Essays on social cohesion, inequality, preferences for redistribution, and prosocial behavior

Inaugural-Dissertation

zur Erlangung des akademischen Grades
einer Doktorin oder eines Doktors
der Wirtschafts- und Sozialwissenschaften der
Wirtschafts- und Sozialwissenschaftlichen Fakultät der
Christian-Albrechts-Universität zu Kiel

vorgelegt von David Gerhard Pipke, M.Sc.
aus Flensburg

Kiel, 2022

Gedruckt mit der Genehmigung der

Wirtschafts- und Sozialwissenschaftlichen Fakultät der

Christian-Albrechts-Universitat zu Kiel

Erstbegutachtung: Prof. Dr. Dr. Ulrich Schmidt
Zweitbegutachtung: Prof. Dr. Tobias Heidland

Tag der Abgabe der Arbeit: 30.08.2022
Tag der mündlichen Prüfung: 28.02.2023
Erteilung der Druckerlaubnis: 28.02.2023

Acknowledgments

I am indebted to so many people who supported me along this journey. However, because of the tremendous amount, it is impossible to mention everybody, especially since I would be afraid to forget someone. I thank everyone. :)

First, I have to thank my thesis supervisor Prof. Dr. Dr. Ulrich Schmidt, who attracted my attention to behavioral economics during his lectures and seminars during my studies. Without him, I probably would have never started this journey. Thank you very much for allowing me to work on my thesis in your research area and for giving me all the freedom to research and work with international experts. In the same breath, I have to mention Gianluca Grimalda, Ph.D., for his patience, precious hints, and cooperation throughout the whole time. Thank you very much for allowing me to learn from you and participate in your research! In challenging times during the first two and a half years of the pandemic, you and the other colleagues you introduced to me made it possible to communicate uncomplicated all the time without being in the same place.

I am also grateful for the inspiring research atmosphere of the Kiel Institute (IfW), even though it took mainly place in online seminars and conferences. Special thanks also to Prof. Dr. Tobias Heidland, who kindly accepted to be my second referee. I am very grateful for the financial support via the Landesgraduiertenstipendium from the Land Schleswig-Holstein. I thank Prof. Dr. Kai Carstensen for his reference letter, which secured this financial support.

Last but not least, I thank my whole family for getting my back and for always supporting me to focus on my tasks. Thank you for your love, your encouragement, and your trustful support. I am grateful that many inspiring persons made it possible for me to finish this Ph.D. thesis during the last three years. Thanks!

For my parents

Contents

List of Tables

List of Figures

Overview of papers and co-authorship

This dissertation consists of the following five papers:

- Grimalda, G. & Pipke, D. (2021). Cross-country evidence on the determinants of preferences for redistribution.
- Schütt, C., Pipke, D., Detlefsen, L., & Grimalda, G. (2022). Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis.
- Bogliacino, F., Grimalda, G., & Pipke, D. (2021). Kind or contented? An experimental investigation of the impact of bonus payments on workers' productivity.
- Pipke, D. (2022). Black Lives Matter: Findings on protests, prosociality, discrimination, and racial attitudes from large-scale online experiments.
- Grimalda, G., Murtin, F., Pipke, D., Putterman, L., & Sutter, M. (2022). The politicized pandemic: Ideological polarization and the behavioral response to COVID-19.

Each co-author contributed significantly to the articles' concept, design, and content. Therefore, all persons entitled to authorship have been so named. The following pages provide signed declarations of compliance with the guidelines of the Deutsche Forschungsgemeinschaft (DFG) regarding authorship, listing the contributions of each co-author to the articles.

Contribution of each co-author and compliance with the guidelines of the Deutsche Forschungsgemeinschaft (DFG) (German Research Foundation)

Article: *Cross-country evidence on the determinants of preferences for redistribution.*

Authors: Gianluca Grimalda, David Pipke

All authors contributed to the study design and concept. David Pipke performed the data analysis and drafted the manuscript under the supervision of Gianluca Grimalda. All authors contributed to interpreting the results and provided critical revisions to the manuscript. Compliance with the DFG-rules *"Sicherung guter wissenschaftlicher Praxis" (Securing good scientific conduct)* is confirmed.

Gianluca Grimalda

David Pipke

Contribution of each co-author and compliance with the guidelines of the Deutsche Forschungsgemeinschaft (DFG) (German Research Foundation)

Article: *Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis.*

Authors: Christoph Schütt, David Pipke, Lena Detlefsen, Gianluca Grimalda

Lena Detlefsen, Gianluca Grimalda, and Christoph Schütt contributed to the study design and concept. David Pipke performed the data analysis and drafted the manuscript under the supervision of Gianluca Grimalda. All authors contributed to interpreting the results and provided critical revisions to the manuscript. Compliance with the DFG-rules *"Sicherung guter wissenschaftlicher Praxis" (Securing good scientific conduct)* is confirmed.

C. Schütt

Christoph Schütt

David Pipke

David Pipke

Lena Detlefsen

Gianluca Grimalda

Contribution of each co-author and compliance with the guidelines of the Deutsche Forschungsgemeinschaft (DFG) (German Research Foundation)

Article: *Kind or contented? An experimental investigation of the impact of bonus payments on workers' productivity.*

Authors: Francesco Bogliacino, Gianluca Grimalda, David Pipke

Francesco Bogliacino and Gianluca Grimalda contributed to the study design and concept. David Pipke performed the data analysis and drafted the manuscript under the supervision of Gianluca Grimalda. All authors contributed to interpreting the results and provided critical revisions to the manuscript. Compliance with the DFG-rules *"Sicherung guter wissenschaftlicher Praxis" (Securing good scientific conduct)* is confirmed.

Francesco Bogliacino

Gianluca Grimalda

David Pipke

Contribution of each co-author and compliance with the guidelines of the Deutsche Forschungsgemeinschaft (DFG) (German Research Foundation)

Article: *The politicized pandemic: Ideological polarization and the behavioral response to COVID-19.*

Authors: Gianluca Grimalda, Fabrice Murtin, David Pipke, Louis Putterman, Matthias Sutter

All authors contributed to the study design and concept. David Pipke performed the data analysis and drafted the manuscript under the supervision of Gianluca Grimalda. All authors contributed to interpreting the results and provided critical revisions to the manuscript. Compliance with the DFG-rules *"Sicherung guter wissenschaftlicher Praxis" (Securing good scientific conduct)* is confirmed.

____
Gianluca Grimalda

____
Fabrice Murtin

____
David Pipke

____
Louis Putterman

____
Matthias Sutter

Introduction

Social cohesion is often described as "the glue" or "the bonds" that hold societies together (Capshaw, 2005; Larsen, 2013). Concordantly, the European Union and the Council of Europe made social cohesion a strategic priority (European Union, 2007). Moreover, social cohesion and its components are deemed as essential prerequisites for functioning, economically successful societies (Putnam, 1993; Fukuyama, 1995; OECD, 2011; Algan and Cahuc, 2014).

Various definitions of social cohesion have been developed over time (Fonseca et al., 2018). Still, there is no final consensus about a clear definition of social cohesion, which, in consequence, remains a rather vague construct (Schiefer and van der Noll, 2017), and its understanding often varies from author to author (Forrest and Kearns, 2001; Chan et al., 2006; Letki, 2008; Schaeffer, 2014). For example, the Council of Europe (2008) defines social cohesion as "the capacity of a society to ensure the well-being of all its members, minimizing disparities and avoiding marginalization," listing, among other factors, income equality, (absence of) discrimination (e.g., against ethnic minorities), and trust in other people and institutions as its defining components. On the other hand, the OECD (2011) describes a cohesive society as one that "works towards the well-being of all its members, fights exclusion and marginalization, creates a sense of belonging, promotes trust, and offers its members the opportunity of upward mobility."

Although there exists no universal definition of social cohesion, a common pattern among the definitions is sharing the view that trust in other people, inclusion, reciprocity, solidarity, preferences for redistribution, and prosocial behavior are undoubtedly interdependent, crucial components of social cohesion and its key dimension of social capital (Putnam, 2000; Forrest and Kearns, 2001; Alesina et al., 2003; Alesina and La Ferrara, 2005a; Easterly et al., 2006; Uslaner, 2012b). I adopt this broad and pragmatic view on social cohesion in this dissertation, which studies several interconnected concepts related to social cohesion in detail and contributes to understanding them in different contexts.

This dissertation consists of five self-containing chapters written in the style of research articles which can be read separately. The five chapters are related to factors that are linked to social cohesion or are even commonly seen as its components or determinants. Each chapter contains its own introduction with a detailed overview of the relevant research literature it contributes to and a discussion of the presented results. The first two chapters deal with preferences for redistribution yet explore different research questions and use different methods. The first chapter analyzes preferences for redistribution based on an extensive cross-country survey. In contrast, the second chapter deals with an experimental investigation of work effort provision in a simplified version of a welfare state. The third chapter tests whether reciprocity, i.e., the disposition to repay kind actions with kindness and spiteful actions with spite (Rabin, 1993; Fehr and Gächter, 2000), drives effort at work, even in the presence of wage inequality. In the fourth chapter, I analyze whether racial attitudes are explained by prosocial and discriminating behavior in economic games and how these behavioral measures interact with the effect of protests on attitudes. Finally, the fifth chapter is about the interactions of political polarization and prosocial behavior with protective behavior and the assessment of the governmental response regarding the COVID-19 pandemic, which urgently demands cooperative and solidly united societies to be successfully fought. The insights gained in these chapters are based on a combination of methods comprising incentivized economic experiments in the laboratory and the field, survey measures, and empirical methods drawing from a connection of different data sources.

The first chapter analyzes preferences for redistribution utilizing data from the OECD's Trustlab initiative that combines large-scale online-surveys with economic experiments (Murtin et al., 2018). Redistribution is one of the essential features of modern welfare states, which redistribute large proportions of their GDP. For example, public spending on social purposes in Germany and France amounts to roughly 26 and 31 percent of the countries' GDPs, respectively, according to the Social Expenditure Database (OECD, 2020). We analyze the preferences for redistribution of more than seven thousand participants from six developed countries (Germany, Italy, Japan, Slovenia, the UK, and the U.S.) using a quantitative outcome variable based on respondents' preferred tax rates facing a realistic budget constraint. At the level of countries, we find that U.S. respondents prefer, on average, the least progressive tax schemes, with Italian respondents being very close to those from the U.S. in their preferred tax rates. On the other hand, respondents from Germany, Slovenia, and the UK are relatively more "pro-redistribution," whereas the tax preferences of Japanese respondents lie between both groups. Our data allows testing the effects of many potential individual determinants of redistributive preferences. Our results confirm many findings from the theoretical and empirical literature (Fong, 2001; Alesina and Giuliano, 2011; Alesina et al., 2018). We find that personal income matters, with the rich demanding less redistribution than the poor, confirming the relevance of monetary self-interest (Corneo and Grüner, 2002). However, the effect of income loses significance when other factors (e.g., beliefs about social mobility and perceived financial security in the next twelve months) are controlled for. Beliefs about social mobility offered by the society are an important predictor of the preferred progressivity of the tax scheme. Respondents who believe in such opportunities provided by society to "climb the ladder" for everyone, on average, prefer less redistributive tax schemes. This finding is consistent with one of the main arguments in the literature postulating that beliefs about the fairness of the economic system matter for preferences for redistribution (Piketty, 1995; Fong, 2001; Alesina and Angeletos, 2005b; Bénabou and Tirole, 2006a; Alesina and Giuliano, 2011; Alesina et al., 2018), particularly when contrasting American and European welfare states (Alesina and Glaeser, 2004). More positive expectations about future household incomes negatively correlate with the demand for redistribution, in line with the Prospect of Upward Mobility hypothesis (the "POUM" hypothesis) (Bénabou and Ok, 2001). Trust in government negatively affects the demand for redistribution across all countries. We interpret this surprising result contrasting previous evidence (Kuziemko et al., 2015; Stantcheva, 2021) as a sign that people who believe that the political elite is corrupt demand more redistribution, in line with theoretical arguments brought forward by Alesina and Angeletos (2005a) and findings by Di Tella et al. (2021). The chapter also contributes to whether immigration and increasing diversity represent a threat to social cohesion (Alesina et al., 1999, 2021a,c, 2022), which is seen as a factor contributing to the less extensive welfare state in the U.S. compared to European countries (Alesina and Glaeser, 2004). More negative attitudes towards immigrants are associated with lower demand for redistribution in the data, but the effect is only significant in the U.S. and Germany. Hence, not all factors are equally important in every country in the sample, contributing to the literature on cultural differences regarding preferences for redistribution (Guillaud, 2013). This first chapter is co-authored with Gianluca Grimalda.

The second chapter is also related to preferences for redistribution which are commonly assumed to travel less across racial and ethnic lines (Alesina and Giuliano, 2011; Stichnoth and van der Straeten, 2013). In a laboratory experiment, we test whether real effort of German university students (the workers) in the slider task (Gill and Prowse, 2012) depends on the identity of a potential recipient in a simplified version of a welfare state. We informed the workers that a third-party allocator could decide about the share of earnings that would be redistributed to either (i) another German citizen, (ii) an asylum seeker, or (iii) an economic migrant. The results demonstrate that, on average, the type of potential recipient does not affect performance in the task. However, workers who indicated a strong identification with their "objective" ingroup of Germans in a post-experimental survey exert significantly less effort if the recipient is from the asylum seeker outgroup. Other questions about outgroups (asylum seekers and economic migrants) do not reveal a similar heterogeneity, suggesting that questions about closeness to one's ingroup may be less prone to social desirability biases (Fong and Luttmer, 2009, 2011). Furthermore, workers with a strong ingroup identification expected relatively larger shares of their earnings to be redistributed by another German to a recipient being an asylum seeker than participants without significant ingroup identification. Hence, workers with a strong identification with their ingroup think of other members of their ingroups as caring relatively more about outgroups than workers without such a strong identification. Interestingly, beliefs about the tax rate do not affect exerted effort. The chapter extends a sizeable existing literature on group loyalty effects, i.e., that people value the well-being of their "ingroup," the group to which they feel connected, more than that of "outgroups" (Tajfel, 1974; Brewer, 1999). It also contributes to the literature on social cohesion, immigration, and enhanced diversity (Schaeffer, 2014; van Staveren and Pervaiz, 2017; Tabellini, 2020). The second chapter is co-authored with Lena Detlefsen, Gianluca Grimalda, and Christoph Schütt.

The third chapter is about a test of the gift exchange hypothesis (Akerlof, 1982) in a natural field experiment (following the definition of Harrison and List (2004)) conducted in Colombia's capital Bogotá. Standard neoclassical economic models predict that workers behave opportunistically, are employed at the market-clearing wage, and provide only minimal effort (Lazear, 2000; List and Rasul, 2011). Several mechanisms such as efficiency wages (Katz, 1986), implicit contracts (Azariadis, 1975), and insider-outsider relationships (Lindbeck and Snower, 1988) have been proposed to explain real-life observations at odds with this prediction. Akerlof (1982) and Akerlof and Yellen (1990) brought up the idea of a gift exchange relationship between firms and workers. Firms pay more than the market-clearing wage in such a relationship, and workers reciprocate with higher than minimal effort. Hence, workers in a gift exchange relationship take a similar role to that of a second-mover in the trust game (Berg et al., 1995). In our experiment, we test whether surprise pay bonuses that induce inequality between two workers affect productivity in a data-entry task. The pay bonuses are assigned either (i) arbitrarily, (ii) to the more productive worker, or (iii) based on economic neediness. Two conditions in which both workers or none receive a bonus serve as reference conditions. The announcement of bonuses after the lunch break was a surprise in all treatment conditions. We clarified what the final earnings would be to rule out confounds, e.g., due to performance incentives. The bonus is an unconditional act of kindness by the employer as workers in our experiment are not obliged to exert (higher) effort in response to the bonus, consistent with the gift exchange hypothesis (Akerlof, 1982; Al-Ubaydli and List, 2019). Thus, our experiment constitutes a clean test of whether workers' effort is motivated by reciprocity, i.e., the exchange of favors for mutual benefit, which is a widely accepted determinant of socially cohesive and functioning economies (Goh et al., 2019; Alan et al., 2021). We propose a simple model combining features of reciprocity (Rabin, 1993) and inequality aversion (Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000) to guide our analysis. Contrasting the gift exchange hypothesis, we find that bonuses lead to decreased productivity. The effect is most potent when both workers receive a bonus and also exists when only one worker benefits. We interpret the result as workers construing the bonus as a sign of the employer being contented with their effort, prompting opportunistic behavior instead of reciprocity, a novel result in the enormous literature on this topic (Fehr et al., 1993; Charness, 2004; Gneezy and List, 2006; Kube et al., 2012). Our results concerning advantageous horizontal pay inequality between both workers point to some relevance of status-seeking behavior (Frank, 1985), mainly when the bonus assignment is due to merit. The third chapter is co-authored with Francesco Bogliacino and Gianluca Grimalda.

In the fourth chapter, I analyze racial prejudice against African Americans and support for one of the main goals of the Black Lives Matter (BLM) movement, using data from two waves of the Trustlab conducted in 2017 and 2020. The data comprise more than two-thousand respondents representative of the U.S. population. The chapter shows that support for one of the main goals of the BLM movement, i.e., that African Americans should be treated with equal respect by the police, and racial prejudice against African Americans correlate with behavioral measures of prosociality and discrimination in incentivized experimental games. I measure discrimination as discrimination against African Americans versus white Americans in interethnic trust games. I.e., respondents who send lower amounts to African Americans than to white Americans in interethnic trust games are classified as discriminating against African Americans. On the other hand, prosociality is an index based on behavior in the dictator and the public goods game. The results show that explicit racial prejudice correlates strongly with discrimination against African Americans in the trust games. On the other hand, (above-median) prosociality is primarily related to the importance of the equal treatment goal of the BLM movement. Discrimination, unlike in the case of racial prejudice, is not predictive of the support for the BLM movement in the general sample. A heterogeneity analysis further shows that (above-median) prosociality correlates slightly negatively with prejudice against African Americans among non-right-wing respondents and positively for right-wing respondents. In addition, discrimination against African Americans correlates negatively with support for the BLM movement among non-right-wing respondents but positively among right-wing respondents. The attitudinal gaps between left-wing and right-wing respondents are mediated by prosociality and the propensity to discriminate to a minor extent only.

Instead, this first set of findings shows that social preferences and ideology more or less independently affect racial attitudes, which are multi-faceted and cannot be explained by political orientation, discrimination, or prosociality alone. This rather methodological set of results contributes to studies examining racial or minority-related attitudes measured in various ways (Stepanikova et al., 2011; Cetre et al., 2020; Haaland and Roth, 2021).

The second contribution of this chapter is an analysis of the effects that BLM protests unfolded on public opinion in the U.S. in 2020 and how these effects interact with the measures of observed behavior in the experiments and political orientation. Overall, the results are consistent with the logic of disruptive action (Sharp, 2013; Shuman et al., 2021). According to such theories, the experience of disruptive events such as protests can affect public opinion and increase the support for the protesters' goals (Kingdon, 1995; Lee, 2002). Regarding the largest eruption of BLM protests in U.S. history following the murder of George Floyd, I show that higher geographical proximity to a more significant amount of contemporaneous BLM protests (protest intensity) correlates with weaker racial prejudice. At the same time, protest intensity correlates with respondents attaching higher importance to equal treatment of African Americans by the police, i.e., one of the movement's primary goals. To some extent, the results suggest that BLM protests negatively affect trust in the police. However, this effect is statistically significant only in some specifications.

The heterogeneity analysis reveals that the relatively more prosocial respondents primarily drive the observed effect of protest intensity on increased support for the movement. I also find that BLM protests affect the support for the BLM movement's primary goal relatively homogeneously across groups of political orientation. On the contrary, the protests' prejudice-decreasing impact is absent among right-wing respondents. Hence, these findings suggest that BLM protests contributed to a further increase in the political polarization on racial issues in the U.S. (Iyengar et al., 2019; Alesina et al., 2021a; Haaland and Roth, 2021; Druckman et al., 2022), whereas support for one of the movement's primary goals was fostered above party lines. First and foremost, the second part of this chapter contributes to research concerning the effects of the 2020 eruption of BLM protests on public opinion (Alesina et al., 2021a; Reny and Newman, 2021; Teeselink and Melios, 2021; Shuman et al., 2022). From a general perspective, the chapter also advances existing research on protests' and historical movements' impact on election outcomes and political attitudes in various domains (Collins and Margo, 2007; Madestam et al., 2013; Wallace et al., 2014; Mazumder, 2018; Enos et al., 2019; Ketchley and El-Rayyes, 2021). To my knowledge, investigating whether (BLM) protests' impact on attitudes interacts with measures of prosociality and discrimination in incentivized games is a novelty to the literature on protests. The fourth chapter is sole-authored.

The fifth chapter analyzes prosocial behavior in the COVID-19 pandemic and the assessment of the governmental response to it. From the pandemic's beginning, protective measures such as self-quarantining and wearing a face mask were linked to prosocial behavior (Betsch et al., 2020; van Bavel et al., 2020). Furthermore, public messaging about those measures emphasized their protection of others and oneself (WHO, 2020a; CDC, 2021). On the other hand, political polarization has been upward-trending in the U.S. for a long time (Iyengar et al., 2019; Svolik, 2019; Gidron et al., 2020; Foa and Mounk, 2017, 2021; McCoy and Somer, 2021), and has been blamed for impeding efforts to control the pandemic, e.g., due to affective polarization weakening prosocial tendencies in society, conflicting messages sent by political leaders and partisan media affecting their followers' beliefs and, consequently, their risk assessments, and a general tendency to politicize public health measures targetting at reducing the spread of the virus (Dryhurst et al., 2020; Bruine de Bruin et al., 2020; Kerr et al., 2021).

The study utilizes data from an online experiment run on a representative sample (along the targeted gender, age, and income dimensions) of 1,120 U.S. Americans to study the interactions of prosociality, ideological polarization, protective behavior, and judging the political crisis management. The survey was conducted during the summer of 2020, when cases and deaths in the U.S. reached a second peak. We measure prosociality using an index based on behavior in standard dictator games and public goods games. The prosociality index correlates positively with protective behavior and worries about the local spread, consistent with our hypotheses. Remarkably, prosociality and political ideology are somewhat independent in affecting health-related behavior. This finding suggests that liberals' higher compliance with COVID-19 regulations is not due to their different degrees of prosociality. Instead, relatively more prosocial people tend to comply more strongly with regulations independently of their political ideology. Yet, most importantly, we find that behavioral differences between liberals and conservatives are up to 4.4 times more minor than their differences in judging the government's crisis management. Moreover, on average, participants with conservative political orientation worried less about the virus spread and reported lower levels of self-quarantining and wearing face masks than participants with liberal political orientation. Furthermore, we observe considerably higher polarization across ideological camps when assessing trust in the political elite and their crisis management capacity. For example, differences between conservatives and liberals in judging the political control of the pandemic are up to five times as large as differences in self-reported worries and behavioral measures. This result suggests that political polarization is considerably larger than behavioral polarization. The chapter contributes to a growing literature on the relevance of prosociality for protective behavior and ideological polarization's detrimental effects on fighting the pandemic (Allcott et al., 2020; Gollwitzer et al., 2020; Campos-Mercade et al., 2021b; Müller and Rau, 2021). Broadly seen, the study also advances the literature examining the impact of social preferences on real-life behavior (Levitt and List, 2007; Franzen and Pointner, 2013; Galizzi and Navarro-Martinez, 2019) and the consequences of political polarization for a wide field of economic outcomes (Kuziemko et al., 2015; Alesina et al., 2021a). The fifth chapter is co-authored with Gianluca Grimalda, Fabrice Murtin, Louis Putterman, and Matthias Sutter.

References

Akerlof, George A. 1982. "Labor Contracts as Partial Gift Exchange." *The Quarterly Journal of Economics*, 97 (4): 543–569.

Akerlof, George A. and Janet L. Yellen. 1990. "The Fair Wage-Effort Hypothesis and Unemployment." *The Quarterly Journal of Economics*, 105 (2): 255–283.

Al-Ubaydli, Omar and John A. List. 2019. "How natural field experiments have enhanced our understanding of unemployment." *Nature Human Behaviour*, 3 (1): 33–39.

Alan, Sule, Ceren Baysan, Mert Gumren, and Elif Kubilay. 2021. "Building Social Cohesion in Ethnically Mixed Schools: An Intervention on Perspective Taking." *The Quarterly Journal of Economics*, 136 (4): 2147–2194.

Alesina, Alberto and George-Marios Angeletos. 2005a. "Corruption, inequality, and fairness." *Journal of Monetary Economics*, 52 (7): 1227–1244.

Alesina, Alberto and George-Marios Angeletos. 2005b. "Fairness and Redistribution." *American Economic Review*, 95 (4): 960–980.

Alesina, Alberto, Reza Baqir, and William Easterly. 1999. "Public Goods and Ethnic Divisions." *The Quarterly Journal of Economics*, 114 (4): 1243–1284.

Alesina, Alberto, Arnaud Devleeschauwer, William Easterly, Sergio Kurlat, and Romain Wacziarg. 2003. "Fractionalization." *Journal of Economic Growth*, 8 (2): 155–194.

Alesina, Alberto, Matteo Ferroni, and Stefanie Stantcheva. 2021a. "Perceptions of racial gaps, their causes, and ways to reduce them." *NBER Working Paper*, 29245.

Alesina, Alberto and Paola Giuliano. 2011. "Preferences for redistribution." In *Handbook of Social Economics*, edited by Benhabib, Jess, Alberto Bisin, and

Matthew O. Jackson Volume 1A. of Handbook of Economics 93–131, Burlington: Elsevier Science.

Alesina, Alberto and Edward Ludwig Glaeser. 2004. *Fighting poverty in the US and Europe: A world of difference*. Oxford: Univ. Press.

Alesina, Alberto and Eliana La Ferrara. 2005a. "Ethnic Diversity and Economic Performance." *Journal of Economic Literature*, 43 (3): 762–800.

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva. 2022. "Immigration and Redistribution." *The Review of Economic Studies*, Accepted Manuscript (0): 1–39.

Alesina, Alberto, Elie Murard, and Hillel Rapoport. 2021c. "Immigration and preferences for redistribution in Europe." *Journal of Economic Geography*, 21 (6): 925–954.

Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. 2018. "Intergenerational Mobility and Preferences for Redistribution." *American Economic Review*, 108 (2): 521–554.

Algan, Yann and Pierre Cahuc. 2014. "Trust, Growth, and Well-Being: New Evidence and Policy Implications." In *Handbook of Economic Growth*, edited by Aghion, Philippe and Steven N. Durlauf 49–120, Burlington: Elsevier Science.

Allcott, Hunt, Levi Boxell, Jacob Conway, Matthew Gentzkow, Michael Thaler, and David Yang. 2020. "Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic." *Journal of Public Economics*, 191 (0): 104254.

Azariadis, Costas. 1975. "Implicit Contracts and Underemployment Equilibria." *Journal of Political Economy*, 83 (6): 1183–1202.

Bénabou, Roland and Efe A. Ok. 2001. "Social Mobility and the Demand for Redistribution: The POUM Hypothesis." *The Quarterly Journal of Economics*, 116 (2): 447–487.

Bénabou, Roland and Jean Tirole. 2006a. "Belief in a Just World and Redistributive Politics." *The Quarterly Journal of Economics*, 121 (2): 699–746.

Berg, Joyce, John Dickhaut, and Kevin McCabe. 1995. "Trust, Reciprocity, and Social History." *Games and Economic Behavior*, 10 (1): 122–142.

Betsch, Cornelia, Lars Korn, Philipp Sprengholz, Lisa Felgendreff, Sarah Eitze, Philipp Schmid, and Robert Böhm. 2020. "Social and behavioral consequences of mask policies during the COVID-19 pandemic." *Proceedings of the National Academy of Sciences of the United States of America*, 117 (36): 21851–21853.

Bolton, Gary E. and Axel Ockenfels. 2000. "ERC: A Theory of Equity, Reciprocity, and Competition." *American Economic Review*, 90 (1): 166–193.

Brewer, Marilynn B. 1999. "The Psychology of Prejudice: Ingroup Love and Outgroup Hate?" *Journal of Social Issues*, 55 (3): 429–444.

Bruine de Bruin, Wändi, Htay-Wah Saw, and Dana P. Goldman. 2020. "Political polarization in US residents' COVID-19 risk perceptions, policy preferences, and protective behaviors." *Journal of Risk and Uncertainty*, 61 (2): 177–194.

Campos-Mercade, Pol, Armando N. Meier, Florian H. Schneider, and Erik Wengström. 2021b. "Prosociality predicts health behaviors during the COVID-19 pandemic." *Journal of Public Economics*, 195 (0): 104367.

Capshaw, N. Clark. 2005. "The Social Cohesion Role of the Public Sector." *Peabody Journal of Education*, 80 (4): 53–77.

CDC. 2021. "Guidance for wearing masks: Help slow the spread of COVID-19." `https://www.cdc.gov/coronavirus/2019-ncov/prevent-getting-sick/cloth-face-cover-guidance.html`.

Cetre, Sophie, Yann Algan, Gianluca Grimalda, Fabrice Murtin, Louis Putterman, Ulrich Schmidt, and Vincent Siegerink. 2020. "Ethnic bias, economic success and trust: Findings from large sample experiments in Germany and the United States through the Trustlab platform." *OECD Statistics Working Papers*, 2020/04.

Chan, Joseph, Ho-Pong To, and Elaine Chan. 2006. "Reconsidering Social Cohesion: Developing a Definition and Analytical Framework for Empirical Research." *Social Indicators Research*, 75 (2): 273–302.

Charness, Gary. 2004. "Attribution and Reciprocity in an Experimental Labor Market." *Journal of Labor Economics*, 22 (3): 665–688.

Collins, William J. and Robert A. Margo. 2007. "The Economic Aftermath of the 1960s Riots in American Cities: Evidence from Property Values." *The Journal of Economic History*, 67 (4): 849–883.

Corneo, Giacomo and Hans Peter Grüner. 2002. "Individual preferences for political redistribution." *Journal of Public Economics*, 83 (1): 83–107.

Council of Europe. 2008. *Report of High-Level Task Force on Social Cohesion: Towards an Active, Fair and Socially Cohesive Europe*. Strasbourg: Conseil de l'Europe.

Di Tella, Rafael, Juan Dubra, and Alejandro Lagomarsino. 2021. "Meet the Oligarchs: Business Legitimacy and Taxation at the Top." *The Journal of Law and Economics*, 64 (4): 651–674.

Druckman, James N., Samara Klar, Yanna Krupnikov, Matthew Levendusky, and John Barry Ryan. 2022. "(Mis)estimating affective polarization." *The Journal of Politics*, 84 (2): 1106–1117.

Dryhurst, Sarah, Claudia R. Schneider, John Kerr, Alexandra L. J. Freeman, Gabriel Recchia, Anne Marthe van der Bles, David Spiegelhalter, and Sander van der Linden. 2020. "Risk perceptions of COVID-19 around the world." *Journal of Risk Research*, 23 (7-8): 994–1006.

Easterly, William, Jozef Ritzen, and Michael Woolcock. 2006. "Social cohesion, institutions, and growth." *Economics & Politics*, 18 (2): 103–120.

Enos, Ryan D., Aaron R. Kaufman, and Melissa L. Sands. 2019. "Can Violent Protest Change Local Policy Support? Evidence from the Aftermath of the 1992 Los Angeles Riot." *American Political Science Review*, 113 (4): 1012–1028.

European Union. 2007. *Cohesion policy 2007 - 13: Commentaries and official texts. European Union, Regional Policy Guide*. Luxembourg: Office for Official Publications of the European Communities.

Fehr, Ernst and Simon Gächter. 2000. "Fairness and Retaliation: The Economics of Reciprocity." *Journal of Economic Perspectives*, 14 (3): 159–182.

Fehr, Ernst, G. Kirchsteiger, and A. Riedl. 1993. "Does Fairness Prevent Market Clearing? An Experimental Investigation." *The Quarterly Journal of Economics*, 108 (2): 437–459.

Fehr, Ernst and Klaus M. Schmidt. 1999. "A Theory of Fairness, Competition, and Cooperation." *The Quarterly Journal of Economics*, 114 (3): 817–868.

Foa, Roberto Stefan and Yascha Mounk. 2017. "The signs of deconsolidation." *Journal of Democracy*, 28 (1): 5–15.

Foa, Roberto Stefan and Yascha Mounk. 2021. "America after Trump: From "clean" to "dirty" democracy?" *Policy Studies*, 42 (5-6): 455–472.

Fong, Christina M. 2001. "Social preferences, self-interest, and the demand for redistribution." *Journal of Public Economics*, 82 (2): 225–246.

Fong, Christina M. and Erzo F.P. Luttmer. 2009. "What Determines Giving to Hurricane Katrina Victims? Experimental Evidence on Racial Group Loyalty." *American Economic Journal: Applied Economics*, 1 (2): 64–87.

Fong, Christina M. and Erzo F.P. Luttmer. 2011. "Do fairness and race matter in generosity? Evidence from a nationally representative charity experiment." *Journal of Public Economics*, 95 (5-6): 372–394.

Fonseca, Xavier, Stephan Lukosch, and Frances Brazier. 2018. "Social cohesion revisited: A new definition and how to characterize it." *Innovation: The European Journal of Social Science Research*, 32 (2): 231–253.

Forrest, Ray and Ade Kearns. 2001. "Social Cohesion, Social Capital and the Neighbourhood." *Urban Studies*, 38 (12): 2125–2143.

Frank, Robert H. 1985. *Choosing the right pond: Human behavior and the quest for status.* New York, NY: Oxford Univ. Press.

Franzen, Axel and Sonja Pointner. 2013. "The external validity of giving in the dictator game." *Experimental Economics*, 16 (2): 155–169.

Fukuyama, Francis. 1995. *Trust: The social virtues and the creation of prosperity.* New York, NY: Free Press.

Galizzi, Matteo M. and Daniel Navarro-Martinez. 2019. "On the external validity of social preference games: A systematic lab-field study." *Management Science*, 65 (3): 976–1002.

Gidron, Noam, James Adams, and Will Horne. 2020. *American affective polarization in comparative perspective.* Cambridge University Press.

Gill, David and Victoria Prowse. 2012. "A Structural Analysis of Disappointment Aversion in a Real Effort Competition." *American Economic Review*, 102 (1): 469–503.

Gneezy, Uri and John A. List. 2006. "Putting Behavioral Economics to Work: Testing for Gift Exchange in Labor Markets Using Field Experiments." *Econometrica*, 74 (5): 1365–1384.

Goh, Debbie, Richard Ling, Liuyu Huang, and Doris Liew. 2019. "News sharing as reciprocal exchanges in social cohesion maintenance." *Information, Communication & Society*, 22 (8): 1128–1144.

Gollwitzer, Anton, Cameron Martel, William J. Brady, Philip Pärnamets, Isaac G. Freedman, Eric D. Knowles, and Jay J. van Bavel. 2020. "Partisan differences in physical distancing are linked to health outcomes during the COVID-19 pandemic." *Nature Human Behaviour*, 4 (11): 1186–1197.

Guillaud, Elvire. 2013. "Preferences for redistribution: An empirical analysis over 33 countries." *The Journal of Economic Inequality*, 11 (1): 57–78.

Haaland, Ingar and Christopher Roth. 2021. "Beliefs about Racial Discrimination and Support for Pro-Black Policies." *Review of Economics and Statistics*, Just Accepted MS (0): 1–38.

Harrison, Glenn W. and John A. List. 2004. "Field Experiments." *Journal of Economic Literature*, 42 (4): 1009–1055.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J. Westwood. 2019. "The origins and consequences of affective polarization in the United States." *Annual Review of Political Science*, 22 (1): 129–146.

Katz, Lawrence F. 1986. "Efficiency Wage Theories: A Partial Evaluation." *NBER Macroeconomics Annual*, 1 (1): 235–276.

Kerr, John, Costas Panagopoulos, and Sander van der Linden. 2021. "Political polarization on COVID-19 pandemic response in the United States." *Personality and Individual Differences*, 179 (5): 110892.

Ketchley, Neil and Thoraya El-Rayyes. 2021. "Unpopular Protest: Mass Mobilization and Attitudes to Democracy in Post-Mubarak Egypt." *The Journal of Politics*, 83 (1): 291–305.

Kingdon, John W. 1995. *Agendas, alternatives, and public policies.* New York, NY: Longman.

Kube, Sebastian, Michel André Maréchal, and Clemens Puppe. 2012. "The Currency of Reciprocity: Gift Exchange in the Workplace." *American Economic Review*, 102 (4): 1644–1662.

Kuziemko, Ilyana, Michael I. Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How elastic are preferences for redistribution? Evidence from randomized survey experiments." *American Economic Review*, 105 (4): 1478–1508.

Larsen, Christian A. 2013. *The rise and fall of social cohesion: The construction and de-construction of social trust in the US UK, Sweden and Denmark*. Oxford: Oxford University Press.

Lazear, Edward P. 2000. "Performance Pay and Productivity." *American Economic Review*, 90 (5): 1346–1361.

Lee, Taeku. 2002. *Mobilizing public opinion: Black insurgency and racial attitudes in the civil rights era*. Studies in communication, media, and public opinion, Chicago, Ill.: Univ. of Chicago Press.

Letki, Natalia. 2008. "Does Diversity Erode Social Cohesion? Social Capital and Race in British Neighbourhoods." *Political Studies*, 56 (1): 99–126.

Levitt, Steven D. and John A. List. 2007. "What do laboratory experiments measuring social preferences reveal about the real world?" *Journal of Economic Perspectives*, 21 (2): 153–174.

Lindbeck, Assar and Dennis Snower. 1988. "Cooperation, Harassment, and Involuntary Unemployment: An Insider-Outsider Approach." *American Economic Review*, 78 (1): 167–188.

List, John A. and Imran Rasul. 2011. "Field Experiments in Labor Economics." In *Handbook of Labor Economics*, edited by Card, David E. and Orley Ashenfelter Volume 4. of Handbooks in Economics 103–228, Amsterdam and New York and New York, N.Y., U.S.A: North-Holland.

Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott. 2013. "Do Political Protests Matter? Evidence from the Tea Party Movement." *The Quarterly Journal of Economics*, 128 (4): 1633–1685.

Mazumder, Soumyajit. 2018. "The Persistent Effect of U.S. Civil Rights Protests on Political Attitudes." *American Journal of Political Science*, 62 (4): 922–935.

McCoy, Jennifer and Murat Somer. 2021. "Overcoming polarization." *Journal of Democracy*, 32 (1): 6–21.

Müller, Stephan and Holger A. Rau. 2021. "Economic preferences and compliance in the social stress test of the COVID-19 crisis." *Journal of Public Economics*, 194 (5): 104322.

Murtin, Fabrice, Lara Fleischer, Vincent Siegerink, Arnstein Aassve, Yann Algan, Romina Boarini, Santiago González, Zsuzsanna Lonti, Gianluca

Grimalda, Rafael Hortala Vallve, Soonhe Kim, David Lee, Louis Putterman, and Conal Smith. 2018. "Trust and its determinants." *OECD Statistics Working Papers*, 2018/02.

OECD. 2011. *Perspectives on Global Development 2012: Social Cohesion in a Shifting World*. Paris: OECD Publishing.

OECD. 2020. "Social Expenditure Database (SOCX)." `http://www.oecd.org/social/expenditure.htm`.

Piketty, Thomas. 1995. "Social Mobility and Redistributive Politics." *The Quarterly Journal of Economics*, 110 (3): 551–584.

Putnam, Robert D. 1993. "The Prosperous Community: Social Capital and Public Life." *The American Prospect*, 4 (13): 35–42.

Putnam, Robert D. 2000. "Bowling Alone: America's Declining Social Capital." In *Culture and Politics*, edited by Crothers, Lane and Charles Lockhart 223–234, New York: Palgrave Macmillan US.

Rabin, Matthew. 1993. "Incorporating Fairness into Game Theory and Economics." *American Economic Review*, 83 (5): 1281–1302.

Reny, Tyler T. and Benjamin J. Newman. 2021. "The Opinion-Mobilizing Effect of Social Protest against Police Violence: Evidence from the 2020 George Floyd Protests." *American Political Science Review*, 115 (4): 1499–1507.

Schaeffer, Merlin. 2014. *Ethnic diversity and social cohesion: Immigration, ethnic fractionalization and potentials for civic action*. Research in migration and ethnic relations series, Farnham, Surrey: Ashgate.

Schiefer, David and Jolanda van der Noll. 2017. "The Essentials of Social Cohesion: A Literature Review." *Social Indicators Research*, 132 (2): 579–603.

Sharp, Gene. 2013. *How nonviolent struggle works*. East Boston, MA: Albert Einstein Institution.

Shuman, Eric, Siwar Hasan-Aslih, Martijn van Zomeren, Tamar Saguy, and Eran Halperin. 2022. "Protest movements involving limited violence can sometimes be effective: Evidence from the 2020 BlackLivesMatter protests." *Proceedings of the National Academy of Sciences of the United States of America*, 119 (14): e2118990119.

Shuman, Eric, Tamar Saguy, Martijn van Zomeren, and Eran Halperin. 2021. "Disrupting the system constructively: Testing the effectiveness of nonnormative nonviolent collective action." *Journal of Personality and Social Psychology*, 121 (4): 819–841.

Stantcheva, Stefanie. 2021. "Understanding Tax Policy: How do People Reason?" *The Quarterly Journal of Economics*, 136 (4): 2309–2369.

Stepanikova, Irena, Jennifer Triplett, and Brent Simpson. 2011. "Implicit racial bias and prosocial behavior." *Social Science Research*, 40 (4): 1186–1195.

Stichnoth, Holger and Karine van der Straeten. 2013. "Ethnic Diversity, Public Spending, and Individual Support for the Welfare State: A Review of the Empirical Literature." *Journal of Economic Surveys*, 27 (2): 364–389.

Svolik, Milan W. 2019. "Polarization versus democracy." *Journal of Democracy*, 30 (3): 20–32.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *The Review of Economic Studies*, 87 (1): 454–486.

Tajfel, Henri. 1974. "Social identity and intergroup behaviour." *Social Science Information*, 13 (2): 65–93.

Teeselink, Bouke Klein and Georgios Melios. 2021. "Weather to Protest: The Effect of Black Lives Matter Protests on the 2020 Presidential Election." *Working Paper*.

Uslaner, Eric M. 2012b. *Segregation and Mistrust*. Cambridge: Cambridge University Press.

van Bavel, Jay J., Katherine Baicker, Paulo S. Boggio, Valerio Capraro, Aleksandra Cichocka, Mina Cikara, Molly J. Crockett, Alia J. Crum, Karen M. Douglas, James N. Druckman, John Drury, Oeindrila Dube, Naomi Ellemers, Eli J. Finkel, James H. Fowler, Michele J. Gelfand, Shihui Han, S. Alexander Haslam, Jolanda Jetten, Shinobu Kitayama et al. 2020. "Using social and behavioural science to support COVID-19 pandemic response." *Nature Human Behaviour*, 4 (5): 460–471.

van Staveren, Irene and Zahid Pervaiz. 2017. "Is it Ethnic Fractionalization or Social Exclusion, Which Affects Social Cohesion?" *Social Indicators Research*, 130 (2): 711–731.

Wallace, Sophia J., Chris Zepeda-Millán, and Michael Jones-Correa. 2014. "Spatial and Temporal Proximity: Examining the Effects of Protests on Political Attitudes." *American Journal of Political Science*, 58 (2): 433–448.

WHO. 2020a. "Advice on the use of masks in the context of COVID-19: Interim guidance 5 June 2020." `https://apps.who.int/iris/handle/10665/332293`.

Chapter 1

Cross-country evidence on the determinants of preferences for redistribution

Gianluca Grimalda[a], David Pipke[b]

An earlier version of this paper is available at http://hdl.handle.net/10419/235614 (permalink) as Grimalda, G. & Pipke, D. (2021). Cross-country evidence on the determinants of preferences for redistribution, Kiel Working Paper, No. 2190, Kiel Institute for the World Economy (IfW Kiel), Kiel.

[a]Kiel Institute for the World Economy (gianluca.grimalda@ifw-kiel.de)
[b]Kiel Institute for the World Economy (david.pipke@ifw-kiel.de)

Abstract

Redistribution differs widely across countries, but our understanding of why this is the case is limited. In democracies, the extent of redistribution should ultimately reflect citizens' preferences. We measure preferences for redistribution in six developed countries through internationally standardized questions in which respondents face realistic budgetary constraints on their choice. We also measure a broad array of demographic, attitudinal, and ideological characteristics and examine their correlations with the preferred redistribution pattern. As expected, individual income is associated with lower demand for redistribution, but this relationship loses significance once controlling for other factors. Beliefs on social mobility have, in the aggregate, the most considerable effect in reducing demand for redistribution, the effect being most extensive in the U.S. but insignificant in Italy and Slovenia. Trust in government negatively affects demand for redistribution across all countries. In line with other studies, we interpret this result as evidence that people who believe that the political elite is corrupt demand more redistribution. Perceived financial security for the next year, a proxy for the Prospect of Upward Mobility (POUM) hypothesis, is also a significant correlate of preferences for redistribution. The effect is most considerable in Japan but minor in the UK and Slovenia. Finally, discrimination against racial minorities is associated with lower demand for redistribution, but the effect is only significant in the U.S. and Germany. Overall, the main theories proposed to account for preferences for redistribution are confirmed to be valid but with significant variation across countries.

JEL Codes: D63, D72, H23, H24, J15

Keywords: Preferences for Redistribution, Taxes, Trust in government, Immigration

1.1 Introduction

Redistribution is one of the defining features of modern welfare states. Developed countries such as the United States and European democracies redistribute large proportions of their GDP via taxes, transfers, and public goods provision. For example, public social spending - an approximation[1] of the extent of redistribution amounted to 18.7% of GDP in the U.S. compared to 28.2% in Italy and 25.9% in Germany in 2019, according to the OECD (2020). At the same time, these countries show notable differences in the extent of their redistribution policies (Alesina and Glaeser, 2004; Alesina and Giuliano, 2011) which is reflected in how strongly market income inequality is decreased due to transfers and taxes[2]. E.g., the United States reveals a reduction of market income inequality by roughly 17%, whereas the OECD average is a substantially larger one-quarter reduction (Causa and Hermansen, 2019). In democracies, governments' decisions should considerably reflect their citizens' demand for redistribution through a process of electoral competition and voting (Persson and Tabellini, 2000).

Despite its relevance (Bartels, 2009), investigating the underlying determinants of preferences for redistribution from a cross-country perspective is lacking. Most of the existing evidence comes from surveys, where the measurement of preferences for redistribution is confounded with (often wrong) beliefs on the specific level of inequality in the respondent's country. Furthermore, purely categorical survey items capturing respondents' willingness to redistribute incomes likely mix preferences over the absolute size of the government with the progressiveness of the tax system as they do not face any budgetary constraint. This paper addresses some of these shortcomings using individual-level data from a six-country online survey of representative samples from the population in the U.S., Germany, the UK, Italy, Slovenia, and Japan, part of the Trustlab project (Murtin et al., 2018), contributing to the growing literature using international online surveys of representative samples of the populations to explore individual preferences and attitudes (Kuziemko et al., 2015; Alesina et al., 2018; Stantcheva, 2021). We examine redistributive preferences measured by preferred tax rates within internationally standardized questions, in which respondents face realistic budgetary constraints on their choice (Alesina et al., 2018, 2022). We examine the correlations between various determinants of individual demand for redistribution and the preferred progressivity of the underlying tax scheme and compare their relevance in a set of countries.

[1]Neither are all branches of public social spending equally redistributive, nor does social spending cover the whole extent of redistribution (e.g., the tax system's progressivity).

[2]Causa and Hermansen (2019) calculate the relative reduction in market income inequality in terms of the Gini index, comparing the Gini index before and after income taxes, social security contributions, and cash transfers (relative to the Gini index before transfers and taxes).

In the standard economic model of public choice, economic agents favor redistributive policies if they expect to be net beneficiaries[3] and oppose them otherwise (Meltzer and Richard, 1981). A fundamental implication of this model is that we should observe increased demand for progressive taxation in countries experiencing higher inequalities. However, this prediction is clearly at odds with reality (Alesina and Glaeser, 2004; Shelton, 2007; Kenworthy and McCall, 2008; Dallinger, 2010). The subsequent literature has tried to incorporate a broader view of individuals' self-interest or to assume that individual preferences are affected by country-specific cultural or ideological traits. For example, it has been shown that the median voter may demand less redistribution when taking into account prospects of upward mobility[4] (the so-called "POUM hypothesis") (Bénabou and Ok, 2001). Alternatively, the median voter may demand more redistribution when considering the uncertainty of income against which redistribution may serve as an insurance device (Varian, 1980; Sinn, 1995).

Recent accounts of preferences for redistribution incorporating cultural or ideological determinants have focused on two ideas. The first idea is that beliefs about the fairness of the economic system and the deservedness of the recipients of the welfare state benefits matter for preferences for redistribution (Alesina and Giuliano, 2011). The role of beliefs has been studied extensively in theoretical models that incorporate multiple self-sustaining equilibria that evolve in the interaction of redistributive politics and beliefs (Piketty, 1995; Alesina and Angeletos, 2005b; Alesina and La Ferrara, 2005b; Bénabou and Tirole, 2006a). Authors draw a line between "European" and "American" equilibria (Alesina and Angeletos, 2005b; Alesina and Giuliano, 2011; Alesina et al., 2018), that may originate in personal or dynastical experiences (Piketty, 1995),

[3]Most empirical studies on preferences (or demand) for redistribution control for current individual income in absolute (Alesina et al., 2001; Luttmer, 2001; Fong, 2001; Alesina and La Ferrara, 2005b; Alesina and Giuliano, 2011) or relative terms (Corneo, 2001; Corneo and Grüner, 2002; Isaksson and Lindskog, 2009; Guillaud, 2013; Karadja et al., 2017; Rueda, 2018). Other studies account for material self-interest by including proxy variables for the socioeconomic status (Jaime-Castillo and Sáez-Lozano, 2014; Keely and Tan, 2008). Typically, studies find that rich persons have a lower demand for redistribution than the poor.

[4]Alesina and La Ferrara (2005b) show for the U.S. that support for redistribution is indeed negatively affected by future income prospects using panel data to construct objective transition probabilities to account for prospects of upward mobility. Ravallion and Lokshin (2000) find lower support for governmental redistribution among people who expect their welfare to fall in the next year. For European countries, Cojocaru (2014) finds that when risk aversion is low, expecting to earn more than average in the future for people currently earning less than average is related to lower support for redistribution compared to people who expect to stay below the average. Checchi and Filippin (2004) offer experimental evidence for the POUM hypothesis by evaluating subjects' demand for redistribution with respect to different income transition matrices. While the original POUM hypothesis is mainly about intragenerational mobility, Gavira et al. (2007) find lower support for redistribution among those who have experienced intergenerational mobility relative to their parents, which is also confirmed by Alesina and Giuliano (2011) using education level differences as proxy variables.

ideology serving as a motivation device (Bénabou and Tirole, 2006a) or (historical) indoctrination (Alesina and Glaeser, 2004; Alesina and Fuchs-Schündeln, 2007).

Beliefs that the economic system is fair and grants opportunities to everyone to 'grow from rags to riches' and fulfill the "American Dream" of increased inter-generational prosperity characterize the 'American' equilibrium. The meritocratic view of self-determined and well-deserved economic success due to high effort or ability supports this equilibrium (Alesina and Giuliano, 2011). The poor are viewed as being primarily responsible for their situation, e.g., due to being lazy or not sharing the same work ethic as the majority. Conversely, the "European" equilibrium hinges upon beliefs that economic success is predetermined by factors outside individual control (Alesina and Glaeser, 2004). This view is consistent with the thought that high incomes are not fully deserved, but are rather the product of luck, birth, connections, or illicit behavior (Alesina and Angeletos, 2005a).

Many studies confirm that beliefs of the "American" type are leading to lower demand for redistribution whereas the "European" type demands more redistribution. In this regard, the perceived fairness of the income-generating process, and in particular the extent to which it offers opportunities for upward mobility as well as beliefs about the reasons to be either rich or poor matter. A study of particular relevance is Alesina et al. (2018) who find Americans (Europeans) being over-optimistic (over-pessimistic) in the sense of estimating actual chances for upward mobility and showing lower (stronger) demand for redistribution. Providing respondents with pessimistic information about intergenerational mobility has an enhancing effect on the support for redistribution in case of left-wing respondents, but no effect on right-wing respondents, probably due to prevailing distrust in government in case of the latter (Alesina et al., 2018). Consistently, Kuziemko et al. (2015) show that people who underestimate the prevalence of inequality do not necessarily react with higher redistribution demand after being informed about its actual extent, claiming this is possibly due to distrust in the government and doubting the government will address the problem. Our paper is also closely related to studies using survey data (Fong, 2001; Corneo, 2001; Corneo and Grüner, 2002; Alesina and Giuliano, 2011) which find that preferences for redistribution are strongly negatively correlated with the perceived level of opportunities for social mobility and the belief that the poor are responsible for their economic situation.

An alternative account of cross-country differences in redistribution relies on racial and ethnic hostility between groups (Alesina and Glaeser, 2004). The key idea is that people from the racial or ethnic majority may decide to "shrink" the size of the welfare state if they realize that the beneficiaries of redistribution are mainly adversary racial or ethnic groups. Accordingly, the larger racial and ethnic fractionalization in the U.S. compared to Europe plays a significant factor in accounting for differences in demand for redistribution (Alesina and Glaeser, 2004; Lee and Roemer, 2006). As ethnic fractionalization increases in Europe due to increased migration, this account implies that the European equilibrium could eventually converge with the American one. This account also receives empirical support. Public spending is inversely related to ethnic fragmentation (Alesina et al., 1999). Concordantly, U.S. cities‘ redistributive spending declined during the diversity-enhancing immigration waves in the early 20th century (Tabellini, 2020). Recent immigration flows have crucially strengthened diversity in Europe and have possibly already affected preferences for redistribution (Dahlberg et al., 2012; Alesina et al., 2021b,c).

Redistribution is regularly found to be traveling less across racial groups (Alesina and Giuliano, 2011), which may be due to several channels. Probably the most studied channel is group loyalty effects (Tajfel, 1974; Luttmer, 2001). Evidence from the U.S. suggests that racial differences affect white people's generosity depending on whether they suspect that Black people are overrepresented among transfer recipients (Luttmer, 2001; Fong and Luttmer, 2011). Economic considerations about immigrants may matter as well. Immigration may be perceived as a threat that may overburden the welfare state resulting in weaker preferences for redistribution. Alternatively, natives may fear increased labor market competition leading to stronger demand for redistribution, ceteris paribus (Finseraas, 2008; Senik et al., 2009; Burgoon et al., 2012; Alesina et al., 2021c, 2022). Alesina et al. (2022) find that respondents, on average, hold exaggeratedly negative views about immigrants' reliance on the welfare state. Providing respondents with an informational treatment about a hard-working immigrant increases support for redistributive politics (Alesina et al., 2022).

We begin our analysis by comparing the stated preferred tax rates across the six countries. We find that U.S. respondents prefer, on average, the least progressive tax schemes. Somewhat unexpectedly, Italian respondents are very close to those from the U.S. in their preferred tax rates. Tax preferences of Japanese respondents lie between those of U.S.-American and Italian respondents on the one side and those of the more "pro-redistribution" respondents from Germany, Slovenia, and the UK on the other side. Exploring the correlational patterns of individual determinants for preferences for redistribution with the dependent variable, the top 40% of the income distribution prefer significantly less progressive tax schemes (measured by the difference between preferred tax rates on the top 1% and bottom 50%) than respondents from the bottom 40% ($p < 0.001$, t-test). This difference, however, loses significance when controlling for future income expectations and social mobility beliefs. Respondents who are optimistic about their future economic situation prefer slightly less progressive schemes. These results align with the POUM hypothesis (Bénabou and Ok, 2001) and suggest that expectations about the future count more than current income in shaping preferences for redistribution. Furthermore, we find that perceived social mobility opportunities offered by society strongly negatively correlate with preferred progressivity. The inspection of country-wise regressions indicates a remarkable heterogeneity in this association that is strongly significant in the U.S., Germany, and the UK but appears to be almost irrelevant in Italy and Slovenia. The relationship between attitudes towards immigrants and progressivity preferences reveals an even more pronounced heterogeneity. Both are statistically significantly positively related in the U.S. and to a lesser extent in Germany, while the relationship is statistically insignificant in the other four countries. Our results indicate that what matters most in attitudes towards immigrants is the perceived effect immigrants may have on the culture in the host country. In contrast, their perceived level of integration appears to be relatively irrelevant. We also find that trust in government is negatively correlated with preferences for redistribution in all countries from our sample. This finding is in contrast to some studies that find a relationship of opposite sign, which is consistent with the view that people need to trust the government's competencies to favor redistribution (Yamamura, 2014; Stantcheva, 2021). However, other studies (e.g., Di Tella et al. (2021) and Barnes (2015)) also find a negative correlation between trust in government and demand for redistribution. In particular, Di Tella et al. (2021) find that trust in government goes hand-in-hand with trust in financial elites. This result suggests that a channel explaining our results is the following. Suppose people believe that the society elites (including politicians and business leaders) are corrupt. In that case, they will find a stronger case for redistribution, in analogy to the argument that preferences for redistribution go hand-in-hand with beliefs over the deservedness of the rich.

The rest of the paper is structured as follows. Section 1.2 describes the OECD Trustlab as our data source, and we briefly summarize the structure of the survey, emphasizing those parts used in this study. In Section 1.3, we show the results of our analysis. We start with a description of relevant attitudes and views across the six countries in our sample before presenting our findings on the preferred tax rates. Section 1.4 concludes.

1.2 Survey, Data, and Methods

1.2.1 The Trustlab

Our data come from the OECD's Trustlab project. The Trustlab is an international initiative that has been run in eight countries (France, Germany, Italy, Japan, Korea, Slovenia, United Kingdom, and the United States) so far. The initiative combines various techniques (e.g., psychometric measures, economic experiments) from different disciplines such as behavioral science and experimental economics with a large-scale survey on respondents' characteristics, attitudinal variables, measures of trust, and preferences for redistribution. The initiative studies the determinants of trust and social preferences from a cross-country perspective. With the data containing more than 1,000 respondents per country chosen to be nationally representative of the working-age population (defined as those aged 15 to 64) in terms of age, gender, and income, Trustlab overcomes one of the main criticisms of the experimental approach, i.e., the focus on university students samples. Data collection took place online between November 2016 and February 2020.[5] Since the question we use to analyze redistributive preferences only exists in the survey run in Germany, Italy, Japan, Slovenia, the UK, and the U.S., our analysis focuses on these six countries.

1.2.2 The Survey

The survey is the third and final of Trustlab's modules. The Trustlab survey covers a broad, mixed range of questions targeting measuring trust and its potential determinants.[6] Respondents arrive at the survey screen after completing a series of economic experiments and implicit association tests in the first and second modules of the online platform, respectively (see Murtin et al. (2018) for a detailed explanation of the first two modules). The questionnaire in English[7] can be found in the Appendix. We briefly summarize the structure of the survey, emphasizing those parts that are important for the understanding of our results. The structure is essentially the same in all participating countries.

Social norms and interpersonal trust. — After checking which device the respondent uses to browse the platform, the survey starts with a diverse range of questions on social norms and trust in other people.

Trust in government. — The second block of questions deals with trust in public and private institutions, followed by a battery of questions concerning satisfaction with the quality of the government. In its most general form, trust in the government is elicited by simply asking respondents how much they trust the government. Answers could range between 0 "I do not trust them at all" and 10 "I completely trust them." Additionally, respondents were asked to state their level of agreement with certain statements focussing on specific dimensions of the perceived quality of public

[5]Data collection of the first wave started in November 2016 in France, followed by Korea in January 2017. The second wave includes data from Slovenia (April 2017), the U.S. (June 2017), Germany (August 2017), and Italy (November 2017). Data from the UK is from March 2018, and Japanese data is from February 2020.

[6]Most of the questions are measured on a 0-to-10 Likert scale.

[7]The questionnaire has been translated for each participating country to ensure a thorough understanding and compatibility with the local culture.

institutions. Evaluated dimensions cover the reliability ("Public institutions deliver public services in the best possible way."), responsiveness ("Public institutions pursue long term objectives."), integrity ("People working in public institutions are ethical and not corrupt."), transparency or openness ("Public institutions are transparent.") and non-discrimination ("Public institutions treat all citizens fairly regardless of their gender, race, age or economic condition.") of the government.[8] Answers could range between 0 "I do not agree at all" and 10 "I completely agree."

Preferred tax rates. — The survey block about trust in the government and an evaluation of the government's competencies precedes a question on preferred tax rates. We base the dependent variables in the regressions later on on the preferred tax rates. We asked respondents to state their preferred fair split[9] of the tax burden in their country on specific groups of the income distribution to sustain current public spending. The four groups are the top 1%, the next 9%, the next 40%, and the bottom 50% of the income distribution. Respondents chose tax rates by moving sliders on the survey screen. A fifth slider below the other sliders moves simultaneously and turns green when the respondent's choice raises enough revenue. To ensure economically meaningful answers, we kept the size of the government fixed. I.e., tax rates are restricted to generate a budget for the government between 97% and 103% of the revenue implied by a proportional (flat) tax rate of 25%. Revenues are calculated based on OECD-average income distribution from the OECD Income Distribution Database. Thus, tax rates measure preferred progressivity instead of being confounded by concerns about the absolute size of the government. The chosen tax rates constitute our main variables of interest measuring preferences for redistribution, similar to Alesina, Stantcheva, and Teso (2018) and Alesina, Miano, and Stantcheva (2022). In contrast to commonly used survey measures, the chosen tax rates allow for a more quantitative examination of redistributive preferences. High (low) tax rates on the rich (poor) are interpreted as a sign of support for redistributive policies.

Social mobility beliefs. — In the Trustlab survey, beliefs about opportunities for social mobility are elicited by the following question: "Some people say there is not much opportunity to get ahead today for the average person. Others say anyone who works hard can climb up the ladder. Which one comes closer to the way you feel about

[8]Besides those direct measures of government's reliability, responsiveness, integrity, openness, and non-discrimination, these dimensions are complemented by asking how the government would react in practical situations related to these issues.

[9]The exact wording in the survey is: "The government currently raises a certain amount of revenues through tax in order to sustain the current level of public spending. In your view, what would be the fair split of tax burden to sustain public spending? Please use the sliders below to tell us how much you think each of the following groups should pay as a percentage of their available resources. Each slider represents a segment of the population with a different income. For example, the top 1% represents a small group of rich people, whereas the bottom 50% is the half of the population that earns the least."

this?”. Answers range between 0 “There is not much opportunity” and 10 “There is plenty of opportunities.”

Economic security. — We asked respondents to state expectations about their household’s financial situation in the next year by the following question. “When it comes to the financial situation of your household, what are your expectations for the 12 months to come? Will the next 12 months be better, worse, or the same?” where 0 “Worse,” 5 “The same” 10 “Better.” Evaluating one’s economic prospects may serve as a proxy for the POUM hypothesis (Bénabou and Ok, 2001; Alesina and La Ferrara, 2005b), as for example Ravallion and Lokshin (2000) use a similar question. Additionally, respondents were asked to evaluate the likelihoods (i) of keeping their current job as well as (ii) of finding a new job within six months in case they lose their current job, both on a scale between 0 “Very unlikely” and 10 “Very likely.” The remainder of the survey covers several other issues.[10] Therefore, we only introduce those items we use in the following analysis.

Attitudes towards immigrants. — The survey includes several questions asking about views on immigrants. The first asks to assess the integration of immigrants on a scale between 0 “Immigrants are not integrated into our society,” and 10 “Immigrants are well integrated into our society.” The second question is about the effect that immigrants may have on the culture in the host country. Respondents may choose between 0 “Our culture is undermined by immigrants” and 10 “Our culture is enriched by immigrants,” expressing the strength to which they believe in multiculturism. Attitudes toward immigrants may be crucial for redistributive preferences for several reasons, as discussed above. The first question is more related to the economic considerations about immigration, i.e., whether people think of immigrants as needier and relatively more dependent on the welfare state. In contrast, we interpret the second as a measure of general preferences for diversity. A question on racial prejudices complements these two measures. Participants are asked to state their opinion on whether they believe that immigrants are, on average, less economically successful because of discrimination and reasons out of their control (“0” on the scale measuring prejudices) or due to lower ability, motivation, and effort (“10” on the scale). We also consider the stated level of trust in immigrants.

Personal information. — The remainder of the survey collects additional personal information about the respondents. For example, we asked respondents to place their political orientation on a scale between 0 “Left” and 10 “Right.” Respondents were also asked for demographics such as gender and age, the place of residence, religious de-

[10] Those questions cover attitudes to international trade, which technologies are used to obtain information, the perceived share of immigrants in the respondent’s neighborhood, and the extent to which the respondent feels connected to other people.

nomination, information about the respondent's and his or her parents' nationality and migration history, and the educational attainment of the respondent and his parents. Furthermore, the Trustlab gathers information about the labor force status and the sector the respondent works in, and his or her income. See Appendix Table 1.16 for further information about the survey items.

1.2.3 Summary Statistics

Table 1.1 shows summary statistics of each country's sample characteristics side-by-side to their respective population means taken from nationally representative sources. Sample means are relatively close to their population values, especially along the targeted dimensions of age and gender. The sample's income distribution reflects the actual income distribution in the U.S., Germany, and Italy closely. In the UK and Slovenia, people from the lower two quintiles have been oversampled, whereas the Japanese sample contains more people from the top two quintiles than would be representative. The other non-targeted dimensions, such as education and employment status, are not as close to representative of the population as the targeted dimension. However, they are not too dissimilar to the actual distribution. The share of females in the Italian sample is significantly higher than in the other sampled countries because it has been supplemented by an extra sampling of 442 women of childbearing age to analyze fertility intentions (Aassve et al., 2018). Summary statistics for the original Italian sample are denoted in parentheses resembling the population values more closely.

1.3 Results

We first discuss descriptive findings from the survey before we analyze the correlational pattern of attitudes, views, expectations, and redistributive preferences in a regression analysis.

