Revealing Zero-sum Thinking in the United Kingdom*

Lucinda Platt Imran Rasul Pratyush Tiwari

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Abstract

Preferences over economic policies are conditioned by the extent to which individuals understand the world in zero-sum terms. Zero-sum thinking (ZST) is the belief that gains for one individual or group necessarily imply corresponding losses for others. We study the prevalence and triggers for ZST among the majority White British population in the UK, using a survey experiment administered to 3200 individuals, fielded in the run up to the high-stakes UK General Election in July 2024. Our experiment presents respondents with qualitative narratives either about the economic success or disadvantage of specific minority groups. We measure how each narrative shifts perceptions of the economic circumstances of minorities, and of the White British ingroup. By examining treatment effects of the narratives on perceptions of the ingroup, our design provides a novel revealed measure of ZST. Estimating conditional average treatment effects, we document that just over 20% of respondents are revealed as ZSTs from either narrative. However, each narrative reveals a distinct type of zero-sum thinker. Those identified through the narrative of minority success hold similar policy preferences to non-ZSTs. In contrast, those identified through the narrative of minority disadvantage are significantly less supportive of pro-poor policies in general, and policies to reduce ethnic inequalities in particular. We show this is not because ZSTs and non-ZSTs differ in how they perceive the economic outcomes of minorities, opportunities available to minorities, or root causes of minority disadvantage. Our results illustrate how ZST can be triggered by light-touch narratives typical of transient information exposure in daily life, but consequences for redistributive preferences depend on the nature of narratives individuals are exposed to. *JEL: J15, P00*.

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1 Introduction

Individuals' views on who wins and loses from policies is consequential for their support for economic policies. A growing body of work has emphasized that zero-sum thinking can help explain policy preferences, where zero-sum thinking (ZST) is the belief that gains for one individual or group necessarily come at the expense of others. Political debates have long been framed in zero-sum terms by actors across the political spectrum. For example, issues spanning borders (trade, immigration), vertical inequalities (wealth, income, or class) or horizontal inequalities (along rural-urban or ethnic divides) are often described in terms of winners and losers. Moreover, recent work suggests zero-sum thinking can remain relevant in non zero-sum choices, even when a policy is widely regarded to benefit voters on average [Johnson *et al.* 2021, Ali *et al.* 2025].[1]

Research on ZST has developed along two lines. A first strand documents origins of zero-sum mindsets, such as exposure to populist rhetoric or ancestral experiences [Ajzenman *et al.* 2026, Chinoy *et al.* 2026]. A second strand examines consequences of zero-sum mindsets: they reduce cooperation and trust, they suppress productive effort in close-knit communities, and they give rise to policy preferences mitigating against win-win policies [Różycka-Tran *et al.* 2015, 2019, Gavrilets and Seabright 2025, Bergeron *et al.* 2026, Chinoy *et al.* 2026].[2]

We build on both strands to open a new line of enquiry, getting inside the black box of what triggers zero-sum thinking, and hence the extent to which such mindsets are malleable and responsive to information, or whether they reflect deep-rooted worldviews. We study the issue in the UK, using an online survey experiment administered to a representative sample of the adult British population. We study zero-sum mindsets through the lens of a specific dimension of horizontal inequality: that between ethnic groups. Horizontal inequalities across ethnic groups consistently rank as a leading concern in countries with long histories of immigration [Clark and D'Ambrosio 2015, Benson *et al.* 2024, Fairness Foundation 2024].

Our survey collects information on perceptions of the majority White British population of the economic circumstances of minority groups, as well as the circumstances of their White British ingroup. The experimental treatments present respondents with qualitative narratives either about the economic success or disadvantage of minority groups, providing examples of specific groups affected, and thereby positioning the White British ingroup as either a relative winner or loser, in comparison to the named minority groups. By examining treatment effects on White British respondent's perceptions of the economic circumstances of the ingroup when presented with such

[1] The idea of zero-sum thinking stems from anthropologist George Foster, who argued that in pre-industrial societies with fixed resources, scarcity led to the perception that an 'individual or a family can improve their position only at the expense of others' [Foster 1965, p297]. Work across the social sciences has documented such mindsets are relevant for contemporary Western societies [Różycka-Tran *et al.* 2015, 2019, Chinoy *et al.* 2026].

[2] These patterns are relatively similar across countries. Różycka-Tran *et al.* [2015, 2019] document in a 36 country study that zero-sum beliefs are associated with worse country-level outcomes such as GDP, civil liberty, pluralism and government functioning, and at the individual level, zero-sum beliefs are associated with lower life satisfaction, and lower positive emotion.

narratives about outgroups, we reveal zero-sum mindsets among the majority. That is, if other groups are seen to be doing better or worse, do perceptions of the standing of the White British ingroup shift in the opposite direction?

Our design provides a novel revealed measure of zero-sum thinking among the majority White British population. This documents not only the prevalence of ZST, but also reveals different triggers for ZST, identified in response to narratives either about the economic success or disadvantage of minorities. The core of our analysis documents, for each type of revealed ZST, how their characteristics, political alignment, policy preferences, perceptions of the economic circumstances of minorities and their ingroup, differ to non-ZSTs.

Figure A1 details the ethnic composition of the UK in the 2021 census. 74% of the population identified as White British. We study the views of this group towards the largest minority groups: Indians, Pakistanis, and Black Caribbeans. These cover long-settled groups in the UK.[3]

While there is often an assumption of minorities being economically disadvantaged, the varying histories of groups in terms of their origins, patterns of settlement and access to resources, means there is substantial variation in outcomes across them in the UK today [Mirza and Warwick 2024]. For example, children from most minority groups now outperform the majority in educational attainment, and rates of entry into universities exceed those of the majority [Britton *et al.* 2021]. Some, such as Indians, have seen marked improvements in labor market outcomes through occupational mobility across generations and high-skilled recent immigration [Zuccotti and Platt 2023]. At the same time, for many groups persistent ethnic gaps remain in employment and wealth accumulation [Mirza and Warwick 2024]. This makes the UK an excellent context in which to study how narratives about the economic success or disadvantage of specific minorities shape perceptions of the majority towards those minorities, and towards their ingroup.

Our survey was administered online by *YouGov* to a nationally representative adult sample, of whom we focus on the 3205 White British respondents. It was purposefully fielded in the immediate run up to the UK General Election in July 2024. That high stakes electoral campaign saw the rise of populist parties, with immigration ranked by the electorate as the most pressing issue [IPSOS 2024]. Political messaging during the campaign provides a set of background primes that all respondents could have been exposed to. We examine whether in this charged atmosphere, there remains potential to shift perceptions of minorities and the ingroup through the provision of light-touch narratives, and whether this reveals zero-sum thinking among the majority.

Our experiment randomly assigned respondents to one of three arms: (i) a control group; (ii) a treatment arm in which they are informed of the successful economic circumstances of minorities, being told: *Ethnic minorities in the UK are performing well in education. In most cases, they*

[3]The UK Office for National Statistics provides the standard question and categories for ethnic groups. These mix racial and national information to classify minority groups based on the most common origins. Ethnic minority groups comprise both UK and foreign-born individuals who select one of the groups other than White British. Poles are not separately classified in the official categories shown in Figure A1. However, they constitute the largest foreign-born group in the UK after Indians, making up a large share of the Other White group.

are getting more/higher qualifications than White British people, even when they come from poor backgrounds. This is particularly true for Indians and also for Pakistanis; (iii) a treatment arm in which they are informed of the disadvantaged economic circumstances of minorities, being told: *Ethnic minorities in the UK face higher unemployment rates than the White British majority. Even among those with similar levels of education, the chances of being unemployed are still higher in most cases. This is particularly true for Black Caribbeans and also for Pakistanis.*

Our survey was structured to ask respondents their perceptions of the economic outcomes, opportunities, and root causes of disadvantage across minority outgroups and their ingroup, and views towards various redistributive and compensatory policies.

On perceptions of economic outcomes across groups, our findings line up with a body of work showing that misperceptions of others are widespread and asymmetric. That is, they are concentrated on one side relative to the truth, and are smaller with regard to the ingroup [Bursztyn and Yang 2022]. We find that perceptions of outgroups can be shifted by the narrative treatments, but the treatments do not shift perceptions of the economic outcomes of the ingroup, which were relatively accurate to begin with.

On perceived opportunities, we examine dimensions linked to the narrative wording: (i) obtaining a good education; (ii) finding work; (iii) earning a decent wage. There is a widespread belief that each minority group lacks opportunities in all three dimensions, but the treatments have little impact on the perceptions the majority hold about opportunities for minorities. However, the most striking pattern that emerges is in views about whether the White British ingroup lack opportunities.

Respondents assigned to the narrative that informs them of the relative educational success of Indian and Pakistanis are significantly more likely to report that the White British ingroup lack opportunities in finding work or earning a decent wage. These responses are large, corresponding to 36% and 27% increases over controls respectively. This reveals zero-sum thinking: that positive outcomes for outgroups might come at the expense of opportunities for the ingroup.

Those assigned to the narrative that informs them of the relative disadvantage in unemployment for Pakistani and Black Caribbean groups are significantly less likely to report that White British individuals lack opportunities in obtaining a good education, the magnitude of the effect corresponding to a 39% decrease. This again reveals a zero-sum mindset: if minorities at the same level of education are faring worse, on average majority respondents become less concerned about educational opportunities available to their ingroup.

We also find evidence of zero-sum thinking when respondents are asked to consider whether labor market discrimination is another cause of disadvantage. Among White British controls, the ranking across groups of the discrimination they are perceived to face matches perceptions of their outcomes and opportunities: Black Caribbeans are perceived to face the most discrimination, followed by Pakistanis and then Indians. The narrative treatments shift these views in the expected direction. However, the largest impact is again found for perceptions towards the

ingroup. Specifically, the narrative highlighting minorities are doing well in education, increases the perception that White British men and women face labor market discrimination, in line with zero-sum thinking.

This first part of analysis reveals zero-sum thinking, on average, among White British respondents, triggered through exposure to both narrative treatments. However, these average effects likely mask substantial heterogeneity across individuals. To explore this, we employ causal forests to estimate individual-level conditional average treatment effects (CATEs) [Athey *et al.* 2019]. We use the CATE estimates to classify individuals who exhibit the strongest zero-sum responses when exposed to information about minority success or disadvantage.

We first document that just over 20% of respondents are classified as zero-sum thinkers from each narrative. We then examine whether individuals revealed to be ZSTs from a given narrative are also predicted to have been identified as a ZST had they been exposed to the other narrative. This sheds light on whether ZST is triggered in response to specific narratives, or whether ZST is more deep-rooted and would be triggered irrespective of which narrative of minority success or disadvantage individuals are exposed to. We find that 7.6% of respondents are classified as a ZST under both narratives, far from the full overlap that would be implied if there were a single underlying trait triggering ZST irrespective of the narrative exposed to.

We then predict, among controls, who is identified as a ZST from each treatment (and who would have been identified as a non-ZST in each treatment). It is this group of predicted ZSTs/non-ZSTs our second set of results analyze. For each type of revealed ZST, we document how their characteristics, political alignment, policy preferences, and perceptions of the economic circumstances of minorities and their ingroup, differ to non-ZSTs.

Irrespective of which narrative they are revealed from, ZSTs are older, lower income, and less likely to be left-leaning. However, ZSTs revealed from each narrative also differ from each other on other margins: relative to those revealed as ZSTs through the narrative of minority success, ZSTs revealed through the narrative of minority disadvantage are less likely to be university educated, less likely to be married, more likely to self-report as right-leaning, reside in areas with a higher share of minorities, and they exhibit less concern for horizontal inequalities between groups, less concern for vertical inequalities, and lower levels of concern for inequalities between ethnic groups.

On political alignment, we consider how ZSTs differ from non-ZSTs using two measures not used in the identification of ZSTs from the causal forest underlying the CATEs. The first measures shifting political party allegiances, constructed from which party respondents voted for in the 2019 General Election and the party they intend to vote for in the 2024 General Election. We classify respondents as having shifted left, shifted right, or remaining unchanged in their political party alignment. Swing voters, critical for outcomes in democracies, are likely among those with shifting party alignment. We find those who have shifted left are overrepresented among those identified as ZST from the narrative of success ($p = .008$). In contrast, those who have shifted right are overrepresented among ZSTs identified from both the narrative of minority success ($p = .054$) and

the narrative of minority disadvantage ($p = .034$).

Our second measure considers respondents' behavior during the Brexit referendum in the UK, perhaps the quintessential zero-sum versus positive sum political debate in recent times in the UK. The successful campaign to leave the EU implied resources sent to the EU were a loss for Britain – a narrative of zero-sum thinking, while the Remain campaign emphasized the mutual benefits of the UK remaining part of the EU – a win-win narrative. We find that ZSTs identified from both narratives are more likely to have voted for the UK to leave the EU.

On policy preferences, we consider how the views of ZSTs differ from non-ZSTs in relation to untargeted pro-poor redistributive policies in general, and policies to reduce ethnic inequalities in particular – all outcomes not used in the identification of ZSTs from the causal forest. The link between ZST and support for redistribution is *ex ante* unclear because how ZST shapes policy views depends on whether: (i) an individual perceives themselves to belong to the group that gains or loses from any zero-sum dynamic; (ii) they normatively believe that zero-sum outcomes should be corrected for or not (irrespective of whether they belong to the winning or losing group). As Chinoy *et al.* [2026] discuss, if an individual perceives themselves as belonging to the gaining group, then pure self-interest motives imply they will not support redistribution. On the other hand, ZSTs can be more likely to support redistributive policies if they want to correct for negative externalities or forms of unfairness that drive the zero-sum dynamic.

We find that ZSTs identified through the narrative of minority disadvantage are significantly less supportive than non-ZSTs of pro-poor redistributive policies in general, and policies to reduce ethnic inequalities in particular. In contrast, the gap in policy views of ZSTs identified through the narrative of minority success and non-ZSTs is far more muted. These results are consistent with self-interested motives dominating among ZSTs identified through the narrative of minority disadvantage. Our results help rationalize why certain groups that stand to gain economically from government redistribution – such as lower educated or lower income groups – can oppose such policies and be seen not to vote in their own economic best interests. More broadly, by better understanding the link between ZST and narratives of ethnic inequality, our results help resolve apparent anomalies between recognition of inequalities and resistance to those policy choices that may address them [Schaeffer *et al.* 2024].

One explanation for these differing policy views between ZSTs and others is that they differ in their perceptions of outgroups. We thus circle back to the earlier analysis to consider how ZSTs view the economic outcomes, opportunities, and root causes of disadvantage for outgroups, and their ingroup. This sheds light directly on whether the lack of support for pro-poor redistributive policies and policies to reduce ethnic inequalities, stems from differential (mis)perceptions of others. Overall, the results highlight that ZSTs and non-ZSTs actually have very similar perceptions of the economic outcomes of outgroups, the opportunities available to them, and the root causes of their disadvantage. In other words, although ZST can be triggered by narratives about outgroups, it is not that those more inclined to zero-sum thinking differ much from others in their perceptions

of the economic circumstances of outgroups, or their ingroup.

At the heart of our analysis lies the novel design that reveals zero-sum thinking through a treatment-response pattern. This is in contrast to standard approaches of measuring zero-sum thinking in psychology and economics through attitudinal scales – examples of which are in Table A1. Rather than identifying ZST as a single character trait, our approach reveals distinct types of ZSTs, depending on whether they are revealed through narratives of minority success or disadvantage. When describing our results, we emphasize the commonality and differences in findings with the earlier literature. These differences can arise from: (i) methodological differences from our revealed approach to identify ZST; (ii) differences across study contexts.

To distinguish these explanations, we use the British Election Study (BES), that fields a battery of items from zero-sum measurement scales similar to those used in the earlier literature, to a nationally representative UK sample. We use this to: (i) establish differences in characteristics of ZSTs as measured from the BES to those revealed through our survey experiment – so understanding the role of different methodological approaches to identify zero-sum mindsets in the UK context; (ii) examine correlates of zero-sum thinking in the UK against the US by comparing results from the BES to similar measures of ZSTs in Chinoy *et al.* [2026] – so understanding differences across UK and US contexts using a similar approach to identify zero-sum mindsets.

We provide three core contributions to the literature.

The first is methodological as we reveal ZST through the design of our survey experiment. This enables us to measure the prevalence of ZST, but also reveals what triggers ZST through majority perceptions of their ingroup shifting in response to narratives about minority success or disadvantage. Our design reveals different types of individual susceptible to ZST, and is less subject to experimenter demand and social desirability bias concerns that might arise from questions asked in standard scales to measure zero-sum thinking (Table A1). Our results illustrate how ZST can be triggered by light-touch interventions typical of transient information exposure in daily life, even against a backdrop of already heightened political debate.

Second, we build on work documenting the origins of zero-sum mindsets [Ajzenman *et al.* 2026, Chinoy *et al.* 2026], and consequences of such mindsets, where work in psychology has stressed the association between ZST and psychological traits, cognition and views of the world [Różycka 2008, Różycka and Wojciszke 2009, Wojciszke *et al.* 2009, Różycka-Tran *et al.* 2019], while work in economics has studied how ZST impacts cooperation, trust, effort and policy preferences [Gavrilets and Seabright 2025, Balietti and Marino-Fages 2026, Bergeron *et al.* 2026, Chinoy *et al.* 2026]. Our work helps understand correlations between ZST and policy preferences in light of how such mindsets are triggered. We do so in the UK context, where experimental evidence on ZST remains scarce [Andrews Fearon *et al.* 2021, Balietti and Marino-Fages 2026].[4]

[4] Andrews Fearon *et al.* [2021] present evidence from 3297 participants from the UK and US examining the link between zero-sum thinking and distrust in democratic institutions, finding ZST undermines trust and cooperation. Balietti and Marino-Fages [2026] use a survey experiment on 2100 participants to understand how zero-sum

Third, we bridge between the nascent literature on zero-sum thinking and a longer-standing literature using survey experiments to understand attitudes toward racial/ethnic inequalities [Kuklinski *et al.* 1997, Haaland and Roth 2023, Alesina *et al.* 2024]. Our multi-group and multi-domain design advances this literature by considering: (i) majority perceptions towards multiple minority groups and towards their ingroup, where the outgroups considered have a long-standing presence in the UK and have a high share of UK-born individuals; (ii) perceptions along interlinked dimensions of the economic outcomes of minorities, opportunities available to them, root causes of their disadvantage, and views of policies to address these horizontal inequalities. We thus move beyond the study of perceptions of others along a single divide such as Black-White, partisan, native-immigrant or native-Muslim [Haaland and Roth 2023, Alesina *et al.* 2024, Schaeffer *et al.* 2024], and do so in the multi-ethnic UK context, where some minorities outperform the majority in economic outcomes, while others underperform. This is key, as narratives about minority success or disadvantage reveal fundamentally different types of individuals identified as ZSTs – depending on whether narratives of minority success and disadvantage are stressed, support for redistribution to address horizontal inequalities will be very different.

Section 2 describes our data and survey experiment. Section 3 presents evidence on whether narratives shift majority perceptions of minorities and their ingroup revealing zero-sum thinking. Section 4 presents the CATE methodology to identify zero-sum thinkers and shows how they differ in characteristics, political alignment, policy preferences, perceptions and feeling towards minorities and their ingroup, to non-ZSTs. Section 5 sheds light on whether our results differ from other work due to differences in methodology and study context. Section 6 concludes by discussing directions for future work. The Appendix provides additional survey details and results.

2 Data

2.1 Sample

YouGov administered our survey to their regular panel of registered members, designed to be representative of the adult UK population. Panel members frequently interact with *YouGov* and are incentivized to complete their online surveys. 4229 individuals were surveyed. For our study of majority views, we focus on the subsample of 3205 respondents who define their ethnicity as White British in data previously provided to *YouGov*. After consenting to take part, in line with ethical practice, respondents were shown an opening message outlining the study purpose: *This is an academic study looking at public attitudes towards inequality and ethnic diversity. It forms part of a research programme on race and ethnicity in the UK.*

The survey was fielded between the 25th and 28th June 2024, in the run up to the UK Gen-

framing affects support for global redistribution. They show a zero-sum prime reduces support for international redistribution as measured through donations to an international NGO.

eral Election on July 4th. That high stakes electoral campaign saw the rise of populist parties, notably Reform UK, which received 14% of the vote share, with immigration being a salient topic throughout the campaign. Table A2 shows the sample characteristics, comparing them to 2021 UK census figures for the White British population. Throughout our analysis, we use individual weights derived by *YouGov* to render the sample to be nationally representative. Our reweighted sample is slightly older, less urban and less educated than the White British population as a whole.

Given our later examination of ZST and policy preferences, Panel A of Table A3 details our primary measure of political leaning: among controls, 9% of respondents self-report being right-leaning, 48% report themselves as centrists, and 16% as left-leaning (the remainder being unsure/don't know). Panel B shows voting intentions in the 2024 General Election: 89% report intending to vote, and 78% report having voted in the Brexit referendum (relative to an actual turn out of 72%). On voting intentions, respondents again reflect the spectrum of political views, with 34% planning to vote Labour, and 19% reporting considering voting for Reform.[5]

2.2 The Survey Experiment

After an initial set of questions on background characteristics, respondents were randomly assigned to one of three treatment arms. In the first treatment, respondents were presented a narrative about the economic circumstances of minorities, being told:

Ethnic minorities in the UK are performing well in education. In most cases, they are getting more/higher qualifications than White British people, even when they come from poor backgrounds. This is particularly true for Indians and also for Pakistanis. We want to know what you think about ethnic diversity in the UK today.

In a second treatment arm, respondents were presented a narrative about the economic disadvantage of minorities:

Ethnic minorities in the UK face higher unemployment rates than the White British majority. Even among those with similar levels of education, the chances of being unemployed are still higher in most cases. This is particularly true for Black Caribbeans and also for Pakistanis. We want to know what you think about ethnic diversity in the UK today.

A third group were assigned as controls (with no placebo narrative).

Political messaging during the election campaign, which could involve zero-sum messaging on issues such as immigration, provides a set of background primes that all respondents could have been exposed to. Any impacts of our treatments are over and above those primes.[6]

[5] The self-reported measure of political leaning is based on a standard *YouGov* question, worded as, *some people talk about 'left', 'right' and 'centre' to describe parties and politicians. With this in mind, where would you place yourself on this scale?* Left-leaning respondents are those that report being 'very left-wing' and 'fairly left-wing'. Centre-leaning respondents combine those that report being 'centre', 'slightly left-of-centre' and 'slightly right-of-centre'. Right-leaning respondents are those that report being 'very right-wing' and 'fairly right-wing'.

[6] Bursztyn *et al.* [2020] document using a series of experiments around the 2016 US Presidential election, that

Figure A2 shows the structure of the experiment. In each treatment, we asked individuals about: (i) their perceptions of the economic outcomes of minorities; (ii) the opportunities available to minorities; (iii) causes of disadvantage across groups – such as a lack of effort, bad luck, or discrimination; (iv) views of policies related to the redistributive role of government, support for pro-poor policies, and support for policies aiming to reduce ethnic inequalities. All survey questions were framed as related to the contemporary situation in the UK.[7]

A central innovation of our experimental design is to probe perceptions among the majority White British population across the linked dimensions above, towards their ingroup and towards *specific* minority groups. Asking respondents their perceptions of the White British ingroup enables us to shed light on dimensions in which the majority reveal zero-sum mindsets in response to exposure to narratives about minorities. Our method is also distinct from earlier approaches to measuring ingroup-outgroup hostility, as reviewed in Bauer and Chytilová [2025]. The minority outgroups we ask White British respondents about are: (i) Indians – referred to in the narrative of minority success; (ii) Black Caribbeans – referred to in the narrative of minority disadvantage; (iii) Pakistanis – referred to in both narratives.[8]

2.3 Interpreting the Treatments

The information provided in the treatments is both general, names specific outgroups, and describes relative advantage and disadvantage. We provide information respondents might be unaware of, enabling perceptions to update but without the detail of quantitative information experiments [Bursztyn and Yang 2022, Haaland *et al.* 2023, Stantcheva 2023, Schaeffer *et al.* 2024].

