed to be equal to k_{nation} .

To calculate the information value of each signal, recall that voters in cluster c want to estimate $\phi = \mu + \gamma_c$. The mean squared errors are:

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$$\begin{aligned}
\text{MSE}(S_{\text{self}}) &= \mathbb{E}[(\mu + \gamma_c + \varepsilon_i - (\mu + \gamma_c))^2] = \text{Var}(\varepsilon) \\
\text{MSE}(S_{\text{wellbeing}}) &= \mathbb{E}[(\mu + \gamma_c + \varepsilon_i + \eta_i - (\mu + \gamma_c))^2] = \text{Var}(\varepsilon) + \text{Var}(\eta) \\
\text{MSE}(S_{\text{nation}}) &= \mathbb{E}[(\mu + \frac{1}{n} \sum \eta_j - (\mu + \gamma_c))^2] = \gamma_c^2 + \frac{\text{Var}(\eta)}{n} \\
\text{MSE}(S_{\text{cluster}}) &= \mathbb{E}[(\mu + \gamma_c + \frac{1}{m} \sum_{j \in c} \eta_j - (\mu + \gamma_c))^2] = \frac{\text{Var}(\eta)}{m}
\end{aligned} \tag{2.14}$$

As shown, the national signal perfectly reveals the common component μ but systematically misses cluster-specific effects and thus has an irreducible error of γ_c^2 .³ The cluster signal reveals both components with vanishing error as cluster size grows. Individual signals capture both components but with noise that doesn't disappear with population size.

As the population sizes grow large, the information values converge to revealing limits:

$$\begin{aligned}
\text{Info}(S_{\text{nation}}) &\rightarrow \frac{1}{\gamma_c^2} \\
\text{Info}(S_{\text{cluster}}) &\rightarrow \infty \\
\text{Info}(S_{\text{self}}) &= \frac{1}{\text{Var}(\varepsilon)} \\
\text{Info}(S_{\text{wellbeing}}) &= \frac{1}{\text{Var}(\varepsilon) + \text{Var}(\eta)}
\end{aligned} \tag{2.15}$$

The ranking of signals under clustered interests therefore depends critically on the magnitude of cluster-specific effects, θ . In the special case of homogeneous minimally clustered interests ($\theta \approx 0$), $\text{Info}(S_{\text{nation}}) \approx \text{Info}(S_{\text{cluster}}) > \text{Info}(S_{\text{self}}) > \text{Info}(S_{\text{wellbeing}})$. In this case, S_{nation} and S_{cluster} are similarly informative, given that the cluster size m is sufficiently large. Thus, only the relative cost of the two signals determines which signal voters choose to buy.

Once we introduce almost any amount of clustering ($\theta > 0$), S_{cluster} becomes

³Note that for cluster A , S_{nation} underestimates the true effect by $|\gamma_c|$, while for cluster B , it overestimates by the same amount. The symmetry ensures that both clusters face the same MSE from using S_{nation} , despite one being overestimated and the other underestimated. While the squared error thus differs across clusters, the average squared error across both clusters thus equals γ_c^2 .

superior to S_{nation} because the latter's systematic error from missing γ_c outweighs the former's small residual noise from diminishing η by m rather than by n .⁴ The degree of clustering matters, however, to the relative ranking of the sociotropic and individual-level signals. When the cluster-level deviations from the average policy effect (γ_c^2) are sufficiently small relative to the individual-level deviations ($\text{Var}(\varepsilon)$), $\text{Info}(S_{\text{nation}}) > \text{Info}(S_{\text{self}}) > \text{Info}(S_{\text{wellbeing}})$. However, as the amount of clustering grows larger relative to $\text{Var}(\varepsilon)$ and $\text{Var}(\eta)$, there is a point at which $\text{Info}(S_{\text{self}})$ and even $\text{Info}(S_{\text{wellbeing}})$ outperform $\text{Info}(S_{\text{nation}})$.

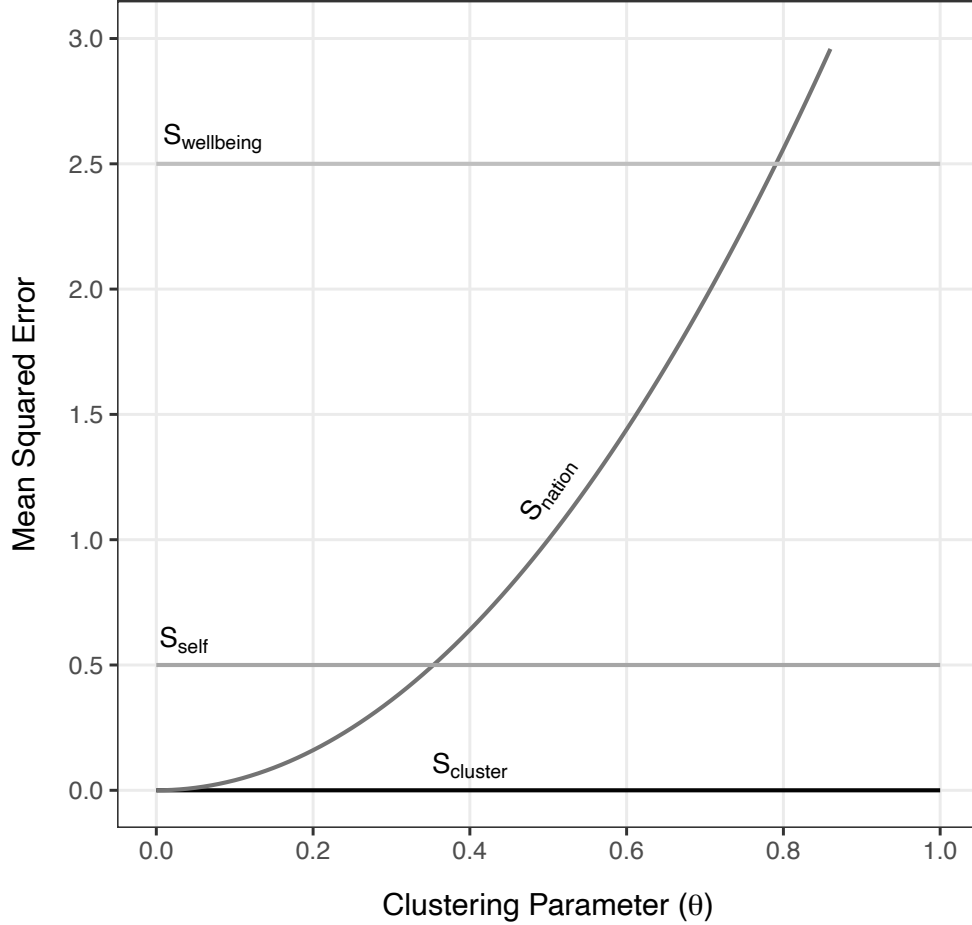
I illustrate this change in informational values with simulated data in Figure 2.1. The y-axis shows the mean squared error for each of the four information signals as a function of values of θ on the x-axis. As shown, in the scenario of homogeneous interests where $\theta = 0$, S_{nation} and S_{cluster} have the lowest error (and given $n = 2m$, S_{nation} mechanically has the lowest error). As θ increases, however, S_{nation} becomes increasingly noisy, while the other three signals keep their precision. This makes S_{cluster} the most informative signal, but also eventually makes S_{self} and $S_{\text{wellbeing}}$ relatively more informative at sufficiently high levels of clustering. While these threshold points are arbitrary in the figure, it illustrates how clustering impacts the relative informativeness of each signal.

The introduction of clustered interests into the translation problem thus results in three important insights. First and foremost, whenever there is meaningful clustering ($\gamma_c \neq 0$), the cluster signal contains strictly more relevant information than the national signal, as it captures both μ and γ_c while the national signal only captures μ . Given similar costs ($k_{\text{cluster}} \approx k_{\text{nation}}$), rational voters should prefer cluster-based information whenever cluster-specific effects exist.

Second, the advantage of S_{cluster} over individual-level signals persists due to the aggregation benefit. Just as S_{nation} dominated S_{self} under homogeneous interests by averaging out individual noise, S_{cluster} dominates S_{self} under clustered interests for the same reason. The key difference is that S_{cluster} accomplishes this while preserving information about cluster-specific effects that S_{nation} structurally cannot capture. Thus,

⁴Note that in finite samples, the comparison becomes more nuanced. With cluster size $m = n/2$, the cluster signal has twice the residual noise of the national signal. This means S_{nation} could outperform S_{cluster} when $\gamma_c^2 + \frac{\text{Var}(\eta)}{n} < \frac{\text{Var}(\eta)}{m}$ which simplifies to $\gamma_c^2 < \text{Var}(\eta)/n$. For small, fragmented clusters where m is much smaller than $n/2$, this condition becomes even easier to satisfy, suggesting that voters in small groups might rationally prefer national signals despite missing group-specific information.

Figure 2.1. Signal Accuracy Under Different Levels of Interest Clustering
(with $\text{Var}(\eta) = 2$, $\text{Var}(\varepsilon) = 0.5$, $n = 2m = 1,000,000$, and the total policy effect $\mu + \gamma_c = 2$)



given interest alignments between the incumbent and voters occur at the group level, more granular signals than those at the group level only add noise without providing additional relevant information.

Third, the model suggests when pocketbook voting (using $S_{\text{wellbeing}}$) might emerge. This happens primarily when clustering is high and the noise terms are small, i.e. the cluster-level deviations γ_c explain a high share of the total variation in policy effects (and in wellbeing) across individuals. The clustering of interests not only makes S_{cluster} more attractive, but also S_{self} and $S_{\text{wellbeing}}$. As clustering increases, $S_{\text{wellbeing}}$ becomes relatively more attractive compared to S_{nation} (though not compared to S_{cluster}) because at least it captures both components, albeit noisily. For individuals for whom the

cost parameter, λ , is high, clustered interests might therefore lead to an increase in pocketbook voting based on the low-cost $S_{\text{wellbeing}}$ rather than group-based voting.

This framework thus provides a unified understanding of when different voting strategies make sense. Sociotropic voting is the optimal approach to the translation problem when politics is primarily about common interests and overall competence ($\theta \approx 0$). Group-based voting becomes rational when politics involves meaningful distributional conflict between clusters ($\theta > 0$). And pocketbook voting emerges when interests are sufficiently clustered and cognitive costs dominate information considerations.

Model Extensions

The model is highly stylized: voters observe retrospective information about how conditions have changed under the incumbent's tenure, pay a cognitive cost to acquire one signal, and then infer a latent 'incumbent alignment' parameter ϕ . None of these specifics are essential to the core logic, however. Below, I sketch several natural extensions and show how they map unto the same underlying tradeoffs.

Non-Material Issues

Although this dissertation focuses on material policy effects (and personal wellbeing), the same inference problem arises in non-material domains such as civil rights or identity politics, which also often operate at the level of groups. In those cases, each cluster c still has a latent "alignment", γ_c (e.g. how favorably a party treats that group's rights) which is partially revealed through direct policy effects as well as group-level conditions. One important difference may be with respect to signal costs. Because changes in rights often have immediate and direct effects, the information costs are likely lower than in the material-policy case, and perhaps especially for S_{self} . Still, in the presence of meaningful clustering of such non-material interests (i.e. $\theta \neq 0$), then observing S_{cluster} would still remain strictly more informative than observing S_{nation} or S_{self} .

Prospective Evaluations of Promised Policies

The model is based on retrospective voting but could easily be extended to future policies as promised in party programs or elite rhetoric by incumbents and opposition parties alike. Traditional retrospective voting models are built on the assumption that parties cannot credibly commit to policy promises, hence voters must resort to evaluations of past performance (Fiorina, 1981; Key, 1966). Likewise, as Fiorina (1981) notes, “knowledge of past performance is cheaper to acquire (it is acquired automatically, in effect) than knowledge of future plans” (p.12). However, credible commitments are not impossible (Drazen and Eslava, 2006; Douthit, Kearney and Stevens, 2012) and there is evidence that voters are sometimes forward-looking (Elinder, Jordahl and Poutvaara, 2015). Moreover, voters have reason to compare their estimates of γ_c for the incumbent party with potential γ_c ’s for competing parties, and this requires some prospective evaluations of other parties’ platforms.

In the prospective scenario, one could allow parties to announce future programs, $\mathbf{P}^{\text{proposed}}$, and conceptualize the information signals as signals about the *expected* future effects of $\mathbf{P}^{\text{proposed}}$ on the nation ($S_{\text{nation}}^{\text{future}}$), themselves ($S_{\text{self}}^{\text{future}}$), and their group ($S_{\text{cluster}}^{\text{future}}$). This would not change the hierarchy of signal costs in any major way as it would scale information values by some additional uncertainty parameter, given that proposed programs are not guaranteed to be fully implemented. This would presumably reduce each $\text{Info}(S_p)$ by an equal amount. This holds as long as proposed policy platforms are not identical to realized policy platforms. If $\mathbf{P}^{\text{proposed}} = \mathbf{P}^{\text{realized}}$, then $S_{\text{self}}^{\text{future}} = p_i^{\text{future}}$ is perfectly informative, and knowing the incumbent’s latent alignment ϕ becomes redundant. However, they can only ever calculate $E[p_i^{\text{future}} \mid \mathbf{P}^{\text{proposed}}]$, which is still a noisy proxy for p_i^{future} if proposed and realized policies only imperfectly overlap, i.e. $\mathbf{P}^{\text{proposed}} \not\subseteq \mathbf{P}^{\text{realized}}$ and $\mathbf{P}^{\text{realized}} \not\subseteq \mathbf{P}^{\text{proposed}}$. In the case of partial overlap, voters still need to understand latent alignment ϕ to account for the effects of the unknown subset of proposed but unrealized policies, and realized but unproposed policies.

Thus, the addition of prospective policy signals about a range of parties does not change the relative ranking of signals: if parties have latent γ_c ’s, voters should still prefer $S_{\text{cluster}}^{\text{future}}$ over $S_{\text{nation}}^{\text{future}}$ and $S_{\text{self}}^{\text{future}}$. Besides concrete policies, this logic also applies to parties’ rhetorical group appeals (Thau, 2021), which signal a party’s future policy effects on a group, $S_{\text{cluster}}^{\text{future}}$, as well as a party’s latent group alignment, γ_c .

The Cost of S_{cluster}

The cost of observing the cluster-level signal, k_{cluster} is assumed to be equal to k_{nation} in the model. This makes for a simpler comparison and implies that S_{cluster} is always preferred over S_{nation} under clustered interests because its information value is strictly greater. In practice, however, k_{cluster} depends on the social group (cluster) in question and will sometimes be lower and sometimes higher than k_{nation} . As discussed further below, S_{cluster} is a cheap signal largely because group members easily access information from each other. Due to social homophily, the tendency of individuals with similar characteristics to disproportionately interact with each other, voters have abundant information about the conditions of a range of social groups from their everyday social context. Still, the degree of homophily is greater for some traits than others and obtaining information about low-homophily groups can be more costly. I discuss these issues more below. The key point is that the cost of S_{cluster} is relatively low, but will be higher for some groups and lower for others relative to the cost of S_{nation} . When $k_{\text{cluster}} > k_{\text{nation}}$, cluster signals will only be preferred if the cost differential is outweighed by higher informativeness (i.e., if the amount of clustering, θ , is sufficiently high).

A further extension of the model in this direction could account for the sampling noise involved in learning about group conditions inductively. This could be incorporated by making m_c represent not the size of i 's cluster c , but rather the size of their sample about wellbeing in cluster c . A small sample would only partially reap the benefits of aggregating the individual noise terms, ε_i . This would give k_{nation} more of an upside even under clustered interests, re-introducing the trade-off between low noise (aggregating ε_i) and high accuracy (capturing γ_c). Such an extension would, however, complicate the model with assumptions about how voters sample group-level information.

Overlapping Identities

The simple model assumes each voter belongs to exactly one cluster c . In reality, individuals hold multiple group memberships (e.g. class, religion, race, region). Then there is not a single 'in-group' signal S_{cluster} but a family $\{S_{\text{cluster}_k}\}_{k=1}^K$, each with its own informativeness $\text{Info}(S_{\text{cluster}_k})$. When there are multiple overlapping conflict

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structures, the incumbent does not make policy along the lines of some pre-defined cleavage, and voters must make some assumptions about which latent ‘cleavages’ along which they should assess the incumbent’s alignment. This introduces a choice of which γ_k it is most informative for the individual to learn about. In simple terms, it depends on the magnitude of group-level deviations across each group dimension (θ_k) and the costs for obtaining signals for each in-group. For instance, a working-class Latino voter might choose between S_{class} and $S_{\text{ethnicity}}$ based on which has larger θ_k , i.e., whether class or ethnic targeting is more pronounced in the current political system. I return to this point of the most relevant groups in a section below.

Changing alignments (γ_c^t) and Heterogeneous Signal Costs (λ_i)

Finally, the model could be realistically extended by allowing two modeled elements to vary. Firstly, voters have varying levels of political sophistication or attentiveness, which affects the cost scaling, λ_i . A well-informed voter with small λ_i can afford to buy more expensive signals (e.g. S_{cluster}) even when $\text{Info}(S_{\text{cluster}})$ is only modestly higher than $\text{Info}(S_{\text{wellbeing}})$. Conversely, low-attention voters with large λ_i may settle for the cheapest signal ($S_{\text{wellbeing}}$), even when its noise terms are large. The threshold value at which rational voters switch to buying $S_{\text{wellbeing}}$ instead of S_{cluster} may therefore be relatively high for some voters, and this could explain why some degree of pocketbook voting prevails.

Secondly, we assume γ_c is fixed for a given incumbent across periods. In reality, parties’ latent group alignments can shift from one term to the next (e.g. for tactical reasons). If γ_c^t evolves stochastically over time, voters face an extra layer of uncertainty. Formally, each signal S_p^t now informs about γ_c^t rather than a fixed γ_c . This only adds another noise term at the same scale for all signals, so it should not alter the overall ranking of information values. However, it may increase the value of forward-looking signals (S_j^{future}) if γ_c^t is volatile.

Summary of Model Results

The theoretical model illustrates how the translation problem varies across different social structures. In a homogeneous society with common interests, voters can just

ignore their personal experience entirely and rely on sociotropic signals to infer incumbent ‘competence’. Since any deviations from the average policy effect are pure noise, they contain no information about how the individual will fare under the incumbent in the future. This is, of course, unrealistic: there must be *some* signal hidden in the ways policy effects are distributed among voters.

The model accounts for this by adding the more realistic assumption that policy effects and interests are clustered. Under this assumption, voters prefer observing policy outcomes at the level of their own cluster or group. As long as there are meaningful cluster-level deviations from the average policy effect ($\theta > 0$), group-based signals provide strictly better estimates of voters’ future welfare than national signals. Specifically, national signals systematically miss the cluster-specific component γ_c , creating an irreducible estimation error that group signals avoid. Further, when interests are clustered, the policy-reasoning and experiential approaches become relatively more informative compared with the sociotropic approach since they contain more information about the interests of the individual’s cluster.

Still, group-level policy effects remain not just cheaper but also more informative than individual-level policy effects, for two reasons. By aggregating across individuals, group-level signals are less noisy, mirroring the benefits of sociotropic voting under conditions of common interests. Moreover, since the incumbent governs not at the level of individuals but at the level of clusters (i.e. there exist γ_c but not γ_i) personal experience has no advantage over group-based signals under conditions of clustered interest conflict. This is a key insight of the model: given that any interest alignments must be clustered at the level of shared interests, i.e. at γ_c , more granular signals than those at the group-level only come at the disadvantage of added noise.

These theoretical results integrate major approaches to the translation problem into one framework and shows how their attractiveness depends critically on the underlying structure of conflict in society. Importantly, they show what happens once we account for Kramer (1983)’s almost trivial observation that policy-making naturally operates at the level of shared interests or ‘clusters’ rather than individuals. As it turns out, group-based voting emerges in these settings as a rational response, even in the absence of any social psychological mechanisms rooted in identity-based esteem or status (Tajfel, 1974; Shayo, 2009) or other ‘consumption benefits’ from group conformity (Akerlof and Kranton, 2000). My argument thus demonstrates the purely informational

value of group-based voting, which makes it the preferred option even for voters who care narrowly about their personal future wellbeing. The next section examines which specific groups become most relevant for this information aggregation and how voters access group-level signals in practice.

Relevant Groups and Information Sources

In a world of clustered policy-making, individuals can advance their own interests by attending to the interests of their social groups. But not all groups are equally useful for this purpose. The formal model assumes a simplified setting in which each voter belongs to a single conflict-relevant group. In reality, voters inhabit multiple overlapping social groups defined by characteristics such as class, race, age, religion, geography and the like. Thus, when voters belong to a family of groups, $\{c_k\}_{k=1}^K$, they face an additional choice of which group they want to obtain information about.

Following the formal model, the relevance of a given group c_k depends on the informativeness of its corresponding signal, S_{cluster_k} , and the cost of obtaining it. Recall that a signal's informative value depends on how accurately it helps estimate the voter's future welfare $\phi = \mu + \gamma_k$. For group signals, this accuracy is determined by two factors: the magnitude of the group-specific effect $|\gamma_k|$ and the degree to which this effect can be cleanly observed (the noise reduction from aggregating over group members).

The key determinant is the group-specific clustering parameter:

$$\theta_k = \frac{|\gamma_k|}{|\mu| + |\gamma_k|}$$

Groups with higher θ_k are more relevant because the incumbent's group-specific treatment represents a larger share of the total systematic policy effect. When θ_k is large, using S_{nation} instead of S_{cluster_k} creates a substantial estimation error of magnitude γ_k . For instance, if manufacturing workers face $\gamma_{\text{manufacturing}} = -3$ while the average effect is $\mu = 2$, then relying on national signals would overestimate their future welfare by 3 units, making occupation-specific signals highly valuable for this group. Conversely, when $\theta_k \approx 0$, the group experiences policy effects very close to the national average, making group-specific signals no more informative than national signals.

What determines whether interests are correlated within a group? Fundamentally,

it depends on how many politically relevant characteristics the group shares. A characteristic such as age, locality, income, or gender, is politically relevant to the extent that it predicts how individuals are affected by government policy (p_i). A characteristic that is highly predictive of how voters are affected by policy is more politically relevant than one that predicts little variation. For example, when a new infrastructure policy targets rural areas, geographical characteristics become more predictive. Mechanically, the more such predictive traits group members have in common, the higher the group's relevance. Thus, stating that a signal about group c_k is highly informative is the same as stating that group c_k experiences highly correlated policy effects due to shared predictive characteristics.

The political relevance of any one trait is partly structural and partly contingent. Some characteristics, like income or employment status, are often structurally relevant across governments because they routinely shape how policies affect people (e.g., through taxation, welfare, or labor regulations). Others, such as religion or region, may vary more in relevance depending on shifting incumbent agendas. If a government, e.g., foregrounds support for rural voters, geographic traits become more predictive of policy outcomes. If a subsequent administration pivots to policies benefiting older adults, age-based groups become more relevant. Still, the structural component implies that group relevance is somewhat sticky, and not entirely determined by contemporaneous policy effects. This is important because it means that incumbents do not have full discretion in making groups relevant: simply targeting a given group with some policy benefit is not sufficient to make voters perceive it as relevant, e.g. if it is highly unusual or has very few structurally relevant characteristics. As such, the varying relevance of different groups is to some extent a constraint the incumbent is facing when making policy.

The second key factor shaping group relevance is the cost of obtaining information about the group's conditions and policy experience. Voters are more likely to rely on groups for which information is cheap and accessible. This raises the larger question: where does group-level information come from? I identify three main sources: informal social observation, political elite messaging, and the media.

First, individuals often acquire group-level information through everyday observation in their social environments. That is because social networks tend to be homophilous, meaning that people disproportionately interact with others who share

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key traits (Kossinets and Watts, 2009; McPherson, Smith-Lovin and Cook, 2001; Fu et al., 2012). This “social sampling” process has been widely documented: people draw political and economic inferences based on the experiences of those around them (Reeves and Gimpel, 2012; Kayser and Peress, 2024; Galesic, Olsson and Rieskamp, 2012; Christensen, 2025; Books and Prysby, 1999; Larsen et al., 2019). Some evidence even suggests that individuals weigh such information more heavily when it comes from economically similar peers (Alt et al., 2022). In this vein, when group members observe each others’ lives, they provide each other with information about how the political system treats their group. Voters can thus observe if any of their in-groups are being affected disproportionately by policies or facing challenges that call for political solutions. This dynamic can be understood as a kind of ‘bottom-up mobilization’ (Lüders et al., 2021; Westheuser and Zollinger, 2024).

Second, political elites provide group-based information through public communication. Party elites often frame their policy platforms in group-based terms (Huber and Haselmayer, 2024) and make clear their intent of helping specific groups with policies through their abundant use of rhetorical group appeals (Thau, 2019; Huber, 2022; Stuckelberger and Tresch, 2024). This kind of elite communication offers voters relatively direct information about both policy effects on groups as well as latent party-group alignments (γ_k). In contrast to socially sampled information, however, this top-down information is constrained by elites’ strategic choices about which groups to make salient. Elites can selectively “cheapen” the cost of information about a group by spotlighting it, like repeatedly emphasizing rural voters or seniors in campaign materials. This may be a genuine signal of group alignment or a tactical move aimed at “identity entrepreneurship” (Westheuser and Zollinger, 2024; Shayegh et al., 2022; Huber, 2022; Huber and Haselmayer, 2024).⁵ Thus, while elite messaging is often a low-cost source of group-level information, it may be provide somewhat less reliable signals about actual group-level policy effects.

Third, the media serves to both amplify and filter the other two sources (van Dalen et al., 2018). News coverage can bring attention to patterns of group-level policy

⁵Besides offering cheap information, it is worth noting that elites’ communication about groups can selectively prime voters’ group identities, i.e. temporarily increasing their psychological salience (Klar, 2013; Jackson, 2011). However, from the perspective of voters, this can be thought of as offering cheap information about γ_k (and thereby θ_k) for the communicator’s party. Thus the distinction between offering cheap information and priming is not obviously clear-cut.

consequences, publicize experiences that would otherwise remain localized, and frame policies in terms of group-specific effects (Shayegh et al., 2022; Andre et al., 2023). In addition, media coverage may simultaneously evaluate or corroborate elite narratives with facts on the ground, counteracting attempts at “identity entrepreneurship” that are not grounded in actual outcomes. In doing so, it plays an significant role in shaping which group signals are accessible to voters.

Taken together, these sources all provide relatively low-cost group-level information, but also cause variation in costs between groups. Social sampling provides especially low-cost information for highly homophilous groups, due to more information exchange between members. Elite messaging lowers costs for groups that parties choose to politicize. And the media helps further reduce costs for groups at the center of public narratives or policy debates. In this way, the cost of obtaining a signal S_{cluster_k} varies across groups and time, depending on social structure, elite strategy, and media attention.

In sum, voters inhabit multiple social groups that vary in relevance depending on both the value and availability of group-level information. Groups become relevant interfaces when they experience correlated policy effects; that is, when they are subject to meaningful incumbent alignment, whether positive or negative, and when information about their political treatment is relatively cheap to access. Social homophily and elite politicization jointly shape these information costs, which helps explain why some groups are more politically useful than others at a given time.

On a final note, this definition of group relevance partially overlaps with groups that are conventionally considered to have “strong” social identities, like certain occupational groups, age groups, ethnic groups, geographical groups, and educational groups. It also predicts that some highly relevant groups are cross-cutting, that is, combine several politically relevant traits, as with e.g. whites without a college degree. Such groups also often see stronger identification (Grigoryan, 2020). But it also diverges from identity-based theories in important ways. For example, occupational groups are not often studied as salient social identities, but they often exhibit high interest correlation and occasional elite politicization, making them highly relevant in this model. Conversely, some genders and sexual orientations are typically considered strong social identities, but if they are weak predictors of policy effects in the economic domain (which is the focus of this dissertation), they will not be among the most

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relevant interfaces. This aligns with some recent research suggesting that voters hold “economic identities” that are distinct from their social identities and play a larger role in voters’ reasoning about their economic interests (Nagler, Zilinsky and Linn, 2022). Thus, the informational theory of group relevance yields some differing predictions about which groups matter, and why. These predictions guide the empirical strategies in the chapters that follow.

Chapter 3

Issues in Methods and Research Design

This chapter discusses four key methodological challenges that arise when studying the group interface of politics and that are common across the dissertation's four empirical papers: the measurement challenge of selecting relevant social groups, the choice of appropriate outcome measures, the challenge of information spillovers between economic perceptions, and the endogeneity of economic outcomes to political attitudes. Throughout, the chapter emphasizes how the combination of observational and experimental approaches across the empirical papers helps address the complementary limitations of each method, providing both realistic evidence of group-based mechanisms in the real world and clean causal identification of the specific mechanisms involved.

Chapter 3

Studying the group interface of politics presents several methodological challenges that are more or less common across the four empirical papers in this dissertation. These challenges arise from the fundamental difficulty of isolating group-based mechanisms from other forms of political behavior, particularly when group membership, personal experiences and political attitudes are all endogenously related. This chapter discusses four key issues that shape the research design across all four papers: the measurement challenge of selecting relevant social groups, the choice of appropriate outcome measures, the challenge of information spillovers between economic perceptions, and the endogeneity of economic outcomes to political attitudes.

Each of these challenges reflects broader tensions in studying the psychological mechanisms underlying political behavior. The group interface theory posits that voters use information about their social groups as a signal for evaluating how well parties and policies serve their interests. However, this mechanism is easily confounded with other established forms of political reasoning that it operates alongside, including sociotropic evaluations (of national conditions) and egotropic evaluations (of personal circumstances). Isolating their independent causal roles of in behavior is typically very difficult.

The solutions I employ across the four papers involve a combination of observational and experimental approaches designed to isolate group-based mechanisms. Where possible, I leverage experimental designs that generate exogenous variation in group-level information while holding other factors constant. When relying on observational data, I mostly employ panel designs that leverage within-individual variation to minimize confounding. Throughout, I strive to be transparent about the limitations these methodological challenges impose on the conclusions that can be drawn.

Measuring In-Groups

A fundamental challenge in studying the group interface lies in defining and measuring social groups that meaningfully capture voters' own self-categorizations. The theoretical model in Chapter 2 suggests that groups become relevant when they are used to experiencing highly correlated policy effects and when information about these correlated effects and general group conditions are known. At a minimum, the knowledge of in-group conditions requires some subjective group membership, implying

that it is subjective rather than objective group memberships that should be measured. Beyond this, however, researchers cannot definitively predict group relevance a priori and very little descriptive research exists on such measures across countries. The core challenge resembles “looking for keys under a streetlight”: researchers can direct empirical attention toward specific groups, but cannot examine all potentially relevant groups simultaneously, and never know what might have been discovered by focusing elsewhere. This constraint is particularly acute in secondary data as well as in experimental research, where treatment designs require pre-defining relevant groups rather than allowing participants to inductively identify their own most salient group memberships.

Across the four papers, I address this challenge in several ways. First, I rely on groups that have established theoretical and empirical precedents as politically relevant in a given context. In Study 1 of the retrospective voting paper (Paper A), I focus on class and regional groups in Britain, which represent classical cleavage dimensions in British politics. In the distributive policy paper (Paper B), I examine rural/urban divisions in Denmark and racial groups in the United States, both of which are well-established as politically salient identities with distinct policy interests. In Paper B, I further let respondents choose which of their two stated in-groups they feel closest to, and subsequently base the experimental manipulation on that in-group.

Second, where experimental designs permit, I employ a “stimulus sampling” approach (Wells and Windschitl, 1999; Fong and Grimmer, 2023; Clifford, Leeper and Rainey, 2023) by including a broad range of potentially relevant groups rather than focusing on a small number of most-likely cases. This strategy, implemented most extensively in the retrospective voting experiments in Paper A and to some extent in the group linkages paper (Paper C), helps ensure that results do not depend on any group-specific sentiments or beliefs while providing more generalizable evidence about the mechanism.

Third, I generally attempt to use group categories that correspond to labels that people would recognize and apply to themselves, rather than artificial analytical constructions. In the experiments (Papers A and B) this is achieved in part by letting respondents pick their own in-groups from pre-defined lists and including some validation measures to ensure that the chosen group categories seemed natural. I also generally chose to use colloquial group labels like ‘the young’ and ‘the elderly’ rather

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than e.g. age brackets, and ‘the working class’ and ‘the middle class’ rather than more elaborate class schemas.

However, these strategies only partially address the fundamental measurement challenge. The studies necessarily constrain attention to pre-defined groups, which may not capture the most relevant group identities in the contexts studied. As such, the empirical tests presented here are likely somewhat conservative estimates of the group interface effects, since they are unlikely to be focusing on the most theoretically relevant groups in every case. In one paper, Paper B, I partially address this limitation by explicitly predicting and testing for heterogeneous effects across groups theorized to have varying political relevance. These results strongly indicate that the selection of groups is critical for the findings: effects are markedly positive for the groups predicted to be relevant while for other groups, the effects are either null or even negative.

As a final note, it is crucial for empirical testing of the theory that the notion of group relevance not become tautological. Given that empirical analyses will invariably reveal heterogeneous effects in finite samples, ‘relevance’ must not serve as a backdoor for selectively reporting the strongest results. It would therefore be methodologically inappropriate to examine heterogeneous effects and subsequently classify groups as relevant or irrelevant inductively, as this approach would artificially inflate results through random sampling variability. Such a strategy would inappropriately shield the theory from disconfirming evidence. Across my papers, I therefore ensure that I only make claims about heterogeneous effects when these are grounded in pre-registered expectations about relevant groups, rather than on relevance defined inductively or post-hoc based on observed results. When I have not pre-registered relevant groups, I base my conclusions on average effects across all measured groups, regardless of any heterogeneities.

Theorized and Measured Outcomes

The core theoretical argument centers on how group-level information shapes incumbent support and perceptions of incumbent-group alignment. These represent the ultimate dependent variables of interest as they capture the most important political consequences of the group interface mechanism. However, practical considerations often necessitate the use of more proximate outcome measures that serve as intermediate

steps on the causal pathway between group-level information and political attitudes. Moreover, when used in conjunction with the main outcomes, such intermediate variables can strengthen the evidence for the theorized mechanism by allowing additional observable implications to be tested.

I employ measures of incumbent support across all four papers, operationalized variously as vote intention, incumbent approval and support for the incumbent party. In Papers A and C, I additionally examine perceptions of incumbent-group alignment using survey items that ask respondents how well they believe parties “look after the interests of” various social groups. These measures provide more direct evidence of the theorized mechanism by capturing voters’ perceptions of the group-party linkages that mediate the relationship between group-level experiences and political support.

However, I also frequently rely on other, more proximate outcomes that represent intermediate steps in the theoretical chain of reasoning. For instance, in Paper B, the primary outcome is support for experimentally manipulated distributive policies rather than incumbent approval per se (although results for this measure are also reported). In Paper A, the key outcome is satisfaction with the state of the economy alongside incumbent approval for a subset of the experiments. These intermediate outcomes are chosen for several reasons.

Pragmatically, measuring incumbent attitudes sometimes makes less theoretical sense due to contextual factors. In one of the Danish experiments in Paper A, the government had only held office for a few months upon data collection, making it unreasonable to hold them accountable for recent economic trends. Similarly, in the hypothetical experimental designs in Paper B, it makes more sense to ask respondents for their preference for the presented hypothetical policy than to ask them how their incumbent support would hypothetically change if the hypothetical policy were implemented.

In addition, intermediate outcomes may allow for more precisely estimated effects because they are more proximate to the treatment (Meyvis and Van Osselaer, 2018). Since I refrain from using deceptive treatments across all experiments, manipulation design is constrained by reality in ways that occasionally limit treatment strength. The use of intermediate outcomes can thus increase statistical power to detect an effect given a fixed sample size and strength of manipulations. The main drawback is, of course, that the magnitude of the total effect of treatment on the ultimate outcome

becomes more ambiguous. Even in cases where the effect of the intermediate outcome on the ultimate outcome is known, the true size of the total effect cannot be known without further assumptions (Blackwell, Ma and Opacic, 2024).

Isolating Effects of Group-Based Perceptions

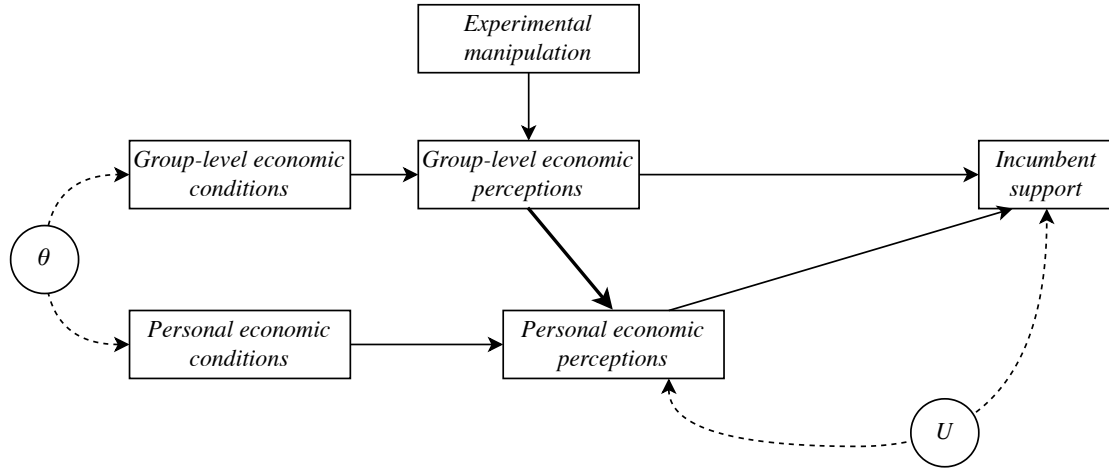
The formal model in Chapter 2 illustrates how information signals at one level (e.g. individual) can be used to infer conditions at other levels (e.g. national). For instance, voters might use personal economic experiences to infer broader economic trends, or they might use national economic information to update their perceptions of group-specific conditions. This represents a well-known methodological challenge in the literature attempting to separate sociotropic and egotropic voting (Books and Prysby, 1999; Kramer, 1983; Bechtel and Liesch, 2020). To credibly claim that perceptions of group-level conditions drive effects on incumbent support, one must exclude the possibility that these effects are driven by correlated perceptions of personal or national conditions.

This constitutes a thorny problem from the perspective of causal inference. To exclude such correlated factors from biasing an estimate of interest, practitioners commonly employ two remedies: statistical adjustment and experimental randomization. However, in their simplest form, both methods require that the correlated factors are either causally *upstream* from the variable of interest or spuriously associated through a third variable. If these factors are themselves *affected* by the variable of interest, these methods do not necessarily solve the inference problem and may create new issues. What makes this case difficult is precisely that people may use information about group-level economic conditions to infer something about personal or national economic conditions. This central methodological problem has plagued existing empirical research on the topic, and my proposed experimental solutions constitute key contributions of Papers A and B.

Excluding Personal Economic Perceptions

A key methodological challenge across Papers A and B lies in separating the effects of personal experiences from group-level influences. This challenge arises because, given

Figure 3.1. Directed acyclic graph illustrating the problem of separating the effects of personal and in-group perceptions.



intra-group correlated interests, individual and group experiences are mechanically related. When a social group experiences improved economic conditions, individual group members are also more likely to experience personal improvements, making it difficult to determine whether political responses reflect group-based or pocketbook motivations.

The directed acyclic graph (DAG) in Figure 3.1 illustrates this issue. Nodes represent variables and directed edges denote effects of one variable on another that are unmediated by other variables in the graph. Round nodes and dashed arrows illustrate unobservables. The effect of interest is indicated by the path from group-level economic conditions through group-level economic perceptions to incumbent support.

As shown, group-level economic conditions and personal economic conditions are mechanically related by the clustering of policy effects, i.e. θ in the formal model in Chapter 2. They each affect group-level economic perceptions and personal economic perceptions, respectively. Since both variables affect incumbent support, naive correlations between group-level economic conditions (or perceptions) and incumbent support are likely to overstate the relationship.

My strategy for addressing this challenge varies between the experiments and observational analyses. In the experiments, I exogenously vary information about group-level conditions or group-level policy effects in Papers A and B, as shown by the “experimental manipulation” node in Figure 3.1. This experimental intervention

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breaks the natural correlation between group-level and personal economic conditions and thereby closes the so-called biasing backdoor-path via θ . It further closes any other biasing backdoor paths between group-level economic perceptions and incumbent support.

Nevertheless, experimentally manipulating group-level economic perceptions is not sufficient to isolate their effect on incumbent support. Specifically, one must make the additional assumption that there are no downstream effects of group-level economic perceptions on personal economic perceptions (as shown by the bold arrow). Such “information spillovers” would threaten the validity of the experimental inference by violating the assumption of “information equivalence” (Dafoe, Zhang and Caughey, 2018). The risk is that personal economic perceptions rather than group-level may be driving the observed effect.

I overcome this problem in the experiments in two ways. In Paper B I simultaneously and independently vary perceptions of personal policy gains, thereby avoiding spillovers by design. In Paper A, I instead measure personal economic perceptions post-treatment to test whether any such spillovers have occurred. Fortunately, there is no evidence that voters update evaluations of their personal economic conditions when informed about group-level economic conditions in this experimental setting.

In the observational analyses, I approach the problem by instead controlling for personal policy effects and pocketbook evaluations to estimate the independent effects of group-level conditions. By conditioning on personal economic perceptions, I close the biasing backdoor path via θ . However, even assuming no other biasing backdoor paths between group-level economic perceptions and incumbent support, this strategy is not perfect for isolating the effect. The problem is that conditioning on personal economic perceptions may introduce new bias insofar as these perceptions are themselves downstream from group-level economic perceptions. The reason is, again, the risk of information spillovers shown by the bold arrow in Figure 3.1. If personal economic perceptions are spuriously associated with incumbent support through unobserved variables (the U node), personal economic perceptions become a collider variable on the path between group-level economic perceptions and incumbent support. Conditioning on the collider opens up a biasing backdoor path through that variable via U , essentially introducing some of the spurious association into the estimated relationship (Elwert and Winship, 2014). Fortunately, the evidence from Paper A suggests little

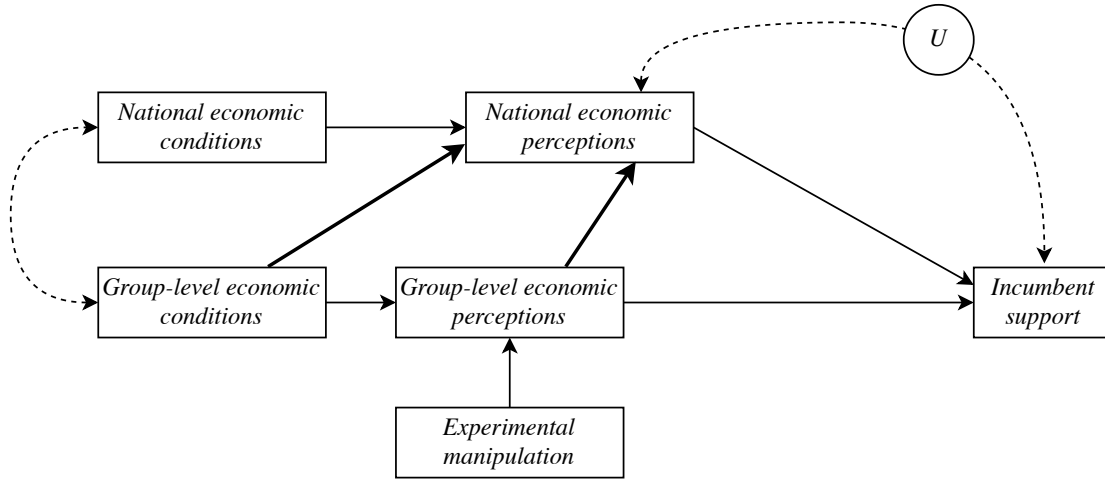
such information spillover occurs under controlled conditions, implying that personal economic perceptions are not an important collider variable. I am further reassured by the fact that observational estimates remain very similar when adjusting and not adjusting for personal economic perceptions.

Excluding National Economic Perceptions

A very similar inference problem occurs for national economic perceptions. As shown in the DAG in Figure 3.2, economic conditions at the national and group levels influence incumbent support independently through economic perceptions of each. However, as shown with the dashed bidirectional arrow, national conditions and group-level conditions are correlated (on the reasonable assumption that $\theta < 1$, i.e. some degree of common interests exists). In addition, as shown with the leftmost bold arrow, national economic perceptions are likely shaped by group-level economic conditions. This occurs because voters are known to make inferences from their immediate social or geographical context to the national economy (Books and Prysby, 1999; Hjorth, Dinesen and Sønderskov, 2016; Larsen et al., 2019; Mondak, Mutz and Huckfeldt, 1996; Galesic, Olsson and Rieskamp, 2012). Given social homophily (McPherson, Smith-Lovin and Cook, 2001), it is highly likely that economic conditions in voters' in-groups shape their perceptions of the national economy (as shown by the leftmost bold arrow). In sum, the effect of group-level economic perceptions on incumbent support is biased by backdoor paths via national economic perceptions.

I tackle this part of the inference problem by both experimentally manipulating group-level economic perceptions (using information treatments) in Papers A and B, and by statistically adjusting for national economic perceptions in an observational study in Paper A. While this closes the backdoor paths in question, it runs into a similar problem as discussed earlier. Like personal economic perceptions, national economic perceptions may be shaped by group-level economic perceptions as shown by the rightmost bold arrow in Figure 3.2. Voters may use, e.g., experimental information about economic conditions in their group to infer something about the state of the national economy, again violating the assumption of information equivalence. In observational settings, this implies that national economic perceptions are likely both a confounder and a collider variable. Conditioning on it (to block the backdoor path)

Figure 3.2. Directed acyclic graph illustrating the problem of separating the effects of national and in-group perceptions.



may therefore introduce new bias by opening a new backdoor path via U .

Experimentally, I overcome this problem by including identical information on national economic conditions across experimental treatments. This ensures that respondents do not make different inferences about national conditions from the varying group-level information, essentially holding national economic perceptions fixed.

In the observational analysis in Paper A, no similar remedy exists and the risk of collider bias from conditioning on national perceptions remains. However, any such collider bias would likely deflate rather than inflate the effect estimates due to the nature of U . For any unobserved variable U to bias estimates upward, it would have to causally affect national economic perceptions and incumbent support in opposite directions (Elwert and Winship, 2014). Most reasonable contenders for U would presumably affect the two variables in the same direction, however. The usual suspects such as partisanship, incumbent attitudes and personal economic conditions would therefore all result in conservative bias. The strategy of conditioning on national economic conditions would thus only make for a harder test of the theorized effect.

Endogeneity of Economic Outcomes

A general challenge in economic voting research lies in the endogeneity of economic outcomes to especially incumbent support. This endogeneity can arise through sev-

eral mechanisms: reverse causality where incumbents strategically target benefits toward existing supporters, dynamic confounding where external shocks simultaneously affect economic conditions and political support, and long-standing party-group coalitions that create systematic relationships between group membership and partisan attachments.

These endogeneity concerns also arise when studying the group interface because many politically relevant groups have established ties to particular parties or ideological positions. For instance, rural voters in many countries have developed systematic alignments with conservative parties, while urban professionals tend to support liberal parties (Hooghe and Marks, 2018). When these groups experience different economic trajectories, it becomes difficult to determine whether political responses reflect genuine retrospective evaluation or simply represent long-standing partisan loyalties. Likewise, the tendency of voters belonging to economically well-performing groups to be more favorable towards the incumbent may reflect incumbents favoring their pre-existing supporters rather than electoral responses to group-level conditions.

I employ several strategies to address these endogeneity concerns across the three papers on economic voting (A, B and D). In the observational analyses, I rely heavily on panel data designs to leverage within-individual variation. I use a combination of individual fixed effects to control for time-invariant confounders, including stable group identities and partisan attachments, and dynamic specifications with lagged dependent variables to control for time-varying (but not time-invariant) unobservables. All three papers employ such models that identify effects from within-individual variation over time, substantially reducing concerns about selection and reverse causality. In one study in Paper B, I instead control for deterministic eligibility criteria for distributive benefits, which helps approximate random assignment for benefit receipt.

However, the most complete solution to endogeneity concerns involves the experimental designs implemented in Papers A and B. The use of experiments is becoming increasingly widespread to tackle this endogeneity problem in the economic voting literature (Healy and Malhotra, 2010; Hart and Matthews, 2023; Simonovits, 2015; Bechtel and Liesch, 2020). I follow this approach in using experiments to generate exogenous variation in perceptions of group economic performance through random assignment of information treatments. Participants cannot select into treatment conditions based on their pre-existing attitudes or group characteristics, eliminating reverse causality

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and selection concerns.

The experimental approach does introduce different limitations, particularly around external validity. Laboratory manipulations of economic information do not capture how voters actually learn about group conditions in real political environments. Experiments thus say little about the extent to which voters hold and reliably update perceptions of in-group economic conditions in response to changes on the ground. Moreover, the short-term effects observed in experiments may not reflect the longer-term processes through which group experiences shape political attachments and party coalitions.

