



# Racial Animus and Support for Labor Market Policies (Pre-Analysis Plan)

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## 1 Study design

Using two parallel experiments, we intend to study the causal effects of providing information about the number of Black people receiving benefits from two labor market policies (Temporary Assistance for Needy Families (sometimes referred to as welfare) and Unemployment Insurance) on support for these policies.

Each experiment will be conducted online (using Connect or Prolific) and will consist of three parts, each separated by two weeks. In the first part (lasting about 6 minutes), we will elicit basic demographic information, whether participants have previously benefited from either policy, political preferences, and both implicit (measured using an Implicit Association Task) and explicit attitudes towards race (measured through a statement of relative preference for Black and White). Respondents will be paid \$1.25 for their participation (\$15 per hour).

In the second part (lasting 5 minutes and paying a base wage of \$1), participants will randomly be sorted into one of four conditions comprising two parallel experiments. In the welfare policy experiment, participants will be asked to estimate the fraction of welfare recipients during 2021 that were Black, how confident they are of this estimate, and then they will be asked about their support for welfare. Before reporting their support, half of these people (selected at random) will be shown the correct answer, so we will be able to assess the causal effect of providing this information on their welfare support, while controlling for their priors. The unemployment insurance experiment will be similar; however, instead of eliciting beliefs about the number of Black people using welfare,

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here we will ask participants to state their beliefs about how many Black people used unemployment insurance in 2021, how confident they are of this estimate, and then randomly provide half of them with the correct number before eliciting their support to estimate the causal affect of this information on respondent support for unemployment insurance.

In each experiment, participants are incentivized to provide the correct belief, with a bonus of \$1 (in a five-minute experiment) for providing a belief that is within two percentage points on either side of the true number.

The third part of the experiment will test the persistence of both the intervention and treatment effects found in the second part. We will elicit incentivized beliefs about, and support for, *both* policies, which will allow us, in exploratory analysis, to consider the role of belief spillovers. Returning participants will be paid another \$1 in base compensation, with the opportunity to earn another \$1 in incentives. All belief bonus payments will be paid after the third part of the study is completed so that bonus payment cannot signal information about the correct racial composition beliefs to people in the control conditions.

For each experiment, there are two primary outcomes, measured at the end of the second part of the experiment. The first is (unincentivized) policy support: “Compared to current levels, you think that welfare (unemployment) benefits should be ...,” with responses collected using a slider ranging from -100% (end the benefits) to +100% (double them). The second is intended to reduce possible experimenter demand effects and to provide a real-stakes signal of support of welfare (unemployment) programs: participants will be able to donate some fraction of a second \$1 bonus to another randomly selected participant who has either received cash assistance sometime during the last five years in the welfare experiment or to someone who has collected unemployment benefits sometime during the last five years in the unemployment insurance experiment.

## 2 Estimating Average Treatment Effects

We will begin with simple comparisons of mean policy support for binned misbeliefs in control and treatment for both experiments, performed after the second stage. We also plan to estimate, for each outcome, the following second stage “information gradient” specification, with and without controls:

$$Y_i = \beta_0 + \beta_1 T_i + \beta_2 (Belief_i - True_i) + \beta_3 T_i (Belief_i - True_i) + \gamma X_i + \epsilon_i$$

where  $Y_i$  is policy support for participant  $i$ ,  $T_i$  is an indicator for the participants who receive the information about the correct number of Black people benefiting from a policy, the difference  $(Belief_i - True_i)$  is the perception gap between a respondent’s belief and the true statistic, and  $X_i$  is a vector of controls.

For participants whose prior beliefs match reality (i.e., where  $Belief_i - True_i = 0$ ), the estimating equation reduces to  $Y_i^k = \beta_0 + \beta_1 T_i + \gamma X_i + \epsilon_i$  and we expect  $\beta_1$  to be equal to 0 because the respondent hasn’t learned anything new from the information. When participants have some misperception of the true number of Black people receiving benefits, we expect the slope of the belief gradient ( $\beta_2$ ) to depend on one’s attitude toward Black people. Participants who are either implicitly or explicitly

biased against Black people should be identified by  $\beta_2 < 0$ , those who hold no relative preference should result in  $\beta_2 = 0$  (i.e., the race of the program participants does not affect support), and those who are biased in favor of Black people should result in  $\beta_2 > 0$ .

Our main interest is in the coefficient  $\beta_3$ , which measures the causal effect of the information provided on a participant’s support for each labor market policy. In particular, if participants are biased against Black policy participants, we expect  $\beta_3 > 0$  because when they learn that there are more (less) Black program participants than they believed, they should support the policy less (more). That said, if participants have the opposite preference toward Black policy participants, the sign of  $\beta_3$  should be reversed. We will also test the supplemental hypothesis  $\beta_3 = -\beta_2$  that information is, in effect, the perfect antidote to misperception.

