# Pre Analysis Plan For Conjoint Experiment on Working Class Appeals

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## 1 Motivation

Theories on the identity mediated group foundations of political behaviour in advanced democracies have experienced a resurgence in interest (e.g. Achen and Bartels 2017). A growing literature examines the effect of parties' identity appeals on political behaviour (e.g. Dancygier 2017; Evans and Tilley 2017; Thau 2019; Robison et al. 2021; Huber et al. 2024). But while the impact of differential policy platforms on group structured voting behaviour is well recognised (e.g Dancygier 2017; Evans and Tilley 2017), the effect of parties' make-up through associations of descriptive representation (e.g Heath 2015; Weeks et al. 2023) and that of direct rhetorical group appeals through symbolic representation is much less understood (e.g Thau 2019; Robison et al. 2021). Some argue that attachments to parties are sticky heuristics, based on a running tally of both past and present party performance (Fiorina 1981). Existing literature has also not directly explored whether group party associations follow such a reputational logic.

To assess these predictions about different forms of identity appeals, I propose a conjoint experimental study of class voting in the UK. Class competition has ceased to be the dominant cleavage in Western Europe (Dalton 2019). Some argue that this depoliticisation stems from socialdemocratic parties' decreased rhetoric (Thau 2019), policy (Evans and Tilley 2017) and descriptive (Heath 2015) appeals to the working class. In a factorial experimental design varying candidates partisanship and rhetorical appeals, Robison et al. (2021) indeed find that candidates who send rhetorical group appeals attract significantly higher support among working class respondents in the US and Denmark. They document similar effects for partisanship and left-wing policy proposals (ibid.). These findings offer an important starting point for my analysis. I seek to replicate

them in a different country context for *political parties*. But I argue my conjoint also introduces several important innovations beyond this replication.

The conjoint set-up allows me to introduce all four dimensions of identity appeals with greater variation on their levels. I also rely on different treatments. To test the effects of reputation I directly reveal specific information about parties' past strategies instead of providing partisan labels. I argue this operationalisation allows a much narrower focus on reputation effects without potential confounding through partisanship. On policy I vary the vagueness of appeals instead of completely omitting proposals for some respondents. This should allow me to separate recovered effects from potential priming effects of policy proposal. Finally, I operationalise rhetorical appeals as campaign emphasis instead of combative quotes. This fits much more naturally with my focus on party instead of candidate strategy, but will also allow me to compare the difference between combative and non-combative group appeals. Importantly, I also add appeals on descriptive representation which, as argued, could present an important fourth dimension of identity appeals.

Moreover, because greater variation on levels and narrower treatments should help me avoid ceiling effects[1], my design enables me to estimate the effect of different identity appeals more directly in comparison even when they are jointly specified. Greater variation also allows me to assess the effect of each dimensions on two potential mediators: perceptions of left-wing policy and perceptions of working class representation[2]. Finally, this richer set-up will allow me to conduct a thorough analysis on the demand side mechanisms behind the relevance of identity for voters more generally[3].

In this pre-analysis plan I outline my conjoint experiment. After giving a brief overview of the design, I derive primary hypotheses and motivate my intended exploratory analysis. Section Three motivates my attribute design. Section Five explains my analytical strategy. After conducting a power analysis on the necessary sample sizes, I conclude with a brief summary of robustness checks I intend to implement.

[1] Robison et al. (2021) hypothesise that they do not find effects for rhetorical appeals when combined with left-partisanship and left economic policy because of such ceiling effects.
[2] Robison et al. (ibid.) only do this for rhetorical appeals.
[3] Albeit exploratory, given a lack of existing research that could inform solid hypotheses.

## 2 Overview

| **Attributes** | **Levels** |
|---|---|
| **Policy Position:** The party's policy platform emphasises pledges to... | ...deliver prosperity to the UK. |
| | ...deliver the economic policy the UK deserves. |
| | ...foster an economy full of opportunity. |
| | ...strengthen workplace and employee rights. |
| | ...increase welfare benefits. |
| | ...increase support for disadvantaged areas. |
| **Past Reputation:** In the past this party has been primarily seen as... | ...seeking support from all parts of UK society. |
| | ...appealing to the working class. |
| | ...appealing to local communities. |
| | ...seeking support from families. |
| **Rhetorical Group Appeals:** The party's campaign emphasises the need to... | ...deliver a better life for all UK citizens. |
| | ...support working people in the UK. |
| | ...deliver for local communities. |
| | ...deliver for families in the UK. |
| **Descriptive Representation:** Among the party's current MPs... | ...one can find voices from all parts of UK society. |
| | ...the top three occupational backgrounds are factory workers, electricians and railway workers. |
| | ...many represent constituencies they have long lived in. |
| | ...many have children. |

Table 1: Attribute Level Structure of Conjoint Design.