1.3.1 Views, Attitudes and Expectations

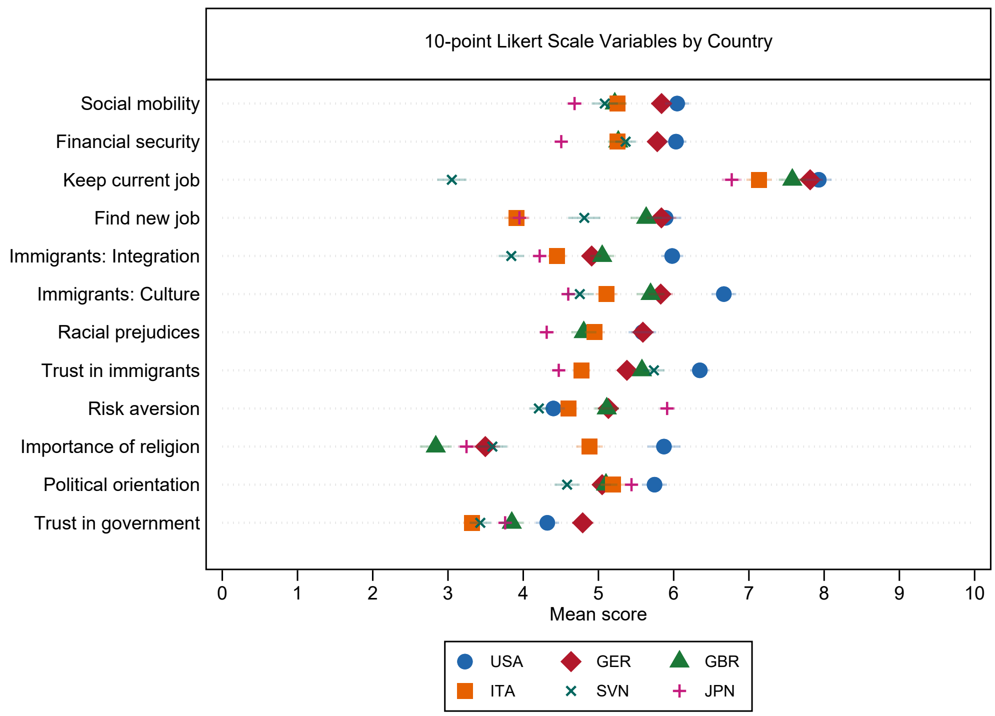
Figure 1.1 shows the means and 95 percent confidence intervals from the variables of interest on an 11-point Likert scale (introduced in Section 1.2.2) broken up by country of survey. In Figure 1.2, we depict means of the same variables by personal characteristics. The first row of both graphics shows the mean answer to the evaluation of opportunities for social mobility offered by society. The next three rows deal with expectations about the financial situation of the household in the next year, the likelihood of keeping the current occupation, and the likelihood of finding a new one in case one loses the current,

Table 1.1: Sample Characteristics

	USA		GER		GBR		ITA		SVN		JPN	
	S	P	S	P	S	P	S	P	S	P	S	P
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
Female	0.51	0.51	0.50	0.51	0.54	0.51	0.66 (0.51)	0.52	0.49	0.50	0.49	0.52
Age: 15-24	0.10	0.14	0.11	0.15	0.13	0.18	0.15 (0.12)	0.15	0.11	0.14	0.11	0.16
Age: 25-54	0.57	0.64	0.67	0.60	0.67	0.62	0.71 (0.69)	0.63	0.68	0.64	0.66	0.63
Age: 55-64	0.33	0.22	0.21	0.25	0.19	0.20	0.14 (0.19)	0.22	0.21	0.22	0.21	0.21
Low income	0.50	0.40	0.36	0.40	0.59	0.40	0.47 (0.47)	0.40	0.67	0.40	0.24	0.40
Medium income	0.20	0.20	0.23	0.20	0.19	0.20	0.20 (0.19)	0.20	0.15	0.20	0.15	0.20
High income	0.31	0.40	0.41	0.40	0.22	0.40	0.32 (0.34)	0.40	0.17	0.40	0.62	0.40
Low educ.	0.20	0.40	0.29	0.26	0.50	0.21	0.48 (0.51)	0.39	0.49	0.56	0.41	0.15
Medium educ.	0.38	0.28	0.37	0.56	0.15	0.34	0.18 (0.17)	0.42	0.16	0.21	0.11	0.38
High educ.	0.42	0.32	0.34	0.18	0.34	0.46	0.34 (0.32)	0.19	0.35	0.24	0.48	0.28
Employed	0.55	0.57	0.62	0.65	0.56	0.64	0.55 (0.56)	0.65	0.63	0.48	0.53	0.52
Self-employed	0.08	0.04	0.07	0.07	0.08	0.12	0.10 (0.11)	0.07	0.07	0.06	0.10	0.05
Unemployed	0.12	0.02	0.05	0.02	0.10	0.03	0.13 (0.11)	0.02	0.11	0.04	0.04	0.03
Out of labor force	0.25	0.37	0.26	0.26	0.26	0.21	0.22 (0.22)	0.26	0.20	0.42	0.32	0.40
Obs.	1,090		1,108		1,053		1,458 (1,016)		1,011		2,504	

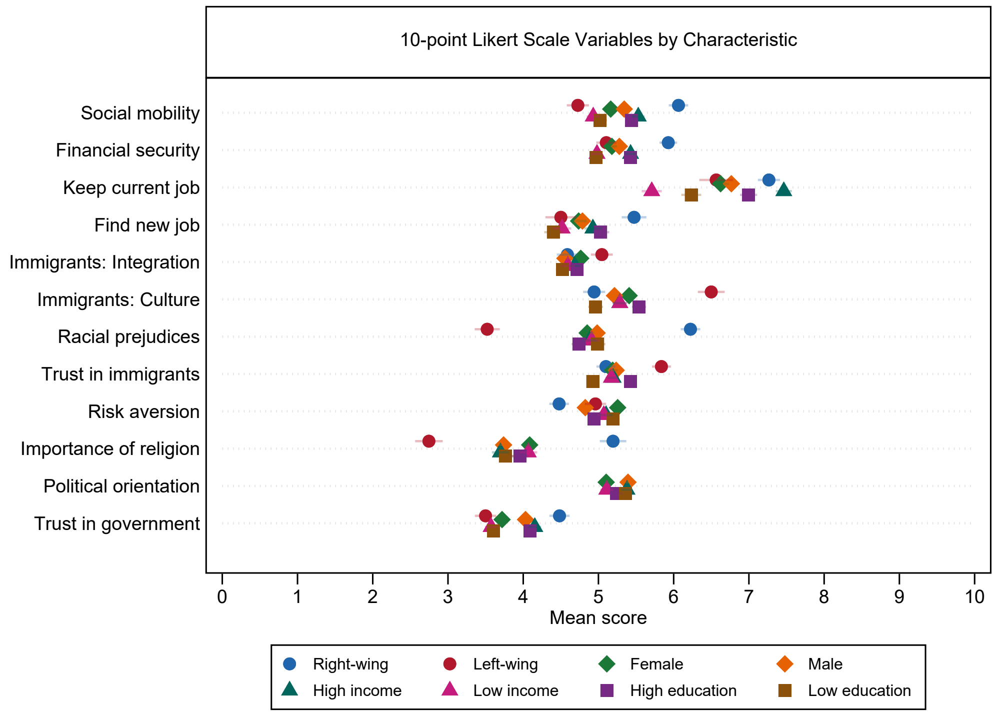
Notes: The table displays means of sample characteristics and their respective values in the working age (15-65 years old) population. Low, medium and high income correspond to the first two, the third and the last two quintiles of the income distribution, respectively. Low education corresponds to an High School degree or less. Medium education equals vocational education or community college degree. High education corresponds to a University degree. Population statistics taken from nationally representative sources shortly listed here: (i) Age and gender statistics taken from the CIA World Fact Book (for all countries). (ii) Educational attainment statistics from the United States Census Bureau (USA), Statistisches Bundesamt (GER, 2018), OECD.Stat (GBR, 2018 and ITA, 2017), Statistical Office of the Republic of Slovenia (SVN, 2017), National Institute of Population and Social Security Research (JPN, 2017). (iii) Employment statistics from U.S. Bureau of Labor Statistics (USA, 2018), Statistisches Bundesamt (GER, 2018), Labor Force Survey (GBR, 2018), Istituto Nazionale di Statistica (ITA, 2016), Statistical Office of the Republic of Slovenia (SVN, 2017), National Institute of Population and Social Security Research (JPN, 2017). See the Appendix for a more detailed description of data sources.

Figure 1.1: Attitudinal Variables by Country



Notes: This figure shows mean answer score and the respective 95 percent confidence interval in the indicated country sample below the graph.

Figure 1.2: Attitudinal Variables by Characteristic



Notes: This figure shows the mean answer scores and their 95 percent confidence intervals for the subgroups defined by the indicated characteristics below the figure. Right-wing (left-wing) respondents stated a political orientation of 7 or above (3 or below) on a scale from 0 (left) to 10 (right). Low education corresponds to a High School degree or less. High education corresponds to a University degree. Low and high income corresponds to the first two and the last two quintiles of the income distribution, respectively.

respectively. In the fifth and sixth row, we show the mean answer score related to attitudes towards immigrants, i.e. an assessment of their integration and their effect on the host country's culture. We also depict the survey measures of racial prejudices and trust in immigrants. Finally, the remaining rows show means of stated risk aversion, the importance of religion, political orientation (towards the right), and trust in the government.

Social mobility. — Beliefs about social mobility opportunities are highest in the U.S., closely followed by Germany. Both countries show similar mean scores, which seems to contrast with the so-called "American exceptionalism" hypothesis (Lipset, 1996; Alesina et al., 2001). In contrast, respondents from Japan are most skeptical about opportunities "to climb the social ladder" by working hard in their country. Respondents from Italy, the UK, and Slovenia are between both extremes. Among personal characteristics, the most noticeable is the difference between political extremes. Right-wing respondents have a significantly more positive assessment of social mobility opportunities than left-wing respondents.

Expectations about economic security. — We explore the answers to three questions inquiring about optimism over the respondents' economic situation. The first question is about the financial situation in the next year. In contrast, the second is about the likelihood of keeping the current job if one is employed. The third is about the likelihood of finding a new job if the respondent loses their current occupation. U.S. and German respondents are most optimistic about their economic situation soon, but respondents from the UK are only marginally behind. The picture in the other countries is slightly more mixed. Japanese are the least optimistic concerning their financial situation in the next year, whereas Slovenians are by far the most pessimistic concerning the possibility of finding a new occupation. Non-surprisingly, when looking at personal characteristics, these expectations are more positive among highly educated and high-income people. Right-wing respondents also report, on average, higher confidence concerning their future economic situation than those respondents leaning to the political left.

Attitudes towards immigrants. — The first two questions asking about views on immigrants are related to their perceived level of integration and their effect on the culture in the host country. Additionally, we examine racial prejudices (not included in the Slovenian survey) and stated trust in immigrants. Generally, we observe a positive correlation between a country's ethnic and racial diversity and the tendency to hold positive views about immigrants. Respondents from the U.S. hold the most optimistic views about immigrants concerning their integration and their effect on the culture. They also report the highest level of trust in immigrants. At the other end of the spectrum, respondents from ethnically homogenous Japan and Slovenia have the most negative news about immigrants. Attitudes of Germans and respondents from the UK are somewhat more favorable toward immigrants than those from Italy, who state average scores slightly to the left of the center. Again, when looking at personal characteristics, political orientation is defining. Right-wing respondents have significantly less positive attitudes towards immigrants than left-wing respondents. The same pattern holds, albeit less markedly, for respondents with low education vis-a-vis the highly educated, but not in the dimension of low vs. high incomes.

Trust in government and political orientation. — Mean scores of trust in the government lie left to the center in all countries. Germans report the highest trust levels, followed by respondents from the U.S. Italians and Slovenians trust their governments the least. On average, Germans, Italians, and respondents from the UK state a political orientation that is close to the center. Respondents from the U.S. lean a bit to the right, whereas Slovenias state a more left-wing orientation. There is only a slight variation concerning personal characteristics in political orientation and trust in the government, with the marginal note that right-wing respondents show, on average, more trust in the government than respondents from the political left.

1.3.2 Preferred Tax Schemes

Figure 1.3 depicts the mean values of preferred tax rates and their 95 percent confidence intervals in each country[11]. The tax rates reveal a clear pattern of similarity in preferred tax rates between U.S. Americans and Italy on one side of the spectrum and respondents from Germany, Slovenia, and the UK on the other side.[12] Tax preferences of Japanese respondents lie somewhere in between the other groups.

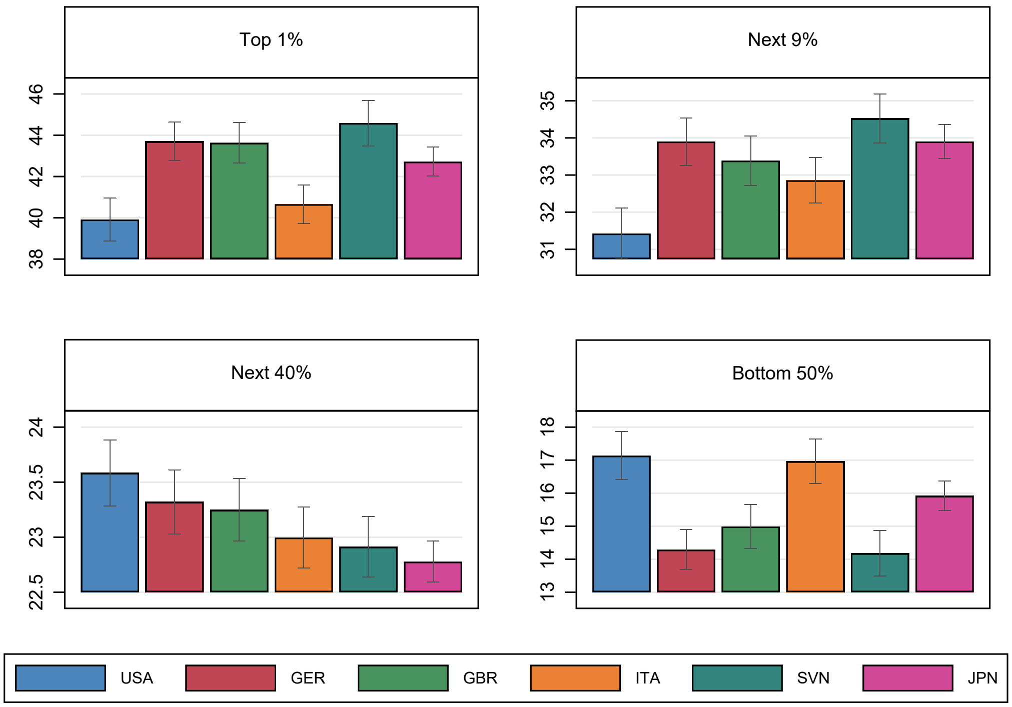
U.S. Americans and Italians, on average, both chose comparatively low tax rates on the "richest" percent and high tax rates on the poor half of the income distribution, which we interpret as relatively regressive redistribution preferences. Two-sided t-tests against the null of equal means between U.S. and Italy cannot be rejected ($p = 0.29$ and $p = 0.74$ for tax rate on the top 1 percent and bottom 50 percent, respectively). On the contrary, respondents from Germany, Slovenia, and the UK reveal more progressive preferences. They chose a relatively high burden on the top 1 percent and a relatively minor burden on the bottom 50 percent. A one-way ANOVA can neither reject the null of equal means for the tax rate on the top 1 percent nor the tax rate on the bottom 50 percent among these three countries ($p = 0.34$ and $p = 0.18$, respectively). In contrast, pairwise t-tests against the null of equal means of the top 1 percent and bottom 50

[11]Detailed summary statistics can be found in the Appendix.

[12]An analysis in the Appendix confirms this result. When we classify tax schemes into progressive and regressive schemes, the share of respondents from the U.S. and Italy choosing tax schemes classified as progressive is significantly lower than in the other countries. In contrast, the share of respondents choosing regressive tax schemes is significantly larger.

percent tax rate between the U.S. or Italian samples versus either Germany, Slovenia or UK samples are all highly significant ($p < 0.001$, see Appendix). This pattern is confirmed by the mean difference between the chosen top 1 percent and the bottom 50 percent tax rates shown in Figure 1.4.

Figure 1.3: Preferred Tax Rates by Country



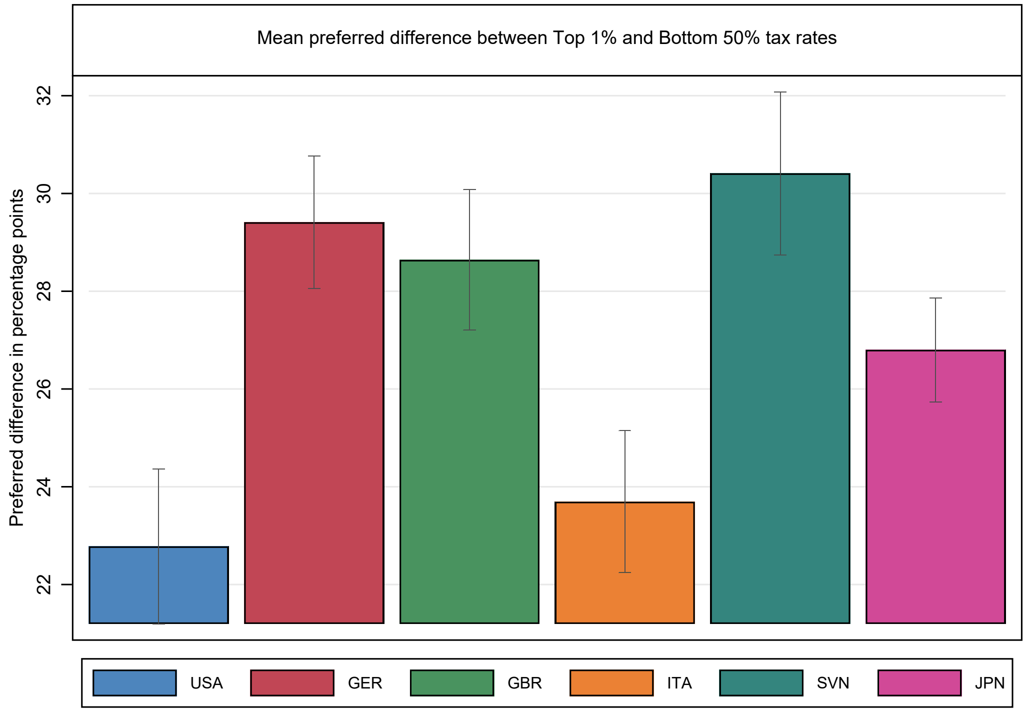
Notes: This figure shows the average chosen tax rates in percentage points in each country. Whiskers span the 95 percent confidence interval.

1.3.3 Regression results

In the following regression analysis, we examine the association of redistributive preferences with the explanatory variables. The difference in preferred tax rates on the top 1 percent and the bottom 50 percent of the income distribution is the dependent variable that measures the preferred progressivity of the underlying tax scheme. We estimated several specifications to test for the explanatory power and significance of different theoretical drivers of preferences for redistribution. All regressions contain country dummies, with the U.S. as the reference category. Regression tables showing coefficients for all control variables can be found in the Appendix.

Table 1.2 reports our key regression results. First, it is noteworthy that the country dummies for Germany, the UK, and Slovenia remain positive, highly significant, and

Figure 1.4: Preferred Progressivity: Difference between Top and Bottom Tax Rates



Notes: This figure shows the average chosen difference between tax rates on the top 1 percent and on the bottom 50 percent in each country. Whiskers span the 95 percent confidence interval.

roughly the same size relative to the U.S. throughout all specifications as in the unconditional mean comparisons (see Figure 1.4). This indicates the prevalence of relatively stronger preferences for redistribution in these countries compared to the U.S. even after controlling for the set of variables in the model. In opposition to this, the dummy for Italy is non-significant in all regressions confirming the relative closeness to the U.S.. Only the country dummy for Japan loses significance when adding more explanatory variables, which indicates that these additional variables explain part of the difference vis-à-vis the U.S.

Income is not the defining determinant for redistributive preferences in the pooled sample. Neither the dummy for the low-income category nor the dummy for the high-income category reaches significance in any specification, with medium incomes serving as the reference category.[13] However, participants in the high-income category prefer lower progressivity than participants from the low-income category ($p = 0.027$,

[13]Unreported regressions omitting the measure for perceived financial security for the next twelve months show that the dummy for high-income people almost reaches significance at the 5 percent level. Since financial security serving as a proxy for the POUM hypothesis is higher for people with higher incomes, this may indicate some multicollinearity.

Wald test, specification in column (1) of Table 1.2).[14] On the other hand, individuals' expectations about their future income are significantly negatively associated with the preferred difference between the top and bottom tax rates, supporting the POUM hypothesis. Furthermore, higher trust in government is negatively associated with preferred progressivity of the underlying tax schemes - or, to frame it differently, stronger distrust in the government correlates with stronger preferences for redistribution. As expected, beliefs about social mobility show a strong association with the dependent variables. People thinking of their society as meritocratic are more averse to redistribution. Furthermore, more positive attitudes towards immigrants are associated with slightly stronger preferences for redistribution in the pooled regression.[15]

Additional regressions in the Appendix (Table 1.14) show that the coefficient for risk aversion has the expected sign. Risk aversion is associated with a stronger preference for redistribution, significant at the 1 percent level. Consistent with the interpretation that religion may act as a substitute for social insurance (Corneo, 2004; Scheve and Stasavage, 2006; Guillaud, 2013), we find that people stating higher importance of religion are more averse to redistribution.

Cross-country comparison. — Table 1.3 shows regressions estimated for each country subsample separately. These regressions enable us to explore whether some factors are relevant for redistributive preferences in all countries or only in a subset. The complete regression tables can be found in the Appendix Table 1.10.

Beginning with income, we see that neither the dummies indicating being in the low-income group or the high-income group, respectively, have a coefficient significantly different from zero in any country. On the other hand, financial security shows a negative correlation with preferred progressivity, although being significant from a statistical point of view only in Japan and the U.S. Interestingly, beliefs about social mobility opportunities showing highly significant aggregate coefficients are practically irrelevant for the preferred progressivity in the Italian and Slovenian subsample. Opposed to this, trust in government shows a statistically significant negative coefficient in all six countries, with the pairwise Wald tests showing no significant differences across countries relative to the U.S. constituting the reference point. In the U.S. subsample, preferred progressivity shows a highly statistically significant positive association with attitudes towards immigrants.[16] In contrast, the coefficient remains largely non-significant in the other countries. Subsequent Wald tests indicate that the U.S. coefficient is statistically

[14]The corresponding p-values from testing the equality of coefficients of the high-income and the low-income category for columns 2-5 are 0.045, 0.107, 0.095, and 0.197, respectively.

[15]We use the average score of the perceived effect of immigrants on the culture in the host country and the perceived level of integration of immigrants to measure attitudes towards immigrants. Results are robust to use stated trust in immigrants or racial prejudices (with reversed sign), whereof the latter measure has many missing values and was not included in the Slovenian survey.

[16]Table 1.11 shows p-values from pairwise Wald tests for all country comparisons.

Table 1.2: Main Regression Results

	(1)	(2)	(3)	(4)	(5)	(6)
GER	7.025****	7.481****	6.975****	7.902****	7.618****	6.831****
	(1.07)	(1.07)	(1.07)	(1.10)	(1.09)	(1.08)
GBR	5.891****	5.593****	4.930****	5.886****	5.498****	4.407****
	(1.13)	(1.12)	(1.12)	(1.16)	(1.16)	(1.11)
ITA	0.255	-0.612	-0.867	-0.231	-0.188	-0.968
	(1.13)	(1.13)	(1.13)	(1.16)	(1.16)	(1.12)
SVN	7.428****	6.351****	5.854****	7.639****	7.399****	6.267****
	(1.21)	(1.20)	(1.21)	(1.28)	(1.28)	(1.24)
JPN	2.796***	2.708**	1.882*	2.517**	2.001*	1.358
	(1.06)	(1.06)	(1.07)	(1.13)	(1.13)	(1.05)
Low income	0.980	0.793	0.746	0.611	0.449	
	(0.84)	(0.85)	(0.85)	(0.88)	(0.87)	
High income	-0.711	-0.739	-0.493	-0.717	-0.576	
	(0.83)	(0.83)	(0.84)	(0.87)	(0.87)	
Financial security	-1.584****	-1.128****	-0.683****	-1.236****	-0.751****	-0.818****
	(0.15)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)
Trust in government		-1.190****	-0.915****	-1.224****	-0.954****	-0.972****
		(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
Social mobility			-1.127****		-1.186****	-1.177****
			(0.14)		(0.15)	(0.15)
Attitudes immigrants				0.351**	0.479***	0.446***
				(0.16)	(0.16)	(0.16)
Constant	29.47****	31.64****	34.66****	30.15****	32.30****	36.25****
	(1.93)	(1.96)	(2.01)	(2.26)	(2.28)	(1.55)
Obs.	7854	7762	7577	7125	7025	7026
R2	0.0346	0.0443	0.0532	0.0465	0.0553	0.0501
Adj. R2	0.0327	0.0422	0.0510	0.0441	0.0528	0.0489

Notes: The table shows OLS regressions. The dependent variable is the preferred difference between tax rates on the top 1 percent and the bottom 50 percent. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. GER, GBR, ITA, SVN, and JPN are country dummies for the respondent's residence with the United States serving as the reference category. Regressions (except column 6) contain controls for age, gender, migration background, occupation dummies, and education dummies. Low income (High income) is a dummy for household income from the first (last) two quintiles. Financial security is the answer to the question "When it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?" where 0 "Worse", 5 "The same" 10 "Better". Trust in government is the answer score to the general trust in government question where 0 "I don't trust them at all" and 10 "I completely trust them". Social mobility is the answer score to the question on about opportunities to climb the social ladder where 0 "There is not much opportunity" and 10 "There is plenty of opportunities". Attitudes (towards) immigrants is the average of both questions related to their perceived level of integration and their effect on the culture in the host country.

significantly larger than in the UK, Italy, and Japan, whereas Germany and Slovenia's differences from the U.S. are less pronounced.

Heterogeneity Analysis. — Table 1.4 shows the results of a heterogeneity analysis concerning four key respondent characteristics that were crucial in determining redistributive preferences (Fong, 2001; Alesina and Giuliano, 2011; Alesina et al., 2018). The dimensions of heterogeneity are the "right-wing" (defined as stating a seven or above on the 10-point political orientation scale), the "rich" (belonging to the two top quintiles of household income), and the "highly educated" (having a university degree) and females. The heterogeneity is explored by interacting the relevant characteristics with the explanatory variable of interest. The interaction term gives the difference in coefficient size relative to respondents not falling into the respective category. More detailed regression tables can be found in the Appendix.

The regressions indicate little heterogeneity across the examined categories concerning the association of social mobility, attitudes towards immigrants, and trust in the government with the dependent variable. An exception is attitudes towards immigrants among the political right-wing, which, unlike in the whole sample (see Table 1.2), negatively correlate with the tax scheme's preferred progressivity (β = -0.854, s.e. = 0.324). The number of observations is significantly lower in the model from the first column due to many respondents refusing to state their political orientation in the survey.

Post-regression accounting. — In Table 1.5, we provide a simple post-regression accounting akin to Corneo (2001). The purpose is to explore how much the explanatory variables can explain the observed cross-country differences in the preferred progressivity, measured as the difference between the tax rate on the top 1 percent and the bottom 50 percent. To this end, we use the coefficients from the pooled regression (fifth column in Table 1.2) and multiply them by the difference in means of the respective explanatory variable between the country from the first row relative to the U.S.-sample mean. As such, the cells in Table 1.5 show the quantity $\beta_k(x_k^i - x_k^{US})$ in which j is one of the other five countries.

The interpretation is as follows. The observed difference between the top and bottom tax rates between German respondents and those from the U.S. is 6.63 percentage points (see Appendix Table 1.6). The country dummy alone explains more than the full magnitude of this observed difference. Differences in means of social mobility beliefs account for roughly 1/20 of the difference, as those beliefs are slightly higher in the U.S. than in Germany. However, the sign of the respective coefficient is negative. Summing up the contribution of all individual-level predictors, the model evaluated at the sample average predicts a difference of about 6.47 percentage points in the preferred progressivity of both countries. While the explanatory power of our main predictors is the

Table 1.3: Country-wise Regressions

	(1) USA	(2) GER	(3) GBR	(4) ITA	(5) SVN	(6) JPN
Low income	-0.075	2.865	1.909	-1.336	-0.930	-0.725
	(1.99)	(2.01)	(2.15)	(2.00)	(2.34)	(2.27)
High income	-1.624	2.070	-0.496	-2.342	-3.278	0.700
	(2.05)	(1.94)	(2.39)	(2.04)	(2.86)	(1.86)
Financial security	-0.792*	-0.580	-0.031	-0.731	-0.324	-1.468****
	(0.45)	(0.41)	(0.40)	(0.46)	(0.48)	(0.35)
Trust in government	-1.160****	-0.681*	-0.882***	-1.059***	-0.983**	-0.743**
	(0.34)	(0.36)	(0.33)	(0.37)	(0.45)	(0.29)
Social mobility	-2.192****	-1.797****	-2.045****	-0.099	-0.451	-0.857***
	(0.38)	(0.35)	(0.36)	(0.40)	(0.42)	(0.31)
Attitudes immigrants	1.512****	0.696*	0.130	0.063	0.634	0.190
	(0.41)	(0.42)	(0.35)	(0.41)	(0.45)	(0.37)
Constant	30.02****	36.60****	36.96****	31.30****	35.27****	36.26****
	(5.32)	(4.49)	(5.17)	(4.92)	(5.70)	(4.54)
Obs.	972	1016	935	1362	875	1865
R2	0.1310	0.0717	0.0976	0.0215	0.0298	0.0491
Adj. R2	0.1180	0.0587	0.0839	0.0113	0.0141	0.0419
Tests against nullhypothesis of jointly equal coefficients						
Low income	p = 0.637					
High income	p = 0.509					
Financial security	p = 0.139					
Attitudes immigrants	p = 0.089					
Social mobility	p = 0.000					
Trust in government	p = 0.914					
Pairwise tests vs. USA		GER	GBR	ITA	SVN	JPN
Low income		p = 0.295	p = 0.495	p = 0.653	p = 0.779	p = 0.828
High income		p = 0.187	p = 0.718	p = 0.803	p = 0.635	p = 0.398
Financial security		p = 0.724	p = 0.201	p = 0.923	p = 0.472	p = 0.232
Attitudes immigrants		p = 0.159	p = 0.010	p = 0.012	p = 0.146	p = 0.016
Social mobility		p = 0.442	p = 0.778	p = 0.000	p = 0.002	p = 0.006
Trust in government		p = 0.329	p = 0.552	p = 0.839	p = 0.750	p = 0.348

Notes: The table shows OLS regressions. The dependent variable is the preferred difference between the top 1 percent and the bottom 50 percent tax rates. The regression model is the same as in column 5 of the main regression results table. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. Regressions contain controls for age, gender, migration background, occupation dummies, and education dummies. The complete set of control variables can be seen in the regressions the Appendix. Tests against equality of coefficients were calculated using seemingly unrelated estimations (STATA's suest command). Low income (High income) is a dummy for household income from the first (last) two quintiles. Financial security is the answer to the question "When it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?" where 0 "Worse", 5 "The same" 10 "Better". Trust in government is the answer score to the general trust in government question where 0 "I don't trust them at all" and 10 "I completely trust them". Social mobility is the answer score to the question on about opportunities to climb the social ladder where 0 "There is not much opportunity" and 10 "There is plenty of opportunities". Attitudes (towards) immigrants is the average of both questions related to their perceived level of integration and their effect on the culture in the host country.

Table 1.4: Heterogeneity with respect to Personal Characteristics

	(1) H = Right-wing	(2) H = High Inc.	(3) H = High Education	(4) H = Female
Social mobility	-1.113****	-1.034****	-1.099****	-1.445****
	(0.19)	(0.19)	(0.20)	(0.21)
Attitudes immigrants	0.830****	0.577***	0.461**	0.605***
	(0.21)	(0.21)	(0.22)	(0.22)
Trust in government	-0.779****	-1.038****	-0.792****	-0.734****
	(0.17)	(0.19)	(0.19)	(0.20)
H × Social mobility	3.024	-0.421	-0.231	0.507*
	(2.18)	(0.28)	(0.28)	(0.28)
H × Attitudes immigrants	-1.684****	-0.271	0.030	-0.253
	(0.38)	(0.32)	(0.31)	(0.31)
H × Trust in government	0.052	0.224	-0.380	-0.438
	(0.35)	(0.28)	(0.28)	(0.28)
Obs.	6096	7025	7025	7025
R2	0.0752	0.0558	0.0559	0.0561
Adj. R2	0.0717	0.0529	0.0529	0.0531

Notes: The table shows OLS regressions. The dependent variable is the preferred difference between tax rates on the top 1 percent and the bottom 50 percent. The regression model is the same as in column 5 of the main regressions table, adding interactions of "H" with social mobility beliefs, attitudes (towards) immigrants and trust in government. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. Right-wing is a dummy for a political orientation of 6 and above on the scale from 0 (left) to 10 (right). High income is a dummy for household income from the first (last) two quintiles. High education is a dummy for having a tertiary degree. Financial security is the answer to the question "When it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?" where 0 "Worse", 5 "The same" 10 "Better". Trust in government is the answer score to the general trust in government question where 0 "I don't trust them at all" and 10 "I completely trust them". Social mobility is the answer score to the question on about opportunities to climb the social ladder where 0 "There is not much opportunity" and 10 "There is plenty of opportunities". Attitudes (towards) immigrants is the average of both questions related to their perceived level of integration and their effect on the culture in the host country.

Table 1.5: Post-regression Accounting

	GER	GBR	ITA	SVN	JPN
Financial security	0.18	0.60	0.61	0.50	1.11
Trust in government	-0.42	0.50	1.01	0.86	0.46
Social mobility	0.31	1.00	0.99	1.16	1.57
Attitudes immigrants	-0.46	-0.46	-0.75	-0.98	-0.91
Controls	-0.75	-1.00	-0.66	-1.01	-0.48
Country dummy	7.62	5.50	-0.19	7.40	2.00
Predicted difference	6.47	6.14	1.00	7.92	3.75
Actual difference	6.63	5.86	0.91	7.63	4.02

Notes: The table shows how much of the difference in the dependent variable (T1-T50) relative to the U.S. can be explained by differences in means of the explanatory variables. Coefficients from regression model in column 5 of the main regressions table. Actual difference shows the difference in means vis-a-vis the U.S. using all available observations (see detailed summary statistics in the Appendix).

least impressive in the comparison of Germany and the U.S., this is not the case for the other countries. For example, differences in means of social mobility beliefs can explain more than one-third of the cross-country difference in progressivity between the U.S. and Japan. This exercise offers two main insights. First, in most pairwise comparisons, the country dummies capture most of the observed country differences, which may point to cross-cultural variation. Second, cross-country differences in social mobility beliefs, attitudes towards immigrants, and trust in government matter, especially when comparing Italy and Japan to the U.S.

Robustness checks. — We performed several robustness checks. First, we have run several regressions where we replaced the composite score thought measure of attitudes towards immigrants by other proxies (see Appendix Table 1.14). Those are the original answer scores concerning the integration of immigrants and their perceived impact on the host country's culture and stated trust in immigrants, and a question on racial prejudices (which has not been part of the questionnaire in Slovenia). Results using these alternative measures are qualitatively equivalent, and coefficients of the other explanatory variables remain largely unaffected. Respondents who stated having higher trust in immigrants prefer more progressive tax schemes. Consistently, stronger racial prejudices are associated with a minor preferred difference between top and bottom tax rates. The negative correlation between racial prejudices and redistributive preferences is statistically significant even after controlling for political orientation. The evaluation of immigrants' integration shows a positive, albeit statistically non-significant, coefficient. In opposition to this, the perceived effect of immigration on the culture in the host country is positive and significant from a statistical point of view. The vital statistical significance of racial prejudices and the higher relevance of the perceived cultural effects of immigration may indicate that the correlation between attitudes towards immigrants and redistributive preferences is driven more by xenophobia or group loyalty than by considerations about the economic effects of immigration.

A similar exercise in which we replaced trust in government in its general form with perceptions of the government's reliability, responsiveness, integrity, openness (transparency), and non-discrimination essentially leads to identical results as above (see Table 1.15 in the Appendix). Respondents who evaluate the government more positively on those dimensions, on average, prefer less progressive tax schemes indicating weaker demand for redistribution. The coefficients are most considerable in magnitude for the government's perceived integrity and transparency. Our results are also robust to different specifications of probit models (see Table 1.12 in the Appendix) using an indicator for progressive[17] preferences as the dependent variable. Qualitatively, this analysis yields the same results concerning the sign and statistical significance of explanatory variables as our regressions, with the difference between the top and bottom tax rates as the dependent variable.

In principle, it could be that the association of the explanatory variables is not symmetric, i.e., that a factor that correlates with higher progressivity in the aggregate has a positive effect on both tax rates on the rich and the poor, with the first dominating in magnitude. However, running separate regressions with the tax rates on the richest percent and the poor half of the population shows that this is not the case (see Appendix Table 1.13). That is, stronger social mobility beliefs that are associated with lower preferred progressivity are at the same time correlating with lower preferred tax rates on the rich and higher preferred tax rates on the poor. The regression coefficients for trust in government and attitudes towards immigrants are symmetric (with different signs) as well. Persuasively, positive attitudes towards immigrants show an even more statistically significant (at the 0.1 percent level) negative coefficient in the regression using the tax rate on the bottom 50 percent as the dependent variable. This finding might arise because respondents expect immigrants to make up a relatively larger part of this (relatively poorer) population group.

Furthermore, country-wise standardized regressions with standardization at the country level indicate that differences in coefficients are not due to different understanding of questions between countries. The ratios of standardized coefficients (see Figure 1.8 in the Appendix) between countries are similar to those from unstandardized regressions in Table 1.3. For example, the standardized coefficient of attitudes towards immigrants in the U.S. is about twice as large as in Germany, as it is in the

[17]We defined progressive tax preferences as $T1 \geq T9 \geq T40 \geq T50$ with pairwise comparisons being strictly larger in at least one case.

unstandardized regressions. The same holds for the coefficients on trust in government and social mobility beliefs.

1.4 Conclusion

This paper reports results from a large-scale survey as part of OECD's Trustlab initiative, which was run on representative samples in six countries (United States, Germany, the UK, Italy, Slovenia, and Japan). We examine preferences for redistribution through a quantitative measure derived from an internationally standardized question on splitting the tax burden across income groups fairly. Our results show that both preferences and the underlying determinants significantly differ between countries. Rather than Americans being "exceptional" (Alesina and Glaeser, 2004), Italian respondents align with U.S. Americans in their choice of tax rates on the richest 1 percent and the poorest 50 percent. Respondents from these two countries, on average, demand the least progressive tax schemes in our sample.[18] Another cluster is formed by German, British and Slovenian respondents who have the most progressive preferences in our sample. Tax preferences from Japanese respondents are located between these two groups.

Our analysis confirms that some of the mechanisms that have been proposed to explain preferences for redistribution matter, but they do so differently in different countries. First, beliefs about social mobility are strongly correlated with preferences for redistribution in the aggregate (Fong, 2001; Alesina and La Ferrara, 2005b; Alesina and Giuliano, 2011). Those believing that anyone who works hard can climb the social ladder are significantly less supportive of redistribution. Looking at standardized regressions (see Figure 1.8 in the Appendix), we can note that social mobility beliefs have the largest effect pooling all countries, almost twice that of attitudes towards immigrants. However, the mechanism of social mobility beliefs is almost irrelevant in Italy and Slovenia. At the same time, it is highly significant in the U.S., Germany, the UK, and to a somewhat smaller extent in Japan. We can also find some support for the POUM hypothesis as preferred progressivity is negatively correlated with the expected personal financial situation during the next year, albeit this coefficient is not statistically significant in all countries. On the other hand, personal income is never a statistically significant predictor in our regressions, thus questioning the relevance of self-interest in a strict sense when asking people about their tax preferences.

Attitudes towards immigrants also matter in the aggregate, as people having more positive views of immigrants show stronger preferences for progressiveness. This corre-

[18] The striking similarity of U.S. and Italian preferences is in line with recent experimental evidence by Grimalda et al. (2018).

lation is stronger using the tax rate on the poor half of the income distribution instead of the tax rate on the top 1 percent as the dependent variable. This finding may be explained due to respondents expecting the share of immigrants to be larger in the former group. However, this variable is only strongly statistically significant in the U.S. and, albeit weakly, in Germany, but not in the other countries. This result suggests that the predicted reduction in demand for redistribution as countries become more ethnically heterogenous (Alesina and Glaeser, 2004) is not (yet) detectable.

Looking at different dimensions of attitudes towards immigrants (see Appendix Table 1.14), we see that preferences for redistribution correlate most strongly with the perceived effect immigrants may have on the host country's culture, racial prejudices, and stated trust in immigrants. On the other hand, point estimates of the perceived level of immigrants' integration on preferred tax rates are small and statistically insignificant which may suggest that discrimination and group loyalty instead of economic considerations are at the basis of this correlation.

We also find that trust in government is negatively associated with preferred progressivity in our sample, with a significant relationship in all countries. Our findings, thus, replicate those by Barnes (2015) and Di Tella et al. (2021) (but not those by Yamamura (2014), Kuziemko et al. (2015), and Stantcheva (2021)). The channel behind this finding is that people stating high trust levels believe that their government already provides fair opportunities for economic mobility so that there is less need for governmental redistribution. Likewise, low trust in government is likely related to higher perceived levels of corruption, which may lead to higher redistribution demand to correct (perceived) inequities (Alesina and Angeletos, 2005a). We offer empirical support for the latter view as measures of integrity and transparency (openness) of the government are negatively correlated with preferred progressivity at a highly statistically significant level having larger coefficient sizes than measures of the government's perceived level of pursuing long-term objectives (responsiveness) or its capability to deliver public services (reliability).

References

Aassve, Arnstein, Letizia Mencarini, Francesco Chiocchio, Francesco Gandolfi, Ariana Gatta, and Francesco Mattioli. 2018. "Trustlab Italy: A New Dataset for the Study of Trust, Family Demography and Personality." *Dondena Working Papers*, 115.

Alesina, Alberto and George-Marios Angeletos. 2005a. "Corruption, inequality, and fairness." *Journal of Monetary Economics*, 52 (7): 1227–1244.

Alesina, Alberto and George-Marios Angeletos. 2005b. "Fairness and Redistribution." *American Economic Review*, 95 (4): 960–980.

Alesina, Alberto, Reza Baqir, and William Easterly. 1999. "Public Goods and Ethnic Divisions." *The Quarterly Journal of Economics*, 114 (4): 1243–1284.

Alesina, Alberto and Nicola Fuchs-Schündeln. 2007. "Good-Bye Lenin (or Not?): The Effect of Communism on People's Preferences." *American Economic Review*, 97 (4): 1507–1528.

Alesina, Alberto and Paola Giuliano. 2011. "Preferences for redistribution." In *Handbook of Social Economics*, edited by Benhabib, Jess, Alberto Bisin, and Matthew O. Jackson Volume 1A. of Handbook of Economics 93–131, Burlington: Elsevier Science.

Alesina, Alberto and Edward Ludwig Glaeser. 2004. *Fighting poverty in the US and Europe: A world of difference*. Oxford: Univ. Press.

Alesina, Alberto, Edward Ludwig Glaeser, and Burce Sacerdote. 2001. "Why Doesn't the United States Have a European-Style Welfare State?" *Brookings Papers on Economic Activity*, 2001 (2): 187–277.

Alesina, Alberto, Johann Harnoss, and Hillel Rapoport. 2021b. "Immigration and the Future of the Welfare State in Europe." *The ANNALS of the American Academy of Political and Social Science*, 697 (1): 120–147.

Alesina, Alberto and Eliana La Ferrara. 2005b. "Preferences for redistribution in the land of opportunities." *Journal of Public Economics*, 89 (5-6): 897–931.

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva. 2022. "Immigration and Redistribution." *The Review of Economic Studies*, Accepted Manuscript (0): 1–39.

Alesina, Alberto, Elie Murard, and Hillel Rapoport. 2021c. "Immigration and preferences for redistribution in Europe." *Journal of Economic Geography*, 21 (6): 925–954.

Alesina, Alberto, Stefanie Stantcheva, and Edoardo Teso. 2018. "Intergenerational Mobility and Preferences for Redistribution." *American Economic Review*, 108 (2): 521–554.

Barnes, Lucy. 2015. "The size and shape of government: Preferences over redistributive tax policy." *Socio-Economic Review*, 13 (1): 55–78.

Bartels, Larry M. 2009. "Economic Inequality and Political Representation." In *The unsustainable American state*, edited by King, Desmond S. and Lawrence R. Jacobs 167–196, Oxford and New York: Oxford University Press.

Bénabou, Roland and Efe A. Ok. 2001. "Social Mobility and the Demand for Redistribution: The POUM Hypothesis." *The Quarterly Journal of Economics*, 116 (2): 447–487.

Bénabou, Roland and Jean Tirole. 2006a. "Belief in a Just World and Redistributive Politics." *The Quarterly Journal of Economics*, 121 (2): 699–746.

Burgoon, Brian, Ferry Koster, and Marcel van Egmond. 2012. "Support for redistribution and the paradox of immigration." *Journal of European Social Policy*, 22 (3): 288–304.

Causa, Orsetta and Mikkel Hermansen. 2019. "Income redistribution through taxes and transfers across OECD countries." *OECD Economics Department Working Papers*, 1453.

Central Intelligence Agency. 2020. "CIA World Factbook." `https://www.cia.gov/the-world-factbook/`.

Checchi, Daniele and Antonio Filippin. 2004. "An Experimental Study of the POUM Hypothesis." In *Inequality, welfare and income distribution*, edited by Cowell, Frank Alan, Amsterdam and Boston: Elsevier JAI.

Cojocaru, Alexandru. 2014. "Prospects of upward mobility and preferences for redistribution: Evidence from the Life in Transition Survey." *European Journal of Political Economy*, 34 (0): 300–314.

Corneo, Giacomo. 2001. "Inequality and the State: Comparing US and German Preferences." *Annales d'Économie et de Statistique*, 2001 (63/64): 283–296.

Corneo, Giacomo. 2004. "Wieso Umverteilung? Einsichten aus ökonomischen Umfrageanalysen." In *Finanzpolitik und Umverteilung*, edited by Genser, Bernd 55–88, Berlin: Duncker & Humblot.

Corneo, Giacomo and Hans Peter Grüner. 2002. "Individual preferences for political redistribution." *Journal of Public Economics*, 83 (1): 83–107.

Dahlberg, Matz, Karin Edmark, and Heléne Lundqvist. 2012. "Ethnic Diversity and Preferences for Redistribution." *Journal of Political Economy*, 120 (1): 41–76.

Dallinger, Ursula. 2010. "Public support for redistribution: What explains cross-national differences?" *Journal of European Social Policy*, 20 (4): 333–349.

Di Tella, Rafael, Juan Dubra, and Alejandro Lagomarsino. 2021. "Meet the Oligarchs: Business Legitimacy and Taxation at the Top." *The Journal of Law and Economics*, 64 (4): 651–674.

Finseraas, Henning. 2008. "Immigration and Preferences for Redistribution: An Empirical Analysis of European Survey Data." *Comparative European Politics*, 6 (4): 407–431.

Fong, Christina M. 2001. "Social preferences, self-interest, and the demand for redistribution." *Journal of Public Economics*, 82 (2): 225–246.

Fong, Christina M. and Erzo F.P. Luttmer. 2011. "Do fairness and race matter in generosity? Evidence from a nationally representative charity experiment." *Journal of Public Economics*, 95 (5-6): 372–394.

Gavira, Alejandro, Carol Graham, and Luis H. B. Braido. 2007. "Social Mobility and Preferences for Redistribution in Latin America." *Economía*, 8 (1): 55–88.

Grimalda, Gianluca, Francesco Farina, and Ulrich Schmidt. 2018. "Preferences for Redistribution in the US, Italy, Norway: An Experimental Study." *Kiel Working Paper*, 2099.

Guillaud, Elvire. 2013. "Preferences for redistribution: An empirical analysis over 33 countries." *The Journal of Economic Inequality*, 11 (1): 57–78.

Isaksson, Ann-Sofie and Annika Lindskog. 2009. "Preferences for redistribution—A country comparison of fairness judgements." *Journal of Economic Behavior & Organization*, 72 (3): 884–902.

Istituto Nazionale di Statistica. 2017. "Italy in Figures: 2016." `https://www.istat.it/en/archivio/200600`.

Jaime-Castillo, Antonio M. and José L. Sáez-Lozano. 2014. "Preferences for tax schemes in OECD countries, self-interest and ideology." *International Political Science Review*, 37 (1): 81–98.

Karadja, Mounir, Johanna Mollerstrom, and David Seim. 2017. "Richer (and Holier) Than Thou? The Effect of Relative Income Improvements on Demand for Redistribution." *The Review of Economics and Statistics*, 99 (2): 201–212.

Keely, Louise C. and Chih Ming Tan. 2008. "Understanding preferences for income redistribution." *Journal of Public Economics*, 92 (5-6): 944–961.

Kenworthy, Lane and Leslie McCall. 2008. "Inequality, public opinion and redistribution." *Socio-Economic Review*, 6 (1): 35–68.

Kuziemko, Ilyana, Michael I. Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How elastic are preferences for redistribution? Evidence from randomized survey experiments." *American Economic Review*, 105 (4): 1478–1508.

Lee, Woojin and John Ernest Roemer. 2006. "Racism and redistribution in the United States: A solution to the problem of American exceptionalism." *Journal of Public Economics*, 90 (6-7): 1027–1052.

Lipset, Seymour Martin. 1996. *American exceptionalism: A double-edged sword.* New York: W.W. Norton.

Luttmer, Erzo F.P. 2001. "Group Loyalty and the Taste for Redistribution." *Journal of Political Economy*, 109 (3): 500–528.

Meltzer, Allan H. and Scott F. Richard. 1981. "A Rational Theory of the Size of Government." *Journal of Political Economy*, 89 (5): 914–927.

Murtin, Fabrice, Lara Fleischer, Vincent Siegerink, Arnstein Aassve, Yann Algan, Romina Boarini, Santiago González, Zsuzsanna Lonti, Gianluca Grimalda, Rafael Hortala Vallve, Soonhe Kim, David Lee, Louis Putterman, and Conal Smith. 2018. "Trust and its determinants." *OECD Statistics Working Papers*, 2018/02.

National Institute of Population and Social Security Research. 2017. "Population Statistics of Japan." `http://www.ipss.go.jp`.

OECD. 2018. *Education at a Glance 2018: OECD Indicators.* Paris: OECD Publishing.

OECD. 2020. "Social Expenditure Database (SOCX)." `http://www.oecd.org/social/expenditure.htm`.

Office for National Statistics. 2013. "2011 Census: Key Statistics and Quick Statistics for Local Authorities in the United Kingdom." `https://www.ons.gov.uk/peoplepopulationandcommunity/populationandmigration/populationestimates/bulletins/keystatisticsandquickstatisticsforlocalauthoritiesintheunitedkingdom/2013-10-11#population`.

Office for National Statistics. 2018. “UK labour market: Estimates of employment, unemployment, economic inactivity and other employment-related statistics for the UK.” `https://www.ons.gov.uk/employmentandlabourmarket/peopleinwork/employmentandemployeetypes/bulletins/uklabourmarket/december2018`.

Persson, Torsten and Guido Enrico Tabellini. 2000. *Political economics: Explaining economic policy.* Zeuthen lecture book series, Cambridge, Mass.: MIT Press.

Piketty, Thomas. 1995. “Social Mobility and Redistributive Politics.” *The Quarterly Journal of Economics*, 110 (3): 551–584.

Ravallion, Martin and Michael Lokshin. 2000. “Who wants to redistribute?” *Journal of Public Economics*, 76 (1): 87–104.

Republic of Slovenia Statistical Office SiStat. 2019. “SiStat Database: Population aged 15 years or more by Sex, Age, Year and Education, reference year 2018.” `https://pxweb.stat.si/SiStat/en`.

Rueda, David. 2018. “Food Comes First, Then Morals: Redistribution Preferences, Parochial Altruism, and Immigration in Western Europe.” *The Journal of Politics*, 80 (1): 225–239.

Scheve, Kenneth and David Stasavage. 2006. “Religion and Preferences for Social Insurance.” *Quarterly Journal of Political Science*, 1 (3): 255–286.

Senik, Claudia, Holger Stichnoth, and Karine van der Straeten. 2009. “Immigration and Natives’ Attitudes towards the Welfare State: Evidence from the European Social Survey.” *Social Indicators Research*, 91 (3): 345–370.

Shelton, Cameron A. 2007. “The size and composition of government expenditure.” *Journal of Public Economics*, 91 (11-12): 2230–2260.

Sinn, Hans-Werner. 1995. “A Theory of the Welfare State.” *The Scandinavian Journal of Economics*, 97 (4): 495–526.

Stantcheva, Stefanie. 2021. “Understanding Tax Policy: How do People Reason?” *The Quarterly Journal of Economics*, 136 (4): 2309–2369.

Statistical Office of the Republic of Slovenia. 2020. “Labour market: Population by activity, sex and cohesion regions (in 1000), Slovenia, quarterly.” `https://pxweb.stat.si/sistat/en/Podrocja/Index/139/labour-market`.

Statistisches Bundesamt. 2018. "Ergebnisse Mikrozensus 2018 Fachserie 1 Reihe 2.2: Ergebnisse des Mikrozensus 2018, hochgerechnet auf Basis des Zensus 2011." `https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bevoelkerung/Migration-Integration/Publikationen/Downloads-Migration/migrationshintergrund-2010220187004.pdf`.

Statistisches Bundesamt. 2019. "Bevölkerung im Alter von 15 Jahren und mehr nach allgemeinen und beruflichen Bildungsabschlüssen nach Jahren." `https://www.destatis.de/DE/Themen/Gesellschaft-Umwelt/Bildung-Forschung-Kultur/Bildungsstand/Tabellen/bildungsabschluss.html`.

Statistisches Bundesamt. 2020. "Eckzahlen zum Arbeitsmarkt, Deutschland: Deutschland für die Jahre 2009, 2018 und 2019." `https://www.destatis.de/DE/Themen/Arbeit/Arbeitsmarkt/Erwerbstaetigkeit/Tabellen/eckwerttabelle.html`.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *The Review of Economic Studies*, 87 (1): 454–486.

Tajfel, Henri. 1974. "Social identity and intergroup behaviour." *Social Science Information*, 13 (2): 65–93.

United States Census Bureau. 2018. "Educational Attainment in the United States: 2018." `https://www.census.gov/data/tables/2018/demo/education-attainment/cps-detailed-tables.html`.

United States Census Bureau. 2019b. "QuickFacts: Population statistics." `https://www.census.gov/quickfacts/fact/table/US/PST045219`.

U.S. Bureau of Labor Statistics. 2020. "Labor Force Statistics from the Current Population Survey." `https://www.bls.gov/cps/cpsaat01.htm`.

Varian, Hal R. 1980. "Redistributive taxation as social insurance." *Journal of Public Economics*, 14 (1): 49–68.

Yamamura, Eiji. 2014. "Trust in government and its effect on preferences for income redistribution and perceived tax burden." *Economics of Governance*, 15 (1): 71–100.

Appendices

1.A Appendix

1.A.1 Descriptive statistics

Table 1.6: Detailed Summary Tax Rates

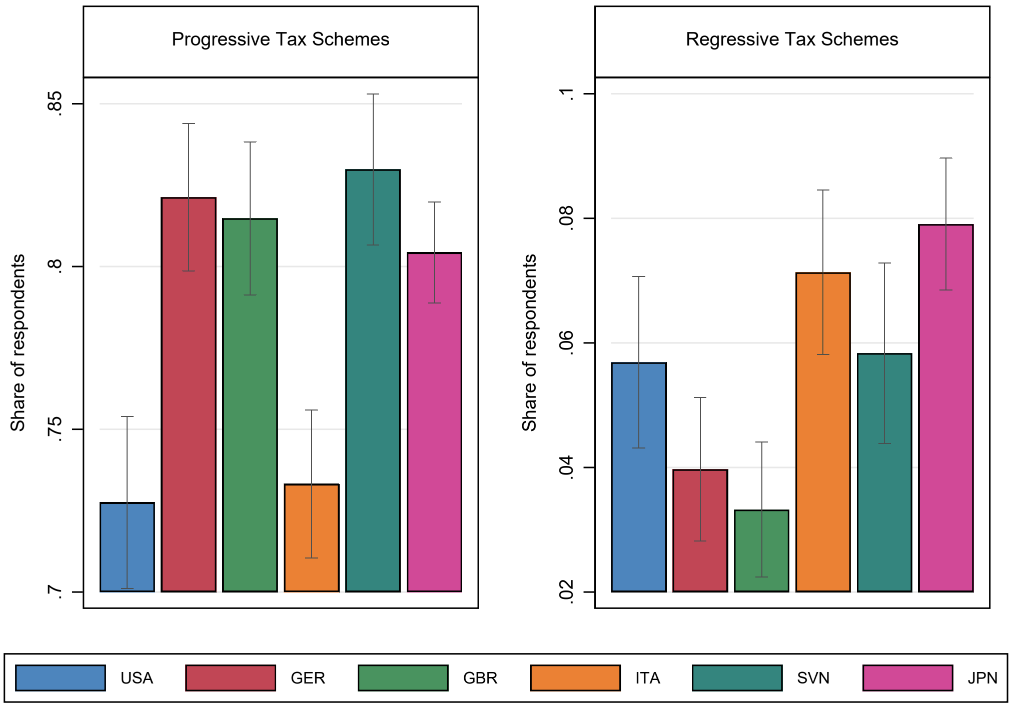
	Tax Top 1%	Tax Next 9%	Tax Next 40%	Tax Bottom 50%	T1-T50
GER					
Obs.	1108	1108	1108	1108	1108
Mean	43.70	33.90	23.32	14.29	29.41
SE	0.48	0.33	0.15	0.31	0.69
Median	45	34	24	13	32
IQR	18	11	4	10	26
Minimum	0	0	0	0	-57
Maximum	75	75	46	75	75
GBR					
Obs.	1053	1053	1053	1053	1053
Mean	43.63	33.39	23.25	14.99	28.64
SE	0.50	0.34	0.14	0.34	0.73
Median	44	33	24	14	30
IQR	16	11	4	11	27
Minimum	1	0	0	0	-70
Maximum	75	75	40	75	75
ITA					
Obs.	1458	1458	1458	1458	1458
Mean	40.66	32.86	23.00	16.97	23.69
SE	0.48	0.31	0.14	0.35	0.74
Median	40	33	24	15	27
IQR	20	11	4	12	33
Minimum	0	0	0	0	-75
Maximum	75	75	50	75	75
JPN					
Obs.	2504	2504	2504	2504	2504
Mean	42.72	33.90	22.78	15.92	26.80
SE	0.36	0.23	0.09	0.23	0.54
Median	45	35	23	14	31
IQR	18	11	5	10	29
Minimum	0	0	0	0	-75
Maximum	75	75	47	75	75

Table 1.6: Detailed Summary Tax Rates

	Tax Top 1%	Tax Next 9%	Tax Next 40%	Tax Bottom 50%	T1-T50
SVN					
Obs.	1011	1011	1011	1011	1011
Mean	44.59	34.52	22.91	14.18	30.41
SE	0.56	0.34	0.14	0.35	0.85
Median	45	35	24	12	34
IQR	21	10	5	10	32
Minimum	0	1	6	0	-72
Maximum	75	75	44	75	75
USA					
Obs.	1090	1090	1090	1090	1090
Mean	39.92	31.43	23.58	17.13	22.78
SE	0.53	0.35	0.15	0.37	0.81
Median	39	30	25	15	23
IQR	21	11	3	14	34
Minimum	0	0	0	0	-75
Maximum	75	75	50	75	75
Total					
Obs.	8224	8224	8224	8224	8224
Mean	42.46	33.40	23.07	15.71	26.75
SE	0.19	0.13	0.05	0.13	0.29
Median	43	33	24	14	30
IQR	19	12	4	11	30
Minimum	0	0	0	0	-75
Maximum	75	75	50	75	75

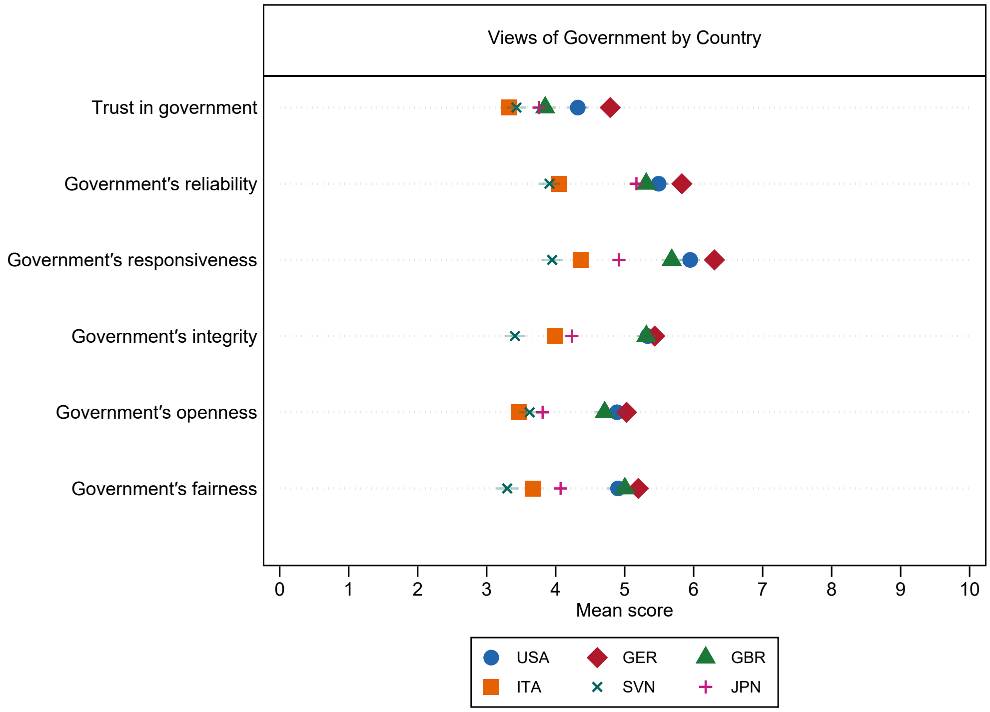
Notes: Detailed summary statistics of preferred tax rate variables. The table shows the mean, standard error, median, interquartile range, minimum, and maximum for the pooled sample and for the country subsamples.

Figure 1.5: Progressive and Regressive Tax Schemes by Country



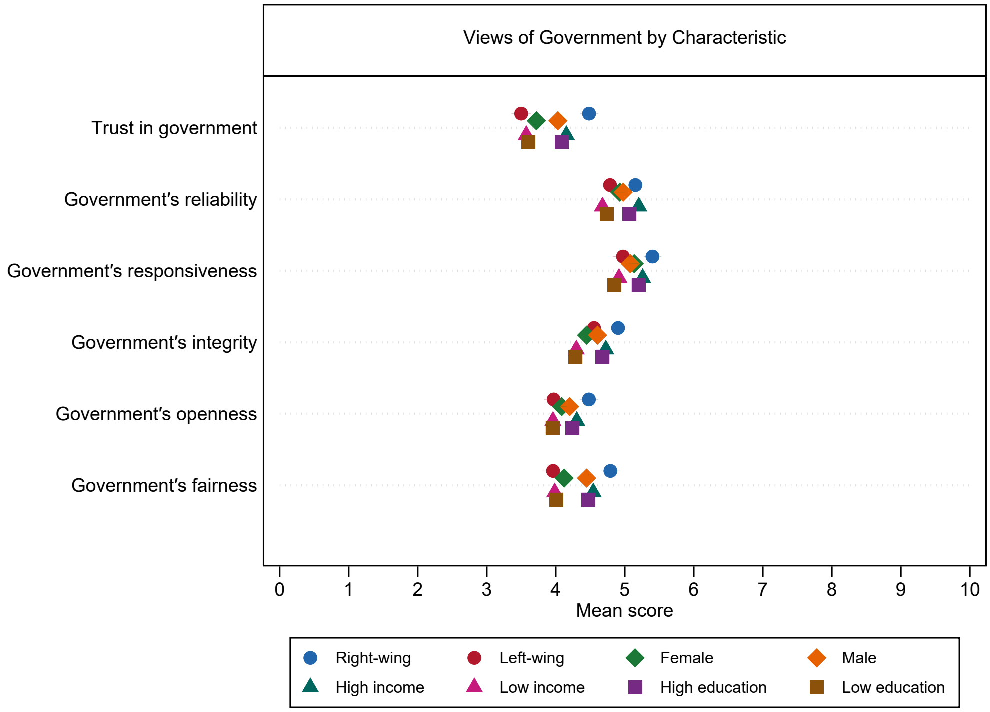
Notes: This figure shows the percentage of respondents stating tax schemes classified as progressive or regressive in each country. Whiskers span the 95 percent confidence interval. We define progressive tax schemes as a tax burden that is weakly descending from the top 1 percent over the next 9 percent and 40 percent to the bottom 50 percent. This definition means that T1 is larger or equal than T9, which is larger or equal than T40, which is larger or equal than T50, of which at least one pairwise comparison must be strictly larger. Conversely, a regressive tax scheme is defined as weakly ascending over the same groups.

Figure 1.6: Views of Government by Country



Notes: This figure shows the average scores for the respective variable on the vertical axis in each country. Whiskers span the 95 percent confidence interval.

Figure 1.7: Views of Government by Characteristic



Notes: This figure shows the average score of views of government variables for the respective subgroups indicated below the figure. Whiskers span the 95 percent confidence interval.

Table 1.7: Preferred Tax Rates across Countries: t-tests

	USA	GER	GBR	ITA	SVN
GER	0.0000				
	0.0000				
GBR	0.0000	0.9205			
	0.0000	0.1251			
ITA	0.2970	0.0000	0.0000		
	0.7378	0.0000	0.0000		
SVN	0.0000	0.2278	0.2021	0.0000	
	0.0000	0.8144	0.0961	0.0000	
JPN	0.0000	0.0998	0.1367	0.0005	0.0051
	0.0051	0.0000	0.0230	0.0113	0.0000

Notes: The table displays p-values from t-tests accounting for unequal variances against the null hypothesis of equal means in the countries from the column vs. from the respective row. The first (second) row for each country shows the p-value for the tax rate on the top 1 percent (bottom 50 percent).

Table 1.8: Summary Statistics by Country

Variable	Pooled	USA	GER	GBR	ITA	JPN
Female	0.441	0.474	0.456	0.598	0.385	0.462
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Migration background	0.107	0.201	0.245	0.030	0.010	0.094
	(0.01)	(0.02)	(0.02)	(0.01)	(0.00)	(0.00)
Age 15-24	0.104	0.0927	0.126	0.094	0.089	0.098
	(0.01)	(0.01)	(0.02)	(0.01)	(0.01)	(0.00)
Age 25-54	0.645	0.719	0.713	0.793	0.679	0.708
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
Age 55-64	0.246	0.172	0.153	0.114	0.218	0.185
	(0.02)	(0.01)	(0.02)	(0.01)	(0.01)	(0.01)
Low education	0.139	0.233	0.421	0.423	0.333	0.315
	(0.01)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Medium education	0.355	0.363	0.146	0.172	0.089	0.203
	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)	(0.01)
High education	0.505	0.403	0.433	0.405	0.578	0.482
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Employed	0.761	0.781	0.793	0.724	0.656	0.726
	(0.02)	(0.02)	(0.02)	(0.02)	(0.01)	(0.01)
Selfemployed	0.088	0.079	0.096	0.129	0.113	0.104
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Unemployed	0.050	0.020	0.034	0.058	0.022	0.035
	(0.01)	(0.01)	(0.01)	(0.01)	(0.00)	(0.00)
Out of labor force	0.102	0.121	0.0774	0.089	0.209	0.135
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social mobility	6.234	5.975	5.709	5.509	4.955	5.543
	(0.11)	(0.09)	(0.11)	(0.08)	(0.07)	(0.04)
Financial security	6.389	6.125	5.649	5.504	4.762	5.520
	(0.09)	(0.08)	(0.10)	(0.07)	(0.06)	(0.04)
Keep job	7.982	7.776	7.586	7.201	6.654	7.290
	(0.10)	(0.10)	(0.11)	(0.10)	(0.08)	(0.04)
Find new job	6.146	5.960	5.795	4.354	4.270	5.075
	(0.13)	(0.11)	(0.13)	(0.10)	(0.08)	(0.05)
Immigration: Integretation	6.134	4.957	5.222	4.459	4.396	4.883
	(0.10)	(0.09)	(0.12)	(0.09)	(0.06)	(0.04)
Immigrants: Culture	6.866	5.915	6.025	5.154	4.667	5.503
	(0.11)	(0.11)	(0.13)	(0.10)	(0.06)	(0.04)
Attitudes immigrants	6.500	5.436	5.623	4.807	4.531	5.193
	(0.09)	(0.09)	(0.11)	(0.08)	(0.05)	(0.04)
Racial prejudices	5.766	5.723	4.964	5.114	4.471	5.087
	(0.12)	(0.09)	(0.12)	(0.09)	(0.06)	(0.04)
Trust in immigrants	6.570	5.536	5.734	4.882	4.649	5.291
	(0.09)	(0.08)	(0.09)	(0.08)	(0.06)	(0.04)
Risk aversion	3.834	4.725	4.665	4.247	5.507	4.746
	(0.11)	(0.09)	(0.12)	(0.08)	(0.07)	(0.04)
Political orientation	5.886	5.167	5.395	5.294	5.503	5.446
	(0.11)	(0.08)	(0.11)	(0.10)	(0.04)	(0.04)
Religion importance	5.987	3.609	3.109	4.991	3.640	4.199
	(0.15)	(0.13)	(0.16)	(0.13)	(0.08)	(0.06)
Trust in government	4.771	5.002	4.285	3.591	4.121	4.291

Table 1.8: Summary Statistics by Country

Variable	Pooled	USA	GER	GBR	ITA	JPN
	(0.11)	(0.09)	(0.12)	(0.09)	(0.07)	(0.04)
Government's reliability	5.766	5.913	5.609	4.215	5.303	5.302
	(0.10)	(0.08)	(0.10)	(0.08)	(0.06)	(0.04)
Government's responsiveness	6.150	6.318	5.877	4.439	5.078	5.438
	(0.10)	(0.08)	(0.10)	(0.08)	(0.06)	(0.04)
Government's integrity	5.714	5.624	5.485	4.221	4.468	4.948
	(0.09)	(0.09)	(0.10)	(0.08)	(0.06)	(0.04)
Government's openess	5.289	5.226	5.117	3.702	4.085	4.530
	(0.11)	(0.09)	(0.11)	(0.08)	(0.06)	(0.04)
Government's fairness	5.405	5.400	5.444	3.946	4.377	4.769
	(0.12)	(0.10)	(0.12)	(0.09)	(0.06)	(0.04)
Trust financial institutions	5.839	4.607	5.044	3.736	5.329	4.920
	(0.11)	(0.09)	(0.11)	(0.08)	(0.06)	(0.04)

Notes: The table shows country-wise summary statistics. Standard errors in parentheses. The first 12 variables are indicator variables. The remaining variables are Likert variables on a scale from 0 to 10.