I therefore employ a combination of observational and experimental evidence in several papers to address these complementary limitations. On the one hand, the observational analyses (especially in Papers A and B) provide evidence that the group interface operates in realistic political settings over meaningful time periods and that voters hold substantive and meaningful perceptions of in-group economic conditions. On the other hand, the experiments provide cleaner causal identification of the specific mechanisms involved. Together, they suggest that the group interface represents a genuine feature of political behavior rather than an artifact of endogenous relationships between group membership and political attitudes.

Chapter 4

Key Findings and Implications

This chapter summarizes and synthesizes the key findings from the dissertation's four empirical papers and discusses their broader implications for research and the workings of democracy. It highlights three key implications of the group interface theory of politics. First, the group interface invites a broader conception of self-interested behavior, showing how voters may pursue self-interest through group-based rather than individualistic reasoning. Second, it reveals the limits of personal experience as a guide for political decision-making. Third, it examines the consequences for democratic accountability, showing how group-based voting can both enhance voters' ability to discipline incumbents by overcoming attribution problems and alter the incentives facing politicians. The chapter concludes by identifying important areas for future research, including questions about the accuracy of group-level perceptions, the role of media and opposition parties in providing group-based information, and the need for theoretical models of how group-based voter behavior shapes incumbent incentives.

Table 4.1: Overview of data and research designs used in articles

Research Question	Data	Methods
A: Do voters sanction incumbents retrospectively for in-group economic conditions?	British election study survey panel (BES) and three original survey experiments fielded in Denmark and the US (original survey 1, 2 and 3).	Survey experiments and panel data estimation.
B: Are voters' responses to distributive policies shaped by perceptions of in-group benefit?	US survey panel and three original survey experiments fielded in Denmark and the US (original survey 2, 4 and 5).	Survey experiments and panel data estimation.
C: Do voters' perceptions of party-group linkages track group appeals in party rhetoric?	British national election studies and survey panel data (BES) and parliamentary speeches from 1997-2022.	Language models trained for automated classification of party rhetoric and panel data estimation.
D: Do voters sanction incumbents for effects of policy on their incomes?	British survey panel data (UKHLS) with yearly waves of 40,000 households from 2010-2019 and data on nearly all tax-and-transfer policies enacted in the UK.	Policy micro-simulation models (UKMOD) and panel data estimation.

A: Retrospective Voting

This paper investigates whether voters evaluate incumbent performance based on the economic conditions of their social in-groups, rather than solely on personal finances (pocketbook voting) or the national economy (sociotropic voting). Standard models of retrospective voting emphasize the national economy as the primary yardstick for evaluating government performance, given its visibility and shared relevance. Yet, this view overlooks the clustered and conflicting nature of interests in society and thus the heterogeneity of economic experiences across social groups. In line with the

theory of groups as the interface of politics, this paper theorizes that voters engage in *group-based retrospective voting*, in which they judge incumbents based on how their social group has fared economically, especially relative to the broader national trend. This behavior would provide a way for voters to assess whether incumbents are aligned with the interests of people like them.

The paper combines observational and experimental evidence to test this theory. First, it uses panel data from the British Election Study to track changes in voters' perceptions of in-group economic performance and incumbent support over time. Social groups are defined by region, class, and other sociodemographic traits. The analysis focuses on within-individual changes, controlling for individuals' sociotropic and pocketbook evaluations. Second, the paper presents three pre-registered survey experiments fielded in Denmark and the United States. In each experiment, respondents are randomly assigned to receive information about the economic performance of different groups (based on age, education, ethnicity, class, and geography) randomly varying whether the stimulus group is an in- or out-group. This provides a strong design for isolating the causal effect of perceived in-group economic outcomes on attitudes towards the incumbent and the state of the economy.

The observational analysis shows that in-group economic performance is strongly associated with support for the incumbent and perceived alignment between the incumbent and respondents' in-groups. These effects are comparable in size to traditional sociotropic effects and persist after accounting for sociotropic and pocketbook evaluations. This suggests that group-level outcomes exert an independent influence on voters' incumbent evaluations, even when personal and national trends are held constant.

The experimental evidence bolsters this conclusion, demonstrating that exposure to positive or negative information about in-group economic performance significantly shifts both economic sentiment and incumbent approval. These effects are strongest when the group is either outperforming or underperforming relative to the national economy. This is consistent with the informational argument, as relative performance is a stronger signal of the incumbent's alignment with the in-group compared to out-groups. The fact that group-based retrospective voting operates independently of both pocketbook and sociotropic considerations clarifies that voters do not simply treat group outcomes as proxies for their own finances or for general macroeconomic conditions. Rather, they interpret group economic trajectories as meaningful political

signals of the incumbent's group loyalties.

The overarching contribution of the paper is to extend retrospective voting theories to economic evaluations of voters' own in-groups. In doing so, it provides direct empirical support for the dissertation's first theoretical prediction: that voters monitor the economic standing of their in-groups and use this information to assess government performance. It thus presents a crucial piece of evidence supporting the argument that social groups act as a political interface through which voters link their personal interests and experiences to political choice.

B: Distributive Policies

This paper examines how voters respond to distributive government policies that target them with material benefits. While standard models in political economy predict that voters will reward incumbents for such policies in line with a pocketbook voting logic, this mechanism has received little empirical scrutiny. This paper offers an alternative explanation that is equally consistent with the data: that voters do not merely respond to their personal gains from policy but also to whether they perceive the policy as benefiting salient social in-groups. In line with the logic of the group interface, when voters interpret a policy as helping "people like me," they view it as a signal of the incumbent's alignment with their interests, regardless of their personal benefit. Moreover, I theorize that voters are likely to infer from their personal benefit that their in-groups benefited, thus making personal benefit endogenous to perceived in-group benefit.

First, using survey data on COVID-era stimulus checks in Denmark and the United States, I show that check recipients became more likely to believe their racial or geographical in-groups also benefited. These findings reveal that distributive perceptions are endogenous to personal benefit, casting doubt on the common attribution of policy effects to pocketbook voting. Second, to isolate the causal role of group-based perceptions, I field three pre-registered experiments in the two countries, randomly varying features of hypothetical cash transfer policies. Across experiments, I find that voters' political support depends at least as much on perceived in-group benefit as on personal gain. Importantly, these effects are highly group-dependent, emerging only for groups with strong political identities. Together, the findings show that group-based responses,

not just pocketbook concerns, shape how voters react to policies that benefit them, helping explain the wide variation in policy effects across contexts.

The paper tests this group-based mechanism using two complementary research designs. First, using survey data on COVID-era stimulus checks during their roll-out in Denmark and the United States, it measures whether people benefited from stimulus checks and how they believed these policies helped their racial or geographic in-groups. In line with expectations, check recipients became more likely to believe their racial or geographical in-groups also benefited from the policy. This finding suggests that distributive perceptions are endogenous to personal benefit, casting doubt on the common attribution of policy effects to pocketbook voting.

Second, the paper presents three pre-registered survey experiments, also fielded in Denmark and the U.S., in which participants evaluate hypothetical cash transfer policies. The experimental design independently varies whether respondents personally benefit from the policy and whether their in-group benefits. This allows for clean identification of the relative impact of personal versus in-group benefit on policy approval and incumbent support. As expected, voters are more supportive of policies benefiting their in-groups, holding their personal benefit constant. In many cases, the in-group benefit effect is as large or larger than the pocketbook effect. However, effects are strongly heterogeneous in line with pre-registered expectations about strong and weak group identities. This can help explain why past studies have found inconsistent electoral responses to distributive spending, as they depend on the extent to which a targeted group matters to voters. In effect, not all policies that confer benefits generate the same political returns: it depends on who benefits.

Together, these findings challenge the standard assumption that distributive policies operate politically through personal pocketbook considerations. Instead, they support a main empirical prediction of the dissertation: that voters evaluate policies through their group-level implications, not just individual material consequences.

C: Group Linkages

This paper investigates how citizens form and update perceptions of which political parties represent the interests of particular social groups—what the literature terms *party-group linkages*. While traditional cleavage theories emphasize the stability of such

linkages as expressions of long-standing group-party alignments, recent work suggests that parties may play a more active role in shaping these perceptions. This paper advances that argument by proposing that voters maintain “running tallies” of party-group associations that respond to elite rhetoric, even in the short term. Specifically, it theorizes that party leaders can shape voters’ perceptions by invoking social groups in positive or negative terms, and that citizens update their beliefs accordingly. This claim speaks directly to the dissertation’s broader argument, in which group-level signals help linking individual voters to parties. If voters monitor how parties speak about their social groups and update their perceptions in response, then group-based rhetoric provides a tractable mechanism through which group-party alignment becomes politically consequential.

To evaluate this theory, the paper develops a novel empirical strategy that combines survey measures of perceived party-group linkages with an automated analysis of rhetorical group appeals in political speech. The study uses British Election Study (BES) cross-sectional and panel surveys from 1997 to 2022 that ask respondents which parties best represent the interests of various social groups. To capture elite group appeals over the same period, we train a language model to classify and score all group references in speeches made by party leaders in the UK House of Commons. The model assigns valence scores to group references, distinguishing between positive, negative, and neutral mentions, and aggregates them over time by party and group. These rhetorical scores are then linked to survey respondents’ perceptions of party-group linkages, allowing us to estimate whether changes in elite rhetoric systematically predict shifts in perceived party alignment with different social groups.

The key finding is that group appeals in elite rhetoric strongly predict voters’ perceptions of party-group linkages over time. When a party speaks, e.g., more positively about a group in parliamentary speeches, voters become significantly more likely to view that party as aligned with the group’s interests. A one standard deviation increase in valenced group appeals is associated with a roughly 12 percentage point increase in perceptions that a party is seen as representing a given group. This effect is robust across a range of model specifications, including individual fixed effects, providing strong evidence that voters track elite signals and incorporate them into their assessments of how much parties care about salient social groups. Further, the relationship is roughly an order of magnitude stronger for appeals that mention policy, compared

with purely ‘symbolic’ appeals, suggesting that voters ultimately care most about group appeals when they are linked to policy intent. Consistent with the mechanism, voters also appear more responsive to political rhetoric during periods where they more closely follow the news.

The broader takeaway is that voters’ perceptions of group-party alignment are highly sensitive to party rhetoric. Far from being fixed reflections of social cleavages, perceptions of which party represents which group are regularly updated in response to elite communication. This paper thus contributes to the dissertation’s overarching theory by empirically validating one of its core behavioral predictions: that voters care about and track group-level political signals in elite rhetoric. It demonstrates how group-party linkages are formed and maintained, thereby supporting the broader claim that group identities function as the primary interface through which voters interpret party behavior and political representation.

D: Pocketbook Voting

This paper asks whether voters hold incumbents accountable for the specific effects of government policy on their personal disposable incomes. It revisits the classic theory of pocketbook voting and evaluates a central assumption underlying its capacity for democratic accountability: that such behavior is *policy-sensitive*. That is, for pocketbook voting to reliably shape incumbents’ incentives, voters must disproportionately respond to income changes that are caused by government policies rather than to idiosyncratic economic variation. Existing literature disagrees as to whether pocketbook voting is thus “policy-based”, driven by policy-induced income changes, or merely “outcome-based”, driven by total income changes. While previous studies have shown that voters sometimes react to specific policies that affect their material well-being, they cannot definitively adjudicate between these alternative modes of pocketbook voting. Specifically, studies of single policies cannot distinguish voters responding to the income shock itself or to income changes regardless of their origin, and also focus mostly on high-salience policies that say little about how voters respond to policies on average. Other studies on how voters attribute responsibility for income changes show that voters are more sensitive to economic changes they believe are caused by policy. Yet, they rely on potentially biased self-reports which say little about how reliably

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voters distinguish actual policy effects. In sum, they leave open the question of how consistently and accurately voters respond to the policy-induced component of their income. This question is directly relevant to the dissertation's broader theoretical claim that voters need group-level information since they cannot reliably use their personal economic experience to translate individual interests into political choices.

To assess the policy-sensitivity of pocketbook voting, the paper develops a novel empirical approach that links large-scale survey panel data from the United Kingdom to a sophisticated policy microsimulation model. Using ten waves of UK Household Longitudinal Study data (2010–2019), covering around 40,000 households annually, it tracks changes in respondents' disposable income over time. These income changes are then decomposed into two components using UKMOD, a policy microsimulation model that models all national tax-and-transfer policies in a given year: (1) the portion of income change directly attributable to government policy and (2) a residual component capturing all other income fluctuations. This decomposition allows the paper to assess whether voters systematically respond more to the policy-induced component or whether they just respond to total income changes. Together, this design provides the most direct test to date of how reliably pocketbook voting sanctions incumbents for the direct income effects of their policies.

The first key finding is that voters do not privilege policy-induced income changes over residual changes when deciding whether to support the incumbent. Across specifications, the analysis finds no evidence that voters place greater weight on income gains or losses caused by government policy relative to those caused by other factors. In other words, there is little behavioral evidence of policy-based pocketbook voting. This pattern holds across different model setups and across subgroups in the electorate, and even for voters reporting a strong interest in politics. This challenges a core assumption in much research on the political economy of elections, which often assumes or implies that voters can distinguish policy-driven gains from other economic experiences.

The second key finding is that total income changes, i.e. the basis for most experiential voting, are a poor proxy for the actual effects of policy. While some theories have suggested that voters might not need to make precise attributions as long as total income tracks policy well (Fiorina's model of "rough justice"), the data show that policy-induced income variation makes up only a small part of total income variation.

At the median, residual income fluctuations account for roughly three-quarters of total disposable income changes, meaning that the majority of what voters experience in their finances is not attributable to government policy. As a result, even when voters vote based on personal income, they are likely responding to “noise” rather than to the incumbent’s actual policies. I estimate that if voters simply respond to total income changes, they will correctly reward or punish the incumbent in line with their policy-induced income changes only 58% of the time – just slightly better than chance. This lack of alignment significantly undermines the disciplining potential of pocketbook voting in practice.

Taken together, these findings suggest that personal economic experience is not a reliable vehicle for translating policy effects into electoral behavior. Voters do sometimes respond to changes in their financial situation, but not in a way that reflects the actual policy record of the incumbent. Consistent with the demandingness of the policy-reasoning approach, voters do not manage to separate out and act on ‘policy-induced’ income changes but instead act on total income changes, i.e. experiential voting. Further, when voting experientially, voters mostly sanction incumbents for noisy income changes unrelated to their policy-making. The paper thus makes an important empirical contribution to the dissertation’s broader argument, suggesting that personal experience is not a reliable solution to the translation problem. It thus motivates the turn to social groups as the more tractable and politically meaningful interface between policy-making and voter behavior, which the other papers investigate more directly.

Key Implications of the Dissertation

A Wider Conception of Self-Interested Behavior

The dissertation invites a broader conception of self-interest than is often studied in political behavior by showing how voters may understand and act on self-interest in broader, group-based terms. In doing so, it challenges the conventional view in political science that self-interest plays only a limited role in shaping political attitudes and behavior. This perspective is particularly influential in studies of political behavior related to the economy and economic policy, where self-interest is easy to operationalize (narrowly) as material personal gains or losses. Classic studies find

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that voters rarely respond to such pocketbook concerns and that sociotropic (national-level) considerations dominate (Sears et al., 1980; Sears and Funk, 1991; Feldman, 1982; Mutz, 2018). As Huddy (2013) summarize this literature, “self-interest has had very circumscribed and limited effects on a range of policies” (p. 740). Because self-interest should be especially clear and consequential in the economic domain, the absence of strong personal-level effects has been taken as decisive evidence against the political significance of self-interest.

However, this conclusion rests on an overly narrow model of how voters perceive and act on their interests. On the account developed here, the limited role of direct personal experience does not imply that voters are indifferent to their own well-being; only that they use different informational strategies to connect their interests to politics. In a complex policy environment where personal signals are noisy and weak, the most efficient way to track one’s material interests may be to follow group-level patterns. Far from irrational or purely “tribal” behavior (Achen and Bartels, 2016, p. 325), group-based responses may thus reflect a more sophisticated and forward-looking form of self-interest that relies on group-level information to infer the alignment of parties with one’s broader economic and social interests. In this sense, group-based reasoning does not replace self-interest but is a way of pursuing it.

This broader conception of self-interest also helps explain why group-level information is so valuable to voters and why political appeals are often framed in group-based or identity-laden terms. As Kalin and Sambanis (2018) note in their review of the social identity literature in political science, many existing theories struggle to explain “why appeals couched in identity, rather than a more direct appeal to materialist motivations, are even necessary,” and tend to reduce the power of such appeals to irrationality or “false consciousness” (p. 241). From the perspective developed here, this puzzle dissolves: appeals to group identities may function as an efficient way to communicate material stakes to voters. When voters lack detailed knowledge of policy but can recognize the group implications of political decisions, identity-based messaging becomes an effective way to communicate economic alignment.

Thus, rather than demonstrating the irrelevance of self-interest, existing findings may instead reflect the limits of overly individualistic measurement strategies. Voters’ political reasoning is often more sophisticated than it appears: they may pursue self-interest not as isolated individuals, but as members of groups that help them map their

experience onto the political landscape. In this light, self-interest may play a broader and more central role in political behavior than conventionally assumed.

The Limits of Personal Experience

A longstanding assumption in political behavior research is that personal experience offers voters a straightforward and accessible basis for connecting their self-interest to politics. As Fiorina (1981) famously argued, voters “need only calculate the changes in their own welfare” (p. 5) to assess incumbent performance. This assumption underpins much empirical work on economic voting and policy feedback, which treats voters’ responses to personal economic gains or losses as evidence of self-interested behavior (e.g., Tilley, Neundorff and Hobolt, 2018; Healy, Persson and Snowberg, 2017; Lerman and McCabe, 2017). It also forms the foundation of strategic models of distributive politics, which presume that politicians can win support through tactical redistribution to individual voters (Levitt and Snyder Jr, 1997; Dixit and Londregan, 1996).

However, as the formal model in Chapter 2 shows, this assumption may be too simplistic about the informational challenge voters face. In practice, personal experience is often a poor guide to a government’s political alignments. Individual-level outcomes reflect substantial idiosyncratic noise and are generally weakly correlated with the effects of policy. Even if voters attend closely to their own welfare, personal outcomes provide little information about the incumbent’s alignment with their underlying interests. Moreover, personal experience is a weak predictor of how the incumbent will govern in the future, which is ultimately the relevant concern for voters acting to further their interests.

From the perspective of rational information use, voters are better served by aggregating across individuals. Group-level signals filter out individual-level noise and offer a more reliable basis for identifying structural policy effects. This is exactly the kind of information needed to judge whether continued support will advance one’s own interests in the longer term.

The limited informational value of personal experience is underpinned by the empirical findings in the dissertation. Paper D shows that personal economic experiences often fail to translate into accurate or consistent political responses: voters struggle to detect and act on policy-induced income changes, leading to indiscriminate pocketbook

voting that poorly reflects actual distributional policy effects. Paper B further demonstrates that effects commonly attributed to personal benefit in the policy feedback literature may, in fact, be driven to a large extent by correlated group-level experiences.

Together, these findings challenge the importance of experiential voting in two ways. First, they suggest that personal experience-based voting may be less widespread than commonly assumed, with apparent pocketbook effects potentially masking underlying group-based behaviors. Second, they demonstrate that when voters do rely on personal experience alone, they may largely be responding to idiosyncratic noise. This makes experiential voting an unreliable source of democratic accountability.

Incumbent Incentives and Democratic Accountability

The perhaps most important implications of this dissertation relate to democratic accountability. While I have argued that group-based behavior allows voters to more effectively pursue their interests, it offers more ambiguous normative implications for democracy. The fact that voters engage in group-based voting has several implications for the incentives that incumbents face and the institutional conditions necessary to sustain accountability.

First and foremost, group-based voting may enhance voters' ability to effectively discipline the incumbent because it allows voters to overcome key attribution problems. Individual income changes are noisy signals of government performance, but when voters consider how policies affect their social groups, they can extract more meaningful and structured cues about incumbent priorities. In contrast to pocketbook or sociotropic voting models that rely on either personal experience or national aggregates, group-level signals reflect how government actions shape the distribution of gains and losses across competing interests in society. In doing so, group-based reasoning can generate stronger, more targeted electoral sanctions for incumbents who neglect voters' interests. This runs counter to prevailing skepticism that group politics undermines accountability. In their rather pessimistic closing chapter, Achen and Bartels (2016) argue that "group politics does not automatically mitigate the challenges to effective political accountability" (p. 319) and call for a "clearer empirical understanding of how group politics works" (p. 325) before more optimistic normative conclusions can be drawn. Where Achen and Bartels express skepticism that voters can successfully hold

“politicians accountable for pursuing group interests” (p. 309), this dissertation suggests that when grounded in shared experience and material outcomes, group-based behavior may help voters pursue their interests more effectively than voting based on personal experience or even policy reasoning.

However, group-based voting alters not just the strength but also the content of the incentives that incumbents face. Rather than maximizing aggregate welfare, incumbents thus face electoral pressures to favor the interests of specific groups. Whereas sociotropic models imply that politicians can expect to benefit from positive-sum policies even if they benefit some more than others, group-based models undermine this incentive as voters may respond to relative losses even amid aggregate gains and vice versa. This may promote more inclusive growth, as incumbents can expect punishment for neglecting disadvantaged groups. But it also introduces new risks. In particular, incumbents can expect greater rewards from reallocating resources selectively to small but pivotal constituencies rather than fostering national economic growth. This dynamic echoes (Ferejohn, 1986)’s argument with respect to pocketbook voting, where weaker sociotropic voting enables incumbents to ‘divide and rule’, undermining the electorate’s ability to incentivize public-regarding policies. This concern is also present in Drazen and Eslava (2006)’s argument that targeted spending becomes more informative when it is perceived as benefiting ‘similar others’ rather than individuals in isolation. This makes it more rational for forward-looking voters to shift their incumbent support even when they recognize that they may be subject to tactical pandering. These incentives for redistribution have traditionally been a major concern in the literature on the political economy of elections, where it is seen as a significant source of economic mismanagement and weakened democratic control (Tufte, 1978; Maskin and Tirole, 2019). Similarly to pocketbook voting, it may thus discourage incumbents from pursuing prudent, broadly beneficial economic policies if such measures impose costs on key parts of the incumbent’s electoral coalition, as is often the case with policies related to, e.g., the green transition or automation (Beiser-McGrath and Bernauer, 2023; Colantone et al., 2023; Bolet, Green and Gonzalez-Eguino, 2023; Stokes, 2016).

At the same time, group-based voting may make tactical redistribution more electorally potent but it also makes it more complicated to achieve than standard pocketbook models suggest. In standard pocketbook-based models of targeted spending, incumbents can target policies at precise slivers of the electorate to swing or turn

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out voters that secure their re-election (Ferejohn, 1986; Battaglini and Coate, 2007, 2008; Tufte, 1978). But if voters react primarily to group-level effects, incumbents must consider not just who benefits materially, but how those benefits map onto salient group identities. As shown in Paper B, some groups elicit stronger responses than others and this complicates incumbents' targeting calculus as they must target the right groups to activate group-based responses and targeting the wrong group may even lead to backlash. The result is a more constrained and less flexible form of tactical redistribution. Group-based behavior may thus imply that tactical redistribution is more powerful where it works but more limited in where it can be applied.

A further complication is that the effectiveness of group-based accountability depends on the salience and framing of group identities, which are factors that are themselves shaped by elite communication. Because voters evaluate economic outcomes through the lens of social groups, governments may be able to strategically prime certain identities to frame their economic management favorably and numb accountability pressures. Such "identity entrepreneurship" (Shayegh et al., 2022; Klar, 2013; Jackson, 2011) may allow incumbents to influence the very basis on which they are held accountable. However, it may also empower opposition parties to challenge such frames and make important group conditions salient. This makes political communication even more powerful for reshaping the meaning of economic outcomes and influencing which groups demand electoral responsiveness.

Depending on the normative weights of these competing arguments, group-based accountability dynamics have some implications for the design of democratic institutions. If group-level perceptions structure effective economic accountability, then accurate and accessible information about group-level economic trends becomes a public good, even though it may appear divisive. Media organizations, civil society, and opposition parties thus play crucial roles in surfacing and scrutinizing such group-level trends. Given that voters' perceptions of incumbent-group alignments matter, and given that elites can effectively shape them with rhetoric (Paper C), it is important that the media reports on group-level trends and corroborates elite claims about such trends. In contrast to conventional warnings about fanning the flames of 'tribal' group-based discourse, the problem may thus lie not in the use of group frames per se, but in the absence of mechanisms to ensure their empirical accuracy. The key challenge, then, is not to avoid group-level reasoning, but to ground it in real, observable information.

Finally, these implications raise some new questions for future research.

- **Perceptions of group performance:** How accurate are voters' perceptions of how their groups are doing, and to what extent are these perceptions shaped by lived experience versus elite messaging? Are voters focusing on the groups where the stakes are highest? The answers to these questions are important to understand the extent to which group-based voting exerts a constraint on incumbents.
- **The groups that matter:** Which groups are most relevant for the interface in practice? While empirical studies grounded in social identity theory tend to focus on group identities with strong affective ties, this dissertation argues that groups with shared interests may matter more, but this claim is not tested systematically. Some recent work by Nagler, Zilinsky and Linn (2022) on "economic identities" has begun taking steps in this direction, showing that when asked to think of "people like me" in an economic context, the groups people have in mind are very different from classical social identity groups. More empirical research is needed to understand which group lenses voters adopt when evaluating policies and performance.
- **Elite communication:** Given that voters use party rhetoric to infer group-party alignments, how closely does such rhetoric track actual policy commitments and distributive effects of policy? Does group-based communication broaden access to valuable information or is it misleading "cheap talk" (Foster, 2021; Crawford and Sobel, 1982)?
- **The role of the media:** How effective are media and opposition actors at informing voters about group-level trends, corroborating elite narratives about group alignments, and holding governments accountable on behalf of groups?
- **Formal theories of incumbent incentives:** What are the exact implications of group-based behavior for the incumbent's governing incentives? As Ashworth and de Mesquita (2014) rightly argue, one cannot draw too specific conclusions about incumbent behavior based on voter behavior alone. We therefore need more theoretical modeling of how group-based voter reasoning reshapes the

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strategic incentives of incumbents. My theoretical model in Chapter 2 illustrates one side of this from the voter's perspective, but comparable models from the incumbent's point of view are needed.

In sum, this dissertation suggests that group-based voting reshapes democratic accountability in potentially constructive and problematic ways. It demonstrates a tractable way for voters to judge incumbents by focusing on how policies affect their social groups, thereby reducing reliance on noisy personal experiences or abstract national indicators. This can strengthen accountability by helping voters better identify when their interests are being served or neglected. At the same time, it introduces a more fragmented incentive structure for incumbents, who may focus on rewarding electorally pivotal groups while ignoring broad-based growth. Group-based voting also creates new avenues for elite manipulation, as political actors can strategically frame which group identities are politically salient and which distributions appear fair. These changes place new demands on democratic institutions: to ensure that group-based reasoning remains grounded in observable facts and that elite narratives about group alignment are subject to scrutiny. Rather than rejecting group-based reasoning as inherently tribal or divisive, scholars and practitioners should recognize its informational role and work to support its accuracy and potential for accountability.

RESEARCH ARTICLES

Chapter 5

You and Whose Economy?: Group-Based Retrospection in Economic Voting

When managing the economy, governments make decisions that influence not only overall growth but also its distribution. How do voters judge incumbents for this? I revisit the idea of *group-based retrospective voting* and argue that voters assess the economic performance of their social in-groups relative to the national economy. By sanctioning the incumbent for in-group performance, voters can incentivize policy-making that better aligns with their interests. I test the theory, first, by estimating the relationship between in-group performance and incumbent support in panel data. This relationship is comparable in magnitude to sociotropic voting. I further conduct three experiments in Denmark and the United States, randomizing information about the performance of groups defined by geography, age, education, ethnicity and class. The findings suggest there are limits to sociotropic voting, as voters want their groups to follow or beat the national trend. This has important implications for electoral accountability and party competition.

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The economy plays a major role in elections. It is consistently ranked as one of the most important issues for voters and is often decisive for incumbents' reelection chances. By rewarding or punishing incumbents for economic conditions, voters can get rid of incompetent leaders and incentivize good governance. The resulting link between economic sentiment and incumbent support is one of the most robust findings in political science (Lewis-Beck and Stegmaier, 2019).

At the heart of the economic voting literature lies the question of whose economy voters care about: the national economy or their own pocketbooks? Decades of research suggest that the former, sociotropic motivations dominate the economic voting calculus (Linn, Nagler and Morales, 2010; Kiewiet and Lewis-Beck, 2011; Lewis-Beck and Stegmaier, 2019; Becher and Donnelly, 2013). National growth is seen as a reliable signal of the incumbent's competence and the country's future prosperity under their leadership. In contrast, fluctuations in people's personal finances are mostly a function of idiosyncratic experiences and provide a noisy signal of incumbent performance (Kiewiet and Lewis-Beck, 2011; Kinder and Kiewiet, 1981; Kramer, 1983; Ansolabehere, Meredith and Snowberg, 2014).

Yet, the dominance of sociotropic voting presents a puzzle. Citizens have diverse, often conflicting interests, and political leaders tend to align with some groups in society over others. Ignoring these conflicting interests seems at odds with much of what political science tells us about voter behavior. Since Converse (Converse, 1964), public opinion research has consistently found social group memberships to influence how voters think about politics (Campbell et al., 1960; Miller, Wlezien and Hildreth, 1991; Kinder and Kam, 2010a; Bornschier et al., 2021; Claassen et al., 2021; Donnelly, 2021b; Elder and O'Brian, 2022). Voters care about groups both as proxies for self-interest and as symbolic reference points tied to their social identities (Sears et al., 1980; Kalin and Sambanis, 2018). It would therefore be surprising if voters ignored how national economic growth is distributed among groups in society.

In this paper, I address this puzzle and argue that voters sanction incumbents for the perceived economic conditions of their own social in-groups. This idea traces back to Conover (1985), who argued that the economic performance of social in-groups – defined by factors such as class, geography and age – offers a useful “middle ground” between the pocketbook and the national economy (p.140). Given conflicting interests in society, common economic trends among group members provide a reliable signal

of how the incumbent's economic management aligns with their interests. The key contribution of this study lies in theoretically clarifying and empirically distinguishing this behavior, what I refer to as *group-based retrospective voting*.

Despite its theoretical merits, direct evidence on such behavior remains limited and inconclusive. The most relevant empirical work comes from the related literature on sub-national or local economic voting, which focuses on geographic groups (e.g. Healy and Lenz, 2017; Ansolabehere, Meredith and Snowberg, 2014; Simonovits, Kates and Szeitl, 2019; Ragusa and Tarpey, 2016; Tucker, 2006). While these studies have made important contributions, they have focused on estimating the observational relationship between local economic conditions and incumbent support. When it comes to understanding individual-level mechanisms, this approach has two significant limitations.

First, there is the common challenge of drawing causal inferences from observational correlations. Economic conditions across various geographic groups are not randomly distributed and incumbents may strategically favor pre-existing supporters (Dixit and Londregan, 1996; Golden and Picci, 2008).

Second, even if the relationship were causal, group-based retrospective voting would not be the sole explanation. One alternative is that group members are responding to their own pocketbooks. If, e.g., group conditions improve, individual group members will on average be doing better as well, making the association observationally equivalent with pocketbook voting. Another possibility is that voters interpret the economic conditions of their group as a signal of broader national trends (Books and Prysby, 1999; Larsen et al., 2019; Bisgaard, Sønderskov and Dinesen, 2016). In that case, voters may reward the incumbent for local growth for sociotropic reasons, and not because they care about the distribution of growth. Despite existing research on sub-national economic voting, we therefore have limited knowledge as to whether in-group economic performance matters to voters, separate from sociotropic or pocketbook concerns.

This paper takes on these challenges to establish whether group-based retrospective voting exists and what it looks like. I approach this task with two empirical designs: one observational and one experimental. For the observational design, I use a large British panel survey to track the economic performance of various geographic and class groups over a decade. I find that changing in-group performance predict changes in

incumbent support as well as changes in perceived incumbent alignment with in-group interests. This within-subject relationship is comparable in magnitude to sociotropic voting.

To broaden the scope and strengthen internal validity, I further conduct a series of experiments. Across three pre-registered experiments conducted in Denmark and the United States, I randomize true information about the economic performance of a total of 34 distinct social groups based on class, geography, education, ethnicity, and age. By varying which group the information describes, the design allows me to get directly at a counterfactual that observational studies cannot: how the subject's response to group information changes when they belong to the group in question. I find a consistent effect of in-group economic performance on satisfaction with the national economy and incumbent approval, especially when the group over- or under-performs the national economy. In sum, voters care about in-group economic performance, especially as it compares with the national trend, in a way that cannot be explained by sociotropic or pocketbook considerations.

This finding carries significant implications for our understanding of political behavior and party politics. First and foremost, it suggests that voters are less sociotropic than commonly assumed. While voters appreciate national growth, they may dislike it if their group falls behind. Conversely, they may forgive a worsening national economy if people like them are outperforming it. This conditionality of sociotropic voting is easily hidden in conventional analyses that do not account for group performance.

Following this, group-based retrospective voting crucially shapes the governing incentives of incumbents. As voters put more weight on group performance and less on the national economy, incumbents can expect greater electoral rewards by favoring electorally pivotal groups. On the flip side, this might allow them to get away with less competent management of the national economy. This dynamic echoes Ferejohn (1986)'s concern about pocketbook voting, namely that it enables incumbents to 'divide and rule' the electorate, weakening democratic control.

Group-based retrospective voting also helps explain notable deviations from traditional economic voting observed in recent studies. Some research has rejected economic explanations for phenomena like the rise of populism based on analyses of egotropic and sociotropic variables (Mutz, 2018; Kates and Tucker, 2019) with overall evidence for such explanations described as "mixed at best" (Berman, 2021). However, my findings

suggest that voters can act on collective economic grievances even when they are faring well individually and national economic conditions are favorable. When voters act on the economic performance of their in-groups, this is not easily captured by standard economic variables. In turn, the economy may play a larger role in electoral phenomena than is often recognized.

Why and How In-Group Economic Performance Matters

Scholars have found strong and remarkably consistent evidence for sociotropic voting (Kiewiet and Lewis-Beck, 2011; Lewis-Beck and Stegmaier, 2019; Linn, Nagler and Morales, 2010; Becher and Donnelly, 2013), and there are compelling reasons for it. National economic indicators are a signal of the incumbent's competence and voters' future prosperity under their continued rule (Alesina, Londregan and Rosenthal, 1993; Fearon, 1999). Moreover, sociotropic voting incentivizes reelection-seeking incumbents, once in office, to do their best to serve their interests (Woon, 2012). This makes sociotropic voting a powerful mechanism for accountability.

Yet, the sociotropic model is in tension with two key facts. One is that citizens have diverse, often conflicting interests, and political leaders inevitably align with some groups in society more than others. For voters, these conflicting interests seem like a relevant consideration. Second, a long tradition in political behavior research finds voters to be profoundly influenced by their group identities (Campbell et al., 1960; Miller, Wlezien and Hildreth, 1991; Kinder and Kam, 2010*a*; Bornschier et al., 2021; Claassen et al., 2021; Donnelly, 2021*b*). As such, it would be natural for voters to pay attention to the economic conditions of groups they identify with in addition to the nation as a whole.

This is the gist of an idea put forward by Conover (1985), who argued that voters assess the economic conditions of their own social in-groups. Echoing other work on the psychology of group-centrism, Conover argued that in-group interests often take on a symbolic value for people who socially identify with their group (Tajfel, 1974). Additionally, group interests can serve as a heuristic for what is in group members' (long-term) personal self-interest (Sears et al., 1980; Fiorina, 2018; Feldman, 1982).

Voters thus care about in-group interests for self-interested and symbolic reasons, although this distinction is mostly conceptual and rarely lends itself to direct empirical testing (Kalin and Sambanis, 2018). Regardless of the underlying motivation, the key point is that voters have reasons to care about in-group interests, and they can further them by punishing and rewarding incumbents for group performance. I refer to this behavior as group-based retrospective voting.

The literature also suggests an alternative, simpler way for voters to account for conflicting interests, namely pocketbook voting, wherein individuals assess incumbents based on their personal economic situation (Fiorina, 1981). While pocketbook voting offers a seemingly straightforward solution, individual financial changes are an unreliable indicator of incumbent policy-making as they mostly result from idiosyncratic life events with little basis in policy (Conover, 1985; Kramer, 1983; Alesina, Roubini and Cohen, 1997; Lewis-Beck, Stuber and Nadeau, 2013). For example, an elderly voter experiencing financial hardship may not have a direct reason to blame the incumbent. But if they observe a broader decline among other elderly people, this more likely reflects the incumbent's management of the economy. As such, aggregating economic outcomes across group members shaves off idiosyncratic pocketbook variation, making group-level trends a better gauge of how the current management of the national economy serves group members' interests. Thus, even purely self-interested voters may be better served in the long term by holding the incumbent accountable for in-group performance than for their own pocketbooks.

To be sure, the logic of group-based retrospection need not be restricted to in-groups. Out-groups can serve as significant political "reference points" (Campbell et al., 1960) which voters care about due to ideology, sympathy/antipathy, or intergroup competition (Elder and O'Brian, 2022; Nelson and Kinder, 1996; Bechtel and Mannino, 2022; Schneider and Ingram, 1993). For instance, voters learning about the declining conditions of the elderly may punish the incumbent out of sympathy with the group. In this paper, however, the focus is limited to in-groups, which I consider central to the mechanism. Future work should explore the distinct role of out-groups for group-based retrospection.

While Conover introduced the notion of group-based retrospective voting, certain aspects remain under-theorized. In the following section, I extend Conover's framework in two main ways. First, I develop a clearer theoretical distinction between group-based

retrospection and other forms of economic voting. Importantly, I argue that group-based evaluations are primarily about relative, not absolute, economic performance, and theorize the nature of this national benchmarking. This benchmarking implies that group-based retrospective voting can counteract sociotropic voting. Second, I outline the conditions under which group-based retrospective voting occurs, emphasizing the role of group relevance and information availability. These theoretical refinements are key for deriving the empirical implications that I test.

Group-Based Retrospection and the Limits of Sociotropic Voting

How does group-based retrospection factor into the economic voting calculus? I argue that in-group performance provides unique information about the economy and whether the incumbent is managing it in line with the group's interests. In-group performance therefore matters over and above voters' views of their pocketbook or the national economy.

For example, suppose a region's economy declines while the broader national economy thrives. All else equal, residents of that region would have reason to view this decline unfavorably, as it signals that the incumbent's economic management is not serving their interests. Even someone personally benefiting from the national boom may interpret their group's struggles as evidence that the economy is not working for people like them. Crucially, for someone in this position, in-group decline does not just matter as an indicator of declining personal pocketbook prospects, it signals, more broadly, that the incumbent is not prioritizing group members' interests. In this way, in-group conditions serve as a distinct economic indicator, carrying intrinsic political significance beyond pocketbook or sociotropic evaluations.

Taking the logic of group-based retrospection one step further, I argue voters should be especially attuned to their group's performance when it diverges from the national trend. This heightened sensitivity arises for two reasons. First, relative group performance provides a clearer signal of the incumbent's alignment with group interests. This mirrors the logic of sociotropic benchmarking across countries, which helps isolate the effects of government policy from broader global trends (Kayser and Peress, 2012; Hansen, Olsen and Bech, 2015; Hart and Matthews, 2022, 2023). Likewise, national benchmarking helps contextualize group performance by providing a measure of the

average effects of policy on ‘everyone else’. If a region’s economic decline mirrors the national trend, it suggests failures in overall economic management rather than region-specific policies. However, when a region lags behind a thriving national economy, it signals a failure to address the group’s particular interests.

Second, a decline in relative rather than absolute group performance can undermine a group’s status. This follows the principles of intergroup competition as described in social identity theory, where material resources represent one competitive dimension (Turner and Tajfel, 1986). Since social standing is defined in relative terms, a group that grows more slowly than others may feel diminished – even if its material conditions are stable or improving (Engler and Weisstanner, 2021; Gidron and Hall, 2017). On this latter account, the national benchmark functions less as an abstract measure for the performance of ‘everyone else’, and more as a proxy for the relative status of salient, competing out-groups. In sum, relative group performance matters both for what it reveals about the incumbent and what it implies about the group’s changing status.

This does not mean that underperforming groups always punish the incumbent or that overperforming groups always reward them, as sociotropic concerns may still outweigh these group-based ones. However, group-based retrospective voting introduces countervailing effects of a growing or shrinking national economy if certain groups of voters exceed or fall behind the national trajectory. Exactly how these effects net out depends on the weight voters put on overall incumbent competence versus alignment with group interests. Ultimately, group-based retrospective voting offers an important correction to the sociotropic model, suggesting that, while voters like national growth, they may dislike it if their groups fall behind the trend and may be more forgiving of national decline if people like them are thriving.

An important question is whether relative performance has asymmetric effects. In principle, voters should care equally about group underperformance and overperformance, as both are equally informative. Yet, according to a long-standing finding in political behavior, voters tend to react more strongly to economic losses than to gains (Soroka, 2006; Bloom and Price, 1975; Larsen, 2021). This suggests voters may be more concerned about falling behind than getting ahead, echoing the literature on ‘fraternal’ or group-based relative deprivation (Engler and Weisstanner, 2021; McKay, 2019; Runciman, 1966). I test this asymmetry in the experimental study.

Key Conditions for Group-Based Retrospection

There are countless ways to divide the electorate into social groups experiencing different economic outcomes. When does the performance of a given group matter to its members? I focus here on two key conditions that must be met: group relevance and group information.

The first condition follows the extensive literature on group-centric behavior in stating that the most influential groups are those that are both personally and politically relevant. Personal relevance stems from shared traits, which are key for group identity and diminish with size as larger groups tend to be more heterogeneous (Shayo, 2009; Grigoryan, 2020). At the same time, groups must be large enough to be politically relevant, as niche groups with idiosyncratic interests are harder to connect to incumbent policy. This creates a trade-off: the most relevant groups are “meso-groups” (Donnelly, 2021*b*) that are small enough to have distinct interests but large enough to matter nationally. As a result, classical cleavage groups (e.g., age, region, class) are more relevant than micro-groups (e.g., niche occupations or neighborhoods), while broad categories like gender or majority ethnic groups matter most when intersected with other identities (Gershon et al., 2019; Perez Brower, 2022; Grigoryan, 2020).

The second condition for group-based retrospection is that voters have access to information about their group’s performance. As with other economic voting, a key source of such information is everyday experience (Kayser and Peress, 2024; Larsen et al., 2019; Bisgaard, Sønderskov and Dinesen, 2016). Due to social homophily, most people disproportionately interact with others from their social groups (Golub and Jackson, 2012; Alt et al., 2022). Perceptions of the group’s over- or under-performance arise when experienced group conditions deviate from perceived conditions of the country as a whole, as conveyed by, e.g., the media or elites. An important implication of this is that group-based retrospection is conditioned by social homophily within the group because it determines access to information about group performance. This is one reason to expect in-group performance to matter more than the performance of (salient) out-groups, but also a reason why some groups, e.g., geographical groups, might be more influential than other groups due to greater intra-group interaction.

Besides personal experience, ‘group-centric’ media reporting and elite communication may also inform perceptions of relative group performance (Nelson and Kinder,

1996). It is not uncommon for the media to cover the economy in terms of the performance of specific groups or localities and diverging trends (Fortunato, Swift and Williams, 2018; Peters et al., 2022). This source is likely most important for groups of high political salience and groups experiencing strongly diverging trends. Nonetheless, such information is typically sporadic compared to the steady flow of everyday experiences.

In summary, for group-based retrospective voting to arise, groups must be both personally and politically relevant and members must have access to information about group performance. With a clearer grasp of how group-based retrospective voting works, the next section discusses methodological limitations in existing research.

Empirical Challenges to Inference in Existing Research

Group-based retrospection has received less attention in the economic voting literature than its egotropic and sociotropic counterparts. The most relevant empirical work comes from the related literature on sub-national or local economic voting, which examines geographic in-groups (e.g. Healy and Lenz, 2017; Ansolabehere, Meredith and Snowberg, 2014; Simonovits, Kates and Szeidl, 2019; Rogers, 2014; Ragusa and Tarpey, 2016; Tucker, 2006). This body of work focuses on estimating the often cross-sectional relationship between sub-national economic conditions and incumbent support, using a combination of economic and survey data.

Although limited to geographic groups, efforts to test this observational relationship are important. Existing findings are mixed, suggesting that there is yet more work to be done.¹ Yet, these studies cannot tell us with much confidence whether voters are engaging in (geographical) group-based retrospective voting or not. This owes to two key challenges, one related to causal inference and one to mechanistic inference. Both can be illustrated with the example from before.

Suppose we observe a city that is now outpacing national growth. Survey data might indicate that residents of this city are strong incumbent supporters, a pattern consistent with group-based retrospective voting. However, this relationship may be

¹While results in, e.g., (Healy and Lenz, 2017), (Newman, 2015), (Tucker, 2006), and (Ansolabehere, Meredith and Snowberg, 2014) are positive, the evidence in (Rogers, 2014) and (Simonovits, Kates and Szeidl, 2019) is more mixed and estimates in (Mutz and Mondak, 1997), (Ragusa and Tarpey, 2016) and (Stiers and Hooghe, 2023) are null.

spurious because group-level economic outcomes are not randomly distributed. In this case, urban prosperity might happen to coincide with an incumbent who generally draws strong support from urban voters, or the incumbent may even have deliberately targeted their pre-existing supporters with resources (Dixit and Londregan, 1996; Golden and Picci, 2008). In sum, group-level economic performance may not be driving the observed relationship.

The issue relating to mechanistic inference is more subtle. Even if the city's prosperity resulted from a perfectly exogenous shock, group-based retrospective voting would not be the sole explanation for the electoral response of city residents. For one, residents may simply be responding to improvements in their personal economic circumstances, making the effect an instance of pocketbook voting. Alternatively, residents could be using local conditions as a cue about the broader national economy (Books and Prysby, 1999; Larsen et al., 2019; Galesic, Olsson and Rieskamp, 2012). As their perceptions of the national economy improve, increased incumbent support could be driven by sociotropic rather than group-based retrospective voting. Several studies on local economic voting acknowledge these alternative explanations (e.g. Reeves and Gimpel, 2012; Ansolabehere, Meredith and Snowberg, 2014; Stiers and Hooghe, 2023; Healy and Lenz, 2017). Even if changes in group economic conditions were to causally affect incumbent support, it is thus not clear that voters are specifically motivated by concern for the economic conditions of their geographic group.

These two inferential challenges, causal and mechanistic, make it difficult to assess group-based retrospective voting through observational data alone. Despite existing research on geographic groups and economic voting, the individual-level mechanism thus remains unsettled. In the next section, I propose two approaches – one observational and one experimental – that overcome these challenges and isolate the mechanism.

Empirical Strategy

To test group-based retrospective voting, two conditions must be met. Firstly, variation in perceptions of in-group economic performance must be plausibly exogenous for estimates to be causal. Secondly, pocketbook and sociotropic evaluations must be held fixed to exclude alternative mechanisms. I achieve this in two distinct designs, one observational and one experimental.

Chapter 5

The observational design uses panel survey data with items that allow for measuring economic changes in respondents' in-groups. Leveraging the panel structure, I estimate two-way fixed effects models of the relationship between in-group economic performance and incumbent support for a range of geographic and class groups. This improves on existing observational analyses of group-based retrospective voting in a few important ways. For one, it uses only within-individual variation, making the causal claim more credible. For instance, pre-existing supporters of the incumbent being targeted with benefits would not bias results. In addition, I adjust for time-varying egotropic and sociotropic evaluations. This plausibly excludes the alternative pocketbook and sociotropic explanations for the relationship. Finally, by directly measuring perceptions of in-group performance, this approach gets closer to the psychological mechanism.

To be sure, this approach does not fully causally identify the effect as group performance trajectories are not random. Yet, it offers what is arguably the most direct observational test of group-based retrospective voting to date. Being observational, it has the advantage of emulating the measures and modeling of most economic voting research. This makes results more comparable to existing estimates in the literature. Additionally, it provides evidence that voters' hold perceptions of in-group performance that are meaningful and predictive.

To fully overcome the causal and mechanistic inference problems and strengthen internal validity, I conduct a series of experiments. The experimental design uses information treatments to generate exogenous variation in perceptions of the economic performance of a range of social in- and out-groups. By altering only perceptions of group performance, pocketbook evaluations are held constant, which I verify by testing for effects on prospective pocketbook performance. Additionally, by comparing only subjects exposed to in-group vs out-group information, sociotropic perceptions are held fixed. As such, this design offers both causal identification and a direct test of whether voters are concerned with in-group performance itself, independent of its implications for the national economy or their near-term personal well-being.