The attributes in my proposed conjoint correspond to each of the four relevant dimensions of

identity appeals: policy, group appeals, descriptive representation and past reputation. On policy I vary levels according to two conditions: concrete and vague policies. Concrete policies indicate support for left-wing economic proposals. Non-policy appeals vary between working class and three neutral appeals: to UK society, to local constituencies, to families. The design is summarised in Table 1.

# 3 Hypotheses

I form four primary hypotheses on the effect of each identity appeal dimension to the working class. My main objective in this experiment concerns assessing their effect on voter support for the appealing party. I also form hypotheses about whether these appeals should activate policy inference or perceptions of *working class representation* to assess the mechanism behind these effects. The richness of the conjoint design also allows for a thorough exploration of the demand side motivations of potential effects beyond these primary objectives. Accordingly, I motivate a broad secondary exploratory analysis. A lack of prior systematic evidence on these questions, prevents me from forming clear hypotheses for this exploratory analysis and I remain agnostic about whether it will eventually yield interesting findings.

## 3.1 Voter Utility: Policy Inference or Group Representation

I argue identity based accounts implicitly follow work on partisanship in conceptualising voter utility as a two dimensional function of individual policy and identity concerns (cf. Adams et al. 2005). Growing evidence suggests a significant impact for this identity channel. In an experimental setting in the US and Denmark, Robison et al. (2021) indeed find that successful rhetorical appeals activate perceptions of "working class representation" instead of inferences about party's left-wing policy positions. Similarly, a lottery that randomly increased the descriptive representation of some constituents from lower socio-economic background in Mexico caused them to significantly increase support for the responsible party and report greater feelings of "empowerment", despite no discernible difference in policy representation (Poertner 2023). Of course, despite these findings, identity appeals might simultaneously activate voters' policy considerations through policy inferences (cf. Robison et al. 2021). Indeed, especially policy based identity appeals should strongly

map onto voters' individual policy considerations. Further quantifying the relative impact of different types of identity appeals on each utility term thus remains an important open question to assess their independence from traditional conceptions of policy based associations.

Nonetheless, and in line with existing experimental findings (Robison et al. 2021), I expect non-policy identity appeals to affect perceptions of *working class representation* but not those of parties' economic policy platforms. Of course, this should be especially likely when voters need not infer policy positions because parties send concrete policy proposals. But I do not form different hypotheses for the effect of non-policy identity appeals on policy inferences and perceived group representation under vague policy proposals. Since such inferences would be more likely in this case, however, finding the same outcomes for both vague and concrete conditions should increase confidence in my proposed mechanism.

### 3.1.1 Symbolic Rhetorical Appeals

In line with existing experimental findings (ibid.), I expect symbolic rhetorical appeals to affect perceptions of *working class representation* but not those of parties' economic policy platforms. In turn, these positive effects on perceptions of *working class representation* should induce individuals who identify with the working class to increase support for the party.

> **H1a:** Symbolic rhetorical working class appeals should not alter individuals' assessment of the parties' economic policy position.
>
> **H1b:** Symbolic rhetorical working class appeals should increase perceived *working class representation*.
>
> **H1c:** Symbolic rhetorical working class appeals should increase support among voters who identify with the working class.

### 3.1.2 Party Make-up

To my knowledge no experimental work has tested the effect of working class descriptive representation on voter support. But observational studies suggest potentially significant effects (Heath 2015). Similar to symbolic appeals I expect these effects to primarily affect perceptions of *working class representation*.

**H2a:** A high share of MPs with working class background should not alter individuals' assessment of the parties' economic policy position.

**H2b:** A high share of MPs with working class background should increase perceived *working class representation*.

**H2c:** A high share of MPs with working class background should increase support among voters who identify with the working class.

### 3.1.3 Reputation

Instrumental theories of partisanship suggest important reputation effects for political behaviour (cf. Fiorina 1981). Indeed, realignments in group associations to parties generally occur slowly (cf. Achen and Bartels 2017), perhaps because of reputation effects. Robison et al. (2021) find significant effects for partisan labels with clear historic group associations. Again, I expect these effects to primarily stem from a greater sense of *working class representation*.

**H3a:** Positive past reputation for working class appeals should not alter individuals' assessment of the parties' economic policy position.

**H3b:** Positive past reputation for working class appeals should increase perceived *working class representation*.

**H3c:** Positive past reputation for working class appeals should increase support among voters who identify with the working class.

### 3.1.4 Left-wing Economic Policy Proposals

My analysis focuses on *non-policy* identity appeals. Indeed, I primarily include policy positions to account for potential masking (inferences of policy position) from other identity appeals. Nonetheless, I can form expectations about the effect of left economic policy proposals. Of course, these should alter respondents' assessments of the parties' policy platform. Simultaneously, however, these proposal might also strengthen perceptions of *working class representation*. Assuming individuals who identify with the working class also hold left-wing views on economic policy (e.g. Beramendi et al. 2015), these two channels should uniformly induce them to increase support for the party making left-wing economic proposals.