Much like the narratives respondents are exposed to during an election campaign, our treatments provide a bundle of qualitative narratives with differing valence. Our narratives name specific groups, their success or disadvantage on defined economic outcomes, and thereby position the ingroup as either a relative winner or loser, in comparison to the named minorities. In line with ethical practice, the narratives are factually correct. They demonstrate a common base for positive and negative outcomes, namely they state some minorities are doing well even if from poor backgrounds, and that others are doing poorly independent of their educational level. The treatments are not intended to suggest that either is necessarily a cause for celebration or con-

Trump's rise causally increased individuals' willingness to publicly express latent anti-immigrant views.

[7] In designing our survey, we drew on principles of good question design [Schaeffer and Presser 2003], aiming to maximize the neutrality of our questions and minimize their cognitive burden. To maintain comparability with the literature, we drew some questions from existing work [Alesina *et al.* 2024, Almas *et. al* 2024, Benson *et. al* 2024], tailoring them to our context and study purpose. We also drew on questions or underlying logic of questions from existing polling studies within the UK to offer both benchmarking and contextual applicability of our questions [Juan-Torres *et al.* 2020, IPSOS 2023, Fairness Foundation 2024].

[8] We also asked White British respondents about Polish and Black Africans, who are not referred to in either treatment, but as shown in Figure A1, constitute prominent minorities in the UK. We do not focus on these responses in the analysis. To limit the cognitive load on respondents, we do not ask about other minorities such as Bangladeshis, Chinese or those of Mixed ethnicity.

cern. To reinforce this, both narratives use the neutral final sentence intended to show a range of reactions could be possible.[9]

The treatments serve two purposes.

First, they allow us to understand whether they shift perceptions towards the White British ingroup in ways consistent with zero-sum mindsets. If other groups are doing better or worse than expected, perceptions of the economic standing of the ingroup might shift in the opposite direction, in line with ZST. Second, they shed light on the malleability of perceptions of the majority towards minorities, in a time of heightened political campaigning, even if that does not trigger ZST.

As with any survey experiment, responses could be biased due to social desirability bias or experimenter demand effects. We intentionally used the neutral language of ethnic diversity, language which was repeated in the last line of each narrative to help avoid positioning the survey in ways that might lead to such biases. The concern is further ameliorated by the fact that: (i) respondents are well used to answering surveys from *YouGov*, are incentivized to complete them, and know their answers are anonymized; (ii) the salience of similar issues during the election campaign acts as a broader contextual priming for respondents, potentially fostering greater readiness to express underlying views [Creighton *et al.* 2015, Haaland *et al.* 2023]; (iii) we assess if narratives shift attitudes towards the White British ingroup, which are less likely to reflect experimenter demand or social desirability bias effects. Overviews of the use of online samples for survey experiments are provided by Haaland *et al.* [2023] and Stantcheva [2023]. They discuss approaches to establishing sample validity, participant attention, and potential respondent biases. Our approach to these issues underpinning the quality of data collection replicates existing work, so we relegate the discussion to the Appendix.[10]

2.4 Estimation and Balance

Following our pre-analysis plan, we estimate treatment effects using the following OLS specification for perception y of individual i towards group g:

$$y_i^g = \alpha^g + \beta_S^g T_{iS} + \beta_D^g T_{iD} + u_i^g, \tag{1}$$

[9] Our treatments are therefore not as specific as those that ask respondents to think about immigration [Alesina *et al.* 2023], inform them about the labor market impact of immigration [Haaland and Roth 2020], present results from correspondence studies [Haaland and Roth 2023], or video treatments providing information on the causes of racial gaps, the evolution of income gaps between Black and white Americans over 50 years, and differences in intergenerational mobility between white and Black children in the US [Alesina *et al.* 2024].

[10] Earlier work has documented limited impacts of social desirability bias or demand effects in survey experiments but this might vary across contexts [de Quidt *et al.* 2018, Cantoni *et al.* 2019, Mummolo and Peterson 2018, Bursztyn *et al.* 2020]. The concern might be more acute in the absence of truth-telling incentives, although incentives can be counterproductive in online surveys, where respondents might seek out information to provide the correct answer when the information is publicly available [Haaland *et al.* 2023].

where T_{iS} is equal to one if i is randomly assigned to the narrative of minority success, and zero otherwise; T_{iD} is analogously defined if i is assigned to the narrative of minority disadvantage. u_i^g is an error term. To reiterate, the treatment effects estimate the combined effect of the qualitative information provided in each narrative – combining reference to specific minorities, specific margins of their success or disadvantage, and potentially framing the ingroup as a relative winner or loser. Table A2 shows treatment arms are well balanced. Our light touch narratives might not always measurably shift perceptions relative to controls. Hence, we also report the p-value on the null that $\beta_S^g = \beta_D^g$, to probe the hypothesis that perceptions towards group g can be shifted through differential exposure to the narratives – especially so for perceptions towards Pakistanis who are named in both treatments. We use individual weights to adjust respondents to match the national profile, and report robust standard errors. For any given perception, an individual might agree or disagree with a statement, and we also allow them to express that they don't know or are unsure. For expositional ease, we focus on the most relevant response categories, that need not always sum to one due to other potential responses not being shown.

3 Perceptions of Minorities and the Ingroup

Our first set of results consider White British respondents' perceptions of the economic outcomes, opportunities, and causes of disadvantage across minority outgroups and their ingroup. Responses to the narrative treatments for views of the ingroup are used to reveal the exact dimensions along which zero-sum thinking among the majority, and how these responses differ with exposure to narratives of economic success or disadvantage.

3.1 Economic Outcomes

We start by considering perceptions of White British respondents of the economic outcomes of minority groups, and their ingroup, along dimensions linked to the wording of the narratives: (i) the share of graduates, by gender, among each group; (ii) the employment rate, by gender, among each group. For expositional ease we first consider perceptions of economic outcomes of men among the three named outgroups and the ingroup, and discuss below the similarity of results for perceptions of outcomes of women. As in Haaland and Roth [2023], we measure perceptions on a quantitative scale that is comparable across respondents with the same interpretation for all.

To establish something like a ground truth to measure the accuracy of perceptions, we use the Quarterly Labour Force Survey (QLFS) from January 2023-December 2024 to derive outcomes for men in each group. Panel A of Figure 1 shows perceptions and reality for graduate shares of each group among controls. White British respondents believe 36% of White British and Indian men are graduates, with this belief falling to 26% and 21% for Pakistani and Black Caribbean men respectively. Reality lines up well with perceptions towards White British men – there is

only a 1pp underestimation of graduate shares for the ingroup. In sharp contrast, there are large misperceptions of graduate shares among outgroups: graduate shares are underestimated by 37pp for Indian men, and by 22pp for Pakistani men. These gaps are around half the true share.

The majority perceive Indian men to have similar graduate shares to their ingroup, but do not recognize that the ingroup actually lags well behind on this outcome. Hence one of the economic success stories of minorities in the UK that is well established among social scientists – their advancement in higher education – appears little known among the majority population.[11]

The right hand side panel of Figure 1 shows the CDF of misperceptions among controls. Following Bursztyn and Yang [2022], we define the asymmetry of misperceptions as:

$$\text{AM} = \frac{\max\{\text{share underestimate, share overestimate}\}}{\min\{\text{share underestimate, share overestimate}\}}, \tag{2}$$

A ratio of one indicates exact symmetry, and the higher the ratio, the larger the asymmetry. Majority perceptions for graduate shares of White British men are symmetric around the truth (AM= 1.18). In contrast, misperceptions towards minorities are highly skewed: AM=14.3 for Indians, 5.4 for Pakistanis and 2.18 for Black Caribbeans.

Panel B of Figure 1 repeats the analysis for employment rates of working age men. Among controls, employment rates are perceived to lie between 70 and 80% for all minorities. The QLFS-derived benchmark suggests the majority underestimate employment rates for White British, Indian and Pakistani men.[12]

Underpinning confidence in the survey responses, our findings line up with the body of evidence, overviewed in Bursztyn and Yang [2022], that show misperceptions of others are widespread and asymmetrically distributed around the truth, while misperceptions about the ingroup are smaller. However, the patterns of misperception contrast with the US-based literature where the White population tends to underestimate racial disadvantage across domains [Kraus *et al.* 2017, Callaghan *et al.* 2021, Haaland and Roth 2023, Alesina *et al.* 2024]. In our setting, the majority underestimate the graduate shares and employment rates among most minorities. Moreover, the majority perceive the ingroup as having the lowest employment rate across groups, despite having the highest perceived graduate share.

Narratives and Perceptions Table 1 shows how these perceptions are impacted by the narrative treatments. Panel A shows results for beliefs about graduate shares. The narrative of minority

[11] These misperceptions are unlikely to be driven by misperceptions of the share of foreign-born among minorities. Table A4 shows there are small differences in graduation or employment rates by country of birth: so whether an individual from a minority group is UK or foreign-born.

[12] There are a few robust predictors of perceptions of economic outcomes of men, by group (Table A5): (i) older and lower educated individuals perceive lower graduate shares and employment rates across groups; (ii) women perceive higher graduate shares and lower employment rates across groups. Political leaning is relevant: relative to left-leaning respondents, centrist and right-leaning respondents perceive lower economic outcomes among minorities. Later in the analysis we consider how these perceptions correlate to identified zero-sum thinking.

success shifts upward perceptions of graduate shares among Indians and Pakistanis – the groups named in this treatment. The magnitude of impacts are not trivial: for example White British respondents assigned to the narrative of success increase their perceived graduate share of Indian men by 3.4pp relative to 36% among controls. There is no significant impact of the narrative of disadvantage (that was worded in terms of minorities' employment). Panel B shows treatment effects for perceived employment rates. Here we find the narrative of disadvantage significantly reduces perceived rates among Pakistani and Black Caribbean men – the groups named in this treatment – so increasing the extent to which their true employment rates are underestimated.[13]

Our results thereby show that even during a period of heightened political campaigning, light-touch narratives can shift beliefs of the majority population about economic outcomes of minorities. These shifts relate to the specific dimension of outcome the information relates to, and the specific minority groups mentioned. The direction of belief shifts is in line with respondents updating, but does not necessarily cause beliefs to align more closely with reality.

Throughout, we find no shift in perceptions of economic outcomes of the ingroup – irrespective of the narrative treatment. Hence zero-sum thinking does not emerge along the dimension of economic outcomes of outgroups and the ingroup.

Finally, we note a strong similarity of levels of misperceptions of economic outcomes for women across groups, correlates of those misperceptions, and treatment effects on misperceptions as those documented for men.[14]

3.2 Opportunities

We next examine how the majority views opportunities available to minorities and to their ingroup. We asked about three opportunities, purposefully linked to the narrative wording: (i) obtaining a good education; (ii) finding work; (iii) earning a decent wage. Each question was framed in the context of opportunities faced in the local neighborhood of the respondent, to capture concrete impressions of what respondents observe rather than hypothetical views about the UK as a whole.

[13] Bursztyn and Yang [2022] describe how in nearly a quarter of studies, perceptions about others shift by more than 50% relative to priors, and around half of the studies find changes in perceptions by at least 25%. Our impacts are smaller, in line with their finding that qualitative treatments have lower impact than the provision of quantitative information.

[14] Panel A of Figure A3 shows perceptions of graduate shares of women across groups. These are underestimated for all outgroups. Panel B shows that among controls, employment shares are perceived to be closer across groups, between 60 and 75% for all minorities. The majority population underestimates employment shares for White British, Indian and Black Caribbean women, and overestimates them for Pakistani women. Table A6 shows that correlates of perceptions of women's economic outcomes across groups are similar to those for perceptions of mens' outcomes: (i) older individuals perceive lower graduate shares across outgroups; (ii) women perceive higher graduate shares and lower employment shares across groups; (iii) the political leaning of White British respondents is again relevant for shaping perceptions of economic outcomes of minorities, all else equal. Panel A of Table A7 documents how perceptions are impacted by the treatments. The success narrative shifts upward perceptions of graduate shares among Indians and Pakistanis – the groups named in this treatment. Panel B shows treatment effects for perceived employment shares. We find weaker treatment effects than for men, but there are differential impacts of the narratives for Indian ($p = .091$) and Pakistani women ($p = .026$).

The results are in Table 2.

Focusing on the control group to begin with, there is a widespread belief among the majority, that minorities lack opportunities in education, work and wages (Columns 1, 4 and 6). The ranking across outgroups lines up with perceptions of outcomes: Black Caribbeans are seen to have the fewest opportunities, followed by Pakistanis and then Indians. The remaining Columns show generally weak impacts of each treatment on the perceived lack of opportunities for minorities.[15]

The most striking pattern that emerges, however, is in views about whether the White British ingroup lack opportunities. Respondents assigned to the narrative that informs them of the relative educational success of Indian and Pakistanis, become significantly more likely to report that White British individuals lack opportunities in finding work or earning a decent wage. These responses correspond to 36% and 27% increases over controls respectively. This response reveals zero-sum thinking – that positive outcomes for outgroups might come at the expense of the ingroup. White British individuals assigned to the narrative that informs them of the relative disadvantage in unemployment for Pakistani and Black Caribbean groups given equal education levels, become significantly less likely to report that White British individuals have few or no opportunities in obtaining a good education, the magnitude of the effect corresponding to a 40% decrease. This again reveals a zero-sum mindset: if minorities at the same level of education are faring worse in the labor market, on average majority respondents become less concerned about educational opportunities available to their ingroup.[16]

To summarize, in contrast to the results on economic outcomes, zero-sum thinking is triggered – by both narratives – along the margin of perceived opportunities of outgroups and the ingroup.

3.3 Causes

Luck versus Effort To shed light on how the majority perceive the root causes of disadvantage across groups, we first asked about the relative importance of luck and effort in driving success: *does lack of effort or bad luck generally have more to do with whether a person from each of the following groups is unsuccessful?* The results are in Table 3.

Focusing on the control group to begin with, we observe: (i) a lack of success for White British individuals is far more often attributed to a lack of effort rather than bad luck (34% versus 7%,

[15] On perceptions of getting a good education, the positive narrative nudges down the perceived lack of opportunities and the disadvantage narrative nudges up the perceived lack of opportunities for the groups named in the treatments. Hence there is differential impact of the narratives towards perceived opportunities of Indians ($p = .035$) and Pakistanis ($p = .009$).

[16] An alternative, non-zero-sum reading of these responses is purely comparative: hearing that outgroups fare badly lowers the reference point against which respondents judge their ingroup, making the ingroup's position look better by contrast (without any belief that outcomes across groups are linked). However, that is unlikely to be the case because the narrative of disadvantage explicitly holds education constant across groups. A comparative recalibration should instead be strongest for the margin the narrative speaks to – employment – yet perceptions that the ingroup lacks opportunities in finding work or earning a decent wage do not respond to the narrative of disadvantage. The decline in concern over ingroup educational opportunities suggests respondents perceive that equally qualified outgroups faring worse implies a more secure position for the ingroup: a zero-sum inference.

$p = .000$); (ii) the opposite attribution is made for Indians, bad luck rather than effort is far more likely to be viewed as driving a lack of success (24% versus 13%, $p = .000$); (iii) for Pakistani and Black Caribbeans, respondents are equally likely to attribute a lack of success to luck and effort.

Neither treatment shifts perceptions of these underlying causes. This suggests perceptions of the causes of disadvantage of the ingroup and minority outgroups are deep rooted and not malleable to light-touch narratives – in line with existing evidence that even quantitative information provision does little to affect such attitudes [Alesina *et al.* 2024].

Labor Market Discrimination The third cause of disadvantage we explore is discrimination: so placing emphasis on systematic forces against a group rather than individual-based causes such as effort or luck. We focus on majority views of labor market discrimination faced by minorities. We again framed questions to be group and gender specific, given gender differentials in labor market participation within groups [Mirza and Warwick 2024]. We asked respondents, *Imagine that the following men applied for a job, which they were qualified for. Please rank them based on how likely you think they would be to be turned down for the job.* Responses are coded so a rank of 1 means the group is perceived to face the least discrimination in labor market hiring. It was made clear to respondents they could place multiple groups into the same rank, so that one group's gain in ranking does not necessarily imply another group falls in their ranking. This avoids apparent zero-sum thinking being baked into responses.[17]

The results towards groups of men are in Table 4. We discuss the similarity of results for women below.

The ranking of perceived discrimination faced by groups matches perceived outcomes and opportunities: Black Caribbean men are perceived to face the most discrimination (rank 4), followed by Pakistanis and Indians, with White British men perceived to face the least discrimination. For minorities, the narrative of minority success tends to reduce perceptions of discrimination faced by that group, while the narrative of disadvantage does the opposite. The differential impacts of the treatments are statistically significant for Indians ($p = .002$) and Pakistanis ($p = .014$).

However, the largest response is induced by the narrative of minority success, increasing the perception that White British men face labor market discrimination – in line with revealed zero-sum thinking. Being informed that some minorities are performing well in education increases the perception among the White British that their ingroup faces more labor market discrimination.

Similar patterns of results holds for perceptions of discrimination faced by women across groups, including revealed zero-sum thinking in response to the narrative of minority success (Table A9).

Overall, our revealed approach to identify zero-sum thinking through the narrative of minority

[17] We validate that respondents understood they could assign multiple groups in the same rank. Table A8 shows the number of respondents that assigned the rank given in the column to the number of groups shown in the row. For example, the first column shows that 49 respondents assigned each of the six groups a rank of 1 and 1,719 respondents assigned only one group a rank of 1. As the total number of respondents was 3,205, if each rank was assigned to exclusively one group, the number in the first row would be 3,205 for each of the six ranks.

success (that positions the ingroup as a relative loser), thus aligns with previous research that presents a link, among high-status groups, between feeling one's group is discriminated against and ZST [Norton and Sommers 2011, Wilkins *et al.* 2015].

4 Identifying Zero-Sum Thinkers

We have so far evidenced dimensions along which zero-sum thinking, on average, among White British respondents, is revealed through the narrative treatments. The narratives capture competitive concerns about ingroup disadvantage triggered by information about outgroup circumstances: the narrative highlighting minority success, increases the majority's perception that White British individuals lack opportunities and face discrimination; the narrative highlighting minority disadvantage, eases concerns that the ingroup lacks opportunities. However, these average effects likely mask substantial heterogeneity across individuals – some respondents may exhibit strong zero-sum responses while others show none.

Our pre-analysis plan specified examining heterogenous impacts by gender, education and political leaning. These results are reported on the Online Appendix. To explore heterogeneous treatment effects in a more flexible way, we use causal forests to estimate individual-level treatment effects [Athey *et al.* 2019].[18]

The causal forest algorithm estimates Conditional Average Treatment Effects (CATEs) for each narrative treatment, for individuals based on combinations of observables (including the pre-specified dimensions of gender, education and political leaning). This sheds light on which respondents exhibit the strongest zero-sum responses when exposed to narratives about minority success or disadvantage.[19]

We implement forests with $10,000$ trees, using individual characteristics to predict treatment

[18] The Online Appendix presents additional results covered in our pre-analysis plan. These relate to: (i) additional results for White British respondents; (ii) a comparison of responses of White British and minority respondents; (iii) heterogeneous impacts of the narratives on White British respondents by political leaning, gender and education; (iv) perceptions of White British respondents towards other minority groups that are not named in either narrative treatment. The Online Appendix is available at `https://www.imranrasul.com/working-papers`.

[19] Following Wager and Athey [2018] and Athey *et al.* [2019], the estimand is the CATE $\tau(x) = E[Y(1) - Y(0)|X = x]$, where $Y(1)$ and $Y(0)$ denote potential outcomes under the narrative and control conditions and x is a respondent's vector of observables. For each outcome we pool the control arm with the relevant narrative arm and fit the forest on this pooled sample. The estimator first partials out covariates: auxiliary regression forests predict the outcome and treatment status from X, and the causal forest is grown on the residualized data [Athey *et al.* 2019]. Each of $10,000$ trees is grown on a random half of the pooled sample and is honest in the sense of Wager and Athey [2018]: the observations used to decide where a tree splits are separate from those used to form its leaf estimates, so the same noise does not drive both the partition and the effects estimated within it. At each node, splits are chosen among a random subset of covariates to maximize the resulting spread in estimated treatment effects across child nodes. The forest prediction $\hat{\tau}(x)$ is then a locally weighted treatment-control contrast in the residualized data, with weights reflecting how often observations share a terminal leaf with x across trees. Respondents' own CATEs, $\widehat{\tau}_i$, are computed out-of-bag – using only trees whose training subsample excluded respondent i – so no respondent's estimate is fitted to their own data. The CATE estimates are then pointwise consistent and asymptotically normal [Wager and Athey 2018, Athey *et al.* 2019].

effect heterogeneity. All the covariates used to make this prediction are collected prior to the implementation of the treatments – so part of the first survey module indicated in Figure A2. These individual characteristics include: (i) respondent age, household income, gender, education, housing tenure, marital status and employment status; (ii) self-reported political leaning; (iii) exposure to minorities, measured by the share of the population of the respondent's local authority that is not White (respondents reside across 346 local authorities in our sample); (iv) their concerns over inequalities – including a measure of horizontal inequalities constructed from views across seven domains (between individuals residing in different regions, social classes, disabled/non-disabled people, genders, religions, gay/straight people, and between individuals of different ages); a measure of vertical inequalities constructed from views across five domains (wealth and income, health and life expectancy, educational attainment, political influence, status and respect); a measure of concern over inequalities between ethnic groups. By using a combination of demographic, political and attitudinal information to predict treatment effect heterogeneity, we aim to narrow down our classification as being precisely driven by zero-sum mindsets rather than reflecting these underlying characteristics/attitudes towards minorities and inequalities.

4.1 CATE Estimates

Panel A of Figure 2 shows the distribution of CATEs for those outcomes where zero-sum thinking is revealed: one in response to the narrative of minority disadvantage – that leads the ingroup to perceive White British individuals do not lack educational opportunities, and three in response to the narrative of minority success – that leads the ingroup to perceive White British individuals lack job opportunities and opportunities to earn a decent wage, and increase perceptions of discrimination against male White British job applicants. We overlay the distribution of CATEs with the previously estimated ATE (dashed red). We reverse the sign for the disadvantage narrative treatment effect so that a positive CATE corresponds to revealed zero-sum thinking.

Panel B of Figure 2 plots the CATE estimates for each individual i, $\widehat{\tau}_i$, ordered from smallest to largest, with their associated confidence interval $(\widehat{\tau}_i^s \pm 1.96\widehat{se}_i^s)$. We identify whether individual i (with a specific covariate profile) subject to narrative s is a zero-sum thinker, denoted D_i^s and defined as:

$$D_i^s = \mathbf{1}\left\{\widehat{\text{ci}}_{\text{lower}}\left(\widehat{\tau}_i^s\right) > 0\right\} = \mathbf{1}\left\{\widehat{\tau}_i^s - 1.96\widehat{se}_i^s > 0\right\}, \tag{3}$$

i.e. $D_i^s = 1$ when the respondent's estimated treatment effect on the experimental outcome is significantly positive; it is set to zero otherwise (except in the rare cases when the CATE is significantly negative when we set it to missing). We focus on two outcomes where the CATEs are precise: perceptions that White British individuals do not lack educational opportunities (revealed from the narrative of disadvantage) and perceptions of discrimination against White British job applicants (revealed from the narrative of success). For these outcomes, 24% and 23% respondents, respectively, have covariate profiles with statistically significant and positive CATEs,

so being identified as zero-sum thinkers.

Two further points are of note. First, the classification rests on treatment effects differing across individuals. The Appendix sets out how we assess this, following the approach of Chernozhukov *et al.* [2018]. Second, as each CATE is a function of respondent's characteristics, $\tau(x)$, the forest also yields a predicted CATE for each individual in the control group: the effect the narrative would have had on someone with the same covariate profile. Control respondents are part of the pooled sample on which each forest is estimated, but, as for all respondents, their CATEs are computed out-of-bag, from trees that do not use their own responses. Applying the same rule (3) to these CATEs, classifies each control respondent as someone who would, or would not, have been revealed as a ZST had they been exposed to each narrative. Doing so, we find that 24.1% of controls are identified as ZSTs from the narrative of disadvantage prediction, and 21.8% of controls are identified as ZSTs from the narrative of success prediction. It is these groups of predicted ZSTs/non-ZSTs from the control group that we focus on in the next part of analysis.[20]

4.2 Overlap in Revealed Zero-sum Thinkers

We first examine whether individuals revealed to be ZSTs from a given narrative are also predicted to have been identified as a ZST had they been exposed to the other narrative. This sheds light on whether ZST is triggered in response to specific light-touch narratives, or whether ZST is more deep-rooted and would be triggered irrespective of which exact narrative of minority success or disadvantage individuals are exposed to.

