# Supplemental Online Materials:
# *Meta-Analysis of the 'Ironic' Effects of Intergroup Contact*

February 11, 2022

## Search Strategy

We used similar non-exclusive search terms for all electronic databases:

1. (contact OR friendship) AND ("perceived discrimination" OR "perceived *advantage" OR "relative deprivation" OR "group discrimination" OR "personal discrimination" OR "group deprivation" OR "perception* of discrimination" OR "perception* of group discrimination" OR "perception* of personal discrimination" OR "rac* discrimination")

2. *group AND (contact OR friendship) AND ("collective action" OR protest OR "collective behavio*r" OR "political behavio*r" OR "social change" OR "social justice")

3. *group AND (contact OR friendship) AND (policy OR policies OR "affirmative action" OR (politi* W/15 attitude*) OR (politi* W/15 preferenc*)) AND (redistribut* OR reparati* OR inequalit* OR equalit* OR injustice* OR justice* OR disadvantage* OR advantage* OR minorit* OR majorit*)

We sent a call for unpublished research to the mailing lists of the *European Association of Social Psychology*, the *Society for Personality and Social Psychology*, the *International Society of Political Psychology*, the *Society for the Psychological Study of Social Issues*, and the *Society of Australasian Social Psychologists*.

## Coding Criteria (Study Setting)

For each sample, we categorized the study setting according to the source of the ingroup's relative disadvantage. This coding scheme is, by necessity, reductive and incomplete. We believe, however, that it can offer some insight into relevant differences in study settings.

We coded a study setting as *short-term migration* if the relevant intergroup context resulted from the short-term movement of people for the purposes of temporary employment or education. Most studies in this category focus on international students.

We coded a study setting as *long-term migration* if the relevant intergroup context resulted from migration, in either the current or previous generations, and if there is an established intergroup hierarchy that disadvantages the relevant ingroup. Examples include Black and Asian people in the United Kingdom and Asian Americans in the United States.

We coded a study setting as *post-slavery* if the ingroup's relative disadvantage resulted from a racial hierarchy rooted in the historical abduction and enslavement of Africans. This category includes Black people in the Americas and the Caribbean.

We coded a study setting as *colonization* if the relevant intergroup context resulted from the advantaged group colonizing and subjugating the disadvantaged group in the place that was originally inhabited by the disadvantaged group. This category includes contexts in which one nation has occupied the territory of another, resulting in an established intergroup hierarchy that disadvantages the relevant ingroup. Examples include Māori in New Zealand and Kurds in Turkey. While many of the hierarchies we categorize as resulting from long-term migration or slavery are rooted in a history of colonization, we use colonization in a narrow sense to describe settler colonies and occupied territories.

We coded a study setting as *religion* if the relevant intergroup context was defined in terms of religious group membership (for example, Muslims in India). In contexts in which religion is a marker of a disadvantaged immigrant group (e.g., Muslims in the Netherlands), we instead categorized the study setting as long-term migration.

We coded a study setting as *caste* if the ingroup's relative disadvantaged resulted from caste hierarchies in South Asia. We coded a study setting as *sexual/gender minorities* if the relevant ingroup was defined in terms of their membership in a discriminated sexual or gender minority (i.e., LGBTQ people). We coded a study setting as *other* if the relevant intergroup context did not fit any of the other categories (e.g., disabled people).