Observational Evidence of Group-Based Retrospective Voting

To examine group-based retrospective voting observationally, I use The British Election Study Internet Panel (BESIP) (Fieldhouse et al., 2024). The panel consists of 25 waves spaced out by intervals of 1-12 months, each with a representative sample of 30,000 respondents. BESIP is one of very few datasets to include repeated measures of voters' perceptions of the economic performance of salient social groups. It also includes a rare item measuring perceptions of how well the incumbent party aligns with the interests of some of these same groups. I use this item to further test the mechanism.

Measuring in-group performance involves two steps: measuring the economic performance of various social groups and sorting respondents into them. With regard to the first, I use an item tapping respondents' perceptions of group performance with the question: "Giving your best guess, how do you think the financial situation of [group] compares with what it was 12 months ago?" This individual-level measure exists in four waves for classes, regions, and the respondent's own 'local community'. Since subjective perceptions of group performance can be endogenous to other attitudes at the individual level, including party choice itself, I also construct a group-level measure by aggregating pocketbook evaluations of self-identifying group members. By averaging across group members' personal economic experience, this measure gets closer to the actual performance of the group, although it is based on members' subjective summary judgments rather than objective indicators. There is evidence that such subjective evaluations closely follow actual income changes, however (Healy, Persson and Snowberg, 2017).

To sort respondents into their respective groups, I rely on self-categorization measures. For class, this is: "Do you ever think of yourself as belonging to any particular class?" and for region, it is simply their reported region of residence. For 'local community', this sorting is not possible. This is not a problem for the individual-level measure since it already asks about their in-group (their own 'local community'). However, it means that it is not possible to calculate the group-level measure for these smaller geographic groups. See Table 5.1 for an overview of the resulting variables.

The main outcome is the respondent's reported likelihood of voting for the incum-

Table 5.1: Overview of Variables in the British Election Study Internet Panel

	Measurement level	Groups measured	Waves observed
In-group performance (grp.-level)	Grp.-by-wave	Class, region	18
In-group performance (ind.-level)	Ind.-by-wave	Class, region, local community	4
Likelihood of incumbent vote	Ind.-by-wave		18
Incumbent looks after group	Ind.-by-wave	Class	6

Note: ‘Waves observed’ refers to the number of waves where the variable overlaps with the other variables of interest.

bent party, i.e. the Conservative Party which was in government during all survey waves.² I control for standard measures of egotropic and sociotropic retrospective evaluations.³ While theoretically important, one issue with controlling for sociotropic evaluations is that they may act as a collider variable insofar as they are affected by in-group performance (Elwert and Winship, 2014). If anything, collider bias is likely to deflate estimates. For any unobserved parent variable(s) to bias estimates upward, they would have to be time-varying and causally affect both sociotropic evaluations and incumbent support but in *opposite* directions. Any variables that affect sociotropic evaluations and incumbent support in the same direction, e.g. partisanship, would instead result in conservative estimates. I therefore choose to keep them in the main model.

I also use the following item as an additional outcome variable: “Some people say that all political parties look after certain groups and are not so concerned about others. How closely do you think the Conservative Party looks after the interests of [group]?”. As theorized, voters care about in-group performance partly because it tells them how well the incumbent aligns with group interests. This measure of group ‘concern’ allows me to test this directly. Due to limited wave coverage, as shown in Table 5.1, I can only estimate this relationship for class performance.

²“How likely is it that you would ever vote for each of the following parties? [Conservatives]” (10-point scale).

³These items ask: “How does the financial situation of your household now compare with what it was 12 months ago?” and “How do you think the general economic situation in this country has changed over the last 12 months?”, respectively.

Observational Results

What is the relationship between changing group-level economic conditions and incumbent party support? I estimate two-way fixed effects models of government support on measures of in-group economic performance, controlling for sociotropic and egotropic evaluations. The inclusion of wave fixed effects is important because it makes the coefficient on group performance reflect changes in performance relative to the population-level trend. It thereby helps capture relative in-group performance as theorized. However, since absolute and relative changes are correlated, the model cannot directly adjudicate which of them are driving the observed behavior. I test this directly with the experiments.

As each voter belongs to several groups that I measure, I first run separate regressions of vote intention on the in-group performance measures for each group type (class, region, local area) and the control variables.⁴ All independent variables are standardized to facilitate comparison. I then use a meta-regression model with equal weighting that effectively averages the coefficients across these models to obtain the pooled estimates shown in column 1 and 2 of Table 5.2.⁵ The unique prediction of group-based retrospective voting is a positive coefficient on in-group economic performance across groups holding sociotropic and egotropic evaluations fixed.

Columns 1 and 2 in Table 5.2 show this relationship for in-group performance as it is perceived by members ('ind.-level') and derived from the average of members' pocketbook evaluations ('grp.-level'). Across models, there is a consistent positive relationship between relative in-group performance and vote intention for the incumbent party. The coefficients on the two measures of economic performance are similar, suggesting that voters hold meaningful and predictive perceptions of the economic conditions of their class and geographic groups.⁶ As group performance, measured as aggregated pocketbook evaluations, improves by one standard deviation compared to the national trend, group members report being 1.8 percentage points more likely

⁴To account for correlated perceptions of in-group performance across group types, I additionally control for perceptions of other in-groups in Appendix 8.2.

⁵I use the meta-regression function `rma` from the `metafor` package in R to pool estimates. See Appendix 8.1 for full results.

⁶Note that the coefficient on perceived class performance is weakened once perceived regional performance is accounted for in Appendix 8.2, suggesting that regional perceptions may be partially driving this relationship.

Table 5.2: Relationship Between Group Performance, Perceived Group Concern and Incumbent Vote Intention.

	Group-based retrospective voting (pooled estimates)		Test of the mechanism	
	Vote intention		Class concern	Vote intention
	(1)	(2)	(3)	(4)
Group performance (grp.-level)	0.18 (0.00)***			
Group performance (ind.-level)		0.12 (0.01)***		
Class performance (grp.-level)			3.81 (0.17)***	
Class concern				0.48 (0.03)***
Egotropic evaluations	0.08 (0.00)***	0.11 (0.02)***	0.06 (0.02)**	0.04 (0.02)*
Sociotropic evaluations	0.32 (0.01)***	0.13 (0.03)***	0.09 (0.03)***	0.09 (0.02)***
TWFE	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes	Yes
Wave N	18	4	6	6
Individual FEs	91125	20406	37493	37493
Total N	354304	22352	53 058	53 058

Note. Estimates from two-way fixed effects models of self-reported likelihood of voting for the incumbent party. All independent variables are standardized. Coefficients in columns 1 and 2 are pooled estimates from several underlying models, one per group type, using simple meta-regression models with equal weights (see Appendix A for unpooled models). For these meta-regressions in columns 1 and 2, the total N and individual FE statistics show the minimum numbers, as they vary slightly between underlying models. * $p < .05$, ** $p < .01$, *** $p < .001$.

to vote for the incumbent party on average. This pooled coefficient is a little more than half the magnitude of that on sociotropic evaluations (and larger than that on egotropic evaluations). In the underlying models shown in Appendix Appendix 8.1, the standardized coefficients on in-group performance and sociotropic evaluations are equal in magnitude in four out of five of the models. This suggests that, while it varies by group, group-based retrospective voting is of a similar order of magnitude to sociotropic voting and substantially significant. The same can be seen in Model 2, where the coefficients on sociotropic evaluations and perceptions of in-group performance are equal.

These estimates are robust to controlling for left-right ideology and attitudes towards austerity policy (see Appendix 8.2). Although post-treatment control variables makes for an imperfect test, it is consistent with voters sanctioning the incumbent for in-group performance rather than a broader ideological shift away from the governing Conservative Party. An auxiliary analysis in Appendix Appendix 8.2 further shows the relationship to hold when the survey measures are replaced by a key objective economic indicator, namely group-level unemployment. Linking BESIP to monthly regional unemployment rates (Office For National Statistics, 2025), the analysis shows that incumbent support in a region follows deviations in regional unemployment from the national average, controlling for sociotropic and egotropic evaluations. Thus, the relationship is not specific to subjective economic perceptions. In sum, in-group economic performance appears to have independent explanatory power for vote choice that is comparable in magnitude to sociotropic voting.

To make it more likely that these results are driven by the theorized relationship, and not other variables correlated with in-group performance, I run a similar set of models for an outcome that is even more specific to the theorized mechanism: perceptions of the incumbent's alignment with the in-group's interests. In line with the theory, I expect in-group performance to predict perceived incumbent concern for that in-group, which in turn predicts incumbent vote intention. The results for these two links are shown in columns 3 and 4 of Table Table 5.2 for class groups.⁷

As expected, Table 5.2 shows a strong positive relationship between changes in

⁷I am implicitly testing a claim about mediation here but avoid specifying a full mediation model as it requires strong assumptions that are unlikely to hold in this case (Acharya, Blackwell and Sen, 2016; Elwert and Winship, 2014). Although I cannot quantify the exact mediating effect, a positive estimate in both models suggests some positive mediating relationship.

class performance and class members' perception that the incumbent party looks after their class interests. The coefficient is very substantial: as aggregated class conditions improve by a standard deviation relative to the national average, perceived incumbent concern with the class among its members increases by 38 percentage points on average. In contrast, egotropic and sociotropic evaluations are only marginally related to perceived incumbent alignment with the group. Column 4 shows that higher perceived concern with in-group interests is, in turn, associated with a substantially stronger incumbent vote intention. To be sure, this latter relationship between two incumbent attitudes is likely endogenous to partisanship. Still, these additional findings provide further evidence that is consistent with voters making inferences about the incumbent when in-group conditions change.

In sum, the panel data analysis shows a within-subject relationship between in-group performance and incumbent support that cannot be easily explained by egotropic or sociotropic evaluations. This holds for both class and geographic groups. Given that previous studies have mostly relied on cross-sectional comparisons without controlling for e.g. sociotropic evaluations, this represents some of the most compelling observational evidence to date that voters are concerned with the economic performance of their social in-groups.

That said, the observational analysis still faces some limitations. The findings are limited to geographic and class groups. More importantly, group performance trends are not random and regression adjustments for other economic evaluations may not fully isolate the mechanism because of measurement error and issues with post-treatment bias (Elwert and Winship, 2014). While the within-subject approach mitigates some concerns, further evidence is needed to fully rule out alternative explanations. To broaden the scope and overcome these issues, I develop an experimental design described in the next section.

Experimental Evidence of Group-Based Retrospective Voting

In recent years, the economic voting literature has increasingly turned to experiments (Simonovits, 2015; Bisgaard, 2019; Bechtel and Liesch, 2020; Hart and Matthews, 2023).

While actual economic conditions cannot be randomized, economic information can, and information treatments thus offer a valuable method for distinguishing psychological mechanisms. In this study, I take this approach to induce exogenous variation in perceptions of in-group performance, while also including information on the national economy. By including or omitting the national-level information, I can also directly test whether voters are most concerned with relative group performance, as theorized. The design is implemented across three surveys, two of which are pre-registered, with minor variations.⁸ The next section describes common features of the design.

Experimental Design

The core feature of the experiment is that it provides exogenous variation in perceptions of in-group economic performance. To obtain the right counterfactual, however, the experiment must also manipulate perceptions of out-group performance. Only by comparing subjects receiving in-group vs. out-group information — at random — is it possible to isolate the mechanism. If treated subjects were simply compared to a control group receiving, e.g., national economic information or no information at all, other differences between conditions could explain the effect, violating the assumption of information equivalence (Dafoe, Zhang and Caughey, 2018). For instance, subjects might react to perceived inequality or simply like positive economic news and dislike bad, regardless of group identity. The downside of this strategy is that the control condition becomes ambiguous, reflecting the average response to information about the included out-groups which is likely positive in some cases and negative in others, as discussed earlier. While out-group information serves as a good benchmark for internal validity, it thus complicates the interpretation of the resulting effect sizes.

To implement this approach, I include two main treatment conditions: an in-group condition and an out-group condition, differing only in whether the same economic stimulus pertains to the subject's in-group or a random out-group. This is operationalized in the steps shown in Figure Figure 5.1. In step a), subjects are asked to choose their in-group within a given group category.⁹ In the figure, this is shown for a group category containing three groups. Within each group, the subject's treatment status is then randomized in step b) to receive an information stimulus about a random group

⁸See Appendix Appendix 8.3 for details on the pre-analysis plans and an overview of omitted analyses.

⁹See Appendix Appendix 8.6 for details on how these groups were chosen and defined in each survey.

from the category in step c). For the main version of treatment, ‘relative group decline’, this involves a text vignette containing two pieces of information: negative information about the recent trajectory of the group (the full arrow with a circle) and positive information about the recent trajectory of the national economy acting as a benchmark (the dotted arrow). In other words, an unequal growth scenario where a given group is under-performing the national economy. The key comparison is thus between those receiving a stimulus about their in-group and those receiving a ‘control’ stimulus about a random out-group (in the same category).¹⁰

I additionally implement two alternative versions of treatment. The first, ‘absolute group decline’, omits the national-level information and presents the negative group-level information alone. The main treatment includes the country benchmark to capture the group-to-country comparison that is hypothesized to matter for group-based retrospection. However, by comparing the effects of the absolute and relative decline treatments, I can test this nuance of the mechanism more cleanly than the observational data could afford.

Second, I implement a ‘relative group improvement’ version of treatment that reverses the economic trends in the main treatment, i.e. by describing the national economy as sluggish and the group as performing better. Following my theory, voters should react to in-group performance both when it over- and under-performs the national trajectory.

Varying Experimental Features

I implement the design in three experiments conducted online in two very different country settings: Denmark and the United States. Table 5.3 gives a brief overview of their key characteristics. Besides varying the national context, the three experiments vary the groups and the treatment information for robustness. The experiments jointly cover a total of 34 different groups across six group categories: age, geography, class, education and ethnicity.

Experiment 1 and 2 were both pre-registered and fielded in Denmark under different governments. Both use the ‘relative group decline’ version of treatment shown in Figure 5.1. Experiment 1 also includes the ‘absolute group decline’ version of treatment

¹⁰In Experiment 1, I also include a ‘pure’ control scenario with no economic information at all. Results are similar when using this as the control group (see Appendix 8.8).

Figure 5.1. A diagram of the progression of the experiment for one group category with three groups (black, dark gray and light gray). First, subjects choose their group in step a). They are then randomly allocated in step b) to receive one of two treatments in step c): a text-based information treatment about their chosen in-group or an out-group. The main treatment is ‘relative group decline’. Two alternative versions of treatment are ‘relative group improvement’, and ‘absolute group decline’ (which simply omits the national-level information).

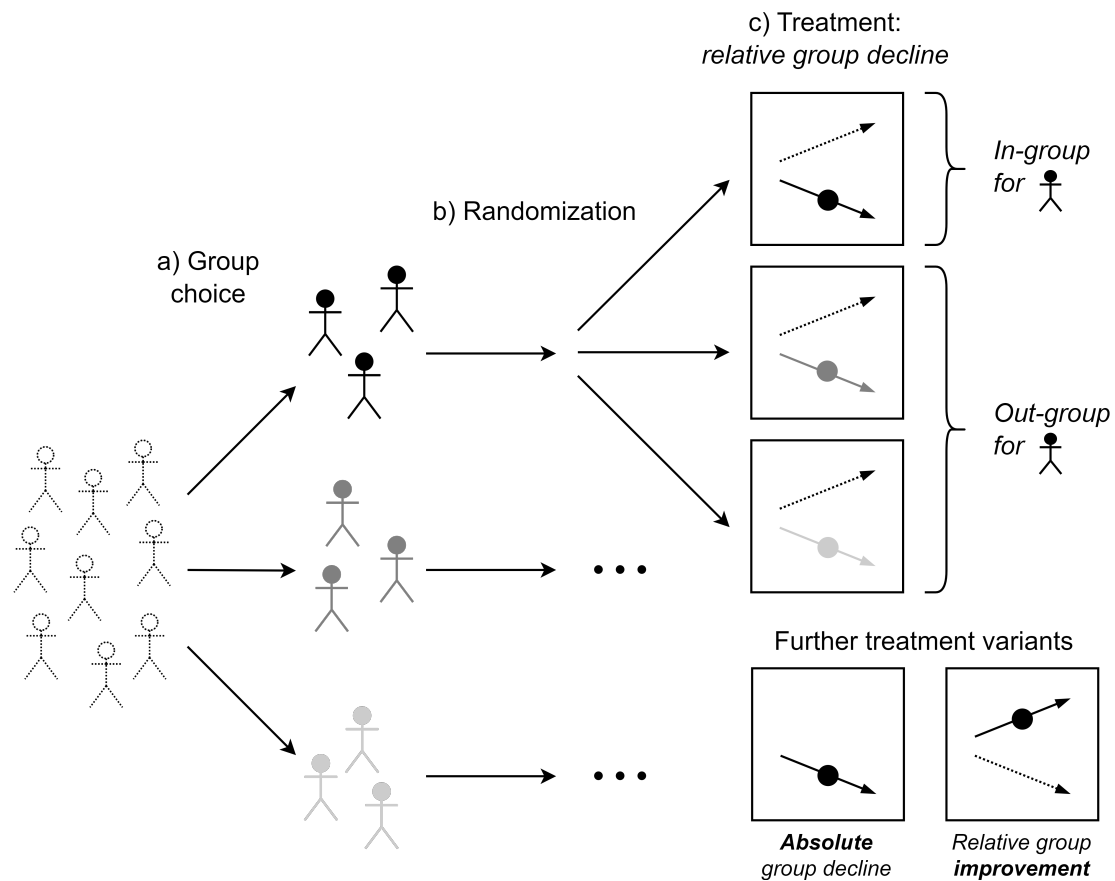


Table 5.3: Overview of the Experiments

	Experiment 1	Experiment 2	Experiment 3
Collection period	April 2022	April 2023	September 2023
Country	Denmark	Denmark	United States
Survey provider	YouGov	Moos-Bjerre/Norstat	Verasight
N	4,020	3,262	2,000
Government	Social Democratic	Centrist coalition	Democratic president
Social groups	Age, geo., class, education	Age, geo.	Ethnicity-by-education
Treatment versions			
Relative decline	Yes	Yes	No
Relative improvement	No	No	Yes
Absolute decline	Yes	No	No

Note. The table shows key features of the three survey experiments. All surveys target the voting-age population. Ethnicity-by-education groups are intersectional groups that combine ethnicity and education.

that omits the national benchmark.

To further probe the generality of the mechanism, Experiment 3 was fielded in the United States.¹¹ Denmark and the United States have markedly different conditions for group-based retrospective voting to emerge. The motivation for group-based retrospective voting is likely stronger in the United States than in Denmark, due to higher economic inequality and lower state generosity. This suggests a higher sensitivity to incumbent concern for group interests in the electorate. In Denmark, by contrast, effects of unequal growth are cushioned by the comparatively generous welfare state. At the same time, higher partisan polarization in the United States may make it harder to change voters' evaluations of the economy and the incumbent with new information (Bisgaard, 2019). These economic and political differences provide a unique opportunity to test whether the mechanism spans rather different national contexts.

In addition to varying the country, Experiment 3 differs in two further ways to allow for a more expansive test of the mechanism. As theorized, larger group categories are often more personally relevant when they are intersected (Gershon et al., 2019; Perez Brower, 2022). In Experiment 3, I therefore use 'ethnicity-by-education' groups, i.e. groups that combine ethnicity and education level as "[ethnicity] with[out] a college degree" (see Appendix 8.6 for a list of these groups). Moreover, Experiment 3

¹¹It was kindly included in Verasight's omnibus survey, which came with some strict space constraints. It therefore includes fewer additional survey items like manipulation checks and covariates.

implements the ‘relative group improvement’ version of treatment shown in Figure 5.1.

Stimulus material

The experiment manipulates subjects’ perceptions of the national economy and the economic conditions of their groups. The design of stimulus material therefore involves two key choices: the choice of economic information and the choice of groups.

Economic information. The stimuli include economic information on both the recent trajectory of the country and the recent trajectory of a social group. I balance two key concerns in the choice and presentation of this information.

First and foremost, deception should be avoided whenever possible. Deceptive treatments can be unethical even when the deception does not harm subjects, because it may make them more skeptical of information provided in surveys and ‘poison the well’ for other researchers (Rousu et al., 2015). Deceptive treatments can also undermine ecological validity (Dickson, 2011) and may lack credibility for subjects. That is especially likely in this case where economic performance naturally varies by group.

At the same time, treatments should be strong enough to move subjects’ beliefs. Both the United States and Denmark were marked by economic pessimism during data collection, with Denmark’s consumer confidence reaching historical lows (Burns, 2023; Mortensen, 2022; Denmark, 2023). Meanwhile, the treatments aim to convince subjects that either the whole or parts of the national economy are in fact doing *well*. To move subjects’ perceptions in the desired direction, there must therefore be sufficient contrast in stimuli between the performance of the group and the national economy, without compromising ecological validity.

To balance these considerations, I design non-deceptive stimuli using true information from the Danish and American National Election Studies. To obtain information about group relative decline or improvement that is relatively strong and consistent across groups, I exploit the fact that there are several economic items in each survey that are similar but have rather different distributions of responses due to variations in wording and emphasis. By calculating the economic statistics for the groups from different variables than the statistics for the national economy, it is possible to get

stimuli that indicate a substantial and consistent gap in economic performance trends between each group and the country as a whole.

This approach produces stimuli that are both moderately strong and relatively homogeneous across groups (see manipulation checks in Appendix 8.4). While the stimuli vary substantially in Experiment 1, they are far more homogeneous in Experiments 2 and 3 (see Appendix 8.9 for details and Appendix Appendix 8.5 for lists of all stimuli.). Still, I design the stimuli to reduce the impact of this variation. The treatments downplay numerical information and give a consistent and strong framing to facilitate a common interpretation. Moreover, each experiment uses different economic information and varies vignette wordings, ensuring that results do not hinge on any of these particulars. Finally, statistically adjusting for heterogeneity in numerical information across stimuli is both simple and effective (Fong and Grimmer, 2023) and I follow my pre-analysis plans in doing so (although results are unchanged). Jointly, these measures allow me to avoid deceptive stimuli without compromising experimental validity. An example stimulus of ‘relative group decline’ from Experiment 1 is shown below:

Experiment 1 (working class). The economy does not develop the same way for everyone. For example, in the latest survey from the Danish Election Project, the proportion of working class people who felt financially insecure increased significantly (73%) compared to the previous survey. By contrast, a large majority (82%) of respondents felt that their economic situation had remained stable or improved.

This example combines information from two DNES survey items, one on current financial security (the working class figure) and one on retrospective economic evaluations (the national figure), the former of which had a more negative distribution than the latter, enabling the contrast in the stimulus. See Appendix 8.9 for more details on these items.

Social groups. With respect to group selection, I take a somewhat conservative approach. Rather than sampling a small set of groups based on a most-likely case logic, I include a broad range of potentially relevant groups. Following the logic of stimulus sampling, casting a wide net ensures that results do not depend on any group-specific sentiments or beliefs, bolstering both internal and external validity (Clifford, Leeper

and Rainey, 2023; Wells and Windschitl, 1999; Fong and Grimmer, 2023). The inclusion of less relevant groups is expected to bias my estimates downward for a conservative but harder test of the theory. In Denmark (Experiment 1 and 2), the chosen group types are class, education level, age and geography. In the United States (Experiment 3), these are ethnicity-by-education level. Subjects are asked to choose an in-group in the beginning of the survey, except for Experiment 3 which sorts subjects directly into ethnic and education groups on the basis of background characteristics registered by the survey provider. The surveys carrying Experiment 1 and 2 included some empirical measures of group identification to validate the relevance of chosen groups (for details, see Appendix 8.6).

Outcome Measures

After treatment, respondents are asked several outcome questions. Group-based retrospective voting posits that in-group economic performance influences members' satisfaction with the economy and thereby their approval of the government's economic management. The most proximate outcome is therefore satisfaction with the current state of the economy, which is measured in all three experiments with the wording: "How satisfied or dissatisfied are you with the way the [Danish/American] economy currently develops?". In addition, Experiment 1 includes an incumbent-centered outcome, asking subjects: "To what extent do you agree or disagree with the way the current government manages the Danish economy?" Unfortunately, this incumbent-centered measure could only be included in Experiment 1. The Danish government had only been in office for four months as Experiment 2 was fielded, so it could not possibly be held responsible for current group performance. In Experiment 3, in the United States, a presidential approval measure was omitted for practical reasons, as this survey allowed only a single outcome question.¹² In addition, both Experiment 1 and 2 include prospective egotropic evaluations as alternative outcomes for verifying the mechanism.

Following the registered pre-analysis plans, I also control for a set of baseline covariates that closely resemble outcome variables to increase the precision of estimates (Clifford, Sheagley and Piston, 2021). See Appendix 8.7 for an overview.

¹²The experiment was kindly included in Verasight's omnibus survey, which came with strict space constraints.

Table 5.4: Effects of In-Group Decline and Improvement

	Experiment 1		Experiment 2	Experiment 3
	Econ. satisfaction	Gov't approval	Econ. satisfaction	Econ. satisfaction
Rel. in-group decline	−0.12 (0.03)**	−0.09 (0.04)*	−0.10 (0.04)*	
Rel. in-group improv.				0.19 (0.08)*
Controls	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	No	No
In-group FEs	Yes	Yes	Yes	Yes
No. of groups	22	22	6	6
N	1388	1379	1881	2000

Note. Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment (in-group performance vs out-group performance information (ref.), with national benchmark) with in-group fixed effects and design controls. Standard errors clustered at the in-group level in Experiment 1. * $p < .05$, ** $p < .01$, *** $p < .001$.

Experimental Results

To test the group-based retrospective voting hypothesis, Table 5.4 shows the average effects of in-group performance across all groups in each of the three experiments.¹³ The coefficient on relative in-group decline/improvement is the average difference between getting the in-group stimulus and the out-group stimulus across all groups. All models include in-group fixed effects, stimulus-controls¹⁴ and baseline covariates as specified in the pre-analysis plans.

There is support for the group-based retrospective voting hypothesis across all three experiments in the form of statistically significant coefficients in the expected directions. Across the 22 groups in Experiment 1, those informed of in-group decline become on average 2.2 percentage points less happy with the state of the economy than those informed of out-group decline, with a similar effect on approval of the government's economic management. This estimate is strikingly similar for the replication in Experiment 2. In Experiment 3, fielded in the United States, the positive effect of belonging to the economically improving group is if anything larger, with subjects in the in-group condition becoming around 4 percentage points more satisfied with the

¹³See Appendix 8.8 for full results and alternative model specifications.

¹⁴Stimulus-controls adjust for numerical heterogeneity in the exact percentage figure mentioned in each group-specific stimulus. Including them makes no substantive difference to results (see Appendix 8.8).

state of the economy on average. This contrasts with earlier studies finding voters to respond more strongly to negative economic information (Soroka, 2006; Bloom and Price, 1975). Clearly, voters are not agnostic to who gets what in a growing or declining economy. When their in-group over-performs the national trend, they like it more, and when it under-performs they like it less, and this shapes their approval of the incumbent's economic management.

To be sure, these estimates are somewhat modest in magnitude. This is not too surprising given the varying relevance of groups, the heterogeneity in out-group stimuli, as well as the difficulty of manipulating economic perceptions during a period of exceptional economic pessimism. Indeed, manipulation checks for Experiment 1 and Experiment 2 show statistically significant but somewhat modest effects of stimuli on perceptions (see Appendix 8.4. The effects should therefore be understood as the result of moving subjects' perceptions of group performance slightly in the direction of the treatment information.

National Comparisons Drive the Effects

The main version of treatment includes a contrasting national benchmark such that group performance deviates from the national trajectory. However, Table 5.4 cannot tell us directly whether the benchmark plays this theorized role. To further probe the mechanism, I compare the effects from Experiment 1 to parallel versions of treatment omitting the national benchmark (i.e. 'absolute group decline' in Figure Figure 5.1). The expectation is that effects are larger for the relative group decline version than the absolute group decline version of treatment.¹⁵

The results are shown in Appendix 8.8 (Table Table 8.15). While the point estimates are negative when the benchmark is omitted, they are small and statistically insignificant.¹⁶ Additionally, the confidence intervals of these non-benchmark treatment effects are wider despite treatment groups being of the same size. This suggests that treatments omitting the benchmark are more ambiguous, likely because it is unclear how the bad group performance compares to the rest of the country. This is consistent with group-based retrospective voting, as it suggests that relative group performance is a

¹⁵Note that this expectation is not part of the pre-registration for Experiment 1 (see Appendix Appendix 8.3).

¹⁶See Appendix 8.8 for full regression results. Only the effect on satisfaction with the economy is statistically significantly smaller without the benchmark ($p < .05$).

stronger signal of the incumbent's alignment with the group's interests. In sum, the experimental effects seem to be driven by the comparison of group performance to the national trend.

Effects Are Not Driven by Pocketbook Expectations

The experiments show that voters respond differently to information about their in-group's economic performance than to comparable out-group information. I have argued that this occurs because voters care about in-group interests. However, an alternative explanation is that voters use this information to update their expectations about their own financial prospects. For instance, learning that people similar to themselves are struggling might make them more pessimistic about their own future well-being. On this account, the observed effects would stem from changes in prospective pocketbook evaluations rather than perceptions of in-group interests.

Importantly, this alternative pocketbook account contrasts with even the self-interested version of group-based retrospective voting. In the latter, voters use group outcomes as a heuristic for their own interests, presuming that a beneficial incumbent for their group will also generally serve their personal interests. In contrast, the pocketbook interpretation I consider here, is more narrow: it posits that voters only use information about similar others to adjust their personal financial expectations. On that account, group performance would be relevant only insofar as it influences pocketbook evaluations, rather than serving as a significant political signal in its own right.

If this pocketbook mechanism were driving the results, we would, at a minimum, expect treatment to affect prospective pocketbook evaluations. To test this, Experiment 1 includes a post-treatment measure of personal financial expectations, which I regress on treatment using the same model specifications as in Table 5.4. Table 5.5 reports the results (full results in Appendix 8.10). As shown, respondents who received in-group versus out-group treatment information did not differ in their personal financial expectations, regardless of the national benchmark. This suggests that treatment did not alter beliefs about future income prospects. Importantly, this finding does not contradict the idea that group-based retrospective voting may ultimately reflect self-interest. However, it does provide evidence against the concern that the experimental results merely capture thinly veiled pocketbook voting.

Table 5.5: Effects of In-Group Decline on Prospective Pocketbook Evaluations

	Without national benchmark	With national benchmark
In-group decline	0.02 (0.04)	−0.04 (0.03)
Clustered SEs	Yes	Yes
In-group FEs	Yes	Yes
No. of groups	22	22
N	1486	1508

Note. Regression of prospective pocketbook evaluations on treatment (in-group decline vs out-group decline information [ref.], with and without national benchmark). Model specifications are identical to those in Table 4. Standard errors clustered at the in-group level. * $p < .05$, ** $p < .01$, *** $p < .001$.

Conclusion and Discussion

Economic voting research has long established that voters are predominantly sociotropic and focus on the national economy. This paper introduces an important qualification to this view: Voters are concerned not only with overall economic growth but also with how it benefits their own social groups. Incumbents should be competent economic managers but also be aligned with in-group interests. As a result, voters adjust their support based on their groups' performance relative to the national economy.

Across analyses of observational and experimental data from the United Kingdom, Denmark, and the United States, this paper finds consistent evidence of group-based retrospective voting. The observational analysis shows that, holding other economic evaluations constant, changes in in-group performance predicts changes in incumbent support and perceptions of the incumbent's alignment with group interests. Moreover, it shows that voters hold meaningful and predictive perceptions of the performance of their class and geographic groups. To strengthen internal validity, I further conduct a series of experiments that manipulate perceptions of different social groups' economic performance, ruling out alternative explanations that linger in observational analyses. The experiments firmly establish that voters care about in-group performance itself, independent of its implications for the national economy or personal financial outcomes. Moreover, they show that effects are driven by perceptions of how the in-group performs relative to the national trend. The effect of relative group performance is symmetric, indicating that voters do not dislike unequal growth as such, as they support it when it

advantages their own group.

While this paper provides substantial evidence for group-based retrospective voting, several questions remain. The study does not directly examine how effects vary with the personal and political relevance of groups, as theorized. The question of relevant groups may also interact with ideology and deservingness perceptions (Guinaudeau et al., 2023; Bechtel and Mannino, 2022; Schneider and Ingram, 1993) as, e.g., privileged groups may be more forgiving when their conditions worsen if they support redistribution. Similarly, voters may care equally about the performance certain salient out-groups in line with reference group theory (Campbell et al., 1960; Elder and O'Brian, 2022). Future research should explore how much group-based retrospective voting varies based on such group traits and systematically examine the role of out-groups. Another important open question is where exactly perceptions of in-group performance come from, and to what extent they are shaped by personal observation as opposed to, e.g., media reporting and elite narratives (Alt et al., 2022; Kayser and Peress, 2024). In addition, this study does not firmly establish whether group-based retrospective voting is motivated by group interests taking on a symbolic value or by voters using group interests as a proxy for their own. This question looms in much research on group-based political behavior (Kalin and Sambanis, 2018) and is an important area of future research.

The findings have significant implications for research on political behavior and party politics. Empirically, group-based retrospective voting offers a new explanation for well-known patterns in the literature. Some studies have rejected economic explanations for, e.g., the rise of populism on the basis of analyses of egotropic and sociotropic variables (Mutz, 2018; Kates and Tucker, 2019) and overall evidence for such explanations has been deemed “mixed at best” (Berman, 2021). However, my findings suggest that voters may be responding to collective economic grievances even when they are faring well individually and national economic conditions are favorable. When voters act on the (perceived) economic performance of their in-groups, this is not directly captured by standard economic variables. Existing studies may therefore have been too quick to dismiss economic explanations for these phenomena.

These findings also have policy implications for addressing electoral discontent, such as that which fueled the rise of populism or the backlash against the green transition (Kurer, 2020; Stokes, 2016; Colantone et al., 2023). Such discontent is often attributed to individual pocketbook grievances, leading to policies like monetary com-

compensation for those affected by trade shocks or green transition measures (Bolet, Green and Gonzalez-Eguino, 2023). However, this may be less effective if voters are more concerned with group-level economic decline. In such cases, targeted investments in affected communities or group-level compensation might be more effective, consistent with findings in recent studies (Bolet, Green and Gonzalez-Eguino, 2023; Gaikwad, Genovese and Tingley, 2022).

Further, my findings suggest that economic changes can shape group-party linkages (Brady and Sniderman, 1985; Thau, 2021; Stubager and Slothuus, 2013). Governing parties might risk losing coalition groups to challenger parties if they fail to deliver for them once in office, potentially disrupting long-standing party-group ties. Group-level economic performance may be a more credible indicator of a party's alignment with group interests than e.g. rhetorical group appeals (Thau, 2021) which risk being seen as "cheap talk" (Foster, 2021). Thus, it may account for shifts in party coalitions over time that are not fully explained by, e.g., policy pledges or rhetorical group appeals (Guinaudeau et al., 2023; Stuckelberger and Tresch, 2024; Thau, 2021).

Lastly, the notion that voters are only conditionally sociotropic has important implications for electoral accountability. On the one hand, it could promote inclusive growth by punishing incumbents who neglect disadvantaged groups. On the other, it risks making governments overly responsive to the interests of small but electorally significant groups (Healy and Lenz, 2017). This dynamic echoes Ferejohn (Ferejohn, 1986)'s argument with respect to pocketbook voting, where weaker sociotropic voting enables incumbents to 'divide and rule', undermining democratic control. Additionally, the fluctuating salience of social identities make them a shaky foundation for accountability (Klar, 2013; Shayo, 2009). Governments may strategically prime certain group identities to frame their economic management favorably and numb accountability pressures. Future research should examine the conditions under which a group's economic conditions become salient, and the role of elites' "social identity entrepreneurship" (Shayegh et al., 2022) in shaping these perceptions. Understanding these dynamics is crucial for determining whether group-based retrospective voting primarily promotes inclusive growth or weakens democratic control.

Chapter 6

Who (Else) Benefits?: Group-Based Responses to Distributive Policies

A central question in public opinion is how voters respond to distributive policies that benefit them. The common expectation is that voters reward incumbents for personal benefit, following a pocketbook voting logic. Yet, the pocketbook explanation has received little direct empirical scrutiny. In this paper, I challenge the pocketbook account and argue that existing studies conflate personal benefits with perceived benefits to voters' in-groups. I theorize a group-based mechanism in which voters respond to distributive policies based on how they believe those policies affect certain salient in-groups. I test the argument in two empirical studies. First, using survey data on COVID-era stimulus checks in Denmark and the United States, I show that check recipients became more likely to believe their racial or geographical in-groups also benefited. These findings reveal that distributive perceptions are endogenous to personal benefit, casting doubt on the common attribution of policy effects to pocketbook voting. Second, to isolate the causal role of group-based perceptions, I field three pre-registered experiments in the two countries, randomly varying features of hypothetical cash transfer policies. Across experiments, I find that voters' political support depends at least as much on perceived in-group benefit as on personal gain. Importantly, these effects are highly group-dependent, emerging only for groups with strong political identities. Together, the findings show that group-based responses, not just pocketbook concerns, shape how voters react to policies that benefit them, helping explain the wide variation in policy effects across contexts.

Chapter 6

How voters respond to distributive policies is a fundamental question for democratic politics. When governments allocate targeted benefits – cash transfers, tax credits, subsidies – they create winners and losers among the electorate. A large empirical literature documents that voters generally reward incumbents for receiving such benefits, though effects vary considerably (Zucco Jr., 2013; Filipovich et al., 2018; Manacorda, Miguel and Vigorito, 2011; Jares and Malhotra, 2025; Blattman, Emeriau and Fiala, 2018). Understanding what drives these responses matters for democratic accountability: If voters respond predictably to personal benefits, incumbents have incentives to redistribute resources rather than focus on national growth (Ferejohn, 1986). Moreover, it becomes harder for incumbents to get away with ‘cheap talk’ and empty promises when voters effectively validate such claims against the policy effects they experience (Fiorina, 1981; Key, 1961). When voters reward personal benefits, this creates strong incentives for strategic redistributive policy-making that could even become all incumbents need to stay in office.

This empirical literature has produced numerous credible causal estimates of distributive policy effects, but has devoted less attention to understanding what drives citizens’ responses. The standard explanation is pocketbook voting, i.e. that voters reward or punish incumbents for changes in their personal finances (Fiorina, 1981; Key, 1961; Healy, Persson and Snowberg, 2017). Applied to distributive policies, the logic is straightforward: recipients support the incumbent because they experience having more money in their pockets. Indeed, the electoral effects of distributive policies are often cited as prime examples of pocketbook voting because of the clarity of the link between government action and personal gain (Healy, Persson and Snowberg, 2017; Tilley, Neundorf and Hobolt, 2018). Although this may seem obvious, the pocketbook explanation has received little direct empirical scrutiny. Moreover, the heterogeneous pattern of empirical findings sits somewhat uneasily with the predictions of pocketbook voting, with studies finding effects that range from strongly positive to null or even negative, even for policies that are clearly attributed to incumbents (Blattman, Emeriau and Fiala, 2018; Filipovich et al., 2018; Jares and Malhotra, 2025). Thus, there may be more to distributive policy effects than the prevailing pocketbook account suggests.

In this paper, I argue that the pocketbook account represents an overly narrow understanding of how voters process distributive benefits. Rather than simply calculating personal gains, voters simultaneously evaluate whether policies benefit their social

in-groups, and this group-based assessment significantly shapes their response. This argument draws on a long tradition in political behavior research finding social group memberships to profoundly influence how voters think about politics, often emphasizing symbolic group identity attachments (Campbell et al., 1960; Miller, Wlezien and Hildreth, 1991; Kinder and Kam, 2010*b*; Bornschier et al., 2021; Claassen et al., 2021; Donnelly, 2021*a*; Elder and O'Brian, 2022). I argue that for individual voters, the impact of a policy on their in-group often provides a clearer signal of the incumbent's alignment with their interests than their own policy benefit alone. Whether for symbolic or instrumental reasons, voters therefore have reason to look beyond their pocketbooks when evaluating distributive spending.

This group-based mechanism has likely been overlooked because it is often observationally equivalent to pocketbook voting: in most cases, both predict recipients will support incumbents more than non-recipients. This is because personal benefit and 'distributive perceptions' of in-group benefit are intrinsically correlated. Spending policies do not benefit individuals at random but necessarily target shared characteristics that map unto salient group boundaries like young people, the elderly, rural residents, single mothers or low-skilled workers. Thus, benefit recipients are both mechanically more likely to belong to beneficiary groups and psychologically prone to inferring that "people like them" benefit. This creates a bundled treatment where personal and group benefits are conflated. When voters identify strongly with beneficiary groups, they may reward incumbents for perceived group benefits regardless of personal gains. By neglecting voters' perceptions of who else benefits, existing research has thus missed a crucial dimension of how distributive policies translate into political support.

Understanding group-based responding to distributive policy matters for both empirical and theoretical reasons. Empirically, it helps explain the puzzle of heterogeneous policy effects. More so than pocketbook voting, the group-based mechanism predicts considerable heterogeneity in electoral responses to distributive policy depending on which group is targeted. That is because the salience of group identities varies and this conditions responses. When policies benefit groups with strong identities, incumbent support may increase substantially; when they target less cohesive or politically relevant groups, effects may be minimal or even negative. The political return to a given policy thus depends less on its objective size than on *who else* is perceived to benefit.

The group-based mechanism also reshapes incumbent incentives. When voters

interpret distributive spending as signaling alignment with their group rather than just personal gain, the informational value of redistribution increases. As (Drazen and Eslava, 2006) argue, when spending is perceived as benefiting similar others, it becomes more rational for forward-looking voters to respond even when they recognize tactical motivations (Drazen and Eslava, 2006). At the same time, group-based responding simultaneously constrains electoral strategies: incumbents cannot simply target whichever voters seem most pivotal but must consider which group identities will actually mobilize support. The mechanism thus makes distributive spending both a more powerful and a more inflexible electoral tool.

To test this argument, I combine observational and experimental evidence from two very different country contexts, the United States and Denmark. First, I use survey data collected during the COVID-19 pandemic to examine whether individuals who received stimulus checks were more likely to perceive that their racial or geographical in-groups also benefited. Using individual fixed effects and controls for policy eligibility, I find consistent evidence that voters infer group-level targeting from personal receipt. This makes it plausible that distributive policy effects are driven by perceived in-group benefit rather than personal gain alone. Second, I test whether perceptions of group-benefit independently affect political support through three pre-registered survey experiments. Following recent experimental designs on pocketbook voting, I randomly assign features of hypothetical cash transfer policies (Bechtel and Liesch, 2020; Beiser-McGrath and Bernauer, 2023). By independently varying subjects' personal receipt of benefits and how policies benefit various geographical and age groups, I can separate the bundled treatment inherent to observational data. Across experiments, I find that voters respond as much or more to perceived in-group benefit as to personal material gain. Consistent with pre-registered expectations, these effects are strongly heterogeneous, existing primarily for specific groups with high identity salience.

Together, these findings demonstrate that group-based responses constitute an important mechanism behind electoral returns to distributive policies, and one that has often been conflated with pocketbook voting. This in turn helps explain the heterogeneous effects of such policies on political support, as they depend on which group identities are perceived to benefit.

Distributive Spending and Pocketbook Voting

The electoral effects of distributive spending have long been central to models of clientelistic and pork-barrel politics (Cox and McCubbins, 1986; Drazen and Eslava, 2006; Dixit and Londregan, 1996). A cornerstone assumption of these models is that “cash transfers sway votes” (Manacorda, Miguel and Vigorito, 2011, p. 2), enabling politicians to use distributive spending tactically to win over pivotal voters. I define *distributive spending* as policies that deliver *excludable* material benefits, i.e. benefits with certain eligibility criteria that only *some* citizens qualify for.¹ Examples include non-universal cash benefits, tax credits, subsidies, or targeted vouchers or coupons. For any such policy, we can distinguish ‘recipients’ and ‘non-recipients’ as two exhaustive and mutually exclusive groups that do (not) or can (not) expect to qualify for benefits.² At the individual level, the ‘distributive policy effect’ is then the counterfactual difference in government support of an individual being a recipient vs a non-recipient.

The standard explanation for positive distributive policy effects invokes pocketbook voting: voters reward or punish the government for changes in their personal finances (Fiorina, 1978; Key, 1966; Healy, Persson and Snowberg, 2017). On this view, the distributive policy effect is simply voters’ reaction to having ‘more money in their pockets’. Whether this can be explained by an intention to reward or by changes in affect (Huber, Hill and Lenz, 2012), the pocketbook view of policy benefits as “income replacement” predicts monotonic positive effects (Greene, 2022, p. 2).

Yet, there is surprisingly little direct empirical support for the pocketbook mechanism. While scholars have largely solved the methodological challenge of identifying causal effects given the issue of selection into recipient status (Cox and McCubbins, 1986; Dixit and Londregan, 1996; Drazen and Eslava, 2006; Corrêa and Cheibub, 2016), they have devoted less attention to testing the underlying causal mechanism. More problematically, their empirical results do not obviously support pocketbook predictions. Some studies find effects that far outlive temporary benefits, which is “inconsistent” with straightforward pocketbook responses (Manacorda, Miguel and Vigorito, 2011; Kogan, 2021). Others find null or even negative effects (Levitt and Snyder Jr, 1997;

¹This is closely related to the concepts of “particularistic” or “selective” benefits in the literature (Van Lancker, Ghysels and Cantillon, 2015).

²While there may be psychological differences between benefiting versus expecting to benefit from a policy, these distinctions are not central to this paper’s argument.

Blattman, Emeriau and Fiala, 2018), even when benefits are substantial in magnitude (Imai, King and Velasco Rivera, 2020; Green, 2006; Filipovich et al., 2018; Guardado and Wantchékon, 2018; Jares and Malhotra, 2025). A key insight from the literatures on economic voting and policy feedback is that some of this variation can be explained by variation in incumbent responsibility and its visibility as not all policies are equally discretionary or “traceable” (Imai, King and Velasco Rivera, 2020; Hamel, 2024; Mettler, 2011; Powell and Whitten, 1993). Still, this does not explain the occasional negative effects nor some prominent null findings for distributive benefits that were highly discretionary, politically salient and clearly attributed to the government by recipients (Filipovich et al., 2018; Jares and Malhotra, 2025; Blattman, Emeriau and Fiala, 2018). These patterns suggest that pocketbook voting provides an incomplete account of distributive policy effects.

Several studies have hinted at alternative mechanisms. Manacorda, Miguel and Vigorito (2011) theorize that voters use distributive spending to infer incumbents’ “redistributive preferences towards ‘people like them’,” while Drazen and Eslava (2006) suggest voters assess whether incumbents are “favoring some groups over others” (p. 30). Some recent work finds evidence consistent with voters responding to community-level policy effects (Gaikwad, Genovese and Tingley, 2022; Fetzer, 2019). In a different strand, the literature on policy feedback has examined how public support for targeted policies depend on perceptions of which groups they target and the extent to which they are broadly perceived as e.g. deserving (Guinaudeau et al., 2023; Bechtel and Liesch, 2020; van Oorschot, 2006; Schneider and Ingram, 1993).

However, these literatures have not explicitly theorized how voters form and respond to perceptions of in-group benefit alongside their personal benefit, nor directly tested such group-based behavior against pocketbook voting. In the following I take up this line of argument and theorize how voters not only care about their own material gain from a policy but also about who (else) benefits, and how this can explain distributive policy effects.

Group-Based Responses to Distributive Spending

Distributive policies inherently operate at the level of groups rather than individuals. As Kramer (1983) notes, “public policies by definition always affect aggregates of

individuals” (p.106). Governments design policies around shared characteristics, such as age, income, or geography, that correlate with meaningful social groups. As a result, distributive policies constitute ‘bundled treatments’, delivering benefits simultaneously to individuals and to the broader groups they belong to. My central claim is that voters largely respond to these group-level effects rather than personal benefits alone.

Since Converse (1964), public opinion research has found political behavior to be deeply rooted in social group attachments based on, e.g., class, residence, age, ethnicity, and gender (Kalin and Sambanis, 2018; Claassen et al., 2021; Thau, 2019; Achen and Bartels, 2016; Jackson and Carsey, 2002). Voters may care about in-group interests out of a concern for fellow group members or because group interests take on a symbolic value (Becker, 2021; Rueda, 2018; Kalin and Sambanis, 2018; Bernhard, Fischbacher and Fehr, 2006; Sears and Funk, 1991). More instrumentally, group outcomes serve as proxies for what is in individuals’ long-term interests (Kalin and Sambanis, 2018; Sears and Funk, 1991; Donnelly, 2021a). By benefiting their group, incumbents signal their underlying alignment with “people like them” (Drazen and Eslava, 2006; Manacorda, Miguel and Vigorito, 2011). Even for non-recipients, their group’s benefit makes them a more likely recipient from the incumbent’s policies in the future. Voters therefore have reason to care about policies benefiting their in-groups regardless of whether they happen to personally benefit or not.