This analysis is in Figure 3. Each position on the x-axis is one covariate combination among White British control respondents. Combinations are defined over all covariates in the causal forests, with the four multi-valued covariates (age, local-area minority share, and the horizontal and vertical inequality-concern indices) coarsened into terciles. For each combination, we plot the survey-weighted share of respondents classified as a ZST under the success narrative (navy, above the axis), and under the disadvantage narrative (orange, below the axis). The marker size corresponds to number of respondents in the combination. The vertical lines partition the combinations into those predicted to be a ZST only under the success narrative (left), only under the disadvantage narrative (right), and those predicted to be ZSTs under both (middle).

Only 7.6% of respondents are classified as a ZST under both narratives, far from the full overlap a single underlying trait would imply. In short, we reveal distinct types of ZST identified from exposure to each narrative of minority success or minority disadvantage, rather than a single, more deep-rooted group of ZSTs, who would be triggered by exposure to either narrative. This is a key advantage of using a revealed measure of ZST, enabling us to go further than measures of ZST

[20] For completeness, Figure A4 repeats the analysis for the one other margin where ZST is revealed in response to the narrative of minority success: when respondents are asked their perceptions of discrimination against female White British job applicants.

based on attitudinal scales. By revealing what triggers ZST, we establish the limited prevalence of more deep-rooted zero-sum mindsets triggered by either narrative.[21]

4.3 Characteristics

Figure 4 shows differences in characteristics of control respondents predicted to be ZSTs versus all others (non-ZSTs), from each narrative treatment. To be clear, these are all covariates used to classify individuals as ZSTs/non-ZSTs. As such, this does not provide independent information, but makes clearer what underlies the ZST classification. We highlight differences that are statistically significant between ZSTs and non-ZSTs, with insignificant differences represented by bars with no fill. Two key results emerge.[22]

First, irrespective of which narrative reveals ZST, relative to non-ZSTs, ZSTs are older, lower income, and less likely to report themselves as left-leaning. Second, ZSTs revealed from each narrative also differ slightly from each other, as seen by the p-value tests of whether the ZST/non-ZST differences are the same across treatments (reported on the right side of Figure 4): in terms of characteristics, ZSTs revealed through the narrative of minority disadvantage are less likely to be university educated, less likely to be married, more likely to self-report as right-leaning, and reside in areas with a higher share of minorities. Some of these findings contrast with what has been documented for ZSTs in the US. For example, Chinoy *et al.* [2026] find that zero-sum mindsets are more common in younger respondents. We come back to consider the issue of characteristics of ZSTs in the UK and US contexts in Section 5.

In our context, the sharpest differences between ZSTs and non-ZSTs are seen in terms of other attitudes: ZSTs revealed through the narrative of minority disadvantage exhibit less concern for horizontal inequalities between groups than ZSTs revealed through the narrative of minority success, less concern for vertical inequalities, and lower levels of concern for inequalities between ethnic groups. We build on this to consider the political alignment and policy views of each set of ZSTs.

4.4 Political Alignment

We probe further the association between political alignment and ZST. This is especially relevant in our context given our survey experiment was purposefully fielded in the immediate run-up to the high stakes UK General Election in July 2024, that saw the rise of populist parties, and

[21] This is against a benchmark of 5.1% of respondents being expected to be predicted as ZSTs if the two classifications were independent. To see this, in the sample used in Figure 3, 23% of respondents are classified as ZST by the disadvantage narrative, and 22% by the success narrative. If the classifications were independent, the share classified by both would be the product of the marginals: $.23 \times .22 = 5.1\%$.

[22] The Figure shows 95% confidence intervals, with a difference marked significant (filled) when the interval excludes zero. As the ZST classification is itself estimated from the first-stage causal forest, we account for the generated regression. To do so, the intervals are constructed from a nested bootstrap that re-estimates the classification in every replication, so first-stage uncertainty is carried through. Full details are in the Appendix.

immigration being a salient topic that was often discussed in zero-sum narratives in political campaigns. Self-reported political leaning of respondents was one of the characteristics used to classify individuals as ZSTs. To therefore establish independent evidence on the link between ZST and political alignment, we consider two relevant dimensions not used in the causal forest underlying the CATE estimates.

First we consider individuals whose political party allegiance has shifted in recent times. We identify them based on the difference in the party they report actually having voted for in the 2019 General Election and the party they intend to vote for in the 2024 General Election. Combining this information we describe respondents as having shifted left, shifted right, or remained unchanged in their political party preference. Swing voters – critical for political outcomes in democracies – are likely to be among those with shifting party preferences. Second we consider respondents' behavior during the Brexit referendum in the UK, perhaps the quintessential zero-sum versus positive sum political debate in recent times in the UK. We use information on whether respondents: (i) voted in the referendum; (ii) voted for the UK to Leave the EU.

Table 5 shows the results. As a benchmark, the first row shows the share of ZSTs among White British respondents identified from each narrative treatment (as predicted among controls). Column 1 shows outcomes among controls. Columns 2 and 3 show for each outcome, the share of respondents classified as ZSTs from each narrative, and the p-value on whether this differs from the overall share identified from each narrative.

Panel A shows results based on shifting political party allegiances. We see that irrespective of the narrative treatment, ZSTs are well represented among those shifting left, shifting right, or whose party support has remained unchanged over the five years preceding our survey. However, those who have shifted left are significantly overrepresented among those identified as ZST from the narrative of success ($p = .008$). Those whose party alignment has remained unchanged are not more or less likely than others to be identified as ZSTs identified from either narrative. In contrast, those who have shifted right are overrepresented among ZSTs identified from both the narrative of minority success ($p = .054$) and the narrative of minority disadvantage ($p = .034$).

Panel B considers voting behavior during the Brexit referendum in the UK. ZSTs identified from the narrative of minority success are more likely to have voted in the referendum ($p = .002$), but that ZSTs identified from either narrative are significantly over represented among those that voted for the UK to leave the EU ($p = .002$**,** 006).

In common with earlier work, our revealed approach to identifying ZSTs shows that ZST is exhibited across the political spectrum [Davidai and Ongis 2019, Chinoy *et al.* 2026]. *A fortiori*, revealed ZST can help explain variation in views *within* groups of similar political leaning. Those with right-shifting political party allegiances and those more engaged with the Vote Leave campaign during the Brexit referendum are more susceptible to ZST, and these mindsets can be triggered by both narratives of minority success and disadvantage.

4.5 Policy Views

An issue underlying many policy debates is who stands to win or lose, or whether a policy would expand total resources or reallocate existing resources meaning gains to some are offset by losses to others. The prevalence of zero-sum mindsets are central to how such debates play out in democracies [Stantcheva 2024, Ali *et al.* 2025]. We thus consider how the policy views of ZSTs differ from others. We do so for views on pro-poor redistributive policies in general, attitudes towards minorities, and support for policies aiming to reduce ethnic inequalities in particular. All survey questions were: (i) not used in our CATE estimates to classify ZSTs; (ii) framed as related to the contemporary situation in the UK. We estimate the following OLS regression for White British respondents in the control group:

$$y_i = \alpha + \beta^s D_i^s + X_i'\gamma + \varepsilon_i, \tag{4}$$

where the outcome y_i is a dummy for whether a respondent strongly agrees/agrees with a given policy statement, D_i^s is our constructed classifier of whether individual i exposed to narrative s is identified from their CATE to be a ZST as defined in (3), and X_i are individual covariates: age, gender, marital status, home ownership, household income, education, employment status, the share of the population of the respondent's local authority that is not White, and political leaning. These covariates enter the ZST-classifier D_i^s in a highly non-linear way, and so we control for them linearly in this specification to capture any direct association between each covariate and policy view y_i. We use survey weights throughout. As D_i^s is a generated regressor, conventional standard error estimates omit a variance component and will generally be downwards biased. We therefore correct standard errors using a nested nonparametric Monte-Carlo bootstrap – details are in the Appendix.

Figure 5 plots the coefficient of interest, $\widehat{\beta}^s$, the partial correlation of ZST with the policy view, along with its 95% confidence interval. The key implication that arises from these results is that depending on whether narratives of minority success and disadvantage are stressed, support for pro-poor redistributive policies, general attitudes towards minorities, and support for policies aiming to reduce ethnic inequalities in particular are very different.

Redistribution Panel A examines support for pro-poor redistributive policies related to income support, schools, housing and neighborhoods. While it has long been recognized that loyalties to racial groups can shape support for redistribution [Luttmer 2001], the link between ZST and support for redistributive policies is *ex ante* unclear. This is because how ZST shapes such policy views depends on whether: (i) an individual perceives themselves to belong to the group that wins or loses from the zero-sum dynamic; (ii) they normatively believe that zero-sum outcomes should be corrected for or not (irrespective of whether they believe themselves to be part of the winning or losing group). In other words, ZST reflects how an individual views economic inequalities across

groups, but not whether they necessarily view that as desirable. As Chinoy *et al.* [2026] discuss, if an individual perceives themselves as belonging to the gaining group, then pure self-interest motives imply they will not support redistribution. On the other hand, ZSTs can be more likely to support redistributive policies if they want to correct for negative externalities or forms of unfairness that drive the zero-sum dynamic, or view themselves as belonging to the losing group.

The results in Panel A show that: (i) ZSTs identified through the narrative of minority disadvantage are significantly *less* supportive of all four pro-poor policies than non-ZSTs – in line with them situating as winners and consequently self-interested motives dominating among ZSTs; (ii) differences in views between ZSTs and non-ZSTs identified through the narrative of minority success are far smaller and not statistically different from zero.[23]

The findings differ from those in the US: Chinoy *et al.* [2026] show that ZSTs support redistributive policies from rich to poor, or the redistribution of resources toward disadvantaged groups. In other words, externality or fairness concerns tend to dominate in the US context, while self-interested concerns dominate in the UK. Our results from ZSTs identified from the narrative of minority disadvantage help rationalize why groups that stand to gain economically from government redistribution – older, lower income and lower educated individuals – can be triggered to oppose such policies and be seen not to vote in their own economic best interest.[24]

Ethnic Diversity Panel B considers attitudes towards minorities along four margins: whether racial discrimination is seen to be a problem, whether government should reduce economics gaps between the majority and minorities, whether ethnic diversity is a positive for UK society, and whether immigration should be reduced. On the whole, we find that ZSTs do not differ from non-ZSTs on most of these margins. Two exceptions are that: (i) ZSTs identified from the narrative of minority success are significantly more likely to feel that immigration should be reduced – in line with evidence from the US where Chinoy *et al.* [2026] document that ZSTs support tighter immigration restrictions; (ii) ZSTs identified from the narrative of minority disadvantage are significantly less likely to view racial discrimination as a big/moderate problem.

Policies to Narrow Ethnic Inequalities The final set of policies considered in Figure 5 relate to those aiming to narrow ethnic inequalities. We asked respondents their views on: investing in education/training, teaching children about British values, financially supporting families with

[23] Ali *et al.* [2025] microfound such views in a model of rational voters based on the idea that uncertainty over the distributional gains of a policy and asymmetric information lead to zero-sum thinking. A voter votes against policies others support out of fear of what their support means for her, even if the policy is in her own interest.

[24] It is natural to ask if there are other non-redistributive policies that ZSTs are more likely to support. We are able to explore this a little as we asked respondents about their support for two such policies: whether the government should spend more on defence and national security, and whether they should spend more on infrastructure. The results in Figure A5 show that ZSTs identified from the narrative of minority disadvantage express significantly lower levels of support for defense spending, and those identified from the narrative of success express significantly lower support for infrastructure spending.

children, and increasing penalties for discrimination. Unlike targeted policies to tackle group disadvantage that might have differential support between ZSTs and non-ZSTs, these general policies targeting root causes may command greater support across the board or appeal to respondents with underlying orientations less closely aligned to ZST. For example, investing in education and training should be more appealing to those holding meritocratic perspectives, while those more concerned with redistribution and preventing poverty would be more likely to favour supporting families with children. Increasing penalties for discrimination is more likely to appeal to those who consider inequalities to derive primarily from discrimination, and teaching about British values should appeal more to those who see group cleavages in terms of failures of cultural integration and assimilation.

The results are in Panel C. We find that ZSTs identified from the narrative of minority disadvantage are significantly less likely to support three of the four policies, including them being significantly less supportive of increasing penalties for discrimination (consistent with the earlier finding that they are less likely to view racial discrimination as a big/moderate problem). The framework set out in Ali *et al.* [2025] can help explain these differences: untargeted policies can leave individuals uncertain over the potential distributional benefits of these policies to their ingroup, and so be less likely to appeal to those with zero-sum mindsets, all else equal.

4.6 Perceptions of Minorities and the Ingroup

These results reinforce the idea that zero-sum thinkers revealed through the narrative treatments represent different types of zero-sum mindset. Those identified through the narrative of minority success show similar policy preferences to non-zero-sum thinkers, but can be triggered by these narratives to think in terms of group competition. In contrast, those identified through a narrative of minority disadvantage maintain systematically different policy preferences. One explanation for these differing policy views between ZSTs and others is that they differ in their perceptions of outgroups and their ingroup. That is, while both see gains for the outgroup as losses for their ingroup and *vice versa*, the salience of what those losses or gains mean for them may differ. We thus come full circle to reconsider the earlier set of outcomes and examine how ZSTs view minorities and their ingroup in terms of their economic outcomes, opportunities, and root causes of disadvantage.

We explore the issue following the same approach as before, estimating the partial correlation of a given perception gap towards a specific minority group between ZST and non-ZSTs, once we condition on individual characteristics, and correct standard errors for the ZST-classifier being a generated regressor. The results are in Figure 6. For expositional ease, we show perception gaps towards one outgroup: Pakistanis, as they are named in both narrative treatments, and for the White British ingroup. We highlight differences that are statistically significant between ZSTs

and non-ZSTs, with insignificant differences represented by bars with no fill.[25]

We find that irrespective of which narrative ZSTs are revealed from, perception gaps between ZSTs and non-ZSTs with regard to the economic outcomes of Pakistani (men), opportunities available to them, and root causes of their disadvantage, are small and often not statistically different. In other words, although zero-sum thinking can be triggered by narratives about outgroups, it is not that perceptions of outgroups themselves differ much between ZSTs and non-ZSTs.[26]

The same is true for perception gaps between ZSTs and non-ZSTs towards their ingroup: we therefore do not find evidence that ZSTs perceive their ingroup to be faring worse on these outcomes than non-ZSTs. This is not because individual characteristics do not correlate with such perceptions: for example, age, gender, education, income and political leaning all have robust partial correlations with perceptions of the economic outcomes of groups (Table A5).[27]

5 Method vs. Context

At the heart of our analysis lies the innovation of revealing zero-sum thinking as a treatment-response pattern. This is in contrast to standard approaches to measuring zero-sum thinking in the literature, through attitudinal scales – some examples of which are in Table A1. Rather than identifying ZST as a single character trait, our approach reveals distinct types of ZSTs, depending on whether mindsets are revealed through narratives of minority success or disadvantage. Throughout we have emphasized the commonality in findings – in the observable correlates of ZSTs across methods and study contexts – as well as differences in findings, such as on policy views. These differences can arise from two sources: (i) alternative methodological approaches to measuring zero-sum mindsets; (ii) differences across study contexts.

To distinguish between explanations, we use the British Election Study (BES), that fields a battery of items from zero-sum measurement scales similar to those used in the literature, to a nationally representative sample of the UK population. We use this to: (i) establish differences in characteristics of ZSTs as measured from the BES to those revealed through our survey experiment – so understanding the role of methodological approaches to identify zero-sum mindsets in the UK context; (ii) examine correlates of zero-sum thinking in the UK against the US by comparing

[25] For completeness, results for Indian and Black Caribbean outgroups are shown in Figure A6. The results for these outgroups are largely in line with those for views of Pakistanis.

[26] The results link to an earlier literature highlighting how racial attitudes of white Americans' – specifically their beliefs about blacks' work ethic – can shape their opposition to welfare [Gilens 1995]. In contrast to the US evidence, in our context we see that ZSTs identified from either narrative are not more likely than non-ZSTs to believe the lack of success of Pakistanis, Indians and Black Caribbeans is due to their lack of effort (Figure 6 and Figure A6).

[27] We also explore a broader explanation based on whether ZSTs differ from non-ZSTs in their more holistic views of the contribution give by groups to society, or their feelings toward groups. To do so, we use information on respondents being asked: (i) *the perceived contribution of the target group to society* (on a 0-10 score); (ii) *how positive the respondent feels about the target group* (on a 0-10 score). We again find no differences in views towards outgroups, and views towards the ingroup, between ZSTs and non-ZSTs.

results from the BES to similar measures of ZSTs in Chinoy *et al.* [2026] – so understanding differences across UK and US contexts using a similar approach to identify zero-sum mindsets.[28]

5.1 A Standard Measure of Zero-sum Thinking

Zero-sum thinking is measured in the BES through six items, each answered on a five-point scale from strongly disagree to strongly agree. Three items are phrased in the zero-sum direction: (i) *one person's loss is another person's gain*; (ii) *the only way to make someone better off is to make someone else worse off*; (iii) *there's only so much to go around. Life is about how much you get.* The other items are phrased in a positive-sum direction: (i) *everyone can be a winner at the same time*; (ii) *there are ways to make everyone better off without anyone losing out*; (iii) *life isn't about winners and losers, everyone can do well.* The positive-sum items are reverse-coded so that higher values always correspond to stronger zero-sum thinking.[29]

Following Chinoy *et al.* [2026], we construct an index from these six items using a single principal-component. We standardize the items and extract the first principal component, capturing the common zero-sum dimension underlying the items and that accounts for 41% of the variance. We orientate the component so that higher scores denote more zero-sum thinking and rescale it to the unit interval.[30]

5.2 Method: BES vs. Experiment

We compare characteristics of ZSTs from the BES-derived measure to those revealed through our experiment – both in the UK context. To make this comparison, we move from using a dichotomous classifier of ZSTs to using two continuous measures of revealed zero-sum thinking: control respondents' predicted conditional treatment effect under each narrative. To place these measures on a common scale to the BES-derived continuous measure, each is standardized to mean zero and unit variance within its own sample using survey weights.[31]

[28] The BES sample mirrors our survey experiment sample in the following ways: (i) it is an online panel survey of the British electorate; (ii) we focus on Wave 14 of the BES, fielded in May 2018, immediately following the May 2018 local elections so also at time of heightened political debate in the UK; (iii) we impose the same sample restrictions as in our main analysis: focusing on White British respondents, yielding a sample of 5,145 individuals; (iv) throughout, we apply the BES Wave 14 survey weights so all statistics are population-representative. Table A10 shows characteristics of our working sample from the BES: this closely mirrors our survey experiment sample in terms of demographic characteristics (Panels A and B). On political characteristics (Panel C), the BES sample is slightly more politically engaged and more right-leaning than in our survey experiment.

[29] The BES items differ from those of Chinoy *et al.* [2026]: as shown in Table A1, their baseline measure is built from four domain specific questions – about income, ethnic groups, citizenship, and international trade – whereas the BES items are general, domain-free statements of the same zero-sum mindset. What we borrow from Chinoy *et al.* [2026] is the construction of the index, not the question wording.

[30] Figure A7 plots the distribution of the BES zero-sum index. This is right-skewed and concentrated below the midpoint of the scale. The BES supplies its own zero-sum score constructed via item-response theory, which we retain as a cross-check. Descriptives for both measures are reported in Panel D of Table A10.

[31] An alternative, but slightly more arbitrary, approach to make the comparison would be to reduce the BES-measure to a binary classification. To match the proportion of ZSTs precisely identified from our CATE estimates,

We compare ZST to non-ZST respondents across seven characteristics measured in both surveys, that can also be grouped in similar ways across surveys. Figure 7 shows the results. Panel A shows four demographic characteristics: gender, education, household income, and age. For all three indices, ZST is stronger among lower educated, lower income and older individuals, with gender differentials in ZST being minimal. While gradients within classifications of each dimension are of the same sign, the experimentally revealed measures show far stronger gradients with education, income and age, than the BES-survey measure.

Panel B considers respondent attitudes: political leaning and whether respondents favor reducing immigration. Each measure picks up that ZST is highest among right-leaning respondents and lowest among left-leaning respondents, but the gradients are again less pronounced in the BES-survey measure than for the experimentally revealed measures. The same pattern holds for views of reducing immigration.

Finally, Panel C shows how each measure varies with the share of the population that is non-White in the local authority of respondents (binned into quartiles). This reveals a divergence in what the BES and revealed measures show. While the BES measure shows that the prevalence of ZST hardly varies with exposure to non-White populations, this is not so for the revealed measures. For those exposed to the narrative of minority success, ZST is more strongly associated with lower minority shares in their vicinity, while the opposite is true for those exposed to the narrative of minority disadvantage.

This comparison demonstrates the main advantage of a revealed approach to identifying ZSTs – identifying distinct types of ZST. The BES measure does not pick up these types by design, nor does it consistently pick up a weighted average on observables of the types of revealed ZST. Standard direct question-scale approaches to identifying ZSTs might not do as well in picking up subtle differences between ZSTs and others because of social desirability biases arising when respondents are asked such questions. This is likely to be the case when zero-sum thinking remains a minority view, potentially one that is stigmatized, and one less associated with the kinds of higher income/education characteristics that survey analysts might be perceived to have. On the other hand, our ZST measures are derived from attitudes in one context: how the majority views minority outgroups and their ingroup, while scale-based measures are less context specific. This opens up an agenda to understand different ways in which to elicit zero-sum mindsets.[32]

BES respondents could then be classified as a zero-sum thinker if their index lies in the top 20% highest values.

[32] As Bursztyn *et al.* [2025] discuss, social desirability biases can arise from: (i) social image concerns (when anonymity is uncertain), as perceived reputational risks incentivize respondents to align responses with social norms; (ii) concerns about social stigma or judgment may motivate respondents to underreport potentially stigmatized behaviors rather than appearing prejudiced; (iii) self-image concerns can still arise when answers are given anonymously, when individuals feel psychological discomfort in acknowledging attitudes that conflict with their internal moral standards or ideal self-image.

5.3 Context: UK vs. US

To compare ZSTs across contexts using similar approaches, we compare characteristics of ZSTs in the UK BES-derived measure with that from the US sample of Chinoy *et al.* [2026]. We can do so along four dimensions: gender, education, household income, and age. We bin the BES demographics to match the categories reported in Chinoy *et al.* [2026], and place both indices on a common scale by standardizing each to mean zero and unit standard deviation. Figure 8 plots the average zero-sum score by demographic group for each source: the BES (in black, with 95% confidence intervals around the weighted group mean), and Chinoy *et al.* [2026] (in red, shown as point estimates).

Qualitatively, the demographic characteristics of zero-sum thinkers align closely between the UK and US: men, lower educated and lower income individuals are more strongly associated with ZST in both countries. The largest divergences are in terms of: (i) the prevalence of ZST among the very highly educated: in the US those with postgraduate education have the strongest ZST mindset, while in the UK they have the weakest and the relationship between ZST and education is monotonic; (ii) the age profiles of ZSTs: in the US, zero-sum thinking is more associated with young individuals while in the UK it is more associated with older ones.[33]

6 Conclusions

At the heart of economic progress in democracies lies the willingness of political actors to propose and explain win-win policies. For voters to support such win-win policies requires that they do not perceive the world in zero-sum terms. This is a fundamental reason for trying to understand the prevalence, nature and malleability of zero-sum mindsets [Stantcheva 2024, Ali *et al.* 2025]. We have set out a new method using a survey experiment to reveal zero-sum thinking as it applies to inequalities across racial/ethnic groups in the United Kingdom. Our design goes beyond documenting the prevalence of ZST among the majority population, to also identify what triggers ZST. We identify distinct narrative triggers of ZSTs, depending on whether mindsets are revealed through narratives of minority success or of disadvantage. We show that ZST can be triggered by light-touch interventions typical of transient information exposure in daily life, even at a time of heightened political debate around a high-stakes general election campaign, where immigration was a salient issue, often discussed in zero-sum terms.

