

Income Inequality and White-on-Black Racial Bias in the United States: Evidence From Project Implicit and Google Trends



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Abstract

Several theories predict that income inequality may produce increased racial bias, but robust tests of this hypothesis are lacking. We examined this relationship at the U.S. state level from 2004 to 2015 using Internal Revenue Service–based income-inequality statistics and two large-scale racial-bias data sources: Project Implicit ($N = 1,554,109$) and Google Trends. Using a multimethod approach, we found evidence of a significant positive within-state association between income inequality and Whites' explicit racial bias. However, the effect was small, with income inequality accounting for 0.4% to 0.7% of within-state variation in racial bias, and was also contingent on model specification, with results dependent on the measure of income inequality used. We found no conclusive evidence linking income inequality to implicit racial bias or racially offensive Google searches. Overall, our findings admit multiple interpretations, but we discuss why statistically small effects of income inequality on explicit racial bias may nonetheless be socially meaningful.

Keywords

income inequality, prejudice, racial and ethnic attitudes and relations, intergroup dynamics, open data, open materials

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Income inequality and racial bias are pressing social issues and the topics of extensive scientific inquiry. Recent increases in income inequality (Piketty & Saez, 2014) have sparked investigations into its impacts on individual and societal well-being (e.g., Wilkinson & Pickett, 2009). Meanwhile, the persistence of racial bias continues to motivate studies of its psychological, social, and institutional precursors and consequences (e.g., Alexander, 2012).

Several social-scientific theories converge on the hypothesis that income inequality may increase racial bias. Epidemiologists Wilkinson and Pickett argue that income inequality intensifies social hierarchies, motivating status seeking via derogation and the subordination of lower status others (Wilkinson, 2005). Given that race is intimately associated with social status in the United States (Moller, Alderson, & Nielsen, 2009), this suggests that income-inequality–related processes should increase racial prejudice among dominant racial-group members.

Two prominent social psychological theories—social-dominance theory (Sidanius & Pratto, 2001) and system-justification theory (Jost, Banaji, & Nosek, 2004)—converge on this hypothesis as well, positing that racism functions as a legitimizing myth or mode of rationalization used to justify group-based social hierarchies. To the extent that income inequality is linked with income differences between racial groups, social-dominance theory and system-justification theory predict that income inequality will breed racial bias among members of dominant racial groups. Marxist accounts of economic inequality posit that income inequality leads wealthy elites to promote racial division among the working classes to prevent unified labor movements (Reich, 1983).

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Existing Empirical Evidence on the Inequality–Racial Bias Linkage

Each of these theories suggests an inequality–racism hypothesis: Income inequality leads to racial bias among individuals from higher status racial groups. To date, however, this question has received little direct empirical attention. Select economic research has framed White-to-Black income ratios as a measure of racial bias and found them to be associated with overall income inequality (e.g., Reich, 1983). Yet if income gaps between the rich and poor fluctuate and Blacks have disproportionately lower incomes, White-to-Black income ratios are likely to covary with overall income inequality in ways unrelated to individual-level racism. Epidemiologists (Kennedy, Kawachi, Lochner, Jones, & Prothrow-Stith, 1997) have documented a correlation between the proportion of U.S. households in poverty and racially biased attributions for Blacks' poverty (e.g., attributions to lower innate abilities). However, poverty and income inequality are distinct phenomena—low income inequality can exist alongside high poverty, and vice versa—so this, too, does not establish an inequality–racism link. And although a recent study documented that greater income inequality in U.S. counties predicted higher police violence against Blacks (Ross, 2015), this finding was not robust to controlling for region-level mean income.

The Present Research: Estimating the Effect of Income Inequality on Racial Bias

In light of the theoretical relevance of the inequality–racism link and the scant empirical evidence, we sought to test the inequality–racism hypothesis using data tracking variation in income inequality and racial bias within and between U.S. states during the period of 2004 to 2015. We obtained data from large-scale publicly available sources and used a multimethod approach to tackle a number of methodological challenges.

Method

Data sources and model specification

Income inequality. Our primary measure of income inequality was derived from state-year-level Gini coefficients computed from the Internal Revenue Service's (IRS's) Statistics of Income (Frank, 2005). Gini coefficients measure income inequality across the entire income distribution and range from 0 (perfect equality) to 1 (perfect inequality). We prefer IRS-based Ginis to the commonly used Gini data provided by the U.S. Census Bureau's American Community

Survey (ACS) for multiple reasons. First, the IRS data are based on all tax filers, whereas the ACS data are based on smaller samples. Second, there are strong disincentives for misreporting income to the IRS but relatively weak disincentives for misreporting income to the ACS, and evidence suggests that low-income respondents tend to overreport and high-income respondents tend to underreport incomes in the latter context (Akhand & Liu, 2002). Third, IRS data are less extensively top coded (capped at a maximum upper limit) than ACS data and thus better for including high-income households. We relied on IRS-based Ginis for focal analyses but included ACS-based Ginis in a specification-curve analysis (Simonsohn, Simmons, & Nelson, 2015; see below).

Project Implicit racial-bias data. Project Implicit (Xu, Nosek, & Greenwald, 2014) has obtained measurements of the racial bias of millions of Americans, beginning in 2003. We focused on the 12-year period from 2004 to 2015 because of data availability (Project Implicit began measuring respondents' political orientation in 2004, and IRS Gini data are available until 2015). We restricted analyses to Whites only because the theories outlined above are primarily relevant to dominant racial groups and because Project Implicit's measures rely on comparing respondents' relative positivity toward Whites and Blacks. Our focus was U.S. states, so we excluded data from Puerto Rico and other territories. We excluded respondents younger than 15 years and older than 64 years because of the relative scarcity of data within these age ranges, which made poststratification weighting infeasible (there were 37,071 respondents younger than 15 years and 31,897 respondents older than 64 years). After these exclusions, the data set contained 1,554,109 responses from Whites (58% female; age: $M = 27.96$ years, $SD = 11.62$) that could be matched to 50 U.S. states and the District of Columbia from 2004 to 2015 (although actual N s varied by outcome because of missingness; see Table 1). Respondents were clustered in 612 state-years (mean responses per state-year = 2,539, median responses = 1,519, $SD = 2,608$, range = 28–16,182).

Implicit-association-test (IAT) scores. Project Implicit collects three main measures of anti-Black bias from respondents. First, the IAT (Greenwald, Nosek, & Banaji, 2003) asks respondents to categorize Black and White faces and positive and negative words (e.g., *beautiful*, *terrible*) via timed key presses. Faster responses on compatible trials, in which White faces and positive words or Black faces and negative words require the same key, compared with incompatible trials, in which “White” and “bad” or “Black” and “good” require the same key, are interpreted as indicating implicit anti-Black bias. Following convention, responses were converted into D scores,

Table 1. Descriptive Statistics and Correlations for Model Variables at Individual and State-Year Levels

Variable	\hat{r}																		
	<i>M</i>	<i>SD</i>	<i>SD</i> (within states)	<i>SD</i> (between states)	Range	<i>N</i>	1	2	3	4	5	6	7	8	9	10	11	12	
1. IAT score	0.39	0.42	0.42	0.02	-1.9-1.83	1,456,884	—	—	—	—	—	—	—	—	—	—	—	—	—
2. Preference for Whites ^a	13.54	3.72	3.72	0.14	0-24	1,439,742	.09	—	—	—	—	—	—	—	—	—	—	—	—
3. Thermometer difference	0.77	1.79	1.78	0.13	-10-10	1,503,863	.21	.37	—	—	—	—	—	—	—	—	—	—	—
4. Liberalism ^a	16.95	8.89	8.71	1.79	0-30	1,554,109	-1.1	-0.08	-0.22	—	—	—	—	—	—	—	—	—	—
5. High school dummy	0.1	0.3	0.3	0.01	0-1	1,554,109	.01	.01	.05	-0.07	—	—	—	—	—	—	—	—	—
6. College dummy	0.54	0.5	0.49	0.06	0-1	1,554,109	.04	.0	.04	-0.15	-0.36	—	—	—	—	—	—	—	—
7. Advanced degree dummy	0.36	0.48	0.48	0.06	0-1	1,554,109	-0.05	.0	-0.07	.2	-0.25	-0.81	—	—	—	—	—	—	—
State-year level																			
1. IAT score	0.39	0.03	0.03	0.02	0.18-0.52	612	—	—	—	—	—	—	—	—	—	—	—	—	—
2. Preference for Whites ^a	13.53	0.6	0.58	0.14	12.26-16.7	612	.29	—	—	—	—	—	—	—	—	—	—	—	—
3. Thermometer difference	0.77	0.23	0.19	0.13	0.01-1.63	612	.79	.54	—	—	—	—	—	—	—	—	—	—	—
4. Liberalism ^a	16.95	2.09	1.08	1.79	11.2-23.84	612	-0.63	-0.2	-0.7	—	—	—	—	—	—	—	—	—	—
5. High school dummy	0.1	0.02	0.02	0.01	0.01-0.21	612	.27	-0.07	.32	-0.26	—	—	—	—	—	—	—	—	—
6. College dummy	0.54	0.08	0.05	0.06	0.29-0.79	612	.41	.03	.49	-0.73	.22	—	—	—	—	—	—	—	—
7. Advanced degree dummy	0.36	0.09	0.06	0.06	0.12-0.69	612	-0.44	-0.01	-0.53	.73	-0.49	-0.96	—	—	—	—	—	—	—
8. Google racial-slur searches ^b	69.95	18.32	12.13	13.71	27.08-157.44	612	.47	.52	.51	-0.31	.16	.07	-0.11	—	—	—	—	—	—
9. IRS Gini	.62	.04	.02	.03	.54-71	612	.04	-0.14	-0.08	.12	-0.14	-0.04	.08	-0.15	—	—	—	—	—
10. ACS Gini	.46	.02	.01	.02	.39-54	612	-0.01	-0.19	-0.21	.27	-0.31	-0.33	.39	-0.06	.7	—	—	—	—
11. Mean income logged	10.88	0.17	0.08	0.15	10.29-11.44	612	-0.33	-0.3	-0.45	.57	-0.12	-0.54	.52	-0.45	.22	.28	—	—	—
12. Poverty rate	14.36	2.82	1.3	2.5	7.1-24.2	612	.13	-0.15	.08	-0.36	-0.23	.35	-0.25	.07	.36	.46	-0.49	—	—
13. Proportion Black	.12	.08	.0	.08	0-58	612	.43	.16	.32	-0.29	-0.05	-0.1	.11	.41	.13	.36	-0.15	.29	—