This group-based mechanism is rooted in *distributive perceptions*: voters’ beliefs about which groups benefit from policy. Unlike personal benefits, which are evident from personal experience, distributive perceptions require voters making inferences about who else benefits from secondary sources. These perceptions are shaped by elite communication and policy design, with policies being more or less visible in their targeting. But distributive perceptions also arise endogenously from personal receipt itself, especially when a policy’s targeting is not made too explicit in the political discourse. Specifically, recipients may treat their own receipt as a cue that the policy benefits “people like them”. This is not an unreasonable inference given that personal receipt implies that the policy benefits people sharing at least some of their characteristics. Such extrapolation from personal experience is well-documented in political behavior, with voters inferring national economic changes from their neighborhoods (Galesic, Olsson and Rieskamp, 2012; Larsen et al., 2019; Baybeck and McClurg, 2005; Bisgaard, Sønderskov and Dinesen, 2016). This implies that recipients

will generally be more likely to perceive their in-groups to benefit, even for policies that are only weakly distributive. Which specific group(s) they infer as benefiting likely depends on various personal and contextual factors like the incumbent's pre-existing group linkages, the stated goals of the policy, as well as their own salient group identities. The key point is that distributive perceptions are likely to arise whether a policy is strongly distributive or not.

Critically, however, it matters which groups benefit, or are perceived to benefit. Not all groups matter equally to their members and political appeals to group identities tend to be more effective for some groups than others (Hersh and Schaffner, 2013; Haffert, Palmtag and Schraff, 2023). One key factor is the salience or strength of a group's political identity, referring to the degree to which voters self-categorize as group members and perceive common political interests (Donnelly, 2021*b*). Targeting groups like 'the upper middle-class' or 'suburban residents' may have little effect if voters do not identify politically with these categories. Indeed, explicit appeals to weak-identity groups can backfire as members may view such targeting as inappropriate or socially divisive, especially if their group is high-status (Robison et al., 2021; Haffert, Palmtag and Schraff, 2023).³ Other group characteristics may also condition distributive policy effects, including perceived deservingness (Bechtel and Liesch, 2020; van Oorschot, 2006; Schneider and Ingram, 1993), social dominance (Haffert, Palmtag and Schraff, 2023), and consistency with prior beliefs about incumbent-group linkages (Thau, 2019; Miller, Wlezien and Hildreth, 1991). I focus on identity strength as the primary moderator, but future work should examine these additional dimensions.⁴

The group-based mechanism thus predicts that distributive policy effects should be strongest for groups with high identity strength and minimal or even negative for groups with weak political identities. This, in turn, can help explain why effects of distributive policies vary as much as they do. The next section addresses the empirical challenge of separating group-based behavior from pocketbook voting.

³Although this mechanism is defined with respect to voters' *in*-groups, voters also care about the plight of certain *out*-groups for ideological or symbolic reasons (Sears et al., 1980; Miller, Wlezien and Hildreth, 1991). Many studies have, e.g., found voters to like parties that are perceived to fight for broadly liked or 'deserving' groups like 'the poor' and 'the elderly' (Guinaudeau et al., 2023; Bechtel and Liesch, 2020; van Oorschot, 2006; Schneider and Ingram, 1993). Such out-group mechanisms are beyond the scope of this paper.

⁴Given the limited number of groups I study, I cannot definitively isolate identity strength from correlated group traits. I return to this issue in the methods section and discussion.

Disentangling Group-Based and Pocketbook Mechanisms Empirically

Research on distributive policy effects typically attributes them to pocketbook voting. However, existing evidence is equally consistent with the group-based mechanism I have theorized. This is because distributive policies constitute bundled treatments: they simultaneously benefit individuals and the groups they belong to. Recipients of a policy benefit are therefore more likely to perceive that their in-groups also benefit. Without accounting for these correlated distributive perceptions, existing work on distributive policy effects has captured the composite effect of this bundled treatment.

This association between recipient status and perceived in-group benefit arises through two channels. First, there is often genuine overlap: when policies target groups, recipients frequently belong to the targeted group. Recipients are therefore more likely to believe their in-group is benefiting for the simple reason that it is true. Second, as discussed in the previous section, recipients may infer group benefit from their personal receipt, treating it as a cue for the policy helping “people like them.” This psychological mechanism means that group-based responses can emerge even for policies with limited actual group-targeting.

The bundled nature of distributive policies creates a fundamental identification challenge. Ideally, one would measure both recipient status and distributive perceptions for relevant groups, allowing simultaneous estimation of pocketbook and group-based effects. However, most studies of distributive policies do not measure such perceptions. Moreover, while scholars have developed credible strategies for obtaining exogenous variation in recipient status, achieving such variation in distributive perceptions is considerably more difficult in observational settings where they are likely endogenous to various political attitudes.

To overcome these challenges, I conduct two studies. First, I use observational data from COVID-19 stimulus programs in Denmark and the United States to demonstrate that recipients of cash benefits indeed become more likely to believe their geographical or racial in-group benefits. These findings show that distributive perceptions are endogenous to personal policy benefit, casting doubt on the common attribution of distributive policy effects to pocketbook voting. Second, I employ survey experiments

in both countries using hypothetical policy proposals. Closely emulating cash transfer policies in both countries at the time, the experiments independently manipulate personal and in-group policy benefits, allowing me to isolate the group-based mechanism from pocketbook voting. As expected, in-group benefit has a substantial effect on political support, but only for strong identity groups. Together, these studies both provide causal evidence for the group-based mechanism and illustrate its observational equivalence with pocketbook voting.

Study I: Group-Based Inferences from COVID-Era Stimulus Checks

To study whether voters use personal policy gains to make inferences about in-group benefit, I leverage survey data from The United States and Denmark. Table 6.1 provides an overview of key features of the two datasets. Both surveys include relevant measures pertaining to COVID-era stimulus checks: the CARES Act stimulus check in the US and the so-called Heat Check in Denmark. In addition, both surveys measure distributive perceptions relating to the stimulus checks for racial and geographical groups, respectively. Importantly, both stimulus policies either did not target, or were not widely understood to target, the groups studied. The ambiguity of their distribution offers an ideal context to test whether personal benefit receipt leads voters to infer that their in-group was also targeted.

Table 6.1: Key features of the surveys.

	US Survey	Danish Survey
Dataset	Annenberg Election Study	Original survey ⁵
Survey type	2 panel waves	Cross-section
Collection period	May & June 2020	April 2023
Population	Citizens of Florida, Michigan, Pennsylvania, and Wisconsin	Danish citizens
N	20,437 (11,691 unique)	3,262
Incumbent party	Republican President	Centrist coalition
Distributive policy	CARES Act stimulus check	the Heat Check
Social groups	Blacks/Whites	Rural/Urban

CARES Act Stimulus Checks and Racial Group Perceptions

The first case examines the CARES Act stimulus checks using data from the Annenberg Election Study Panel, which sampled the voting-age population in four battleground states throughout 2020 and 2021 (Center, 2024). I focus on the first two waves fielded during the Spring 2020 rollout of the CARES Act (H.R. 748), the largest economic stimulus package in U.S. history (Wire, 2020). Signed into law on March 27, the CARES Act included \$300 billion in one-time cash payments to individuals of \$1200 with additional amounts for children, conditional on income. While the legislation included a range of further measures – expanded unemployment benefits, forgivable loans for small businesses, and students grants – the stimulus checks represented a highly visible direct transfer to individuals.

The policy's broad-based design makes it particularly suitable for testing group-based inferences. Rather than explicitly targeting racial groups, the CARES Act used income thresholds and emphasized helping those hardest hit by the pandemic (Sloan, 2020). While this massive federal spending benefited Americans broadly, it disproportionately benefited those with lower incomes and lower job security (Peterson Foundation, 2024). This creates potential for recipients to make group-based inferences to racial groups without clear distributive signals from policymakers.

My identification strategy exploits the staggered rollout of stimulus payments. While the law passed in March 2020, payments began mid-April and continued for several months (Peterson Foundation, 2024). With two waves of the Annenberg survey fielded in May and June of 2020, I capture substantial variation in receipt over a short time span with the share of stimulus check recipients increasing from 78.6% in the May wave to 92.1% in the June wave. The mid-rollout timing of the survey allows me to use individual fixed effects to isolate the causal impact of personal receipt on group-based perceptions, eliminating bias from stable individual characteristics that might predict both receipt and perceptions.

I measure personal recipient status with a survey item asking whether respondents received “a coronavirus relief payment, also called a stimulus payment from the federal government” on behalf of themselves or family members in their household. The key

⁵Fielded by the private consultancy, Moos-Bjerre, which uses Norstat's survey panel. For information about data quality, see: <https://norstat.co/solutions/sample-only/>.

outcome measures distributive perceptions: respondents rated whether White Americans and Black Americans, “got what they deserved” from the federal government’s COVID-related economic plans.⁶

This measurement strategy has two important features. First, while the outcome captures perceptions of all COVID economic measures (not just stimulus checks), the stimulus checks were the perhaps most visible and widely distributed component, making personal receipt a meaningful predictor of broader distributive perceptions. Second, combined with measures of respondents’ racial identity, this allows me to test whether personal receipt increases perceptions that one’s own racial in-group benefited from federal COVID policies.

Danish Heat Check and Geographic Group Perceptions

The Danish case provides complementary evidence using a different policy context and group dimension. In August of 2022, the Danish government distributed the “Heat Check” (Varmecheck), a one-off payment of 6,000 DKK (\$900) without application to all households that satisfied two criteria: i) a gross household income below 706,000 DKK (\$107,000) in 2021 and ii) use of either natural gas or electricity as the primary heat source (or district heating in a few designated areas with large natural gas components).

This policy creates a favorable setting for isolating the mechanism for several reasons. First, eligibility was determined by objective and easily measurable criteria, allowing me to control directly for the factors determining selection into recipient status. Second, while the policy had a strong geographic skew, favoring rural areas where gas and electric heating are more common, this targeting was not obvious from the eligibility criteria or government communication which instead emphasized helping “households squeezed by large price increases” (The Danish Ministry of Climate, Energy and Utilities, 2022). Around 16% of Danish households received the check but almost none of them were in the four largest cities.⁷ A systematic media search shows that in

⁶See section 8.12 for item wordings. The survey also includes other distributive perceptions for, e.g., “large corporations”, “small businesses” and “working families”. I exclude these from the analysis as they do not capture social groups and/or do not allow for cleanly measuring in-group membership.

⁷In particular, the four largest cities in Denmark enjoy near-universal coverage of district heating, thereby excluding nearly all their residents from the benefit. The four largest cities are Copenhagen, Aarhus, Odense and Aalborg which all have more than 100,000 inhabitants and jointly make up a third of the population. The next-largest cities are all significantly smaller with around 50,000-60,000 inhabitants.

the period from when the Heat Check bill was first proposed until the check was paid out, only 2.2 percent of all media articles mentioning the Heat Check also mentioned words like “large cities”, “small cities”, “rural” or “countryside” (see Appendix 8.11). The result was a policy that implicitly benefited rural residents (23% of self-identified rural respondents received it versus 7% of urban respondents; see section 8.11) while maintaining ambiguous distributive signals. Survey data confirm limited awareness of its targeting among the public with respondents evenly split when asked which geographic groups benefited most (see section 8.11). The fact that the Heat Check’s targeting was essentially unknown makes it an ideal case for testing how voters infer in-group benefit from their personal policy experience.

I fielded a cross-sectional survey of 3,262 citizens in April 2023, roughly eight months after the Heat Check was distributed.⁸ Personal receipt is measured directly by asking whether the respondent’s household received the Heat Check, while distributive perceptions are captured by asking respondents to rate their beliefs regarding the policy’s distribution between rural and urban populations (on a scale ranging from benefiting only rural residents to benefiting only urban residents.)

The key methodological advantage is that I observe the exact variables determining eligibility: gross household income and primary household heating source. This allows me to control for the policy’s assignment mechanism. Since these variables jointly and completely determine eligibility, controlling for them provides near-conditionally independent variation in receipt status. In principle, any remaining bias must come not from unobserved confounders but from either misspecification of the functional form, which is to some extent unavoidable, or measurement error in the eligibility variables or reported benefit receipt. This approach allows for isolating the causal effect with minimal bias.

Results

Are voters more likely to think that their in-group benefits from a policy when they benefit personally? I test this directly by regressing perceptions of how geographical (Denmark) or racial (US) groups benefited from the stimulus policies on personal recipient status. The distributive perception variables are coded to match each respondent’s

⁸This survey also included Experiment 2 in Study II. The experiment was administered between the Heat Check receipt item and the distributive perception item.

own reported in-group membership. The Danish models include controls for household income and heat source to account for selection into Heat Check eligibility, while the US models include individual fixed effects to estimate the effects of changes in recipient status within individuals. Given the one-month span between panel waves, most unobserved confounding should be time-invariant and thus captured by the fixed effects. See section 8.13 for detailed model specifications.

Table 6.2: Effects of recipient status on distributive perceptions

	Denmark	United States
	Benefits geographical in-group	Benefits racial in-group
Received check	0.04 (0.01)**	0.03 (0.01)***
N	1568	16 908
Std.Errors	iid	panel id
Individual FE		✓
Eligibility controls	✓	

Note. Results from regression models of perceived in-group benefit on personal receipt of stimulus checks. Outcomes are on 0-1 scales. The model on the Danish sample controls for Heat Check eligibility criteria (household income and heat source) and geographical group membership. The model excludes “people from mid-sized cities” since the outcome only measures distributive perceptions for the two other groups, “rural people” and “people from large cities”. The model on the US sample includes individual fixed effects and cluster errors by respondent. The outcome in this model is recoded such that it measures the distributive perception of the respondent’s own racial in-group (“white Americans” for white respondents and “black Americans” for black respondents). + p<.10, * p<.05, ** p<.01, *** p<.001.

Table 6.2 shows that receiving a benefit increases perceptions of in-group benefit by 3-4 percentage points on average across groups. To be sure, this effect is modest in size, and most variation in distributive perceptions stems from other sources. Still, it suggests that voters form perceptions of in-group benefit from their own personal policy gain, even when these groups are not explicitly targeted by the policies in question.

The consistency of effects across both contexts and the groups involved is notable. Given the short time span between panel waves, the US estimate approximates the immediate process of drawing group-based inferences from personal benefit receipt. The Danish estimate, based on a survey fielded eight months after benefit distribution, indicates that these updated perceptions are rather durable. Together, these patterns suggest that personal benefit receipt leads to genuine updating of distributive

perceptions, not merely temporary priming.

These findings are important because they indicate that distributive perceptions are endogenous to personal policy benefit. This casts doubt on the common attribution of distributive policy effects to pocketbook voting in the literature. If receiving a policy benefit affects both one's material situation and one's perceptions of in-group benefit, then what appears to be pocketbook voting may actually reflect group-based considerations – even for broadly framed and diffuse policies.

However, demonstrating that personal benefits shape group perceptions does not yet prove that group-based mechanisms drive policy attitudes. For the group-based account to challenge pocketbook explanations, distributive perceptions must have an independent causal effect on attitudes, beyond any direct material effects. To test whether group-based perceptions actually explain distributive policy effects, I turn to experimental evidence that can isolate these mechanisms.

Study II: Experimental Evidence from Hypothetical Cash Transfers

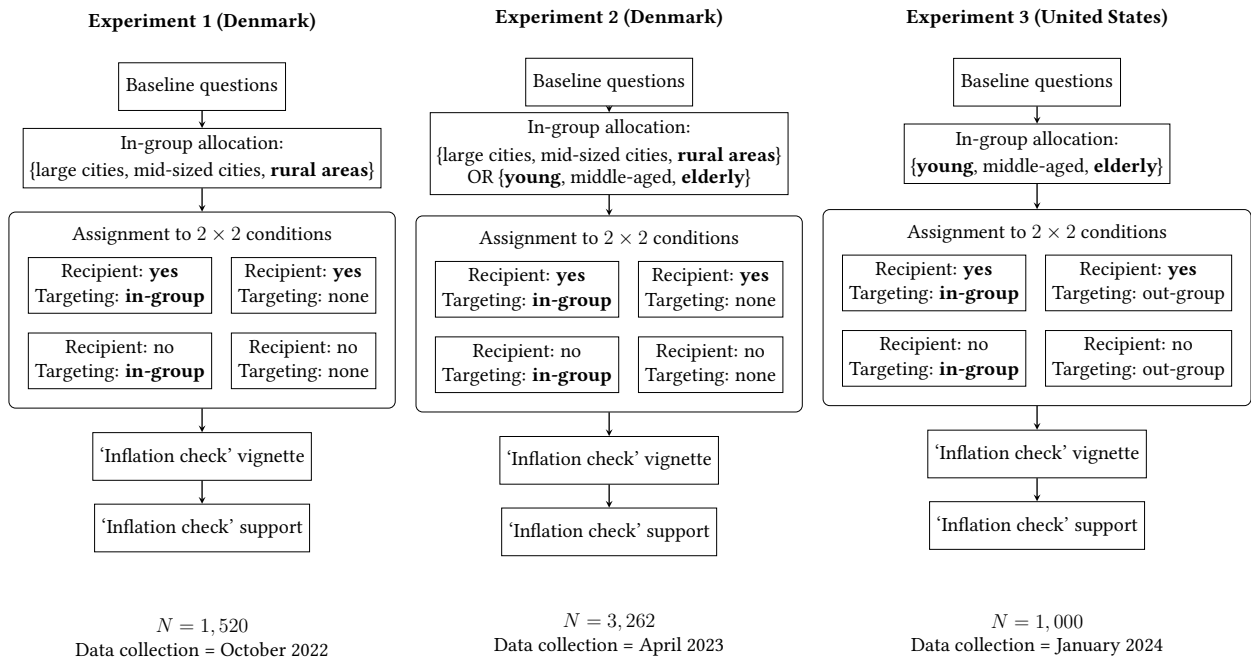
Research on economic voting has increasingly turned to survey experiments (Simonovits, 2015; Hart and Matthews, 2023). In two recent studies, Beiser-McGrath and Bernauer (2023) and Bechtel and Mannino (2022) randomly assign information about the effects of hypothetical economic policies to estimate the strength of pocketbook voting. Here, I follow a similar logic, using hypothetical policy proposals to examine the relative influence of pocketbook and group-based mechanisms.

I field three versions of the experiment, two in Denmark and one in the United States, implementing a pre-registered design with small variations as shown in Figure 6.1.⁹ Pre-registered model specifications are shown in section 8.15. All experiments begin with a set of baseline questions on e.g. partisanship and general support for stimulus checks, used as controls to increase the precision of estimates (Clifford, Sheagley and Piston, 2021) as pre-registered. Next, subjects select their primary in-group from a list of three either geographical (Exp. 1 and 2) or age-based (Exp. 2 and 3) categories. They

⁹Experiment 1: <https://osf.io/bq68x>. Experiment 2: <https://osf.io/tywmx>. Experiment 3: <https://osf.io/k82av>.

then receive a treatment vignette describing a hypothetical “inflation check”, followed by measures of support for the proposed policy.

Figure 6.1. Survey experiment flow diagrams for three experiments conducted in Denmark and the United States. Strong identity groups marked in bold. The pocketbook effect is captured by the contrast between recipients and non-recipients. The group-based effect is captured by the contrast between those getting a policy that targets their in-group and those getting a policy that targets *no* group (Exp. 1 and 2) or targets *an out-group* (Exp. 3). Recipient status refers to the subject’s household in Exp. 1 but the subject personally in Exp. 2 and 3.



The treatment design consists of a 2×2 factorial that presents a hypothetical ‘inflation check’ proposal by the government. Closely resembling actual cash transfer policies in Denmark and the United States at the time, it describes a one-off cash benefit paid to selected households¹⁰ on the basis of need. As shown in the treatment node in Figure 6.1, it independently varies two aspects of the policy: 1) whether the subject personally receives the benefit or not and 2) whether the policy disproportionately benefits the subject’s chosen in-group or not.

This independent variation is crucial for causal identification. In observational settings, personal and group benefits typically correlate, making it impossible to separate their effects. By decoupling these factors experimentally, I can estimate the pure

¹⁰In Experiment 1, the treatment refers to the subject’s household, whereas it refers to the subject personally in Experiments 2 and 3.

effect of group-based considerations, holding personal material gains constant, and vice versa.

Following my theoretical argument, and as pre-registered, I expect group-based effects to vary systematically across different group identities. Groups with stronger political salience (as indicated by bold formatting in Figure 6.1) should show larger responses to in-group targeting than groups with weaker identities. This heterogeneity test provides additional evidence for the group-based mechanism while helping explain why distributive policy effects vary across contexts.

To probe the generality of the group-based mechanism, I study it in two markedly different settings: Denmark and the United States. The United States features high economic inequality, social diversity, and a residual welfare state, which should all heighten voters' sensitivity to group-targeted benefits as voters seek signals of which groups the incumbent prioritizes. Denmark's universal welfare state and social homogeneity create a contrasting environment where group-based redistribution may be less salient, as comprehensive social insurance reduces group competition for resources. At the same time, Denmark's multiparty system and proportional representation may actually facilitate group-based responses by allowing clearer party-group linkages than the U.S. two-party system with its broad coalitions. Further, the stronger partisan polarization in the U.S. may dampen voters' responsiveness to new policy information by anchoring perceptions of the incumbent (Bisgaard, 2019). These differences thus provide a valuable test of whether the group-based mechanism operates across markedly different institutional and partisan environments.

Treatment vignettes

The treatment vignettes combine a shared description of a hypothetical cash transfer policy with experimentally varying benefits for the subject personally and their chosen in-group. This results in the following vignette structure:

Experiment 1 and 2. "Inflation is a growing problem in Denmark. Imagine that the current government proposes a large inflation check of 12,000 DKK to alleviate the problem. It is paid to people that need it the most (based on various criteria, e.g. income). Your household¹¹ [receives | does not

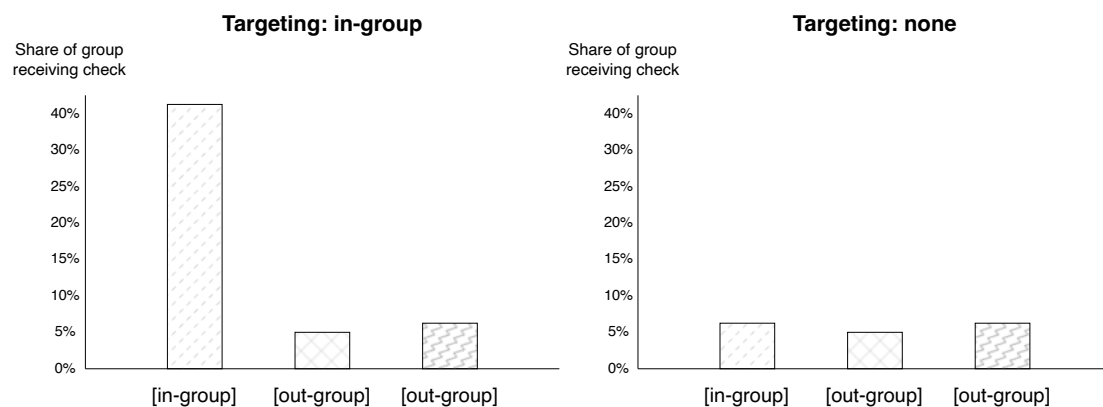
¹¹In Experiment 2, this says 'you' instead of 'your household'.

receive] the benefit. As the graph shows, the check benefits [the whole population | people mostly in [in-group]].”

Experiment 3. “In the wake of two years of inflation, many Americans are grappling with the high cost of living. Imagine that the federal government proposes a large Inflation Relief Check of \$2,000 to alleviate this issue. It is paid to people that need it the most (based on various criteria, e.g. income). Your household [is | is not] eligible for this benefit. The check is designed such that it [mostly benefits [in-group] | mostly benefits [random out-group]].”

For Experiment 1 and 2, the text is accompanied by a visual stimulus showing the distribution of the benefit in a bar chart, in accordance with the text. A generic version of each visual stimulus is shown in Figure 6.2. In Experiment 3, the visual stimulus is omitted.

Figure 6.2. Generic versions of the visual stimuli accompanying the treatment vignettes in Experiments 1 and 2 (translated to English).



Manipulating Distributive Perceptions

In designing the experimental vignettes, a natural starting point would be randomizing information about the distributive impacts of the actual stimulus checks examined in Study 1, controlling for recipient status. However, deceiving voters with fictitious benefit distributions is ethically problematic. Moreover, such made-up distributions might not be credible to subjects.

In light of these drawbacks, I opt instead for a hypothetical stimulus check referred to as an “inflation check”. Similar hypothetical experiments have recently been used to scrutinize the psychological mechanisms of economic voting in the literature (Hart and Matthews, 2023; Bechtel and Liesch, 2020; Beiser-McGrath and Bernauer, 2023) and recent work has found such ‘situational’ hypotheticality to make no meaningful difference to results (Brutger et al., 2022). Further, I design the hypothetical treatments to be highly ecologically valid in their context. The two Danish experiments were fielded at a time where the Danish government had enacted similar cash transfer schemes like the ‘Heat Check’ and the ‘Elderly Check’, and there were even negotiations on an “inflation check” at the time (The Danish Ministry of Finance, 2023). The cash benefit of 12,000 DKK (\$1,800) is double the amount of the Heat Check and roughly the amount of the Elderly Check (10,000 DKK (\$1,530) paid in two installments (Ritzau, 2023)). This makes it a large but not unrealistic payment. In addition, the degree of group benefit is within a realistic range. As it happens, both the Heat Check and the Elderly Check strongly targeted geographical and age groups, respectively.

In the US context, Experiment 3 was fielded at a time where many state-level inflation relief checks had been recently implemented, some of which targeted the elderly (Winters, 2022). The Trump administration also implemented federal stimulus payments in 2020 and 2021, including the CARES Act stimulus checks. Besides matching the amount for the Danish experimental checks, the \$2,000 amount is comparable to existing inflation relief policies, some of which paid up to \$3,400 per household (Department, 2024). In sum, the hypothetical stimulus checks are strong treatments with respect to both their cash amounts and their distributions but remain ecologically valid in their context.

In contrast to the observational study of COVID stimulus checks, the hypothetical cash transfers are designed to be relatively explicit about how they benefit social groups. This is necessary to induce sufficient variation in the relevant distributive perceptions. However, I implement this targeting indirectly: the group-based distribution is indicated only through the reported ex-post distribution of the policy benefit across groups, while the policy framing itself remains neutral and broad-based across all conditions. Echoing actual stimulus policies at the time, the policy is consistently described as helping people in need based on criteria like income, rather than as explicitly targeting specific social groups. While this indirect approach likely reduces the strength of treatments compared

to scenarios with overtly group-specific eligibility criteria or government messaging, it offers several methodological advantages.

First, it strengthens ecological validity. Governments often preserve ambiguity in their communication on distributive policies to avoid alienating voters (Hersh and Schaffner, 2013). An explicitly group-targeted policy could be politically unrealistic, particularly for some included groups like “the middle-aged” that rarely receive overt policy attention. Second, the more subtle targeting approach prevents subjects from drawing systematically different inferences about government intent across treatment conditions, which would confound the comparison of interest (Dafoe, Zhang and Caughey, 2018).¹² Including overtly targeted eligibility criteria (e.g. age for the age groups) or an explicit group-based rationale (e.g. “to help young people”) would run the risk of conflating the effects of distributive benefits with the symbolic appeal made to the group in question (Robison et al., 2021). Finally, indirect targeting represents a more demanding test of the group-based mechanism but also generalizes to a broader set of cases. If subjects respond to in-group targeting even when it is this indirect, it suggests a more deep-rooted tendency to evaluate policies on the basis of in-group outcomes; one that extends beyond cases of explicit group appeals to the more common scenario of policies with ambiguous distributive signals.

For the control conditions, in which the in-group does not stand to benefit, I use both a neutral ‘no targeting’ scenario and an ‘out-group benefit’ scenario as shown in Figure 6.1.¹³ The former scenario, used in Experiments 1 and 2, shows if it makes a difference whether a stimulus check is distributed equally or disproportionately benefits the in-group. However, voters might prefer equal to unequal distributions, which could bias this comparison. To avoid this, Experiment 3 uses a policy that has the same distribution in both conditions, varying only whether it is the in-group or a random out-group that is the main beneficiary. This ensures that it is only the membership of the benefiting group that differs across subjects.

¹²To be sure, the eligibility criteria could be even more specific but that would risk undermining the realism of treatment. If eligibility criteria were more explicitly group-based, subjects might independently deduce their personal benefit status and the policy’s distributive pattern, undermining the credibility of the experimental manipulation.

¹³Experiment 1 also includes two further distributive scenarios: one where the in-group is ‘de-targeted’ and one where everyone gets the high benefit level. See section 8.16 for an overview of all four scenarios. The alternative scenarios are used to test (and exclude) alternative explanations, e.g. that voters like the in-group benefit scenario because the total sum of benefits is higher, generally dislike unequal distributions, or the like. See section 8.16 for some empirical results.

Chosen Social Groups

I focus on age-based and geographical groups for two main reasons. Firstly, all groups of a given type should be realistic beneficiaries of the policy. This excludes e.g. class groups, because cash transfers disproportionately benefiting upper-class groups would be highly unusual and not fit a common policy rationale. Secondly, there should preferably be some variation in identity strength across groups to test the expected effect heterogeneity. In addition to testing whether distributive policy effects exist, this allows me to test whether they also vary as expected.

On the basis of these considerations, I chose three geographical and three age-based groups, as shown in Figure 6.1. Each of these groups could plausibly be the main beneficiary of a distributive policy. Moreover, there is substantial variation in identity strength across them. In line with the literature, young people and elderly people tend to have stronger age identities than the middle-aged, whereas place-based identities tend to be stronger in rural areas than in large and mid-sized cities (Haffert, Palmtag and Schraff, 2023; Belanche, Casaló and Rubio, 2021; Easterbrook, Kuppens and Manstead, 2020; Surridge, 2007). Denmark is no exception to these general patterns with young, old, and rural identities being highly salient in contemporaneous Danish politics (Dausgaard and Stubager, 2024) and recent research has documented high levels of ‘rural consciousness’ (Hansen and Stubager, 2021; Nyholt, Hansen and Kjær, 2024). For each of the three experiments, I pre-registered an expectation that the effect of in-group benefit would be stronger for these three strong-identity groups than for the others.¹⁴ I measure these identities subjectively, asking respondents which of the three (geographical or age-based) groups they feel like they best fit into.

Outcome Measure

Finally, the key outcome measure is support for the policy on a Likert scale. Experiment 1 and 3 additionally include an outcome question about hypothetical support for the incumbent if the government were to implement the policy, and these results are also reported (and similar to those for policy support; see section 8.17). Although the incumbent question gets more directly at the theoretical construct of interest – support

¹⁴To be sure, I cannot exclude other group characteristics correlated with identity strength from driving any effect heterogeneity. I return to this point in the discussion.

for the incumbent enacting the policy – the hypothetical nature of the policy makes it a somewhat awkward and potentially inaccurate measure, which is why it was dropped. This is a cost of using non-deceptive hypothetical scenarios. Still, the policy support measure is better for eliciting subjects' preferences for group benefit in this case.

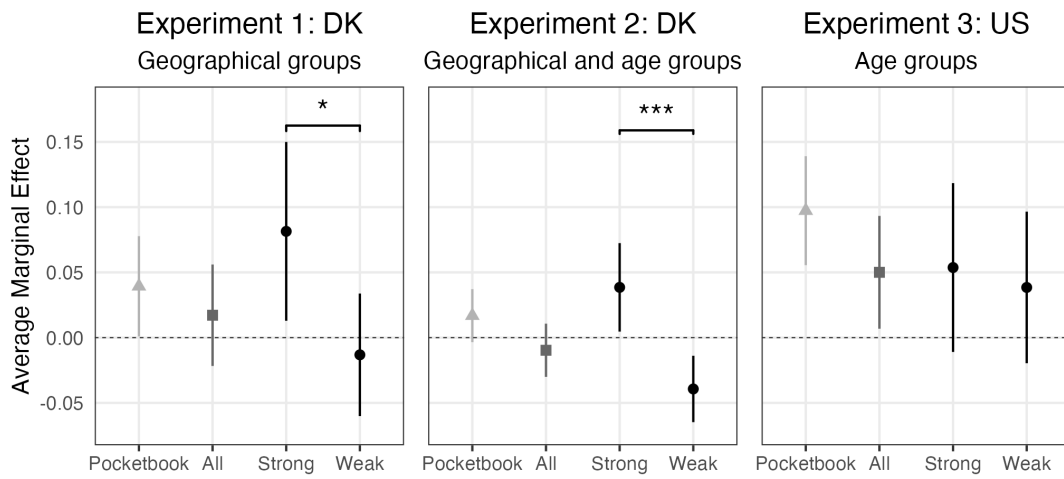
Results

Figure 6.3 presents the effects of personal receipt and in-group benefit on support for the inflation check across the three experiments. Average effects are shown for the full sample and separately for the weak and strong identity groups. As shown, group benefit matters even when personal benefit receipt is accounted for. While the average effect of in-group benefit is only positive and significant in Experiment 3, this masks considerable effect heterogeneity between strong and weak identity groups as theorized. For strong identity groups – the young, the elderly and rural people – support for the policy consistently increases when it benefits their in-group.¹⁵ This positive effect is significant and substantial in all three experiments with estimates ranging between 4 and 8 percentage points. For weak identity groups – people from large and mid-sized cities, and the middle-aged – there are no, or even negative effects of in-group benefit, except for the age groups in the US experiment where the effects for strong and weak identity groups are similar. The results are robust to the changes to the experimental design introduced in Experiment 3, including omitting the visual distribution of the benefit as well as using out-group benefit as control condition instead of no group benefit. The effects are also similar in magnitude and statistically significant for incumbent support in Experiment 1 (see section 8.17).

On the other hand, once group benefit is accounted for, pure pocketbook effects are surprisingly small. In the experiments fielded in Denmark (1 and 2), the estimated effects of personally receiving the benefit, holding group benefit constant, is around 2-5 points and it is only statistically significant in Experiment 1. In the US experiment (3), the pocketbook effect is significant and more substantial at around 10 points. Still, in-group benefit appears as important as the pocketbook in explaining support for distributive policy for strong group identities. Given the prospect of negative distributive policy effects, the pocketbook effect might even be canceled out by group-based response

¹⁵As shown in additional results from Experiment 1 in section 8.16, voters prefer their group being targeted as opposed to just benefiting, but the difference is small and not statistically significant.

Figure 6.3. Average marginal effects of randomized personal check receipt (‘pocketbook’), in-group benefit (‘all’) and in-group benefit split by group membership (‘strong’ and ‘weak’). Include controls for baseline covariates. The outcome is support for the proposed stimulus check policy rescaled to a 0-1 scale. For geographical groups (Exp. 1 and 2), ‘strong’ identity groups are rural people and ‘weak’ identity groups are people from large and mid-sized cities. For age groups (Exp. 2 and 3) ‘strong’ identity groups are the young and the elderly, and ‘weak’ identity groups are the middle-aged. The baseline condition in Experiment 1 and 2 is a scenario where the benefit is equally shared and no group is targeted. The baseline condition in Experiment 3 is a scenario where a random out-group is targeted.



for certain (weak) identity groups. This could help explain why distributive policy effects in observational studies are often null for large cash benefits, and sometimes even negative (Levitt and Snyder Jr, 1997; Blattman, Emeriau and Fiala, 2018).

Discussion and Conclusion

The extant literature on the electoral effects of distributive policies often makes a common assumption: that they can be explained by their material impact on voters’ pocketbooks. Indeed, the electoral effect of distributive policies is typically seen as a prime example of pocketbook voting. In this paper, I challenge this assumption and argue that existing research has conflated pocketbook voting with group-based motivations. Voters often care about in-group interests, for both symbolic and self-interested reasons, and those who benefit from a distributive policy are more likely to

believe their in-group is benefiting from it. This belief is both rooted in reality, since recipients are mechanically more likely to belong to the benefiting group, but also caused by voters inferring from their personal benefit that their in-group benefits. The distributive policy effect may thus be driven by voters reacting to the perceived benefit to their group in addition to their personal gain, even for broadly framed and diffuse policies.

In analyses of stimulus checks in the United States and Denmark, this is exactly what I find. Using observational data on COVID-era stimulus checks in the United States and Denmark, I first demonstrate that personal benefit receipt increases perceptions that one's racial or geographical in-group benefited from the policy. I then isolate the causal impact of these perceptions using three pre-registered experiments in Denmark and the United States that independently manipulate personal and in-group benefits from hypothetical cash transfers. Perceptions of in-group benefit positively affect policy support and appear at least as important as direct personal benefits. Crucially, these effects vary systematically across groups: they are positive and significant for groups associated with strong political identities – like rural residents, young people, and Blacks – but null or even negative for weak identity groups – like the middle-aged or Whites. Those who benefit from a policy may therefore partly be rewarding the incumbent for helping their group rather than themselves.

These findings help resolve a puzzle in the distributive politics literature: why it is not always the case that “cash transfers sway votes” (Manacorda, Miguel and Vigorito, 2011, p. 2). Existing studies show considerable heterogeneity in distributive policy effects with occasional negative and null effects even for benefits that are highly discretionary, politically salient and clearly attributed to the government by recipients (Blattman, Emeriau and Fiala, 2018; Filipovich et al., 2018; Jares and Malhotra, 2025). In ignoring voters' perceptions of who (else) benefits, existing literature has overlooked an important source of variation in distributive policy effects. The group-based mechanism I identify suggests that the electoral effects of distributive policies depend fundamentally on *who* is perceived to benefit, not just *how much* they benefit individually. Policies that disproportionately help groups with strong political identities should generate larger electoral returns than those benefiting groups with weak identities. When groups with little political relevance benefit distributive spending may even reduce rather than increase incumbent support among recipients.

Some limitations to my analysis remain. First, while I find systematic variation in group-based responses that correlates with predicted identity strength, I do not measure identity strength directly, instead relying on previous empirical work on the groups in question. Even if measured, identity strength may correlate with other group traits that could drive the observed heterogeneity such as the in-group's perceived deservingness or social subordination (Haffert, Palmtag and Schraff, 2023; Robison et al., 2021) or its linkage to the party in power delivering the benefit (Thau, 2019). With the limited number of groups included here, I cannot definitively parse out these alternative drivers of effect heterogeneity. Future studies should follow in the footsteps of, e.g., Haffert, Palmtag and Schraff (2023) and Hersh and Schaffner (2013)'s work on group appeals in more systematically theorizing and testing which group attributes make distributive policies effective for some groups but not for others. Nonetheless, the effect heterogeneity I uncover is important in its own right, as it suggests that distributive policy effects depend on who benefits even if group identity strength is not the full explanation.

Second, while I demonstrate that perceptions of in-group benefit matter for policy support, important questions remain about how these perceptions form. I find that even for policies with ambiguous distributive signals, voters hold beliefs about the extent to which their in-groups benefit. I show that these are partially explained by subjects' personal benefit from the policy. Still, the origin and accuracy of voters' distributive perceptions remains an important open question.

Third, it is an open question how this paper's findings generalize to other types of distributive policies and especially those that more explicitly target particular groups. On the one hand, my focus on policies with relatively broad-based rationales suggests that the group-based mechanism is widespread. On the other, distributive policies are sometimes accompanied by political rhetoric that contextualizes who the policy benefits and why. By keeping this communicative aspect at a minimum in my experiments, my results leave open the question of how explicit targeting moderates the effects. I expect such communication to amplify effects but future work should further examine how political rhetoric and policy design interact to shape distributive perceptions.

These limitations notwithstanding, my results have several important implications. They first and foremost shed new light on existing findings in the literature. Rather than pocketbook voting, empirical estimates reflect a composite effect of the pocketbook

and group-based mechanisms. This can help explain why they vary as much as they do.

These findings also invite a broader conception of self-interest than typically studied in political behavior. While the diminished role of the pocketbook mechanism might suggest that material self-interest is less politically relevant than often assumed, my results point in a different direction: voters may understand and act on self-interest in broader, group-based terms. As others have argued, self-interest can extend beyond immediate personal gains to include the welfare of salient in-groups, serving as proxies for members' own long-term interests (e.g., Feldman, 1984; Kalin and Sambanis, 2018). Far from irrational or purely "tribal" behavior (Achen and Bartels, 2016, p. 325), group-based responses may reflect a more sophisticated form of self-interest that uses group cues to infer the alignment of parties with one's broader economic and social interests. In this view, how the incumbent treats one's group becomes a meaningful signal of how they are likely to govern going forward. The mixed results for pocketbook voting thus may not indicate that self-interest is irrelevant, but rather that voters reason about it in more nuanced and forward-looking ways.

Finally, my findings have implications for how incumbents can – and cannot – use distributive spending for electoral gain. Consistent with arguments by Drazen and Eslava (2006), voters appear to interpret distributive policies not merely as individual economic benefits but as signals of an incumbent's alignment with certain social groups. When voters perceive that "people like me" benefit, they infer more information about the incumbent's priorities and likely future behavior, making it rational to shift political support. This group-based behavior amplifies the electoral payoff of distributive spending beyond its direct material effects.

At the same time, the group-based mechanism complicates the assumption that targeted spending always increases incumbent support among recipients. The electoral effects of distributive policies depend critically on which groups benefit and how this benefit is perceived. This introduces new strategic constraints on electoral targeting: not all groups respond equally to distributive benefits and some may even respond negatively. While group-based responses make distributive spending a potentially more powerful electoral tool than previously assumed, they also introduce new strategic constraints on its use.

Chapter 7

Elite Rhetoric and the Running Tally of Party-Group Linkages

With Frederik Hjorth.

Parties' linkages to social groups are key to electoral competition. While traditionally explained in terms of long-standing social cleavages, newer theories assign some role to parties in shaping group linkages. We argue that party elites have even more influence over group linkages than afforded in existing accounts: citizens infer group linkages from 'running tallies' of recent group appeals in elite rhetoric. To test this theory, we develop a novel automated approach that uses language models to measure group appeals observationally. Using data from the UK, we connect citizens' perceived group linkages in surveys to group appeals in parliamentary speech spanning three decades. We find that group linkages robustly track party elites' rhetoric. The association is strongest for group appeals with policy content and among recent news media consumers. Our findings imply that party elites have considerable power to shape group linkages, even in the short run.

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Chapter 7

In early 2024 Keir Starmer, leader of the UK Labour Party, addressed members of Jewish Labour, a party membership organization representing the Jewish community. Starmer appealed directly to the Jewish community: “I dragged my party away from the abyss, and I will never let Britain go anywhere near it either. This country will be safe for you and your children”. With his speech, the Labour leader sought to signal to voters that the Labour Party looks after the interests of the Jewish community. More generally, the speech exemplifies politicians’ efforts to use rhetoric to forge party-group linkages, i.e. widely perceived links between parties and voter constituencies defined by shared group affiliation.

Efforts to forge or strengthen linkages between parties and social groups are understandable in light of the centrality of groups in public opinion. (In the interest of fluency, we refer to party-group linkages in the following simply as ‘group linkages’.) Both classic (Converse, 1964) and more recent (Elder and O’Brian, 2022; Mason, Wronski and Kane, 2021) studies point to perceived group linkages as a key ingredient of party reputations. As a consequence, party leaders have an incentive to rhetorically portray their party as a steward of the interests of social groups within their constituency or groups that are well-liked among their supporters or in the broader electorate (Huber, 2022; Stuckelberger and Tresch, 2024). But are such efforts likely to succeed?

Political scientists have tended to view this question with skepticism. Classical cleavage theory in the tradition of Lipset and Rokkan (1967) sees parties as mostly stable expressions of social group conflict and group linkages as reflections of these cleavages. Exemplifying this view, Lipset and Rokkan (1967) stress that once “established and entrenched, it will prove difficult to change” parties’ ties to social groups (p. 30). As group linkages across Western democracies have nonetheless undergone substantial transformations since Lipset and Rokkan (1967), scholars have sought to develop alternative accounts. One strand of modern cleavage theory emphasizes bottom-up changes in the underlying social structure such as shifting class structures (Oesch, 2006), the emergence of new cleavages beyond class (Stubager, 2009; Bornschier et al., 2021), and increasingly cross-cutting social identities (Dassonneville, 2023). This approach, however, leaves little room for party repositioning (Evans, 2013; Hooghe and Marks, 2018). Another strand, the ‘political choice approach’, does emphasize the role of parties’ changing ideological platforms as a top-down driver of change (Evans and Tilley, 2012, 2017). Even in this tradition, however, changes in group linkages are

typically understood as unfolding gradually, at the level of electoral cycles (Westheuser and Zollinger, 2024) and have mostly been studied in terms of broader shifts in party strategy. This implies that outside of these longer-run, gradual changes, parties are limited in their ability to actively compete on this important dimension of electoral politics.

In this paper, we provide new theory and evidence on how public perceptions of group linkages change in response to elite rhetoric. Specifically, we argue that group appeals, i.e., valenced references to social groups in public speech, offer an effective medium for parties to convey group linkages. (We use ‘valence’ in the psychological sense, i.e., loaded with positive, neutral, or negative affect). Further, we posit that citizens notice and update ‘running tallies’ of group linkages based on recent party rhetoric, similar to the well-described running tally conceptualization of party performance (Fiorina, 1981). The key implication is that group linkages respond to group appeals in elite rhetoric. As such, parties have latitude to shape electorally important perceptions among voters, even in the short term.

To test this theory, we examine how public perceptions of group linkages as expressed in British election surveys track party elites’ group appeals in speeches in the UK House of Commons. (Note that our conceptualization of ‘elites’ encompasses all prominent elected party officials, e.g., all party MPs not limited to the party’s leading figures.) We measure the latter using a novel, automated approach to measuring group appeals in political rhetoric. Consistent with our expectations, we find that perceived group linkages in the electorate robustly track group appeals in parliamentary speech: our estimates imply that making references to a given group more positive by one standard deviation is associated with around 12 percentage points stronger perceived group linkages. In further analyses, we find these effects to be driven by group appeals to especially religious, class, and age groups, in line with research on the role of elites in shaping religion and class cleavages in Western democracies (Evans and Graaf, 2013). We also provide a rare test of the theoretical claim that group appeals are more credible and thus effective when they are ‘substantive’, i.e., include a mention of policy (Thau, 2021). While purely ‘symbolic’ appeals are effective on their own, substantive appeals appear to be more effective by an order of magnitude. Still, we find that controlling for changes in party policy towards the groups we study does not diminish the overall relationship.

We build on recent work examining how elites shape voter cleavages. Studies within the aforementioned ‘political choice’ approach emphasize the role of changing party platforms in shaping class voting in the UK (Evans and Tilley, 2012, 2017) as well as class and religious voting across Western democracies (Evans and Graaf, 2013; Evans, 2013). Another body of work emphasizing the role of elites is the nascent literature on ‘group appeals’ (e.g., Thau, 2023; Dolinsky, 2023; Stuckelberger and Tresch, 2024; Horn et al., 2021). We are thus not the first to consider the role of elite rhetoric in shaping group-based voting.

However, the existing literature has not directly examined whether and how group linkages change as a result of party rhetoric. The modern cleavage literature has described how social groups have changed their alignment to parties but has not examined the influence of elite rhetoric (Bornschieer et al., 2021, 2024). The group appeals literature, on the other hand, has mostly focused on explaining parties’ use of group appeals as an outcome rather than its downstream effects (Huber, 2022; Huber and Haselmayer, 2024; Thau, 2019; Stuckelberger and Tresch, 2024). Recent experimental work provides evidence for effects of group appeals on political attitudes but considers effects on either policy support (Huber, Meyer and Wagner, 2024) or candidate-specific perceptions (Robison et al., 2021).

Most pertinently, Thau (2021) examines how class voting declined as Labour Party rhetoric turned away from the working class and towards business interests under Tony Blair. However, the study considers the ‘reduced-form’ association between party rhetoric and class voting without examining group linkages per se. More importantly, this and much other evidence for the top-down perspective comes from Tony Blair’s transformation of the UK Labour Party with respect to class groups, a special case of a large-scale party rebranding that was famously a years-long, highly costly, and intensely contested effort (Coates, 2005). Thus, little remains known about the malleability of group linkages in the short term and outside of rare, major party rebranding efforts.

Taken together, we contribute to the existing literature in a number of ways. Theoretically, we extend the concept of a cognitive ‘running tally’ to the concept of group linkages, which helps explain their short-term responsiveness to group appeals. We contribute methodologically by developing a new approach using language models to capture group appeals in speech with high accuracy. This approach can be used by scholars to study group appeals observationally at scale to address other substantive

questions. To this end, we make our fine-tuned model publicly available.¹ Empirically, we provide novel descriptive evidence of parties' use of group appeals, and we document a robust association between these appeals and group linkages, a finding which suggests group linkages are more responsive to short-run changes in elite rhetoric than previously appreciated. Our empirical findings also speak to an ongoing debate in the group appeals literature about the role of policy content in group appeals. Briefly put, we find that while purely symbolic appeals do in fact shape group linkages, the estimated effect of 'substantive' appeals that explicitly reference policy is roughly an order of magnitude greater.