At the same time, we document that only 7.6% of respondents can be classified as a ZST under both narratives, far from the full overlap a single underlying trait would imply. In short, we reveal

[33] The differences across ages can be driven by cohort effects. Chinoy *et al.* [2026] document that accounting for year of birth and country of birth fixed effects, early life exposure to economic growth is negatively associated with zero-sum thinking in contemporary times. Relatedly, Besley *et al.* [2026] document using data on three million respondents in 166 countries since 1990 that cohorts experiencing stronger episodes of economic growth are more likely to express trust in government later in life.

distinct types of ZST identified from exposure to each narrative of minority success or minority disadvantage, rather than a single, more deep-rooted group of ZSTs, who would be triggered by exposure to either narrative.

Our analysis leads naturally to a host of avenues for future work: (i) on the precise ways in which narratives that trigger zero-sum mindsets are framed, as well as how win-win attitudes can be stimulated by similarly light-touch narratives; (ii) whether and how results might differ based on alternative cleavages, such as issues spanning borders (trade, immigration), vertical inequalities (wealth, income, class) or other dimensions of horizontal inequalities (rural-urban, gender); (iii) whether minorities also exhibit zero-sum mindsets.[34]

We conclude by focusing on directions of research we find particularly important to pursue given our findings and the collective evidence now established on zero-sum mindsets.

The first is understanding incentives for political leaders to strategically use triggers to induce zero-sum thinking – a specific form of exacerbating divisions for political gain [Glaeser 2005]. We document that ZST is exhibited by individuals from across the political spectrum, and that swing voters and those that voted for the UK to leave the EU, are overrepresented among ZSTs. ZST can not only be exploited to strengthen divides, but as we show, can be triggered by messaging that results in individuals positioning themselves as gaining only at the expense of others. While our narratives were purposefully designed to be emotionally neutral, that is of course not the case for real world narratives supplied by politicians. Those narrative, might for example, be explicitly framed around conflict and anger. Intriguing evidence on conflictual political discourse comes from Algan *et al.* [2026], who document the rise of anger in US political communication, that emotional online communication receives more engagement on social media, and that experimental exposure to emotional messaging impacts policy views of voters: negative emotions increase support for protectionism and restricting immigration for example – classic issues framed in zero-sum terms. This suggests a rich agenda exists studying the supply of zero-sum narratives, how they are framed, and which outgroups are referred to.[35]

[34]The Online Appendix presents some preliminary analysis on this last issue. Our sample includes a sufficiently large sample of Indians to enable us to explore and compare their responses. Among other results we show that: (i) perceptions of Indians of the economic outcomes of their ingroup are closer to reality than the perceptions held by the majority ingroup towards Indians; (ii) Indians and White British respondents hold similar views of the opportunities available to each group; (iii) on non-targeted policies, Indian respondents are more supportive of investing in education and training and of financially supporting families with children, suggesting a degree of self-interested reasoning given Indians' educational success and their greater likelihood of living in families with children. Indian respondents exposed to the narrative of success – which informs them Indians are performing well in education – become significantly less supportive of financially supporting families with children (by 30pp), entirely closing the gap with White British respondents. Indian respondents exposed to the narrative of disadvantage – which does not mention Indians, but informs them that other minorities face higher unemployment – increase their support for teaching children about British values by 23pp (48% of the Indian control mean), overtaking support among White British respondents and reaching the level among right-leaning White British respondents. This is consistent with Indians holding hierarchical attitudes that regard other minorities as lacking the integration that might generate the economic success Indians enjoy, and points to attitudes between minorities as another important direction for future research.

[35]Ottinger and Posch [2025] provide historical evidence on the issue from the late 19th century Southern US.

The second issue is the relationship between the rise of populist sentiments and zero-sum thinking [Guriev and Papaioannou 2022]. Populist movements can often be classified through particular zero-sum worldviews – where outgroups and ingroups might span borders, or vertical and horizontal inequalities within a society. On the link from exposure to populist leaders to zero-sum mindsets, evidence from Ajzenman *et al.* [2026] – combining data on populist leaders with surveys of respondents across 50 countries – shows greater exposure to such leaders during early adulthood is associated with stronger zero-sum beliefs and voting intentions later in life. They present evidence the key mechanism for this is exposure to populist rhetoric, rather than experiences of economic hardship. More broadly, the existence of ZST makes room for a certain kind of policy-making, emphasizing potential winners and losers, and reducing support for cross-group transfers by framing economic relations as inherently competitive [Ali *et al.* 2025]. This process might then lie behind the economic losses associated with populist governments [Funke *et al.* 2023]. Better evidencing these processes, and the feedback loop between populist leaders and zero-sum mindsets, seems essential to understanding apparently counterintuitive policy preferences, and the dynamics of social attitudes and democratic choices.

A Appendix

A.1 Survey Design and Validation

We discuss some of the issues raised by Haaland *et al.* [2023] and Stantcheva [2023] on the use of online survey experiments, approaches to establishing the validity of samples, ensuring the attention of participants, and validating data quality.

Design As with other online panels, *YouGov* incentivizes respondents with survey points for completing questionnaires, that can be redeemed for rewards. Earlier work has shown the similarity of offline and online respondents in political opinion surveys [Grewenig *et al.* 2022], and Haaland and Roth [2023] find similar survey experiment results from a representative online panel and a probability-based sample. As described in Stantcheva [2023], the literature is replete with examples validating self-reported beliefs in surveys with real world behaviors.

The background of the 2024 General Election campaign offers a consistent priming context across treatments. However, to capture baseline responses including any priming effects of the survey introduction, we asked respondents up front (before the treatments were implemented) their views on: (i) vertical inequalities across domains; (ii) horizontal inequalities between groups. The results in Table A11 show that views among our respondents line up closely with other online

They show the Democratic Party had strong political incentives to incite racial outrage, and Democratic elites had influence over newspapers. They present evidence in line with Democratic elites orchestrating a campaign of anti-Black propaganda to sway white voters.

surveys conducted either earlier in 2024 prior to the General Election being announced [Fairness Foundation 2024] or those conducted in times of aggregate uncertainty such as the pandemic [Benson *et al.* 2024]. Specifically we find that: (i) the majority of respondents are concerned over vertical inequalities along any given dimension, with the top ranked dimension being inequalities in wealth and income (80%); (ii) horizontal inequalities along ethnic lines are considered the most pressing problem, likely reflecting in part the topic priming, but again at least 40% of respondents report concern on any given dimension of horizontal inequality.[36]

Attention The modal survey response was received on 26th June 2024, eight days before the General Election. The median survey response time was 19 minutes across treatment arms, with a slightly longer right tail of response times for those exposed to the narrative of disadvantage. This survey length is similar to Alesina *et al.* [2024] where the median completion time was 21 minutes. To check for those that either rush through the survey or are inattentive, we redo our analysis in Tables 1 to 4 dropping the bottom and top 2% of survey response times. Doing so, we find near identical results both for control means and treatment effects.

Biases Survey biases include: (i) moderacy, extreme response and acquiescence bias – relating to respondents following systematic response patterns irrespective of the question; (ii) experimenter demand effects (EDE) – where treated respondents form views about the experimenter's expectations that differs from controls; (iii) social desirability bias (SDB) – that stems from respondents' desire to project a positive self-image.

To check for (i) we measure the share of responses to which an individual reports 'don't know' or 'not sure', and the share of responses for which an extreme response is given. Columns 1 to 3 of Table A12 show that among White British respondents in controls, 16% of responses are don't know or not sure. This does not change much with either treatment. Columns 4 to 6 show that around 24% of responses are at the extremes of available options, and this is relatively stable across treatments. The next row shows that minority respondents are less likely to report don't know or not sure when exposed to the narrative of disadvantage, and are more likely to report extreme responses in each treatment arm. The second panel repeats the analysis by political leaning of White British respondents. Political leaning does not strongly correlate to the proportion of don't know or not sure responses but as expected, centrists are less likely to provide extreme responses than left- and right-leaning individuals. The final panel considers response patterns by survey duration, comparing those in the top/bottom five percentiles in response time to others. Those with the fastest response time are significantly more likely to report don't know or not sure, but

[36] A *Fairness Foundation* survey, from which we took some questions, conducted in January 2024 on 2050 adults in the UK, found that: (i) the top ranked dimension of vertical inequality was also wealth and income, considered by 75% of respondents as being a big or moderate problem; (ii) 58% of respondents considered horizontal ethnic inequalities to be a big or moderate problem, ranked above concerns over other dimensions of inequality such as gender, religion, age and sexual identity [Fairness Foundation 2024].

response time does not relate to the incidence of extreme responses.

Solutions to EDE and SDB are often quite similar. Online surveys relieve some forms of social pressure that can arise in face-to-face surveys as respondents take the survey in private. Anonymity has been argued to be a powerful guard against such biases [Hoffman *et al.* 1994]. In our study, *YouGov* inform respondents of their data privacy and anonymity. Moreover, (i) the survey methodology literature indicates lower problems with SDB in online surveys [Tourangeau and Yan 2007]; (ii) fielding the study in the build-up to the General Election not only acted as a general prime, but was a period when a wide range of views were being expressed on issues relating to immigration, diminishing the influence of one specific norm.[37]

As a further check, we exploit the fact that some of our questions are drawn from existing, high-quality, representative surveys. Following the suggestion in Stantcheva [2023], we use these as benchmarks and this serves as an extra validation beyond the comparison of socioeconomic characteristics. Table A13 shows how our responses align with ones from the surveys from which we took questions. These comparisons are made for the most recent survey and we note they sometimes refer to racial groups, rather than the fine-grained ethnic groups we base our analysis on. In most cases the outside estimates and our control means are well aligned.

A.2 Heterogeneous Treatment Effects

To establish heterogeneous treatment effects, that underlies our classification, we use a check in the spirit of the sorted group average treatment effects of Chernozhukov *et al.* [2018]. More precisely, we sort respondents by their predicted CATE, and then estimate the effect of each narrative separately for those with high and low predicted effects, using their actual responses. If the predicted CATEs only reflect noise around a common treatment effect, the two groups should respond similarly; if they capture real differences in how individuals respond, those predicted to respond more should do so. We focus on two outcomes: perceptions that White British individuals lack educational opportunities (revealed from the narrative of disadvantage) and perceptions of discrimination against White British job applicants (revealed from the narrative of success).

For these outcomes, we define the groups in two ways. First, we compare those whose predicted CATE lies above/below the ATE. Second, we compare respondents whose predicted CATE lies above the upper bound of the 95% confidence interval of the ATE to those whose predicted CATE lies below its lower bound. For each comparison, we regress the outcome on the treatment indicator, an indicator for the high group, and their interaction, controlling for the covariates used in the causal forest.[38]

[37] The *YouGov* policy is something all users read and consent to, and contains sections on *How and why we use your personal data*, *How we store and protect your personal data*, and make clear that data collected is used to produce aggregated and anonymous research. Further details are available here: https://account.yougov.com/gb-en/account/privacy-policy.

[38] The estimates can be interpreted as treatment effects because: (i) as narratives are randomly assigned and group membership depends only on covariates, treated and control respondents within each group are comparable; (ii)

Table A14 shows the results. Across both narratives and group definitions, respondents predicted to respond more do so: treatment effects are larger and statistically significant among those with high predicted CATEs, and mostly insignificant among those with low predicted CATEs. For the narrative of success, among respondents with predicted CATEs above the ATE, exposure to the narrative raises the perceived discrimination rank of White British applicants by .445 ($p = .000$), while among those below it the effect is .102 and not different from zero ($p = .110$). The difference between the groups is .343 ($p = .044$ using a two-sided test; $p = .022$ using a one-sided test). Comparing those above and below the confidence interval of the ATE, the effects are .617 ($p = .003$) and .498 ($p = .007$) respectively, with the difference of .118 being less precisely estimated. For the narrative of disadvantage, the effect among those above the confidence interval of the ATE is .027 ($p = .083$) and .020 among those below it ($p = .812$). The corresponding differences are positive but imprecisely estimated.

A.3 Standard Errors

The variance of $\widehat{\beta}^s$ has two components:

$$\mathrm{Var}\left(\widehat{\beta}\right) = \underbrace{\mathbb{E}_D\big[\,\mathrm{Var}(\widehat{\beta} \mid D)\big]}_{\text{second-stage sampling}} + \underbrace{\mathrm{Var}_D\left(\mathbb{E}[\widehat{\beta} \mid D]\right)}_{\text{classification uncertainty}} . \tag{5}$$

The first term is the ordinary sampling variance of the OLS estimator given the classification; the second is the additional variance arising because D is itself estimated from the first-stage forest. Rather than estimating the two components separately, we estimate the total directly with a nested nonparametric bootstrap of the full two-step procedure [Efron 1979, Horowitz 2001]. In each replication $b = 1, \ldots, B$ (with $B = 1000$) we: (i) resample respondents with replacement, stratified by treatment so that treatment arm sizes remain at their realized values; (ii) refit the causal forest on the resampled data, with the same covariates and specification as in the main analysis; (iii) re-derive the classification by applying (3) to the replication's own CATE and variance estimates; and (iv) re-estimate (4) on the resampled data. As every replication reruns the entire pipeline on its own sample, both components of (5), and their covariance, enter the bootstrap distribution jointly. Refitting the forest redraws the full CATE vector while preserving the cross-respondent correlation induced by shared trees, and re-estimating on the resampled data restores the ordinary sampling variation.

Confidence intervals apply the bootstrap principle to the estimation error: the distribution of $\widehat{\beta} - \beta$ is approximated by the bootstrap distribution of the deviations $\widehat{\beta}^*_b - \widehat{\beta}$, since $\widehat{\beta}$ is the true parameter of the bootstrap world. Writing q_α for the α-quantile of the replicate coefficients

each individual's predicted CATE is constructed only from trees grown on subsamples that exclude that individual, and within each tree separate subsamples are used to determine splits and to estimate effects – so an individual's own response plays no role in determining the group to which they are assigned.

$\{\widehat{\beta}_b^*\}_{b=1}^B$, the equal-tailed 95% basic bootstrap interval [Davison and Hinkley 1997], is given by:

$$\left[\,\widehat{\beta} - (q_{0.975} - \widehat{\beta}),\ \widehat{\beta} - (q_{0.025} - \widehat{\beta})\,\right] = \left[\,2\widehat{\beta} - q_{0.975},\ 2\widehat{\beta} - q_{0.025}\,\right], \tag{6}$$

in which the upper deviation quantile sets the lower endpoint and vice versa. We report (6) for the individual coefficients: every confidence interval shown for an individual coefficient is computed explicitly from (6), and a coefficient is marked significant at the 5% level when zero lies outside this interval. Unlike a normal-approximation interval, (6) is not in general symmetric around $\widehat{\beta}$: its endpoints are read off the empirical deviation distribution, so any asymmetry of that distribution enters the interval directly, moving both endpoints in the direction that corrects for it.

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Table 1: Economic Outcomes of Men

Regression coefficients, standard errors in parentheses, p-values in brackets

A. Graduate Shares

	(1) Quarterly Labor Force Survey	(2) Control Mean	[1 = 2]	(3) ΔNarrative of Success	(4) ΔNarrative of Disadvantage	[3 = 4]
Out of 100 men above the age of 25 in the UK from each of the following groups, roughly how many do you think have a university degree?						
Indian	.721	.355	[.000]	.034***	.006	[.016]
		(.008)		(.011)	(.011)	
Pakistani	.480	.260	[.000]	.038***	-.011	[.000]
		(.007)		(.010)	(.010)	
Black Caribbean	.275	.211	[.000]	.013	-.002	[.088]
		(.006)		(.008)	(.008)	
White British	.370	.360	[.131]	-.009	-.001	[.394]
		(.006)		(.009)	(.009)	

B. Employment Rates

	(1) 1 - Unem. rate	(1b) Share working	(2) Control Mean	[1b = 2]	(3) ΔNarrative of Success	(4) ΔNarrative of Disadvantage	[3 = 4]
Out of 100 men of working age in the UK from each of the following groups, roughly how many would you say are working?							
Indian	.963	.912	.784	[.000]	.018*	-.010	[.007]
			(.007)		(.010)	(.011)	
Pakistani	.947	.811	.760	[.000]	.010	-.023**	[.003]
			(.008)		(.010)	(.012)	
Black Caribbean	.944	.737	.742	[.483]	-.008	-.019*	[.303]
			(.007)		(.010)	(.012)	
White British	.973	.832	.724	[.000]	.001	.000	[.938]
			(.006)		(.009)	(.010)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regressions results of each outcome, a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Columns 1 and 1b report minority group estimates for men constructed from the Quarterly Labour Force Survey from January 2023 - December 2024.

Table 2: Opportunities

Regression coefficients, standard errors in parentheses, p-values in brackets

	(1) Control Mean	(2) ΔNarrative of Success	(3) ΔNarrative of Disadvantage	[2 = 3]	(4) Control Mean	(5) ΔNarrative of Success	(6) ΔNarrative of Disadvantage	[5 = 6]	(7) Control Mean	(8) ΔNarrative of Success	(9) ΔNarrative of Disadvantage	[8 = 9]
Do the following groups have few or no opportunities to...where you live?												
		Get a good education				**Get a job**				**Earn a decent wage**		
Indian	.103	-.027*	.004	[.035]	.139	-.003	-.007	[.788]	.200	.010	.007	[.869]
	(.010)	(.014)	(.015)		(.012)	(.017)	(.017)		(.014)	(.020)	(.020)	
Pakistani	.131	-.025	.019	[.009]	.175	-.015	.000	[.394]	.256	-.013	-.011	[.938]
	(.011)	(.015)	(.017)		(.013)	(.018)	(.019)		(.015)	(.021)	(.021)	
Black Caribbean	.153	-.012	.011	[.184]	.198	-.005	-.021	[.393]	.282	.015	-.014	[.185]
	(.012)	(.017)	(.018)		(.014)	(.019)	(.019)		(.015)	(.022)	(.022)	
White British	.066	.006	-.026**	[.007]	.097	.035**	-.018	[.001]	.156	.042**	-.013	[.004]
	(.009)	(.013)	(.011)		(.010)	(.016)	(.014)		(.013)	(.019)	(.018)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regressions results of each outcome, a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Respondents could answer each question with plenty, some, few or no opportunities, or don't know. We combine few and no opportunities for each outcome.

Table 3: Luck vs Effort

Regression coefficients, standard errors in parentheses, p-values in brackets

	Luck vs Effort Control Means [1] = [4]	(1) Control Mean	(2) ΔNarrative of Success	(3) ΔNarrative of Disadvantage	[2 = 3]	(4) Control Mean	(5) ΔNarrative of Success	(6) ΔNarrative of Disadvantage	[5 = 6]
Does lack of effort or bad luck generally have more to do with whether a person from each of the following groups is unsuccessful?									
			Bad luck				**Lack of Effort**		
Indian	[.000]	.240	-.009	.005	[.479]	.128	.005	-.015	[.229]
		(.015)	(.020)	(.021)		(.012)	(.017)	(.017)	
Pakistani	[.978]	.186	.011	.007	[.805]	.187	-.000	-.001	[.962]
		(.014)	(.019)	(.019)		(.014)	(.019)	(.019)	
Black Caribbean	[.284]	.174	-.019	.003	[.210]	.197	.010	-.005	[.448]
		(.013)	(.018)	(.019)		(.014)	(.020)	(.020)	
White British	[.000]	.074	.009	-.012	[.091]	.339	.026	-.000	[.259]
		(.009)	(.013)	(.013)		(.016)	(.023)	(.023)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regressions results of each outcome, a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Respondents could answer the question with bad luck, a lack of effort, an equal mix of both or don't know.

Table 4: Labor Market Discrimination

Regression coefficients, standard errors in parentheses
p-values in brackets

	(1) Control Mean	(2) ΔNarrative of Success	(3) ΔNarrative of Disadvantage	[2 = 3]
Imagine that the following men applied for a job, which they were qualified for. Please rank them based on how likely you think they would be to be turned down for the job.				
Indian	3.08	-.085	.129*	[.002]
	(.049)	(.068)	(.073)	
Pakistani	3.81	-.136*	.055	[.014]
	(.059)	(.080)	(.082)	
Black Caribbean	4.00	-.060	.043	[.239]
	(.062)	(.086)	(.089)	
White British	1.96	.245***	.012	[.013]
	(.062)	(.089)	(.091)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regression results of each outcome on a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Respondents ranked six hypothetical groups (of men) by how likely they would be to be turned down for a job they were qualified for; the outcome is the rank assigned to each group. Respondents could assign the same rank to multiple groups, and answer 'not sure'; the share answering 'not sure' is .19, .24, .25 and .26 for White British, Indian, Pakistani and Black Caribbean men, respectively.

Table 5: Zero-sum Thinking and Political Alignment

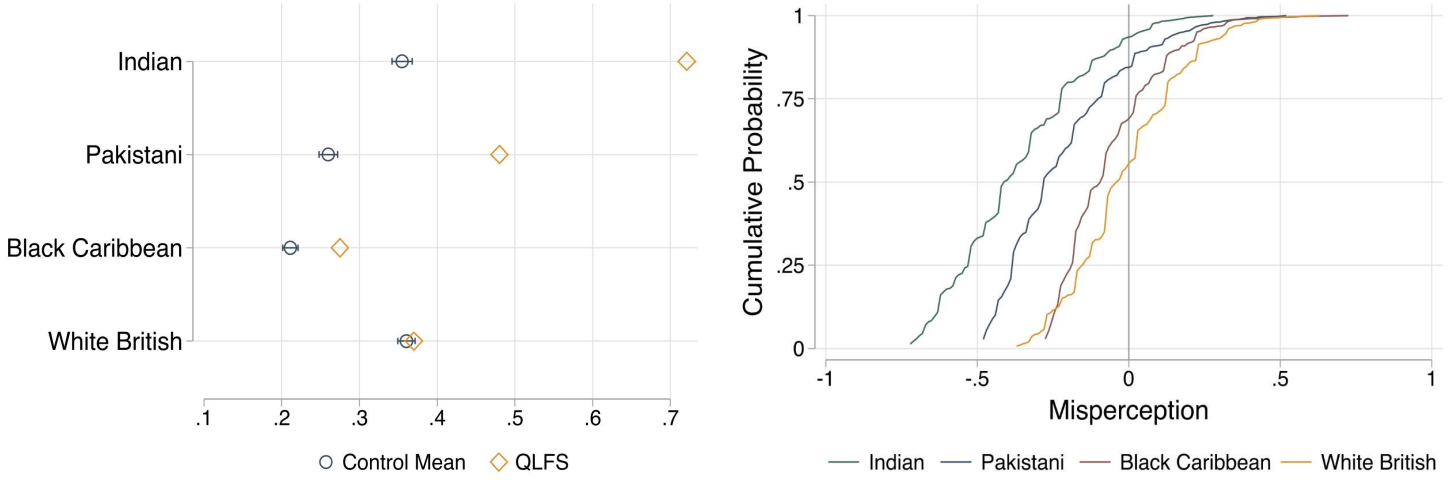
Share of respondents that show zero-sum thinking, p-values in brackets

	(1) Share in controls	(2) Narrative of Success	(3) Narrative of Disadvantage
Overall		.218	.241
A. Political Movers: actual party vote in 2019 General Election and intended party vote in 2024 General Election			
Moved to Left	.118	.296	.221
		[.008]	[.656]
Unchanged	.332	.239	.224
		[.782]	[.360]
Moved to Right	.156	.288	.314
		[.054]	[.034]
B. EU Referendum 2016 (Brexit)			
Voted	.783	.255	.250
		[.002]	[.458]
Voted Leave	.537	.318	.334
		[.002]	[.006]

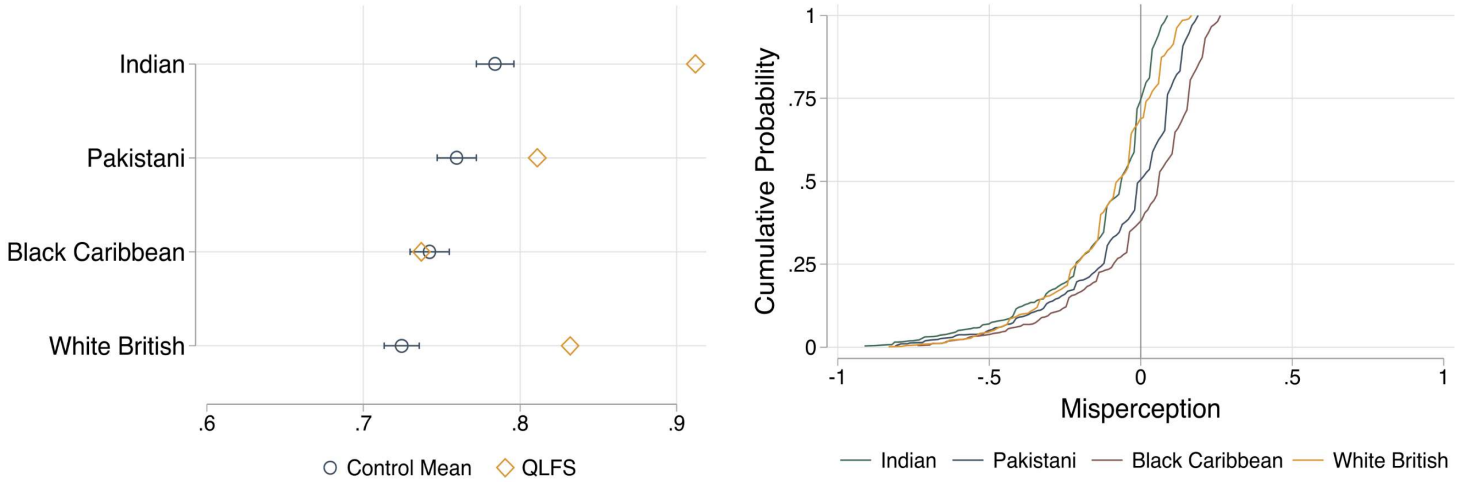
Notes: We report the weighted share of Control-arm respondents classified as zero-sum thinkers within each political subgroup, separately for the success- and disadvantage narrative classifications. The sample is restricted to White British respondents and observations are reweighted to fit the national profile. Zero-sum thinkers are identified from conditional average treatment effects estimated by a causal forest: success- (disadvantage-) narrative zero-sum thinkers are respondents whose 95% confidence interval on the success- (disadvantage-) prime individual treatment effect lies strictly above zero. The table reports shares across two sets of subgroups. Political movers (GE 2019 → GE 2024) classify respondents into Unchanged (voted for the same party at both elections), Moved to right (e.g. 2019 Labour → 2024 Conservative or Reform; 2019 Liberal Democrat → 2024 Reform/Conservative; 2019 Green → 2024 Labour) or Moved to left (e.g. 2019 Brexit Party → 2024 Conservative; 2019 Conservative/Brexit Party → 2024 Labour/Liberal Democrat) based on the respondent's reported 2019 General Election vote and 2024 General Election vote; respondents whose 2019 or 2024 vote is unknown, skipped, or who did not vote in 2024 are placed in an "Other moves" bucket that is not shown. EU Referendum vote uses the YouGov profile variable on the 2016 referendum: Voted (any) is the share among respondents who report having voted in the referendum, and Voted Leave is the share among respondents who report having voted to leave the EU. The Overall row uses the full Control-arm sample with a valid ZST classification. We test whether each group's share of ZST respondents is the same as the overall share and report the respective p-values. p-values test each subgroup's ZST share against the rest of the Control arm (equivalent to equality with the overall share). The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore use p-values from a bootstrap of the full estimation pipeline, with 1,000 replications; details can be found in the Appendix.

