When Contact Causes a Public-Private Divide: A Framework and Evidence from a Parasocial Contact Experiment

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Abstract

Contact research examines a wide array of attitudinal and behavioral outcomes, yet rarely treats as consequential whether those outcomes are public or private—visible or not to ingroup members. This paper argues that contact can affect private and public outcomes differently, with implications both for detecting effects and understanding how contact produces social change. I develop a framework showing how the social environment, intervention pathway, and initial conditions shape whether contact changes private outcomes, public outcomes, both, or neither. I then test a central implication—that contact can have smaller effects on public than private outcomes for those facing social costs. I use an online experiment, assigning 1,118 White Americans to parasocial contact with a Black or White couple from a reality television program and cross-randomize them to report attitudes anonymously or to a virtual White enumerator, capturing private versus public expression. Among respondents facing higher social costs, contact improved private trait evaluations but produced no detectable change in evaluations expressed to the enumerator. Behavioral intentions showed a similar, though less precise, pattern. Contact can thus change private outcomes without a corresponding change in public ones, both limiting the visibility of its effects and revealing a potential barrier to social change.

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1 Introduction

Intergroup contact is one of the most widely studied approaches to reducing prejudice and improving relations between social groups (Pettigrew and Tropp, 2011; Paluck, Green and Green, 2019; Lowe, 2025; Banas, Bessarabova and Massey, 2020). Since the seminal work of Allport (1954), scholars have examined its effects on a wide range of attitudinal and behavioral outcomes, including stereotypes, affective evaluations, policy preferences, behavior toward outgroup members, intergroup cooperation, and collective action.

Critically, the outcomes researchers study differ along an important but often overlooked dimension: whether they are *public* or *private*—that is, whether they are visible to a socially relevant audience. Attitudes reported on surveys can fall in either domain: responses vary with survey privacy, interviewer characteristics, and perceived audiences (Krysan, 1998; Krysan and Couper, 2003; Tourangeau and Yan, 2007; Huddy and Feldman, 2009; Adida et al., 2016; Blair, Coppock and Moor, 2020; Bursztyn et al., 2025).[1] Behaviors likewise vary in visibility. Contact studies have measured private behavior through allocation games (Lowe, 2025), individual votes concerning outgroup members (Lowe, 2021), donations divided between ingroup and outgroup organization (Silva-Vargas and Loiacono, 2026; Mousa, 2020), and individual willingness to interact with outgroup members (Burlacu, Azzolini and Podestà, 2026; Anisfeld and Pandiloski, 2024). Other measures capture potentially visible behavior, including cooperation with outgroup members (Mousa, 2020), signaling within a social network (Larson and Lewis, 2025; Siddique, Vlassopoulos and Zenou, 2026), and collective action (Hässler et al., 2020). Whether an attitude or behavior is visible to a socially relevant audience is not always clear from how researchers describe the outcome measure, however.[2]

[1] Private and public attitudes here refer to whether explicit attitudes are expressed before a socially relevant audience, not to automatic versus controlled processing or implicit versus explicit attitudes (Devine, 1989; Greenwald and Banaji, 1995; Fazio and Olson, 2003). Explicit attitudes are common in prejudice reduction research (Paluck et al., 2021).

[2] Anonymity in allocation games often concerns the recipient rather than the respondent's audience: the game may conceal who receives a transfer while leaving the choice visible to an enumerator. Behavior outcomes like volunteering for an organization that protects outgroup members (Murrar and Brauer, 2018) or visiting an outgroup member's home (Mousa, 2020) may or may not be

This distinction between private and public outcomes matters because a contact intervention need not have similar effects on them. Private outcomes depend on individuals' personal beliefs about outgroup members, which contact may change through empathy, stereotype reduction, or other pathways. Public outcomes depend on those beliefs, but their visibility also makes them subject to the social incentives attached to expression and action. The main implication is that contact—depending on how it works and the social environment—might affect private outcomes, public outcomes, both, or neither. While it is widely appreciated that social factors can cause private and public outcomes to diverge (Kelman, 1958; Plant and Devine, 1998; Crandall, Eshleman and O'brien, 2002; White, Laird and Allen, 2014), the possibility that contact *causes* change in one domain more than the other has received less attention.

Addressing this matters for two reasons. First, conclusions about whether contact works may depend on which outcomes a study measures. For instance, an intervention that changes private but not public outcomes may appear ineffective if a study only measures public attitudes and behaviors, creating potential discrepancies within and across studies. Second, both private and public outcomes matter for social change. Private change can accumulate through individual choices, while public expression can shift perceived norms and contribute to social cascades (Granovetter, 1978; Bicchieri, 2016; Tankard and Paluck, 2016). Yet social change also depends on the relationship between private and public outcomes. When favorable private views remain unexpressed, people may misperceive social norms, limiting broader change (Noelle-Neumann, 1974; Prentice and Miller, 1993; Kuran, 1995; Bursztyn, González and Yanagizawa-Drott, 2020). Conversely, public norm compliance without corresponding private change may give the appearance of broad social support unmatched by meaningful private action (Kelman, 1958). When contact has asymmetric effects on private and public outcomes, it can create, widen, or narrow the gap between them, introducing or removing barriers to social change. A study measuring only one domain cannot establish how contact changes that relationship.

This paper first develops a framework for explaining when contact changes private outcomes,

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observed, depending on how the action occurs. Survey attitudes may likewise be described as private because community members do not observe them, even when an enumerator does (Siddique, Vlassopoulos and Zenou, 2026).

public outcomes, both, or neither. It considers the social environment, the pathway through which contact operates, and individuals' private and public positions before treatment. The paper then focuses on one possibility, that contact can have larger effects on private than on public outcomes, especially among individuals facing higher social costs for favorable public expression or action toward an outgroup. This focus matters because interventions are frequently deployed in settings where individuals are both personally prejudiced and embedded in social networks that support prejudice such that favorable public expression or action toward an outgroup is socially costly (Paluck, 2009; Scacco and Warren, 2018; Mousa, 2020; Siddique, Vlassopoulos and Zenou, 2026). Furthermore, this asymmetry is particularly important to investigate as private but not public change could create or deepen the kind of divergence associated with preference falsification and the barriers to social change described above.

I test for asymmetric effects in a large-scale, pre-registered online media experiment examining how parasocial contact affects White Americans' attitudes and behavior toward Black Americans.[3] A sample of 1,671 White respondents were randomly assigned to watch approximately 25 minutes of curated content from the reality television show *Married at First Sight*, featuring either a successful Black (outgroup) or successful White (ingroup) couple navigating married life. Parasocial contact—positive, one-sided exposure to outgroup members through media—is theorized as a media analogue of direct contact (Schiappa, Gregg and Hewes, 2005; Alrababa'h et al., 2021) and can improve attitudes and behavior toward outgroups (Paluck, 2009; Goldman, 2012; Alrababa'h et al., 2021; Siddique, Vlassopoulos and Zenou, 2026). The Black couples' stories offered emotionally engaging, counter-stereotypical portrayals that could change personal beliefs about Black Americans.

Testing for asymmetry requires measuring private and public effects using attitudinal or behavior outcomes that vary only in their visibility, holding other aspects of the outcome constant. The main test comes from a virtual enumerator experiment in which respondents were independently randomly assigned to a *private* condition, where they reported anonymously, or a *public* condition, where they gave responses to a virtual ingroup enumerator. The intervention, questions, and outcomes were identical across conditions; only the perceived audience varied by design. This design

[3]The anonymized preregistration is available here; Appendix L documents correspondence between the manuscript and the preregistration plan.

builds on research showing that reported racial attitudes vary with survey privacy and interviewer characteristics (Sigall and Page, 1971; Kuklinski, Cobb and Gilens, 1997; Plant and Devine, 1998; Krysan and Couper, 2003), and it captures a core feature of public expression: reporting one's attitudes to a socially relevant audience. While survey research generally treats the audience as a source of measurement distortion to be minimized (Bursztyn et al., 2025), I instead treat responsiveness to an audience as substantively meaningful, taking up Krysan and Couper (2003)'s characterization of social desirability as a useful "social microscope." I then examine whether similar patterns emerge for behavioral intentions and behavior visible to respondents' own social networks, which provide less controlled tests of the same logic.

The results support the possibility that contact can have asymmetric private and public effects. On average, parasocial contact increased warmth toward Black Americans, support for social change, and positive trait evaluations. For trait evaluations, however, these averages conceal important variation. Among respondents facing higher social costs, contact improved evaluations in private but not when expressed to an ingroup enumerator. While the asymmetry does not emerge for all attitudinal outcomes, that it emerges for trait evaluations is nonetheless consequential because stereotyping is a central outcome in the contact literature. The behavioral intentions results also follow the expected pattern—showing a bigger public–private asymmetry among those facing higher versus lower social costs—although they are more imprecisely estimated. Together, these findings show that contact may affect private outcomes without corresponding effects on public expression or action, with implications for understanding both whether it works at all and its potential—and limits—to bring about social change.

2 A Framework for Private and Public Contact Effects

Contact research typically asks whether an intervention changes attitudes or behavior toward an outgroup. This paper argues that the answer can depend on whether those outcomes are private or public—that is, whether they are visible to a socially relevant audience. Attitudes can be expressed privately in an anonymous survey or publicly to an enumerator, family member, or coworker. Behaviors likewise vary in visibility: a person might sign a petition anonymously or attach their name where others can see it. The framework, developed formally in Appendix A,

considers how contact affects the same attitude or action under conditions that differ in visibility.

The argument builds on an intuition familiar from research on self-censorship (Noelle-Neumann, 1974; Kuran, 1995) and the public expression of socially sensitive attitudes (Plant and Devine, 1998; Krysan and Couper, 2003): what people are willing to say or do can depend on who might observe them. Here, I consider fellow ingroup members as the relevant audience. Personal networks are highly homogeneous, particularly by race (McPherson, Smith-Lovin and Cook, 2001), so those most likely to observe an expression or action are often ingroup members. People also adjust what they say to match those around them (Asch, 1956), and conformity pressure can be stronger when it comes from members of one's own group (Abrams et al., 1990).

Consider whether an individual expresses a favorable attitude or takes a favorable action toward an outgroup, privately and publicly.[4] In private, this decision depends on whether its personal value outweighs its effort, material, psychological, and other nonsocial costs. In public, it also depends on anticipated social consequences. An individual may refrain from favorable public expression because fellow ingroup members would disapprove, or express a favorable position because doing so brings approval. These social incentives depend both on the direction of perceived ingroup norms and on how much weight the individual places on others' reactions. Favorable public expression can therefore be less likely than private expression under social costs, or more likely under social benefits.[5]

How contact changes private and public outcomes depends on how it operates.[6] Contact inter-

[4]For simplicity, I begin with a binary decision of whether to express a favorable attitude or take a favorable action. The logic extends to ordered and continuous outcomes by treating each successively higher level of favorable expression or action as a separate decision about whether to express or do at least that much. Changes in the amount expressed or done then reflect changes in these decisions in each domain. At the population level, effects aggregate changes across individuals. Appendix A develops the formal argument.

[5]The framework also allows for neutral environments in which social factors do not affect the decision.

[6]The discussion focuses on favorable change: contact may make favorable expression or action more worthwhile, or leave it unchanged. Negative contact and backlash are possible but lie outside the predictions developed here.

ventions take different forms—direct, indirect, or media-based—but are generally theorized to work through two broad pathways. First, contact can change *personal beliefs*, meaning how individuals think and feel about outgroup members (Allport, 1954; Pettigrew and Tropp, 2011). Such change may occur through reduced stereotyping, intergroup anxiety, or perceived threat, or through increased empathy, perceived similarity, and perspective-taking (Pettigrew and Tropp, 2008; Banas, Bessarabova and Massey, 2020; Scacco and Warren, 2018). These changes can increase the personal value of favorable expression or action, making it more likely both in private and public.

Second, contact can change *perceived social norms*, specifically beliefs about how fellow ingroup members regard favorable attitudes or behavior toward the outgroup (Paluck, 2009; Pettigrew and Tropp, 2011; Banas, Bessarabova and Massey, 2020; Littman, Scacco and Weiss, 2023). Observing ingroup peers interact with outgroup members, directly or through media, can convey both what other ingroup members do and what they approve of (Bandura, 1986; De Tezanos-Pinto, Bratt and Brown, 2010; Vezzali et al., 2014; Grady, Iannantuoni and Winters, 2021; González, 2025). Contact may therefore reduce the anticipated social costs or increase the social benefits of favorable public expression. This pathway can change public expression or action without changing what individuals would express or do privately.[7]

The argument thus far builds on established accounts of why private and public outcomes differ and how contact can change personal beliefs and perceived norms. The contribution here is to explain how these changes produce different public–private effect patterns and how those patterns reshape the relationship between private and public outcomes. Which pattern emerges depends on whether favorable public expression or action carries social costs or benefits, the pathways through which contact operates, and what individuals initially express or do privately and publicly. Contact can have similar effects on both outcomes, no effect on either, a larger effect on private outcomes, or a larger effect on public outcomes. I refer to the first two as *symmetric effects* and the latter two as *asymmetric effects*.

[7]The framework distinguishes changes in social incentives from changes in personal beliefs in the short run. Changed norms may become internalized over time (González, 2025), but gaps between perceived norms and personal beliefs can persist (Kelman, 1958; Crandall, Eshleman and O'brien, 2002). The framework therefore does not assume that internalization is immediate.

The discussion that follows focuses on individuals facing social costs, for the reasons given in the introduction; Appendix A develops the social benefit cases. Under social costs, contact can have symmetric positive effects when individuals initially do not express a favorable attitude or take a favorable action toward the outgroup in either domain. An intervention that changes their personal beliefs enough—alone or alongside a change in perceived norms—can lead them to do so in both domains. Alternatively, contact may have symmetric null effects, leaving both outcomes unchanged. This could occur when individuals already express or act favorably toward the outgroup privately and publicly, or when the intervention is insufficiently strong to change either decision.

At the individual level, asymmetric effects arise when contact changes an outcome in one domain but not the other. The main asymmetry of interest in this paper is private change without corresponding public change. Starting from the same initial public–private conditions—no favorable expression or action in either domain—contact may increase the personal value of favorable expression or action enough to make it worthwhile privately but not publicly. This can occur even if contact also reduces social costs, provided the remaining costs are sufficient to discourage public expression or action. In this way, contact can create new public–private gaps and, in the aggregate, deepen preference falsification (Kuran, 1995), potentially limiting its contribution to broader social change. Detecting this change may require measuring private attitudes or behaviors.

The opposite asymmetry is also possible: larger effects on public than on private outcomes. Consider individuals who initially express a favorable attitude or take a favorable action privately but not publicly, consistent with pre-existing preference falsification. Contact that increases the personal value of favorable expression or action, reduces its social costs, or both may lead them to do so publicly, while the private outcome remains unchanged.[8] In this way, contact can narrow an existing public–private gap, reducing preference falsification and potentially facilitating broader social change.[9] Detecting this change may require measuring public attitudes or behaviors.

[8]Because each attitude or action decision is binary, contact cannot increase favorable outcomes in a domain where an individual already expresses or acts favorably.

[9]This is consistent with research on misperceived social norms, which shows that norms interventions can bring behavior closer to privately held support (Bursztyn, González and Yanagizawa-Drott, 2020). The framework suggests that contact could also narrow the gap by changing personal beliefs

Overall, the framework yields two implications. First, even within the same social environment—here, social costs—contact can produce different public–private effect patterns: positive effects on both outcomes, no effect on either, or a larger effect in one domain than the other. These patterns matter both for understanding what contact changes and for detecting effects that might be missed if only one domain is measured. Second, under social costs, these patterns have different implications for how contact reshapes the public–private relationship. Larger private effects can create or widen a gap, deepening preference falsification, while larger public effects can bring public expression or action into closer alignment with existing private support. Examining these patterns in light of the social environment can therefore clarify both the potential and the limits of contact as a source of broader social change.

The preregistered hypotheses focused on the possibility of larger private than public effects:[10]

H4 A positive effect on *private* outcomes.

H5 A null (or small positive) effect on *public* outcomes.

H6 A negative asymmetry—a smaller effect on *public* than on *private* outcomes.

The framework clarifies the conditions under which this pattern can emerge. Under social costs, individuals who initially do not express or act favorably in either domain may change privately without changing publicly. I expected this possibility to be particularly relevant where personal prejudice and prejudiced ingroup norms coexist; Appendix D reports descriptive evidence of these conditions in the sample. For this individual-level possibility to produce a larger average private effect, however, changes confined to the private domain must outweigh changes confined to the public domain.

Although the preregistered hypotheses did not explicitly condition on social costs, they were developed with socially costly expression in mind (Appendices G.1 and L). I therefore make the corresponding conditional predictions for higher-cost individuals explicit. Because these predictions were not preregistered, I treat them as exploratory. Specifically, contact can yield:

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enough to make public expression or action worthwhile despite remaining social costs.

[10] Appendix G.1 presents the full set of hypotheses; H1 through H3 concern the average effects of parasocial contact and are tested in Section 5.

H4$_{HC}$ A positive effect on *private* outcomes for individuals with higher social costs.

H5$_{HC}$ A null (or small positive) effect on *public* outcomes for individuals with higher social costs.

H6$_{HC}$ A negative asymmetry—a smaller effect on *public* than on *private* outcomes—for individuals with higher social costs.

The lower-cost group serves as a comparison. Individuals in this group may face weaker social costs, neutral social incentives, or social benefits, so I do not make parallel directional predictions for their private and public effects. Instead, I examine whether the predicted pattern is more pronounced among individuals facing higher costs.[11] This yields two additional exploratory predictions:

H5$_{SC}$ Contact will have a smaller effect on *public* outcomes for individuals with higher relative to lower social costs.

H6$_{SC}$ The negative asymmetry—a smaller effect on *public* than on *private* outcomes—will be larger for individuals with higher relative to lower social costs.

3 The Parasocial Contact Experiment

I test the hypotheses using a media-based experiment to examine the effect of parasocial contact with Black Americans on White racial attitudes. The parasocial contact hypothesis holds that positive exposure to outgroup characters can reduce prejudice and improve intergroup attitudes (Schiappa, Gregg and Hewes, 2005). This focus on positive effects differentiates contact from

[11] These expectations require an additional assumption about changes in the lower-cost group. Under social benefits, individuals who initially express or act favorably publicly but not privately can also change privately without changing publicly. If sufficiently common, these changes could produce an equally large or larger private–public asymmetry in the lower-cost group. The predicted cross-group difference therefore depends on these private-only changes among individuals facing social benefits being insufficient to offset the larger private effects expected among higher-cost individuals; Appendix A develops the relevant conditions.

general media exposure which may be positive or negative and need not engage viewers or foster emotional ties. Researchers have increasingly drawn on this framework to examine how exposure to outgroup exemplars—such as Barack Obama (Goldman, 2012) or Mo Salah (Alrababa'h et al., 2021)—shapes attitudes towards outgroups as a whole. Parasocial contact is also common in edutainment interventions, which leverage entertaining media narratives to promote social change (Paluck, 2009; Grady, Iannantuoni and Winters, 2021; Siddique, Vlassopoulos and Zenou, 2026).

The predictions developed in Section 2 require only that contact change personal beliefs. Parasocial contact is indeed theorized to operate primarily through personal-belief pathways such as empathy and stereotype reduction, which distinguishes it from vicarious contact, operating through social learning and norm change (Joyce and Harwood, 2014; Banas, Bessarabova and Massey, 2020). But it may also shift perceived norms—by prompting viewers to infer others' reactions, signaling popularity, or encouraging discussion (Papa et al., 2000; Chwe, 2013; Tankard and Paluck, 2016). The predicted asymmetric effects survives so long as any such change in social norms does not transform social costs into social benefits. The empirical tests evaluate outcomes associated with each pathway.

3.1 Racial Attitudes and Social Context in the United States

The United States provides an informative setting in which to examine parasocial contact effects. Racial prejudice persists and remains socially and politically consequential (Pew Research Center, 2015, 2019; Morgan, 2022; Tesler, 2013; Engelhardt et al., 2019), and white racial attitudes have grown increasingly polarized (Jardina and Ollerenshaw, 2022). At the same time, persistent residential and social segregation limits opportunities for meaningful interracial contact, making media an important source of exposure to outgroup members (McPherson, Smith-Lovin and Cook, 2001; Enos, 2017).

The social consequences of expressing favorable racial attitudes also vary substantially. A widely held norm against prejudice makes explicit expressions of bias socially unacceptable in many contexts (Crandall, Eshleman and O'brien, 2002; Blinder, Ford and Ivarsflaten, 2013; Hopkins and Washington, 2020). That norm is not fixed, however: rhetoric targeting particular groups raises the perceived acceptability of prejudice toward them (Crandall, Miller and White, 2018; Arnold et al., 2026). Moreover, as personal networks are homogeneous, particularly by race (McPherson, Smith-

Lovin and Cook, 2001), individuals encounter these normative signals disproportionately through ingroup members whose approval carries added weight (Abrams et al., 1990). White Americans therefore differ not only in their personal prejudice toward Black Americans but also in their social environments and their beliefs over whether favorable public expression or action towards Black Americans would be met with approval or disapproval from other ingroup members.

3.2 The Experiment

The intervention uses content from the reality television series *Married at First Sight* (MAFS), in which couples meet for the first time at the altar and spend eight weeks navigating married life. The program follows relatively ordinary individuals confronting familiar relationship challenges, encouraging viewers to develop affective attachments to the featured couples.

The analysis compares two randomly assigned conditions: a 25-minute "mini-season" featuring either a successful Black (outgroup) couple or a comparable White (ingroup) couple. Within each condition, respondents were further randomized between two couples, with each treatment–control pair drawn from the same season (Figure 1). Appendix Figure B.3.1 summarizes the design, with further details on design and randomization in Appendix B. This pairing makes treatment and control content unusually comparable—a longstanding challenge in parasocial contact research (Banas, Bessarabova and Massey, 2020)—by holding genre, production quality, and narrative structure largely constant while varying the race of the featured couple.

The mini-seasons provided positive and emotionally engaging depictions that, for the Black couples, were also counter-stereotypical (Ramasubramanian, 2011, 2015). The content was thus designed to foster parasocial ties with the featured couples and encourage more favorable beliefs about the broader outgroup. Within the parasocial conditions, the storylines were also edited to minimize content that could shift perceived ingroup norms, including vicarious contact in which the ingroup characters form friendships with outgroup characters. Treatment checks reported below assess whether viewers became engaged with the couples and generalized from them to Black Americans more broadly.

Figure 1: Parasocial Contact Experiment Couples

Couples A and B

Couples C and D

Notes: Ingroup couples appear on the left and outgroup couples on the right. Control participants were randomly assigned to view either Couple A or Couple C; treatment participants viewed either Couple B or Couple D.

4 Research Design

The broader design combines a parasocial contact experiment with a cross-randomized virtual enumerator experiment (following a 2x2 factorial design) and measures of private and public behavioral intentions and actions. This section summarizes the sample, interventions, outcomes, and construction of social cost measures; Appendix B provides a design flow diagram and full design details.

4.1 Sample and Recruitment

A sample of 1,671 White Americans was recruited through CloudResearch Connect in October 2023, with soft quotas for gender and baseline outgroup attitudes. Eligible participants were non-Hispanic U.S. citizens aged 18–50 who had not previously watched *Married at First Sight*. After consent and a pre-treatment survey, respondents who passed an attention check were assigned with equal probability to ingroup parasocial contact (control), outgroup parasocial contact (treatment), or vicarious contact condition.[12] The focal comparison is between the two parasocial contact conditions: 1,118 respondents were assigned to them, 1,032 completed the survey, and 987 completed the behavior outcome. Appendix C reports baseline characteristics on the sample.

4.2 Outcome Measures and Public–Private Variation

The post-treatment survey collected a range of outcomes; Appendix F provides item wording and index construction. Respondents reported their reactions to the characters and the parasocial ties they formed. The two main attitudinal outcomes are warmth toward Black Americans and support for social change. The warmth index combines a feeling thermometer, desire for more Black friends, and acceptance of interracial marriage. Support for social change is a single item asking whether progress toward racial equality has gone too far, been about right, or not gone far enough.

The survey also measured trait evaluations of Black Americans as an indicator of stereotyping, a central outcome in contact research. Respondents estimated the proportions of Black and White Americans who are hard-working, intelligent, family-oriented, and committed in relationships. Other outcomes relevant to the personal-belief pathway include empathy, perceived similarity, and comfort with intergroup interaction. To assess the perceived norms pathway, the survey asked about perceived desire to have more Black friends among White Americans generally and among respondents' White friends and family members.

Where parallel evaluations of Black and White Americans are available, I use Black-only measures as primary because they most directly capture change toward the outgroup. White-only and Black–White difference measures help assess whether effects instead reflect broader shifts in evalu-

[12]The vicarious contact condition was pre-registered as a separate experiment and is analyzed separately.

ations of both groups.[13] Respondents also reported behavioral intentions and had an opportunity to take actions; these measures are described below.

4.2.1 Measuring Private and Public Attitudes

The central empirical challenge is to compare private and public effects on outcomes that are otherwise comparable. The virtual enumerator experiment, detailed in Appendix B.4, asks the same attitude questions, with identical response options and format, under conditions that differ only in the perceived audience. I independently randomized respondents to a *private* condition, in which they reported their attitudes through a standard online survey, or a *public* condition, in which they reported them to a virtual White (ingroup) enumerator. The enumerator was introduced with an image and a brief chat-style exchange, and the image remained visible throughout the post-treatment survey. Respondents in the public condition saw one of eight enumerators varying in age and gender, so the manipulation was not confounded with any single individual's characteristics. The manipulation registered: 96 percent of respondents in the public condition said they had a White enumerator, compared with 3 percent in the private condition (Appendix E.3).

The private condition provides the closest observable approximation of respondents' explicit private attitudes, consistent with evidence that self-administered surveys are perceived as more private and elicit more candid answers to sensitive questions (Tourangeau and Yan, 2007; Bursztyn et al., 2025). This does not require that self-administration capture true private beliefs—respondents may anticipate broader audiences even when responding anonymously (Lindsey, 2023)—only that it better approximates private expression.

The public condition instead captures expression under perceived visibility to an unfamiliar but socially relevant ingroup audience. Substantial general evidence (Blair, Coppock and Moor, 2020; Tourangeau and Yan, 2007; Bursztyn et al., 2025)—and evidence specific to the reporting of racial attitudes (Kuklinski, Cobb and Gilens, 1997; Krysan, 1998; Huddy and Feldman, 2009)—shows that individuals view enumerators as social referents and adjust their answers to perceived social expectations. The design follows previous efforts to detect such differences: Tourangeau, Couper

[13] The difference measure was preregistered as primary. Because it can also change when evaluations of the ingroup shift, I treat the Black-only measure as primary here; see Appendix L.

and Steiger (2003) varied social presence cues in self-administered surveys, Plant and Devine (1998) measured stereotype endorsement under public and private reporting conditions, and Krysan and Couper (2003) varied the race of a virtual interviewer. What is different here is the application to estimating treatment effects rather than diagnosing the sensitivity of a measure, and the emphasis on the ingroup membership of the observing audience.[14] Consistent with this literature, I interpret the public condition as capturing expression under anticipated social evaluation. The design does not separate the processes through which that operates—concern about others' judgments, activation of social identity, or heightened awareness of group norms—but the comparison does not require it: the question is whether audience context changes the estimated effect of contact.

A final advantage is that respondents answer the same direct questions in both conditions, so the comparison extends across multiple otherwise identical outcomes rather than requiring a separate indirect measure for each attitude, as a list experiment would. This matters because reporting warmth, attributing traits, and endorsing social change are distinct expressive acts that may carry different benefits, effort costs, and social consequences, so the experiment can reveal whether contact produces symmetric effects on some outcomes and asymmetric effects on others.

4.2.2 Measuring Private and Public Behavior

The behavior measures test whether patterns observed in the virtual enumerator experiment extend to intentions and actions, including actions visible to respondents' own social networks. The comparison is less controlled than that permitted by the virtual enumerator experiment, however: private and public measures involve different actions that may vary in effort, benefits, and other features besides visibility.

The survey measured intentions to take eight behavioral actions to support racial equality, grouped into preregistered private and public indices. Private items included voting for a Black candidate, voting for a White candidate who supports racial equality, reading an article, and signing an anonymous petition. Public items concerned sharing an article, confronting a friend or family

[14] The focus on an ingroup audience also differentiates the design from studies of demand effects, where the audience is the researcher (Mummolo and Peterson, 2019). Results in Appendices E.3 and I.4 weigh against attributing the public-condition effects to demand.

member, signing a public petition, and attending a protest. The preregistered analysis estimates effects on each index separately. Additional analyses compare two more closely matched pairs—reading versus sharing an article, and signing a private versus public petition (Paler, Marshall and Atallah, 2018)—to assess the private–public difference more directly.

Actual behavior was measured through a contest at the end of the survey (Appendix B.5). Participants could improve their chances of winning a gift card by completing up to eight actions, four related to racial justice and four unrelated. They were independently randomized to a private contest, with actions hidden from their social networks, or a public contest, with actions visible to network members. The preregistered analysis estimates contact effects separately in each version, testing whether contact increases engagement with race-related actions relative to unrelated actions.

4.2.3 Measuring Social Costs

The framework predicts that favorable public responses are more constrained when respondents perceive prejudiced ingroup norms and place greater weight on them. I measure the *direction* of this pressure using a factor score from six pre-treatment questions about perceived White attitudes toward Black neighbors and racially insensitive jokes. I proxy its *weight* with network homogeneity, measured by whether more than half of a respondent's social contacts are White. Respondents above the median of the perceived norms score who also have predominantly White networks are classified as facing higher social costs, a group comprising approximately 41 percent of the sample. Appendix D details the preregistered items, construction of the measure, alternative codings, and sample characteristics.[15]

The binary measure is designed primarily to identify respondents expected to face higher social costs. Those coded 0 may face weaker costs, neutral pressure, or social benefits, each of which can generate different private and public effects. I therefore make no directional prediction for the lower cost group but use it as a benchmark for the cross-group comparisons ($H5_{SC}$ and $H6_{SC}$). Because

[15]The items were preregistered, but the analyses combine them in different ways, see Appendices D and L). The pre-analysis plan discussed higher cost heterogeneity did not formally pre-register these tests for the parasocial contact experiment; they are thus treated as exploratory as discussed in Section 2 and Appendix L.

social costs were measured rather than assigned, those comparisons describe conditional treatment effects, not the causal effect of social costs (for more on inference involving the higher-lower cost comparisons, see Appendices A.4 and G.3).

4.3 Estimation

As detailed in Appendix G.3, I estimate average treatment effects using the pre-registered specification:

$$Y_i = \alpha + \delta Contact_i + X_i'\psi + \mu_j + \epsilon_i$$

where Y_i is the outcome of interest, $Contact_i$ indicates assignment to outgroup parasocial contact, X_i includes pre-specified covariates and an indicator for assignment to vicarious contact, and μ_j denotes implementation wave $\times$ MAFS season fixed effects (see Appendix B.6). The contact effect is therefore identified by comparing Couple B with its same-season ingroup counterpart, Couple A, and Couple D with its same-season ingroup counterpart, Couple C. Robust standard errors are reported throughout. For behavioral intentions and behavior, this equation estimates the average effect of parasocial contact on each private or public outcome. For attitudes, it estimates the effect pooled across the private and public reporting conditions.

To assess whether treatment effects differ between privately held and publicly expressed attitudes, I estimate the following interaction model:

$$Y_i = \alpha + \beta_1 Contact_i + \beta_2 Public_i + \beta_3 (Contact_i \times Public_i) + X_i'\psi + \mu_j + \epsilon_i$$

where $Public_i$ indicates assignment to the public virtual enumerator condition. Here, β_1 is the contact effect on privately reported attitudes, and β_3 is the public-minus-private difference in contact effects. A negative β_3 indicates a smaller effect under public than private reporting.

For attitudinal outcomes, I extend this equation with an indicator for higher social costs, the Contact $\times$ Public $\times$ HighCost interaction, and all lower-order interactions. I use the resulting coefficient combinations to estimate private and public treatment effects, and their difference, within each cost group. For behavioral intentions, I estimate interactions between contact and higher social costs separately for private and public outcomes. Appendix G.3 details these tests, including the

exploratory comparisons of matched behavioral intentions.

4.4 Data Quality Checks

Baseline covariates were generally balanced across experimental conditions, with some chance imbalance; the main estimates adjust for the prespecified covariates (Appendix E.1). There is no evidence of differential attrition or inattention by contact or virtual enumerator assignment (Appendix E.2). Participants assigned to outgroup contact were, however, more likely to infer the study's purpose, raising the possibility of experimental demand To probe this concern, Appendix H.3 compares effects across the two Black couples. Effects are concentrated largely among viewers of Couple D, a pattern less consistent with a uniform response to perceived experimental expectations than with responses to the content of the intervention.

5 Contact Treatment Checks and Average Effects

Respondents' reactions indicate that the treatment created the engagement and parasocial ties necessary for parasocial contact (see Appendix E.3). Across conditions, 88 percent reported being engaged with the content, 73 percent cared about the couples' outcomes, and 76 percent liked the couples overall. Respondents generally described the characters as likable and relatable, expressed interest in their lives, and perceived them as sharing their values. Reactions to the Black couples were at least as favorable as reactions to the White couples and were positive toward both male and female characters. Open-ended responses likewise suggest that respondents identified with the Black couples and, in some cases, explicitly contrasted their portrayals with negative racial stereotypes. The contact treatment also had the potential to generalize beyond the individuals depicted: 67 percent viewed the Black male character and 74 percent viewed the Black female character as representative of Black Americans. Finally, 52 percent perceived the content as mostly or entirely authentic, while 85 percent perceived at least half of it as authentic.

Following this evidence for parasocial ties, Figure 2 presents the average effects of the contact treatment on the two main attitudinal outcomes—warmth and support for social change—as well as on measures associated with the personal belief and social norms pathways (for detailed results on all outcomes, see Appendix H). The treatment improved both main attitudinal outcomes. The

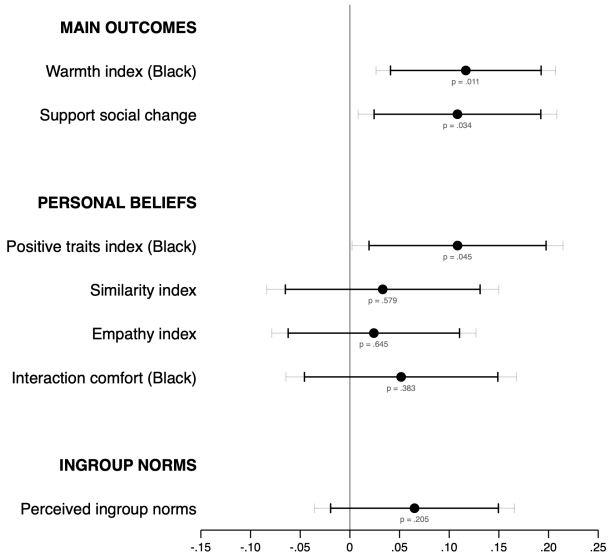
contact treatment increased the Black warmth index by 0.12 standard deviations ($p = .011$), it also yielded a .09 standard deviation improvement in the Black–White difference ($p = .033$), showing this result was driven by changes in attitudes specifically towards Black Americans. The largest component effect of the warmth index was on respondents' desire for more Black friends, which increased by 0.11 units ($p = .004$). The absence of a corresponding effect on desire for more White friends produced a 0.10-unit improvement in the Black–White difference ($p = .012$). The treatment also increased support for social change by 0.08 units ($p = .034$).

Figure 2 also reports effects on measures associated with the personal belief pathway. The clearest evidence comes from the positive traits index, indicating a reduction in negative stereotyping. The contact treatment increased the Black positive traits index by 0.11 standard deviations ($p = .045$), again with little corresponding change in evaluations of White Americans. The resulting 0.09 standard deviation improvement in the Black–White difference ($p = .082$) further suggests that the result reflects more favorable evaluations of the outgroup rather than a general shift in trait ratings. The pattern is broadly consistent across the four main component traits (Appendix H.1.1). Evidence of change in the other measured personal belief pathways is more limited, with no detectable effects on perceived similarity, empathy, or comfort interacting with outgroup strangers.[16] The contact treatment also had no detectable effect on perceived ingroup norms regarding the desirability of outgroup friendships.

Taken together, these results establish that the contact treatment produced meaningful improvements in warmth, support for social change, and stereotype-relevant trait evaluations. The effect on trait evaluations is especially important for the public–private hypotheses because the predicted asymmetry requires contact to change personal beliefs, whether or not it also changes perceived social norms. These average effects therefore establish the empirical premise for the public–private analysis that follows.

[16] As preregistered, the treatment was not expected to affect every measure associated with personal belief change; evidence of change in at least one pathway was interpreted as support for the operation of this pathway.

Figure 2: Average Treatment Effects on Attitudes



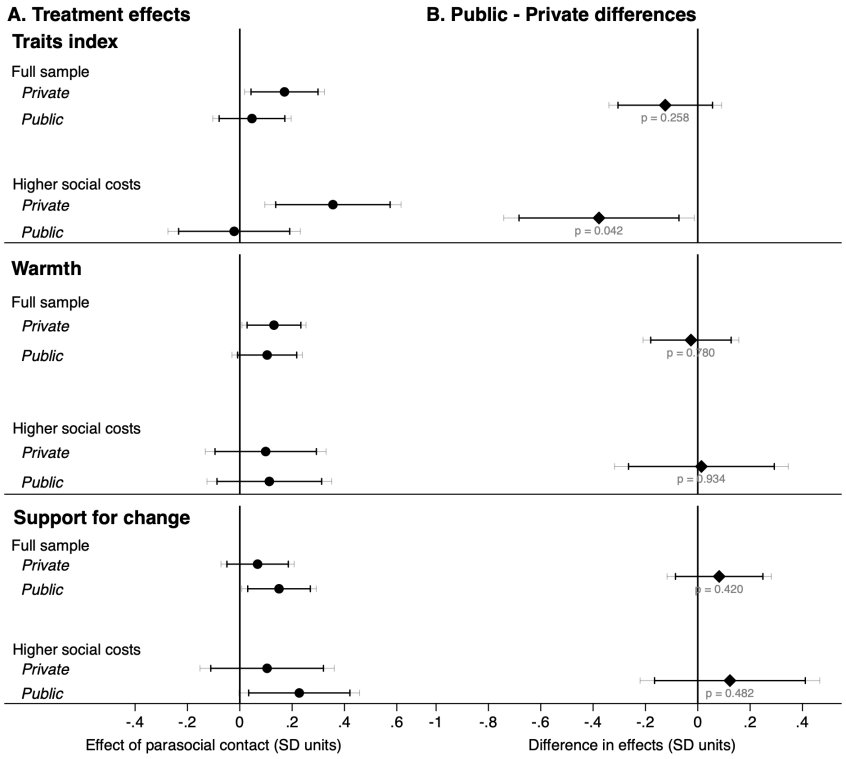
Note: Points report estimated average treatment effects of parasocial contact from regressions including prespecified covariates and wave fixed effects. Horizontal lines show 90% (dark) and 95% (light) confidence intervals. The main outcomes measure warmth toward Black Americans and support for social change benefiting Black Americans. The remaining indices measure positive trait evaluations, perceived similarity, empathy, comfort interacting with Black Americans, and perceived ingroup support for close friendships with Black Americans. All outcomes are standardized using the control group mean and standard deviation.