Note: All statistics have been weighted to correct for nonrepresentativeness. IAT = implicit association test; IRS = Internal Revenue Service; ACS = American Community Survey.

^aPreference for Whites was converted to a scale from 0 to 24 and liberalism to a scale from 0 to 30 to combine different-length versions of each scale. ^bThe scale for Google racial-slur searches is arbitrary. Scores track proportions of searches for the racial slur *nigger* within state-years; West Virginia's 2015 proportion is scored 100, and each other state-year is scored relatively.

representing individuals' mean latencies on incompatible trials minus mean latencies on compatible trials, divided by the standard deviation of all trials. Scores greater than 0 indicate implicit anti-Black bias, with larger scores indicating greater bias (Greenwald et al., 2003).

Preference for Whites. Respondents reported their explicit preference for Whites compared with Blacks on a scale ranging from “*I strongly prefer African Americans to European Americans*” to “*I strongly prefer European Americans to African Americans.*” The middle option was “*I like European Americans and African Americans equally.*” From 2004 to 2006, this was a 5-point scale, but from 2007 to 2013, it became a 7-point scale. We converted scores on both versions into scales ranging from 0 to 24 (5-point range = 0, 6, 12, 18, 24; 7-point range = 0, 4, 8, 12, 16, 20, 24) and combined them.

Thermometer bias. Respondents were asked to rate “how warm or cold you feel toward the following groups” for European Americans and African Americans separately on 11-point scales (0 = *coldest feelings*, 5 = *neutral*, 10 = *warmest feelings*). We computed the difference between these scores for each respondent (warmth toward European Americans minus warmth toward African Americans).

Google Trends racial-bias data. Our second source of racial bias data was Google Trends (<https://trends.google.com/trends/>), which allows access to state-level data on the relative proportion of all Google searches containing specific terms. Past research has operationalized region-level racial bias as the relative proportion of Google searches within regions that contained the racial slur *nigger*. This measure has been validated against other measures of explicit racial bias and aggregate-level demographic characteristics and found to be related to reduced likelihood of voting for Barack Obama (Stephens-Davidowitz, 2014) and increased mortality rates among African Americans (Chae et al., 2015).

Google Trends provided two forms of data used in the current project. First, we used cross-sectional data for single years on the relative proportion of searches containing the slur within each state. These data are standardized by Google, with the state displaying the highest proportion set to 100 and other states scored relatively. Second, we used time-series data for single states on the relative proportion of searches containing the slur in each month since 2004. These data are also standardized by Google, with the month showing the highest proportion set to 100 and each other month scored relatively. These standardizations mean that Google's cross-sectional data do not provide information about changes over time, nor do the time-series

data provide information about comparisons between states. However, it was possible to combine data from each format to incorporate both kinds of information. The absolute scale of the resulting measurements became arbitrary through this process, but the relative comparisons across states and years were correct. A fuller account of how we constructed the Google slur searches measure is provided in the Supplemental Material available online.

Control variables. A central challenge for research using observational data is model specification. Neglecting to control for variables that causally affect both focal predictors and outcomes (*confounds*) biases estimates, but so, too, does controlling for variables that are causally affected by focal predictors and outcomes (*colliders*) or that intervene between predictors and outcomes (*mediators*). This difficulty is compounded when there is no definitive evidence as to the causal relationships among potential control variables, predictors, and outcomes.

We responded to this challenge in two ways. First, we carefully chose a preferred model, and we explain our reasoning behind each chosen covariate. Focal tests of the inequality hypothesis were performed using this preferred model. Second, we performed a specification-curve analysis (Simonsohn et al., 2015), which offered a robustness check of our results to alternative model specifications, and provide a transparent account of the impact of each control variable on the results. We describe the specification analysis further below. Our control variables were as follows.

State-year mean income (logged). Overall income levels are generally associated with both income inequality and a diverse range of social outcomes, so it is standard practice to control for regional average incomes when attempting to estimate effects of inequality. It is also routine to log the mean incomes because of the tendency for them to have diminishing marginal effects (Blakely & Kawachi, 2001). State-year measures of mean income per capita were obtained from IRS data (Frank, 2005), converted into units of thousands, and log transformed.

Educational achievement and state-year poverty rate. It is well understood that nonlinear effects of individual-level income on outcomes can confound estimated effects of inequality (e.g., Lynch, Smith, Kaplan, & House, 2000). This presented a challenge for the current project because we did not have access to individual-level income data for most of our sample. However, we did have the ability to test the validity of this concern.

One step that we took to guard against confounding by the effect of nonlinear individual income was to control for its influence with proxy measures: education and

state-year-level poverty rates. Educational achievement is itself a plausible confound of any nonlinear relationship between income and racial bias because it may causally affect both variables. It therefore made for a natural proxy for individual-level income. Project Implicit assigns participants to 1 of 14 educational categories, but many categories are rare. We placed respondents into 1 of 3 well-represented categories: high school graduates and below (317,362 respondents), college attendees and graduates (901,217 respondents), and advanced degree holders (335,530 respondents).

Poverty rates are also a useful proxy to control for nonlinear income effects. To see why, it is necessary to understand how nonlinear effects of individual income can produce spurious effects of inequality. Figure 1 illustrates this. The top panel depicts racial bias as a nonlinear function of income and shows individual-level data for two states (A and B), each with two inhabitants. States A and B each have the same mean income but have more equal (state A) or more unequal (state B) income distributions. The more unequal state (B) exhibits higher racial bias because of the nonlinear effect of income.

The top panel of Figure 1 also shows why controlling for poverty rates helps address this problem where it exists. More unequal states appear more racially biased in this scenario because they have more low-income individuals, whose lower income levels increase racial bias exponentially. Because poverty rates provide a measure of the relative number of low-income individuals within a state, they help account for this nonlinearity.

Importantly, controlling for poverty rates does not completely unbiased estimates of income-inequality effects when there are nonlinear income effects. However, it does substantially reduce bias in estimated effects of income inequality in such a context, which we demonstrate via simulation in the Supplemental Material. Also, we found that if income was measured with moderate amounts of error (true and measured incomes correlate at $r \sim .8$), controlling for poverty rates was virtually as effective at removing bias as controlling for nonlinear income effects themselves (albeit with increased variance). Thus, if controlling for poverty rates had little or no effect on estimates of income inequality, it could be taken as evidence that there is little bias to be reduced. We obtained estimates of state-year poverty rates from U.S. Census Bureau data.

State-year proportion Black. The proportion of Blacks within U.S. regions has been linked to both income inequality (Moller et al., 2009) and anti-Black discrimination (Angle, 1992) and is argued to confound the relationship between income inequality and health outcomes in the United States (Deaton & Lubotsky, 2003). We therefore considered it another important potential

confound of the relationship between income inequality and racial bias. Estimates of state-year proportion Black were obtained from the U.S. Census Bureau.

Individual-level political orientation. Considerable evidence suggests that political ideology, racial attitudes, and attitudes toward social hierarchy are intimately connected in the United States (Sidanius & Pratto, 2001). We theorized that ideological stances toward social hierarchy might also confound the inequality-racism relationship by predisposing Whites to adopt racist ideas and also to support nonegalitarian policies and institutions that produce greater income inequality. To account for this possibility, we included measures of individual-level left/right political orientations, as measured by Project Implicit, in our preferred models. Until 2006, this was a 6-point scale (1 = *strongly conservative*, 6 = *strongly liberal*), after which it was changed to a 7-point scale (1 = *strongly conservative*, 7 = *strongly liberal*). We converted scores on both versions into 30-point scales (6-point range = 0, 6, 12, 18, 24, 30; 7-point range = 0, 5, 10, 15, 20, 25, 30) and combined them.