Figure 1: Perceived Economic Outcomes of Men

A. Graduate Shares

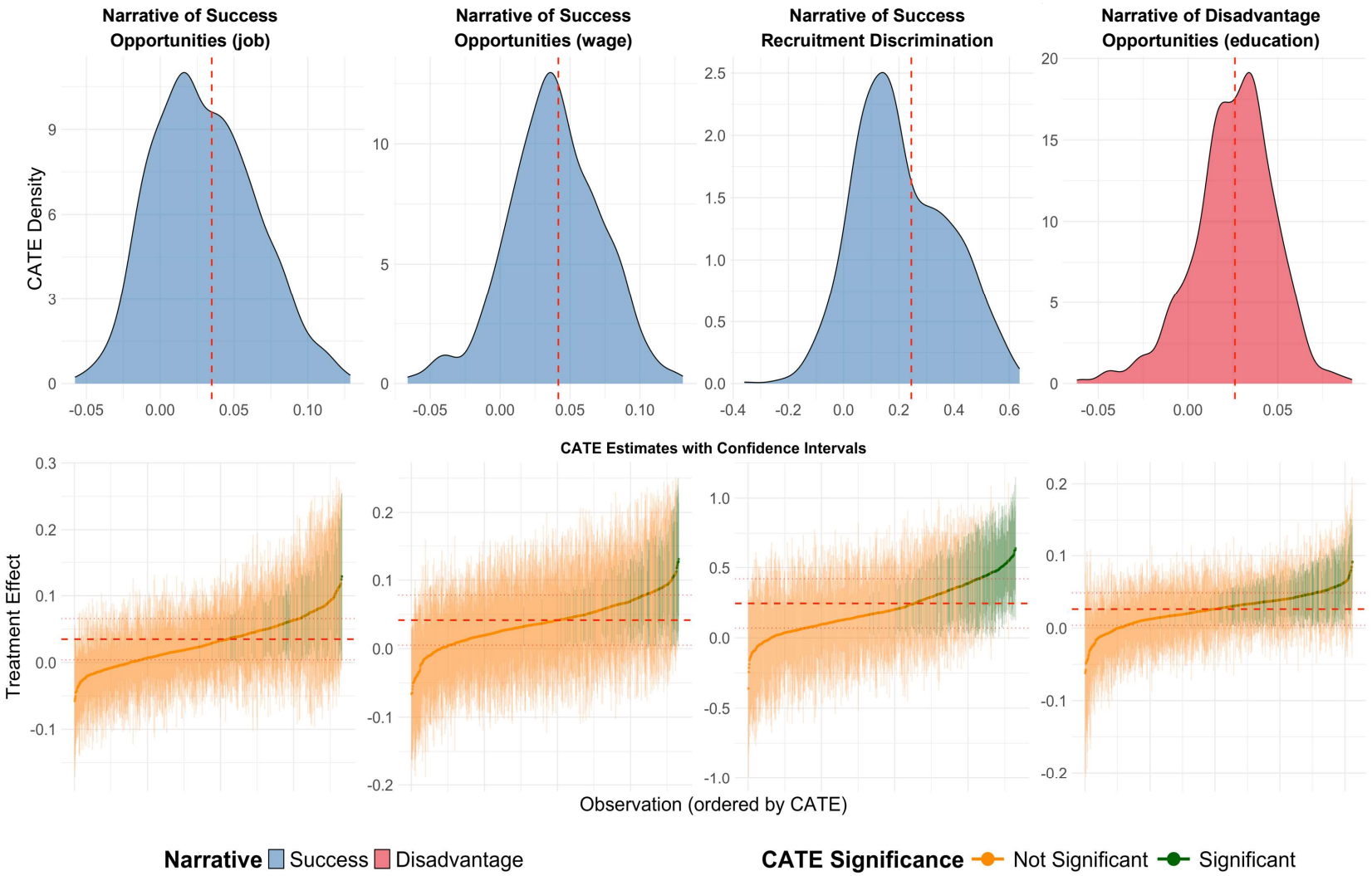


B. Employment Shares



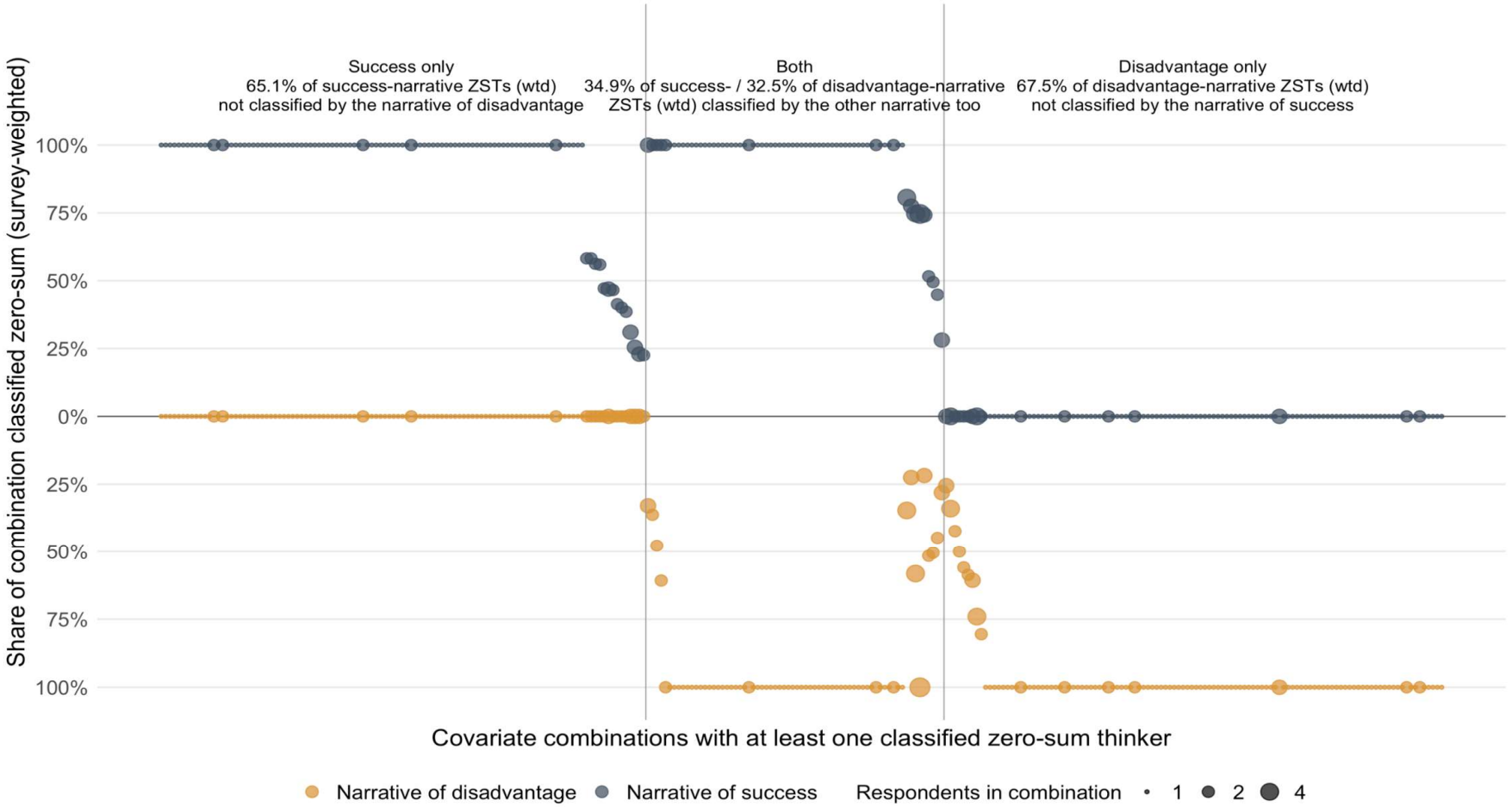
Notes: On graduate shares, we asked, Out of 100 men above the age of 25 in the UK from each of the following groups, roughly how many do you think have a university degree? On employment shares, we asked, Out of 100 men of working age in the UK from each of the following groups, roughly how many would you say are working? The left hand side of each panel reports means for respondents in the Control arm and the estimates from constructed from Quarterly Labour Force Survey (QLFS) from January 2023 - December 2024. The right hand side shows the distribution of misperception for respondents in the Control arm. The sample is restricted to White British respondents, observations are reweighted to fit the national profile. We calculate the degree of misperception by taking the control mean and subtracting the group estimates from the QLFS. Hence positive misperceptions imply the control mean is higher than the group estimate.

Figure 2: Treatment Effect Heterogeneity



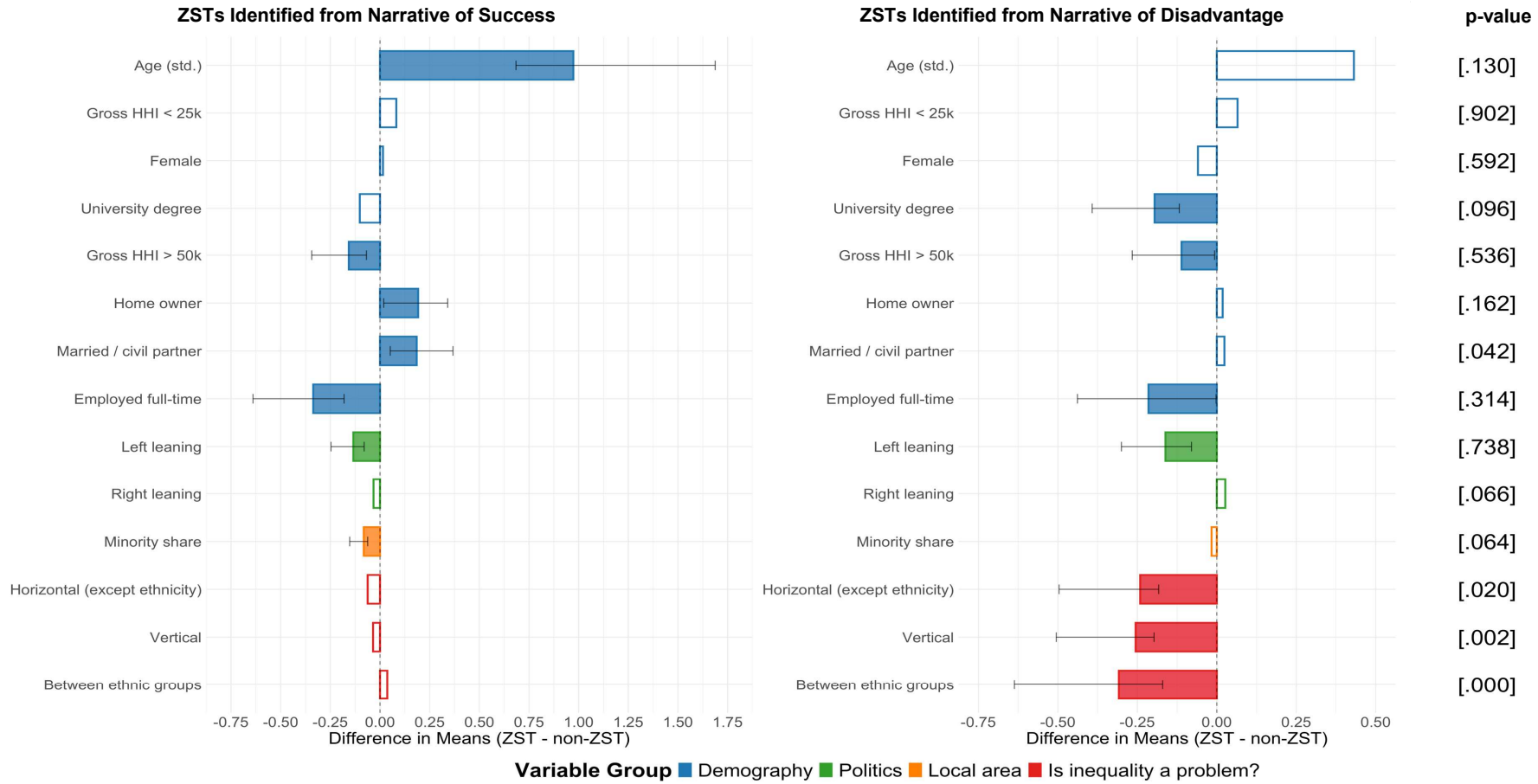
Notes: The figure provides an overview of treatment effect heterogeneity of the narrative treatment for four outcome variables. For each of the outcome variables, the top row shows a density plot of conditional average treatment effect (CATE) obtained from a causal forest for each of the four outcomes. A higher CATE estimate corresponds to a higher zero sum thinking (the estimates in the fourth panel have been adjusted to be consistent with this). The bottom panel plots CATE estimates for each respondent that has received a treatment in the survey. For each outcome, the respondents are ranked from the lowest to the highest CATE and plotted along with the 95% confidence interval. The red dashed and dotted lines plot the ATE and the corresponding confidence interval for each of the outcomes. The outcome in the first two panels is whether the White British ingroup has few or no opportunities for jobs and wages; in the third, the perceived likelihood that White British men are turned down for a job; and in the fourth, whether the ingroup has few or no opportunities for education. The variables used to generate heterogeneity in the causal forest are whether the respondent regards three types of inequality as a problem (between ethnic groups; horizontal inequalities other than ethnicity; vertical inequalities), age, gender, university degree, household income (below £25k; above £50k), political leaning (left; right), home ownership, being married or in a civil partnership, being employed full-time, and the local-area minority population share. All variables are either dummy variables that take the value of either 0 or 1 or variables with values between 0 and 1, except age, which has been standardized using control group mean and standard deviation. We grow 10,000 trees and use honest sampling to avoid overfitting. The sample is restricted to White British respondents and observations are reweighted to fit the national profile. The red dashed line in both panels display the ATE estimates and the red dotted line in the bottom panel displays 95% confidence intervals.

Figure 3: Overlap Between ZSTs Identified from Each Narrative



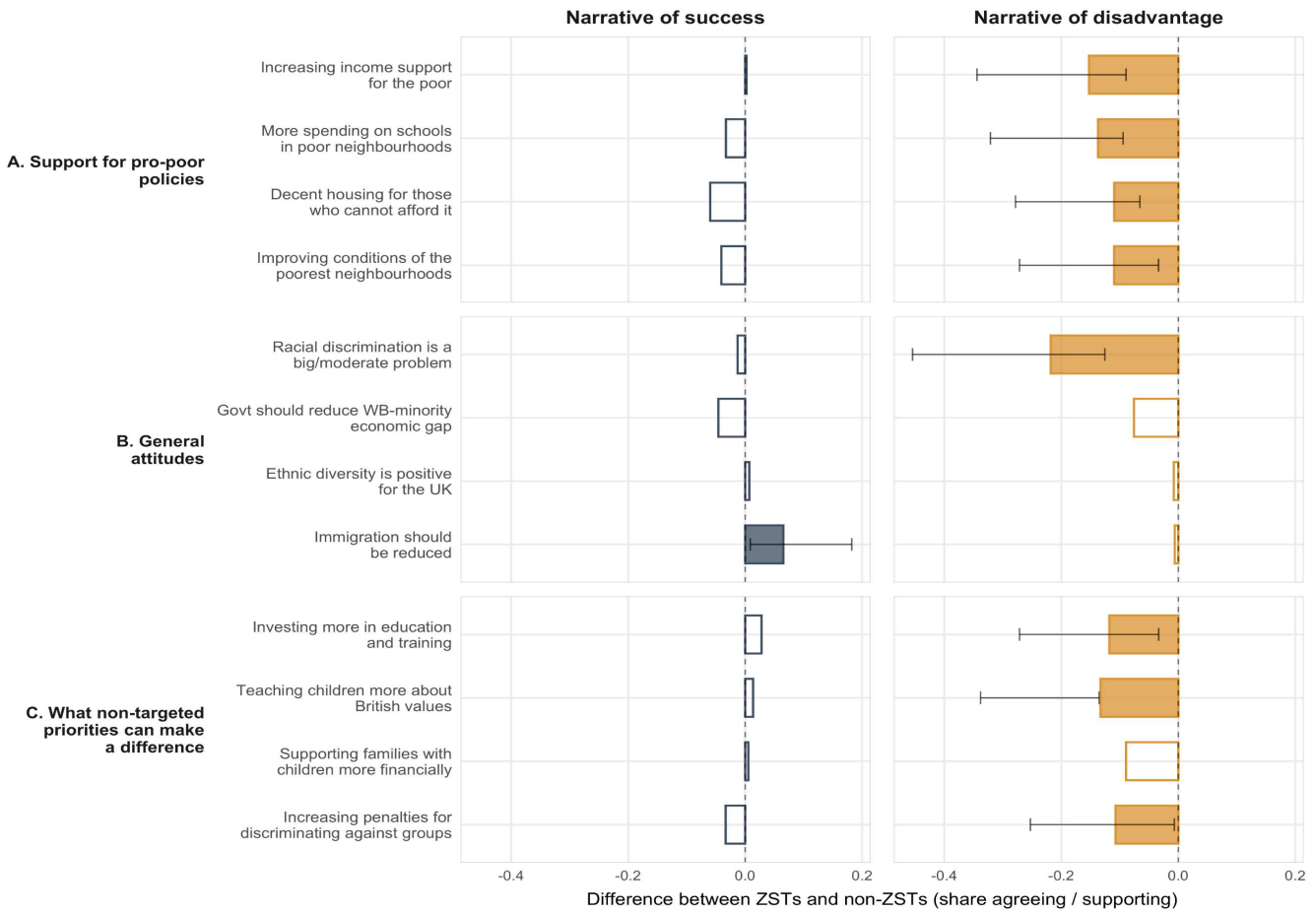
Notes: The sample is 864 White British respondents in the Control arm for whom both narrative models are defined; 214 of the 1,078 control respondents have no success-narrative prediction because they answered "Not sure" to the recruitment-discrimination outcome. Combinations are defined over the fourteen covariates used by the causal forests; the ten covariates not coarsened into terciles enter as observed. The 864 respondents fall into 746 combinations, of which the 293 containing at least one respondent classified as a zero-sum thinker (ZST) under either narrative are shown. A respondent is classified as a ZST under a narrative if the lower bound of the 95% confidence interval around their out-of-bag CATE from that narrative's causal forest is strictly positive. The middle block contains combinations in which at least one respondent is classified under each narrative; these need not be the same respondents. Within the left block, combinations are ordered by the success-narrative share (descending); within the right block, by the disadvantage-narrative share (ascending); and within the middle block, by the success-narrative share (descending) and then the disadvantage-narrative share (ascending). As no two control respondents share identical covariate values, shares strictly between 0 and 100 per cent arise only where a combination pools respondents who differ within a tercile bin. The percentages reported above each block are computed over respondents rather than combinations. Observations are reweighted to fit the national profile.