1.B Online Appendix

Supplementary Online Material for
Cross-Country Evidence on the Determinants of Preferences for Redistribution
Gianluca Grimalda, David Pipke

1.B.1 Description of Data Sources

We collected data for the population statistics from reliable representative sources. Whenever possible, we used data from the year in which the survey was carried out in the respective country.

1. Age and sex composition of the population

    (a) Data taken from the CIA World Fact Book for all six countries in the sample (Central Intelligence Agency, 2020).

2. Ethnic groups

    (a) USA: Data taken from the United States Census Bureau, based on populaton estimates from 2010 (United States Census Bureau, 2019b).

    (b) Germany: Estimates based on calculations using the number of people with migration background (at least one of the parents not born in Germany) from the respective countries. Turkish ethnicity approximated by migration background from the Republic of Turkey. Eastern European ethnicity comprises migration backgrounds from Belarus, Bulgaria, Czechia, Hungary, Moldova, Poland, Romania, Russia, Slovakia, Ukraine, Serbia, Bosnia and Herzegovina, Kosovo. Data from Statistisches Bundesamts publication "Ergebnisse des Mikrozensus 2018 - Fachserie 1 Reihe 2.2 - 2018" (Statistisches Bundesamt, 2018).

    (c) UK: Data from the 2011 Census of the Office for National Statistics which was published in 2013 (Office for National Statistics, 2013).

3. Educational attainment: Data for educational attainment is not perfectly comparable across countries. We searched for the most accurate equivalent of U.S. education levels of which we classified "High-school degree or less" as low education, a non-tertiary degree as medium education and a tertiary degree as high education.

    (a) USA: Data from the publication of the United States Cenus Bureau "Educational Attainment of the Population 18 Years and Over, by Age, Sex, Race, and Hispanic Origin: 2018" (United States Census Bureau, 2018). Low education is high-school degree or less. Medium education is a non-tertiary degree whereas High-education corresponds to University degrees (Bachelor, Master, Professional or Doctoral degree).

(b) Germany: Data from Statistisches Bundesamt. Publication "Bevoelkerung im Alter von 15 Jahren und mehr nach allgemeinen und beruflichen Bildungsabschluessen nach Jahren". Estimates from 2017 based on the Mikrozensus 2011 (Statistisches Bundesamt, 2019). Low education corresponds to "Abitur or below". Medium education is Ausbildung or Fachschulabschluss. High education is Bachelor, Master, Diplom or Promotion (degrees from Fachhochschulen or Universities).

(c) Italy: Data (reference year 2017) from the OECD Statistics website "Educational attainment of 25-64 year-olds" (OECD, 2018). Low education is "below upper-secondary", medium education is "upper-secondary or post-secondary non-tertiary education" and high education is "tertiary education".

(d) United Kingdom: Data (reference year 2018) from the OECD Statistics website (same source as for Italy) "Educational attainment of 25-64 year-olds" (OECD, 2018). Low education is "below upper-secondary", medium education is "upper-secondary or post-secondary non-tertiary education" and high education is "tertiary education"

(e) Slovenia: Data (2018 reference year) from Stat.si's (Statistical Office of the Republic of Slovenia) publication "Population aged 15 years or more by SEX, AGE, YEAR and EDUCATION" (Republic of Slovenia Statistical Office SiStat, 2019). Low education comprises "Basic or less", medium education is "Upper secondary" and high education is "Tertiary education".

(f) Japan: Data (2010 reference year) based on Population Census of Japan from the publication "Proportion of Persons 15 Years of Age and Over by Age, Sex and Educational Attainment: 1970, 2010" by the National Institute of Population and Social Security Research (National Institute of Population and Social Security Research, 2017). Low education "Primary education" medium education is "Secondary education" and high education is "High grade education".

4. Labor market statistics

   (a) USA: Data from the U.S. Bureau of Labor Statistics. Empoyment statistics for the civilian noninstitutional population (16 and older, no army, no inmates) (U.S. Bureau of Labor Statistics, 2020). Averaged over all four quarterly periods of 2018.

(b) Germany: Data (working population aged 15-74) from Statistisches Bundesamt "Eckzahlen zum Arbeitsmarkt, Deutschland" for the reference year 2018 (Statistisches Bundesamt, 2020).

(c) Italy: Data from the Istituto Nazionale di Statistica (ISTAT) for the reference year 2016 (population aged 15 and over). Published as part of the "Italy in Figures" series (Istituto Nazionale di Statistica, 2017).

(d) United Kingdom: Employment data (population aged from 16 to 64 years) from the Office for National statistics in the UK for the reference year 2018 (Office for National Statistics, 2018).

(e) Slovenia: Employment statistics from the Statistical Office of the Republic of Slovenia (Stat.si) for the reference year 2017 (population aged 15 and over) (Statistical Office of the Republic of Slovenia, 2020). Averaged over all reported four quartely periods of 2017.

(f) Japan: Data on employment (population aged 14 and over) from the National Institute of Population and Social Security Research for the reference year 2015 (National Institute of Population and Social Security Research, 2017)

1.B.2 Additional analyses

Table 1.9: Complete Table: Pooled Sample Regression

	(1)	(2)	(3)	(4)	(5)	(6)
	Pooled	Pooled	Pooled	Pooled	Pooled	Pooled
Female	1.274**	1.083*	0.941	0.723	0.638	
	(0.61)	(0.61)	(0.61)	(0.63)	(0.63)	
Age/10	0.422*	0.519**	0.499**	0.525**	0.545**	
	(0.23)	(0.23)	(0.23)	(0.25)	(0.25)	
Migration	-2.462**	-1.985*	-2.012*	-2.139**	-2.268**	
	(1.03)	(1.03)	(1.03)	(1.06)	(1.06)	
Low income	0.980	0.793	0.746	0.611	0.449	
	(0.84)	(0.85)	(0.85)	(0.88)	(0.87)	
High income	-0.711	-0.739	-0.493	-0.717	-0.576	
	(0.83)	(0.83)	(0.84)	(0.87)	(0.87)	
Self-employed	4.006****	3.436***	3.627****	3.657***	3.788****	
	(1.06)	(1.07)	(1.07)	(1.12)	(1.11)	
Unemployed	1.293	1.074	1.024	1.110	0.880	
	(1.22)	(1.22)	(1.23)	(1.27)	(1.28)	
Inactive	1.571**	1.663**	1.878***	1.708**	2.081***	

Table 1.9: Complete Table: Pooled Sample Regression

	(1) Pooled	(2) Pooled	(3) Pooled	(4) Pooled	(5) Pooled	(6) Pooled
	(0.71)	(0.72)	(0.72)	(0.75)	(0.75)	
Low education	-2.550***	-2.339***	-2.260***	-2.002**	-1.919**	
	(0.83)	(0.83)	(0.84)	(0.87)	(0.87)	
High education	-0.155	0.208	0.325	0.271	0.353	
	(0.81)	(0.81)	(0.82)	(0.85)	(0.85)	
GER	7.025****	7.481****	6.975****	7.902****	7.618****	6.831****
	(1.07)	(1.07)	(1.07)	(1.10)	(1.09)	(1.08)
GBR	5.891****	5.593****	4.930****	5.886****	5.498****	4.407****
	(1.13)	(1.12)	(1.12)	(1.16)	(1.16)	(1.11)
ITA	0.255	-0.612	-0.867	-0.231	-0.188	-0.968
	(1.13)	(1.13)	(1.13)	(1.16)	(1.16)	(1.12)
SVN	7.428****	6.351****	5.854****	7.639****	7.399****	6.267****
	(1.21)	(1.20)	(1.21)	(1.28)	(1.28)	(1.24)
JPN	2.796***	2.708**	1.882*	2.517**	2.001*	1.358
	(1.06)	(1.06)	(1.07)	(1.13)	(1.13)	(1.05)
Financial security	-1.584****	-1.128****	-0.683****	-1.236****	-0.751****	-0.818****
	(0.15)	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)
Trust in government		-1.190****	-0.915****	-1.224****	-0.954****	-0.972****
		(0.13)	(0.13)	(0.14)	(0.14)	(0.14)
Social mobility			-1.127****		-1.186****	-1.177****
			(0.14)		(0.15)	(0.15)
Attitudes immigrants				0.351**	0.479***	0.446***
				(0.16)	(0.16)	(0.16)
Constant	29.47****	31.64****	34.66****	30.15****	32.30****	36.25****
	(1.93)	(1.96)	(2.01)	(2.26)	(2.28)	(1.55)
Obs.	7854	7762	7577	7125	7025	7026
R2	0.0346	0.0443	0.0532	0.0465	0.0553	0.0501
Adj. R2	0.0327	0.0422	0.0510	0.0441	0.0528	0.0489

Notes: The table shows OLS regressions. Robust standard errors in parentheses. The dependent variable is the difference between preferred tax rates on the top 1 percent and the bottom 50 percent. Migration is a dummy indicating a migration background (either the respondent or parents immigrated). Low (high) income indicates an income in the first (last) two quartiles. Self-employed, unemployed, and inactive are indicator variables for the respondent's labor force status. Employed serves as the base category. The remaining variables are country dummies or refer to survey measures. Wald tests of the null hypothesis of equal coefficients for the low-income and high-income categories yield p-values of 0.027, 0.045, 0.107, 0.095, and 0.197 for the columns (1-5), respectively.

Table 1.10: Complete Tables: Country-wise Regressions

	(1) USA	(2) GER	(3) GBR	(4) ITA	(5) SVN	(6) JPN
Female	2.319	1.538	-1.170	1.314	-0.467	0.940
	(1.68)	(1.42)	(1.61)	(1.64)	(1.80)	(1.35)
Age/10	1.309**	0.919	0.686	0.0643	0.490	0.208
	(0.63)	(0.57)	(0.63)	(0.68)	(0.76)	(0.49)
Migration	-7.329***	-0.0211	-5.211***	0.556	2.349	-14.880
	(2.78)	(1.90)	(1.96)	(3.72)	(2.35)	(9.53)
Low income	-0.0748	2.865	1.909	-1.336	-0.93	-0.725
	(1.99)	(2.01)	(2.15)	(2.00)	(2.34)	(2.27)
High income	-1.624	2.070	-0.496	-2.342	-3.278	0.700
	(2.05)	(1.94)	(2.39)	(2.04)	(2.86)	(1.86)
Self-employed	2.057	1.469	4.572	3.006	8.510**	4.027*
	(3.13)	(2.71)	(3.09)	(2.31)	(3.40)	(2.19)
Unemployed	-0.477	3.296	-7.117**	2.639	3.952	2.859
	(2.77)	(3.31)	(3.42)	(2.61)	(3.25)	(3.43)
Inactive	0.586	2.075	-0.239	3.828*	0.553	2.521*
	(2.08)	(1.76)	(1.79)	(2.09)	(2.29)	(1.51)
Low education	-0.796	-3.479**	3.872*	-2.332	-2.491	-1.890
	(2.45)	(1.77)	(2.31)	(2.09)	(2.67)	(2.38)
High education	0.980	-1.202	5.102**	-0.411	-2.197	1.864
	(1.79)	(1.77)	(2.49)	(2.11)	(2.73)	(2.28)
Financial security	-0.792*	-0.580	-0.031	-0.731	-0.324	-1.468****
	(0.45)	(0.41)	(0.40)	(0.46)	(0.48)	(0.35)
Trust in government	-1.160****	-0.681*	-0.882***	-1.059***	-0.983**	-0.743**
	(0.34)	(0.36)	(0.33)	(0.37)	(0.45)	(0.29)
Social mobility	-2.192****	-1.797****	-2.045****	-0.0992	-0.451	-0.857***
	(0.38)	(0.35)	(0.36)	(0.40)	(0.42)	(0.31)
Attitudes immigrants	1.512****	0.696*	0.130	0.063	0.634	0.190
	(0.41)	(0.42)	(0.35)	(0.41)	(0.45)	(0.37)
Const	30.02****	36.60****	36.96****	31.30****	35.27****	36.26****
	(5.32)	(4.49)	(5.17)	(4.92)	(5.70)	(4.54)
Obs.	972	1016	935	1362	875	1865
R2	0.1310	0.0717	0.0976	0.0215	0.0298	0.0491
Adj. R2	0.1180	0.0587	0.0839	0.0113	0.0141	0.0419

Notes: The table shows the complete regression estimates for the country-wise regressions. The dependent variable is the difference between preferred tax rates on the top 1 percent and the bottom 50 percent. Robust standard errors in parentheses. Migration is a dummy indicating a migration background (either the respondent or parents immigrated). Low (high) income indicates an income in the first (last) two quartiles. Self-employed, unemployed, and inactive are indicator variables for the respondent's labor force status. Employed serves as the base category. The remaining variables are defined in the main text.

Table 1.11: Country-wise Regressions: p-values

	vs. USA	vs. GER	vs. GBR	vs. ITA	vs. SVN	vs. JPN
USA		0.295	0.495	0.653	0.779	0.828
		0.187	0.718	0.803	0.635	0.398
		0.724	0.201	0.923	0.472	0.232
		0.159	0.010	0.012	0.146	0.016
		0.442	0.778	0.000	0.002	0.006
		0.329	0.552	0.839	0.750	0.348
GER	0.295		0.744	0.136	0.215	0.234
	0.187		0.400	0.115	0.118	0.608
	0.724		0.336	0.804	0.685	0.100
	0.159		0.297	0.276	0.919	0.363
	0.442		0.618	0.001	0.013	0.043
	0.329		0.677	0.461	0.598	0.894
GBR	0.495	0.744		0.266	0.368	0.397
	0.718	0.400		0.554	0.451	0.691
	0.201	0.336		0.246	0.637	0.007
	0.010	0.297		0.900	0.376	0.906
	0.778	0.618		0.000	0.004	0.012
	0.552	0.677		0.718	0.855	0.749
ITA	0.653	0.136	0.266		0.894	0.839
	0.803	0.115	0.554		0.788	0.269
	0.923	0.804	0.246		0.537	0.199
	0.012	0.276	0.900		0.346	0.817
	0.000	0.001	0.000		0.541	0.130
	0.839	0.461	0.718		0.894	0.499
SVN	0.779	0.215	0.368	0.894		0.949
	0.635	0.118	0.451	0.788		0.240
	0.472	0.685	0.637	0.537		0.054
	0.146	0.919	0.376	0.346		0.446
	0.002	0.013	0.004	0.541		0.436
	0.750	0.598	0.855	0.894		0.652

Notes: The table shows p-values from tests against the null hypothesis that coefficients across countries (comparing countries listed in the first column to those in the first rows) are equal. The table refers to the country-wise regressions shown in the Table above and in the main text. Rows 1-5 for each country show the test for the coefficient for the variables Low income, High income, Financial security, Attitudes (towards) immigrants, Social mobility, and Trust in government, respectively. Tests against equality of coefficients were calculated using seemingly unrelated estimations (STATA's suest command).

Table 1.12: Probit Regressions

	(1) Pooled	(2) Pooled	(3) Pooled	(4) Pooled	(5) Pooled	(6) Pooled
Low income	0.011	0.006	0.013	0.010	0.010	
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	
High income	0.060	0.058	0.072	0.060	0.070	
	(0.05)	(0.05)	(0.05)	(0.05)	(0.05)	
GER	0.310****	0.325****	0.311****	0.327****	0.325****	0.299****
	(0.06)	(0.06)	(0.06)	(0.06)	(0.07)	(0.06)
GBR	0.302****	0.300****	0.269****	0.286****	0.268****	0.209***
	(0.06)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
ITA	-0.015	-0.043	-0.059	-0.043	-0.045	-0.081
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
SVN	0.359****	0.320****	0.288****	0.357****	0.343****	0.276****
	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)	(0.07)
JPN	0.169***	0.162***	0.116**	0.138**	0.109*	0.117**
	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
Financial security	-0.0683****	-0.0539****	-0.0399****	-0.0613****	-0.0450****	-0.0459****
	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Trust in government		-0.0340****	-0.0241***	-0.0386****	-0.0297****	-0.0297****
		(0.01)	(0.01)	(0.01)	(0.01)	(0.01)
Social mobility beliefs			-0.0393****		-0.0418****	-0.0398****
			(0.01)		(0.01)	(0.01)
Attitudes immigrants				0.0204**	0.0254***	0.0239***
				(0.01)	(0.01)	(0.01)
Constant	0.917****	0.970****	1.083****	0.917****	0.997****	1.158****
	(0.11)	(0.11)	(0.11)	(0.12)	(0.12)	(0.08)
Obs.	7854	7762	7577	7125	7025	7026
R2 McFadden	0.0242	0.0265	0.0296	0.0275	0.0311	0.0268
Log-lik.	-3916.5	-3847.2	-3747.8	-3568.5	-3505.7	-3521.3

Notes: The table shows probit regression results with the binary dependent variable indicating progressive taxation preferences (1 = progressive preferences, i.e., preferred tax rates for higher income groups at least as large (T1 >= T9 >= T40 >= T50) as for lower income groups with strictly larger in at least one comparison). Robust standard errors in parentheses. All regressions contain controls for age, gender, education, and labor force status. Same independent variables are included as in main text regressions.

Table 1.13: Pooled Sample Regression with T1 and T50 as Dependent Variable

	(1) T1	(2) T1	(3) T50	(4) T50
Female	0.576	0.616	-0.062	-0.119
	(0.42)	(0.45)	(0.28)	(0.31)
Age/10	0.288*	0.295*	-0.258**	-0.233**
	(0.16)	(0.17)	(0.11)	(0.12)
Migration	-1.094	-1.522*	1.173**	1.235**
	(0.68)	(0.78)	(0.50)	(0.57)
Low income	0.496	0.848	0.047	-0.076
	(0.58)	(0.64)	(0.39)	(0.44)
High income	-0.576	-0.131	0.000	-0.248
	(0.58)	(0.61)	(0.39)	(0.41)
Self-employed	2.318***	1.367*	-1.470***	-0.939*
	(0.72)	(0.78)	(0.50)	(0.55)
Unemployed	0.581	0.250	-0.299	-0.462
	(0.83)	(0.92)	(0.59)	(0.68)
Inactive	0.978*	0.716	-1.102****	-1.155****
	(0.50)	(0.53)	(0.33)	(0.35)
Low education	-1.186**	-0.795	0.733*	0.664
	(0.57)	(0.62)	(0.40)	(0.43)
High education	-0.0137	0.154	-0.366	-0.373
	(0.56)	(0.60)	(0.38)	(0.40)
GER	4.439****	4.442****	-3.180****	-2.889****
	(0.74)	(0.74)	(0.51)	(0.51)
GBR	3.555****	3.079****	-1.943****	-1.386**
	(0.77)	(0.79)	(0.54)	(0.56)
ITA	-0.019	-0.172	0.169	0.642
	(0.76)	(0.76)	(0.54)	(0.54)
SVN	4.354****		-3.045****	
	(0.84)		(0.57)	
JPN	1.652**	1.189	-0.349	0.401
	(0.75)	(0.75)	(0.51)	(0.51)
Financial security	-0.448****	-0.438****	0.303****	0.293****
	(0.12)	(0.12)	(0.08)	(0.09)
Trust in government	-0.624****	-0.562****	0.330****	0.280****
	(0.09)	(0.10)	(0.07)	(0.07)
Social mobility beliefs	-0.762****	-0.817****	0.424****	0.401****
	(0.10)	(0.11)	(0.07)	(0.07)
Attitudes immigrants	0.206*		-0.274****	
	(0.11)		(0.07)	
Racial prejudices		-0.534****		0.438****
		(0.09)		(0.07)
Constant	46.63****	50.30****	14.33****	10.68****
	(1.47)	(1.47)	(1.06)	(1.04)
Obs.	7025	5907	7025	5907
R2	0.0516	0.0649	0.0394	0.0504
Adj. R2	0.0491	0.0620	0.0368	0.0475

Table 1.13: Pooled Sample Regression with T1 and T50 as Dependent Variable

	(1)	(2)	(3)	(4)
	T1	T1	T50	T50

Notes: The table shows OLS regressions. Robust standard errors in parentheses. The dependent variable in the first two columns is the preferred tax rate for the top 1 percent and the preferred tax rate on the bottom 50 percent in the last two columns. Female is a dummy variable equal to one if the respondent is female. Age/10 is the age in years divided by ten. Migration is a dummy indicating a migration background (either the respondent or parents immigrated). Low (high) income indicates an income in the first (last) two quartiles. Self-employed, unemployed, and inactive are indicator variables for the respondent's labor force status. Employed serves as the base category. The remaining variables are country dummies or refer to survey measures.

Table 1.14: Additional Pooled Sample Regressions

	(1) Pooled	(2) Pooled	(3) Pooled	(4) Pooled	(5) Pooled	(6) Pooled
Female	0.546	1.093*	0.790	0.871	0.815	0.239
	(0.64)	(0.63)	(0.63)	(0.63)	(0.68)	(0.72)
Age/10	0.619**	0.701***	0.557**	0.716***	0.631**	0.744***
	(0.25)	(0.24)	(0.24)	(0.24)	(0.26)	(0.27)
Migration	-1.685	-1.568	-1.625	-1.800*	-2.554**	-2.847**
	(1.06)	(1.06)	(1.06)	(1.05)	(1.19)	(1.22)
Low income	0.285	0.664	0.481	0.533	0.941	1.102
	(0.88)	(0.87)	(0.86)	(0.86)	(0.96)	(1.02)
High income	-0.912	-0.595	-0.763	-0.711	-0.021	-0.151
	(0.87)	(0.86)	(0.86)	(0.86)	(0.91)	(0.96)
Self-employed	3.737****	3.599***	3.630***	3.413***	2.226*	2.066*
	(1.12)	(1.11)	(1.11)	(1.11)	(1.21)	(1.25)
Unemployed	0.868	0.993	0.926	1.280	0.986	0.631
	(1.28)	(1.28)	(1.26)	(1.25)	(1.43)	(1.53)
Inactive	1.838**	2.164***	2.023***	1.946***	1.758**	1.820**
	(0.76)	(0.75)	(0.75)	(0.75)	(0.80)	(0.84)
Low education	-1.889**	-2.228**	-1.837**	-1.894**	-1.511	-1.928*
	(0.88)	(0.87)	(0.87)	(0.86)	(0.94)	(1.01)
High education	0.540	0.157	0.518	0.552	0.544	0.213
	(0.85)	(0.84)	(0.84)	(0.83)	(0.90)	(0.95)
GER	6.454****	6.532****	6.221****	6.652****	6.745****	5.711****
	(1.12)	(1.12)	(1.12)	(1.10)	(1.13)	(1.17)
GBR	4.435****	4.288****	4.021****	4.600****	3.905***	3.874***
	(1.20)	(1.19)	(1.19)	(1.18)	(1.23)	(1.28)
ITA	-0.176	-0.650	-0.648	-0.290	-0.794	-1.735
	(1.16)	(1.16)	(1.15)	(1.14)	(1.17)	(1.24)
SVN	7.104****	5.267****	6.001****	6.673****		
	(1.30)	(1.25)	(1.28)	(1.25)		
JPN	1.094	1.848	0.977	1.446	0.521	0.935
	(1.16)	(1.14)	(1.14)	(1.13)	(1.15)	(1.20)
Financial security	-0.630****	-0.640****	-0.678****	-0.658****	-0.668****	-0.620***
	(0.18)	(0.17)	(0.17)	(0.17)	(0.19)	(0.20)
Trust in government	-0.858****	-0.954****	-0.810****	-0.908****	-0.817****	-0.731****
	(0.15)	(0.15)	(0.14)	(0.14)	(0.15)	(0.16)
Social mobility beliefs	-1.101****	-1.106****	-1.091****	-1.147****	-1.205****	-1.124****
	(0.15)	(0.15)	(0.15)	(0.15)	(0.16)	(0.17)
Attitudes immigrants	0.478***					
	(0.17)					
Trust in immigrants		0.469***				
		(0.17)				
Immigrants integration			0.120			
			(0.15)			
Immigrants culture				0.433****		
				(0.12)		
Racial prejudices					-0.919****	-0.682****
					(0.14)	(0.16)
Religion importance	-0.316***	-0.397****	-0.388****	-0.356****	-0.231**	-0.115
	(0.10)	(0.10)	(0.10)	(0.10)	(0.11)	(0.11)

Table 1.14: Additional Pooled Sample Regressions

	(1) Pooled	(2) Pooled	(3) Pooled	(4) Pooled	(5) Pooled	(6) Pooled
Risk aversion	0.453***					
	(0.14)					
Political orientation						-1.255****
						(0.19)
Constant	30.34****	32.78****	35.04****	32.61****	39.67****	43.89****
	(2.49)	(2.20)	(2.21)	(2.21)	(2.26)	(2.38)
Obs.	6873	7043	7032	7070	5807	5169
R2	0.0588	0.0574	0.0557	0.0594	0.0719	0.0883
Adj. R2	0.0560	0.0547	0.0530	0.0567	0.0688	0.0847

Notes: The table shows OLS regressions. Robust standard errors in parentheses. The dependent variable is the difference between preferred tax rates on the top 1 percent and the bottom 50 percent. Migration is a dummy indicating a migration background (either the respondent or parents immigrated). Low (high) income indicates an income in the first (last) two quartiles. Self-employed, unemployed, and inactive are indicator variables for the respondent's labor force status. Employed serves as the base category. The remaining variables are country dummies or refer to survey measures.

Table 1.15: Exploring Various Measures of Trust in Government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Female	0.638	0.733	0.689	0.595	0.852	0.490	0.758	0.962
	(0.63)	(0.64)	(0.65)	(0.64)	(0.64)	(0.64)	(0.64)	(0.63)
Age/10	0.545**	0.438*	0.462*	0.455*	0.486**	0.485**	0.482*	0.497**
	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.25)	(0.24)
Migration	-2.268**	-2.406**	-2.283**	-2.387**	-2.509**	-2.678**	-2.485**	-2.557**
	(1.06)	(1.07)	(1.09)	(1.08)	(1.08)	(1.08)	(1.07)	(1.05)
Low income	0.449	0.432	0.067	0.484	0.610	0.482	0.475	0.54
	(0.87)	(0.89)	(0.90)	(0.90)	(0.89)	(0.89)	(0.88)	(0.87)
High income	-0.576	-0.556	-0.593	-0.525	-0.443	-0.588	-0.452	-0.397
	(0.87)	(0.87)	(0.89)	(0.88)	(0.87)	(0.88)	(0.87)	(0.86)
Self-employed	3.788****	4.120****	4.204****	3.897****	3.800****	3.512***	4.050****	3.789****
	(1.11)	(1.13)	(1.14)	(1.13)	(1.12)	(1.12)	(1.12)	(1.11)
Unemployed	0.880	1.520	0.816	0.865	0.818	0.859	1.110	0.928
	(1.28)	(1.30)	(1.38)	(1.32)	(1.33)	(1.30)	(1.30)	(1.28)
Inactive	2.081***	2.170***	2.657****	2.224***	2.137***	1.966**	2.167***	2.080***
	(0.75)	(0.76)	(0.78)	(0.76)	(0.76)	(0.77)	(0.76)	(0.75)
Low education	-1.919**	-2.046**	-1.766*	-2.162**	-1.887**	-2.391***	-1.915**	-1.914**
	(0.87)	(0.88)	(0.91)	(0.89)	(0.89)	(0.88)	(0.88)	(0.87)
High education	0.353	0.117	0.242	0.124	0.077	0.0514	0.315	0.111
	(0.85)	(0.86)	(0.88)	(0.87)	(0.86)	(0.86)	(0.85)	(0.85)
GER	7.618****	7.468****	6.989****	7.298****	7.407****	7.450****	7.114****	6.160****
	(1.09)	(1.11)	(1.14)	(1.12)	(1.11)	(1.11)	(1.10)	(1.10)
GBR	5.498****	5.919****	5.896****	6.012****	5.789****	5.990****	5.365****	5.274****
	(1.16)	(1.18)	(1.21)	(1.18)	(1.19)	(1.19)	(1.17)	(1.16)
ITA	-0.188	-0.320	-0.221	-0.311	-0.532	-0.172	-0.185	-1.177
	(1.16)	(1.19)	(1.23)	(1.18)	(1.18)	(1.17)	(1.18)	(1.20)
SVN	7.399****	7.284****	7.173****	7.026****	7.095****	7.164****	7.284****	7.168****
	(1.28)	(1.30)	(1.34)	(1.29)	(1.30)	(1.29)	(1.30)	(1.29)
JPN	2.001*	2.041*	1.263	1.641	1.484	1.907*	1.251	1.753
	(1.13)	(1.15)	(1.17)	(1.15)	(1.14)	(1.14)	(1.15)	(1.13)
Financial security	-0.751****	-0.902****	-0.952****	-0.918****	-0.777****	-0.871****	-0.923****	-0.855****

Table 1.15: Exploring Various Measures of Trust in Government

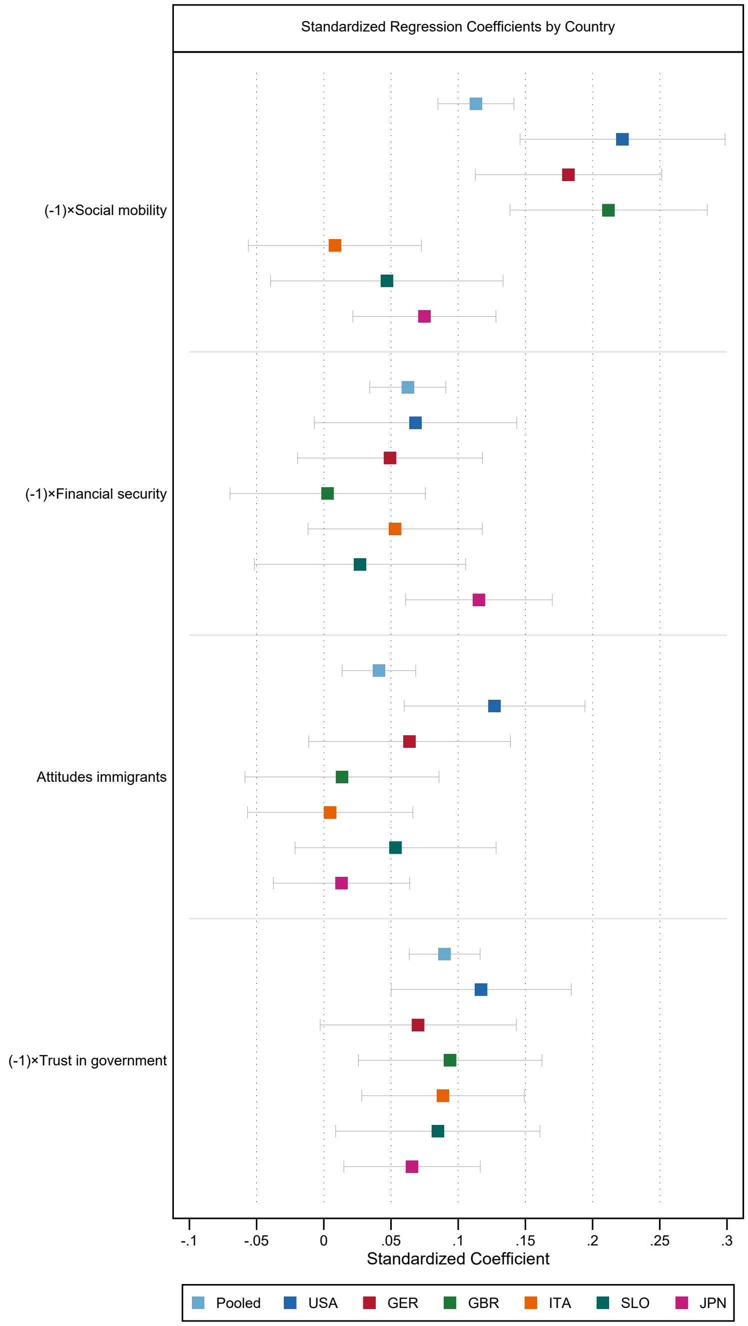
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	(0.17)	(0.17)	(0.18)	(0.18)	(0.18)	(0.18)	(0.18)	(0.17)
Trust in government	-0.954****							
	(0.14)							
Government reliability		-0.584****						
		(0.16)						
Government responsiveness			-0.410***					
			(0.16)					
Government integrity				-0.803****				
				(0.16)				
Government openess					-0.927****			
					(0.15)			
Government fairness						-0.793****		
						(0.14)		
Trust civil servants							-0.350**	
							(0.15)	
Trust financial institutions								-0.792****
								(0.15)
Social mobility beliefs	-1.186****	-1.290****	-1.353****	-1.237****	-1.274****	-1.227****	-1.362****	-1.245****
	(0.15)	(0.15)	(0.16)	(0.15)	(0.15)	(0.15)	(0.15)	(0.15)
Attitudes immigrants	0.479***	0.411**	0.406**	0.496***	0.543***	0.449***	0.374**	0.417***
	(0.16)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.17)	(0.16)

Table 1.15: Exploring Various Measures of Trust in Government

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Constant	32.30****	33.61****	33.36****	33.99****	33.13****	33.65****	32.92****	34.01****
	(2.28)	(2.34)	(2.40)	(2.34)	(2.32)	(2.31)	(2.32)	(2.31)
Obs.	7025	6890	6643	6781	6833	6854	6966	7030
R2	0.0553	0.0526	0.0511	0.0543	0.056	0.0551	0.0508	0.0537
Adj. R2	0.0528	0.0500	0.0483	0.0516	0.0534	0.0525	0.0482	0.0512

Notes: The table shows OLS regressions exploring the correlation of preferences for redistribution with various measures of trust in the government and its institutions. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. The dependent variable is the difference between preferred tax rates on the top 1 percent and the bottom 50 percent. Migration is a dummy indicating a migration background (either the respondent or parents immigrated). Low (high) income indicates an income in the first (last) two quartiles. Self-employed, unemployed, and inactive are indicator variables for the respondent's labor force status. Employed serves as the base category. The remaining variables are country dummies or refer to survey measures.

Figure 1.8: Standardized Coefficients by Country



Notes: This figure shows standardized (X-Y-standardization) regression coefficients from country-wise subsample regressions using the same model as in column (5) of the main regression results table. Standardization (substract mean and divide by standard deviation) within each subsample. The interpretation is that a one standard-deviation increase of the independent variable in the respective country is associated with change in standard deviations of the dependent variable.

1.2.3 Questions from the Trustlab

Table 1.16: Definitions, Questions, and Value Ranges from the Trustlab Questionnaire

Variable	Survey Question or Definition	Value Range
Altruism survey	How willing are you to give to good causes without expecting anything in return?	0 (Completely unwilling to do so) - 10 (Very willing to do so)
Donation	Voluntary donation to UNICEF after completion of Trustlab	0-40 USD
Risk willingness survey	How do you see yourself: are you generally a person who tries to avoid taking risks, or are you fully prepared to take risks?	0 (Generally unwilling to take risks) – 10 (Fully prepared to take risks)
Risk aversion	10 – Risk willingness (to inverse the meaning)	0-10
Pos. reciprocity survey	When someone does me a favor, I am willing to return it'. How well does this statement describe you as a person?	0 (Completely unwilling to do so) -10 (Very willing to do so)
Neg. reciprocity survey	How willing are you to punish someone who treats others unfairly, even if there may be costs for you?	0 (Completely unwilling to do so) -10 (Very willing to do so)
Age	Age in years	Number of years
Female	Gender dummy	1 (Female), 0 (Male/Other)
Respondent born in country	Respondent born in country dummy	1 (yes), 0 (no)
One or more parents born outside	One or more parents born outside the country dummy	1 (yes), 0 (no)
Migration (background)	Dummy for respondent born outside the country and/or one or more parents born outside the country	1 (yes), 0 (no)
Education	What is the highest level of education you have completed?	0 (Less than high school); 1 (High school or less); 2 (Some college); 3 (Diploma, trades certificate or other post school qualification other than university); 4 (Undergraduate degree (e.g., BA, BS)); 5 (Post-graduate degree)
Low education	Highschool or less (1)	
High education	Tertiary degree (4-5)	
Household income	In the last 12 months, what was the total income of your household after (before in the U.S.) taxes have been deducted? (Income can come salaries and wages, profit from self-employment, interest, rent, pension, social insurance payments and other benefits, among others)	Number in USD

Table 1.16: Definitions, Questions, and Value Ranges from the Trustlab Questionnaire

Variable	Survey Question or Definition	Value Range
Household income quintile	Just to confirm, which of these income bands corresponds best to your household income? Remember, we are asking for your household income, after taxes have been deducted. [Choice of 5 even quintiles based on country income distribution]	1 (first quintile) – 5 (last quintile)
Labor force status	Which of these bests describes your situation? [Employee], [Employer/self-employed], [Unemployed] or [Outside the labor force (e.g., homemaker, student, retired, unable to work)]	0 (Employee); 1 (Employer/self-employed); 2 (Unemployed); 3 (Outside the labor force)
Job sector	Do you currently work in the. . . ? [Central, regional or local government administration], [Public sector], [Private (for profit) sector], [Not for profit sector] or [Not applicable]	0 (Administration); 1 (Public); 2 (Private); 3 (Non-profit); 4 (Not applicable)
Social mobility (beliefs)	Some people say there is not much opportunity to get ahead today for the average person. Others say anyone who works hard can climb up the ladder. Which one comes closer to the way you feel about this?	0 (There is not much opportunity) -10 (There is plenty of opportunity)
Financial security	When it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?	0 (Worse) – 5 (The same) – 10 (Better)
Job security “keep current job”	How likely do you think it is that you will still have a job in 6 months (if you have one now)?	0 (Very unlikely) – 10 (Very likely)
Job security “find new job”	If you were to lose your job, how likely is it that you would find a job with a similar salary within 6 months?	0 (Very unlikely) – 10 (Very likely)
Values, attitudes, and views		
Racial prejudices	On the average Blacks/African Americans have worse jobs, income, and housing than white people. Do you think the differences are mainly due to discrimination and disadvantages of educational opportunity, mainly due to differences in in-born ability, motivation, and effort, or some combination?	0 (Mainly discrimination and lack of opportunity – 10 (Mainly lesser ability, motivation and effort)
Perceived diversity	How high do you estimate the percentage of people of non-[Country] origin in your neighborhood to be? With non-[Country] origin we mean people who	0-100 (Percentage)

Table 1.16: Definitions, Questions, and Value Ranges from the Trustlab Questionnaire

Variable	Survey Question or Definition	Value Range
	were not born in [Country] or of whom at least one parent was not born in [Country]. Please give a percentage between 0 and 100.	
Political orientation	In political matters, people often talk of “the left” and “the right.” How would you place your views on this scale, generally speaking?	0 (Left) – 5 (Center) – 10 (Right)
Political inefficacy	To what extent do you agree with the following statement: ‘People like me don’t have any say about what the government does?’	0 (I don’t agree at all) – 10 (I completely agree)
Immigrants: Integration	To what extent do you agree with the following statements?	0 (‘Immigrants are not integrated in our society’) – 10 (‘Immigrants are well integrated in our society’)
Immigrants: Culture	To what extent do you agree with the following statements?	0 (‘Our culture is undermined by immigrants’) – 10 (‘Our culture is enriched by immigrants’)
Attitudes towards immigrants	Unweighted average of Immigrants: Integration and Culture	0 - 10
Importance of religion	How important would you say religion is in your own life?	0 (Not important at all) – 10 (Very important)
Trust in others 1	In general, how much do you trust most people? (OECD question)	0 (Not at all) – 10 (Completely)
Trust in others 2	Generally speaking, would you say that most people can be trusted, or that you can’t be too careful in dealing with people? (Rosenberg)	0 (You can’t be too careful) – 10 (Most people can be trusted)
Trust financial institutations	How much trust do you have in financial institutions (e.g., banks)?	0 (I don’t trust them at all) – 10 (I completely trust them)
Trust in immigrants	How much do you trust people who immigrated?	0 (I don’t trust them at all) – 10 (I completely trust them)
Trust civil servants	How much trust do you have in civil servants?	0 (I don’t trust them at all) – 10 (I completely trust them)
Trust in government	How much trust do you have in the government?	0 (I don’t trust them at all) – 10 (I completely trust them)
Government’s reliability	Public institutions deliver public services in the best possible way.	0 (I don’t agree at all) – 10 (I completely agree)
Government’s responsiveness	Public institutions pursue long term objectives.	0 (I don’t agree at all) – 10 (I completely agree)
Government’s integrity	People working in public institutions are ethical and not corrupt.	0 (I don’t agree at all) – 10 (I completely agree)

Table 1.16: Definitions, Questions, and Value Ranges from the Trustlab Questionnaire

Variable	Survey Question or Definition	Value Range
Government's openness	Public institutions are transparent.	0 (I don't agree at all) – 10 (I completely agree)
Government's fairness	Public institutions treat all citizens fairly, regardless of their gender, race, age or economic condition.	0 (I don't agree at all) – 10 (I completely agree)
Preferred tax rates	The government currently raises a certain amount of revenues through tax in order to sustain the current level of public spending. In your view, what would be the fair split of tax burden to sustain public spending? Each slider	
Tax Top 1%	represents a segment of the population with a different income. For	0-75
Tax Next 9 %	example, the top 1% represents a small group of rich people,	0-75
Tax Next 40%	whereas the bottom 50% is the half of the population that earns the least.	0-75
Tax Bottom 50%	Please use the sliders below to tell us how much you think each of the following groups should pay as a percentage of their available resources.	0-75

Chapter 2

Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis

Christoph Schütt[a] ⓡ David Pipke[b] ⓡ Lena Detlefsen[c] ⓡ Gianluca Grimalda[d]

An earlier version of this paper is available at http://hdl.handle.net/10419/261386 (permalink) as Schütt, C., Pipke, D., Detlefsen, L., & Grimalda, G. (2022). Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis, Kiel Working Paper, No. 2228, Kiel Institute for the World Economy (IfW Kiel), Kiel.

The order of authors' names has been randomized using the AEA Author Randomization Tool (Confirmation code: 7515hDn59G8z). We are very grateful for helpful and constructive comments from Erzo F.P. Luttmer. The Mercator Foundation has funded this study as part of the Mercator Dialogue on Asylum and Migration (MEDAM). We thank Esther Ademmer for her contributions to the study's design.

[a]Kiel Institute for the World Economy & Helmut-Schmidt-Universität / Universität der Bundeswehr Hamburg (christoph.schuett@ifw-kiel.de)
[b]Kiel Institute for the World Economy (david.pipke@ifw-kiel.de)
[c]Kiel Institute for the World Economy (lena.detlefsen@ifw-kiel.de)
[d]Kiel Institute for the World Economy (gianluca.grimalda@ifw-kiel.de)

Abstract

Ethnic discrimination is ubiquitous and has been shown to exert adverse effects on income redistribution. The reason is that a country's ethnic majority, if richer than the average, may be unwilling to transfer resources to the country's ethnic minorities if poorer than the average. A yet untested mechanism is that a country's ethnic majority may reduce their work effort knowing that their income will finance redistribution to ethnic minorities. We test for this mechanism experimentally in triadic interactions. A German citizen acting as a worker is randomly matched with a recipient who can be another German, an economic migrant, or an asylum seeker in Germany. Workers know that another German citizen may transfer part of their earnings to the recipient. The recipient does not exert any work effort. Even if the recipient's identity does not affect effort in the aggregate, social identity strongly moderates this relationship. Participants with a strong German identity, i.e., who report feeling close to other Germans, exert significantly less effort than other participants if the recipient is an asylum seeker. They also exert more effort when matched with a German recipient than an asylum seeker, while participants with a less strong German identity do the opposite. Moreover, participants with a strong German identity exert slightly more effort when matched with economic migrants than with asylum seekers, while others tend to do the opposite, albeit statistically insignificantly. Workers' beliefs over the third party's redistribution rate do not mediate such results and are generally inaccurate.

JEL Codes: C91, H23, I31, J15, J30

Keywords: Redistribution, Discrimination, Taxes, Beliefs, Real effort, Experiment

2.1 Introduction

Since the influential work by Alesina and Glaeser (2004), it has been argued that increased ethnic heterogeneity may lead to less income redistribution and shrinking welfare states. The reason is that ethnic minorities, such as Blacks or Hispanics in the U.S., typically occupy the lower tiers of the income distribution. Ethnic majorities, such as Whites in the U.S., who are motivated by aversion toward ethnic minorities, will thus demand low redistribution to provide little benefit to ethnic minorities. The greater ethnic homogeneity in Europe compared to the U.S. thus partly explains the higher redistribution rates in the former than the latter. This hypothesis has received extensive empirical support (Luttmer, 2001; Alesina and Giuliano, 2011; Alesina et al., 2021c), by carrying out online-survey experiments (Alesina et al., 2021a, 2022) or by exploiting exogenous migrant placement policies (Dahlberg et al., 2012).[1] Besides, another strand of the literature finds ethnic diversity associated with lower quantity and quality of public goods provision (Alesina et al., 1999; Algan et al., 2016; Tabellini, 2020). One of the leading explanations proposed for these findings is ingroup favoritism or group loyalty effects (Luttmer, 2001). The rationale behind this concept is that people may attach a higher value to the well-being of their "ingroup," the group to which they feel connected, as compared to others (the "outgroup") (Tajfel et al., 1971; Brewer, 1999). Group loyalty effects have been extensively studied in the (socio-)psychological and recent economic literature (Balliet et al., 2014; Romano et al., 2017, 2021a).[2] However, the literature remains largely silent on whether outgroup members among potential welfare state beneficiaries may even lead to a withdrawal of working effort by the native population.

Our paper aims to fill this gap in the literature. We report evidence from an experiment testing whether people's work commitment in a real-effort task is affected by the migration status of potential beneficiaries of earning redistribution. Participants from a University student pool (the "workers") of German citizenship could earn money depending on their performance in a real-effort task (Gill and Prowse, 2012). Participants were informed that a third-party allocator (the "allocator" in the following) would be able to transfer part of their earnings to another person (the "recipient"). The allocator could choose any tax rate from 0% (in which case the initial earnings would be earned in full by the worker) to 100% (in which case all of the worker's earnings would be transferred to the recipient). This setting mimics a vastly simplified - and rather extreme at the high end of redistribution - version of a welfare state. The experimental design is similar to the first phases in Cappelen et al. (2013) and Almås et al. (2020).

[1] See the survey of the literature by Stichnoth and van der Straeten (2013).

[2] See Anderson et al. (2006) and Cooper and Kagel (2016) for reviews of the literature.

Each participant performed a slider task (Gill and Prowse, 2012) for three rounds. In a between-subject design, we used three treatments varying whether the recipient was (i) a German citizen, (ii) an asylum seeker, or (iii) an economic migrant. In this non-strategic interaction, the recipient could not influence the worker's payoffs. Hence, the design allows studying preference-based group effects independently of beliefs about whether actions could be reciprocated in the future.[3]

The optimal strategy for a self-interested worker is always to perform the highest possible effort compatible with the marginal disutility of effort - which should not vary over treatments. We hypothesized, nevertheless, that workers would be more inclined to exert higher effort when the recipient is a fellow country person rather than an immigrant (Hypothesis 1). This would be the case if ingroup favoritism (based on nationality) applied to effort levels similar to what has been observed in prior research (see literature cited above). Our second hypothesis was that the higher the expected tax rate, the lower workers' effort (Hypothesis 2).

While our analyses do not reveal any group effects on task performance in the aggregate, we find results consistent with our hypotheses after splitting the sample according to a simple measure of workers' identification with Germans, i.e., their *national identification*. This measure is based on a simple question where we asked participants to state how close they feel to Germans. We divide the sample into those who report feeling "very close" or "close" to Germans[4] ("*Close*" henceforth) and those who do not ("*Non-Close*" henceforth). As noted by Fong and Luttmer (2009), a question on subjective closeness is likely to be less prone to social desirability bias than other commonly used questions on racial or ethnic identification, where subjects might feel reluctant to reveal an aversion against a specific group of people. We find that *Close* participants exert significantly less effort than *Non-Close* participants if the recipient is an asylum seeker. Moreover, *Close* participants exert more effort when matched with a German recipient than an asylum seeker, while *Non-Close* participants do the opposite. Although the latter two results are either weakly significant or at the margins of significance, the difference of the difference is statistically significant. Moreover, *Close* and *Non-Close* participants also seem to differ in the way they treat economic migrants and asylum seekers. *Close* participants exert slightly more effort when matched with economic migrants than with asylum seekers, while *Non-Close* participants tend to do the opposite, albeit at statistically insignificant levels. The difference of the difference is, in this case, weakly significant. Contrary to Hypothesis 2, workers' beliefs over the

[3]See Everett et al. (2015) for an extensive review on the role of beliefs and preferences in explaining prosocial behavior and Durrheim et al. (2016) for the role of expectations of ingroup reciprocity.

[4]We use the answer score to the question "How close are the following groups to you?" which could range from 1 "very close" to 5 "very distant". Groups were "People in your town", "Germans", "Europeans", and "Peope all over the world". See Appendix.

degree of redistribution are not related to effort. However, it is noticeable that *Close* participants expect relatively larger shares of their earnings to be redistributed toward an asylum seeker than *Non-Close* participants. This expectation is wrong as allocators transfer significantly more to German recipients than asylum seekers (Grimalda et al., 2022a). While workers from the *Non-Close* group correctly anticipate that German recipients will benefit most from redistribution, they grossly under-estimate the actual size of the transfers accruing to asylum seekers. Overall, these results entail that the discrimination effect we find in the *Close* group primarily by a decrease in pure altruism toward asylum seekers, rather than statistical discrimination (Becker, 1971), where statistical discrimination in this setting entails a belief that the allocator will mostly favor redistribution toward asylum seekers.

From a broad perspective, our paper contributes to the vast literature shedding light on the relevance of ethnic diversity for preferences for redistribution (Alesina and Glaeser, 2004; Fong and Luttmer, 2009; Alesina and Giuliano, 2011; Alesina et al., 2021c, 2022) and public goods provision (Alesina et al., 1999; Algan et al., 2016; Tabellini, 2020). We add to this literature finding that people with a strong ingroup identification even act against their monetary self-interest by exerting less effort if potential beneficiaries of the welfare system are from an outgroup. Our results extend those from Hedegaard and Tyran (2018) to a different context, who find that entrepreneurs prefer selecting workers from their nationality rather than from a different nationality even when the former has lower productivity than the latter, thus reducing their expected profits. Our study also contributes to the experimental literature on labor market relationships, which shows that productivity may be affected by transient mood and states of happiness (Oswald et al., 2015). Our paper is also closely related to the literature on self-image derived from membership in a social group (Tajfel et al., 1971; Turner et al., 1979; Tajfel, 1982), which has been extensively studied in social psychology and sociology (Tajfel, 1982; Brewer, 1999) before it was introduced to the economics literature by Akerlof and Kranton (2000). Various studies in the experimental-economic literature have documented group effects in dictator and two-person response games (Chen and Li, 2009; Ockenfels and Werner, 2014; Tanaka and Camerer, 2016; Abbink and Harris, 2019), coordination games (Goette et al., 2006; Charness et al., 2007; Charness and Rustichini, 2011; Guala et al., 2013), trust games (Hargreaves Heap and Zizzo, 2009; Slonim and Guillen, 2010; Falk and Zehnder, 2013), (third-party) punishment games (Bernhard et al., 2006; Goette et al., 2006; Abbink et al., 2010), contests (Abbink et al., 2010; Chakravarty et al., 2016) and variants of public goods games (Tajfel et al., 1993; Solow and Kirkwood, 2002; Eckel and Grossman, 2005; Croson et al., 2008; Charness et al., 2014) in which members of the ingroup are typically treated preferentially compared to outgroup members. These effects are generally found to be present in minimal groups that are assigned completely randomly (Tajfel et al., 1971; Chen and Li, 2009; Sutter, 2009) or artificially enhanced (e.g., by performing a common task such as puzzle-solving or identifying paintings) (Eckel and Grossman, 2005; Chen and Li, 2009; Hargreaves Heap and Zizzo, 2009; Rong et al., 2016) and as in naturally occurring groups that may be based on gender, ethnicity, religious affiliation, or membership in universities, organizations, or political parties (Fershtman and Gneezy, 2001; Bernhard et al., 2006; Goette et al., 2006; Croson et al., 2008; Charness and Rustichini, 2011; Falk and Zehnder, 2013; Ockenfels and Werner, 2014; Kranton and Sanders, 2017; Abbink and Harris, 2019). Our contribution to this literature is twofold, as our study involves naturally occurring groups, namely German citizens, asylum seekers, and economic migrants, in a situation akin to a welfare state. In addition, we methodologically extend the research on group effects, contributing to the growing literature that utilizes real-effort tasks in the lab (for a comparison of stated effort and real effort methods see Charness et al. (2018)), instead of the formerly dominating approach using stated costly effort (Fehr et al., 1993, 1998a). To the best of our knowledge, this is the first study on whether real effort in the laboratory depends on the characteristics of potential beneficiaries knowing that part of one's earnings is subject to redistribution.

The paper is structured as follows. Section 2.2 outlines the experimental design and the theoretical background. Section 2.3 presents the results. Section 2.4 discusses the findings and concludes.

2.2 Experimental design and theoretical background

The experiment took place during ten sessions in the laboratory for experimental economics at the University of Kiel. Four sessions in the same laboratory took place in September and October 2019 and two sessions in January 2020, thus ensuring participants used the same technical devices in all sessions. All participants attended only one session.

The experiment discussed in this paper is part of a research project on preferences for redistribution for which hypotheses and analysis plans were pre-registered in the OSF Registries (available at https://osf.io/xj7tf). Even if the hypotheses for this experiment were not pre-registered, we would view them as straightforward extensions of existing theories and evidence. They are ultimately in line with the project's overall hypotheses.

The sample comprises 172 students from the University of Kiel acting as workers. 86 participants identified as females, 85 as males, and one as non-binary. The mean

Table 2.1: Balance Table

	Asylum seeker	German	Economic migrant	Total	F-test
Female	0.564	0.410	0.534	0.500	0.207
	(0.067)	(0.063)	(0.066)	(0.038)	
Age in years	26.164	25.508	25.483	25.707	0.684
	(0.663)	(0.600)	(0.511)	(0.341)	
Dual citizenship	0.073	0.033	0.052	0.052	0.626
	(0.035)	(0.023)	(0.029)	(0.017)	
Born in Germany	0.909	0.951	0.983	0.948	0.190
	(0.039)	(0.028)	(0.017)	(0.017)	
Political orientation	2.527	2.508	2.569	2.534	0.854
	(0.068)	(0.086)	(0.074)	(0.044)	
Closeness	2.309	2.339	2.362	2.337	0.947
	(0.103)	(0.110)	(0.127)	(0.066)	

Notes: The table shows background characteristics for the participants in our experiment. Means and standard errors (in parentheses) reported. "Female" is the average share of females. "Age in years" is the average age in years. "Dual citizenship" is the share of participants holding a dual citizenship. "Born in Germany" is the share of participants born in Germany. "Political orientation" is ranging from 1 (very left) to 5 (very right). "Closeness" is a measure of closeness to Germans, ranging from 1 (very close) to 5 (very distant). The last column reports p-values from an F-test of joint significance in a regression of background characteristics on treatment indicators.

age was 25.7 years.[5] The vast majority, 163 participants, was born in Germany, as were most of their parents (162 and 155 of their mothers and fathers, respectively). Nine participants reported having dual citizenship besides their German nationality. Their political orientation, measured on an interval ranging from 1 (extremely left-wing) to 5 (extremely right-wing), has a distribution slightly skewed to the left from the center (mean = 2.5, SE = 0.04), as typical for a university student pool. Table 2.1 shows that the treatments were balanced concerning observable characteristics.

Task. — We used a variant of the widely used slider task, first introduced by Gill and Prowse (2012), which has recently been used in laboratory labor market experiments (Araujo et al., 2016; Chen and Schildberg-Hörisch, 2019; Gill and Prowse, 2019). After a general explanation, participants performed three rounds of the slider task. Participants were shown a screen with 50 sliders in a randomly determined initial position in each round. Each slider could be positioned between 0 and 100 (see Appendix for a screenshot). Sliders should be moved to their midpoint with the computer's mouse at 50. Participants could earn 5 Euros if they completed at least 25 out of 50 sliders, whereas earnings would be zero below this threshold, as described by equation 2.1. For each centered slider above the threshold, they could receive additional 20 Cents such that earnings m were capped at 10 Euros. Participants were told that they would

[5] We excluded one participant's observation from the analysis who did not center any slider during the three rounds, although she touched 31, 30 and 30 sliders, respectively. Our results are robust to including this observation and using the number of touched sliders as dependent variable.

be paid according to their performance in a randomly chosen round, determining their payoff. Hence, the earnings maximizing strategy was exerting the highest possible effort in each round.

$$m = \begin{cases} 0 & e < 25 \\ (e - 25) \cdot 0.2 + 5 & e \geqq 25 \end{cases} \tag{2.1}$$

Treatments. — Before performing the task, we informed participants (the "workers") that their final payoffs would, in addition to their performance, also depend on the choice made by a third person (the "allocator"). The allocator could redistribute earnings (shares between 0 and 100 percent in steps of 20 percent) from them to another person (the "recipient"). The experimental design is similar to the situation faced by "stakeholders" and "workers" during the first phases of the experiments by Cappelen et al. (2013) and Almås et al. (2020). Utilizing a non-strategic interaction in which the recipient has no possibility to react to the worker's behavior allows studying preferences independently from belief-based group effects, which could originate from repeated game strategies (Everett et al., 2015).

Subjects were randomly assigned to one out of three possible treatment conditions in a between-subject design. In each of the three conditions, we varied the recipient's background. The recipient was either (i) a German citizen, (ii) an asylum seeker, or (iii) an economic migrant (the exact wording was "migrant for economic reasons"). The allocator was always described as a German citizen. Thus, the allocator belonged to what may be presumed to represent the subject's "ingroup" in the current context. After each round, we elicited workers‘ beliefs about the tax chosen by the third-party allocator. The tax determines the share of earnings transferred to the recipient. We incentivized the elicitation of beliefs by an additional payment (worth 50 Euro cents) for correct beliefs about the tax rate.

In addition, in line with what was done for the allocator's decisions, we manipulated the efficiency of the redistribution mechanism. During the first round, the efficiency factor was always equal to one, i.e., subjects knew that the allocator could transfer earnings one-to-one from them to the recipient. The order of the efficiency factor in the second and third round, where each Euro from the worker transferred to the recipient would be either doubled (factor 2) or halved (factor 0.5), was randomized. This efficiency manipulation enables us to see how individuals weigh fairness and efficiency motives in their preferences (Cappelen et al., 2013; Durante et al., 2014).

Theoretical background. — To guide our analysis of workers' behavior, we assume a simple utility function (equation 2.2) of the following type.

$$U = m(e) \cdot (1 - t^e) - c(e) + \theta_i \cdot m(e) \cdot t^e \tag{2.2}$$

Agents are assumed to derive utility from their expected earnings $m(e) \cdot (1 - t^e)$, wherein t^e is the expected share to be redistributed (the "tax rate"), minus their costs of providing effort $c(e)$. Furthermore, they are assumed to have social preferences weighted by $\theta_i \leqq 1$ towards the recipient, such that exerting effort to benefit the recipient may generate additional utility (Ariely et al., 2008). The index i identifies the three possible identities of recipients: $i = \{G, E, A\}$, where G identifies German recipients, E economic migrants, and A asylum seekers. Since preferences for ingroup favoritism appears to be widespread (Luttmer, 2001; Chen and Li, 2009; Fong and Luttmer, 2011; Romano et al., 2017), it is natural to assume that : $\theta_G > \theta_E$ and $\theta_G > \theta_A$. It is more challenging to hypothesize regarding the relative value of θ_E and θ_A. On the one hand, it is plausible that asylum seekers suffer less discrimination than economic migrants because they are needier and deserve compensation for their past traumatic experiences. On the other hand, economic immigrants may be seen more favorably than asylum seekers for their availability to work. In the lack of any solid theoretical argument going in one direction or the other, we posit the following order in equation 2.3.

$$\theta_G > \theta_E = \theta_A \tag{2.3}$$

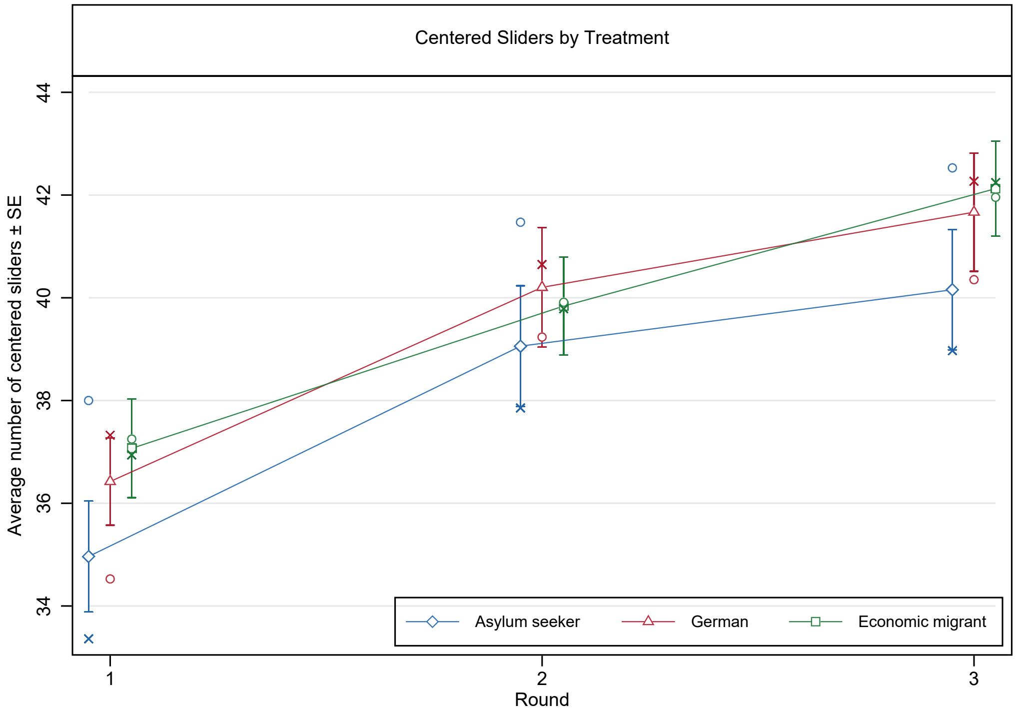
We assume an invertible cost function $c(e)$ fulfilling the regularity conditions $c'(e) > 0$, $c''(e) > 0$ and $\lim_{e \to \infty} c(e) = \infty$.

$$e^* = \begin{cases} c'^{-1}\left[0.2 \cdot (1 - t^e + \theta \cdot t^e)\right] & e^* > 25 \\ 0 & e^* \leq 25 \end{cases} \tag{2.4}$$

The optimal effort level e^* depends negatively on the tax rate and positively on θ_i in case of an interior solution, considering the payoff determining mechanism. Moreover, suppose the social preferences parameter θ_i towards potential beneficiaries varies with the recipient's identity (German, asylum seeker, or economic migrant) due to group identity effects. In that case, we should observe differences in exerted effort across treatments. Hence, the above simple model leads to two main hypotheses.

- *Hypothesis 1:* Due to ingroup favoritism, we expected effort levels to be higher if the recipient was a member of participants' ingroup (German citizen) than when the recipient was an asylum seeker or an economic migrant.

Figure 2.1: Effort by Recipient for each Round



Notes: This figure shows mean number of centered sliders ±1*SE by round of experiment. Crosses (circles) show the mean for the subgroup of people reporting to be close (neutral of distant) to Germans.

- *Hypothesis 2:* We expected that the larger the beliefs about the rate to be imposed by the allocator, the lower the exerted effort.

In the following section, we analyze the experimental data[6].

2.3 Results

2.3.1 Descriptive results

Table 2.2 provides summary statistics[7] of the number of centered sliders as well as of the beliefs about tax rates to be imposed by the third-person allocator. Figure 2.1 depicts means and their standard errors for the number of centered sliders by communicated type of the recipient and self-reported identification with the objective ingroup of Germans.

[6]The dataset and code are stored in the OSF repository and provided to reviewers for the purpose of replication at https://osf.io/hmjvw/?view_only=04b98feb2b904b32be87f9d5f4cfb4a6.

[7]More detailed summary statistics are provided in the Appendix Table 2.10.

Table 2.2: Centered Sliders and Tax Beliefs by Round and Efficiency

	By round				By efficiency factor	
Centered sliders	Round 1	Round 2	Round 3	All rounds	2x	0.5x
Asylum seeker	34.96	39.06	40.16	37.98	39.67	39.55
	(1.08)	(1.18)	(1.17)	(0.68)	(1.20)	(1.15)
Obs.	55	51	51	157	51	51
German	36.42	40.20	41.67	39.34	40.37	41.50
	(0.85)	(1.16)	(1.15)	(0.63)	(1.19)	(1.13)
Obs.	59	54	54	167	54	54
Economic migrant	36.44	39.84	42.13	39.65	40.73	41.23
	(1.13)	(0.95)	(0.93)	(0.57)	(0.93)	(0.97)
Obs.	58	56	56	170	56	56
All recipients	36.17	39.71	41.35	39.01	40.27	40.79
	(0.56)	(0.63)	(0.62)	(0.36)	(0.64)	(0.62)
Obs.	172	161	161	494	161	161
Beliefs about tax rate	Round 1	Round 2	Round 3	All rounds	2x	0.5x
Asylum seeker	35.60	35.27	33.55	34.83	28.94	39.88
	(3.83)	(4.42)	(4.47)	(2.43)	(4.12)	(4.62)
Obs.	55	51	51	157	51	51
German	32.73	33.57	35.00	33.74	29.06	39.52
	(2.90)	(3.74)	(3.68)	(1.97)	(3.51)	(3.76)
Obs.	59	54	54	167	54	54
Economic migrant	34.34	34.95	34.59	34.62	28.13	41.41
	(3.41)	(3.53)	(3.69)	(2.03)	(2.91)	(4.00)
Obs.	58	56	56	170	56	56
All recipients	34.19	34.59	34.40	34.39	28.70	40.29
	(1.94)	(2.23)	(2.26)	(1.23)	(2.02)	(2.37)
Obs.	172	161	161	494	161	161

Notes: The table shows means and standard errors (in parentheses) for centered sliders and beliefs about taxes by treatment, round and efficiency factor. The efficiency factor always equals one in the first round.

On average, our subjects complete 39 sliders in each 2-minutes period across the three treatments and three rounds. In all treatments, we observe significant learning effects as the number of centered sliders increases from an average of 36.2 sliders in the first round to 41.4 in the third round ($p < 0.001$, two-sided t-test). The number of observations is 172 in the first and 161 in the second and third rounds. Unfortunately, an omission in the programming prevents us from determining the order of rounds for observations from the last two sessions conducted in the laboratory. Consequently, we only used data from the first round for these two sessions. A first glimpse at the mean number of completed sliders in Table 2.2 (also depicted in Figure 2.1) reveals only a slight variation between the three treatments, indicating weak effects from group identity on average. The average number of centered sliders over all rounds is 37.98 for the recipient being an asylum seeker, 39.34 for the recipient being a German citizen, and 40.73 when the recipient is an economic migrant.