6 Effects on Private and Public Attitudes

Following the strong evidence of average effects, I now examine evidence for the primary predicted asymmetry: that contact will have a smaller effect on public relative to private expression of attitude change, especially for those facing higher social costs. Figure 3 reports standardized treatment effects on the two general attitudinal outcomes—warmth and support for social change—and on positive trait evaluations, the measure providing the clearest evidence of personal belief change. For each outcome, Panel A reports the private and public effects in the full sample and then among respondents facing higher social costs. Panel B reports the corresponding public-minus-private differences. Appendix I presents the complete estimates, including results for respondents facing lower social costs and the formal comparisons between cost groups.

The predicted divergence does not emerge for warmth or support for social change. For both outcomes, the positive average effects reported above were similar in the private and public conditions—consistent with more symmetric effects. This was true in the full sample and among respondents facing higher social costs, and the estimated public–private differences were small and statistically indistinguishable from zero. The experiment therefore provides no evidence that visibility

Figure 3: Effects on Private Attitudes and Public Expression



Note: Points report estimated effects of contact treatment on standardized trait evaluations, warmth toward Black Americans, and support for social change. Panel A reports treatment effects on private attitudes and public expression in the full sample and among respondents facing higher social costs. Panel B reports the corresponding public–private differences in treatment effects; negative estimates indicate that the treatment effect was smaller in public than in private. Dark and light horizontal lines represent 90 and 95 percent confidence intervals, respectively. Reported p-values correspond to the public–private differences. All outcomes are standardized using the mean and standard deviation of the control group. Models include the prespecified covariates and wave fixed effects, with heteroskedasticity-robust standard errors.

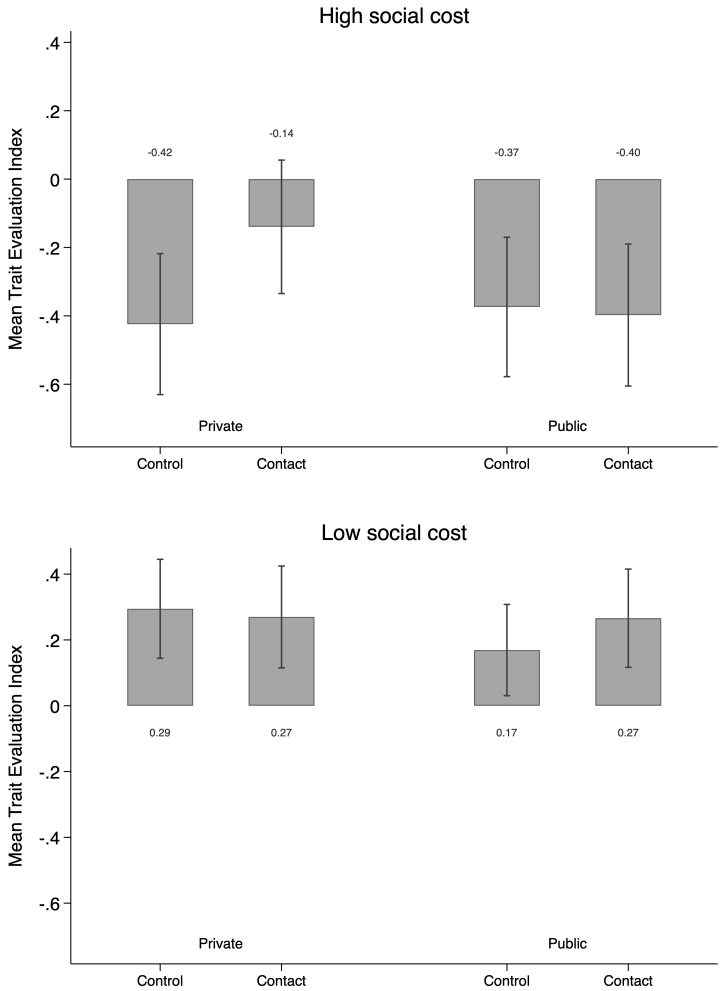
constrained the effects of contact on these two outcomes.

However, evidence of the predicted asymmetry appears for positive trait evaluations. In the full sample, the treatment increased positive trait evaluations by 0.17 standard deviations when responses were private ($p = .028$), compared with 0.05 standard deviations when responses were public ($p = .541$). The resulting public–private difference was negative, as predicted, but not statistically distinguishable from zero (-0.12 standard deviations, $p = .258$). The full-sample results therefore follow the predicted ordering but do not, on their own, establish that the private and public effects differed.

The pattern is substantially sharper among respondents facing higher social costs. Within this group, the treatment increased positive trait evaluations by 0.36 standard deviations when responses were private ($p = .007$), but had essentially no effect when responses were public (-0.02 standard

deviations, $p = .867$). The public effect was therefore 0.38 standard deviations smaller than the private effect ($p = .042$). Thus, among respondents for whom favorable expression was most socially costly, contact changed privately reported stereotypes without producing a corresponding change in public expression. The component estimates generally follow the same pattern, indicating that the index-level result is not attributable solely to one trait (Appendix Table I.2.1). The moderate correlations among the items ($r = 0.38$–0.68) further indicate that the index captures related but distinct trait judgments. The formal triple-interaction test also distinguishes the conditional patterns: the public–private difference was 0.48 standard deviations more negative among respondents facing higher rather than lower social costs ($p = .042$).

Figure 4: Mean Levels of Trait Evaluations by Contact and Public Conditions



Note: Bars report raw mean values of the trait-evaluation index by contact treatment condition, separately by visibility and social-cost condition. Error bars indicate 95 percent confidence intervals around the raw means. Bracketed labels report unadjusted contact–control differences. Covariate-adjusted treatment effects are reported in Figure 3.

Figure 4 provides additional insight into the trait result, displaying raw mean evaluations across treatment, visibility, and social cost conditions. Among respondents facing higher social costs, evaluations of Black Americans were comparatively unfavorable across all four conditions. The raw means mirror the pattern in the adjusted estimates reported in Figure 3: the most pronounced contact–control difference appears for private evaluations among higher-cost respondents, whereas no comparable improvement appears in public expression. This pattern illustrates the central intuition: where favorable public expression remains costly, contact-induced changes in private attitudes need not become publicly expressed. Respondents facing lower social costs provide a useful descriptive benchmark: their evaluations were more favorable in both visibility conditions and differed little between contact and control. The raw means shown here are descriptive; Figure 3 and Appendix Table I.2.1 report the covariate-adjusted treatment effects and formal public–private comparisons.

This pattern also rules out the possibility that the private effect among higher-cost respondents reflects greater scope for change. Higher cost respondents began with substantially lower private trait evaluations than lower cost respondents, even after conditioning on pretreatment prejudice, raising the possibility that they simply had more room to improve. That initial difference does not, by itself, explain the divergence across reporting conditions. If greater scope for change were driving the result, contact should have improved evaluations among higher-cost respondents in both private and public conditions, not just the former one.

The trait result is robust in its qualitative form. Across the alternative binary measures of social costs reported in Appendix I.3, the treatment generally improves private trait evaluations, produces little detectable change in public expression, and yields a negative public–private difference. The precision of the interaction estimates varies across specifications, but the direction of the pattern is stable. Results using the continuous measure of perceived ingroup norms follow the predicted pattern—as norms become more prejudiced, contact has increasingly negative effects on public expression. This indicates the results are not an artifact of a particular binary coding. The pattern also persists when respondents who gave the same answer to every trait item are excluded, though this analysis conditions on a post-treatment response pattern and is best read as descriptive.

6.1 Discussion

Among respondents facing higher social costs, contact improved privately reported trait evaluations but produced no detectable improvement in evaluations expressed to an ingroup enumerator, yielding a statistically significant public–private difference. This is consistent with contact changing private evaluations without overcoming the social barriers to expressing them publicly. The result matters because stereotypes are a canonical outcome in contact research (Allport, 1954; Pettigrew and Tropp, 2011; Huddy and Feldman, 2009) and are often less responsive than affective attitudes or behavior, a pattern generally taken to suggest that they resist change (Tropp and Pettigrew, 2005; Paluck et al., 2021). For example, Siddique, Vlassopoulos and Zenou (2026) find that a documentary improved prosocial behavior toward an ethnic minority but not stereotypes. Although they consider private updating unlikely because responses were elicited privately, their surveys were administered by enumerators and observability did not vary by design. The present findings suggest that some contact effects on stereotypes may go undetected when responses are visible to others, particularly where favorable expression carries social costs.

The asymmetry did not extend to warmth or support for social change. Contact improved both outcomes, with similar private and public effects, including among higher-cost respondents. The framework clarifies how such symmetric effects could arise under social costs: individuals who initially do not express a favorable attitude in either domain may come to do so in both if the gain in personal value is large enough to overcome both nonsocial and social costs. The results suggest that favorable private change was less readily expressed publicly for trait evaluations than for the other measures, although the design cannot establish why the outcomes differed. Nevertheless, such variation is consistent with evidence that racial attitude measures differ in sensitivity to visibility and social context (Sigall and Page, 1971; Kuklinski, Cobb and Gilens, 1997; Krysan, 1998; Krysan and Couper, 2003). While evidence for asymmetry is narrower than anticipated, its concentration in one outcome is also consistent with the framework's premise that expressing each attitude involves a separate decision with its own costs and benefits.

The outcome-specific nature of the finding does not diminish its measurement implication. Had trait evaluations been measured only publicly, the study would have detected no contact effect on this central outcome among higher-cost respondents. The ability to detect treatment effects

can thus depend on both the social environment and whether a survey captures private or public expression; overlooking visibility may produce incomplete or seemingly inconsistent results within and across studies.

7 Effects on Private and Public Behavior

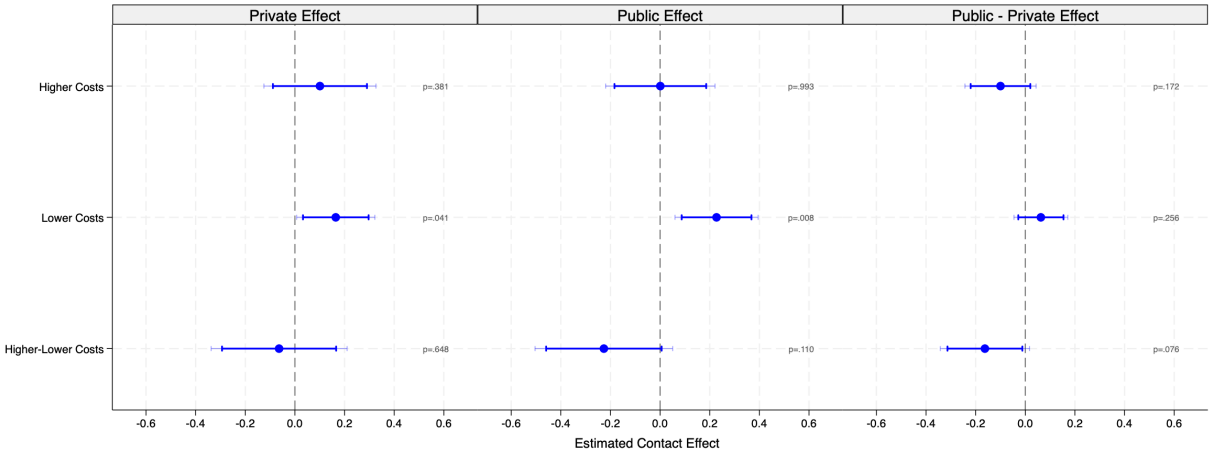
I next examine behavioral intentions and actual behavior in the Gleam Contest. These outcomes test whether the public–private logic extends to intended and observed actions, including actions visible within respondents' own social networks. The comparison is less controlled than the virtual enumerator experiment because private and public actions differ in benefits and effort costs as well as in visibility. For that reason, the preregistered private- and public-action indices are not a test of the asymmetry: their component actions differ substantively, so any difference between them confounds visibility with the nature of the act. I report them for completeness, then turn to the matched pairs—reading versus sharing an article and signing a private versus public petition—which hold the underlying action more nearly constant and provide the closer behavioral analogue to the enumerator comparison.

Looking first at behavioral intentions, the preregistered indices show that contact affected intentions to act: there was little overall effect on the private action index, though effects varied across items, and a positive effect on the public action index (see Appendix J.1). These estimates indicate that contact changed intentions to take both private and public actions, but they do not isolate the role of visibility, since the items composing the two indices differ substantively.

The behavioral analysis that most closely approximates the virtual enumerator experiment uses two matched pairs: reading versus sharing an article and signing a private (anonymous) versus public (non-anonymous) petition (Paler, Marshall and Atallah, 2018). These actions differ in visibility while plausibly being more similar on other dimensions than the broader sets of private and public behaviors. Figure 5 reports the combined results; pair-specific estimates appear in Appendix Table J.2.1.

The pattern is remarkably consistent with the predictions, although some estimates are imprecise. Among higher-cost respondents (Figure 5, top row), the estimated private effect is positive but highly imprecise (0.10, $p = .381$), while the estimated public effect is approximately zero (0.00,

Figure 5: Effects on Intentions to Take Matched Actions



Notes: Points report the estimated effects of parasocial contact on matched private- and public-action measures. The private measure averages intentions to read an article and sign a petition privately; the public measure averages intentions to share the article and sign a petition publicly. "Higher−Lower Costs" reports the difference between the conditional effects. The third column reports public-minus-private effects, making its final row the difference-in-differences. Thick and thin lines show 90 and 95 percent confidence intervals, respectively. Outcomes are reported in their original units. Models adjust for the preregistered covariates and randomization strata, and use heteroskedasticity-robust standard errors.

$p = .993$), producing a negative but statistically insignificant public–private difference of -0.10 ($p = .172$). While these effects are imprecise, their contrast with the effects among lower-cost respondents is notable. In that group, contact had positive and statistically significant private and public effects, producing a small, positive public–private difference. Together, these patterns produce a public–private difference that is 0.16 units more negative among higher- than lower-cost respondents ($p = .076$), statistically significant at the 10 percent level and in the direction predicted by H6_{SC}.

The higher–lower comparisons help identify the variation underlying this result. As shown in the middle column, the public effect is approximately zero among higher-cost respondents but positive among lower-cost respondents, producing a higher–lower difference of -0.23 units ($p = .110$), directionally consistent with H5_{SC}. By comparison, the contact treatment had suggestively positive private effects for both higher and lower cost groups, resulting in a small difference between the two (-0.06, $p = .648$). The results are therefore consistent with the predicted asymmetry: the estimated public-minus-private difference in treatment effects was more negative among higher-cost than lower-cost respondents. Overall, the matched-action estimates are imprecise, but their consistency with the full pattern of predictions—and across the two component pairs detailed in Appendix J.2.1—is notable.

Finally, despite positive effects on several behavioral intentions, contact produced no detectable effects on actual behavior in the incentivized contest (Appendix J.4). Although null, these results remain informative. A null effect on public behavior is difficult to diagnose because a treatment induced increase in the personal value of acting may be insufficient to overcome effort costs, social costs, or both. The private outcome provides the diagnostic leverage, because it entails effort costs without the additional costs of public expression. Had contact moved private behavior while leaving public behavior unchanged, social costs would be the natural explanation for the public null. That contact moved neither indicates that the treatment-induced change in the personal value of acting was insufficient to overcome even the effort costs of private action. The implication for intervention design is that reducing social costs would be unlikely to generate public behavior here: where private change has yet to be realized, the binding constraint lies in the personal value of acting or in the effort required, not in visibility. Measuring both domains is therefore valuable not only for detecting effects that vary with visibility but for identifying which barrier an intervention has failed to overcome.

8 Conclusion

This paper contributes to research on contact in two ways. First, it develops a framework explaining how the social environment, the intervention pathway, and the initial public–private conditions shape whether contact affects private outcomes, public outcomes, both, or neither. Second, it provides evidence for one consequential possibility: contact can change private outcomes without changing public ones among those facing higher social costs. Parasocial contact improved private trait evaluations among higher cost respondents while producing no detectable change in evaluations expressed to an ingroup enumerator. Behavioral intentions showed a similar, though less precise, pattern.

One implication of these findings is that visibility can affect whether researchers detect an effect of contact. Had trait evaluations been measured only by the virtual ingroup enumerator, this study would have found no detectable effect among higher cost respondents. Contact research elicits outcomes under widely varying conditions, yet visibility is rarely identified as a theoretically or empirically consequential feature of an outcome. Across studies, differences in how outcomes are

elicited, or in the social pressures respondents face, could produce seemingly inconsistent findings about similar interventions. Within a study, differences attributed to the type of outcome measured may sometimes reflect whether those outcomes are private or public.

One instance of this problem concerns the attitude–behavior gap (Paluck et al., 2021). Classic theories hold that attitude change precedes behavioral change (Ajzen, 1991; Harmon-Jones, Armstrong and Olson, 2019), yet prejudice-reduction research has found behavioral change without corresponding attitude change (Scacco and Warren, 2018; Mousa, 2020), while Lowe (2025) finds smaller effects on incentivized behavior than on self-reported outcomes. The framework suggests that some apparent attitude–behavior gaps may also reflect a public–private gap. A study measuring private attitudes and public behavior could find that attitudes change while behavior does not because social costs constrain public action. Conversely, a study measuring public attitudes through an enumerator-administered survey and private behavior could find behavioral change without a corresponding change in expressed attitudes. Understanding the relationship between attitude and behavioral change may therefore require measuring both kinds of outcomes under private and public conditions.

These inferential problems point to three recommendations. First, researchers should be more explicit in specifying whether an attitudinal or behavioral outcome is visible to others, and thus subject to social incentives. Second, they should consider whether the intervention can plausibly move the outcome in the domain being measured. This requires attention to the social environment, the initial public–private relationship, and whether the intervention changes personal beliefs, social incentives, or both. Third, where effects could differ across domains, researchers should measure both private and public outcomes to detect change that might otherwise be missed and to establish where it occurs.

Another implication of this study concerns social change. Private and public outcomes need not change together, and an intervention may itself alter the gap between them. Existing theories offer different expectations about how such gaps evolve. They may be minimal or transitory if people update their beliefs in response to new norms or become more willing to act on beliefs they already hold (Larson and Lewis, 2025; de Mesquita, Shadmehr et al., 2023; Terry and Hogg, 1996; Harmon-Jones, Armstrong and Olson, 2019; Bicchieri, 2005; Cialdini, Reno and Kallgren, 1990; Tankard and Paluck, 2016). Accounts of preference falsification, pluralistic ignorance, and

misperceived norms, however, show why gaps can persist (Kuran, 1995; Prentice and Miller, 1993; Bursztyn, González and Yanagizawa-Drott, 2020).

Comparatively little has been said about how contact interventions affect this relationship between private and public outcomes. The framework takes a step toward that account, responding to Paluck (2009)'s call for a more systematic understanding of how prejudiced beliefs, norms, and behaviors relate in contact interventions. Depending on the intervention pathway and the social environment, contact can narrow an existing gap or create or widen one by changing outcomes in one domain without corresponding change in the other. The results illustrate how private change can emerge while public expression remains unchanged among those facing higher social costs. Studying how interventions reshape the *relationship* between private and public outcomes can therefore reveal both where change has occurred and what may constrain its wider expression.

This also suggests an important direction for future research. Outcomes here were measured immediately after treatment, which identifies where change first emerges and whether asymmetry arises at all—the precondition for asking what happens next. Short-run measures alert us to whether a treatment has asymmetric effects in the first place, and thus whether further intervention might be needed to bring the domains into alignment. Measures over time would then show how the relationship evolves: whether private and public outcomes eventually align or continue to diverge. Together, these would illuminate the dynamics by which contact can shape broader social change.

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A A Framework for Understanding Contact Effects on Private and Public Outcomes

This appendix formalizes the framework underlying the theory and hypotheses in Section 2. It represents the adoption of a favorable private or public response as a threshold decision, identifies the pathways through which contact may move respondents across these thresholds, and shows how the social environment and individuals' initial positions shape the resulting treatment effects. It then derives the conditions under which these individual transitions produce asymmetric population-level effects and maps those predictions onto the empirical analysis.

A.1 The Utility Model

A.1.1 Private and Public Utility

Consider respondent i deciding whether to express a favorable attitude or take a favorable action toward an outgroup. A *response* refers to a particular attitude or action—for example, endorsing a stereotype or signing a petition. A *domain* refers to whether that response is given privately or publicly, indexed by $d \in \{Priv, Pub\}$. A response given privately is shielded from meaningful social observation, whereas one given publicly is visible to others. In this study, the relevant audience is the respondent's perceived ingroup.

The framework considers responses that can be given in either domain, holding the response fixed and varying only its visibility. It therefore yields predictions about private versus public endorsement of a stereotype, or about willingness to sign a petition privately versus publicly, but not about how these two responses compare to one another. Different responses are distinct objects of choice and may have different utility components. All terms are therefore implicitly indexed by the response under consideration, and this index is suppressed for notational simplicity. The domain index is retained throughout, because the comparison between private and public expression of the same response is the object of interest.

Let $B_{id} \in \mathbb{R}$ denote respondent i's personal valuation of the favorable response in domain d. When $B_{id} > 0$, the respondent derives personal utility from the favorable response; when $B_{id} < 0$, the respondent derives personal disutility from it, as may occur when the respondent holds prejudiced beliefs about the outgroup.

Let $C_{E,id} \geq 0$ denote the nonsocial costs of adopting the favorable response in domain d, including effort, material costs, or other barriers unrelated to anticipated social consequences.[1]
Let $S_i \in \mathbb{R}$ denote the direction of the perceived ingroup norm governing favorable public expression. $S_i > 0$ indicates that a favorable public response is socially costly (e.g., it would meet with ingroup disapproval), $S_i < 0$ that it is socially beneficial (e.g., it would meet with ingroup approval), and $S_i = 0$ that the social environment is neutral. Let $\sigma_i \geq 0$ denote the weight this norm carries for respondent i. When $\sigma_i = 0$, the norm does not affect the respondent's utility; larger values indicate greater sensitivity to it. This sensitivity may reflect greater fear of ingroup sanctioning or a more homogeneous social network that reinforces conformity pressure.

Respondent i's social cost wedge, which combines the direction of the perceived ingroup norm with the weight it carries for the respondent, is therefore

$$\omega_i \equiv \sigma_i S_i.$$

Finally, let α_d indicate whether the response is visible to other ingroup members:

$$\alpha_d = \begin{cases} 0 & \text{if } d = Priv, \\ 1 & \text{if } d = Pub. \end{cases}$$

[1] It is important to distinguish effort costs from social costs for clear predictions. For example, Hässler et al. (2020) characterize petition signing and protest as low- versus high-cost collective actions, but define these costs in terms of effort, with little attention to possible social costs.

The utility of the favorable response is

$$U_{id} = B_{id} - C_{E,id} - \alpha_d \omega_i. \tag{A.1}$$

This yields

$$U_i^{Priv} = B_{iPriv} - C_{E,iPriv}, \qquad U_i^{Pub} = B_{iPub} - C_{E,iPub} - \omega_i.$$

For a given response, when personal valuation and nonsocial costs are held constant across domains, the social cost wedge is the only difference between private and public utility. It lowers public relative to private utility when $\omega_i > 0$ (social costs), raises it when $\omega_i < 0$ (social benefits), and leaves private and public utility equal when $\omega_i = 0$. These components may still differ across responses, however.

A.1.2 Expression as a Threshold Decision

The framework represents adoption of the favorable response as a threshold decision. Respondent i gives the favorable response in domain d whenever its utility is weakly positive:

$$Y_{id} = \mathbf{1}\left\{U_{id} \geq 0\right\}, \qquad d \in \{Priv, Pub\}.$$

Here, $Y_{id} = 1$ indicates a favorable response and $Y_{id} = 0$ otherwise. Locating the threshold at zero is a normalization: any fixed threshold can be absorbed into $C_{E,i}$ without changing the framework's implications. An intervention changes the observed response only when it moves utility from below to above this threshold. Individual treatment effects therefore depend on both the change in utility and the respondent's initial position relative to the threshold.

Comparing private and public responses requires an assumption of cross-domain comparability: for a given response, personal valuation and nonsocial costs are constant across domains, which differ only in visibility:

$$B_{iPriv} = B_{iPub} = B_i, \qquad C_{E,iPriv} = C_{E,iPub} = C_{E,i}.$$

Under this assumption, the private and public decision rules are

$$Y_i^{Priv} = 1 \iff B_i - C_{E,i} \geq 0, \qquad Y_i^{Pub} = 1 \iff B_i - C_{E,i} \geq \omega_i.$$

Although both responses are evaluated against a zero utility threshold, the social cost wedge changes how much personal valuation, net of nonsocial costs, a public response requires. When $\omega_i > 0$, the public threshold is more demanding than the private threshold; when $\omega_i < 0$, it is less demanding; and when $\omega_i = 0$, the thresholds coincide.

Cross-domain comparability is most plausible when the private and public conditions present the same response and differ only in visibility. When they instead present different attitudes or actions, personal valuation and nonsocial costs may also differ across domains. Their threshold ordering can then no longer be attributed to the social cost wedge alone.

A.1.3 Intervention Pathways

A contact intervention may change utility through two distinct pathways. The *personal belief pathway* changes the respondent's personal valuation of the favorable response, B_i, through mechanisms such as empathy, learning, stereotype reduction, or reduced perceived threat. The *social incentive pathway* changes the direction of the perceived ingroup norm, S_i, including expectations of social approval or disapproval. The two pathways may operate separately or jointly.

Let $t \in \{0, 1\}$ denote contact treatment status, where $t = 0$ denotes control and $t = 1$ treatment. Let $\delta_{B,i} \geq 0$ denote a favorable treatment-induced change in respondent i's personal valuation and $\delta_{S,i} \geq 0$ a favorable treatment-induced change in the perceived ingroup norm. Because positive values of S_i represent social costs, a favorable change reduces it:

$$B_i(1) = B_i(0) + \delta_{B,i}, \qquad S_i(1) = S_i(0) - \delta_{S,i}.$$

These restrictions allow treatment to leave respondents unchanged or move them in a favorable direction but rule out individual-level backlash. They operate as a scope condition rather than as a prediction: a negative treatment effect in either domain falls outside the framework's restrictions rather than being explained by them, and would indicate that the intervention operates in ways the framework does not represent.

Holding σ_i fixed, the resulting changes in private and public utility are

$$\Delta U_i^{Priv} = \delta_{B,i}, \qquad \Delta U_i^{Pub} = \delta_{B,i} + \sigma_i \delta_{S,i}.$$

The two pathways affect threshold crossing differently. A favorable change in personal beliefs increases the personal valuation of the response and therefore raises both private and public utility, potentially moving the respondent across either or both thresholds. A favorable change in perceived ingroup norms makes the public threshold less demanding without directly changing the private threshold. Under social costs, it narrows the distance between the thresholds; under social benefits, it increases the public advantage. As long as social costs remain positive, the public threshold remains more demanding than the private threshold. If social costs fall to zero, the thresholds coincide; if they become social benefits, their ordering reverses. The predictions below allow treatment to reduce social costs or increase social benefits but assume that it does not transform social costs into social benefits, meaning that $\delta_{S,i} \leq S_i(0)$ whenever $S_i(0) > 0$. Because $\delta_{S,i} \geq 0$, a change from social benefits to social costs is already ruled out.

The predictions below apply to interventions for which the personal belief pathway is operative, meaning $\delta_{B,i} > 0$ for at least some individuals. The social incentive pathway may also operate. The analysis does not consider interventions operating solely through the social incentive pathway, which may change public utility without changing private utility. This scope reflects the positive parasocial contact intervention studied here, which is designed and theorized to change personal beliefs about outgroup members.

A.1.4 Initial Conditions

The framework has so far identified the social cost wedge and the intervention pathways as two features that shape private and public effects. The third is an individual's initial condition, meaning their position relative to the private and public thresholds before treatment.

Formally, an initial condition is represented by an ordered pair (I_i^{Priv}, I_i^{Pub}), where $I_i^d = 1$ indicates that the individual is above the threshold in domain d before treatment and $I_i^d = 0$ indicates that they are below it. Four initial conditions are possible. In *Neither* $(0, 0)$, the individual is below both thresholds and therefore does not give the favorable response privately or publicly. In *Private-leading* $(1, 0)$, the individual is above the private threshold but below the public threshold, producing a favorable private response but not a favorable public response. In *Public-leading* $(0, 1)$, the individual is below the private threshold but above the public threshold. In *Both* $(1, 1)$, the individual is above both thresholds.

These initial conditions capture familiar forms of public–private alignment and divergence. *Private-leading* describes favorable private expression or action without corresponding public expression or action, a pattern consistent with preference falsification and capable of sustaining pluralistic ignorance. *Public-leading* describes favorable public expression or action without a corresponding private response, as under norm compliance without internalization. *Neither* and *Both* represent alignment on the unfavorable and favorable responses, respectively. These labels describe response configurations rather than the psychological or social processes that produced them.

Initial conditions matter because they determine where favorable movement remains possible. With binary threshold crossings, an individual beginning in *Neither* retains room to move favorably in either or both domains. An individual beginning in *Private-leading* can move favorably only in public, while one beginning in *Public-leading* can move favorably only in private. An individual beginning in *Both* has no remaining

scope for favorable movement on either response. Initial conditions therefore restrict, but do not determine, the treatment-effect pattern. Beginning in *Neither* permits symmetric movement across both thresholds or asymmetric movement across only one. Beginning in *Private-leading* or *Public-leading* permits only asymmetric non-null effects, while beginning in *Both* permits only a null effect. The next section shows how initial conditions combine with the social environment and intervention pathway to shape private and public effects.

A.2 Predicted Individual-Level Private and Public Effects

Three features structure individual treatment-effect patterns. The social environment determines the relative ordering of the private and public thresholds—which is more demanding, or whether they coincide. The intervention pathway determines which components of utility change, while the initial condition determines the respondent's position relative to each threshold and where favorable movement remains possible. Together, these features determine whether treatment moves the respondent across neither threshold, one threshold, or both. The resulting effect may therefore be private only, public only, both, or neither.

Table A.2.1 summarizes the possible individual treatment-effect patterns and their implications for the relationship between private and public outcomes. The first column identifies whether an individual faces social costs, social benefits, or neither for a favorable response. This social environment determines the ordering of the private and public thresholds and, therefore, which initial conditions are possible. Under social costs, for example, an individual may begin in *Neither*, *Private-leading*, or *Both*, but not *Public-leading*: anyone who has crossed the more demanding public threshold must also have crossed the private threshold.

The second column lists the initial conditions that remain possible in each social environment, using the ordered pairs defined above. The third column shows the possible individual treatment effects from each initial condition. An individual beginning in *Neither* under social costs, for example, may cross neither threshold, only the private threshold, or both thresholds, depending on the pathway and magnitude of the treatment-induced utility change and the individual's position relative to each threshold. Crossing both thresholds produces symmetric effects, whereas crossing only one produces an asymmetric effect. More generally, the range of possible effects across social environments and initial conditions highlights how sensitive the detection of treatment effects may be to the choice of outcome measure.

The fourth column identifies the post-treatment condition. An individual beginning in *Neither* under social costs, for example, remains there following a null effect, moves to *Private-leading* following a private-only effect, and moves to *Both* following an effect on both outcomes. The final column indicates whether each transition preserves alignment, creates or deepens public–private divergence, or reduces or eliminates existing divergence. These distinctions matter for understanding how interventions affect social change.

A.2.1 The Main Case: Social Costs

The paper focuses on the possibility that contact has a smaller effect on public than private outcomes, especially among those facing higher social costs. Table A.2.1 clarifies when this pattern can emerge. Consider individuals who face social costs and begin in *Neither*, below both thresholds. Because the public threshold is more demanding, a contact-induced change in personal beliefs, and thus in the personal value of a favorable response, may move them across the private threshold without moving them across the public threshold. Contact may also change perceived social norms and thereby lower the public threshold. The private-only pattern persists as long as the combined changes are sufficient to cross the private threshold but not the public threshold.

The table also shows why this individual-level possibility need not produce the same population-level pattern. Individuals facing social costs may begin in different conditions and lie at different distances from the thresholds. Whether the private effect exceeds the public effect in the aggregate therefore depends on the distribution of these initial conditions and treatment-induced transitions, as discussed next.

Table A.2.1: Possible Individual-Level Treatment Effects

Social environ.	**Initial condition**	**Possible treatment effect**	**Resulting condition**	**Implication**
Costs	Neither $(0,0)$	No effect	Neither $(0,0)$	Alignment preserved
Costs	Neither $(0,0)$	Private only	Private-leading $(1,0)$	Divergence created
Costs	Neither $(0,0)$	Private and public	Both $(1,1)$	Alignment preserved
Costs	Private-leading $(1,0)$	No effect	Private-leading $(1,0)$	Divergence preserved
Costs	Private-leading $(1,0)$	Public only	Both $(1,1)$	Divergence eliminated
Costs	Both $(1,1)$	No effect	Both $(1,1)$	Alignment preserved
Benefits	Neither $(0,0)$	No effect	Neither $(0,0)$	Alignment preserved
Benefits	Neither $(0,0)$	Public only	Public-leading $(0,1)$	Divergence created
Benefits	Neither $(0,0)$	Private and public	Both $(1,1)$	Alignment preserved
Benefits	Public-leading $(0,1)$	No effect	Public-leading $(0,1)$	Divergence preserved
Benefits	Public-leading $(0,1)$	Private only	Both $(1,1)$	Divergence eliminated
Benefits	Both $(1,1)$	No effect	Both $(1,1)$	Alignment preserved
Neutral	Neither $(0,0)$	No effect	Neither $(0,0)$	Alignment preserved
Neutral	Neither $(0,0)$	Private and public	Both $(1,1)$	Alignment preserved
Neutral	Both $(1,1)$	No effect	Both $(1,1)$	Alignment preserved

Notes: The table presents possible transitions when the personal belief pathway is operative, $\delta_{B,i} > 0$. The social incentive pathway may be inactive, $\delta_{S,i} = 0$, or operate jointly with the personal belief pathway, $\delta_{S,i} > 0$. Both changes are restricted to be favorable, ruling out individual-level backlash. Changes in perceived ingroup norms may reduce social costs or increase existing social benefits but may not transform social costs into social benefits; in the neutral case, the social cost wedge is assumed to remain zero. Under these restrictions, a change in perceived ingroup norms may affect which possible transition occurs without adding transitions beyond those shown. When the social environment is neutral, the private and public thresholds coincide, so only private-and-public or null effects are possible and the Private-leading and Public-leading initial conditions do not arise.

A.3 From Individual Transitions to Population-Level Effects

Empirical treatment effects aggregate transitions among individuals in different initial conditions and at different distances from the thresholds. An individual-level pattern therefore carries over to the population only when the transitions producing it are sufficiently prevalent. The analysis considers this aggregation under social costs.

For binary responses and favorable treatment-induced transitions, let $\pi_{Priv,C}$, $\pi_{Pub,C}$, and $\pi_{Both,C}$ denote the shares of the social-cost population experiencing private-only, public-only, and joint effects, respectively. The remaining respondents experience no effect. The population-level effects are therefore

$$ATE_C^{Priv} = \pi_{Priv,C} + \pi_{Both,C}, \qquad ATE_C^{Pub} = \pi_{Pub,C} + \pi_{Both,C},$$

and their difference is

$$ATE_C^{Pub} - ATE_C^{Priv} = \pi_{Pub,C} - \pi_{Priv,C}.$$

These expressions rely on the no-backlash restriction introduced above. Because treatment cannot move individuals in an unfavorable direction, no offsetting negative transitions enter either effect, and each population-level effect is a sum of favorable transition shares.

Joint transitions contribute equally to the private and public effects and therefore cancel from their difference. Under social costs, private-only effects arise when individuals move from *Neither* to *Private-leading*, whereas public-only effects arise when individuals move from *Private-leading* to *Both*. These asymmetric transitions offset one another in the public–private difference, which is negative when the first is more prevalent than the second.

The main population-level pattern of interest requires private-only transitions to outnumber public-only transitions, a condition that also ensures that some individuals cross the private threshold. Under these conditions,

$$ATE_C^{Priv} > 0, \qquad ATE_C^{Pub} \geq 0, \qquad ATE_C^{Pub} - ATE_C^{Priv} < 0.$$

This pattern is consistent with private-only transitions from *Neither* to *Private-leading* driving the asymmetry, but does not imply that they are the only transitions occurring. Joint transitions may contribute to both effects, while public-only transitions may partially offset private-only transitions. A negative public–private difference thus identifies the relative prevalence of the two asymmetric patterns, not the full distribution of individual transitions. Estimating the private and public effects separately is necessary to establish whether the asymmetry accompanies favorable private movement and how much favorable public movement also occurs.

A.4 Mapping the Framework to the Hypotheses and Empirical Tests

The hypotheses were preregistered before the complete framework was developed. The framework therefore does not generate the preregistered hypotheses after the fact. Rather, it clarifies the individual transitions and population-level conditions under which the anticipated treatment-effect pattern should emerge, as well as the additional assumptions required to test it using observed higher- and lower-cost groups.

Table A.4.1 summarizes the main preregistered and exploratory public–private hypotheses and the corresponding exploratory conditional predictions (see Appendix G.3 for the complete set of hypotheses). Table A.4.2 organizes the preregistered and exploratory hypotheses by sample and estimand. This table shows how the empirical strategy—which employs the logic of a triple difference in the effects of contact on private versus public outcomes for those with higher versus lower social costs—tests these hypotheses.

H4 predicts a positive private effect, H5 predicts a null or small positive public effect, and H6 predicts a larger private than public effect:

$$ATE^{Priv} > 0, \qquad ATE^{Pub} \geq 0, \qquad ATE^{Pub} - ATE^{Priv} < 0.$$

H6 is the main hypothesis about public–private asymmetry; H4 and H5 identify the component effects needed to interpret that difference as favorable private change accompanied by smaller public change.

A.4.1 Predicted Public and Private Effects Among Higher Cost Individuals

H4–H6 were motivated by socially costly expression, and the preregistration repeatedly stated that the predicted pattern should hold primarily among those facing high social costs (see Appendix L). Because the hypotheses themselves were not formally conditioned on social costs, however, H4–H6 remain pooled tests. In formulating them, I implicitly treated respondents as varying in the magnitude of the social costs they faced and did not consider that favorable public expression might instead carry social benefits for some. This led to the expectation that the public–private asymmetry would hold across the sample, though more strongly among respondents facing higher costs, and thus to the choice of pooled hypotheses.

The complete framework clarifies the limits of that expectation. Even under social costs, the population-level pattern depends on the relative prevalence of private-only and public-only transitions. Moreover, once social benefits are possible, respondents for whom favorable public expression is rewarded may exhibit a weaker or reversed pattern. The pooled prediction may therefore fail even when the focal pattern is strong among respondents facing high costs. Hypotheses $H4_{HC}$–$H6_{HC}$ make these conditional predictions explicit, with $H6_{HC}$ as the main public–private test. They do not replace H4–H6, which are evaluated as written, but refine the scope conditions of the original argument. The conditional tests are therefore exploratory in formal status but follow directly from the preregistered theoretical mechanism.

Table A.4.1: Pre-Registered and Exploratory Public–Private Hypotheses

PAP	Paper	Predicted Effect of Parasocial Contact
Public–Private Hypotheses		
H5	**H6**	Smaller effect on public than on private outcomes.
H3	H4	Positive effect on private outcomes.
H4	H5	Null or small positive effect on public outcomes.
Main Social-Cost Hypotheses: Higher Social Costs (Exploratory)		
	$\mathbf{H6}_{HC}$	Smaller effect on public than on private outcomes for those with higher social costs.
	$\mathrm{H4}_{HC}$	Positive effect on private outcomes for those with higher social costs.
	$\mathrm{H5}_{HC}$	Null or small positive effect on public outcomes for those with higher social costs.
Higher vs. Lower Cost Comparisons (Exploratory)		
	$\mathbf{H6}_{SC}$	Greater public–private asymmetry for those with higher vs. lower social costs.
	$\mathrm{H5}_{SC}$	Smaller effect on public outcomes for those with higher vs. lower social costs.