Figure 4: Characteristic Differences Between ZSTs and non-ZSTs



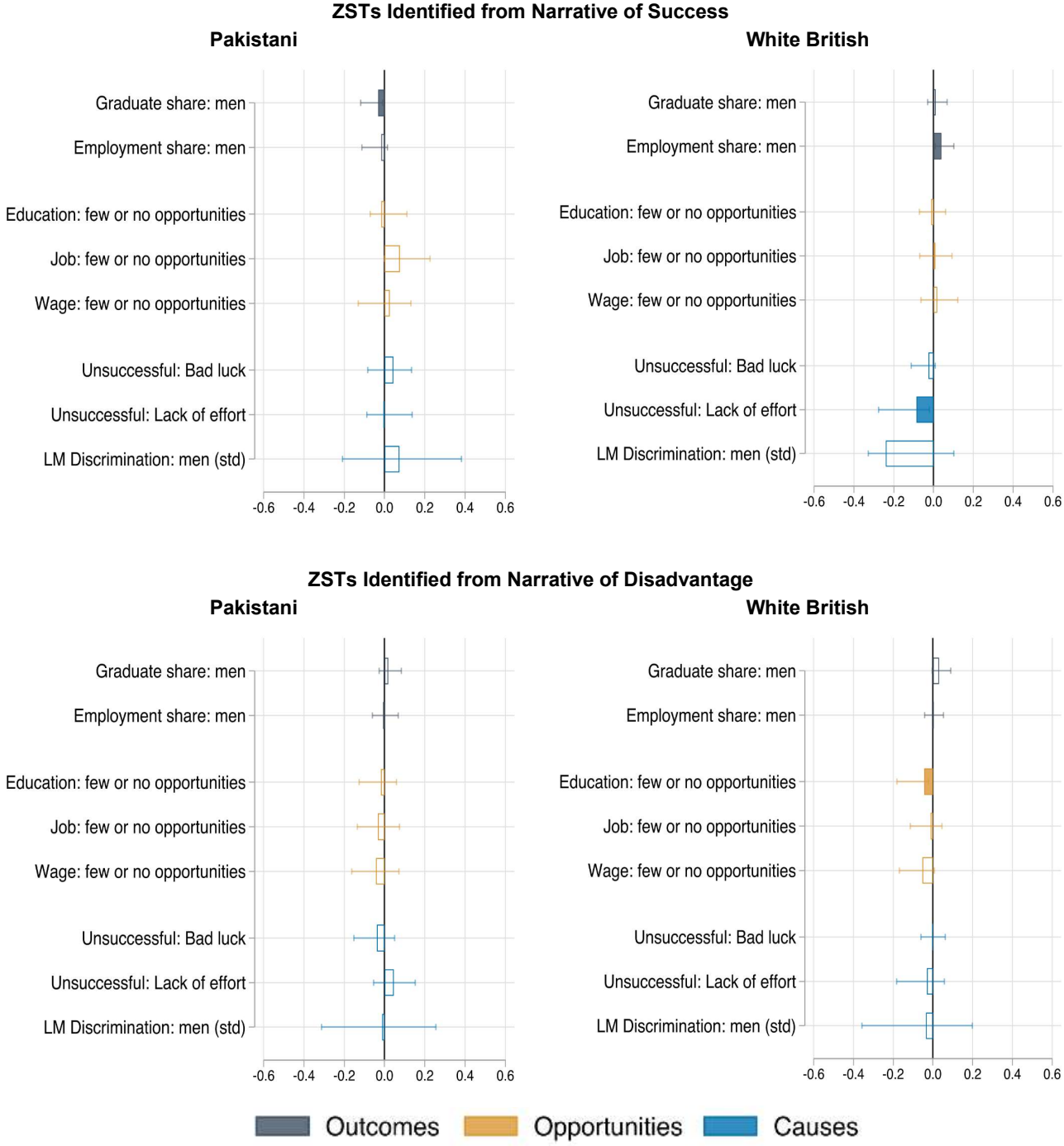
Notes: The figure plots differences in mean characteristics between control-arm respondents predicted to be zero-sum thinkers (ZSTs) and those predicted not to be. Predictions come from the causal forests described earlier: a respondent is classified as a ZST if the lower bound of the 95% confidence interval around their out-of-bag CATE is strictly positive, and as a non-ZST if that interval contains zero. We focus on two outcome variables - beliefs about whether the White British ingroup has few or no opportunities for education and the beliefs about discrimination faced by White British job applicants. The right-hand column reports the p-value of a test that the two panels' differences are equal. Every characteristic other than age is scaled 0–1, age is standardized to mean zero and unit variance using the weighted control-arm distribution. Minority share is the share of the population of the respondent's local authority that is not White. The three red bars measure whether the respondent regards inequality as a problem - vertical is mean across 5 domains (inequalities of wealth and income, health and life expectancy, educational attainment, political influence, and status and respect), horizontal is analogous mean across between-group inequalities (between people living in different regions, from different social classes, disabled and non-disabled people, men and women, people of different religions, gay and straight people, and people of different ages) and "between ethnic groups" is based on a single number about inequality between people from different ethnic groups. Home owner, married / civil partner and employed full-time are dummies (Demography). The sample is restricted to White British respondents in the Control arm and observations are reweighted to fit the national profile. The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore compute confidence intervals from a bootstrap of the full estimation pipeline with 1,000 replications: the 95% intervals shown are basic bootstrap intervals, read off the distribution of replicate coefficients around the estimate, and need not be symmetric; details can be found in the Appendix.

Figure 5: Policy Views of Zero-sum Thinkers



Notes: The figure plots OLS coefficients and 95% confidence intervals on an indicator for zero-sum thinking across three groups of dependent variables, shown as Panels A–C. The left column uses the narrative of success classification of zero-sum thinkers and the right column uses the narrative of disadvantage classification; the two types are identified independently, so each estimate comes from a separate regression. Panel A (Support for Pro-Poor Policies) reports estimates for four items asking the respondent's support for different areas of government spending; the dependent variable for each item is a dummy equal to one if the respondent answers "strongly support" or "tend to support", and zero otherwise (including "don't know"). Panel B (General Attitudes) reports estimates for four non-group attitudes: whether racial discrimination is a problem, whether government should reduce the White British–minority economic gap, whether ethnic diversity has a positive impact on life in the UK, and whether immigration should be reduced; the dependent variable is a dummy equal to one if the respondent answers 'a big problem' or 'a moderate problem' (discrimination), 'strongly agree' or 'agree' (racial gap), 'very positive' or 'fairly positive' (diversity), or 'reduced a lot' or 'reduced a little' (immigration), and zero otherwise (including 'don't know'). Panel C (What Non-targeted Priorities Can Make a Difference) reports estimates for four government actions to reduce inequalities between ethnic groups; the dependent variable for each action is a dummy equal to one if the respondent thinks the government could make "a great deal" or "a fair amount" of difference through that action, and zero otherwise (including "don't know"). Filled bars denote coefficients whose 95% confidence interval excludes zero; hollow bars those whose interval includes zero. Each regression conditions on age, gender, marital status, home ownership, education, employment status, household income, political leaning, and the local-area minority share. We additionally report, for each group and narrative, a joint Wald test of the null that the group's ZST coefficients are all zero, using the bootstrap covariance of the coefficients, reported only for the disadvantage narrative. The sample is restricted to White British respondents in the Control arm and observations are reweighted to fit the national profile. The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore report standard errors from a bootstrap of the full estimation pipeline, with 1,000 replications; details can be found in the Appendix.

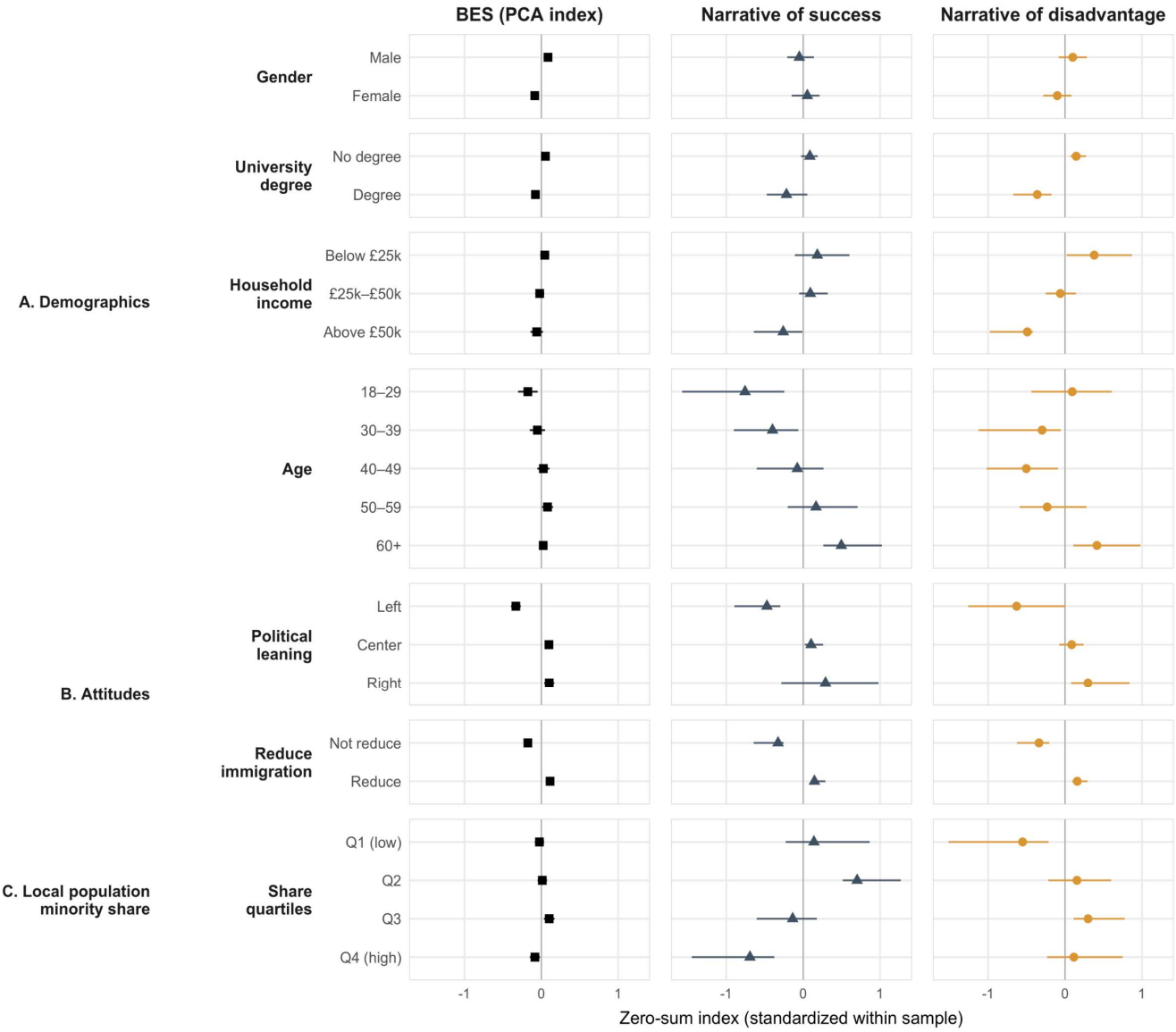
Figure 6: ZST and Perceptions of Others and the Ingroup



Notes: The figure plots OLS coefficients and 95% confidence intervals on an indicator for zero-sum thinking across eight group-specific perceptions, organised into four sub-panels: the top row uses the narrative of success classification of zero-sum thinkers and the bottom row the narrative of disadvantage classification, each shown for two target groups — Pakistani and White British. The two types are identified independently, so each estimate comes from a separate regression. Within each sub-panel the same items appear on the y-axis from top to bottom, organised into three colour-coded categories. The first category (Outcomes) covers perceived economic circumstances: the respondent's perceived graduate share and perceived employment share among men of the target group (0–1 proportions). The second category (Opportunities) contains three binary indicators equal to one if the respondent thinks members of the target group have "few or no opportunities" in education, jobs, and wages, and zero otherwise (including "don't know"). The third category (Causes) covers attributions of the group's outcomes: two dummies equal to one if the respondent attributes an unsuccessful member of the target group's outcome to "bad luck" or to "lack of effort", respectively, and zero otherwise (including "an equal mix of both" and "don't know"), and the respondent's perceived likelihood that men of the target group would be turned down for a job they are qualified for (1–6 ranking, coded so that higher values mean more likely to be turned down; z-standardized within the Control arm by target group). Filled bars denote coefficients whose 95% confidence interval excludes zero; hollow bars those whose interval includes zero. Each regression conditions on age, gender, marital status, home ownership, education, employment status, household income, political leaning, and the local-area minority share. The sample is restricted to White British respondents in the Control arm and observations are reweighted to fit the national profile. The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore compute confidence intervals from a bootstrap of the full estimation pipeline with 1,000 replications: the 95% intervals shown are basic bootstrap intervals, read off the distribution of replicate coefficients around the estimate, and need not be symmetric; details can be found in Appendix.

Figure 7: Comparison of Measures of Zero-sum Thinking

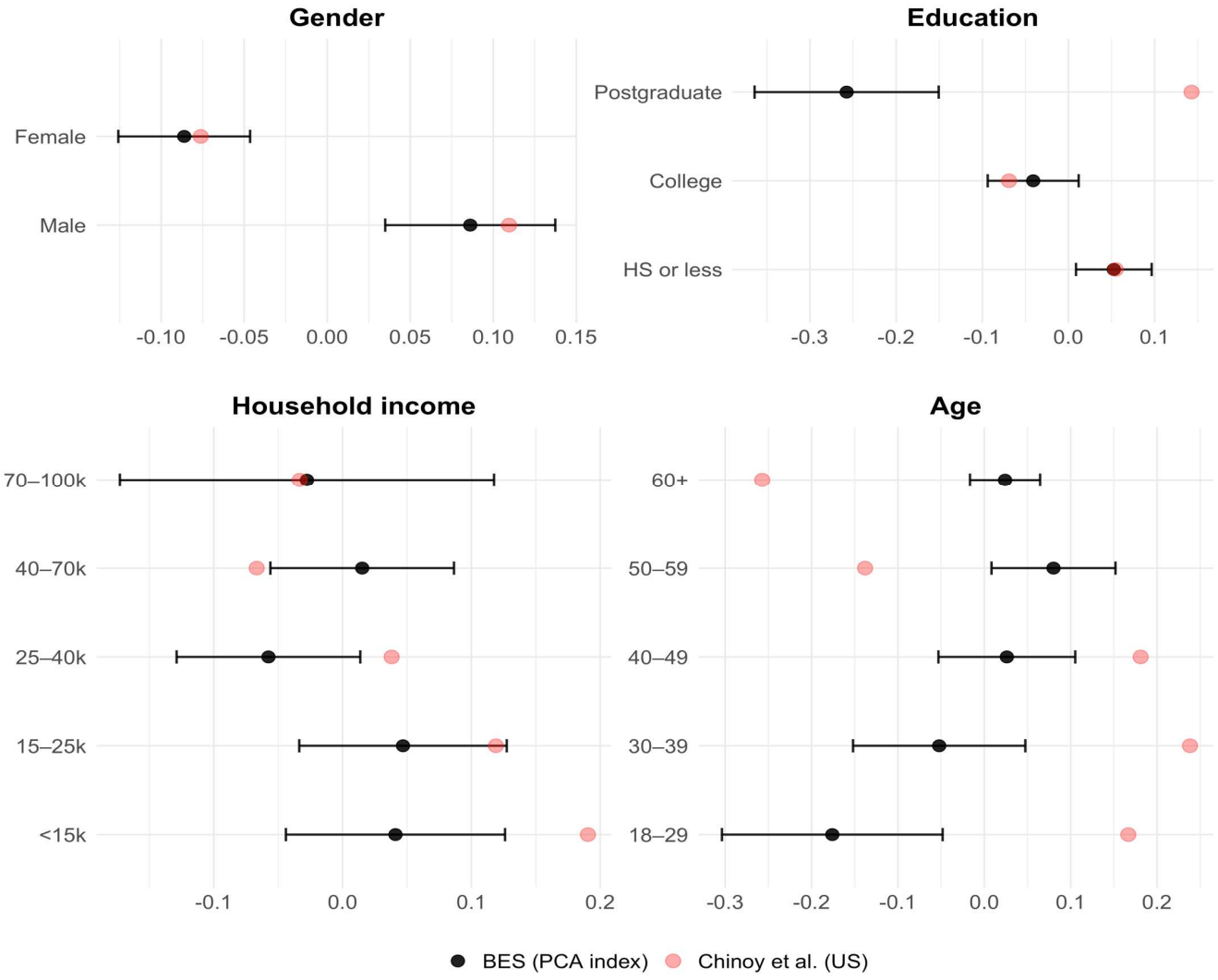
British election survey and experimentally revealed measures



Notes: Each panel shows mean standardised zero-sum thinking by respondent characteristic, in the British Election Study (BES) and in our survey experiment. For BES, we construct a zero-sum index (taking values in 0-1) following Chinoy et al. [2026] methodology, applying principal component analysis on six survey questions. Our survey contributes two continuous measures of revealed zero-sum thinking: respondents' predicted conditional treatment effects on beliefs under the narrative of disadvantage and under the narrative of success, for respondents in the control arm. To place the three measures on a common scale, each is standardised to mean zero and unit variance within its own sample using survey weights; panel entries are weighted group means with 95% confidence intervals. Household income is grouped into three bands and political leaning into left, centre, and right. Minority share is the share of the population of the respondent's local authority that is not White, grouped into within-sample weighted quartiles, so quartile boundaries differ across the three samples. Both samples are restricted to White British respondents, and observations are reweighted to fit the national profile; the survey series use respondents in the Control arm. For BES, the 95% confidence intervals of the weighted group means uses robust standard errors that account for the survey weights. The survey measures are themselves causal-forest estimates rather than observed data, so their intervals come from a bootstrap of the full estimation pipeline: in each of 1,000 replications we resample respondents with replacement (holding treatment-arm sizes fixed), re-estimate the causal forest, re-predict the Control-arm CATEs and recompute the standardised group means. The intervals shown are basic bootstrap intervals.

Figure 8: Zero-sum Thinking in the UK and US

British Election Survey and Chinoy et al. [2026]



Notes: Each panel shows how the zero-sum index varies for respondents that vary in the characteristic in question, in the British Election Survey (BES) and adds the estimates reported in Chinoy et al. [2026] for comparison. Our estimation of the zero-sum index follows the methodology employed in Chinoy et al. [2026] closely. Additionally, to ensure comparison, we standardize the indices to ensure they have a mean of 0 and a standard deviation of 1. Furthermore, for education and income variables, the individual groups did not align perfectly between the two data sources - we combined groups to ensure comparability. Sampling weights are used for the BES estimates.

Table A1: Examples of Attitudinal Scales to Measure Zero-sum Thinking

Rozycka-Tran et al. [2015] Belief in Life as a Zero-sum Game scale, developed from Wojciszket et al. [2009]

Successes of some people are usually failures of others
If someone gets richer, it means that somebody else gets poorer
Life is so devised that when somebody gains, others have to lose
In most situations, interests of different people are inconsistent
Life is like tennis game - a person wins only when others lose
When some people are getting poorer, it means that other people are getting richer
When someone does much for others, he or she loses
The wealth of a few is acquired at the expense of many
When a person does much for the good of others, he or she profits as well
Those who give much to others receive much from them
People who do much for their own good frequently benefit others as well
When the number of rich people increases in the country, the poorer people benefit as well

Chinoy et al. 2026:

In the United States, there are many different ethnic groups (Blacks, Whites, Asians, Hispanics, etc.). If one ethnic group becomes richer, this generally comes at the expense of other groups in the country
In the United States, there are those with American citizenship and those without. If those without American citizenship do better economically, this will generally come at the expense of American citizens.
In international trade, if one country makes more money, then it is generally the case that the other country makes less money
In the United States, there are many different income classes. If one group becomes wealthier, it is usually the case that this comes at the expense of other groups

Wilkins et al. 2015:

When women work they are taking jobs away from men
When women get rights they are taking rights away from men
Rights for women mean that men lose out
As women face less sexism, men end up facing more sexism
Less discrimination against women means more discrimination against men
Efforts to reduce discrimination against women have led to increased discrimination against men

Blacks take jobs away from Whites
When racial minorities get rights, they are taking rights away from Whites
Rights for Blacks mean Whites lose out
As Blacks face less racism, Whites end up facing more racism
Less discrimination against minorities means more discrimination against Whites
Efforts to reduce discrimination against minorities have led to increased discrimination against Whites

Bergeron et al. [2026] in context of Democratic Republic of Congo

People only make money when others lose money/no one needs to lose money for others to make money
Businesses only make money when others lose money/no one needs to lose money for businesses to make money
If one person in a village gets very wealthy, other people in the village will become poorer/If one person in a village gets very wealthy, other people in the village will not necessarily become poorer
In trade, if one party gains, the other party loses/In trade, it is possible for both parties to gain at the same time
A person can only gain power by taking it away from others/A person can gain power without taking it away from others
Gaining happiness requires taking it away from others/It is possible for everyone to be happy
If one farmer has a very large crop, his neighbor is likely to also have a very large crop/If one farmer has a very large crop, his neighbor is likely to have a small crop
The success of the wealthy generally helps other people in the community/The success of the wealthy generally hurts other people in the community
Most wealth is created without exploiting others/Most wealth is obtained by exploiting others
Most of the wealth of the rich was created without taking it from others/Most of the wealth of the rich was obtained by taking it from others

Table A2: Balance on Respondent Characteristics

Means, standard deviation in parentheses, p-values in brackets

	(1) 2021 Census	(2) Control Mean	(3) Narrative of Success	(4) Narrative of Disadvantage	[1 = 2]	[2 = 3]	[2 = 4]	[3 = 4]
Observations		1078	1098	1029				
A. Demographics								
Age	43.0	50.3	51.7	50.6	[.000]	[.102]	[.747]	[.185]
		(17.7)	(17.2)	(17.4)				
Female	.509	.497	.507	.547	[.518]	[.694]	[.042]	[.097]
Married	.435	.571	.560	.573	[.000]	[.649]	[.950]	[.607]
Born in UK	.974	.951	.961	.964	[.004]	[.315]	[.208]	[.779]
Urban	.824	.739	.721	.733	[.000]	[.408]	[.785]	[.589]
Home owner	.684	.589	.575	.595	[.000]	[.562]	[.787]	[.396]
B. Education, Employment and Income								
Undergraduate or higher	.339	.287	.258	.289	[.000]	[.160]	[.893]	[.131]
Employed	.587	.393	.397	.405	[.000]	[.878]	[.626]	[.737]
Gross HH Income Below £25K	.340	.340	.330	.312	[.978]	[.697]	[.278]	[.474]
Gross HH Income Between £25K-£50K	.329	.341	.355	.359	[.509]	[.575]	[.485]	[.875]
Gross HH Income Above £50K	.331	.319	.315	.329	[.472]	[.861]	[.694]	[.574]

Notes: We report descriptive statistics for the sample of White British respondents, where observations are reweighted to fit the national profile. The p-values are calculated from three pairwise regressions for each outcome on a constant and a treatment dummie, where robust standard errors are calculated. Undergraduate or higher includes respondents with at least a university or CNAA first degree (e.g. BA, BSc, BEd.). Column 1 reports outcomes for the White British population as estimated for the 2021 Census, unless otherwise stated. Gross Household income figures are calculated using 2022-23 Households Below Average Income (HBAI) dataset.

Table A3: Balance on Political Preferences of Respondents

Means, standard deviation in parentheses, p-values in brackets

	(1) Control Mean	(2) Narrative of Success	(3) Narrative of Disadvantage	[1 = 2]	[1 = 3]	[2 = 3]
Observations	1078	1098	1029			
A. Political Leaning						
Right	.088	.119	.102	[.029]	[.314]	[.247]
Centre	.482	.441	.455	[.087]	[.276]	[.557]
Left	.163	.169	.168	[.697]	[.736]	[.964]
B. Voting Intentions for General Ele						
Planning to Vote	.891	.882	.877	[.577]	[.426]	[.806]
Planning to Vote Labour	.336	.332	.325	[.869]	[.659]	[.779]
Planning to Vote Conservative	.146	.154	.139	[.622]	[.727]	[.398]
Planning to Vote Reform	.188	.193	.173	[.812]	[.473]	[.333]
Planning to Vote Liberal Democrat	.087	.086	.091	[.943]	[.784]	[.731]
Planning to Vote Green	.062	.053	.070	[.423]	[.505]	[.150]
Planning to Vote Some Other Party	.182	.183	.201	[.977]	[.353]	[.367]
C. General Election 2019						
Voted	.795	.818	.788	[.264]	[.746]	[.152]
Voted Conservative	.462	.472	.462	[.692]	[.988]	[.708]
Voted Labour	.294	.297	.278	[.902]	[.508]	[.428]
Voted Liberal Democrat	.104	.100	.123	[.833]	[.263]	[.177]
Voted Green	.021	.023	.035	[.763]	[.102]	[.188]
Voted Brexit Party	.029	.026	.011	[.739]	[.016]	[.028]
Voted Some Other Party	.090	.081	.091	[.540]	[.965]	[.523]
D. EU Referendum 2016						
Voted	.783	.800	.784	[.446]	[.988]	[.460]
Voted Leave	.537	.559	.550	[.380]	[.614]	[.725]

Notes: We report descriptive statistics for the sample of White British respondents, where observations are reweighted to fit the national profile. The p-values are calculated from an OLS regression for each outcome on a constant and treatment dummies, where robust standard errors are calculated. In Panel A, left-leaning respondents are those that report being 'very left-wing' and 'fairly left-wing'. Centre-leaning respondents combine those that report being 'centre', 'slightly left-of-centre' and 'slightly right-of-centre'. Right-leaning respondents are those that report being 'very right-wing' and 'fairly right-wing'. The shares for political leaning don't sum to 1 as some respondents don't have political leaning data (they either answered "Don't know" or skipped). In Panels B and C, the share of vote for individual parties are calculated using repondents who either said they were plannig to vote (Panel B) or that they had voted (Panel C); and thus sum up to one. The share for 'planning to vote for some other party' includes respondents that answered 'don't know'. In Panel D, the share who voted leave is computed only using repondents that voted in the referendum.