Similarly, there is only a slight variation between treatments in the beliefs about the tax rate. Participants expected an average tax rate of 34.83 when the recipient is an asylum seeker, 33.74 when the recipient is a German citizen, and 34.62 for economic migrants. Instead, expected tax rates are considerably higher when the efficiency factor of the underlying redistribution mechanism is lower. Averaging over all treatment conditions, participants expected a tax rate of 28.7 percent for the doubling factor and 40.29 for the transfer-halving factor.

2.3.2 Regression results

To provide a quantitative assessment of the participants' behavior, we fit a random-effects Tobit model for panel data. Equation 2.5 describes the regression model in its base form. The Tobit model accounts for censoring in the latent dependent variable y^*_{it}. In this context, using the number of completed sliders as dependent variable, the latent variable may be interpreted as capturing the propensity to exert effort, or the desired level of effort. The effort variable is censored from below at 0 and above at 50. Beliefs about taxes are censored from below at 0 and above at 100. α is the intercept, $c'$is a vector of controls, and u_{it} is the error term.

$$y^*_{it} = \alpha + \beta_{GER} \cdot GER + \beta_{ECON} \cdot ECON + \sum_{t=1}^{2} \delta_t \cdot r_t + \gamma \cdot DOUBLE + c'\eta + u_{it} \quad (2.5)$$

The regression model allows quantifying the treatment effects, i.e., the effect of varying recipient identity (asylum seeker, German citizen, economic migrant), as well as to control for learning and individual-level variation. β_{GER} and β_{ECON} are regression coefficients for the treatment indicators, with the recipient being an asylum seeker serving as the base category. Because the treatment variables are time-invariant, we cannot use a fixed-effects model. The regressions include indicators for the second and third round ($\sum_{t=1}^{2} \delta_t \cdot r_t$) to account for learning effects. $DOUBLE$ is an indicator variable for the transfer-doubling efficiency factor.

Without violating the rank condition, we can either include dummies for the second and the third round and one of the efficiency factors (either doubling or one half) or for both efficiency factors but only for one of the rounds. With the number of centered sliders as the outcome variable, the round dummies are highly significant and statistically different from each other according to a Wald test ($p < 0.001$). In contrast, coefficients for efficiency factors in unreported regressions do not reach statistical significance. The opposite holds for beliefs about the tax rates imposed by the allocator as the dependent variable. We thus included round indicators in the effort regressions and efficiency factor indicators in the case of the beliefs regressions. Estimating a pooled OLS regression with standard errors clustered at the individual level leads qualitatively to the same results as the Tobit model showing only minor differences in standard errors. The following subsections discuss the results concerning exerted effort and elicited beliefs about tax rates based on the Tobit model for panel data.

2.3.2.1 Effort

The first three columns of Table 2.3 show regression results in which the number of centered sliders serves as the dependent variable. Regressions in columns (2) and (3) show results from a regression where we added interactions between the treatment indicators and the variable *Close*, which is a simple measure of subjective identification with the (in-)group of (other) Germans. Concretely, the variable *Close* is equal to 1 if a subject stated to feel close or very close to (other) Germans ($N = 110$), and it is 0 if a subject placed themselves as neutral, distant, or very distant ($N = 62$). Figure 2.2 shows the main results concerning the between- and within-group comparisons, where we contrast participants based on their reported closeness to other Germans.

Aggregate results. —Indicator variables for the second and third rounds turn out to be positive, with point estimates of about 3.9 and 5.7 relative to the first period. These indicate the presence of learning effects that are statistically highly significant ($p < 0.001$). Gender has a statistically significant effect, as females completed roughly six sliders ($p < 0.001$, Wald test) less than male participants. This result may be due to men's higher familiarity with video games. The efficiency of the redistribution mechanism shows no significant effect on effort. The Tobit regression reveals no treatment effects from the recipient's identity in the aggregate, as coefficients on the

Table 2.3: Main Results: Tobit Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
German	0.287	-4.143*	-4.152*	-0.510	17.16*
	(1.33)	(2.28)	(2.27)	(5.85)	(10.01)
Economic migrant	1.290	-1.772	-1.781	1.251	15.96*
	(1.33)	(2.14)	(2.14)	(5.83)	(9.47)
Round 2	3.921****	3.900****	3.895****		
	(0.47)	(0.47)	(0.47)		
Round 3	5.708****	5.691****	5.686****	0.009	0.001
	(0.47)	(0.47)	(0.46)	(2.22)	(2.22)
Female	-6.002****	-5.968****	-5.965****	-5.095	-5.225
	(1.11)	(1.09)	(1.09)	(4.83)	(4.76)
2x Efficiency	-0.452	-0.450	-0.450	-5.519**	-5.514**
	(0.42)	(0.42)	(0.42)	(2.49)	(2.49)
0.5x Efficiency				7.328***	7.330***
				(2.47)	(2.47)
Belief about tax	-0.003	-0.001			
	(0.01)	(0.01)			
Close		-4.065**	-4.076**		18.90**
		(1.98)	(1.97)		(8.76)
German × Close		6.663**	6.678**		-26.49**
		(2.78)	(2.78)		(12.19)
Economic migrant × Close		4.736*	4.750*		-22.57*
		(2.71)	(2.71)		(11.91)
Constant	42.60****	45.55****	45.52****	35.29***	22.03*
	(2.74)	(2.99)	(2.97)	(11.82)	(13.03)
Obs.	494	494	494	494	494
Right-censored	46	46	46	20	20
Left-censored	0	0	0	75	75
No. of panels	172	172	172	172	172

Table 2.3: Main Results: Tobit Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
Log-likelihood	-1438.8	-1435.8	-1435.8	-1955.6	-1952.8
Hypothesis tests (p-values)					
Round 2 = Round 3	p = 0.000	p = 0.000	p = 0.000		
2x Eff. = 0.5x Eff.				p = 0.000	p = 0.000
German × Close = 0		p = 0.017	p = 0.016		p = 0.029
Economic migrant × Close = 0		p = 0.081	p = 0.079		p = 0.058
German = 0	p = 0.830	p = 0.069	p = 0.068	p = 0.930	p = 0.086
Economic migrant = 0	p = 0.332	p = 0.408	p = 0.406	p = 0.830	p = 0.092
German = Economic migrant	p = 0.446	p = 0.271	p = 0.271	p = 0.759	p = 0.898
Close = 0		p = 0.040	p = 0.039		p = 0.031
Close + German × Close = 0		p = 0.186	p = 0.185		p = 0.374
Close + Economic migrant × Close = 0		p = 0.718	p = 0.717		p = 0.650
German + German × Close = 0		p = 0.117	p = 0.115		p = 0.183
Economic migrant + Economic migrant × Close = 0		p = 0.074	p = 0.073		p = 0.360
Economic migrant (1 + Close) = German (1 + Close)		p = 0.785	p = 0.786		p = 0.702

Notes: The table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50 for the first three columns, and at 100 for the fourth and the fifth column. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects with age larger or equal 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task.

recipient's characteristics are not significantly different from zero (first column of Table 2.3). Participants complete roughly one slider less when Person 2 is German than when she is an economic migrant (p = 0.446), and 0.3 sliders more when Person 2 is German than when Person 2 is an asylum seeker (p = 0.830). We thus do not observe any bias based on objective affiliation to their ingroup, given that all our subjects were students of German citizenship.

Heterogeneity within groups defined by their closeness. — The picture changes if we consider the degree of participant's closeness to German identity. Participants who reported a strong identification with Germans are by 2.5 completed sliders less successful in the slider task when the recipient is an asylum seeker than when the recipient is German. This result borders the 10% level of statistical significance (p = 0.117, Wald test) in the whole sample. If we remove extreme outlier observations according to the Tukey's fences method, however, the difference reaches marginal statistical significance (p = 0.099) (see Appendix Table 2.5). Participants with higher closeness to German identity complete also roughly 3 sliders more when the recipient is an economic migrant than when the recipient is an asylum seeker (p = 0.074). There are no significant differences in effort when the recipient is a German or an economic migrant in the group having a close identification with Germans (insignificant difference = 0.4 completed sliders more when the recipient is an economic migrant instead of a German, p = 0.785).

Conversely, participants without strong identification with Germans exert *lower* effort if the recipient is German compared to when she is an asylum seeker (difference = -4.1 sliders, p = 0.069). In addition, there is a tendency in this group to exert lower effort in the treatment where the recipient is an economic migrant relative to when the recipient is an asylum seeker, albeit not reaching statistical significance at conventional levels (difference = -1.8 sliders, p = 0.408). The difference between the recipient being an economic migrant or a German does not reach statistical significance in the participants with a low identification (difference = 2.4 sliders, p = 0.271).

Heterogeneity between groups defined by their closeness. — Comparing the effort between groups defined by the strength of their identification with Germans, we can report the following results. Participants reporting a strong identification with other Germans exert significantly[8] less effort when the recipient is an asylum seeker than participants without a strong identification with Germans (difference = -4.1 sliders, p = 0.040). In addition, on average, effort is higher in the group of participants who identify with their ingroup when the recipient is a German citizen, as compared to those

[8]The p-value from a one-sided t-test accounting for unequal variances using the mean number of sliders over the three periods between those with and without a strong ingroup identification is p = 0.029.

who reported no identification, albeit not statistical significance at the 10 percent level (difference = 2.6 sliders, p = 0.186, Wald test). There are no significant differences between groups defined by their reported closeness when the recipient is an economic migrant (difference = 0.7 sliders more among the *Close* participants, p = 0.718).

Finally, we can compare the differences in differences when the recipient is an asylum seeker versus when the recipient is either a German or an economic migrant between the groups defined by their identification with (other) Germans. Participants with a strong identification with Germans exert significantly more effort if the recipient is another German than when the recipient is an asylum seeker relative to the same difference among participants without a strong identification with Germans (difference = 6.7 sliders, p = 0.017). When the recipient is an asylum seeker or an economic migrant, the difference in difference only reaches statistical significance at the 10 percent level (difference = 4.7 sliders, p = 0.081). Hence, consistent with previous results, the treatment effect of the recipient being a German or an economic migrant instead of an asylum seeker is positive among those with a solid self-reported identification with Germans relative to those without a strong level of identification.

Views on outgroups. — We cannot replicate the heterogeneity in treatment effects we find concerning participants' self-reported identification with Germans using PCA indices based on questions on views about immigrants in general, asylum seekers, and economic migrants (see Table 2.7 in the Appendix). We find no statistically significant heterogeneity at all interacting treatment indicators with dummy variables equal to one if views on these groups as captured by the PCA indices are less favorable than the median value in the sample. This finding supports the interpretation that a survey question concerning closeness to a respondent's ingroup is less affected by social-desirability biases. Instead, respondents may be more reluctant to report negative attitudes or prejudices in questions about outgroups.

Taste-based discrimination and gender. — In our framework, there are two competing explanations for ingroup biases, which can arise either due to the presence of taste-based discrimination or due to expectations about the share of earnings that the third-party allocator would transfer to the recipient. Regression results from column (3) of Table 2.3 show that beliefs about the share to be redistributed do not show statistically significant effects on exerted effort. Furthermore, when we contrast regression results from column (2) and column (3), we observe that controlling for beliefs about the share to be transferred does not affect treatment effects.

Overall, these results favor an explanation of effort differences among those reporting a strong ingroup identification based on taste-based discrimination instead of being caused by an expectation to be taxed more strongly in case the potential recipient is from the asylum seeker outgroup. In contrast to part of the previous literature

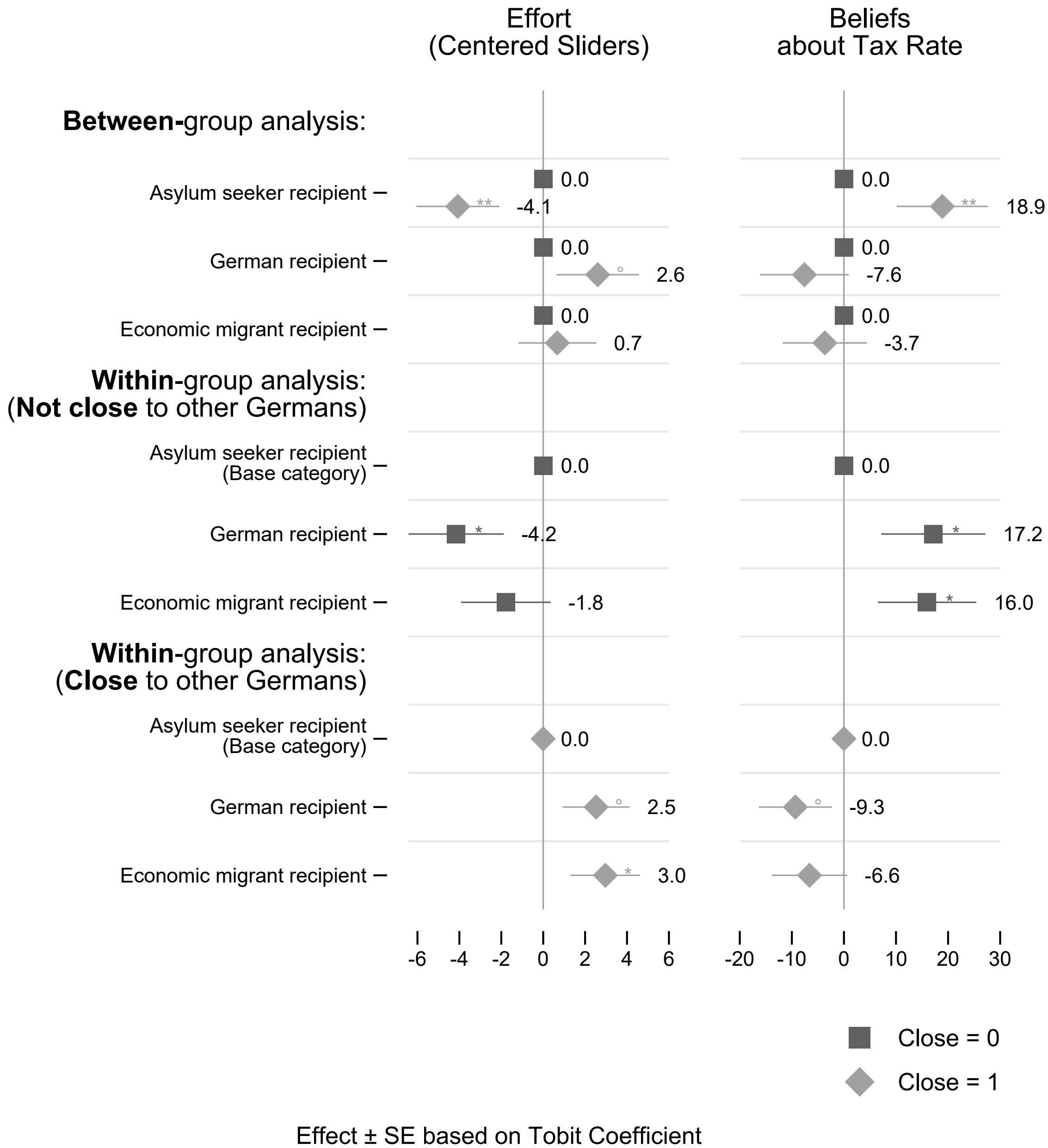
(Fershtman and Gneezy, 2001; Solow and Kirkwood, 2002; Croson et al., 2008), we do not find a differential strength of group loyalty effects along the gender dimension. There is no significant difference between females and males in the reaction to the treatments while female participants generally performed slightly worse on the slider task, as can be seen in Appendix Table 2.5.

2.3.2.2 Beliefs about tax rates

In columns (4) and (5) in Table 2.3, we show regression results with the expected share of participants' earnings that the allocator would redistribute as the dependent variable. Akin to the analysis of exerted effort, we interact treatment indicators with the variable Close, i.e., the dummy for self-reported identification with the ingroup (other Germans) in the fourth column of Table 2.3. As noted above, the regressions with beliefs as dependent variables do not contain an indicator for the second round, allowing us to control the transfer-halving efficiency factor. Apart from this adjustment, we use the equivalent right-hand-side variables as in the third column.

Aggregate results. — In contrast to findings concerning exerted effort, the efficiency factor plays a vital role in the tax rate beliefs. Participants, on average, expect a 5.5 percentage point smaller tax rate when the redistributed share of earnings would be doubled ($p = 0.027$). On the contrary, for an efficiency factor equal to 0.5, participants' tax beliefs are, on average, 7.3 percentage points higher ($p = 0.003$). Both effects are not only statistically significantly different relative to the base category of a one-to-one transfer but also significantly different from each other ($p < 0.001$, Wald test). These findings are consistent with the idea that participants expected the allocator to be willing to transfer a minimum amount of money to the recipient. As a result, participants expected allocators to completely disregard efficiency concerns and transfer more when it was less efficient. Hence, the participants' average belief contrasts the prediction of economic theory and recent experimental findings (Krawczyk, 2010; Almås et al., 2020) that allocators may choose to redistribute less if redistribution involves a cost due to efficiency losses. In another experiment related to this project, Grimalda et al. (2022a) analyze allocators' choices about the share to be redistributed from the workers to different types of recipients, involving 1807 participants from a quasi-representative sample of the German population. Remarkably, Grimalda et al. (2022a) find that workers' expectations in the present experiment were correct, as allocators transferred 16.6% more when the efficiency factor was 0.5 instead of one. Furthermore, the allocators transferred 5.6% more when the efficiency factor was two than when it was 0.5 - something that workers failed to anticipate, albeit the difference in expectations between these two cases is not significant. This pattern of preferences, which disregards

Figure 2.2: Main Results: Between-group Analysis and Within-group Analysis



Notes: This figure shows the main results based on the Tobit regressions. The dependent variable in the first (second) column is the number of centered sliders (beliefs about tax rate). In the first three rows, the results of the between-group comparison, i.e., between those who report feeling close and those who do not, are depicted. Those who do not feel close to other Germans serve as the base category. The remaining six rows show the differences within the groups based on their reported closeness to other Germans, whereby the recipient being an asylum seeker is the base category. (**, *, °) indicate two-sided p-values below 0.05, 0.1, and 0.2, respectively.

efficiency concerns but seemingly aims to guarantee a minimum earning level to the recipient, is compatible with a Rawlsian or a "Boulding" social welfare function (Traub et al., 2005).

Somewhat mirroring our results from the analysis of effort, on average, we do not find any significant treatment effects from the recipient's characterization as either an asylum seeker, a German citizen, or an economic migrant on the stated beliefs (see Table 2.3, column 3).

Heterogeneity within groups defined by their closeness. — However, as in the case of effort, this aggregate result masks a significant heterogeneity of treatment effects concerning participants' identification with (other) Germans. Participants who reported to be close to Germans expected relatively lower tax rates when the recipient is either a German (difference = -9.3 percentage points, p = 0.183) or an economic migrant (difference = -6.6 percentage points, p = 0.360) in comparison with the base category of an asylum seeker as the recipient, thereby not reaching statistical significance. There are no significant differences in tax beliefs between the treatments when the recipient is a German or an economic migrant in this group (difference = -2.7 percentage points lower expected tax rates when the recipient is a German, p = 0.702).

On the other hand, those participants who do not report to identify with Germans, expected the allocator to impose marginally significantly higher tax rates when the recipient was described either as a German (difference = 17.2 percentage points, p = 0.086) or as an economic migrant (difference = 16 percentage points, p = 0.092), relative to the treatment when the recipient was an asylum seeker.

Beliefs vs. allocators' actual choices. — We compare the allocators' actual choices concerning recipients' identities from Grimalda et al. (2022a) with workers' beliefs in the present experiment. Allocators, on average, redistribute the most to German recipients (45.6 percent) followed by recipients that are asylum seekers (41.2 percent) and economic migrants (37.8 percent) (Grimalda et al., 2022a). Hence, the beliefs of participants who reported feeling close to (other) Germans and those who did not report feeling close were incorrect concerning ordering the share to be redistributed to the three types of recipients. On the one hand, those with a strong German identity expected asylum seekers to benefit most from redistribution and German recipients to benefit the least. In contrast, the actual choices by allocators reveal that German recipients benefitted the most, and economic migrants benefited even less than asylum seekers (Grimalda et al., 2022a). On the other hand, those not having a strong German identity correctly anticipated that Germans would have benefitted the most from redistribution. However, they expected asylum seekers to benefit even less than economic migrants. In contrast, it was the other way around concerning actual allocators' redistribution choices towards asylum seekers and economic migrants (Grimalda et al., 2022a). It is also remarkable that those not reporting strong German identity expected asylum seeker recipients to be penalized four times more (relative to the redistribution towards German recipients) than was, in fact, the case. In general, allocators discriminated across recipients' groups at a lower rate than workers expected.

Heterogeneity between groups defined by their closeness. — As in the case of effort, only one difference between those who reported a strong identification with their ingroup compared to those who did not report strong identification reaches statistical significance. Participants who reported feeling close to their ingroup of (other) Germans expect a larger share of their earnings to be redistributed when the recipient is an asylum seeker compared to those who did not report a strong identification (difference = 18.9 percentage points, p = 0.031). There are no significant differences between both groups' beliefs when the recipient is either a German (difference = -7.6 percentage points lower expectations among *Close* participants, p = 0.374) or an economic migrant (difference = -3.7 percentage points lower expectations among *Close* participants, p = 0.650).

We also looked at the differences in differences in beliefs between groups defined by their identification with Germans. The result for expected tax rates is similar to what we found with the number of centered sliders as the dependent variable. Namely, participants with a strong identification with Germans relative to those without solid identification with Germans expect significantly lower tax rates when the recipient is a German compared to when the recipient is an asylum seeker (difference = -26.5 percentage points, p = 0.029). The same applies to the treatments where the recipient is an economic migrant instead of an asylum seeker but only reaches marginal statistical significance (difference = -22.6 percentage points, p = 0.058).

2.4 Conclusion

We report results from an experiment in which a student sample with exclusively German citizenship exert real effort in a variant of the slider task (Gill and Prowse, 2012, 2019) to study ingroup favoritism in a setting that resembles a simplified version of a welfare state. We informed participants that part of their earnings might be redistributed to a recipient, whereby the choice of the transfer-determining tax rate lies in the hands of a third-person allocator. In three treatments, administered in a between-subject design, the recipient is either (i) a German citizen, (ii) an asylum seeker, or (iii) an economic migrant.

The extant literature has found that ethnic heterogeneity may affect the welfare state in several dimensions, such as income redistribution and public goods provision. This paper aimed to examine whether ethnic heterogeneity may also affect workers' propensity to exert effort, knowing that earning redistribution may affect either fellow country people or immigrants, distinguishing between economic migrants and asylum seekers. We found that, even if we cannot detect an effect of the recipient's identity in the aggregate, this hides an essential difference between people who closely identify with other Germans and people who do not. The former group tends to exert less effort when the recipient is an asylum seeker than the latter group. Workers closely identifying with other Germans also tend to put more effort when the redistribution recipient is German, or an economic migrant than when the recipient is an asylum seeker, while workers not identifying with other Germans tend to do the opposite.

Our analysis shows that lower altruism toward asylum seekers rather than statistical discrimination leads to the observed discrimination. In our context, statistical discrimination would operate through the belief that the allocator will benefit asylum seekers more than others. While it is indeed the case that *Close* participants expect, on average, higher redistribution toward asylum seekers, we show that this belief does not significantly affect their effort (see Table 2.3 and Appendix Table 2.9). While, in principle, there could be room for statistical discrimination to operate, its effect is negligible, according to our findings. Instead, the reduced effort by *Close* participants is almost entirely driven by reduced altruism or taste-based discrimination (Becker, 1974).

Of course, one should be cautious when extrapolating our findings to the real world. In particular, the stakes involved (10 Euros at maximum) were small compared to usually taxed real-world incomes. Moreover, the situation in the laboratory and the slider task are artificial. In particular, the recipient in the experiment could not undertake work and contribute to the welfare state. This design choice was made to identify the possible effect of ethnic heterogeneity on individual effort in the neatest possible way. In reality, immigrants contribute to the welfare state. Therefore, the effect of ethnic heterogeneity we observed in the experiment may arguably be interpreted as the upper bound of what is the case in real life. Nonetheless, it is well-known that people tend to grossly underestimate the immigrants' contribution to the tax revenues and the economy. The effect in real life may thus be not so distant from the effect detected in the experiment, especially for people with a strong ingroup identity. Overall, we believe that showing that a fraction of people with a strong ingroup identification tend to sacrifice potential earnings if members of an outgroup could be beneficiaries is relevant for many societies facing increased heterogeneity due to immigration. However, further research is needed to explore how these findings may translate into the field and non-student populations.

References

Abbink, Klaus, Jordi Brandts, Benedikt Herrmann, and Henrik Orzen. 2010. "Intergroup Conflict and Intra-Group Punishment in an Experimental Contest Game." *American Economic Review*, 100 (1): 420–447.

Abbink, Klaus and Donna Harris. 2019. "In-group favouritism and out-group discrimination in naturally occurring groups." *PloS one*, 14 (9): e0221616.

Akerlof, George A. and Rachel E. Kranton. 2000. "Economics and Identity." *The Quarterly Journal of Economics*, 115 (3): 715–753.

Alesina, Alberto, Reza Baqir, and William Easterly. 1999. "Public Goods and Ethnic Divisions." *The Quarterly Journal of Economics*, 114 (4): 1243–1284.

Alesina, Alberto, Matteo Ferroni, and Stefanie Stantcheva. 2021a. "Perceptions of racial gaps, their causes, and ways to reduce them." *NBER Working Paper*, 29245.

Alesina, Alberto and Paola Giuliano. 2011. "Preferences for redistribution." In *Handbook of Social Economics*, edited by Benhabib, Jess, Alberto Bisin, and Matthew O. Jackson Volume 1A. of Handbook of Economics 93–131, Burlington: Elsevier Science.

Alesina, Alberto and Edward Ludwig Glaeser. 2004. *Fighting poverty in the US and Europe: A world of difference*. Oxford: Univ. Press.

Alesina, Alberto, Armando Miano, and Stefanie Stantcheva. 2022. "Immigration and Redistribution." *The Review of Economic Studies*, Accepted Manuscript (0): 1–39.

Alesina, Alberto, Elie Murard, and Hillel Rapoport. 2021c. "Immigration and preferences for redistribution in Europe." *Journal of Economic Geography*, 21 (6): 925–954.

Algan, Yann, Camille Hémet, and David D. Laitin. 2016. "The Social Effects of Ethnic Diversity at the Local Level: A Natural Experiment with Exogenous Residential Allocation." *Journal of Political Economy*, 124 (3): 696–733.

Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden. 2020. "Cutthroat Capitalism versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians?" *Journal of Political Economy*, 128 (5): 1753–1788.

Anderson, Lisa R., Roland G. Fryer, and Charles A. Holt. 2006. "Discrimination: Experimental evidence from psychology and economics." In *Handbook on the economics of discrimination*, edited by Rodgers, William M. 97–115, Cheltenham, U.K and Northampton, Mass: Edward Elgar.

Araujo, Felipe A., Erin Carbone, Lynn Conell-Price, Marli W. Dunietz, Ania Jaroszewicz, Rachel Landsman, Diego Lamé, Lise Vesterlund, Stephanie W. Wang, and Alistair J. Wilson. 2016. "The slider task: An example of restricted inference on incentive effects." *Journal of the Economic Science Association*, 2 (1): 1–12.

Ariely, Dan, Emir Kamenica, and Dražen Prelec. 2008. "Man's search for meaning: The case of Legos." *Journal of Economic Behavior & Organization*, 67 (3-4): 671–677.

Balliet, Daniel, Junhui Wu, and Carsten K. W. de Dreu. 2014. "Ingroup favoritism in cooperation: A meta-analysis." *Psychological Bulletin*, 140 (6): 1556–1581.

Becker, Gary S. 1971. *The economics of discrimination: 2d ed.* Economics research studies of the economics research center of the university of chicago, Chicago: University of Chicago Press.

Becker, Gary S. 1974. "A Theory of Social Interactions." *Journal of Political Economy*, 82 (6): 1063–1093.

Bernhard, Helen, Ernst Fehr, and Urs Fischbacher. 2006. "Group Affiliation and Altruistic Norm Enforcement." *American Economic Review*, 96 (2): 217–221.

Brewer, Marilynn B. 1999. "The Psychology of Prejudice: Ingroup Love and Outgroup Hate?" *Journal of Social Issues*, 55 (3): 429–444.

Cappelen, Alexander W., James Konow, Erik Ø. Sørensen, and Bertil Tungodden. 2013. "Just Luck: An Experimental Study of Risk-Taking and Fairness." *American Economic Review*, 103 (4): 1398–1413.

Chakravarty, Surajeet, Miguel A. Fonseca, Sudeep Ghosh, and Sugata Marjit. 2016. "Religious fragmentation, social identity and cooperation: Evidence from an artefactual field experiment in India." *European Economic Review*, 90 (3): 265–279.

Charness, Gary, Ramón Cobo-Reyes, and Natalia Jiménez. 2014. "Identities, selection, and contributions in a public-goods game." *Games and Economic Behavior*, 87 (3): 322–338.

Charness, Gary, Uri Gneezy, and Austin Henderson. 2018. "Experimental methods: Measuring effort in economics experiments." *Journal of Economic Behavior & Organization*, 149 (2): 74–87.

Charness, Gary, Luca Rigotti, and Aldo Rustichini. 2007. "Individual behavior and group membership." *American Economic Review*, 97 (4): 1340–1352.

Charness, Gary and Aldo Rustichini. 2011. "Gender differences in cooperation with group membership." *Games and Economic Behavior*, 72 (1): 77–85.

Chen, Si and Hannah Schildberg-Hörisch. 2019. "Looking at the bright side: The motivational value of confidence." *European Economic Review*, 120 (2): 103302.

Chen, Yan and Sherry Xin Li. 2009. "Group Identity and Social Preferences." *American Economic Review*, 99 (1): 431–457.

Cooper, David J. and John H. Kagel. 2016. "Other-Regarding Preferences: A Selective Survey of Experimental Results." In *The Handbook of Experimental Economics, Volume 2*, edited by Kagel, John H. and Alvin E. Roth 217–289, Princeton: Princeton University Press.

Croson, Rachel T.A., Melanie Marks, and Jessica Snyder. 2008. "Groups Work for Women: Gender and Group Identity in Social Dilemmas." *Negotiation Journal*, 24 (4): 411–427.

Dahlberg, Matz, Karin Edmark, and Heléne Lundqvist. 2012. "Ethnic Diversity and Preferences for Redistribution." *Journal of Political Economy*, 120 (1): 41–76.

Durante, Ruben, Louis Putterman, and Joël van der Weele. 2014. "Preferences for Redistribution and Perception of Fairness: An Experimental Study." *Journal of the European Economic Association*, 12 (4): 1059–1086.

Durrheim, Kevin, Michael Quayle, Colin G. Tredoux, Kim Titlestad, and Larry Tooke. 2016. "Investigating the Evolution of Ingroup Favoritism Using a Minimal Group Interaction Paradigm: The Effects of Inter- and Intragroup Interdependence." *PloS one*, 11 (11): e0165974.

Eckel, Catherine C. and Philip J. Grossman. 2005. "Managing diversity by creating team identity." *Journal of Economic Behavior & Organization*, 58 (3): 371–392.

Everett, Jim A. C., Nadira S. Faber, and Molly J. Crockett. 2015. "Preferences and beliefs in ingroup favoritism." *Frontiers in Behavioral Neuroscience*, 9 (15): 1–21.

Falk, Armin and Christian Zehnder. 2013. "A city-wide experiment on trust discrimination." *Journal of Public Economics*, 100 (2): 15–27.

Fehr, Ernst, Erich Kirchler, Andreas Weichbold, and Simon Gächter. 1998a. "When Social Norms Overpower Competition: Gift Exchange in Experimental Labor Markets." *Journal of Labor Economics*, 16 (2): 324–351.

Fehr, Ernst, G. Kirchsteiger, and A. Riedl. 1993. "Does Fairness Prevent Market Clearing? An Experimental Investigation." *The Quarterly Journal of Economics*, 108 (2): 437–459.

Fershtman, C. and U. Gneezy. 2001. "Discrimination in a Segmented Society: An Experimental Approach." *The Quarterly Journal of Economics*, 116 (1): 351–377.

Fong, Christina M. and Erzo F.P. Luttmer. 2009. "What Determines Giving to Hurricane Katrina Victims? Experimental Evidence on Racial Group Loyalty." *American Economic Journal: Applied Economics*, 1 (2): 64–87.

Fong, Christina M. and Erzo F.P. Luttmer. 2011. "Do fairness and race matter in generosity? Evidence from a nationally representative charity experiment." *Journal of Public Economics*, 95 (5-6): 372–394.

Gill, David and Victoria Prowse. 2012. "A Structural Analysis of Disappointment Aversion in a Real Effort Competition." *American Economic Review*, 102 (1): 469–503.

Gill, David and Victoria Prowse. 2019. "Measuring costly effort using the slider task." *Journal of Behavioral and Experimental Finance*, 21 (2): 1–9.

Goette, Lorenz, David Huffman, and Stephan Meier. 2006. "The Impact of Group Membership on Cooperation and Norm Enforcement: Evidence Using Random Assignment to Real Social Groups." *American Economic Review*, 96 (2): 212–216.

Grimalda, Gianluca, Lena Detlefsen, Fabian Paetzel, and Christoph Schütt. 2022a. "Preferences for redistribution and discrimination: Ethnic diversity, education and volunteering matter." *mimeo.*

Guala, Francesco, Luigi Mittone, and Matteo Ploner. 2013. "Group membership, team preferences, and expectations." *Journal of Economic Behavior & Organization*, 86 (0): 183–190.

Hargreaves Heap, Shaun P. and Daniel J. Zizzo. 2009. "The Value of Groups." *American Economic Review*, 99 (1): 295–323.

Hedegaard, Morten S. and Jean-Robert Tyran. 2018. "The Price of Prejudice." *American Economic Journal: Applied Economics*, 10 (1): 40–63.

Kranton, Rachel E. and Seth G. Sanders. 2017. "Groupy versus Non-Groupy Social Preferences: Personality, Region, and Political Party." *American Economic Review: Papers & Proceedings*, 107 (5): 65–69.

Krawczyk, Michał W. 2010. "A glimpse through the veil of ignorance: Equality of opportunity and support for redistribution." *Journal of Public Economics*, 94 (1-2): 131–141.

Luttmer, Erzo F.P. 2001. "Group Loyalty and the Taste for Redistribution." *Journal of Political Economy*, 109 (3): 500–528.

Ockenfels, Axel and Peter Werner. 2014. "Beliefs and ingroup favoritism." *Journal of Economic Behavior & Organization*, 108 (3): 453–462.

Oswald, Andrew J., Eugenio Proto, and Daniel Sgroi. 2015. "Happiness and Productivity." *Journal of Labor Economics*, 33 (4): 789–822.

Romano, Angelo, Daniel Balliet, Toshio Yamagishi, and James H. Liu. 2017. "Parochial trust and cooperation across 17 societies." *Proceedings of the National Academy of Sciences of the United States of America*, 114 (48): 12702–12707.

Romano, Angelo, Giuliana Spadaro, Daniel Balliet, Jeff Joireman, Caspar van Lissa, Shuxian Jin, Maximilian Agostini, Jocelyn J. Bélanger, Ben Gützkow, Jannis Kreienkamp, N. Pontus Leander, and PsyCorona Collaboration. 2021a. "Cooperation and trust across societies during the COVID-19 pandemic." *Journal of Cross-Cultural Psychology*, 52 (7): 622–642.

Rong, Rong, Daniel Houser, and Anovia Yifan Dai. 2016. "Money or friends: Social identity and deception in networks." *European Economic Review*, 90 (4): 56–66.

Slonim, Robert and Pablo Guillen. 2010. "Gender selection discrimination: Evidence from a Trust game." *Journal of Economic Behavior & Organization*, 76 (2): 385–405.

Solow, John L. and Nicole Kirkwood. 2002. "Group identity and gender in public goods experiments." *Journal of Economic Behavior & Organization*, 48 (4): 403–412.

Stichnoth, Holger and Karine van der Straeten. 2013. "Ethnic Diversity, Public Spending, and Individual Support for the Welfare State: A Review of the Empirical Literature." *Journal of Economic Surveys*, 27 (2): 364–389.

Sutter, Matthias. 2009. "Individual Behavior and Group Membership: Comment." *American Economic Review*, 99 (5): 2247–2257.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *The Review of Economic Studies*, 87 (1): 454–486.

Tajfel, Henri. 1982. "Social Psychology of Intergroup Relations." *Annual Review of Psychology*, 33 (1): 1–39.

Tajfel, Henri, M. G. Billig, R. P. Bundy, and Claude Flament. 1971. "Social categorization and intergroup behaviour." *European Journal of Social Psychology*, 1 (2): 149–178.

Tajfel, Henri, Jane Sell, W. I. Griffith, and Rick K. Wilson. 1993. "Are Women More Cooperative Than Men in Social Dilemmas?" *Social Psychology Quarterly*, 56 (3): 211–222.

Tanaka, Tomomi and Colin F. Camerer. 2016. "Trait perceptions influence economic out-group bias: Lab and field evidence from Vietnam." *Experimental Economics*, 19 (3): 513–534.

Traub, Stefan, Christian Seidl, Ulrich Schmidt, and Maria Vittoria Levati. 2005. "Friedman, Harsanyi, Rawls, Boulding – or somebody else? An experimental investigation of distributive justice." *Social Choice and Welfare*, 24 (2): 283–309.

Turner, J. C., R. J. Brown, and Henri Tajfel. 1979. "Social comparison and group interest in ingroup favouritism." *European Journal of Social Psychology*, 9 (2): 187–204.

Appendices

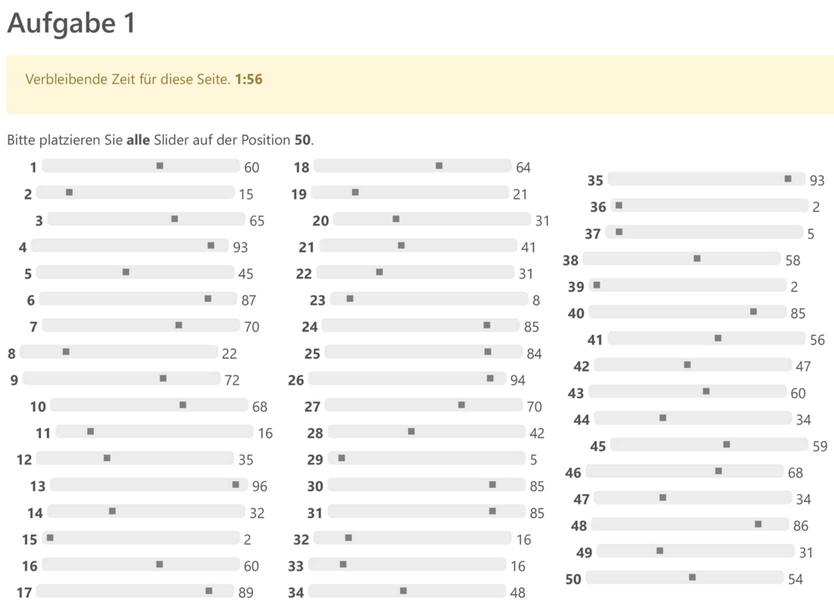
2.A Appendix

Table 2.4: Detailed Sample Characteristics

	Mean	SD	Median	IQR	Min	Max	Obs
Female	0.50	0.50	0.5	1	0	1	172
Age in years	25.71	4.53	25	4.5	18	47	172
Dual citizenship	0.05	0.22	0	0	0	1	172
Participant born in Germany	0.95	0.22	1	0	0	1	172
Mother born in Germany	0.94	0.23	1	0	0	1	172
Father born in Germany	0.90	0.30	1	0	0	1	172
Political left to right	2.53	0.59	3	1	1	4	172
Closeness	2.34	0.86	2	1	1	5	172

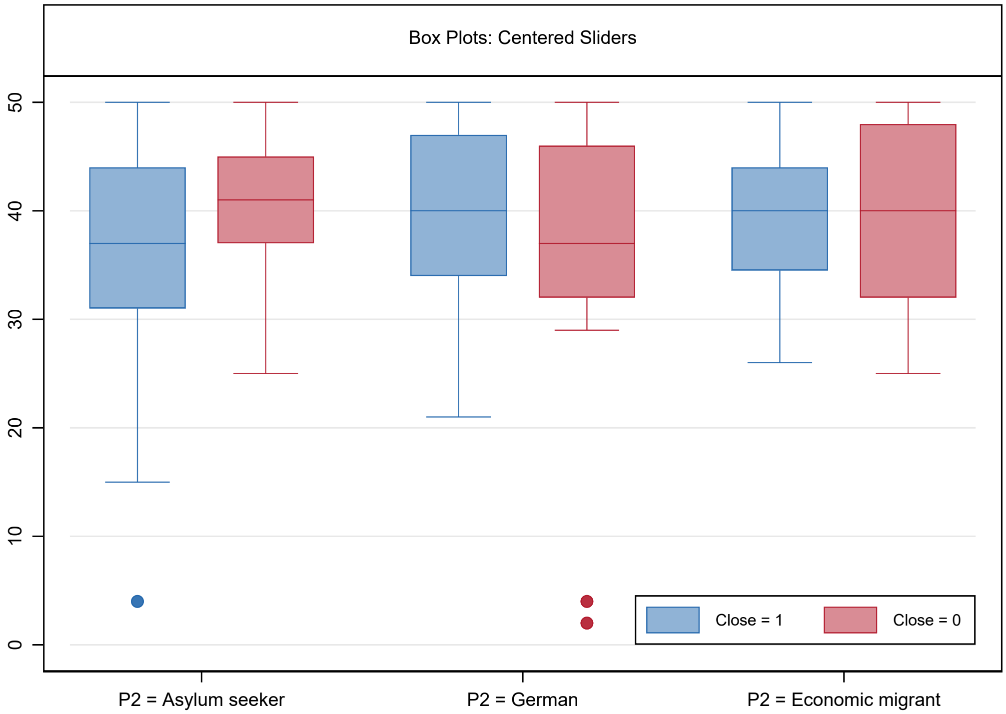
Notes: Table displays summary statistics of sample characteristics. "Female" is the average share of females. "Age in years" is the average age in years. "Dual citizenship" is the share of participants holding a dual citizenship. "Participant born in Germany" is the share of participants born in Germany, analogously for "Mother/Father born in Germany" variables. "Political left to right" is ranging from 1 (very left) to 5 (very right). "Closeness" is a measure of closeness to Germans, ranging from 1 (very close) to 5 (very distant).

Figure 2.3: Screenshot: Slider Task



Notes: This figure shows an exemplary screenshot from the variant of the slider task we used in the computer laboratory.

Figure 2.4: Boxplot: Effort by Treatment and Closeness



Notes: This figure shows Tukey's boxplots for the effort measure by treatment and self-reported ingroup identification.

2.B Online Appendix

Supplementary Online Material for

Does ethnic heterogeneity decrease workers' effort in the presence of income redistribution? An experimental analysis

Christoph Schütt ⓡ David Pipke ⓡ Lena Detlefsen ⓡ Gianluca Grimalda

2.B.1 Further Analyses

The regressions in this section provide several additional insights that serve as robustness checks for our main results. In regressions in the first column of Table 2.5, we excluded extreme values in the number of centered sliders by the method of Tukey's fences (more than 1.5 times the IQR below or above the 25th and 75th percentile over all treatment conditions, respectively). We interacted the female dummy with treatments in the second column to explore heterogeneity with respect to gender. The results are unaffected by excluding outliers. There is no significant treatment heterogeneity concerning gender. Table 2.6 shows the results from the same regression models as in the main text (see Table 2.3) using OLS instead of Tobit random-effects. The OLS regressions deliver qualitatively equivalent results.

Table 2.7 shows results from regressions where we replaced the closeness indicator to explore treatment heterogeneity with indicator variables related to the respondent's views on immigrants, asylum seekers, and economic migrants. The indicator variables are equal to one if the PCA-index based on the questions in 2.B.4 concerning views on immigrants, asylum seekers, and economic migrants indicates views that are worse (less positive views on the groups) than the median views in the sample. We tested the index reliability using Cronbach's alpha. The three indices concerning views on immigrants in general, asylum seekers, and economic migrants have an alpha of 0.745 (five items, average interitem covariance = 0.326), 0.757 (six items, average interitem covariance = 0.249), and 0.548 (four items, average interitem covariance = 0.232), respectively. Unlike utilizing the closeness indicator, the results show no significant treatment heterogeneity. This finding supports the view that such questions are more likely to be prone to social desirability biases than questions focusing on closeness to specific groups, which do not imply animosity towards outgroups.

Table 2.5: Robustness Checks: Outliers and Female

	(1) Effort	(2) Effort
German	-3.052	0.626
	(2.14)	(1.88)
Economic migrant	-1.821	2.104
	(2.01)	(2.00)
Round 2	3.997****	3.923****
	(0.43)	(0.47)
Round 3	5.780****	5.711****
	(0.42)	(0.47)
Female	-6.078****	-5.322***
	(1.03)	(1.93)
2x Efficiency	-0.394	-0.490
	(0.40)	(0.44)
Belief about tax	0.005	-0.003
	(0.01)	(0.01)
German × Female		-0.558
		(2.69)
Economic migrant × Female		-1.476
		(2.71)
Close	-4.023**	
	(1.86)	
German × Close	5.542**	
	(2.62)	
Economic Migrant × Close	4.732*	
	(2.55)	
Constant	45.62****	42.12****
	(2.81)	(2.95)
Obs.	491	494
Right-censored	46	46
Left-censored	0	0
No. of panels	172	172
Log-likelihood	-1388.2	-1438.7
Round 2 = Round 3	$p < 0.001$	$p < 0.001$
2x Eff. = 0.5x Eff.		
German = Economic migrant	$p = 0.543$	$p = 0.419$
German = 0	$p = 0.154$	$p = 0.739$
Economic migrant = 0	$p = 0.366$	$p = 0.293$
Close = 0	$p = 0.030$	
Close + German × Close = 0	$p = 0.412$	
Close + Economic migrant × Close = 0	$p = 0.684$	
German + German × Close = 0	$p = 0.099$	
Economic migrant + Economic Migrant × Close = 0	$p = 0.062$	
Economic migrant (1 + Close) = German (1 + Close)	$p = 0.783$	

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of centered sliders. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Regression in the first column excludes extreme outliers for which the number of centered sliders lies outside the Tukey's fence defined by 1.5 times the IQR below or above the 25th and 75th percentile. Below "Log-likelihood" we report p-values from Wald tests.

Table 2.6: OLS Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
German	0.444	-3.528	-3.606	-1.876	12.715*
	(1.32)	(2.51)	(2.56)	(4.51)	(6.92)
Economic migrant	1.266	-1.663	-1.735	0.078	11.839
	(1.32)	(2.08)	(2.08)	(4.74)	(7.50)
Round 2	3.694****	3.644****	3.602****		
	(0.43)	(0.43)	(0.43)		
Round 3	5.321****	5.273****	5.232****	-0.265	-0.265
	(0.47)	(0.47)	(0.47)	(2.26)	(2.26)
Female	-5.356****	-5.280****	-5.253****	-4.387	-4.448
	(1.03)	(1.03)	(1.02)	(3.72)	(3.65)
2x Efficiency	-0.661*	-0.576	-0.505	-4.719**	-4.719**
	(0.38)	(0.37)	(0.32)	(1.89)	(1.89)
0.5x Efficiency				6.878***	6.878***
				(2.16)	(2.17)
Belief about tax	-0.013	-0.006			
	(0.02)	(0.02)			
Close		-3.994**	-4.080**		14.068*
		(1.94)	(1.98)		(7.26)
German × Close		5.948**	6.080**		-21.728**
		(2.88)	(2.97)		(8.91)
Economic Migrant × Close		4.469*	4.579*		-18.196*
		(2.57)	(2.60)		(9.60)
Constant	41.865****	44.527****	44.322****	44.262****	33.675***
	(2.17)	(2.50)	(2.49)	(9.47)	(10.60)
Obs.	494	494	494	494	494
No. Clusters	172	172	172	172	172
R2	0.210	0.232	0.231	0.051	0.077
Adj. R2	0.192	0.209	0.211	0.031	0.052
Hypothesis tests (p-values)					
Round 2 = Round 3	p < 0.001	p < 0.001	p < 0.001		

Table 2.6: OLS Regressions

	(1) Effort	(2) Effort	(3) Effort	(4) Beliefs	(5) Beliefs
2x Eff. = 0.5x Eff.				$p < 0.001$	$p < 0.001$
German = Economic migrant	$p = 0.473$	$p = 0.449$	$p = 0.448$	$p = 0.626$	$p = 0.893$
German = 0	$p = 0.737$	$p = 0.162$	$p = 0.161$	$p = 0.678$	$p = 0.068$
Economic migrant = 0	$p = 0.339$	$p = 0.426$	$p = 0.406$	$p = 0.987$	$p = 0.116$
Close = 0		$p = 0.041$	$p = 0.041$		$p = 0.054$
Close + German × Close = 0		$p = 0.369$	$p = 0.363$		$p = 0.143$
Close + Economic migrant × Close = 0		$p = 0.778$	$p = 0.767$		$p = 0.508$
German + German × Close = 0		$p = 0.106$	$p = 0.102$		$p = 0.110$
Economic migrant + Economic Migrant × Close = 0		$p = 0.078$	$p = 0.077$		$p = 0.282$
Economic migrant (1 + Close) = German (1 + Close)		$p = 0.752$	$p = 0.762$		$p = 0.590$
German × Close = 0		$p = 0.040$	$p = 0.042$		$p = 0.016$
Economic migrant × Close = 0		$p = 0.083$	$p = 0.080$		$p = 0.060$

Notes: Table shows OLS regression results. Standard errors (clustered at the individual level) in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Dependent variables are the number of centered sliders (first three columns) and beliefs about the tax rate (last two columns).

Table 2.7: Treatment Heterogeneity w.r.t. Views

	(1) Effort	(2) Effort	(3) Effort
German	-0.075	-0.961	-0.664
	(1.93)	(1.90)	(1.91)
Economic migrant	2.323	2.123	0.346
	(1.82)	(1.94)	(1.80)
Round 2	3.927****	3.929****	3.915****
	(0.47)	(0.47)	(0.47)
Round 3	5.714****	5.718****	5.704****
	(0.47)	(0.47)	(0.47)
Female	-6.031****	-5.950****	-5.918****
	(1.11)	(1.10)	(1.11)
2x Efficiency	-0.493	-0.497	-0.476
	(0.44)	(0.44)	(0.437)
Belief about tax	-0.004	-0.004	-0.002
	(0.01)	(0.01)	(0.01)
German × H	0.550	2.369	1.882
	(2.69)	(2.68)	(2.70)
Economic migrant × H	-2.304	-1.955	2.048
	(2.71)	(2.68)	(2.73)
H	0.718	-1.595	-0.807
	(1.91)	(1.90)	(2.00)
Constant	42.419****	44.243****	42.607****
	(3.02)	(3.017)	(2.971)
Obs.	494	494	494
Right-censored	46	46	46
Left-censored	0	0	0
No. of panels	172	172	172
Log-likelihood	-1400	-1400	-1400

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of centered sliders. H is a dummy indicating a value of worse than the median concerning the PCA-index of attitudes towards asylum seekers (column 1), economic migrants (column 2), and migrants in general (column 3). Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task.

Table 2.8: Touched Sliders as Effort Measure

	(1) Effort	(2) Effort	(3) Effort
German	0.042	-3.627*	-3.574*
	(1.23)	(2.10)	(2.10)
Economic migrant	0.532	-2.410	-2.356
	(1.22)	(1.98)	(1.98)
Round 2	4.133****	4.114****	4.148****
	(0.47)	(0.46)	(0.46)
Round 3	5.557****	5.539****	5.571****
	(0.46)	(0.46)	(0.46)
Female	-5.956****	-5.917****	-5.938****
	(1.02)	(1.01)	(1.01)
2x Efficiency	-0.533	-0.507	-0.564
	(0.43)	(0.43)	(0.41)
Belief about tax	0.003	0.005	
	(0.01)	(0.01)	
Close		-3.740**	-3.673**
		(1.83)	(1.82)
German × Close		5.534**	5.438**
		(2.57)	(2.56)
Economic Migrant × Close		4.567*	4.483*
		(2.50)	(2.50)
Constant	44.605****	47.224****	47.397****
	(2.53)	(2.76)	(2.74)
Obs.	494	494	494
Right-censored	57	57	57
Left-censored	0	0	0
No. of panels	172	172	172
Log-likelihood	-1400	-1400	-1400
Round 2 = Round 3	p < 0.001	p < 0.001	p < 0.001
German × Close = 0		p = 0.031	p = 0.034
Economic migrant × Close = 0		p = 0.068	p = 0.073
German = Economic migrant	p = 0.686	p = 0.539	p = 0.540
German = 0	p = 0.973	p = 0.084	p = 0.088
Economic migrant = 0	p = 0.664	p = 0.223	p = 0.233
Close = 0		p = 0.041	p = 0.044
Close + German × Close = 0		p = 0.322	p = 0.330
Close + Economic migrant × Close = 0		p = 0.629	p = 0.636
German + German × Close = 0		p = 0.198	p = 0.208
Economic migrant + Economic Migrant × Close = 0		p = 0.159	p = 0.164
Economic migrant (1 + Close) = German (1 + Close)		p = 0.867	p = 0.862

Notes: Table shows panel data regression results from a Tobit random-effects model accounting for left-censoring at 0 and right-censoring at 50. Dependent variable is the number of touched sliders. Robust standard errors in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$. All regression include a control for self-reported political orientation, subjects older than 30 years, a dummy indicating data being from the last session (only data for the first round), and a dummy variable for little fun reported in the questionnaire during the task. Below "Log-likelihood" we report p-values from Wald tests.

Table 2.9: Mediation of Effort Differences by Beliefs

	Model M1 (No Beliefs)	Model M2 (With Beliefs)	Difference (M1 - M2)	Mediation (Percentage)
Close to other Germans				
German = Asylum seeker	2.53	2.52	0.01	0.24%
German = Economic migrant	-0.44	-0.44	0.00	-0.23%
Economic migrant = Asylum seeker	2.97	2.96	0.01	0.17%
Not close to other Germans				
German = Asylum seeker	-4.15	-4.14	-0.01	0.22%
German = Economic migrant	-2.37	-2.37	0.00	0.00%
Economic migrant = Asylum seeker	-1.78	-1.77	-0.01	0.51%
Close = 1 vs. Close = 0				
German	2.60	2.60	0.00	0.15%
Asylum seeker	-4.08	-4.07	-0.01	0.27%
Economic migrant	0.67	0.67	0.00	0.45%

Notes: The table shows the differences in effort dependent on the recipient's type for the model without (columns 1) and with beliefs about the tax rate (column 2) included in the regression model. Differences are always calculated as the effort in the first-mentioned condition minus the second condition. Coefficients are based on the main results of columns 2 and 3 in the main regression results table. The mediation in percentage terms is calculated as the difference between the estimates based on the models with and without beliefs, relative to the model without beliefs.

2.B.2 Detailed summary statistics: Effort and beliefs

Table 2.10: Detailed Summary Statistics: Effort and Beliefs

	Mean	SE	Median	IQR	Min	Max	Obs.
Efforts							
Round 1							
P2 Asylum seeker	34.96	1.08	35	10	4	50	55
P2 German	36.42	0.85	35	9	21	50	59
P2 Economic migrant	37.07	0.96	36.5	11	25	50	58
All P2	36.17	0.56	35	10	4	50	172
Round 2							
P2 Asylum seeker	39.06	1.17	41	13	15	50	51
P2 German	40.20	1.16	40.5	12	2	50	54
P2 Economic migrant	39.84	0.95	40	12	25	50	56
All P2	39.71	0.63	40	12	2	50	161
Round 3							
P2 Asylum seeker	40.16	1.17	42	12	15	50	51
P2 German	41.67	1.15	44.5	11	4	50	54
P2 Economic migrant	42.13	0.93	43.5	12	26	50	56
All P2	41.35	0.62	44	12	4	50	161
All Rounds							
P2 Asylum seeker	37.98	0.68	39	13	4	50	157
P2 German	39.34	0.63	39	13	2	50	167
P2 Economic migrant	39.65	0.57	40	13	25	50	170
All P2	39.01	0.36	39	13	2	50	494
Efficiency 2x							
P2 Asylum seeker	39.67	1.20	41	13	15	50	51
P2 German	40.37	1.19	41	13	4	50	54
P2 Economic migrant	40.73	0.93	41	12	25	50	56
All P2	40.27	0.64	41	12	4	50	161
Efficiency 0.5x							
P2 Asylum seeker	39.55	1.15	41	13	15	50	51
P2 German	41.50	1.13	44	12	2	50	54
P2 Economic migrant	41.23	0.97	41.5	13.5	26	50	56
All P2	40.79	0.62	42	13	2	50	161
Beliefs							
Round 1							
P2 Asylum seeker	35.60	3.83	40	58	0	100	55
P2 German	32.73	2.90	40	20	0	100	59
P2 Economic migrant	34.34	3.41	40	40	0	100	58
All P2	34.19	1.94	40	36	0	100	172
Round 2							
P2 Asylum seeker	35.27	4.42	40	58	0	100	51
P2 German	33.57	3.74	30	57	0	100	54
P2 Economic migrant	34.95	3.53	40	36	0	100	56
All P2	34.59	2.23	40	57	0	100	161
Round 3							
P2 Asylum seeker	33.55	4.47	20	58	0	100	51
P2 German	35.00	3.68	40	40	0	100	54

Table 2.10: Detailed Summary Statistics: Effort and Beliefs

	Mean	SE	Median	IQR	Min	Max	Obs.
P2 Economic migrant	34.59	3.69	30	47.5	0	100	56
All P2	34.40	2.26	40	58	0	100	161
All Rounds							
P2 Asylum seeker	34.83	2.43	40	58	0	100	157
P2 German	33.74	1.97	40	20	0	100	167
P2 Economic migrant	34.62	2.03	40	55	0	100	170
All P2	34.39	1.23	40	57	0	100	494
Efficiency 2x							
P2 Asylum seeker	28.94	4.12	20	38	0	100	51
P2 German	29.06	3.51	20	38	0	100	54
P2 Economic migrant	28.13	2.91	20	27.5	0	100	56
All P2	28.70	2.02	20	38	0	100	161
Efficiency 0.5x							
P2 Asylum seeker	39.88	4.62	40	58	0	100	51
P2 German	39.52	3.76	40	40	0	100	54
P2 Economic migrant	41.41	4.00	40	55.5	0	100	56
All P2	40.29	2.37	40	57	0	100	161

Notes: Table shows detailed summary statistics for effort (no. of centered sliders) and beliefs about tax rates to be chosen by the third person. Efficiency factor always equal to 1 in first round.

2.B.3 Instructions in the experiment

We outline the structure of the experimental platform on which the participants completed the task, stated their beliefs about the share to be redistributed by the third-person allocator, and filled out a questionnaire. The whole experiment was conducted at the computer laboratory, where participants received the following (translated) instructions on their computers. The text in square brackets varies across the different treatments. Text written in italics was not shown to participants. The original instructions in the German language are available upon request.

First Part: Introduction You are taking part in a study on economic decisions and are then asked to make several decisions. Please read the following instructions carefully. In this study, you have the opportunity to earn money, which you will be paid out individually and receive in cash at the end of the study. During the study, you are not allowed to talk to the other participants. If you have a question, we ask you to raise your hand, after which an experimenter will come to you and help you.

The study consists of two parts. In the first part of the study, you will be asked to complete three tasks. In these tasks, you have the opportunity to earn money. The amount of your earnings depends on someone else's decision. The second part of the study consists of a questionnaire. Please read the following explanations carefully.

Second Part: Explanations In this first part of the study, two other people are involved in addition to you. We will call them Person 2 and Person 3. Person 2 and Person 3 are real people that exist in reality. Therefore, any information you receive about either person is truthful. Both individuals are not participating in this study but have already participated in another study.

We ask you to complete three tasks below. In these tasks, you have the opportunity to earn money. After processing of the tasks, a task is randomly selected as payment-relevant. Your earnings from this randomly selected task can then be transferred in whole or in part to Person 2. Person 3 decides how much is transferred. So Person 3 can transfer 0%, 20%, 40%, 60%, 80% or 100% of your earnings to Person 2. Person 3's decisions were made prior to this study in another study. We will randomly assign you a decision of a person 3. At the end of the study, your earnings and carryover will be paid to you and Person 2 according to Person 3's decision.

In each of the tasks, you will be shown 50 sliders. You can set each slider to any position between 0 and 100 by pressing and dragging the slider to the desired position with your computer's mouse. You will see the current position displayed on the right side of the slider. Your task is to place all sliders on position 50. You have 2 minutes to do this.

In the image below, you can see two examples. The top slider has a current position of 28. So, it is not correctly placed. The lower slider has the current position of 50 and is therefore correctly placed.

If you manage to place at least 25 of the 50 sliders in the correct position, you will receive €5. If you cannot do this, you will not receive any payout from the respective task. For each additionally correctly placed slider, you will receive €0.20.

Third Part: Task explanation Before you process the task, you will receive the following information about Person 2 and Person 3. Person 3 is a German citizen who earned €5 in a previous study. Person 2 is [An asylum seeker / A German citizen / An economic migrant] who has not earned €5 in a previous study.

[The amount that Person 3 transfers from you to Person 2 is transferred one-to-one in this task / The amount that Person 3 transfers from you to Person 2 is doubled in this task. Hence, twice the selected amount goes to person 2. / The amount that Person 3 transfers from you to Person 2 is halved in this task. Hence, half of the selected amount goes to person 2.]

Fourth Part: Slider task (Effort measure) and subsequent beliefs elicitation *Participants were shown a screen with 50 sliders in a randomly determined initial position in each round, as depicted in Figure 2.3 above. After each round of performing the slider task, beliefs about the share redistributed by the third-person allocator were elicited.*

What do you think? Which percentage of your earnings from this task will person 3 transfer from you to the [asylum seeker / German citizen / economic migrant]? If your estimate is correct, you will receive an additional €0.5.

[Remember that the amount is transferred one to one. / Remember that twice the amount is transferred. / Remember that half of the amount is transferred.]

Fifth Part: Questionnaire *We list the questionnaire's items we used in the analyses in Section 2.B.4.*

Sixth Part: Comments and end of the study Here you have the opportunity to give us feedback on the study: *(Empty text-box where participants could provide feedback.)*

Thank you for your participation. Task [1 / 2 / 3] was randomly determined to be relevant for payout. In this task, you have correctly placed # sliders.

(Participants were informed about their earnings depending on their performance and the correctness of their beliefs.)

2.B.4 Items from the questionnaire

Below we show relevant questions from the post-experimental questionnaire (translated from the original German version) that we used to construct variables for our analyses. The original German version of the questionnaire is available upon request.

- How old are you? (Enter your age)
- Please enter your gender: 1 Male 2 Female 3 Non-binary
- Do you have another citizenship besides German? 1 Yes 0 No
- Were you born in Germany? 1 Yes 0 No
- Was your mother born in Germany? 2 Don't know 1 Yes 0 No
- Was your father born in Germany? 2 Don't know 1 Yes 0 No
- Were your grandparents born in Germany? 1 Yes 2 No 3 Partly 4 Don't know
- Do you belong to a religious group? If yes, which one? (1 I don't belong to any religion 2 Protestant church 3 Catholic church 4 Christian Orthodox churches 5 Islam 6 Judaism 7 Other)
- Please use the following scale to indicate how much you enjoyed the tasks: Very much 1 2 3 4 5 6 7 Not at all
- How close are you to the following groups? (Scale: 1 Very close 2 Close 3 Not decidedly 4 Distant 5 Very distant)
  - People in your city
  - Germans
  - Europeans
  - People all over the world
- Many people use the terms 'left' and 'right' to denote different political views. If you think about your own political views, where would you place them on this scale? 1 Very left 2 Left 3 Center 4 Right 5 Very right

We base our PCA-indices used in the regressions in Table 2.7 on the following questions focussing on attitudes towards immigrants (in general), asylum seekers, and economic migrants.

- Views on immigrants (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)
  - Immigrants increase crime rates.
  - Immigrants are generally good for Germans Economy.
  - Immigrants are taking jobs away from people who were born in Germany.
  - The foreigners living in the Federal Republic should adapt their lifestyle to the lifestyle of the Germans.

- Germany is currently taking in too many migrants.

- Views on asylum seekers (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)
  - People who have received asylum in Germany should receive financial support from the German state to ensure their livelihood.
  - People who have received asylum in Germany should receive free access to support that facilitates integration.
  - Asylum seekers who have not yet received asylum in Germany should receive financial support from the German state to ensure their livelihood.
  - Asylum seekers who have not yet received asylum in Germany should receive free access to support that facilitates integration.
  - People who apply for asylum in Germany are mainly politically persecuted people who have a right to asylum.
  - People who apply for asylum in Germany are mainly people who come to Germany for economic reasons and have no right to asylum.

- Views on economic migrants (Scale: 1 Strongly agree 2 Agree 3 Neutral 4 Disagree 5 Strongly disagree)
  - Migrants who came to Germany for economic reasons and have no right to asylum should receive financial support from the German state to ensure their livelihood.
  - Migrants who have come to Germany for economic reasons and have no right to asylum should receive free access to support that facilitates integration.
  - Migrants who come to Germany for economic reasons are mainly citizens from other European countries.
  - Migrants who come to Germany for economic reasons are mainly citizens from non-European countries.

Chapter 3

Kind or contented? An experimental investigation of the impact of bonus payments on workers' productivity

Francesco Bogliacino[a], Gianluca Grimalda[b], David Pipke[c]

An earlier version of this paper is available at http://hdl.handle.net/10419/246590 (permalink) as Bogliacino, F., Grimalda, G., & Pipke, D. (2021). Kind or contented? An investigation of the gift exchange hypothesis in a natural field experiment in Colombia, Kiel Working Paper, No. 2199, Kiel Institute for the World Economy (IfW Kiel), Kiel.

Bogliacino acknowledges funding from Fundación Universitaria Konrad Lorenz (Internal Grant 7INV1141: "Desigualdad, aspiraciones y esfuerzo: un análisis experimental"), Open Evidence (Research Grant 009_Inequalities "Desigualdad, Aspiraciones y esfuerzo: Un análisis experimental"), and Universidad Nacional (Internal Grant 26007 "Desigualdad, Aspiraciones y Compromiso Individual"). We are grateful for helpful and constructive comments from Oriana Bandiera, Alexander W. Cappelen, John List, Till Requate, Ulrich Schmidt, Bertil Tungodden, and all the participants at the BEBES seminar in Bogotá, the Brown Bag seminar at Kiel University, the FAIR Workshop at NHH, and the Special ESA Meeting 2022 in Buenos Aires. Suelen Castiblanco, Laura Jiménez and Daniel Reyes provided excellent research assistance.

[a]Universidad Nacional de Colombia (fbogliacino@unal.edu.co)
[b]Kiel Institute for the World Economy (gianluca.grimalda@ifw-kiel.de)
[c]Kiel Institute for the World Economy (david.pipke@ifw-kiel.de)

Abstract

The gift exchange hypothesis postulates that workers reciprocate above market-clearing wages with above-minimum productivity. However, the impacts on workers' productivity of selective bonuses and of the reason offered for bonuses are unclear. In a natural field experiment, we assign unexpected monetary bonuses to either one or both workers in a pair. Selective bonuses reward relative productivity, need, or luck. Overall, bonuses decrease productivity, especially when both receive bonuses. Bonus recipients and nonrecipients react differently to treatments, probably because of status-seeking or inequality-averse behavior. We calibrate a theoretical model embedding inequality aversion where workers can react reciprocally or opportunistically to bonuses.