Year fixed effects. In recent decades, the United States has seen overall increases in income inequality but also downward trends in many measures of racial bias (Bobo, Charles, Krysan, Simmons, & Fredrickson, 2012). We considered these opposing nation-level trends to be another potential confound of estimates of the within-state effect of income inequality because together they render it more likely that observations in more recent years would display lower racial bias but greater income inequality. Because we were interested in making inferences about states rather than the nation as a whole, we controlled for overall trends in racial bias by including fixed effects for year of measurement (Curran & Bauer, 2011).

Descriptive statistics and correlations for all model variables are displayed in Table 1 (an extended version of this table with confidence intervals, or CIs, for bivariate correlations is included in the Supplemental Material).

Hierarchical linear models

Our focal analyses used hierarchical linear modeling (HLM). We treated the Project Implicit data as having three levels, with individuals nested in state-years nested in states, and included random intercepts for state-years and for states to account for this three-level nested structure. Separate models were fitted for each of the three outcome measures. Models were fitted using the *lme4* and *lmerTest* packages in the R programming environment (R Core Team, 2018).

Variance decomposition. Our data had multiple levels of variation because of the nested structure: variation

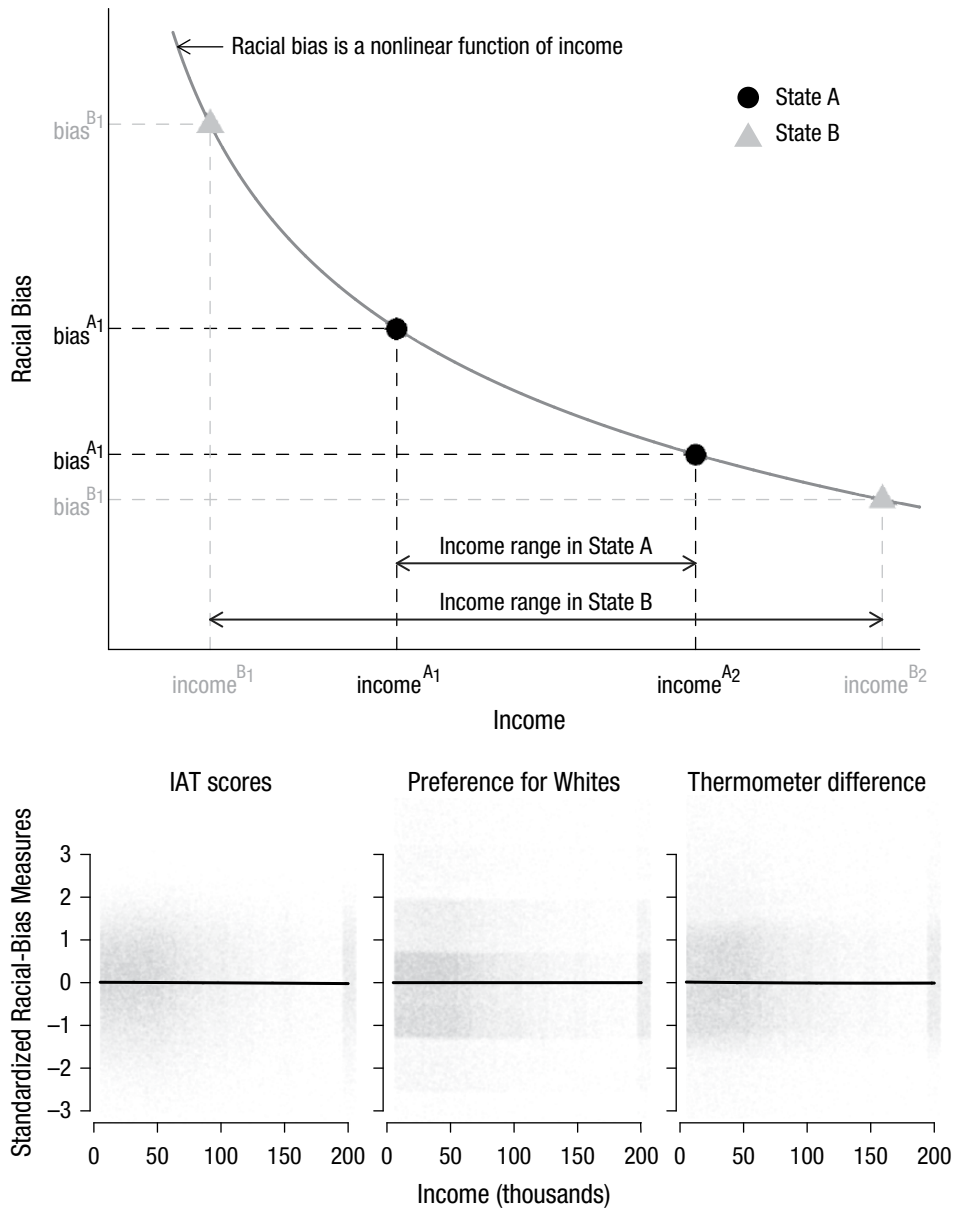


Fig. 1. Racial bias as a nonlinear function of income (top panel) and relationship between income and three measures of racial bias (bottom panels). The top panel shows individual-level data for two states (A and B), each with two inhabitants. States A and B each have the same mean income but have more equal (state A) or more unequal (state B) income distributions. The more unequal state (B) exhibits higher racial bias because of the nonlinear effect of income. The bottom panels depict the observed null relationship between income and each of three measures of racial bias, controlling for model covariates. Points are jittered, and lines display second-order polynomial lines of best fit.

between individuals within state-years around state-year means (individual-level variation), variation over time within states of state-year means around overall state means (state-year-level variation), and variation between states in terms of overall state means (state-level variation). We decomposed individual-level predictors (education, political orientation) into these three levels of

variation prior to HLM analyses. Decomposing variance in this way allowed predictors to have different effects at these different levels of analysis and avoided constraining effects across levels to equality (Zyphur, Kaplan, & Christian, 2008). To illustrate, for any given individual-level predictor x measured at the individual level, scores (and thus variances) were decomposed as follows:

$$x_{ijt} = x_j + (x_{jt} - x_j) + (x_{ijt} - x_{jt}), \quad (1)$$

where x_{ijt} is the score on x for individual i in year t in state j , and variation and effects in the model can be understood as follows: The Level 3 component associated with a state (across individuals and years) is x_j , which is the overall state mean of x for state j ; the Level 2 component associated with a given year for a particular state is $x_{jt} - x_j$, which is the deviation of the state-year mean x_{jt} from the overall state mean x_j ; and the Level 1 component associated with a particular individual for a given year and state is $x_{ijt} - x_{jt}$, which is the deviation of an individual's x_{ijt} from the state-year mean x_{jt} . The effects of these separate components of x on an individual-level outcome y can then be modeled using HLM as follows:

$$y_{ijt} = \beta_{0ijt} + \beta_1(x_{ijt} - x_{jt}) + e_{ijt}, \quad (2)$$

$$\beta_{0ijt} = \beta_{0j} + \beta_2(x_{jt} - x_j) + u_{jt}, \quad (3)$$

$$\beta_{0j} = \gamma_{00} + \beta_3 x_j + u_j, \quad (4)$$

where y_{ijt} is the outcome y for individual i in year t in state j ; β_{0ijt} is the intercept (i.e., model-estimated average) for state-year tj ; β_1 is the individual-level effect of x ; e_{ijt} is the individual-level residual; β_{0j} is the intercept (i.e., model-estimated average) for state j ; β_2 is the state-year-level effect of x ; u_{jt} is the state-year-level residual for state-year tj , which allows its state-year-level intercept to randomly vary; γ_{00} is the grand intercept; β_3 is the state-level effect of x ; and u_j is the state-level residual for state j , which allows its state-level intercept to randomly vary.

State-year-level predictors (income inequality, mean income logged, poverty rate, proportion Black) had no individual-level variation so were necessarily decomposed into variation only within and between states. Thus, for any given predictor x measured at the state-year level, scores (and thus variances) were decomposed as follows:

$$x_{jt} = x_j + (x_{jt} - x_j), \quad (5)$$

where x_{jt} is the score on x for year t in state j , and $x_{jt} - x_j$ is the deviation of the state-year mean x_{jt} from the overall state mean x_j .

For Google slur searches, we had access only to state-year-level data, so models treated data as having two levels, with state-years nested within states, and included random intercepts only at the state level, like so:

$$y_{jt} = \beta_{0j} + \beta_1(x_{jt} - x_j) + e_{jt} \quad (6)$$

$$\beta_{0j} = \gamma_{00} + \beta_2 x_j + u_j, \quad (7)$$

where y_{jt} is the mean of outcome y for year t in state j ; β_{0j} is the intercept (i.e., model-estimated average) for state j ; β_1 is the within-state effect of x ; e_{jt} is the state-year-level residual; β_2 is the between-state effect of x ; u_j is the state-level residual for state j , which allows its state-level intercept to randomly vary; and γ_{00} is the grand intercept.