## Moderator Analyses

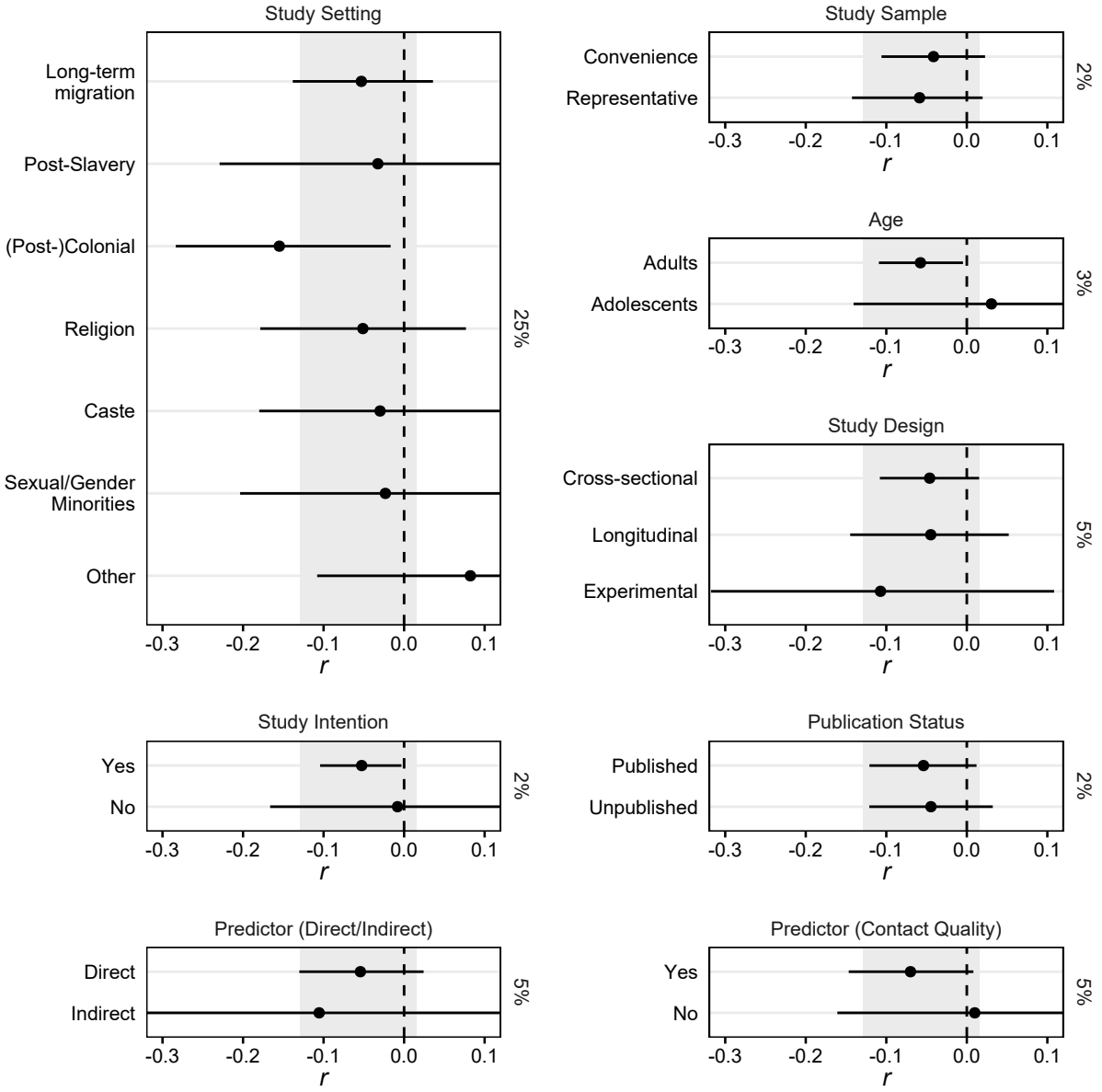
First, we ran a series of Bayesian random-effects meta-regression models to estimate how much of the between-samples heterogeneity was explained by specific categorical moderator variables (`run_moderator_analyses.R`). Figures S1 and S2 show the results for collective action and policy support. Comparisons were inconclusive because we had an insufficient number of effect sizes per category.

Second, we ran another three random-effects meta-regression models with Muthukrishna et al.'s (2020) measure of cultural distance, where available, as a continuous moderator variable. Because cultural distance was a country-level moderator variable, we used two-level random-effects models in which we estimated country-specific deviations from the mean effect size as well as sample-specific deviations from the country-specific effect size. Figure 5 (in the main text) show the inconclusive results for all three outcomes.

Third, we used meta-regression trees to discover interactions between moderator variables that best explained heterogeneity in effect sizes (Li et al., 2017). As recommended, we ran random-effects meta-regression tree analyses using the look-ahead strategy (Li et al., 2020). We set the pruning parameter to $c = 0$ because, for this analysis, we prioritized exploration over error control (`run_exploratory_moderator_analyses.R`). For both collective action and policy support, the algorithm found that no moderator or interaction of moderators explained between-samples heterogeneity. This result is not surprising, however, as both analyses used fewer than 40 effect sizes, the minimum number required for the algorithm to perform well in detecting even simple interaction effects (Li et al., 2017). As all moderator analyses were inconclusive for collective action and policy support, we only report results for perceived injustice in the main text.