Table A4: Economic Outcomes of Men, by Country of Birth

Standard errors in parentheses

A. Graduate Shares

	Quarterly Labor Force Survey			Survey
	(1) All men	(1a) UK born	(1b) Not UK born	(2) Control Mean
Out of 100 men above the age of 25 in the UK from each of the following groups, roughly how many do you think have a university degree?				
Indian	.721	.694	.734	.355
				(.008)
Pakistani	.480	.510	.452	.260
				(.007)
Black Caribbean	.275	.267	.299	.211
				(.006)
White British	.370	.370		.360
				(.006)

B. Employment Rates

	Quarterly Labor Force Survey - Share working			Survey
	(1) All men	(1a) UK born	(1b) Not UK born	(2) Control Mean
Out of 100 men of working age in the UK from each of the following groups, roughly how many would you say are working?				
Indian	.912	.899	.918	.784
				(.007)
Pakistani	.811	.792	.829	.760
				(.008)
Black Caribbean	.737	.715	.806	.742
				(.007)
White British	.832	.832		.724
				(.006)

Notes: Columns 1, 1a and 1b display estimates from Quarterly Labour Force Survey from January 2023 - December 2024 and Column 2 displays the mean response from the control arm of the survey.

Table A5: Correlates of Perceptions of Economic Outcomes of Men

Regression estimates, robust standard errors, p-values in brackets

	Graduate Share				Employment Rate			
	(1) White British	(2) Indian	(3) Pakistani	(4) Black Carribean	(5) White British	(6) Indian	(7) Pakistani	(8) Black Carribean
Age	-.001**	-.001**	-.001	-.001**	.000	-.001	-.001**	-.001***
	(.000)	(.001)	(.000)	(.000)	(.000)	(.001)	(.001)	(.000)
Female	.058***	.046***	.029**	.049***	-.052***	-.016	-.018	-.027*
	(.013)	(.016)	(.015)	(.011)	(.013)	(.016)	(.016)	(.016)
Undergraduate or higher	-.010	.026	.011	.009	.043***	.060***	.066***	.043***
	(.013)	(.017)	(.015)	(.012)	(.014)	(.014)	(.015)	(.016)
Gross HH Income £25K-£50K	-.020	-.004	-.030	-.040**	.047**	.002	-.028	-.010
	(.020)	(.024)	(.021)	(.017)	(.019)	(.024)	(.025)	(.023)
Gross HH Income >£50K	-.022	-.026	-.016	-.051***	.053**	.018	-.003	.007
	(.023)	(.028)	(.026)	(.020)	(.021)	(.023)	(.023)	(.023)
Political Leaning - Centre	.009	-.025	-.055***	-.050***	-.036**	-.034**	-.058***	-.072***
	(.016)	(.021)	(.018)	(.016)	(.015)	(.016)	(.015)	(.016)
Political Leaning - Right	-.012	-.030	-.126***	-.095***	-.050**	-.056**	-.133***	-.159***
	(.026)	(.032)	(.025)	(.021)	(.025)	(.025)	(.033)	(.037)
Centre = Right	[.364]	[.844]	[.001]	[.007]	[.545]	[.347]	[.015]	[.012]
Mean outcome	.360	.355	.260	.211	.724	.784	.760	.742
QLFS	.370	.721	.480	.275	.832	.912	.811	.737
Other control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1078	1078	1078	1078	1078	1078	1078	1078

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regression results for respondents in the Control arm. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Other control variables include a dummy for being married/in a civil partnership, a dummy for residing in an urban area, and a dummy for home ownership. Political leaning enters as three indicators — centre-leaning, right-leaning, and leaning not stated (respondents answering 'Don't know') — with left-leaning respondents as the omitted category; the coefficient on the not-stated indicator is included in the regression but not reported. Left-leaning respondents are those that report being ‘very left-wing’ and ‘fairly left-wing’. Centre-leaning respondents combine those that report being ‘centre’, ‘slightly left-of-centre’ and ‘slightly right-of-centre’. Right-leaning respondents are those that report being ‘very right-wing’ and ‘fairly right-wing’. Gross household income enters as indicators for £25K–£50K, above £50K, and income not reported, with below £25K as the omitted category; the not-reported coefficient is likewise unreported. At the foot of each Column we report minority group estimates for men constructed from the Quarterly Labour Force Survey from January 2023-December 2024.

Table A6: Correlates of Perceptions of Economic Outcomes of Women

Regression estimates, robust standard errors, p-values in brackets

	Graduate Share				Employment Rate			
	(1) White British	(2) Indian	(3) Pakistani	(4) Black Carribean	(5) White British	(6) Indian	(7) Pakistani	(8) Black Carribean
Age	-.002***	-.002***	-.001***	-.001***	.001**	-.000	.000	-.001
	(.001)	(.001)	(.000)	(.000)	(.000)	(.001)	(.001)	(.001)
Female	.073***	.050***	.043***	.045***	-.047***	-.035*	-.030	-.020
	(.015)	(.014)	(.012)	(.012)	(.013)	(.018)	(.019)	(.017)
Undergraduate or higher	-.013	-.013	-.001	-.009	.032**	.030	.041**	.059***
	(.015)	(.016)	(.014)	(.012)	(.015)	(.020)	(.019)	(.016)
Gross HH Income £25K-£50K	-.010	-.018	-.020	-.036**	.041**	-.008	-.028	-.027
	(.021)	(.022)	(.019)	(.017)	(.021)	(.028)	(.029)	(.025)
Gross HH Income >£50K	-.029	-.029	-.013	-.022	.067***	-.010	-.031	-.018
	(.023)	(.024)	(.021)	(.021)	(.024)	(.031)	(.031)	(.025)
Political Leaning - Centre	-.010	-.040**	-.062***	-.065***	-.042**	-.068***	-.093***	-.074***
	(.017)	(.019)	(.017)	(.016)	(.017)	(.021)	(.022)	(.018)
Political Leaning - Right	-.007	-.003	-.101***	-.096***	-.059**	-.102***	-.213***	-.150***
	(.029)	(.037)	(.024)	(.023)	(.025)	(.036)	(.039)	(.036)
Centre = Right	[.916]	[.249]	[.051]	[.113]	[.428]	[.312]	[.001]	[.025]
Mean outcome	.380	.281	.200	.199	.702	.663	.629	.695
QLFS	.404	.681	.436	.452	.767	.772	.549	.747
Other control variables	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Observations	1078	1078	1078	1078	1078	1078	1078	1078

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regression results for respondents in the Control arm. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Other control variables include a dummy for being married/in a civil partnership, a dummy for residing in an urban area, and a dummy for home ownership. Political leaning enters as three indicators — centre-leaning, right-leaning, and leaning not stated (respondents answering 'Don't know') — with left-leaning respondents as the omitted category; the coefficient on the not-stated indicator is included in the regression but not reported. Left-leaning respondents are those that report being ‘very left-wing’ and ‘fairly left-wing’. Centre-leaning respondents combine those that report being ‘centre’, ‘slightly left-of-centre’ and ‘slightly right-of-centre’. Right-leaning respondents are those that report being ‘very right-wing’ and ‘fairly right-wing’. Gross household income enters as indicators for £25K–£50K, above £50K, and income not reported, with below £25K as the omitted category; the not-reported coefficient is likewise unreported. At the foot of each Column we report minority group estimates for men constructed from the Quarterly Labour Force Survey from January 2023-December 2024.

Table A7: Economic Outcomes of Women

Regression coefficients, standard errors in parentheses, p-values in brackets

A. Graduate Shares

	(1) Quarterly Labor Force Survey	(2) Control Mean	[1 = 2]	(3) ΔNarrative of Success	(4) ΔNarrative of Disadvantage	[3 = 4]
Out of 100 women above the age of 25 in the UK from each of the following groups, roughly how many do you think have a university degree?						
Indian	.681	.281	[.000]	.037***	.003	[.002]
		(.007)		(.011)	(.011)	
Pakistani	.436	.200	[.000]	.038***	-.003	[.000]
		(.006)		(.009)	(.009)	
Black Caribbean	.452	.199	[.000]	.014*	-.007	[.012]
		(.006)		(.008)	(.008)	
White British	.404	.380	[.001]	-.003	.003	[.532]
		(.007)		(.010)	(.010)	

B. Employment Rates

	(1) 1 - Unem. rate	(1b) Share working	(2) Control Mean	[1b = 2]	(3) ΔNarrative of Success	(4) ΔNarrative of Disadvantage	[3 = 4]
Out of 100 women of working age in the UK from each of the following groups, roughly how many would you say are working?							
Indian	.947	.772	.663	[.000]	.006	-.015	[.091]
			(.009)		(.012)	(.013)	
Pakistani	.896	.549	.629	[.000]	.011	-.018	[.026]
			(.009)		(.013)	(.013)	
Black Caribbean	.969	.747	.695	[.000]	.003	-.013	[.150]
			(.008)		(.011)	(.012)	
White British	.978	.767	.702	[.000]	-.006	.004	[.264]
			(.007)		(.009)	(.009)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regressions results of each outcome, a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Column 1 report minority group estimates constructed from the Quarterly Labour Force Survey from January 2023 - December 2024.

Table A8: Labor Market Discrimination - Response Summary

Number of Groups Assigned	Rank						
	(1)	(2)	(3)	(4)	(5)	(6)	Not sure
1	1,719	1,409	1,342	1,311	1,271	1,217	61
More than 1	344	347	524	518	273	270	825
2	182	194	251	274	199	200	58
3	59	63	105	107	48	32	43
4	37	44	80	60	15	22	67
5	17	35	48	26	7	12	108
6	49	11	40	51	4	4	549

Notes: For each column and the given rank, each row displays number of respondents that assigned that rank to the number of ethnic groups given in the first column. For example, looking at the first column, 49 is the number of respondents that assigned each of the 6 groups a rank of 1 and 1,719 is the number of respondents that assigned only one group a rank of 1. Since the total number of respondents were 3,205, if each rank was assigned to exclusively one ethnic group, the number in the first row of the table would be 3,205 for each of the 6 ranks (assuming no respondent responded with a "Not sure").

Table A9: Labor Market Discrimination, Women

Regression coefficients, standard errors in parentheses
p-values in brackets

	(1) Control Mean	(2) ΔNarrative of Success	(3) ΔNarrative of Disadvantage	[2 = 3]
Imagine that the following women applied for a job, which they were qualified for. Please rank them based on how likely you think they would be to be turned down for the job.				
Indian	3.22	-.066	.061	[.085]
	(.055)	(.073)	(.078)	
Pakistani	3.96	-.184**	.047	[.005]
	(.062)	(.083)	(.087)	
Black Caribbean	3.97	-.027	-.002	[.781]
	(.063)	(.087)	(.091)	
White British	1.88	.220***	.071	[.096]
	(.056)	(.083)	(.086)	

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report OLS regressions results of each outcome, a constant and treatment dummies. The sample is restricted to White British respondents, observations are reweighted to fit the national profile, and robust standard errors are reported. Respondents ranked six hypothetical women by how likely they would be to be turned down for a job they were qualified for; the outcome is the rank assigned to each group. Respondents could assign the same rank to multiple groups, and answer 'not sure'; the share of respondents that answered ‘not sure’ is .19, .26, .27 and .27 for White British, Indian, Pakistani and Black Caribbean women, respectively.

Table A10: BES Respondent Characteristics

Means, standard deviation in parentheses

	(1) Mean	(2) S.D.	(3) Median	(4) Min	(5) Max
A. Demographics					
Age	51.4	(16.2)	52.0	18.0	89.0
Female	.500	(.500)			
Married	.493	(.500)			
Born in UK	.980	(.139)			
B. Education, Employment and Income					
Undergraduate or higher	.405	(.491)			
Employed	.404	(.491)			
Home owner	.660	(.474)			
Gross HH income: Below £25k	.317	(.465)			
Gross HH income: £25k–£50k	.297	(.457)			
Gross HH income: Above £50k	.157	(.364)			
Local area minority share	.140	(.140)	.082	.015	.692
C. Politics					
Political attention (0–10)	6.24	(2.48)	7.00	.000	10.0
Political leaning — Left	.207	(.406)			
Political leaning — Centre	.338	(.473)			
Political leaning — Right	.237	(.425)			
D. Attitudes					
Zero-sum index (PCA)	.413	(.153)	.409	.000	1.00
Zero-sum IRT (θ)	-.010	(.874)	-.039	-3.37	2.71
Immigration attitude — Reduced	.543	(.498)			
Immigration attitude — Same	.186	(.389)			
Immigration attitude — Increased	.193	(.395)			
Discrimination seen as a problem	.563	(.496)			
Equal opportunities for minorities — Not far enough	.181	(.385)			
Equal opportunities for minorities — About right	.398	(.490)			
Equal opportunities for minorities — Gone too far	.344	(.475)			
Observations	5,145				

Notes: We report descriptive statistics for the sample of White British respondents in the 14th wave of the British Election Survey (BES), where observations are reweighted using BES weights to fit the national profile. Undergraduate or higher includes respondents with an undergraduate or a postgraduate degree. Zero-sum IRT (θ) is the zero-sum measure that is present in the BES, already calculated using the questions on zero-sum thinking in the survey by applying Item Response Theory (IRT) - by modeling the relationship between an individual's latent trait (zero-sum mindset) and their response to the questions on zero-sum thinking asked in the survey. Zero-sum index (PCA) is a composite measure of zero-sum thinking that we calculate, by employing principal component analysis (PCA) following the approach of Chinoy et al. [2026].

Table A11: Balance on Views on Inequality

Means, p-values in brackets

	(1) Control Mean	(2) Narrative of Success	(3) Narrative of Disadvantage	[1 = 2]	[1 = 3]	[2 = 3]
A. Vertical Inequalities						
To what extent, if at all, do you think inequalities in the following areas are a big/moderate problem in the UK today? Inequalities of..						
Wealth and income	.795	.796	.811	[.986]	[.442]	[.450]
Health and life expectancy	.680	.662	.700	[.422]	[.385]	[.096]
Educational attainment	.634	.673	.678	[.098]	[.067]	[.837]
Political influence	.617	.635	.604	[.445]	[.593]	[.196]
Status and respect	.596	.618	.619	[.361]	[.331]	[.943]
B. Horizontal Inequalities						
To what extent, if at all, do you think inequalities between the following groups in society are a big/moderate problem in the UK today?						
People from different ethnic groups	.618	.637	.664	[.432]	[.056]	[.254]
People from different social classes	.593	.599	.615	[.803]	[.356]	[.497]
Disabled and non-disabled people	.572	.581	.616	[.702]	[.071]	[.150]
People of different religions	.547	.564	.559	[.490]	[.628]	[.846]
People living in different regions	.483	.527	.528	[.069]	[.067]	[.960]
People of different ages	.465	.468	.472	[.899]	[.768]	[.864]
Men and women	.437	.439	.470	[.919]	[.173]	[.202]
Homosexual and heterosexual people	.405	.393	.436	[.608]	[.200]	[.072]

Notes: We report descriptive statistics for the sample of White British respondents, where observations are reweighted to fit the national profile. The p-values are calculated from an OLS regression for each outcome on a constant and treatment dummies, where robust standard errors are calculated. For the questions in both panels, respondents could also answer each issue was ‘a small problem’, ‘not a problem’ or ‘don’t know’.

Table A12: Moderacy Biases and Extreme Responses

Means, p-values in brackets

		(1) Control	(2) Narrative of Success	(3) Narrative of Disadvantage	(4) Control	(5) Narrative of Success	(6) Narrative of Disadvantage
Respondent Characteristics			**Don't Know or Not Sure**			**Extreme Response**	
Ethnicity	White British	.155	.139	.147	.244	.252	.246
	Not White British	.137	.120	.112	.271	.273	.270
	WB = NWB	[.233]	[.114]	[.003]	[.005]	[.021]	[.018]
Political Leaning	White British - Left Leaning	.124	.121	.109	.258	.274	.277
	White British - Centre Leaning	.130	.115	.132	.245	.240	.230
	Left = Centre	[.718]	[.610]	[.122]	[.282]	[.001]	[.000]
	White British - Right Leaning	.103	.091	.126	.307	.328	.323
	Left = Right	[.324]	[.050]	[.482]	[.021]	[.001]	[.122]
	Centre = Right	[.148]	[.080]	[.830]	[.001]	[.000]	[.001]
Survey Response Duration	White British - Fastest 5 percentile	.202	.215	.259	.218	.245	.224
	White British - 5th-95th percentile	.120	.108	.117	.260	.264	.257
	Fastest = 5th - 95th	[.195]	[.004]	[.003]	[.112]	[.448]	[.213]
	White British - Slowest 5 percentile	.128	.094	.116	.214	.257	.234
	Fastest = Slowest	[.277]	[.004]	[.008]	[.926]	[.690]	[.771]
	Slowest = 5th - 95th	[.789]	[.478]	[.990]	[.054]	[.737]	[.330]

Notes: For each treatment arm, Columns 1, 2 and 3 report the mean of share of questions where the respondent answered "Don't know" or "Not sure" (of questions which had either of those as a response option). Columns 4, 5 and 6 report the mean of share of questions where respondents have answered with responses at either end of the spectrum (extreme response). Observations are reweighted to fit the national profile.

Table A13: External Estimates

Means and outside estimates

Source	Question		(1) Control Mean	(2) Outside Estimate	Year	Sample	Comment
More in Common: Common Ground and Division in 2020s Britain	How positively or negatively do you feel about each of the following groups? Please answer on the following scale, where 0 means 'very negative' and 10 means 'very positive'.	Indian	6.20	6.30	2020	UK representative	"Black people"
		Pakistani	5.33	5.80			
		Black Caribbean	5.96	6.60			
		White British	7.26	7.10			
Ipsos/British Future Immigration Attitudes Tracker	Do you think the number of immigrants coming to the UK nowadays should be…reduced?		.631	.550	2024	UK representative	
British Social Attitudes Survey (Culture Wars)	Have attempts to give equal opportunities to the following groups not gone far enough?	Indian	.211	.450	2021	UK representative	Asked for Black and Asian people, and not individual groups.
		Pakistani	.239	.450	2021		
		Black Caribbean	.278	.450	2021		
		White British	.212				
	Have attempts to give equal opportunities to the following groups gone too far?	Indian	.159	.190	2021		Asked for Black and Asian people, and not individual groups.
		Pakistani	.191	.190	2021		
		Black Caribbean	.180	.190	2021		
		White British	.060				
British Social Attitudes Survey	Do you agree, disagree or neither agree nor disagree with the following statement? "The government should redistribute income from the better-off to those who are less well off."	Strongly Agree/Agree	.471	.519	2022	UK representative	
		Strongly Disagree/Disagree	.267	.250	2022		

Notes: This table presents comparable estimates for questions in our survey, whenever the same questions had been asked from a UK sample in other surveys. We provide the source of these estimates, their year and the sample used in each of these surveys. We then provide control mean from our survey for a comparable sample. For example, if the outside estimate is from a UK representative sample, we provide estimates using all the respondents in our survey (not just White British), weighted to fit the national profile.

Table A14: Treatment Effect in High and Low Groups

Regression coefficients, standard errors in parentheses, p-values in brackets

	Narrative of Success		Narrative of Disadvantage	
	Above vs below ATE	Above vs below ATE CI	Above vs below ATE	Above vs below ATE CI
Effect in high group	.445***	.617***	.027**	.063**
	(.129)	(.208)	(.012)	(.031)
Effect in low group	.102	.498***	.020	.029
	(.110)	(.186)	(.019)	(.025)
Difference	.343**	.118	.006	.033
	(.171)	(.279)	(.023)	(.040)
p, two-sided	[.044]	[.672]	[.778]	[.401]
p, one-sided	[.022]	[.336]	[.389]	[.200]
Respondents in high group	672	244	1092	273
Respondents in low group	1104	393	1015	338

Notes: *** indicates significance at the 1% level, ** at the 5% level and * at the 10% level. We report treatment effects of each narrative for respondents with high and low predicted conditional average treatment effects (CATEs), and the difference between them. In the first column of each narrative, the high (low) group is respondents whose predicted CATE lies above (below) the average treatment effect (ATE) of that narrative reported in Tables 2 and 4; in the second, respondents whose predicted CATE lies above the upper (below the lower) bound of the 95% confidence interval of the ATE, with respondents whose predicted CATE lies inside the interval excluded. Predicted CATEs are the out-of-bag estimates of the causal forest. Each column reports results from an OLS regression of the outcome on a treatment dummy, a dummy for the high group, and their interaction, controlling for the covariates used in the causal forest. The effect in the low group is the coefficient on the treatment dummy, the effect in the high group is the sum of the coefficients on the treatment dummy and the interaction, and the difference is the coefficient on the interaction. For the narrative of success, the outcome is the rank assigned to White British men when ranking six hypothetical men by how likely they would be to be turned down for a job they were qualified for. For the narrative of disadvantage, the outcome is whether the respondent believes the White British ingroup has few or no opportunities for education, with the sign reversed so that a positive effect corresponds to zero-sum thinking. The sample is restricted to White British respondents in the control arm and the respective narrative arm, observations are reweighted to fit the national profile, and robust standard errors are reported in parentheses. Significance stars are based on two-sided tests.