We stress that while we find that group linkages are malleable, this does not imply that they are highly variable in practice. In fact, as we show below, group linkages are only moderately time-varying. How do our findings square with this relative stability? To the extent that group linkages are "sticky" (Hooghe and Marks, 2018; Adams, 2012, p. 119), our results suggest this is not because parties cannot effectively change voters' minds. Rather, it is because parties are constrained in what they can credibly communicate. We revisit the question of constraints on group linkages below.

We proceed as follows. In the next section, we develop our theoretical account of how group appeals shape group linkages. In the following section, we present our strategy for eliciting group linkages from surveys and group appeals from parliamentary speech, as well as our modeling strategy. We also present a validation exercise demonstrating that parliamentary speech is in our case a reliable proxy for public-facing communication. We then present descriptive results and regression estimates. We also present auxiliary analyses testing heterogeneity across group types and the moderating role of explicit policy references. In the concluding section, we draw out the main implications of our findings and suggest possible directions for future work.

How Group Appeals Shape Group Linkages

Political parties play a key structuring role in politics (Dalton, Farrell and McAllister, 2011). By having distinct profiles on policy issues and connections to different groups in society, parties help citizens simplify complicated political decisions. By resorting to

¹The model is available for use on the machine learning platform HuggingFace: Positive Classifier, Negative Classifier.

simplified “mental pictures” of what parties stand for and who they represent, voters need not know all the details about every issue position of every party to know who to vote for (Lupia and McCubbins, 1998; Christensen, Skytte and Slothuus, 2023). These collections of mental pictures, commonly known as *party reputations*, are ubiquitous in electorates across various countries and party systems (Brewer, 2010; Dalton, Farrell and McAllister, 2011; Goggin, Henderson and Theodoridis, 2020; Ahler and Sood, 2018; Nicholson and Segura, 2012; Rothschild et al., 2019).

A key component of a party’s reputation is the perception of *whom* the party represents, sometimes referred to as its “constituency-based ownership” (Petrocik, 1996; Stubager and Slothuus, 2013) or *group linkages* (Thau, 2019; Miller, Wlezien and Hildreth, 1991). Going back to Converse (1964), a long line of research has found these perceived connections between parties and social groups to be central for voters’ reasoning about politics (Miller, Wlezien and Hildreth, 1991; Klar, 2013; Dalton, 2018). Group linkages first and foremost shape party choice. When asked directly, perceptions of parties’ group ties are one of the main reasons voters cite for their vote choice (Dalton, 2018). According to one influential theory, voters have long-standing symbolic attitudes towards various groups, and group linkages help them translate these affective stances to parties and policies (Sears, 1993). In this way, group linkages allow voters to simplify complicated voting decisions (Skytte, Slothuus and Christensen, 2024; Converse, 1964; Brady and Sniderman, 1985).

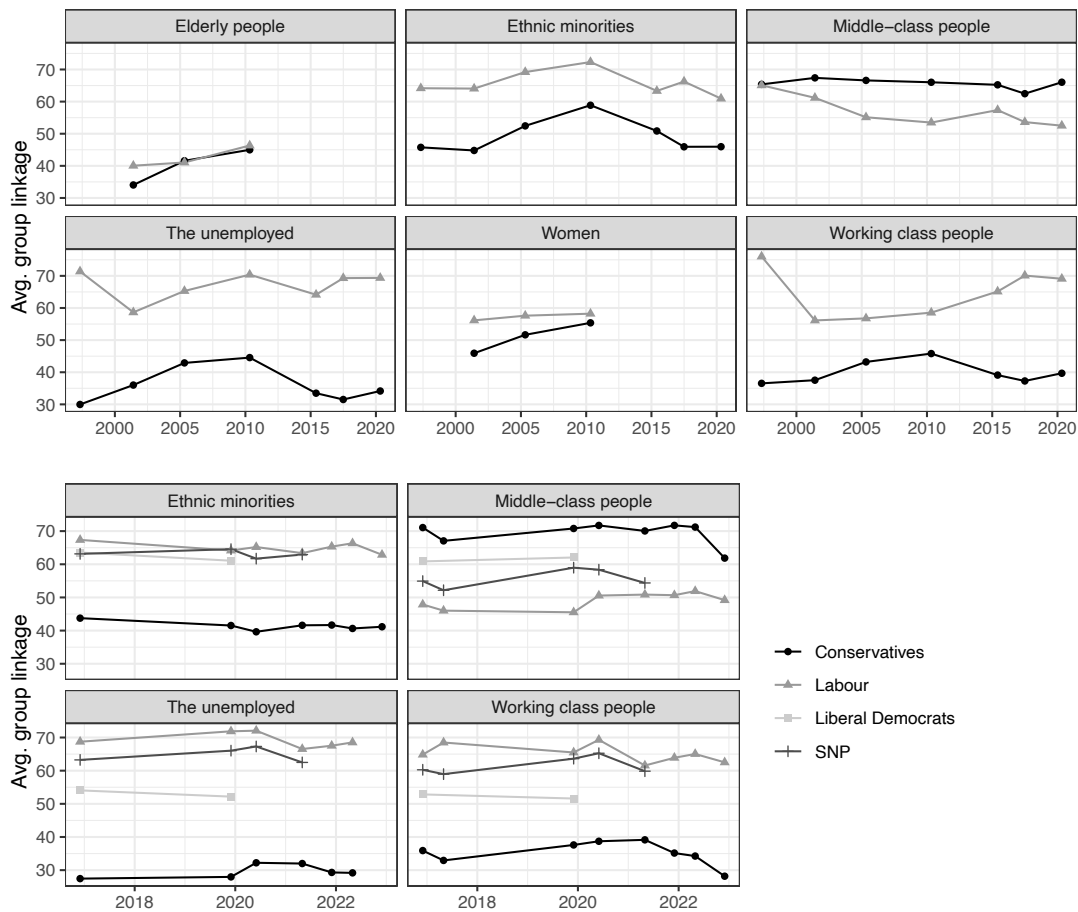
Beyond vote choice, group linkages also shape how voters interpret policy and new issues emerging on the political agenda. If a new policy is opposed by a party with a reputation for representing e.g. the elderly, citizens are able to make accurate inferences about the policy’s consequences for the elderly despite knowing little about the policy’s substance (Christensen, Skytte and Slothuus, 2023; Lupia and McCubbins, 1998; Brady and Sniderman, 1985). Group linkages are thus a powerful tool for citizens to reason about parties as well as politics more broadly.

How group linkages change

Given the importance of group linkages for political behavior, it is central to understand how they are formed and when and why they change. Consider Figure ??, which shows trends in group linkages in the two data sources we study here, the British Election

Study (BES, left panel) and the BES Internet Panel (BESIP, right panel) covering the period 1997-2022 (Fieldhouse et al., 2022, 2023). We elaborate on the measurement below, but briefly put, group linkages capture respondents' perception that a given party "looks after the interests of" a given group on a 4-point scale. We rescale this item to a 0-100 scale to facilitate interpretation. Because time scales, parties, and groups queried differ between the two data sources, we present them separately.

Figure 7.1. Group linkage trends in the British Election Study (BES, top panel) and the BES Internet Panel (BESIP, bottom panel), wave/survey averages. In each case, averages reflect answers to the question "How closely do you think the <party> looks after the interests of <group>" (rescaled to a 0-100 scale). For more details, see 'Measuring group linkages' below. Only parties and groups with observations across multiple years are plotted.



As shown in Figure ??, group linkages exhibit non-trivial variability over time. While the lines rarely cross, the changing size of the gaps between parties can carry substantial electoral implications. For instance, the narrowing of the gap between Labour and the Conservatives' working class linkages from 1997 to 2005 was famously electorally significant (Thau, 2021; Evans and Tilley, 2012, 2017). What drives such changes in group linkages?

While little empirical research has examined changes in group linkages directly, this question is closely related to the large literature on changes in group-based voting. Two competing perspectives dominate this literature. On the one hand, cleavage theory in the tradition of Lipset and Rokkan (1967) considers changes in group-based voting to be a matter of “epochal structural shifts” (Westheuser and Zollinger, 2024, p. 4) and “gradual trends” (Evans, 2013, p. 635) driven by ‘bottom-up’ changes in the social structure (Oesch, 2006; Stubager, 2009; Bornschier et al., 2021). On the other hand, the ‘political choice’ perspective emphasizes the role of ‘top-down’ changes in party strategy, which can produce changes at the more granular level of electoral cycles (Evans, 2013; Westheuser and Zollinger, 2024). A shared assumption across both perspectives is that group linkage perceptions are somewhat sticky and require either structural changes or concerted strategic efforts by party elites to change. In the following section, we lay out an alternative mechanism by which group linkages change in response to short-run changes in political rhetoric.

Group linkages and group appeals

Building on the top-down view, we argue that group linkages respond to the way parties communicate publicly about groups. Rhetorical appeals to groups are abundant in party communication (Thau, 2019; Huber, 2022; Dolinsky, 2023; Stuckelberger and Tresch, 2024) and they provide signals to voters of which groups a party cares about and represents, and which groups they do not. These rhetorical signals are effective exactly because they are ubiquitous (Thau, 2019) and require no or little political or subject-specific knowledge for citizens to make sense of. In addition, as instances of valenced speech, group appeals will typically have some emotional or moral charge making them more likely to leave a psychological impression (Scott, O'Donnell and Sereno, 2012; Lipsitz, 2018; Potter, Lang and Bolls, 2008).

In the conceptual language of Fiorina's (1981) influential theory of party identification, we suggest that voters keep 'running tallies' of parties' use of group appeals over time, and that these tallies help shape perceptions of group linkages. As a given party makes positive and negative appeals to a given group, voters sum over recent appeals to assess the current valence of the party's linkage with the group. Much like the pocketbook, group appeals in political speech are easy to decode, and voters need only evaluate a recent window of such rhetoric to estimate the party's current stance towards the group. In this sense, the running tally serves as a low-effort heuristic for the broad electorate to update or reconstruct their perceptions of parties' group linkages.

This notion of group linkages as responsive to running tallies of elite rhetoric builds on the emerging literature on group appeals. Our theory can thus be seen as an operationalization of a key mechanism in this literature (e.g., Dolinsky, 2023; Horn et al., 2021; Stuckelberger and Tresch, 2024; Thau, 2019). As Thau (2021) argues, group appeals affect voting because they "prime [citizens] to think about politics in terms of these same group categories and how they relate to parties" (p. 677) and directly provide the "interstitial 'linking' information indicating why a given party or policy [proposed by that party] is relevant to [a given] group" (Converse, 1964, p. 236-237, in Thau (2019)). We operationalize the idea in our main hypothesis as follows:

Hypothesis 1 *When a party uses more positive appeals and/or fewer negative appeals to a group, voters become more likely to think the party is looking after the group's interests, and vice versa.*

We follow Thau (2019) in defining a group appeal as "a party associating or dissociating itself (or another party) with a specific group category" (see also Thau, 2023). An important feature of this definition is that group appeals are valenced. Much existing work in this area focuses on how much parties talk about each group, implying that group linkage perceptions are driven by group salience (Horn et al., 2021; Riethmüller, Dehne and Al-Gaddooa, 2024). On the running tally account, however, valence is key. Negative appeals to a group cancel out positive appeals, and unvalenced or ambiguous talk about groups does not lead voters to update group linkage perceptions. We test this key aspect of our theory directly in the analysis.

Our definition does not specify the exact forms group appeals can take. Some

recent work has begun qualifying this (Huber and Dolinsky, 2023). Here, we narrow Thau's definition and depart somewhat from Huber and Dolinsky (2023) in stressing the symbolic nature of group appeals and limiting them to political speech. There are innumerable ways for a party to "(dis)associate" itself with a group, and including all of them in the concept risks stretching it to an extent where it ceases to be useful (Sartori, 1970). We therefore do not count e.g. a targeted policy on its own as a group appeal to the targeted group, nor the (deliberate) choice of a minority candidate in a party as an appeal to the minority group in itself.

Note that while our definition allows for both positive and negative valence—and our measurement strategy incorporates this—this does not imply that positive and negative appeals are equally likely. Since politicians generally speaking choose rhetoric that maximizes electoral appeal, positive appeals should constitute the lion's share of group appeals. In contrast, we would expect to see negative appeals mainly in cases where (i) the target group is widely disliked by the party's core electorate, or (ii) the negative appeal implies a positive appeal to a competing group.

Importantly, we hypothesize that group appeals change perceived group linkages across the electorate as a whole. While members of the group in question naturally have more at stake and may be influenced more strongly by appeals to their own groups, party linkages to out-groups can also shape policy and party support. This aligns with existing work finding that voters like parties and policies that are perceived to fight for broadly liked, high-deservingness groups like 'the poor' and 'the elderly' (Guinaudeau et al., 2023; Schneider and Ingram, 1993; van Oorschot, 2006) and that parties may even mobilize support in certain groups by making negative appeals to specific out-groups they dislike (Stuckelberger and Tresch, 2024). Likewise, group linkages are not limited to voters' self-identified party. Generally, voters keep track of not only party reputations for the parties they support but regularly incorporate the reputations of out-parties in their political decision-making (Christensen, Skytte and Slothuus, 2023). In particular, parties may link social groups with opposing parties negatively, for example, by associating an out-party with groups like 'the rich', which are disliked broadly or by in-party supporters. As such, it matters how group linkages are shaped by group appeals in the electorate as a whole.

Constraints on group appeals

A key implication of our running tally theory of group linkages is that parties can shape them through the concerted use of group appeals. If, for instance, the Labour Party were to suddenly stop talking about issues facing Londoners, voters should register this and update their perceptions of Labour's linkages with urban people. This implication is broadly in line with claims that group linkages are not something for parties to take for granted (Mair, 2013) and, more broadly, speaks to political elites' power to activate social cleavages (Robison et al., 2021).

This naturally raises the question: why not appeal maximally to all groups? Parties are naturally interested in maximizing their standing among voters, and have in other contexts been shown to strategically appeal broadly in order to be 'everything to everyone' (Somer-Topcu, 2015). Still, we highlight four reasons why parties are in practice constrained in their use of group appeals.

First of all, some group linkages reflect long-standing alignments between parties and social groups, which group appeals are not likely to affect on their own (Stuckelberger and Tresch, 2024). Hence, group appeals are in practice most likely to be observed when they reaffirm existing alignments, or when they target weakly aligned groups. Second, and relatedly, parties are constrained by their ability to match rhetoric with policy. Repeated symbolic appeals without policy content are likely to engender public pressure for corresponding policy, lest they be accused of 'cheap talk'. As parties respond to this pressure, they must grapple with policy-relevant scarcities. Hence, group appeals will inevitably have some connection to binding policy tradeoffs. Third, even setting aside policy tradeoffs, symbolic appeals are not without risk. Research on voter targeting/tailoring by political campaigns is generally suggestive of limited electoral gains, either due to mistargeting (Hersh and Schaffner, 2013) or backlash from overly overt tailoring (Gahn, 2024). This inevitably constrains how overtly politicians can appeal to social groups without risking backlash. Fourth and lastly, group appeals are sometimes partially mutually exclusive. Within major categories such as gender, ethnicity, or region, voters are in some cases likely to make inferences across groups. For example, a politician repeatedly stressing her party's focus on the interests of rural people can, in doing so, strengthen her party's linkage to that constituency, but urban voters are likely to infer that this comes at their expense. These negative spillover

effects mean that, in practice, parties must prioritize appealing to some groups over others in a somewhat consistent fashion.

Methods and Data

We now turn to our empirical strategy. Our goal is to link voters' perceived group linkages to parties' group appeals. In this section, we describe our measurement of each of these in turn. Lastly, we present our estimation approach, including threats to causal inference.

Measuring group linkages

We begin by obtaining a time-varying measure of citizens' perceptions of a diverse set of group linkages. In the following, we use the term *dyad* to refer to any specific party-group linkage: for example, we consider the link between Labour and working-class people as one dyad, the link between Labour and elderly people as another dyad, and so on. To obtain time-varying measures of group linkages at the dyad level we turn to the British Election Study (BES). The BES comprises both a panel component, the BES Internet Panel (BESIP) running from 2015-2023, and a series of cross-sectional election surveys (BESES) going back to 1964 (Fieldhouse et al., 2022, 2023). To maximize coverage, we pool them to obtain a combined dataset spanning 1997-2022 with a total of 16 surveys of which 8 are panel waves. Our main results are based on the combined dataset but we conduct additional analyses on the panel data subset to leverage within-individual variation. Given the significantly larger sample sizes for the BESIP waves than the BESES, most of our observations are concentrated in the period from 2010-2022.

The included waves of BESIP and BESES all ask respondents the following question for each major political party: *"Some people say that all political parties look after certain groups and are not so concerned about others. How closely do you think the <party> looks after the interests of..."*, followed by a number of different groups. For each group, the respondent then answers on a four-point scale ranging from *Not at all closely* to *Very closely*.

This survey item closely tracks our chosen theoretical definition of group linkages. It stands to reason that a respondent seeing a party as "looking after the interests

of” a given group is thereby expressing that the party represents the interests of that group, corresponding to our definition of a group linkage. Hence, we consider this item a theoretically valid measure of each respondent’s perception of dyad-level group linkages at a given time.

Each survey contains this measure of group linkage for several dyads across several UK political parties and distinct groups. While the dyads included vary from survey to survey, there is also substantial overlap. We choose here to focus on the four major political parties: Labour, the Conservatives, the Scottish National Party (SNP), and the Liberal Democrats. These all had more than five seats in the House of Commons during the entire period under study. With respect to groups, we exclude only a few groups e.g. trade unions and big business, which are in a conceptual gray zone, as well as “people in my local area”, which cannot be similarly linked to parliamentary speech. In sum, we have a total of 55 party-group dyads observed across 16 surveys. See Table 8.21 in Appendix 8.18 for an overview of the parties, groups, and waves in the BES data that we use and Table 8.22 for more granular details on available dyads in each wave.

We combine these surveys by modeling each individual dyad-level response so that each respondent is observed multiple times in each survey. After omitting nonresponses this yields a combined survey data set of 1,695,236 response-level observations from 83,504 unique respondents. Because of temporal variation in survey responses – even within the same survey wave – respondents at each interview date are exposed to a unique running tally of recent party-dyad-specific group appeals. We now turn to how we connect these responses to parties’ group appeals.

Measuring group appeals

To measure parties’ group appeals we turn to parliamentary speech. To fix terminology, in the following, we use the term ‘group mention’ to refer to any mention of a group that may or may not constitute a proper group appeal. In broad strokes, our measurement approach proceeds in three stages: first, we identify all group mentions in a corpus of speeches from the UK House of Commons. Second, we create a summary measure of the valence of each reference, coding mentions that do not constitute a positive or negative group appeal as neutral. Third and lastly, we link each survey-based group linkage response as described above to a running tally of group appeals in the months

preceding the survey response.

Our use of parliamentary data breaks with standard practice in observational studies of group appeals, the vast majority of which are instead based on party manifestos (e.g., Thau, 2019, 2021, 2023; Horn et al., 2021; Huber, 2022; Huber, Meyer and Wagner, 2024), with Evans and Tilley (2017) being a notable exception. However, relative to manifestos parliamentary speech has far higher temporal resolution. This feature is critical for our identification strategy, which relies on tracking within-party changes in group linkages in response to changes in group appeals. In contrast, an approach linking surveys to manifesto data would need to track within-party changes across years or decades, with far fewer external factors held constant.

In addition to the high frequency of parliamentary speech, which is critical for capturing short-term fluctuations in group appeals, we consider parliamentary speech a useful window into party communication for two reasons. First of all, clips from parliamentary speeches often circulate in traditional and on social media, and this is a likely channel through which voters are exposed to group appeals. As a consequence, MPs speak in parliament knowing that any given excerpt of their speech may be picked up by the media. Second, parliamentary speech takes place within the context of parties' overall communication strategies. Hence, a measure of parties' group appeals based on parliamentary speeches is to some extent a proxy for these parties' use of group appeals in their communication more broadly. To be sure, these are merely theoretical arguments. After describing our measurement strategy in more detail, we validate this assumption, showing that group appeals in parliamentary speeches and press releases correspond closely.

To collect group appeals from parliamentary speech, we use the speech corpus for the House of Commons available in two parliamentary speech databases, *ParlSpeech V2* for speeches from 1997-2019 (Rauh and Schwalbach, 2020), and *ParlaMint 3.0* for speeches from 2020-2022 (Erjavec et al., 2023). Jointly, these datasets contain the complete universe of more than 1.6 million House of Commons Speeches held in the 25-year period under study.

Identifying group mentions

The first task is to identify the set of potential group appeals. To do so, we first break the speeches down to the sentence level. Disaggregating speeches to the sentence level has the advantage that we are able to isolate the specific contexts of group appeals. This disaggregation step yields around 13.4 million sentences, each linked to speech and speaker characteristics.

Once speeches are disaggregated to the sentence level, we locate group mentions using a dictionary approach. We rely on the English-language group appeals dictionary developed in Dolinsky, Huber and Horne (2023). While this dictionary is carefully made, it is not developed specifically for parliamentary speech. To ensure that the dictionary retrieves group mentions with as high accuracy as possible, we read through a sample of sentences to identify and filter out frequent false positives. Our augmented dictionary (see Appendix 8.19) identifies one or more mentions of our 13 groups in just shy of 550,000 sentences, corresponding to 4 percent of sentences. Hence, group appeals occur in a small proportion of all sentences, like virtually all features of natural language (Baayen, 2001). However, at the level of parliamentary debates, group mentions are common: out of the 3,555 observed days in our data, group mentions feature in all but two. This cuts across parties. Across all party-week dyads, 93 pct. feature group mentions at least once.

Capturing appeal valence

A purely dictionary-based approach would simply count each of these identified mentions as a group appeal. However, this approach is flawed for two reasons. First of all, many group mentions are not in any meaningful way group appeals. For example, the sentence “Why be more Catholic than the Pope on this issue?” matches our dictionary for Christians, but the phrase is clearly used in the idiomatic sense. Our measurement strategy needs to be able to weed out such false positives.

More importantly, group appeals cannot be assumed to be all positive, but can also be ambiguous or outright negative. Following our theorization, the *valence* of group appeals is key to their function in associating and dissociating groups and parties (Thau, 2019). This is also clear from empirical inspection. Consider, for example, this sentence, spoken by a Labour MP in 2014: “Some 85% of the tax allowance will go to men”. From

the context, it is clear that the Labour MP is critical of men receiving more of the tax allowance. Hence, this sentence would not lead a voter to infer that Labour looks after the interests of men per se. In fact, by casual inspection, mentions of ‘men’ rarely constitute positive appeals and are instead either residual false positives (e.g., phrases like ‘businessmen’) or outright negative (as above). Hence, simple mention counts are not a reliable proxy for group appeals.

To solve this measurement problem, we turn to the literature on stance detection. In natural language processing, ‘stance detection’, usually considered as a subproblem of sentiment analysis, refers to the problem of detecting whether a given text’s stance toward a given target is positive, negative, or neutral (Küçük and Can, 2020). Stance detection methods are relevant in this case because the theoretical valences of group appeals—positive, negative, or neutral—are closely related to what these methods are optimized to predict. We implement this approach using *PoliStance*, a large language model trained for zero-shot classification of stances towards political groups and people.² *PoliStance* is itself built on DeBERTa, a transformer model developed for text classification (Laurer et al., 2023). While *PoliStance* can perform stance detection without any tuning or exemplification (i.e., ‘zero-shot’), performance improves greatly when the model is fine-tuned, i.e. allowed to learn from a set of annotated sentences.

We proceed in two separate steps. First, we manually annotate a set of sentences for group appeals. Consistent with the logic of stance detection, we annotate each sentence as either a negative appeal (-1), a positive appeal (1), or a fully neutral or non-appeal (0) based on a detailed coding scheme. To assess intercoder reliability, both authors independently code a subsample of 40 sentences. This test yields satisfactory intercoder reliability (Krippendorff’s $\alpha = .72$). Importantly, since our eventual independent variable is a running tally of valence, non-appeals as well as any residual false positives from the dictionary-based group detection are coded to 0 and will therefore not affect the measure. Since appeals are empirically highly imbalanced, with far more positive appeals than negative ones, we prompt the large language model GPT-4 to predict valence labels for large samples of appeals and use these predictions to oversample likely negative appeals. We conduct multiple rounds of annotation to maximize balance across valence categories in the annotated set. In total, we manually annotate 2,534 sentences for fine-tuning. Appendix 8.20 presents examples of sentences annotated as

²See <https://huggingface.co/mlburnham/deberta-v3-large-polistance-affect-v1.0>

positive and negative appeals.

In the second step, we use these annotated sentences to fine-tune PoliStance. Because appeals in the annotated set are imbalanced, even with the boosted subset of negative appeals, a multiclass prediction model performs poorly. Instead, we fit two separate models, one predicting positive appeals (vs. neutral) and one predicting negative appeals (vs. neutral). Both models perform well ($F1_{pos} = .88$, $F1_{neg} = .82$). We present full accuracy statistics for both models in Appendix 8.20.

For each of the roughly 550,000 sentences in our data, we now have separate model predictions of whether the sentence has positive and negative valence respectively. Using these models, we define the *net valence* (NV) of sentence s as the predicted probability of a positive appeal minus the predicted probability of a negative appeal:

$$NV_s = Pr(v_s = 1) - Pr(v_s = -1)$$

The measure captures our best estimate of whether each sentence is a negative or positive appeal. At the sentence level, net valence ranges from -1 (the models are fully confident the sentence is negative) and 1 (the opposite is true). Importantly, if the models are not confident an appeal is either positive or negative, net valence will equal zero and will thus not affect the running tally.

Validation: Group appeals in parliamentary speech vs public-facing communication

While parliamentary speeches are an attractive data source for the kind of fine-grained temporal analysis we need to test our theory, they also come with an important caveat: parliamentary speeches generally do not reach voters, at least not directly. As such, their utility hinges on the extent to which they are transmitted to the public via media coverage, or alternatively, reflect broader patterns in party rhetoric that are also present in more public-facing communication.

To assess the validity of using parliamentary speeches as a proxy for more visible party rhetoric, we compare them to a more explicitly public-facing source: party press releases from the PARTYPRESS database (Erfort, Stoetzer and Klüver, 2023), which includes all press releases issued by major UK parties from 2010 to 2019. We code group appeals in this corpus using the same procedure as for parliamentary speeches, covering

the same groups and parties in that period. This yields a dataset of approximately 20,000 mentions.

However, the press release data are too sparse to support the main analysis. While the median party-group dyad occurs 67 times per year in parliamentary speeches, it occurs only 3 times per year in press releases. As shown in Figure 8.12 (Appendix 8.21), this large gap makes it infeasible to use press releases for the kind of high-resolution temporal analysis our approach requires.

Instead, we use the press release data to validate our parliamentary speech measures. Specifically, we compute the average net valence for each group-party dyad by quarter and compare values across the two sources. We find that the relationship between the two is strongly positive ($r = .68$, $t = 31.3$, $p < .001$). In Appendix 8.21 we visualize and report regression estimates of this relationship. In sum, the valence of group appeals in parliamentary speeches closely mirrors that of press releases, bolstering our confidence that our speech-based measures constitute a useful proxy for group appeals in public-facing party rhetoric more broadly.

Model specification

We now turn to model specification. We estimate a series of regression models using the following specification setup:

$$\text{Group linkage}_{ijkt} = \beta_1 \sum_{t=-90}^{t=-1} \text{NV}_{ijt} + \beta_2 n_{ijt} + \alpha_i + \delta_j + \gamma_k + \lambda_t \quad (7.1)$$

In this specification $\text{Group linkage}_{ijkt}$, our main dependent variable, is the linkage between party j and group i perceived by survey respondent k at time t . Our independent variable of interest is the summed net valence of sentences spoken by party j MPs about group i in the 90 days preceding the survey response. This quantity represents the cumulative rhetorical input that voters can observe and incorporate into their tallies of group-party linkages. While the psychological “running tallies” in voters’ minds are not directly observable, we thus measure the observable input to that process. Hence, our estimate of interest, β_1 , captures how this observable rhetorical record relates to perceived group linkages in voters’ minds.

To be sure, a bivariate association between group linkages and group appeals in itself tells us little. For example, voters are likely to perceive Labour as representing

working-class people due to its historical role as a political movement for the working class. For the same reason, Labour MPs will likely dedicate relatively more attention to the working class, and this alone would give rise to an association between group appeals and perceived group linkages.

To guard against this confounding from long-standing party reputations, our identification strategy uses the richness of our data to implement party fixed effects and group fixed effects, which isolate short-term within-party variation in group appeals and group linkages, as well as other fixed effects for a large subset. We include combinations of α_i , δ_j , γ_k , λ_t , fixed effects for group, party, time, and respondent, respectively, with the most restrictive specification including all fixed effects at once. To be sure, while this restrictive set of fixed effects accounts for a range of time-invariant confounders and common shocks, our observational approach is not immune to confounding. Specifically, β_1 could be confounded by dyad-specific events that jointly affect parties' use of group appeals as well as perceived group linkages for that dyad.

We also include n_{ijt} , the number of dyad appeals in the exposure window. This turns β_1 into an intuitive quantity that closely follows our theory: β_1 is the increase in a party-group linkage from shifting one group mention from neutral to positive, or from negative to neutral, at a fixed number of group mentions. It thereby directly captures the impact of an increase in the average of appeal valence, holding the group's salience in party discourse constant.

To illustrate the importance of controlling for mention frequency, consider the effect of adding a large volume of 'lukewarm' group appeals with far-below-average valence to a positive running tally. Mechanically, this addition would increase the net valence sum (NV_{ijt}) but it would also drag the average valence down. By including a control for mention frequency, we allow voters' perceptions to be influenced more strongly by a given net valence sum when it is spread out over few (strongly valenced) appeals rather than many (weakly valenced) appeals. This contention is indirectly tested by the model: if correct, the coefficient on appeal frequency, β_2 , should be negative, reflecting that voters respond negatively to the decreasing average valence as the number of appeals increases but the net valence sum remains constant.³

³For instance, 100 mentions with a neutral valence of 0.05 sum to a net valence sum of 5, but so do 5 mentions with a maximally positive valence of 1. A null β_2 indicates that voters are indifferent between these two, whereas a negative β_2 indicates that the former scenario influences voters' running tallies less than the latter.

It is important to clarify that the modeled variation in the independent variable occurs not at the level of unique respondents but rather at the level of unique 90-day exposure windows. This window-level variation is more granular than survey waves, as survey waves typically span between 1 and 4 months. Respondents interviewed at various points during each wave will therefore face different exposure windows for the same dyad. Yet, this variation is considerably less granular than individual-level observations since all respondents interviewed on the same date have identical exposure windows for each dyad. This is worth bearing in mind when interpreting the models, as there is somewhat less true variation than each model N would seem to suggest. Finally, since we observe group linkages at the level of individual responses, while respondents are exposed to the same rhetoric about a given party-group dyad at any given time, our standard errors need to account for this nested structure. To do so, we cluster standard errors at the dyad-wave level.

Results

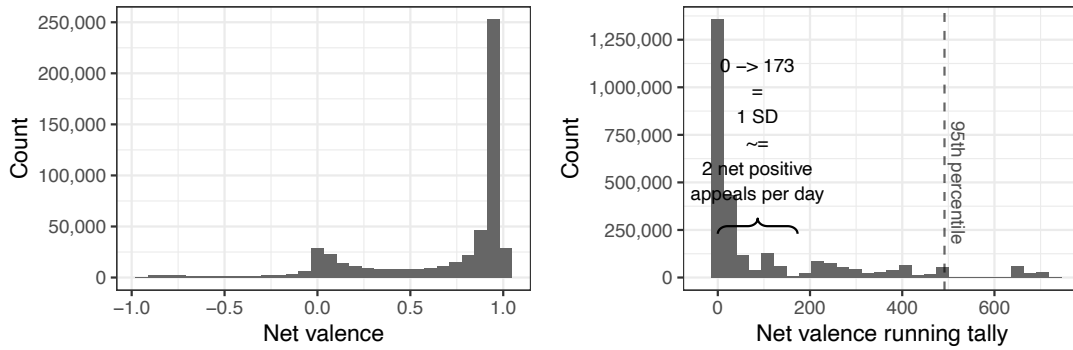
We now turn to results. Before presenting model estimates, we characterize descriptively how group appeals vary over time and across parties and groups in our data.

Descriptive results

We first consider overall variation in our independent variable. Figure 7.2 shows distributions of all net valences of group appeals (panel a) and all 90-day running tallies (panel b).

As shown in panel (a) of Figure 7.2, most group appeals are unambiguously positive, with the classifier confidently predicting positive valence. Still, there is considerable variation: 26 pct. of appeals have net valence under 0.5, and 8 pct. are negatively valenced, underscoring the importance of accounting for valence when analyzing group appeals. Panel (b) shows the distribution of 90-day running tallies at the dyad level, i.e., our main independent variable. As shown, the distribution is right-skewed with a large share of tallies close to 0, reflecting both the dominance of positive group appeals and the fact that most 3-month periods contain few if any valenced appeals. To illustrate a meaningful change in the running tally, panel (b) visualizes a change of

Figure 7.2. Distributions of net valences of all group appeals (panel a) and all 90-day running tallies (panel b). Across running tallies, a 1 standard deviation increase corresponds to a net valence increase of 173, which is equivalent to adding roughly two additional net positive appeals per day during the 90-day window. The dashed line shows the 95th percentile of the distribution.



(a) Distribution of all net valences.

(b) Distribution of all running tallies.

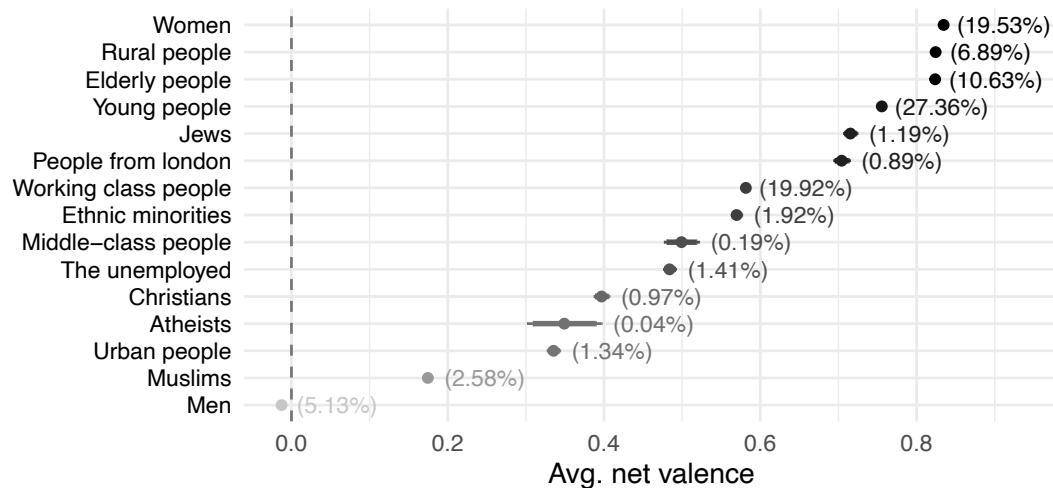
one standard deviation, i.e., moving from 0 to 173, which is equivalent to roughly two additional net positive appeals per day during the exposure window.

The skewed distribution in panel (b) also reflects the fact that the average net valence varies considerably between groups. We show this by plotting group-specific distributions in Appendix 8.22. Some groups (e.g., women) are targets of mostly positive appeals, others (e.g., young people) a mix of neutral and positive appeals. Still, the overall skew gives rise to concerns that results could be driven by right-tail outliers. We check for this by estimating a model excluding observations above the 95th percentile, shown by the dashed line.

We now consider how valence in group appeals varies across target groups. Figure 7.3 shows the average net valence across all appeals by target group as well as the overall share of appeals to the group (in parentheses).

As seen in Figure 7.3, valence varies considerably across groups. While some are mentioned nearly universally positively (women, the elderly), others have much lower average valence, implying they are often targets of negative appeals (men, Muslims). On balance, averages are positive, with men the only group where negative valence slightly outnumbers positive valence. The highest averages shown in Figure 7.3 are in fact close to the maximum possible average, indicating that these groups are referenced nearly unanimously positively.

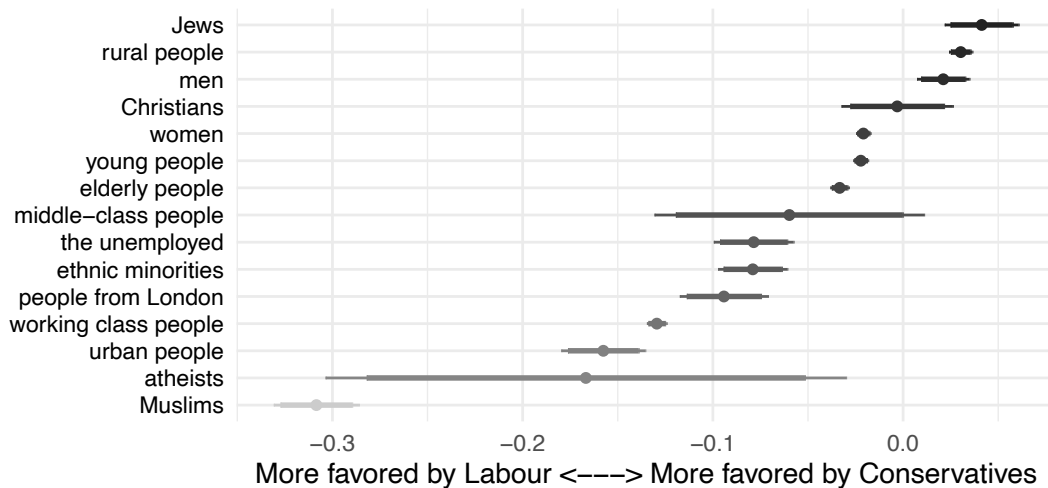
Figure 7.3. Average net valence by target group from 1997-2019. Color shading reflects average value. Parentheses show the total share of all appeals made to the group. Each point is an estimate from a no-intercept regression of net valence on target group. Error bars show 90 and 95 pct. confidence intervals.



As a last descriptive result, we consider partisan differences in group appeals. To simplify exposition, we focus on differences between Labour and Conservative MPs. Since these parties are the pillars of the traditional UK two-party system, theoretical expectations of to which groups they should be more likely to appeal are more well-defined. Moreover, due to their size, Labour and Conservatives account for the vast majority of the data: 88 pct. of group appeals in our data are by MPs from either of these parties. Figure 7.4 shows the average difference in valence between Conservatives and Labour for each group.

Figure 7.4 reveals a very recognizable set of party-group linkages. Conservative MPs appeal relatively more positively to men and Christians, both generally right-aligned groups in contemporary UK politics. The largest differential is for Jews, a group which, while historically aligned with Labour, has recently leaned Conservative (Barclay, Sobolewska and Ford, 2019). Conversely, Labour MPs appeal relatively more positively to Muslims, atheists, and working-class people, all groups solidly embedded in the Labour electoral coalition. In other words, two-party differences in group appeals reflect each party's voter base. In Appendix F, we show how these two-party gaps vary over time for the most frequently mentioned groups.

Figure 7.4. Conservative - Labour gaps in net valence by group from 1997-2019. Each point is an estimate from a regression of net valence on an indicator of Conservative party affiliation among all appeals to the group in question. Error bars show 90 and 95 pct. confidence intervals.



Overall, the descriptive variation in group valence, both overall and between parties, lends a high degree of face validity to our measure of group appeals. We now turn to estimates from our regression models linking group appeals to group linkages expressed in surveys.

Regression estimates

Table 7.1 presents estimates from various specifications of the model outlined in equation 7.1 above.

Models 1-4 in Table 7.1 differ in terms of the number and composition of fixed effects. Model 4 is identical to Model 3 but excludes the 5% most extreme cases with respect to net valence. The estimate of interest, *Net valence (sum)*, is in the top row. As shown, the coefficient on the net valence sum is consistently positive and statistically significant. The coefficient is fairly robust in terms of magnitude, ranging between .06 and .13 across specifications. As theorized, this is driven by the valence of appeals and not their quantity, as emphasized by the control for the number of group mentions in the models. Further, the coefficient on the number of mentions is negative as expected, indicating that voters' perceptions are influenced more strongly by a given net valence

Table 7.1: Estimates from regressing group linkages on group appeals.

	Model 1	Model 2	Model 3	Model 4
Net valence (sum)	0.128*** (0.029)	0.059** (0.021)	0.071* (0.028)	0.087* (0.036)
Number of appeals	−0.093*** (0.019)	−0.044** (0.014)	−0.053* (0.020)	−0.060** (0.023)
N	1 325 239	1 325 239	1 325 239	1 285 515
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓
FE: Wave		✓	✓	✓
FE: Party			✓	✓
Restricted range				✓

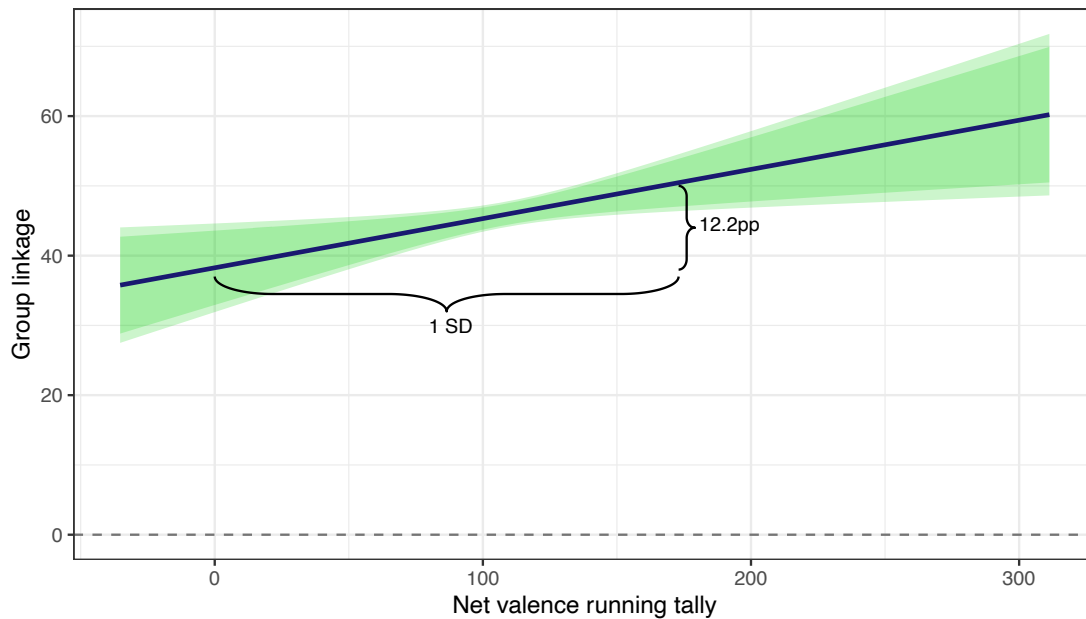
sum when it is spread out over few (strongly valenced) appeals rather than many (weakly valenced) appeals. In Appendix 8.24 we present ‘naïve’ models that regress group linkages just on counts of group mentions estimate a precise and consistent null. This appendix also reports results from a simple ‘pooled’ specification without any fixed effects, which results in a positive but small and noisy estimate. This is not too surprising given the vast heterogeneity in valence between dyads shown earlier. As we theorize running tallies to be group- and party-specific, we would not necessarily expect a positive overall correlation between dyad valence and group linkage. Finally, Appendix 8.24 also reports results from an ordered logit model treating the outcome variable as a four-point ordinal scale.

The estimates shown in Table 7.1 treat all the survey responses as cross-sectional and do not make use of the fact that a subset of the survey data is panel data. In Appendix 8.25 we present a set of analyses that apply individual-level fixed effects to isolate within-respondent variation. In these models, coefficients are if anything larger and remain statistically significant.

It is worth noting that the estimates shown in Table 7.1 are based on windows of exposure to group appeals of 3 months. However, the results are not sensitive to this specific window length. As we show in Appendix 8.26, net valence sums tallied across varying window sizes produce similar point estimates (although precision is predictably weakened as the window is shortened).

To make sense of the magnitude of the coefficients, Figure 7.5 visualizes predicted

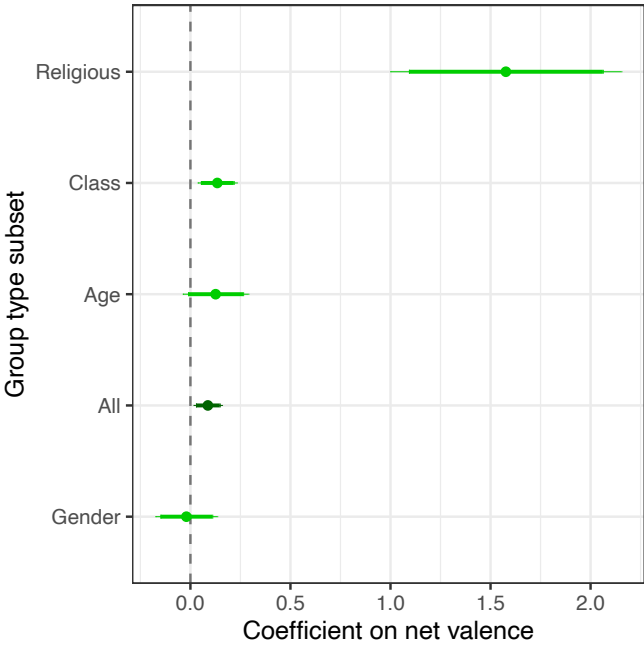
Figure 7.5. Predicted group linkage values in response to different net valence sums. Based on Model 3 in Table 7.1. The plot shows predicted values ranging from the minimum of the net valence running tally to 2 standard deviations above the minimum. A unit increase in the independent variable represents a +1 increase in the net valence of one mention during 3 months (holding the number of mentions constant).



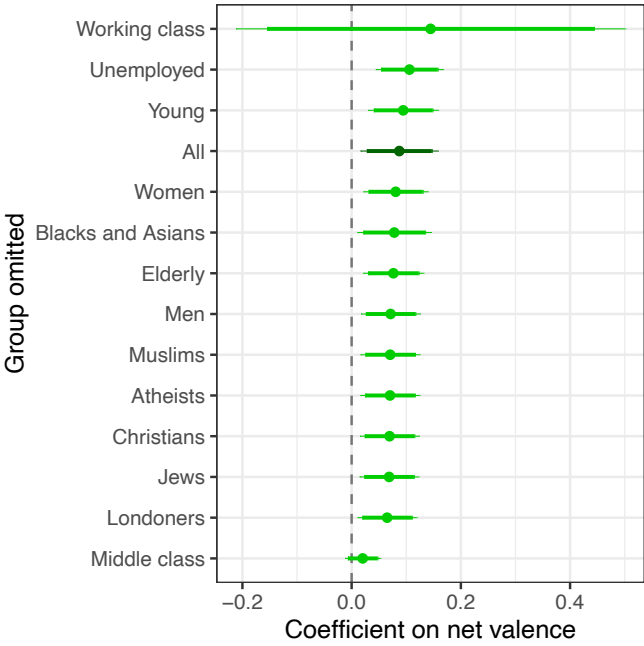
levels of group linkage across the observed range of net valence sums. We base our calculations on the coefficient in Model 3 of Table 7.1, the most restrictive specification on the full sample.

The bracket in Figure 7.5 shows the predicted level of group linkage for a difference in the running tally of one standard deviation, corresponding to an increase in the net valence sum from 0 to 173. In substantive terms, this can be understood as a party's MPs switching roughly two group mentions from neutral to positive, or negative to neutral, every day over the 90-day period. We estimate that such a difference is associated with the group linkage for the relevant party-group dyad improving by 12.2 percentage points, a substantial increase. In sum, our estimates indicate that group linkages reflect short-run exposure to group appeals from party elites.

Figure 7.6. Estimates when subsetting to each group type (left panel) and omitting each group (right panel). All estimates are based on the specification from Model 3 in Table 7.1. Light grey dots are group (type)-specific estimates, the dark grey ‘All’ estimate is the overall estimate presented in Table 7.1. Groups and group types are ordered by coefficient. Thin and thick error bars represent 95 and 90 pct. confidence intervals, respectively.



(a) Estimates subsetting by group type.



(b) Estimates omitting each group.