Standardization. All continuous variables were transformed into weighted z scores prior to decomposition into individual-level, state-year-level, and state-level components to facilitate interpretation of results. This allowed model estimates to be interpretable as standardized coefficients—the estimated change for an outcome in standard-deviation units for an increase in predictors of 1 standard deviation. Educational achievement was modeled via dummy variables indicating advanced degree holders and high-school-educated respondents, with college-educated respondents set as the reference group.

Poststratification weighting. The Project Implicit data set is made up of respondents who voluntarily completed measures of implicit and explicit racial bias online. As a result, it provides a nonrepresentative sample, with respondents being on average younger and more female than the U.S. population. Google Trends' sample is also not representative because it is weighted according to which individuals perform the most Google searches.

We attempted to address this challenge by using poststratification weighting in all analyses. For Project Implicit data, this entailed giving observations within gender, age, and state subgroups weights proportionate to their overall representation in the U.S. population. Respondents were allocated into cells according to age group (15–24, 25–44, and 45–64 years), gender, state of residence, and year of measurement, and proportions of our sample falling within each cell were computed. We then used demographic data from the U.S. Census Bureau to obtain demographically accurate population proportions for each cell. Weights were computed for individuals in each cell by dividing cells' accurate population proportions by their observed proportions in our data. Thus, if individuals within a cell made up .01 of the population but only .005 of our sample, they would be assigned a weight of 2 (.01/.005).

For data aggregated at the state-year level (which we used in bootstrapping procedures; see below), we used individual-level weights to compute weighted means

of each variable in each state-year and then summed each state-year's weights to create state-year-level weights. Google Trends data were available only at the state-year level and so were weighted only according to state-years' relative populations.

Aggregation. Power and specification-curve analyses relied on bootstrapping, which proved infeasible with the full Project Implicit individual-level data set in terms of computing power and time. As a result, we aggregated Project Implicit data at the state-year level for these analyses.

This aggregation was possible with minimal impact on results because our goal was to test the inequality–racism hypothesis, and income inequality was defined and measured as a state-year-level variable. Thus, it was orthogonal to variation within state-years (i.e., differences between people within a given state in a given year) on all other variables. Thus, provided that appropriate weights were used to account for differently sized states, aggregation to the state-year level had minimal impacts on estimated effects of income inequality. In the Supplemental Material, we report results produced by rerunning our focal models with aggregated data and show that estimated effects of income inequality were essentially unchanged compared with models fitted on individual-level data. We also offer a simulation demonstrating this redundancy of within-state-year variation for estimated slopes of state-year-level variables.

This aggregation also helps clarify that relationships observed between income inequality and outcomes are due to covariation at the aggregate level. Because income inequality is inherently ecological, it is orthogonal to variation across individuals within social aggregates. So, although all theories outlined above propose cross-level effects, whereby an ecological feature of a social aggregate (income inequality) exerts effects on an individual-level variable (racial bias), such cross-level effects of income inequality can be detected only by examining covariation between income inequality and aggregates of individuals. It is therefore important to remember that although we were seeking to test cross-level effects of income inequality on individuals, all effects we observed were by necessity due to aggregate-level relationships.

System generalized-method-of-moments (GMM) models

Whereas the present project was focused on the effect of income inequality on racial bias, there are also theoretical arguments for predicting an effect of racial bias on income inequality. For example, social-dominance theory and system-justification theory posit ways in which racial bias among members of dominant racial

groups not only can serve to rationalize but also can exacerbate intergroup inequalities—for example, by affecting education, employment opportunities, and biases in the criminal justice system (Jost et al., 2004; Sidanius & Pratto, 2001). Thus, even if all third-variable confounds are controlled for, estimated effects of income inequality on racial bias could still be biased by reverse causality.

We attempted to address this by using system GMM estimators (Arellano & Bond, 1991). This is an econometric approach to panel data that uses instrumental variables, a tool for making causal inferences when experiments are not possible. In brief, an instrumental variable is correlated with an endogenous predictor but is not itself a part of the model (i.e., it is not affected by the dependent variable, does not have a direct influence on the dependent variable, and is uncorrelated with the error term). Instrumental-variable estimators can be conceptually described as consisting of two stages. First, the endogenous predictor variable is regressed on the instruments available, and the predicted values of this model are saved. Then, the predicted values are used in place of the endogenous predictors to estimate those predictors' causal effects. The reasoning is that if an exogenous instrument can be found, then this variable's overlap with a focal, endogenous predictor is also exogenous. In turn, this exogenous part of a focal predictor can be used to predict an outcome and strengthen causal inferences.

System GMM models incorporate instrumentation within a system of equations on the basis of both levels of variables (observed state-level values of income inequality at time $t = \text{inequality}_{i,t}$) and first differences (within-state changes in income inequality at time $t = \Delta \text{inequality}_{i,t} = \text{inequality}_{i,t} - \text{inequality}_{i,t-1}$). In the system, first differences of outcomes are predicted from first differences of regressors, with lagged observations of levels used as instruments (e.g., $\text{inequality}_{i,t-1}$ and $\text{inequality}_{i,t-2}$ are instruments for $\Delta \text{inequality}_{i,t}$). Additionally, levels of outcomes are predicted from levels of predictors, with first differences used as instruments (e.g., $\Delta \text{inequality}_{i,t}$ and $\Delta \text{inequality}_{i,t-1}$ are instruments for $\text{inequality}_{i,t}$).

This process helps to account for bidirectionality under several assumptions. First, it must be assumed that the instruments are not affected by the outcomes. This means that lagged levels of a regressor must not be affected by current first differences of an outcome, and differences of a regressor must not be affected by current levels of an outcome. Additionally, it must be assumed that the only way that instruments affect outcomes is via their influence on the endogenous regressor. These assumptions are strong but are also testable using observed residuals and the Hansen test of overidentifying restrictions (Hansen,

1982), in which rejection of the null hypothesis suggests endogeneity in instruments.¹

We fitted system GMM models to data aggregated into weighted means at the state-year level. Unlike we did in our HLM approach, we did not decompose variation in predictors into its within- and between-state components because system GMM requires a full panel data set and automatically eliminates between-state effects. Models included all covariates discussed above, including fixed effects for year of measurement. All racial-bias outcomes were again modeled separately. We used available lags of differences and levels, respectively, as instruments but economized on the number of instruments by collapsing them using the standard Anderson-Hsiao instrumental-variables estimation, which is helpful with small sample sizes, as in our case of U.S. states. Models were fitted via Stata's *xtabond2* command (Roodman, 2009).

Testing individual income effects

To further guard against confounding by nonlinear effects of individual income, we tested for the existence of such effects using a subset of 55,571 respondents from the Project Implicit data for which income was measured during 2015. Using this subsample of individual-level data, we ran a further series of HLMs, fitting separate models for each of Project Implicit's racial-bias measures. All covariates from focal models were included except year fixed effects, which were controlled by default because all data were from 2015. Predictors measured at the individual level (education, liberalism, income) were decomposed into their individual-level variation (individual deviations from state means) and state-level variation (variation between state-level means). Random intercepts were included for states.

For each outcome, models were fitted in three stages: (a) We fitted our preferred model excluding individual-level income as a covariate, (b) we included individual-level income, and (c) we included individual-level income squared. This allowed us to observe (a) whether including nonlinear income effects led to a significantly better model fit in Stage 3 and (b) the magnitude of change in estimated effects of income inequality resulting from including nonlinear income effects. We also repeated this process using both ACS Gini measures.

Specification-curve analyses

Our preferred model for estimating the effect of income inequality on racial bias included six key covariates: state-year mean income logged, state-year poverty rate, state-year proportion Black, political orientation, educational

achievement, and year fixed effects. However, other researchers may disagree with this model specification. For example, political orientation was included because it may act as a common cause of racial bias and income inequality. But other researchers could reasonably argue that political orientation is a common effect of racial bias and income inequality (a collider) or a result of racial bias and cause of income inequality (a mediator) and so should not have been included as a covariate. All participants of such debates will typically have difficulty conclusively arguing that their conception of the causal processes in question is correct. Specification-curve analysis (Simonsohn et al., 2015) is one response to this uncertainty: Estimate all reasonable models and assess the sum of evidence obtained from them as a whole. Doing so provides evidence of the robustness of effects to alternative model specifications and researcher decisions and allows a transparent account of the impact of each covariate over results.