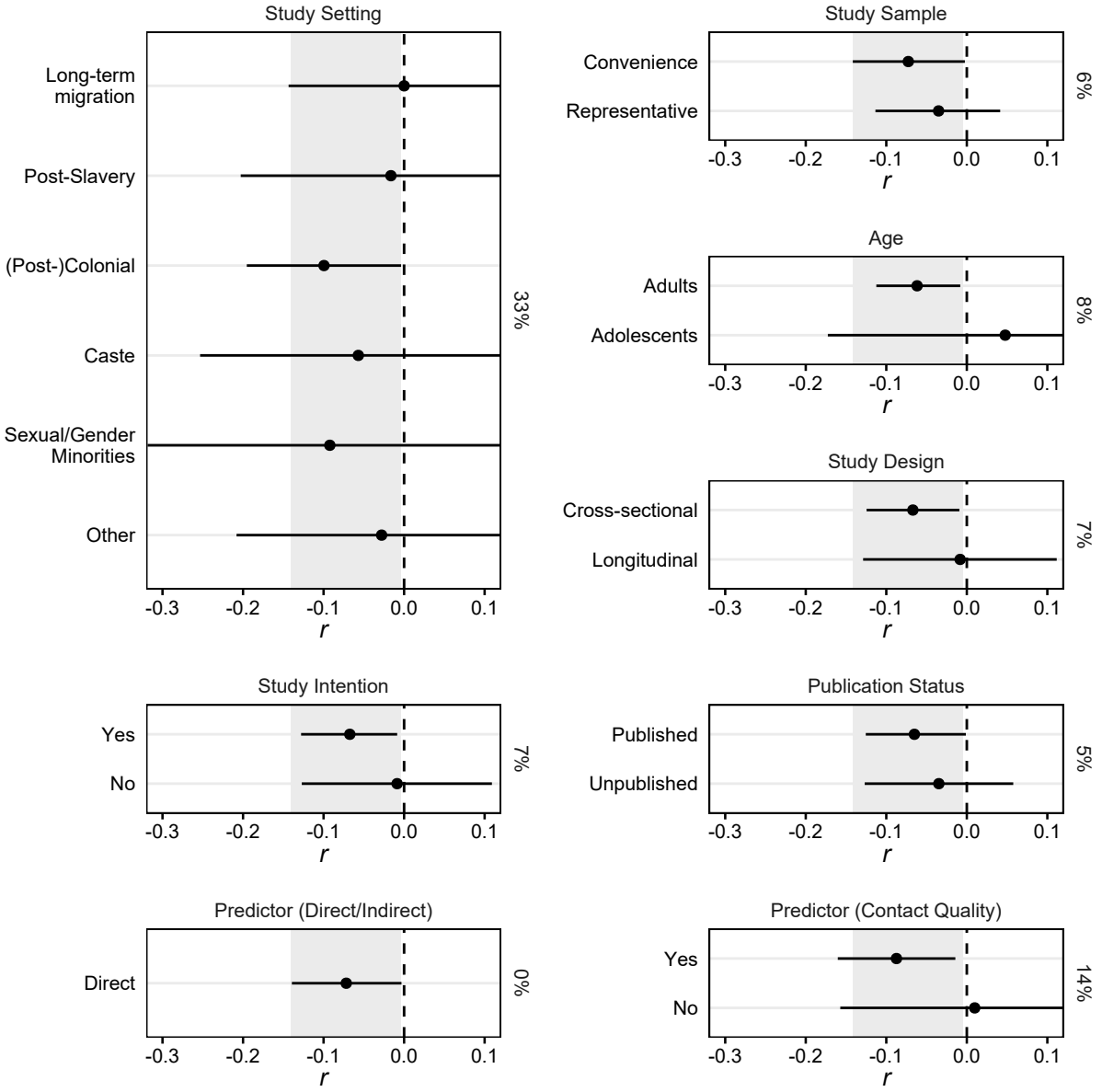
**Figure S1**

Estimated effect sizes for the association between intergroup contact and collective action as a function of various categorical moderator variables