Heterogeneous effects: group types, policy content, and news consumption

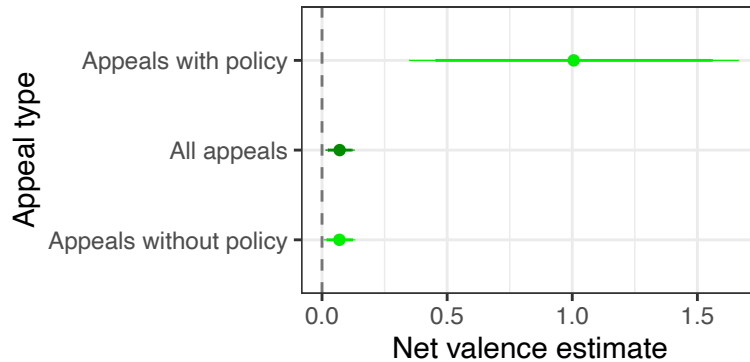
We next present a set of auxiliary analyses to probe effect heterogeneity and assess the robustness of the main result. Specifically, we examine how the main effect varies across (i) the social groups involved, (ii) whether group appeals reference policy, and (iii) individual-level news consumption.

We first consider heterogeneity across the types of groups to which party elites can appeal. This connects to concerns about external validity. For example, class has famously played a particular role in British politics and has been the focus of key studies on the topic (Thau, 2021, 2023; Stubager and Thau, 2023; Robison et al., 2021) (although, as Evans and Tilley (2017) argue, this role has diminished in recent decades). This naturally raises the concern that the overall result could be driven by British voters' reactions to, e.g., class appeals specifically. Moreover, our overall estimate is inevitably mostly driven by the groups that account for most observed group linkages (working class, middle class, Black & Asian, and unemployed).

To examine this, we consider heterogeneity across group types. Since sample coverage differs widely by group, we cannot carry out well-powered analyses for each single group. We therefore bundle the groups together according to their type as shown in Table 8.21, resulting in four group types: religious, class, age, and gender. To preserve power, we consider only groups which can be subsumed under an overarching group type. We then test for heterogeneity in two ways: by re-estimating the model for each group type separately, and a 'jackknife' type test re-estimating the model with each group omitted. Results are shown in Figure 7.6. All estimates rely on the most restrictive model specification, i.e. Model 3 in Table 7.1.

As shown in panel (a) of Figure 7.6 the main heterogeneity at the group type level is that the coefficient for religious groups is noticeably larger. The other group type-specific estimates vary around the overall estimate. Estimates for class- and age-based groups are larger than the overall estimate, and the estimate for gender is smaller and indistinguishable from zero. Panel (b) reveals that coefficient magnitudes are robust to omitting each group from the data. Only when omitting class-based groups does the estimate not reach statistical significance, but this reflects the loss of power from dropping by far the most common group type in the data.

Figure 7.7. Estimates of the coefficient on net valence by whether the appeal includes an explicit reference to policy. Specifications are identical to Model 4 in Table 7.1. Light grey dots are estimates with and without policy references, while the dark grey ‘All’ estimate is the overall estimate presented in Table 7.1. Thin and thick error bars represent 95 and 90 pct. confidence intervals, respectively.



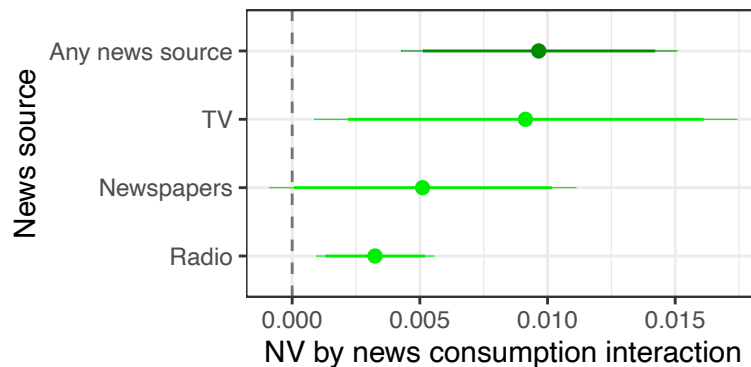
We next examine how the effect varies between group appeals that include or do not include references to policy. Group appeals are sometimes used in conjunction with policy statements, as when a party criticizes a policy for harming a particular group, and existing research has debated whether such ‘substantive’ group appeals have stronger effects on group linkages than purely ‘symbolic’ ones (Thau, 2021, 2019; Horn et al., 2021; Huber, 2022) with, e.g., Thau (2021) arguing that “the most lucrative strategy probably lies in combining the two electoral appeals” (p. 686). Yet, this has mostly been the subject of theoretical debate (though see Robison et al., 2021).

To test this, we code all sentences in our data for explicit references to policy using a custom dictionary (see Appendix 8.29 for details.) We then re-estimate our preferred specification (Model 3 in Table 7.1) in two ways: one counting only group appeals with explicit references to policy, and one counting only appeals without references to policy. Figure 7.7 presents the results.

As shown in Figure 7.7, appeals with and without policy both yield significant coefficients, consistent with the main result. This suggests that the main result is not solely driven by references to policy which happen to coincide with group appeals. At the same time, Figure 7.7 indicates that group appeals accompanied by policy mentions are roughly an order of magnitude more effective compared to those without. This lends support to contentions in earlier work (e.g., Thau, 2021) that while symbolic appeals are effective, linking them to policy can greatly enhance their effects on voters.

At the same time, policy does not seem to drive the effect on its own. In Appendix 8.30 we show that our main result is also robust to an alternative strategy for accounting for the role of policy, where we include a party-level measure of time-varying policy towards a set of groups as a statistical control. In sum, group appeals seem to matter over and above policy, although appeals explicitly tied to policy appear more effective.

Figure 7.8. Moderation by recent news consumption. Estimates of the coefficient on net valence by whether the respondent recently consumed news from various sources. Specifications are based on the most restrictive panel model with individual-fixed effects, Model 4 in Table 8.30. Light grey dots are interaction terms from interacting each news source indicator with the net valence measure, and the dark grey dot is the interaction term from a variable indicating consumption of any of the three news sources. Thin and thick error bars represent 95 and 90 pct. confidence intervals, respectively.



Finally, we turn to heterogeneous effects between voters more or less likely to have been exposed to elite rhetoric. To the extent that group appeals in parliamentary speeches reach voters directly, they do so through media coverage. We would therefore expect the relationship to be strongest for voters at times where they consume news media. Leveraging our panel dataset, we conduct a within-respondent analysis to examine this moderation effect, interacting net valence sum with measures of reported recent exposure to media coverage (see Appendix 8.31 for details). Figure 7.8 shows the results. As expected, the relationship is significantly stronger in periods where respondents consume news compared to periods where they do not. This bolsters confidence in our mechanism, suggesting that individual voters are more sensitive to group appeals when they are more likely to be exposed to them.

Conclusion and Discussion

Shared perceptions of what parties stand for and who they represent play a key structuring role in politics. A central component of a party's reputation is the perception of *whom* the party represents—i.e., its set of group linkages—which influences how voters reason about politics (Miller, Wlezien and Hildreth, 1991; Dalton, 2018; Elder and O'Brian, 2022; Mason, Wronski and Kane, 2021). Scholars offer divergent accounts on how group linkages change, emphasizing either gradual changes in the social structure (Hooghe and Marks, 2018; Bornschier et al., 2021) or party positioning at the level of electoral cycles (Evans and Tilley, 2012, 2017). However, neither account leaves room for short-run changes in response to elite rhetoric.

We present new theory and evidence on how group linkages change in response to elite rhetoric. Specifically, we argue that citizens keep 'running tallies' of group linkages based on recent political rhetoric. By implication, group linkages respond to group appeals in elite rhetoric. To test this expectation, we use a language model to produce high-quality annotations of group appeals in 1.6 million parliamentary speeches in the UK House of Commons. We then examine how group linkages expressed in surveys track party elites' group appeals. Consistent with our expectation, we find that group linkages robustly track parties' use of group appeals. By our estimates, a one standard deviation change in group appeal valence improves a perceived group-party linkage by around 12.2 percentage points. These results challenge the conventional view of group linkages as sticky, suggesting instead that party elites have some latitude to change these in the short run.

Some limitations to our analysis remain. First, we exploit a rare data opportunity to link mass group linkages and elite group appeals in present-day UK. This naturally raises the question of generalizability to other contexts. We show that our findings are fairly robust across group types, and thus not likely driven by a uniquely British attention to class. Still, we study an era of relative volatility in the UK party system, which may also make group linkages relatively less fixed. While this relative volatility is mirrored in European politics writ large (De Vries and Hobolt, 2020), it is an open question how our findings generalize to more static party systems.

Second, our observational approach inevitably leaves residual concerns about causal inference. While we are able to include a rich set of controls, including fixed effects

for groups, parties, and survey waves, we cannot rule out all confounders. Moreover, because our data is observational, we observe group appeals ‘along the equilibrium path’, i.e. the appeals party elites choose to make, perhaps because they are perceived as more likely to work. Future experimental work could examine the efficacy of group appeals that are rarely seen in the wild.

Lastly, we stress that our measure of group appeals is designed to capture explicit appeals. This excludes implicit or ‘dog whistle’ appeals that are richly theorized (e.g., Tesler, 2017), but in practice too subtle for our classifier to pick up. However, group appeals are likely to also operate at the implicit level. For example, while we find striking average negativity in explicit appeals to men, it is possible that politicians mask positive appeals to men through gendered ‘group implicating’ phrases (Winter, 2008) like ‘workers’, ‘troops’, or ‘motorists’. We consider the implicit level of group appeals an important avenue for future research.

These caveats notwithstanding, our results have several important implications. First and foremost, they suggest that a cornerstone assumption of the group appeals literature is warranted. While rarely studied, and never in non-experimental settings, the link we have demonstrated is critical for claims that parties can use group appeals strategically to shape perceptions of group-party links and voter behavior (Huber, 2022; Huber and Haselmayer, 2024; Thau, 2019). We also provide rare evidence on the relative effectiveness of group appeals with and without mentions of policy. Importantly, group appeals work even in the absence of explicit references to policy, but their effectiveness is greatly enhanced when tied to policy. Jointly, these results underscore the importance of studying how parties use group appeals for understanding electoral outcomes.

Secondly, our results suggest that party reputations are more malleable than is often assumed. If, for instance, the Labour Party were to suddenly stop talking about issues facing Londoners, voters should register this and immediately update their perceptions of Labour’s linkages with urban people. This stands in contrast to other major approaches to group linkages and group-based voting that emphasize either bottom-up “epochal structural shifts” (Westheuser and Zollinger, 2024, p. 4) in the social structure (Bornschieer et al., 2021; Lipset and Rokkan, 1967) or larger top-down shifts in party strategy (Evans and Tilley, 2012, 2017; Evans and Graaf, 2013; Evans, 2013). The effectiveness of group appeals may seem surprising given inherent issues with the credibility of political rhetoric and ‘cheap talk’ (e.g. Fiorina, 1981). Yet, it aligns with

Chapter 7

other work demonstrating political elites' power to activate social cleavages (Robison et al., 2021; Klar, 2013). Moreover, there are good reasons why group appeals would leave such strong impressions on voters, specifically their typical emotional or moral charge and ease of interpretation.

What does the malleability of group linkages mean for party competition? Most obviously, it suggests that the use of group appeals is an important element of party strategy and competition. Group linkages may also be increasingly malleable as voters become more volatile, as is the case in the UK (Fieldhouse et al., 2020) and Europe more broadly (Dassonneville, 2023). The flipside of this malleability, however, is that group linkages are not something parties can take for granted (Mair, 2013). Connections between parties and groups must be maintained in party rhetoric to remain relevant and salient in voters' minds.

Chapter 8

Is Pocketbook Voting Sensitive to Policy?

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.

Chapter 8

*“... 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 (Tilley, Neundorf and Hobolt, 2018; Healy, Persson and Snowberg, 2017; Bechtel and Liesch, 2020). 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; Powell and Whitten, 1993; Duch and Stevenson, 2008).

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 subjectively 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 (Larsen, 2021; Feldman, 1982). 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 responding 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; Tufte, 1978; Maskin and Tirole, 2019). 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 (Tilley, Neundorff and Hobolt, 2018; Healy, Persson and Snowberg, 2017; Bechtel and Liesch, 2020). 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, espe-

cially 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 simplicity², we can model i 's binary incumbent vote choice as a function of these components as follows:

$$\text{incumbent vote}_{it} = \beta_p \Delta y_{it}^{\text{policy}} + \beta_r \Delta y_{it}^{\text{residual}} \quad (8.1)$$

where the sum of $\Delta y_{it}^{\text{policy}}$ and $\Delta y_{it}^{\text{residual}}$ 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

¹To 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.

²One can think of these as an added term, α_{it} , in Equation 8.1 representing the sum weight of alternative considerations.

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 heuristics to judge when policy is responsible for pocketbook changes and when it is not (Tilley, Neundorf and Hobolt, 2018; Fowler and Hall, 2018; Fowler and Montagnes, 2015, 2023; Ashworth, de Mesquita and Friedenberg, 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 (Mutz, 1994; Ciobanu, 2024) 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

³Equivalently, 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.

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; Gomez and Wilson, 2001; Healy, Malhotra and Mo, 2010; Healy and Malhotra, 2010; Wolfers, 2002; Feldman, 1984) and many people do not routinely scrutinize their payslips nor possess the required financial literacy to fully understand them (Peters, 1991; Abeler and Jäger, 2015; Bhargava and Manoli, 2015). As Tilley, Neundorff 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 (Kim and Gulotty, 2024; Stutzmann, 2025; Jares and Malhotra, 2025; Gingrich, 2019). 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

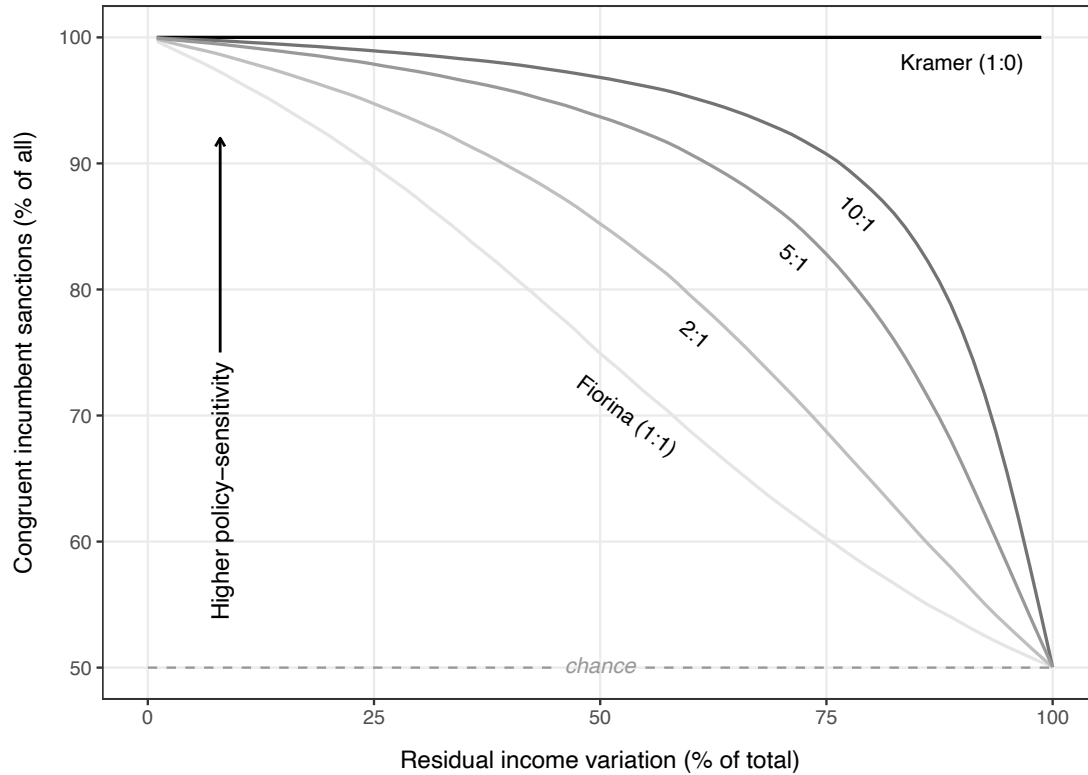
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that without attribution, there is essentially no policy-sensitivity (Achen and Bartels, 2016; Wolfers, 2002; Kramer, 1983; Sances, 2017) while others disagree (Fiorina, 1981; Ferejohn, 1986; Key, 1966). This disagreement can mostly be reduced to a 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 8.1. 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 8.1, 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 8.32 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 8.1.

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.

Figure 8.1. 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 8.32 for technical details on the simulations.



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 (Lau and Sears, 1981; Tilley, Neundorf and Hobolt, 2018; Abramowitz, Lanoue and Ramesh, 1988; Feldman, 1982; Marsh and Tilley, 2010; 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 (Conover, 1984; Feldman, 1984; Brody and Sniderman, 1977). 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 (Zucco Jr., 2013; Pop-Eleches, 2012; Manacorda, Miguel and Vigorito, 2011; Kogan, 2021; Rendleman and Yoder, 2024), especially when policies are highly visible, salient, publicized and traceable to government (Shanks-Booth and Mettler, 2019; Mettler, 2011; Hamel, 2024; 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; Wolfers, 2002; Healy, Malhotra and Mo, 2010; Healy and Malhotra, 2010; 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 (Powell and Whitten, 1993; Duch and Stevenson, 2008; Hobolt, Tilley and Banducci, 2013). 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 8.1 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 (Elkjær and Mushövel, 2023; Avram and Popova, 2022).

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 (Richiardi, Collado and Popova, 2021; Bargain and Callan, 2010). 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 pocket-book 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 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

⁴The 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.

⁵UKMOD 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.

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., Zucco Jr., 2013; Pop-Eleches, 2012; Mettler, 2011; Sances, 2017; Rendleman and Yoder, 2024). 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 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, assumptions, and results (Richiardi, Collado and Popova, 2021; Bronka, Popova and Richiardi, 2023).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

⁶UKMOD 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.

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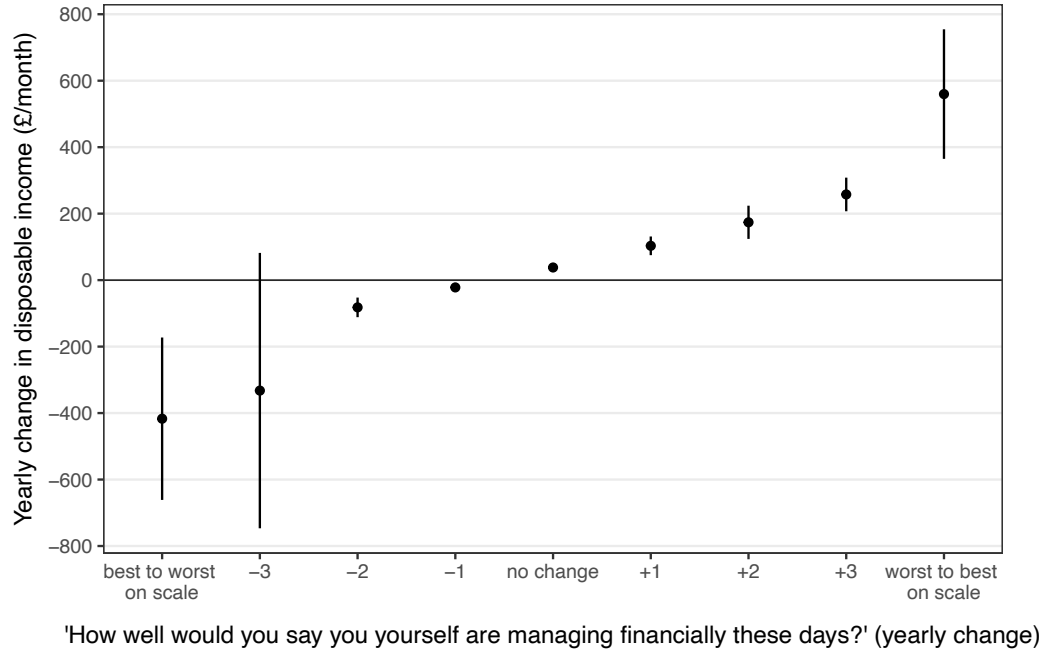
pensions. It excludes contributory pensions and disability benefits, which require administrative records or medical history not available in the survey. On the tax side, UKMOD fully simulates personal income taxes and social insurance contributions but excludes capital and wealth taxes, which cannot 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 8.2 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

⁷A 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 8.2. Average change in total disposable income by change in subjective financial evaluations across respondent-year observations.



change in income of 68 £/month on average ($SE = 6.07$, $p < 0.001$). 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:

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

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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 as⁹:

$$\Delta y_{i,0 \rightarrow 1}^{total} = \underbrace{\{s_1(c_{i,1}) - s_0(c_{i,1})\}}_{\Delta y_{i,0 \rightarrow 1}^{policy}} + \underbrace{\{s_0(c_{i,1}) - s_0(c_{i,0})\}}_{\Delta y_{i,0 \rightarrow 1}^{residual}} \quad (8.2)$$

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

⁸Mathematically, 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.

⁹This is a slight simplification of the decomposition in Bargain and Callan (2010) which includes uprating parameters and an inflation component 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 adjusts market incomes to the policy system year.

the year of the policy system applied.¹⁰ Uprating factors are likewise applied to adjust $s_1(c_{i,1})$ to nominal levels of period 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 8.33 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 8.34). In addition, yearly income changes correspond closely to the time-frame on which pocketbook voting is usually

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

¹¹This 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.

¹²An 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.

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, Neundorff 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 Foucault, 2013; Lewis-Beck, Nadeau and Elias, 2008). 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 (Powell and Whitten, 1993; Duch and

Stevenson, 2008).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 Jobseekers 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 from 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, Neundorff and Hobolt (2018), the

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

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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, Neundorff 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 8.36 for model specifications). This approach is equivalent to including 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, Neundorff 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 8.38). 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 including two-way fixed effects models and models that control for lagged policy-induced and residual income changes to account for temporal dependence. See Appendix 8.36 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 8.38).

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 income changes

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 8.3 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

¹⁴UKHLS 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 8.3. 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.

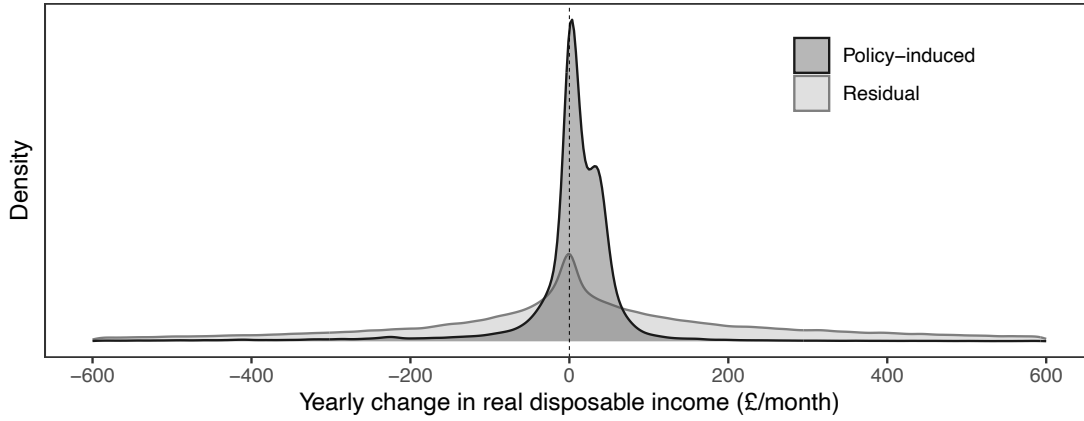


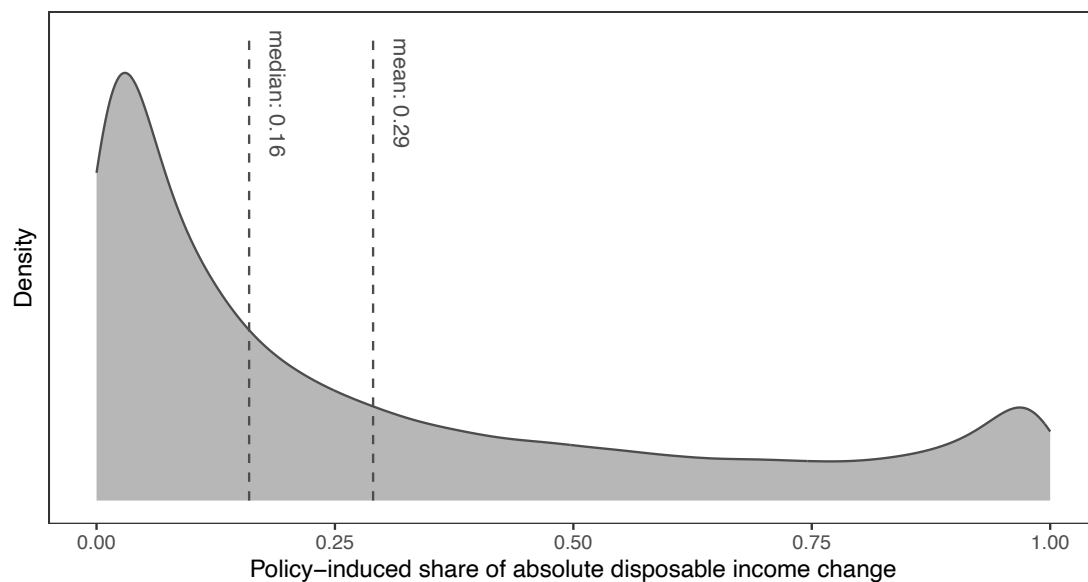
Figure 8.1 showing the importance of the residual for policy-sensitivity: the larger the residual relative to policy-induced income variation, the more correct attributions voters need to make congruent incumbent sanctions. Figure 8.4 shows the relative share of policy-induced income changes across respondent-year 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 8.1. 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 8.37). Given the ob-

¹⁵I 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.

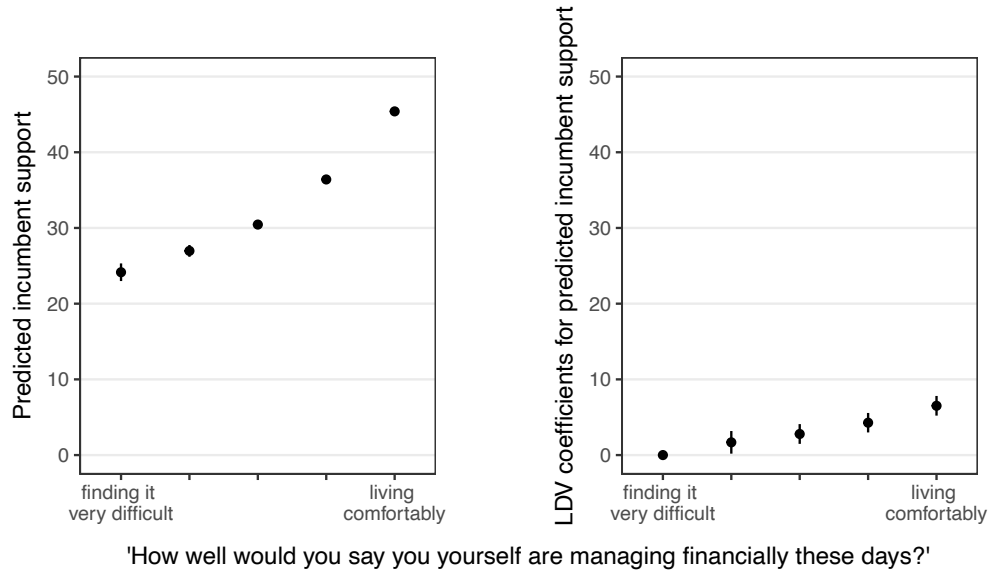
served income changes, classical outcome-based pocketbook voting would lead voters to incongruently punish the incumbent 23% of the time and incongruently reward the incumbent 19% of the time. In total, voters' sanctions would be congruent in just 58% of all cases, performing just marginally better than chance.

Figure 8.4. 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 8.5. 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 voting

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 8.5 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 8.5. More rarely, existing studies estimate the relationship between actual income changes and incumbent support; I also show this alongside the main results in Table 8.1 and more extensively in Appendix 8.35. 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 incumbent support on decomposed disposable income

Table 8.1: 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	70 798	70 798	70 798	70 798
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. * $p < .05$, ** $p < .01$, *** $p < .001$.

changes using lagged dependent variable models.

Table 8.1 shows the results. As shown, there is little sign of policy attribution in pocketbook voting. The coefficient on total income changes is 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 model.

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 models¹⁶. 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 eligible for very small benefits, and using individual-level rather than household-level income changes (see Appendix 8.38). I further find no significant differences in these relationships by the party in power (see Appendix 8.39). 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.

¹⁶The 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.

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 8.38). 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 8.2 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* 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

Table 8.2: 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. * $p < .05$, ** $p < .01$, *** $p < .001$.

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 8.3 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.

Table 8.3: 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.

To further probe the main result, Appendix 8.40 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 appearing 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

¹⁷Note 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).

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 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 (Key, 1966; Fiorina, 1978; 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 overshadowed 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 (Gaikwad, Genovese and Tingley, 2022; Fetzner, 2019). 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 (Lau and Sears, 1981; Tilley, Neundorff and Hobolt, 2018; Abramowitz, Lanoue and Ramesh, 1988; Feldman, 1982; Marsh and Tilley, 2010). Likewise, research on policy feedback has in some cases found voters to respond to economic policies that affect them (Zucco Jr., 2013; Pop-Eleches, 2012; Manacorda, Miguel and Vigorito, 2011; Kogan, 2021; Rendleman and Yoder, 2024). 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 (Elkjær and Mushövel, 2023; Avram and Popova, 2022), these models offer a uniquely detailed account of how policy changes shape voters' financial situations while covering whole-

sale 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 tangible 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; Tufte, 1978; Maskin and Tirole, 2019). 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 (Maskin and Tirole, 2019; Battaglini and Coate, 2007, 2008). While such tactical redistribution or 'pork-barrel' politics is often thought to undermine accountability (Ferejohn, 1986; Tufte, 1978; Lizzeri and Persico, 2005) 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 (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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Danish Summary

I kernen af den liberale demokratiske teori er forestillingen om, at regeringer henter deres legitimitet fra evnen til at repræsentere borgernes individuelle interesser. Demokratiske valg fungerer som mekanismen bag dette løfte om repræsentativitet: gennem stemmeafgivning udtrykker borgere deres præferencer og sikrer, at disse vægtes ligeligt i regeringsdannelsen. Alligevel rummer denne vision en grundlæggende spænding. Selvom demokratiske regeringer formodes at repræsentere individuelle interesser, vedrører deres politikudformning ikke individer som sådan. Offentlige politikker påvirker per definition altid aggregater af individer, som deler politisk relevante træk, hvilket skaber en kløft mellem vælgernes personlige interesser og regeringens beslutninger på aggregeret niveau. For at individuelle interesser kan repræsenteres, må vælgere på en eller anden måde oversætte deres personlige interesser til politiske valg: de må løse det, man kan kalde "oversættelsesproblemet" mellem det personlige og det politiske. Denne afhandling undersøger, hvordan vælgere navigerer i denne fundamentale udfordring.

Afhandlingens hovedargument er, at sociale grupper fungerer som en afgørende grænseflade mellem individuelle vælgere og det politiske system og hjælper vælgere med at løse oversættelsesproblemet mere effektivt, end hvis de udelukkende skulle forlade sig på personlige erfaringer eller nationale forhold. Det centrale forskningsspørgsmål lyder: Hvordan bruger vælgere sociale grupper til at evaluere politikker og partier? Afhandlingen argumenterer for, at både politikudformning og vælgernes politiske interesser opererer på gruppeniveau, hvilket gør sociale grupper til det naturlige punkt, hvor borgere og regeringer mødes. Når politikere udformer politik, opererer de nødvendigvis på gruppeniveau snarere end individniveau og målretter fælles karakteristika som alder, indkomst eller geografi. På samme måde samler individuelle interesser sig naturligt i det, vi kender som sociale grupper, da mennesker med fælles sociodemografiske karakteristika ofte har overlappende interesser på tværs af flere dimensioner. Denne klynge dannelse af både politikudformning og interesser skaber betingelser, hvor vælgere kan lære mest om regeringens overensstemmelse med deres interesser ved at bruge information om deres sociale grupper. Afhandlingens primære

bidrag er empirisk at demonstrere, at gruppebaseret politisk tænkning repræsenterer en rationel og udbredt reaktion på de informationsmæssige udfordringer, vælgere står over for, hvilket udfordrer fremherskende forklaringer, der karakteriserer gruppebaseret adfærd som noget rent tribalt eller følelsesmæssigt.

Afhandlingen er artikelbaseret og består af en projektramme (kapitel 1–4) og fire empiriske forskningsartikler (kapitel 5–8), hvoraf tre er solo-forfattede og én er medfattet. Forskningsartiklerne er selvstændige og kan læses uden at læse projektrammen. Ligeledes opsummerer projektrammen hele projektet ved at uddybe det overordnede argument og hvordan hver artikel passer ind i det, diskutere eksisterende litteratur om gruppebaseret politisk adfærd, udvikle en formel teoretisering af gruppe-grænsefladen i politik, diskutere tværgående metodiske udfordringer og opsummere centrale fund og deres implikationer for forskning og demokratiets funktionsmåde.

Den første empiriske artikel, “You and Whose Economy?: Group-Based Retrospection in Economic Voting”, undersøger, om vælgere evaluerer den siddende regerings præstation baseret på deres sociale indgruppers økonomiske vilkår. Ved hjælp af paneldata fra British Election Study og tre præregistrerede eksperimenter i Danmark og USA finder studiet, at vælgere systematisk vurderer den siddende regerings præstation baseret på, hvordan deres sociale grupper har klarer sig økonomisk, især relativt til den nationale økonomi. Denne gruppebaserede retrospektive stemmeadfærd fungerer uafhængigt af både personlige økonomiske forhold og nationale økonomiske vilkår, hvilket antyder vigtige begrænsninger i rent sociotropiske modeller for økonomisk stemmeadfærd. Effekterne er sammenlignelige i størrelse med traditionel sociotropisk stemmeadfærd, hvilket indikerer, at gruppebaserede overvejelser repræsenterer en fundamental dimension af vælgeransvar.

Den anden artikel, “Who (Else) Benefits?: Group-Based Responses to Distributive Policies”, udfordrer den konventionelle antagelse om, at vælgere primært reagerer på målrettede regeringspolitikker gennem lommebogsovervejelser. Ved at undersøge COVID-stimuluspolitikker i Danmark og USA i surveys og tre præregistrerede eksperimenter, demonstrerer studiet, at vælgernes reaktioner på materielle fordele formes mindst lige så meget af opfattet indgruppefordel som personlige økonomiske gevinster. Effekterne varierer betydeligt med gruppeidentitetsstyrke, hvor positive reaktioner koncentrerer blandt grupper med stærke politiske identiteter. Disse fund hjælper med at forklare den blandede empiriske evidens om fordelingspolitiske effekter og antyder,

at det valgmæssige udbytte af målrettede udgifter afhænger af, hvilke grupper der drager fordel, og hvor klart denne målretning opfattes.

Den tredje artikel, "Elite Rhetoric and the Running Tally of Party-Group Linkages", medforfattet med Frederik Hjorth, undersøger, hvordan borgere danner og opdaterer opfattelser af, hvilke politiske partier der repræsenterer bestemte sociale grupper. Ved hjælp af en ny automatiseret tilgang med sprogmodeller til at måle såkaldte retoriske gruppeappeller i 1,6 millioner parlamentstaler forbinder studiet partiretorik med survey-målinger af opfattede parti-gruppe-forbindelser over tre årtier i Storbritannien. Fundene demonstrerer, at gruppeforbindelser robust følger partieliters retorik, hvor en standardafvigelses stigning i positive gruppeappeller er forbundet med cirka 12 procent-points forbedringer i opfattede parti-gruppe-forbindelser. Studiet leverer evidens for, at vælgere opretholder 'løbende opgørelser' af parti-gruppe-forbindelser, som reagerer systematisk på elitekommunikation. Dette udfordrer opfattelser af gruppeforbindelser som faste afspejlinger af sociale skillelinjer og understøtter det bredere argument om, at gruppeidentiteter fungerer som den primære grænseflade, gennem hvilken vælgere fortolker partiadfærd og politisk repræsentation.

Den fjerde artikel, "Is Pocketbook Voting Sensitive to Policy?", adresserer en central antagelse bag teorien om at stemme med tegnebogen, 'pocketbook voting', som en mekanisme for at holde demokratiske regeringer til ansvar. Ved hjælp af en ny tilgang, der forbinder survey-paneldata med mikrosimulationsmodeller, dekomponerer studiet respondenters disponible indkomstændringer i politik-inducerede og residuale komponenter på tværs af et årti i Storbritannien. Analysen viser, at vælgere ikke formår at prioritere politik-inducerede indkomstændringer over andre indkomstændringer, når de evaluerer den siddende regering, men i stedet reagerer på totale indkomstændringer uden at skele til deres årsag. Desuden udgør politik-inducerede ændringer kun en lille del af den totale indkomstvariation, hvilket betyder, at 'pocketbook voting' kun stemmer overens med faktiske politikeffekter marginalt oftere end tilfældigt. Disse fund antyder betydelige begrænsninger i vælgernes evne til at holde regeringer ansvarlige gennem personlige økonomiske erfaringer, hvilket forstærker vigtigheden af gruppe-baserede heuristikker som alternative veje til at holde regeringer til ansvar for deres politik.

Appendices for Research Papers

The appendix can be found in the online dissertation PDF on:
<https://chdausgaard.github.io/uploads/phd-thesis.pdf>.



Appendix (Chapter 6)

8.1 Underlying panel regressions for each group type

Table 8.4 shows the estimated relationship between in-group performance and incumbent vote intention for geographical and class groups. Group performance (grp.-level) is the average of retrospective pocketbook evaluations of members of the subject's in-group (standardized). Group performance (ind.-level) is the subject's subjective retrospective evaluations of in-group conditions (standardized). The coefficients in this table are averaged together to produce the pooled coefficients in Table 5.2 in the main text, using the `rma` function from the `metafor` package in 'R'. As I am interested in a simple average, I use basic equal-effects models that weigh estimates from each regression equally.

Table 8.4: Relationship Between Group Performance and Incumbent Vote Intention, Unpooled Models by Group Type

	Region (grp.-level)	Class (grp.-level)	Region (ind.-level)	Local community (ind.-level)	Class (ind.-level)
Group performance	0.10 (0.03)***	0.32 (0.03)***	0.13 (0.04)***	0.11 (0.04)**	0.17 (0.06)**
Egotropic evaluations	0.08 (0.01)***	0.08 (0.01)***	0.10 (0.04)**	0.11 (0.04)**	0.13 (0.07)*
Sociotropic evaluations	0.32 (0.01)***	0.32 (0.01)***	0.12 (0.04)**	0.13 (0.04)**	0.16 (0.07)*
TWFE	Yes	Yes	Yes	Yes	Yes
Clustered SEs	Individual	Individual	Individual	Individual	Individual
Wave N	18	18	4	4	4
Total N	354304	354304	40023	39557	22352
Individual FEs	91125	91125	33331	33016	20406

Note: Estimates from two-way fixed effects models. * $p < .05$, ** $p < .01$, *** $p < .001$.

8.2 Additional observational analyses

Table 8.5 shows the estimated relationship between in-group performance and incumbent vote intention averaged across class and geographical groups. It differs from the main results in Table 2 only in controlling for two time-varying measures of ideology. Both models include a control for left-right self-placement on an 11-point scale. Model 1 further includes a control for views towards the government's austerity agenda during much of the period on a 5-point scale ("Do you think [cuts to public spending in general] has gone too far or not far enough?"). This latter variable is excluded from Model 2 as it is not measured in the same waves as the perceived in-group performance variable.

Table 8.5: Relationship Between Group Performance and Incumbent Vote Intention, With Ideology Controls

	Group-based retrospection (pooled estimates)	
	(1)	(2)
Group performance (grp.-level)	0.14 (0.00)***	
Group performance (ind.-level)		0.23 (0.01)***
Austerity too far	−0.30 (0.01)***	
Left-right placement	0.25 (0.01)***	0.25 (0.01)***
Egotropic evaluations	0.09 (0.01)***	0.11 (0.02)***
Sociotropic evaluations	0.30 (0.01)***	0.08 (0.03)**
TWFE	Yes	Yes
Clustered SEs	Yes	Yes
Wave N	8	4
Individual FEs	49294	17839
Total N	94858	19496

Note: Estimates from two-way fixed effects models. The dependent variable, incumbent vote intention, is on a 10-point scale. * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 8.6 shows the estimated relationship between in-group performance and incumbent vote intention for geographical and class groups with and without controls for other in-group perceptions. Group performance (grp.-level) is the average of retrospective pocketbook evaluations of members of the subject's in-group stated in the column header, while the specific group performance variables (like 'Region performance') are the other in-group performance variables included as controls. Models without controls are the same as in Table A.1, included for comparison. As shown, coefficients on class performance perceptions diminish upon the inclusion of regional performance perceptions but not vice versa. This is consistent with class performance perceptions being largely derived from observations in the local context, thus making them conditional on local economic changes.

Table 8.6: Relationship Between Group Performance and Incumbent Vote Intention, With Controls for Other Group Perceptions

	Region (grp.-level)		Local community (grp.-level)		Class (grp.-level)	
	No controls	With controls	No controls	With controls	No controls	With controls
Group performance (grp.-level)	0.13 (0.04)***	0.15 (0.08)+	0.11 (0.04)**	0.13 (0.07)+	0.17 (0.06)**	0.07 (0.08)
Region performance (grp.-level)						0.10 (0.09)
Local performance (grp.-level)						0.12 (0.08)
Class performance (grp.-level)		0.10 (0.07)		0.11 (0.07)		
Egotropic evaluations	0.10 (0.04)**	0.13 (0.07)+	0.11 (0.04)**	0.12 (0.07)+	0.13 (0.07)*	0.12 (0.07)+
Sociotropic evaluations	0.12 (0.04)**	0.12 (0.07)+	0.13 (0.04)**	0.14 (0.07)+	0.16 (0.07)*	0.11 (0.08)
TWFE	Yes	Yes	Yes	Yes	Yes	Yes
Clustered SEs	Individual	Individual	Individual	Individual	Individual	Individual
Wave N	4	4	4	4	4	4
Total N	40023	21312	39557	21086	22352	20551
Individual FEs	33325	19510	33010	19310	20400	18859

Note: Estimates from two-way fixed effects models. The dependent variable, incumbent vote intention, is on a 10-point scale. + $p < .10$, * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 8.7 shows the estimated relationship between incumbent vote intention and the monthly unemployment rate in the respondent's region. The data cover the 11 regions of the United Kingdom, excluding Northern Ireland. I use seasonally adjusted, monthly unemployment data published by the Office For National Statistics (2025), which follow the International Labour Organization's definition of unemployed

people as “without a job, want a job, have actively sought work in the last 4 weeks and are available to start work in the next 2 weeks, or, out of work, have found a job and are waiting to start it in the next 2 weeks.” Following the main model specification, these models include two-way fixed effects and cluster standard errors at the panelist-level, and additional specifications control for economic evaluations and lag the unemployment variable.

Table 8.7: Relationship Between Regional Unemployment and Incumbent Vote Intention

	Bivariate TWFE	W. controls	Lagged IV
Regional unemployment	−0.0480 (0.0095)***	−0.0445 (0.0098)***	
Regional unemployment (lag)			−0.0203 (0.0101)*
Egotropic evaluations		0.0854 (0.0057)***	0.0831 (0.0058)***
Sociotropic evaluations		0.3228 (0.0071)***	0.3118 (0.0072)***
TWFE	Yes	Yes	Yes
Clustered SEs	Yes	Yes	Yes
Wave N	21	18	17
Individual FEs	90706	87808	85300
Total N	435 616	350 527	336 457

Note: Estimates from two-way fixed effects models. The dependent variable, incumbent vote intention, is on a 10-point scale. + p<.10 * p<.05, ** p<.01, *** p<.001.

8.3 Pre-analysis plans and omitted analyses

Experiments 1 and 2 were pre-registered on OSF before data collection for the specific study began. Experiment 3 was included on an omnibus survey shortly before it was fielded, and was therefore not pre-registered in time. However, it is analyzed in the same way as the two first experiments, i.e. by comparing those getting in-group and out-group information, controlling for baseline controls and stimulus intensity, and including in-group fixed effects.

All analyses and tests in the paper were conducted and reported in accordance with the pre-analysis plans (PAPs). There are only two minor deviations from the PAPs. Firstly, pre-registered analysis procedures for Experiments 1 and 2 specified that respondents answering ‘don’t know’ to baseline in-group questions would be included in the control condition. With credit to two reviewers pointing this out, this was a mistake as it undermined randomization. While it makes no difference to results, I have chosen to exclude them from the analysis. Secondly, while Experiment 1 is analyzed as described in the PAP, its main hypothesis, namely the effect of *in-group relative performance* is not framed as the main effect in the PAP, which is agnostic between effect sizes of *relative* and *absolute* performance. This expectation was therefore not pre-registered for Experiment 1. However, in line with this expectation, Experiment 2 and 3 focus just on *in-group relative performance* as the effect of interest (and therefore only include treatment conditions with the national benchmark).

8.3.1 Pre-registered analyses not included in the paper

For simplicity, a few of the pre-registered analyses were omitted from the paper.

Experiment 1

The PAP registers both linear and ordered logit models, but models of the latter type were omitted.

The PAP registers some expectations regarding heterogeneous effects of identity strength, political interest, baseline retrospective economic evaluations, and specific groups. These analyses were left out of the paper.

The PAP describes two further outcome variables not used in the paper. One is

post-treatment vote intention: “If there were an election tomorrow, how likely would you be to vote to re-elect the Social Democratic government?” (0-10 scale). The other is preferred party with respect to economic policy: “Which party do you think has the best economic policies?” (pre-specified list of parties). These were included in earlier versions of the paper but omitted here.

Finally, the PAP describes an analysis that omits those who receive stimuli for Central Jutland, West Zealand, West Jutland and the middle class. These groups have the lowest-intensity treatments (describing changes of only 3-11%). The paper only estimates the effect for the full sample.

Experiment 2

The PAP describes one further outcome, namely the question: “The Social Democratic Party has been in power for almost four years. How much do you agree or disagree with the following statement:”When the Social Democrats make policy, they take into account how it will affect [IN-GROUP].” Importantly, there was an election six months prior to the survey, and a new government was announced three months before. This appeared to muddle responses with nearly half of respondents (48%) answering “don’t know” or “neither agree nor disagree”. This outcome was therefore omitted.

Experiment 2 has three randomized conditions in total: in-group information with the national benchmark, out-group information with the national benchmark, and a condition with only the national benchmark. The PAP describes two uses for this latter treatment condition: to test whether those getting *relative in-group decline* information compared to just the national benchmark 1) change their perceptions of in-group performance (an alternative manipulation check) and 2) change their pocketbook evaluations. These analyses are omitted (the manipulation check in Table 8.8 instead compares those getting in-group and out-group information, as in Experiment 1).

The PAP also describes subgroup effects for ‘marginal’ groups, i.e. rural people, young people, and elderly people. In addition, it describes some interactions with another experiment on the same survey. These were omitted for simplicity.

8.4 Manipulation checks

The manipulation check item in Experiment 1 asks: “How do you think the financial situation of [in-group] is today compared to 12 months ago?” (3 pt. scale). Experiment 2 more fittingly asks about *relative* in-group performance: “Do you think that [in-group] is currently doing better, worse, or the same as the rest of the population?” (10 pt. scale). There was no manipulation check for Experiment 3 as the number of survey items was too restricted.

Table 8.8 shows the results of manipulation checks for Experiment 1 and 2. Like the experiments, the coefficients compare those getting in-group vs out-group information treatments (‘treatment’). It also reports effects of different treatment intensities due to differences in stimuli (‘treatment intensity’). In Experiment 1, the treatment itself has no significant effect on perceptions of absolute in-group decline. This is perhaps not too surprising given that perceptions of the economy were already very pessimistic. Nonetheless, subjects who got a stronger in-group stimulus did get more pessimistic about their in-group’s economic situation. Moving from the weakest to the strongest intensity stimulus worsened subjects’ group perceptions by more than 10 percentage points.

In Experiment 2, treatment had a much clearer effect on perceptions of group relative performance. Stimulus intensity did not have a consistent negative effect, in part because Experiment 2 had consistently strong stimuli. Thus, results are consistent with both experimental treatments having moved relative perceptions to a significant extent and absolute performance perceptions to a lesser extent. Perceptions of absolute group performance were mostly moved with high intensity stimuli.

Table 8.8: Manipulation Checks

	Experiment 1		Experiment 2
	Relative decline treatment	Absolute decline treatment	Relative decline treatment
Treatment	−0.051 (0.032)	0.049 (0.033)	−0.235 (0.041) ^{***}
Treatment intensity	−0.005 (0.001) ^{***}	−0.003 (0.001) ^{***}	−0.006 (0.003) ⁺
N	1357	1362	1811

Note: The dependent variable is perceived economic situation of the group they got negative information about in the stimulus (3-point scale). The reference is the average perception for control subjects receiving no group-level information. + p<.10 * p<.05, ** p<.01, *** p<.001.