Figure A1: Ethnic Diversity in the UK, 2021 Census

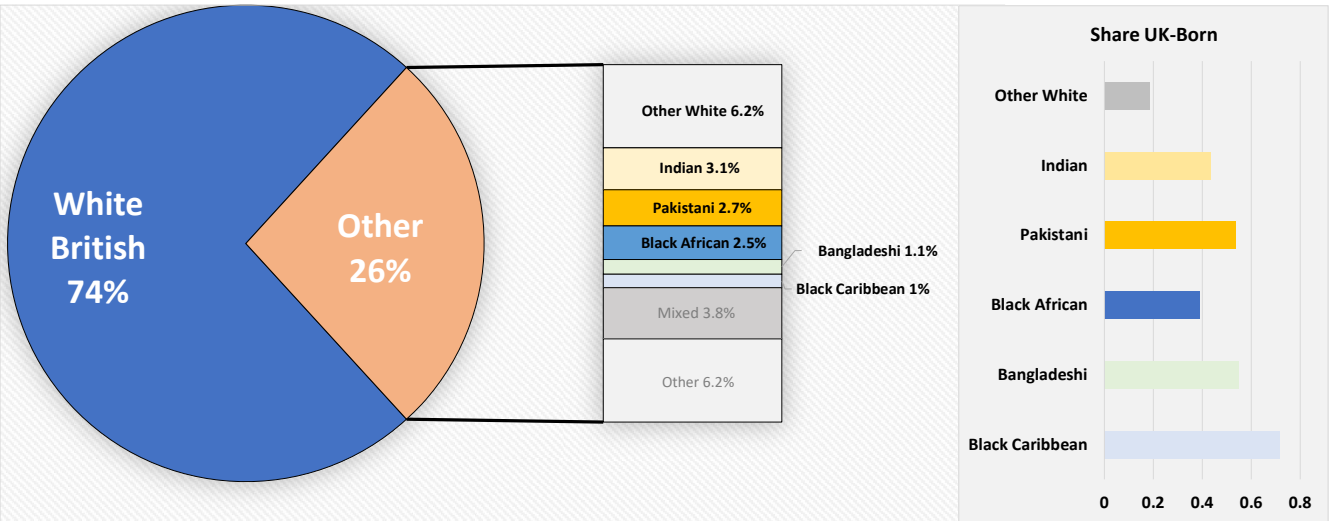


Figure A2: Structure of the Survey Experiment

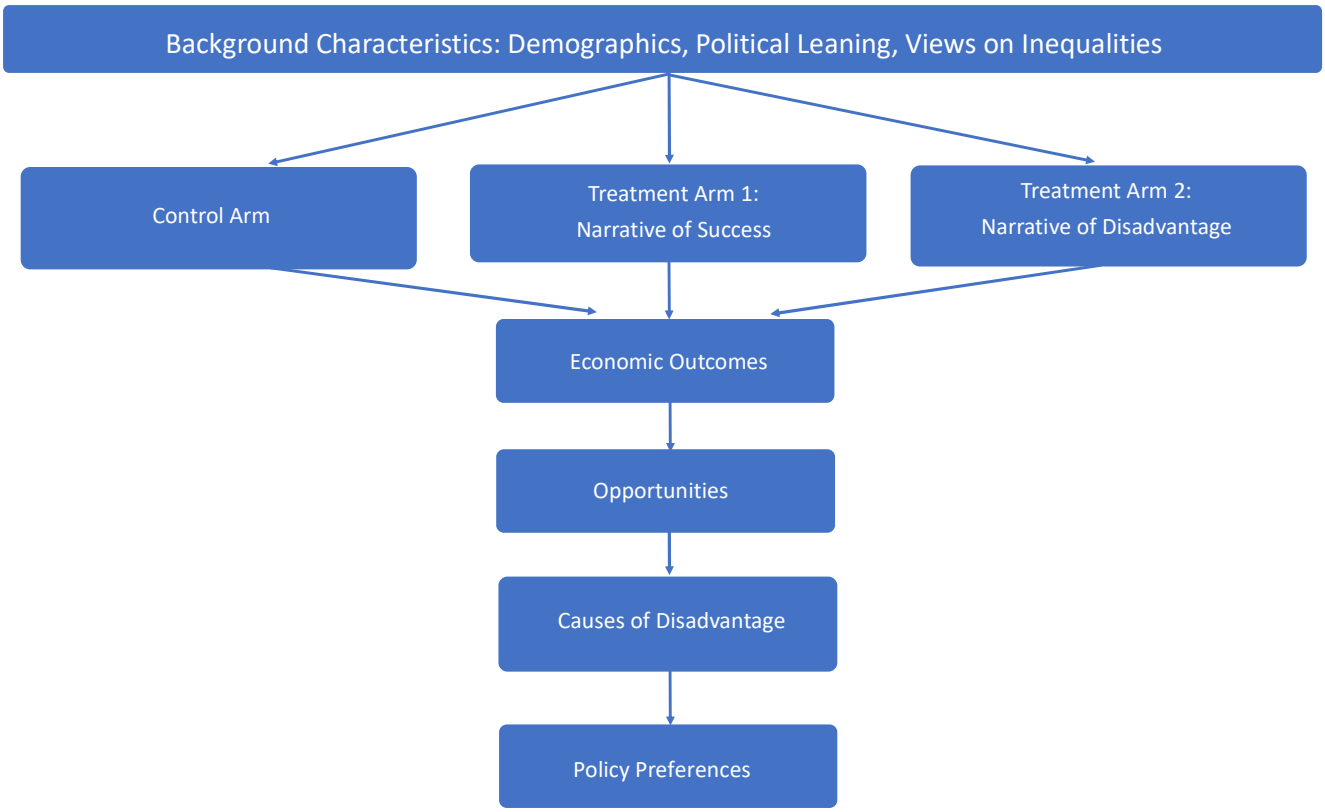
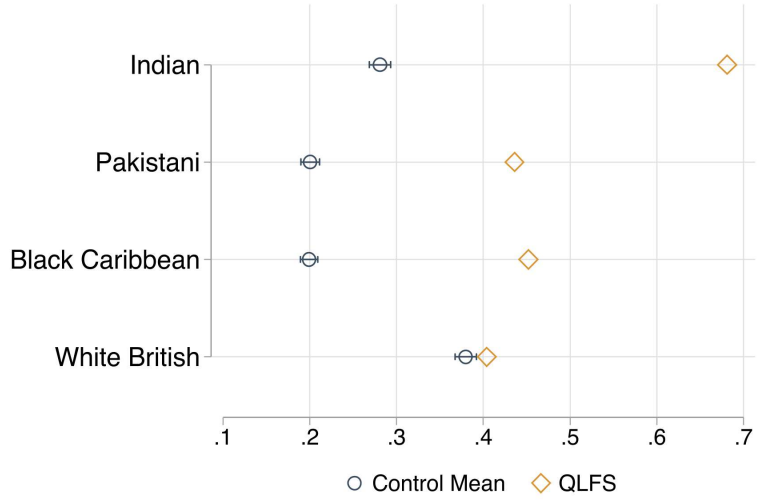
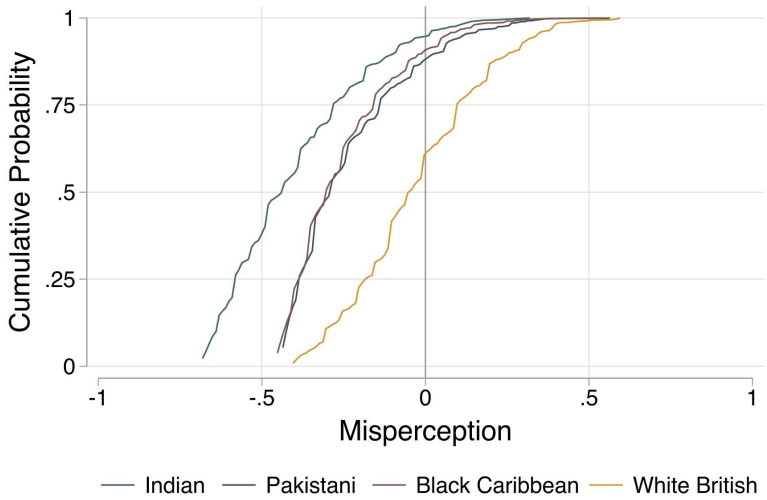


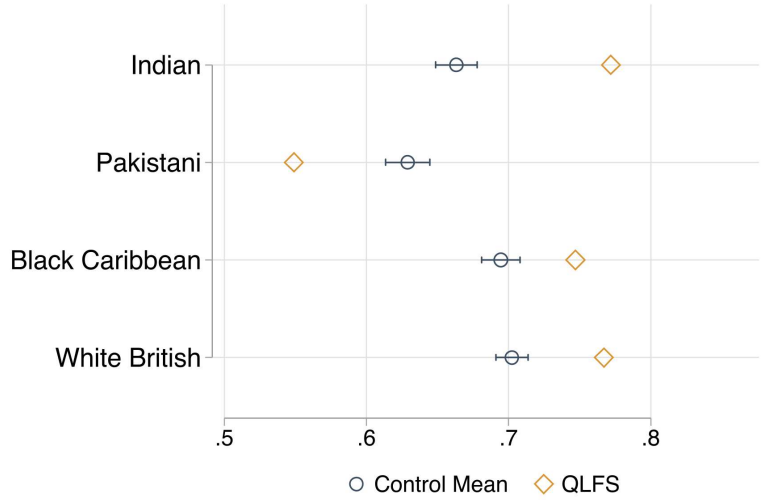
Figure A3: Perceived Economic Outcomes of Women

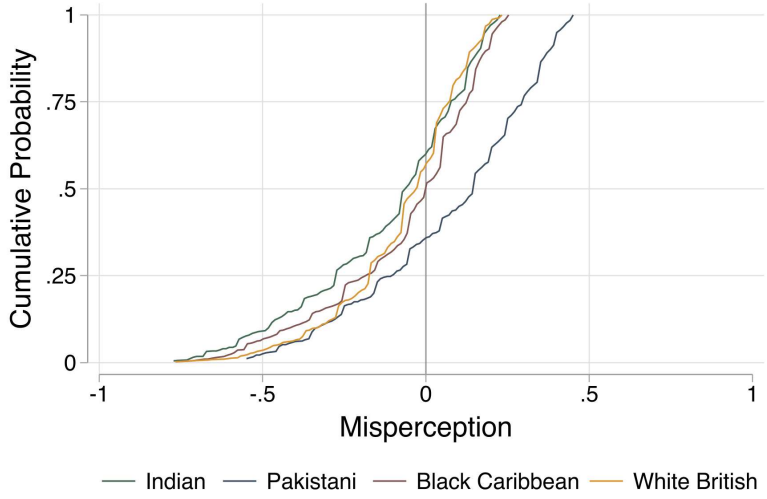
A. Graduate Shares





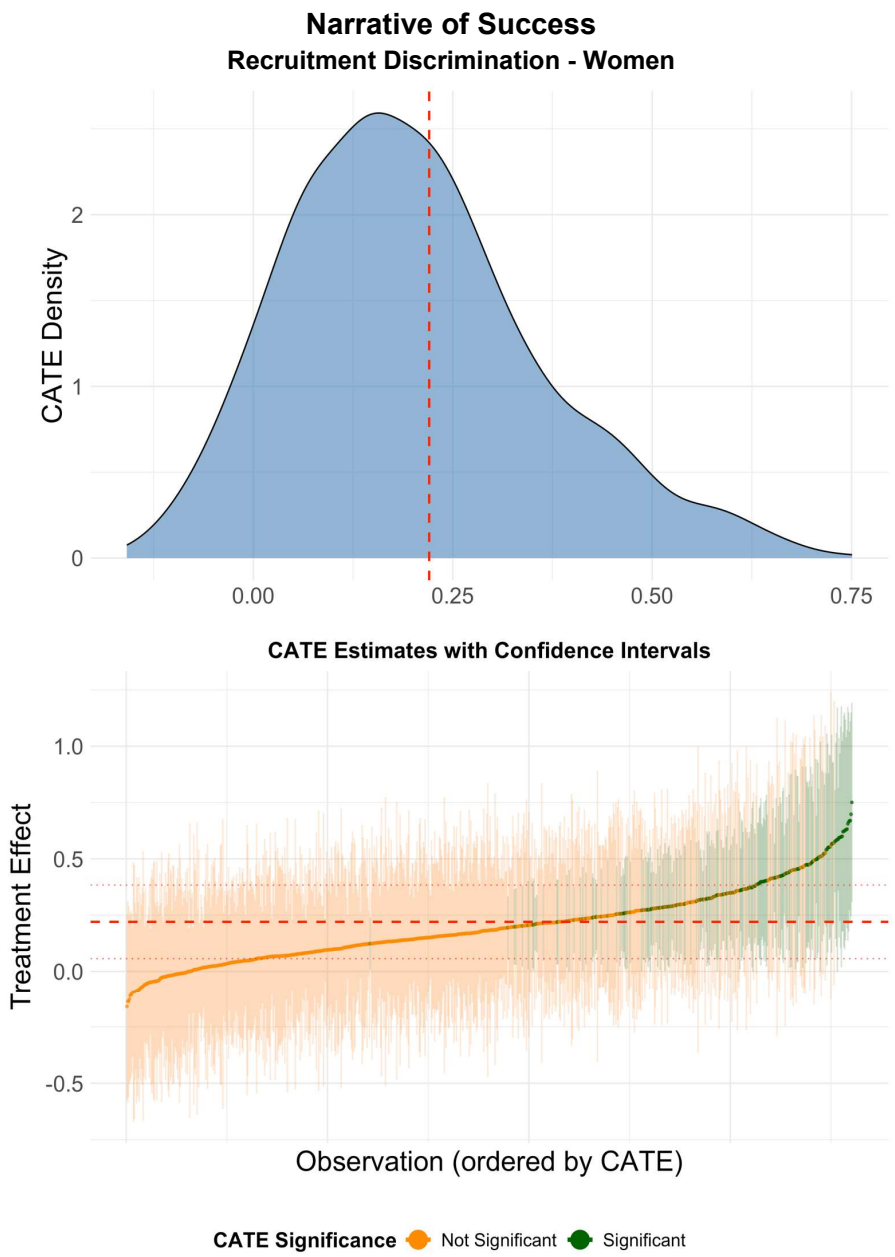
B. Employment Shares





Notes: On graduate shares, we asked, Out of 100 women above the age of 25 in the UK from each of the following groups, roughly how many do you think have a university degree? On employment shares, we asked, Out of 100 women of working age in the UK from each of the following groups, roughly how many would you say are working? The left hand side of each panel reports means for respondents in the Control arm and the estimates from constructed from Quarterly Labour Force Survey (QLFS) from January 2023 - December 2024. The right hand side shows the distribution of misperception for respondents in the Control arm. The sample is restricted to White British respondents, observations are reweighted to fit the national profile. We calculate the degree of misperception by taking the control mean and subtracting the group estimates from the QLFS. Hence positive misperceptions imply the control mean is higher than the group estimate.

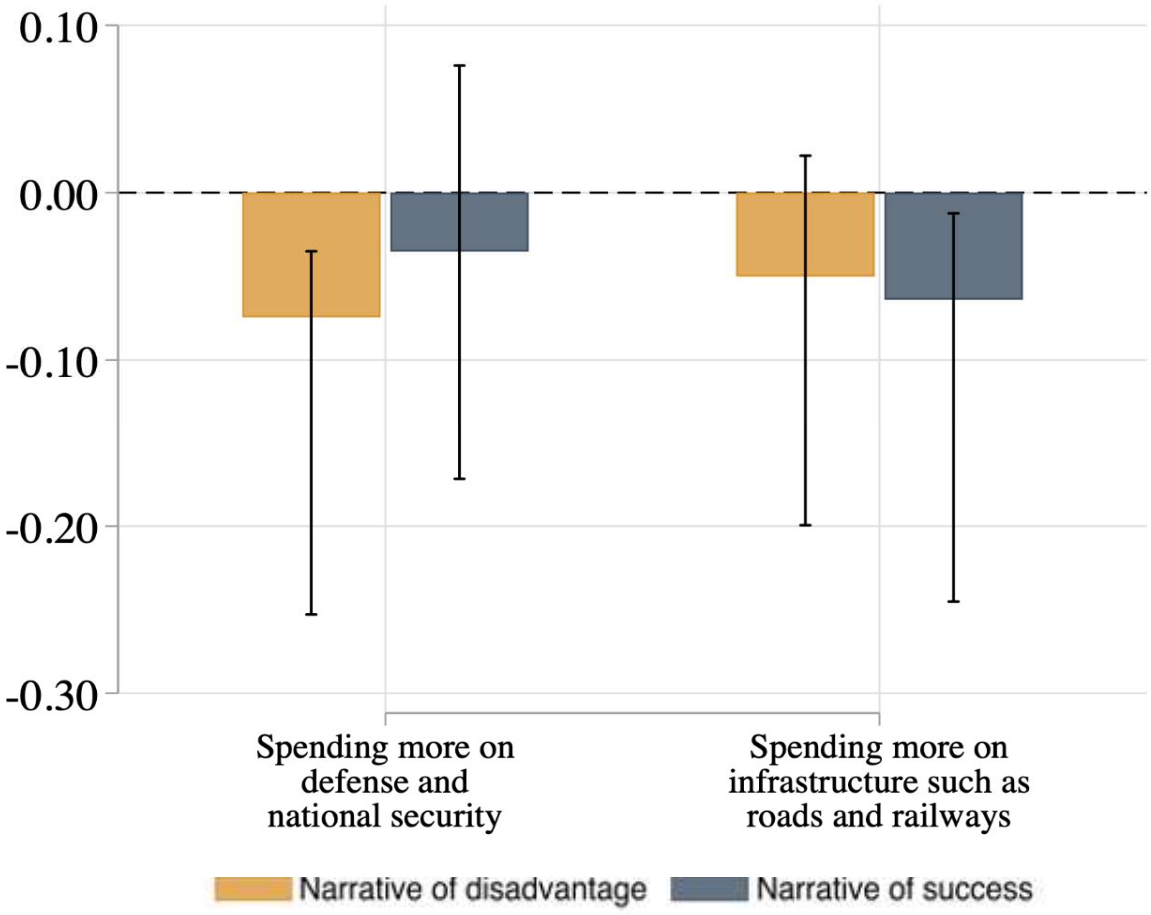
Figure A4: Treatment Effect Heterogeneity



Notes: This figure provides an overview of treatment effect heterogeneity of the narrative treatment on an outcome variable that measures beliefs about discrimination in hiring against White British female applicants. It mirrors the recruitment-discrimination panel of Figure 2 but uses the women's version of the hiring-discrimination question. The top row shows a density plot of conditional average treatment effect (CATE) obtained from a causal forest for the outcome variables. A higher CATE estimate corresponds to a higher zero sum thinking. The bottom panel plots CATE estimates for each respondent that has received a treatment in the survey. For each outcome, the respondents are ranked from the lowest to the highest CATE and plotted along with the 95% confidence interval. The red dashed and dotted lines plot the ATE and the corresponding confidence interval for each of the outcomes. The variables used to generate heterogeneity in the causal forest are whether the respondent regards three types of inequality as a problem (between ethnic groups; horizontal inequalities other than ethnicity; vertical inequalities), age, gender, university degree, household income (below £25k; above £50k), political leaning (left; right), home ownership, being married or in a civil partnership, being employed full-time, and the local-area minority population share. All variables are either dummy variables that take the value of either 0 or 1 or variables with values between 0 and 1, except age, which has been standardized using control group mean and standard deviation. We grow 10,000 trees and use honest sampling to avoid overfitting. The sample is restricted to White British respondents and observations are reweighted to fit the national profile. The red dashed line in both panels display the ATE estimates and the red dotted line in the bottom panel displays 95% confidence intervals.

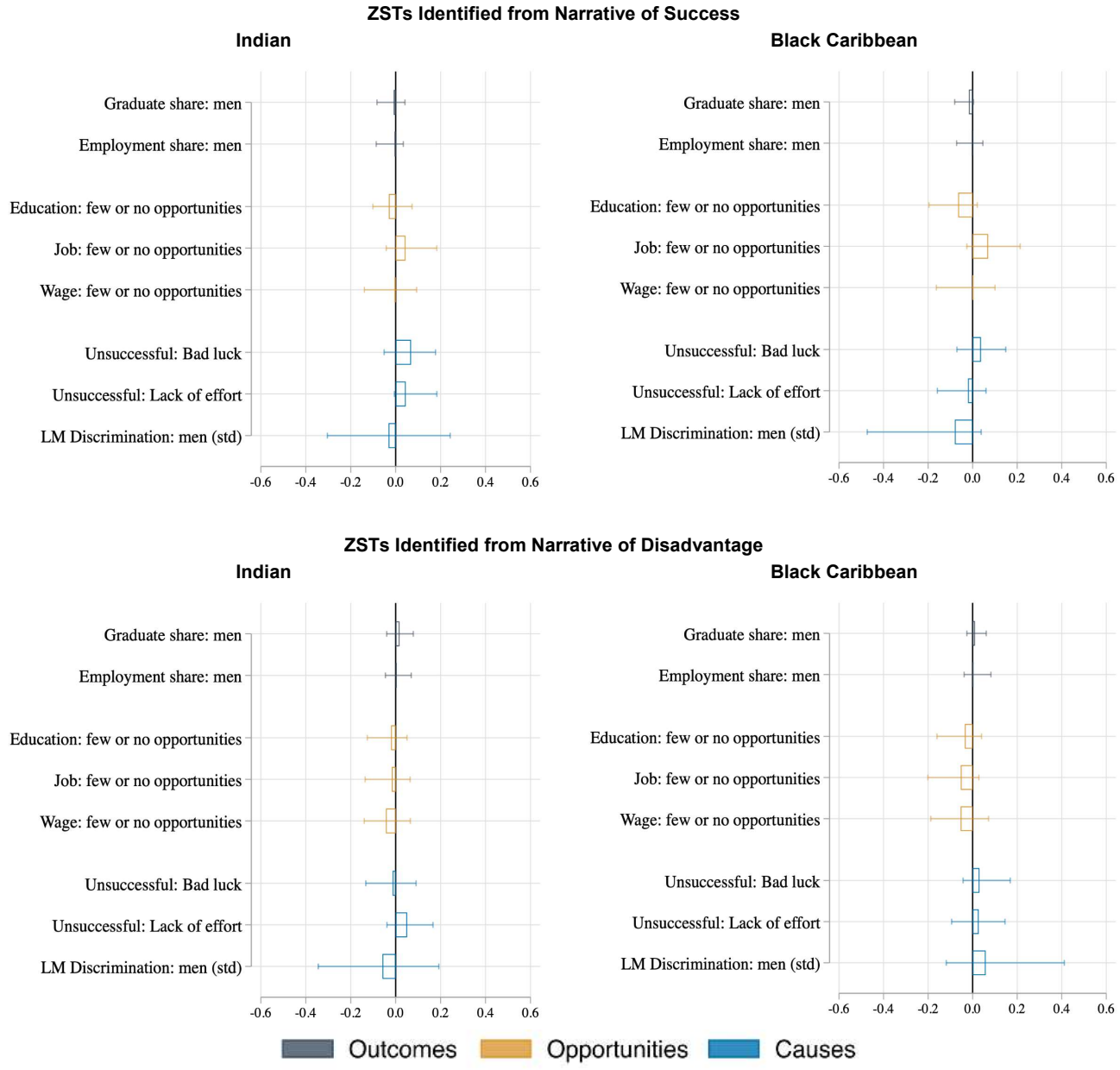
Figure A5: Other Policy Views of Zero-sum Thinkers

Other Spending Priorities: Defense and Infrastructure



Notes: The figure plots OLS coefficients and 95% confidence intervals on an indicator for zero-sum thinking for the two government-spending items omitted from the main pro-poor policies panel (Figure 5, Panel A right): spending more on defense and national security, and spending more on infrastructure such as roads and railways. For each item the dependent variable is a dummy equal to one if the respondent answers "strongly support" or "tend to support", and zero otherwise (including "don't know"). The orange bar uses the narrative of disadvantage classification of zero-sum thinkers and the navy bar uses the narrative of success classification; the two types are identified independently, so each estimate comes from a separate regression. Each regression conditions on age, gender, marital status, home ownership, education, employment status, household income, political leaning, and the local-area minority share. The sample is restricted to White British respondents in the Control arm and observations are reweighted to fit the national profile. The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore compute confidence intervals from a bootstrap of the full estimation pipeline with 1,000 replications: the 95% intervals shown are basic bootstrap intervals, read off the distribution of replicate coefficients around the estimate, and need not be symmetric; details can be found in Appendix.

Figure A6: ZST and Perceptions of Others



Notes: The figure plots OLS coefficients and 95% confidence intervals on an indicator for zero-sum thinking across ten group-specific outcomes, organised into four sub-panels: the top row uses the narrative of success classification of zero-sum thinkers and the bottom row the narrative of disadvantage classification, each shown for two target groups — Indian and Black Caribbean. The two types are identified independently, so each estimate comes from a separate regression. Within each sub-panel the same ten items appear on the y-axis from top to bottom, organised into three colour-coded categories. The first category (Outcomes) covers perceived economic circumstances: the respondent's perceived graduate share and perceived employment share among men of the target group (0–1 proportions). The second category (Opportunities) contains three binary indicators equal to one if the respondent thinks members of the target group have "few or no opportunities" in education, jobs, and wages, and zero otherwise (including "don't know"). The third category (Causes) covers attributions of the group's outcomes: two dummies equal to one if the respondent attributes an unsuccessful member of the target group's outcome to "bad luck" or to "lack of effort", respectively, and zero otherwise (including "an equal mix of both" and "don't know"), and the respondent's perceived likelihood that men of the target group would be turned down for a job they are qualified for (1–6 ranking, coded so that higher values mean more likely to be turned down; z-standardised within the Control arm by target group). Each regression conditions on age, gender, marital status, home ownership, education, employment status, household income, political leaning, and the local-area minority share. The sample is restricted to White British respondents in the Control arm and observations are reweighted to fit the national profile. The zero-sum-thinking indicator is itself a thresholded causal-forest estimate rather than observed data — we therefore compute confidence intervals from a bootstrap of the full estimation pipeline with 1,000 replications: the 95% intervals shown are basic bootstrap intervals, read off the distribution of replicate coefficients around the estimate, and need not be symmetric; details can be found in Appendix.

Figure A7: Distribution of ZST Parameter, BES



Notes: This figure plots a composite measure of zero-sum thinking calculated by employing principal component analysis (PCA) following the approach of Chinoy et al. [2026]. We use six zero-sum questions from the British Election Study (wave 14), coded on a 5-point Likert scale, and reverse-code 3 variables prior to analysis to ensure all items are oriented in the same direction, where higher values indicate stronger zero-sum thinking. The analysis is restricted to White British respondents with complete data across all six variables. All variables are standardized (mean = 0, standard deviation = 1) before conducting PCA to ensure equal weighting in the analysis. We extract the first principal component (PC1) of the six standardised items. PC1 has an eigenvalue of 2.47 and accounts for 41.2% of the total variance, roughly twice the share of the second component. All six items load positively on PC1 with similar magnitudes (.32 to .45), so the index reflects a common orientation across all six questions rather than any single item. Following Chinoy et al. [2026], we use this first component as a summary index of zero-sum thinking. We do not interpret the PCA as establishing that the items measure a single latent dimension; the index is a variance-weighted summary of the dominant common variation in responses. Individual zero-sum scores are calculated as the PC1 score for each respondent, then normalized to a 0-1 scale. This transformation facilitates interpretation, where zero represents the minimum level of zero-sum thinking in the sample and one represents the maximum.