JEL Codes: C93, D63, D91, J31, J33

Keywords: Gift exchange, Employer-worker Relationship, Pay inequality, Natural field experiment, Reciprocity, Fairness, Contentment

3.1 Introduction

According to standard competitive market theory, workers are employed at the market-clearing wage and provide the minimal effort required not to get fired by the firm (Lazear, 2000). Several economic mechanisms such as efficiency wages (Katz, 1986), implicit contracts (Azariadis, 1975), and insider-outsider relationships (Lindbeck and Snower, 1988) can explain evidence at odds with this prediction. Another such mechanism is eminently psychological and rests on the notion of reciprocity (Rabin, 1993). That is the disposition to repay kind actions with kindness and spiteful actions with spite (Fehr and Gächter, 1998, 2000). The seminal work by Akerlof (1982) and Akerlof and Yellen (1990) reported anecdotal evidence consistent with what was called a "gift exchange" (Adams, 1963). Firms paying wages higher than the market-clearing level would be repaid by workers through the provision of effort above the minimal possible. The gift exchange hypothesis has received support in both laboratory experiments (Fehr et al., 1993, 1998a; Charness, 2004) and controlled experiments conducted in natural settings (Gneezy and List, 2006; Cohn et al., 2015; Gilchrist et al., 2016; Englmaier and Leider, 2020). However, some natural experiments have suggested that such productivity gains may be transient (Gneezy and List, 2006; Bellemare and Shearer, 2009) or even negligible (Kube et al., 2012; Esteves-Sorenson, 2018; DellaVigna et al., 2022). Overall, it is still an open question whether individual behavior at the workplace follows the gift exchange hypothesis.

Two topics have not been systematically investigated in this literature. Firstly, the firm may be willing to increase the wages only for some of its workers. This would be the case should the firm's resources be insufficient to increase all workers' wages. Alternatively, it could be a firm's goal to reward the most productive workers. It is unclear how workers will react to selective rewards. To a large extent, experimental studies on gift exchange have only analyzed dyadic relationships where only one principal and one agent interact. Such studies are then silent on how horizontal inequality affects workers' reciprocity. Self-regarding individuals should be indifferent to what happens to others. However, both experimental studies and surveys suggest that many individuals are inequality-averse. That is, they experience envy if someone else is rewarded more than themselves and compassion otherwise (Fehr et al., 2009; Fehr and Schmidt, 1999; Bolton and Ockenfels, 2000). If inequality aversion is strong enough, then both disadvantaged and advantaged workers may react negatively to the firm offering selective rewards, thus reducing their effort. Such a strategy may then backfire on the firm.

Secondly, people are generally found to be more tolerant of inequality when this can be justified on the grounds of fairness (Konow, 2003). Typically, when inequality is caused by a worker being more productive than another worker because of their higher effort or ability, the resulting inequality is more accepted than when it is caused by luck (Krawczyk, 2010; Alesina and Giuliano, 2011; Durante et al., 2014; Almås et al., 2020). However, even if the firm tried to follow fair criteria to select who is the most deserving worker to reward, a self-serving bias[1] may cause unrewarded workers not to recognize the fairness of the process. Workers' resentment may lead to shirking or sabotage (Bewley, 1999). Conversely, if workers find the justification of rewards as fair, they may be willing to respond with positive reciprocity even if the reward is directed to another worker. It is then essential to ascertain how individuals weigh up reciprocity and inequality aversion with respect to different possible sources of inequality, which may be perceived as either fair or unfair. The existing literature has mainly analyzed only one possible source of wage inequality at a time, typically focusing on pay disparity that appeared arbitrary and not explicitly linked to individual merit (Hennig-Schmidt et al., 2010; Cohn et al., 2014), whereas our study introduces three different reasons for horizontal wage inequality.

The present study provides evidence on these topics from a controlled field experiment conducted in a natural work environment. We invited pairs of workers to a university department for a one-day data entry job which workers conducted individually. After workers finished their morning session, the job instructor announced a surprise bonus payment for one worker, worth one-third of the previously announced earnings.[2] We manipulated the source of earnings inequality across three treatments. The bonus was assigned based on (a) relative productivity in the Productivity treatment (with the more productive worker in the morning session receiving the bonus); (b) relative needs in the Needs treatment (evaluated through a measure of participants' socioeconomic status); or (c) no justification in the Arbitrary treatment (a method which we expected as being perceived as arbitrary by workers). These three treatment conditions are contrasted with two benchmark conditions in which (d) no worker receives a bonus (Control condition), or (e) both workers receive the bonus (Double Bonus condition). In this way, we can weigh motivations from reciprocity and inequality aversion as a response to the bonus by both the bonus recipient and the nonrecipient.

[1] Miller and Ross (1975) define the self-serving bias as individual psychological dispositions whereby "[...] people indulge both in self-protective attributions under conditions of failure and in self-enhancing attributions under conditions of success". A self-serving bias, then, prompts individuals to attribute their failure to situational factors, and their success to their own abilities (Deffains et al., 2016). See also Mezulis et al. (2004) and Babcock and Loewenstein (1997).

[2] The announcement of bonuses after the lunch break was a surprise in all treatment conditions and made clear what the final earnings would be to rule out confounds, e.g., due to performance incentives. Hence, the bonus is an unconditional act of kindness by the employer, consistent with the gift exchange hypothesis (Akerlof, 1982), without further contracted obligations or incentives. In somewhat related literature, Bradler et al. (2019) compare unconditional gifts and incentives by performance bonuses in two different tasks in the laboratory. See also Kosfeld and Neckermann (2011) and Gibbs et al. (2017) for field experiments studying the effects of rewards creating performance incentives.

We do this for three different sources of inequality, which we expected to be viewed as fair (in the Productivity treatment), unfair (in the Arbitrary treatment), or morally acceptable (in the Needs treatment). Assigning bonuses based on relative needs is a novel feature that has not yet been investigated in the context of labor market relations to the best of our knowledge.[3] We conjectured that this method may have stirred moral approval by both workers, thus spurring workers' dispositions to positive reciprocity.

We believe that the labor market interaction from our naturalistic setting is ideally suited to investigate gift exchange effects in the presence of horizontal pay inequality. Identifying causal effects from firm-level data would be confounded by the existence of many unobservable factors that could drive an observed correlation between effort and wages. This is because firms' strategies are equilibrium responses to exogenous conditions that are jointly determined with workers' behavior. Various unobservable drivers of work morale such as reputational concerns, social norms, rules of behavior, managerial practices, and interpersonal relationships may affect workers' productivity upon changes in the firm wage structure. Even when we could observe discrete changes in remuneration policy, a selection problem in identifying a proper counterfactual would arise because multiple omitted variables may vary at the same time of the treatment across organizations. Our experimental approach minimizes such confounds and allows for causal inference.

The gift exchange hypothesis has received extensive support in the context of laboratory experiments (Fehr et al., 1993; Fehr and Falk, 1999; Charness, 2004; Charness et al., 2004) and some support in field studies (Cohn et al., 2015; Gilchrist et al., 2016; Englmaier and Leider, 2020). However, other studies predominantly utilizing data entry tasks found only statistically insignificant effects of monetary pay rises (Hennig-Schmidt et al., 2010; Kube et al., 2012, 2013; Al-Ubaydli et al., 2015; Esteves-Sorenson, 2018; DellaVigna et al., 2022) on productivity, were limited to the supply of extra work (DellaVigna et al., 2022), or effects turned out to be largely transient (Gneezy and List, 2006; Bellemare and Shearer, 2009). Gneezy and List (2006) report initial productivity boosts in response to surprisingly higher than advertised wages in two samples. However, the productivity rise waned quickly after the gift over the six hours of the experiment. The student workers were either hired to perform a data entry task for a library or a fundraising task for a charity. While the productivity boost lost statistical significance as early as after 90 minutes among students hired to perform the data entry task, it also diminished but remained overall statistically significant with the fundraising task. Bellemare and Shearer (2009) report a similar finding based on

[3]In his discussion of distributive justice, Konow (2003) suggests that assigning resources based on relative needs is one principle, among several others, that is usually seen as fair by individuals. Nicklisch and Paetzel (2020) provide experimental support to this claim.

an experiment in a real tree-planting firm in British Columbia. A one-day pay rise significantly increased the number of trees planted on that day compared to previous and subsequent days, especially among experienced workers.

Our study contributes to understanding how horizontal inequality affects productivity at the workplace. Laboratory studies examining pay inequality between two workers generally found that workers are less sensitive to other workers' treatment than their own treatment. Ceteris paribus, workers' behavior is also less responsive to advantageous inequality than disadvantageous inequality (Charness et al., 2007; Gächter and Thöni, 2010; Bracha et al., 2015). Only few studies investigated the effects of pay inequality within natural field experiments. Hennig-Schmidt et al. (2010) did not find evidence for peer comparison effects, neither for advantageous nor disadvantageous wage inequality. Cohn et al. (2014) found that workers whose wage was cut decreased effort twice as much as when both team members' wage was cut. However, this was not the case when only the coworker's wage was cut, pointing to an asymmetric effect. Breza et al. (2018) found that wage inequality led to lower attendance and output when coworkers' productivity was hard to observe in a large-scale field experiment with Indian manufacturing workers. However, such a negative effect of inequality disappeared when workers learned that inequality reflected differences in their baseline productivity. Cullen and Perez-Truglia (2022) find that workers' effort increases when they learn that their managers earn more than them, but the opposite occurs when their coworkers earn more. Overall, preexisting studies investigating selective worker rewards had done so either in a laboratory context devoid of many features characterizing real-life work relationships or in a context where only one possible source of inequality - either luck or productivity - existed. Our framework enables us to systematically assess the relevance of different sources of inequality in workers' productivity responses to selective pay rises.

This paper also contributes to understanding the roles of fairness, legitimacy, and the behavioral responses to procedures at the workplace (Kahneman et al., 1986; Konow, 2000, 2003; Bolton et al., 2005; Cappelen et al., 2007; Trautmann, 2009) and the theoretical literature using social preferences to explain labor market outcomes. Our theoretical model merges two types of social preferences that have rarely been considered jointly thus far (Charness and Rabin, 2002; Falk and Fischbacher, 2006; Cox et al., 2007; Cohn et al., 2014), i.e., reciprocity (Rabin, 1993) and inequality aversion (Fehr and Schmidt, 1999). We hypothesized that workers would be willing to "punish" (Fehr and Fischbacher, 2004) the firm with lower effort when not receiving the bonus while the other worker received the bonus. For the bonus recipient, our model accommodates decreasing effort if the worker is inequality averse as in Fehr and Schmidt (1999), or an increase in the effort if the worker desires to repay the firm for having been singled out as the bonus recipient as in the status-seeking model of Frank (1985).

Contrary to the gift exchange hypothesis, our main result is that pay rises decreased productivity. This effect was most substantial in the Double Bonus condition, where both workers received the bonus. It also occurred in all three treatments where only one worker received the bonus. Our post-diction is that this result is due to what we define as a *contentment* effect. Workers may interpret the bonus as a signal that the manager is content with their work and is unlikely to punish or even fire them, thus feeling entitled to reduce their effort. Our finding of an effort decrease in response to the unconditional bonus payment is consistent with traditional labor market models viewing workers as behaving opportunistically rather than positively reciprocating kind actions by their employer (List and Rasul, 2011). Although traditional economic models predict this behavior, the emergence of a contentment effect has not been described before despite the wealth of studies on this topic.

Furthermore, we do not find evidence that advantageous pay inequality leads to inequality aversion and thus negative reciprocity. Conversely, the productivity of bonus recipients in all three treatments is higher than in the Double Bonus condition, reaching statistical significance in the Productivity treatment. This result may be interpreted as weak evidence for status-seeking preferences as opposed to an aversion against advantageous pay inequality (Frank, 1985; Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011; Heffetz and Frank, 2011). Similarly, there are no significant effects of disadvantageous inequality among nonrecipients when the bonus is assigned arbitrarily or to the more productive worker. Only when the bonus is assigned to the worker who is needier, nonrecipients significantly reduce their effort relative to the Control condition. This result suggests that workers view a justification of pay inequality based on needs as inappropriate in the labor market.

Our main results are robust to several checks, including adjusting p-values for multiple hypothesis testing (see Section 3.4.4). Concerning the generalizability of our empirical results, we report on selection, attrition, naturalness, and scaling, the *SANS conditions* posed by List (2020), in the discussion (also see Goldszmidt et al. (2020) and Holz et al. (2020)).

The remainder of our paper is structured as follows. The following Section 3.2 describes the experimental design. In Section 3.3, we develop a theoretical model of worker effort based on which we develop our hypotheses regarding the experiment. Empirical results are presented in Section 3.4. Section 3.5 discusses the results, emphasizing their generalizability particularly to cultural contexts different from Colombia. Section 3.6 concludes.

3.2 Experimental design and procedures

Recruitment. — The study received ethical approval (#7INV1141) from the university review board of the Fundación Universitaria Konrad Lorenz, where the research sessions were conducted. The university is located in Colombia's capital Bogotá. Participants were recruited through advertisements at the university and social networks, ensuring that our sample includes both university students and people with lower levels of education (see Appendix Table 3.4 for sample characteristics). The advertisement invited people to register for a one-time work opportunity ruling out reemployment. This aspect should exclude reputational concerns and repeated game strategies. In addition, the advertisement mentioned that some basic abilities in computer work were needed. Participants were requested to send a CV as a part of their job application. No applicant was turned down, and no participant left the session after its start. The advertised hourly rate of 15,000 Pesos per hour (about 6 USD) stood well above the standard payment for temporary work. A pilot study ascertained that such a wage rate was necessary to ensure a smooth recruitment process and attract applicants with the required skill level, considering the possibility of significant commuting times in Bogotá. Workers' payment was independent of the number of entries that workers completed. A desired effort level was not mentioned throughout the recruitment process or the initial induction session. The final sample contains 236 participants[4] in 126 daily sessions between October 2014 and January 2015. In 15 cases where a recruited (co-)worker did not show up at the appointed time, a confederate acted as a coworker. The 15 participants were unaware of working with a confederate.[5] Confederates were not part of the analysis sample.

The sessions. — On each working day of fieldwork, two participants were invited to come to the university at 10 am. The same female research assistant acted as an instructor in all sessions. The fact that the working sessions were part of an experiment and that pay conditions would be manipulated across sessions was concealed to participants. A female or a male aid was also present on a rota basis. Workers were asked to sign an informed consent form for the handling of their personal data before the instructor started explaining the job. The signing of the standard consent form was customary by the University's regulations. It would have also been necessary if the work opportunity had been offered without any research purposes, thus not affecting the naturalness of the work opportunity. The work consisted of entering the street addresses of some randomly selected phone users into an Excel spreadsheet. Workers

[4]We had to remove one observation for which technical problems with the USB connector made it impossible to recover the exact output distribution between the morning and the afternoon session.

[5]The robustness checks in Section 3.4.4 show that our results are unaffected by excluding the 15 recruited workers who unconsciously worked with a confederate.

were prevented from talking with each other throughout the working day and carried out their work in separate rooms. This procedure prevented peer pressure or group bonding effects and workers communicating information on their productivity (Falk and Ichino, 2006). In addition, workers' computers were connected via the internet to our server computer. In this way, workers' hourly output could be monitored by researchers. The morning session lasted two hours. After a fifteen-minute lunch break[6], workers were separately reconvened to the job instructor's room, where the instructor announced treatment-specific instructions for the afternoon session in front of the workers Treatment conditions were randomized and administered according to a sequence that was randomly selected before the start of fieldwork. The afternoon session lasted three hours. At the end of the workday, workers filled out a questionnaire inquiring about demographic characteristics and their evaluation of the work session. Finally, they received their payment.

The treatments. — In the Control condition, work continued without any announcement during the break. In the Double Bonus condition, a bonus payment of 25,000 Pesos (about 10.5 USD, worth one-third of advertised earnings) was paid to both workers, mentioning no specific reason. Finally, workers were told that a 25,000 Pesos bonus would be paid to one worker in single-bonus treatments. Three different justifications for the bonus assignment were provided: The worker who had the higher number of entries in the morning session received the bonus in the productivity treatment. In the Needs treatment, the worker residing in the block[7] classified as less affluent according to the official evaluation of Bogotá city council ("estratificación socioeconómica") received the bonus.[8] In the Arbitrary treatment, a worker was assigned the bonus without giving any information on the used criteria. Instead, we mentioned that neither relative productivity nor relative needs determined the bonus assignment.[9] The surprise bonus' announcement (see instructions) emphasized the workers' final earnings (including the

[6]Lunch was provided for free by the researchers. The preference for a relatively short lunch break emerged during the pilot study, as workers typically preferred a short break to finish earlier.

[7]Each Bogotá dwelling is assigned a ranking (so-called "Estratificación socioeconómica"), ranging from 1 to 6, which classifies the value and quality of the housing to differentiate the payment of utility bills. The ranking used in the research session was taken from a copy of a utility bill, which participants were asked to bring to the work session. This procedure ensured that the Bonus assignment in the Needs treatment was based on truthful reporting. We asked participants to bring such a bill and hand it over to the instructor at the beginning of the session.

[8]The evaluation of socioeconomic stratification is commonly used as a proxy for socioeconomic status in Colombia (Hagenlocher et al., 2013; Martinsson et al., 2015; Bogliacino et al., 2018). If both blocks had the same evaluation, the bonus was assigned to the worker whose block was located in the poorer (according to the city council's classification) district. The majority of roughly 90 percent of Colombia's population lives in one of the first three strata considered poor, with considerable differences in the quality of dwellings even between these three.

[9]Each worker had a 50% probability of being assigned the bonus. We did not release this information because workers may have perceived an unbiased random procedure as a fair criterion (Bolton et al., 2005; Krawczyk, 2011; Trautmann, 2009; Schurter and Wilson, 2009). Our intention was instead to

bonus payment). This feature was to rule out that workers expected additional bonuses. Hence, it excludes the possibility of confounds, e.g., from performance incentives, which could otherwise influence the unconditional nature of the bonus (without further contracted obligations).

These three treatments were meant to evaluate workers' reactions to three different methods to introduce inequality in the wage structure, expecting that acceptance of inequality would be higher in treatments with higher perceived fairness. As Bewley (1999) argued, workers' perceived fairness of the internal pay structure depends on whether pay differences are based on reasonable and impartial criteria. The Productivity treatment arguably can be deemed as reasonable and impartial, as paying a productivity premium is common in the labor market and is justifiable by the objective to reward the worker bringing the higher profit to the firm. At the other side of the spectrum, the Arbitrary treatment is unlikely to be seens as reasonable and impartial, precisely because no reason is offered for the bonus assignment. This treatment was meant to mirror situations in the workplace where workers perceive promotions or differential treatments of workers as unfair.

Finally, the Needs treatment introduces a rarely used method in the labor market. Nonetheless, relative needs are psychologically salient for many people (Konow, 2003) and have wide-ranging policy relevance. Allocation of resources based on needs is often invoked as a principle of distributive justice (Nicklisch and Paetzel, 2020). Affirmative action or means-based intervention may be interpreted as preferential treatments based on needs. The purpose of the Needs treatment is to investigate workers' reaction to rewards that arguably redresses real-life inequalities, and may therefore be considered equitable for reasons different from economic productivity (Konow, 2003). We conjectured that the Needs treatment would be perceived as lying in the middle of the fairness spectrum, with the Productivity and Arbitrary treatments lying at its extremes.

Instructions, scripts, and experiment protocol are available in the Online Appendix (Section 3.B.2). The data and code were deposited at the Open Science Framework and are available for the purpose of replication upon request.

3.3 Theoretical framework

We propose a simple theoretical model of worker effort that shall guide our experiment's analysis. In the tradition of the gift exchange hypothesis (Akerlof, 1982; Akerlof and Yellen, 1990), the model captures the idea that workers' effort choice under fixed hourly pay depends on the generosity of the wage in comparison with the standard. We also

maximize the perceived unfairness of the procedure. The Arbitrary treatment was indeed perceived as the least fair of the three (see Section 3.4.3).

assume that individuals are concerned with horizontal pay inequality, i.e., how the own wage compares to the coworkers' wage. Finally, we build on a series of more general models incorporating reciprocity motives (Rabin, 1993; Dufwenberg and Kirchsteiger, 2004) or inequality aversion (Fehr and Schmidt, 1999) into individuals' utility functions.

3.3.1 The setup

Akin to DellaVigna et al. (2022), we propose a utility function for a worker that depends on three components:

$$U_i = w_i - c(e_i) + K_{f_i} \cdot e_i \tag{3.1}$$

The first two components are purely self-regarding, as they only depend on variables attaining to the agent. w_i is the wage earned by the worker, and $c(e_i)$ the disutility from exerting effort. As standard, we assume an invertible convex cost function $c(e_i) = re_i^2$ with $r > 0$ that is increasing in effort and fulfills the regularity conditions ensuring a unique solution. Those are $c'(e) > 0$, $c''(e) > 0$ and $\lim_{e\to\infty} c(e) = \infty$. The third component of (3.1) is other-regarding as it includes variables pertaining to other agents' payoffs. We call it the *social preferences* component. It is the product of social preferences towards the firm K_{f_i} and worker's effort e_i.

$$\begin{aligned} K_{f_i} = \kappa + \varphi \{ l \cdot (w_i - w_{norm}) + m \cdot (w_j - w_{norm}) \} \\ + \left\{ -(I_{w_i > w_j} \cdot \beta - I_{w_j > w_i} \cdot \alpha) \cdot (w_i - w_j) \right\} \end{aligned} \tag{3.2}$$

The social preferences towards the firm K_{f_i} consist of three parts. The first is a constant κ which may capture unconditional value attributed to the firm's payoff. This value may originate from altruism (Becker, 1974), social norms demanding positive effort in employment relations (Bénabou and Tirole, 2006b), or even utility from doing useful work (Ariely et al., 2008).[10] κ may also be interpreted as capturing fears of getting punished when not providing an acceptable level of effort to the employer (Lazear, 2000). In addition, we assume that workers' concerns about the employer's payoffs are affected by two terms. The first term captures the "gift exchange" Akerlof (1982) component. It expresses the worker's sensitivity to the firm paying wages above or below an exogenous norm. Such a social norm may coincide with the market equilibrium or the level set in previous bargaining or past interactions. Wages equal to the norm are perceived as neither kind nor unkind. In general, this motivation should apply to both the worker's own and the other worker's wage (Akerlof, 1982). Accordingly, the higher $(w_i - w_{norm})$

[10] See DellaVigna et al. (2022) for a similar theoretical approach.

and $(w_j - w_{norm})$, the higher the perceived kindness, and the stronger are the social preferences towards the firm. We assume a self-serving bias by agent i , such that the agent's own wage matters no less than the other agent's wage, i.e. $l \geq m \geq 0$. $l = m$ corresponds to agent i holding the treatment of agent j precisely on a par with the treatment of her own wage. If $l > m \geq 0$, agent i will consider her own pay as more relevant than the coworker pay's as relevant to assess judge the firm's kindness. In the case of $m = 0$ and $l > 0$, agent i will only be concerned with her own wage. According to the gift exchange hypothesis, the weight that measures the relative relevance of the gift exchange component for the social preferences is positive: $\varphi > 0$. Nonetheless, one may put forward an alternative hypothesis. A worker receiving a wage higher than w_{norm} may think that the firm wants to manifest its *contentment* for the worker's behavior. The worker may, in this case, feel entitled to provide *less* effort, rather than *more,* possibly because the worker now has less fear of being fired. Hence, a wage rise may be taken as a justification to reduce effort, rather than increase it. We can model the "contentment hypothesis" with $\varphi < 0$.

The third term in K_{f_i} refers to the horizontal inequality between coworkers' wages. $I_{()}$ is an indicator function that takes the value of 1 if the subscripted condition is fulfilled. Individuals are concerned with both advantageous $(w_i > w_j)$ and disadvantageous $(w_i < w_j)$ inequality (Fehr and Schmidt, 1999). Advantageous and disadvantageous inequalities are weighted with $-\beta$ and $-\alpha$, respectively. Fehr and Schmidt (1999) posit that individuals dislike both advantageous and disadvantageous inequality, experiencing *compassion* when earning more than the other worker, and *envy* when earning less. Setting $\alpha \geq 0$ and $\beta \geq 0$ accordingly, individuals perceive the firm as less kind if the wage differential is positive. Individuals may be expected to be more sensitive to disadvantageous inequality than advantageous inequality (see evidence by Loewenstein et al. (1989) and Fehr and Schmidt (1999)) so that $\alpha \geq \beta \geq 0$. Another possibility is that individuals are not driven by compassion when earning more than another individual but rather enjoy relative status (Frank, 1985) or experience spite. In this case, individuals attach a positive utility to earning more than the other, rather than disutility. Experiments demonstrate that a non-negligible share of individuals indeed displays spiteful preferences (Abbink and Sadrieh, 2009; Abbink and Herrmann, 2011; Fehr et al., 2013). We can capture status concerns or spite when earning more and envy when earning less than a coworker by setting $\alpha \geq 0 \geq \beta$.

Given that (3.2) and e_i enter (3.1) multiplicatively, the worker will maximize his or her utility by exerting positive effort as long as his or her social preferences or sentiments for the firm are positive, i.e., $K_{f_i} > 0$. The utility function for the second worker j is assumed to be identical to that of worker i with reversed indices i and j. In our experimental setup, wages can only take two values. Either the wage is equal to the fixed wage w, or the worker additionally receives a lump-sum bonus B. Then, the wage of worker i can be expressed as $w_i = w + I_{Bonus_i} \cdot B$. Worker i's maximization problem can be written as:

$$\underset{e_i \in \mathbb{R}}{\operatorname{argmax}}\, w_i - c(e_i) + K_{f_i} \cdot e_i \tag{3.3}$$

Maximizing utility with respect to effort gives the first order condition defining the optimal level of effort e_i^* provided by worker i:

$$\frac{\delta U_i}{\delta e_i} = -c'(e_i) + K_f = 0 \iff e_i^* = c'^{-1}(K_f) = \frac{K_f}{2r} \tag{3.4}$$

The second-order condition is automatically satisfied since $c''(e) > 0$. The optimal effort choice depends negatively on parameter r from the quadratic effort-cost function and positively on the social preferences towards the firm. For $K_f = 0$, the optimal level of effort is zero. In the case of $K_f < 0$, the optimal effort is negative, which may be interpreted as intentionally harming (e.g., by acts of sabotage) the firm's objectives.

3.3.2 Hypotheses

Our primary hypotheses are grounded on assumptions that are more directly in line with the existing literature. However, our model is general enough to accommodate alternative hypotheses. In particular, our primary hypothesis is that workers' behavior is driven by reciprocity motives akin to the gift exchange hypothesis with respect to both the own wage and the other worker's wage, i.e., $\varphi > 0$; $l > m > 0$. Our primary assumption is also that workers feel both envy and compassion (as opposed to spite) when inequality exists, i.e., $-\alpha < -\beta < 0$. Analytical derivations of the underlying predictions generated by the model can be found in the Online Appendix, Section 3.B.1.4.

The first set of hypotheses concerns how workers react to the bonus payment, regardless of inequality concerns:

- *Hypothesis 1a* (*Gift exchange*): Workers increase their effort in response to a bonus payment. Alternatively:
- *Hypothesis 1b* (*Contentment*): Workers decrease their effort in response to a bonus payment.

The comparison between the Double Bonus condition and the Control condition provides a clean test of the first set of hypotheses because inequality is absent by construction. The Double Bonus condition is arguably the condition in which the perceived kindness of the firm should be at its highest.

The second and third hypotheses concern how inequality affects the effort of the worker not receiving the bonus and receiving the bonus, respectively. In general, aversion against horizontal pay inequality should lead to lower perceived kindness of the firm in single-bonus treatments, ceteris paribus. A disadvantaged worker will only increase effort if the satisfaction for the other worker's payoffs exceeds envy in its impact on the perceived kindness of the firm. This assumption is equivalent to $\varphi m > \alpha_T$, where T is the index for the treatment condition. On the other hand, an advantaged worker will increase effort as long as the gift exchange component outweighs the effect of compassion. From the perspective of our model parameters, this corresponds to $\varphi_1 l > \beta_T$. Conversely, if the advantaged worker is motivated by status-seeking (or spite), then she will attach positive utility to earning more than the other worker, as $\beta_T < 0$. Effort increases more for the advantaged worker than for the disadvantaged worker under the assumptions that own payoffs have a more substantial weight than others' payoffs ($l > m > 0$) and that envy is a stronger psychological motivation than compassion ($\alpha > \beta$).

- *Hypothesis 2* (*Envy*): Aversion against disadvantageous horizontal pay inequality leads to negative effects on bonus nonrecipients' effort because of envious feelings ($\alpha > 0$). Accordingly, nonrecipients productivity should drop in single-bonus treatments compared to the Control condition.

- *Hypothesis 3a (Compassion)*: Compassion leads to bonus recipients to decrease their output in comparison with the Double Bonus condition ($\beta > 0$).

- *Hypothesis 3b (Status seeking/Spite)*: Status-seeking leads to bonus recipients to increase their output compared to the Double Bonus condition ($\beta < 0$).

The fourth hypothesis concerns the effect of the fairness of the source of inequality on effort. According to the discussion in Section 3.2, we posit the following:

- *Hypothesis 4a (Fairness Perception):* The Productivity treatment and the Needs treatment are perceived as fairer than the Arbitrary treatment, because a reason is provided for the bonus allocation in the former but not in the latter.

- *Hypothesis 4b (Fairness Relevance)*: The higher the perceived fairness, the higher is the effort. In particular, the effort by both bonus recipients and nonrecipients should be higher in the Productivity treatment than in the Arbitrary treatment.

Likewise, effort should be higher in the Needs treatment than in the Arbitrary treatment.[11]

3.4 Results

We report the general patterns of the experiment results in Section 3.4.1. Section 3.4.2 provides the econometric analysis and the calibration of the utility function parameters. Section 3.4.3 analyzes perceptions of treatment fairness and their impact on productivity. Section 3.4.4 performs robustness checks of the main results.

3.4.1 Descriptive results

Table 3.1 consolidates productivity within the morning and afternoon sessions, thus providing an overview of average productivity during the pre- and post-treatment phases. We use the number of typed characters per hour as our primary outcome measure because it provides the most precise measurement of individual effort. In Section 3.4.4, we analyze the robustness of our results to using alternative outcome measures. Overall, the average number of typed characters per hour increased from roughly 971 during the morning session to about 1057 entries per hour during the afternoon session, a statistically significant increase (Wilcoxon matched-pairs signed-rank test: $p < 0.001$, $N = 236$). Since reemployment concerns were ruled out by design (see Sections 3.2 and 3.5), and since we observe a significant productivity increase in the Control condition, learning must have played a role in such a productivity increase.

Figure 3.1 plots hourly productivity broken down by treatment and recipient status (bonus recipient vs. nonrecipient). A visual inspection of Figure 3.1 and Table 3.1 reveals little variability in productivity during the morning shift. Indeed, non-parametric tests fail to reject the null hypothesis of equal productivity (Kruskal-Wallis rank test: $p = 0.750$, $N = 236$) across the five treatment conditions at the level of worker pairs in the morning sessions. This result suggests that our treatment manipulation, as intended, was exogenous to workers' inherent ability.[12] We cannot reject the null hypothesis of equal productivity between bonus recipients and nonrecipients in the afternoon session (Wilcoxon rank-sum test: $p = 0.409$, $N = 236$). Furthermore, we can neither reject the null that productivity was the same in the afternoon session across the eight treatment conditions (Kruskall-Wallis rank test: $p = 0.582$, $N = 236$), nor can we reject the null between the five conditions at the level of worker pairs (Kruskall-Wallis rank test: p =

[11] Our research is exploratory as to whether productivity is higher or the same in the Productivity treatment compared to the Needs treatment.

[12] In the Online Appendix Table 3.5, we further show that treatment conditions are balanced with respect to several observable characteristics.

Table 3.1: Average Number of Characters per Hour by Session

Condition	Morning	Afternoon	Diff.	p-value	Obs.
Pooled	971.53	1057.40	85.87	< 0.01	236
	(274.44)	(285.14)	(201.70)		
Control	941.26	1110.10	168.85	< 0.01	39
	(235.46)	(262.94)	(217.12)		
Double	1026.71	1048.78	22.07	0.39	26
	(269.02)	(329.50)	(134.94)		
Arbitrary B	1031.29	1089.83	58.54	0.10	29
	(299.82)	(313.31)	(208.69)		
Arbitrary NB	913.22	1064.33	151.11	< 0.01	27
	(289.30)	(302.57)	(166.39)		
Productivity B	1000.11	1092.95	92.83	< 0.01	31
	(263.54)	(232.64)	(163.02)		
Productivity NB	872.20	990.92	118.72	< 0.01	28
	(226.72)	(298.51)	(202.11)		
Needs B	1017.47	1055.05	37.58	0.07	29
	(369.39)	(320.66)	(260.78)		
Needs NB	977.13	978.48	1.35	0.79	27
	(208.02)	(219.65)	(171.80)		

Notes: Standard deviations in parentheses. p-values from Wilcoxon two-sided sign-rank tests. B (NB) indicates bonus recipients (nonrecipients).

0.713, N = 236). Thus, on average, the payment of bonuses apparently failed to induce higher productivity in the afternoon session.

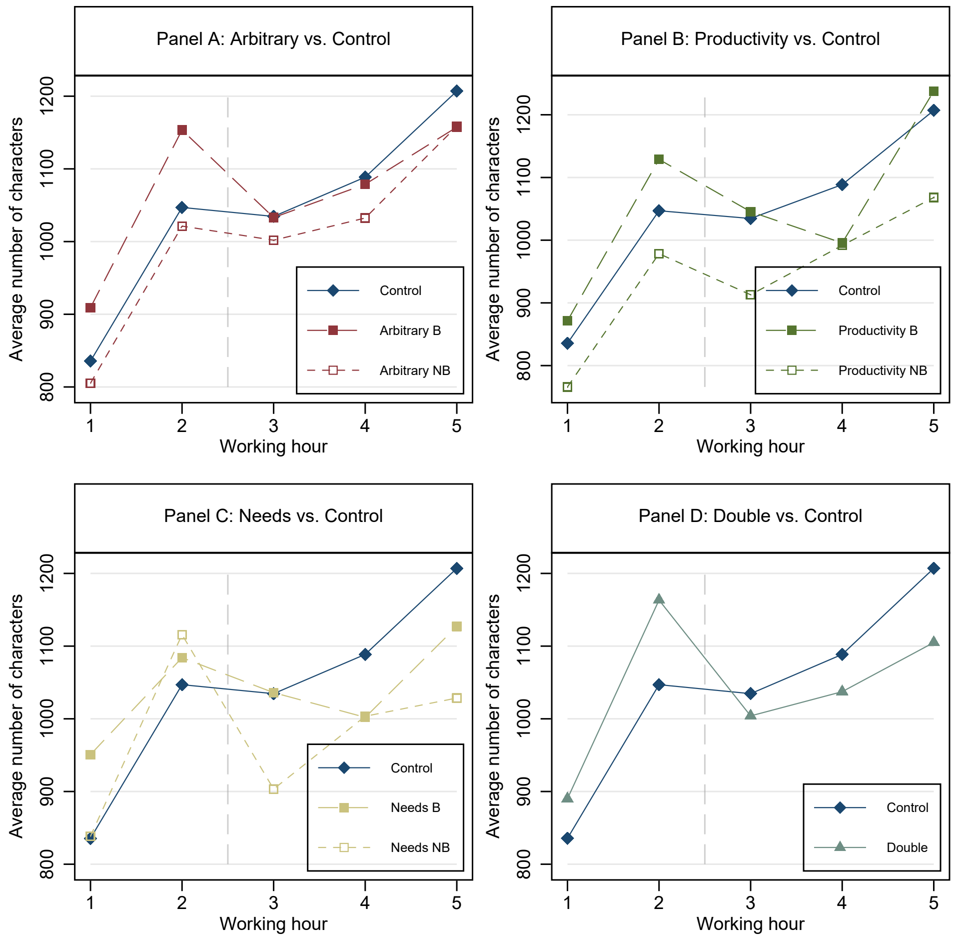
Most of the previous experimental literature found a productivity burst after receiving the bonus, which may fade away with time (see Section 3.1). However, we find the contrary in our experiment. While productivity increases sizably within both the two-hour morning session (pre-treatment) and the three-hour afternoon session (post-treatment), productivity in the third hour is always lower than in the second hour (see Figure 3.1). Moreover, this finding holds in every treatment condition and the Control condition.[13]

3.4.2 Econometric analysis and parameter calibration

Because all workers have experienced the same working conditions during the morning session, differences in the changes of productivity from the morning to the afternoon session across treatments can be causally attributed to the treatment variation. Therefore, we base our analysis of treatment effects on the following difference-in-difference

[13] Since a short lunch break took place between the second and third hour (see Section 3.2), part of the productivity drop in the third hour may be due to a physiological drop in concentration due to food digestion, which we could call a "siesta effect".

Figure 3.1: Hourly Data: Typed Characters



Notes: The average number of typed characters is plotted for the five hours of the experiment for each treatment condition. The Control condition is plotted in every graph for reasons of comparison. If our assistants could not track hourly data for one hour due to technical reasons, they assigned the output count to the next hour. This practice leads to virtual productivity peaks. Due to this, the above figure shows "smoothed" data in which the average productivity of both hours was assigned to each of the hours in these cases. The figure looks qualitatively identical when plotting only observations for which the output was recorded for every single hour.

regression model:

$$y_{it} = b_0 + \mu_i + b_1(Double \times a) + \sum_T b_{2_T} \cdot (Advantaged_T \times a) \\ + \sum_T b_{3_T}(Disadvantaged_T \times a) + \gamma \cdot a + \epsilon_{it} \quad (3.5)$$

y_{it} denotes the outcome measure of worker i in session t. The constant b_0 captures the average outcome per hour in the morning session ($t = 1$). Individual fixed effects μ_i control for individual heterogeneity in time-invariant characteristics, e.g. typing skills or the ability to concentrate, which may affect productivity. The indicator variable a equals one if the observation is from the post-intervention ($t = 2$) period in the afternoon. We allow for the error term ϵ_{it} to be clustered at the worker pair level at which we administered the treatments (Abadie et al., 2017). The interactions $Double \times a$, $Advantaged_T \times a$, and $Disadvantaged_T \times a$ of treatment indicators (for each of the single-bonus treatments) with the post-intervention indicator deliver our estimated coefficients of interest. They capture the differences across treatments in the productivity change between the morning and the afternoon session, relative to the Control condition. According to an F-test, individual fixed effects are highly significant ($p < 0.001$, F-test).

Table 3.2 presents the regression results. The first two columns display coefficients for the number of typed characters as the dependent variable. While we look at treatment conditions separately in the first and the third columns, single-bonus treatment conditions are pooled in the second and fourth columns. Table 3.3 shows the calibrated parameters for the utility function developed in Section 3.3.1. A detailed explanation of the calibration exercise can be found in Section 3.B.1.5 of the Online Appendix. The last two columns of Table 3.2 use the accuracy rate (share of correct entries relative to all entries), i.e., a measure of output quality, as the dependent variable. The results relative to quality are discussed in the robustness checks; see Section 3.4.4, where we also report results from multiple hypothesis testing adjustments.

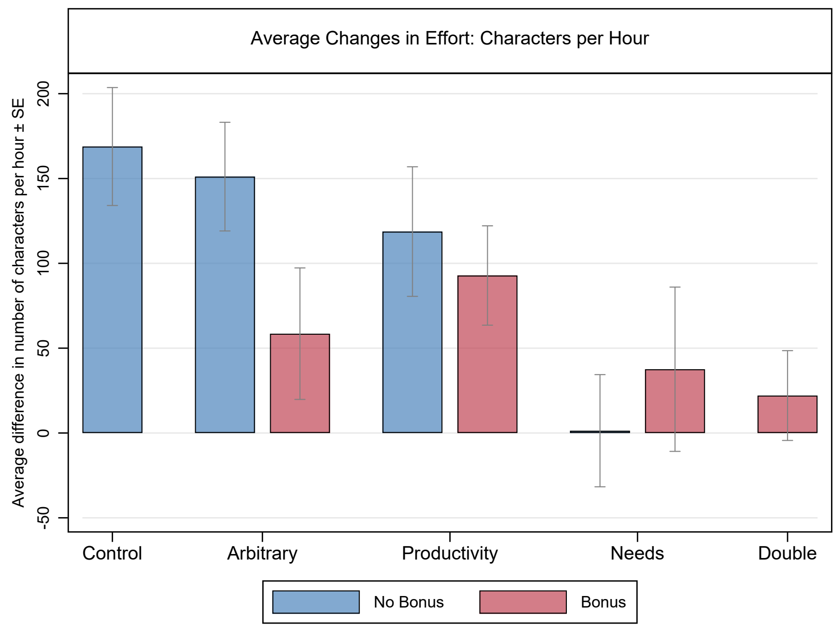
The cleanest way to test for behavior compatible with the gift exchange hypothesis (Hypothesis 1) is to compare the Control condition with the Double Bonus condition because this comparison is not confounded with the introduction of earnings inequality between the two workers. We find that workers in the Double Bonus condition significantly reduced effort in response to a bonus relative to the Control condition (-15 percent of the morning session average, -146.8 characters per hour, $p = 0.001$). The estimated parameter φ, capturing the willingness to reciprocate a wage increase in (3.1) and (3.2), equals -11.7 and is significantly lower than 0 ($p < 0.001$), pointing to a contentment effect rather than a gift exchange effect (see Section 3.3.1). Bonus

Table 3.2: Treatment Effects

Dependent variable:	(1) Characters	(2) Characters	(3) Quality	(4) Quality
Double × Afternoon	-146.8****	-146.8****	0.06	0.06
	(43.00)	(42.81)	(0.53)	(0.53)
Arbitrary NB × Afternoon	-17.74		0.02	
	(49.47)		(0.50)	
Arbitrary B × Afternoon	-110.3**		0.18	
	(54.05)		(0.50)	
Productivity NB × Afternoon	-50.13		0.02	
	(53.64)		(0.87)	
Productivity B × Afternoon	-76.01		-0.81	
	(47.81)		(0.51)	
Needs NB × Afternoon	-167.5***		0.16	
	(50.14)		(0.51)	
Needs B × Afternoon	-131.3**		0.02	
	(61.29)		(0.56)	
Advantaged × Afternoon		-105.2**		0.07
		(43.95)		(0.44)
Disadvantaged × Afternoon		-78.11*		-0.21
		(43.19)		(0.38)
Afternoon	168.8****	168.8****	-0.70**	-0.70**
	(37.88)	(37.72)	(0.29)	(0.29)
Constant	971.5****	971.5****	98.01****	98.01****
	(6.06)	(6.25)	(0.08)	(0.08)
No. Individuals	236	236	236	236
No. Clusters	126	126	126	126
Hypothesis tests (p-values)				
Double = Arbitary B	p = 0.405		p = 0.834	
Double = Productivity B	p = 0.049		p = 0.160	
Double = Needs B	p = 0.767		p = 0.958	
Double = Advantaged		p = 0.173		p = 0.596
Arbitrary B = Productivity B	p = 0.479		p = 0.094	
Arbitrary B = Needs B	p = 0.735		p = 0.799	
Needs B = Productivity B	p = 0.328		p = 0.194	
Arbitrary NB = Productivity NB	p = 0.514		p = 0.998	
Arbitary NB = Needs NB	p = 0.001		p = 0.819	
Needs NB = Productivity NB	p = 0.021		p = 0.885	

Notes: This table shows fixed effects regression results. The dependent variable in the first two columns is the average of characters entered per hour. The dependent variable in the remaining two columns is the share of correct entries relative to all entries. Standard errors in parentheses clustered at session level. **** $p < 0.001$, *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$. Hypothesis tests are Wald tests of the null hypotheses that the changes in the dependent variable are equal in the contrasted conditions. Advantaged (Disadvantaged) refers to the pooled bonus recipients (nonrecipients) in single-bonus treatment conditions.

Figure 3.2: Changes in Average Hourly Productivity across Treatment Conditions



Notes: The figure displays the mean difference and its standard error of the average hourly number of typed characters between the morning and afternoon session in the respective treatment conditions.

recipients, on average, decreased productivity from the morning to the afternoon session relative to the Control condition pooling the three single-bonus conditions, confirming the prevalence of a contentment effect (-10.8 percent[14], -105.2 characters per hour, p = 0.018). We conclude:

- *Result 1:* Productivity in the Double Bonus condition decreased by roughly 15 percent in response to the wage bonus, reaching statistical significance at the 1 percent level. This result is consistent with a contentment effect (Hypothesis 1b) rather than a gift exchange effect (Hypothesis 1a).

To test Hypotheses 2 and Hypothesis 3 about the impact of earnings inequality on effort, we contrast productivity changes among bonus recipients (nonrecipients) in single-bonus treatments relative to the productivity change of workers in the Double Bonus (Control) condition. As the monetary payoff is the same in these treatment conditions, differences in productivity changes must be attributed to the payoff differential relative to the other worker. Consistently with Hypothesis 2, the calibration of the envy parameter α yields a positive sign pooling the single-bonus treatments, albeit not significantly different from zero.

[14]We express the productivity change in percentage terms of the morning session average (971.5 characters per hour).

To be seen as an explorative exercise as it is not part of our hypotheses laid out in the previous section, we also check whether the treatments inducing pay inequality affect effort relative to the relevant benchmark condition. Nonrecipients in the Arbitrary and Productivity treatments do not reveal statistically significant differences in their output change relative to the Control condition (-1.8 percent and -5.2 percent, -17.7 and -50.1 characters per hour, $p = 0.721$ and $p = 0.352$, respectively). On the contrary, nonrecipients in the Needs treatment strongly reduced effort relative to the Control condition (-17.2 percent, -167.5 characters per hour, $p = 0.001$).[15] As for advantageous pay inequality, contrary to our primary Hypothesis 3a, we do not find negative effects on productivity. Output per hour increased among bonus recipients relative to the Double Bonus condition, the calibrated parameter β equaling -3.33 pooling the single-bonus treatments. However, this difference is not significantly different from zero (+4.3 percent, 41.6 characters per hour, $p = 0.173$). The sign is negative within each treatment, being significantly different from 0 only in the Productivity treatment (+7.3 percent, +70.8 characters per hour, $p = 0.049$, Wald test), but not in the Needs (+1.6 percent, +15.5 characters per hour, $p = 0.767$) nor the Arbitrary treatment (+3.8 percent, +36.5 characters per hour, $p = 0.405$). This result is consistent with Hypothesis 3b of status-seeking motivations rather than compassion (see Section 3.3.1). We conclude:

- *Result 2:* Disadvantageous pay inequality tended to yield a significantly negative effect on productivity, consistently with Hypothesis 2, although this was statistically significant (at the 1 percent level) only in the Needs treatment.

- *Result 3:* Advantageous pay inequality tended to yield a significantly positive effect on productivity, consistently with Hypothesis 3b of status-seeking behavior (rather than compassion). However, this was statistically significant (at the 5 percent level) only in the Productivity treatment.

3.4.3 Impact of procedural fairness

In addition to the results on productivity in the previous sections, questions from the post-experiment questionnaire allow analyzing the perception of pay fairness across treatments delivering additional insights. More precisely, we asked workers to rate (a) the adequacy of payment, (b) the fairness of how workers had been treated, and whether (c) their own earnings or (d) the coworker's earnings in the afternoon session

[15] As a further robustness check, we compared the output change among nonrecipients in the Productivity treatment to those workers in the Control condition who were less productive than their coworker in the morning, leaving conclusions unchanged (-62 characters per hour, $p = 0.8$, rank-sum test).

Table 3.3: Calibrated Model Parameters

	Pooled	Arbitrary	Productivity	Needs
φ	-11.74	-11.74	-11.74	-11.74
	(3.43)****	(3.44)****	(3.44)****	(3.44)****
β_T	-3.33	-2.92	-5.66	-1.24
	(2.42)	(3.47)	(2.84)**	(4.18)
α_T	6.25	1.42	4.01	13.40
	(3.45)	(3.96)	(4.29)	(4.01)***
Hypothesis tests	β_T		α_T	
Arbitrary = Productivity	p = 0.479		p = 0.514	
Arbitrary = Needs	p = 0.735		p = 0.001	
Needs = Productivity	p = 0.328		p = 0.021	

Notes: The table shows calibrated structural parameters for each single-bonus treatment and pooled treatments. We assume m = 0, i.e. gift exchange or contentment effects are only dependent on own income for the calibration. Parameter of the quadratic cost function is set to r = 1. **** indicates p < 0.001, *** indicates p < 0.01, ** indicates p <0.05 and * indicates p < 0.1 of significant difference to zero.

were deserved. Answers could range from 1 (absolute disagreement) to 4 (absolute agreement). Figure 3.3 shows mean answer scores by treatment condition. Section 3.B.1.7 in the Online Appendix reports an analysis of differences between treatments from OLS regressions.

Most workers perceived the payment as adequate, with little difference between treatments (see Online Appendix Table 3.10).[16] Remarkably, all Double Bonus workers reported being treated fair, constituting the condition with the highest perceived fairness. Nonrecipients in the Arbitrary treatment stand out for reporting the lowest average score for the overall fairness of how the workers were treated. Their evaluation score is marginally significantly lower than under the Control condition (p = 0.074, Wald test) and the Double Bonus condition (p = 0.006, Wald test). It is also at the margin of being significantly lower relative to nonrecipients under the Productivity treatment (p = 0.12, Wald test; Table 3.10, column 2). Considering bonus recipients and nonrecipients together, reported fairness in the Arbitrary treatment is significantly lower than in the Productivity treatment (p = 0.025, Wald test) and the Double Bonus condition (p = 0.001, Wald-test; Table 3.9, column 2).

Perception of deservedness of their own earnings in the afternoon session was highest for bonus recipients in the Productivity and the Double Bonus condition, reaching (marginal) statistical significance relative to the Control condition (p = 0.074 and p = 0.081, respectively Wald tests; Table 3.10, column 4). The most sizable differences across treatments are observed when participants rated coworkers' earnings

[16]Nonrecipients in the Arbitrary and the Needs treatment report the lowest scores, in case of the latter significantly lower than in the Double Bonus condition (p = 0.036, Wald test).

deservedness. Again, workers in the Arbitrary treatments expressed the lowest scores. Nonrecipients in the Arbitrary treatment report a lower score than workers in the Double Bonus condition (p = 0.050, Wald test). Bonus recipients' score was even lower than nonrecipients' score within the Arbitrary treatment and was lower than in the Control condition (p = 0.110, Wald test), the Double Bonus condition (p = 0.003, Wald test), and compared to bonus recipients in the Productivity treatment (p = 0.071, Wald test) and the Needs treatment (p = 0.063, Wald test). Bonus recipients and nonrecipients in the Needs treatment rated the deservedness of coworker's earnings lower than workers under the Double Bonus condition (p = 0.046 and p = 0.048, respectively, Wald tests).

However, these mild differences in self-reported fairness perceptions in both Arbitrary treatment conditions failed to result in significant behavioral responses, i.e., productivity differences. More precisely, we detect no difference between Productivity and Arbitrary treatments in the calibrated parameters relative to inequality aversion neither for bonus recipients (p = 0.479, Wald test) nor for nonrecipients (p = 0.514, Wald test) (see Table 3.3). Hence, our data do not yield robust support for Hypothesis 4b. Bonus recipients' output increased more in the Productivity treatment than the Needs treatment, although the difference is insignificant at conventional levels (+5.7 percent, +55.3 characters per hour, p = 0.328, Wald test). Contrary to our expectations, nonrecipients' output dropped in the Needs treatment significantly relative to both the Arbitrary (-15.4 percent, -149.8 characters per hour, p = 0.001, Wald test) and the Productivity (-12.1 percent, -117.4 characters per hour, p = 0.021, Wald test) treatment. Thus, we conclude:

- *Result 4:* Hypothesis 4a concerning fairness perceptions receives some support, albeit at often statistically insignificant levels. The Arbitrary treatment is generally perceived as less fair than the Productivity treatment, although differences reach statistical significance at the 5% level only considering bonus recipients and nonrecipients jointly. However, such differences in perception do not lead to any significant change in productivity in the Arbitrary treatment compared to the other single-bonus conditions, thus contradicting Hypothesis 4b. Surprisingly, we observe a considerable and statistically significant drop in productivity by nonrecipients in the Needs treatment. However, this drop is not reflected in fairness or deservedness ratings.

3.4.4 Robustness checks

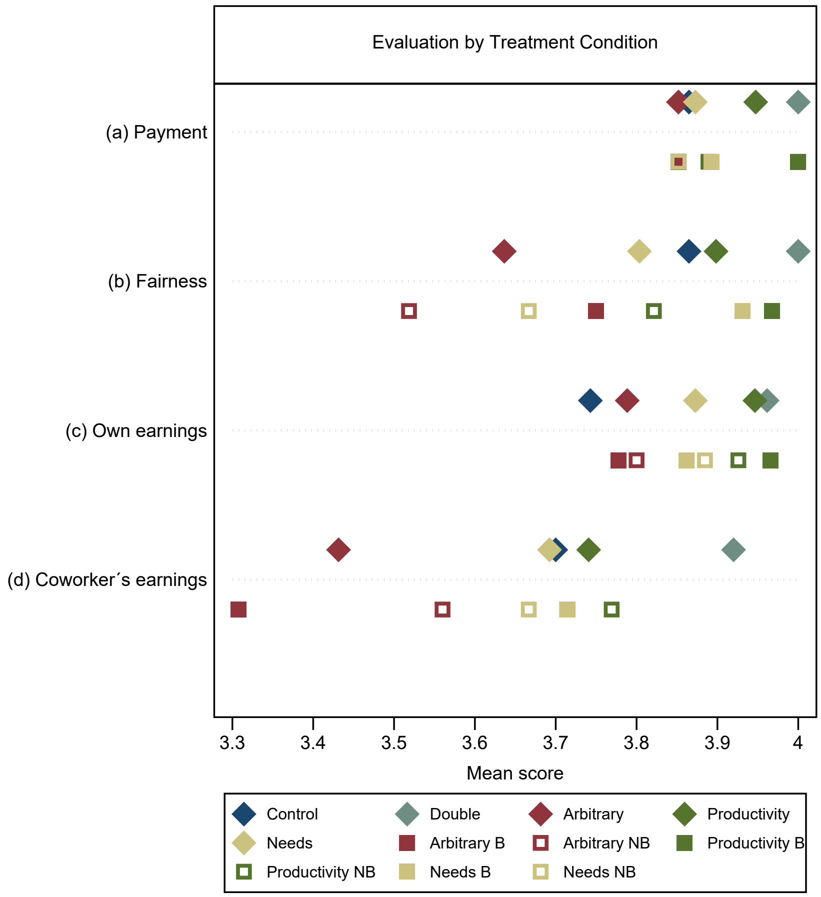
In principle, workers' behavioral reactions to the treatments are not restricted to the quantity margin of their output but possibly extend to their quality. In other words, workers may, consciously or unconsciously, adjust the precision of their work to either repay a gift from the employer or, adversely, punish them. In particular, workers dissatisfied with their treatment may try to "sabotage" the firm, keeping the number of typed characters per hour relatively unchanged but willingly introducing mistakes in their output. This behavior would result in mistakes of various kinds, such as spelling mistakes, omissions, entry swaps, in the output returned by the worker. Therefore, we investigated treatment effects on the quality margin, i.e., on the share of correct entries relative to all entries, in the afternoon session compared to the morning session in the third and fourth columns of Table 3.2.

Our analysis fails to find any appreciable quality changes between the afternoon and morning sessions, neither in the aggregate nor across treatments. The share of correct entries relative to all entries is roughly 98 percent across all conditions, only slightly varying between conditions. Consequently, a non-parametric test of the null of equal changes in output quality between the morning and afternoon sessions across treatment groups cannot be rejected ($p = 0.570$, $N = 236$, Kruskal-Wallis test). Similarly, results are not qualitatively impacted by evaluating the number of correct entries instead of typed characters as the outcome variable, providing a composite measure of the output quantity and quality. Both findings indicate that the treatments do not significantly impact the quality dimension of work. Thus, we can also rule out that the contentment effect was driven by working with less speed but higher precision.

Furthermore, the results documented in the previous sections are unaffected by several robustness checks. Pairwise treatment comparisons with Wilcoxon rank-sum tests, as shown in Table 3.12 in the Online Appendix, yield identical conclusions to the regression analysis. We do not find any sign of a trend[17] in the output changes from the morning to the afternoon session over the 126 days of data collection, which speaks against the possibility of contagion effects among workers. Omitting outliers by the method of Tukey's fences does not have a significant impact on our results. Results are also robust to excluding fifteen observations where a confederate acted as the second worker if a recruited (co-)worker did not show up (see Section 3.2 and Table 3.11). In the Online Appendix, we plot empirical cumulative distribution functions (ECDFs) (see Section 3.B.1.2). The ECDFs show that the contentment effect is not only apparent in the differences in means, but also when we consider the full distributions, thus revealing a broad behavioral pattern. The ECDF corresponding to the productivity change in the Double Bonus condition is clearly shifted to the left compared to the ECDF of the Control condition. Workers who received a bonus in the single-bonus treatments also showed productivity changes more similar to those in the Double Bonus condition,

[17]The p-value of the time trend is $p = 0.6$ in an OLS regression with the output change as dependent variable.

Figure 3.3: Post-experimental Evaluation



Notes: This figure shows the average answer score from the post-experimental survey by treatment condition. Arbitrary, Productivity, and Needs are averages over the respective conditions of recipients and nonrecipients. Answers could range between 1 "absolutely disagree" and 4 "absolutely agree" with respect to the following statements: (a) The payment I received was adequate. (b) The treatment of the two persons hired was fair. (c) The earnings I received in the second part of the day were deserved. (d) The earnings my coworker received in the second part of the day were deserved.

consistent with the presence of a contentment effect among them. On the contrary, the productivity changes of nonrecipients in single-bonus treatments are more similar to those of the Control condition, except for the nonrecipients in the Needs treatment whose productivity relatively decreased.

As our experiment involves contrasting several conditions with each other or the relevant control condition, a further robustness check is to control the familywise error rate by adjusting for multiple hypothesis testing (List et al., 2019). We shortly refer to the p-values based on the step-down procedure by Romano and Wolf (2005) concerning our hypotheses from Section 3.3.2 in the main text. For the adjusted p-values of ancillary results and those utilizing the conservative Bonferroni-Holm correction, we refer to the Appendix Section 3.B.1.3. Our main result concerning the contentment effect remains highly significant (Romano-Wolf $p = 0.003$, see Table 3.6). Similarly, the negative effect of disadvantageous pay inequality on effort remains marginally significant (Romano-Wolf $p = 0.088$, pooling nonrecipients from single-bonus treatments). This negative effect of disadvantageous pay inequality also holds concerning the most vigorous reaction when the bonus was assigned to the other worker in the Needs treatment (Romano-Wolf $p = 0.015$, Table 3.8). Furthermore, the negative effect on effort among nonrecipients in the Needs treatment remains statistically significant relative to nonrecipients in the Arbitrary (Romano-Wolf $p = 0.012$, Table 3.7) and Productivity treatments (Romano-Wolf $p = 0.073$). On the other hand, the supplementary result concerning the effort increase among bonus recipients in the Productivity treatment relative to the Double Bonus condition is no longer statistically significant (Romano-Wolf $p = 0.175$).

3.5 Discussion

Experimental results are typically assessed in terms of internal and external validity. Esteves-Sorenson (2018) considers eight possible confounds, primarily affecting the internal validity in gift exchange studies, whose relevance for our study we discuss in the following. First, peer effects may create idiosyncratic effects on outcomes. Such effects were minimized in our setting because workers were prevented from interacting and communicating at any stage of the work session (see Section 3.2). Second, Esteves-Sorenson (2018) discusses concerns that within-subject pay manipulation may lead agents to inflate their effort after receiving a bonus because they want to avoid experiencing disutility from being perceived as selfish, a factor that may be confounded with reciprocity. However, this factor seems to be irrelevant in our naturalistic setting as we observe treated participants displaying, if anything, selfish behavior in comparison to non-treated participants, as the former reduce their productivity after receiving the bonus relative to the Control condition. Moreover, as we study a natural field experiment (Harrison and List, 2004; Al-Ubaydli and List, 2017), participants ignore being part of an experiment and are not motivated to comply with the researcher's perceived goals. Third, we also do not believe that insufficient wage raises played a part in our experiment. The bonus equaled 33% of the advertised wage rate, and this is at least on par with other experiments that found a gift exchange effect (Bellemare and Shearer, 2009; Cohn et al., 2015; Gilchrist et al., 2016).

Fourth, effort ceiling effects were likely absent in our experiment because the workers demonstrated that higher productivity was possible. Productivity peaked in the last hour of the experiment and the three-hour afternoon session gave ample time to workers to reciprocate the bonus. Fifth, fatigue effects most likely also played a minor role. Productivity was particularly low after the lunch break offering time to recover but, on average, increased in all treatment conditions and the Control condition over time (see Figure 3.1). Sixth, we cannot rule out that the relatively high wage set in the Control condition, relative to the standard wage rate, led to the selection of workers abler than usual (Esteves-Sorenson, 2018). Nevertheless, the base wage was arguably appropriate, taking into account the commuting time and travel costs for Bogotá residents, and its value was decided after the pilot experiment's indication that a lower wage may have led to significant attrition. Moreover, the random assignment of workers into treatment minimizes the chance that results based on differences between treatments are due to imbalances in workers' abilities (see Section 3.4.1 for the discussion of treatment exogeneity concerning pre-intervention productivity).

Seventh, as for reemployment concerns, we followed the best practice from other experiments and repeatedly ruled out, starting from the recruitment stage, that there would be any possibility for the worker to be re-employed in the future. The one-time nature is crucial because the worker's effort reaction has to be the product of a desire to reciprocate kind actions rather than an attempt to build up a fruitful long-term relationship for an internally valid test of the gift exchange hypothesis (Al-Ubaydli and List, 2019). We cannot, of course, exclude that participants did not fully believe, or take into account, this announcement. However, if this were the case, it would be unclear why reemployment concerns should affect control and treatment conditions differently. Relatedly, nonrecipients in single-bonus treatments may have felt motivated to increase their effort levels if they had thought that not receiving the bonus was a signal for lower reemployment chances. Likewise, nonrecipients may have imagined that another bonus would have been paid at the end of the working day. However, the announcement (see the instructions) clarified what final earnings would be to rule out such motivations, e.g., from performance incentives. If these motivations were nonetheless at play, they should have presumably led nonrecipients in the Productivity treatment to raise their effort because the bonus depended directly on workers' output. However, productivity by nonrecipients was not higher in the Productivity treatment than in the Arbitrary treatment in the afternoon session relative to the morning session (see Figure 3.2), suggesting that this factor did not play a significant role. More generally, productivity by nonrecipients in single-bonus treatments was in every case lower than in the Control condition in the afternoon session relative to the morning session. This finding suggests that effects due to reemployment concerns or the desire to receive a hypothetical second bonus were largely irrelevant.

The eighth and final point of concern Esteves-Sorenson (2018) is that of small samples. Our study had an overall larger sample size than other experiments detecting gift exchange in natural experimental labor markets (Gneezy and List, 2006; Kube et al., 2012). Hence, we had adequate power to detect the existence of reciprocity effects. However, one may argue that we were still under-powered to capture differences between the various single-bonus treatments' effects, particularly between the Productivity and the Arbitrary treatments. We observe minimal differences between Arbitrary and Productivity treatments. The observed effect sizes amount to a Cohen's d of roughly 0.18 comparing both treatments for bonus recipients and nonrecipients (see Table 3.1). This result makes us doubtful that we are incurring a Type-II error. Only a larger sample size could fully ascertain this aspect.[18]

An additional concern is that the strength of a reciprocal response may depend on whether the value of effort for the employer is emphasized to the workers, as discussed in parts of the gift exchange literature (Englmaier and Leider, 2020). We cannot exclude the relevance of this concern to our study. However, we deliberately chose not to emphasize the importance of the work for the employer as we believed this could have had a detrimental effect on the naturalness of the setting. Nevertheless, even if the value of effort for the employer in performing the relatively tedious data entry task may not have been clear to the workers, this fact alone cannot explain why we observe a significant decrease in effort in response to the bonus. This remark is critical since emphasizing the importance of effort is far from standard in the gift exchange literature

[18] Laboratory studies comparing analogous treatments in Dictator Games find effect sizes in the range of d = 0.4, requiring a massive sample size of roughly 100 observations in each contrasted condition to ensure an a priori power of 80 percent, given alpha = 0.05. Our initial assumption was that the effect size in our sample would have been considerably larger than d = 0.4 for two reasons. First, we oriented at the famous gift exchange study by Gneezy and List (2006) who found an effect size for the gift exchange effect of roughly d = 0.65 in the fundraising task, for which 26 participants in each contrasted treatment conditions suffice to reach an a-priori power of 80 percent with an alpha of 0.05. Second, we were convinced that the Arbitrary treatment would have caused a more significant negative effect than treatments where assignment to roles is determined by an unbiased "fair" lottery (Bolton et al., 2005; Grimalda et al., 2016a). We also believed that effects would have been more prominent in a natural setting than in a laboratory. However, from an ex-post perspective, our assumptions did not turn out to be the case on either account.

(Gneezy and List, 2006; Kube et al., 2012). However, it might well be that emphasizing the importance of effort might have rendered the emergence of a contentment effect less likely, e.g., by inducing sympathy for the employer, increasing the likelihood for reciprocal behavior.

In terms of our results' generalizability, we support List (2020), who points out, "*all results are externally valid to some setting, and no result will be externally valid to all settings.*" We follow the request in List (2020) to report on the SANS conditions, i.e., selection, attrition, naturalness, and scaling. First, in terms of selection, recruited workers (through advertisements at the university and social networks) are predominantly young and have a student or unemployment background. At first glance, the, on average, young age of participants can be seen as a potential limitation of our study. Although a substantial portion of participants was not the usual convenience sample of university students, we acknowledge significant differences across different age groups. Research comparing experimental student samples and nationally representative samples (Bellemare and Kröger, 2007; Cappelen et al., 2015) generally find that the former tend to behave more selfishly than other groups in the population. If that was the case, we might expect different patterns of reciprocity from workers of older age. However, we believe that the sample composition should not be seen as a valid threat to external validity but instead reflects the target population. That is to say, recruiting workers for a short-term opportunity of unskilled work in a naturalistic setting will always lead to a sample leaning to the younger and, by its very nature, unemployed or student parts of the population (see OECD (2002b) and ter Weel (2018)). This feature arguably reflects for whom such a work opportunity is economically reasonable and similarly impacted the sample composition of virtually every previous investigation of the gift exchange hypothesis in temporary work environments (Gneezy and List, 2006; Hennig-Schmidt et al., 2010; Kube et al., 2012). Thus, they resemble the natural characteristics of the underlying population applying for unskilled temporary work opportunities (OECD, 2002b) rendering the sample composition realistic for the target setting (List, 2020).

As conducting elaborate field experiments always carry the risk of attrition, this is the second of the SANS conditions. As reported in Section 3.2 above, one out of the two recruited (co-)workers for each session did not show up in fifteen cases. Hence, fifteen out of 236 workers in our sample were unawarely working with a confederate. However, dropping those observations from the sample does not influence our results, as shown in the robustness checks (Section 3.4.4). We have no further information on whether the workers who did not appear at the appointment were intrinsically different in their motivations or incentives from the workers in the target population, which, in any case, should only cover workers who show up at the appointed time.

A crucial consideration regarding the naturalness condition is whether the experiment places subjects on an artificial or natural margin when making their choices (List, 2020). We are convinced that workers faced definite situational similarities to real-life temporary work opportunities as we used a natural field experiment that fulfills both conditions posed by Harrison and List (2004). First, workers were unaware of taking part in an experiment. Second, the recruitment process, task, and work environment were natural since they would be the same if the work had been done devoid of any research purposes, i.e., without treatments and observation of workers' behavior. In such a setting, people are generally less restricted in their behavior, favoring the emergence of unforeseen results (List and Rasul, 2011). We believe that most of our design elements are standard components of real-life labor contracts, albeit to a minor degree in terms of the Needs treatment. Bonus payments are widespread in labor markets worldwide, and linking them to productivity is common (Bewley, 1999). Admittedly, it would be extravagant for a company to offer a selective bonus without giving any justification for it, as we do in the Arbitrary treatment. Nevertheless, it is also arguably the case that most workers, when prone to a self-serving bias, may fail to recognize the legitimacy of the justification given by a manager for assigning the bonus to another worker. Thus workers may consider the selective bonus tantamount to being arbitrary (Bewley, 1999). Selective payments based on needs are rare in real-life labor market contracts. As argued in Section 3.2, this treatment's purpose was to investigate reactions to an allocation principle having psychological salience and being policy-relevant. A possible explanation for the substantial productivity decrease among nonrecipients in the Needs treatment could precisely be that bonus assignment due to social status is not considered by workers to be a fair or appropriate reason at the workplace (Kahneman et al., 1986).