8.5 Overview of experimental stimuli

Table 8.9: Overview of Experimental Manipulations

	Experiment 1	Experiment 2	Experiment 3
Absolute group decline	The economy does not develop the same way for everyone. For example, in the latest survey from the Danish Election Project, the proportion of [group] who felt their economic situation was insecure had increased significantly ([X]%) compared to the previous survey.		
Relative group decline	[absolute group decline +] ... In contrast, a large majority (82%) of subjects felt that their economic situation had remained the same or improved.	Inflation is on its way down and according to new figures from The Danish Election Project, more than 7 out of 10 Danes now believe that their economic situation is stable or improving. The figures also show that the vast majority of [group] cannot see improvements in their personal finances ([X]%) and that many [group members] feel financially insecure.	
Relative group improvement			Most Americans have struggled economically during the past year. The country has seen rising prices, and increasing economic pessimism with consumer confidence hitting levels below those during the pandemic. However, this is not felt equally by all groups in society. According to the latest survey by the American National Election Studies, [group] for instance reported increasing economic optimism. The number of [group members] feeling worried about their economic situation had fallen by [X].

8.6 Pre-defined social groups for the experiments

The process of dividing each group category into groups was guided by two considerations: personal relevance and political relevance. This involved, firstly, that groups should be small enough to have sufficient commonalities, yet large enough to be politically and economically relevant. Secondly, they should be defined in ways that group

members were expected to resonate with. To achieve this, I focus on five relevant group types: social class, education level, place, age, and ethnicity (Easterbrook, Kuppens and Manstead, 2020; Surridge, 2007; Donnelly, 2021*a*; Klar, 2013; Jones, 2023; Shayo, 2020; Stubager, 2009). Besides satisfying the theoretical criteria, these were straightforward to operationalize and fitted the data used to construct the economic information stimuli. Other relevant groups, e.g. groups based on occupation/profession, would be too granular for the available data. Moreover, the experimental design necessitates jointly exhaustive categories within each chosen group type that are all to some extent personally and politically relevant.

8.6.1 Group definitions

Age (Experiment 1 and 2). In Experiment 1, I use 10-year age bands to capture important life stages, resulting in 6 age groups. Experiment 2 uses three larger groups: young people, middle-aged people and elderly people. These may be less artificial and more subjectively relevant but also capture fewer life-stage specific common interests.

Geography (Experiment 1 and 2). I define relatively small geographical groups in Experiment 1 using a mixture of regions and larger cities. This results in a total of 12 groups with varying relevance. Experiment 2 splits the population into just three ‘place’ groups: ‘large cities’, ‘middle-sized cities’ and ‘rural areas’.

Social class (Experiment 1). The data used to construct stimuli only allowed me to distinguish middle class and working class subjects, and so I use these two categories only.

Education level (Experiment 1). I use two groups: short and long education. Although large, these groups arguably have more subjective resonance than more granular, formal definitions and have been empirically validated in a Danish context (Stubager, 2009).

Education-by-ethnicity (Experiment 3). I use just two education levels as in Experiment 1, here being college degree/no college degree. For ethnicity, I include just the three largest ethnic groups: White, Black and Hispanic. Due to the sample size of

just 1000 subjects, smaller ethnic groups (crossed with education) would have been statistically impractical.

8.6.2 Group allocation in the surveys

In Experiment 1, subjects were first randomly allocated to a group dimension (age, geography, education, or class) and then asked: “There are different social groups in society. Some of these are based on [age | geography | education level | class]. Which of these groups would you say you belong to?” Subjects that answered “don’t know” to q1 were treated with a random (out-)group but have been excluded from the analysis.

In Experiment 2, subjects were asked to sort themselves into groups with three questions. Here, they got to choose which group dimension was more relevant to them. All participants answered the following sequence of three questions: i) “There are many political dividing lines in society. One of them is between urban and rural areas. When you think of yourself, which of these groups do you best fit into?”, ii) “Another dividing line is between younger and older population groups. When you think of your own age, which of these groups do you best fit into?”, iii) “Most people feel closer to some groups than to others. Which of these groups do you think you have the most in common with?”. The latter question asked respondents to choose from their two previously selected groups, or “I have equally as much in common with both” or “Don’t know”. Subjects were then allocated to the group they mentioned as most important, and allocated one at random if they did not choose one. For subjects answering “don’t know” to either the geographical or age question, the other group was assumed to be their strongest identity (and they skipped the group importance question as a result.) Subjects that answered “don’t know” to both group questions were given a random-group stimuli but discarded in the analysis.

In Experiment 3, subjects were allocated to groups on the basis of the survey provider, Verasight’s, pre-recorded background records on survey participants.

Empirical validation of group measures in Experiment 1 and 2 can be accessed upon request from the author.

8.7 Overview of measured variables in experiments

Table 8.10: Overview of Measured Variables

Measure	Wording	Scale	Experiment(s) included in
Egotropic evaluations	How would you say your economic situation is today compared to 12 months ago?	5-point	1 (control), 2 (outcome)
Sociotropic evaluations	How would you say Denmark's economic situation is today compared to 12 months ago?	5-point	1, 2 (control)
Government economic approval	How satisfied or dissatisfied are you with the government's handling of the economy?	5-point	2 (control)
Party preference	Which party did you vote for in the last election?	2 categories (voted for gov't party or not)	1, 2 (control)
Party identification	Would you call yourself a Democrat, Republican or an independent?	3 categories	3 (control)
Satisfaction with economy	How satisfied or dissatisfied are you with the way the [Danish/American] economy currently develops?	5-point	1, 2, 3 (outcome)
Government economic approval	To what extent do you agree or disagree with the way the current government manages the Danish economy?	5-point	1 (outcome)

8.8 Full results and alternative specifications for the experiments

Table 8.11 shows the full regression results from the models in Table 5.4 and Table 8.12 shows the same results across alternative model specifications. Table 8.13 shows the results from Experiment 1 using a 'pure' no-information control condition. Table 8.15 shows results comparing absolute and relative group performance treatments in Experiment 1. Table 8.14 replicates the regression results from the main models,

restricting the control condition to those who received stimuli with out-groups that were highly dissimilar to their in-group. For Experiment 1, this includes both of the education and class groups from the original analysis. For age groups, it includes only people in their 20s or 30s receiving out-group stimuli about people in their 60s or 70+, and vice versa. For geographical groups, it includes people from Copenhagen who receive (out-group) stimuli about other people from Zealand (except North Zealand), Jutland or Funen; people from West Zealand, South Zealand or Funen, receiving stimuli about people from the three largest cities Copenhagen, Aarhus or Aalborg; people from Odense who receive stimuli about people from Copenhagen; and people from Jutland who receive stimuli about people from Copenhagen or North Zealand. For Experiment 2, the ‘middle’ groups are excluded, i.e. the ‘middle aged’ and ‘people from mid-sized cities’. For Experiment 3, it includes non-degree whites receiving stimuli about degree-holding blacks or hispanics; degree-holding whites receiving stimuli about non-degree blacks or hispanics; non-degree blacks and hispanics receiving stimuli about degree-holding whites; and degree-holding blacks and hispanics receiving stimuli about non-degree whites.

Across all models, ‘Treatment intensity’ is a continuous indicator for the statistic included in each stimulus and ‘Stimulus’ coefficients (Experiment 1) encode which type of information is included in the stimulus (ref. is high financial insecurity). For party ID, ref. is the Democratic party. For gov’t vote intention, this is the majority ‘red bloc’ parties for Experiment 1 and the three parties in the centrist coalition for Experiment 2. Standard errors (in parentheses) clustered at the in-group level in Experiment 1, and not clustered in Experiment 2 and 3.

Table 8.11: Effects of In-Group Decline and Improvement (Full Results)

	Experiment 1		Experiment 2	Experiment 3
	Econ. satisfaction	Govt. approval	Econ. satisfaction	Econ. satisfaction
In-group decline	−0.12 (0.03)**	−0.09 (0.04)*		
In-group improvement				0.19 (0.08)*
Treatment intensity	0.00 (0.00)	0.00 (0.00)	−0.02 (0.01)*	0.01 (0.19)
Stimulus: high job insecurity	0.04 (0.12)	−0.09 (0.14)		
Stimulus: low financial security	−0.02 (0.14)	−0.04 (0.13)		
Sociotropic eval's	0.49 (0.04)***	0.32 (0.03)***	0.30 (0.02)***	
Egotropic eval's	0.24 (0.04)***	0.12 (0.04)**		
Gov't vote intention	0.26 (0.05)***	1.07 (0.06)***	0.14 (0.04)**	
Party ID: Rep.				−1.05 (0.07)***
Party ID: Indep.				−0.81 (0.08)***
Party ID: other				−0.65 (0.10)***
Clustered SEs	Yes	Yes	No	No
In-group FEs	Yes	Yes	Yes	Yes
No. of groups	22	22	6	6
N	1388	1379	1881	2000

Note: Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' and 'In-group improvement' variables show the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). * $p < .05$, ** $p < .01$, *** $p < .001$.

Table 8.12: Effects of In-Group Decline and Improvement Across Alternative Model Specifications

	Experiment 1				Experiment 2		Experiment 3	
	Economic satisfaction		Government approval		Economic satisfaction		Economic satisfaction	
	Bivariate	No FEs	Bivariate	No FEs	Bivariate	No FEs	Bivariate	No FEs
In-group decline	−0.12 (0.05)*	−0.12 (0.03)***	−0.12 (0.06)*	−0.08 (0.04)*	−0.16 (0.05)**	−0.10 (0.04)*		
In-group improvement							0.18 (0.08)*	0.19 (0.08)*
Treatment intensity		0.00 (0.00)		0.00 (0.00)		0.00 (0.00)		0.03 (0.19)
Stimulus: high financial insecurity		0.02 (0.08)		−0.02 (0.07)				
Stimulus: high job insecurity		0.03 (0.10)		0.04 (0.08)				
Stimulus: low financial security		0.08 (0.09)		0.07 (0.06)				
Sociotropic eval's		0.49 (0.04)***		0.31 (0.03)***		0.31 (0.02)***		
Egotropic eval's		0.24 (0.04)***		0.12 (0.04)**				
Gov't vote intention		0.25 (0.05)***		1.06 (0.06)***		0.13 (0.04)**		
Party ID: Rep.								−1.15 (0.07)***
Party ID: other								−0.78 (0.10)***
Clustered SEs	Yes	Yes	Yes	Yes	No	No	No	No
In-group FEs	Yes	No	Yes	No	Yes	No	Yes	No
No. of groups	22	22	22	22	6	6	6	6
N	1388	1388	1379	1379	1881	1881	2000	2000

Note: Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' and 'In-group improvement' variables show the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). * p<.05, ** p<.01, *** p<.001.

Table 8.13: Effects of In-Group Decline, With No-Information Control Scenario

	In-group decline	
	Economic satisfaction	Government approval
In-group decline	−0.18 (0.06)**	−0.20 (0.04)***
Treatment intensity	0.00 (0.00)	0.00 (0.00)
Stimulus: high job insecurity	0.16 (0.13)	0.22 (0.18)
Stimulus: low financial security	0.22 (0.07)**	0.47 (0.05)***
Sociotropic eval's	0.47 (0.05)***	0.34 (0.04)***
Egotropic eval's	0.25 (0.05)***	0.11 (0.03)**
Gov't vote intention	0.19 (0.04)***	1.01 (0.05)***
In-group clustered SEs	Yes	Yes
In-group fixed effects	Yes	Yes
No. of groups	22	22
N	1373	1359

Note: Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' variable shows the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). * p<.05, ** p<.01, *** p<.001.

Table 8.14: Effects of In-Group Decline and Improvement, with Control Stimuli Restricted to Most Dissimilar Out-Groups

	Experiment 1		Experiment 2	Experiment 3
	Econ. satisfaction	Gov't approval	Econ. satisfaction	Econ. satisfaction
Rel. in-group decline	−0.12 (0.04)**	−0.11 (0.05)*	−0.16 (0.16)	
Rel. in-group improvement				0.19 (0.09)*
Controls	Yes	Yes	Yes	Yes
Clustered SEs	Yes	Yes	No	No
In-group FEs	Yes	Yes	Yes	Yes
No. of groups	22	22	6	6
Treated/untreated N	680/447	676/455	980/254	309/506
N	1127	1131	1234	815

Note: Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' and 'In-group improvement' variables show the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). * p<.05, ** p<.01, *** p<.001.

Table 8.15: Effects of Absolute and Relative In-group Decline

	With national benchmark		Without national benchmark		Interaction model	
	Econ. satisfaction	Govt. approval	Econ. satisfaction	Govt. approval	Econ. satisfaction	Govt. approval
	1	2	3	4	5	6
Treatment effect	−0.11 (0.03)**	−0.08 (0.04)*	−0.03 (0.05)	−0.07 (0.05)	−0.01 (0.04)	−0.05 (0.05)
National benchmark					0.09 (0.03)**	−0.01 (0.05)
Treatment X national benchmark					−0.12 (0.05)*	−0.05 (0.06)
Treatment intensity	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)	0.00 (0.00)
Stimulus: high job insecurity	0.07 (0.10)	−0.02 (0.12)	−0.15 (0.15)	−0.22 (0.13)	−0.02 (0.06)	−0.08 (0.08)
Stimulus: low financial security	−0.01 (0.12)	0.05 (0.14)	−0.05 (0.16)	−0.15 (0.10)	−0.02 (0.10)	−0.02 (0.08)
Sociotropic eval's	0.50 (0.04)***	0.31 (0.03)***	0.55 (0.02)***	0.40 (0.04)***	0.52 (0.02)***	0.35 (0.02)***
Egotropic eval's	0.24 (0.04)***	0.12 (0.04)**	0.29 (0.03)***	0.12 (0.03)**	0.26 (0.02)***	0.12 (0.03)***
Govt. vote intention	0.29 (0.06)***	1.08 (0.05)***	0.22 (0.04)***	1.01 (0.07)***	0.25 (0.04)***	1.05 (0.05)***
Clustered SEs	Yes	Yes	Yes	Yes	Yes	Yes
In-group FEs	Yes	Yes	Yes	Yes	Yes	Yes
No. of groups	22	22	22	22	22	22
N	1501	1489	1478	1460	2979	2949

Note: Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' and 'In-group improvement' variables show the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). Models 1 and 2 show effects the effects of treatment including the national benchmark (identical to 'relative in-group decline' effects in Table 4). Models 3 and 4 show the same effects for the treatment conditions without the national benchmark. Models 5 and 6 compare these two effects, by interacting two variables: one indicating getting in-group vs out-group information, and one indicating getting the national benchmark vs not. * p<.05, ** p<.01, *** p<.001.

8.9 Stimuli construction from national election studies

8.9.1 Experiment 1

The stimuli for Experiment 1 are shown in Table 8.16. In designing them, I used three economic wellbeing indicators from the 2015 and 2019 DNES that were broad and unambiguously valenced: “How is your household’s economic situation today compared with a year ago?”, “How secure do you feel in your current economic situation?”, and “How worried are you about losing your current job?”. The first variable had the most positive answer distribution and was therefore used for the *positive* national figure (82% of voters felt that their situation had remained stable or improved). The two other variables had more pessimistic answer distributions and were analyzed for negative group-level figures. After sorting respondents into the chosen social groups, I computed how the two indicators changed for each group between the DNES 2015 and the DNES 2019, both for the proportion feeling secure/not worried and the proportion feeling insecure/worried (by merging the categories on either side of the neutral option). As I looked for *negative* changes in group performance, both a decrease in the proportion feeling secure/not worried and an increase in the proportion feeling insecure/worried could be used for the stimuli.

Most people happened to report declining economic wellbeing in this interval on these variables. As a result, all these groups saw declines on at least one survey item. However, not all groups saw declining conditions on the same items and the intensity (i.e. absolute percentage change) varied substantially.

Table 8.16: Overview of Group-Specific Stimuli for Experiment 1

Group	Stimulus
(All)	The economy is not developing in the same way for everyone. For example, in the latest survey from the Danish Election Project, more [GROUP] felt that their economic situation was worse today than a year ago. For example, the proportion of [GROUP STIMULUS] compared to the previous survey. By contrast, a large majority (82%) of people felt that their economic situation had improved or remained the same.
Group-specific stimuli	
people with a short education	who felt insecure about their financial situation increased significantly (25%)
people with a long education	who felt insecure about their financial situation increased significantly (20%)
middle class people	who felt secure in their job decreased significantly (6%)
working class people	who felt insecure about their financial situation increased significantly (73%)
people from Copenhagen	who felt insecure about their financial situation increased significantly (63%)
people from North Zealand	who were worried about losing their job increased significantly (17%)
people from West Zealand	who felt secure about their financial situation decreased significantly (3%)
people from South Zealand	who were worried about losing their job increased significantly (42%)
people from Funen	who felt insecure about their financial situation increased significantly (40%)
people from Odense	who were worried about losing their job increased significantly (38%)
people from Aarhus	who were worried about losing their job increased significantly (93%)
people from Aalborg	who felt insecure about their financial situation increased significantly (33%)
people from Central Jutland	who felt secure about their financial situation decreased significantly (3%)
people from North Jutland	who felt insecure about their financial situation increased significantly (33%)
people from South Jutland	who felt insecure about their financial situation increased significantly (67%)
people from West Jutland	who felt secure in their job decreased significantly (11%)
people in their 20s	who felt insecure about their financial situation increased significantly (29%)
people in their 30s	who were worried about losing their job increased significantly (24%)
people in their 40s	who felt insecure about their financial situation increased significantly (25%)
people in their 50s	who felt insecure about their financial situation increased significantly (50%)
people in their 60s	who felt insecure about their financial situation increased significantly (80%)
people over 70	who felt insecure about their financial situation increased significantly (67%)

To balance the concern with keeping treatments as similar as possible with the need to maximize treatment intensity, I chose treatments based on the following decision rule: pick the percentage increase in insecurity (the information type with highest group coverage and average intensity) *except* if it is missing or <20%; then pick the alternative available for the group with the highest intensity. This results in 15 out of 22 groups receiving information of the same type (proportion feeling economically insecure). See Table 8.16 for an overview. Note that differences between stimuli are controlled for in the analysis with a continuous variable indicating ‘intensity’, i.e. the quoted percentage change, as well as a categorical variable for stimulus ‘type’, i.e. indicating which of the four types of economic developments is described. A more detailed overview of this selection process is available upon request from the author.

8.9.2 Experiment 2

Table 8.17: Overview of Group-Specific Stimuli for Experiment 2

Group	Stimulus
(All)	Denmark’s economy is healthy in many ways. Inflation is on the way down and according to new figures from The Danish Election Project, more than 7 out of 10 Danes now believe that their financial situation is stable or improving. However, this does not apply to everyone. The numbers also show that the vast majority of [GROUP STIMULUS] and that many people in [GROUP] feel financially insecure.
Group-specific stimuli	
people in large cities	do not see improvements in their personal finances (72%)
people in medium-sized cities	do not see improvements in their personal finances (74%)
people in the countryside	do not see improvements in their personal finances (77%)
young people	do not see improvements in their personal finances (69%)
middle-aged people	do not see improvements in their personal finances (64%)
elderly people	do not see improvements in their personal finances (85%)

The stimuli for Experiment 2 are shown in Table 8.17. In designing them, I used two economic wellbeing indicators from the 2022 DNES: “How is your household’s economic situation today compared with a year ago?” and “How secure do you feel in your current economic situation?”. Again, the first variable was used for the *positive* national figure that 7 in 10 voters felt that their situation had remained stable or improved.

To construct group-level decline, I used both of the variables. The stimulus reads:

“The numbers also show that the vast majority of [GROUP MEMBERS] do not see improvements in their personal finances (X%) and that many people in [GROUP] feel financially insecure.” The financial security variable is the basis for the latter claim in the sentence. Here, ‘many’ refers to the 13-20 percent of each group that reported feeling financially “insecure” or “very insecure” in the 2022 DNES. The first part of the sentence refers to a number calculated for each group by merging those saying they have seen no or a negative change in their personal economic situation over the past year (which was a large majority for every group). This process was far simpler than in Experiment 1 because there were such strong and negative responses in all groups. As such, it does not even rely on different questions to get the apparent divergence between group and national performance: it just uses the large proportion reporting ‘no change’ in both numbers, but frame them slightly differently as their situation either being “stable or improving” (positive) or that they “do not see improvements” (negative).

8.9.3 Experiment 3

Table 8.18: Overview of Group-Specific Stimuli for Experiment 3

Group	Stimulus
(All)	Most Americans have struggled economically during the past year. The country has seen rising prices, and increasing economic pessimism with consumer confidence hitting levels below those during the pandemic. However, this is not felt equally by all groups in society. According to the latest survey by the American National Election Studies, [GROUP] for instance reported increasing economic optimism. The number of [GROUP STIMULUS].
Group-specific stimuli	
white non-college graduates	feeling worried about their economic situation has been more than halved
black non-college graduates	feeling worried about their economic situation had fallen by nearly a third
hispanic non-college graduates	feeling worried about their economic situation had fallen by nearly a third
white college graduates	feeling worried about their economic situation had fallen by nearly two thirds
black college graduates	feeling worried about their economic situation had fallen by nearly two thirds
hispanic college graduates	feeling worried about their economic situation had fallen by more than a third

The stimuli for Experiment 3 are shown in Table 8.18. In designing them, I used the following item from the 2020 and 2016 ANES: “So far as you and your family are concerned, how worried are you about your current financial situation?” For this experiment, the treatment was relative in-group *improvement*. I therefore used positive changes in this variable for each group to construct the stimuli. Specifically, I summed the proportion reporting that they were “extremely” or “very worried” (the other categories were “moderately worried”, “a little worried” and “not at all worried”) in 2016 and 2020. This proportion declined substantially for every group in the period (ranging from 28% to 63%). To smoothen out the differences and make the numbers more intuitive, they were reported qualitatively as either falling by “nearly a third” (28%, 30%), “more than a third” (34%), “more than half” (56%), or “nearly two thirds” (61%, 63%).

To get the contrasting *negative* national-level information, I did not use the ANES but instead referred to recent and well-known developments in the US economy, i.e. rising prices and historically low consumer confidence (Hsu, 2023).

8.10 Experimental effects are not driven by pocket-book expectations

Table 8.19: Effects of In-Group Decline on Prospective Pocketbook Evaluations

	Without national benchmark	With national benchmark
In-group decline	0.02 (0.04)	−0.04 (0.03)
Treatment intensity	0.00 (0.00)+	0.00 (0.00)
Stimulus: high job insecurity	−0.09 (0.08)	−0.08 (0.13)
Stimulus: low financial security	−0.14 (0.15)	−0.17 (0.12)
Sociotropic eval's	0.06 (0.04)	0.15 (0.03)***
Egotropic eval's	0.34 (0.05)***	0.40 (0.03)***
Govt. vote intention	−0.03 (0.04)	0.00 (0.03)
Clustered SEs	Yes	Yes
In-group FEs	Yes	Yes
No. of groups	22	22
N	1486	1508

Note: Estimates from linear regression models of prospective pocketbook evaluations (5-point scale) on treatment (in-group performance vs out-group performance information (ref.); with and without national benchmark). Estimates from linear regression models of satisfaction with the current state of the economy and approval of the incumbent's handling of the economy (5-point scales) on treatment with in-group fixed effects and design controls. The coefficients on the 'In-group decline' variable shows the gap between those getting in-group stimuli vs those getting out-group stimuli (ref.). * p<.05, ** p<.01, *** p<.001.

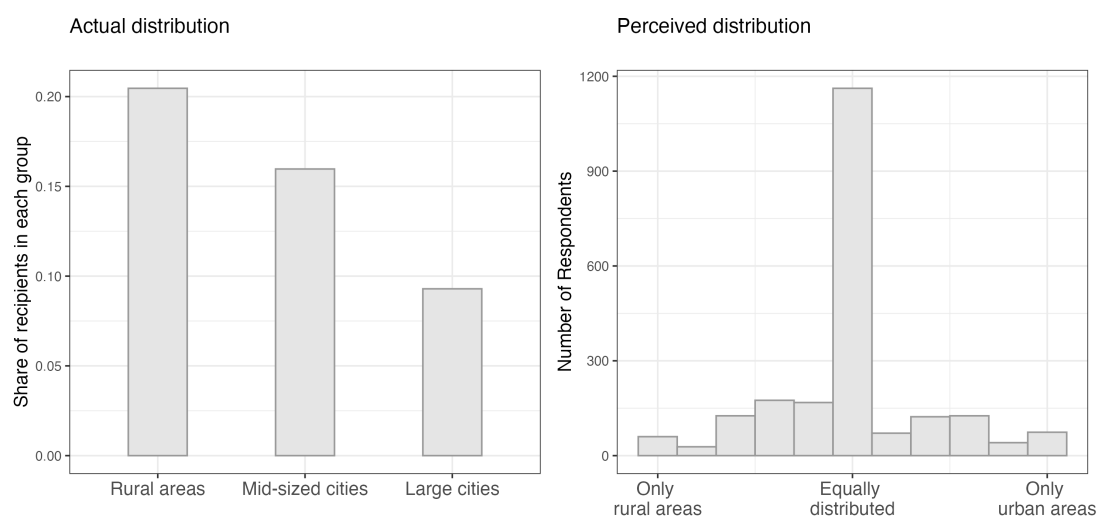
Note. Estimates from two-way fixed effects models of self-reported likelihood of voting for the incumbent party. All independent variables are standardized. Coefficients in columns 1 and 2 are pooled estimates from several underlying models, one per group type, using simple meta-regression models with equal weights (see Appendix A for unpooled models). For these meta-regressions in columns 1 and 2, the total N and individual FE statistics show the minimum numbers, as they vary slightly between underlying models. * p<.05, ** p<.01, *** p<.001.

Appendix (Chapter 7)

8.11 The Distribution of the Heat Check

As seen in the left panel of Figure 8.6, the Heat Check went disproportionately to voters outside the large cities. However, when asked how the Heat Check was distributed geographically, respondents were clearly unsure, as shown in the right panel. 34% answered “don’t know” and a further 36% chose the midpoint of the scale which was labeled “Urban and rural areas benefited equally”. The remaining 30% of respondents fell somewhat equally on either side of the midpoint with a slight skew towards the truth: that rural people benefited most. The most accurate answer on this 11-point scale would arguably be the lowest or next-lowest value on the scale, and this was chosen by just around 5% of respondents.

Figure 8.6. Actual and perceived distribution of Heat Check recipients. The left panel shows the share of self-identified Heat Check recipients by chosen geographical group. The right panel is a histogram of all respondents’ perceptions of how the Heat Check was distributed among urban and rural areas.



I further conduct a basic media search on the Danish national media archive of *Infomedia* for articles mentioning the heat check between it was first proposed and mentioned

in the media (January 27, 2022) until the date at which all heat check transfers were completed (August 10, 2022) (The Danish Ministry of Climate, Energy and Utilities, 2022). As shown in Table 8.20 total of 2843 articles mention the heat check during this period. Of these, just 62 additionally mention any of the terms “large cities”, “small[er] cities”, “countryside” or a common term for rural Denmark (“udkantsdanmark”), anywhere in the article text. This is equivalent to 2.2 percent of all articles mentioning the heat check during the period.

Note that this likely represents a liberal estimate of articles that truly document the rural targeting of the heat check, as these keywords are relatively common for unrelated reasons. A manual inspection of the 62 articles confirm this picture as very few in fact mention these keywords in connection to the heat check’s targeting. Among the few that do, some even wrongly imply that the check was designed to target the large cities, with, e.g., one article on a major news site citing disgruntled rural inhabitants accusing the governing party of targeting “their voters [who] are in the cities [while] the rest of us are ignored, because we live in the countryside, where people don’t vote for the left-wing to the same extent” (Harder, 2022). This confirms the prevailing ambiguity and lack of information regarding the targeting of the heat check.

Table 8.20: Media Search Keys and Results (January 27 – August 10, 2022)

Search Keys
('varmecheck' OR 'varmechecken')
('varmecheck' OR 'varmechecken') AND ('store byer' OR 'udkantsdanmark' OR 'på landet' OR 'små byer' OR 'mindre byer')

8.12 Question wordings

Observational: Denmark

- *Group membership (geographical)*
 - “There are many political dividing lines in society. One of them is between urban and rural areas. When you think of yourself, which of these groups do you best fit into?”
 - * People from large cities (e.g. Copenhagen, Aarhus, Odense); People from mid-sized cities (e.g. Viborg, Svendborg, Roskilde); People from rural areas
- *Heat Check eligibility (heat source)*
 - “What is the primary heat source in your home?”
 - * District heating; Heat from oil boiler; Heat from natural gas boiler; Heat from pellet or wood-fired boiler; Heat pump (incl. geothermal heating); Electric heating (e.g. electric radiator); Other; Don’t know
- *Heat Check eligibility (household income)*
 - “What is your household’s total annual income, gross - i.e. before tax?”
 - * 17 income brackets
- *Heat Check receipt*
 - “Did your household receive the 6,000 DKK "heat check" that was paid out in August? (Note: it was only paid to one member of each household).”
 - * Yes; No; Don’t know
- *Perceived distribution of Heat Check (geographical)*
 - “Thinking about how the Heat Check was distributed geographically, to what extent do you think it benefited people in urban and rural areas?”
 - * 0 - it benefited only rural people; 10 - it benefited only urban people
- *Government support (outcome)*
 - “How satisfied or dissatisfied are you with the government’s handling of the economy?”
 - * Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Somewhat dissatisfied; Very dissatisfied; Don’t know

Observational: United States (Annenberg Election Study)

- *CARES Act stimulus check receipt*
 - “Since the start of the coronavirus pandemic, have you or someone in your family who is currently living with you experienced any of the following? [Received a coronavirus relief payment, also called a stimulus payment from the federal government]”
 - * Yes; No
- *Perceived distribution of CARES Act benefits (racial)*
 - “Thinking about the federal government’s plans to address the economic consequences of the coronavirus pandemic, how much do you think [White Americans/Black or African Americans] got what they deserved from these plans?”
 - * Got much more than they deserve; Got somewhat more than they deserve; Got about what they deserve; Got somewhat less than they deserve; Got much less than they deserve
- *Trump vote (outcome)*
 - “Thinking about the general election for president in November, 2020, if that election were held today, and the candidates were (Joe Biden, the Democrat), and (Donald Trump, the Republican), for whom would you vote?”
 - * Joe Biden, the Democrat; Donald Trump, the Republican; Someone else (SPECIFY); Would not vote for President

Experiment 1 (translated from Danish)

- *Group membership (geographical)*
 - “Which of the following societal groups best describes you?”
 - * People from large cities (e.g. Copenhagen, Aarhus, Odense); People from mid-sized cities (e.g. Viborg, Svendborg, Roskilde); People from rural areas
- *Heat check support (pre-treatment)*

- “In general, do you think that direct payments such as the “heat check” paid out in August are a good way to deal with the problem of inflation?”
- * Yes, very good; Yes, somewhat good; Neither good nor bad; No, somewhat bad; No, very bad; Don’t know
- *Government support (pre-treatment)*
 - “How satisfied are you with the government’s handling of Denmark’s economic situation?”
 - * Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Somewhat dissatisfied; Very dissatisfied; Don’t know
- *Outcome: check support*
 - “To what extent do you support the proposed ‘inflation check’?”
 - * To a high degree; To some degree; Neither or; To a low degree; Not at all; Don’t know
- *Outcome: government support*
 - “How satisfied would you be with the government’s management of the economy if it enacted this ‘inflation check’?”
 - * Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Somewhat dissatisfied; Very dissatisfied; Don’t know

Experiment 2 (translated from Danish)

- *Group membership (geographical)*
 - “There are many political divides in society. One of them is between urban and rural. When you think about yourself, which of these groups do you fit into best?”
 - * People from large cities (e.g. Copenhagen, Aarhus, Odense); People from mid-sized cities (e.g. Viborg, Svendborg, Roskilde); People from rural areas
- *Group membership (age)*
 - “Another divide is between younger and older population groups. When you think about your own age, which of these groups do you fit into best?”
 - * Young people; Middle-aged people; Elderly people

- *Generic stimulus check support (pre-treatment)*
 - “How much do you agree or disagree that the government should pay out cash grants to help Danes who are particularly affected by rising living costs?”
 - * Strongly agree; Somewhat agree; Neither agree nor disagree; Somewhat disagree; Strongly disagree; Don’t know
- *Government support (pre-treatment)*
 - “How satisfied or dissatisfied are you with the government’s handling of the economy?”
 - * Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Somewhat dissatisfied; Very dissatisfied; Don’t know
- *Outcome: check support*
 - “To what extent do you support the proposed ‘inflation check’?”
 - * 0 - not at all support; 10 - fully support

Experiment 3

- *Federal government trust (pre-treatment)*
 - “In general, how much do you trust the federal government in Washington D.C.?”
 - * None; A little; A moderate amount; A lot; A great deal
- *Group membership (age)*
 - “If you were asked to choose, which of the following groups would you say that you belong to?”
 - * Young people; Middle-aged people; Elderly people
- *Government support (pre-treatment)*
 - “To what extent do you approve or disapprove of the way the federal government is handling the economy?”
 - * Strongly approve; Somewhat approve; Neither approve nor disapprove; Somewhat disapprove; Strongly disapprove; Don’t know

- *Outcome: check support*
 - “To what extent do you support or oppose this proposed Inflation Relief Check?”
 - * Strongly support; Somewhat support; Neither support nor oppose; Somewhat oppose; Strongly oppose; Don’t know
- *Outcome: government support*
 - “Suppose the government implemented this Inflation Relief Check today. How would you rate your satisfaction with the government’s overall handling of the economy?”
 - * Very satisfied; Somewhat satisfied; Neither satisfied nor dissatisfied; Somewhat dissatisfied; Very dissatisfied; Don’t know

8.13 Model Specifications for Study 1

Danish data

The estimates from Denmark in Table 5.2 are from models of the form:

$$ingrouptargeted_i = \alpha + \beta_1 recipient_i + \beta_2 ruralurban_i + \beta_3 heatsource_i + \beta_4 income_i + \epsilon_i$$

where *ingroup targeted* is the perception that the respondent's in-group was targeted by the policy (on a scale from 'only benefited [out-group]' to 'only benefited [in-group]'); *recipient* indicates whether the subject received the heat check or not; *heat source* is an indicator for eligible heat sources for the heat check; *income* is income measured in 17 bands, included as a continuous variable; and *rural urban* indicates geographical in-group.

US data

For the analysis of US data in Table 5.2 the estimated models are of the form:

$$benefits\ ingroup_i = \alpha_i + \beta_1 received\ stimulus_{it} + \epsilon_{it}$$

where α_i are individual fixed effects; *benefits ingroup* is the perception that the respondent's in-group benefited from the stimulus measures; and *received stimulus* indicates whether the respondent personally received a stimulus check.

8.14 Consistency with Pre-Registered Analysis Plans

I pre-registered the analyses and hypotheses for the three experiments.¹⁸ My analyses follow these plans in that all analyses are carried out as pre-specified. There are only a few minor deviations from the described analyses as well as a few omitted analyses. I elaborate below.

Experiment 1

My analysis differs from the report in three distinct respects. First, I don't cluster errors at the level of treatment in the analysis because there are too few clusters: since I just focus on scenario A and C, I get a total of 12 treatment clusters (ingroup \times recipient status \times ingroup targeting = $3 \times 2 \times 2$) which is half of what I would have had with the full set of treatments. This is a problem because cluster-robust errors behave badly when there are too few clusters Özlér (2012). Still, the results are robust to using cluster-robust errors as shown in Figure 8.7.

Second, I omit analyses of an additional outcome variable: voters' perception that the government is proposing the 'inflation check' tactically to win votes, with the expectation that personal recipients are less likely to believe this. Although this hypothesis is in fact supported, the outcome was omitted for simplicity.

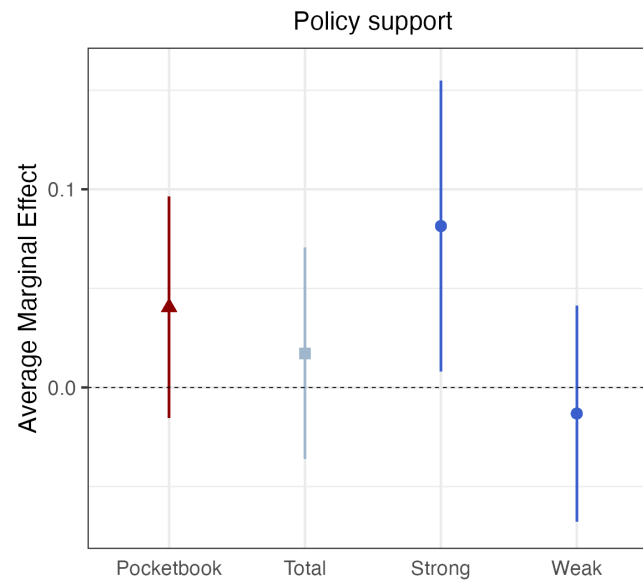
Third, I omit analyses of effect moderation by personal economic wellbeing and satisfaction with Danish democracy. These are not relevant to the focus of this paper.

Experiment 2

For experiment 2, the only omitted analysis is a model testing whether the effect of receiving the check is moderated by in-group targeting (as specified by interacting personal benefit and in-group benefit). This is not a direct implication of the theoretical argument in this paper and omitted for simplicity.

¹⁸Experiment 1: <https://osf.io/bq68x>. Experiment 2: <https://osf.io/tywmx>. Experiment 3: <https://osf.io/k82av>.

Figure 8.7. Results from Experiment 1 using robust errors clustered at the 12 treatment clusters.



Experiment 3

All specified analyses are reported. The pre-registered models are regressions of the outcome on each of the treatment variables, controlling for baseline government approval, age group membership, and other baseline covariates that are predictive of the outcome. Note that as I mention in the pre-registration, my experiment was included on a collaborative survey, and I therefore include pre-treatment covariates that are predictive of the outcome. I therefore include party identification, living in an urban/rural area, and trust in government.

8.15 Model Specifications for Experiments

The estimates are from models of the following general form:

$$outcome_i = \alpha + \beta_1 ingroup\ targeted_i + \beta_2 recipient_i + \beta_3 baseline\ controls_i + \epsilon_i$$

$$outcome_i = \alpha + \beta_1 ingroup\ targeted_i + \beta_2 recipient_i + \beta_3 baseline\ controls_i \\ + \beta_4 grouptype_i + \beta_5 ingroup\ targeted_i \times grouptype_i + \epsilon_i$$

where *outcome* is approval of the proposed check and approval for the government, respectively; *ingrouptargeted* is a dummy that is coded 1 when the in-group is targeted and 0 when the check is distributed equally; *recipient* is a dummy indicating whether the subject personally receives the benefit; *baselinecontrols* is a set of baseline measure of government support and general support for stimulus checks (and a few additional measures in Experiment 3; see section 8.14); and *grouptype* is an indicator of whether the in-group is a strong or weak identity group. In Experiment 2, I follow my pre-analysis plan in interacting treatments with all centered covariates, following advice from Lin, Green and Coppock (2016).

8.16 Results Across All Distributive Treatments (Experiment 1)

Figure 8.8 shows the four visual stimuli used in Experiment 1 and the four variations of the text they are associated with in the vignettes. The main analysis in the paper focuses on conditions A and C. However, it might be that voters are responding not to the unique targeting of the group but just its total benefit level. In that case, there would be no higher policy support among those in condition C than in condition B. Following a similar logic, those in conditions A and D should show similar (low) support if voters just cared about absolute rather than relative levels.

Figure 8.8. Generic versions of the four visual treatment stimuli in Experiment 1.

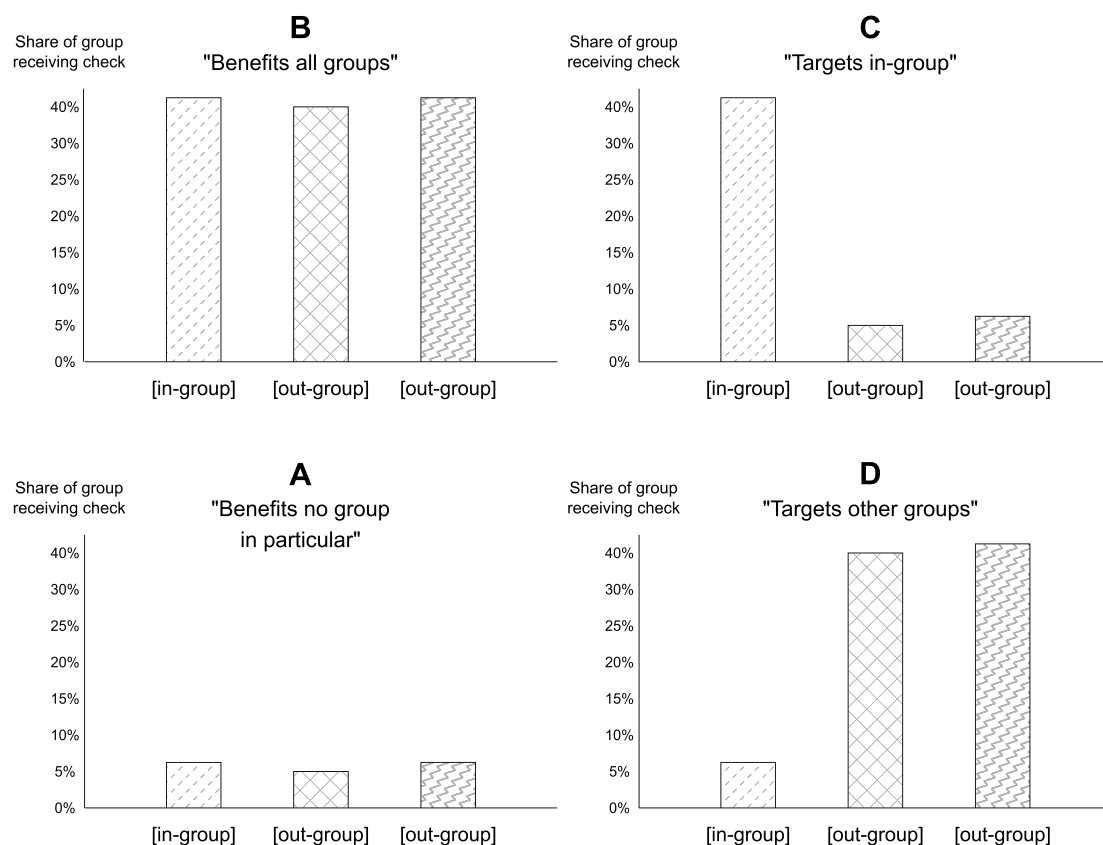
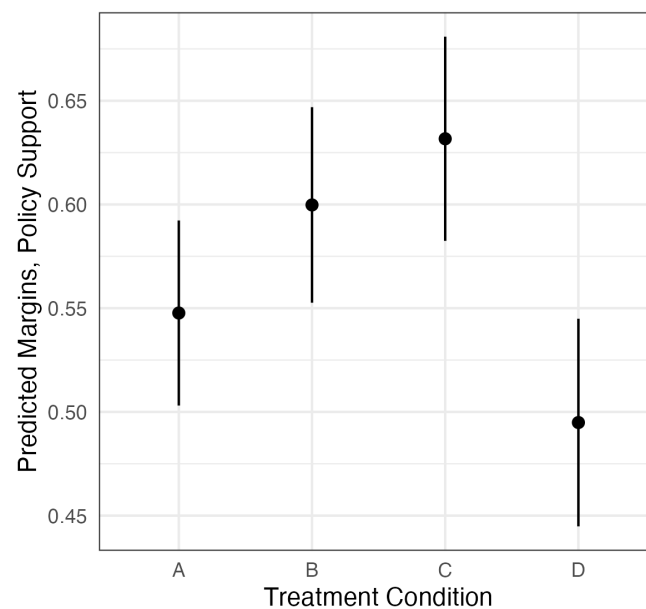


Figure 8.9 shows the estimated effects for strong identity groups across all four conditions. The contrast between A and C is the contrast reported in the main text. Group C, where the group is uniquely targeted, indeed has the highest level of support,

but it is not statistically significantly higher than the level for group B. Meanwhile, voters appear to discriminate between conditions A and D, showing lower support when other groups get more than the in-group, and this difference is statistically significant. This suggests a mix of motivations: group members both care about the absolute level of in-group benefit but also its level relative to other groups. Note, however, that statistical power is relatively low for this full set of comparisons ($N = 443$).

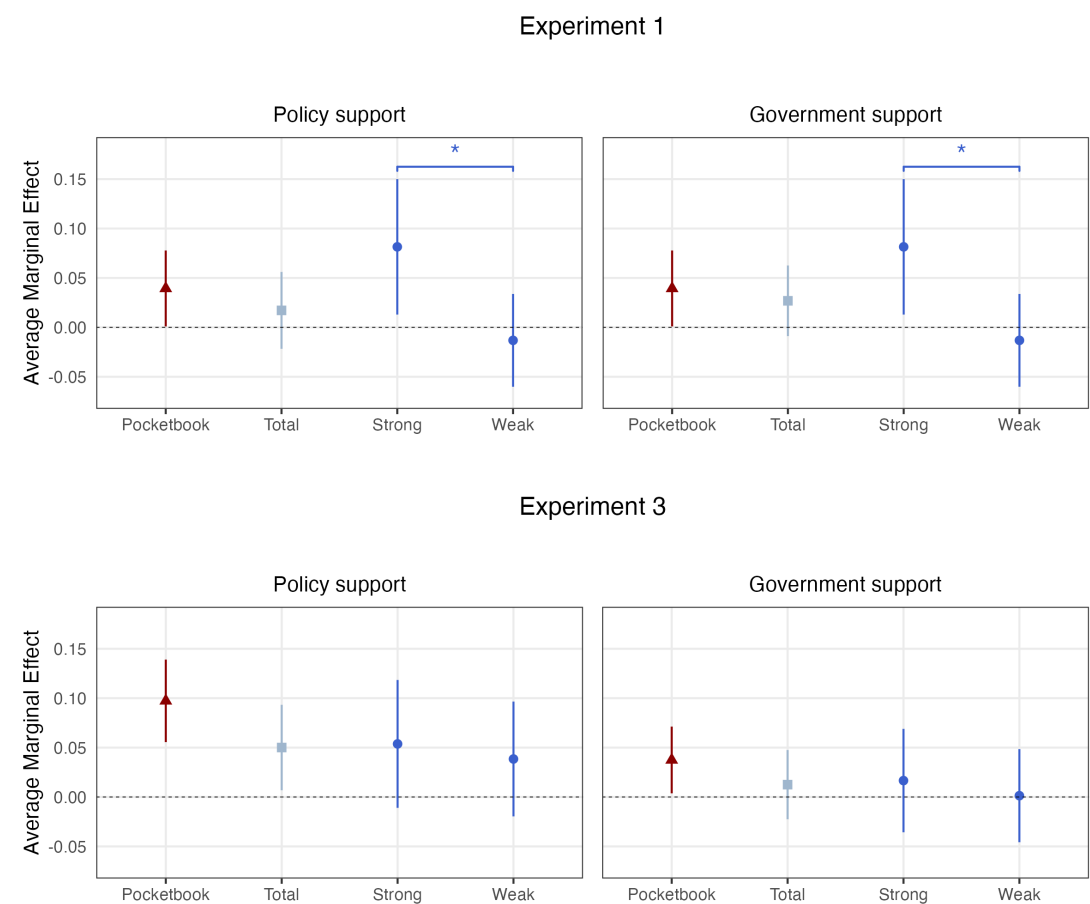
Figure 8.9. Effects of all four conditions of group targeting Experiment 1, subset to strong identity groups.



8.17 Experimental Results for Incumbent Support

Figure 8.10 shows the estimated effects for the alternative outcome, hypothetical government support if the proposed policy were to be implemented. The left panels shows the estimates from the main analysis for the policy support outcome, for comparison.

Figure 8.10. Effects of personal benefit (‘pocketbook’) and in-group targeting (‘total’) by group (‘strong’, ‘weak’) on policy support and government support.



Appendix (Chapter 8)

8.18 Overview of BES surveys

Table 8.21: Groups, parties, and surveys available for group linkage item in BESIP and BES data.

Groups	Parties	Surveys
<i>Class</i>	Labour	May-July 1997
Middle class people	Conservatives	June-September 2001
Working class people	SNP	February-May 2005
Unemployed people	Liberal Democrats	May-July 2005
<i>Religion</i>		May-September 2010
Jews		May-September 2015
Christians		November-December 2016*
Atheists		May-June 2017*
Muslims		June-October 2017
<i>Gender</i>		March 2019*
Women		December 2019*
Men		January-June 2020
<i>Age</i>		June 2020*
Young people		May 2021*
Retired people/pensioners		November-December 2021*
<i>Ethnicity</i>		May 2022*
Black and Asian people		
<i>Geography</i>		
People in London		

*BESIP panel waves.