**H4a:** Left-wing economic policy proposals should alter individuals' assessment of the parties' economic policy position

**H4b:** Left-wing economic policy proposals should increase perceived *working class representation*.

**H4c:** Left-wing economic policy proposals should increase a party's support among individuals who identify with the working class.

Finally, I am interested in comparing the effect of policy proposals to non-policy appeals. Robison et al. (2021) find similar effect sizes for policy and rhetorical appeals. I form similar expectations.

**H5:** Left-wing economic policy proposals should have a similar effect on a party's support among individuals who identify with the working class as non-policy appeals.

## 3.2 Motivation: Social Psychological or Rational Choice

Social identity theory (SIT) explains the independent relevance of group representation with the identity affirming act of voting for group representatives (Huddy, Mason, et al. 2016). By supporting parties with perceived ties to the group, identifiers can heighten the self-perceived status of their identities relative to other out-groups both by achieving greater political relevance for their in-group *and* by acting in conformity with other in-group members (Huddy 2013).

Insurance motivated theories on the relevance of identity for redistributive preferences, on the other hand, highlight how strong identifiers' perceptions of convergence between group and self well-being could induce individuals to adopt identity mediated preferences (Alt and Iversen 2017). Heuristic reasoning might then induce positive reactions to parties' group-commitment signals. The relevance of such heuristics in a complex and uncertain policy environment has of course long been recognised by rational choice accounts of voting behaviour (cf. Fiorina 1981; Ferree et al. 2021). Such reasoning echoes debates about the relevance of descriptive representation for female voters with "uncrysatllised" preferences (Campbell et al. 2010).

I contend these considerations are highly relevant to distinguish identity appeals from policy driven party voter associations. The more voters value group representation for "symbolic" SIT motivations the more identity appeals should be seen as orthogonal considerations that might allow parties to compensate "unappealing" policy offers. A lack of prior systematic comparison between

these potential mechanisms prevents me from forming further primary hypotheses. Instead I plan to conduct a comprehensive exploratory analysis based on such potential divergences.

### 3.2.1 The Primacy of Policy Positions

Status affirmation can but need not occur through specific policy changes. SIT motivations might accordingly be fully orthogonal to policy based considerations[4]. Contrastingly, because the ultimate aims of insurance reasoners are representative present and future policies, respondents should generally weigh policy signals much more. Parties might accordingly be unable to fully offset policy with identity motivations below a minimum level of correspondence. For instance, merely fielding female candidates without directly appealing to women might be insufficient for populist radical right (PRR) parties (Weeks et al. 2023), while appeals to Muslim voters could be relatively ineffective when not paired with sufficiently culturally conservative policy platforms (Dancygier 2017).

Because parties' policy proposals vary between neutral (vague) and positive (concrete) appeals I cannot include a strong test on the effect of "strains" between different dimensions. Moreover, ceiling effects on respondents' ability/willingness to shift support in the experimental setting could artificially decrease complementarities between different dimensions. Indeed, Robison et al. (2021) hypothesise that they do not find significant effects for rhetorical appeals because of such ceiling effects. Nonetheless, high complementarities (i.e. positive interaction effects between dimensions) between policy positions and non-policy identity appeals could point to a higher relevance for insurance motivations.

### 3.2.2 Broader Complementarity or Substitutability between Dimensions

A second important difference between the two potential motivations concerns the immediacy of payoffs. While benefits associated with status affirmation should be activated quite immediately by identity appeals, insurance reasons for group representation draw from projected future payoffs in an uncertain policy environment (cf. Alt and Iversen 2017). To gain individuals' trust on group representation in this second setting, parties might need to send costly signals about their commitment to the group. This might more generally imply that the substitutability between

[4] i.e. identity considerations could be equally strong regardless of policy, potentially even compelling voters to vote against "their interests" (e.g. Achen and Bartels 2017).

different identity appeals should be higher under SIT motivated identity voting. Because voters immediately reward present status affirmations, consistency between these different dimensions – that would signal stronger commitments to one group – could matter relatively less. Mindful of similar potential ceiling effects, I thus analyise the complementarity between identity appeals more generally for all dimensions.

#### 3.2.3 Working Class Sentiment Moderators

The primacy of identification over "objective" socio-economic factors is apparent for the socio-psychological mechanism behind group representation. But even if identification might not arise in an entirely rational way (i.e. does not perfectly collapse to socio-economic status), it would not be irrational to take identification as important heuristic once formed. It is thus not trivial to argue that effects primarily moderated through identification should necessarily lead to a rejection of rational choice motivations. Nonetheless, if appeal effects are as effectively or more effectively moderated through *objective markers*, this should point to a higher relevance for insurance motivations.