To perform specification-curve analysis, we first defined a set of reasonable models. Here, we took an agnostic approach and treated all of our state-year-level covariates (education, liberalism, mean income logged, poverty rate, and proportion Black) as up for reasonable debate, so we fitted models with all 32 possible combinations of these covariates. All specifications included year fixed effects because the removal of overall trends to identify unit-level relationships has been widely recommended and we considered it uncontroversial (Curran & Bauer, 2011). We used both our preferred IRS Ginis and the common alternative ACS Ginis as alternative measures of income inequality. This created 64 total model specifications. All models were HLMs fitted to aggregate data as described above and included random intercepts for state.

With this set of specifications defined, we ran each of the 64 models using the observed data and saved estimated effects of income inequality from each model. For each of the 64 specifications, we again decomposed variation in predictors into within- and between-state variation and modeled our four racial-bias outcomes separately. Given both within-state and between-state effects of income inequality for each of our four outcomes, this resulted in eight sets of 64 estimated effects. These sets, arranged from their lowest to highest estimates, represent the curves that give specification-curve analysis its name. Each curve is displayed in the Supplemental Material.

We then performed statistical inference using each of the eight specification curves. Following Simonsohn et al. (2015), we used median estimates within curves as our test statistic and bootstrapping to estimate its sampling distributions. This entailed randomly resampling our data set with replacement 1,000 times, recalculating

each specification curve on the bootstrapped samples, computing median estimates within each bootstrapped curve, and using the resulting 1,000 median estimates for each curve to compute 95% CIs.

Nonindependent observations

Close inspection of the Project Implicit data suggested that it likely contained multiple measurements obtained from the same individuals. For example, it was normal to see consecutive rows in the data set containing the same demographic information: gender, age, race/ethnicity, religion, location, occupation, and so on. Our strategy for handling this was to (a) create different algorithms for treating observations as nonindependent (e.g., considering all observations with matching demographic information recorded on the same day to be for the same person), (b) rerun focal analyses under the assumed nonindependence implied by each algorithm, and (c) examine whether doing so changed key results. We did not find this process to produce any marked changes to our results, so we relegated further details to the Supplemental Material.

Results

Focal model results

We collectively performed eight tests of the inequality-racism hypothesis using focal HLMs, with both within-state (i.e., state-year-level) and between-state (i.e., state-level) effects tested for each of the four outcomes. Results are presented in Tables 2 and 3, and the relationships between within- and between-state variation in income inequality and each outcome, controlling for model covariates, are also visualized in Figure 2.

There was no significant within-state relationship between income inequality and IAT scores, $\hat{\beta} = 0.004$, $SE = 0.004$, 95% CI = [-0.005, 0.012], $t(337.462) = 0.841$, $p = .401$. There was a significant between-state relationship, $\hat{\beta} = 0.019$, $SE = 0.008$, 95% CI = [0.003, 0.036], $t(41.788) = 2.345$, $p = .024$; however, adjusting alpha levels for the family of eight tests with the Benjamini-Hochberg procedure to control false-discovery rate at an overall alpha of .05 rendered this latter result nonsignificant.

There was a significant within-state relationship between income inequality and preference for Whites, $\hat{\beta} = 0.02$, $SE = 0.006$, 95% CI = [0.009, 0.031], $t(458.247) = 3.453$, $p = .001$, but no significant between-state relationship, $\hat{\beta} = -0.001$, $SE = 0.007$, 95% CI = [-0.014, 0.011], $t(39.082) = -0.21$, $p = .835$. Similarly for thermometer difference, there was a significant within-state relationship, $\hat{\beta} = 0.017$, $SE = 0.005$, 95% CI = [0.008,

$0.026]$, $t(407.597) = 3.721$, $p < .001$, but no significant between-state relationship, $\hat{\beta} = 0.009$, $SE = 0.012$, 95% CI = [-0.014, 0.032], $t(42.859) = 0.787$, $p = .435$. Both of these within-state effects remained significant after Benjamini-Hochberg adjustment. For Google racial-slur searches, neither the within-state relationship, $\hat{\beta} = 0.069$, $SE = 0.037$, 95% CI = [-0.003, 0.142], $t(542.137) = 1.87$, $p = .062$, nor the between-state relationship, $\hat{\beta} = 0.056$, $SE = 0.152$, 95% CI = [-0.241, 0.353], $t(42.478) = 0.37$, $p = .713$, was significant.

Focal models therefore detected significant within-state relationships between income inequality and Project Implicit's two measures of explicit bias: preference for Whites and thermometer difference, with slopes on income inequality indicating that increases of 1 standard deviation in income inequality were associated with increases in individuals' explicit racial bias of around 2% of a standard deviation. These effects were relatively small compared with overall nationwide trends. By contrast, year fixed effects suggested that average responses on both measures of explicit bias shifted downward by around 40% and 30% of a standard deviation, respectively, across the 12-year study period.

To more clearly quantify and contextualize the magnitude of these effects, we computed individual-level, state-year-level, and state-level r^2 for each model and computed changes in each statistic with and without each predictor to quantify predictors' unique contributions (Δr^2 in Table 2). Including income inequality in models increased state-year-level r^2 of models predicting preference for Whites by 0.4% and increased state-year-level r^2 of models predicting thermometer difference by 0.7%. These effects were substantially smaller than the variance uniquely accounted for by year fixed effects, which captured both overall trends and year-to-year nationwide variation and uniquely increased within-state r^2 by 33% for preference for Whites and by 21% for thermometer difference. However, the unique contribution of income inequality at the state-year level for these outcomes was greater than virtually all other predictors. After year fixed effects, only the effect of political orientation (liberalism in Table 1) on the thermometer-difference outcome measure uniquely explained more state-year-level variation (2%) in explicit bias than income inequality.

Moreover, in the case of preference for Whites, the size of the effect of income inequality on explicit bias was surprisingly comparable with individual-level predictors. A within-state change in income inequality of 1 standard deviation, for example, was associated with an increase in preference for Whites of 2% of a standard deviation. The effect is similar to the estimated individual-level effect of having only a high school education compared with a college education, which was associated

Table 2. Fixed Effects From Hierarchical Linear Models Fitted on Individual-Level Data

Variable	IAT score			Preference for Whites			Thermometer difference			Google slur search						
	$\hat{\beta}$ (SE)	df	p	Δr^2	$\hat{\beta}$ (SE)	df	p	Δr^2	$\hat{\beta}$ (SE)	df	p	Δr^2				
Intercept	-0.152 (0.078)	45,536	.059		0.255 (0.064)	47,41	< .001		-0.1 (0.111)	44,917	.372		-2.28 (1.44)	44,09	.12	
Liberalism	-0.097 (0.001)	1,456,015.5	< .001	.009	-0.079 (0.001)	1,439,057.26	< .001	.006	-0.201 (0.001)	1,503,120.38	< .001	.038				
Advanced degree	-0.053 (0.002)	1,456,015.5	< .001	6×10^{-4}	0.028 (0.002)	1,439,057.26	< .001	2×10^{-4}	-0.043 (0.002)	1,503,120.38	< .001	4×10^{-4}				
High school	-0.005 (0.003)	1,456,015.5	.074	2×10^{-6}	0.024 (0.003)	1,439,057.26	< .001	5×10^{-5}	0.102 (0.003)	1,503,120.38	< .001	9×10^{-4}				
					Individual-level fixed effects											
Income inequality (IRS Gini)	0.004 (0.004)	337,462	.401	.002	0.02 (0.006)	458,247	.001	.004	0.017 (0.005)	407,597	< .001	.007	0.07 (0.04)	542.14	.062	.002
Liberalism	-0.162 (0.031)	459,703	< .001	.038	-0.076 (0.04)	580,906	.059	.001	-0.214 (0.034)	533,818	< .001	.02	0.21 (0.31)	542.14	.487	2×10^{-4}
Advanced degree	-0.035 (0.056)	445,147	.531	0	-0.012 (0.073)	566,884	.867	3×10^{-5}	-0.056 (0.061)	524,361	.356	6×10^{-4}	-0.33 (0.54)	542.14	.541	2×10^{-4}
High school	-0.635 (0.122)	430,522	< .001	.022	-0.159 (0.153)	554,089	.3	1×10^{-4}	-0.282 (0.129)	509,255	.029	.002	-0.14 (1.15)	542.14	.9	7×10^{-6}
Mean state income (logged)	0.01 (0.01)	458,867	.328	8×10^{-4}	0.01 (0.013)	591,586	.442	1×10^{-4}	0.016 (0.011)	545,548	.153	.002	0.28 (0.1)	542.2	.004	.004
State poverty rate	0.003 (0.011)	362,596	.744	0	-0.016 (0.014)	503,924	.254	2×10^{-4}	0.002 (0.011)	444,651	.853	8×10^{-5}	0.08 (0.09)	542.2	.385	4×10^{-4}
State proportion Black	0.001 (0.043)	740,006	.984	4×10^{-5}	-0.09 (0.052)	1,040,494	.083	9×10^{-4}	-0.019 (0.045)	931,665	.674	0	-0.06 (0.42)	542.14	.889	1×10^{-5}
2005	0.052 (0.011)	347,653	< .001		0.004 (0.015)	480,018	.782		0.004 (0.012)	423,969	.744		0.16 (0.09)	542.15	.077	
2006	0.033 (0.014)	386,252	.016		-0.154 (0.018)	512,998	< .001		-0.017 (0.015)	462,974	.243		-0.41 (0.12)	542.17	.001	
2007	0.085 (0.017)	402,907	< .001		-0.344 (0.023)	529,251	< .001		-0.03 (0.019)	480,258	.104		-0.66 (0.16)	542.16	< .001	
2008	0.035 (0.014)	379,292	.012		-0.355 (0.018)	507,737	< .001		-0.057 (0.015)	455,363	< .001		-0.21 (0.12)	542.17	.079	
2009	0.016 (0.014)	377,091	.241		-0.332 (0.018)	505,787	< .001		-0.051 (0.015)	453,517	.001		-1.3 (0.12)	542.15	< .001	
2010	0.03 (0.016)	377,153	.066		-0.372 (0.021)	508,124	< .001		-0.085 (0.017)	453,042	< .001		-1.48 (0.14)	542.17	< .001	