Table 8.22: Dependent variable coverage of each group by survey wave.

Data collection [month(s)/year]	5-7/97	6-9/01	2-5/05	5-7/05	5-9/10	5-9/15	11-12/16	5-6/17	6-10/17	3/19	12/19	1-6/20	6/20	5/21	11-12/21	5/22
BES election surveys	1997 GE	2001 GE	2005 GE	2005 GE	2010 GE	2015 GE			2017 GE			2019 GE				
BES internet panel							w10	w12		w15	w19		w20	w21	w22	w23
Middle class people	3615	3900	4791	4791	1843	2987	30237	34394	2194	0	8105	3946	7902	30281	6975	30949
Working class people	3615	3900	4791	4791	1843	2987	30237	34394	2194	0	8105	3946	7902	30281	6975	30949
Unemployed people	3615	3900	4791	4791	1843	2987	30237	0	2194	0	8105	3946	7902	30281	6975	30949
Black and Asian people	3615	3900	4791	4791	1843	2987	30237	0	2194	0	8105	3946	7902	30281	6975	30949
Retired people/pensioners	0	3900	4791	4791	1843	0	0	0	0	0	0	0	0	0	6975	30949
Young people	0	0	0	0	0	0	0	0	0	0	0	0	0	0	6975	30949
People in London	0	0	0	0	0	0	0	0	0	0	0	0	0	0	6975	30949
Women	0	3900	4791	4791	1843	0	0	0	0	0	0	0	0	0	0	7878
Men	0	0	0	0	0	0	0	0	0	0	0	0	0	0	0	7878
Muslims	0	0	0	0	0	0	0	0	0	6982	0	0	0	0	0	0
Jews	0	0	0	0	0	0	0	0	0	6982	0	0	0	0	0	0
Christians	0	0	0	0	0	0	0	0	0	6982	0	0	0	0	0	0
Atheists	0	0	0	0	0	0	0	0	0	6982	0	0	0	0	0	0

8.19 Group mention dictionary

Middle class people: middle income earner*, middle class, average earner*, middle-income, white collar worker*, white-collar worker*

Working class people: working class, worker*, employee*, working famil*, chavs, manual labourer*, primary producer*, on low income*, poor people, the poor, on the poverty line, the lowest paid, lower class, lower-class, low income earner*, low income class, on social security, on lower incomes, on low wages, on poverty wages, on the dole, on or near the poverty line, low-income famil*, on modest income*, low-wage job*, the lowest-paid, zero-hours contract*, zero-hour contract*, wage-earner*, wage-earning, unskilled worker*, blue collar worker*, blue-collar worker*, pink collar worker*, pink-collar worker*

Unemployed people: unemployed people, without a job, the jobless, jobseeker*, without employment, long-term unemployed, unemployed person, the unemployed, who are unemployed

Retired people/pensioners: old people, older people, pensioner*, senior citizen*, elderly, retired people, retiree*, nearing retirement, reaching retirement, beyond retirement

Young people: student*, undergraduates, young people, youth*, teenager*, teens, adolescent*, young adult*, under-18s, under-21s, under-25s, under 18s, under 21s, under 25s, 13-year-old*, 14-year-old*, 15-year-old*, 16-year-old*, 17-year-old*, 18-year-old*, 19-year-old*, 20-year-old*, 21-year-old*, 22-year-old*, 23-year-old*, 24-year-old*, 25-year-old*

Rural people: country dweller*, rural resident*, rural area*, rural population, rural inhabitan*, rural communit*, commuter*, rural and coastal communit*, outside urban area*, countryside

People in London: city dweller*, urbanite*, urban resident*, city resident*, inner-city area*, urban area*, city area*, urban communit*, metropolitan area*, people in london, londoner*, inner-london, working in london, based in london

Black and Asian people: black british, africans, caribeans, BME, BAME, pakistani, bangladeshi, bengali, indian people, chinese people, black people, asian people, afro-caribbean, minority ethnic, black and asian, caribbean men, caribbean women, african men, african women, indian men, indian women, black men, black women, asians,

chinese men, chinese women, indian british, asian british, pakistani british, bangladeshi british, chinese british, ethnic minorities, black communit*, asian communit*, ethnic communit*

Muslims: muslim*, islam*

Jews: jew*, judais*, judeo*

Christians: catholic*, protestant*, anglican*, evangelical*

Atheists: non-believer*, atheist*

Women: women*, female

Men: men, men's, male

8.20 Automated classification of group appeals

To augment our automated classification, we train our BERT-based model to classify sentences in the following format: “British politician from the <party> (in <government status>) mentioning a group (<group>): ’<sentence>’”. This has the advantage of supplying the model with contextually relevant information about each sentence, improving accuracy. We reproduce examples of annotated sentences in the same format here.

Table 8.23: Sample of sentences annotated as positive group appeals

Text	
1 British politician from the Conservative Party (in opposition) mentioning a group (urban people): ‘The Attorney General is well aware that drug trafficking is an issue not just for urban areas, but for rural areas, villages and towns.’	
2 British politician from the Conservative Party (in opposition) mentioning a group (rural people): ‘Although I was a member of an Administration who by nature, support and backing were closer perhaps to rural areas than his, it was not always easy to win what I needed to win.’	
3 British politician from the Conservative Party (in opposition) mentioning a group (working class people): ‘I draw the attention of the House to early-day motion 1320 on Health and Safety Executive job cuts, which points out the lack of resources being made available for maintaining health and safety for UK workers, and the fact that staff numbers have fallen from 4,282 in April 2004 to 3,225 in March 2007.’	
4 British politician from the Liberal Democrats (in opposition) mentioning a group (ethnic minorities): ‘To follow up the point about ethnic minorities, it is interesting to note that 10 per cent. of the British Army is not British, with one in 10 soldiers belonging to one of 57 other nationalities.’	
5 British politician from the Conservative Party (in opposition) mentioning a group (urban people): ‘As the honorable Gentleman said, the proliferation of knives, particularly these unpleasant zombie knives, has caused a huge problem, particularly in urban areas and especially in London.’	
6 British politician from the Liberal Democrats (in opposition) mentioning a group (middle-class people): ‘Grandiose plans for public spending might help in the long term, but low and middle-income families need more money in their pockets right now.’	
7 British politician from the Conservative Party (in opposition) mentioning a group (ethnic minorities): ‘Should not the denial of women’s rights be a matter of concern to men, the denial of the rights of ethnic minorities be a matter of concern to those who do not belong to one, and the denial of the rights of gays be a matter of concern those who are not gay?’	
8 British politician from the Conservative Party (in opposition) mentioning a group (Christians): ‘We should recognise the feelings in the Muslim community about that, just as we should respect the position taken by Catholic and Jewish schools.’	
9 British politician from the Scottish National Party (in opposition) mentioning a group (women): ‘It is important to acknowledge, as other honorable Members have pointed out, that the vast majority of men are not violent towards women, but the evidence shows that such violence is perpetrated overwhelmingly by men.’	
10 British politician from the Labour Party (in government) mentioning a group (elderly people): ‘It is easy to poke fun at the Liberal Democrats for wanting to channel some of our existing resources to the oldest pensioners, but any serious person knows that that should be part of the new consensus.’	

Accuracy statistics for these models are shown in Table 8.25. As shown, both models

Table 8.24: Sample of sentences annotated as negative group appeals

	Text
1	British politician from the Labour Party (in government) mentioning a group (men): 'Women suffer horribly from violence at men's hands in the home and on the streets.'
2	British politician from the Labour Party (in government) mentioning a group (rural people): 'The power to put subsidy into rural areas is contained in the new Postal Services Bill, but the fund for deprived urban areas is exclusively for those areas, and will be ring-fenced accordingly.'
3	British politician from the Labour Party (in government) mentioning a group (young people): 'This new regime has reduced the number of institutions able to bring students to the UK from over 4,000 to approximately 2,000.'
4	British politician from the Labour Party (in government) mentioning a group (Muslims): 'In 1997 he ran Al-Ansar, an Arabic newspaper that supported the Algerian Armed Islamic Group-GIA.'
5	British politician from the Conservative Party (in opposition) mentioning a group (urban people): 'I know that the honorable Lady was not in this place during Labour's rule, but I would say gently to her that had she not been asleep under a tree like Ferdinand the Bull, she might have noticed that during the period from 1997 to 2010 a Labour Government exacerbated the educational funding gap between rural and urban areas.'
6	British politician from the Labour Party (in government) mentioning a group (Jews): 'Sharon's response to that is part ethnic cleansing-ensuring that it is impossible for people to live in the area because of the impact of the wall and the use of such things as the planning laws in and around Jerusalem, which have, in effect, judaised large parts of the outskirts of Jerusalem-and part hustling Palestinians into what can be described only as Bantustans in the west bank and Gaza.'
7	British politician from the Conservative Party (in opposition) mentioning a group (Muslims): 'It seeks the destruction of the state of Israel and the establishment of an Islamic republic in Lebanon.'
8	British politician from the Liberal Democrats (in opposition) mentioning a group (women): 'Regrettably, many of these offences are committed by women-Courtney Love being a case in point-who seem to have the same capacity to imbibe and behave badly as the men.'
9	British politician from the Conservative Party (in opposition) mentioning a group (Muslims): 'He will be aware from the ISC report that the tragic events of 7/7 followed years of failure, going back to before 1997, to appreciate the scale of the Islamist threat.'
10	British politician from the Labour Party (in government) mentioning a group (rural people): 'The Government have allocated an additional £30 million to rural policing when, on every comparison of crime between urban and rural areas, it is urban areas that should have that extra policing.'

perform well, with precision, recall, and F1 score above .8. The negative valence model performs slightly less well, likely owing to the smaller proportion of negative appeals in the training data (20 percent).

Table 8.25: Accuracy Statistics for Group Appeal Valence Prediction Models

Model	Precision	Recall	F1 Score
Positive Valence Model	0.88	0.88	0.88
Negative Valence Model	0.82	0.82	0.82

8.21 Validating group appeal valence in parliamentary speeches against press releases

To validate the valence of group appeals in parliamentary speeches, we compare them with group appeals in party press releases from the PARTYPRESS database (Erfort, Stoetzer and Klüver, 2023). The database includes all press releases by the major parties in the UK, including the four parties we focus on, over ten years from 2010 to 2019. Using the same procedure as for the parliamentary speeches, we identify and code all group appeals in the period for the same groups and parties. Mirroring the key measure of interest in our analysis, we then compute the average net valence for each group-party dyad by quarter in the nine-year period.

Table 8.26 presents estimates from regressing the measure of dyad-level quarterly average net valence for the parliamentary speeches onto the same measure for the press releases. Model 1 shows the bivariate coefficient. Models 2-4 include various fixed effects and varied error clustering. As shown, there is a robust relationship between how parties talk about each group in each quarterly period across the two data sources.

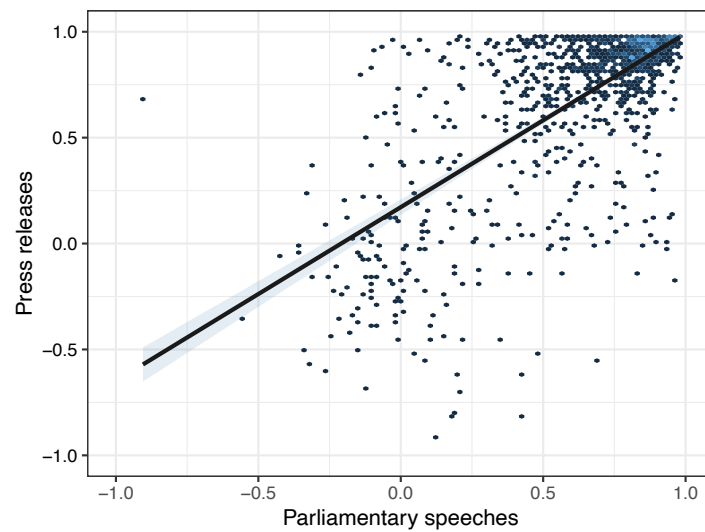
Table 8.26: Estimates from regressing quarterly group appeal valence in parliamentary speech on press releases.

	Model 1	Model 2	Model 3	Model 4
Net valence (average)	0.820*** (0.026)	0.232** (0.066)	0.180* (0.077)	0.180+ (0.096)
Intercept	0.172*** (0.019)			
N	1143	1143	1143	1143
Std.Errors	IID	group	group	dyad
FE: Group		✓	✓	✓
FE: Party			✓	✓

In Figure 8.11, we visualize the correlation between quarterly dyad-level valences for parliamentary speeches (x-axis) and party press releases (y-axis).

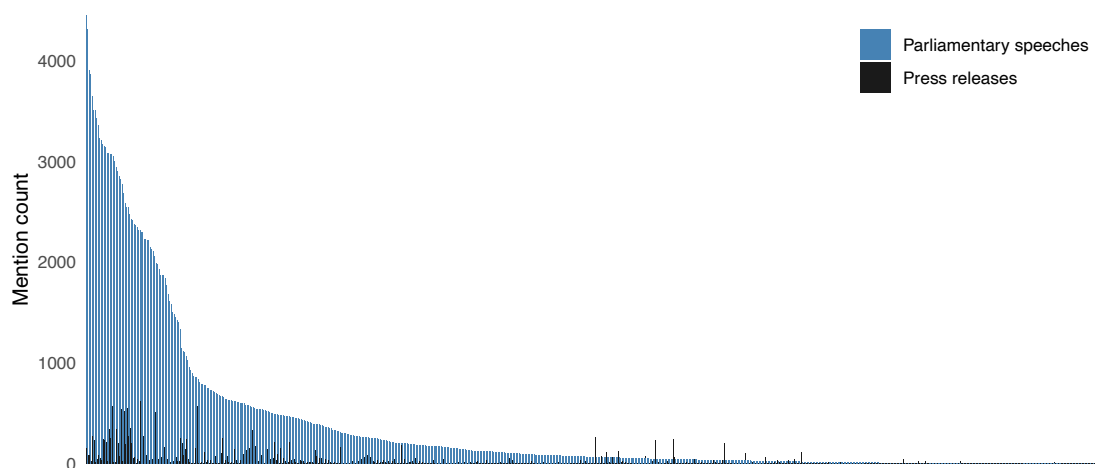
Figure 8.12 shows the number of mentions per dyad-year in the main dataset, parliamentary speeches, compared to in press releases. As shown, there are far fewer observations in the press releases dataset, even in the time period of common coverage

Figure 8.11. Quarterly average net valence by dyad for parliamentary speeches (x-axis) and party press releases (y-axis), 2010-2019. Overlaid linear regression line. Color shading reflects point density. Speech and press release dyad valences are correlated at $r = .68$ ($t = 31.3, p < .001$).



(2010 to 2019), which is why press releases do not offer sufficient statistical power to serve as a basis for the main analysis.

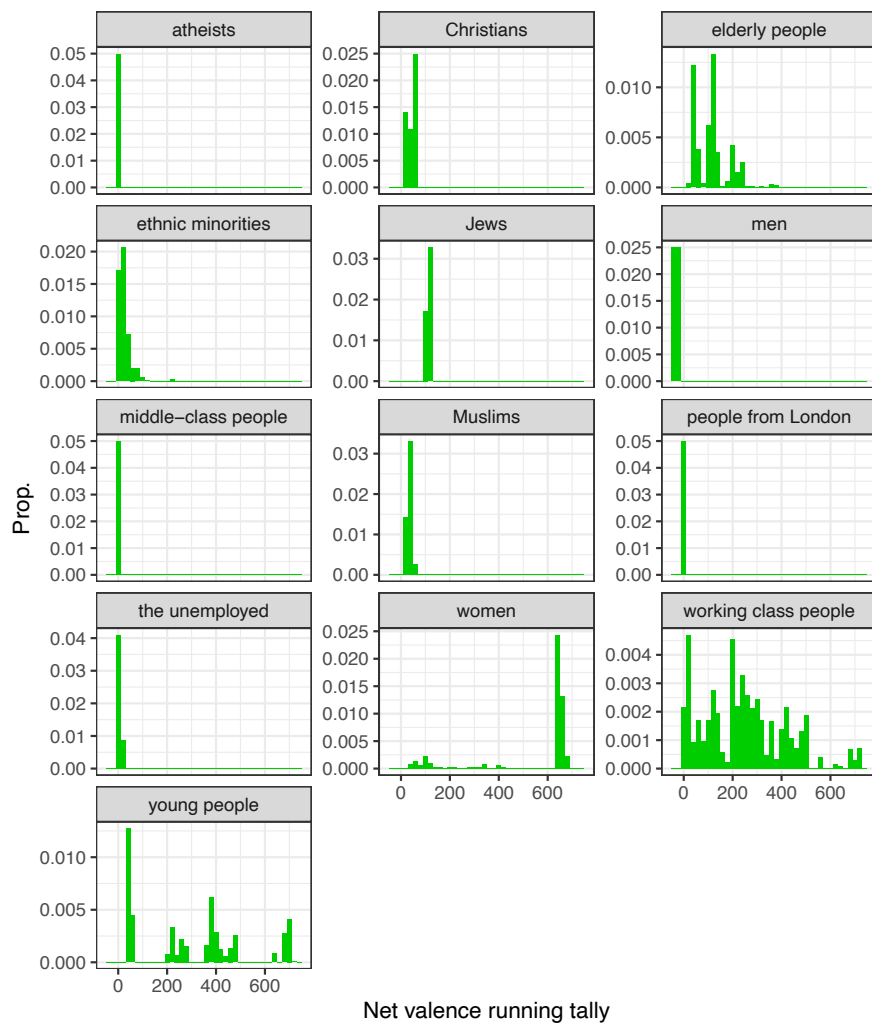
Figure 8.12. Bar chart of mention counts per dyad-year across the two datasets.



8.22 Distribution of running tallies by group

Figure 8.13 shows the distribution of net valence across a 30-day period separately for each group.

Figure 8.13. Distribution of net valence by group.



8.23 Descriptives on average valences of group appeals

Table 8.27 shows the estimates underlying Figure 7.3. Table 8.28 shows the estimates underlying Figure 7.4.

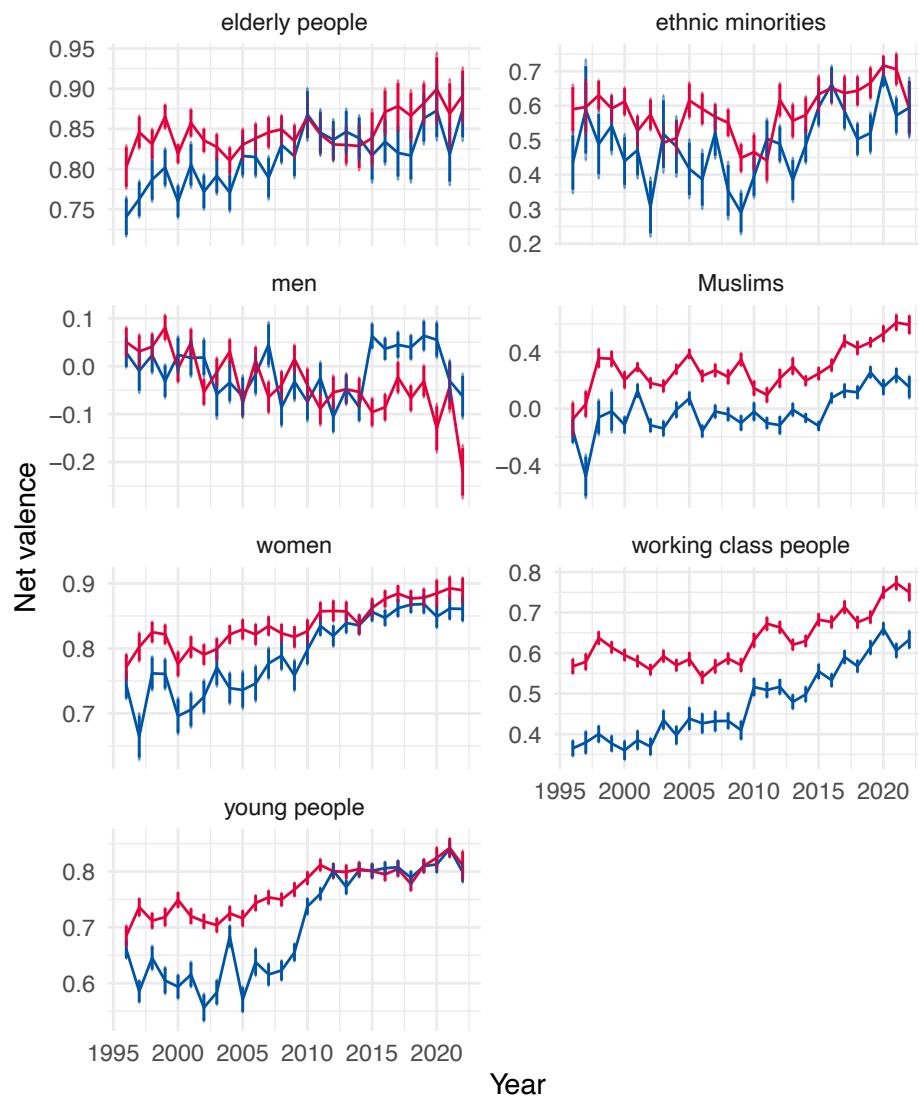
Table 8.27: Estimates from no-intercept regression of net valence on target group indicators.

	(1)
Atheists	0.349*** (0.024)
Christians	0.397*** (0.005)
Jews	0.715*** (0.005)
Muslims	0.175*** (0.003)
Working class people	0.582*** (0.001)
Middle-class people	0.499*** (0.011)
The unemployed	0.484*** (0.004)
Ethnic minorities	0.570*** (0.004)
Men	−0.013*** (0.002)
Women	0.835*** (0.001)
People from London	0.704*** (0.005)
Elderly people	0.824*** (0.002)
Young people	0.755*** (0.001)
N	538 863

In Figure 8.14 we show dyad-level net valence by year for the two major parties (Labour, shown in red, and Conservatives, shown in blue), and the seven largest groups

by size (all groups making up at least 2 pct. of all mentions).

Figure 8.14. Net valence by group over time, Labour (red) vs. Conservatives (blue). Each dot shows the year-specific average net valence of all group appeals for the party-group dyad. Error bars represent 95 pct. confidence intervals.



Like the group linkages shown in Figure 1 in the manuscript, group appeals exhibit a mix of change and stability. For example, while Labour consistently appeals more positively to working-class people and Muslims, the partisan gap for elderly people fluctuates, and appeals to men have seen considerable partisan polarization in recent years.

Table 8.28: Estimates from regressions of net valence on an indicator of Conservative party affiliation among all appeals to the group in question.

	Christ.	Jews	Musl.	Athe.	Eld.	Ethn. min.	Men	Mid- class	Lond.	Unemp.	Wom.	Work. class	Young
Cons.	-0.003 (0.015)	0.041*** (0.010)	-0.308*** (0.011)	-0.167* (0.070)	-0.033*** (0.002)	-0.079*** (0.009)	0.021** (0.007)	-0.060+ (0.036)	-0.094*** (0.012)	-0.079*** (0.011)	-0.021*** (0.002)	-0.129*** (0.003)	-0.022*** (0.002)
Intcpt.	0.401*** (0.011)	0.692*** (0.007)	0.304*** (0.008)	0.421*** (0.051)	0.841*** (0.002)	0.596*** (0.005)	-0.024** (0.005)	0.510*** (0.023)	0.746*** (0.007)	0.521*** (0.007)	0.842*** (0.001)	0.629*** (0.002)	0.766*** (0.001)
N	4521	5938	12723	195	48658	9342	24152	893	4262	6873	91709	94697	130722

8.24 Results from alternative specifications of the main model

Table 8.29 shows results from alternative model specifications. Model 1 shows the main specification from Table 7.1 without any fixed effects. Models 2-5 shows specifications with similar fixed effects as Table 7.1 but using only counts of group mentions as the independent variable, i.e. without accounting for valence. Model 6 shows the main specification from Table 7.1 estimated using ordered logit instead of OLS. Model 7 shows an alternative specification of the main effect which interacts counts of group mentions interacted with the average valence of group mentions. As shown, there is a positive interaction between the valence and number of appeals, indicating that increasing the number of appeals has a more positive effect when their valence is positive. (Note that the coefficient on 'average valence' is fictive since 'average valence' is undefined when 'number of appeals' equals 0.)

Table 8.29: Alternative estimates from regressing group linkages on net valence and counts of group mentions.

	Model 1	Model 2	Model 3	Model 4	Model 5	Model 6	Model 7
Net valence (sum)	0.044 (0.032)					0.004*** (0.000)	
Number of appeals	-0.042+ (0.023)	-0.011 (0.008)	-0.003 (0.011)	-0.003 (0.011)	-0.003 (0.007)	-0.003*** (0.000)	-0.060** (0.021)
Average valence							-14.439* (5.822)
Average valence X number of appeals							0.080** (0.029)
Intercept	55.285*** (2.019)	55.248*** (2.027)					
N	1 291 290	1 291 290	1 291 290	1 291 290	1 291 290	1 291 290	1 291 290
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group			✓	✓	✓	✓	✓
FE: Wave				✓	✓	✓	✓
FE: Party					✓	✓	✓

8.25 BESIP estimates with individual-level fixed effects

Table 8.30 presents estimates from various specifications of the model on the panel subset of the data. The difference from Table 7.1 is therefore solely in the inclusion of individual-level fixed effects.

Table 8.30: Estimates from regressing group linkages on group appeals.

	Model 1	Model 2	Model 3	Model 4
Net valence (sum)	0.147*** (0.040)	0.156*** (0.041)	0.167*** (0.046)	0.084* (0.035)
Number of appeals	−0.101*** (0.023)	−0.109*** (0.023)	−0.118*** (0.028)	−0.059* (0.024)
N	1 157 783	1 157 783	1 157 783	1 157 783
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓
FE: Wave			✓	✓
FE: Party				✓
FE: ID		✓	✓	✓

Models 1-4 in Table 8.30 differ in terms of the number and composition of fixed effects. The estimate of interest, *Net valence*, is in the top row. As shown, the coefficient on net valence is consistently positive and statistically significant. The coefficient is fairly robust in terms of magnitude, ranging between .08 and .17 across specifications even as individual-level fixed effects are added.

8.26 Varying exposure windows

Figure 8.15 shows estimated coefficients on net valence from our main specification (including group, party and wave fixed effects) across varying window sizes: 60 days, 75 days, 90 days (as in our main results), 105 days, and 120 days. Table 8.31 shows the underlying regression estimates.

Figure 8.15. Estimates from regressing group linkages on net valences based on 2, 2.5, 3, 3.5, and 4 month exposure windows.

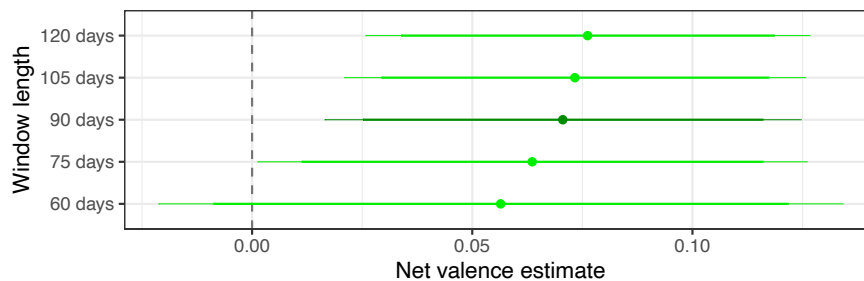


Table 8.31: Estimates from regressing group linkages on group appeals, varying exposure window length.

	60 days	75 days	90 days	105 days	120 days
Net valence (sum)	0.056 (0.040)	0.064* (0.032)	0.071* (0.028)	0.073** (0.027)	0.076** (0.026)
Number of appeals	-0.044+ (0.025)	-0.050* (0.022)	-0.053* (0.020)	-0.056** (0.020)	-0.061** (0.019)
N	1 325 239	1 325 239	1 325 239	1 325 239	1 325 239
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓	✓
FE: Wave	✓	✓	✓	✓	✓
FE: Party	✓	✓	✓	✓	✓

8.27 Heterogeneity across group types

Table 8.32 shows the estimates underlying Figure 7.6a. Table 8.33 shows the estimates underlying Figure 7.6b.

Table 8.32: Estimates from regressing group linkages on group appeals, by group type.

	Religious	Class	Age	Gender	All
Net valence (sum)	1.577** (0.295)	0.134** (0.050)	0.126 (0.084)	−0.020 (0.079)	0.087* (0.036)
Number of appeals	−0.845*** (0.141)	−0.082* (0.033)	−0.139+ (0.071)	0.014 (0.074)	−0.060** (0.023)
N	33 322	840 467	139 403	36 980	1 285 515
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓	✓
FE: Wave	✓	✓	✓	✓	✓
FE: Party	✓	✓	✓	✓	✓

Figure 8.16. Estimates from regressing group linkages on net valences leaving out one dyad at a time.

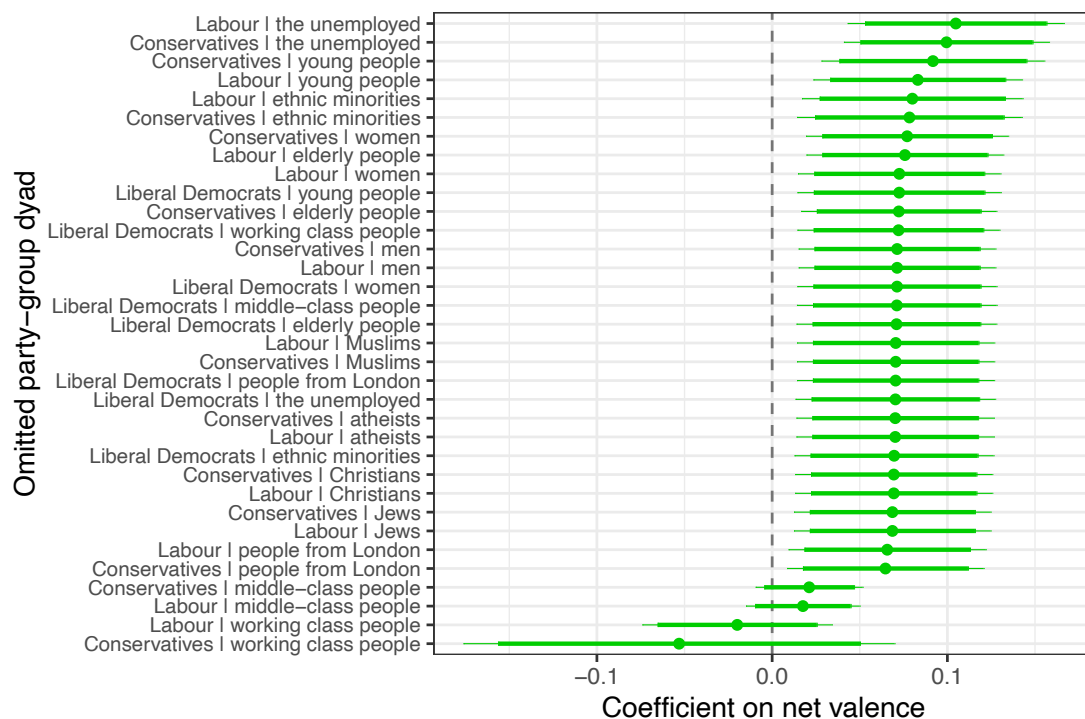


Table 8.33: Estimates from regressing group linkages on group appeals, omitting each group.

	Atheists	Christ.	Jews	Muslims	Middle class	Working class	Unemployed	Young	Elderly	Men	Women	Blacks & Asians	Loners	All
NV (sum)	0.070* (0.028)	0.069* (0.028)	0.069* (0.028)	0.071* (0.028)	0.020 (0.016)	0.145 (0.182)	0.106*** (0.031)	0.094** (0.033)	0.076** (0.028)	0.071* (0.028)	0.081** (0.030)	0.078* (0.034)	0.065* (0.028)	0.087* (0.036)
N of app's	-0.053* (0.021)	-0.052* (0.020)	-0.052* (0.020)	-0.053* (0.020)	-0.018 (0.012)	-0.141 (0.157)	-0.077*** (0.022)	-0.064** (0.022)	-0.055** (0.021)	-0.054** (0.020)	-0.058** (0.021)	-0.059* (0.026)	-0.049* (0.021)	-0.060** (0.023)
N	1 317 343	1 316 839	1 316 664	1 316 788	1 024 591	1 020 484	1 090 175	1 263 583	1 247 492	1 313 473	1 300 025	1 110 409	1 265 002	1 285 515
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE: Wave	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓
FE: Party	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓	✓

8.28 The moderating role of policy

Table 8.34 shows the estimates underlying Figure 7.7.

Table 8.34: Estimates from regressing group linkages on group appeals, by explicit policy mention.

	All appeals	Appeals without policy	Appeals with policy
Net valence (sum)	0.071* (0.028)	0.069* (0.029)	1.003** (0.326)
Number of appeals	−0.053* (0.020)	−0.052* (0.022)	−0.758** (0.268)
N	1 325 239	1 325 239	1 325 239
Std.Errors	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓
FE: Wave	✓	✓	✓
FE: Party	✓	✓	✓

8.29 Measuring explicit references to policy

Explicit policy mention codebook: bill*, legislat*, policy*, polici*, law*, amendment*

Table 8.35: Example sentences with and without explicit policy mentions

Appeals with explicit policy mention	Appeals without explicit policy mention
That provision, which is discriminatory and applies only to Roman Catholics, is wrong and should be removed from our statute law.	Our young men in particular are missing the boat, and the proportion of young males going to university is lower than it was in 1999.
Because of the Government's slash and burn policy, 70% of councils are having to cut social care, leaving old people to choose between help with washing and help with eating.	It is extraordinarily difficult for the elderly or the injured to get in and out of their home.
Reimbursement and other proposals in the Bill will lead to increased quality and choice for older people.	Tens of thousands of Catholics in Wales are in mourning, and we stand in sympathy and support with them.
All politicians talk about giving older people dignity and security, and a nationwide policy on pets could help lift one particular burden from many older people.	The honorable Gentleman says that he does not care whether 10% or 90% of MPs are women, but I care.
The Bill is not about men versus women, but about true equality between men and women, and I therefore commend it to the House.	One of my key ambitions in my new role is to raise the status of social workers in our society.

Appeals with explicit policy mention	Appeals without explicit policy mention
We have also invested in programmes and policies to respond to the specific needs of black, Asian, and minority ethnic groups—for example, through outreach programmes to help economically inactive minority ethnic women into work.	I know that there is concern about the potential, random industrialisation of the countryside.
The changes in the Bill will support the achievements of those young people from difficult backgrounds, such as those with special educational needs or disability.	That is the issue that the public and Jewish people have.
A 2019 report by the Women and Equalities Committee recognised that Gypsy, Roma, and Traveller communities are one of the most persecuted groups in Europe, yet the Government seek literally to persecute them further through the Bill.	I have no difficulty in reiterating, and joining the honorable Gentleman in acknowledging, the vast sacrifices made by the men of Ulster—it was men—during the first world war.

8.30 Accounting for changes in party policy

To account for changes in party policy in the period under study, we use the V-Party dataset (Lindberg et al., 2022). The dataset includes expert-coded election-year assessments of party policy positions for the full range of parties and election years in our data (Pemstein et al., 2018). Several of the measured party positions more or less directly concern some of the groups we study, allowing us to capture changing policies for selected party-group dyads.

Table 8.36: V-Party survey items on party position and the groups they cover.

Policy position	Covered group(s)	Item text
Economic left-right	Working class people, middle class people, the unemployed	Please locate the party in terms of its overall ideological stance on economic issues. Clarification: Parties on the economic left want government to play an active role in the economy. This includes higher taxes, more regulation and government spending and a more generous welfare state. Parties on the economic right emphasize a reduced economic role for government: privatization, lower taxes, less regulation, less government spending, and a leaner welfare state. (7-point scale)
Welfare	Working class people, middle class people, the unemployed	To what extent does the party promote means-tested or universalistic welfare policies? (6-point scale)
Working women	Women	To what extent does this party support the equal participation of women in the labor market? Clarification: Measures that support the equal participation of women in the labor market include - but are not limited to - legal provisions on equal treatment and pay, parental leave and financial support for child care. (5-point scale)
Cultural superiority	Ethnic minorities	To what extent does the party leadership promote the cultural superiority of a specific social group or the nation as a whole? Clarification: This question refers to key non-economic cleavages in society, which could, for example, be based on caste, ethnicity, language, race, region, religion, or some combination thereof. This question further refers to cultural issues related to the national history and identity of a country. This question does not pertain to social groups based on gender or sexual orientation. (5-point scale)

Specifically, we merge four party policy variables from V-Party into our dataset: one on overall economic left-right position, one on welfare, one on women, and one

on cultural superiority. These allow us to run versions of our main analyses that are subset to these group types while controlling for changing party policy with respect to the group. See Table 8.36 for an overview of these items. Note that some items are a better fit for the group than others; in particular, the ‘cultural superiority’ item is only partially about policy towards ethnic minorities.

We run our main regression specification for each row in Table 8.36, subsetting to the group(s) in the second column with and without controlling for the corresponding policy item in the first column. The results are shown in Table 8.37. As shown, point estimates remain significant when controlling for changes in party position and barely change . This also holds when we re-run our main model on all groups with all four policy position variables included. The only exception is for ethnic minority groups where a null estimate becomes a significant negative.

Table 8.37: Estimates from regressing group linkages on net valence and counts of group mentions, controlling for relevant dimension of party policy position.

	Class	Class	Women	Women	Ethn. min.	Ethn. min.	All	All
Net valence (sum)	0.137** (0.051)	0.142** (0.052)	0.132* (0.049)	0.190*** (0.034)	-0.018 (0.040)	-0.179* (0.069)	0.071* (0.029)	0.076* (0.031)
Number of appeals	-0.081* (0.034)	-0.086* (0.036)	-0.135** (0.039)	-0.191*** (0.028)	0.005 (0.031)	0.113* (0.049)	-0.052* (0.021)	-0.057* (0.023)
Party: economy		-8.343 (9.980)						-3.644 (6.634)
Party: welfare		-19.963 (20.984)						-15.657 (13.666)
Party: women				-9.693** (2.289)				5.085 (6.484)
Party: cult. sup.						-6.225* (2.874)		-12.849 (13.390)
N	813 866	813 866	25 167	25 167	208 908	208 908	1 291 290	1 291 290
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: group	✓	✓	✓	✓	✓	✓	✓	✓
FE: wave	✓	✓	✓	✓	✓	✓	✓	✓
FE: party	✓	✓	✓	✓	✓	✓	✓	✓

8.31 The moderating role of news consumption

To measure respondents' consumption of news media, we use repeated survey items from BESIP asking respondents in 12 waves: "During the last seven days, on average how much time (if any) have you spent per day following news about politics or current affairs from each of these sources?" for each of the following: television (TV), newspapers (including online), radio, internet (not including online newspapers), talking to other people. The response scale has five steps: "None, no time at all", "Less than 1/2 hour", "1/2 hour to 1 hour", "1 to 2 hours", "More than 2 hours" and "Don't know". While these variables capture very recent news consumption (the past week), we use them as a proxy for periods in which respondents are more attuned to the news. We focus just on the distinction between the consumption of any news and no news rather than the more granular time estimates, which are both more susceptible to recall bias and likely fluctuate substantially week-on-week over a 3-month period and thus would introduce excessive noise in our proxy measure.

To construct moderating variables, we focus on TV, newspapers and radio, which are the sources in which media reporting from parliamentary speeches can occur (once online newspapers are excluded from the 'internet' category). Thus, it is the consumption of these sources that we would expect to moderate the main effect of group appeals in parliamentary speeches. To distinguish those who currently consume news from those who do not, we construct binary indicator variables for each source, as well as a joint indicator variable for any news consumption across all three sources. We then interact each of these variables with our net valence variable in a panel model. We can thus estimate whether individuals are more responsive to group appeals in parliamentary speeches in periods where they follow the news. Note that there is only common coverage between the dependent variable and the media consumption variables in four waves (from 2017-2020), which limits the generalizability of the results.

Figure 7.8 in the main text shows estimated coefficients on net valence from our panel model specification (including group, party, wave and individual fixed effects) interacted with each news consumption indicator. Table 8.38 shows the underlying regression estimates. As shown, the coefficient on net valence is significantly larger in periods where respondents consume news compared to periods where they do not. The difference is substantial at around 12 percent of the baseline panel estimate in

Table 8.30 (although note that this interaction model is run only on a subset of the panel model's data).

Table 8.38: Estimates from regressing group linkages on group appeals, moderated by recent news consumption.

	TV	Newspapers	Radio	Any news source
Net valence (sum)	0.275 (0.167)	0.281 (0.168)	0.282 (0.168)	0.273 (0.167)
Number of appeals	-0.191 (0.120)	-0.192 (0.120)	-0.192 (0.120)	-0.191 (0.120)
Consume news	-0.222 (0.712)	-0.474 (0.605)	0.463 (0.414)	0.142 (0.740)
Net valence X consume news	0.009* (0.004)	0.005 (0.003)	0.003** (0.001)	0.010** (0.003)
Num.Obs.	276 723	276 993	277 847	273 911
Std.Errors	dyad-wave	dyad-wave	dyad-wave	dyad-wave
FE: Group	✓	✓	✓	✓
FE: Wave	✓	✓	✓	✓
FE: Party	✓	✓	✓	✓
FE: ID	✓	✓	✓	✓

Appendix (Chapter 9)

8.32 Simulations of policy-sensitivity

Figure 8.1 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:

$$\begin{aligned}\text{policy} &\sim \mathcal{N}(0, \delta_s) \\ \text{residual} &\sim \mathcal{N}(0, \delta_n)\end{aligned}\tag{8.3}$$

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

$$\text{residual strength} = \frac{\delta_n}{\delta_s + \delta_n}\tag{8.4}$$

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 8.1 by varying the β weights as follows:

$$\begin{aligned}
\text{Kramer } (\beta_s > 0, \beta_n = 0) & \begin{cases} 1, & \text{if policy} > 0 \\ 0, & \text{otherwise} \end{cases} \\
\text{Fiorina } (\beta_s = \beta_n > 0) & \begin{cases} 1, & \text{if policy} + \text{residual} > 0 \\ 0, & \text{otherwise} \end{cases} \\
\beta_s = 2 \times \beta_n > 0 & \begin{cases} 1, & \text{if } 2 \times \text{policy} + \text{residual} > 0 \\ 0, & \text{otherwise} \end{cases} \\
\beta_s = 5 \times \beta_n > 0 & \begin{cases} 1, & \text{if } 5 \times \text{policy} + \text{residual} > 0 \\ 0, & \text{otherwise} \end{cases} \\
\beta_s = 10 \times \beta_n > 0 & \begin{cases} 1, & \text{if } 10 \times \text{policy} + \text{residual} > 0 \\ 0, & \text{otherwise} \end{cases}
\end{aligned}$$

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.

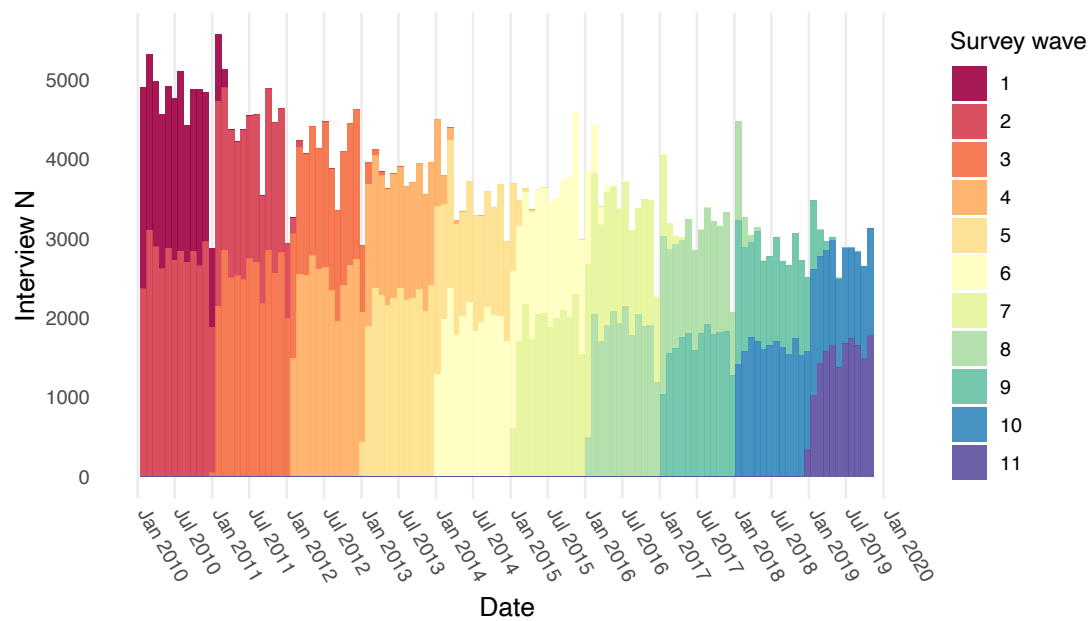
8.33 Timing of budget statements

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 (House of Commons Library, 2015; HM Treasury, 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).

8.34 Timing of UKHLS survey waves

Figure 8.17. How survey respondents are distributed across waves of the UKHLS.



8.35 Validation of standard pocketbook voting relationships

Figure 8.18. 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 8.5 with household-clustered standard errors.

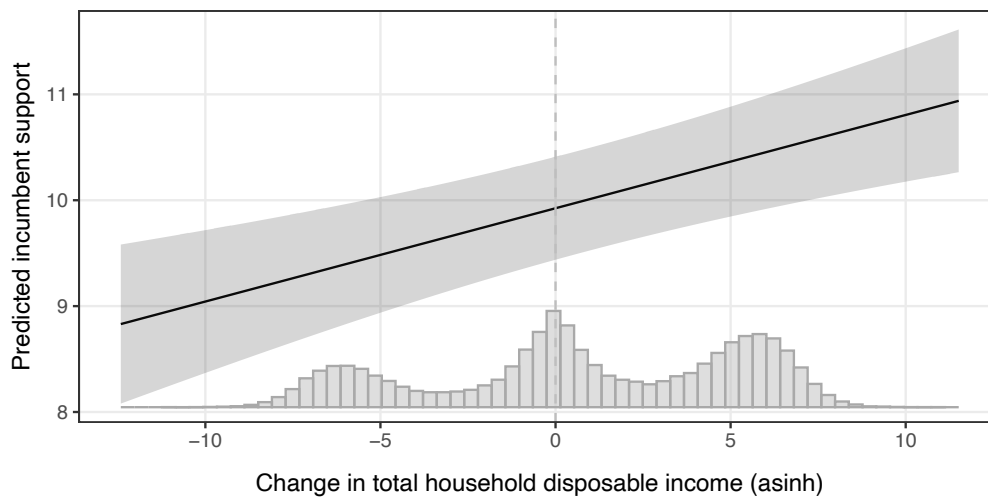


Table 8.39: Estimates from regressing incumbent support on pocketbook evaluations and calculated total income changes.

	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.

8.36 Model specifications

To estimate how decomposed pocketbook changes affect incumbent support, I estimate two kinds of models: lagged dependent variable models (Equation 8.5) and two-way fixed effects models (Equation 8.6) as shown below.

$$v_{it} = \beta_p \Delta y_{it}^{policy} + \beta_t \Delta y_{it}^{total} + v_{it-1} + \boldsymbol{\mu}_{it} + \varepsilon_{it} \quad (8.5)$$

$$v_{it} = \beta_p \Delta y_{it}^{policy} + \beta_t \Delta y_{it}^{total} + \boldsymbol{\mu}_{it} + \alpha_i + \eta_t + \varepsilon_{it} \quad (8.6)$$

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. $\boldsymbol{\mu}_{it}$ 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).¹⁹ 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 8.38).

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 8.38). 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

¹⁹UKHLS 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.

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 8.6 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 when they come from policy. In the more complex model specifications in Equations 8.6 and 8.5 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.

8.37 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 8.40: 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 8.40. 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.

8.38 Robustness to varying model specifications

Table 8.41 shows results of regressing incumbent support on decomposed disposable income changes across a variety of models that deviate from the main specifications in Table 8.1. 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 8.1 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 8.41: 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	70 798	70 798	153 214	148 709	153 214	71 757	70 677
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 8.42 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, Neundorff 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 8.42: 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	70 798	70 798	45 390	44 125	16 701
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.

8.39 Effects by governing period

Table 8.43 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 8.43: 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.

8.40 Heterogeneous effects

Figure 8.19. Coefficients from LDV regression of incumbent support on total disposable income changes (inverse hyperbolic sine transformation) based on the model specification in Equation 8.5. Black estimates are coefficients on Δy^{signal} , grey estimates are coefficients on Δy^{noise} . ‘Delta signal valence’ captures whether the policy-induced income shock for a given observation is positive or negative.