### 3.3 Evaluation vs. Identification

My argument has focused on individuals' self-identification or group membership. But some argue that these mechanisms might extend to other groups through individuals' affective altruistic (punititve) desire to support (punish) positively (negatively) evaluated groups more generally (Conover 1988). Minority groups in the US, for instance, appear to support other minority groups because of empathetic sentiments (Sirin et al. 2016). In Germany, naming social groups as beneficiaries (or victims) of policy proposals indeed influences respondents' support according to their evaluation of the group (Huber et al. 2024). Crucially, this altruistic evaluation logic might significantly lower the penalty of appealing to narrow groups such as minorities. Similar to objective occupation, I suggest it can be assessed through moderation analysis. If *group evaluation* provides an (more) effective moderator, this should point to a higher relevance for this evaluation logics.

### 3.4 Alienation

My demand side logic can be extended to individuals who would perceive working class appeals as dis-appeal. Historically, the obvious "opponent" of the working class in the UK are upper middle class individuals, although in previous experimental research, these individuals do not appear to negatively react to working class appeals (Robison et al. 2021). Another operationalisation centres around negative affective evaluations of the working class (Huber et al. 2024). Huber et al. (ibid.) indeed find that negative evaluation of referenced beneficiaries of policy proposals reduces respondent support for these proposals among. Because I do not anticipate enough respondents with these negative views or upper middle class identification, I do not form primary hypotheses about such backlash effects. Nonetheless I plan to explore potential dis-appeal effects among individuals who do not identify with the working class.

## 4 Attribute Design

I split the four attributes of my conjoint in two broad categories: economic policy propositions together with non-policy identity appeals. I operationalise the broader category of non-policy identity appeals with three attributes: past reputation, rhetorical group appeals and descriptive representation (Table 1). For each attribute, the neutral category serves as the baseline in my analysis. I primarily include identity appeals to families and local communities to prevent priming, benchmarking or experimenter demand effects arising from respondents' (subconscious) understanding of the experimental design that might violate the stability assumption underlying conjoint analysis (see discussion below).

### 4.1 Policy Attributes

I include six levels measuring positions on economic policies (Table 1). Parties differ according to their support for economically left-wing concrete policy proposals. Where parties do not support any specific proposal, I assign a vague statement that does not reveal anything about their economic policy platforms. The statement "deliver prosperity to the UK" serves as baseline for my analysis. Importantly, moreover, I contend all of these positions should not directly contradict other working class based appeals.

I argue my three concrete policy statements all represent proposals for greater state intervention in the economy (e.g. Beramendi et al. 2015). They do not represent highly abstract ideological appeals, but concrete plans that, following existing research on working class preferences, should appeal to individuals who identify with the working class (e.g Oesch and Rennwald 2018). At the same time I avoid including items that threaten ceiling effects because of their overwhelming popularity (e.g. raising taxes on the rich) (Rueda and Stegmueller 2019).

## 4.2 Rhetorical Appeals

I diverge from Robison et al. (2021) in operationalising rhetorical appeals as campaign emphasis (Table 1). While their experiment assesses the effect of conflictual candidate quotes about the need to defend working class interests against the rich, I focus on the costliness of campaign emphasis and its signalling function about the relevance of a particular group for a party.

My motivation for this operationalisation is twofold. I suggest introducing information about a party's campaign emphasis instead of a specific quote fits much more naturally with my focus on party instead of candidate appeals. Indeed, Grant and Evans (2023) suggest that perceptions about their relative standing in the party are a major reason why British working class voters have deserted the Labour party. Moreover, I am interested in the effect of purely positive working class appeals. Haffert et al. (2024) document significantly positive effects for similarly operationalised rural rhetorical appeals without a conflictual dimension. Replicating their findings for the working class and comparing my results to the conflictual appeals in Robison et al. (2021) presents an important innovation.

## 4.3 Descriptive Representation

I introduce descriptive representation by revealing information about the background of current MPs of the party (Table 1). While this should be a straightforward signal about the possibility to gain working class descriptive representation by supporting the party in question, some scholars argue that candidate background effects operate through relatively subconscious channels that are not necessarily activated through text based information. When presented with candidate photographs, for instance, voters show significantly higher support for more attractive candidates

(Ahler et al. 2017). Thus, while text based experimental analysis generally shows voters slightly favouring female candidates (e.g. Schwarz and Coppock 2022), voters might significantly penalise female candidates because of their perceived (un)attractiveness in the real world (Bernhard 2018). Such photographic based discrimination for or against working class candidates appears unlikely. Yet, there are alternative subconscious cues about working class identities. For instance, accents generally vary across social classes (Pitts and Gallois 2019), especially in the UK (Levon et al. 2022).