The table reports the main preregistered public–private hypotheses and their exploratory conditional extensions. HC subscripts denote exploratory hypotheses conditioned on the higher-cost group, and SC subscripts denote exploratory hypotheses concerning heterogeneity by social costs.

Appendix D describes the pretreatment measures used to classify respondents as facing relatively higher or lower social costs. The higher-cost group provides the closest empirical analogue to the framework's social-cost environment and is therefore the primary group for evaluating the conditional public–private asymmetry. The measures nevertheless approximate rather than identify the social cost versus neutral and social benefit wedge: they order respondents by relative social costs but do not perfectly establish the sign of the wedge, locate individuals relative to the thresholds, or reveal their initial conditions.

Testing the public–private asymmetry (H4–H6 and $\mathrm{H4}_{HC}$–$\mathrm{H6}_{HC}$) within the high cost population requires cross-domain comparability: the outcomes must hold constant the underlying response and its personal valuation and nonsocial costs while varying its visibility. The virtual enumerator experiment described in Appendix B.4 provides the strongest approximation of this condition. It holds the attitude measure constant and randomly assigns its visibility, α_d, by eliciting responses either privately or before an ingroup audience. The behavior analysis instead compares substantively matched but distinct private and public actions that may differ in personal valuation and nonsocial costs. It therefore provides a secondary external validity check rather than an equally strong test of public–private asymmetry. Appendix F describes how these measures are constructed and Appendix G.3 further describes how this data is used.

A.4.2 The Lower Cost Group and Higher–Lower Hypotheses

Dividing respondents into higher- and lower-cost groups necessarily creates a parallel lower-cost group. The lower-cost group includes a larger share of individuals in neutral environments or environments where favorable public expression carries social benefits. I use the lower-cost group as a benchmark for the higher-cost group, but I do not test specific hypotheses within it because the original study preregistered no such hypotheses and because the lower-cost category pools a more heterogeneous set of social environments. Its role is instead to establish whether the predicted public–private pattern is in fact more pronounced for those facing higher social costs.

As such, the framework supports two additional cross-group hypotheses. Among respondents facing lower social costs, contact may have positive effects on private outcomes, public outcomes, or both. Because this group includes respondents facing low costs, neutral incentives, and social benefits, however, the framework does not yield a clear prediction about whether the private or public effect should be larger. The public–private difference within the lower-cost group is therefore indeterminate. Nevertheless, holding other determinants of initial conditions and treatment responsiveness comparable, lower social costs make the public threshold easier to cross relative to the private threshold. The public–private difference should therefore be less negative among lower- than among higher-cost respondents. Equivalently, the higher-minus-lower

difference in the public–private gap should be negative, producing H6$_{SC}$. Correspondingly, and holding the same determinants comparable, the more demanding public threshold faced by higher-cost respondents should yield fewer public threshold crossings, so the public effect of contact should be smaller among higher- than among lower-cost respondents, producing H5$_{SC}$.

The framework does not generate an unconditional prediction for whether contact has a larger private effect among respondents facing higher rather than lower social costs (H4$_{SC}$). The framework distinguishes the two cost groups only by the social-cost wedge. Because that wedge enters public but not private utility, the framework predicts a null higher–lower difference in private effects when the groups are otherwise comparable. In practice, however, higher- and lower-cost respondents likely differ in other ways that may shape treatment effects, including the personal value of a favorable response (B), receptiveness to the treatment itself (δ_B), and effort costs (C_E). Importantly, nothing in the framework requires the empirical groups to be otherwise identical. When they differ in ways other than the social-cost wedge, less inferential weight should be placed on higher–lower comparisons because these comparisons may partly reflect (uncontrolled for) differences between the groups.

The higher–lower difference in private effects therefore provides a diagnostic of cost-group comparability. A substantial difference in private effects is informative against comparability: when it appears, differences in public effects could reflect not only social costs but also other differences between the groups. A small difference is consistent with comparability but does not establish it because offsetting differences in B, δ_B, and C_E can produce similar private effects. H6$_{SC}$ is less vulnerable to this problem than H5$_{SC}$ because differencing the private and public effects removes group differences that shift both effects by the same amount. It does not eliminate the problem entirely, however, because group differences that affect the two domains differently cannot be ruled out. Because categorization to the higher- and lower-cost groups is observational, H5$_{SC}$ and H6$_{SC}$ are interpreted as secondary evidence of treatment-effect heterogeneity associated with social costs, not as estimates of the causal effect of social costs.

Table A.4.2: Predicted Treatment Effect Patterns by Social Cost Group

Sample	**Private effect**	**Public effect**	**Public − private**
Full sample	> 0 (H4)	≈ 0 or small > 0 (H5)	< 0 (H6)
Higher costs	> 0 (H4$_{HC}$)	≥ 0 (H5$_{HC}$)	< 0 **(H6$_{HC}$)**
Lower costs (benchmark)	≥ 0	≥ 0	Indeterminate
Higher − lower	=0 if comparable, indeterminate otherwise (H4$_{SC}$)	< 0 (H5$_{SC}$)	< 0 (H6$_{SC}$)

Notes: The full-sample row reports the preregistered H4–H6 predictions. The higher cost (HC) row reports the main exploratory hypotheses. The lower cost estimates are benchmark expectations without hypothesis labels. SC subscripts denote higher-minus-lower social-cost contrasts. The private higher-minus-lower contrast (H4$_{SC}$) equals zero if the groups are otherwise comparable and differ only in $\sigma_i S_i$; the framework does not require this condition, and no unconditional directional prediction is stated. The conditions required to interpret the SC contrasts are discussed in Appendix G.3.

A.4.3 Ordered and Continuous Outcomes

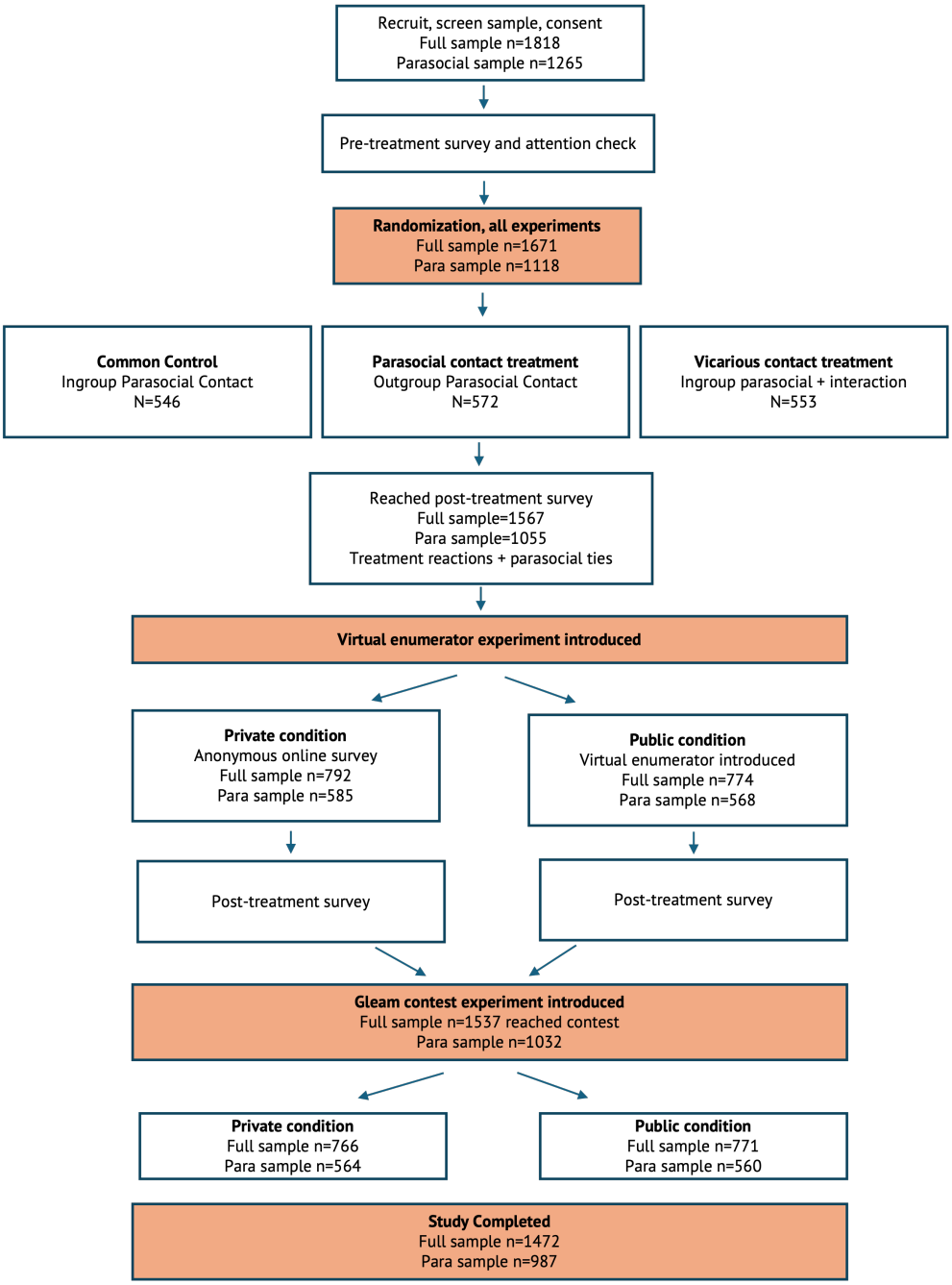
Finally, although the framework represents responses as binary threshold crossings, the empirical analysis also includes ordered and continuous outcomes. Appendix F explains how the threshold logic extends to these outcomes, and Appendix G.3 provides details on how the comparisons in Table A.4.2 are formally estimated with the given data.

B Research Design

B.1 Overview

This appendix provides detail on the research design and empirical strategy, summarized in this appendix. In particular, it summarizes how the sample was recruited, the main experiments—including a contact experiment, a virtual enumerator experiment, and a contest experiment—and data collection. Additionally, it contains details on the randomization procedure and on research ethics. The research design is summarized in Figure B.1.1 and each component is discussed in depth below.

Figure B.1.1: Research Design Overview



B.2 Sample Recruitment and Pre-Treatment Survey

As described in Section 4.1, a sample of White Americans initially were recruited through CloudResearch Connect in 12 waves between October 9–27, 2023. Eligibility was restricted to White, non-Hispanic U.S. citizens aged 18–50 with no prior exposure to *Married at First Sight*. Soft quotas within each wave aimed to balance the sample by gender and baseline outgroup attitudes.[2] Implementing the study in waves allowed us to vary the order of the available actions in the behavioral outcome measure to reduce ordering effects (see below).

To make the study resemble marketing research and minimize demand effects, participants were informed that The Media Research Project was conducting the study to understand how viewers responded to television content and characters. The consent script stated that the study would take 60 minutes and include 20–30 minutes of reality television content.

After providing informed consent, participants completed a pretreatment survey containing measures used as control variables, for balance checks, to describe the sample, and to construct the social-cost measures. Summary statistics for all pretreatment measures appear in Appendix C, balance checks in Appendix E.1, and details on the construction of the social-cost measures in Section D. A link to the complete survey instrument is available in Appendix M.

At the end of the pretreatment survey, respondents completed an attention check. Those who passed were randomly assigned to treatment conditions across the experimental arms. Appendix B.6 describes how the randomization was implemented.

B.3 The Parasocial Contact Experiment

The parasocial contact experiment was implemented following the pre-treatment survey and attention check. Section 3.2 describes the parasocial contact experiment, which uses content from the reality television show *Married at First Sight* (*MAFS*). The show provides a useful setting for the experiment because it follows Black and White couples through parallel storylines involving emotionally engaging and familiar relationship challenges. This format can foster viewer attachment and identification while presenting counter-stereotypical portrayals of Black Americans as emotionally intelligent, committed, and relationship-oriented (Ramasubramanian, 2011, 2015).

The parallel storylines also enable comparable treatment and control conditions. Treatment participants followed a successful Black couple, while control participants followed a matched White couple from the same season. Holding the content largely constant while varying the couple's race addresses a core challenge in parasocial contact research: establishing a credible counterfactual (Banas, Bessarabova and Massey, 2020).[3] The selected content also minimized depictions of interactions between ingroup and outgroup characters, allowing the treatment to isolate parasocial contact from vicarious contact as much as possible.[4]

The broader design included three experimental arms: a common control arm featuring ingroup parasocial contact, a parasocial contact treatment arm featuring outgroup parasocial contact, and a vicarious contact treatment arm featuring ingroup parasocial contact alongside depictions of cross-group friendship. As discussed in Section 4.3, respondents assigned to the vicarious contact arm are retained in the estimation sample for precision, but its effects are not analyzed here. The arm was preregistered as a separate experiment and

[2] To measure baseline outgroup attitudes, CloudResearch fielded the following question to platform members several days before the study: "Thinking about the kinds of people you would like to have living in your neighborhood, how much would you like or dislike having more neighbors who are [Black/White/Asian/Latino]?" Respondents who answered "like a lot" or "like somewhat" were coded as less prejudiced; those who answered "dislike a lot," "dislike somewhat," or "neutral" were coded as more prejudiced.

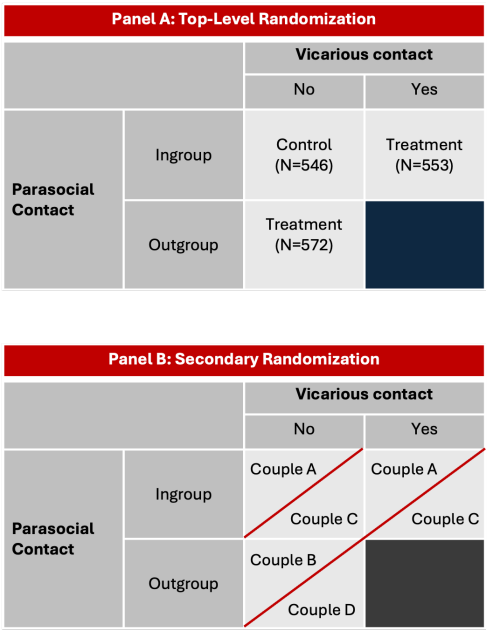
[3] Prior studies have used no-contact controls (e.g., Schiappa, Gregg and Hewes, 2005) or compared content from different shows (e.g., Murrar and Brauer, 2018).

[4] At the time of the study, *MAFS* had not featured interracial couples. Such content would blur the distinction between parasocial and vicarious contact.

produced no detectable effects.

Conditional on their initial assignment, participants were assigned with equal probability to one of two couples (Figure 1). Control participants viewed either ingroup Couple A or C, while parasocial-contact participants viewed either outgroup Couple B or D. Participants assigned to Couple B were further randomized between versions that did or did not include brief references to "Black love."[5]

Figure B.3.1: Contact Experiment Design



B.4 The Virtual Enumerator Experiment and Post-Treatment Survey

Following the contact experiment, all participants completed a post-treatment survey. The first questions assessed reactions to the content and whether it succeeded in producing parasocial ties; results are reported in Appendix E.3. The virtual enumerator experiment was then introduced, before respondents completed measures of the main attitudinal outcomes and mechanisms. Summary statistics for all outcome measures appear in Appendix F.

As described in Section 4.2.1, respondents were assigned with equal probability to either the *private* or *public* condition. In the private condition, respondents completed the measures anonymously. In the public condition, respondents were told that their responses would be observed by a virtual ingroup enumerator and were randomly assigned to one of eight enumerators who varied in gender and age, as shown in Figure B.4.1. The enumerator was introduced using an image and chat bubbles (Figure B.4.2), and the image remained visible throughout the rest of the post-treatment survey.

Section 4.2.1 also explains how the virtual enumerator experiment tests whether contact effects vary when attitudes are reported to an ingroup audience rather than privately. This audience manipulation is distinct from experimenter demand, which refers to respondents changing their answers in response to what they infer the researchers expect (Mummolo and Peterson, 2019). The public condition instead varies whether those answers are observed by an identified ingroup audience, the theoretically relevant social audience in the framework. Both conditions were administered within the same study context. Appendix E.2 shows that assignment to the public condition did not affect respondents' ability to identify the study's purpose

[5]This comparison was preregistered for a separate study and is not analyzed here.

and discusses additional checks related to experimenter demand.

Figure B.4.1: Enumerator Images

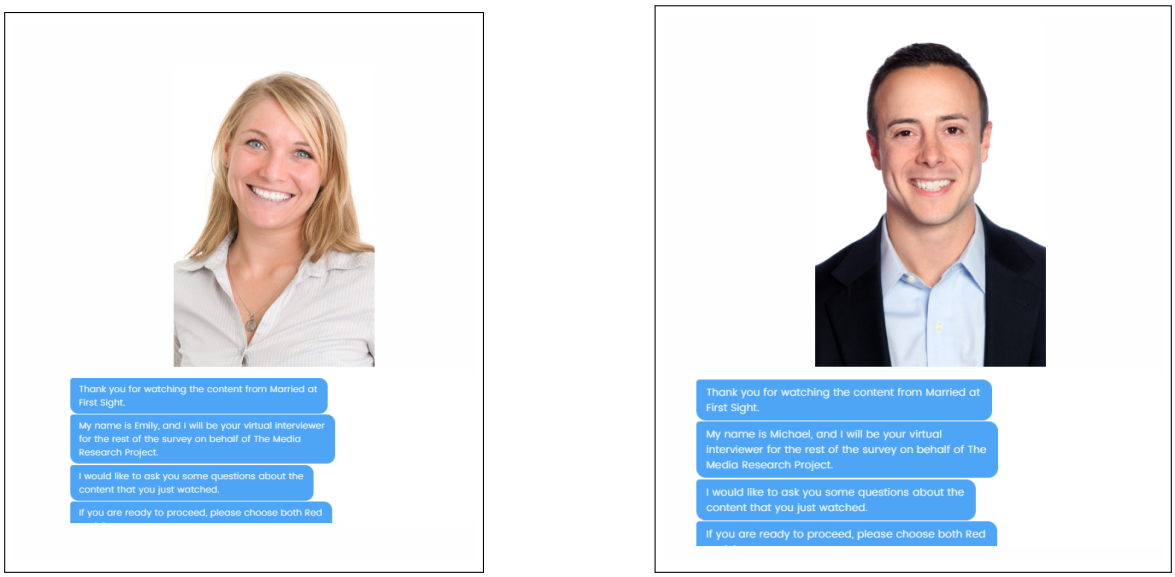


Figure B.4.2: Examples of Virtual Ingroup Enumerator Introductions

In terms of the framework developed in Appendix A, the virtual enumerator experiment randomizes α_d. It is approximately zero in the anonymous private condition and positive in the public condition. The main benefit of the design is that it varies visibility while holding other outcome features, including personal value, treatment responsiveness, and nonsocial effort costs, fixed. It therefore approximates domain comparability as closely as possible.

Treatment checks reported in Appendix E.2 confirm that respondents were aware that they were reporting to an ingroup enumerator.

B.5 The Contest Experiment

The study concluded with an incentivized behavioral measure of willingness to take private and public action. The experiment was embedded at the end of the post-treatment survey and framed as a contest to win a $500 Amazon gift card.[6] The contest was implemented through Gleam.io, a marketing platform that allows entrants to complete actions in exchange for additional entries that increase their chances of winning. Each contest included eight possible actions worth one additional entry each, and Gleam automatically verified each action before awarding the entry.

To test the effects of contact on private and public behavior, participants were randomly assigned to one of two versions of the contest: one containing only private actions, such as reading an article, and one containing only public actions, such as sharing an article on social media.[7] In each version, participants could complete up to eight actions, four of which were related to racial justice and four of which were not. The non-race-related actions allowed participants to increase their chances of winning without taking race-related actions. Table B.5.1 lists all actions, and Figure B.5.1 shows the private and public versions of the contest.

The two primary outcome measures are (1) whether participants entered the contest to which they were assigned and (2) the difference between the number of race-related and non-race-related actions they completed. The first measure captures the decision to participate after viewing the available actions: participants who were more willing to complete those actions should have been more likely to enter. The second measure captures willingness to select race-related actions relative to other opportunities to obtain additional entries. To assess the effects of contact on private and public behavior, both outcomes are analyzed separately within the private and public contest conditions.

Table B.5.1: Gleam Contest Measures

	Race-related action	**Not race-related action**
Private contest	1. Browse Netflix's BLM collection 2. Watch the trailer for "13th" 3. Read about documentaries on race 4. Sign a private petition on racial justice in policing	1. Browse Netflix's general collection 2. Watch the trailer for "Making a Murderer" 3. Read about documentaries on bizarre stories 4. Sign a private petition on a humanitarian airlift to Ukraine
Public contest	1. Share Netflix's BLM collection on social media 2. Share the trailer for "13th" 3. Share an article about documentaries on race 4. Sign a public petition on racial justice in policing	1. Share Netflix's general collection on social media 2. Share the trailer for "Making a Murderer" 3. Share an article about documentaries on bizarre stories 4. Sign a public petition on a humanitarian airlift to Ukraine

Notes: Table lists the private and public race and non-race related actions available in the Gleam contest.

B.6 Randomization Details

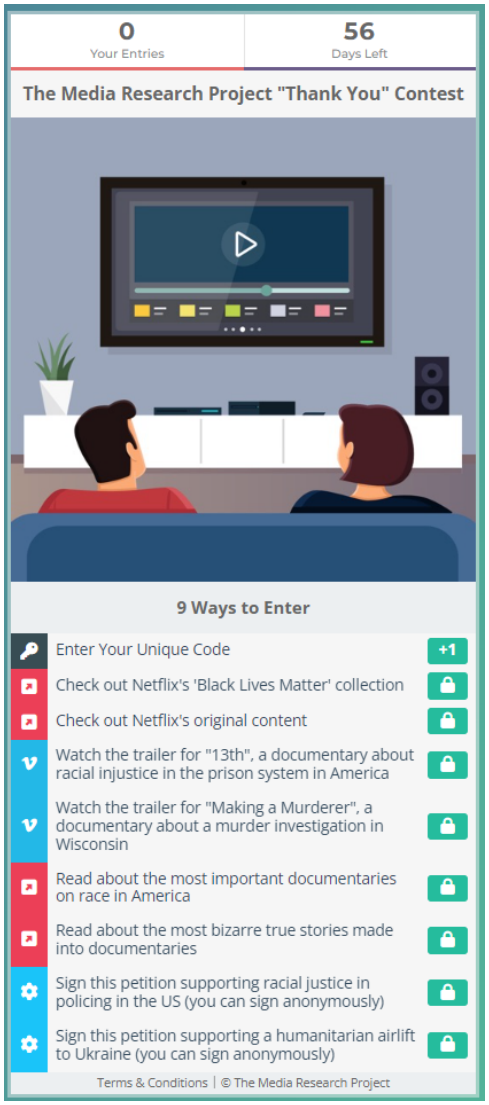
The preceding sections describe the assignments within each experimental component. This section explains how those assignments were combined and implemented. Respondents who passed the pretreatment attention check were assigned across the media contact, virtual enumerator, and Gleam contest experiments using 48 unique randomization profiles, shown in Table B.6.1.[8] Assignments to the three experiments were mutually orthogonal.

Participants were assigned to profiles using block randomization within survey waves. As each wave began, incoming participants were assigned to a randomly drawn profile without replacement. Once all profiles had been used, a new cycle began. This approach produced a near-even distribution of treatment combinations

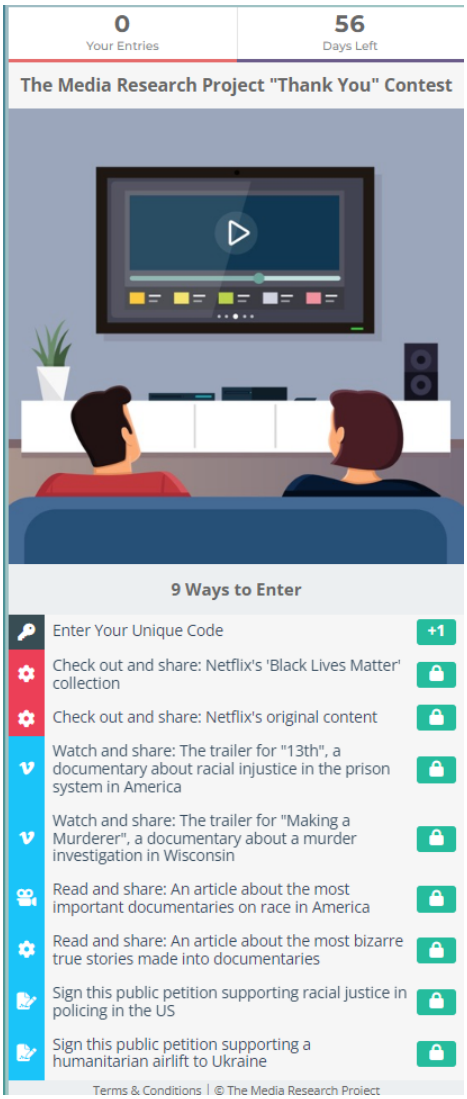
[6] Winners were drawn after data collection was completed and all entries were verified. One winner was selected from each of the private and public contests.

[7] Separate private and public contests were used rather than a single contest combining both types of actions to avoid an unwieldy list and mechanical dependencies, such as reading and then sharing the same article.

[8] The design included additional random assignments related to post-treatment vignette experiments, as specified in the pre-analysis plan. These assignments were conducted orthogonally and are controlled for in the analysis.



(a) Private Contest



(b) Public Contest

Figure B.5.1: Gleam public and private contest images

within each wave and ensured that treatment effects could be estimated within waves.

The order of the actions in the Gleam contest was also randomly varied across survey waves to reduce ordering effects. The study was fielded in 12 waves between October 9–27, 2023, targeting 96–144 participants per wave. These targets allowed each of the 48 profiles to be assigned to 2–3 participants per wave while permitting the order of the Gleam actions to be re-randomized over the course of implementation. Survey launch times were also randomized across waves so that participants in different U.S. time zones had an opportunity to enter before the quota was filled. For example, a wave beginning at 9 a.m. ET could otherwise fill before many potential West Coast participants began the study.

Table B.6.1: Randomization Profiles

Resp No.	Treat	Para	Vic	Treat	Enum	Gleam
1	Control	0	0	Couple A	0	0
2	Control	0	0	Couple A	0	1
3	Control	0	0	Couple A	0	0
4	Control	0	0	Couple A	0	1
5	Control	0	0	Couple A	1	0
6	Control	0	0	Couple A	1	1
7	Control	0	0	Couple A	1	0
8	Control	0	0	Couple A	1	1
9	Control	0	0	Couple C	0	0
10	Control	0	0	Couple C	0	1
11	Control	0	0	Couple C	0	0
12	Control	0	0	Couple C	0	1
13	Control	0	0	Couple C	1	0
14	Control	0	0	Couple C	1	1
15	Control	0	0	Couple C	1	0
16	Control	0	0	Couple C	1	1
17	Vic	0	1	Couple A	0	0
18	Vic	0	1	Couple A	0	1
19	Vic	0	1	Couple A	0	0
20	Vic	0	1	Couple A	0	1
21	Vic	0	1	Couple A	1	0
22	Vic	0	1	Couple A	1	1
23	Vic	0	1	Couple A	1	0
24	Vic	0	1	Couple A	1	1
25	Vic	0	1	Couple C	0	0
26	Vic	0	1	Couple C	0	1
27	Vic	0	1	Couple C	0	0
28	Vic	0	1	Couple C	0	1
29	Vic	0	1	Couple C	1	0
30	Vic	0	1	Couple C	1	1
31	Vic	0	1	Couple C	1	0
32	Vic	0	1	Couple C	1	1
33	Para	1	0	Couple B (Black love)	0	0
34	Para	1	0	Couple B (Black love)	0	1
35	Para	1	0	Couple B (Black love)	1	0
36	Para	1	0	Couple B (Black love)	1	1
37	Para	1	0	Couple B (No Bl love)	0	0
38	Para	1	0	Couple B (No Bl love)	0	1
39	Para	1	0	Couple B (No Bl love)	1	0
40	Para	1	0	Couple B (No Bl love)	1	1
41	Para	1	0	Couple D	0	0
42	Para	1	0	Couple D	0	1
43	Para	1	0	Couple D	0	0
44	Para	1	0	Couple D	0	1
45	Para	1	0	Couple D	1	0
46	Para	1	0	Couple D	1	1
47	Para	1	0	Couple D	1	0
48	Para	1	0	Couple D	1	1

Notes: Table shows the 48 randomization profiles created to obtain optimal balance across the contact, enumerator, and gleam contest experiments. Respondents were randomly assigned to one profile without replacement; once all profiles were exhausted, a new block was initiated.

B.7 Research Ethics

This study involved human subjects and received approval from the Institutional Review Board (IRB) at [REDACTED]. Participants provided informed consent and were recruited through CloudResearch following their standard protocols. Participation was voluntary, and respondents could exit the survey at any point.

The study posed minimal risk. Treatments consisted of exposure to publicly available media content and survey questions concerning social and political attitudes, including attitudes toward racial groups. No deception was used beyond withholding specific hypotheses, and no personally identifying information was collected or retained. Data were analyzed in aggregate, and respondents were compensated according to standard platform practices.

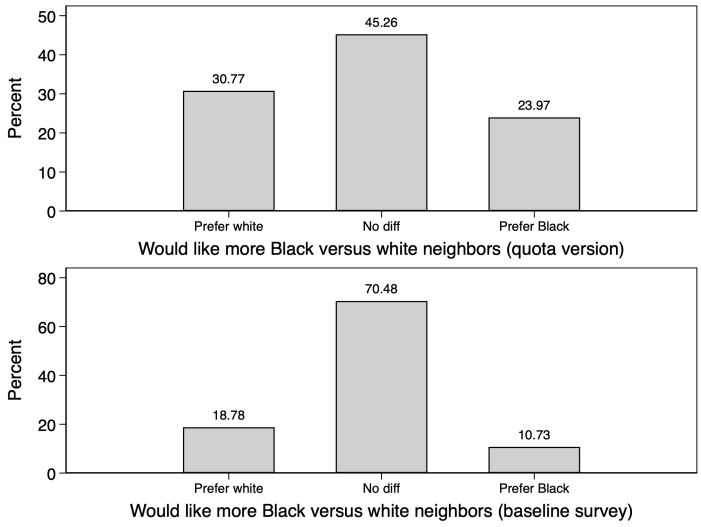
C Summary and Descriptive Statistics: Pre-Treatment Variables

C.1 Summary Statistics

Table C.1.1 presents summary statistics for all pretreatment variables, including the control variables and their components. The control set includes all variables in Sections I (Demographics) and II (Regions), with Pacific as the base category; the social media usage index in Section III; the outgroup shows index in Section IV; liking more White neighbors and the disliking-outgroups indices in Section V; share Black, share White, and ingroup sanctioning in Section VI; and the two ingroup norms indices in Section VII. Section VIII presents descriptive statistics for the variables used to construct the high-social-cost measures described in Appendix D.

Figure C.1.1 shows the distribution of baseline personal prejudice. Respondents reported how much they would like or dislike having more Black neighbors and more White neighbors, and the measure captures the difference between these responses. The items were administered at two points: several days before the study, when they were used to stratify the sample, and again during the pretreatment survey. In the pretreatment survey, most respondents expressed no difference between Black and White neighbors, but a meaningful minority—approximately 19 percent—expressed a stronger preference for White neighbors. Thus, although relative preference for White neighbors was not the dominant position in the sample, it remained sufficiently prevalent to produce meaningful variation in baseline prejudice (see also Figure D.4.1).

Figure C.1.1: Personal Prejudice



Notes: The figure shows the distribution of the difference in respondents' preferences for having more Black versus White neighbors. The top panel reports the measure collected for quota sampling, and the bottom panel reports the measure collected during the pretreatment survey.

Table C.1.1: Summary Statistics Pre-Treatment Variables

	Min	Max	Mean	SD	N	Notes on scales
I. Demographics						
Age	18	49	34.86	7.37	1118	Numeric
Female	0	1	0.49	0.50	1118	1=female, 0 otherwise
Education completed	1	6	4.39	1.07	1118	1=primary school, 6=graduate school
Income	1	26	18.98	4.91	1118	1=¡$1,000, 26=¿$170,000
Urban	0	1	0.25	0.44	1118	1=urban, 0=rural, semi-urban
Party ID	0	1	0.23	0.42	1118	1=Republican, 0 otherwise
Share of network married	1	5	3.00	1.14	1118	1=almost none, 5=almost all
Like reality tv	1	5	3.35	1.25	1118	1=dislike a lot, 5=like a lot
II. Region						
Southeast	0	1	0.06	0.24	1118	1=living in region, 0 otherwise
New England	0	1	0.04	0.20	1118	1=living in region, 0 otherwise
Northeast	0	1	0.16	0.37	1118	1=living in region, 0 otherwise
Midwest East	0	1	0.18	0.38	1118	1=living in region, 0 otherwise
Midwest West	0	1	0.07	0.25	1118	1=living in region, 0 otherwise
South Atlantic	0	1	0.22	0.41	1118	1=living in region, 0 otherwise
South Central	0	1	0.09	0.29	1118	1=living in region, 0 otherwise
Mountain	0	1	0.07	0.25	1118	1=living in region, 0 otherwise
Pacific	0	1	0.11	0.31	1118	1=living in region, 0 otherwise
III. Social Media						
Index social media	-2.21	1.51	0.00	1.00	1118	ICW index
Facebook use	1	7	4.86	2.25	1118	1=never, 7=multiple times a day
Twitter/X use	1	7	4.10	2.37	1118	1=never, 7=multiple times a day
Instagram use	1	7	4.73	2.23	1118	1=never, 7=multiple times a day
IV. Previous media contact						
Index outgroup shows	-0.61	3.43	0.04	1.00	1118	ICW index
Black-ish	-1	1	-0.51	0.85	1118	1=yes, 0=maybe/don't know, -1=no
Dear White People	-1	1	-0.83	0.54	1118	1=yes, 0=maybe/don't know, -1=no
Abbott Elementary	-1	1	-0.57	0.82	1118	1=yes, 0=maybe/don't know, -1=no
V. Personal prejudice						
Like more white neighbors	1	5	3.78	0.89	1118	1=dislike a lot, 5=like a lot
Dislike more Black neighbors (index)	-2.02	3.02	0.01	1.00	1118	ICW index
Quota version	1	5	2.34	0.98	1118	1=like a lot, 5=dislike a lot
Baseline survey version	1	5	2.41	0.98	1118	1=like a lot, 5=dislike a lot
Like White–Black difference	-1	1	0.08	0.54	1118	1=like White more, 0=both equally, -1=like Black more
Dislike other outgroup neighbors (index)	-1.53	3.16	-0.01	1.01	1118	ICW index
Asian neighbors	1	5	2.22	0.87	1118	1=like a lot, 5=dislike a lot
Latino neighbors	1	5	2.39	0.94	1118	1=like a lot, 5=dislike a lot
VI. Social Network						
Share Black	1	5	1.74	0.73	1118	1=almost none, 5=almost all
Share White	1	5	4.09	0.84	1118	1=almost none, 5=almost all
Homogeneous ingroup network*	0	1	0.78	0.41	1118	1=more than half of network is white, 0 otherwise
Ingroup sanctioning	1	4	2.48	0.81	1118	1=very comfortable, 4=very uncomfortable
Fear of ingroup sanctioning*	0	1	0.51	0.50	1118	1=very uncomfortable, 0 otherwise
VII. Ingroup norms support prejudice						
Index norms: want Black neighbors	-2.37	2.34	0.02	0.02	1118	ICW index
Ingroup general	1	5	3.25	0.85	1118	1=almost all want more Black neighbors, 5=almost none
White family members	1	5	3.07	1.13	1118	1=almost all want more Black neighbors, 5=almost none
White friends	1	5	2.67	1.08	1118	1=almost all want more Black neighbors, 5=almost none
Index norms: racial jokes acceptable	-1.27	2.74	0.08	1.05	1118	ICW index
Ingroup general	1	5	2.23	1.17	1118	1=very unacceptable, 5=very acceptable
White family members	1	5	2.52	1.46	1118	1=very unacceptable, 5=very acceptable
White friends	1	5	2.31	1.45	1118	1=very unacceptable, 5=very acceptable
VIII. Higher Social Cost Indicators						
Prejudiced norms (factor)	0	1	0.49	0.50	1118	1=prej norms (above median, factor score), 0 otherwise
Prejudiced norms (factor) + homogeneous network	0	1	0.40	0.49	1118	1=prej norms + homogeneous network, 0 otherwise
Prejudiced norms (factor) + fear of sanctioning	0	1	0.40	0.49	1118	1=prej norms + fear, 0, otherwise

Notes: The table reports summary statistics (minimum, maximum, mean, standard deviation, and sample size) for all pre-treatment variables in the parasocial contact sample. Variables are presented in their original scales, though some have been recoded for consistency in directionality. Asterisks (*) indicate variables used to construct the main high social cost moderator.

D Construction of Social Cost Measures

The social cost environment plays a key role in the framework and the analysis, given the focus on asymmetric effects under high social costs (Appendix A). The pre-analysis plan specified three pre-treatment variables for measuring social costs: perceived ingroup norms, network homogeneity, and fear of social sanctioning. The framework subsequently formalizes the distinction these items draw. Perceived ingroup norms capture the *direction* of social pressure, S_i—whether norms discourage or encourage favorable attitudes and behavior toward Black Americans—while network homogeneity and fear of social sanctioning capture the *weight* respondents place on that pressure, σ_i. Together these form the social cost wedge $\omega_i = \sigma_i S_i$. The measures described below therefore precede the framework rather than having been constructed to fit it.

The pre-analysis plan preregistered heterogeneity by social cost for the vicarious contact treatment but not for the parasocial contact treatment (see Appendix L). This initial focus reflected the expectation that vicarious contact—which was designed to shift perceived ingroup norms—would matter most for individuals facing higher social costs. The plan nevertheless stated more generally that the predicted asymmetric effects should emerge primarily among those facing higher social costs (Appendix A.4 and Appendix L). I therefore extend the social cost measure preregistered for the vicarious contact experiment to the parasocial contact experiment, using the same items and classification. This extension is principled but exploratory: it follows the theoretical logic set out in the pre-analysis plan, while the application to parasocial contact was not itself preregistered.

The remainder of this section proceeds as follows. I first describe the items measuring the direction of perceived social pressure and the continuous and binary indices constructed from them, then the two proxies for the weight respondents place on that pressure. I then explain how these components are combined into the primary binary indicator of higher versus lower social costs, describe the alternative binary measures and the continuous measure used to assess robustness, and report the correlates of the resulting classification.

D.1 Social Pressure Direction

To assess perceived ingroup norms, respondents completed two batteries of three questions each. Figure D.1.1 displays the response distributions for all six items. Throughout, all items and indices are coded so that higher values indicate more prejudiced perceived ingroup norms.

The first battery measured perceived support for Black residential integration by asking: "If you had to guess, how many [White people in this country/your White family members/your White friends] would like having more Black neighbors?" Responses were recorded on a five-point scale ranging from 1 = almost all to 5 = almost none.

The second battery measured perceived acceptance of everyday expressions of prejudice by asking: "If you had to guess, how acceptable would it be, if at all, to make a racially insensitive comment or joke about Black people to [most White people/your White friends/your White family members]?" Responses were recorded on a five-point scale ranging from 1 = very unacceptable to 5 = very acceptable.