Regarding the naturalness of the task, data entry is typical for a broad range of clerical work. It demands only widespread skills and allows for exact measurement of productivity, making it the most popular task in the gift exchange literature leveraging natural field experiments (Gneezy and List, 2006; Kube et al., 2012; DellaVigna et al., 2022). It offers the obvious advantage of being a task without specific skill requirements and delivers a standardized measure of effort that can be compared across different settings.

Furthermore, our naturalistic setting resembles a work environment in which workers provide individual effort, whereas, in many jobs, outcomes result from a collective effort by several coworkers. Since such efforts cannot be singled out easily, adding this dimension into our experiment would have introduced undesirable confounds. In addition, short-term temporary work opportunities often rely on individual effort to increase flexibility and avoid the necessity of building harmonic teams for the employer. Another feature of our study than can be seen as a potential limitation, albeit a necessary design choice to ensure internal validity, is the focus on short-term employment (see also point seven in the discussion above). Hence, we cannot evaluate the long-term effects of the treatments. Nevertheless, the relevance of casual employment and temporary work contracts is extreme in countries with higher informality rates and is also rapidly increasing in developed countries (International Labour Organization, 2016; OECD, 2019a). Finally, as our research does not belong to the programmatic studies speaking to policymakers, the scaling condition is not applicable (List, 2020).

A further relevant aspect that deserves particular attention is the relevance of cultural effects in determining our results, whose discussion we see as an extension to reporting on the SANS conditions (List, 2020). Several studies have unveiled the relevance of culture for economic outcomes (Guiso et al., 2006; Bandiera and Fischer, 2013). A natural concern is that it is unclear whether our findings would replicate in a different cultural context, particularly in high-income Western societies where previous field tests of the gift exchange hypothesis were performed (Gneezy and List, 2006; Kube et al., 2012; Cohn et al., 2015). We believe that two points are in order on this aspect. First of all, Henrich et al. (2010) and Henrich (2020) have forcefully argued that a disproportionate amount of research has been carried out in Western societies and that people from these societies are somewhat peculiar compared to other societies. Suppose economics, as a science, strives for developing universally valid notions. In that case, it should confront itself with cultural diversity and investigate the extent to which principles of behavior are truly universal or culture-specific. Moreover, Western societies are probably the exception, rather than the norm, regarding cultural or psychological traits (Henrich et al., 2010). Hence, even if a cultural peculiarity (as compared to rich Western countries) should partially drive our results, this should not discount their relevance. This argument is especially valid from the background that Colombia has a population of more than 50 million people, which itself arguably constitutes a highly relevant entity to study.

Secondly, the growing body of literature on cross-cultural analysis enables us to gauge the impact of the cultural traits specific to Colombia. For example, the mapping of social preferences in the Global Preference Survey (GPS) (Falk et al., 2018) shows that people from Colombia have an average score on positive reciprocity (the variable particularly relevant in a gift exchange situation) comparable to Northern Americans and Europeans. On the grounds of this study, one may expect that the results found in Colombia would be generalizable to Western countries. If one digs deeper into cultural traits, though, some differences emerge. According to the six-dimension model of national culture by Hofstede et al. (2010), Colombia ranks as one of the most collectivistic countries worldwide within the collectivist-individualism dimension (see also Hofstede Insights, 2020). Colombia also tops the rankings for power distance, i.e., the degree to which "the less powerful members of institutions and organizations within a country expect and accept an unequal distribution of power" (Hofstede et al., 2010). On the other hand, North-Western countries typically rank high in individualism and low in power distance. One may conjecture that collectivism – when workers perceive themselves as belonging to a different group than the entrepreneur – and power distance contribute to workers perceiving themselves as socially distant from the entrepreneur. If that is the case, workers may not perceive the payment of a bonus as a "kind" act that warrants gratitude. Alternatively, they may expect that the entrepreneur will not construe their extra effort as an act of kindness. As a result, they may not feel the need, or the moral obligation, to reciprocate a change in the wage rate, even if such a change goes to their advantage. In societies where power distance and collectivism are not dominant cultural traits, employees may engage (or perceive to) with entrepreneurs on relatively equal standing. They may then have more psychological incentives to perform positive reciprocity. In another natural experiment spanning different cultural areas, Bandiera et al. (2020) found that employees from most collectivistic countries respond less to wage incentives in contracts offered by companies. Our findings, to some extent, mirror their results. However, the incentivization scheme in Bandiera et al. (2020) relies on standard economic incentives rather than positive reciprocity. Overall, such results suggest that monetary incentives and bonus payments intended to elicit an intrinsic desire to pay good actions back in kind may be construed differently across different cultures at a basic level.

3.6 Concluding remarks

The main result of this paper has been the finding of significant productivity decreases in response to a bonus payment in the context of a one-day labor contract involving two workers. A decrease among bonus recipients occurs in all treatments considered, but the effect is more significant when both workers are bonus recipients. The result contradicts the gift exchange hypothesis (Akerlof, 1982) and is consistent with what we labeled a contentment effect: The worker interprets the bonus payment as a signal that the entrepreneur is satisfied with the worker's performance, with no moral obligation to reciprocate. Hence, our finding suggests that workers' behavior in this natural field experiment follows the logic assumed in traditional economic models (Lazear, 2000), viewing workers as behaving opportunistically (List and Rasul, 2011).

We also find that social comparison effects only played a minor role: workers who received a bonus as sole beneficiaries tended to increase productivity, albeit statistically insignificantly, compared to the Double Bonus condition, pointing to status-seeking preferences (Frank, 1985). Consistent with the observation that relative wages matter when evaluating whether the treatment was fair (Bewley, 1999), a bonus may create a perception of higher recognition when the wage of the non-rewarded serves as a reference point (Kahneman and Tversky, 1979). Hence, this reaction partially offsets the contentment effect. Nonrecipients significantly reduced productivity only in the Needs treatment. In general, workers' behavior responded only marginally to different justifications for pay inequality. More research is needed to ascertain the extent to which these results would hold across different age and cultural groups. Given that we only observed one data point for the base wage and the bonus payment, further research should also investigate the possible existence of a "ceiling effect" in reciprocity and a diminishing sensitivity to kindness.

Overall, our findings echo Bewley's (1999) insight that introducing pay inequality in the wage structure of a company may backfire. However, we have uncovered a new channel whereby this effect manifests itself, which has not to do with work morale but instead to workers' lack of responsiveness to the "gift" offered by the firm.

References

Abadie, Alberto, Susan Athey, Guido Imbens, and Jeffrey Wooldridge. 2017. "When Should You Adjust Standard Errors for Clustering?" *NBER Working Paper*, 24003.

Abbink, Klaus and Benedikt Herrmann. 2011. "The Moral Costs of Nastiness." *Economic Inquiry*, 49 (2): 631–633.

Abbink, Klaus and Abdolkarim Sadrieh. 2009. "The pleasure of being nasty." *Economics Letters*, 105 (3): 306–308.

Adams, J. Stacy. 1963. "Toward an understanding of inequity." *Journal of Abnormal Psychology*, 67 (5): 422–436.

Akerlof, George A. 1982. "Labor Contracts as Partial Gift Exchange." *The Quarterly Journal of Economics*, 97 (4): 543–569.

Akerlof, George A. and Janet L. Yellen. 1990. "The Fair Wage-Effort Hypothesis and Unemployment." *The Quarterly Journal of Economics*, 105 (2): 255–283.

Al-Ubaydli, Omar, Steffen Andersen, Uri Gneezy, and John A. List. 2015. "Carrots that look like sticks: Toward an understanding of multitasking incentive schemes." *Southern Economic Journal*, 81 (3): 538–561.

Al-Ubaydli, Omar and John A. List. 2017. "Field Experiments in Markets." In *Handbook of economic field experiments*, edited by Banerjee, Abhijit V. and Esther Duflo Volume 1. of Handbooks in Economics 271–307, Amsterdam and Oxford: North-Holland an imprint of Elsevier.

Al-Ubaydli, Omar and John A. List. 2019. "How natural field experiments have enhanced our understanding of unemployment." *Nature Human Behaviour*, 3 (1): 33–39.

Alesina, Alberto and Paola Giuliano. 2011. "Preferences for redistribution." In *Handbook of Social Economics*, edited by Benhabib, Jess, Alberto Bisin, and Matthew O. Jackson Volume 1A. of Handbook of Economics 93–131, Burlington: Elsevier Science.

Almås, Ingvild, Alexander W. Cappelen, and Bertil Tungodden. 2020. "Cutthroat Capitalism versus Cuddly Socialism: Are Americans More Meritocratic and Efficiency-Seeking than Scandinavians?" *Journal of Political Economy*, 128 (5): 1753–1788.

Ariely, Dan, Emir Kamenica, and Dražen Prelec. 2008. "Man's search for meaning: The case of Legos." *Journal of Economic Behavior & Organization*, 67 (3-4): 671–677.

Azariadis, Costas. 1975. "Implicit Contracts and Underemployment Equilibria." *Journal of Political Economy*, 83 (6): 1183–1202.

Babcock, Linda and George Loewenstein. 1997. "Explaining Bargaining Impasse: The Role of Self-Serving Biases." *Journal of Economic Perspectives*, 11 (1): 109–126.

Bandiera, Oriana, Amanda Dahlstrand-Rudin, and Greg Fischer. 2020. "Incentives and Culture: Evidence from a Multi-Country Field Experiment." *Working Paper*.

Bandiera, Oriana and Greg Fischer. 2013. "Can "good" HR practices be exported? Evidence from a field experiment in Ghana." *Working Paper*.

Becker, Gary S. 1974. "A Theory of Social Interactions." *Journal of Political Economy*, 82 (6): 1063–1093.

Bellemare, Charles and Sabine Kröger. 2007. "On representative social capital." *European Economic Review*, 51 (1): 183–202.

Bellemare, Charles and Bruce Shearer. 2009. "Gift giving and worker productivity: Evidence from a firm-level experiment." *Games and Economic Behavior*, 67 (1): 233–244.

Bénabou, Roland and Jean Tirole. 2006b. "Incentives and Prosocial Behavior." *American Economic Review*, 96 (5): 1652–1678.

Bewley, Truman F. 1999. *Why wages don't fall during a recession.* Cambridge, Mass: Harvard University Press.

Bogliacino, Francesco, Laura Jiménez Lozano, and Daniel Reyes. 2018. "Socioeconomic stratification and stereotyping: Lab-in-the-field evidence from Colombia." *International Review of Economics*, 65 (1): 77–118.

Bolton, Gary E., Jordi Brandts, and Axel Ockenfels. 2005. "Fair Procedures: Evidence from Games Involving Lotteries." *The Economic Journal*, 115 (506): 1054–1076.

Bolton, Gary E. and Axel Ockenfels. 2000. "ERC: A Theory of Equity, Reciprocity, and Competition." *American Economic Review*, 90 (1): 166–193.

Bracha, Anat, Uri Gneezy, and George Loewenstein. 2015. "Relative Pay and Labor Supply." *Journal of Labor Economics*, 33 (2): 297–315.

Bradler, Christiane, Susanne Neckermann, and Arne J. Warnke. 2019. "Incentivizing Creativity: A Large-Scale Experiment with Performance Bonuses and Gifts." *Journal of Labor Economics*, 37 (3): 793–851.

Breza, Emily, Supreet Kaur, and Yogita Shamdasani. 2018. "The Morale Effects of Pay Inequality*." *The Quarterly Journal of Economics*, 133 (2): 611–663.

Cappelen, Alexander W., Astri Drange Hole, Erik Ø. Sørensen, and Bertil Tungodden. 2007. "The Pluralism of Fairness Ideals: An Experimental Approach." *American Economic Review*, 97 (3): 818–827.

Cappelen, Alexander W., Knut Nygaard, Erik Ø. Sørensen, and Bertil Tungodden. 2015. "Social preferences in the lab: A comparison of students and a representative population." *The Scandinavian Journal of Economics*, 117 (4): 1306–1326.

Charness, G. and M. Rabin. 2002. "Understanding Social Preferences with Simple Tests." *The Quarterly Journal of Economics*, 117 (3): 817–869.

Charness, Gary. 2004. “Attribution and Reciprocity in an Experimental Labor Market.” *Journal of Labor Economics*, 22 (3): 665–688.

Charness, Gary, Guillaume R. Frechette, and John H. Kagel. 2004. “How Robust is Laboratory Gift Exchange?” *Experimental Economics*, 7 (2): 189–205.

Charness, Gary, Luca Rigotti, and Aldo Rustichini. 2007. “Individual behavior and group membership.” *American Economic Review*, 97 (4): 1340–1352.

Clarke, Damian. 2021. “RWOLF2: Stata module to calculate Romano-Wolf step-down p-values for multiple hypothesis testing.” `https://ideas.repec.org/c/boc/bocode/s458970.html`.

Cohn, Alain, Ernst Fehr, and Lorenz Goette. 2015. “Fair Wages and Effort Provision: Combining Evidence from a Choice Experiment and a Field Experiment.” *Management Science*, 61 (8): 1777–1794.

Cohn, Alain, Ernst Fehr, Benedikt Herrmann, and Frédéric Schneider. 2014. “Social comparison and effort provision: Evidence from a field experiment.” *Journal of the European Economic Association*, 12 (4): 877–898.

Cox, James C., Daniel Friedman, and Steven Gjerstad. 2007. “A tractable model of reciprocity and fairness.” *Games and Economic Behavior*, 59 (1): 17–45.

Cullen, Zoë and Ricardo Perez-Truglia. 2022. “How Much Does Your Boss Make? The Effects of Salary Comparisons.” *Journal of Political Economy*, 130 (3): 766–822.

Deffains, Bruno, Romain Espinosa, and Christian Thöni. 2016. “Political self-serving bias and redistribution.” *Journal of Public Economics*, 134 (3): 67–74.

DellaVigna, Stefano, John A. List, Ulrike Malmendier, and Gautam Rao. 2022. “Estimating Social Preferences and Gift Exchange at Work.” *American Economic Review*, 112 (3): 1038–1074.

Dufwenberg, Martin and Georg Kirchsteiger. 2004. “A theory of sequential reciprocity.” *Games and Economic Behavior*, 47 (2): 268–298.

Durante, Ruben, Louis Putterman, and Joël van der Weele. 2014. “Preferences for Redistribution and Perception of Fairness: An Experimental Study.” *Journal of the European Economic Association*, 12 (4): 1059–1086.

Englmaier, Florian and Stephen Leider. 2020. “Managerial Payoff and Gift-Exchange in the Field.” *Review of Industrial Organization*, 56 (2): 259–280.

Esteves-Sorenson, Constança. 2018. "Gift Exchange in the Workplace: Addressing the Conflicting Evidence with a Careful Test." *Management Science*, 64 (9): 4365–4388.

Falk, Armin, Anke Becker, Thomas Dohmen, Benjamin Enke, David Huffman, and Uwe Sunde. 2018. "Global Evidence on Economic Preferences*." *The Quarterly Journal of Economics*, 133 (4): 1645–1692.

Falk, Armin and Urs Fischbacher. 2006. "A theory of reciprocity." *Games and Economic Behavior*, 54 (2): 293–315.

Falk, Armin and Andrea Ichino. 2006. "Clean Evidence on Peer Effects." *Journal of Labor Economics*, 24 (1): 39–57.

Fehr, Ernst and Armin Falk. 1999. "Wage Rigidity in a Competitive Incomplete Contract Market." *Journal of Political Economy*, 107 (1): 106–134.

Fehr, Ernst and Urs Fischbacher. 2004. "Third-party punishment and social norms." *Evolution and Human Behavior*, 25 (2): 63–87.

Fehr, Ernst and Simon Gächter. 1998. "Reciprocity and economics: The economic implications of Homo Reciprocans." *European Economic Review*, 42 (3-5): 845–859.

Fehr, Ernst and Simon Gächter. 2000. "Fairness and Retaliation: The Economics of Reciprocity." *Journal of Economic Perspectives*, 14 (3): 159–182.

Fehr, Ernst, Daniela Glätzle-Rützler, and Matthias Sutter. 2013. "The development of egalitarianism, altruism, spite and parochialism in childhood and adolescence." *European Economic Review*, 64 (1): 369–383.

Fehr, Ernst, Lorenz Goette, and Christian Zehnder. 2009. "A Behavioral Account of the Labor Market: The Role of Fairness Concerns." *Annual Review of Economics*, 1 (1): 355–384.

Fehr, Ernst, Erich Kirchler, Andreas Weichbold, and Simon Gächter. 1998a. "When Social Norms Overpower Competition: Gift Exchange in Experimental Labor Markets." *Journal of Labor Economics*, 16 (2): 324–351.

Fehr, Ernst, G. Kirchsteiger, and A. Riedl. 1993. "Does Fairness Prevent Market Clearing? An Experimental Investigation." *The Quarterly Journal of Economics*, 108 (2): 437–459.

Fehr, Ernst and Klaus M. Schmidt. 1999. "A Theory of Fairness, Competition, and Cooperation." *The Quarterly Journal of Economics*, 114 (3): 817–868.

Frank, Robert H. 1985. *Choosing the right pond: Human behavior and the quest for status*. New York, NY: Oxford Univ. Press.

Gächter, Simon and Christian Thöni. 2010. "Social comparison and performance: Experimental evidence on the fair wage–effort hypothesis." *Journal of Economic Behavior & Organization*, 76 (3): 531–543.

Gibbs, Michael, Susanne Neckermann, and Christoph Siemroth. 2017. "A Field Experiment in Motivating Employee Ideas." *The Review of Economics and Statistics*, 99 (4): 577–590.

Gilchrist, Duncan S., Michael Luca, and Deepak Malhotra. 2016. "When 3 + 1 > 4: Gift Structure and Reciprocity in the Field." *Management Science*, 62 (9): 2639–2650.

Gneezy, Uri and John A. List. 2006. "Putting Behavioral Economics to Work: Testing for Gift Exchange in Labor Markets Using Field Experiments." *Econometrica*, 74 (5): 1365–1384.

Goldszmidt, Ariel, John A. List, Robert Metcalfe, Ian Muir, V. Kerry Smith, and Jenny Wang. 2020. "The Value of Time in the United States: Estimates from Nationwide Natural Field Experiments." *NBER Working Paper*, 28208.

Grimalda, Gianluca, Anirban Kar, and Eugenio Proto. 2016a. "Procedural fairness in lotteries assigning initial roles in a dynamic setting." *Experimental Economics*, 19 (4): 819–841.

Guiso, Luigi, Paola Sapienza, and Luigi Zingales. 2006. "Does Culture Affect Economic Outcomes?" *Journal of Economic Perspectives*, 20 (2): 23–48.

Hagenlocher, Michael, Eric Delmelle, Irene Casas, and Stefan Kienberger. 2013. "Assessing socioeconomic vulnerability to dengue fever in Cali, Colombia: Statistical vs expert-based modeling." *International Journal of Health Geographics*, 12 (36): 1–14.

Harrison, Glenn W. and John A. List. 2004. "Field Experiments." *Journal of Economic Literature*, 42 (4): 1009–1055.

Heffetz, Ori and Robert H. Frank. 2011. "Chapter 3 - Preferences for Status: Evidence and Economic Implications." In *Handbook of Social Economics*, edited by Benhabib, Jess, Alberto Bisin, and Matthew O. Jackson, Burlington: Elsevier Science.

Hennig-Schmidt, Heike, Abdolkarim Sadrieh, and Bettina Rockenbach. 2010. "In Search of Workers' Real Effort Reciprocity-a Field and a Laboratory Experiment." *Journal of the European Economic Association*, 8 (4): 817–837.

Henrich, Joseph. 2020. *The WEIRDest People in the World: How the West Became Psychologically Peculiar and Particularly Prosperous.* [S.I.]: Farrar, Straus and Giroux.

Henrich, Joseph, Steven J. Heine, and Ara Norenzayan. 2010. "The weirdest people in the world?" *The Behavioral and Brain Sciences*, 33 (2-3): 61–83; discussion 83–135.

Hofstede, Geert, Gert Jan Hofstede, and Michael Minkov. 2010. *Cultures and organizations: Software of the mind: Intercultural cooperation and its importance for survival: Revised and expanded third edition.* New York, NY: McGraw-Hill.

Hofstede Insights. 2020. "Country comparisons." `https://www.hofstede-insights.com/`.

Holz, Justin, John A. List, Alejandro Zentner, Marvin Cardoza, and Joaquin Zentner. 2020. "The $100 Million Nudge: Increasing Tax Compliance of Businesses and the Self-Employed using a Natural Field Experiment." *NBER Working Paper*, 27666.

International Labour Organization. 2016. *Non-standard employment around the world: Understanding challenges, shaping prospects.* Geneva: International Labour Office.

Kahneman, Daniel, Jack L. Knetsch, and Richard H. Thaler. 1986. "Fairness as a Constraint on Profit Seeking: Entitlements in the Market." *The American Economic Review*, 76 (4): 728–741.

Kahneman, Daniel and Amos Tversky. 1979. "Prospect Theory: An Analysis of Decision under Risk." *Econometrica*, 47 (2): 263–291.

Katz, Lawrence F. 1986. "Efficiency Wage Theories: A Partial Evaluation." *NBER Macroeconomics Annual*, 1 (1): 235–276.

Konow, James. 2000. "Fair Shares: Accountability and Cognitive Dissonance in Allocation Decisions." *American Economic Review*, 90 (4): 1072–1092.

Konow, James. 2003. "Which Is the Fairest One of All? A Positive Analysis of Justice Theories." *Journal of Economic Literature*, 41 (4): 1188–1239.

Kosfeld, Michael and Susanne Neckermann. 2011. "Getting More Work for Nothing? Symbolic Awards and Worker Performance." *American Economic Journal: Microeconomics*, 3 (3): 86–99.

Krawczyk, Michał W. 2010. "A glimpse through the veil of ignorance: Equality of opportunity and support for redistribution." *Journal of Public Economics*, 94 (1-2): 131–141.

Krawczyk, Michał W. 2011. "A model of procedural and distributive fairness." *Theory and Decision*, 70 (1): 111–128.

Kube, Sebastian, Michel André Maréchal, and Clemens Puppe. 2012. "The Currency of Reciprocity: Gift Exchange in the Workplace." *American Economic Review*, 102 (4): 1644–1662.

Kube, Sebastian, Michel André Maréchal, and Clemens Puppe. 2013. "Do wage cuts damage work morale? Evidence from a natural field experiment." *Journal of the European Economic Association*, 11 (4): 853–870.

Lazear, Edward P. 2000. "Performance Pay and Productivity." *American Economic Review*, 90 (5): 1346–1361.

Lindbeck, Assar and Dennis Snower. 1988. "Cooperation, Harassment, and Involuntary Unemployment: An Insider-Outsider Approach." *American Economic Review*, 78 (1): 167–188.

List, John A. 2020. "Non est Disputandum de Generalizability? A Glimpse into The External Validity Trial." *NBER Working Paper*, 27535.

List, John A. and Imran Rasul. 2011. "Field Experiments in Labor Economics." In *Handbook of Labor Economics*, edited by Card, David E. and Orley Ashenfelter Volume 4. of Handbooks in Economics 103–228, Amsterdam and New York and New York, N.Y., U.S.A: North-Holland.

List, John A., Azeem M. Shaikh, and Yang Xu. 2019. "Multiple hypothesis testing in experimental economics." *Experimental Economics*, 22 (4): 773–793.

Loewenstein, George F., Leigh Thompson, and Max H. Bazerman. 1989. "Social utility and decision making in interpersonal contexts." *Journal of Personality and Social Psychology*, 57 (3): 426–441.

Martinsson, Peter, Clara Villegas-Palacio, and Conny Wollbrant. 2015. "Cooperation and social classes: Evidence from Colombia." *Social Choice and Welfare*, 45 (4): 829–848.

Mezulis, Amy H., Lyn Y. Abramson, Janet S. Hyde, and Benjamin L. Hankin. 2004. "Is there a universal positivity bias in attributions? A meta-analytic review of individual, developmental, and cultural differences in the self-serving attributional bias." *Psychological Bulletin*, 130 (5): 711–747.

Miller, Dale T. and Michael Ross. 1975. "Self-serving biases in the attribution of causality: Fact or fiction?" *Psychological Bulletin*, 82 (2): 213–225.

Nicklisch, Andreas and Fabian Paetzel. 2020. "Need-Based Justice and Distribution Procedures: The Perspective of Economics." In *Need-Based Distributive Justice*, edited by Traub, Stefan and Bernhard Kittel 161–190, Cham: Springer International Publishing.

OECD. ed. 2002a. *OECD Employment Outlook 2002*. Paris: Organisation for Economic Co-operation and Development.

OECD. 2002b. "Taking the Measure of Temporary Employment." In *OECD Employment Outlook 2002*, edited by OECD 127–185, Paris: Organisation for Economic Co-operation and Development.

OECD. 2019a. *The future of work: The future of work*. Volume 2019. of OECD employment outlook, Paris: OECD Publishing.

Rabin, Matthew. 1993. "Incorporating Fairness into Game Theory and Economics." *American Economic Review*, 83 (5): 1281–1302.

Romano, Joseph P. and Michael Wolf. 2005. "Stepwise Multiple Testing as Formalized Data Snooping." *Econometrica*, 73 (4): 1237–1282.

Schurter, Karl and Bart J. Wilson. 2009. "Justice and Fairness in the Dictator Game." *Southern Economic Journal*, 76 (1): 130–145.

ter Weel, Bas. 2018. "The Rise of Temporary Work in Europe." *De Economist*, 166 (4): 397–401.

Trautmann, Stefan T. 2009. "A tractable model of process fairness under risk." *Journal of Economic Psychology*, 30 (5): 803–813.

Appendices

3.A Appendix

Table 3.4: Sample Characteristics

Variable	Obs	Mean	Std. Dev.	Min	Max
Age	235	21.16	1.92	18	27
Female	234	0.54	0.50	0	1
Married	236	0.08	0.27	0	1
Occupation					
Student	236	0.83	0.38	0	1
Unemployed	236	0.10	0.30	0	1
Other	236	0.07	0.25	0	1
Estratificación socioeconómica					
1	234	0.05	0.21	0	1
2	234	0.38	0.49	0	1
3	234	0.47	0.50	0	1
4	234	0.09	0.28	0	1
5	234	0.02	0.13	0	1
6	234	0.00	0.07	0	1
Education					
High school	236	0.10	0.30	0	1
Some college semester	236	0.55	0.50	0	1
Technical degree (Técnico)	236	0.15	0.36	0	1
University degree	236	0.19	0.39	0	1

Notes: Table shows summary statistics for the participants' characteristics.

3.B Online Appendix

Supplementary Online Material for

Kind or contented? An experimental investigation of the impact of bonus payments on workers' productivity

Francesco Bogliacino, Gianluca Grimalda, David Pipke

In Section 3.B.1 of the Online Appendix, we provide additional analyses and more detailed derivations based on our theoretical model. Section 3.B.2 contains the experiment instructions, the protocol, and the final questionnaire.

3.B.1 Analyses and Derivations

3.B.1.1 Treatment Balance

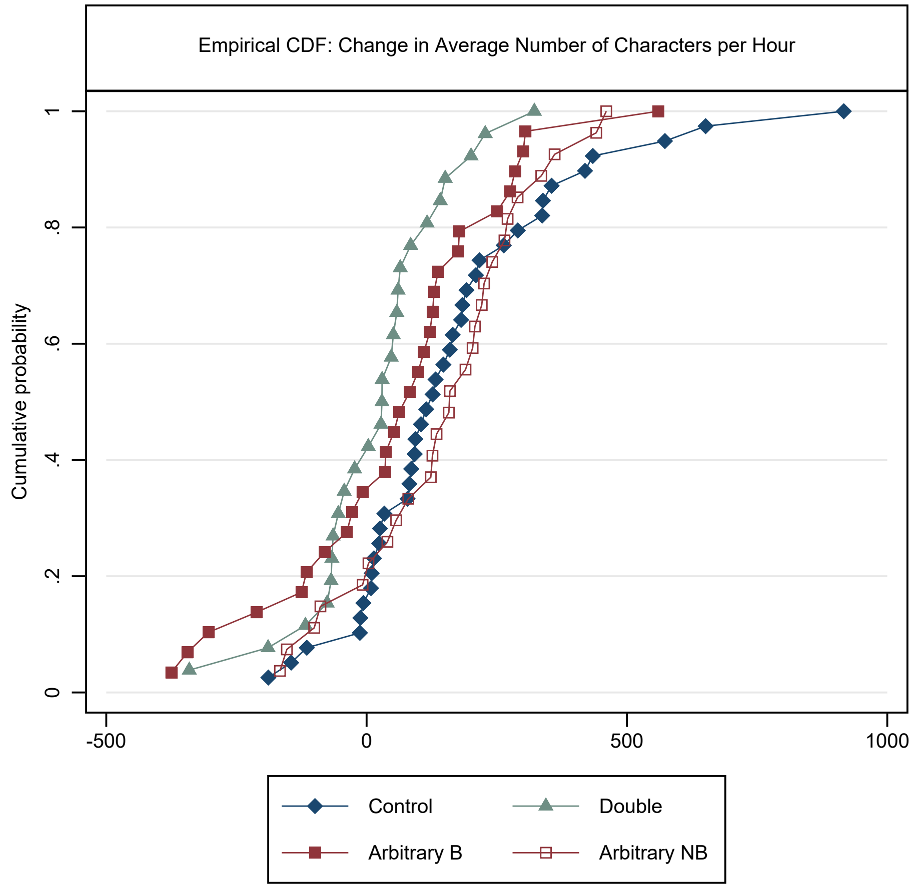
Table 3.5: Balance Table

	Control	Arbitrary	Productivity	Needs	Double	Total	F-test
Age	21.711	20.446	21.288	21.321	21.269	21.162	0.008
	(2.205)	(1.548)	(1.894)	(1.879)	(2.070)	(1.921)	
Female	0.590	0.518	0.627	0.491	0.423	0.540	0.368
	(0.498)	(0.504)	(0.488)	(0.505)	(0.504)	(0.499)	
Married	0.128	0.054	0.085	0.071	0.077	0.081	0.818
	(0.339)	(0.227)	(0.281)	(0.260)	(0.272)	(0.273)	
Estrato	2.526	2.582	2.678	2.673	2.962	2.661	0.208
	(0.687)	(0.896)	(0.753)	(0.840)	(0.774)	(0.805)	
Degree	0.359	0.393	0.322	0.304	0.269	0.335	0.787
	(0.486)	(0.493)	(0.471)	(0.464)	(0.452)	(0.473)	

Notes: The table displays background characteristics (first column) for each treatment condition (first row) at the level of worker pairs and for the total sample. On average, workers are slightly younger in the Arbitrary treatment condition, significant according to an F-test, although the mean difference to the other conditions is 1.25 years at the maximum. "Degree" is a dummy for having a higher-education degree (Technical "Técnico" or University degree). The p-value we report in the last column is from an F-test of joint significance in a regression of the background characteristic on treatment indicators.

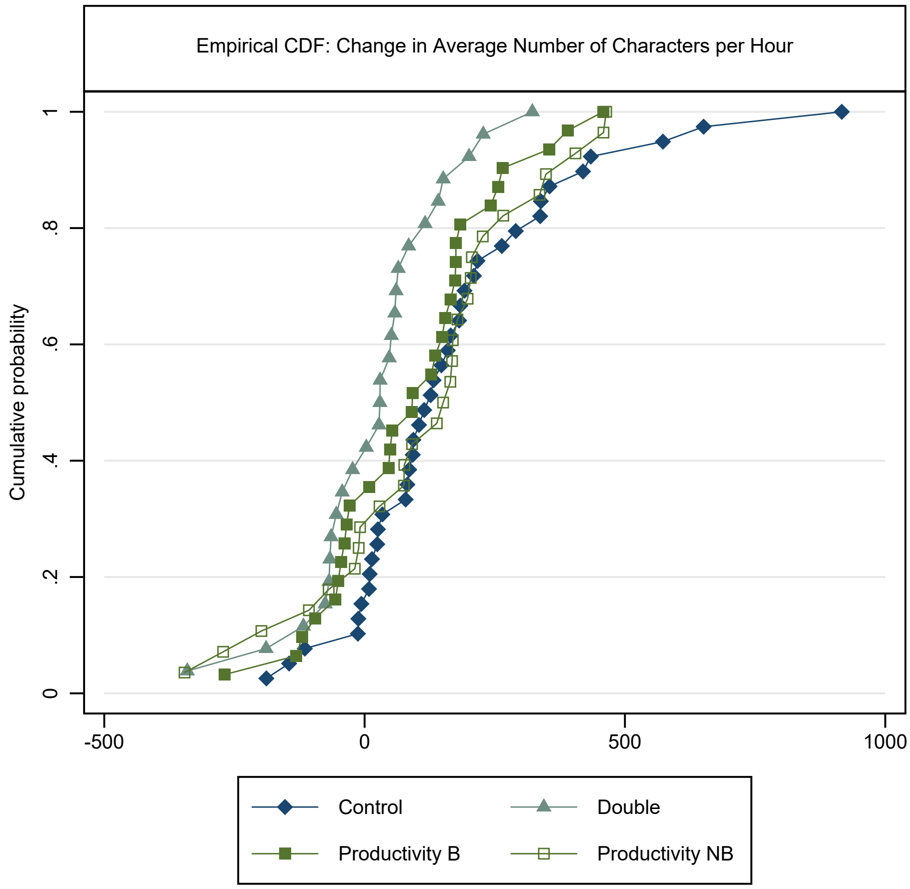
3.B.1.2 Empirical distribution functions of productivity changes

Figure 3.4: ECDF: Arbitrary versus Reference Conditions



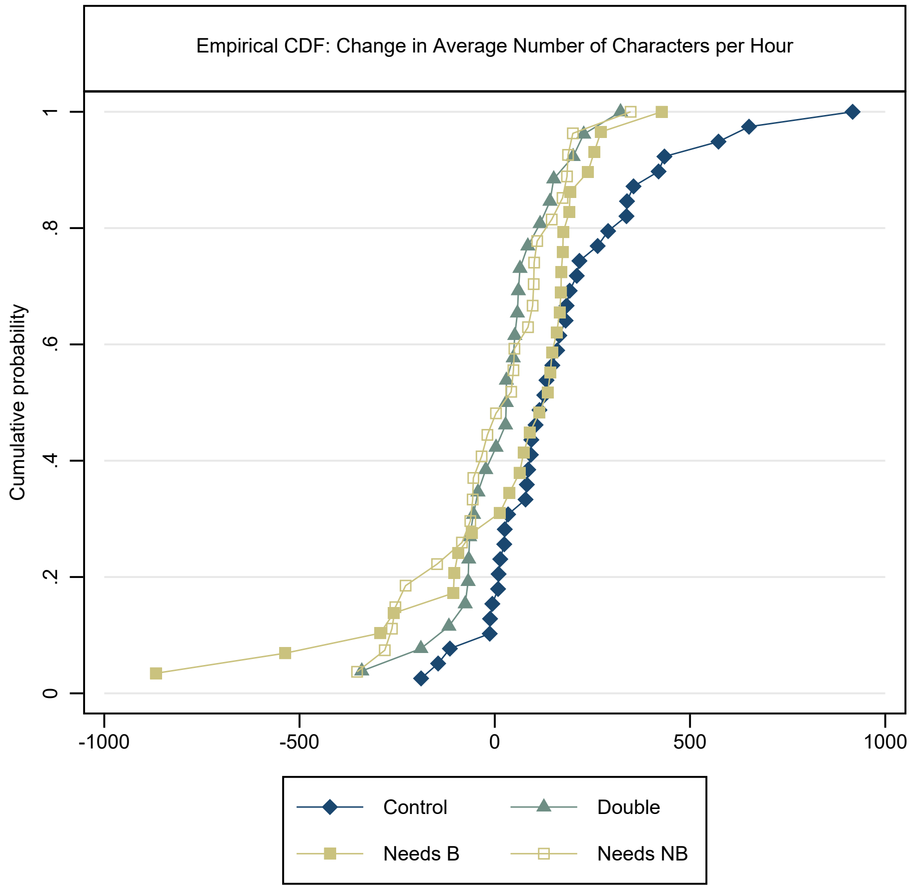
Notes: This figure shows the empirical distribution function of productivity changes between the morning and the afternoon session (change in average output in characters per hour between sessions). The plotted conditions are indicated in the legend.

Figure 3.5: ECDF: Productivity versus Reference Conditions



Notes: This figure shows the empirical distribution function of productivity changes between the morning and the afternoon session (change in average output in characters per hour between sessions). The plotted conditions are indicated in the legend.

Figure 3.6: ECDF: Needs versus Reference Conditions



Notes: This figure shows the empirical distribution function of productivity changes between the morning and the afternoon session (change in average output in characters per hour between sessions). The plotted conditions are indicated in the legend.

3.B.1.3 Multiple Hypothesis Testing Adjustments

Because experiments often involve comparing outcomes across several treatment conditions with each other or relative to the relevant control condition, reporting p-values using methods controlling the familywise error rate should become the best practice in experimental work (List et al., 2019). The following tables show unadjusted p-values, Romano-Wolf p-values using the step-down procedure (Romano and Wolf, 2005; Clarke, 2021), and the classical Bonferroni-Holm p-values.

Table 3.6: p-values with Multiple Testing Adjustments: Main Results

		Multiple testing adjustment		
	Differences	Unadjusted p-values	Romano-Wolf p-values	Bonferroni-Holm p-values
Double = Control	-146.8	0.001	0.003	0.003
Disadvantaged = Control	-78.1	0.073	0.088	0.146
Advantaged = Double	105.2	0.173	0.131	0.173

Notes: The table reports unadjusted and adjusted p-values from our main hypotheses. Differences refer to the estimates in column 2 of Table 2. Romano-Wolf p-values with 5000 bootstrapping replications. k = 3 is the number of unadjusted p-values under consideration. k_i is the number of p-values among the k p-values at least as large as the unadjusted p_i. The Bonferroni-Holm p-values are defined as min(1, k_i p_i). Romano-Wolf p-values calculated with the STATA package rwolf2 using the step-down procedure.

Table 3.7: p-values with Multiple Testing Adjustments: Different Justifications between Single-Bonus Treatments

	Differences	Multiple testing adjustment		
		Unadjusted p-values	Romano-Wolf p-values	Bonferroni-Holm p-values
Arbitary B = Productivity B	-34.3	0.479	0.791	1
Arbitary B = Needs B	21.0	0.735	0.791	0.735
Productivity B = Needs B	55.3	0.329	0.657	1
Arbitary NB = Productivity NB	32.4	0.515	0.791	1
Arbitary NB = Needs NB	149.8	0.001	0.012	0.008
Productivity NB = Needs NB	117.4	0.021	0.073	0.105

Notes: The table reports unadjusted and adjusted p-values from comparing productivity changes between bonus recipients and nonrecipients across different justifications. Differences refer to the estimates in column 1 of Table 2. Romano-Wolf p-values with 5000 bootstrapping replications. k = 6 is the number of unadjusted p-values under consideration. k_i is the number of p-values among the k p-values at least as large as the unadjusted p_i. The Bonferroni-Holm p-values are defined as min(1, k_i p_i). Romano-Wolf p-values calculated with the STATA package rwolf2 using the step-down procedure.

Table 3.8: p-values with Multiple Testing Adjustments: Additional Results concerning Single-Bonus Treatments versus relevant Benchmark Conditions

	Differences	Multiple testing adjustment		
		Unadjusted p-values	Romano-Wolf p-values	Bonferroni-Holm p-values
Arbitrary NB = Control	-17.7	0.721	0.910	1
Productivity NB = Control	-50.1	0.352	0.748	1
Needs NB = Control	-167.5	0.001	0.015	0.007
Arbitary B = Double	36.5	0.405	0.748	1
Productivity B = Double	70.8	0.049	0.175	0.244
Needs B = Double	15.5	0.767	0.910	0.767

Notes: The table reports unadjusted and adjusted p-values from contrasting single-bonus conditions to the relevant benchmark conditions. Differences refer to the estimates in column 1 of Table 2. Romano-Wolf p-values with 5000 bootstrapping replications. k = 6 is the number of unadjusted p-values under consideration. k_i is the number of p-values among the k p-values at least as large as the unadjusted p_i. The Bonferroni-Holm p-values are defined as min(1, k_i p_i). Romano-Wolf p-values calculated with the STATA package rwolf2 using the step-down procedure.

3.B.1.4 Calculation of effort predictions

We examine optimal effort choices in reaction to possible payoff combinations which can arise in our setup. In the first period, no bonus is paid by the firm such that $w_i = w_j = w$ holds, i.e. there is no wage inequality. The optimal level of effort is equal to:

$$\hat{e_1} = \frac{1}{2r}[\kappa + \varphi(l+m)(w - w_{norm})] \tag{3.6}$$

The effort level in the first period must be equal across treatments. The second period differs across treatment conditions depending on whom received the bonus. In the Control condition, no worker receives the bonus in the second period. Nevertheless, learning or fatigue effects are possible. We therefore set the effort level in the second period to be equal to the sum of the first period effort and a random variable $\tilde{L}$. We assume $\tilde{L}$ to be normally distributed across individuals with mean μ_L and variance σ_L^2.

$$e_{2\hat{Control}} = \hat{e_1} + \tilde{L^i} \tag{3.7}$$

In the Double Bonus condition, both workers receive the bonus, hence the second term capturing pay inequality of equation (3.2) equals zero.

- If both workers receive a bonus ($w_i = w_j = w + B$), the difference in the optimal effort between the second and first period must be equal to:

$$\Delta e_{DOUBL\hat{E}-BONUS} = \frac{v}{2r}\varphi(l+m)B + \tilde{L} \tag{3.8}$$

- The difference in optimal effort between the first and the second period for the worker receiving the bonus (whom we label as ADV)is:

$$\Delta\hat{e}_T^{ADV} = \frac{1}{2r}\left[\varphi\left\{l \cdot B\right\} + \left\{-\alpha_T \cdot B\right\}\right] + \tilde{L} \tag{3.9}$$

where $T = \{PRODUCTIVITY; NEEDS; ARBITRARY\}$ are the three single-bonus treatment conditions.

- The difference in optimal effort between the first and the second period for the worker not receiving the bonus (whom we label as DIS):

$$\Delta\hat{e}_T^{DIS} = \frac{1}{2r}\left[\varphi\left\{m \cdot B\right\} + \left\{-\beta_T \cdot B\right\}\right] + \tilde{L} \tag{3.10}$$

We can now examine how optimal effort responds to bonus payments in our model's framework, assuming $\tilde{L} > 0$. First, in case of a situation in which both workers receive a bonus $B > 0$, effort unambigously increases relative to the first period if $\varphi > 0$ since

$$\varphi_1 \cdot (l+m) \cdot B + \tilde{L} > 0 \tag{3.11}$$

In this situation of a "Double Bonus", the firm does not induce any wage inequality between the workers. The predicted behaviour by the model corresponds to the assumed reciprocating behaviour of paying back a kind action of the firm with kindness. If instead only one worker receives the bonus, the firm is no longer seen as unambigously kind in our proposed model. The effort of the benefitted worker increases relative to the first period in which payoffs were equal if

$$\{\varphi l + (-\alpha_T)\} B + \tilde{L} > 0 \tag{3.12}$$

holds. This corresponds to a situation where the appreciation of the gift received by worker i outweighs inequality aversion in the effect on the perceived kindness of the firm.

In case of $\alpha_T < 0$, which corresponds to a preference for advantageous inequality under the respective justification T, effort may increase even more than under the Double Bonus if this preference is stronger than the weighted delight for the other worker:

$$\varphi \{l \cdot B\} + (-\alpha_T \cdot B) > \varphi(l+m)B \Leftrightarrow (-\alpha_T) > \varphi_1 \cdot m \tag{3.13}$$

If instead the other worker receives the bonus, the disadvantaged worker's effort increases relative to the first period without payoff inequality if

$$\begin{aligned} &\varphi \{m \cdot B\} + (-\beta_T \cdot B) > 0 \\ \Longleftrightarrow\ &\varphi \cdot m - \beta_T > 0 \Leftrightarrow \varphi_1 \cdot m > \beta_T \end{aligned} \tag{3.14}$$

holds. In words, the disadvantaged worker must be delighted strongly enough for the other worker who receives the bonus such that the total effect on the perceived kindness of the firm is still positive despite the inequality in payoffs. Comparing both cases of inequality, the increase in effort from period 1 to period 2 is smaller in the case when the worker is disadvantaged than when the worker is benefitted by the bonus assignment if

$$\begin{aligned} &\varphi \cdot l \cdot B + (-\alpha_T \cdot B) > \varphi \cdot m \cdot B + (-\beta_T \cdot B) \\ &\qquad \Leftrightarrow \varphi \cdot l + (-\alpha_T) > \varphi_1 \cdot m + (-\beta_T) \Leftrightarrow \varphi(l-m) > \alpha_T - \beta_T \end{aligned} \tag{3.15}$$

, which holds for a parameterization in which $l > m$ (higher weight for own than coworker's earnings in gift exchange part) and $\beta > \alpha$ (envy is stronger than compassion) hold, assuming $\varphi > 0$. Predicted effort changes vary with the parameters l and m

defining interdependent preferences. For reasons of clarity, we assume $\varphi > 0$ in the following discussion. First, we take a look at the case of a purely selfish worker i, i.e. for which $m = 0$, i.e. a purely selfish worker does not care about the other worker's payoffs in the gift exchange part. In this case, worker i's effort changes by $\frac{1}{2r}\varphi B$ from the first to the second period if worker i receives the bonus. In turn, effort of a purely selfish worker i decreases by $\frac{1}{2r}(-\beta_T \cdot B)$ if worker j receives the bonus since there is no compensating effect on the perceived kindness of the firm by feeling delighted for the other worker. Spiteful preferences which put a negative weight of the other worker's material payoffs, can be captured by the model assuming an $m < 0$. If the other worker "j" receives the bonus, the effort of the spiteful worker "i" changes by $\frac{1}{2r}(\varphi \cdot m \cdot B + (-\beta_T \cdot B))$. Hence, the negative effect of "envy" due to disadvantageous wage inequality on the perceived kindness of the firm gets further amplified by the spitefulness. The total effect on worker i's effort in case of spiteful ($m < 0$) preferences will be negative if $\varphi \cdot m < \beta_T$ which automatically holds for $\varphi > 0$.

3.B.1.5 Calibration on experimental data

The model shall serve as a structural micro-foundation for the reduced-form estimation with observed effort as the outcome variable. The identification strategy leads to under-identification of our key theoretical parameters. We would like to estimate the following set of parameters: $\{\varphi l; \varphi m; \alpha_T; \beta_T\}$, whereof $\beta_T; \alpha_T$ are vectors containing a unique parameter related to advantageous and disadvantageous inequality aversion, respectively, for each treatment. We only have seven treatment conditions in addition to the Control treatment: The Double Bonus condition and the three other treatment conditions where we observe effort levels under both, advantageous and disadvantageous wage inequality. The under-identification is due to the fact that, in our setup, we can only observe β_T together with l (weight of the own wage in gift exchange part), and α_T together with m (weight of the other worker's wage in the gift exchange part), but we cannot observe, say, β_T together with m, because we do not have the possibility to compare treatments with the same size of inequality but at the same time different size of the bonus. Due to those limitations, we assume $m = 0$. This means that the worker experiences a gift exchange or contenment effect only with respect to oneself, but not with respect to the other worker. This assumption seems relatively mild in the context of our theoretical framework.

We also assume that the parameter r from the quadratic effort cost function is a normally distributed random variable, which we rescale to have mean 1. In order to recover the parameters of our theoretical model from the observed behavior in the experiment, we estimate a fixed-effects regression model with the average hourly effort (either measured in characters, entries or correct entries) y_{it} in the two periods, given by the morning and the afternoon session, as dependent variable. To be precise, we estimate the following differences-in-differences regression:

$$\begin{aligned} y_{it} = b_0 + \mu_i + b_1(Double \times a) + \sum_T b_{2_T} \cdot (Advantaged_T \times a) \\ + \sum_T b_{3_T}(Disadvantaged_T \times a) + \gamma \cdot a + \epsilon_{it} \end{aligned} \tag{3.16}$$

to calibrate our model. This differences-in-differences approach eliminates the estimation of some unobservable parameters, such as w_{norm}. While in the simplified world of our theoretical model, the intercept would be zero, the constant term b_0 which corresponds to the change in effort in the Control group captures effects not accounted for in the theoretical model (e.g. learning and/or fatigue effects, model parameter $\tilde{L}$). Hence, the Control group serves as the reference category[19] and enables us to get rid of the $\tilde{L}$ function. The indicator functions for each single-bonus treatment are also included as vectors. The coefficient vectors b_{2_T} and b_{3_T} contain the regression coefficients from each of the single-bonus treatments in the afternoon. Due to the unobservability of effort costs, we assume $r = 1$ in our quadratic cost function such that $c(e) = e^2$. The remaining coefficients allow to identify the parameters φ, α_T and β_T. The parameters α_T and β_T defining inequality aversion can be inferred for each single-bonus treatment separately to identify differences in the source of horizontal inequality.

3.B.1.6 Relating regression coefficents to model parameters

Simplification: $m = 0; l = 1$ Our estimated regression coefficients can be related to the theoretical parameters from the model equations as follows. We take the difference in optimal effort as given by the model solution from the model subsection between the first and second period and impose the simplifying restrictions. From the observed effort change in the Double Bonus condition where no payoff inequality arises we arrive at an expression to determine the product φ. This defines the reaction of worker effort to a symmetric pay rise without wage inequality.

$$\begin{aligned} \Delta e_{Double} = b_1 = \frac{1}{2r}\varphi(w + B - w_{norm}) - \frac{1}{2r}\varphi(w - w_{norm}) = \frac{1}{2r}\varphi \cdot B \\ \iff \varphi = b_1 \frac{2r}{B} \end{aligned} \tag{3.17}$$

[19]Without adding controls this is equivalent to compare the conditional means of the difference in provided effort levels between the treatment conditions.

Applying the same procedure for the difference in effort when either receiving or not receiving the bonus, we can express the remaining theoretical model parameters in terms of the fitted coefficients b_1,b_{2_T} and b_{3_T} (matching the $b's$ from the regression model) after rearranging equations.

$$\Delta e_{Advantaged_T} = b_2 = \frac{1}{2r}(\varphi + (-\beta)) \cdot B = b_1 - \beta_T \cdot \frac{B}{2r} \Longleftrightarrow \beta_T = \frac{2r \cdot (b_1 - b_2)}{B} \tag{3.18}$$

$$\Delta e_{Disadvantaged_T} = b_3 = \frac{1}{2r}(\varphi + (-\alpha_T)) \cdot B \Longleftrightarrow \alpha_T = -b_3 \cdot \frac{2r}{B} \tag{3.19}$$

3.B.1.7 Post-experimental questionnaire: Satisfaction analysis

Table 3.9: Regressions: Satisfaction Questions (Pooled Treatment Conditions)

	(1) Payment Adequate	(2) Treatment Fair	(3) Own profits deserved	(4) Other's profits deserved
Double	0.135	0.135	0.219*	0.220
	(0.085)	(0.085)	(0.124)	(0.150)
Arbitrary	-0.013	-0.229*	0.046	-0.269
	(0.115)	(0.136)	(0.141)	(0.195)
Productivity	0.083	0.033	0.204*	0.041
	(0.090)	(0.096)	(0.121)	(0.154)
Needs	0.008	-0.061	0.130	-0.008
	(0.098)	(0.106)	(0.128)	(0.152)
Constant	3.865****	3.865****	3.743****	3.700****
	(0.085)	(0.085)	(0.118)	(0.140)
Obs.	228	233	224	212
R2	0.0145	0.0447	0.0337	0.0527
Hypothesis tests (p-values)				
Double = Control	0.113	0.114	0.079	0.144
Arbitrary = Control	0.910	0.095	0.748	0.171
Arbitrary = Double	0.062	0.001	0.048	0.001
Arbitrary = Productivity	0.256	0.025	0.061	0.041
Arbitrary = Needs	0.822	0.181	0.366	0.079
Productivity = Control	0.358	0.728	0.096	0.792
Productivity = Double	0.072	0.024	0.751	0.031
Productivity = Needs	0.195	0.228	0.208	0.575
Needs = Control	0.936	0.565	0.312	0.960
Needs = Double	0.011	0.003	0.158	0.004

Notes: The table shows OLS regression results from post-experimental questionnaire. P-values for pairwise tests between treatment conditions reported. Standard errors (clustered at session level) in parentheses. Column (1)-(4) have the answer score (1 absolutely disagree - 4 absolutely agree) to the following statements as the dependent variable. (1) The payment I received was adequate. (2) The treatment of the two persons hired was fair. (3) The earnings I received in the second part of the day were deserved. (4) The earnings received by my coworker in the second part of the day were deserved.

Table 3.10: Regressions: Satisfaction Questions

	(1)	(2)	(3)	(4)
Double	0.135	0.135	0.219*	0.220
	(0.085)	(0.085)	(0.124)	(0.151)
Arbitrary × NB	-0.013	-0.346*	0.057	-0.140
	(0.144)	(0.192)	(0.155)	(0.224)
Arbitrary × B	-0.013	-0.115	0.035	-0.392
	(0.144)	(0.158)	(0.171)	(0.244)
Productivity × NB	0.024	-0.043	0.183	0.069
	(0.105)	(0.124)	(0.129)	(0.165)
Productivity × B	0.135	0.103	0.223*	0.0143
	(0.085)	(0.091)	(0.124)	(0.174)
Needs × NB	-0.013	-0.198	0.142	-0.033
	(0.110)	(0.16)	(0.146)	(0.182)
Needs × B	0.028	0.066	0.119	0.014
	(0.116)	(0.098)	(0.135)	(0.166)
Constant	3.865****	3.865****	3.743****	3.700****
	(0.085)	(0.085)	(0.119)	(0.141)
Obs.	228	233	224	212
R2	0.019	0.074	0.035	0.062
Hypothesis tests (p-values)				
Double = Control	0.115	0.116	0.081	0.147
Arbitrary NB = Control	0.928	0.074	0.713	0.534
Productivity NB = Control	0.820	0.727	0.159	0.675
Needs NB = Control	0.906	0.207	0.333	0.855
Arbitrary B = Control	0.928	0.468	0.839	0.110
Productivity B = Control	0.115	0.262	0.074	0.935
Needs B = Control	0.810	0.500	0.380	0.932
Arbitrary NB = Double	0.203	0.006	0.133	0.050
Productivity NB = Double	0.074	0.050	0.577	0.132
Needs NB = Double	0.036	0.012	0.408	0.048
Double = Arbitary B	0.203	0.062	0.157	0.003
Double = Productivity B	1.000	0.321	0.938	0.074
Double = Needs B	0.176	0.153	0.189	0.046
Arbitrary B = Productivity B	0.203	0.113	0.146	0.071
Arbitrary B = Needs B	0.770	0.201	0.547	0.063
Needs B = Productivity B	0.176	0.527	0.164	1.000
Arbitrary NB = Productivity NB	0.778	0.121	0.265	0.282
Arbitary NB = Needs NB	1.000	0.494	0.520	0.610
Needs NB = Productivity NB	0.691	0.332	0.677	0.474

Notes: The table shows OLS regression results from the post-experimental questionnaire. P-values for pairwise tests between treatment conditions reported. Standard errors (clustered at session level) in parentheses. Columns (1)-(4) have the answer score (1 absolutely disagree - 4 absolutely agree) to the following statements as the dependent variable. (1) The payment I received was adequate. (2) The treatment of the two persons hired was fair. (3) The earnings I received in the second part of the day were deserved. (4) The earnings received by my coworker in the second part of the day were deserved.

3.B.1.8 Additional Regressions

Table 3.11: Panel Data Fixed-Effects Model Regressions

	Whole Sample			Outliers removed	
	(1)	(2)	(3)	(4)	(5)
	Entries	Correct entries	Characters	Entries	Correct entries
Double × Afternoon	-5.885**	-5.645**	-127.101****	-4.676**	-4.434*
	(2.58)	(2.57)	(37.24)	(2.27)	(2.25)
Arbitrary NB × Afternoon	-0.324	-0.151	1.940	0.885	1.060
	(2.65)	(2.57)	(44.56)	(2.35)	(2.25)
Arbitrary B × Afternoon	-5.807*	-5.506*	-90.637*	-4.598*	-4.295
	(3.00)	(2.96)	(49.60)	(2.74)	(2.68)
Productivity NB × Afternoon	-0.797	-0.936	-30.451	0.411	0.275
	(3.14)	(3.12)	(49.15)	(2.89)	(2.86)
Productivity B × Afternoon	-3.658	-3.993	-56.338	-2.449	-2.781
	(2.55)	(2.55)	(42.71)	(2.24)	(2.22)
Needs NB × Afternoon	-9.151***	-8.713***	-147.819***	-7.942***	-7.501***
	(2.96)	(2.88)	(45.30)	(2.70)	(2.60)
Needs B × Afternoon	-4.031	-3.805	-79.254	-2.868	-2.665
	(2.89)	(2.82)	(48.51)	(2.67)	(2.59)
Afternoon	9.410****	8.799****	149.171****	8.202****	7.588****
	(2.05)	(2.01)	(31.19)	(1.64)	(1.58)
Constant	55.178****	54.138****	968.748****	55.278****	54.235****
	(0.36)	(0.35)	(5.73)	(0.34)	(0.34)

Table 3.11: Panel Data Fixed-Effects Model Regressions

	Whole Sample			Outliers removed	
	(1)	(2)	(3)	(4)	(5)
	Entries	Correct entries	Characters	Entries	Correct entries
No. Individuals	236	236	234	234	234
No. Clusters	126	126	126	126	126
Hypothesis tests (p-values)					
Double = Arbitrary B	0.977	0.959	0.405	0.977	0.959
Double = Productivity B	0.309	0.462	0.049	0.309	0.462
Double = Needs B	0.471	0.471	0.261	0.492	0.498
Arbitrary B = Productivity B	0.421	0.573	0.479	0.421	0.573
Arbitrary B = Needs B	0.553	0.564	0.832	0.570	0.587
Needs B = Productivity B	0.883	0.941	0.628	0.872	0.964
Arbitrary NB = Productivity NB	0.871	0.786	0.515	0.871	0.786
Arbitary NB = Needs NB	0.002	0.001	0.001	0.002	0.001
Needs NB = Productivity NB	0.010	0.015	0.021	0.010	0.015

Notes: The table shows fixed-effects panel data regression results. The first two columns show the results for entries and correct entries as the dependent variables using the whole sample as in the main text. The remaining columns show the results for the same dependent variables plus characters when excluding observations whose change in effort lies outside of Tukey's outer fences (below or above the first or third quartile by three times the interquartile range, respectively), thus excluding severe outliers also visible in the ECDFs above. Clustered standard errors at the session (couple) level in parentheses. * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$, **** $p < 0.001$.

3.B.1.9 Rank-sum Tests: Differences-in-Differences across Treatment Conditions

Table 3.12 shows treatment effects calculated as pairwise differences in differences between treatment conditions. We compare the change in the treatment condition from the first column to the change in the treatment condition of the first row (differences in changes from morning to afternoon productivity across treatment conditions).

Table 3.12: Treatment Effects by Treatment Condition

	Control	Arbitrary B	Arbitrary NB	Productivity B	Productivity NB	Needs B	Needs NB
Arbitrary B	-110.31						
	(-65.33%)						
p-value	0.09						
Arbitrary NB	-17.74	92.58					
	(-10.50%)	(54.83%)					
p-value	0.72	0.07					
Productivity B	-76.01	34.30	-58.28				
	(-45.02%)	(20.31%)	(-34.51%)				
p-value	0.19	0.63	0.16				
Productivity NB	-50.13	60.19	-32.39	25.89			
	(-29.69%)	(35.64%)	(-19.18%)	(15.33%)			
p-value	0.71	0.21	0.54	0.39			
Needs B	-131.27	-20.95	-113.53	-55.25	-81.14		
	(-77.74%)	(-12.41%)	(-67.24%)	(-32.72%)	(-48.05%)		
p-value	0.24	0.69	0.10	0.84	0.31		
Needs NB	-167.49	-57.18	-149.76	-91.48	-117.37	-36.23	
	(-99.19%)	(-33.86%)	(-88.69%)	(-54.18%)	(-69.51%)	(-21.46%)	
p-value	0.00	0.27	0.00	0.08	0.02	0.15	
Double	-146.78	-36.46	-129.04	-70.76	96.65	-15.51	-20.72
	(-86.93%)	(-21.60%)	(-76.42%)	(-41.91%)	(57.24%)	(-9.19%)	(-12.27%)
p-value	0.00	0.29	0.00	0.09	0.02	0.12	0.89

Notes: The table shows treatment effects calculated as pairwise differences in differences in average productivity (typed characters per hour) from the morning to the afternoon session. ATE as differences in differences (afternoon-morning) between compared treatments in absolute and percentage terms. We compare the difference in the treatment condition from the first column relative to the difference in the treatment condition from the first row. P-values from non-parametric two-sided Wilcoxon rank-sum tests against the null hypothesis of equal distributions in the pairwise compared treatment groups.

3.B.2 Instructions and Experimental Protocol

We report the experiment protocol and the instructions administered in the experiment in Sections 3.B.2.1 to 3.B.2.5. All the text read aloud to participants is reported in italics. In order to ensure that the same information was administered to participants across sessions, the lead research assistant would read most of such instructions from a written script. Section 3.B.2.6 reports further notes to the assistants. Section 3.B.2.7 reports the instruction sheet given to participants to carry out their task. Section 3.B.2.8 reports the final questionnaire. The original script in Spanish is available upon request.

3.B.2.1 Welcome

The lead research assistant (RA1 henceforth) (or the other research assistant, RA2 henceforth) greets the pair of workers at the Business School faculty or at the Konrad Lorenz University entrance. We try to avoid them talking with each other. We take one of them into the faculty lounge while the other sits waiting outside. We hand out the informed consent form, asking them to read it and to sign it. Tell them to please wait quietly before the day work starts. When they are both present and have signed the consent form, the schedule of the working day can be explained to them.

(At the Faculty reception desk or at the Assistant office): *Good morning guys, did you bring the copies of your electricity bills? Please give them to me.*

(If a participant does not bring them, he/she is asked to please call someone at home who can send them to him/her).

Before we begin, I am going to ask you to please read these informed consent form, which is to assure you that all the information you provide us today will be treated confidentially. If you have any questions, please let me know, otherwise please enter your ID and today's date and sign.

Once the informed consent is signed, the participants are asked to enter the staff room and sit in front of the computer where the task is explained.

Good morning again, thank you very much for your interest in collaborating with us. My name is [State name of lead researcher]*, and here with me is* [State name of the other research assistant present].

RA2 acknowledges the introduction.

I am going to read the following instructions from a written text, because I do not want to forget important details. The work for which we are hiring you is part of a research project conducted and funded by a consortium of universities. We want to *send some questionnaires to residents of Bogotá. For that purpose, we need to prepare the labels with the data of the recipients. Additionally, we want it to be a casual sample of Bogotá residents and so we will use a version of the telephone directory to extract the addresses of the residents. Both of you are going to do the same task individually. You will stay in two different rooms. Each of you will be responsible for the room and the computer we give you, so we will ask you not to leave the room. If, for any reason, you do need to leave the room, I will give you shortly a phone number you can call. Laura/Daniel or I will come and stay in the rooms while you are out.*

The work is divided into a two-hour session, a 15 minute break and then a three-hour session. The hourly rate is $15,000, which means that at the end of the five hours you will receive $75,000. Your contract is for today only. That is, under the terms of this project, you will not be offered a new contract. Do you agree with these conditions? Now I will show you your task for this job.

RA2 shows the Excel sheet ADDRESSES.xslx.

On your computer you will find the file ADDRESSES, this is the file you have to fill in. This file has two columns, the City column, which will always be Bogotá because the directories are from Bogotá, and the Address column, which is the one you must fill in. Note that in the Address column, only the address goes, neither name nor telephone, if there is information of apartment, office, neighborhood, etc, include it, otherwise, only the address that appears. Now, where are you going to get the information to fill in the ADDRESSES file? From the COORDINATES file, which as its name indicates are search coordinates.

RA2 opens the second Excel sheet COORDINATES.xlsx.

As you can see, the file has the columns Page, Column and Entry, so in this first case for example, you search in your directory for page 93, column 4 and entry number 4.

RA2 shows an exercise.

Do you have any questions, is everything clear? Well, then to verify that it is clear, please do this example yourself.

RA2 invites the participants to execute an example.

If that is clear, there is only one exceptional case and it is this:

RA2 shows an example of entries that have no address.

Suppose you check your file, and the coordinate is this one that has no address associated with it, in that case then, you jump to the next address, and you write down *the next available address, you do not leave blanks in the database, but you just write down the next address.*

Well, finally I am going to ask you to please save each time you make an entry, either on the floppy disk icon or Control + G so as not to lose information.

Now, I am going to accompany Participant 1 to the room. I will be right back for you (Participant 2).

3.B.2.2 Morning Session

(In the offices of each participant):

Well, here I leave you your two files, I leave you water and in this envelope I leave you the instruction sheet with all the information I gave you a moment ago, with more detail, and the phone number where you can call us for anything you need. If you don't have credit on your phone, you can use this cell phone. I leave you this envelope that has the example of how the labels we are going to print from the database you are helping us to build. So, it's XX:XX (state time)*; we will come round in two hours to let you know that the first session is over.*

As soon as she closes the door, RA1 starts the timer and calls RA2 to activate his/her stopwatch. RA2 is present in the assistant office, but does not speak to the participant.

Repeat last set of operations for the second participant.

3.B.2.3 End of Morning Session

Every hour, RA2 saves the file in a folder and takes note of the number of completed addresses (and the number of characters) and does a random check of its accuracy/error rate. After exactly two hours, RA1 and RA2 go into the two offices separately.

Hi, the first 2-hour session is over. I brought you a snack, do you want to go to the bathroom or something? While you're out I'm going to save your work. I'm going to ask you to please stay away from the computer during these 15 minutes and exercise your hands to avoid fatigue. I will come back in 15 minutes for you to start the second session.

RA1 and RA2 take care that participants do not communicate with each other and go to the bathroom one at a time. They save the Excel file on the USB stick. RA2 computes relative productivity in Productivity Treatment; checks home quality as reported in the utility bill in Need Treatment; performs an unbiased random draw among the two workers in the Arbitrary treatment. It is then determined which participant will receive the bonus and who will not in the Treatment conditions. After

15 minutes, RA1 goes to each office and convenes the two participants to the staff room. RA2 is present in the teachers' room. When the two participants arrive, RA2 leaves the assistant office to watch the classrooms.