(continued)

Table 2. (continued)

Variable	IAT score			Preference for Whites			Thermometer difference			Google slur search						
	$\hat{\beta}$ (SE)	df	p	Δr^2	$\hat{\beta}$ (SE)	df	p	Δr^2	$\hat{\beta}$ (SE)	df	p	Δr^2				
2011	0.037 (0.018)	386.262	.039		-0.352 (0.023)	516.535	< .001		-0.07 (0.019)	462.584	< .001		-1.27 (0.16)	542.19	< .001	
2012	0.043 (0.02)	405.652	.033		-0.345 (0.027)	535.604	< .001		-0.068 (0.022)	484.432	.002		-1.46 (0.18)	542.21	< .001	
2013	0.016 (0.02)	406.857	.408		-0.363 (0.026)	539.785	< .001		-0.105 (0.021)	488.323	< .001		-1.41 (0.18)	542.21	< .001	
2014	-0.051 (0.022)	436.432	.021		-0.376 (0.029)	564.869	< .001		-0.218 (0.024)	518.412	< .001		-1.71 (0.21)	542.22	< .001	
2015	-0.071 (0.023)	444.33	.002		-0.391 (0.029)	571.621	< .001		-0.285 (0.024)	526.352	< .001		-1.66 (0.21)	542.22	< .001	
Year fixed effects				.218				.328				.214			.258	
					Between-state fixed effects											
Inequality (IRS Gini)	0.019 (0.008)	41.788	.024	.041	-0.001 (0.007)	39.082	.835	.004	0.009 (0.012)	42.859	.435	.003	0.06 (0.15)	42.48	.713	.001
Liberalism	-0.17 (0.044)	42.289	< .001	.106	-0.068 (0.035)	39.681	.06	.094	-0.283 (0.064)	42.914	< .001	.126	-1.28 (0.82)	42.84	.127	.024
Advanced degree	0.215 (0.162)	41.358	.191	.012	-0.123 (0.129)	38.467	.346	.044	0.178 (0.232)	42.486	.447	.003	5.16 (3)	42.01	.092	.033
High school	0.461 (0.381)	43.635	.233	.008	0.673 (0.307)	41.93	.034	.125	1.083 (0.54)	43.857	.051	.024	13.7 (7.04)	44.52	.058	.044
Mean state income (logged)	-0.034 (0.01)	46.246	.002	.071	0.013 (0.008)	45.024	.108	.131	-0.01 (0.014)	45.199	.499	.002	-0.47 (0.19)	46.84	.015	.067
State poverty rate	-0.036 (0.011)	44.359	.001	.078	0.014 (0.009)	42.513	.114	.128	-0.006 (0.015)	44.175	.688	8×10^{-4}	-0.04 (0.2)	45	.854	-5×10^{-5}
State proportion Black	0.027 (0.006)	42.943	< .001	.175	0.025 (0.005)	40.767	< .001	0	0.028 (0.008)	43.254	.001	.082	0.25 (0.1)	43.59	.021	.066

Note: Values in the Δr^2 column refer to changes at each predictor's variance level: individual, state-year, or state. Values of 0 indicate cases in which the estimated variance of random effects increased after predictors were included. IAT = implicit association test; IRS = Internal Revenue Service.

Table 3. Random Effects From Hierarchical Linear Models Fitted on Individual-Level Data

Effect	IAT score			Preference for Whites			Thermometer difference			Google slur search		
	\widehat{SD}	N	Model r^2	\widehat{SD}	N	Model r^2	\widehat{SD}	N	Model r^2	\widehat{SD}	N	Model r^2
State	0.03	51	.749	0.018	51	.875	0.045	51	.752	0.591	51	.556
State-year	0.039	612	.706	0.057	612	.429	0.044	612	.852	18.667	612	.728
Residual	0.992	1,456,884	.01	0.985	1,439,742	.006	0.97	1,503,863	.043			

Note: IAT = implicit association test.

with an increase in preference for Whites of 2.4% of a standard deviation. Moreover, the income-inequality effect was around a quarter of the magnitude of the estimated individual-level effect of a standard-deviation increase in liberalism, which was associated with a decrease in preference for Whites of around 8% of a standard deviation. For thermometer difference, individual-level variables had stronger effects. A change in income inequality of 1 standard deviation was associated with a change in bias of 0.17% of a standard deviation. This was around one sixth as large as the estimated effect of having a high school education compared with a college education, which was associated with an increase in thermometer difference of around 10% of a standard deviation, and one twelfth as large as the estimated effect of an increase in individual-level liberalism of 1 standard deviation, which was associated with a decrease in thermometer difference of around 20% of a standard deviation.

Power analyses

To obtain bootstrapped estimates of statistical power at the observed effect sizes, we randomly resampled with replacement 1,000 times from the 612 state-years, rerunning focal models each time and recording whether estimated effects of income inequality were significant at a two-tailed α of .05. Using this bootstrapping procedure, we estimated that power to detect within-state income-inequality effects of the observed size for each outcome was .18 for IAT scores, .96 for preference for Whites, .98 for thermometer difference, and .48 for Google racial-slur searches. Estimated power to detect between-state income-inequality effects of the observed sizes was .61 for IAT scores but 0 for preference for Whites, thermometer difference, and Google racial-slur searches (here, 0 indicates that no bootstrapped data sets returned significant effects). Bootstrapped estimates of power curves across different effect sizes are reported in the Supplemental Material.

Another relevant question is the extent to which the observed effects of income inequality on explicit bias constitute a falsifiable alternative hypothesis. To test this question, we adjusted each state-year's scores on the preference and thermometer difference to make the effect of income inequality on each measure exactly 0

(for details, see the Supplemental Material). We then bootstrapped 1,000 samples of 612 state-years from these null data, seeking to falsify the directional hypothesis that $\beta = 0.02$ under a true null. One-sided CIs excluded 0.02 (thus falsifying the hypothesis that $\beta = 0.02$) on 98.2% of iterations for preference for Whites and on 99.6% of trials for thermometer difference.

System GMM model

We fitted four separate system GMM models for each racial-bias outcome as described above. Arellano-Bond autocorrelation tests (Arellano & Bond, 1991) and Hansen tests of overidentifying restrictions (Hansen, 1982) suggested that models were well specified (all $ps > .05$). The estimated effect of income inequality was statistically significant for the three Project Implicit outcomes but not for Google racial-slur searches—IAT scores: $\hat{\beta} = 0.013$, $SE = 0.003$, 95% CI = [0.007, 0.019], $t(50) = 4.13$, $p < .001$; preference for Whites: $\hat{\beta} = 0.022$, $SE = 0.005$, 95% CI = [0.012, 0.032], $t(50) = 4.55$, $p < .001$; thermometer difference: $\hat{\beta} = 0.014$, $SE = 0.006$, 95% CI = [0.002, 0.025], $t(50) = 2.44$, $p = .018$; and Google racial-slur searches: $\hat{\beta} = 0.032$, $SE = 0.053$, 95% CI = [-0.075, 0.138], $t(50) = 0.6$, $p = .553$. No conclusions regarding statistical significance were altered by Benjamini-Hochberg adjustments for four tests. Full model and diagnostic test results are available in the Supplemental Material.

Test of nonlinear income effects

Six model comparisons tested for nonlinear effects of income. Including income squared as a predictor led to a significantly better fit for two of six models: those predicting preference for Whites using the IRS Gini measure, $\Delta\chi^2(2) = 7.025$, $p = .03$, and those predicting preference for Whites with the ACS Gini measure, $\Delta\chi^2(2) = 6.823$, $p = .033$. However, Benjamini-Hochberg adjustments for six tests rendered all results nonsignificant, suggesting little evidence of a nonlinear relationship between individual-level income and racial bias. Full model results are reported in the Supplemental Material. The bottom three panels in Figure 1 display the individual-level nonlinear relationship between income and each racial-bias measure, controlling for model covariates.