Because there is good reason to believe that changes in the belief gradient may also depend on whether participants believe the information they receive is “good” or “bad” news, we also plan to examine behavior on each side of the true value separately (Kahneman, 2011).

We shall estimate belief persistence using a similar specification:

$$(Belief_i^{+1} - True_i) = \gamma_0 + \gamma_1 T_i + \gamma_2 (Belief_i - True_i) + \gamma_3 T_i (Belief_i - True_i) + \gamma_4 X_i + \epsilon_i \quad (1)$$

where  $Belief_i^{+1}$  is the third stage belief of participant  $i$ . Here,  $\gamma_2$  is a measure of the stability or persistence of beliefs in the control population, where  $\gamma_2 = 1$  is a natural hypothesis to test. The coefficient  $\gamma_3$  measures the “pass through” of information provision, and our prior is that  $\gamma_3 \leq 0$ . That is, the larger the misperceptions gap in the second stage, the smaller it will be in the third if participants update based on new information, and we shall test this hypothesis. We shall also test whether  $\gamma_2 + \gamma_3 = 0$ , that is, whether information provision reduces mean misperception to zero.

We also plan to estimate two variants of this specification, one in which misperception about policy  $j$  is regressed on policy  $k$  misperception, treatment and interaction, to evaluate the extent of belief spillover, and another in which the outcome is not third stage misperception, but rather participation, to determine whether attrition is predicted by either treatment or misbelief. (There is another variation of the same specification, in which second stage misbeliefs are regressed on third stage misbeliefs and treatment indicators, that serves as a placebo test.)

Last, as additional exploratory analysis, we are interested in whether third stage preferences are predicted by third stage misbeliefs:

$$Y_i^{+1} = \alpha_0 + \alpha_1 (Belief_i^{+1,j} - True_i^j) + \alpha_2 (Belief_i^{+1,k} - True_i^k) + \alpha_3 X_i + \epsilon_i \quad (2)$$

where  $j$  and  $k$  denote the two beliefs. Because  $Belief_i^{+1,j}$  and  $Belief_i^{+1,k}$  are not exogenous, we shall instrument for both using assignments to treatment.

### 3 Subgroups

We expect that the belief gradient and treatment effects will differ by participant characteristics that capture possible differences in confidence, attitudes towards Black people, support for labor market policies, more generally, and possible exposure to (or use of) said policies.

Our principal hypothesis is that the effects will differ for those who score high and low on either our implicit or explicit measures of racism, and by self-identified race. To evaluate, we will first estimate our baseline models for median sample splits. We'll then estimate an extended version of the model in which racism, first as indicator and then as continuous variable, is included both as standalone regressor and interacted with misbelief, treatment and the interaction of treatment and misbelief, where the coefficient on the latter is the estimate of interest.

As secondary hypotheses, we will consider other subgroup differences, including participant political preferences (the attitudes of liberals and conservatives toward race and labor market policies might differ) and socioeconomic status. These individual characteristics will be collected in part one of the experiment.

Finally, using participants' self-reported confidence in their beliefs about the number of Black program participants, we want to allow for the fact that treatment effects may differ across participants who have weak priors and strong priors. For example, two participants who express the same quantitative belief, but differ in their certainty, may change their beliefs in differing degrees in response to the information treatment. There are two possible mechanisms: 1) rational Bayesian updating and 2) a behavioral response to a salient surprise.

Under rational Bayesian updating, we anticipate treatment effects which increase with participants' uncertainty - the weight of the information signal in belief construction will be greatest for individuals with weak priors. In addition, when beliefs are elicited in the third wave these individuals with weak priors may be more likely to persistently update them in response to the information treatment.

A behavioral response to surprising information, however, may prompt treatment effects in the opposite direction. Individuals who receive an information signal which counters a strong belief may overrespond relative to those with initially weak beliefs. The effect of this surprising information may also lead to more belief persistence among those with strong priors.

To empirically evaluate the role of confidence in mediating treatment effects, we plan to split the sample at the median level of confidence and test for differences in treatment effects and belief persistence between and hi- and low-confidence participants. Given the possibility of statistical discrimination, we are also interested in the interaction between self-reported confidence and the role of racism. To explore this, we plan to split participants along the median level of bias against Blacks (both the implicit and explicit measures) and separately estimate Equation 1 within these two subgroups. We also plan to re-estimate equation 1, amending the  $\gamma_3$  term to now include a triple interaction of the treatment, racial bias, and misperception terms.

## 4 Power

With the approval of the Middlebury College IRB (Protocol 125, Policy views and beliefs about racial composition), we conducted a pilot experiment in December of 2023 with a total of 380 participants who did parts one and two of the experiment. Our power calculations, based on the results of this pilot, are consistent with the rule of thumb offered in Haaland et al. (2023), who

suggest gathering enough observations to detect a 0.15 standard deviation effect (with power = 0.8, significance = 0.05). Using this benchmark, our pilot indicates that we should gather between 600 and 700 observations per experimental arm or a total of approximately 2800.

## 5 References

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