The two batteries capture related but distinct dimensions of perceived ingroup norms. The neighborhood items measure perceived opposition to residential integration, whereas the joke items measure perceived acceptance of more commonplace expressions of prejudice. Both reflect the broader normative climate surrounding race but may capture different manifestations of that climate. I therefore construct measures that combine information across both batteries or summarize each battery separately, along with binary versions used to construct the high social cost indicators.

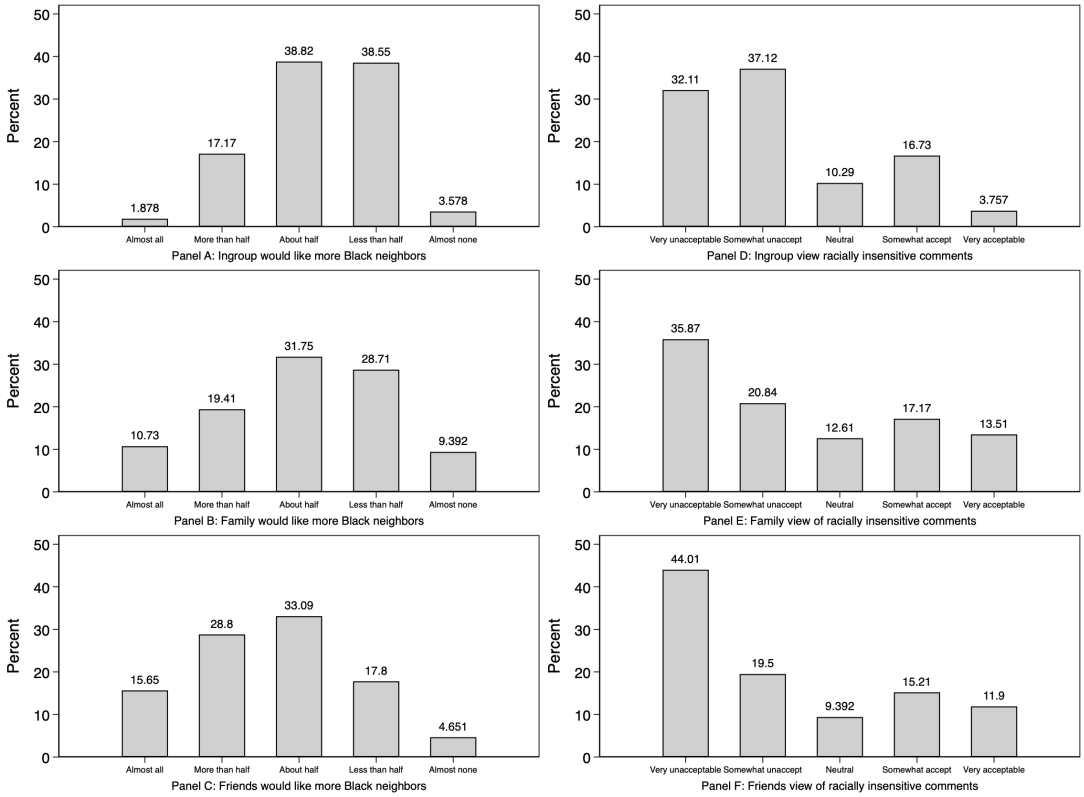
I first construct a continuous measure that is a factor score estimated from all six items. This measure treats the responses as imperfect indicators of a broader underlying dimension of perceived ingroup norms and weights each item according to its relationship with that dimension. The first factor has an eigenvalue of 1.92, compared with 0.70 for the second factor, supporting a one-factor solution. I standardize the resulting

factor score, with higher values indicating more prejudiced perceived ingroup norms. Figure D.1.2 displays its distribution. I also construct a binary version by dichotomizing the standardized score at its median, with values above the median indicating relatively more prejudiced perceived norms.

As alternatives, I construct two inverse covariance weighted (ICW) indices corresponding to the neighborhood and joke batteries (Anderson, 2008). Within each battery, the ICW index combines the three indicators while accounting for their covariance. Because correlations across the two batteries are lower than correlations among items within each battery, I construct separate indices rather than combining all six items into a single ICW index. To construct a binary measure, I code respondents as 1 if they score above the median on either the neighborhood or joke index and 0 otherwise. This "either" measure allows for the possibility that relatively prejudiced perceived norms in either domain may generate adverse social pressure, even when norms in the other domain are less unfavorable. Because it takes the union of two above-median groups, it necessarily classifies more than half of respondents as perceiving relatively prejudiced norms and therefore rests on a different base rate than the factor-score split.

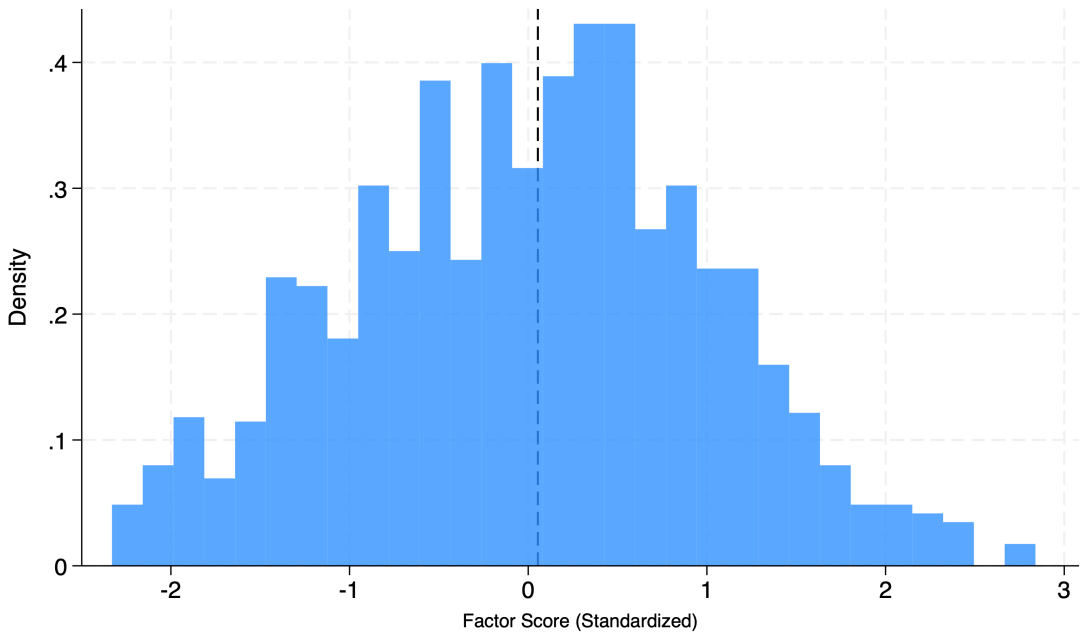
Because these binary measures use distribution-based thresholds, they capture relatively more versus less prejudiced perceived norms rather than distinguishing explicitly among costly, beneficial, and neutral normative environments.

Figure D.1.1: Perceived Social Norms: Item Response Distributions



Notes: Figure displays the response distributions for six measures of perceived ingroup norms. The first three items (left column) capture the perceived share of ingroup members—general public, family, and friends—who would like more Black neighbors. The remaining three items (right column) capture perceptions of how acceptable racially insensitive jokes are to the same ingroup categories.

Figure D.1.2: Perceived Social Norms: Distribution of the Factor Score



Notes: Histogram showing the distribution of standardized factor scores from a one-factor solution based on six items measuring prejudiced ingroup norms. Dashed line indicates the median of the distribution (.054).

D.2 Social Pressure Weight

I use two measures as proxies for the weight respondents place on social pressure. The first is network homogeneity, which captures the extent to which respondents are embedded in social environments dominated by members of their racial ingroup and therefore may be more exposed to conformity pressures arising from perceived ingroup norms (McPherson, Smith-Lovin and Cook, 2001). Respondents were asked: "Thinking about the people you spend time with in your daily life (such as your friends, family, neighbors, co-workers, etc.), how many would you say are White?" Responses were recorded on a five-point scale ranging from 1 = almost none to 5 = almost all. Respondents who report that more than half or almost all of their social contacts are White are classified as having homogeneous social networks, a criterion satisfied by approximately 78 percent of the sample.

The second is a direct measure of self-reported fear of social sanctioning, which captures respondents' discomfort with acting contrary to the preferences of people they know. Respondents were asked: "Thinking about the people you spend time with in your daily life, how comfortable or uncomfortable would you be doing something that they would not approve of?" Responses were recorded on a four-point scale ranging from 1 = very comfortable to 4 = not comfortable at all. I dichotomize the measure between the two comfortable and two uncomfortable response categories. Respondents who report being either "not too comfortable" or "not comfortable at all" are coded as having high fear of social sanctioning. Approximately 49 percent of the sample falls into this category.

D.3 Composite Measures of Social Costs

I construct binary measures that distinguish respondents facing higher versus lower social costs. The pre-analysis plan proposed combining all three pre-treatment variables into a single composite, but that composite classifies too few respondents to support stable comparisons across experimental conditions. The measures

used here therefore pair perceived ingroup norms with one of the two weight variables, or use perceived ingroup norms alone. All are constructed from pre-treatment items, and the primary measure was selected on the theoretical and sample size grounds described below, without reference to treatment effects.

The primary measure for both the virtual enumerator experiment and the behavioral intentions analysis combines the factor score with network homogeneity. This choice reflects theoretical, consistency, and sample size considerations. First, network homogeneity captures respondents' embeddedness in the ingroup whose perceived norms shape social pressure. In the virtual enumerator experiment, although the enumerator does not belong to respondents' existing social networks, perceived ingroup norms likely carry greater weight for respondents who are more strongly embedded in that ingroup. For the behavioral intentions analysis, network homogeneity is relevant because the outcomes concern actions that may be visible to respondents' broader social networks. Second, using the same measure across analyses provides a consistent basis for evaluating whether social costs moderate treatment effects on both public expression and public behavior. Third, this measure classifies approximately 41 percent of respondents as facing higher social costs, compared with approximately 23 percent for the alternative measure based on fear of social sanctioning. The larger subgroup supports more precise and stable comparisons across experimental conditions.

Appendices I.3 and J.3 assess the robustness of the results to alternative binary measures, including those that capture the direction of social pressure alone and those that combine direction with fear of social sanctioning. Results are presented for the following six binary measures:

1. `hsc_norms_factor_netw`: perceived ingroup norms above the median of the factor score and a homogeneous social network (main measure).
2. `hsc_norms_factor_fear`: perceived ingroup norms above the median of the factor score and high fear of social sanctioning (robustness).
3. `hsc_norms_factor`: perceived ingroup norms above the median of the factor score (robustness).
4. `hsc_norms_icw_netw`: perceived ingroup norms above the median of either ICW index and a homogeneous social network (robustness).
5. `hsc_norms_icw_fear`: perceived ingroup norms above the median of either ICW index and high fear of social sanctioning (robustness).
6. `hsc_norms_icw`: perceived ingroup norms above the median of either ICW index (robustness).

For the attitudes analysis, I additionally assess robustness using the continuous measure of perceived ingroup norms, which captures variation in the direction of social pressure without incorporating a measure of its weight. This better approximates the benefit–neutral–cost range of perceived ingroup norms and also shows that results are not driven by a particular cutoff in the binary coding.

D.4 Correlates of Higher Social Costs

Table D.4.1 reports correlates of the primary social cost measures and their underlying components. Respondents with more prejudiced baseline attitudes toward Black neighbors report more prejudiced perceived ingroup norms and are more likely to be classified as facing higher social costs. The results also support distinguishing between the direction and weight of social pressure: respondents who report greater fear of social sanctioning do not necessarily perceive more prejudiced ingroup norms, while respondents with more homogeneous White networks tend to perceive more prejudiced norms. Gender likewise exhibits different relationships with these components, with women reporting less prejudiced perceived norms but greater fear of social sanctioning. These patterns suggest that the composite measures capture related but distinct dimensions of respondents' social environments.

Figure D.4.1 shows the relationship between respondents' baseline personal prejudice toward Black Americans and their perceptions of prejudiced ingroup norms. Although the two measures are positively correlated ($r = 0.48$), respondents vary substantially in their combinations of personal prejudice and perceived ingroup

Table D.4.1: Correlates of Social Costs

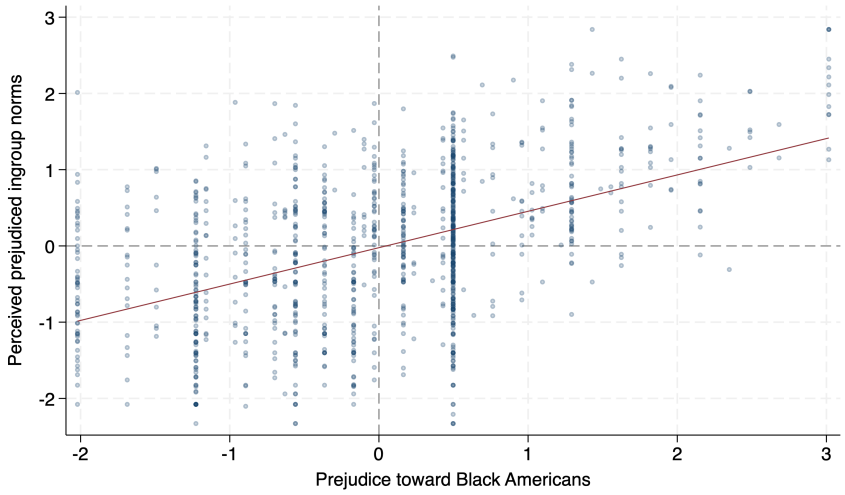
	Composite Measures		Components		
	Norms + network	Norms + fear	Norms	Network	Fear
Age	0.00	0.00	0.00	0.01***	-0.01
	(0.00)	(0.00)	(0.00)	(0.00)	(0.00)
Female	-0.07**	0.03	-0.16***	0.10*	0.15***
	(0.03)	(0.03)	(0.05)	(0.05)	(0.05)
Education	0.01	-0.01	0.03	0.00	-0.01
	(0.01)	(0.01)	(0.02)	(0.02)	(0.02)
Income	-0.01*	0.00	-0.01	0.00	0.00
	(0.00)	(0.00)	(0.01)	(0.01)	(0.01)
Urban	-0.04	0.07**	0.07	-0.23***	0.07
	(0.03)	(0.03)	(0.05)	(0.06)	(0.06)
Republican	0.06*	0.02	0.03	0.01	0.06
	(0.04)	(0.03)	(0.06)	(0.06)	(0.06)
Prefer White neighbors	-0.02	-0.02	-0.04	0.00	-0.05*
	(0.02)	(0.01)	(0.03)	(0.03)	(0.03)
Prejudice index	0.17***	0.10***	0.41***	0.09***	0.03
	(0.01)	(0.01)	(0.02)	(0.03)	(0.03)
Fear of ingroup sanctioning	-0.02		-0.10***	0.07**	
	(0.02)		(0.03)	(0.03)	
Network: Share white		0.03**	0.07***		0.06**
		(0.01)	(0.03)		(0.03)
Prejudiced norms: neighbors				0.08***	-0.03
				(0.03)	(0.03)
Prejudiced norms: jokes				0.00	-0.10***
				(0.03)	(0.03)
Constant	0.59	0.29	0.23	3.59	2.45
	(0.12)	(0.11)	(0.22)	(0.21)	(0.21)
N	1118	1118	1118	1118	1118

Notes: $^{*}p < 0.10$, $^{**}p < 0.05$, $^{***}p < 0.01$. The table reports associations between respondent characteristics and measures of social costs and their components. Column 1 reports correlates of the primary binary measure of higher social costs, which combines above-median perceived prejudiced ingroup norms with a predominantly White social network. Column 2 reports correlates of the alternative measure combining above-median perceived prejudiced ingroup norms with high fear of social sanctioning. Columns 3–5 report correlates of the underlying components: the standardized factor score for perceived ingroup norms, the four-point fear of social sanctioning scale, and the five-point measure of White network composition. Each specification excludes variables used to construct its dependent variable. All models include randomization-stratum fixed effects. Robust standard errors are reported in parentheses.

norms. Some respondents report both relatively high personal prejudice and relatively prejudiced ingroup norms, a configuration in which favorable attitudes toward Black Americans may initially be absent in both private and public. Others report relatively low personal prejudice despite perceiving relatively prejudiced ingroup norms, a configuration in which favorable private attitudes may not be expressed publicly. The presence of both configurations helps illustrate how an intervention that changes personal attitudes without changing perceived ingroup norms could move respondents from the former to the latter, producing more favorable private attitudes without corresponding changes in public expression.

The correlation nevertheless means that the higher-cost group contains a disproportionate share of respondents with more prejudiced baseline attitudes, so higher–lower comparisons should not be read as isolating the effect of perceived norms alone. Reassuringly, the focal results remain substantively similar after controlling for pretreatment prejudice and allowing the effects of contact across private and public conditions to vary with it (Appendix I.3).

Figure D.4.1: Correlation Between Personal Prejudice and Perceived Ingroup Norms



Notes: The figure shows the relationship between baseline personal prejudice toward Black Americans and perceived prejudiced ingroup norms. Each point represents a respondent. Dashed lines indicate the sample means for each measure, dividing respondents into four groups according to whether their personal prejudice and perceived prejudiced ingroup norms are above or below average. While the red line indicates a positive association between the two variables, the correlation is moderate ($r = 0.48$).

E Checks

E.1 Balance

Table E.1.1 reports balance tests based on assignment to treatment in the contact experiment, virtual enumerator experiment, and contest experiment.

Overall, baseline characteristics were well balanced across experimental conditions. The table evaluates 34 pretreatment variables across four treatment comparisons, for a total of 136 tests. Only five differences were statistically significant at the 5 percent level, fewer than would be expected by chance. Three occurred in the parasocial contact comparison: respondents assigned to parasocial contact were somewhat less likely to be female, perceived more prejudiced ingroup norms regarding racial jokes, and reported less fear of ingroup sanctioning. The latter two differences are relevant because these variables enter the social-cost measures, although they move in opposite directions. Because all assignments were made simultaneously and orthogonally before treatment, the pattern is consistent with chance imbalance. The main analyses include pretreatment controls, and results are similar without covariate adjustment. The conditional analyses also make the focal private–public comparisons among respondents classified as facing similar levels of social costs, reducing their sensitivity to chance imbalance in the components of the social-cost measures.

Table E.1.1: Balance

		PANEL A: CONTACT EXPERIMENTS									PANEL B: PUBLIC-PRIVATE EXPERIMENTS									
		Control	Parasocial Contact				Vicarious Contact				Control	Enum (Public)				Control	Gleam (Public)			
		mean	b	se	p	Count	b	se	p	Count	mean	b	se	p	Count	Mean	b	se	p	Count
1	Age	34.92	-0.11	0.44	0.804	0	0.49	0.45	0.269	0	34.90	-0.07	0.44	0.877	0	34.98	-0.23	0.44	0.598	0
2	Female (%)	0.53	-0.07	0.03	0.024	1	-0.02	0.03	0.412	0	0.47	0.04	0.03	0.187	0	0.49	0.01	0.03	0.854	0
3	Education	4.38	0.02	0.06	0.702	0	0.08	0.07	0.228	0	4.42	-0.05	0.06	0.461	0	4.40	-0.02	0.06	0.713	0
4	Income	19.08	-0.18	0.29	0.545	0	-0.04	0.30	0.896	0	19.09	-0.22	0.29	0.458	0	19.17	-0.38	0.29	0.197	0
5	Urban	0.26	0.00	0.03	0.859	0	0.01	0.03	0.671	0	0.24	0.04	0.03	0.160	0	0.27	-0.04	0.03	0.130	0
6	Party ID (% Republication)	0.23	0.01	0.03	0.775	0	-0.01	0.03	0.586	0	0.24	-0.02	0.03	0.419	0	0.24	-0.02	0.03	0.401	0
7	Share of network married	3.01	-0.01	0.07	0.832	0	0.07	0.07	0.299	0	3.03	-0.05	0.07	0.434	0	3.03	-0.07	0.07	0.321	0
8	Like reality tv	3.33	0.05	0.07	0.528	0	0.04	0.07	0.595	0	3.33	0.06	0.07	0.459	0	3.37	-0.02	0.07	0.740	0
9	Region: Southeast	0.07	-0.02	0.01	0.238	0	-0.01	0.01	0.667	0	0.06	0.00	0.01	0.841	0	0.06	0.00	0.01	0.853	0
10	Region: New England	0.04	0.01	0.01	0.463	0	0.00	0.01	0.968	0	0.04	0.00	0.01	0.970	0	0.04	0.00	0.01	0.960	0
11	Region: Northeast	0.17	-0.01	0.02	0.551	0	-0.03	0.02	0.211	0	0.18	-0.03	0.02	0.259	0	0.18	-0.03	0.02	0.248	0
12	Region: Midwest East	0.19	-0.02	0.02	0.288	0	-0.01	0.02	0.677	0	0.17	0.03	0.02	0.215	0	0.19	-0.03	0.02	0.201	0
13	Region: Midwest West	0.06	0.02	0.02	0.274	0	0.01	0.02	0.369	0	0.08	-0.02	0.01	0.261	0	0.08	-0.02	0.01	0.255	0
14	Region: Southatlantic	0.22	0.00	0.02	0.851	0	0.02	0.03	0.541	0	0.23	-0.03	0.02	0.184	0	0.19	0.05	0.02	0.047	1
15	Region: South Central	0.09	0.00	0.02	0.869	0	-0.01	0.02	0.627	0	0.09	0.01	0.02	0.576	0	0.10	-0.02	0.02	0.269	0
16	Region: Mountain	0.06	0.02	0.01	0.262	0	0.01	0.01	0.499	0	0.07	0.00	0.01	0.866	0	0.05	0.03	0.01	0.080	0
17	Region: Pacific	0.10	0.01	0.02	0.440	0	0.01	0.02	0.489	0	0.09	0.04	0.02	0.052	0	0.10	0.01	0.02	0.438	0
18	Index social media	0.00	0.06	0.06	0.327	0	0.04	0.06	0.512	0	0.03	0.01	0.06	0.926	0	0.05	-0.03	0.06	0.558	0
19	Like ingroup	3.76	0.03	0.05	0.530	0	0.08	0.05	0.144	0	3.80	-0.04	0.05	0.427	0	3.79	-0.02	0.05	0.657	0
20	Index like outgroup (excl. Black)	0.00	0.01	0.06	0.869	0	0.09	0.06	0.148	0	0.03	-0.04	0.06	0.479	0	0.01	-0.02	0.06	0.767	0
21	Index like outgroup (Black)	0.00	-0.03	0.06	0.663	0	0.03	0.06	0.593	0	-0.01	-0.01	0.06	0.858	0	-0.06	0.09	0.06	0.129	0
22	Share of network Black	1.74	-0.01	0.04	0.833	0	0.02	0.04	0.654	0	1.70	0.07	0.04	0.121	0	1.74	0.00	0.04	0.938	0
23	Share of network white	4.09	0.01	0.05	0.871	0	-0.01	0.05	0.869	0	4.14	-0.09	0.05	0.068	0	4.09	0.01	0.05	0.825	0
24	Index norms neighbors	0.00	0.03	0.06	0.614	0	0.08	0.06	0.173	0	0.00	0.04	0.06	0.533	0	0.05	-0.07	0.06	0.245	0
25	Index norms joke	0.00	0.16	0.06	0.009	1	0.05	0.06	0.426	0	0.06	0.05	0.06	0.442	0	0.07	0.03	0.06	0.641	0
26	Fear ingroup sanctioning	2.55	-0.15	0.05	0.002	1	-0.12	0.05	0.017	1	2.47	0.02	0.05	0.629	0	2.47	0.01	0.05	0.820	0
27	Index previous media contact	0.00	0.07	0.06	0.232	0	-0.03	0.06	0.556	0	0.00	0.07	0.06	0.269	0	-0.02	0.11	0.06	0.072	0
28	Enumerator treatment	0.50	0.01	0.03	0.861	0	-0.01	0.03	0.834	0	0.00	.	.	.	.	0.50	0.00	0.03	0.999	0
29	Male enum	0.23	0.03	0.03	0.222	0	0.04	0.03	0.165	0	0.00	.	.	.	.	0.26	-0.03	0.03	0.343	0
30	Fem enum	0.25	-0.02	0.03	0.380	0	-0.04	0.03	0.105	0	0.00	.	.	.	.	0.22	0.02	0.03	0.450	0
31	Black Mathises (survey experiment)	0.33	-0.03	0.03	0.367	0	0.03	0.03	0.277	0	0.30	0.03	0.03	0.364	0	0.30	0.02	0.03	0.460	0
32	Black police officer (survey experiment)	0.34	0.00	0.03	0.944	0	-0.02	0.03	0.524	0	0.37	-0.05	0.03	0.101	0	0.36	-0.03	0.03	0.238	0
33	Black mayor (survey experiment)	0.50	0.02	0.03	0.605	0	0.02	0.03	0.616	0	0.50	0.01	0.03	0.730	0	0.47	0.06	0.03	0.051	0
34	Gleam public contest	0.50	0.00	0.03	0.909	0	0.01	0.03	0.788	0	0.50	0.00	0.03	0.999	0	.	.	.	.	.
						3				1					0					1
					%	0.09			%	0.03				%	0.00				%	0.03

Notes: Table presents balance tests for the parasocial and vicarious contact experiments (Panel A) as well as for the enumerator and Gleam contest experiments (Panel B). Control mean refers to the raw mean in the respective control groups. Coefficients, standard errors, and p-values are obtained from separate regressions of each covariates on the two treatment indicators in the full sample. The count column is 1 if the p-value is $\leq .05$.

E.2 Attrition and Attention

Table E.2.1 reports tests of the contact and virtual enumerator experiments on a battery of attrition, attention, and related implementation measures. The models interact the parasocial contact treatment indicator with assignment to the public condition in the enumerator experiment. The control mean captures the mean among respondents assigned to the contact control and private enumerator conditions. The Contact coefficient estimates the effect of parasocial contact in the private condition, while the Public coefficient estimates the effect of the public condition among contact-control respondents. The interaction estimates whether the effect of parasocial contact differs between the private and public conditions.

Attrition: Attrition was limited overall. Among respondents assigned to both the contact control and private enumerator conditions, 94 percent reached the contest (column 1) and 89 percent reached the end of the study (column 2). Neither assignment to parasocial contact, assignment to the public condition, nor their interaction was statistically significant, providing no evidence of differential attrition across the four focal experimental conditions.

Attention: Attention was high across all groups. Participants in the parasocial contact control group answered an average of 6.46 of seven attention checks correctly (column 3). Neither assignment to parasocial contact, assignment to the public condition, nor their interaction was statistically significant, providing no evidence of differential attrition across the four focal experimental conditions.

Virtual enumerator checks: Columns 7–8 present checks on the virtual enumerator experiment. Responses are coded as correct when respondents in the public condition correctly identified the enumerator's sex or race and when respondents in the private condition correctly reported that no enumerator was present. Among respondents assigned to the contact control and private enumerator conditions, 94 and 96 percent answered the sex and race questions correctly, respectively. Assignment to the public condition increased correct responses to the sex question by four percentage points and had no detectable effect on correct responses to the race question, which were already high. Column 9 provides the clearest check of the manipulation: only 4 percent of respondents in the private condition reported having a White enumerator, compared with approximately 96 percent in the public condition. Neither parasocial contact nor the contact-by-enumerator interaction affected these responses. These results indicate that respondents were aware of whether an enumerator was present and, when present, that the enumerator was White.

Duration: Participants assigned to the contact control and private enumerator conditions took approximately 54 minutes to complete the study with outliers included (column 10) and 50 minutes with outliers excluded (column 11). In the private condition, assignment to parasocial contact increased duration by approximately 12 and 8 minutes, respectively. The contact-by-enumerator interaction was not statistically significant when outliers were included. Differences in duration were not accompanied by differential attrition or attention across the four experimental conditions.

Survey purpose: Despite efforts to mask the study's intent, 64 percent of respondents assigned to the contact control and private enumerator conditions correctly identified that it concerned racial attitudes. Assignment to parasocial contact increased this share by 15 percentage points (column 12). Analyses by outgroup couple, presented in Appendix H.3, suggest that awareness of the study's purpose does not account for the estimated treatment effects on main outcomes and mechanisms. Contact increased awareness among respondents assigned to both outgroup couples, whereas the effects on subsequent attitudinal outcomes were driven by only one couple. This divergence is inconsistent with a simple demand-effects explanation in which recognizing the study's purpose produces more favorable responses. Assignment to the public condition had no detectable effect on identifying the study's purpose. The contact-by-enumerator interaction also had no detectable effect on identifying the study's purpose or on respondents' overall reactions to the study.

Overall reaction: About 79 percent of participants assigned to the contact control and private enumerator conditions responded positively to the study (column 13), while 20 percent rated the experience as somewhat or very negative. Overall reactions did not differ across the four experimental conditions.

Table E.2.1: Attrition and Attention Checks

	Attrition		Attention Treatment/Survey				Attention Enumerator Exp.			Duration		Reaction	
	Reached contest (1)	Reached end (2)	Total correct (3)	Survey correct (4)	Treat correct (5)	Final chk correct (6)	Sex correct (7)	Race correct (8)	Enum white (9)	Minutes w/outlier (10)	Minutes no outliers (11)	Survey purpose (12)	Reaction overall (13)
Contact	0.00	0.03	-0.05	-0.01	-0.03	-0.01	0.02	0.01	-0.02	12.28*	6.16***	0.15***	0.00
	(0.02)	(0.03)	(0.07)	(0.01)	(0.06)	(0.01)	(0.02)	(0.02)	(0.02)	(7.08)	(1.91)	(0.04)	(0.04)
	0.991	0.185	0.465	0.545	0.660	0.413	0.288	0.385	0.292	0.083	0.001	0.000	0.992
Public	-0.03	-0.03	-0.03	0.00	-0.02	0.00	0.04**	0.00	0.92***	-4.37	-1.39	-0.05	0.02
	(0.02)	(0.03)	(0.07)	(0.01)	(0.06)	(0.01)	(0.02)	(0.02)	(0.02)	(3.99)	(1.51)	(0.04)	(0.04)
	0.168	0.322	0.665	0.738	0.732	0.953	0.025	0.987	0.000	0.273	0.357	0.242	0.555
Contact*Public	0.00	-0.03	-0.04	0.00	-0.07	0.01	-0.02	0.00	0.03	-4.72	-0.26	0.07	0.01
	(0.03)	(0.04)	(0.10)	(0.02)	(0.09)	(0.01)	(0.02)	(0.02)	(0.02)	(7.68)	(2.57)	(0.06)	(0.05)
	0.926	0.457	0.677	0.841	0.437	0.304	0.322	0.858	0.234	0.538	0.918	0.213	0.775
Control Mean	0.94	0.89	6.46	2.99	2.47	0.99	0.94	0.96	0.04	53.64	50.04	0.64	0.79
N (full sample)	1671	1671	1539	1545	1558	1539	1539	1538	1538	1671	1664	1538	1537
N (Para)	1118	1118	1034	1038	1048	1034	1034	1033	1033	1118	1113	1033	1032

Notes: Each column reports estimates from a separate OLS regression interacting assignment to parasocial contact with assignment to the public condition in the virtual enumerator experiment. The Contact coefficient estimates the effect of parasocial contact in the private condition. The Public coefficient estimates the effect of the public enumerator condition among respondents assigned to the contact control. Contact × Public estimates the difference in the contact effect between the public and private conditions. The control mean is the mean among respondents assigned to the contact control and private enumerator conditions. All models include randomization-strata fixed effects and corresponding indicators for the vicarious contact treatment and its interaction with the public condition; these coefficients are not reported. Robust standard errors appear in parentheses, with p-values reported beneath them. In columns 7–8, responses are coded as correct when respondents in the public condition correctly identify the enumerator's sex or race and respondents in the private condition correctly report that no enumerator was present. Column 9 indicates whether respondents reported having a White enumerator. Duration excluding outliers is reported in column 11. "N (full sample)" reports the number of observations included in each regression; "N (Para)" reports the number assigned to either parasocial contact or its common control. * $p < .10$, ** $p < .05$, *** $p < .01$.

E.3 Parasocial Contact Treatment Checks

Figure E.3.1 shows reactions to four general measures of engagement with the content: engaged overall, cared about the couple, liked content, and liked couple. Overall, participants reacted with similar positivity to both ingroup and outgroup couples. If anything, they reacted more positively to the outgroup couples.

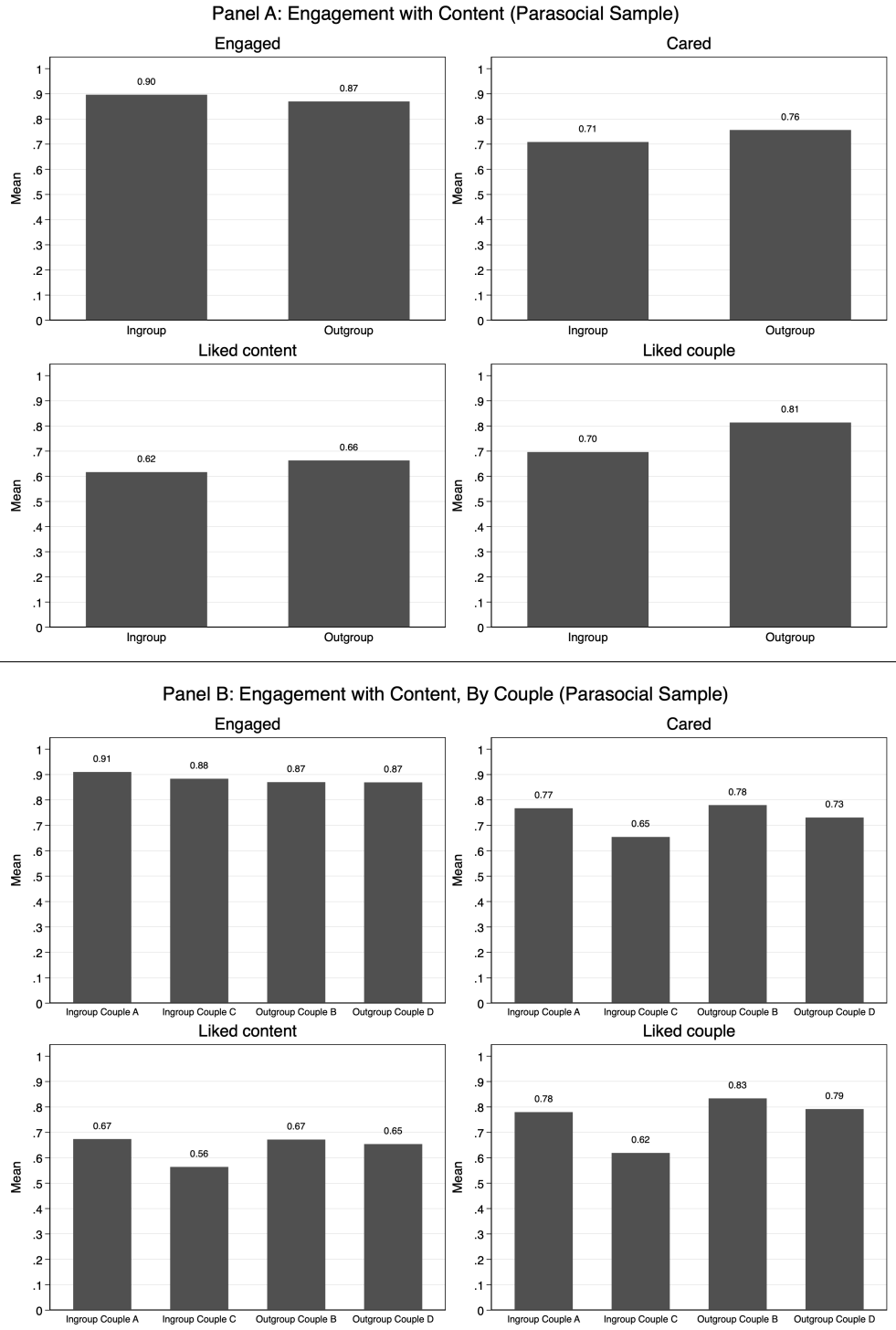
Figure E.3.2 shows responses to a number of questions designed to capture whether the treatment created parasocial ties to the male and female characters. These questions were adapted from standard batteries of questions to measure parasocial relations and ties, as follows:

- To what extent, if at all, do you want to know how [NAME] is doing now that the show is over?
- To what extent, if at all, do you feel that you understand [NAME]?
- How much, if at all, did you relate to [NAME]?
- How much, if at all, would you say that [NAME] shares your values?
- Overall, do you think [NAME] is a likeable or unlikeable person?
- To what extent, if at all, could you see yourself being friends with [NAME]?
- How typical, if at all, is [MALE CHARACTER] of other [white/Black MEN] in this country?

Generally the responses were positive, were similar for ingroup and outgroup characters, and were similar for male and female characters. Open-ended responses reinforce these findings. One viewer noted: "*I really liked how both Amani and Woody [Couple B] went into this situation with an open mind and a willingness to compromise... I see a lot of my own marriage reflected back at me...*" Another praised Greg and Deonna [Couple D]: "*They had many admirable qualities—high achievers, rooted in faith, pleasant personalities, home owners, valued family... They remind me somewhat of myself and my husband.*"

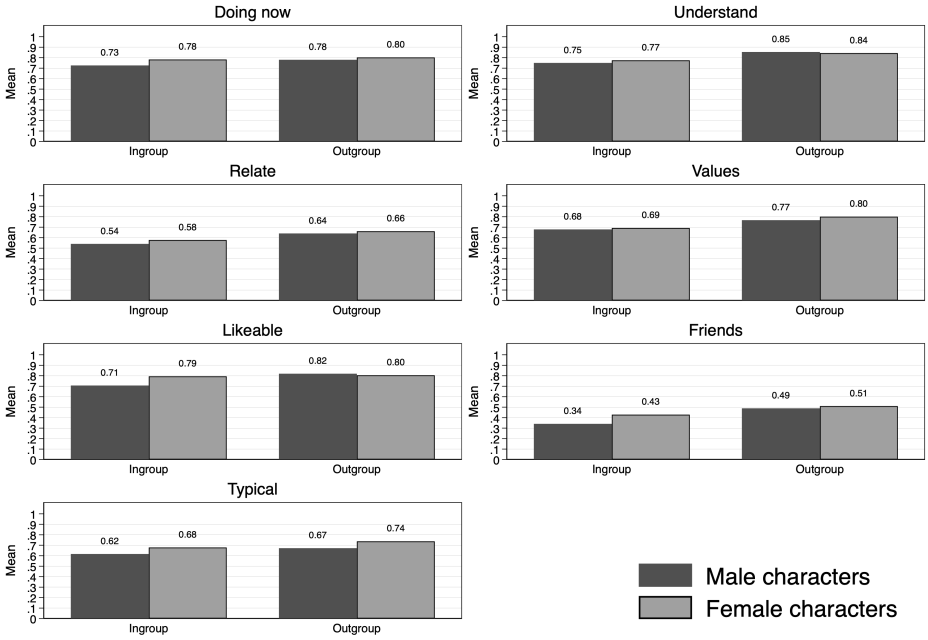
Several respondents referenced race explicitly, revealing that the treatment engaged with and potentially challenged negative stereotypes. One respondent contrasted the portrayal with "*how media presents most Blacks as poor, uneducated or thugs*" while another expressed: *"I like that they are a working-class Black couple... There are so few Black people (at least in the Midwest) who hold down jobs, have long-term commitments, and who make responsible choices."* These comments suggest the treatment provided counter-stereotypical portrayals that surprised respondents holding negative baseline views.

Figure E.3.1: Overall Engagement with Content



Notes: Figure shows participant engagement with the content for those assigned to outgroup and ingroup parasocial contact (Panel A) and by couple (Panel B). Individual figures display results for responses to survey questions measuring overall engagement, whether the respondent cared about the couple, whether they liked the content, and whether they liked the couple. Analysis is restricted to the parasocial contact sample (excludes those assigned to vicarious contact).