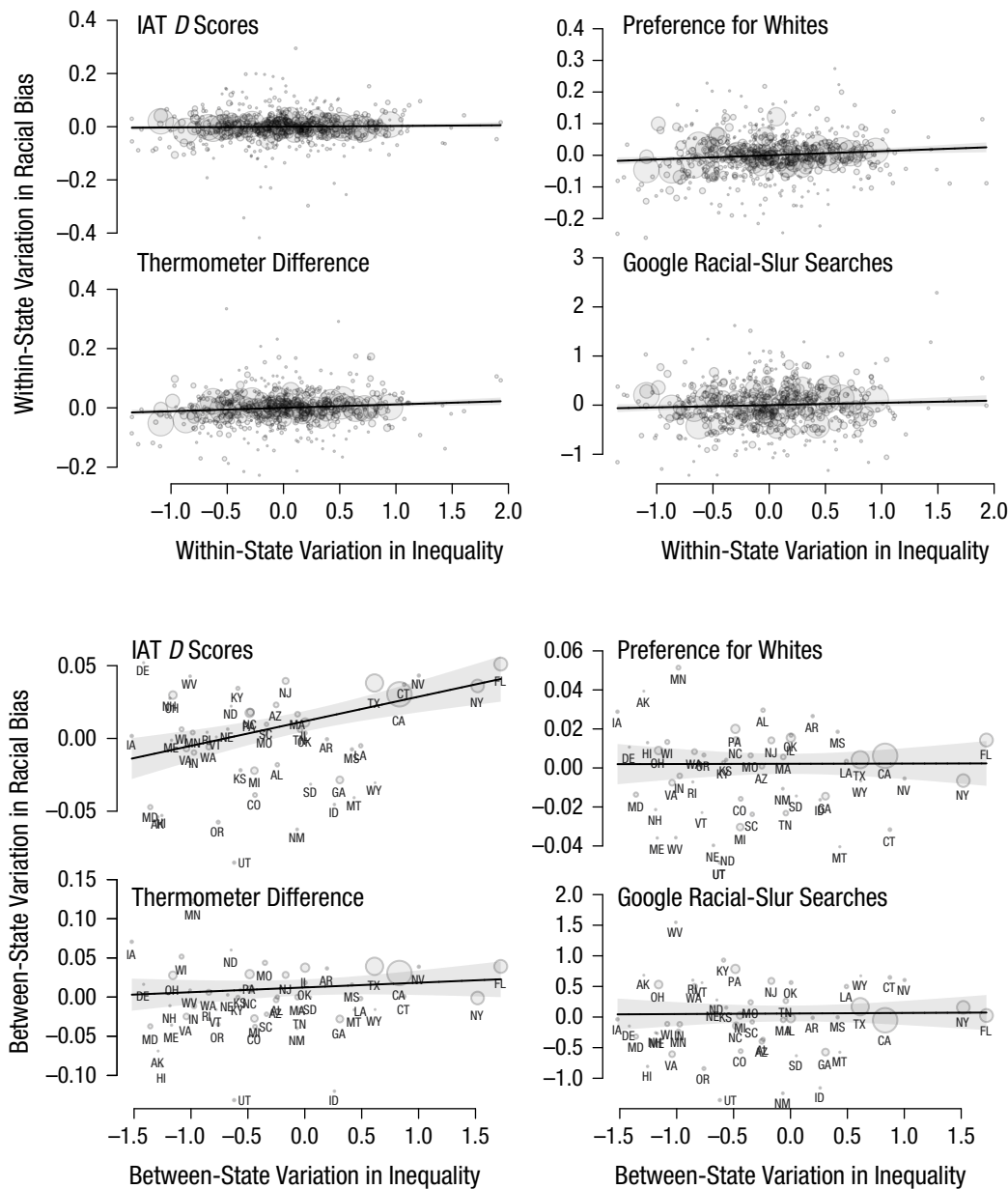


Fig. 2. Scatterplots (with ordinary-least-squares lines of best fit) showing the relationship between variation in income inequality and variation in each of four measures of racial bias, controlling for model covariates. Within-state variation is shown in the top panels, and between-state variation is shown in the bottom panels. Points in the top four panels represent state-year deviations from state means; points in the bottom four panels represent state means. Shaded regions are 95% confidence intervals. Points are sized according to states' populations. Implicit-association-test (IAT) scores, preference for Whites, and thermometer difference were z-scored at the individual level; Google racial-slur searches were z-scored at the state-year level.

Specification-curve results

Specification-curve results were noteworthy for a number of reasons. First, overall inferences based on specification curves diverged from conclusions based on focal models. For within-state relationships, bootstrapped 95% CIs for each curve's median estimates excluded 0 only for the within-state effect of income

inequality on preference for Whites, 95% CI = [0.005, 0.067]. By contrast, for between-state relationships, CIs excluded 0 for three of four measures—IAT scores: 95% CI = [0.005, 0.026], thermometer difference: 95% CI = [0.008, 0.026], and Google racial-slur searches: 95% CI = [0.079, 0.18]. Median estimates of within- and between-state effects of income inequality from each specification curve as well as bootstrapped 95% CIs are

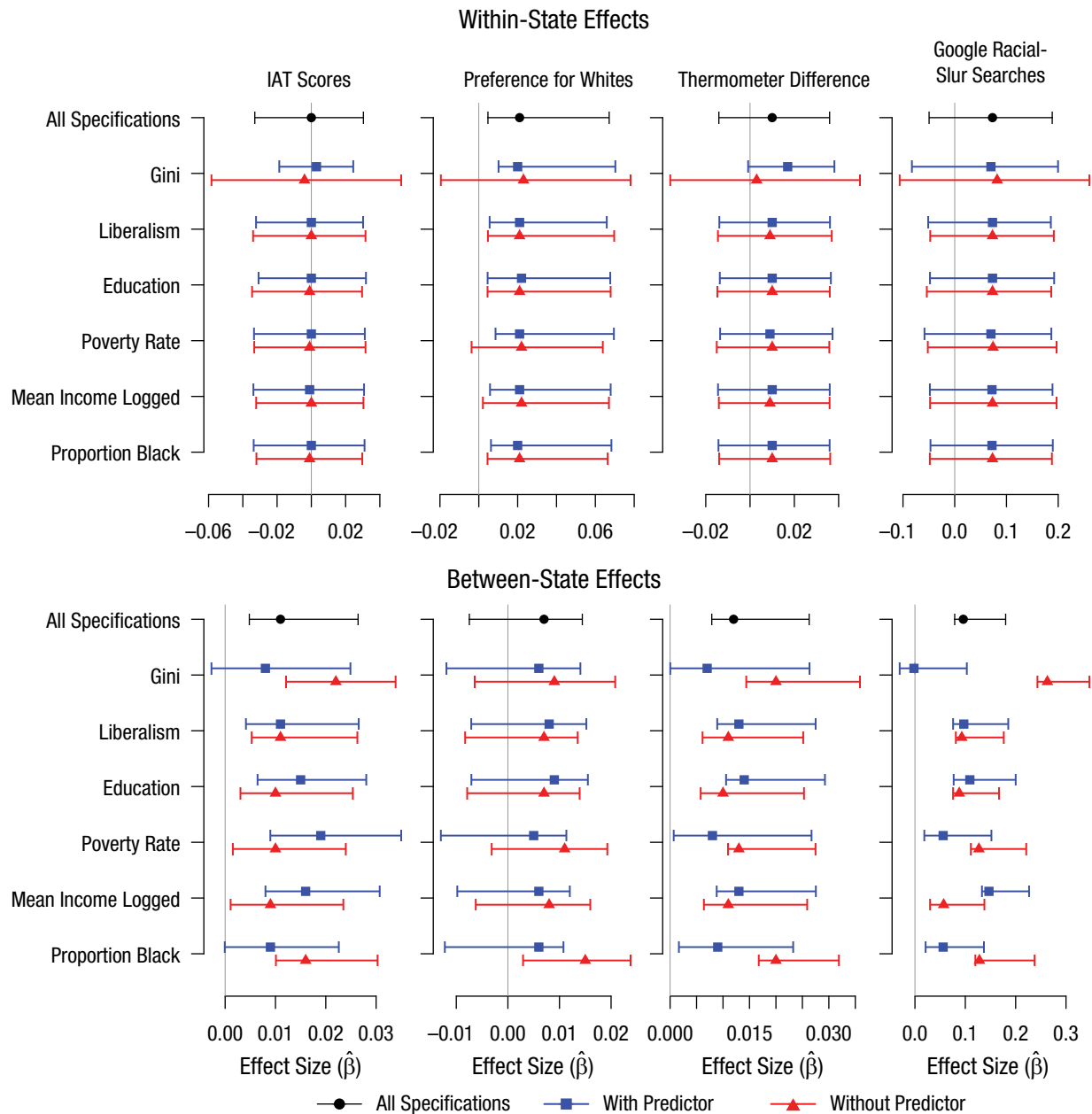


Fig. 3. Median estimated within- and between-state effects of income inequality from specification curves (and 95% confidence intervals) computed from 1,000 bootstrapped data sets. For Gini results, estimates in blue used the Internal Revenue Service (IRS) Gini measure, and estimates in red used the American Community Survey Gini measure. For all other measures, blue and red indicate estimates based on specifications with and without each predictor, respectively. IAT = implicit association test.

displayed in black in Figure 3 (each of the eight specification curves is shown individually in the Supplemental Material).