I refrain from introducing candidates' background in this subtle way for three reasons. In my analysis I want to focus on aggregate party strategies. Introducing candidate specific information does not generally present a natural fit with this level of analysis. Such information would need to focus on party leaders to maintain clear ties with aggregate party strategy. But leader focused treatments fail to reflect the deeper and more sustainable bases of party composition resulting from broad working class clustering among MPs. They might accordingly remain weaker than those focused on broader information about parties' MPs. Moreover, real world examples of parties' strategic descriptive appeals appear broadly restricted to the MP level (cf. Weeks et al. 2023; Dancygier 2017). A leader focused treatment might accordingly fail to map onto real world party strategic variation. Finally, I suggest that concerns about the non-subtleness of my treatment might be exaggerated. Candidates' background are commonly emphasised during campaigns, both through media focus and deliberate promotions (e.g. Clarke et al. 2004). Such discussions about past occupations of MPs should activate similar cognitive processes as my treatments.

### 4.4 Reputation

I reveal parties' past reputation through their past appealing strategies (Table 1). Because it cannot be immediately manipulated by contemporaneous party decisions, party reputation of course differs from my other attributes. Moreover, in the real world reputation is clearly tied to partisan labels (cf. Robison et al. 2021).

Yet, a partisan label treatment would also most likely activate further non working class related considerations among voters, including, most obviously partisanship (e.g. Huddy, Bankert, et al. 2018). I balance these considerations by revealing the past emphasis in parties' campaigning

strategies. I argue this effectively imitates real world reputations that are often formed through broad aggregations of contemporaneous assessments of parties' actions by interest groups, media actors and more proximate social networks of voters (e.g. Cramer 2016). This operationalisation also follows my conceptualisation of reputation as average of past strategies.

### 4.5 Profile Generation

Taken together, in my conjoint respondents will assess the profiles of two parties six times, for instance after viewing the following Table 2. Because I only include attribute levels that should be minimally compatible with each other I do not need to form conditional randomisation schedules. Instead I can randomise across all attributes. I give equal weight to each level for all attributes. Following standard practice in conjoint experiments, I will also randomly vary the order of attributes across participants (but not across each participant's task) to avoid any potential ordering effects.

| **Attribute** | **Party A** | **Party B** |
|---|---|---|
| The party's policy platform emphasises pledges to... | ...increase support for disadvantaged areas. | ...deliver the economic policy the UK deserves. |
| In the past this party has been primarily seen as... | ...appealing to local communities. | ...appealing to the working class. |
| The party's campaign emphasises the need to... | ...support working people in the UK. | ...deliver for families in the UK. |
| Among the party's current MPs... | ...one can find voices from all parts of UK society. | ...many represent constituencies they have long lived in. |

Table 2: Second Dimension Policy Attributes and Levels.

## 5 Measurement and Analysis

In conjoint experiments, the necessary power to assess hypotheses about multiple treatment arms is reached through repeated observations for the same participants. In my case, respondents will

be asked to assess two profiles of parties with varying attributes *six* times. Treatment assignments occur at the individual profile level, i.e. for each party description I randomly vary each of the four relevant party attributes. Respondents are then asked to perform evaluations on the profile, including stating a preference for one of the two options. These tasks measure the main outcomes of interest for my experiment.

## 5.1 Outcome Measurement

Following standard practice in conjoint experiments, I include both forced-choice and rating based outcome measures (cf. Hainmueller et al. 2014). Both of these outcomes measure participants' evaluation of described parties. I ask respondents to choose their preferred party, simulating a vote decision, and to evaluate each party individually on an eleven point scale ranging from 0 (very poorly) to 10 (very highly). Including both measures should increase the validity of my results and reduce potential measurement errors. The first models a potentially more realistic "voting" scenario, while the second allows for a more direct measure of respondents' preferences for each party (ibid.). Of course effects should be consistent between the two measures (ibid.).

To probe potential mechanisms driving my results I also collect evaluations on individuals' perception of parties' economic policy stances on a 0 (very left) to 10 (very right) scale and a measure of "working class representation". Following Robison et al. (2021) I operationalise this measure on a 0 (not at all) to 10 (very much) scale that asks respondents how much they believe the party cares about people like them. Finally, to preclude any potential ordering effects I will randomly vary the order of these outcome measures across respondents (but not across tasks for the same respondent).

Finally, I will ask respondents who have completed the conjoint an open ended question: "How did you decide on these evaluations? What was most important for your reasoning?". Qualitative evidence from this question may further help me explore the mechanisms behind effects.

## 5.2 Pre-Treatment Measures

Before the conjoint experiment, I will measure standard demographic variables for each respondent (age, gender, income, education, occupation, political interest, political placement on economic

and second dimension scale, etc.). Income and occupation provide "objective" measurements for working class membership for my moderation analysis.

I also ask respondents about their identification with the working class. Robison et al. (2021) operationalise this question by providing respondents with six categories: lower, working, lower middle, middle, upper middle, or upper class. I follow this schema and, like them, further collapse these into four categories: lower/working, lower middle, middle, and upper middle/upper class.