Figure E.3.2: Parasocial Relations for Ingroup/Outgroup Male and Female Characters



Notes: Figure shows responses to multiple measures of parasocial relations for the ingroup and outgroup male and female characters. Analysis is restricted to the parasocial contact sample

F Outcome Measures

F.1 Outcome Measures and Summary Statistics

The post-treatment survey included multiple measures of the main outcomes and proposed mechanisms. The complete survey instrument can be found in Appendix M.

All outcome measures were coded so that higher values indicate more favorable attitudes or greater willingness to support racial equality. Items preregistered as capturing the same concept were combined into an inverse-covariance-weighted (ICW) index following Anderson (2008). Index construction uses the standard implementation developed by Cyrus Samii (https://cyrussamii.com/?p=2656); see the replication files.

Several attitudinal measures included parallel evaluations of Black and White Americans. Although the difference (Black-minus-White) rating was preregistered as the primary outcome, the analyses emphasize evaluations of Black Americans because they provide the most direct test of whether contact improved attitudes toward the outgroup (see Appendix L). White-only outcomes and the preregistered Black–White difference measures are also reported in the appendix results to assess whether the results reflect changes in evaluations of Black Americans rather than more general shifts in evaluations of both groups.

F.1.1 Main Attitudinal Outcomes

Table F.1.1 reports summary statistics for the main attitudinal outcomes and measures associated with changes in personal beliefs and perceived social norms.

Warmth. Warmth toward Black and White Americans is measured using feeling thermometers, the desire for more Black or White friends, and support for interracial marriage. The Black index includes three items: the feeling thermometer, desire for more Black friends, and support for interracial marriage. The White and Black-minus-White indices are based on the first two items because the interracial-marriage item did not have a corresponding White component.

Support for social change. Support for social change is measured using a single item: "When it comes to ensuring that Black people have the same rights and opportunities as White people, do you think this country has gone too far, been about right, or not gone far enough?"

F.1.2 Mechanism-Associated Outcomes

Personal beliefs. The survey included multiple outcomes associated with personal belief change. The positive-traits measure captured stereotypes as generalized beliefs about group characteristics. Respondents estimated the percentage of Black and White Americans possessing a strong work ethic, intelligence, family values, and commitment in relationships.[9] The items were combined into indices capturing positive evaluations of Black Americans, positive evaluations of White Americans, and the Black–White difference. The survey also included measures of empathy, perceived similarity, and comfort with intergroup interaction.

Perceived ingroup norms. Perceived ingroup norms were measured after treatment using questions asking: "To what extent, if any, do you think that [White people in this country/your White friends/your White family members] want to have more close friends who are Black?" The items were combined into a single inverse-covariance-weighted index.

[9] A fifth item—"nonaggressive"—is excluded from the main index because it produced a large Black–White difference in the expected direction that was driven primarily by a shift in evaluations of White Americans rather than more favorable evaluations of Black Americans. Including it would therefore inflate the apparent improvement in outgroup stereotypes. Table H.1.1 reports estimates both including and excluding this item and shows robustness to excluding it.

F.1.3 Behavior Outcomes

Table F.1.2 reports summary statistics for the behavior outcomes.

Behavioral intentions. Participants reported how likely they were to engage in eight actions supporting racial equality. Four were explicitly private: voting by secret ballot for a Black candidate who supports addressing racial injustice, voting by secret ballot for a White candidate who supports addressing racial injustice, reading an article, and signing a petition anonymously. Four were explicitly public: sharing an article on social media, confronting a White friend or family member, signing a public petition, and attending a protest. These items were used to construct preregistered indices of private and public behavioral intentions using inverse-covariance weighting.

To improve cross-domain comparability, the analyses also compare two closely matched pairs of private and public actions: reading an article privately versus sharing the same article publicly, and signing a petition anonymously versus signing the petition publicly (Paler, Marshall and Atallah, 2018). A combined matched-pair measure averages intentions to read the article and sign the petition anonymously for the private outcome, and intentions to share the article and sign the petition publicly for the public outcome. I use equally weighted means rather than factor or ICW indices to preserve the one-to-one correspondence between the private and public actions.

Contest behavior. As described in Appendix B.5, the survey concluded with an incentivized behavioral measure that allowed participants to obtain additional contest entries by completing actions. The two primary outcomes are (1) whether participants entered the contest to which they were assigned and (2) the difference between the numbers of race-related and non-race-related actions they completed. The first captures the decision to participate after viewing the available actions. The second captures willingness to select race-related actions relative to other opportunities to obtain additional entries. Both outcomes are analyzed separately within the private and public contest conditions.

F.2 Mapping the Framework onto Ordered and Continuous Outcomes

Appendix A derives individual-level predictions for binary responses because the theoretical object of interest is whether an intervention moves an individual across the relevant response threshold. Many of the outcomes summarized above, however, measure attitudes and behavior using ordered scales or composite indices. These outcomes can be understood as aggregating movement across multiple latent response thresholds rather than a single binary threshold. The binary framework is therefore the special case of a single threshold separating a favorable from an unfavorable response.

Let ΔY_{ik}^{d} indicate whether individual i crosses threshold k in domain $d \in \{Priv, Pub\}$. For an ordered outcome composed of K thresholds,

$$ATE_g^d = E_g\left[\sum_{k=1}^{K} \Delta Y_{ik}^{d}\right]. \tag{F.1}$$

Because individuals may cross multiple thresholds, the public–private difference no longer has a literal interpretation as the difference in the shares of individuals experiencing private-only and public-only transitions. Instead, it captures the aggregate difference in threshold crossings across the two domains. Changes in personal beliefs shift both private and public utility, while changes in perceived social norms additionally shift public utility. A smaller public effect emerges when the resulting domain-specific utility changes produce fewer threshold crossings publicly than privately for enough respondents.

Importantly, the lower public starting point produced by social costs does not by itself guarantee a smaller public effect on a ordered or continuous outcome. The result also depends on where respondents begin relative to the thresholds composing the scale. Private positions near the upper bound may leave little room for favorable private movement, while positions far below the responsive portion of the scale may compress both effects toward zero. The counterpart of the framework's population-level condition is therefore that

Table F.1.1: Summary Statistics: Attitudes

	min	max	mean	sd	N	Notes on scales
PANEL A: MAIN ATTITUDINAL OUTCOMES						
1. Warmth Indices						
Black index	-4.13	1.51	0.02	1.01	1037	ICW index (three items)
White index	-3.30	2.03	0.00	1.02	1037	ICW index (two items)
Difference index	-4.68	4.31	0.01	1.06	1037	ICW index (two items)
Warmth components						
Thermometer						
Black	0	10	7.015	2.188	1037	0=extremely cold, 10=extremely warm
White	0	10	7.031	2.073	1037	0=extremely cold, 10=extremely warm
Black–White difference	-10	9	-0.015	2.422	1037	Black-minus-White ratings
Friends						
Want more Black friends	1	4	2.79	0.69	1037	1=don't want at all, 4=want a lot
Want more White friends	1	4	2.69	0.71	1037	1=don't want at all, 4=want a lot
Black–White difference	-3	3	0.10	0.80	1037	Black-minus-White ratings
Support intermarriage	1	5	4.54	0.90	1037	1=very unwilling, 5=very willing to support
2. Support social change	1	3	2.47	0.73	1037	1=gone too far, 2=about right, 3=not gone far enough
PANEL B: PERSONAL BELIEF MECHANISMS						
3. Positive trait evaluation indices						
Black index (four items)	-3.95	2.28	0.02	1.02	1035	ICW index (four items, excludes 'nonaggressive')
White index (four items)	-3.68	2.77	0.01	1.01	1035	ICW index(four items, excludes 'nonaggressive')
Difference index (four items	-5.23	4.78	0.02	1.04	1034	ICW index (four items, excludes 'nonaggressive')
Black index (five items)	-4.11	2.42	0.01	1.02	1035	ICW index
White index (five items)	-3.62	2.91	-0.01	1.01	1035	ICW index
Difference index (five items)	-5.23	4.89	0.02	1.04	1034	ICW index
Positive trait components						
Hardworking						
Black	0	10	6.41	1.96	1035	0=share: none, 10=share: all
White	0	10	6.36	1.67	1035	0=share: none, 10=share: all
Difference	-8	9	0.04	1.89	1034	Black-minus-White rating
Intelligent						
Black	0	10	6.54	1.89	1035	0=share: none, 10=share: all
White	0	10	6.29	1.80	1035	0=share: none, 10=share: all
Difference	-7	7	0.24	1.62	1034	Black-minus-White rating
Family-oriented						
Black	0	10	6.25	2.20	1035	0=share: none, 10=share: all
White	0	10	6.01	1.98	1035	0=share: none, 10=share: all
Difference	-9	10	0.23	2.30	1034	Black-minus-White rating
Committed						
Black	0	10	6.37	2.05	1035	0=share: none, 10=share: all
White	0	10	6.70	1.78	1035	0=share: none, 10=share: all
Difference	-8	9	-0.33	1.73	1034	Black-minus-White rating
Nonaggressive						
Black	0	10	6.06	2.11	1035	0=share: none, 10=share: all
White	0	10	5.84	2.09	1035	0=share: none, 10=share: all
Difference	-9	9	0.21	2.12	1034	Black-minus-White rating
4. Similarity						
Index	-2.94	2.30	-0.01	1.00	1035	ICW index
Interests/general	1	5	3.07	0.78	1035	1=strongly disagree, 5=strongly agree
Values	1	5	3.50	1.11	1035	1=very different, 5=very similar
5. Empathy						
Index	-3.04	1.73	0.00	0.98	1034	ICW index
Empathy (general)	1	5	3.28	1.03	1034	1=very hard, 5=very easy
Empathy (policing)	1	7	5.25	1.64	1034	1=very hard, 5=very easy
6. Comfort around						
Black strangers	1	5	3.79	1.12	1037	1=very uncomfortable, 5=very comfortable
White strangers	1	5	3.96	1.06	1037	1=very uncomfortable, 5=very comfortable
Difference	-4	3	-0.17	0.72	1037	Black-minus-White rating
PANEL C: SOCIAL NORMS MECHANISMS						
7. Perceived ingroup norms index	-3.04	2.55	0.02	1.01	1035	ICW index
General	1	4	2.63	0.59	1035	1=don't want at all, 4=want a lot
Family	1	4	2.80	0.65	1035	1=don't want at all, 4=want a lot
Friends	1	4	2.46	0.73	1035	1=don't want at all, 4=want a lot

Notes: Table shows summary statistics (min, max, mean, standard deviation, sample size) for the parasocial contact sample for main outcome and mechanism measures.

Table F.1.2: Summary Statistics: Behavior Outcomes

	min	max	mean	sd	N	Notes on scales
PANEL A: BEHAVIORAL INTENTIONS						
1. Private actions index	-2.30	0.98	0.01	0.99	1037	ICW index
Vote Black candidate	1	5	3.73	1.39	1037	1=very unlikely, 5=very likely
Vote White candidate	1	5	3.68	1.34	1037	1=very unlikely, 5=very likely
Read an article	1	5	3.96	1.31	1037	1=very unlikely, 5=very likely
Sign a private petition	1	5	3.77	1.46	1037	1=very unlikely, 5=very likely
2. Public actions index	-1.97	1.39	0.03	0.99	1037	ICW index
Confront friends	1	5	2.99	1.50	1037	1=very unlikely, 5=very likely
Public protest	1	5	3.77	1.31	1037	1=very unlikely, 5=very likely
Share an article	1	5	3.65	1.47	1037	1=very unlikely, 5=very likely
Sign a public petition	1	5	2.86	1.50	1037	1=very unlikely, 5=very likely
PANEL B: ACTUAL BEHAVIOR (GLEAM CONTEST)						
3. Entered private contest	0	1	0.53	0.50	527	1=entered private contest, 0 otherwise
4. Private actions (difference)	-2	3	-0.02	0.34	527	Difference: Private race–nonrace actions
Total race-related actions	0	4	0.94	1.54	527	Total race-related private actions
Netflix BLM	0	1	0.26	0.44	527	1=took action, 0 otherwise
Documentary	0	1	0.25	0.43	527	1=took action, 0 otherwise
Trailer	0	1	0.27	0.44	527	1=took action, 0 otherwise
Petition	0	1	0.17	0.38	527	1=took action, 0 otherwise
Total nonrace-related actions	0	4	0.96	1.54	527	Total nonrace-related private actions
Netflix BLM	0	1	0.26	0.44	527	1=took action, 0 otherwise
Documentary	0	1	0.25	0.44	527	1=took action, 0 otherwise
Trailer	0	1	0.27	0.44	527	1=took action, 0 otherwise
Petition	0	1	0.18	0.39	527	1=took action, 0 otherwise
5. Entered public contest	0	1	0.53	0.50	528	1=entered public contest, 0 otherwise
6. Public actions (difference)	-1	1	0.00	0.14	528	Difference: Public race–nonrace actions
Total race-related public actions	0	4	0.12	0.60	528	Total race-related public actions
Netflix BLM	0	1	0.03	0.16	528	1=took action, 0 otherwise
Documentary	0	1	0.03	0.17	528	1=took action, 0 otherwise
Trailer	0	1	0.04	0.19	528	1=took action, 0 otherwise
Petition	0	1	0.03	0.17	528	1=took action, 0 otherwise
Total non-race related public actions	0	4	0.13	0.61	528	Total nonrace-related public actions
Netflix BLM	0	1	0.03	0.18	528	1=took action, 0 otherwise
Documentary	0	1	0.03	0.16	528	1=took action, 0 otherwise
Trailer	0	1	0.03	0.18	528	1=took action, 0 otherwise
Petition	0	1	0.03	0.17	528	1=took action, 0 otherwise

Notes: The table reports summary statistics (minimum, maximum, mean, standard deviation, and sample size) for the main behavioral outcomes in the parasocial-contact sample. For the Gleam contest outcomes, respondents who were assigned to the contact condition but exited the survey before reaching the contest are coded as having taken no action (0).

enough respondents lie within a responsive portion of the scale privately while the social-cost wedge places their public positions in a less responsive portion.

Using ordered or continuous outcomes also changes the interpretation of a null public effect. When treatment changes personal beliefs, public utility changes even if no respondent crosses the single threshold in the binary representation. Movement may therefore occur across thresholds within an ordered or continuous outcome despite a zero binary treatment effect. The corresponding prediction is consequently one of attenuation rather than a structural zero: public latent utility can shift while producing less movement in the observed public outcome than in the private outcome. If contact also changes perceived social norms in a favorable direction, this change adds to the public utility shift. The asymmetric prediction then requires that the resulting public movement remain smaller than the private movement.

G Hypotheses and Estimation

G.1 Preregistered and Exploratory Hypotheses

Table G.1.1 summarizes all preregistered and exploratory hypotheses. The column labeled *PAP* reports the hypothesis numbers in the original pre-analysis plan, while the column labeled *Paper* reports the updated numbering used in this manuscript. For clarity, I use the paper numbering throughout the discussion below.

Panels A and B present hypotheses about the average effects of contact on the main outcomes and mechanisms. Panel C presents the main pre-registered public–private hypotheses. H4 predicts a positive private effect, H5 predicts a null or small positive public effect, and H6 predicts a smaller public than private effect. H6 is the central hypothesis about asymmetric treatment effects, while H4 and H5 establish whether that asymmetry takes the predicted form of favorable private change accompanied by smaller public change. These hypotheses were preregistered for the pooled sample.

Panels D and E present exploratory hypotheses concerning the predicted asymmetry among respondents facing higher social costs and differences between the higher- and lower-cost groups. These hypotheses and their exploratory status are discussed in detail in Appendix A.4 and Appendix L.

Table G.1.1: Preregistered and Exploratory Hypotheses

PAP	Paper	Predicted Effect of Parasocial Contact
Panel A. Average Treatment Effect Hypothesis		
H1	**H1**	Increased warmth (tolerance) and support for social change.
Panel B. Mechanism Hypotheses		
M1	**H2a**	Improved trait evaluations (reduced negative stereotyping).
M2/M5	**H2b**	Increased perceived similarity and shared values (reduced value threat).
M3	**H2c**	Increased outgroup empathy.
M4	**H2d**	Increased comfort interacting with the outgroup (reduced anxiety).
M6	**H3**	Null or small positive effect on perceived ingroup norms.
Panel C. Public–Private Hypotheses		
H5	**H6**	Smaller effect on public than on private outcomes.
H3	H4	Positive effect on private outcomes.
H4	H5	Null or small positive effect on public outcomes.
Panel D. Main Social-Cost Hypotheses: Higher Social Costs (Exploratory)		
	$\mathbf{H6}_{HC}$	Smaller effect on public than on private outcomes for those with higher social costs.
	$\mathrm{H4}_{HC}$	Positive effect on private outcomes for those with higher social costs.
	$\mathrm{H5}_{HC}$	Null or small positive effect on public outcomes for those with higher social costs.
Panel E. Higher vs. Lower Cost Comparisons (Exploratory)		
	$\mathbf{H6}_{SC}$	Greater public–private asymmetry for those with higher vs. lower social costs.
	$\mathrm{H5}_{SC}$	Smaller effect on public outcomes for those with higher vs. lower social costs.

Notes: Panels A–C report the preregistered hypotheses. Panels D–E report their exploratory conditional extensions. HC subscripts denote hypotheses among respondents classified as facing higher social costs, while SC subscripts denote higher-minus-lower social-cost comparisons.

G.2 Data and Inference

Different components of the data permit different tests of the preregistered and exploratory hypotheses. Table G.2.1 summarizes which hypotheses each component can evaluate and the evidentiary role it plays.

The main goal is to test the effects of contact on private versus public outcomes, especially among respondents facing higher social costs. The virtual enumerator experiment provides the primary test of H4–H6 and

$H4_{HC}$–$H6_{HC}$. It randomizes α_d, varying whether responses are socially visible. Cross-domain comparability holds by construction: the same attitudinal items are administered in both conditions, holding B_{id} and $C_{E,id}$ fixed while varying only visibility and the associated social incentives. Because assignment to both contact and visibility conditions is randomized, respondents are also comparable across conditions in expectation. The experiment additionally permits tests of the secondary hypotheses $H5_{SC}$ and $H6_{SC}$, subject to the limitations of comparing observed cost groups discussed in Appendix A.4.

The private and public behavioral intention indices support separate tests of H4 and H5; their higher-cost counterparts $H4_{HC}$ and $H5_{HC}$; and $H5_{SC}$, which compares public effects across cost groups and therefore requires no cross-domain comparison. They do not support tests of H6, $H6_{HC}$, or $H6_{SC}$. As preregistered, the full intention indices measure substantively different private and public actions that likely differ in the personal value of taking the action, responsiveness to treatment, and effort costs. The cross-domain comparability assumption discussed in Appendix A is therefore unlikely to hold, making direct comparisons of these outcomes uninformative.

I also analyze closely matched pairs of behavioral intentions as complementary tests of the same within-group, across-domain pattern. Two pairs—reading versus sharing an article and signing a petition privately versus publicly—more closely approximate cross-domain comparability because the paired actions differ primarily in whether they are observable to others. The comparison between private and public petition signing (but not reading/sharing) was preregistered. The matched pairs provide behavioral intention analogues to the comparison captured experimentally by the virtual enumerator design. I use them to evaluate the patterns corresponding to H4–H6 and $H4_{HC}$–$H6_{HC}$, as well as the secondary hypotheses $H5_{SC}$ and $H6_{SC}$. These comparisons remain complementary because visibility is not itself randomly assigned and the paired actions may still differ in other respects.

Finally, I use the Gleam contest outcomes to test H4 and H5. As preregistered, the design does not support a test of H6 because the private and public actions do not satisfy cross-domain comparability. Moreover, because respondents were already randomly divided between the private and public contests, the resulting samples are too small to support further analysis by social costs.

Table G.2.1: Evidentiary Role of Study Components

			Hypotheses evaluated		
Component	**Private–public comparison**	**Visibility**	**Pooled/Within group**	**Across cost groups**	**Evidentiary role**
Virtual enumerator	Same attitudinal outcome	Randomized	H4–H6; $H4_{HC}$–$H6_{HC}$	$H5_{SC}$, $H6_{SC}$	Primary
Matched intentions	Similar actions	Feature of action	H4–H6; $H4_{HC}$–$H6_{HC}$	$H5_{SC}$, $H6_{SC}$	Complementary; behavior extension
Other intentions	Different actions	Feature of action	H4, H5; $H4_{HC}$, $H5_{HC}$	$H5_{SC}$	Components; behavior extension
Contest outcomes	Different actions	Feature of action	H4, H5 only	—	Extension to actual behavior

Notes: Behavioral outcomes assess whether effects extend beyond attitudinal reports. Matched intentions also provide complementary tests of private–public asymmetry. Cross-domain comparability is required only for hypotheses comparing private and public effects (the H6 family); hypotheses comparing treatment effects within a single domain do not require it, which is why components with non-comparable outcomes can still support H4, H5, and $H5_{SC}$. Hypotheses in the across cost groups column compare treatment effects between observed rather than randomly assigned groups and are secondary throughout; $H5_{SC}$ requires cross-group comparability in full, while $H6_{SC}$ requires only that group differences are not domain-specific.

G.3 Estimation

G.3.1 Average Treatment Effects

H1-H3 involve average treatment effect estimates. I estimate average treatment effects using the preregistered specification:

$$Y_i = \alpha + \beta_1 Contact_i + X_i'\psi + \mu_j + \epsilon_i \tag{G.1}$$

where Y_i is the outcome of interest for individual i and $Contact_i \in \{0, 1\}$ is a treatment assignment indicator equal to one if the respondent was assigned to the outgroup parasocial contact condition and zero if assigned to the control condition. The main coefficient of interest is β_1 where $\beta_1 > 0$ indicates more favorable outcomes toward Black Americans. The regressions also include a vector of pre-specified controls, $X_i'\psi$, shown in

Appendix Table C.1.1. All regressions include implementation wave × *MAFS* season fixed effects (μ_j), which account for differences across survey fielding periods and minor production differences across the television seasons from which couples were drawn (see Appendix B.1). Robust standard errors are reported throughout.

For tests of H4-H6 using the behavior outcomes, this specification estimates the average effect of parasocial contact separately for private and public outcomes. For attitudinal outcomes, estimates are pooled across the private and public measurement conditions.

G.3.2 Public-Private Attitudes Tests

To test whether the contact treatment has different effects private and public attitudes, I estimate:

$$Y_i = \alpha + \beta_1 Contact_i + \beta_2 Public_i + \beta_3 (Contact_i \times Public_i) + X_i'\psi + \mu_j + \epsilon_i \tag{G.2}$$

where $Public_i$ indicates assignment to the public condition in the virtual enumerator experiment, and all other terms are defined as above. In this specification, β_1 captures the contact effect on private attitudes while β_3 captures the public–private difference in contact treatment effects. I additionally calculate the marginal effect of contact in the public condition given by $\beta_1 + \beta_3$. Thus, to evaluate the hypotheses, I have:

- H4: $\beta_1 > 0$
- H5: $\beta_1 + \beta_3 \approx 0$ or small> 0
- H6: $\beta_3 < 0$

G.3.3 Public–Private Attitude Tests by Social Cost

The primary tests of H4_{HC}–H6_{HC} use a triple interaction among contact, public visibility, and social costs. The model estimates the private and public contact effects within the higher cost group and the difference between them. It also estimates H5_{SC} and H6_{SC} and the private higher minus lower contrast used to interpret them. I estimate the following model, where HC_i indicates respondents classified as facing higher social costs:

$$\begin{aligned} Y_i = \alpha &+ \beta_1\, Contact_i + \beta_2\, Public_i + \beta_3\, (Contact_i \times Public_i) \\ &+ \beta_4\, HC_i + \beta_5\, (Contact_i \times HC_i) + \beta_6\, (Public_i \times HC_i) \\ &+ \beta_7\, (Contact_i \times Public_i \times HC_i) + X_i'\psi + \mu_j + \epsilon_i \end{aligned} \tag{G.3}$$

I then use regression coefficients and linear combinations of coefficients to estimate the following contact treatment effects and contrasts. Table G.3.1 shows which coefficients are uses to obtain the comparisons in Table A.4.2

Table G.3.1: Estimands from Triple Interaction Model

Sample	**Private effect**	**Public effect**	**Public−Private**
Higher costs	$\beta_1 + \beta_5$	$\beta_1 + \beta_3 + \beta_5 + \beta_7$	$\beta_3 + \beta_7$
Lower costs	β_1	$\beta_1 + \beta_3$	β_3
Higher−Lower	β_5	$\beta_5 + \beta_7$	β_7

Notes: The table maps coefficients from the triple-interaction model onto the conditional treatment effects and comparisons in Table A.4.2. Single coefficients are reported directly from the regression; expressions containing multiple coefficients are estimated as linear combinations.

G.3.4 Effects on Behavioral Outcomes within Domains across Social Costs

To estimate treatment effect variation across social cost groups within each behavior outcome, I replace $Public_i$ with HC_i in Equation G.2 and estimate each outcome separately. The contact-by-higher-cost interaction estimates the higher minus lower difference in the contact effect. For public outcomes, this interaction tests $H5_{SC}$; for private outcomes, it is reported as an interpretive diagnostic without a directional prediction.

Most private and public behavior outcomes differ in personal benefits and effort costs as well as visibility, so comparisons between them do not provide direct tests of H6, $H6_{HC}$, or $H6_{SC}$. For the substantively matched reading-sharing and private-public petition pairs, I combine the outcome models using seemingly unrelated estimation to account for covariance across equations. Linear combinations of the coefficients estimate the private and public contact effects within the higher cost group and their difference, providing complementary tests of $H4_{HC}$–$H6_{HC}$. I also compare the higher minus lower treatment effect contrasts across the paired outcomes, providing a complementary test of $H6_{SC}$.

H Average Contact Effects on Attitudes

H.1 Main Outcomes

Table H.1.1 reports the average effects of parasocial contact on the main outcomes and proposed mechanisms. Panel A reports warmth and support for social change, Panel B reports personal-belief mechanisms, and Panel C reports perceived ingroup norms. Appendix F provides complete descriptions of the outcome measures.

Briefly, the Black warmth index combines the feeling thermometer, desire for more Black friends, and support for intermarriage. Because the intermarriage item has no parallel White component, the White and Black–White difference indices contain only the feeling thermometer and friendship items. The Black index is the primary measure of warmth toward the outgroup, while the difference index assesses whether effects are specific to evaluations of Black Americans rather than reflecting general changes in evaluations of both groups. Appendix L discusses the relationship between these outcomes and the pre-analysis plan.

Estimates for all three components of the Black Warmth index are positive, although they vary in magnitude and precision. The treatment had little effect on feeling thermometer ratings (0.03, $p = .734$), but increased respondents' desire to have more Black friends (0.11, $p = .004$). The estimate for support for intermarriage was also positive but imprecisely estimated (0.08, $p = .116$). The results for the friendship items provide the clearest evidence that the effect is specific to attitudes toward Black Americans. The treatment increased respondents' desire to have more Black friends but had no effect on their desire to have more White friends. Consequently, it increased the difference between wanting more Black and more White friends by 0.10 units ($p = .012$). The treatment also increased average support for social change by 0.08 units ($p = .034$).

H.2 Results for Mechanism-Related Outcomes

Table H.1.1 presents estimates of the average effects of parasocial contact on outcomes associated with the two theorized pathways of change: personal beliefs and perceived ingroup norms. The table reports results for each pathway and its individual components. Bolded coefficients correspond to the results reported in Figure 2. The main prediction is that treatment would change at least one personal-belief measure—positive trait evaluations, perceived similarity, outgroup empathy, or comfort with intergroup interaction—while having little effect on perceived ingroup norms.

The evidence of personal-belief change is concentrated in evaluations of positive traits among Black Americans. As described in Appendix F, the main index comprises four traits: hardworking, intelligent, family-oriented, and committed. The Black index combines evaluations of Black Americans, while the White index and Black–White difference index help establish whether the results reflect improved evaluations of the outgroup rather than general changes in trait evaluations. Treatment improved evaluations of Black Americans

Table H.1.1: Average Contact Treatment Effects

	Control	Contact Effects			N	
	mean	coef	s.e.	p-val	Full	Para
PANEL A: MAIN OUTCOMES						
1. Warmth						
Black index (three items)	**0.00**	**0.12****	**(0.05)**	**0.011**	**1543**	**1037**
White index (two items)	0.00	-0.02	(0.05)	0.674	1543	1037
Difference index (two items)	0.00	0.09**	(0.04)	0.033	1543	1037
Thermometer						
Black	7.07	0.03	(0.10)	0.734	1543	1037
White	7.07	-0.10	(0.11)	0.358	1543	1037
Difference	0.00	0.13	(0.10)	0.202	1543	1037
Want more [...] friends						
Black	2.75	0.11***	(0.04)	0.004	1543	1037
White	2.68	0.01	(0.04)	0.844	1543	1037
Difference	0.07	0.10**	(0.04)	0.012	1543	1037
Support intermarriage	4.53	0.08	(0.05)	0.116	1543	1037
2. Support for Social Change	**0.45**	**0.08****	**(0.04)**	**0.034**	**1542**	**1037**
PANEL B: PERSONAL BELIEF MECHANISMS						
3. Positive Trait Evaluations						
Black (4-item index)	**0.00**	**0.11****	**(0.05)**	**0.045**	**1540**	**1035**
White (4-item index)	0.00	0.04	(0.06)	0.484	1540	1035
Difference (4-item index)	0.00	0.09*	(0.05)	0.082	1539	1034
Black (5-item index)	0.00	0.09*	(0.05)	0.093	1540	1035
White(5-item index)	0.00	0.00	(0.06)	0.984	1540	1035
Difference (5-item index)	0.00	0.10*	(0.05)	0.064	1539	1034
Hardworking						
Black	6.34	0.22**	(0.11)	0.042	1540	1035
White	6.29	0.16	(0.10)	0.114	1540	1035
Difference	0.05	0.06	(0.10)	0.537	1539	1034
Intelligent						
Black	6.51	0.13	(0.10)	0.210	1540	1035
White	6.28	0.03	(0.10)	0.786	1540	1035
Difference	0.23	0.11	(0.09)	0.224	1539	1034
Family-oriented						
Black	6.22	0.20	(0.12)	0.101	1540	1035
White	5.99	0.09	(0.12)	0.467	1540	1035
Difference	0.21	0.13	(0.13)	0.349	1539	1034
Committed						
Black	6.34	0.16	(0.12)	0.189	1540	1035
White	6.72	-0.02	(0.11)	0.824	1540	1035
Difference	-0.39	0.19*	(0.10)	0.064	1539	1034
Nonaggressive						
Black	6.09	0.05	(0.12)	0.661	1540	1035
White	5.92	-0.15	(0.13)	0.250	1540	1035
Difference	0.16	0.20*	(0.11)	0.073	1539	1034
4. Perceived similarity						
Index	**0.00**	**0.03**	**(0.06)**	**0.579**	**1540**	**1035**
Perceived general similarity	3.08	0.00	(0.05)	0.957	1540	1035
Perceived values similarity	3.50	0.06	(0.07)	0.375	1540	1035
5. Empathy						
Index	**0.00**	**0.02**	**(0.05)**	**0.645**	**1539**	**1034**
Empathy (general)	3.30	-0.03	(0.06)	0.603	1539	1034
Empathy (policing)	5.24	0.12	(0.08)	0.159	1539	1034
6. Interaction comfort						
Black	**3.78**	**0.06**	**(0.07)**	**0.383**	**1542**	**1037**
White	3.96	0.01	(0.07)	0.906	1542	1037
Difference	-0.17	0.05	(0.04)	0.198	1542	1037
PANEL C: SOCIAL NORMS						
7. Perceived ingroup norms						
Index	**0.00**	**0.07**	**(0.05)**	**0.205**	**1540**	**1035**
General	2.62	0.04	(0.03)	0.251	1540	1035
Family	2.44	0.04	(0.04)	0.317	1540	1035
Friends	2.80	0.03	(0.03)	0.407	1540	1035

Notes: $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$. The table reports average effects of the contact treatment on the main outcomes (Panel A), personal belief mechanisms (Panel B), and perceived ingroup norms (Panel C). Measures labeled "Black" and "White" evaluate Black and White Americans, respectively; "Difference" is calculated as Black minus White. All indices are inverse covariance weighted. Components are reported on their original scales; their coding and ranges appear in Table F.1.1. The four-item trait indices exclude "Nonaggressive"; the five-item indices include it. Control means are unadjusted averages in the control group. Treatment effects are estimated with the preregistered covariates and randomization strata fixed effects; robust standard errors appear in parentheses. "Full" reports the full analytic sample, while "Para" reports the number of respondents in the contact and control conditions used for the focal contrast.

by 0.11 standard deviations and the Black–White difference in evaluations by 0.09 standard deviations. Results for the individual traits show a similar pattern.

For completeness, the table also reports results for a five-item index that includes a fifth trait, nonaggressive, corresponding to the trait measures specified in the pre-analysis plan. The positive treatment effect on the Black–White difference for this item, however, is driven primarily by changes in evaluations of White Americans rather than Black Americans.[10] Because the paper focuses on whether contact improves evaluations of the outgroup, the four-item index is used throughout the main analysis. The substantive conclusions are similar using either index.

Beyond positive trait evaluations, there is little evidence that contact affected the other personal-belief measures, including perceived similarity, empathy, or comfort interacting with outgroup strangers.[11] Panel C also shows no detectable effect on perceived ingroup norms regarding the desirability of outgroup friendships.

As detailed in Appendix A, the predicted treatment-effect pattern requires the personal-belief pathway to be operative but does not require contact to operate solely through personal-belief change. The positive trait-evaluation results provide evidence that contact changed personal beliefs and therefore satisfied this condition.[12]

H.3 Average Effects by Outgroup Couple

This section examines whether the effects of parasocial contact vary by outgroup couple. The analysis was preregistered and serves three purposes. First, it assesses whether treatment effects were consistent across the two outgroup couples or concentrated among respondents assigned to one couple. Second, it examines whether variation in attitudinal effects corresponds with variation in the parasocial ties viewers formed with the characters. Third, it provides additional evidence for evaluating whether the observed effects are attributable to experimenter demand.

Table H.3.1 reports treatment effects separately for respondents assigned to outgroup Couple B and Couple D, with each compared to its corresponding ingroup control condition. Panel B examines parasocial ties to the male and female characters using the measures described in Appendix E.3. Relative to their respective ingroup control conditions, assignment to Couple D increased parasocial ties to both characters, whereas assignment to Couple B did not. Panel C shows a similar pattern for the attitudinal outcomes: assignment to Couple D increased warmth toward Black Americans, support for social change, and positive trait evaluations, while the corresponding estimates for Couple B were smaller and not statistically distinguishable from zero.

This pattern is consistent with parasocial contact theory, which predicts that treatment effects depend on the affective bonds viewers form with outgroup characters. The strength of those bonds may naturally vary across the individuals portrayed. The study cannot identify which characteristics of the couples produced this variation, nor is that its primary objective. The relevant finding is that the couple who generated stronger parasocial ties also produced more favorable attitudinal effects.

The couple specific results also help address concerns about experimenter demand. As shown in Table E.2.1 and reproduced in Panel A of Table H.3.1, assignment to either outgroup couple increased respondents' ability to identify the study's purpose by similar amounts: 21 percentage points for Couple B and 18 percentage points for Couple D. If the attitudinal effects were driven primarily by respondents recognizing the study's purpose and adjusting their answers accordingly, more similar effects across the two couples would be expected. Instead, favorable attitudinal effects emerged for Couple D, which also generated stronger

[10] One possible explanation is that scenes of conflict and conflict resolution involving a White couple in the control condition improved perceptions of White Americans as calm and nonaggressive.

[11] As preregistered, effects were not expected across every specific measure; change in at least one was interpreted as evidence of personal-belief change.

[12] See Appendix K for multiple-hypothesis-testing corrections.

parasocial ties. This pattern is difficult to reconcile with an explanation based solely on experimenter demand.

Table H.3.1: Effects of Contact by Outgroup Couple

	Control	Couple B			Couple D		
	mean	*coef*	*s.e.*	*p-val*	*coef*	*s.e.*	*p-val*
PANEL A: GUESSED PURPOSES							
Guessed purpose (%)	0.61	0.21***	(0.04)	0.000	0.18***	(0.04)	0.000
PANEL B: PARASOCIAL TIES							
Male characters (Index)	0.00	0.02	(0.08)	0.841	0.60***	(0.08)	0.000
Female characters (Index)	0.00	0.04	(0.07)	0.537	0.35***	(0.08)	0.000
PANEL C: ATTITUDINAL OUTCOMES							
Warmth toward Black Americans (Index)	0.00	0.06	(0.06)	0.323	0.17***	(0.06)	0.005
Support for Social Change	0.45	0.06	(0.05)	0.212	0.10**	(0.05)	0.046
Positive Trait Evaluations of Black Americans (4 item index)	0.00	0.04	(0.07)	0.542	0.17**	(0.07)	0.017

Notes: *p<.10, ** p<.05, *** p<.01. Table reports treatment effects separately for respondents assigned to Couple B (Amani and Woody) and Couple D (Gregory and Deonna), with both couples compared to the ingroup control condition. Parasocial ties indices capture respondents' evaluations of the male and female characters. Indices are standardized based on the mean and standard deviation of the control group; support for social change is presented in its original three-point scale.

I Contact Effects on Private and Public Attitudes

I.1 Pooled Effects on Private and Public Attitudes

Table I.1.1 reports estimates of the effects of contact on private and public attitudes, as measured through the virtual enumerator experiment. Bolded coefficients correspond to the results presented in Figure 3 in the main text. Panel I presents results for the warmth index and its components, Panel II presents results for support for social change, and Panel III presents results for the positive-trait-evaluations index and its components. Within each set of outcomes, Panel A reports estimates from the interaction model in Equation G.2, while Panel B reports the marginal effect of contact in the public condition. Figure I.1.1 provides a visual summary of the results across a broader set of outcomes, including the main indices, their individual components, and both evaluations of Black Americans and Black–White differences.