To better understand why specification curves appeared to suggest evidence of between-state relationships whereas focal models had provided evidence only for within-state relationships, we broke down results by predictors, subsetting all model specifications into those with and without each predictor and recomputing median

estimates and CIs for each subset. Figure 3 presents results from specifications with (in blue) and without (in red) each predictor.

Subsetting results suggested that the primary reason for the discrepancy between our focal models and overall specification-curve results was the difference between the IRS and ACS Gini measures. Broadly, the IRS measure exhibited stronger within-state associations with the racial-bias measures, whereas the ACS measure

exhibited stronger between-state associations (see the red and blue lines for “IRS Gini” in the top and bottom panels of Figure 3; red indicates specifications with the IRS Gini, and blue indicates specifications with the ACS Gini). For thermometer difference, this difference was especially apparent, with model specifications using the IRS Gini measure more suggestive of a within-state relationship, 95% CI = $[-0.001, 0.038]$, but models using the ACS measure supportive of a between-state relationship, 95% CI = $[0.014, 0.036]$.

Subsetted results also revealed that the inclusion and exclusion of model covariates appeared to have much greater influence on between-state estimates of the effect of income inequality than within-state estimates (this is displayed by the greater similarity between blue and red points in the top four panels compared with the bottom four panels of Fig. 3). For example, between-state effects in specifications controlling for proportion Black were substantially lower than in specifications without proportion Black (see the blue and red points adjacent to “Proportion Black” in the bottom four panels), suggesting that it may indeed act as a confound of the between-state association between income inequality and racial bias. Controlling for poverty rates also appeared to systematically lower estimated between-state estimates for outcomes other than IAT scores. By contrast, no covariates appeared to have any systematic impact on estimates of the within-state effect of income inequality.

Discussion

A common claim in theories addressing social hierarchy is that economic inequality increases racism. Using data resources made available by Project Implicit and Google Trends, we sought to provide the first robust empirical test of this hypothesis. Over a 12-year period, we examined the effect of income inequality on racial bias at the U.S. state-year level using what we believe to be the largest and most comprehensive data set available.

Overall, our results were mixed. Consistent with the inequality–racism hypothesis, our results showed evidence in line with a small positive effect of U.S. state-level income inequality on Whites’ explicit racial bias, as assessed by Project Implicit’s measures of preference for Whites and thermometer difference. For these two measures, we found similar-sized within-state effects of income inequality in both focal HLMs and system GMM models. Specification curves showed that these effects were partially dependent on our model specification and were primarily observed using IRS Gini data. However, as discussed above, there are good reasons to believe that the IRS Gini data provide a better measure of income inequality than the ACS Gini data. Our tests found little evidence of nonlinear effects of income on

either measure, and specification-curve analysis showed that the inclusion or exclusion of covariates had little effect on within-state estimates.

By contrast, we found only equivocal evidence of a relationship between income inequality and implicit racial bias assessed by Project Implicit’s IAT measure. Our focal HLM detected a significant between-state relationship between income inequality and IAT scores, and our system GMM model also detected a positive effect. However, the former was not robust to adjustment for multiple tests. Specification-curve analysis also provided support for the between-state relationship, although this result was driven largely by the less-preferred ACS Gini measure (see Fig. 3). Moreover, as Figure 3 shows, controlling for proportion Blacks in state populations substantially reduced the between-state relationship. This also raises concerns because it is well understood that statistically controlling for confounders that are measured with error (as all of our model variables likely are) does not fully remove confounding biases (Westfall & Yarkoni, 2016). Results of models predicting Google racial-slur searches provided no clear evidence of a relationship with income inequality.

Together, these results raise a number of questions. First, why was the inequality–racism effect observed for explicit bias measures but not for implicit bias or the behavioral measure of Google searches? One possibility is that in regions with lower income inequality, people may feel stronger social pressure to appear more egalitarian and that this social-desirability bias is what led participants in more equal regions to report lower explicit racial bias but to not appear any less biased on implicit or behavioral measures.

Our data cannot completely rule out such an explanation. Yet if we assume that individuals more likely to self-present as racially unbiased are also likely to self-report as more liberal, we think that the lack of any systematic impact of controlling for individuals’ political orientation on within-state estimates (see Fig. 3) calls such an interpretation into question.

It is also important to note that although the question of what is respectively measured or missed by explicit and implicit measures is complex and unresolved, to date there is little evidence that implicit bias measures predict meaningful discriminatory behaviors any better than explicit measures (Oswald, Mitchell, Blanton, Jaccard, & Tetlock, 2013). Using Project Implicit data, Leitner, Hehman, Ayduk, and Mendoza-Denton (2016) found that the explicit measures were a better predictor of Blacks’ circulatory disease death rates at the county level than were implicit measures. This suggests that the Project Implicit explicit bias measures may capture socially significant aspects of racial bias in ways that implicit measures miss. So, whereas our results can be seen as consistent with a social-desirability effect, we

believe that they must also be seen as consistent with an effect of income inequality on meaningful racial bias.

Another question is whether the size of the observed effects renders them worthy of or amenable to scientific study. In terms of experimental social psychology, it is difficult to answer this question. Theoretically, small effects in observational data can be magnified in the lab, where error variance due to aggregate trends, ecological variables, and individual-level differences can be eliminated. A lab experiment would therefore not in theory need to detect an effect as small in terms of explained variance as the present study. A more pressing problem may be that to provide compelling causal evidence, a lab experiment would require an effective and ecologically valid method of manipulating income inequality capable of mimicking the experience of living in a more or less equal environment. It is unclear what such a method might entail.

Yet, as we have shown, an effect of the size we have observed can be subject to well-powered tests—and effectively falsified—provided access to observational samples of sufficient size. Moreover, statistically small effects can be meaningful when their effects are cumulative. Abelson (1985) showed that professional baseball players' batting averages explain only 0.3% of variance in whether they obtain a hit in any given at bat. Yet this does not mean that players' batting averages are a meaningless variable for predicting success at bat. Rather, it means that their effect becomes meaningful only when we consider how many at bats each player has over an entire season or career. Likewise, a small effect of income inequality on Whites' explicit racial bias may be cumulatively meaningful, both by affecting entire state populations of Whites and by the many repeated instances in which African Americans' lives can be impacted by Whites' racial bias.

Several other limitations should also be noted. First, we relied primarily on Project Implicit's large but non-representative data set of volunteers, which cannot be considered a random sample. It is therefore unclear how well findings from this sample can be generalized to the U.S. population at large or to other countries. Even after poststratification weighting, it remains possible that the individuals in Project Implicit's data set differ in important ways from the rest of the U.S. population with regard to their racial bias and, perhaps, their response to income inequality.

Additionally, although our results are partially consistent with various theories, they unfortunately do not allow us to adjudicate between them and are consistent with a range of explanations. Data availability also led us to restrict our analyses specifically to income inequality rather than wealth inequality. This too was regrettable because for each of the theories we have studied, wealth inequality can be considered at least as

important as income inequality. We hope that further work can undertake the task of disentangling these mechanisms and constructs.

In sum, our findings suggested a small effect of income inequality on explicit racial bias but did not clearly support an effect of income inequality on implicit bias or Internet searches. This is consistent with multiple possible explanations, and the size of the observed effects suggests that changes in income inequality are unlikely to substantially affect overall downward trends on indices of explicit bias. However, in light of the social importance of the outcome in question and the ability of statistically small effects to be cumulatively meaningful in large enough numbers, we believe that increased levels of explicit racism should be considered a legitimate potential negative consequence of increased income inequality.

Action Editor

Brent W. Roberts served as action editor for this article.

Author Contributions

P. Connor developed the study concept. P. Connor, V. Sarafidis, and M. J. Zyphur analyzed and interpreted the data. P. Connor drafted the manuscript, and all the authors provided critical revisions. All the authors approved the final manuscript for submission.

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The author(s) declared that there were no conflicts of interest with respect to the authorship or the publication of this article.

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Open Practices



All data, materials, and code have been made publicly available via the Open Science Framework and can be accessed at osf.io/myqn9. The design and analysis plans for this study were not preregistered. The complete Open Practices Disclosure for this article can be found at <http://journals.sagepub.com/doi/suppl/10.1177/0956797618815441>. This article has received the badges for Open Data and Open Materials. More information about the Open Practices badges can be found at <http://www.psychologicalscience.org/publications/badges>.

Note

1. System GMM models also allow users to test for autocorrelation aside from fixed effects (another phenomenon capable of rendering some lags invalid as instruments) via Arellano-Bond tests (Arellano & Bond, 1991).

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