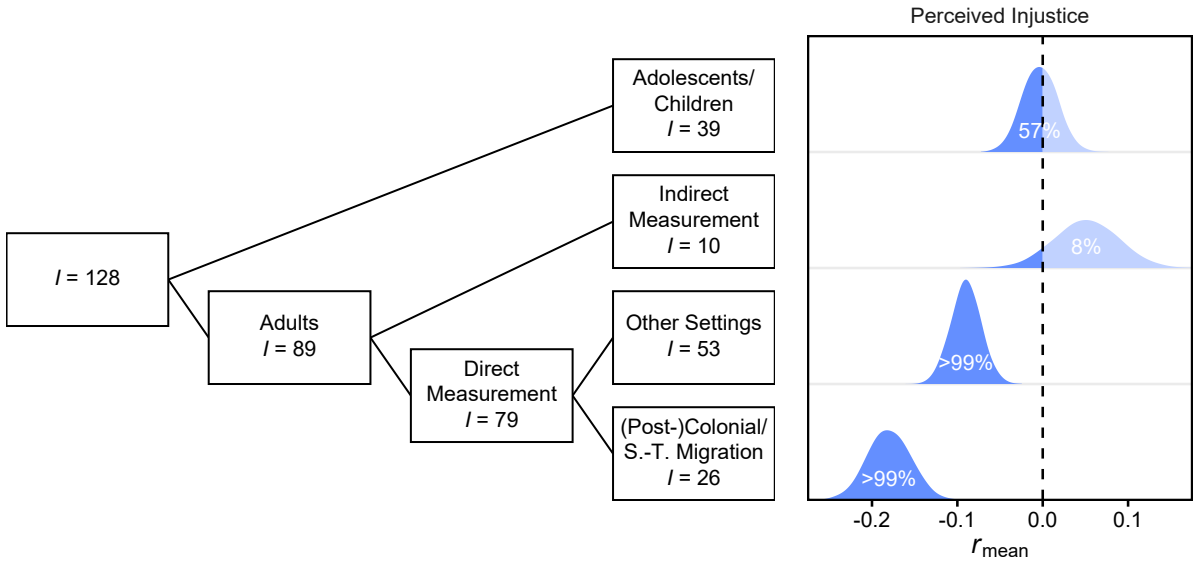

*Note.* Intervals enclose the 95% most plausible estimates of the category-specific effect size. Shaded ribbons enclose the 95% most plausible estimates of the mean effect size from the main analyses. Percentages indicate the estimated between-sample variance explained by each moderator variable.

**Figure S2**
Estimated effect sizes for the association between intergroup contact and policy support as a function of various categorical moderator variables




*Note.* Intervals enclose the 95% most plausible estimates of the category-specific effect size. Shaded ribbons enclose the 95% most plausible estimates of the mean effect size from the main analyses. Percentages indicate the estimated between-sample variance explained by each moderator variable.

**Figure S3**
Results from the random-effects meta-regression tree analysis




*Note.* Posterior distributions for the estimated correlation coefficient in each leaf of the meta-regression tree, highlighting the proportion of posterior samples for which $r_{\text{mean}} < 0$. In this analysis, we did not consider whether studies measured direct, qualitatively positive contact. S.-T. Migration = Short-Term Migration.

## Moderator Analyses Without Contact Quality

In an earlier version of our manuscript, we did not consider the distinction between contact quantity and quality as a moderator. As we added this moderator during peer review and as doing so changed the results of some of our moderator analyses, we report our original findings from the moderator analyses below. We used meta-regression trees to discover interactions between moderator variables that best explained heterogeneity in effect sizes (Li et al., 2017, 2020). Figure S3 shows the resulting meta-regression model, which explained more variance across samples than any individual moderator ($R^2$ = 30%). We found that intergroup contact was associated with less perceived injustice only in studies that focused on adults and that measured intergroup contact directly. Among these studies, this association was stronger in settings in which the groups' relative inequality stemmed from short-term migration or colonization ($r$ = −.18, [−.23, −.13]) than in other settings ($r$ = −.09, [−.12, −.06]).

## Unpublished Studies

Some of the unpublished data included in the meta-analysis were based on studies that involved one of the co-authors of the present research. For the sake of transparency, we describe each of these studies and refer to published research based on these studies.