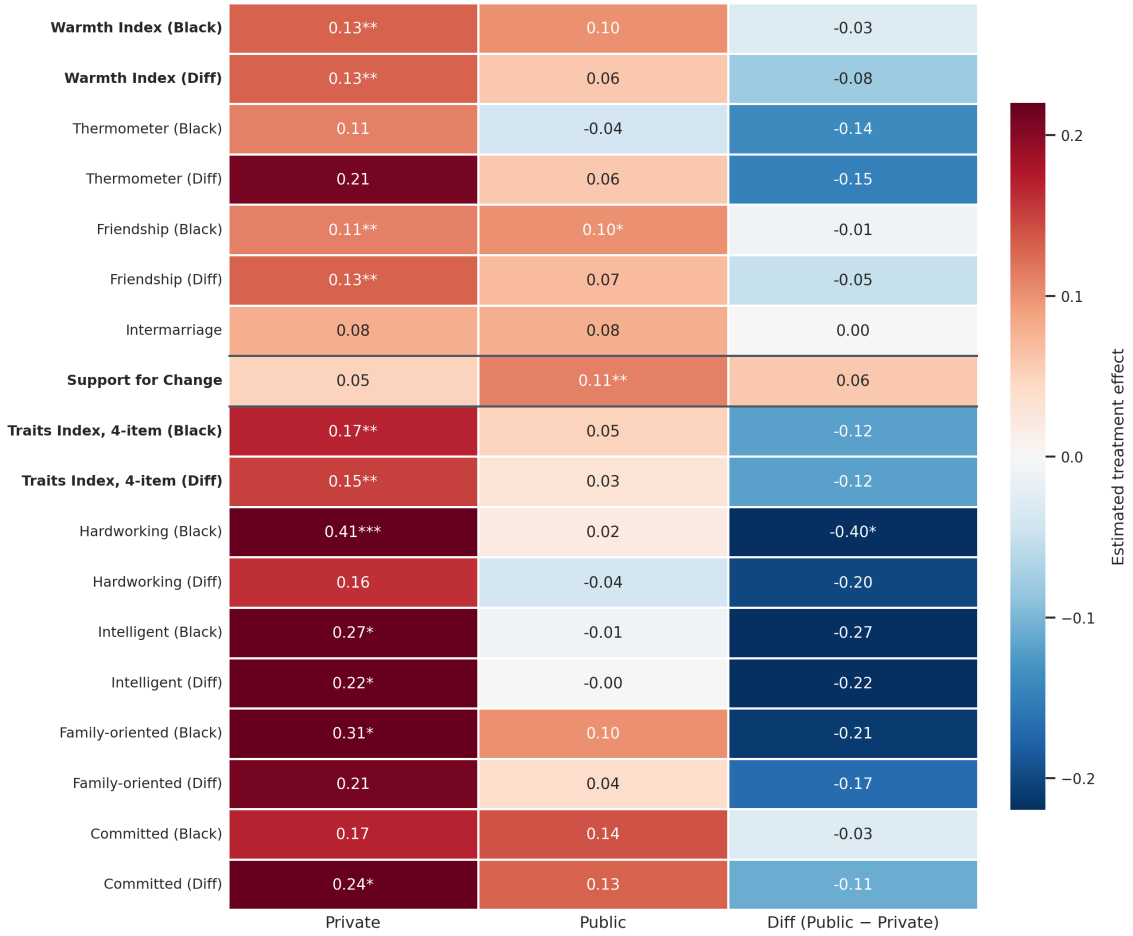
The predicted pattern is not evident across all outcomes but appears most clearly for positive-trait evaluations. Contact improved trait evaluations by 0.17 standard deviations in the private condition ($p = .028$) but produced no detectable improvement in the public condition. The interaction between contact and the public condition is negative, as predicted, although imprecisely estimated (-0.12 standard deviations, $p = .258$). The same pattern appears for three of the four individual traits. In the private condition, contact increased evaluations of Black Americans as hardworking by 0.41 units ($p = .006$), intelligent by 0.27 units ($p = .075$), and family-oriented by 0.31 units ($p = .083$). The corresponding public effects are substantially smaller and statistically insignificant. The private–public difference is particularly pronounced for evaluations of Black Americans as hardworking (-0.40 units, $p = .060$). In contrast, neither the warmth index nor support for social change follows the same pattern, suggesting that the effects of contact on private and public attitudes vary across outcomes.

Table I.1.1: Effects of Contact on Private and Public Attitudes

	I. WARMTH				II. SUPPORT	III. POSITIVE TRAIT EVALUATIONS				
	Index	**Components (Black Americans)**			**CHANGE**	**Index**	**Components (Black Americans)**			
	(3-Item)	Thermometer	Friends	Intermarriage		**(4-item)**	Hardworking	Intelligent	Family-oriented	Committed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: Interaction Regression Results										
Contact (B1)	**0.12****	0.11	0.11**	0.08	**0.05**	**0.17****	0.41***	0.27*	0.31*	0.17
s.e.	**(0.06)**	(0.14)	(0.05)	(0.07)	**(0.05)**	**(0.08)**	(0.15)	(0.15)	(0.18)	(0.17)
pval	**0.049**	0.451	0.028	0.251	**0.340**	**0.028**	0.006	0.075	0.083	0.301
Public (B2)	-0.08	0.03	0.01	-0.10	-0.07	-0.05	0.01	0.02	-0.13	-0.15
s.e.	(0.07)	(0.14)	(0.05)	(0.07)	(0.05)	(0.08)	(0.15)	(0.14)	(0.18)	(0.17)
pval	0.237	0.831	0.923	0.175	0.226	0.525	0.965	0.897	0.476	0.377
Contact*Public (B3)	**-0.04**	-0.14	-0.01	0.00	**0.06**	**-0.12**	-0.40*	-0.27	-0.21	-0.03
s.e.	**(0.09)**	(0.21)	(0.07)	(0.10)	**(0.08)**	**(0.11)**	(0.21)	(0.21)	(0.25)	(0.24)
pval	**0.670**	0.498	0.895	0.986	**0.420**	**0.258**	0.060	0.196	0.408	0.907
PANEL B: Effects for High Cost Individuals										
Contact Effect (Public) (B1+B3)	**0.08**	-0.04	0.10*	0.08	**0.11****	**0.05**	0.02	-0.01	0.10	0.14
s.e.	**(0.07)**	(0.15)	(0.05)	(0.07)	**(0.05)**	**(0.08)**	(0.15)	(0.15)	(0.18)	(0.17)
pval	**0.213**	0.817	0.050	0.268	**0.039**	**0.541**	0.913	0.972	0.570	0.398
Control mean	0.05	7.12	2.76	4.59	0.47	0.04	6.38	6.54	6.29	6.45
N	1543	1543	1543	1543	1542	1540	1540	1540	1540	1540
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: The table reports results from separate regressions of each outcome on indicators for contact, assignment to the public (versus private) enumerator condition, and their interaction, as specified in Equation G.2. Panel B reports the marginal effect of the contact treatment in the public condition. Indices are standardized using the mean and standard deviation of the control group; individual components are reported in their original units. Raw control-group means are included to illustrate baseline outcome levels. Regression estimates are adjusted for controls and fixed effects.

Figure I.1.1: Summary of Effects on Private and Public Attitudes



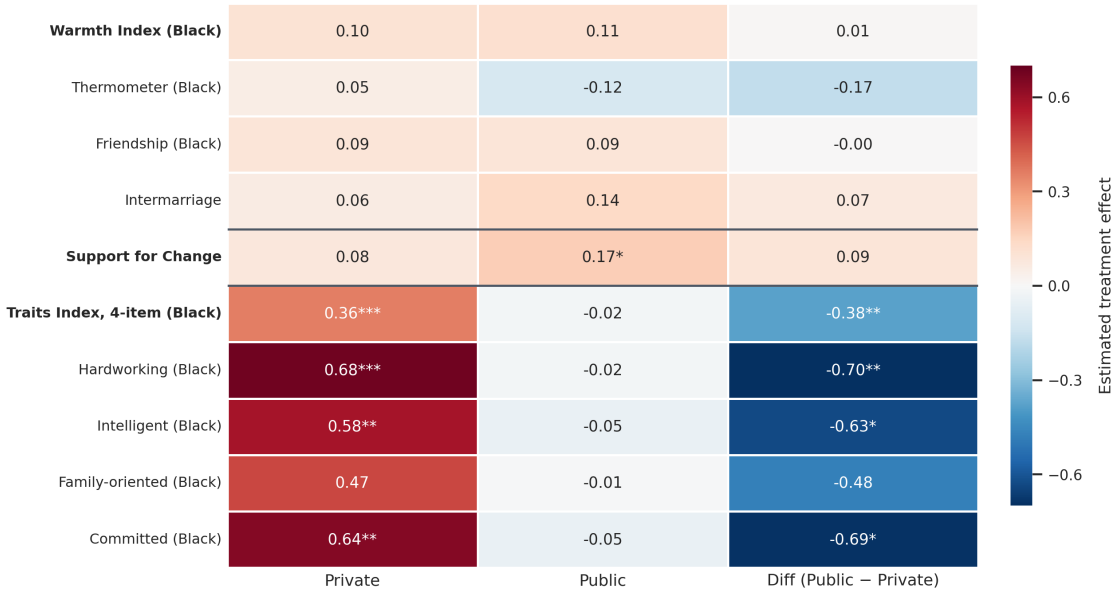
Notes: Cells show estimated effects of the contact treatment in private and public conditions, as well as their difference. Index values are standardized regression coefficients; individual components are reported in their original units. Red indicates positive effects and blue indicates negative effects. The expectation is positive effects in private (red), null or small positive effects in public (gray or light red), and a negative difference (blue). Significance: * $p < 0.10$, ** $p < 0.05$, *** $p < 0.01$.

I.2 Effects on Private and Public Attitudes by Social Cost

Table I.2.1 reports estimates of the effects of contact on private and public attitudes by exposure to social costs. Bolded coefficients correspond to the results presented in Figure 3 in the main text. Panel I presents results for the warmth index and its components, Panel II presents results for support for social change, and Panel III presents results for the positive-trait-evaluations index and its components. Within each set of outcomes, Panel A reports the marginal effects of contact in the private and public conditions among respondents facing higher social costs. Panel B reports estimates from the triple-interaction model in Equation G.3. Figure I.2.1 provides a visual summary of the results among higher-cost respondents.

Panel A shows that, among respondents facing higher social costs, contact significantly improves private evaluations of Black Americans' positive traits but has no detectable effect on public evaluations. This pattern is consistent across all four trait items: the private treatment effects are positive for evaluations of Black Americans as hardworking, intelligent, family-oriented, and committed, while the public treatment effects are uniformly close to zero. Although the private effect for the family-oriented item is not statistically significant, the consistency across items indicates that the index-level result is not driven by any single trait. Panel B further shows that respondents facing higher social costs report more negative private trait evaluations in the control condition, even after controlling for pretreatment personal prejudice. Contact also produces larger improvements in private trait evaluations among these respondents than among respondents facing lower social costs. The negative three-way interaction for the trait index indicates that the divergence between private and public treatment effects is greater among respondents facing higher social costs (-.48 SD $p = .042$). Comparable patterns do not emerge for warmth or support for social change.

Figure I.2.1: Summary of Effects on Private and Public Attitudes Among High Cost Individuals



Notes: Cells show estimated effects of the contact treatment in private and public conditions, as well as their difference among those facing high social costs. Index values are standardized regression coefficients; individual components are reported in their original units. Red indicates positive effects and blue indicates negative effects. The expectation is positive effects in private (red), null or small positive effects in public (gray or light red), and a negative difference (blue). Significance: * p < 0.10, ** p < 0.05, *** p < 0.01.

Table I.2.1: Effects of Contact on Private and Public Attitudes by Social Cost

	I. WARMTH				II. SUPPORT	III. POSITIVE TRAIT EVALUATIONS				
	Index	**Components (Black Americans)**			**CHANGE**	**Index**	**Components (Black Americans)**			
	(3-Item)	Thermometer	Friends	Intermarriage		(4-item)	Hardworking	Intelligent	Family-oriented	Committed
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
PANEL A: Effects for High Costs Individuals										
Contact (Private, High Cost)	**0.10**	0.05	0.09	0.06	**0.08**	**0.36*****	0.68***	0.58**	0.47	0.64**
s.e.	**(0.12)**	(0.23)	(0.09)	(0.13)	**(0.10)**	**(0.13)**	(0.24)	(0.24)	(0.30)	(0.29)
pval	**0.400**	0.819	0.311	0.633	**0.425**	**0.007**	0.005	0.017	0.116	0.027
Contact (Public, High Cost)	**0.11**	-0.12	0.09	0.14	**0.17***	**-0.02**	-0.02	-0.05	-0.01	-0.05
s.e.	**(0.12)**	(0.24)	(0.09)	(0.12)	**(0.09)**	**(0.13)**	(0.25)	(0.23)	(0.30)	(0.28)
pval	**0.353**	0.622	0.286	0.267	**0.053**	**0.867**	0.928	0.829	0.960	0.856
Difference in Contact Effects (Public-Private, High Cost)	**0.01**	-0.17	0.00	0.07	**0.09**	**-0.38****	-0.70**	-0.63*	-0.48	-0.69*
s.e.	**(0.17)**	(0.33)	(0.12)	(0.18)	**(0.13)**	**(0.19)**	(0.35)	(0.33)	(0.42)	(0.40)
pval	**0.934**	0.608	0.968	0.685	**0.482**	**0.042**	0.045	0.061	0.253	0.086
PANEL B: Triple Interaction Regression Results										
Contact (Private, Low Cost) (B1)	0.11	0.06	0.11*	0.06	0.03	0.02	0.19	0.02	0.13	-0.16
s.e.	(0.07)	(0.18)	(0.06)	(0.07)	(0.06)	(0.10)	(0.20)	(0.20)	(0.23)	(0.20)
pval	0.120	0.725	0.085	0.407	0.673	0.857	0.336	0.912	0.578	0.431
Public (Ingroup Contact, Low Cost) (B2)	-0.04	0.05	0.03	-0.11	-0.08	-0.17*	-0.07	-0.09	-0.31	-0.46**
s.e.	(0.08)	(0.19)	(0.06)	(0.08)	(0.07)	(0.10)	(0.19)	(0.19)	(0.22)	(0.21)
pval	0.594	0.779	0.589	0.169	0.244	0.080	0.708	0.615	0.150	0.025
Contact*Public (Low Cost) (B3)	0.01	0.00	0.02	0.00	0.05	0.10	-0.10	0.06	0.08	0.47
s.e.	(0.11)	(0.28)	(0.09)	(0.11)	(0.09)	(0.14)	(0.28)	(0.28)	(0.31)	(0.30)
pval	0.907	0.992	0.842	0.982	0.607	0.484	0.723	0.832	0.805	0.117
High Cost (Ingroup Contact, Private)(B4)	-0.23**	-0.38*	-0.14*	-0.19	-0.11	-0.41***	-0.56**	-0.57**	-0.67**	-0.75***
s.e.	(0.11)	(0.21)	(0.08)	(0.12)	(0.09)	(0.12)	(0.23)	(0.22)	(0.27)	(0.26)
pval	0.030	0.073	0.088	0.115	0.190	0.001	0.016	0.010	0.014	0.004
Contact*High Cost (Private) (B5)	-0.01	-0.01	-0.02	0.00	0.05	0.34**	0.49	0.56*	0.34	0.80**
s.e.	(0.14)	(0.29)	(0.11)	(0.15)	(0.11)	(0.17)	(0.31)	(0.31)	(0.37)	(0.35)
pval	0.921	0.969	0.837	0.984	0.651	0.042	0.118	0.075	0.358	0.023
Public*High Cost (Ingroup Contact) (B6)	-0.08	-0.17	-0.10	0.00	0.02	0.25	0.11	0.20	0.38	0.76**
s.e.	(0.15)	(0.30)	(0.12)	(0.16)	(0.12)	(0.17)	(0.32)	(0.31)	(0.37)	(0.36)
pval	0.604	0.567	0.387	0.989	0.836	0.137	0.741	0.517	0.306	0.037
Contact*Public*High Cost (B7)	0.00	-0.17	-0.01	0.08	0.04	-0.48**	-0.60	-0.69	-0.56	-1.16**
s.e.	(0.20)	(0.43)	(0.15)	(0.21)	(0.16)	(0.23)	(0.45)	(0.44)	(0.53)	(0.50)
pval	0.997	0.700	0.929	0.722	0.779	0.042	0.178	0.118	0.286	0.021
Control mean	0.29	7.60	2.89	4.75	0.58	0.29	6.78	6.93	6.72	6.85
N	1543	1543	1543	1543	1542	1540	1540	1540	1540	1540
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: Table reports results from separate regressions of each outcome on indicators for parasocial contact, assignment to the public (versus private) enumerator condition, and their interaction, as specified in Equation G.2. All outcomes are standardized by the mean and standard deviation of the control group. Raw means are included to illustrate baseline levels across experimental conditions. Regression estimates are adjusted for controls and fixed effects.

Finally, Table I.2.2 maps the treatment effects on the positive traits index onto the predictions in Table A.4.2. The higher cost row supports the focal predicted pattern: contact produced a positive effect on private attitudes, no detectable effect on public expression, and a statistically significant negative public–private difference. Among lower cost respondents, neither treatment effect was distinguishable from zero, and the private and public effects did not significantly differ. This produces the predicted triple interaction effect. While the higher–lower public effect was in the predicted negative direction it is not significant.

The first column helps interpret these comparisons. Of note, the first column indicates that the contact treatment had a substantially bigger effect on higher versus lower cost individuals. This suggests the two groups different differed substantially in their underlying responsiveness to contact, even after covariate adjustment. Thus, while the higher-lower comparisons pose inferential challenges, what is driving the effect of the contact treatment for the higher cost group remains clear: a big effect on private, null effect on public, producing a strong negative asymmetry.

Table I.2.2: Contact Effects on the Positive Traits Index by Visibility and Social Costs

	Private effect	**Public effect**	**Public − private**
Higher costs	0.36***	-0.02	-0.38**
	(0.13)	(0.13)	(0.19)
Lower costs	0.02	0.12	0.10
	(0.10)	(0.10)	(0.14)
Higher − lower	0.34**	-0.14	-0.48**
	(0.17)	(0.16)	(0.23)

Notes: The table presents results from the triple interaction model and associated linear combinations in the 3×3 matrix format of Table A.4.2. Differences are calculated as public minus private across columns and higher costs minus lower costs across rows. Private and public refer to the randomly assigned virtual enumerator conditions. Higher and lower cost groups are defined using the pretreatment social cost measure described in Appendix D. The outcome is standardized. Models include covariates and randomization strata fixed effects. Robust standard errors appear in parentheses. $N = 1{,}540$. $^{*}p < .10$, $^{**}p < .05$, $^{***}p < .01$.

I.3 Robustness Checks

This section reports several checks assessing the robustness of the results in Appendix I.2.

Table I.3.1 assesses the robustness of the positive trait evaluations results across the six measures of higher social costs described in Appendix D. Bolded estimates correspond to the main results presented in Figure 3 and Table I.2.1. The pattern is consistent across all six measures: positive and statistically significant effects in the private condition, null effects in the public condition, and negative public–private differences. These differences are statistically significant in four of the six specifications, indicating larger treatment effects on private than on public attitudes.

Figure I.3.1 assesses the robustness of these results using the continuous perceived ingroup norms factor score. Although the three-way interaction is not statistically significant, the pattern is consistent with the results obtained using the binary measures. At lower values of the factor score, where perceived ingroup norms are less prejudiced, contact has similar and suggestively positive effects on private and public attitudes. As perceived norms become more prejudiced, the public–private difference becomes increasingly negative, indicating larger effects on private than on public attitudes. This pattern suggests that the binary results are not solely an artifact of the cutoff used to define the cost groups. It also supports the interpretation of the lower-cost group as containing more respondents for whom favorable public expression is relatively unconstrained: toward the lower end of the continuous measure, the private and public effects are similar, as the framework anticipates when social costs are low, neutral, or offset by social benefits.

One concern is that these estimated moderation by social costs may reflect the association between perceived ingroup norms and respondents' own prejudice. To assess this possibility, I re-estimate the main specification

while allowing the effects of contact across private and public conditions to vary independently with pretreatment prejudice. Specifically, the model includes the full interaction of contact treatment, enumerator condition, and pretreatment prejudice alongside the corresponding interaction with the social-cost measure. This allows personal prejudice to account for both treatment responsiveness and private–public differences in that responsiveness. The results, available in the replication code, show the Contact×Public×HighCost interaction remains substantively large after this adjustment ($\hat{\beta} = -0.445$, SE = 0.246, $p = .070$). By contrast, there is no evidence that pretreatment prejudice independently moderates the private–public treatment-effect difference ($\hat{\beta} = -0.044$, SE = 0.135, $p = .743$). These results indicate that the main pattern is not attributable simply to the association between perceived norms and respondents' own prejudice.

Additionally, I examine correlations among the individual trait items. Although the four items are positively correlated, the correlations range from 0.37 to 0.72 in the full sample and from 0.34 to 0.66 among respondents facing higher social costs. The items therefore capture related but distinct trait evaluations.

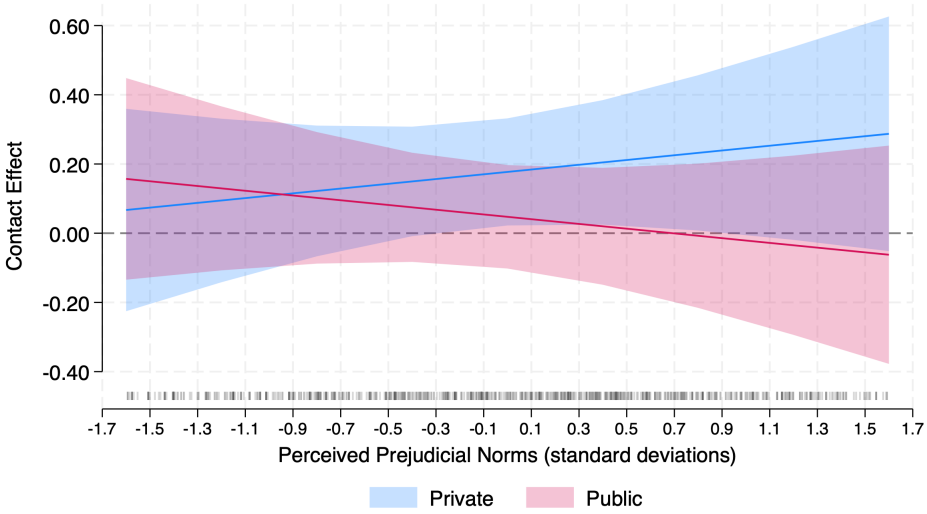
Finally, as a sensitivity analysis, I re-estimate the models after excluding the approximately 10 percent of respondents who provided identical responses across all four trait items. Because response variation is itself measured after treatment, this is not treated as a preferred specification. The substantive pattern nevertheless remains unchanged: parasocial contact improves private trait evaluations among respondents facing higher social costs, with no corresponding improvement in public evaluations.

Table I.3.1: Effects of Contact on Private vs. Public Attitudes for Higher Cost Individuals: Robustness

	DV: POSITIVE TRAIT EVALUATIONS INDEX (4-item)					
	Direction		Direction + Network		Direction + Fear	
	Factor	Either	**Factor**	Either	Factor	Either
Contact Effect (Private)	0.28**	0.21**	**0.36*** **	0.26**	0.41**	0.32**
s.e.	(0.12)	(0.10)	**(0.13)**	(0.11)	(0.18)	(0.15)
pval	0.024	0.038	**0.007**	0.018	0.018	0.033
Contact Effect (Public)	-0.03	0.02	**-0.02**	0.03	-0.13	-0.04
s.e.	(0.12)	(0.10)	**(0.13)**	(0.11)	(0.18)	(0.15)
pval	0.816	0.801	**0.867**	0.768	0.472	0.762
Difference in Effects (Public-Private)	-0.30*	-0.18	**-0.38** **	-0.22	-0.54**	-0.36*
s.e.	(0.17)	(0.14)	**(0.19)**	(0.16)	(0.25)	(0.21)
pval	0.072	0.190	**0.042**	0.153	0.029	0.082
Total N	1540	1540	**1540**	1540	1540	1540
Higher-cost N	768	1042	**626**	836	348	473

Notes: Table reports the effects of the contact treatment on private and public positive trait evaluations among respondents facing high social costs using alternative measures of social costs. Direction measures capture perceived ingroup norms, either as a standardized factor score (Factor) or based on either of two component indices (Either). Direction + Network additionally incorporates racial network composition, and Direction + Fear additionally incorporates fear of social sanctioning. Estimates are marginal effects derived from the triple-interaction model in Equation G.3. Outcomes are standardized by the mean and standard deviation of the control group. All regressions include controls and fixed effects.

Figure I.3.1: Contact Effects Across Perceived Ingroup Norms By Social Costs



Notes: Figure reports the public and private effects of the contact treatment on positive trait evaluations across levels of perceived prejudicial ingroup norms. Perceived norms are measured using a standardized factor score, with higher values indicating more prejudicial norms. Estimates are derived from a triple-interaction model analogous to Equation G.3, replacing the high social costs indicator with the continuous factor score. Shaded areas indicate 95 percent confidence intervals, and tick marks show the distribution of perceived norms in the estimation sample. Outcomes are standardized by the mean and standard deviation of the control group. All regressions include controls and fixed effects.

I.4 Exploratory Results by Outgroup Couple

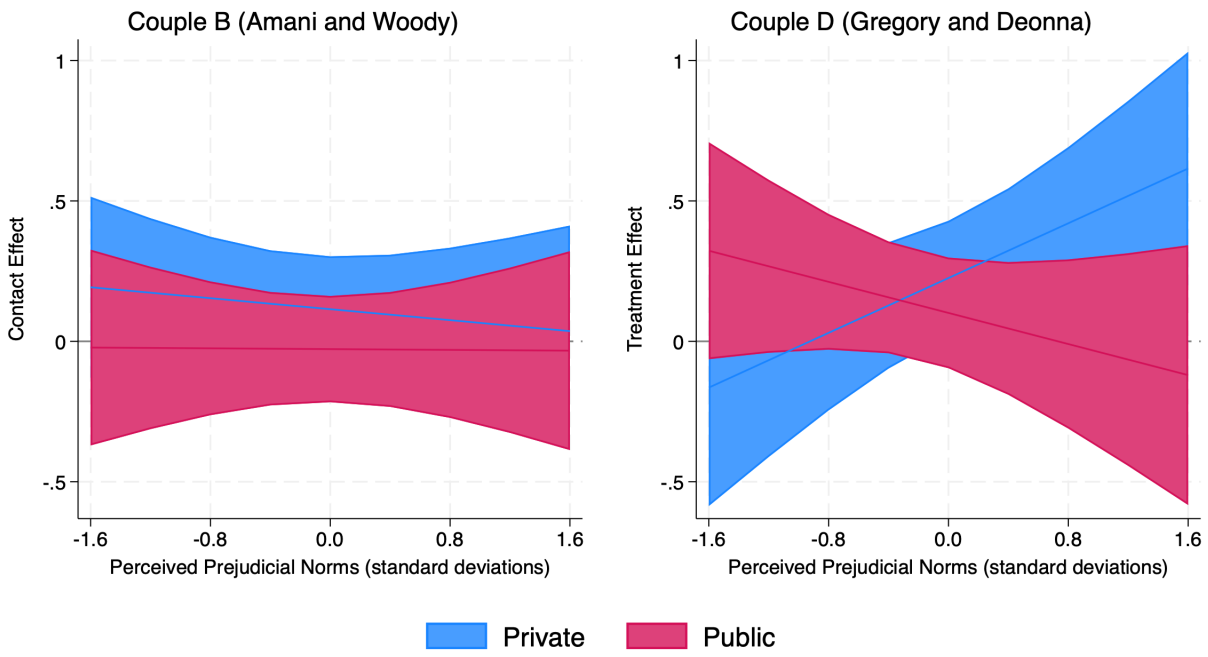
As an exploratory extension, I examine whether perceived ingroup norms moderate private and public treatment effects differently across the two outgroup couples. This analysis builds on Appendix H.3, which shows that Couple D generated stronger parasocial ties and more favorable attitudinal effects relative to its same-season ingroup counterpart, while Couple B did not. The couple comparison also probes whether the public enumerator induced researcher demand rather than pressure from an ingroup audience: a uniform researcher-demand response to that condition would predict similar Contact × Public effects for both couples, whereas the observed effects vary by couple.

Figure I.4.1 plots the conditional treatment effects across the distribution of perceived prejudiced ingroup norms. For Couple B, the private and public effects remain relatively similar across the distribution, providing little evidence that their difference varies systematically with perceived norms. For Couple D, the effects increasingly diverge as perceived norms become more prejudiced. At the tolerant end of the distribution, the public effect is positive and larger than the private effect, although both estimates are imprecise. This pattern is consistent with favorable public expression being relatively unconstrained or carrying social benefits. As perceived norms become more prejudiced, the private effect becomes increasingly positive while the public effect declines, producing a progressively more negative public–private difference.

This pattern is a more pronounced version of the relationship shown in Figure I.3.1. Among respondents assigned to Couple D, the point estimates shift from larger public effects under tolerant perceived norms to effects concentrated in private attitudes under prejudiced perceived norms. The divergence reflects both increasing private effects and decreasing public effects. The framework predicts similar private effects across levels of perceived norms only when respondents are otherwise comparable, so the increasing private effect neither supports nor contradicts that prediction. Because perceived norms are observed rather than randomly assigned, respondents who perceive more prejudiced norms may differ in ways that leave greater scope

for Couple D to change their private attitudes. The widening public–private difference is therefore consistent with $H6_{SC}$, but it cannot be attributed to differences in social costs alone.

Figure I.4.1: Difference in Contact Effects Across Perceived Ingroup Norms by Outgroup Couple



Notes: Figure reports the public and private effects of the contact treatment on positive trait evaluations across levels of perceived prejudicial ingroup norms, separately by outgroup couple. Perceived norms are measured using a standardized factor score, with higher values indicating more prejudicial norms. Estimates are derived from a triple-interaction model analogous to Equation G.3, replacing the contact indicator with separate indicators of assignment to either Couple B or D and the high social costs indicator with the continuous factor score. Shaded areas indicate 95 percent confidence intervals, and tick marks show the distribution of perceived norms in the estimation sample. Outcomes are standardized using the mean and standard deviation of the control group. All regressions include controls and fixed effects.

J Contact Effects on Private and Public Behavior

This appendix examines whether the effects of contact extend beyond the attitudinal responses measured in the virtual enumerator experiment to two types of behavioral outcomes: self-reported intentions to take private and public actions and behavior measured through the Gleam Contest. I first report detailed results for the behavioral intentions and then examine the contest outcomes.

As discussed in Appendix G.2, these outcomes provide less internally controlled tests of private–public differences than the virtual enumerator experiment because most private and public actions differ not only in visibility but also in their perceived benefits and effort costs. I therefore estimate treatment effects separately for the private and public action indices to evaluate H4 and H5, but do not compare these indices directly to test H6. Direct private–public comparisons instead focus on two approximately matched pairs of intentions: reading versus sharing an article and signing a petition privately versus publicly. These pairs

provide the closest behavioral approximation to the virtual enumerator comparison, although they do not hold the underlying action fully constant across visibility conditions. The Gleam Contest similarly evaluates the preregistered average private and public predictions in H4 and H5, but its smaller experimental cells do not support further analysis by social costs.

J.1 Pooled Effects on Intentions to Take Private and Public Actions

I first conduct the preregistered tests of the effects of contact on intentions to undertake private and public actions (H4 and H5). The preregistered indices exhibit high internal consistency. Cronbach's α is 0.90 for the private action index and 0.85 for the public action index in the control condition, with corresponding values of 0.91 and 0.86 in the contact condition.

An exploratory factor analysis (see replication files) primarily identifies a strong common dimension: the first factor has an eigenvalue of 5.10, and all eight items load strongly on it, with loadings ranging from 0.70 to 0.89. Additional factors have substantially smaller eigenvalues and do not reproduce a clean private–public structure. When the analysis is constrained to two factors, the four public actions load predominantly on the same factor, providing some reassurance that they form a coherent set. The two private voting items load predominantly on the other factor, while the remaining private actions—reading an article and signing a petition anonymously—load on both. This pattern is similar in the contact and control conditions.

The absence of a clean private–public factor structure is somewhat surprising but does not undermine the theoretical distinction between the indices. The indices classify actions according to whether their performance would be visible to others—an observable feature of the action rather than a distinct latent disposition. Visibility is theorized to moderate responsiveness to treatment, even if it does not strongly organize baseline willingness. The factor structure also indicates that the private index contains substantively different kinds of actions, consistent with the item-level variation reported below.

Table J.1.1 reports the average effects of parasocial contact on intentions to take private and public actions. I highlight two main results. First, the estimated effect on the private action index is positive but not statistically significant (0.07 SD, $p = .168$). It is worth noting that the aggregate estimate masks meaningful item-level variation. Contact had little effect on respondents' willingness to vote for either a Black or White candidate who supports racial equality. By contrast, it increased willingness to read an article about racial inequality by an estimated 0.12 units ($p = .085$) and willingness to sign an anonymous petition by 0.16 units ($p = .038$). One possible explanation is that vote choice is a higher-stakes decision shaped by considerations extending beyond racial attitudes, making it less responsive to contact than lower-stakes actions such as reading an article or signing an anonymous petition.

Second, the contact treatment increased the public action index by 0.11 SD ($p = .017$), with positive point estimates across all four component behaviors and statistically significant effects on willingness to confront a friend and sign a public petition. The average behavioral intention results therefore provide mixed support for H4 and H5.

Table J.1.1: Effects of Contact on Intentions to Take Private and Public Action

	Control	Contact Effects			N	
	mean	coef	se	p-val	Full	Para
Panel A: Private actions						
Private action index	0.00	0.07	(0.05)	0.168	1542	1037
Vote Black candidate	3.77	-0.01	(0.07)	0.900	1542	1037
Vote white candidate	3.73	-0.04	(0.07)	0.599	1542	1037
Read an article	3.93	0.12*	(0.07)	0.085	1542	1037
Sign a private petition	3.72	0.16**	(0.08)	0.038	1542	1037
Panel B: Public actions						
Public action index	0.00	0.11**	(0.05)	0.017	1542	1037
Confront friend	3.75	0.14**	(0.07)	0.040	1542	1037
Attend a public protest	2.81	0.13	(0.08)	0.117	1542	1037
Share an article	2.95	0.08	(0.08)	0.300	1542	1037
Sign a public petition	3.59	0.20***	(0.08)	0.008	1542	1037

Notes: *p<.10, ** p<.05, *** p<.01. Table reports the effects of the contact treatment on intention to take private and public actions. Pre-registered private and public action indices are standardized using the mean and standard deviation of the control group; index components are presented in their original five-point response scales. Control means are calculated among respondents assigned to the ingroup control condition. Robust standard errors appear in parentheses. "Full" reports the total estimation sample, and "Para" reports the number of respondents in the parasocial contact and control conditions.

J.2 Effects on Intentions to Take Private and Public Actions By Social Cost

J.2.1 Matched Actions Analysis

Table J.2.1 presents the results corresponding to Figure 5 in the main text and in the format of Table A.4.2. Panel A combines the two matched pairs: the private-action measure averages intentions to read an article privately and sign a petition anonymously, while the public-action measure averages intentions to share the article publicly and sign the petition publicly (see Appendix F for details). Panels B and C report the component results for reading versus sharing and private versus public petition signing, respectively.

Although the estimates are imprecise, their pattern is remarkably consistent with the predictions. Looking first at Panel A, among respondents facing higher social costs, contact increased private-action intentions by 0.10 units ($p = .381$) but had approximately no effect on public-action intentions (0.00, $p = .993$), producing a negative public–private difference of -0.10 ($p = .172$). Panels B and C show that this pattern—suggestively larger private than public effects among higher-cost respondents—emerges for both matched pairs.

Among respondents facing lower social costs, by contrast, contact had positive private and public effects and a relatively small, positive public–private difference. This pattern is consistent with public movement being less constrained relative to private movement when social costs are low.[13] Together, the patterns in the two groups produce negative higher–lower differences in the public–private contrast in all three panels. For the combined measure in Panel A, this difference is -0.16 units ($p = .076$), as predicted by $H6_{SC}$.

The higher–lower comparisons help identify the source of this result. Panel A shows that the effect of contact on public intentions is 0.23 units smaller among higher-cost respondents ($p = .110$), which is directionally consistent with $H5_{SC}$. The same pattern emerges for both the read–share and private–public petition pairs. By comparison, the higher–lower difference in private effects for the combined measure is small (-0.06, $p = .648$). The results are therefore consistent with the more negative public–private pattern under higher costs arising primarily from attenuation of the public effect rather than from broader differences in responsiveness between the groups.

Overall, the matched-action estimates are imprecise, but their consistency across the two pairs is notable. The component effects differ from those observed for trait evaluations in the virtual enumerator experiment,

[13] As shown in Table A.4.2, the direction of the public–private difference within the lower-cost group is theoretically indeterminate.

but the resulting public–private patterns are consistent with the same predictions. This variation is also consistent with the framework, which treats each outcome as a distinct expression decision with its own benefits, costs, and threshold.

Table J.2.1: Effects of Contact on Matched Private and Public Behavioral Intentions by Social Cost

PANEL A: MATCHED ACTIONS (COMBINED)			
	Private Effects	Public Effects	Public-Private
Higher Cost	0.10	0.00	-0.10
	(0.12)	(0.11)	(0.07)
	0.381	0.993	0.172
Lower Cost	0.16**	0.23***	0.06
	(0.08)	(0.09)	(0.05)
	0.041	0.008	0.256
Higher-Lower	-0.06	-0.23	-0.16*
	(0.14)	(0.14)	(0.09)
	0.648	0.110	0.076
PANEL B: READ VS. SHARE ARTICLE			
	Private Effects	Public Effects	Public-Private
Higher Cost	0.11	-0.04	-0.15
	(0.12)	(0.13)	(0.12)
	0.373	0.771	0.232
Lower Cost	0.13	0.16	0.03
	(0.09)	(0.10)	(0.10)
	0.139	0.125	0.727
Higher-Lower	-0.02	-0.20	-0.18
	(0.15)	(0.17)	(0.16)
	0.916	0.234	0.248
PANEL C: PRIVATE VS. PUBLIC PETITION			
	Private Effects	Public Effects	Public-Private
Higher Cost	0.09	0.04	-0.05
	(0.13)	(0.13)	(0.07)
	0.497	0.763	0.486
Lower Cost	0.20**	0.29***	0.09*
	(0.10)	(0.09)	(0.06)
	0.04	0.002	0.096
Higher-Lower	-0.11	-0.25	-0.14
	(0.16)	(0.16)	(0.09)
	0.497	0.113	0.120

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Cells report the estimated effect of parasocial contact on matched behavioral intention outcomes. Panel A combines the two matched pairs: the private measure is the equally weighted mean of intentions to read an article privately and sign a petition privately, while the public measure is the equally weighted mean of intentions to share the article publicly and sign the petition publicly. Panel B compares private reading with public sharing, and Panel C compares private with public petition signing. All measures are reported in their original units. The third column reports the difference between the public and private treatment effects (public minus private). The final row of each panel reports the difference between the conditional treatment effects (higher minus lower social costs); its final cell is the corresponding difference-in-differences. Estimates in the first two columns are obtained from separate regressions for each outcome. Differences in the third column are calculated across regressions using `suest` to account for cross-equation covariance. Heteroskedasticity-robust standard errors are in parentheses, with p-values below.