3.B.2.4 Treatment

The treatment sequence across sessions is the following:

1. Productivity Treatment
2. Need Treatment
3. Productivity Treatment
4. Arbitrary Treatment
5. Double Bonus Condition
6. Control Condition
7. Arbitrary Treatment
8. Needs Treatment
9. Control Condition
10. Double Bonus Condition

(In the Assistant room):

1. Control Condition: *We are ready to proceed with work. The work is the same as in the first session, but the duration of the second session will be 3 hours and the salary will be the same as in the first part.*

2. Productivity Treatment: *We are ready to proceed with work. The work is the same as the first session, but the duration of the second session will be 3 hours. The salary for today's afternoon will be 15000 per hour as in the morning. However, the research director wants to pay a bonus of 25,000 pesos to one of you. To decide who would get the bonus we reviewed the number of entries and characters you completed in the first session and taking those criteria into account, then you* (indicating the participant receiving the bonus) *will receive the bonus, which means we will pay you $75,000 for the five hours plus a $25,000 bonus for a total of $100,000. In the meantime, you* (indicating the participant not receiving the bonus) *will receive the $75,000 that we agreed upon this morning.*

3. Needs Treatment: *We are ready to proceed with work. The work is the same as the first session, but the duration of the second session will be 3 hours. The salary for today's afternoon will be 15000 per hour as in the morning. However, the research director wants to pay a bonus of 25000 pesos to one of the couple's workers. To decide who would get the bonus we reviewed the socio-economic strata of your houses, the poverty level of the locality where you live and their age. Taking those criteria into account, then you* (indicating the participant receiving the bonus) *will receive the bonus, which means that we will pay you $75,000 for the five hours plus a $25,000 bonus for a total of $100,000. In the meantime, you* (indicating the participant not receiving the bonus) *will receive the $75,000 that we agreed upon this morning.*

4. Arbitrary Treatment: *We are ready to proceed with work. The work is the same as the first session, but the duration of the second session will be 3 hours. The salary for today's afternoon will be 15000 per hour as in the morning. However, the research director wants to pay a bonus to one of you. Unfortunately, he did not have time to review who completed the most entries in the first session, or your socioeconomic status, age or other criteria. He chose you* (indicating the participant receiving the bonus) *to receive the bonus, which means we will pay you $75,000 for the five hours plus a $25,000 bonus for a total of $100,000. On the other hand, you* (indicating the participant receiving the bonus) *will receive the $75,000 that we agreed on this morning.*

5. Double Bonus Condition: *We are ready to proceed with the work. The work is the same as the first session, but the duration of the second session will be 3 hours. The salary for today's afternoon will be 15,000 per hour as in the morning. However, the research director wants to pay you both a bonus of 25,000 pesos, which means we will pay you $75,000 for the five hours plus a $25,000 bonus for a total of $100,000.*

Thank you. I will now take you to your offices.

RA1 separately takes the workers to their offices.

3.B.2.5 Afternoon session

As of now, the second session begins, it is XX:XX at XX:XX (state the time) *and in three hours we will finish the second work session. Thank you.*

Every hour, RA2 saves the file in a folder and takes note of the number of completed addresses (and the number of characters) and does a random check of its accuracy/error rate. After exactly three hours, RA2 goes to the first office:

The 3 hours are up. We would be grateful if you could answer this evaluation questionnaire. The questionnaire will be associated with a code so that your identity does not appear and the confidentiality of the information you provide is guaranteed. We will collect your questionnaire in this box.

Show ballot box, save all data in the USB flash drive and hand in the questionnaire.

I will be back in a few minutes with the money. In the meantime, please fill out the questionnaire.

RA2 goes to the other office and repeats. After 3-4 minutes she comes back to the first office.

Thank you very much for your cooperation. Please insert the questionnaire in the ballot box. Here is your money. Please confirm that it is complete (wait for the money to be counted). Now please fill out the receipt and I will take you to the elevator.

RA2 brings Worker 1 to the elevator, then repeats the process with Worker 2.

3.B.2.6 Further notes for assistants

Important tips when interacting with participants

1. Do not mention anything related to other workers.
2. Ask immediately for their CV (if for some reason this was not sent with the email) and the electricity bill.
3. Mention that it is a one-day job; there will be no possibility of contract extension.
4. Do not give any reference as to what we are expecting in terms of productivity.
5. Do not anticipate anything related to the bonus in the second session.
6. Always give the same answers to similar questions.
7. If you don't know what to say, stress that you are the project assistant and are only executing directives from the project coordinator; don't make anything up!
8. If a participant does not show up, find a university student willing to work.

Answers to Frequently Asked Questions

- Why do I have to fill out the consent form, and why the questionnaire?

The consent serves as a statement that you are aware of all procedures and the use of the data. The questionnaire is solely for our evaluation of working conditions and your satisfaction.

- What happens if I cannot complete all the addresses on my sheet?

Don't worry, nothing happens.

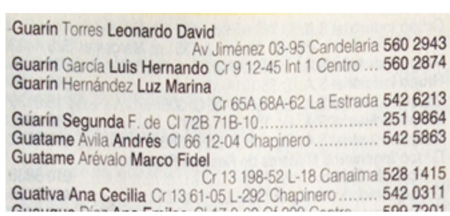
- I am entitled to an hour's break.

Since this is not a full workday, we would expect 15 minutes of rest to be sufficient. We also do this so that a student has more time to attend classes. However, if you need a longer break, we can accommodate it.

3.B.2.7 Task Instruction Sheet

1. Open the sheet COORDINATES SURNAME.xlsx.
2. Read the entries in the given order (starting with line 1, then 2, then 3 etc.).
3. The first number is the page number in the telephone directory.
4. The second number is the column number in the page (starting from the left).
5. The third number is the address line starting from the top. If the number is greater than the number of addresses, continue counting (same column) from top to bottom until you reach the number that appears in COORDINATES SURNAME.xlsx.

Figure 3.7: Task Example


Guarín Torres Leonardo David
Av Jiménez 03-95 Candelaria 560 2943
Guarín García Luis Hernando Cr 9 12-45 Int 1 Centro ... 560 2874
Guarín Hernández Luz Marina
Cr 65A 68A-62 La Estrada 542 6213
Guarín Segunda F. de Cl 72B 71B-10 .... 251 9864
Guatame Ávila Andrés Cl 66 12-04 Chapinero 542 5863
Guatame Arévalo Marco Fidel
Cr 13 198-52 L-18 Canaima 528 1415
Guativa Ana Cecilia Cr 13 61-05 L-292 Chapinero 542 0311

1. If the corresponding entry does not have an address but only a telephone number, please record the subsequent address, i.e. that in the next entry.
2. Open the sheet ADDRESSES SURNAME.xlsx.
3. Fill in the box in the "Address" column.
4. Fill in the "City" box by typing "Bogota".

5. Save.
6. Your room is reserved, if anyone asks something, answer that it is reserved by Professor Castiblanco from the Business School.
7. Remember that you are responsible for the computer and the classroom.
8. If you have any questions, please call immediately 314 3 06 XX XX XX.

3.B.2.8 Final Questionnaire

Code:

Q1) Sex: M F

Q2) Age: ________ years

Q3) What is your marital status?

- Married/Living with partner
- Single
- Separated/Divorced/Widow

Q4) According to your utility bills, what is the tier of your current home or neighborhood?

1	2	3	4	5	6

Q5) What is your level of education?

- None
- Primary School
- High school
- Some university, but no graduation
- Technical
- University degree

Q6) What is your father's level of education?

- None
- Primary School
- High school
- Some university, but no graduation
- Technical
- University degree

Q7) What is your mother's level of education?

- None
- Primary School
- High school
- Some university, but no graduation
- Technical
- University degree

Q8) What is your occupation?

- Student
- Unemployed, retired, housewife, househusband
- Other (specify) ____

Q9) For each of the statements below, please indicate your level of agreement or disagreement.

a. The work organization was effective.

Strongly disagree	1	2	3	4	Strongly agree

b. The payment I received was appropriate.

Strongly disagree	1	2	3	4	Strongly agree

c. The treatment received by the two people hired was equitable.

Strongly disagree	1	2	3	4	Strongly agree

d. The earnings I received in the second part of the workday were well deserved.

Strongly disagree	1	2	3	4	Strongly agree

e. The earnings that the other person hired in the second part of the workday received were well deserved.

Strongly disagree	1	2	3	4	Strongly agree

Q10) Below are some possible reasons why some individuals succeed and others do not. On a scale of one to four, where one represents "not important" and five "very important"; indicate, in determining a person's success, how important it is:

a. Money inherited from family.

Not important	1	2	3	4	Very important

b. Hard work and initiative.

Not important	1	2	3	4	Very important

c. Connections and familiarity with the right people.

Not important	1	2	3	4	Very important

Q11) Among the following two factors, which factor do you consider to be more important for a person to be in a state of poverty?

- Lack of effort and work commitment on the part of the person.
- Luck or events that are not in the person's control

Q12) How much do you agree that the government needs to reduce the gap between rich and poor, either by raising taxes for the rich or by providing income assistance to the poorest? Please indicate how strongly you agree by marking a number from one to four on the scale below, where one indicates "strongly disagree" and four indicates "strongly agree".

Strongly disagree	1	2	3	4	Strongly agree

Q13) How did you hear about this project? You can check more than one option.

- Through a poster
- By email
- Through social media
- Through a friend
- Through another person who was hired
- Other (specify) ____

Q14) Please express, if you wish, your opinions about this workday. Thank you for your cooperation.

Chapter 4

Black Lives Matter: Findings on protests, prosociality, discrimination, and racial attitudes from large-scale online experiments

David Pipke[a]

I am very grateful to Gianluca Grimalda, Fabrice Murtin, Louis Putterman, and Matthias Sutter for their permission to use the Trustlab data for this paper.

[a]Kiel Institute for the World Economy (david.pipke@ifw-kiel.de)

Abstract

Using data from two waves of online experiments run on representative samples of the U.S. population in 2017 and 2020, this paper shows that prejudice against African Americans and the support for one of the primary policy goals of the Black Lives Matter movement correlate with behavior in incentivized experimental games. Prosociality in dictator and public goods games correlates positively with attaching a higher priority to supporting one of the movement's primary policy goals but only weakly correlates with prejudice. People who discriminate against African Americans in trust games compared to white Americans tend to have more prejudice but do not report substantially lower support for the movement's goal. The heterogeneity of correlations concerning several dimensions, including political orientation, is discussed. In the second wave, which was run shortly after the death of George Floyd, more significant numbers of geographically closer BLM protests correlate with higher support for the movement's goal, weaker racial prejudice, and, to a lesser extent, lower trust in the police. The heterogeneity analysis shows that the relatively more prosocial respondents drive the effects of BLM protests on the support for the movement's goal. Furthermore, the results suggest that BLM protests led to stronger polarization between right-wing respondents and the rest of the political spectrum regarding prejudice against African Americans but homogenously fostered support for the movement across party lines.

JEL Codes: D72, D74, D91, J15, J16

Keywords: Black Lives Matter, George Floyd, 2020, Protest, Movement, Prosociality, Discrimination, Prejudice

4.1 Introduction

Despite the efforts to reduce them, racial divides in the U.S. are substantial. Numerous studies have documented the relevance of racial attitudes and discrimination in explaining racial divides in many domains, such as labor, housing, or education (Donohue III and Levitt, 2001; Bertrand and Mullainathan, 2004; Lang and Manove, 2011; Alesina and La Ferrara, 2014; Bayer et al., 2017; Alesina et al., 2021a).[1] African Americans not only have less than 15 percent of the wealth of white Americans and are much more likely to live in poverty (CNN, 2021), but they are also 3.5 times more likely to be killed from police violence than white Americans (GBD 2019 Police Violence US Subnational Collaborators, 2021). The death of the 46-years old, unarmed George Floyd in May 2020 at the hands of a former American police officer stands in a long and sad history of police violence against African Americans in the United States. Moreover, it led to the largest eruption of Black Lives Matter (BLM) protests in the U.S. and globally, which rendered the BLM movement the largest mass movement for racial justice in the history of the U.S. (Francis and Wright-Rigueur, 2021).[2]

A well-known hypothesis is that attitudes towards (racial) minorities and prosocial preferences are interdependent, e.g., in terms of redistribution (Alesina et al., 1999; Gilens, 1999; Fong and Luttmer, 2011; Luttmer, 2001; Alesina and Stantcheva, 2020; Tabellini, 2020).[3] Nevertheless, studies linking measures of racial attitudes to behavior in incentivized economic experiments are scarce, even more so concerning the investigation of heterogeneous effects from the experience of disruptive events along the dimensions of observed behavior in the experiments. Drawing from the rich experimental measures of the OECD's Trustlab (Murtin et al., 2018), this study aims to advance the understanding of the relationship between racial attitudes, i.e., racial prejudice and support for one of the main goals of the BLM movement expressed in a survey and experimental measures of discrimination and prosociality. The timing of the survey allows for studying the interdependence of racial attitudes and measures for the intensity of exposure to protests with their focus on highlighting racism and systemic police violence against African Americans and how they interact with the experimental in a nationally representative sample of the U.S. adult population.

This paper is related to studies examining racial or minority related attitudes measured in various ways.[4] Cetre et al. (2020) find that people who transfer different

[1]See the reviews in Rodgers (2006) and Lang and Spitzer (2020).

[2]Francis and Wright-Rigueur (2021) provide a historical overview regarding the origins of the BLM movement and its predecessors fighting racial injustice, structural racism, and state violence.

[3]Mollerstrom et al. (2021) show that voluntary giving correlates positively with group-wide redistribution in small groups.

[4]Recently, studies investigated the effect of information treatments on racial attitudes. Haaland and Roth (2021) uncover significant partisan gaps when asking for quantitative beliefs about the

amounts in the interethnic trust game (TG) dependent on the receiver's ethnicity have less positive attitudes towards immigrants, using the first wave of the Trustlab from the U.S. and Germany. Stepanikova et al. (2011) find that racial bias in an Implicit Association Test (IAT) is negatively correlated with generosity towards Blacks in a dictator game.[5] Peyton and Huber (2021) find that measures of racial prejudice predict behavior in the Ultimatum Game (UG), whereas racial resentments do not. Carlsson and Eriksson (2016) find that survey measures indicate more negative attitudes towards minorities in municipalities with stronger discrimination measured in a correspondence study in the Swedish housing market. Concordantly, Fong and Luttmer (2009) report that respondents who report feeling close to their race give less to members of other ethnic groups than their own. This paper connects to the existing literature demonstrating the distinct correlational pattern of prosociality and discrimination on the one hand, with racial prejudice against African Americans and the support for one of the movement's primary goals on the other hand. The findings in this paper suggest that racial attitudes and opinions about racial issues are multi-faceted. Prejudice is relatively more related to discrimination in the interethnic trust games. In contrast, the support for one of the main goals of the BLM movement, i.e., the importance attached to equal treatment of African Americans by the police, is foremost associated with prosociality in the general population.

The results demonstrate that discrimination against African Americans relative to white Americans in interethnic trust games correlates with more substantial racial prejudice against African Americans expressed in the survey. However, discrimination shows almost no effect on the support for one of the BLM movement's primary goals, i.e., the importance attached to equal treatment of African Americans by the police. Prosociality measured in standard dictator and public goods games, on the other hand, is strongly positively correlated with support for the movement's goal (that African Americans should be treated with equal respect by the police). In contrast, prosociality shows almost no correlation with racial prejudice against African Americans. These results mask a significant heterogeneity along the lines of political orientation. Prosociality is significantly negatively related to racial prejudice against African Americans among non-right-wing respondents but positively among right-wing

extent of discrimination in the labor market and on Airbnb found in the studies by Bertrand and Mullainathan (2004) and Edelman et al. (2017), respectively. An information treatment about the true extent of discrimination harmonizes beliefs but not gaps in desired levels of pro-African-American policies. Alesina et al. (2021a) find that African Americans and white Democrats are more likely to attribute racial gaps to past slavery, discrimination, and racism than Republicans. Moreover, partisan differences already exist among teenagers. Like Haaland and Roth (2021), they show that information about the extent of racial gaps does not shift respondents' policy views, whereas a video explanation of systemic racism makes respondents more supportive of policies targeting racial inequities (Alesina et al., 2021a).

[5] Triplett (2012) surveys evidence on explicit racial bias and racial bias in the IAT.

respondents. Conjecturing that right-wing respondents believed their prosocial behavior in the games would mainly target white Americans may be a possible explanation for this result (see the last section). In contrast, there is no significant heterogeneity in the positive correlation of prosociality with support for the BLM movement's primary goal. Discrimination against African Americans versus white Americans in the interethnic trust game, on the other hand, correlates negatively with support for the movement's goal among non-right-wing and positively among right-wing respondents. However, despite the heterogeneity of correlations, experimentally measured prosociality and discrimination do not explain the vast gaps in racial attitudes between political camps, i.e., left-wing and right-wing respondents. The correlation of prosociality with racial prejudice against African Americans also yields a nonuniform result across both U.S. waves of the Trustlab. The correlation is slightly negative in the first wave but positive in the second wave.

This paper further aims to improve the understanding of whether racial attitudes and public opinion are affected by the disruptive wave of BLM protests in 2020 following the death of George Floyd. According to sociological and psycho-sociological theories of disruptive action, focusing events, and activated opinions, such disruptive events can affect political agenda-setting and achieve shifts in public opinion (Kingdon, 1995; Birkand, 1998; Lee, 2002). This paper provides supporting evidence for such theories. I further explore the interaction of experiencing the BLM movement's protests with the propensity of discrimination and prosocial behavior in standard experimental economic games, advancing existing research on protests' ability to shape public opinion.

The results contribute to an extensive literature on protests' and historical movements' impact on election outcomes and political attitudes in various other domains (Collins and Margo, 2007; Madestam et al., 2013; Wallace et al., 2014; Mazumder, 2018; Enos et al., 2019; Ketchley and El-Rayyes, 2021). For instance, Madestam et al. (2013) report that protests associated with the Tea Party Movement in 2009 led to increased public support for their political positions and higher Republican vote shares in subsequent midterm elections. More closely related, Sawyer and Gampa (2018) study change in implicit and explicit racial attitudes for a different period of BLM protests between 2009 and 2016, finding that white and Black Americans shift to a less strong preference for their own races. Similarly, Mazumder (2019) reports that a significant number of BLM protests in 2014 following the police killing of Michael Brown and Eric Garner reduced whites' racial prejudice, with the effect being more substantial for younger people. However, in certain circumstances, protests may also harm a movement by reducing public support for it, e.g., when impacting daily life activities. In this regard, Ketchley and El-Rayyes (2021) provides evidence that post-Mubarak Egypt protests were associated with less favorable views on democracy when protests in respondents' districts were long-lasting and targeted public space. Wasow (2020) finds that violent actions by African American protesters led to increased votes for the Republican party among white Americans. Similarly, Feinberg et al. (2020) find that extreme actions reduce support for movements due to detrimental effects on observers' emotional connection.

For the recent eruption of protest, Alesina et al. (2021a) report increases in attributing racial gaps to racism and discrimination and critical views about the police among white respondents after the death of George Floyd, which, however, fade by the end of June 2020. Dunivin et al. (2022) show that protests following the death of George Floyd shifted the public discourse in social media and newspapers in the U.S toward the movement's agenda, whereby the increased attention is sustained after the intensity slowed down. Using regionally aggregated data, Teeselink and Melios (2021) show that counties with more substantial BLM protest activity experienced an increase in support for Biden in the General Election of 2020. Moreover, they found that awareness of racial discrimination increased in those counties. Shuman et al. (2022) find no effect of BLM protests on prejudice among liberals and conservatives. However, the authors report a positive effect of protests on support for the goals of the BLM movement among conservatives living in liberal areas, especially when protests were a mix of violent and non-violent ones.[6] Finally, Reny and Newman (2021) show that the public opinion in the U.S. around the death of George Floyd shifted to a less favorable view on the police and increased perceived discrimination against African Americans, which, however, does not extend to conservative respondents, thereby contributing to increasing polarization and racialization.

This paper complements this literature, demonstrating a negative correlation between the contemporary local intensity of BLM protests and racial prejudice and a positive correlation with the support for the movement's goals.[7] Furthermore, BLM protests show a detrimental effect on trust in the police. This effect, however, is statistically significant only in some specifications and more so among African American respondents. These results suggest that the BLM movement could impact attitudes and views, at least in the short term. Moreover, the impact of BLM protests on the importance attached to equal treatment of African Americans by the police (one of the movement's main goals) is relatively homogeneous across political orientations. As opposed, the protests' impact on racial prejudice is considerably weaker for right-

[6] The vast majority of pro-BLM protests were non-violent, with only 6 percent covered in the ACLED data involving "reports of violence, clashes with police, vandalism, looting, or other destructive activity" (Kishi et al., 2021).

[7] The Trustlab data allows studying the explicit measure of support for one of the movement's main policy goals in addition to racial prejudice against African Americans. The study of Shuman et al. (2022) shares this advantage over existing research without survey questions concerning the BLM movement.

wing respondents than for the rest of the political spectrum. This failure to find a significant effect for right-wing respondents is consistent with the overall comparison of both Trustlab waves, which reveals that solely non-right-wing respondents in the second wave express slightly weaker racial prejudice against African Americans. In contrast, prejudice against African Americans is more or less constant among right-wing respondents across both waves, following the U.S's long-standing upward trend of political polarization (Levendusky, 2018; Iyengar et al., 2019; Druckman et al., 2021a). Furthermore, the effects of experiencing BLM protests on increased support for the movement and decreased prejudice against African Americans are primarily driven by respondents who do not discriminate against African Americans in the games and, regarding support for the movement, those who behave relatively more prosocial.

The remainder of this paper is structured as follows. Section 4.2 describes the Trustlab methodology and the sample characteristics. Section 4.3 presents the results. Section 4.4 concludes.

4.2 Trustlab Methodology & Variables & Sample

4.2.1 Trustlab Methodology

The OECD's Trustlab initiative has been run in eight countries (France, Germany, Italy, Japan, Korea, Slovenia, United Kingdom, and the United States) (Murtin et al., 2018). The initiative combines large-scale surveys on representatives samples of the countries' adult populations with incentivized economic experiments.[8] Hence, the Trustlab overcomes a frequent criticism of experimental approaches relying on small, non-representative samples (Cappelen et al., 2015). For a detailed description of the Trustlab platform, see Murtin et al. (2018).[9]

The analyses in this paper primarily draw from data from the two waves of the Trustlab initiative conducted in the United States. Because the Trustlab survey‘s recruitment for the second wave began shortly after the death of George Floyd in Minneapolis on the 25th of May 2020, leading to an enormous number of BLM protests in the U.S. and globally (Kishi et al., 2021; Dave et al., 2020), combining the survey with data on the timing and location of protests (Raleigh et al., 2010) associated with the BLM movement from ACLED (2021) offers a unique opportunity to explore the association of multi-faceted attitudes and experimental variables with the experience

[8]The survey company targeted representativeness regarding age, gender, and income. The Appendix shows that the sample is accurately representative along these targeted dimensions and close in terms of non-targeted dimensions such as political ideology.

[9]The questionnaire from the Trustlab platform can be found at the Open Science Framework https://osf.io/ebnm8/.

of a mass movement. In the following, I briefly describe the structure of the Trustlab and which variables I constructed based on its measures.

Experimental module. — The first module of the Trustlab consists of several incentivized experiments. First, participants play standard versions of the trust game (TG) (Berg et al., 1995), the public goods game (PGG), and the dictator game (DG) (Forsythe et al., 1994).[10] In every game, participants have an initial endowment of 10 USD. In the TG, participants could send any integer share of their endowment to a receiver. Transfers are multiplied by three. In the PGG, participants knew that they play with three other participants who could contribute to a joint project by transferring any integer share of their endowment. The money transferred to the project would be multiplied by 1.6 and split equally between all 4 group members independent of their contribution. Finally, in the DG, participants could transfer any integer share of their endowment to another participant from the U.S.

In addition to standard experimental games, U.S. Trustlab participants complete two interethnic trust games (TGs). In both interethnic TGs, participants make three decisions for the ethnicity of the receiver being White, African American, and Hispanic.[11] For the first interethnic TG, the platform does not reveal any information about the receiver's income. In the case of the second interethnic TG, the receiver is described as belonging to the richest 20 percent of the U.S. people. A novelty of the second U.S. wave is that participants play two interethnic DGs (with and without top 20 percent income information) encompassing the same ethnic groups as in the TGs. Receiver ethnicities (White, African American, Hispanic) appear in a randomized order in both games.

Survey module. — In the final survey module of the Trustlab participants answer a wide range of questions on trust, satisfaction with governmental institutions, and (political) attitudes.[12] Alongside, participants answer several questions on their demographics, such as their gender, race, educational attainment, and their economic situation. See Appendix Section 4.B.1 for an excerpt of relevant items from the questionnaire. Information on the respondents' zip codes allow to match the Trustlab data with fine-grained information at the geographical level.[13]

[10]Risk preferences are elicited by lottery choices (Eckel and Grossman, 2002).

[11]See Cetre et al. (2020) for an in-depth analysis of ingroup-outgroup relations in the interethnic TG using U.S. and German Trustlab samples.

[12]In the first wave of the U.S. Trustlab, participants completed a module of Implicit Association Tests which are no longer part of the second wave.

[13]In the Appendix Section 4.B.2, I describe the process of retrieving the local data and providing a detailed list of the local variables from various sources.

4.2.2 Variables

Outcome variables. — The primary outcome variables related to racial attitudes are based on two Trustlab survey items, both being 11-point Likert-scale questions on the 0 to 10 interval. The first question refers to the demonstrations following the death of George Floyd and is thus only available from the second wave. It reads, "*Recent weeks saw renewed attention to the interactions between African Americans and policemen, with the death of George Floyd in Minneapolis, in particular, leading to large demonstrations. Where would you say that your own reaction lies along a scale from: 0 = The issue has been overblown by the media to: 10 = It should be an urgent priority of our society and leaders to reform our police departments so that African Americans are treated with equal respect and can feel trust in the police, rather than fear.*". I will refer to this question as a proxy measuring the **support for the BLM movement's primary goal** or as the importance attached to that equal respect should be an urgent priority (calling for police reforms).[14]

The second question asks for the believed causes of racial disparities in economic outcomes and is intended to measure **prejudice toward African Americans** in general: "*On the average Blacks/African Americans have worse jobs, income, and housing than white people. Do you think the differences are mainly due to discrimination and disadvantages of educational opportunity, mainly due to differences in inborn ability, motivation, and effort, or some combination?*". Respondents could place their views between 0 ("Mainly discrimination and educational disadvantage") and 10 ("Mainly lesser ability, motivation and effort"). In the following, I refer to this question as "Prejudice against African Americans", or short as the prejudice variable.

The third outcome variable is based on the question measuring **trust in the police**. Respondents were asked, *"When answering the following questions, please think about the United States institutions. How much trust do you have in the following?"* where they could state their score for "The police" between 0 ("I do not trust them at all") and 10 ("I completely trust them").

Experimental measures of prosociality and discrimination. — Two measures in the following analyses are based on participants' behavior in the experiments. First, the index of prosociality is constructed based on choices in the dictator game (DG) and the public goods game (PGG). The index is the standardized (subtracted the mean and divided by the standard deviation) average of the standardized choices (monetary transfers) in both games.[15] The index solely relies on DG and PGG choices because they

[14]For space reasons, I abbreviate the variable in captions or figures as "support (for the) BLM movement."

[15]The same index of prosociality is used in the study by Grimalda et al. (2022b) based on the Trustlab data from the second U.S. wave.

are clean from expectations about others' behavior, and the (selfish) equilibrium is not to send (in the DG) or contribute (in the PGG) at all in both games. A Cronbach's alpha of 0.63 (average interitem covariance = 0.48), calculated based on both standardized components, indicates an appropriate level of index reliability.

Respondents are classified as "prosocial" if their prosociality index score exceeds the sample median.[16] The second measure is an indicator variable for discrimination against African Americans in the interethnic trust games (TGs). The indicator takes the value of one if the Trustlab participant sends less to an African American participant than to a white person in - at least - one of the interethnic TGs.

Further (local) variables. — Data on the location and timing of BLM protests come from the ACLED (2021) project. Based on the ACLED (2021) data, the primary variable of protest intensity in the analyses in the following section is the natural logarithm of one plus the sum of inverse distances of BLM protests to the respondent's zip code in the time frame between the survey date and 14 days before the respondent took part. In addition, I analyze other variables measuring the local and temporal intensity of BLM protests for Trustlab respondents in the Online Appendix. Further variables serving as contextual controls comprise the number of corona cases per 100,000 inhabitants in the respondent's county up to the survey date from the The New York Times (2021), self-reported (in the survey) exposure to COVID-19, and various indicators of local racial gaps matched with the Trustlab data. A detailed list of the local variables alongside the respective data sources is provided in the Online Appendix Section 4.B.2.

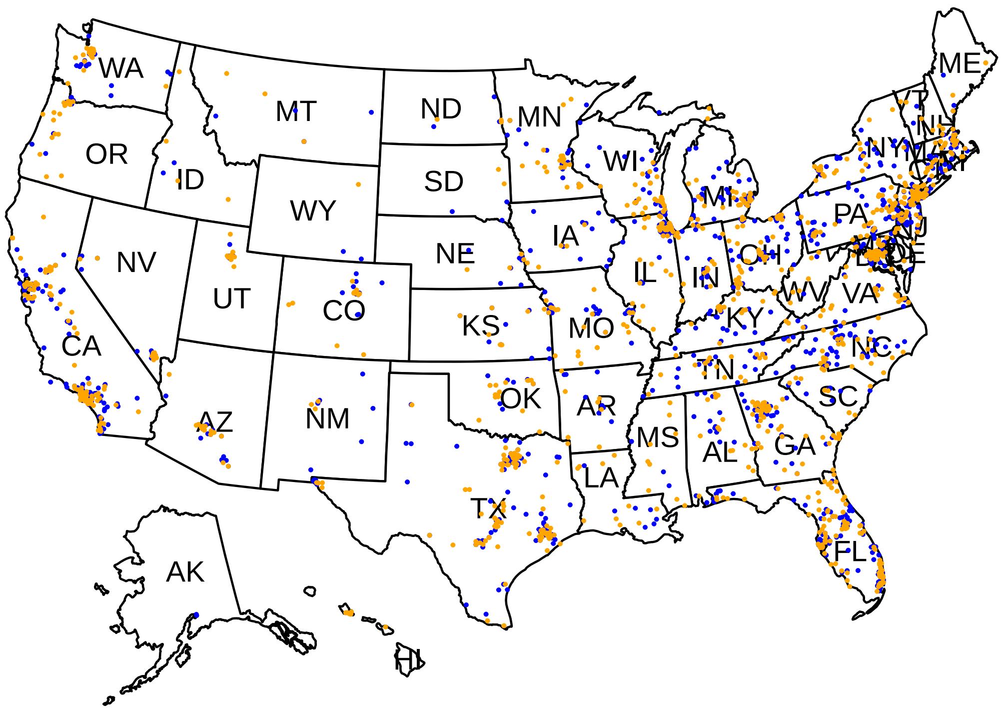
4.2.3 Sample Characteristics

Table 4.1 in the Appendix shows sample characteristics for both waves of the U.S. Trustlab. The whole sample covers 2,210 participants, of which 1,090 participated in the first wave and 1,120 in the second wave. The sample is broadly representative of the U.S. population in the targeted dimensions of age, gender, and income. The first wave's data collection started on June 2nd and finished on September 7th, 2017. Data collection of the second wave of the Trustlab started, about three years later, on June 12th, 2020, shortly after the death of George Floyd on May 25th, and completed on September 7th in the same year.[17] Figure 4.1 shows the geographical distribution of Trustlab participants from both waves over the U.S.

[16]In the Appendix, I also report analyses using the original prosociality index instead of the variable based on the median split yielding equivalent conclusions.

[17]COVID-19 cases and deaths in the U.S. were quickly rising at this time (The New York Times, 2021).

Figure 4.1: Location of Trustlab Participants



Notes: This figure shows the geographical distribution of participants in both waves of the U.S. Trustlab based on their zip codes. Blue (orange) dots indicate participants from the first (second) wave.

4.3 Results

4.3.1 Descriptives

Attitudes by Characteristics. — Figure 4.2 shows mean answer scores for the three outcome variables (± standard error) by respondents' characteristics. Panel A shows the average scores over both waves ("U.S. Citizen") and the averages for both extremes of the political spectrum.[18] Panel B depicts the averages by groups defined by socio-demographic characteristics.

The average score for the support for one of the main goals of the BLM movement, calling for police reform (i.e., the importance attached to equal treatment of African Americans by the police), is 6.4 (SD = 3.36, N = 1120). More than 56 percent of the sample reports a score of 6 or above, indicating a strong awareness of racial issues in the second wave sample. Nonetheless, the second considered outcome variable indicates a substantial level of racial prejudice (M = 5.5, SD = 3.02). The average score for trust in the police is 6.1 (SD = 2.63). The defining characteristics concerning the support for (one of) the BLM movement's main goals, racial prejudice against them, and trust in the police are both extremes of the political spectrum and racial groups.[19] Right-wing respondents have stronger racial prejudice (M = 7.2 vs. M = 2.7), report higher levels of trust in the police (M = 7.2 vs. M = 4.6) score lower whether on the support for the movement (M = 5.5 vs. M = 8.6) than left-wing respondents. Among the socio-demographic characteristics, especially disparities between ethnicities are remarkable. African Americans score higher on support for the movement (M = 8.3 vs. M = 6.2), lower on racial prejudice against African Americans (M = 4.2 vs. M = 5.7), and lower on trust in the police (M = 4.3 vs. M = 6.5) than white Americans. The differences between both extremes of the political spectrum and African Americans and white Americans depicted in Figure 4.2 are all statistically highly significant ($p < 0.001$, t-tests, see Table 4.2). The other contrasted socio-demographic groups reveal slightly less pronounced differences. High education is associated with weaker prejudice, higher trust in the police, and more vital support for the BLM movement's primary goal. In contrast, males are slightly more prone to prejudice against African Americans and tend to have higher trust in the police than females.

[18] Political orientation is elicited by the question "In political matters, people often talk of "the left" and "the right." How would you place your views on this scale, generally speaking?" where 0 is "left" and 10 is "right". Respondents are classified as right-wing (left-wing) for scores larger (smaller) or equal 7 (3).

[19] The self-reported ethnic categories of "African Americans" and "whites" refer to "non-Hispanic blacks" and "non-Hispanic whites," respectively. The terms "African Americans" and "Blacks" are used interchangeably throughout the paper, following the terminology of the the Trustlab survey and the United States Census Bureau (see https://www.census.gov/topics/population/race/about.html).

Behavior in Experiments. — On average, participants sent almost half of their endowment in the DG (M = 4.8, SD = 2.7) and contributed roughly 61.1 percent (M = 6.1, SD = 3.2) to the joint project in the PGG. The prosociality index based on transfers in the DG and PGG is relatively constant across waves, with a t-test showing no significant difference (p = 0.265, N = 2210, two-sided t-test). In the first wave, 11.4 percent (SD = 0.3) of participants sent less to African Americans than to white Americans in one of the interethnic TGs. This share is slightly larger in the second wave (M = 0.14, SD = 0.35), with the difference being marginally significant (p = 0.047, N = 2210, two-sided t-test).[20]

BLM Protests. — On average, there were 7.2 protests (SD = 11.2) of the BLM movement in respondents' counties during the time frame back 14 days from when the respondents participated up to the day of the survey. The minimum number of protests in a respondent's county during this time frame was 0, and the maximum was 90. Similarly, counting protests in a radius of 50 kilometers around the centroid of a respondent's zip code (ZCTA) yields a mean of 21.5 protests, with a minimum of 0 and a maximum of 202 protests. Overall, these numbers underline the currency of the topic during the second wave of the Trustlab.

4.3.2 Main Results

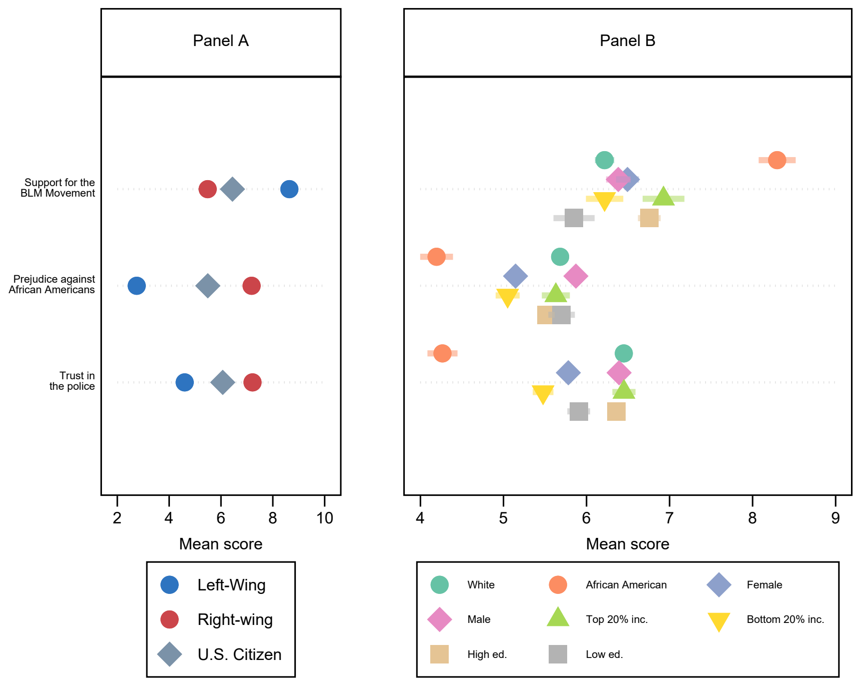
Section 4.3.2.1 uncovers distinct correlational patterns of experimental measures of discrimination and prosociality with the support for one of the BLM movement's primary policy goals and racial prejudice against African Americans that vary across camps of political orientation. Section 4.3.2.2 analyzes the impact of BLM protests on the attitudinal outcome variables. It leverages the experimental measures of prosociality and discrimination to explore the heterogeneity of the protests' impact on respondents' attitudes.

4.3.2.1 Behavior in Experiments and Racial Attitudes

Figure 4.3 depicts OLS regression coefficients and their 95 percent confidence intervals for the explanatory variables indicated on the y-axis (see the Table 4.5 in the Online Appendix for the underlying regression results). The dependent variables in the two columns are (i) support for one of the main goals of the BLM movement and (ii) prejudice against African Americans, respectively. The figure depicts coefficients for two explanatory variables. The first is an indicator variable ("Prosocial") indicating an above-median prosociality index (based on transfers in the DG and the PGG, see the

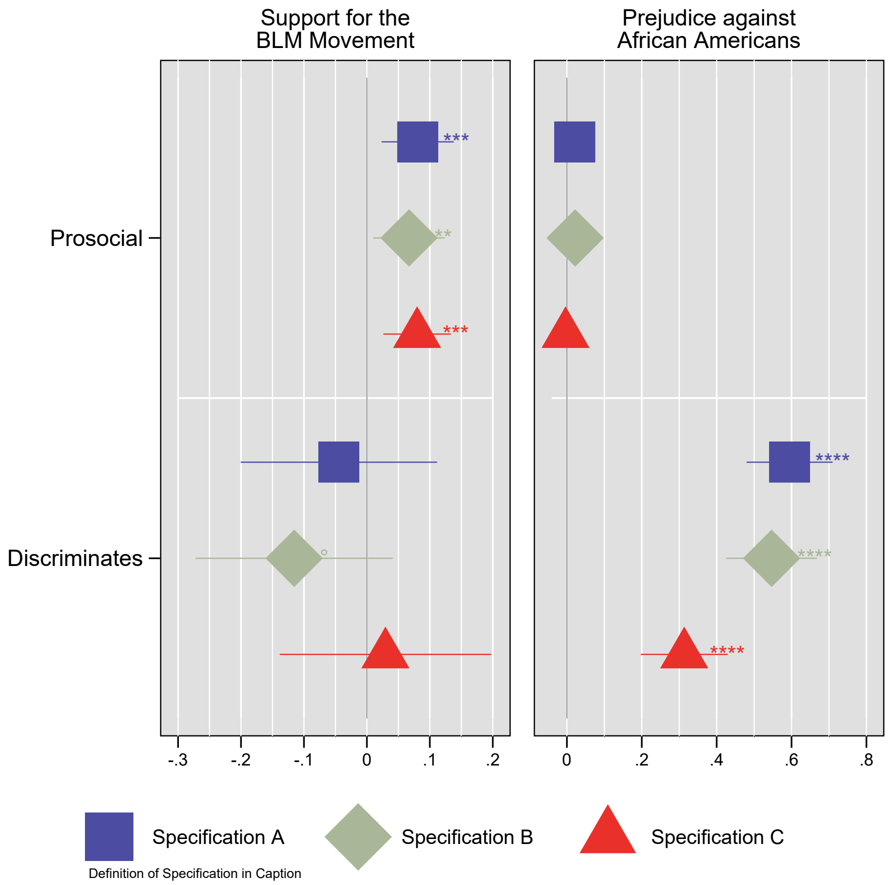
[20]The difference between both waves is not statistically significant in an OLS regression controlling for political orientation dummies and race (p= 0.122).

Figure 4.2: Attitudes by Characteristics



Notes: The figure shows the mean answer score (± 1*SE) for the three outcome variables (support for the primary goal of the BLM movement, racial prejudice against African Americans, and trust in the police). The questions (see the section above) are on the 0-10 interval. Panel A shows the mean values for right-wing and left-wing respondents and the overall mean ("U.S. Citizen"). Panel B shows the mean scores for females and males, African Americans and white Americans, people in the top 20 percent income quintile and people in the bottom 20 percent income quintile, people with a high education level (Tertiary diploma) and people with a low education level (High school or less).

Figure 4.3: Main Results: Prosociality and Discrimination



Notes: The figure shows standardized coefficients and 95 percent confidence intervals from OLS regressions. Dependent and independent variables (y-axis) standardized except for the dummy variables. Standard errors robust (clustered at the county-level) in Panel A (B). (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively. The indicator Prosocial is equal to 1 if the prosociality index based on transfers in the standard DG and contributions in the PGG (see the section above) is above the sample median. The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). Regression specifications vary by Panel. Panel A: Specification A contains only a dummy for the Trustlab Wave 2, the experimental measures of above-median prosociality and discrimination, and race dummies. Specification B additionally includes the set of socio-demographics (age, age-squared, sex, top and bottom income dummies, education dummies, urbanization dummies, and employment dummies). Specification C adds political orientation dummies.

previous section). The second is the indicator variable (“Discriminates”) that equals one for discrimination against African Americans relative to white Americans in at least one of the interethnic TGs. The dependent variables are standardized. Coefficients from indicator variables, e.g., for the above-median prosociality and discrimination against African Americans relative to white Americans in the experiments, are denoted as b’s in the following. As depicted in the legend, the regression models differ by the variables included. The “Specification A” model in Figure 4.3 only contains race and wave dummies besides the variables shown on the y-axis. The “Specification B” model adds socio-demographic variables (denoted in the caption). Finally, the third specification contains all variables from the previous model and dummies for the respondent’s political orientation.

Prosocial and discriminating behavior. — Prosocial respondents, i.e., those with an above-median level of prosociality, express stronger support for one of the main goals of the BLM movement, i.e., that the treatment of African Americans with equal respect by the police should be an urgent priority. Across specifications, the estimated coefficient is of similar size and statistically significant (b = 0.08, p = 0.006; b = 0.07, p = 0.021; b = 0.08, p = 0.004, respectively). On the contrary, prosocial behavior is relatively weakly correlated with racial prejudice against African Americans.[21] As such, the coefficient of “Prosocial” is only slightly positive in the first two specifications without controlling for political orientation (b = 0.02, p = 0.330; b = 0.02, p = 0.307) and even smaller when doing so (b = -0.00, p = 0.847, respectively).[22] Those respondents who discriminate against African Americans relative to white Americans in at least one of the interethnic TGs, have stronger prejudice against African Americans in every specification (b = 0.59, $p < 0.001$; b = 0.55, $p < 0.001$; b = 0.31, $p < 0.001$, respectively). On the contrary, the dummy for discriminative behavior does not show significant effects on the variable measuring support for the primary goal of the BLM movement.[23]

Heterogeneity analysis. — Results depicted in Figure 4.3 mask a significant heterogeneity concerning participants’ political orientation on the estimated effects of the

[21]The finding that prosociality is only weakly correlated with racial prejudice is not homogeneous across both waves. Instead, the interaction term of the Prosocial indicator and the dummy for the second wave is statistically significant (b =0.17, p = 0.023). Results in Table 4.10 in the Appendix show that the correlation is negative in the first wave of the Trustlab, i.e. prosocial respondents have weaker prejudice (b = -0.10, p = 0.074). In the second wave, this association is reversed, as prosocial respondents, albeit not statistically significant, have even slightly more substantial prejudice against African Americans (b = 0.08, p = 0.154).

[22]The heterogeneity analysis at the end of this subsection shows that the coefficient is positive for right-wing respondents and negative for non-right-wing respondents.

[23]Similar results arise using a dummy variable indicating discrimination against African Americans relative to white Americans in the interethnic dictator game (DG). The results are qualitatively identical, examining discrimination in any interethnic DG (with and without income information). However, this analysis is restricted to the second wave as the first wave did not contain the interethnic DG.

experimental variables. Figure 4.4 shows the results from an heterogeneity analysis where the indicator variables for prosocial respondents and those who discriminate against African Americans in the experimental games are interacted with indicator variables for (a) right-wing political orientation, (b) female sex, and (c) African American ethnicity (also see Online Appendix Table 4.10).

For respondents who did not identify as right-wings, above-median prosociality is negatively and statistically significantly correlated with weaker prejudice against African Americans ($b = -0.09$, $p = 0.073$). In contrast, the partial correlation of prosociality is positive for right-wing respondents ($b = 0.14$, $p = 0.017$) and significantly different to that of non-right-wings ($b = 0.23$, $p = 0.003$, for the interaction Prosocial $\times$ H).

The partial correlation of discrimination against African Americans in one of the TGs with racial prejudice is significantly lower (but positive) among right-wing respondents ($b = 0.18$, $p = 0.010$) than among non-right-wings ($b = -0.29$, $p = 0.015$ for the interaction Discriminates $\times$ H), for whom discrimination is associated with much stronger prejudice ($b = 0.47$, $p < 0.001$). Regarding the support for the BLM movement outcome variable, discrimination in one of the TGs is associated with lower scores among the non-right-wing ($b = -0.54$, $p < 0.001$) but, surprisingly, with higher scores ($b = 0.42$, $p < 0.001$) for the right-wing respondents ($b = 0.95$, $p < 0.001$ for the interaction Discriminates $\times$ H). Hence, right-wing respondents who discriminate against African Americans relative to white Americans in the interethnic trust games state more support for the BLM movement than those right-wing respondents who do not discriminate against them in the games. The partial correlation of above-median prosociality with support for the movement's goal is, however, positive for non-right-wings ($b = 0.11$, $p = 0.100$) and right-wings ($b = 0.20$, $p = 0.031$), with the difference being not statistically significant ($b = 0.08$, $p = 0.467$ for the interaction Prosocial $\times$ H).

Furthermore, as shown in Section 4.3.1, the differences in the outcome variables between right-wing and left-wing respondents are remarkable. Differences between camps of political orientation amount up to 0.9 of an SD ($p < 0.001$) and 1.5 of an SD ($p < 0.001$) for the support for the BLM movement and racial prejudice variables, respectively. Differences between right-wing and left-wing respondents with respect to both outcome variables are only mediated to a negligible extent by the variables for behavior in the experiments. Hence, prosociality and discrimination (see Online Appendix Table 4.19) do not explain the significant ideological gaps between political camps. This finding suggests that differences between both groups are relatively independent of their prosociality or propensity to discriminate and are instead based on ideology.[24]

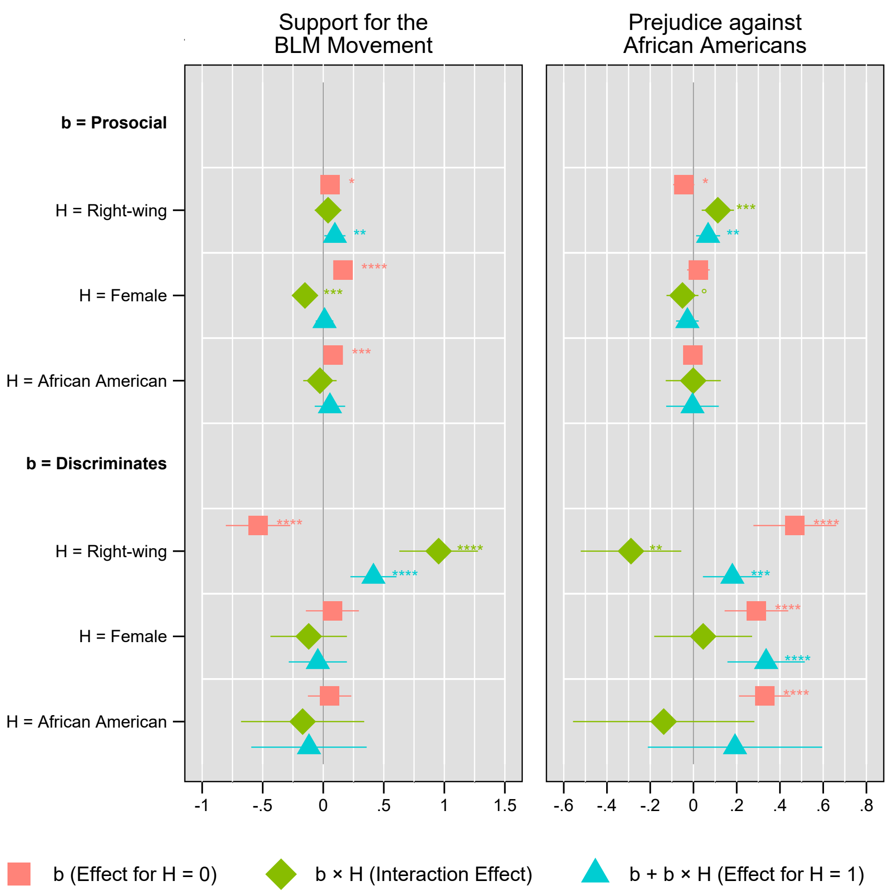
The heterogeneity analysis concerning the female sex shows that the positive association of above-median prosociality with support for the goal of the BLM movement is driven by male respondents, for which the coefficient is highly significant (b = 0.33, $p < 0.001$). Instead, the coefficient is tiny for female respondents (b = 0.02, p = 0.777), with the difference to males being strongly significant (b = -0.31, p = 0.005, for the interaction Prosocial $\times$ H). There is no statistically significant heterogeneity along the gender dimension for the discrimination indicator with support for the BLM movement's primary goal as the outcome variable. There is also no significant heterogeneity between males and females regarding racial prejudice against African Americans as the outcome variable for the effects of prosociality and discrimination. Similarly, the heterogeneity analysis does not reveal significantly different correlational patterns among African American respondents relative to the remaining ethnicities regarding the experimental behavior.

Robustness checks. — Results regarding prosociality are qualitatively equivalent when using the standardized prosociality index instead of the indicator for above-median prosociality (see Table 4.6 in the Online Appendix). Furthermore, an alternative proxy for prosociality available from the Trustlab are participants' voluntary donations to UNICEF. Since earnings are different across participants, the share of donated earnings serves as the alternative prosociality measure. As shown in Online Appendix Table 4.7, respondents who donate larger shares of their earnings score significantly higher on the support for the BLM movement ($\beta = 0.14$, $p < 0.001$) and only marginally lower on "Prejudice against African Americans" ($\beta = -0.04$, p = 0.106). The correlational pattern of the discrimination dummy with the outcome variables remains qualitatively the same. There is no significant effect on the support for the BLM movement's primary goal but a significant positive correlation with racial prejudice.[25]

[24]Prejudice against African Americans is, on average, slightly weaker in the 2020 wave than in the 2017 wave of the Trustlab (see Online Appendix Table 4.5). This result is most pronounced in the first and third specification (b = -0.08, p = 0.068, b = -0.05, p = 0.287, b = -0.08, p = 0.042). The analysis in Table 4.10 in the Appendix exploring the heterogeneity reveals that those participants who did not identify as right-wing drive the reported decreased racial prejudice against African Americans between both waves of the Trustlab (b = -0.14, p = 0.007). In fact, the point estimate is even positive for right-wing participants meaning an increase of racial prejudice between 2017 and 2020, albeit far from being statistically significant (b = 0.02, p = 0.762; b = 0.16, p = 0.040 for the interaction Wave 2 $\times$ H). The results are consistent with the findings concerning the impact of BLM protests in the following subsection.

[25]The voluntary donation variable has 323 (485) missing values in the first (second) wave. Results are equivalent in terms of their sign and statistical significance when coding missing values as zero donations. Coefficients from a model with the same covariates as in the third specification from Figure 4.3 reported. Regressions with donations control for the actual payoff earned by the respondent. Results are equivalent with and without controlling for the actual earnings.

Figure 4.4: Heterogeneity Analysis: Prosociality and Discrimination



Notes: The figure shows (sums of) standardized coefficients and 95 percent confidence intervals from OLS regressions. Dependent and independent variables (y-axis) standardized except for the dummy variables. Standard errors robust (clustered at the county-level) in Panel A (B). (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively. The heterogeneity analysis with respect to right-wing politicial orientation, female sex, and African American ethnicitiy contains the same control variables as in the model specification "C" of the respective main results figure. The underlying regression tables are provided in the Appendix.

Figure 4.14 in the Appendix depicts coefficients on the outcome variables for both components of the prosociality index and transfers and trustworthiness in the TG. DG transfers and PGG contributions reveal a similar correlational pattern with the outcomes as the index. Furthermore, trust and trustworthiness in the TG without information about the receiver's ethnicity do so as well. This finding underlines the robustness of the distinct correlational pattern of behavior in the experiments with the outcome variables.

In addition, the data from the second wave allows to compare behavior in an interethnic DG as a measure of prosociality, or generosity, specifically towards African Americans and the generosity in the DG without ethnic information. Regressions show that DG transfers towards African Americans correlate similarly with both outcome measures as the prosociality index based on the DG and the PGG without information about the race of the receiver.[26] In particular, altruism towards African Americans does not correlate significantly with racial prejudice, unlike discrimination in the interethnic TG. However, it does positively correlate with the support for the BLM movement (see Online Appendix Table 4.8).[27]

Local racial gaps. — The fine-grained local information allows examining the correlations between local exposure to racial gaps and views on racial issues (see Online Appendix Table 4.9). Similar to Alesina et al. (2021a), I use the share of African American residents and an index of local racial gaps in the respondents' neighborhoods. The index increases in African Americans' (economic) disadvantages compared to white people at the zip code level. It is the first principal component of the differences between Black and white people in (a) two levels of educational attainment (the percentages of less than High School and Bachelor's degree or higher), (b) the unemployment rates, and (c) per-capita incomes.[28] A Cronbach's alpha (based on standardized components) of 0.56 (average interitem covariance = 0.24) indicates an appropriate index reliability. The share of African American residents correlates positively with the strength of prejudice against African Americans ($\beta = 0.06$ $p = 0.008$). On the contrary, the correlation with the support for the BLM movement's primary goal is weak ($\beta = 0.01$, $p = 0.608$). The picture is the other way round for the strength of local racial gaps. Respondents living in a zip code with more substantial racial gaps report higher support for the BLM movement's main goal ($\beta = 0.07$, $p = 0.029$). However, local exposure to

[26] The transfer in the standard DG (without race information) restricted to the second wave delivers qualitatively identical results.

[27] The results are also qualitatively unaffected by controlling for trust measured in the TG without information about the receiver's ethnicity.

[28] The local data on population composition and racial gaps are from the 5-year ACS of 2015-2019. Results are qualitatively similar when including less fine-grained variables from Opportunity Insights (2021) at the county level, albeit only reaching statistical significance in the model without individual controls. Alesina et al. (2021a) use a similar set of variables.

racial gaps does not correlate with prejudice against African Americans (β = -0.00, p = 0.943). The results on the other variables, i.e., behavior in the experiment and the difference between waves, are qualitatively unaffected by adding local variables to the model (see Online Appendix Table 4.9).[29]

4.3.2.2 The Impact of Black Lives Matter Protests

Figure 4.5 depicts OLS regression coefficients and their 95 percent confidence intervals for the explanatory variables based on the quantities indicated on the y-axis (see Table 4.12 and Table 4.13 in the Online Appendix for the underlying regression results). The dependent variables in the three columns are (i) support for one of the main policy goals of the BLM movement, (ii) prejudice against African Americans, and (iii) trust in the police, respectively.

The explanatory variables for which coefficients are depicted are different measures of the intensity of contemporaneous BLM protests.[30] The time frame used to construct the following protest variables in Figure 4.5 is between the respondent-specific survey date and 14 days before. Only protests during the respondent-specific time frames contribute to the variables.

The explanatory variables are (i) the natural logarithm of one plus the sum of inverse distances of BLM protests (in the time frame) to the respondent's zip code (based on the centroid of the zip code (ZCTA))[31], (ii) the natural logarithm of one plus the number of protests (in the time frame) in the county where the respondent resides, (iii) the natural logarithm of one plus the number of protests (in the time frame) within a radius of 50 kilometers around the respondent's location, and (iv) an indicator that is equal to one for the respondent living in a county with an above-median (relative to the sample median) value of the natural logarithm of one plus the number of protests (in the time frame) in the respective county. The dependent and independent variables (coefficients denoted as β's) are standardized, except for indicator variables (coefficients from indicator variables are denoted as b's in the following).

The regressions on which the coefficients of Figure 4.5 are based differ from the specifications in the previous subsection. The first specification already contains the

[29] Panel A in Figure 4.3 depicts results without local variables to use the highest number of observations because of 15 missing values in zip codes (and local variables).

[30] Respondents from U.S. Pacific Islands region were excluded in the analysis of the impact of BLM protests.

[31] I applied the ln(x+1) transformation due to the right-skewed distribution of the (original) protest variables with many values close or equal to zero and to de-emphasize outliers (Metcalf and Casey, 2016b). Most of the results are qualitatively similar without applying the transformation. However, the effects of protest variables on the prejudice against African Americans outcome variable become statistically insignificant without the transformation, whereas effects on the support for the movement outcome remain mostly statistically significant.

complete set of individual socio-demographic controls (including respondent's race) as in "Specification B" from the previous subsection (Section 4.3.2.1). The second model further controls for local population characteristics (the share of residents younger than 18 years and older than 65 years, the share of female residents, and the share of African American residents), the principal component index of local racial gaps, and a dummy equal to one if the Republican party won the general election in 2016 in the respondent's county. Finally, the third model controls for the same variables as the previous specification and political orientation dummies. As the pandemic may affect views on racial topics[32], the regressions control for an indicator of personal exposure to COVID-19 and the natural logarithm of one plus the absolute number of confirmed COVID-19 cases per 100,000 inhabitants at the county level up to the survey date.[33] In addition, the three specifications contain a linear time trend for the days since the death of George Floyd.

BLM Protests. — The variable based on inverse-distance weighted BLM protests positively correlates with the support for the BLM movement's main goal. This result holds true for all three specifications. The coefficient is largest in the first specification ($\beta = 0.17$, $p < 0.001$) but remains statistically significant also when controlling for local variables and political orientation ($\beta = 0.12$, $p = 0.007$). The impact of BLM protests on prejudice against African Americans is of the reversed sign. A more substantial exposure to BLM protests is associated with relatively weaker prejudice ($\beta = -0.13$, $p = 0.009$; $\beta = -0.15$, $p = 0.004$; $\beta = -0.09$, $p = 0.023$, respectively). The exposure to

[32]A large literature considers the effects of important experiences such as natural disasters or recessions on attitudes and preferences (Malmendier and Nagel, 2011; Giuliano and Spilimbergo, 2014; Cassar et al., 2017) that extend to prosocial attitudes, particularly across racial or ethnic lines (Solnit, 2009; Bauer et al., 2016; Becchetti et al., 2017; Cassar et al., 2017). Undoubtedly, the life-threatening pandemic and its even more pronounced consequences for marginalized groups (Endocrinology, 2020; Elias et al., 2021) can impact views on racial issues. Historically seen, pandemics have frequently been times of increasing "othering", racial animosity, deepened marginalization of disadvantaged groups, and blaming these groups for the disease's spread (Ungar, 1998; Washer, 2004; Dionne and Turkmen, 2020; White, 2020). Considerable parts of the literature concerning the current pandemic focused on attitudes towards Asian descent (Reny and Barreto, 2020; Lu and Sheng, 2022; Tahmasbi et al., 2021), motivated by President Trump's and others framing the SARS-COV2 as the "Wuhan virus," and similar discriminatory terms. For example, Lu and Sheng (2022) document a rise in anti-Asian attitudes at the pandemic beginning and Tahmasbi et al. (2021) provide evidence for an increase in Sinophobic content on internet platforms following the COVID-19 outbreak. In case of the U.S., Reny and Barreto (2020) find that worries about the pandemic are associated with less positive attitudes towards Asian- but not Black Americans. Bartoš et al. (2021) show that increasing the salience of the COVID-19 pandemic in a sample from the Czech Republic induces greater hostility against foreigners from other countries (European Union, U.S., Asian countries) in terms of causing financial harm without personal benefit. Furthermore, Cunningham and Wigfall (2020) demonstrate a positive correlation between racial bias and COVID-19 cases at the U.S. county level.

[33]33.8 percent (N = 372) of the respondents reported exposure to COVID-19 in one way or another. The most common reason is that a friend or family member the respondent does not live with has been diagnosed (18.9 percent of the sample). The average total number of cases per 100,00 inhabitants at the county level was 896.9 (SD = 862.4) up to the respondent-specific dates of the survey.

BLM protests tends to show detoriating effects on trust in the police which, however, are weaker than for the other outcome variables and reach (marginal) statistical significance in the first specification only (β = -0.08, $p = 0.059$). Overall, the other measures of the BLM protest's intensity show results going in the same direction as those with the variable based on the inverse-distance weighted number of protests. However, the (slightly) negative impact on trust in the police appears to be more vital for the more locally restricted measures, such as the variable based on protests within 50 kilometers around where the respondent resides.

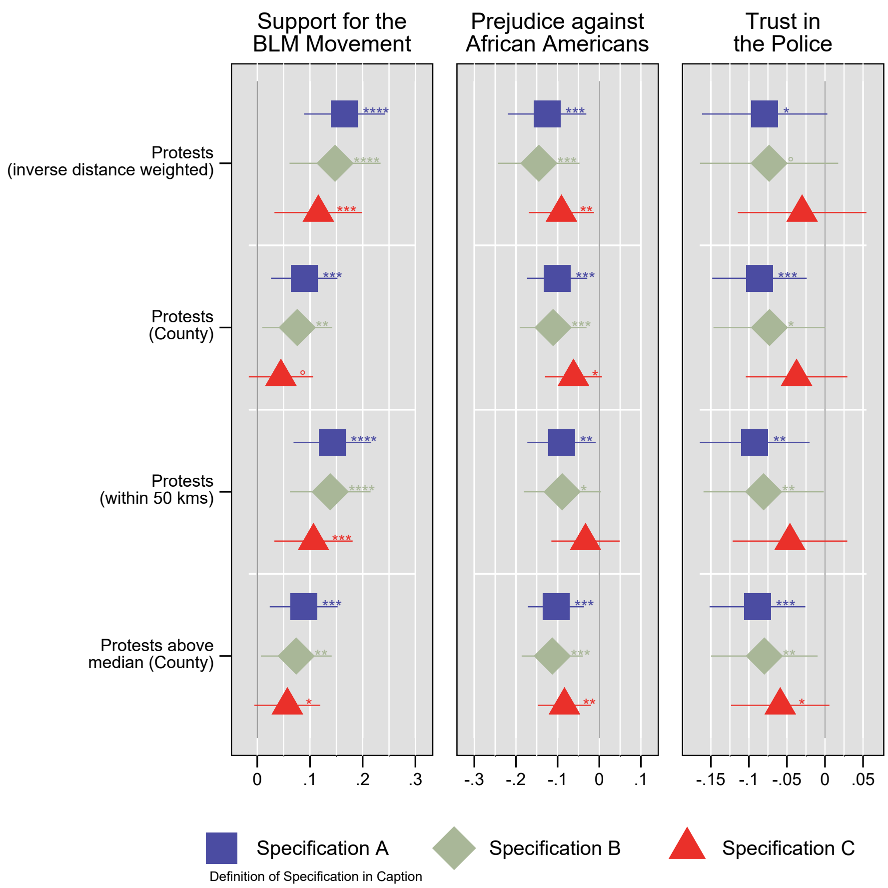
Heterogeneity analysis. — Figure 4.6 depicts results from a heterogeneity analysis where indicators for the groups of (a) right-wing respondents, (b) female respondents, (c) African American respondents, (d) respondents with an above-median score of the prosociality index, and (e) respondents who discriminate against African Americans in the interethnic TGs are interacted with the measure of protest intensity (also see regressions in Online Appendix Tables 4.14, 4.15, and 4.16). The model specifications in Figure 4.6 are based on the model "Specification C" from Figure 4.5 using the variable based on inverse-distance weighted BLM protests as the explanatory variable of interest.

Political orientation. — The effect of BLM protests on the support for the BLM movement is positive for right-wing respondents (β = 0.11, $p = 0.027$) and for the rest of the political spectrum (β = 0.12, $p = 0.021$), with the difference far from being statistically significant (β = -0.01, $p = 0.888$ for the interaction BLM × H). However, in the case of racial prejudice, the effect of BLM protests is considerably weaker for right-wing respondents (β = -0.04, $p = 0.300$) than for non-right-wing respondents (β = -0.14, $p = 0.010$). While for right-wing respondents, the effect is no longer statistically significant, the difference relative to non-right-wing respondents reaches marginal statistical significance (β = 0.1, $p = 0.071$ for the interaction BLM × H). There is no statistically significant heterogeneity concerning political orientation for the impact on trust in the police. Supplementary analyses in the Online Appendix Table 4.14 do not reveal any statistically significant heterogeneity of BLM protests on any of the outcomes concerning whether the county was pro-Trump in the 2016 general election, unlike in Shuman et al. (2022).[34]

Experimental behavior. — For respondents with an above-median index of prosociality, the effect of BLM protests on the support for the movement outcome variable is significantly larger than among respondents below (or equal to) the median, as can be seen in Figure 4.6 (β = 0.12, $p = 0.011$, for the interaction BLM × H). In fact, the effect is statistically insignificant for the respondents without above-median prosociality (β = 0.05, $p = 0.362$), whereas it is strongly significant among the more prosocial respondents

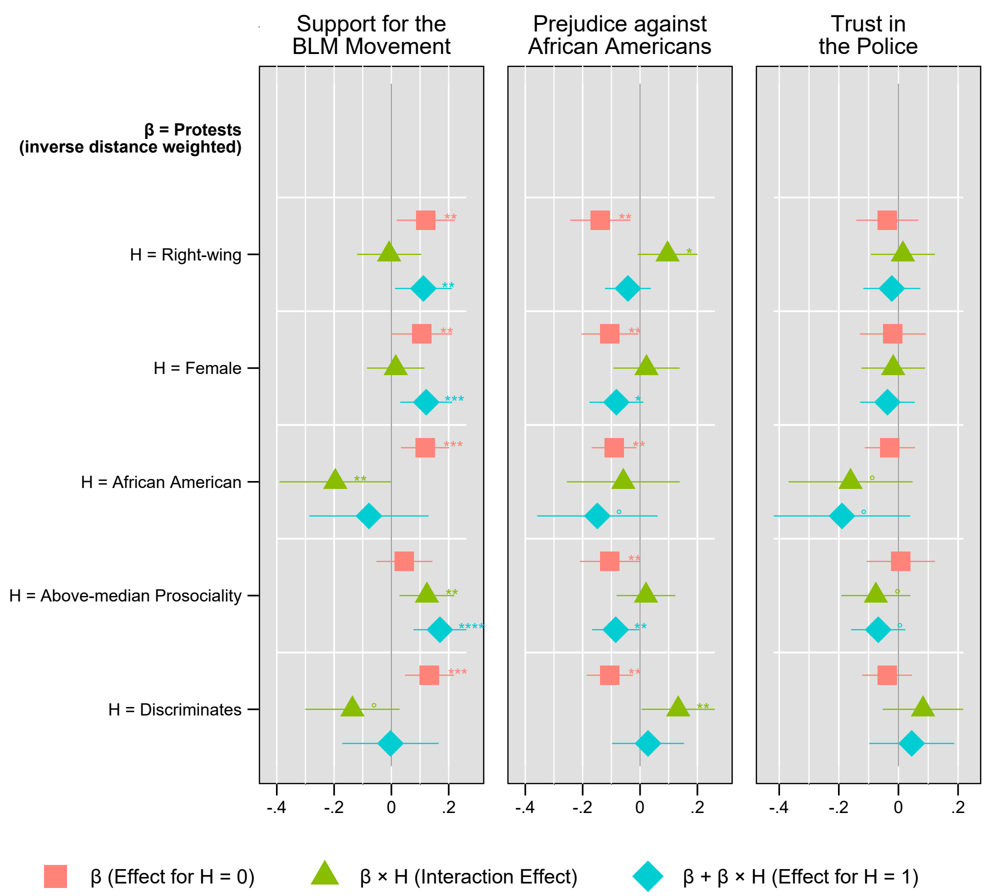
[34]The results are virtually equivalent to using a dummy indicating a win for Trump in the 2020 general election at the county level.