Finally, to test the moderating effect of group affect for identity appeals, I need to collect participants' group evaluations before the experiment. In the US such group evaluations have long been captured by a group thermometer scale, that asks respondents to evaluate social groups on a 0 (very poorly) to 100 (very highly) scale (e.g. Miller et al. 1991). Adapting this measure to a simpler 0 to 10 scale I will measure group evaluation for families, immigrants, managers of large corporations, the working class, rural people and urban people.

### 5.3 Main Analysis

I will assess my primary hypotheses through non-parametric estimation of the average marginal causal effect (AMCE) of attribute levels, interacted with my three measures of individuals' working class identification/assessment. The AMCE represents the causal effect of changing an attribute level relative to the baseline level, averaged across all profile combinations that arise from varying attributes other than the attribute of interest (Hainmueller et al. 2014). In the absence of conditionality constraints on conjoint combinations, it is conveniently estimated with a simple linear regression on dummies of all attribute levels other than the baseline. In my proposed conjoint:

$$E_{ji} = \alpha_{WCA} + \beta^{k}_{WCA} E_{kj} + \gamma^{1}_{WCA} PR_j + \gamma^{2}_{WCA} GA_j + \gamma^{3}_{WCA} DR_j + \varepsilon_i \tag{1}$$

i.e. respondent i's evaluations of party j follows from the party's economic policy positions ($E_{kj}$), past reputation ($PR_j$), rhetorical group appeals ($GA_j$) and descriptive representation ($DR_j$), moderated by respondent i's assessment of the working class ($WCA$: operationalised through identification, evaluation and objective socio-economic status). Since each respondent evaluates multiple profile combinations I cluster my standard errors at the respondent level (cf. ibid.). AMCE estimation in R is possible with the cjoint package (Barari et al. 2023).

### 5.4 Interaction Effects

The magnitude of attribute interaction effects in conjoint experiments is sensitive to the choice of the baseline attribute levels and likely to produce false positives between the many attribute levels (Egami and Imai 2019). Egami and Imai (ibid.) instead propose an estimator for the average marginal interaction effect (AMIE). The AMIE measures the degree of interaction between two attributes by averaging across the difference between all possible profiles that is unexplained by the within attribute average marginal level effects (ibid.). It is insensitive to baseline specifications and easily regularised (ibid.). AMIE estimates between attributes can be further disaggregated to examine specific level combinations (ibid.). I will employ it to explore possible first order interaction effects between different attributes. AMIE estimation is implemented in R with the findit package (Egami, Ratkovic, et al. 2019).

To further explore heterogeneity based on the respondent characteristics I collect before the experiment, I will employ Bayesian Additive Regression Trees (BART) (cf. Green and Kern 2012). BARTs generates a counterfactual outcome schedule from the realised experiment to map heterogeneity in respondent causal effects (ibid.). As non-parametric machine learning algorithm BARTs systematically assess any potential heterogeneity based on supplied respondent characteristics (ibid.). I will employ them to explore potential respondent heterogeneity in AMCE and AMIE. BART estimation in R is possible with the BART package (McCulloch et al. 2024).

## 6 Power Analysis

To assess the viability of my proposed conjoint experiment I simulate a power analysis for expected effect sizes. The analysis informs my proposed implementation through an online experiment with 1000 respondents.

### 6.1 Effect Sizes

My hypothesised effect sizes are moderated by working class identification. I will oversample on socio-economic working class markers to ensure at least 50% working class members in my sample. These members should predominantly identify as working class. Indeed, even among the general UK population 60% of respondents viewed themselves as working class in 2016 (Evans

and Mellon 2016). I accordingly assume that 75% of my sample will identify as working class. Of course, positive evaluations of the working class should be even more prevalent, because they might extend beyond identifiers.

Existing experimental research on candidates' rhetorical group appeals to the working class finds large effects, moving respondents 20 percentage points on the evaluation scale (Robison et al. 2021). Robison et al. (ibid.) find similar effect sizes for the reputation of a candidate's party – operationalised through actual party labels of a moderate left wing and right wing party in the US and Denmark – and left-wing policy proposals. To my knowledge no experimental evidence on the effect of descriptive representation on party support exists. In an observational study of the UK, however, Heath (2015) argues that descriptive representation rather than policy divergence best explains the working class vote concentration for the Labour party.

Simultaneously, however, Robison et al. (2021) caution that ceiling effects might explain the insignificant effect of rhetorical appeals when combined with party labels and policy pledges. I argue such ceiling effects should be less concerning for my conjoint that allows for greater level variation and no partisan anchoring since I operationalise reputation differently. Conversely, my diverging operationalisation of rhetorical group appeals as campaign emphasis instead of conflictual statement might lower effect sizes[5]. I accordingly form conservative expectations about the effect of my three working class appeals and three policy positions on respondents who identify with the working class, placing all of them at the lower end of 0.1 standard deviation effect sizes. This roughly corresponds to a low effect size of 5 percentage points in conjoint studies (e.g Hainmueller et al. 2014; Schuessler and Freitag 2020). I also replicate my analysis for even smaller expected AMCEs of 4 and 3 percentage points.