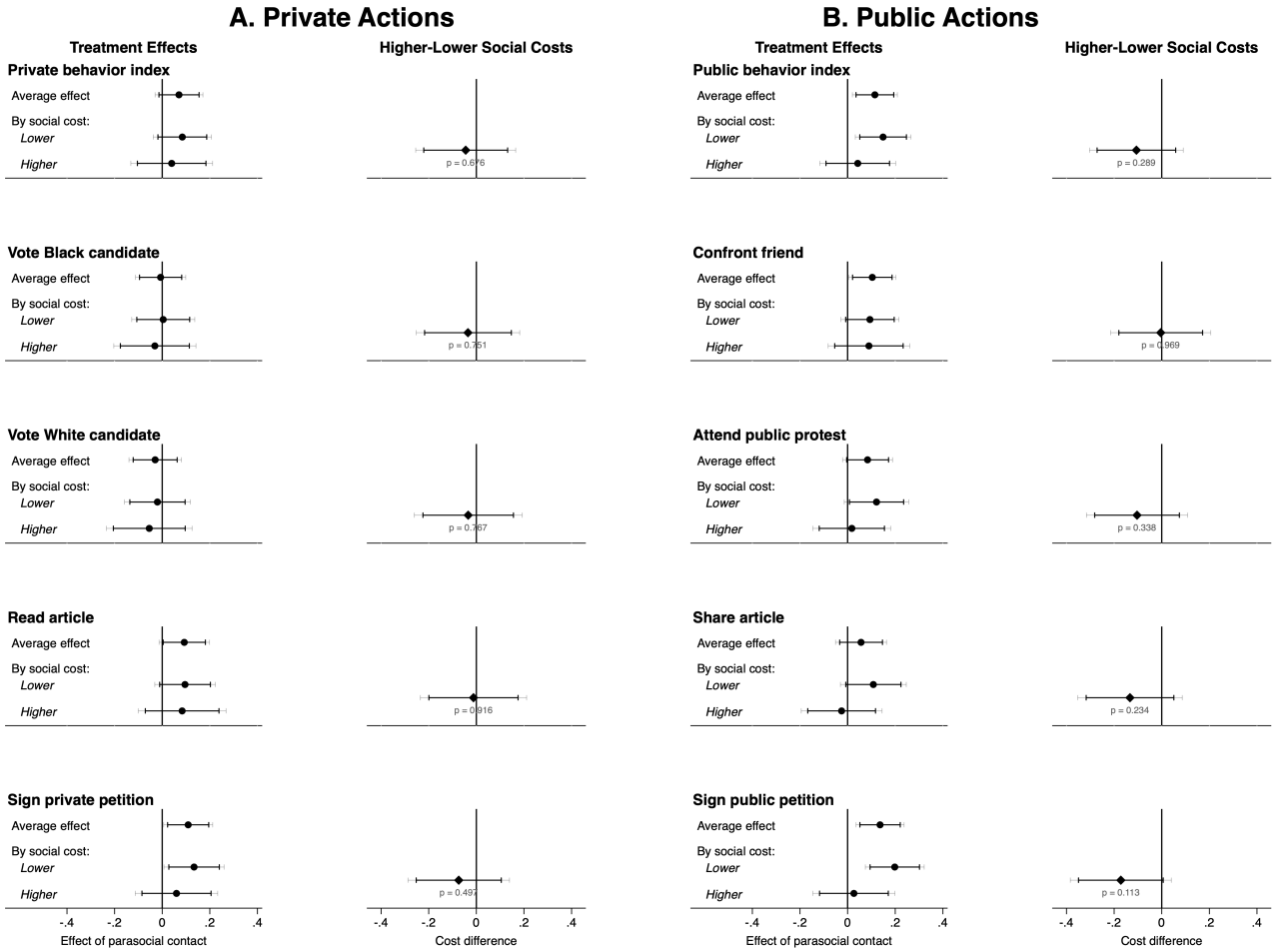
J.2.2 Results for the Broader Behavioral Intention Measures

Figure J.2.1, with corresponding regressions in Table J.2.2, examines whether the broader set of behavioral intentions exhibits the same cross-group pattern. The results provide additional, suggestive support for H5$_{SC}$. The positive average effect of contact on public intentions is concentrated largely among respondents

facing lower social costs. Contact increased the public-action index by 0.15 standard deviations ($p = .012$) among lower-cost respondents, compared with 0.04 standard deviations ($p = .600$) among higher-cost respondents. The resulting higher–lower difference is -0.11 standard deviations, in the predicted direction but imprecisely estimated ($p = .289$). The same directional pattern appears for three of the four items in the public-action index.

The corresponding private-action results largely reflect the matched-action analysis presented above. The higher–lower differences for private reading and petition signing are smaller than those for their corresponding public actions, while the two voting items show no detectable treatment effects in either cost group. The cross-group divergence is therefore concentrated more strongly among public actions, consistent with $H5_{SC}$. These results remain suggestive because the cross-group estimates are imprecise and the component outcomes are correlated rather than independent replications.

Figure J.2.1: Effects on Intentions to Take Private and Public Actions by Social Cost



Notes: Points report the estimated effects of parasocial contact on each behavioral intention outcome in the full sample and among respondents facing lower and higher social costs. Horizontal lines show 90 and 95 percent confidence intervals. Diamonds report differences between the conditional effects (higher minus lower costs); negative estimates indicate smaller effects among higher-cost respondents. Index outcomes are standardized, while individual items are reported in their original units. Models adjust for the preregistered covariates and randomization strata and use heteroskedasticity-robust standard errors.

Table J.2.2: Contact Effects on Intentions to Take Private and Public Actions by Social Cost

	I. PRIVATE ACTIONS					II. PUBLIC ACTIONS				
	Index	Components				Index	Components			
	Private Actions	Vote Black	Vote White	Read Article	Private Petition	Public Actions	Confront Friend	Participate Protest	Share Article	Public Petition
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)
Contact (Lower Cost)	0.08	0.01	-0.03	0.13	0.20**	0.15**	0.12	0.18*	0.16	0.29***
	(0.06)	(0.09)	(0.09)	(0.09)	(0.10)	(0.06)	(0.08)	(0.10)	(0.10)	(0.09)
	0.175	0.949	0.777	0.139	0.038	0.012	0.130	0.079	0.125	0.002
Contact (Higher Cost)	0.04	-0.04	-0.07	0.11	0.09	0.04	0.12	0.03	-0.04	0.04
	(0.09)	(0.12)	(0.12)	(0.12)	(0.13)	(0.08)	(0.12)	(0.13)	(0.13)	(0.13)
	0.649	0.726	0.554	0.373	0.497	0.600	0.305	0.831	0.771	0.763
Difference (Higher-Lower)	-0.04	-0.05	-0.05	-0.02	-0.11	-0.11	-0.01	-0.16	-0.20	-0.25
	(0.11)	(0.15)	(0.15)	(0.15)	(0.16)	(0.10)	(0.14)	(0.16)	(0.17)	(0.16)
	0.676	0.751	0.767	0.916	0.497	0.289	0.969	0.338	0.234	0.113
Higher Cost (Control)	-0.11	-0.16	-0.10	-0.20*	-0.08	-0.14*	-0.42***	0.01	0.00	-0.05
	(0.08)	(0.11)	(0.11)	(0.11)	(0.12)	(0.08)	(0.11)	(0.12)	(0.12)	(0.12)
	0.160	0.166	0.371	0.079	0.541	0.065	0.000	0.927	0.985	0.669
Control Mean	0.18	4.00	3.92	4.16	3.93	0.22	4.09	3.03	3.15	3.81
N	1542	1542	1542	1542	1542	1542	1542	1542	1542	1542
Controls	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes
Fixed Effects	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes	Yes

Notes: *p<.10, ** p<.05, *** p<.01. Each column reports results from a single regression interacting parasocial contact with an indicator for higher social costs. "Contact (Lower Cost)" reports the estimated treatment coefficient; "Difference (Higher − Lower)" reports the coefficient on the interaction between parasocial contact and higher social costs; and "Higher Cost" reports the coefficient on the higher-cost indicator. "Contact (Higher Cost)" reports the calculated marginal treatment effect among respondents facing higher social costs. Control means are calculated among lower-cost respondents assigned to the ingroup control condition. Action indices are standardized using the mean and standard deviation of the control group; individual items are presented in their original response scales. All models include covariates and strata fixed effects. Robust standard errors appear in parentheses, with p-values reported below.

J.3 Robustness Checks

J.3.1 General Robustness Checks

Table J.3.1 assesses the robustness of the contact effects on public-action intentions to alternative measures of higher social costs. The pattern is consistent across measures: contact increases public-action intentions among respondents facing lower social costs but has little effect among those facing higher costs, producing negative but imprecisely estimated interactions.

For completeness, additional sensitivity analyses reported in the replication code exclude respondents who provided identical answers across the behavioral intention items. Approximately 13 percent of respondents exhibited this pattern, and contact reduced its prevalence by about four percentage points. Excluding these respondents produces the same general pattern of larger public effects among lower- than higher-cost respondents. These estimates should be interpreted cautiously, however, because response invariance is measured after treatment and is itself affected by treatment assignment.

J.3.2 Exploratory Results by Outgroup Couple

Figure J.3.1 and Table J.3.2 examine effects on the combined matched private and public behavioral intention measures separately by outgroup couple. As in the main analysis, I compare the private and public effects among respondents facing higher social costs and examine how that difference varies across cost groups. These couple-specific analyses were not preregistered and should be interpreted as exploratory.

For Couple B, the average effects on the matched private and public measures were small and statistically indistinguishable from zero. Among higher-cost respondents, the private effect was close to zero, while the public effect was negative, producing a public–private difference of -0.18 ($p = .035$). The corresponding difference was positive among lower-cost respondents, yielding a higher–lower difference in the public–private contrast of -0.27 ($p = .016$). The Couple B results therefore indicate that the public–private pattern varies with perceived social costs, but they do not exhibit the full predicted pattern of positive private change accompanied by smaller, nonnegative public change. Moreover, the cross-group contrast is driven largely

Table J.3.1: Contact Effects on Intentions to Take Public Action: Robustness

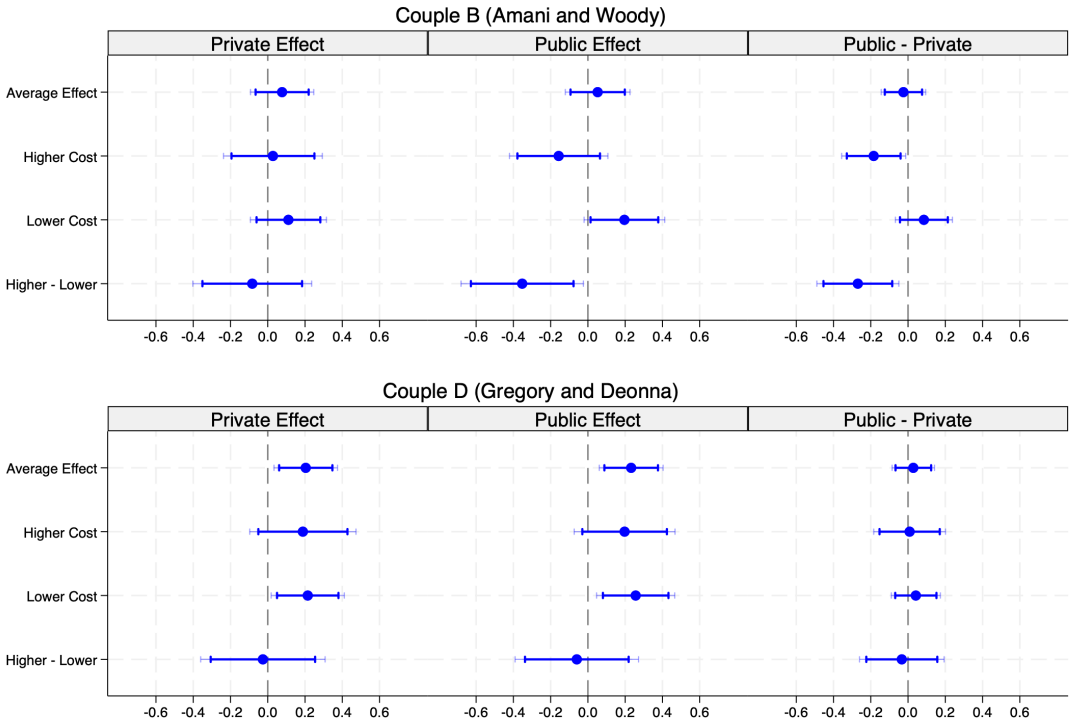
	DV: PUBLIC ACTION INDEX					
	Direction		Direction + Network		Direction + Fear	
	Factor	Either	**Factor**	Either	Factor	Either
Contact Effect (Lower Cost)	0.16**	0.17**	**0.15****	0.13**	0.13**	0.13**
s.e.	(0.06)	(0.08)	**(0.06)**	(0.07)	(0.06)	(0.06)
pval	0.014	0.028	**0.012**	0.044	0.025	0.025
Higher Cost	-0.17**	-0.08	**-0.14***	-0.03	-0.10	-0.04
s.e.	(0.07)	(0.07)	**(0.08)**	(0.07)	(0.08)	(0.08)
pval	0.023	0.253	**0.065**	0.706	0.237	0.564
Contact*Higher Cost	-0.11	-0.10	**-0.11**	-0.06	-0.10	-0.09
s.e.	(0.10)	(0.10)	**(0.10)**	(0.10)	(0.12)	(0.11)
pval	0.276	0.329	**0.289**	0.558	0.381	0.390
Contact Effect (Higher Cost)	0.05	0.07	**0.04**	0.08	0.02	0.04
s.e.	(0.07)	(0.06)	**(0.08)**	(0.07)	(0.10)	(0.09)
pval	0.472	0.238	**0.600**	0.274	0.816	0.647

Notes: The table reports contact effects on the public-action index using alternative measures of social costs. Direction measures capture perceived ingroup norms using either a standardized factor score (Factor) or the two component indices (Either). Direction + Network additionally incorporates racial network composition, while Direction + Fear incorporates fear of social sanctioning. The outcome index is standardized using the control-group mean and standard deviation. All models adjust for the preregistered covariates and randomization strata and use heteroskedasticity-robust standard errors. * $p < .10$, ** $p < .05$, *** $p < .01$.

by variation in public effects across observed cost groups and cannot be attributed exclusively to social costs.

Couple D produced positive average effects on both matched measures. The private and public effects remained similar within each cost group: among higher cost respondents, the estimated effects were 0.19 and 0.20, respectively, and the higher–lower difference in the public–private contrast was close to zero. Overall, the results suggest possible couple-level variation in how contact affected private and public behavioral intentions. The analysis does not establish that the patterns differ reliably across couples, however, and the couple-specific estimates should not be interpreted as independent tests of the framework.

Figure J.3.1: Effects on Intentions to Take Matched Actions By Couple



Notes: Points report the effects of each outgroup couple relative to the ingroup control condition. The matched private measure averages intentions to read an article privately and sign an anonymous petition; the matched public measure averages intentions to share the article publicly and sign a public petition. "Average Effect" reports full-sample effects; "Lower Cost" and "Higher Cost" report effects by social-cost group; and "Higher − Lower" reports their difference. The third column reports public-minus-private effects, making its final row the difference-in-differences. Thick and thin lines show 90 and 95 percent confidence intervals, respectively. Outcomes range from 0 to 1. Models adjust for the preregistered covariates and randomization strata and use robust standard errors. Private–public comparisons use `suest`.

Table J.3.2: Effects on Intentions to Take Matched Actions by Couple

	Panel A: Couple B		
Social Costs	**Private Effects**	**Public Effects**	**Public - Private**
Average Effect	0.08	0.05	-0.02
	(0.09)	(0.09)	(0.06)
	0.374	0.555	0.684
Higher Cost	0.03	-0.16	-0.18**
	(0.14)	(0.13)	(0.09)
	0.838	0.245	0.035
Lower Cost	0.11	0.20*	0.08
	(0.10)	(0.11)	(0.08)
	0.287	0.076	0.276
Higher - Lower	-0.08	-0.35**	-0.27**
	(0.16)	(0.17)	(0.11)
	0.609	0.035	0.016

	Panel B: Couple D		
Social Costs	**Private Effects**	**Public Effects**	**Public - Private**
Average Effect	0.20**	0.23***	0.03
	(0.09)	(0.09)	(0.06)
	0.019	0.008	0.627
Higher Cost	0.19	0.20	0.01
	(0.15)	(0.14)	(0.10)
	0.195	0.154	0.931
Lower Cost	0.21**	0.26**	0.04
	(0.10)	(0.11)	(0.07)
	0.032	0.016	0.534
Higher - Lower	-0.03	-0.06	-0.03
	(0.17)	(0.17)	(0.12)
	0.878	0.725	0.773

Notes: * $p < .10$, ** $p < .05$, *** $p < .01$. Cells report the effects of contact disaggregated by outgroup couple on the matched private and public action outcomes. "Average Effect" reports full-sample effects; "Lower Cost" and "Higher Cost" report effects by social cost group; and "Higher − Lower" reports their difference. The third column reports public-minus-private effects, making its final row the difference-in-differences. Outcomes are measured in their original scale. Models adjust for the preregistered covariates and randomization strata and use robust standard errors. Private–public comparisons use `suest`. Standard errors are in parentheses, with p-values below.

J.4 Gleam Contest

Table J.4.1 presents the effects of contact on actual behavior measured through the Gleam Contest. Overall, approximately 55 percent of respondents entered the contest, and 21 percent took at least one action. Willingness to act differed substantially across conditions: 35 percent of respondents offered private actions took at least one, compared with only 6 percent of those offered public actions. While this difference is itself notable, there is little evidence that contact affected either private or public race-related actions, whether considered on their own or relative to non-race-related actions. These results provide little support for H4, which predicted a positive private effect, but are consistent with H5's prediction of a null or small positive public effect.

Although null, these results remain theoretically informative. A public null is difficult to interpret on its own because public behavior requires the personal value of acting to overcome both effort and social costs. The private outcome provides diagnostic leverage because it entails effort costs without the additional costs of public expression. A positive private effect paired with a null public effect would therefore suggest that social costs inhibited the translation of changed preferences into public behavior. That is not the pattern observed here: contact affected neither private nor public action. Within the framework, this joint null is consistent with the post-treatment personal value of acting $(B_i + \delta_{B,i})$ being insufficient to overcome even the effort costs associated with private behavior. The public null therefore cannot be attributed specifically to social costs. Social costs may have further raised the threshold for public action, but they were not the only barrier preventing behavioral change.

Although null, these results remain theoretically informative. A public null is difficult to interpret on its own because public behavior requires the personal value of acting to overcome both effort and social costs. The private outcome provides diagnostic leverage because it entails effort costs without the additional costs of public expression. Here, contact affected neither private nor public action, which is consistent with the post-treatment personal value of acting $(B_i + \delta_{B,i})$ being insufficient to overcome even the effort costs associated with private behavior. The public null therefore cannot be attributed specifically to social costs, although those costs may have further raised the threshold for public action. For intervention design, this pattern points first to strengthening the treatment-induced change in personal value or reducing the effort required to act; reducing social costs may also be useful, but is unlikely to generate public behavior if the threshold for private action has not yet been crossed.

Table J.4.1: Contact Effects on Private vs. Public Behavior

	Control	Contact Effects			N	
	mean	coef	se	p-val	Full	Para
Private contest entry	0.55	-0.03	(0.04)	0.514	770	520
Total private difference	-0.03	0.04	(0.03)	0.189	770	520
Total race related private actions	1.01	-0.08	(0.14)	0.551	770	520
Netflix BLM	0.28	-0.03	(0.04)	0.434	770	520
Documentary	0.26	-0.01	(0.04)	0.734	770	520
Trailer	0.30	-0.04	(0.04)	0.272	770	520
Petition	0.17	0.00	(0.03)	0.897	770	520
Total nonrace related private actions	1.04	-0.12	(0.14)	0.367	770	520
Netflix	0.29	-0.04	(0.04)	0.304	770	520
Documentary	0.28	-0.04	(0.04)	0.255	770	520
Trailer	0.29	-0.03	(0.04)	0.386	770	520
Petition	0.19	-0.01	(0.04)	0.865	770	520
Publlic contest entry	0.56	-0.04	(0.04)	0.425	780	523
Total public difference	0.00	0.01	(0.01)	0.697	780	523
Total race related public actions	0.12	0.00	(0.05)	0.993	780	523
Netflix BLM	0.03	0.00	(0.01)	0.827	780	523
Documentary	0.03	0.00	(0.02)	0.788	780	523
Trailer	0.04	0.01	(0.02)	0.769	780	523
Petition	0.03	0.00	(0.01)	0.853	780	523
Total nonrace related public actions	0.13	0.00	(0.05)	0.931	780	523
Netflix	0.04	0.00	(0.02)	0.804	780	523
Documentary	0.03	0.00	(0.01)	0.844	780	523
Trailer	0.04	0.00	(0.02)	0.890	780	523
Petition	0.03	0.00	(0.02)	0.774	780	523

Notes: * p<.10, ** p<.05, *** p<.01. Table shows the effects of the contact treatment on actual behavior (gleam contest) outcomes where each row is a different outcome.

K Multiple Hypothesis Correction

K.1 Correction

Table K.1.1 reports sharpened q-values (Anderson, 2008) for each family of tests, with families defined by the hypothesis structure in Table G.1.1. The pre-analysis plan specified correction for the average treatment effects; I extend the same procedure to the public–private tests.

Panel A reports the average treatment effects. Within the primary families, the effects on both main outcomes remain significant after correction (warmth index and support for social change, both $q = .023$), as does the effect on the public action index ($q = .036$); the effect on the private action index is weaker ($q = .092$). The contest outcomes show no detectable effects before or after correction. Among the mechanism outcomes, the effect on positive trait evaluations does not remain significant within the personal-beliefs family ($q = .220$), though it falls to $q = .082$ in the broader attitudinal family, where the main-outcome effects lower the estimated share of true null hypotheses. Under the correction across all thirteen outcomes, no individual test remains significant at conventional levels; the smallest values are $q = .125$ for the warmth and public action indices.

Panels B through F report the public–private tests. In the pooled sample, the private effects on warmth and positive traits remain significant at the 90 percent confidence level after correction (both $q = .080$), and the pooled private–public differences show no detectable asymmetry on any outcome. Among higher-cost respondents, the private effect on positive trait evaluations remains significant ($q = .022$; $q = .044$ when private and difference tests are corrected jointly). The corresponding private–public difference does not ($q = .145$; $q = .118$ jointly), nor does the cross-group triple interaction ($q = .145$). The behavioral tests show

no detectable effects after correction, with the cross-group comparison on the combined matched-actions measure the strongest ($p = .076$).

These results support the average treatment effects on the main outcomes, while the public–private asymmetry should not be treated as confirmed. The higher-cost trait result is best read as initial evidence that the divergence the framework describes can occur: the private effect survives correction, the public estimate among the same respondents is close to zero, and the index, the cross-group coefficient, and the continuous-norms specification point in the same direction.

Table K.1.1: Multiple Hypothesis Correction: Unadjusted p-Values and Sharpened q-Values by Family

Panel A. Average treatment effects (H1–H3)

		Uncorr. p	Main Outcomes (P)	Personal Beliefs (P)	Behavioral Intentions (P)	Contest Behavior (P)	Attitudes Combined (B)	All (B)
Main outcomes	Warmth index	0.011	0.023				0.071	0.125
	Support for social change	0.034	0.023				0.082	0.142
	Private action index	0.168			0.092			0.358
	Public action index	0.017			0.036			0.125
	Entered contest, private	0.514				1.000		0.615
	Entered contest, public	0.425				1.000		0.615
	Actions, private	0.189				1.000		0.358
	Actions, public	0.697				1.000		0.615
Mechanism-related	Positive traits index	0.045		0.220			0.082	0.142
	Similarity index	0.579		0.938			0.477	0.615
	Empathy	0.645		0.938			0.477	0.615
	Comfort around (difference)	0.383		0.938			0.404	0.615
	Perceived social norms[a]	0.205						0.358
	Tests in family (k)		2	4	2	4	6	13

Panel B. Public–private attitudes, pooled (H4–H6), pre-registered

		Uncorr. p	H4 Private	H5 Public	H6 Difference	All (B)
Private	Warmth index	0.049	0.080			0.173
	Support for social change	0.340	0.128			0.507
	Positive traits index	0.028	0.080			0.173
Public	Warmth index	0.213		0.271		
	Support for social change	0.039		0.133		
	Positive traits index	0.541		0.470		
Difference	Warmth index	0.670			1.000	0.808
	Support for social change	0.420			1.000	0.507
	Positive traits index	0.258			1.000	0.507
	Tests in family (k)		3		3	6

Panel C. Public–private attitudes, higher social costs ($\mathbf{H4}_{HC}$–$\mathbf{H6}_{HC}$), exploratory

		Uncorr. p	$H4_{HC}$ Private	$H5_{HC}$ Public	$H6_{HC}$ Difference	All (B)
Private	Warmth index	0.400	0.396			0.628
	Support for social change	0.425	0.396			0.628
	Positive traits index	0.007	0.022			0.044
Public	Warmth index	0.353		0.546		
	Support for social change	0.053		0.190		
	Positive traits index	0.867		1.000		
Difference	Warmth index	0.934			1.000	1.000
	Support for social change	0.482			0.931	0.628
	Positive traits index	0.042			0.145	0.118
	Tests in family (k)		3		3	6

Panel D. Cross-group comparison, attitudes ($\mathbf{H6}_{SC}$), exploratory secondary

		Uncorr. p	$H6_{SC}$ Triple interaction
Triple interaction	Warmth index	0.997	1.000
	Support for social change	0.779	1.000
	Positive traits index	0.042	0.145
	Tests in family (k)		3

Panel E. Public–private behavior, matched pairs, higher social costs ($\mathbf{H4}_{HC}$–$\mathbf{H6}_{HC}$)

		Uncorr. p	$H4_{HC}$ Private	$H5_{HC}$ Public	$H6_{HC}$ Difference	All (B)
Private	Matched actions (combined)[b]	0.381	0.381			0.525
Public	Matched actions (combined)[b]	0.993		0.993		
Difference	Matched actions (combined)[b]	0.172			0.172	0.525
	Tests in family (k)		1		1	2

Panel F. Cross-group comparison, behavior ($\mathbf{H6}_{SC}$), exploratory secondary

		Uncorr. p	$H6_{SC}$ Triple interaction
Triple interaction	Matched actions (combined)[b]	0.076	0.076
	Tests in family (k)		1

Notes. Each row reports one test. Columns report correction families, and k gives the number of tests in each family. Families are defined by the hypothesis structure in Table G.1.1, not by the observed estimates. (P) denotes a primary family; (B) denotes a broader family definition, reported as a sensitivity check. Within each family, p-values are adjusted using sharpened q-values as in Anderson (2008). Because the procedure estimates the number of true null hypotheses from the family itself, a q-value can fall below its unadjusted p-value when most tests in a family have small p-values, and a test can receive a smaller q-value in a broader family than in a narrower one. The broader families are therefore not uniformly more conservative than the primary families. Families with $k = 1$ require no adjustment, and unadjusted p-values are reported.

Panel A. Primary families follow the outcome groupings in the pre-analysis plan. The broader families combine all attitudinal outcomes and all outcomes. The four components of the positive traits index are reported as decomposition and are not counted as separate tests.

Panels B and C. Primary families correct the private (H4) and difference (H6) tests separately across the three attitude outcomes; the broader column treats them as a single family of six. Public estimates (H5) are implied by H4 and H6, since the difference equals the private estimate minus the public estimate. Their adjusted p-values are reported for completeness. Panel B is pre-registered and Panel C is exploratory, so they are corrected as separate families.

Panel E. Panel E applies the structure of Panel C to the combined matched-actions measure. Each primary family contains a single test; the broader column treats the private and difference tests as a family of two.

Panels D and F. The cross-group comparison is estimated on the full sample and tests whether the public–private difference is larger among higher-cost than lower-cost respondents. It forms a secondary exploratory family, defined separately for attitudes and behavior because the outcome types differ.

[a] Perceived social norms (H3) predicts a null (or small positive) effect and would be rejected by a significant negative effect. Because it primarily predicted a null it is only included in the all-outcomes family.

[b] The combined measure aggregates the individual matched private and public actions. The individual pairs are reported as decomposition and are not counted as separate tests.

L Relationship to the Pre-Analysis Plan

The study was preregistered on September 9, 2023, before data collection began. This appendix explains the relationship between the preregistered analysis and the analysis presented in the paper, including deviations from the pre-analysis plan (PAP) and analytic decisions that, while consistent with it, warrant further explanation.

Role of the conceptual framework. The hypotheses were preregistered before the complete threshold framework was developed. The framework therefore does not generate the preregistered hypotheses after the fact. Rather, it formalizes the theoretical logic underlying them and clarifies the individual transitions, population-level conditions, and cross-domain comparability assumptions required for the anticipated private–public pattern to emerge. The preregistered hypotheses are evaluated as written. Predictions introduced by the completed framework are separately labeled and treated as exploratory.

Preregistered and conditional hypotheses. H4–H6 were motivated by socially costly expression. The PAP repeatedly stated that changes in personal beliefs might not be expressed publicly when favorable expression remains socially costly. For example, p. 2 states: "Parasocial contact, if it primarily operates on personal beliefs, might alter how a person thinks or feels about an outgroup in private but have no impact on what they say or do publicly if expressing tolerance or support for social change remains socially costly." See also p. 9: "Changes in personal beliefs will not necessarily be expressed publicly if doing so would be socially costly." Because the hypotheses themselves were not formally conditioned on social costs, however, H4–H6 remain pooled tests. In formulating them, I implicitly treated respondents as varying in the magnitude of the social costs they faced and did not consider that favorable public expression might instead carry social benefits for some respondents. This led to the expectation that the private–public pattern would hold across the sample, though more strongly among respondents facing higher costs.

The completed framework clarifies the limits of that expectation. Even among respondents facing social costs, the population-level pattern depends on the relative prevalence of private-only and public-only transitions. Moreover, respondents for whom favorable public expression carries social benefits may exhibit a weaker or reversed pattern. A pooled prediction may therefore fail even when the expected asymmetry is present among respondents facing higher costs. $H4_{HC}$–$H6_{HC}$ make the conditional predictions for the higher-cost group explicit, while $H5_{SC}$ and $H6_{SC}$ compare the relevant effects across higher- and lower-cost respondents. These hypotheses do not replace H4–H6, which are evaluated as preregistered. They refine the scope conditions of the original argument and are exploratory in formal status.

Heterogeneous effects by social cost. The PAP preregistered heterogeneity analyses by social cost for the vicarious contact treatment but not for the parasocial contact treatment. That distinction reflected the initial expectation that vicarious contact, which was designed to shift perceived ingroup norms, would be especially consequential for respondents facing higher social costs. Nevertheless, the relevance of social costs to parasocial contact was also central to the preregistered argument. The PAP stated that parasocial contact might change private beliefs without changing public expression when favorable expression remained socially costly (see above). The conditional parasocial-contact analyses extend this preregistered theoretical mechanism to the treatment arm examined in this paper but were not themselves preregistered. They are therefore presented as exploratory tests rather than as replacements for the pooled hypotheses.

Operationalizing higher social costs. The PAP conceptualized higher social costs as combining three features: perceived prejudiced ingroup norms, exposure to a relevant ingroup network, and susceptibility to ingroup sanctioning (pp. 8, 22, 37). It proposed identifying higher-cost respondents as those meeting all three conditions. In practice, intersecting the three measures produced a small group and unstable estimates in the triple-interaction models. I therefore use a measure combining perceived prejudiced ingroup norms with the racial homogeneity of respondents' social networks as the primary measure throughout the analysis (see Appendix D). This definition produces the largest of the theoretically relevant higher-cost groups, reduces the problem of sparse cells, and allows the same classification to be used consistently across outcomes. I use the measure combining perceived norms with fear of sanctioning as a secondary specification and report

robustness to multiple alternative definitions, including measures based on perceived norms alone and the original three-component definition, in Appendices I.3 and J.3.

Outgroup and difference outcomes. The PAP designated Black-minus-White difference measures as the primary outcomes when parallel evaluations of Black and White Americans were available. The paper instead emphasizes evaluations of Black Americans because these provide the most direct measure of whether outgroup contact improved attitudes toward the outgroup. This change in emphasis is a departure from the PAP. It became important because the ingroup contact condition could itself affect evaluations of White Americans, causing difference scores to change even when evaluations of Black Americans did not. To preserve the preregistered analysis, the paper reports Black, White, and Black-minus-White results in Appendix H. The difference measures remain useful for establishing whether changes are specific to evaluations of Black Americans rather than reflecting a general shift in evaluations of both groups. Analyses of private and public attitudes focus on evaluations of Black Americans.

Positive-trait index. The PAP specified a five-item positive-trait index that included perceptions of whether Black and White Americans were nonaggressive. The main analysis uses a four-item index comprising evaluations of whether group members are hardworking, intelligent, family-oriented, and committed in relationships. The nonaggressive item was excluded after examining its component results because its favorable Black-minus-White treatment effect arose primarily from changes in evaluations of White Americans rather than improved evaluations of Black Americans. This exclusion was not preregistered. The five-item index specified in the PAP is also reported in Appendix H; substantive conclusions are similar using either measure.

Matched behavioral intention analysis. The PAP preregistered separate private- and public-action indices for testing H4 and H5. It did not propose comparing the complete indices as a test of H6 because the private and public actions differ in their benefits and effort costs as well as their visibility (p. 39). The paper follows that restriction and does not interpret the difference between the full indices as a test of H6. The PAP did, however, identify private and public petition signing as a more directly comparable pair. The paper reports that preregistered comparison and adds an exploratory comparison between reading an article privately and sharing it publicly. It also combines the two pairs using equally weighted means to preserve their one-to-one correspondence. The reading–sharing comparison, the combined matched measure, and the analyses of these measures by social cost were not preregistered. They are presented as exploratory behavioral analogues to the virtual enumerator comparison, with the limitations of comparing actions that may still differ in ways other than visibility made explicit.

Similarity index. The PAP specified perceived general similarity and values similarity as separate mechanisms (p. 44). The paper combines them into a single index because they capture closely related perceptions and are empirically correlated. Combining them improves reliability and reduces the number of tests. Results for the separate items are reported in Table H.1.1.

Vicarious contact experiment. The design included a separately preregistered vicarious contact arm with public–private hypotheses paralleling those for parasocial contact (PAP, p. 10). The treatment combined ingroup parasocial contact with depictions of cross-group friendship and was intended primarily to change perceived ingroup norms. It was preregistered as a distinct experiment (pp. 41–42), rather than as a component of the parasocial-contact treatment analyzed here. Results from that experiment are being analyzed in a separate paper and are therefore outside the scope of this manuscript. The vicarious contact arm is retained in the estimation sample for precision, with assignment to that arm included in all relevant models.

Hypothesis numbering and direction of the interaction. The manuscript renumbers the PAP hypotheses to organize them by outcome and estimand; Table G.1.1 reports the exact correspondence. The PAP expressed the principal interaction hypothesis as a larger contact effect on private than public outcomes. The paper defines the contrast as public minus private and therefore expresses the same prediction as a negative interaction. This change affects only the sign used to describe the contrast, not the underlying estimand, estimation, or substantive prediction.

M Complete Survey Instrument

The complete survey instrument appears on the following pages. The survey was programmed and administered in Qualtrics, with all random assignments implemented within the platform.