**Bracegirdle (2020)** is based on data from a longitudinal social network study of two schools in a town in North West England conducted by a graduate students who, at the time, was co-supervised by the first author of the present research. The study included measures intended to test a broad

range of questions, for example, to disentangle how contact and socialization effects shape outgroup attitudes in diverse friendship networks (Bracegirdle et al., 2022). Bracegirdle (2020) includes data from Asian adolescents who reported their cross-group friendships with White adolescents ($N$ = 829).

**Schäfer et al. (2018)** is based on data from a longitudinal survey of Asian British and White British people and included a broad range of measures related to intergroup contact and intergroup relations in England. Schäfer et al. (2018) includes data from Asian British people who reported their contact experiences with White British people, as well as their perceived discrimination and collective action intentions ($N$ = 751).

**Sengupta (2020a)** and **Sengupta (2020b)** is based on data from the Centre for the Monitoring of the Indian Economy's consumer pyramid sample, a representative national sample of the Indian population. In 2017, this sample consisted of 161,183 households (112,657 rural and 48,526 urban households). Households were selected by dividing 26 of the 29 States of India into sub-state "Homogenous Regions"—sets of neighboring districts with similar climatic conditions and urbanization levels—and then randomly sampling villages/blocks of towns, and households within each region. During August and September, 2017, interviewers attempted to make face-to-face contact with each household. One member of the household was asked to volunteer to complete the verbally administered survey. In all, 134,531 people were successfully reached by interviewers. The survey included measures intended to test a broad range of questions, for example, about the relationship between ambivalent sexism and violence against women (Sengupta, 2021). For the meta-analysis, we calculated correlation coefficients between items measuring positive and negative contact, group identification, perceived discrimination, and collective action intentions. Items in the survey were first translated from English into ten Indian languages (Hindi, Urdu, Marathi, Punjabi, Bengali, Tamil, Telugu, Malayalam, Odiya and Gujarati), and then independently back-translated to ensure accuracy. Surveys were administered in whichever language the participants were most comfortable with (including English). Sengupta (2020a) includes data from participants who belonged to three disadvantaged caste groups and reported their contact experiences with members of upper castes ($N$ = 96,547). Sengupta (2020b) includes data from participants who belonged to four religious minority groups and reported their contact experiences with Hindus ($N$ = 7,440).

**Sengupta & Sibley (2020)** is based on data from a large-scale national longitudinal study in New Zealand, the New Zealand Attitudes and Values Study (NZAVS; `https://www.psych.auckland.ac.nz/en/about/new-zealand-attitudes-and-values-study.html`). Sengupta & Sibley (2020) includes data from Māori, Pacific Islanders, and Asians who reported their contact experiences with European New Zealanders ($N$ = 3187).

**Wölfer & Hewstone (2019)** is based on data from a social network study across ten schools in England which was conducted as part of a project for which the first author of the present research worked as a postdoctoral researcher. A survey company collected data from the same grade (Year 10, equivalent to 9th grade in the US) at each school. Data were collected between January and March 2018. Students completed a pen-and-paper survey in a classroom session (40–45 minutes). As part of a larger study on intergroup contact among young people, the survey included items measuring cross-group friendship as well as perceptions of discrimination. Wölfer & Hewstone (2019) included data from Black, Asian, and other ethnic minority students who reported their contact experiences with White people ($N$ = 713).

# References


*Bracegirdle, C. (2020). *Social network study of two diverse schools in Northern England* [Unpublished dataset]. University of Oxford.

Bracegirdle, C., Reimer, N. K., Zalk, M. van, Hewstone, M., & Wölfer, R. (2022). Disentangling contact and socialization effects on outgroup attitudes in diverse friendship networks. *Journal of Personality and Social Psychology*, *122*(1), 1–15. https://doi.org/10.1037/pspa0000240

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*Sengupta, N. K. (2020a). *Inter-caste contact in India* [Unpublished dataset]. University of Kent.

*Sengupta, N. K. (2020b). *Inter-religious contact in India* [Unpublished dataset]. University of Kent.

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*Sengupta, N. K., & Sibley, C. G. (2020). *Intergroup contact and political attitudes among ethnic minorities in New Zealand* [Unpublished dataset]. The University of Auckland.

*Wölfer, R., & Hewstone, M. (2019). *Social integration in diverse societies: The importance of contact experiences in youth* [Unpublished dataset]. University of Oxford.