Schuessler and Freitag (ibid.) show that the relevant parameters for power analysis in conjoint experiments are the number of levels of each attribute, the treatment assignment probability and the hypothesised AMCEs. The number of attributes, on the other hand, does not affect power calculations because increases in "noise" around effect sizes can be controlled for. Other than policy proposals, my attributes all include four levels with treatment assignment probabilities of 25 %. For policy I group levels in two categories – vague and concrete – and calculate effect sizes accordingly. My effective levels are thus 2 and treatment assignments probability is 50 % and I can

[5]Although such a costly signal might be more important to voters with rational choice motivations.

restrict my analysis to the more demanding non-policy dimensions. Moreover, assuming similarly sized or larger effects on perceptions about the party's group representation and policy position (for left-wing policy proposals), I can accordingly limit my simulation analysis to one non-policy attribute.

## 6.2 Simulation

Figure 1 illustrates the power curve for my hypothesised effect size and a 75 % share of working class respondents in the sample. I follow Schuessler and Freitag (2020) and run these simulations with their corresponding R implementation (see Appendix for documentation). If these assumptions hold, my conjoint would be sufficiently powered for sample sizes bigger than around 700 respondents for 5 percentage point effect sizes, 1100 respondents for 4 percentage point effects sizes and 1950 for 3 percentage point effect sizes (under the common power-threshold of 0.8). I implement the experiment for a sample of 1000 respondents. While a larger sample size would be desireable considering this power analysis I implement this experiment with limited resources for a Masters thesis. An implementation for 1000 respondents still achieves a good balance between cost considerations and concerns about a potentially underpowered experiment. My sample size should reliably detect AMCEs with sizes between 4 and 5 percentage points.

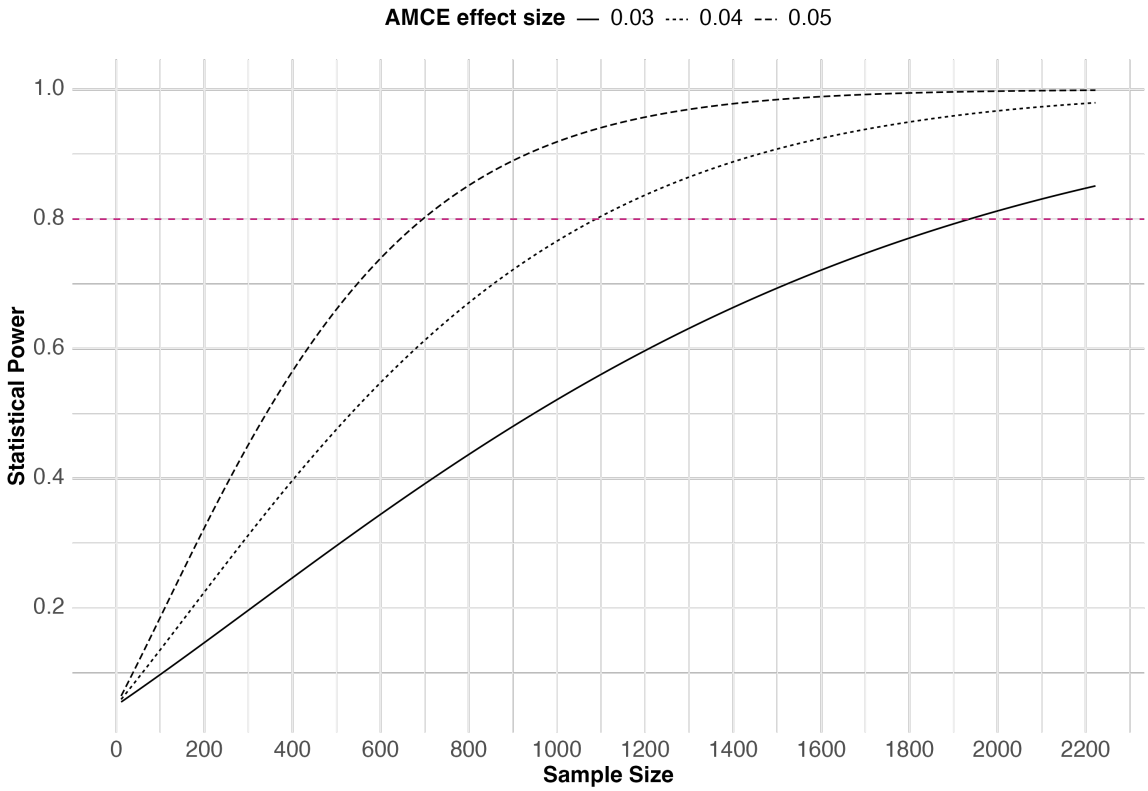


Figure 1: Power Curve for Three Possible AMCEs Levels

# 7 Robustness Checks

Finally, I discuss potential threats to my identification. These include the possibility of false positives, inadequate randomisation, spillover effects across conjoint choice rounds and issues of non-compliance that are especially likely in online conjoint experiments. These threats inform the robustness checks I plan to implement.