ID	MEASURE	RESPONSE OPTIONS	USE	HYP TEST	INDEX/MEAUSRE
MODULE 1: SCREENING					
TEXT: Thank you for your interest in participating in this study. Please begin by answering the following questions.					
q1_age	What is your birth date? Please enter as MM/DD/YYYY (including slashes).	MM/DD/YYYY	Screening, control	--	
q2_citizen	What is your citizenship status? Are you [...]	(1) A U.S. citizen; (2) A U.S. citizen born in Puerto Rico, the U.S. Virgin, or the Northern Marianas Islands; (3) Not a U.S. citizen	Screening	--	
q3_race	What is your race? Mark all that apply.	1=White (non-Hispanic), 2=Black or African American, 3=Hispanic or Latino, 4=American Indian or Alaskan Native, 5=Native Hawaiian or Other Pacific Islander, 6=Asian, 7=Other race [ALLOW FOR MORE THAN ONE]	Screening	--	
q4_	Please look at the following list of shows and indicate for each whether you have watched at least one episode of this show previously. [RANDOMIZE ORDER]		--	--	
q4_mafs	Married at First Sight	1=Yes, 2=No, 3=Maybe/I don't know	Screening	--	
q4_bachelor	The Bachelor	1=Yes, 2=No, 3=Maybe/I don't know	Decoy	--	
q4_love	Love is Blind	1=Yes, 2=No, 3=Maybe/I don't know	Decoy	--	
q4_queen	Queen Charlotte	1=Yes, 2=No, 3=Maybe/I don't know	Decoy	--	
q4_thrones	Game of Thrones	1=Yes, 2=No, 3=Maybe/I don't know	Decoy	--	
q4_wed	Wednesday	1=Yes, 2=No, 3=Maybe/I don't know	Decoy	--	
SCREENING: Keep only if q1=18-45, q2=1, q3=1 only, q4_mafs=2					
MODULE 2: CONSENT					
Thank you for your interest in participating in this study. Please read the following information closely. At the end you will be asked if you consent to participate. This study is being conducted by researchers affiliated with The Media Research Project. The goal of the study is to better understand how people feel about the content and characters they view on television. **This study will require about 45 minutes of your time. You will watch about 25 minutes of television content and answer about 20 minutes of survey questions.** Your participation has several benefits. Your responses will help to improve understanding of how people view different characters on television and different groups in society. You will also receive compensation if you complete the survey. Additionally, upon completing the survey, you will have a chance to enter a contest to win a **$500 Amazon gift card**. There are minimal risks to participating. Your participation is completely voluntary. You can choose to withdraw from the study at any time. If you choose to participate, your responses will be kept confidential, combined with those from other participants, and published in the form of generalizations about responses as a whole. All data will be stored in a secure location accessible only to the research team. The research team retains the right to use and publish data that has no information that would reveal the identity of respondents. If you choose to participate in the contest to win the $500 Amazon gift card, you will be asked to provide an email address so that you can be informed of the results. More information on this will be provided at the end of the study. Consent I have read this consent form and I understand what is being requested of me as a participant in this study. I have been given satisfactory answers to my questions. I certify that I am at least 18 years of age.					
q5_consent	Do you consent to participate in this study?	1=Yes, 2=No	Screening	--	
SCREENING: Keep only if q5=1					
MODULE 3: PRE-TREATMENT SURVEY [DO NOT RANDOMIZE Q ORDER IN MODULE]					
TEXT: Thank you. Please provide some more information about yourself.					
q6_gender	What is your gender?	1=Female, 2=Male, 3=Prefer not to say, 4=Prefer to self describe [Write in]	Control	--	
q7_educ	What is the highest level of education you have **completed**?	1=Primary/elementary school, 2=Middle school, 3=Secondary/High School, 4=Vocational/Technical School/Associate's Degree, 5=Bachelor's Degree, 6 Graduate Degree (Masters, Ph.D., etc)	Control	--	
q8_state	What state do you live in?	Drop Down Menu	Control (by region)	--	
q9_netw	Thinking about the people you spend time with in your daily life (such as your fiends, family, neighbors, co-workers, etc.), how many would you say [...]: [RANDOMIZE ORDER]		--	--	
_netw_age	Are about the same age as you	1=almost none, 2=less than half, 3=about half, 4=more than half, 5=almost all	Decoy	--	
_netw_white	Are White	1=almost none, 2=less than half, 3=about half, 4=more than half, 5=almost all	Control/Het Effects	C2	high_soc_cost
_netw_married	Are married	1=almost none, 2=less than half, 3=about half, 4=more than half, 5=almost all	Control	--	
_netw_black	Are Black	1=almost none, 2=less than half, 3=about half, 4=more than half, 5=almost all	Control	--	
q10_sanction	Thinking about the people you spend time with in your daily life, how comfortable or uncomfortable would you be doing something that they would not approve of?	1=very comfortable, 2=somewhat comfortable, 3=not too comfortable, 4=not comfortable at all	Control/Het Effects	C2	high_soc_cost
q11_inc	To the best of your knowledge, what is your household income?	1= Under $1,000 2 =$1,000 to 2,999 3=$3,000 to 3,999 4=$4,000 to 4,999 5=$5,000 to 5,999 6=$6,000 to 6,999 7=$7,000 to 7,999 8=$8,000 to 9,999 9=$10,000 to 12,499 10=$12,500 to 14,999 11=$15,000 to 17,499 12=$17,500 to 19,999 13=$20,000 to 22,499 14= $22,500 to 24,999 15= $25,000 to 29,999 16= $30,000 to 34,999 17= $35,000 to 39,999 18=$40,000 to 49,999 19=$50,000 to 59,999 20=$60,000 to 74,999 21=$75,000 to $89,999 22= $90,000 to $109,999 23=$110,000 to $129,999 24=$130,000 to $149,999 25=$150,000 to $169,999 26=$170,000 or over 77= Don't know	Control	--	
q12_urb	Would you describe your neighborhood as urban, semi-urban, or rural?	1=Urban, 2=Semi-Urban, 3=Rural, 4=other (write-in)	Control	--	
q13_	Thinking about the kinds of people who you like to have living in your neighborhood, how much would you like or dislike having more neighbors who are...: [RANDOMIZE ORDER]		--	--	
q13_prej_black	Black	1=dislike a lot, 2=dislike somewhat, 3=neither like nor dislike, 4=like somewhat, 5=like a lot	Control, Het Effects	C1	q10_neighbor_diff (q10_neighbor_white-black)
q13_prej_white	White	1=dislike a lot, 2=dislike somewhat, 3=neither like nor dislike, 4=like somewhat, 5=like a lot	Control, Het Effects	C1	q10_neighbor_diff (q10_neighbor_white-black)
q13_prej_asian	Asian	1=dislike a lot, 2=dislike somewhat, 3=neither like nor dislike, 4=like somewhat, 5=like a lot	Decoy	--	
q13_prej_hispanic	Hispanic/Latino	1=dislike a lot, 2=dislike somewhat, 3=neither like nor dislike, 4=like somewhat, 5=like a lot	Decoy	--	
q14_norms	If you had to guess, how many [...] would like having more Black neighbors?		--	--	
q14_norms_gen	**White people in this country**	1=almost all, 2=more than half, 3=about half, 4=less than half, 5=almost none	Control, Het Effects	C2	index_pre_norms, index_pre_norms_gen, high_soc_cost
q14_norms_fam	of your **White family members**	1=almost all, 2=more than half, 3=about half, 4=less than half, 5=almost none	Control, Het Effects	C2	index_pre_norms, index_pre_norms_netw, high_soc_cost
q14_norms_friends	of your **White friends**	1=almost all, 2=more than half, 3=about half, 4=less than half, 5=almost none	Control, Het Effects	C2	index_pre_norms, index_pre_norms_netw, high_soc_cost
q15_sm	When it comes to social media, how often do you use [...] [RANDOMIZE ORDER]		--	--	
q15_sm_fb	Facebook	1=A few times every day, 2=About once a day, 3=A few times a week, 4=About once a week, 5=Once or twice a month, 6=Less than once a month, 7=never	Control	--	index_sm, no_sm
q15_sm_twitter	Twitter/X	1=A few times every day, 2=About once a day, 3=A few times a week, 4=About once a week, 5=Once or twice a month, 6=Less than once a month, 7=never	Control	--	index_sm, no_sm
q15_sm_ig	Instagram	1=A few times every day, 2=About once a day, 3=A few times a week, 4=About once a week, 5=Once or twice a month, 6=Less than once a month, 7=never	Control	--	index_sm, no_sm
q16_norms_joke	If you had to guess, how acceptable would it be, if at all, to make a racially insensitve comment or joke about Black people to [...]? [RANDOMIZE ORDER]		--	--	
q16_norms_joke_gen	**most White people**	1=very acceptable, 2=somewhat acceptable, 3=neither acceptable nor unacceptable, 4=somewhat unacceptable, 5=very unacceptable	Control, Het Effects	C2	index_pre_norms, index_pre_norms_gen, high_soc_cost
q16_norms_joke_friends	your White friends	1=very acceptable, 2=somewhat acceptable, 3=neither acceptable nor unacceptable, 4=somewhat unacceptable, 5=very unacceptable	Control, Het Effects	C2	index_pre_norms, index_pre_norms_netw, high_soc_cost
q16_norms_joke_fam	your White **family members**	1=very acceptable, 2=somewhat acceptable, 3=neither acceptable nor unacceptable, 4=somewhat unacceptable, 5=very unacceptable	Control, Het Effects	C2	index_pre_norms, index_pre_norms_netw, high_soc_cost
qX_religion	What is your present religion, if any? Do you consider yourself...	1=Protestant, 2=Roman Catholic, 3=Mormon, 4=Orthodox (such as Greek or Russian Orthodox), 5=Jewish, 6=Muslim, 7=Buddhist, 8=Hindu, 9=Atheist, 10=Agnostic, 11=Nothing in particular, 12=Something else (write in)			
q17_party	Generally speaking, do you usually think of yourself as a Republican, a Democrat, an Independent, or what?	1=Republican, 2=Democrat, 3=leaning Republican, 4=leaning Democrat, 5=Independent, 6=Other [Write in]	Control	--	
q18_reality	To what extent would you say you like or dislike watching reality television?	1=like a lot, 2=like somewhat, 3=neither like nor dislike, 4=dislike somewhat, 5=dislike a lot	Control	--	
q19_shows	Please look at the following list of shows and indicate whether you have watched at least one episode of this show previously [RANDOMIZE ORDER]		--	--	
q19_shows_blackish	Black-ish	1= yes, 2=no, 3=maybe/I don't know	Decoy/control for Q56 analysis (on viewing preferences) only	--	
q19_shows_dear	Dear White People	1= yes, 2=no, 3=maybe/I don't know		--	
q19_shows_outlander	Outlander	1= yes, 2=no, 3=maybe/I don't know		--	
q19_shows_park	Parks and Recreation	1= yes, 2=no, 3=maybe/I don't know		--	
q19_shows_atypical	Atypical	1= yes, 2=no, 3=maybe/I don't know		--	
q19_shows_bridgeton	Bridgerton	1= yes, 2=no, 3=maybe/I don't know		--	
q19_shows_abbott	Abbott Elementary	1= yes, 2=no, 3=maybe/I don't know		--	
MODULE 4: MEDIATED CONTACT EXPERIMENT					
TEXT: Now it is time to watch some television content. You will be watching content from the television show Married at First Sight. Please ensure that your headphones or speakers are working properly and that you are in a good environment for viewing. The content will last about 30 minutes and then we will ask you follow-up questions so please watch the content closely. This has to be completed in one sitting so please begin only once you are ready.					
q20_attn_pre	If you are ready to proceed, please choose [COLOR] [RANDOMIZE]	1=red, 2=blue, 3=green, 4=yellow	Screening	--	

SCREENING: Keep only if q20_attn_pre is correct					
[MEDIATED CONTACT EXPERIMENT HERE]					
[VIRTUAL ENUMERATOR EXPERIMENT BEGINS HERE]					
enum_control	Thank you for watching the content from Married at First Sight. Please proceed to answer some questions about the content that you just watched.		--	--	
enum_treat	[SHOW ENUMERATOR IMAGE] "Thank you for watching the content from Married at First Sight. My name is [NAME] and I will be your virtual interviewer for the rest of the survey on behalf of The Media Research Project. I would like to ask you some questions about the content that you just watched."		--	--	
q21_attn_post	If you are ready to proceed, please choose both [COLORS] [RANDOMIZE]	1=red, 2=blue, 3=green, 4=yellow	Survey attention	--	tot_num_correct, num_correct_surv_check
MODULE 5: POST-TREATMENT CHECK [DO NOT RANDOMIZE Q ORDER]					
q22_check_honeymoon	What did [COUPLE NAME] do for their honeymoon?	1=Beach vacation, 2=Cruise, 3=Italian getaway, 4=Road trip, 5=I don't know [Randomize 1-4, 5]	Treatment attention	--	tot_num_correct, num_correct_treat_check
q23_check_anniv	What did [COUPLE NAME] do for their one month anniversary?	1=Went back to where they got married, 2=Exchanged gifts, 3=Went on a mini-vacation, 4=They didn't celebrate their anniversary, 5=I don't know [Randomize 1-4, 5]	Treatment attention	--	tot_num_correct, num_correct_treat_check
q24_check_friends	Based on what you saw, did [COUPLE NAME] ever spend time with other couples matched on *Married at First Sight*?	1=yes, 2=no, 3=I don't know	Treatment attention	--	tot_num_correct, num_correct_treat_check
q25_mafs_real	Based on what you saw, to what extent is Married at First Sight real or scripted?	1=almost entirely scripted, 2=mostly scripted, 3=about half scripted, half real, 4=mostly real, 5=almost entirely real.	Descrip/PSR	--	
MODULE 6: NARRATIVE INVOLVEMENT/PSR					
MODULE 6A: NARRATIVE INVOLVEMENT/ENGAGEMENT [DO NOT RANDOMIZE Qs]					
q26_distracted	How distracted or engaged were you while watching the show?	1=very distracted, 2=somewhat distracted, 3=neither distracted nor engaged, 4=somewhat engaged, 5=very engaged	Descrip/PSR check	--	index_pos_reaction
q27_care	As you were watching, how much did you care, if at all, about what would happen to [COUPLE NAME]?	1=cared a lot, 2=cared somewhat, 3=did not really care, 4=did not care at all	Descrip/PSR check	--	index_pos_reaction
q28_like_content	How much did you like or dislike watching the *Married at First Sight* content?	1=liked a lot, 2=liked somewhat, 3=neither liked nor disliked, 4=disliked somewhat, 5=disliked a lot	Descrip/PSR check	--	index_pos_reaction
MODULE 6B: REACTIONS TO MALE CHARACTERS [FIRST RANDOMIZE ORDER OF 5b AND 5c AND RANDOMIZE ORDER OF QS IN BLOCK]					
TEXT: Please answer a few questions about how you feel about [MALE CHARACTER] [SHOW IMAGE]:					
q29_m_psr_now	To what extent, if at all, do you want to know how [NAME] is doing now that the show is over?	1=definitely want to know, 2=somewhat want to know, 3=don't really want to know, 4=don't want to know at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q30_m_psr_understand	To what extent, if at all, do you feel that you understand [NAME]?	1=understand well, 2=understand somewhat, 3=don't really understand, 4=don't understand at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q31_m_psr_relate	How much, if at all, did you relate to [NAME]?	1=related a lot, 2=related somewhat, 3=did not really relate, 4=did not relate at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q32_m_psr_values	How much, if at all, would you say that [NAME] shares your values?	1=shares a lot, 2=shares somewhat, 3=doesn't really share, 4=doesn't share at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q33_m_psr_likeable	Overall, do you think [NAME] is a likeable or unlikeable person?	1=very likeable, 2=somewhat likeable, 3=neither likeable nor unlikeable, 4=somewhat unlikeable, 5=very unlikeable [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q34_m_psr_friends	To what extent, if at all, could you see yourself being friends with [NAME]?	1=definitely yes, 2=probably yes, 3=maybe, 4=probably not, 5=definitely not [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_m
q35_m_psr_typical	How typical, if at all, is [MALE CHARACTER] of other [White/Black MEN] in this country?	1=very typical, 2=somewhat typical, 3=not too typical, 4=not typical at all [RANDOMLY REVERSE]	Descrip/Treat check	--	
MODULE 6C: REACTIONS TO FEMALE CHARACTERS [RANDOMIZE BLOCK AND ORDER WITHIN BLOCK]					
TEXT: Please answer a few questions about how you feel about [FEMALE CHARACTER] [SHOW IMAGE]:					
q36_f_psr_now	To what extent, if at all, do you want to know how [NAME] is doing now that the show is over?	1=definitely want to know, 2=somewhat want to know, 3=don't really want to know, 4=don't want to know at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q37_f_psr_understand	To what extent, if at all, do you feel that you understand [NAME]?	1=understand well, 2=understand somewhat, 3=don't really understand, 4=don't understand at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q38_f_psr_relate	How much, if at all, did you relate to [NAME]?	1=related a lot, 2=related somewhat, 3=did not really relate, 4=did not relate at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q39_f_psr_values	How much, if at all, would you say that [NAME] shares your values?	1=shares a lot, 2=shares somewhat, 3=doesn't really share, 4=doesn't share at all [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q40_f_psr_likeable	Overall, do you think [NAME] is a likeable or unlikeable person?	1=very likeable, 2=somewhat likeable, 3=neither likeable nor unlikeable, 4=somewhat unlikeable, 5=very unlikeable [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q41_f_psr_friends	To what extent, if at all, could you see yourself being friends with [NAME]?	1=definitely yes, 2=probably yes, 3=maybe, 4=probably not, 5=definitely not [RANDOMLY REVERSE]	Descrip/PSR check	--	index_psr_f
q42_f_psr_typical	How typical, if at all, is [FEMALE CHARACTER] of other [White/Black WOMEN] in this country?	1=very typical, 2=somewhat typical, 3=not too typical, 4=not typical at all [RANDOMLY REVERSE]	Descrip/Treat check	--	
MODULE 6D: VICARIOUS REACTIONS [DO NOT RANDOMIZE Q ORDER]					
TEXT: Now please answer some questions about [COUPLE NAME] (pictured below), a couple that [COUPLE NAME] interacted with.					
q43_friends_show	How would you describe [Black COUPLE NAMES]'s friendship with [White COUPLE NAMES] on the show? Was it:	1=very strong, 2=somewhat strong, 3=neither strong nor weak, 4=somewhat weak, 5=very weak [RANDOMLY REVERSE]	Descrip/Vic check	--	
q44_friends_still	How likely or unlikely is it that [Black COUPLE NAMES] are friends with [White COUPLE NAMES] now that the show is over?	1=very likely, 2=somewhat likely, 3=not too likely, 4=not likely at all [RANDOMLY REVERSE]	Descrip/Vic check	--	
MODULE 6E: VICARIOUS REACTIONS (AB SEASON ONLY)					
TEXT: Now please answer some questions about Karen and Miles (pictured below), a couple that [COUPLE NAME] interacted with.					
q45_friends_show_sp	How would you describe [COUPLE NAMES]'s friendship with Karen and Miles on the show? Was it:	1=very strong, 2=somewhat strong, 3=neither strong nor weak, 4=somewhat weak, 5=very weak	Descrip/Vic check	--	
q46_friends_still_sp	How likely or unlikely is it that [COUPLE NAMES] are friends with Karen and Miles now that the show is over?	1=very likely, 2=somewhat likely, 3=not too likely, 4=not likely at all	Descrip/Vic check	--	
MODULE 6F: OPEN QUESTIONS					
q47_like_overall	Overall, how much did you like or dislike [COUPLE NAME]?	1=liked a lot, 2=liked somewhat, 3=neither liked nor disliked, 4=disliked somewhat, 5=disliked a lot	Descrip/PSR check	--	index_pos_reaction
q47_like_open	We would like to better understand your response to the previous question. What did you like or dislike about [COUPLE NAME]?	OPEN	Descip/fraud check	--	
MODULE 7: INDIRECT OUTCOMES					
MODULE 7A: VIRTUAL ENUMERATOR EXPERIMENT			--	--	
enum_control	Thank you for your participation so far. There are about 15 more minutes of questions to complete the survey. If you complete the survey you will receive full compensation and have a chance to enter a contest to win a $500 Amazon gift certificate.		--	--	
enum_treat	[SHOW ENUMERATOR IMAGE] "Thank you for your participation so far. There are about 15 more minutes of questions to complete the survey. If you complete the survey you will receive full compensation and have a chance to enter a contest to win a $500 Amazon gift certificate."		--	--	
q48_attn_check	If you are ready to proceed, please choose [COLOR] [RANDOMIZE]	1=red, 2=blue, 3=green, 4=yellow	Survey attention	--	tot_num_correct, num_correct_surv_check
MODULE 7B: FRIENDSHIP MATCHING CONJOINT					
q_49	Let's say that there is a new television show that aims to match people for the purpose of making friends. As part of its background research, this show wants to know more about the kinds of friendships that people like yourself might be interested in making. Please take a look at the following profiles and indicate if you would or would not be interested in meeting this person for a possible friendship match.		--	--	
q49_1_	Profile 1	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_2	Profile 2	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_3	Profile 3	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_4	Profile 4	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_5	Profile 5	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_6	Profile 6	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_7	Profile 7	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
q49_8	Profile 8	1=yes, 2=no	Outcome	H1, H2	diff_match (# black_match - # white_match)
MODULE 7C: POLICE VIGNETTE EXPERIMENT					
TEXT: The Media Research Project is interested in how people react to stories that appear in the media. Consider the following events based on a true incident recently reported recently in the news. In Muskegon, Michigan Rob and Reyna Mathis were returning home after eating dinner out. They were stopped by Officer Anderson just outside their home for going 35mph in a 25mph zone. They were hurrying home because Mr. Mathis urgently needed to go the bathroom. [SHOW PHOTOS, RANDOMIZE RACE OF THE MATHIS'S AND OFFICER ANDERSON] Mr. Mathis asked if he could briefly go inside his home to use the bathroom. Officer Anderson denied this request. He detained the couple for over 30 minutes, which is longer than an average traffic stop, and repeatedly refused Mr. Mathis's pleas to let him use the bathroom. When the Mathises' young children came outside crying, Mrs. Mathis opened the vehicle door to try to reassure them. Officer Anderson reacted aggressively, angrily ordering Mrs. Mathis out of the car. When she hesitated, he forcefully put her in a headlock and arrested her for obstructing a police officer.					
q50_emp	How reasonable or unreasonable was the behavior of [...] in this situation? [RANDOMIZE ORDER]		--		
q50_emp_mr	Mr. Mathis	1=very reasonable, 2=somewhat reasonable, 3=neither reasonable nor unreasonable, 4=somewhat unreasonable, 5=very unreasonable [RANDOMLY REVERSE]	Mechanism	M3	index_emp_mathis
q50_emp_mrs	Mrs. Mathis	1=very reasonable, 2=somewhat reasonable, 3=neither reasonable nor unreasonable, 4=somewhat unreasonable, 5=very unreasonable [RANDOMLY REVERSE]	Mechanism	M3	index_emp_mathis
q51_fair	How fair or unfair was the behavior of Officer Anderson towards [...] in this situation? [RANDOMIZE ORDER]				
q51_fair_mr	Mr. Mathis	1=very fair, 2=somewhat fair, 3=neither fair nor unfair, 4=somewhat unfair, 5=very unfair [RANDOMLY REVERSE]	Outcome (Legit study)	LH1-LH3	index_fair_mathis
q51_fair_mrs	Mrs. Mathis	1=very fair, 2=somewhat fair, 3=neither fair nor unfair, 4=somewhat unfair, 5=very unfair [RANDOMLY REVERSE]	Outcome (Legit study)	LH1-LH3	index_fair_mathis
q52_obey	If you were the Mathises, how likely or unlikely would you have been to accept Officer Anderson's orders, even if you thought he was in the wrong?	1=extremely likely, 2=very likely, 3=somewhat likely, 4=neither likely nor unlikely, 5=somewhat unlikely, 6=very unlikely, 7=extremely unlikely	Outcome (Legit study)	LH1-LH3	

q53_punish	After the incident, Mr. and Mrs. Mathis filed a complaint with the police department against Officer Anderson. Upon investigation, it was found that this was the latest in a series of incidents in which Officer Anderson had behaved with unreasonable and excessive force during a routine encounter. Of the following options, which do you think would be the most appropriate action for the Chief of Police to take regarding Officer Anderson? The options range from least severe (no punishment) to most severe (fired and prosecuted)	1 = no punishment; 2=formal, written reprimand; 3=suspension with pay; 4=suspension without pay; 5=Fired; 6=Fired and prosecuted	Outcome	H1, H2	
MODULE 7D: MAYOR VIGNETTE EXPERIMENT					
TEXT: Now consider the following hypothetical scenario based on recent current events reported in the media. [RANDOMIZE IMAGES] Imagine that you live in a town where Mayor Williams, an Independent, holds office. Mayor Williams is now introducing a tax to reduce the use of disposable plastic bags. Everyone will be required to pay a tax of twenty-five cents ($.25) for each plastic bag that they use at the store. You know that it is possible to get around this tax at self checkout by not reporting that you used a plastic bag even when you did.				
q54_mayor_comply	How likely, if at all, would you be to comply with Mayor Williams's bag tax by reporting at self checkout the number of plastic bags that you actually used?	1=extremely unlikely, 2=very unlikely, 3=somewhat unlikely 4=neither likely nor unlikely 5=somewhat likely, 6=very likely, 7=extremely likely	Outcome (Legit study)	LH1-LH3	
q55_mayor_trust	How much do you trust or distrust Mayor Williams to do the right thing for your community?	1=trust a lot, 2=trust somewhat, 3=neither trust nor distrust, 4=distrust somewhat, 5=distrust a lot	Outcome (Legit study)	LH1-LH3	
MODULE 7E: FUTURE MEDIATED CONTACT					
q56_show	Please share more about the kinds of programs you are interested in watching. Below is a list of recent shows with brief descriptions. Please indicate how interested, if at all, you would be in watching each show. You will then be asked to rank the shows from the one you are most to least interested in watching. [RANDOMIZE SHOW ORDER]		--	--	
q56_show_blackish	Black-ish -- A sitcom that follows an upper-middle class Black family as they juggle personal and race-related issues.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Outcome	H1, H2	index_like_outgrp_shows
_show_parks	Parks and Recreation -- A sitcom that follows a perky, mid-level bureaucrat in the Parks Department of Pawnee, Indiana.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Decoy	--	--
_show_dear	Dear White People -- A comedy-drama that follows Black college students as they address issues surrounding modern American race relations.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Outcome	H1, H2	index_like_outgrp_shows
_show_atyp	Atypical -- A comedy-drama that follows the story of an autistic teenager looking for romance.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Decoy	--	--
_show_bridgerton	Bridgerton -- An historical romance featuring a multi-racial cast set in early 1800s London during the social season where marriageable youth of nobility are launched into society.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Outcome (exploratory)	H1, H2	index_like_outgrp_shows (TBD)
_show_outlander	Outlander -- An historical romance in which a WWII-era nurse in Scotland finds herself transported back in time to 1743 where she falls in love with a dashing Highland warrior.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Decoy	--	--
_show_abbott	Abbott Elementary -- A sitcom about the lives of the teachers in a predominantly Black and under-funded public school.	1=very interested, 2=somewhat interested, 3=not too interested, 4=not interested at all	Outcome	H1, H2	index_like_outgrp_shows
_shows_rank	Now please rank each of the shows described above in the order in which you are interested in watching it. A rank of "1" indicates the show you are most interested in watching and a rank of "7" indicates the show you are least interested in watching. For each show, please select a number to indicate how you want to rank it. You can use each number only once.	1=Black-ish, 2=Parks and Rec, 3=Dear White People, 4=Atypical, 5=Bridgerton, 6=Outlander, 7=Abbott Elementary	Outcome	H1, H2	sum_rank_outgrp_shows
q57_emmy	2023 is the year of the 75th annual Primetime Emmy Awards, which recognize outstanding work in primetime entertainment programming on television. The Emmys released their nominees for all categories in June. Of the following nominees, who do you think should win for [...]? [DO NOT RANDOMIZE ORDER]		--	--	--
_emmy_reality	Outstanding reality competition program	1=RuPaul's Drag Race, 2=Survivor, 3=The Amazing Race, 4=Top Chef, 5=The Voice, 6=I don't know [RANDOMIZE 1-5, not 6]	Decoy	--	--
_emmy_actor	Outstanding lead actor in a comedy series	1=Bill Hader, "Barry"; 2=Jason Segel, "Shrinking", 3=Martin Short, "Only Murders in the Building", 4=Jason Sudeikis, "Ted Lasso", 5=Jeremy Allen White, "The Bear", 6=I don't know	Decoy	--	--
_emmy_actress	Outstanding lead actress in a comedy series	1=Christina Applegate, "Dead to Me"; 2=Rachel Brosnahan, "The Marvelous Mrs. Maisel"; 3=Quinta Brunson, "Abbott Elementary", 4=Natasha Lyonne,"Poker Face"; 5=Jenna Ortega, "Wednesday", 6=I don't know	Decoy	--	--
MODULE 8: MAIN OUTCOMES (DIRECT) [DO NOT RANDOMIZE Q ORDER]					
[VIRTUAL ENUMERATOR EXPERIMENT REFRESH, IMAGE APPEARS WITH EACH QUESTION GOING FORWARDS]					
enum_control	People often have different preferences for the kinds of people and shows they like to watch on television. The Media Research Project is interested in learning about your attitudes and feelings towards different groups in order to understand the kinds of content, characters, and people you like to watch on television and engage with in real life. There are no right or wrong answers so please just answer honestly.		--	--	
enum_treat	[SHOW ENUMERATOR IMAGE] ""People often have different preferences for the kinds of people and shows they like to watch on television. The Media Research Project is interested in learning about your attitudes and feelings towards different groups in order to understand better the kinds of content, characters, and people you like to watch on television and engage with in real life."		--	--	
q58_attn_check	If you are ready to continue, please select the image of a hawk.	1=giraffe, 2=gorilla, 3=moose, 4=hawk [RANDOMIZE ORDER]	Survey attention	--	tot_num_correct, num_correct_surv_check
q59_therm	We'd like to get your feelings towards different groups of people in this country on a "feeling thermometer." On the thermometer, 0 means that you feel as coldly and negatively towards that group as possible. The number 10 means that you feel as warmly and favorably towards that group as possible. The number 5 means that your feelings towards that group are neutral (neither warm nor cold).		--	--	
q59_therm_black	How do you feel about Black Americans	0-10 IMAGE	Outcome	H1, H2, H3a-H8a	q59_therm_diff (q59_therm_black-white), index_more_tol
q59_therm_white	How do you feel about White Americans	0-10 IMAGE	Outcome	H1, H2, H3a-H8a	q59_therm_diff (q59_therm_black-white), index_more_tol
q59_therm_asian	How do you feel about Asian Americans?	0-10 IMAGE	Decoy	--	
q59_therm_autism	How do you feel about people with autism?	0-10 IMAGE	Decoy	--	
q60_friend	To what extent, if any, do you want to have more close friends who are [...]?		--	--	
q60_friend_black	Black	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Outcome	H1, H2, H3a-H8a	q60_friend_diff (q60_friend_black-_white), index_more_tol
q60_friend_white	White	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Outcome	H1, H2, H3a-H8a	q60_friend_diff (q60_friend_black-_white), index_more_tol
q61_marry	If the opportunity arose, how willing or unwilling would you be to welcome a Black person into your immediate family thorough marriage?	1=very willing, 2=somewhat willing, 3=neither willing nor unwilling, 4=somewhat unwilling, 5=very unwilling [RANDOMLY REVERSE]	Outcome	H1, H2, H3a-H8a	index_more_tol
q62_change	Racial issues have been getting a lot of attention in the media. When it comes to ensuring Black people have equal rights and opportunities to White people, do you think this country has [...]?	1=gone too far, 2=not gone far enough, 3=been about right	Outcome/Alternative	H1, H2, H3a-H8a OR A2	
q63_act	People differ in how likely they are to take action to address racial inequality involving Black Americans. If given the opportunity, how likely or unlikely would you be to do each of the following: [RANDOMIZE]		--	--	
q63_act_priv_vote_w	Vote by secret ballot for a White political candidate who supports doing more to address racial injustice	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_priv_act
q63_act_priv_vote_b	Vote by secret ballot for a Black political candidate who supports doing more to address racial injustice	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_priv_act
q63_act_pub_protest	Attend a non-violent public protest to support racial equality	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_pub_act
q63_act_priv_petition	Sign a petition to support racial equality if you could sign anonymously.	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_priv_act
q63_act_pub_petition	Sign a petition to support racial equality if you had to sign your name.	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_pub_act
q63_act_priv_read	Read an article to learn more about racial injustice	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_priv_act
q63_act_pub_share	Share an article on social media to encourage others to learn more about racial injustice	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_pub_act
q63_act_pub_confront	Confront a white friend or family member if they were to make a racially insensitive comment or joke	1=very likely, 2=somewhat likely, 3=neither likely nor unlikely, 4=somewhat unlikely, 5=very unlikely	Outcome	H1, H2, H3b-H8b	index_act, index_pub_act
MODULE 8: DECOY					
q64_character_decoy	As you know, The Media Research Project is interested in learning about your attitudes and feelings towards different groups in order to understand the kinds of content, characters, and people you like to engage with on television and in real life. Below is a list of characters considered by some to be among the best on television in recent years. If you had to choose, which of the following characters would you consider the best character on television? IndicYou can also select "none of the above" or "I don't know."	1=Walter White (Breaking Bad) 2=Olivia Pope (Scandal) 3=Randall Pearson (This is Us) 4-Tyrion Lannister (Game of Thrones) 5=Jack Bauer (24) 6=Tracy Jordan (30 Rock) 7=Stringer Bell (The Wire) 8=Elizabeth Jennings (The Americans) 9=Jason Mendoza (The Good Place) 10=Cookie Lyon (Empire) 11=Moira Rose (Schlitt's Creek) 12=Eve Polastri (Killing Eve) 13=None of the above 14=I don't know [RANDOMIZE 1-12, NOT 13 AND 14]	Decoy	--	
MODULE 9: MECHANISMS [RANDOMIZE Q ORDER]					
q65_norms	To what extent, if any, do you think that [...] want to have more close friends who are Black? [DO NOT RANDOMIZE ORDER]		--	--	
q65_norms_gen	**White people in this country**	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Mech	M6, H3a-H8a	index_post_norms
q65_norms_friends	your White **friends**	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Mech	M6, H3a-H8a	index_post_norms, index_post_norms_netw
q65_norms_fam	your White **family members**	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Mech	M6, H3a-H8a	index_post_norms, index_post_norms_netw
q65_norms_black	To what extent, if any, do you think that **Black people** in this country want to have more close friends who are White?	1=want a lot, 2=want somewhat, 3=don't really want, 4=don't want at all	Alt Mech	M7	--
q66_stereo_w	Thinking about **White people in this country**, how many, if any, would you say [...]? [RANDOMIZE ORDER]		--	--	--

q66_stereo_w_work	Have a good work ethic	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_ingrp, index_stereo_diff
q66_stereo_w_intel	Are intelligent	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_ingrp, index_stereo_diff
q66_stereo_w_famval	Have weak family values	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_ingrp, index_stereo_diff
q66_stereo_w_viol	Are prone to anger or violence	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_ingrp, index_stereo_diff
q66_stereo_w_commit	Are seeking committment in romantic relationships	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_ingrp, index_stereo_diff
q67_stereo_b	Thinking about **Black people in this country**, how many, if any, would you say [...]? [RANDOMIZE ORDER]		--	--	--
q67_stereo_b_work	Have a good work ethic	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_outgrp, index_stereo_diff
q67_stereo_b_intel	Are intelligent	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_outgrp, index_stereo_diff
q67_stereo_b_famval	Have weak family values	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_outgrp, index_stereo_diff
q67_stereo_b_viol	Are prone to anger or violence	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_outgrp, index_stereo_diff
q67_stereo_b_commit	Are seeking committment in romantic relationships	0-10 scale where 0=none, 5=about half, 10=all	Mech	M1, H3a-H8a	index_stereo_outgrp, index_stereo_diff
q68_sim	How much do you agree or disagree with each of the following statements:		--	--	
q68_sim_white	White people in this country have a lot in common with each other.	1=strongly disagree, 2=somewhat disagree, 3=neither agree nor disagree, 4=somewhat agree, 5=strongly agree	Alt explanation	A3, H3a-H8a	
q68_sim_black	Black people in this country have a lot in common with each other.	1=strongly disagree, 2=somewhat disagree, 3=neither agree nor disagree, 4=somewhat agree, 5=strongly agree	Alt explanation	A1, H3a-H8a	
q68_sim_white_black	White people and Black people in this country have a lot in common with each other.	1=strongly disagree, 2=somewhat disagree, 3=neither agree nor disagree, 4=somewhat agree, 5=strongly agree	Mech	M2, H3a-H8a	
q69_sim_values	To what extent are the *values* held by White and Black people in this country similar or different?	1=very similar, 2=somewhat similar, 3=neither similar nor different, 4=somewhat different, 5=very different	Mech	M5, H3a-H8a	
q70_emp	How easy or hard is it for you to understand the perspectives or emotions of someone who is Black?	1=very easy, 2=somewhat easy, 3=neither easy nor hard, 4=somewhat hard, 5=very hard			
q71_emp_police	Black people in this country often fear that ordinary interactions with the police might escalate to excessive or unjustified use of force by police officers. How concerned, if at all, are you about the well-being of an ordinary Black perosn when they are stopped by the police?	1=extremely concerned, 2=very concerned, 3=somewhat concerned, 4=neither concerned nor unconcerned, 5=somewhat unconcerned, 6=very unconcerned, 7=extremely unconcerned	Mech	M3, H3a-H8a	index_emp
q72_anxiety	Imagine yourself having a conversation with someone whom you've never met before. How comfortable or uncomfortable would you feel in that situation if that person were...		--	--	
q72_anxiety_white	White?	1=very uncomfortable, 2=somewhat uncomfortable, 3=neither comfortable nor uncomfortable, 4=somewhat comfortable, 5=very comfortable	Mech	M3, H3a-H8a	q72_anxiety_diff (q72_anxiety_black-white)
q72_anxiety_black	Black?	1=very uncomfortable, 2=somewhat uncomfortable, 3=neither comfortable nor uncomfortable, 4=somewhat comfortable, 5=very comfortable	Mech	M3, H3a-H8a	q72_anxiety_diff (q72_anxiety_black-white)
q72_anxiety_disabled	Autistic?	1=very uncomfortable, 2=somewhat uncomfortable, 3=neither comfortable nor uncomfortable, 4=somewhat comfortable, 5=very comfortable	Decoy	--	
q73_discrim	In the United States today, how much or how little discrimination is there against Black people, limiting their opportunities?	1=a lot of discrimination, 2=some discrimination, 3=a little discrimination, 4=no discrimination at all	Alt explanation	A2, H3a-H8a	
q74_never_comfort	How much do you agree or disagree with the statement: White and Black people in this country can never really be comfortable with each other, even if they are close friends.	1=strongly disagree, 2=somewhat disagree, 3=neither agree nor disagree, 4=somewhat agree, 5=strongly agree	Alt explanation	A4, H3a-H8a	
MODULE 10: DECOY [DO NOT RANDOMIZE ORDER]					
q75_strike_heard	Another topic that has received a lot of coverage in the media recently is the strike by the Writers and Actors Guilds. How much, if anything, would you say that you have heard about the strike?	1=a lot, 2=some, 3=not too much, 4=nothing at all	Decoy	--	
q76_strike_shows	The strike will cause significant delays in the production of many hit television shows. Of the following shows that will be affected, which do you hope will be the least delayed:	1=White Lotus, 2=The Last of Us, 3=Abbott Elementary, 4=Saturday Night Live, 5=Andor, 6=Euphoria, 7=I don't know [Randomize 1,5, not 6]	Decoy	--	
q77_emmy_delay	It was recently announced that the 2023 Emmy Awards, which was set to air on September 18, is also being delayed due to the strike. How likely or unlikely are you to watch the 75th annual Emmy awards whenever they air?	1=very likely, 2=somewhat likely, 3=not likely or unlikely, 4=somewhat unlikely, 5=very unlikely	Decoy	--	
	Of the following nominees for a 2023 Emmy Award, who do you think should win for [...]?		--	--	
q78_emmy_drama_actor	Outstanding lead actor in a drama series	1=Jeff Bridges (The Old Man), 2=Brian Cox (Succession), 3=Kieran Culkin (Succession), 4=Bob Odenkirk (Better Call Saul), 5=Pedro Pascal (The Last of Us), 6=Jeremy Strong (Succession), 7=I don't know	Decoy	--	
q79_emmy_drama_actress	Outstanding lead actress in a drama series	1=Sharon Horgan (Bad Sisters), 2=Melanie Lynskey (Yellowjackets), 3=Elisabeth Moss (The Handmaid's Tale), 4=Bella Ramsey (The Last of Us), 5=Keri Russell (The Diplomat), 6=Sarah Snook (Succession), 7=I don't know	Decoy	--	
MODULE 11: FINAL CHECKS [DO NOT RANDOMIZE Q ORDER]					
TEXT: We are almost finished! Just a few more questions.				--	
q80_check_mafs	What was the name of the tv show that you watched?	1=Married at First Sight, 2=The Bachelor, 3=Love is Blind, 4=The Bachelorette, 5= Love Island, 6=I don't know [RANDOMIZE 1-5, not 6]	Treatment attention	--	tot_num_correct, num_correct_treat_check
q81_check_enum_sex	What was the sex of your virtual interviewer?	1=I didn't have a virtual interviewer, 2=male, 3=female, 4=I don't know.	Survey attn/decoy	--	tot_num_correct, num_correct_surv_check
q82_check_enum_race	What was the race of your virtual interviewer?	1=I didn't have a virtual interviewer, 2=White (non-Hispanic), 3=Black or African American, 4=Hispanic or Latino, 5=American Indian or Alaskan Native, 6=Native Hawaiian or Other Pacific Islander, 7=Asian, 8=Other race, 7= I don't know	Survey attention	--	tot_num_correct, num_correct_surv_check
q83_check_purpose	In your opinion, which of the following statements best describes the main purpose of this survey? Is it understanding [...]	1=Reactions to reality television, 2=Preferences over television content, 3=Preferences over television characters , 4=Racial attitudes, 5=Reactions to current events, 6=I don't know [RANDOMIZE 1-5, not 6]	Check	--	
q84_reaction_overall	Would you describe your overall experience with this survey as positive or negative?	1=very positive, 2=somewhat positive 3=neither positive nor negative, 4=somewhat negative, 5=very negative	Check	--	
q85_reaction_open	We would like to better understand your response to the previous question. What made your overall experience with the survey positive or negative?	OPEN	Descrip/fraud check	--	
MODULE 12: GLEAM CONTEST AND EXPERIMENT					
TEXT: Thank you for participating in the study. This completes the survey. As a thank you, The Media Research Project would like to offer you a chance to enter a contest to win a $500 Amazon gift card. This contest is open only to the 1500 people who participate in this study. Participation is totally voluntary. If you prefer not to enter, scroll to the bottom and click the 'Next' arrow. If you choose to enter, take three simple steps: (1) Enter the following code when prompted: [UNIQUE CODE] (2) Enter your name and email address so you can be notified if you win. (3) Decide if you want to take any additional actions from the options below to increase your chances of winning. This is not required but every action you take counts as an additional contest entry and increases your chances of winning. Please follow the instructions closely so your actions can be verified. **A note on your privacy**: You are only being asked for your name and email address so that you can be notified if you win. If you provide this information, The Media Research Project will be able to link your survey responses and your Gleam actions to your identity. Your survey responses and Gleam actions will be kept strictly confidential and stored in encrypted files so that no one outside of the project will be able to connect your survey and Gleam actions to your identity. The Media Research Project will not release or make publicly available any of your personal identifying information.					
	The Media Research Project "Thank You" Contest -- Private				
gleam_entered	Entered contest		Outcome	H1, H2	gleam_entered
gleam_priv_race_trailer	Viewed race-related documentary		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_priv_nonrace_trailer	Viewed non-race-related documentary		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_priv_race_docs	Read about race-related documentaries		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_priv_nonrace_docs	Read about non-race-related documentaries		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_priv_race_petition	Signed a private race-related petition		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_priv_nonrace_petition	Signed a private non-race-related petition		Outcome	H1, H2, H3b, H6b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
	The Media Research Project "Thank You" Contest -- Public				
gleam_entered	Entered contest		Outcome	H1, H2	gleam_entered
gleam_pub_race_trailer	Posted about race-related documentary		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_pub_nonrace_trailer	Posted about non-race-related documentary		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_pub_race_docs	Posted about race-related documentaries		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_pub_nonrace_docs	Posted about non-race-related documentaries		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_pub_race_petition	Signed a public race-related petition		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
gleam_pub_nonrace_petition	Signed a public non-race-related petition		Outcome	H1, H2, H4b, H7b	gleam_tot_race, gleam_diff (gleam_tot_race-nonrace)
CONSENT END					

Thank you for participating in this study for The Media Research Project.

If you have questions or concerns, would like to withdraw from the study, or would like to receive a copy of the final aggregate results of this study, please contact:

[REDACTED]

Questions about Your Rights as a Research Subject:

[REDACTED]