## 7.1 Multiple Hypothesis Testing

Conjoint experiments with multiple treatment arms and hypotheses are likely to falsely reject some of their null hypotheses (cf. Liu and Shiraito 2023). Accordingly, I plan to implement the Ash correction method as post hoc regularisation as an important robustness check to account for the likelihood of false positives amid multiple hypotheses testing (ibid.). Broadly speaking, Ash correction introduces information on the possibility of false positives by assuming that true

effect sizes follow a unimodal distribution with a "spike" at zero (Liu and Shiraito 2023). This assumption is then used to reduce sampling variance and shrink estimated coefficients, resulting in the reduction of significance levels for small absolute effects and narrower confidence intervals for large estimates (ibid.). Ash is easily implemented in R with the ashr package (Stephens et al. 2023).

## 7.2 Balance Tests & Attrition

Although random assignment of profiles should guarantee balanced attribute level assignment, it is common practice in experimental research to explicitly test the independence assumption through balance tests. If randomisation succeeded, respondent samples for different attribute levels should be balanced (Hainmueller et al. 2014). Accordingly, to assess the balance of my main attributes I will test for the joint insignificance of respondent demographic characteristics for level assignment as a robustness check. I do not anticipate significant attrition concerns, but will nonetheless estimate balance checks conditional on response rates for all levels of my attributes.

## 7.3 Spillovers

The estimation of AMCEs assumes stability across different choices, i.e. the same two profiles should yield exactly the same evaluations by the same respondent for every possible position among choice tasks (ibid.). This assumption might be violated if earlier choices benchmark participants' responses or allow them to adjust to inferred experimenter demands (ibid.). Formally, one way of testing the plausibility of the stability assumption is the comparison of ACMEs across different rounds choice (ibid.). I will conduct this test as robustness check. I can conduct similar comparisons across different attribute and outcome measure orderings.

## 7.4 Non-Compliance

Finally, conjoint experiments are particularly liable to non-compliance through "speeders", respondents that do not carefully assess profile information before completing outcome measures (e.g. Younger-Khan et al. 2024). To address biases arising from such speeders I will exclude respondents who spend less than two minutes on each choice. As a robustness check I will re-run

my analysis with the full sample (Younger-Khan et al. 2024). The randomness of speeder decision making should not impact my estimation beyond an increase of noise. I do not expect systematic variation in speeders. But even under systematic variation my recovered causal effects would of course remain valid (although population inferences would not).

# 8 Appendix: Power Analysis

```
#dataframe to run simulations
d <- expand.grid(
    #range of possible amces
    amce = c(0.03, 0.04, 0.05),
    #possible effective sample sizes
    n = seq(from = 100, to = 20000, length.out = 1000),
    #significance level
    alpha = 0.05,
    #four levels to each attribute
    levels = 4,
    #treatment assignment probability 0.25
    treat.prob = 0.25,
    sims = 10000
    )

#run simulations
df <- list2DF(do.call(cjpowr_amce, d))

#power plot
power_curve <- df %>%
  mutate(amce = as.factor(amce)) %>%
  #translate effective sample size into real sample size
  #two profiles and six tasks
  #share of working class 75%: no expectation about effects for other 25%
  mutate(n = n/(2*6*0.75)) %>%
  ggplot() +
  # by sample size, power and evaluation distribution
  aes(x = n, y = power, linetype = amce) +
  # smooth over discrete power values to generate power curve
  geom_line() +
  # 0.8 threshold
  geom_hline(yintercept = 0.8, colour = "#C6227F", linetype = "dashed") +
  # y axis 0-1
  scale_y_continuous(breaks = seq(0, 1, 0.2)) +
  scale_x_continuous(breaks = seq(0, 2200, 200)) +
  # labels
  labs(x = "<b>Sample Size</b>",
         y = "<b>Statistical Power</b>",
         linetype = "<b>AMCE effect size</b>") +
  theme_minimal() +
  # change text size
  theme(legend.position = "top",
        axis.title.x = element_markdown(size = 14),
        axis.text.x = element_markdown(size = 14),
        axis.title.y = element_markdown(size = 14),
```

```
        axis.text.y = element_markdown(size = 14),
        legend.title = element_markdown(size = 14),
        legend.text = element_markdown(size = 14))

# export figure
ggsave("power_curve.png", power_curve, width = 10, height = 7)
```

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