Figure 4.5: Main Results: BLM Protests



Notes: The figure shows standardized coefficients and 95 percent confidence intervals from OLS regressions. Dependent and independent variables standardized except for the dummy variables. The explanatory variables (based on transformations of the quantities listed on the y-axis) are (i) the natural logarithm of one plus the sum of inverse distances of BLM protests (in the time frame) to the respondent's zip code's centroid, (ii) the natural logarithm of one plus the number of protests in the county where the respondent resides, (iii) the natural logarithm of one plus the number of protests within a radius of 50 kilometers around the respondent's location, and (iv) an indicator that is equal to one for the respondent living in a county with an above-median (relative to the sample median) natural logarithm of one plus the number of protests in the respective county (all variables based on the respondent-specific time frames). The time frame used to construct the protest variables is between the respondent-specific survey date and 14 days before. Only protests during the respondent-specific time frames contribute to the variables. Standard errors robust (clustered at the county-level) in Panel A (B). (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively. Specification A contains the complete set of socio-demographics (including race dummies), a linear time trend since the death of George Floyd, the natural logarithm of one plus the number of COVID-19 cases per 100k inhabitants at the county level, the COVID-19 exposure dummy, and the respective BLM protest variable. Specification B adds controls for the share of residents below 18 and above 65, the share of female residents, the share of African American residents, the index for local racial gaps, and a dummy that is one if the Republican party won the 2016 General Election in the respondents' county. Specification C adds the political orientation dummies.

Figure 4.6: Heterogeneity Analysis: BLM Protests



Notes: The figure shows (sums of) standardized coefficients and 95 percent confidence intervals from OLS regressions to explore the heterogeneity of coefficients with respect to selected characteristics of the respondents. The heterogeneity analysis with respect to right-wing politicial orientation, female sex, African American ethnicitiy, above-median prosociality, and discrimination in one of the interethnic trust games contains the same control variables as the model specification "C" of the respective main results figure. The explanatory variable for BLM protest intensity is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the survey date and 14 days before to the respondent's location. Separate regressions for each indicator variable H (heterogeneity category). The underlying regression tables are provided in the Appendix.

($\beta = 0.17$, $p < 0.001$). Similarly, the effect is statistically significant among respondents who do not discriminate in the interethnic TGs ($\beta = 0.13$, $p = 0.002$), whereas it is indistinguishable from zero for the discriminating respondents ($\beta = -0.00$, $p = 0.971$). The heterogeneity, however, reaches only marginal statistical significance ($\beta = -0.14$, $p = 0.105$, for the interaction BLM $\times$ H).

There is no statistically significant heterogeneity concerning above-median prosociality for the effect of BLM protests on racial prejudice ($\beta = 0.02$, $p = 0.690$, for the interaction BLM $\times$ H) and only marginal statistical significance concerning the protests' effect on trust in the police ($\beta = -0.08$, $p = 0.199$, for the interaction BLM $\times$ H). However, the effect of BLM protests on racial prejudice is entirely driven by non-discriminating respondents ($\beta = -0.11$, $p = 0.011$), with the heterogeneity relative to discriminating respondents being statistically significant ($\beta = 0.13$, $p = 0.041$, for the interaction BLM $\times$ H). Discrimination against African Americans does not show significant heterogeneity regarding the impact of protests on trust in the police ($\beta = 0.08$, $p = 0.231$, for the interaction BLM $\times$ H).

Further heterogeneity dimensions. — Figure 4.6 also shows that the effect of BLM protests on the support for the movement's goals is driven by the non-African American respondents ($\beta = 0.12$, $p = 0.006$). For African American respondents, the effect is significantly smaller than for the other ethnic groups ($\beta = -0.20$, $p = 0.049$, for the interaction BLM $\times$ H) and not statistically significant ($\beta = -0.08$, $p = 0.461$). For the prejudice outcome, there is no statistically significant heterogeneity with respect to African American ethnicity ($\beta = -0.06$, $p = 0.561$, for the interaction BLM $\times$ H). However, the negative impact of protests on trust in the police is slightly stronger among African Americans ($\beta = -0.19$, $p = 0.105$), with the difference relative to other ethnicities reaching marginal statistical significance ($\beta = -0.16$, $p = 0.130$, for the interaction BLM $\times$ H). Finally, males and females were not differently impacted by the experience of protests as there is no statistically significant heterogeneity along the gender dimension for the three outcome variables.

Robustness checks. — Supplementary analyses in the Online Appendix show that the results are qualitatively equivalent using different time frames (going back five, seven, or ten days from the survey's date) and additional measures of protest intensity, as can be seen in Tables 4.17 and 4.13. In Section 4.B.4 of the Online Appendix, I use weather data on local rainfall and temperature around the survey date combined with election results at the county level as instruments for the contemporaneous intensity of BLM protests. These analyses controlling for a comprehensive set of variables at the individual (e.g., political orientation) and local levels (e.g., socio-demographic characteristics of the local population) replicate the main results from the OLS regressions of protests' impact on attitudes.

4.4 Concluding remarks

This paper reports findings based on data from two large-scale online experiments run on representative samples of the U.S. population during the summers of 2017 and 2020. The results demonstrate that support for one of the main policy goals of the BLM movement, i.e., that African Americans should be treated with equal respect, and racial prejudice toward African Americans correlate with behavior in incentivized experimental games. More substantial explicit racial prejudice correlates strongly with discrimination against African Americans versus white Americans in interethnic trust games. On the contrary, the importance attached to equal treatment of African Americans by the police is first and foremost related to "general" prosociality that need not be linked explicitly to the receiver's race. Discrimination, unlike in the case of racial prejudice, is not predictive of the support for the BLM movement in the general sample. Nevertheless, prejudice toward African Americans does not correlate coherently with experimental prosociality in both waves. The correlation is weakly negative during the first and slightly positive during the second wave, with marginal statistical significance in both cases.

The heterogeneity analysis further shows that prosociality correlates slightly negatively with prejudice against African Americans among non-right-wing respondents and positively for right-wing respondents. A possible explanation for this puzzling finding is that the prosociality index among right-wing respondents could (partially) measure their desire to defend the privileged position (Wildman, 1996) of white people in significant parts of U.S. society. This interpretation appears plausible because prosociality in the games is measured without mentioning other players' ethnicities but by describing them as U.S. citizens. Hence, right-wing respondents' might have expressed prosociality toward the white majority through their choices if they interpret U.S. citizens as primarily referring to white Americans.[35] In addition, discrimination against African Americans in the experiments is positively associated with racial prejudice among right-wing and non-right-wing respondents. Finally, discrimination against African Americans is negatively associated with support for the BLM movement's primary goal among non-right-wing respondents but positively among right-wing respondents.

Moreover, the vast attitudinal gaps between left-wing and right-wing respondents are only negligibly mediated by experimentally-measured prosociality and the propensity to discriminate in the incentivized games. Hence, these findings show that social preferences and ideology more or less independently affect racial attitudes. Overall, the results underline that racial attitudes in the U.S. are multi-faceted and cannot be

[35]Far-right respondents tend to lean toward radical ideologies, e.g., white supremacism, racism, and extreme conservatism (Berlet and Lyons, 2000; Earle and Hodson, 2022) and may even view only white Americans as U.S. citizens in the first place.

explained by political orientation, discrimination, or prosociality alone, complementing a quickly growing literature on racial and ethnic issues and their relationship to (social) preferences and political polarization (Haaland and Roth, 2021; Alesina et al., 2021a).

The second contribution originates from analyzing the impact of protests associated with the BLM movement following the death of George Floyd. The number and proximity of contemporaneous BLM protests are positively related to the support for the movement's goal of equal treatment of African Americans by the police and weaker prejudice against them. To some extent, a more significant number of contemporary protests is associated with less trust in the police, with a relatively more pronounced effect among African American respondents. However, the long-run stability of these "opinion-mobilizing" effects is unclear as results from Alesina et al. (2021a) suggest that effects were somewhat temporary and waned the longer the time since George Floyd's death (also see Reny and Newman (2021)). In addition, the results on racial attitudes, i.e., support for the BLM movement's main goal and prejudice against African Americans, and on trust in the police presented in this paper rest on correlations. As such, their causal interpretation should be taken cautiously, although the regressions control for a diverse set of potentially confounding variables in two representative samples of the U.S. population. A supplemental analysis in the Appendix (see Section 4.B.4), leveraging an instrumental variables approach, further suggests a causal interpretation of the protests' effects on the outcome variables being plausible. Furthermore, the results regarding protests' impact on public opinion are consistent with the logic of disruptive action (Sharp, 2013; Shuman et al., 2021), according to which the experience of local protests can affect public opinion and increase the support for the protesters' goals. Strikingly, the combination with experimentally measured prosociality reveals that the relatively more-prosocial respondents primarily drive the effect of protests on the support for the BLM movement. Similarly, the effect is more substantial among those respondents who do not discriminate against African Americans in the experimental games and who also drive the correlation of more substantial exposure to protests with weaker prejudice.

Finally, the impact of BLM protests on support for the BLM movement's primary goal found in this study is relatively homogenous across groups of political orientation. However, the protests' prejudice-decreasing impact seems absent among right-wing respondents. Thus, the results in this paper indicate that BLM protests contributed to a further deepening of the substantial political polarization (Iyengar et al., 2019; Alesina et al., 2021a; Haaland and Roth, 2021; Druckman et al., 2022), especially on racial issues and prejudice in the U.S., but that it fostered support for one of the movement's primary goals above party lines.

References

Abatzoglou, John T. 2013. "Development of gridded surface meteorological data for ecological applications and modelling." *International Journal of Climatology*, 33 (1): 121–131.

ACLED. 2021. "Armed Conflict Location & Event Data Project (ACLED): Bringing clarity to crisis." `https://acleddata.com`.

Alesina, Alberto, Reza Baqir, and William Easterly. 1999. "Public Goods and Ethnic Divisions." *The Quarterly Journal of Economics*, 114 (4): 1243–1284.

Alesina, Alberto, Matteo Ferroni, and Stefanie Stantcheva. 2021a. "Perceptions of racial gaps, their causes, and ways to reduce them." *NBER Working Paper*, 29245.

Alesina, Alberto and Eliana La Ferrara. 2014. "A Test of Racial Bias in Capital Sentencing." *American Economic Review*, 104 (11): 3397–3433.

Alesina, Alberto and Stefanie Stantcheva. 2020. "Diversity, Immigration, and Redistribution." *AEA Papers and Proceedings*, 110 (0): 329–334.

Bartoš, Vojtěch, Michal Bauer, Jana Cahlíková, and Julie Chytilová. 2021. "Covid-19 crisis and hostility against foreigners." *European Economic Review*, 137 (3): 103818.

Bauer, Michal, Christopher Blattman, Julie Chytilová, Joseph Henrich, Edward Miguel, and Tamar Mitts. 2016. "Can War Foster Cooperation?" *Journal of Economic Perspectives*, 30 (3): 249–274.

Baum, Christopher F., Mark E. Schaffer, and Steven Stillman. 2002. "IVREG2: Stata module for extended instrumental variables/2SLS and GMM estimation." `https://ideas.repec.org/c/boc/bocode/s425401.html`.

Bayer, Patrick, Marcus Casey, Fernando Ferreira, and Robert McMillan. 2017. "Racial and ethnic price differentials in the housing market." *Journal of Urban Economics*, 102 (1): 91–105.

Becchetti, Leonardo, Stefano Castriota, and Pierluigi Conzo. 2017. "Disaster, Aid, and Preferences: The Long-run Impact of the Tsunami on Giving in Sri Lanka." *World Development*, 94 (2): 157–173.

Berg, Joyce, John Dickhaut, and Kevin McCabe. 1995. "Trust, Reciprocity, and Social History." *Games and Economic Behavior*, 10 (1): 122–142.

Berlet, Chip and Matthew N. Lyons. 2000. *Right-wing populism in America: Too close for comfort.* Critical perspectives, New York, NY: Guilford Press.

Bertrand, Marianne and Sendhil Mullainathan. 2004. "Are Emily and Greg More Employable Than Lakisha and Jamal? A Field Experiment on Labor Market Discrimination." *American Economic Review*, 94 (4): 991–1013.

Birkand, Thomas A. 1998. "Focusing Events, Mobilization, and Agenda Setting." *Journal of Public Policy*, 18 (1): 53–74.

Cappelen, Alexander W., Knut Nygaard, Erik Ø. Sørensen, and Bertil Tungodden. 2015. "Social preferences in the lab: A comparison of students and a representative population." *The Scandinavian Journal of Economics*, 117 (4): 1306–1326.

Carlsson, Magnus and Stefan Eriksson. 2016. "Do attitudes expressed in surveys predict ethnic discrimination?" *Ethnic and Racial Studies*, 40 (10): 1739–1757.

Cassar, Alessandra, Andrew Healy, and Carl von Kessler. 2017. "Trust, Risk, and Time Preferences After a Natural Disaster: Experimental Evidence from Thailand." *World Development*, 94 (3): 90–105.

Cetre, Sophie, Yann Algan, Gianluca Grimalda, Fabrice Murtin, Louis Putterman, Ulrich Schmidt, and Vincent Siegerink. 2020. "Ethnic bias, economic success and trust: Findings from large sample experiments in Germany and the United States through the Trustlab platform." *OECD Statistics Working Papers*, 2020/04.

CNN. 2021. "US Black-White inequality in 4 charts." https://edition.cnn.com/2021/06/01/politics/black–white–racial–wealth–gap/index.html, `https://edition.cnn.com/2021/06/01/politics/black-white-racial-wealth-gap/index.html`.

Collins, William J. and Robert A. Margo. 2007. "The Economic Aftermath of the 1960s Riots in American Cities: Evidence from Property Values." *The Journal of Economic History*, 67 (4): 849–883.

Cunningham, George B. and Lisa T. Wigfall. 2020. "Race, explicit racial attitudes, implicit racial attitudes, and COVID-19 cases and deaths: An analysis of counties in the United States." *PloS one*, 15 (11): e0242044.

Dave, Dhaval M., Andrew I. Friedson, Kyutaro Matsuzawa, Joseph J. Sabia, and Samuel Safford. 2020. "Black Lives Matter Protests, Social Distancing, and COVID-19." *NBER Working Paper*, 27408.

Dionne, Kim Yi and Fulya Felicity Turkmen. 2020. "The Politics of Pandemic Othering: Putting COVID-19 in Global and Historical Context." *International Organization*, 74 (S1): E213–E230.

Donohue III, John J. and Steven D. Levitt. 2001. "The Impact of Race on Policing and Arrests." *The Journal of Law and Economics*, 44 (2): 367–394.

Druckman, James N., Samara Klar, Yanna Krupnikov, Matthew Levendusky, and John Barry Ryan. 2021a. "Affective polarization, local contexts and public opinion in America." *Nature Human Behaviour*, 5 (1): 28–38.

Druckman, James N., Samara Klar, Yanna Krupnikov, Matthew Levendusky, and John Barry Ryan. 2022. "(Mis)estimating affective polarization." *The Journal of Politics*, 84 (2): 1106–1117.

Dunivin, Zackary O., Harry Yaojun Yan, Jelani Ince, and Fabio Rojas. 2022. "Black Lives Matter protests shift public discourse." *Proceedings of the National Academy of Sciences of the United States of America*, 119 (10): e2117320119.

Earle, Megan and Gordon Hodson. 2022. "Dealing with declining dominance: White identification and anti-immigrant hostility in the US." *Group Processes & Intergroup Relations*, 25 (3): 727–745.

Eckel, Catherine C. and Philip J. Grossman. 2002. "Sex differences and statistical stereotyping in attitudes toward financial risk." *Evolution and Human Behavior*, 23 (4): 281–295.

Edelman, Benjamin, Michael Luca, and Dan Svirsky. 2017. "Racial Discrimination in the Sharing Economy: Evidence from a Field Experiment." *American Economic Journal: Applied Economics*, 9 (2): 1–22.

Elias, Amanuel, Jehonathan Ben, Fethi Mansouri, and Yin Paradies. 2021. "Racism and nationalism during and beyond the COVID-19 pandemic." *Ethnic and Racial Studies*, 44 (5): 783–793.

Endocrinology, The Lancet Diabetes &. 2020. "COVID-19 and Racism—a double edged dagger." *The Lancet Diabetes & Endocrinology*, 8 (8): 649.

Enos, Ryan D., Aaron R. Kaufman, and Melissa L. Sands. 2019. "Can Violent Protest Change Local Policy Support? Evidence from the Aftermath of the 1992 Los Angeles Riot." *American Political Science Review*, 113 (4): 1012–1028.

Feinberg, Matthew, Robb Willer, and Chloe Kovacheff. 2020. "The activist's dilemma: Extreme protest actions reduce popular support for social movements." *Journal of Personality and Social Psychology*, 119 (5): 1086–1111.

Fong, Christina M. and Erzo F.P. Luttmer. 2009. "What Determines Giving to Hurricane Katrina Victims? Experimental Evidence on Racial Group Loyalty." *American Economic Journal: Applied Economics*, 1 (2): 64–87.

Fong, Christina M. and Erzo F.P. Luttmer. 2011. "Do fairness and race matter in generosity? Evidence from a nationally representative charity experiment." *Journal of Public Economics*, 95 (5-6): 372–394.

Forsythe, Robert, Joel L. Horowitz, N. E. Savin, and Martin Sefton. 1994. "Fairness in Simple Bargaining Experiments." *Games and Economic Behavior*, 6 (3): 347–369.

Francis, Megan Ming and Leah Wright-Rigueur. 2021. "Black Lives Matter in Historical Perspective." *Annual Review of Law and Social Science*, 17 (1): 441–458.

GBD 2019 Police Violence US Subnational Collaborators. 2021. "Fatal police violence by race and state in the USA, 1980–2019: A network meta-regression." *The Lancet*, 398 (10307): 1239–1255.

Gilens, Martin. 1999. *Why Americans hate welfare: Race, media, and the politics of antipoverty policy*. Studies in communication, media, and public opinion, Chicago: University of Chicago Press.

Giuliano, Paola and A. Spilimbergo. 2014. "Growing up in a Recession." *The Review of Economic Studies*, 81 (2): 787–817.

Grimalda, Gianluca, Fabrice Murtin, David Pipke, Louis Putterman, and Matthias Sutter. 2022b. "The Politicized Pandemic: Ideological Polarization and the Behavioral Response to COVID-19." *MPI Collective Goods Discussion Paper*, 2022/1.

Haaland, Ingar and Christopher Roth. 2021. "Beliefs about Racial Discrimination and Support for Pro-Black Policies." *Review of Economics and Statistics*, Just Accepted MS (0): 1–38.

Healy, Kieran. 2020. "covdata: COVID-19 case and mortality time series." `http://kjhealy.github.io/covdata`.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J. Westwood. 2019. "The origins and consequences of affective polarization in the United States." *Annual Review of Political Science*, 22 (1): 129–146.

Johnson, Mike, Max Joseph, and Eric R. Scott. 2021. "mikejohnson51/climateR."

Ketchley, Neil and Thoraya El-Rayyes. 2021. "Unpopular Protest: Mass Mobilization and Attitudes to Democracy in Post-Mubarak Egypt." *The Journal of Politics*, 83 (1): 291–305.

Kingdon, John W. 1995. *Agendas, alternatives, and public policies.* New York, NY: Longman.

Kishi, Roudabeh, Stall, Hampton, Wolfson, Aaron, and Sam Jones. 2021. "ACLED - A Year of Racial Justice Protests: Key Trends in Demonstrations Supporting the BLM Movement." 1–20, `https://acleddata.com/`.

Lang, Kevin and Michael Manove. 2011. "Education and Labor Market Discrimination." *American Economic Review*, 101 (4): 1467–1496.

Lang, Kevin and Ariella Kahn-Lang Spitzer. 2020. "Race Discrimination: An Economic Perspective." *Journal of Economic Perspectives*, 34 (2): 68–89.

Lee, Taeku. 2002. *Mobilizing public opinion: Black insurgency and racial attitudes in the civil rights era*. Studies in communication, media, and public opinion, Chicago, Ill.: Univ. of Chicago Press.

Levendusky, Matthew S. 2018. "Americans, not partisans: Can priming American national identity reduce affective polarization? l." *The Journal of Politics*, 80 (1): 59–70.

Lu, Runjing and Sophie Yanying Sheng. 2022. "How racial animus forms and spreads: Evidence from the coronavirus pandemic: Evidence from the Coronavirus Pandemic." *Journal of Economic Behavior & Organization*, 200 (0): 82–98.

Luttmer, Erzo F.P. 2001. "Group Loyalty and the Taste for Redistribution." *Journal of Political Economy*, 109 (3): 500–528.

Madestam, Andreas, Daniel Shoag, Stan Veuger, and David Yanagizawa-Drott. 2013. "Do Political Protests Matter? Evidence from the Tea Party Movement." *The Quarterly Journal of Economics*, 128 (4): 1633–1685.

Malmendier, Ulrike and Stefan Nagel. 2011. "Depression Babies: Do Macroeconomic Experiences Affect Risk Taking?" *The Quarterly Journal of Economics*, 126 (1): 373–416.

Mazumder, Soumyajit. 2018. "The Persistent Effect of U.S. Civil Rights Protests on Political Attitudes." *American Journal of Political Science*, 62 (4): 922–935.

Mazumder, Soumyajit. 2019. "Black Lives Matter for Whites' Racial Prejudice: Assessing the Role of Social Movements in Shaping Racial Attitudes in the United States." *Preprint*.

McGovern, Tony, Stephen Larson, Bill Morris, and Matt Hodges. 2020. "United States General Election presidential results by county from 2008 to 2020." `https://github.com/tonmcg/US_County_Level_Election_Results_08-20`.

Metcalf, Leigh and William Casey. 2016b. "Introduction to data analysis." In *Cybersecurity and Applied Mathematics*, edited by Metcalf, Leigh and William Casey 43–65, Saint Louis: Elsevier Science.

Mollerstrom, Johanna, Avner Strulov-Shlain, and Dmitry Taubinsky. 2021. "Preferences for Giving Versus Preferences for Redistribution." *NBER Working Paper*, 29375.

Murtin, Fabrice, Lara Fleischer, Vincent Siegerink, Arnstein Aassve, Yann Algan, Romina Boarini, Santiago González, Zsuzsanna Lonti, Gianluca Grimalda, Rafael Hortala Vallve, Soonhe Kim, David Lee, Louis Putterman, and Conal Smith. 2018. "Trust and its determinants." *OECD Statistics Working Papers*, 2018/02.

OECD. 2019b. "Trustlab." `http://www.oecd.org/sdd/trustlab.htm`.

Opportunity Insights. 2021. "Data Library: Publicly available data." `https://opportunityinsights.org/`.

Padgham, Mark, Michael D. Sumner, and Charles F. F. Karney. 2021. "Package 'geodist'." `https://github.com/hypertidy/geodist`.

Peyton, Kyle and Gregory A. Huber. 2021. "Racial Resentment, Prejudice, and Discrimination." *The Journal of Politics*, 83 (4): 1829–1836.

Raleigh, Clionadh, Andrew Linke, Håvard Hegre, and Joakim Karlsen. 2010. "Introducing ACLED: An Armed Conflict Location and Event Dataset." *Journal of Peace Research*, 47 (5): 651–660.

Reny, Tyler T. and Matt A. Barreto. 2020. "Xenophobia in the time of pandemic: Othering, anti-Asian attitudes, and COVID-19." *Politics, Groups, and Identities*, 42 (2): 1–24.

Reny, Tyler T. and Benjamin J. Newman. 2021. "The Opinion-Mobilizing Effect of Social Protest against Police Violence: Evidence from the 2020 George Floyd Protests." *American Political Science Review*, 115 (4): 1499–1507.

Rodgers, William M. ed. 2006. *Handbook on the economics of discrimination*. Elgar original reference, Cheltenham, U.K and Northampton, Mass: Edward Elgar.

Rozzi, Gavin C. 2021. "zipcodeR: Advancing the analysis of spatial data at the ZIP code level in R." *Software Impacts*, 9 (0): 100099.

Sawyer, Jeremy and Anup Gampa. 2018. "Implicit and Explicit Racial Attitudes Changed During Black Lives Matter." *Personality & Social Psychology Bulletin*, 44 (7): 1039–1059.

Sharp, Gene. 2013. *How nonviolent struggle works*. East Boston, MA: Albert Einstein Institution.

Shuman, Eric, Siwar Hasan-Aslih, Martijn van Zomeren, Tamar Saguy, and Eran Halperin. 2022. "Protest movements involving limited violence can sometimes be effective: Evidence from the 2020 BlackLivesMatter protests." *Proceedings of the National Academy of Sciences of the United States of America*, 119 (14): e2118990119.

Shuman, Eric, Tamar Saguy, Martijn van Zomeren, and Eran Halperin. 2021. "Disrupting the system constructively: Testing the effectiveness of nonnormative nonviolent collective action." *Journal of Personality and Social Psychology*, 121 (4): 819–841.

Solnit, Rebecca. 2009. *A paradise built in hell: The extraordinary communities that arise in disaster*. New York, NY: Viking.

Stepanikova, Irena, Jennifer Triplett, and Brent Simpson. 2011. "Implicit racial bias and prosocial behavior." *Social Science Research*, 40 (4): 1186–1195.

Tabellini, Marco. 2020. "Gifts of the Immigrants, Woes of the Natives: Lessons from the Age of Mass Migration." *The Review of Economic Studies*, 87 (1): 454–486.

Tahmasbi, Fatemeh, Leonard Schild, Chen Ling, Jeremy Blackburn, Gianluca Stringhini, Yang Zhang, and Savvas Zannettou. 2021. " "Go eat a bat, Chang!": On the Emergence of Sinophobic Behavior on Web Communities in the Face of COVID-19." In *Proceedings of the Web Conference 2021*, edited by Leskovec, Jure, Marko Grobelnik, Marc Najork, Jie Tang, and Leila Zia 1122–1133, New York, NY, USA: ACM.

Teeselink, Bouke Klein and Georgios Melios. 2021. "Weather to Protest: The Effect of Black Lives Matter Protests on the 2020 Presidential Election." *Working Paper*.

The New York Times. 2021. "Coronavirus in the U.S. Latest Map and Case Count." `https://www.nytimes.com/interactive/2021/us/covid-cas`.

Triplett, Jennifer. 2012. "Racial Bias and Prosocial Behavior." *Sociology Compass*, 6 (1): 86–96.

UDS Mapper. 2021. "ZIP CODE TO ZCTA CROSSWALK." `https://udsmapper.org/zip-code-to-zcta-crosswalk/`.

Ungar, Sheldon. 1998. "Hot Crises and Media Reassurance: A Comparison of Emerging Diseases and Ebola Zaire." *The British Journal of Sociology*, 49 (1): 36–56.

United States Census Bureau. 2020. "American Community Survey 5-Year Data: 2015-2019 ACS 5-Year estimates." `https://www.census.gov/data/developers/data-sets/acs-5year.html`.

United States Census Bureau. 2021. "County population totals: 2010-2020." `https://www2.census.gov/programs-surveys/popest/datasets/`.

Walker, Kyle, Matt Herman, and Kris Eberwein. 2021. "tidycensus: R Package 'tidycensus'." `https://cloud.r-project.org/web/packages/tidycensus/index.html`.

Wallace, Sophia J., Chris Zepeda-Millán, and Michael Jones-Correa. 2014. "Spatial and Temporal Proximity: Examining the Effects of Protests on Political Attitudes." *American Journal of Political Science*, 58 (2): 433–448.

Washer, Peter. 2004. "Representations of SARS in the British newspapers." *Social Science & Medicine (1982)*, 59 (12): 2561–2571.

Wasow, Omar. 2020. "Agenda Seeding: How 1960s Black Protests Moved Elites, Public Opinion and Voting." *American Political Science Review*, 114 (3): 638–659.

White, Alexandre I. R. 2020. "Historical linkages: Epidemic threat, economic risk, and xenophobia." *The Lancet*, 395 (10232): 1250–1251.

Wikipedia. 2021. "List of current United States governors." https://en.wikipedia.org/wiki/List_of_current_United_States_governors.

Wildman, Stephanie M. 1996. *Privilege Revealed: How Invisible Preference Undermines America.* Critical America, New York: NYU Press.

Appendices

4.A Appendix

4.A.1 Descriptives

Table 4.1: Sample Characteristics

	Wave I		Wave II	
	Sample mean	Population mean	Sample mean	Population mean
Targeted characteristics				
Female	0.51	0.52	0.55	0.52
Age	45.39		47.96	
Age 18-20	0.04	0.05	0.04	0.05
Age 21-44	0.41	0.41	0.40	0.41
Age 45 and above	0.56	0.54	0.56	0.54
Bottom income	0.23	0.20	0.23	0.20
Medium income	0.61	0.60	0.62	0.60
Top income	0.16	0.20	0.15	0.20
Non-targeted characteristics				
White	0.71	0.60	0.74	0.60
African-American	0.11	0.13	0.11	0.13
Hispanic	0.11	0.16	0.09	0.16
Asian American	0.04	0.06	0.03	0.06
High school or less	0.20	0.40	0.17	0.38
Some college	0.38	0.29	0.33	0.28
Tertiary diploma	0.42	0.31	0.50	0.35
Employed	0.55	0.56	0.49	0.53
Self-employed	0.08	0.04	0.09	0.04
Unemployed	0.12	0.03	0.16	0.05
Out of the labor force	0.25	0.37	0.26	0.38

Notes: This table displays means (unless stated otherwise) of sample characteristics for both waves of the Trustlab and the respective population means taken from representative sources. Except "Age", all variables are binary. Bottom (top) income refers to the bottom (top) 20 percent. Sources: Labor force statistics from the U.S. Bureau of Labor Statistics (https://www.bls.gov/cps/cpsaat01.htm). Age and gender statistics from the CIA World Factbook (2017 and 2020 est.) (https://www.cia.gov/the-world-factbook/countries/united-states/) and the U.S. Census Bureau (data from 2017 and 2019) (https://www.census.gov/data/tables/2019/demo/age-and-sex/2019-age-sex-composition.html). Population shares of age groups adjusted to the population aged 15 years and above. Race and ethnicity statistics (2016 est.) from the U.S. Census Bureau (https://www.census.gov/quickfacts/fact/table/US/PST045219, Population estimates from 2019).

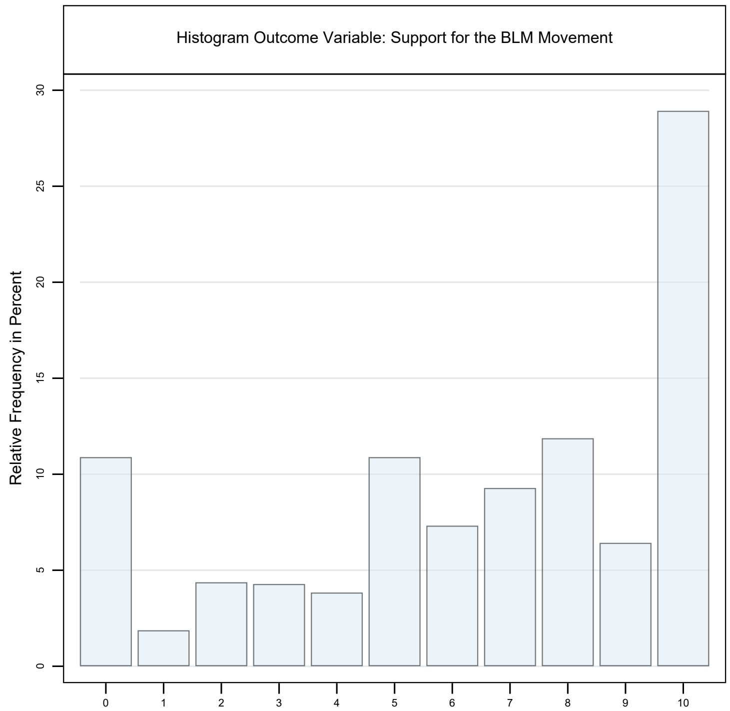
Table 4.2: Outcome Variables by Characteristics

	Right-wing		Left-wing		
	M	SD	M	SD	p-value
Support BLM Movement	5.487	3.395	8.640	2.356	0.000
Racial Prejudice	7.182	2.308	2.749	2.622	0.000
Trust in Police	7.219	2.216	4.601	2.676	0.000
	African-American		White		
	M	SD	M	SD	p-value
Support BLM Movement	8.296	2.492	6.218	3.352	0.000
Racial Prejudice	4.195	2.971	5.683	2.975	0.000
Trust in Police	4.266	2.810	6.450	2.464	0.000
	Female		Male		
	M	SD	M	SD	p-value
Support BLM Movement	6.494	3.408	6.383	3.315	0.585
Racial Prejudice	5.146	3.049	5.872	2.933	0.000
Trust in Police	5.781	2.643	6.393	2.587	0.000
	Top Income		Bottom Income		
	M	SD	M	SD	p-value
Support BLM Movement	6.927	3.234	6.218	3.605	0.041
Racial Prejudice	5.631	3.046	5.050	3.022	0.009
Trust in Police	6.451	2.553	5.477	2.787	0.000
	High Educ.		Low Educ.		
	M	SD	M	SD	p-value
Support BLM Movement	6.756	3.244	5.850	3.440	0.001
Racial Prejudice	5.518	3.019	5.700	2.992	0.335
Trust in Police	6.363	2.464	5.906	2.761	0.002

Notes: The table shows means, standard deviations, and results (p-values) from a two-sided t-test against the null hypothesis of equal means by characteristic groups of the sample.

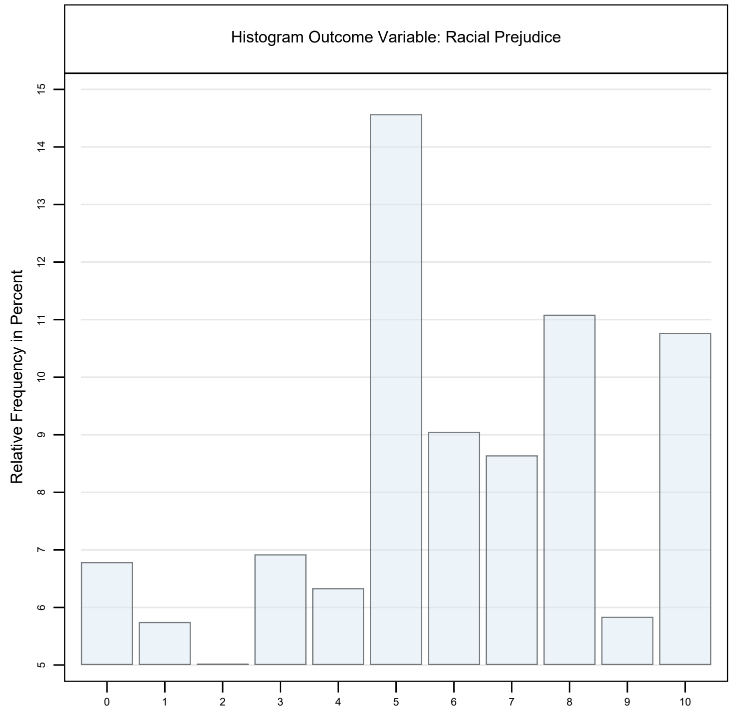
4.A.2 Additional Figures

Figure 4.7: Histogram: Support for the BLM Movement



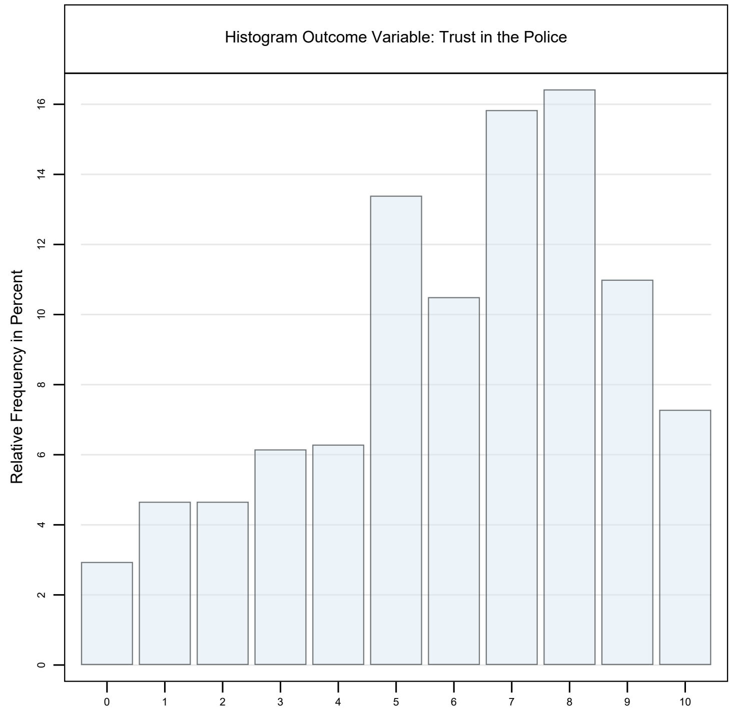
Notes: The figure shows a histogram of the answers to whether it should be an urgent priority to reform police departments so that African Americans are treated with equal respect and can feel trust in the police rather than fear.

Figure 4.8: Histogram: Racial Prejudice against African Americans



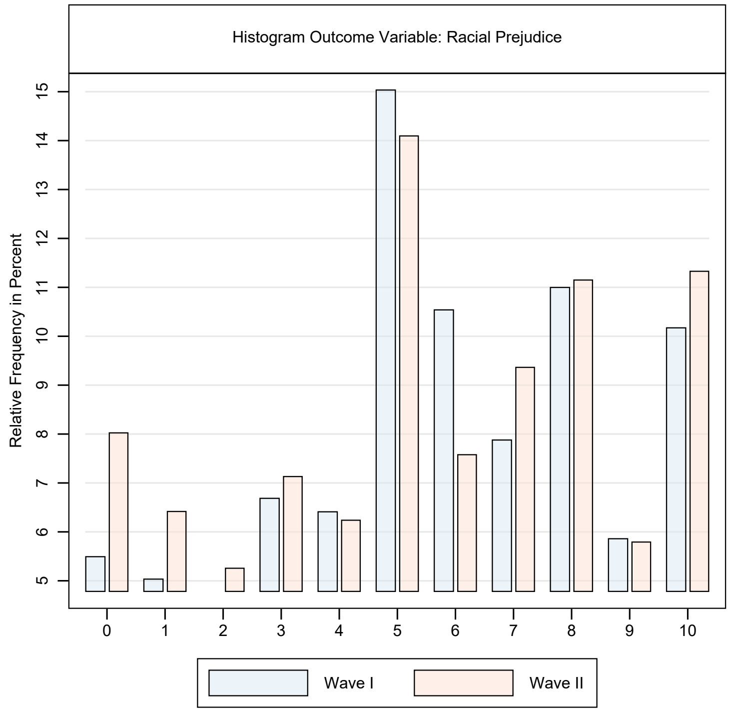
Notes: The figure shows a histogram of the answers to whether differences in economic outcomes between African Americans and white Americans are mainly discrimination and lack of opportunity (0), or mainly lesser ability, motivation and effort (10).

Figure 4.9: Histogram: Trust in the Police



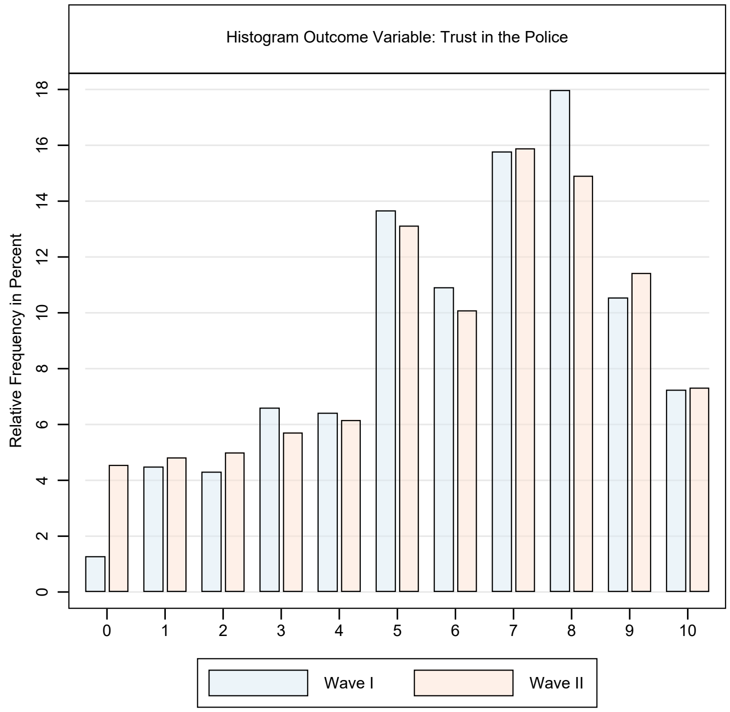
Notes: The figure shows a histogram of the level of trust in the police where 0 is "I do not trust them at all" and 10 is "I completely trust them".

Figure 4.10: Histogram: Racial Prejudice by Wave



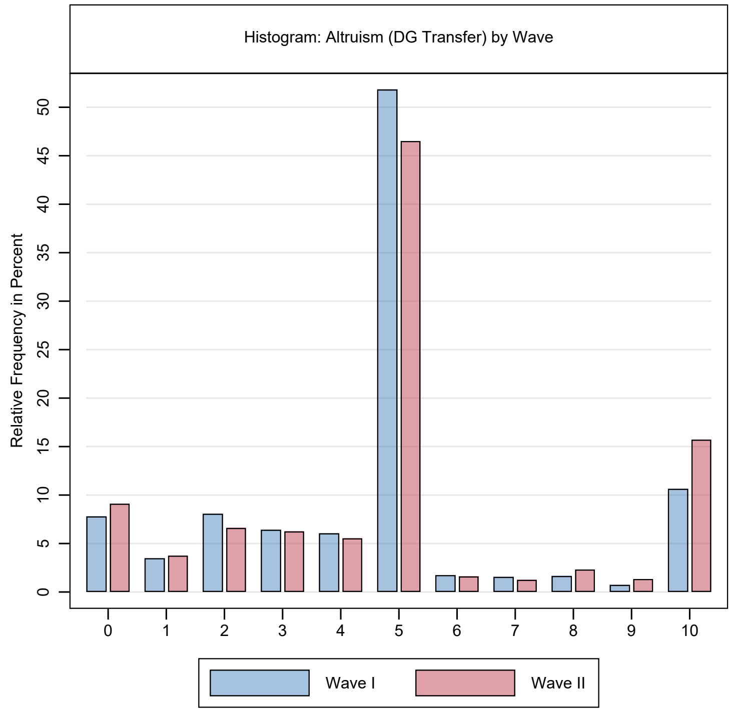
Notes: The figure shows a histogram of the answers to whether differences in economic outcomes between African Americans and white Americans are mainly discrimination and lack of opportunity (0), or mainly lesser ability, motivation and effort (10) for both waves of the U.S. Trustlab.

Figure 4.11: Histogram: Trust in the Police by Wave



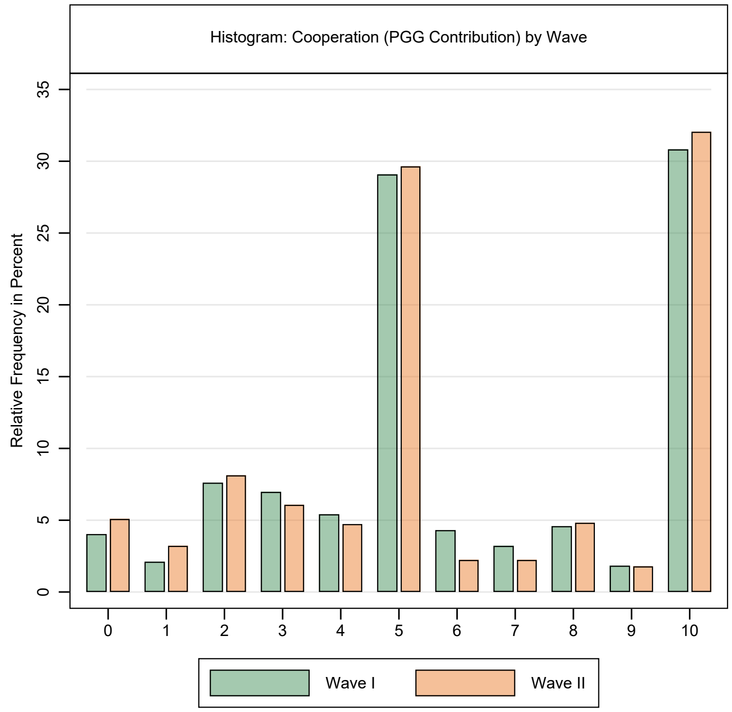
Notes: The figure shows a histogram of the answers of the level of trust in the police where 0 is "I do not trust them at all" and 10 is "I completely trust them" for both waves of the U.S. Trustlab.

Figure 4.12: Histogram: Altruism (Transfer in the DG) by Wave



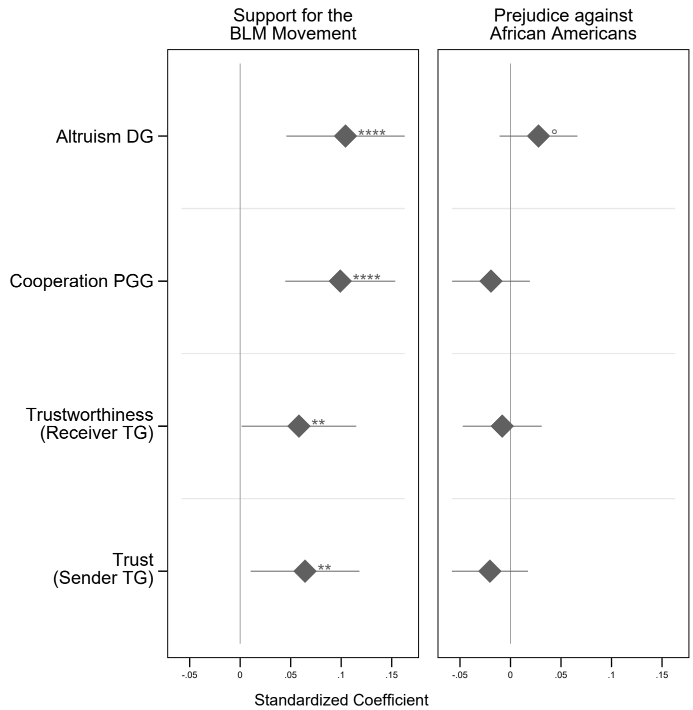
Notes: The figure shows a histogram of the transfers in the dictator game for both waves of the U.S. Trustlab.

Figure 4.13: Histogram: Cooperation (Contribution in the PGG) by Wave



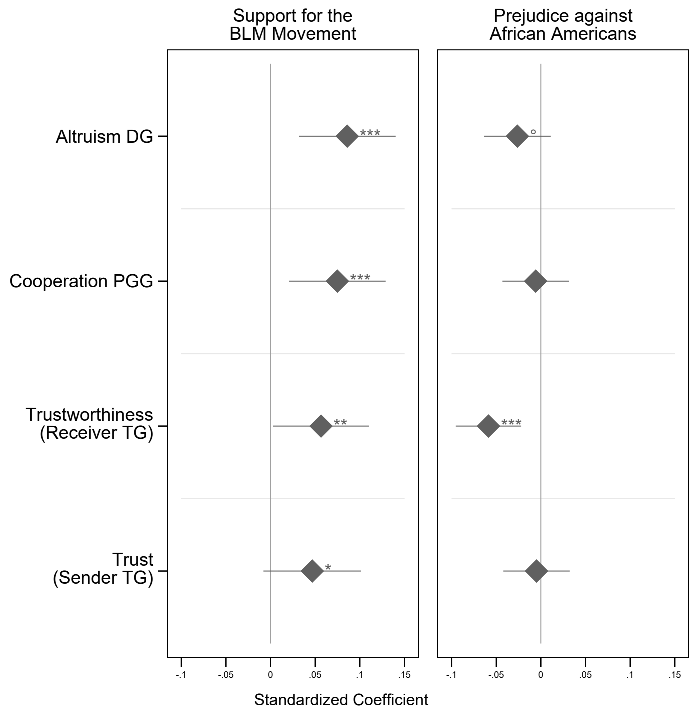
Notes: The figure shows a histogram of the contributions in the public goods game for both waves of the U.S. Trustlab.

Figure 4.14: Coefficients of Experimental Measures of Prosociality



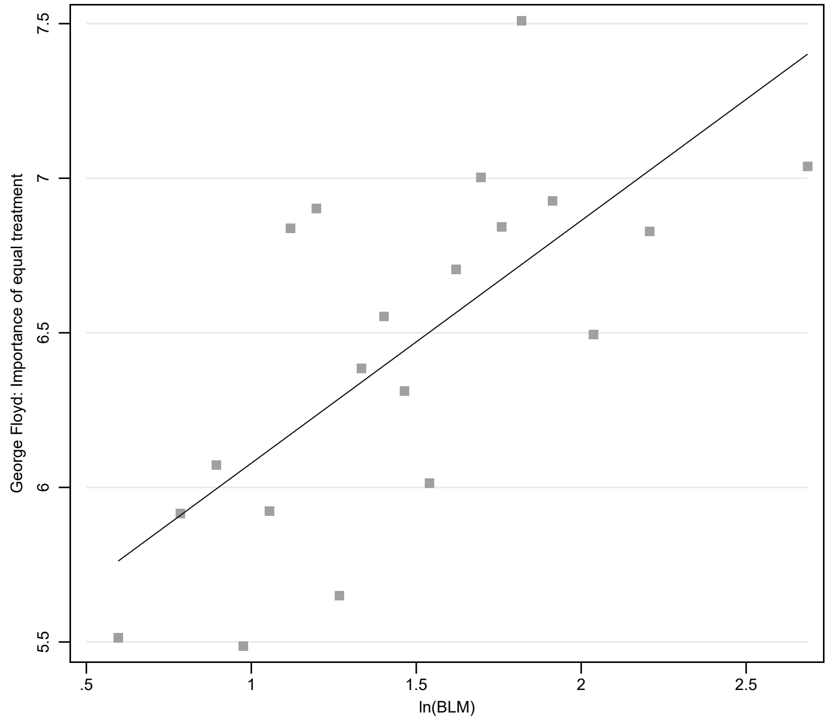
Notes: The figure shows standardized coefficients and 95 percent confidence intervals for the explanatory variables indicated on the y-axis from OLS regressions. Dependent and independent variables standardized. Robust standard errors. (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively. Each row shows the coefficient from separate OLS regressions. The variables on the y-axis replace the prosociality index in a specification otherwise identical to Specification C in Panel A from the main texts analysis. Altruism DG is the choice in the DG without ethnic information about the receiver. Cooperation PGG is the contribution to the common project in the PGG. Trustworthiness is the average over the amounts returned in the TG for each possible sender's choice (sending 0,1,..., or 10 USD) elicited by the strategy method. Trust is the amount sent in the standard TG without ethnic information about the receiver.

Figure 4.15: Coefficients of Experimental Measures of above-median Prosociality



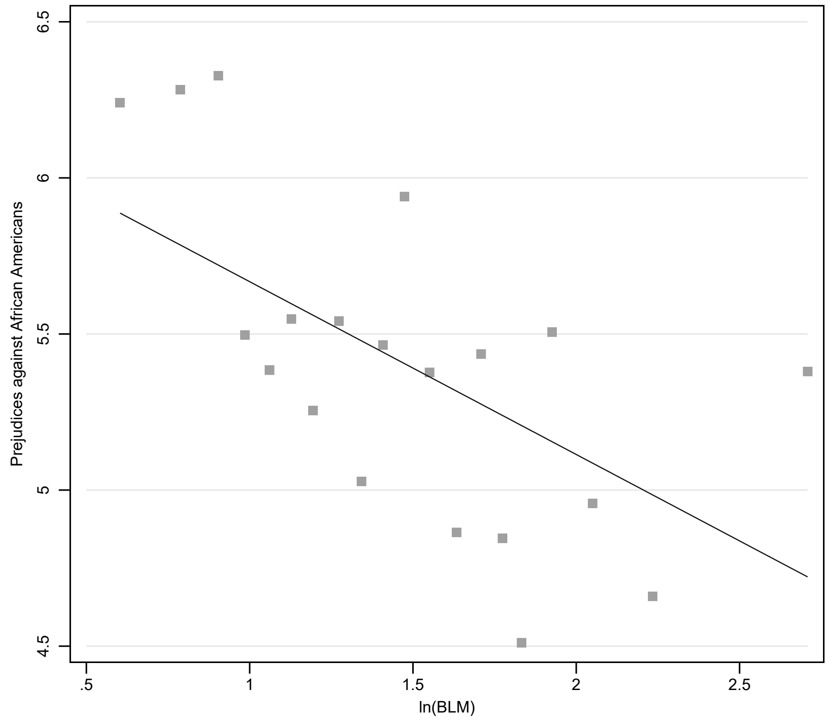
Notes: The figure shows standardized coefficients and 95 percent confidence intervals for the dummies indicating above median values of the explanatory variables indicated on the y-axis from OLS regressions. Dependent variables standardized. Robust standard errors. (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively. Each row shows the coefficient from separate OLS regressions. The variables on the y-axis replace the dummy indicating an above-median prosociality index in a specification otherwise identical to Specification C in Panel A from the main texts analysis. Altruism DG indicates an above-median the choice in the DG without ethnic information about the receiver. Cooperation PGG indicates an above-median contribution to the common project in the PGG. Trustworthiness indicates an above-median average over the amounts returned in the TG for each possible sender's choice (sending 0,1,..., or 10 USD) elicited by the strategy method. Trust indicates an above-median amount sent in the standard TG without ethnic information about the receiver.

Figure 4.16: Binned Scatterplot: Support for the BLM Movement



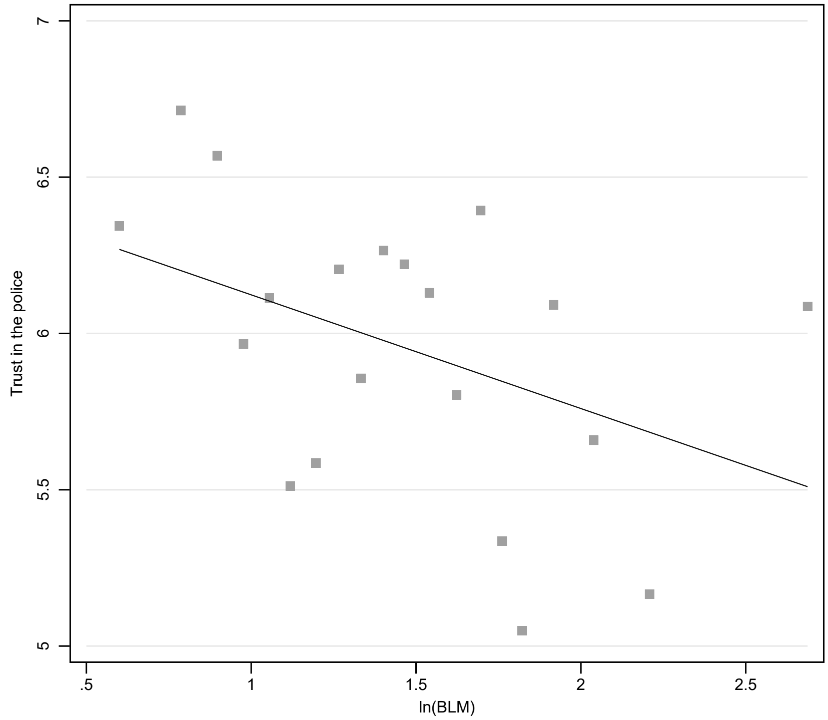
Notes: The figure shows a linear regression fit of the outcome variable (Support for the BLM movement) on the y-axis on the explanatory variable (the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the survey date and 14 days before to the respondent's location) on the x-axis. The regressions contain the full set of socio-demographic controls and the linear time trend since the death of George Floyd. Dots mark the means of the standardized outcome variable for each quantile of the standardized explanatory variable.

Figure 4.17: Binned Scatterplot: Racial Prejudice against African Americans



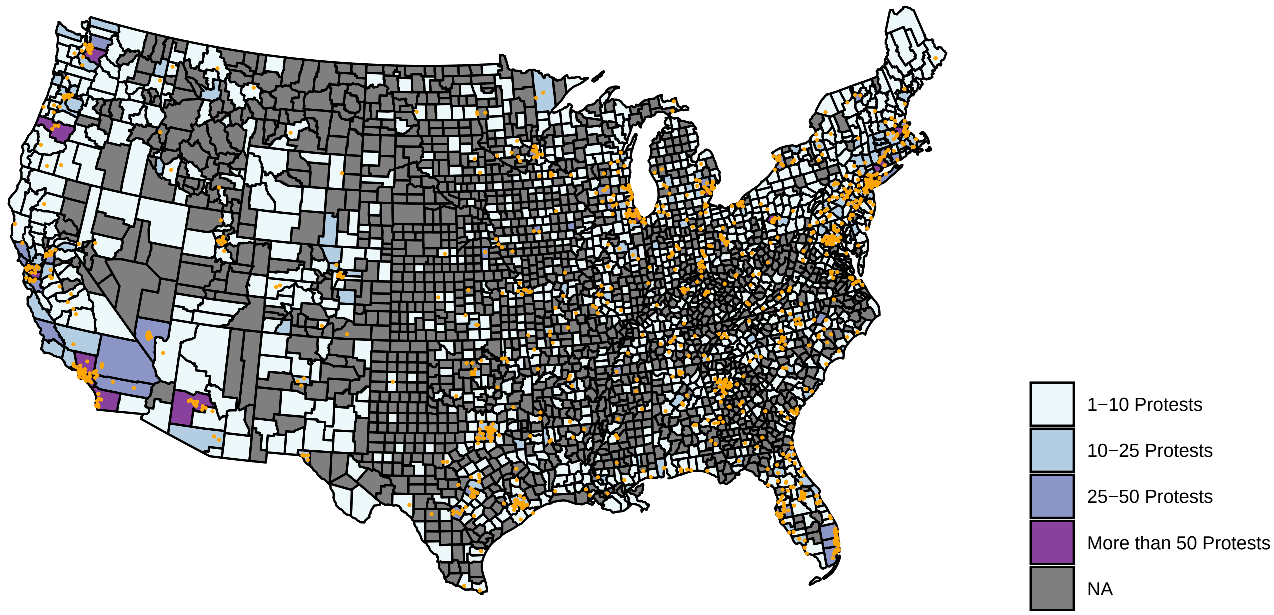
Notes: The figure shows a linear regression fit of the outcome variable (Racial prejudice against African Americans) on the y-axis on the explanatory variable (the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the survey date and 14 days before to the respondent's location) on the x-axis. The regressions contain the full set of socio-demographic controls and the linear time trend since the death of George Floyd. Dots mark the means of the standardized outcome variable for each quantile of the standardized explanatory variable.

Figure 4.18: Binned Scatterplot: Trust in the Police



Notes: The figure shows a linear regression fit of the outcome variable (Trust in the Police) on the y-axis on the explanatory variable (the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the survey date and 14 days before to the respondent's location) on the x-axis. The regressions contain the full set of socio-demographic controls and the linear time trend since the death of George Floyd. Dots mark the means of the standardized outcome variable for each quantile of the standardized explanatory variable.

Figure 4.19: Trustlab Participants (Wave 2) and Number of BLM Protests in U.S. Counties



Notes: This figure shows the places of residents of participants (orange dots) in the second wave of the U.S. Trustlab and the number of BLM protests (based on the ACLED dataset) between the 29th of May, 14 days before the Trustlab survey began, and the 7th of September when the Trustlab survey was completed.

4.B Online Appendix

Supplementary Online Material for

Black Lives Matter: Findings on protests, prosociality, discrimination, and racial attitudes from large-scale online experiments

David Pipke

4.B.1 Variable Definitions and Selected Questions from the Trustlab Survey

This section describes the variables used in the analyses that are directly derived from the Trustlab survey or its experimental modules.

- Demographic variables from the Trustlab:
  - Female: Gender dummy.
  - Age: Age in years.
  - Age squared: Age in years squared.
  - Bottom income: Income in the first quintile (bottom 20 percent) of the income distribution.
  - Top income: Income in the fifth quintile (top 20 percent) of the income distribution.
  - High school or less.: Educational attainment not higher than High school degree (base category is "Some college").
  - Tertiary diploma.: Educational attainment not higher than Tertiary diploma, e.g., Undergraduate or Postgraduate degree (base category is "Some college").
  - Town: Urbanization category dummy (base category is "Rural").
  - City: Urbanization category dummy (base category is "Rural").
  - Self-employed, Unemployed, Inactive: Labor force status dummies relative to the base category "Employed".
  - African American, Hispanic, Asian: Race/Ethnicity dummies relative to the base category "White".
  - Right-wing, Left-wing: Dummy variable indicating a score of 7 or above (3 or below) in the question "In political matters, people often talk of "the left" and "the right." How would you place your views on this scale, generally

speaking?" where respondents could place their views between "0 - Left" and "10 - Right".

  - Wave 2: Dummy for the second (2020) wave of the U.S. Trustlab.

- Variables based on behavior in the experiments:

  - Prosociality: Index based on the behavior in the dictator game and the public goods game. The index is the standardized average over both standardized (subtracted the mean and divided by the standard deviation) components.

  - Discriminates: Dummy variable indicating that respondent sent less to an African American receiver than to a white receiver in one of the interethnic trust games (with or without income information).

- Self-reported exposure to COVID-19

  - The indicator variable measuring exposure to COVID-19 is one if at least one of the following conditions is fulfilled. The conditions are that the respondent reports that (i) a household member, (ii) a family member or close friend, or (iii) a neighbor was diagnosed or hospitalized due to COVID-19 or (iv) if a family member or a neighbor died from it.

- Outcome survey variables from the Trustlab:

  - On the average Blacks/African Americans have worse jobs, income, and housing than white people. Do you think the differences are mainly due to discrimination and disadvantages of educational opportunity, mainly due to differences in in-born ability, motivation, and effort, or some combination? What number best represents your view, if zero means mainly discrimination and lack of opportunity, and ten means mainly lesser ability, motivation and effort?

    * Mainly discrimination and educational disadvantage - 0 1 2 3 4 5 6 7 8 9 10 - Mainly lesser ability, motivation and effort

  - Recent weeks saw renewed attention to the interactions between African Americans and policemen, with the death of George Floyd in Minneapolis in particular leading to large demonstrations. Where would you say that your own reaction lies along a scale from: 0 = The issue has been overblown by the media to: 10 = It should be an urgent priority of our society and leaders to reform our police departments so that African Americans are treated with equal respect and can feel trust in the police, rather than fear.

    * The issue has been overblown by the media - 0 1 2 3 4 5 6 7 8 9 10 - It should be an urgent priority of our society and leaders to reform our police departments so that African Americans are treated with equal respect and can feel trust in the police, rather than fear.

- When answering the following questions, please think about the United States institutions. How much trust do you have in the following?
    * The police: I do not trust them at all - 0 1 2 3 4 5 6 7 8 9 10 - I completely trust them

4.B.2 Local Variables and Data Sources

This section describes the local variables matched with the Trustlab data based on the respondents' geographical location. The geographical location of respondents was inferred from their zip codes. I used the crosswalk by UDS Mapper (2021) to assign each participant's zip code to a ZIP Code Tabulation Area (ZCTA). The ZCTA is an aggregated level comprising several zip codes at which data from the U.S. Census and the 5-year American Community Survey (ACS) are available. Via the R package "zipcodeR" (Rozzi, 2021), I retrieved the coordinates (longitude and latitude) of the ZCTA's centroids and assigned the ZCTAs to their respective counties and states.

Based on this, I matched the Trustlab data with data on counties' total numbers of population and data on their population density (population per square mile) from the United States Census Bureau (2021). Data on general election results at the county level was compiled by McGovern et al. (2020). Data from Opportunity Insights (2021) at the county level include several economically relevant differences between Black and white Americans. Local variables at the zip code level were retrieved from the 5-year American Community Survey (ACS) using the R package "tidycensus" (Walker et al., 2021). Data on location and timing of Black Lives Matter (BLM) protests are from the Armed Conflict Location & Event Data Project (ACLED, 2021). Meteorological data are from GridMET and TerraClimate, downloaded using the R package climateR (Johnson et al., 2021)

- American Community Survey[36], 2015-2019 ACS 5-Year estimates at the zip code (ZCTA) level (United States Census Bureau, 2020), accessed with the R package tidycensus (Walker et al., 2021).
  - Share of female residents
  - Share of white residents
  - Share of African-American residents
  - Share of people above 65 years
  - Share of people below 18 years
  - Local Gini index of household income inequality
  - Educational attainment: Share of people with less than High school degree (for total population, whites, and African Americans)
  - Educational attainment: Share of people with Bachelor's degree or higher (for total population, whites, and African Americans)

[36]See https://api.census.gov/data/2019/acs/acs5/variables.html for an overview of the variables in the ACS.

- Unemployment rate: Share of unemployed persons from civilian labor force (for total population, whites, and African Americans)
- Income per capita: per capita income of residents in the past 12 months in 2019 inflation-adjusted dollars (for total population, whites, and African Americans)

- Opportunity Insights (2021) data (http://https://opportunityinsights.org/data) at the county level; dataset "All Outcomes by County, Race, Gender and Parental Income Percentile":
  - Intergenerational mobility: Probability of reaching the top quintile of the national household income distribution (among children born in the same year) in 2014-15 (for whites and African Americans)
  - Incarceration rate: Fraction incarcerated on April 1st, 2010 (for whites and African Americans)
  - Share of two-parents families: Fraction of children claimed by two people in the year they are linked to parents (for whites and African Americans)
  - Teenage pregnancy share: Fraction of women who grew up in the given tract who ever claimed a child who was born when they were between the ages of 13 and 19 as a dependent at any point (for whites and African Americans)
  - Community college degree rate: Fraction of children who have at least a community college degree (among children who received the ACS or the 2000 Census long form at age 25+) (for whites and African Americans)

- Data on location and timing of BLM protests from ACLED (2021) covering more than 11,000 protests associated with the BLM movement after the death of George Floyd (Kishi et al., 2021).

- County population data (estimates for 2019 and 2020) from United States Census Bureau (2021). Population density data (population per square mile) is based on the United States Census of 2010.

- County level outcomes from the General Elections in 2016 and 2020 from McGovern et al. (2020).

- Indicators for whether the state governor was from the Democrats or Republicans at the time of the survey are based on the overview in Wikipedia (2021).

- Weather data (maximum temperature in kelvin and precipitation in millimeters) obtained with the R package climateR (Johnson et al., 2021) from gridMET (Abatzoglou, 2013) (see http://www.climatologylab.org/gridmet.html).

- Number of days with comfortable temperature defined as number of days with maximum temperature between 18 and 26 degree Celsius (291.15 and 299.15 kelvin).

- Monthly weather data normals obtained with the R package climateR (Johnson et al., 2021) from TerraClimate
(see http://www.climatologylab.org/terraclimate.html).

- COVID-19 statistics on cases and deaths obtained from the The New York Times (2021) at the county-level using the R package "covdata" (Healy, 2020).

Four types of variables were constructed to capture the exposure to BLM protests based on the ACLED (2021) data (see the report by Kishi et al. (2021)):

- The number of BLM protests in the county where the respondent lives on the day when the respondent was surveyed and for the 1, 3, 5, 7, 10, or 14 days before (the natural logarithm of one, to account for zeros, plus these variables was used in regression analyses).

- The number of BLM protests within a radius of 25, 50, or 100 kilometers on the day when the respondent was surveyed and for the 1, 3, 5, 7, 10, or 14 days before (the natural logarithm of one, to account for zeros, plus these variables was used in regression analyses).

- The natural logarithm of one plus the sum of BLM protests' inverse distances (1/distance) to the respondents' location (for each respondent j) of residence (i.e., protests that are further away have less influence) between the day when the respondent was surveyed and for the 1, 3, 5, 7, 10, or 14 days before, i.e., $protestintensity_j = ln(1 + \sum_{i=1}^{N_j} \frac{1}{Distance_i^j})$, where i is the counter for the N_j protests taking place in the respondent-specific time frame for respondent j. The respondent's location of residence was inferred from their zip codes (ZCTA). The distances between the geographical coordinates of the centroid of the zip codes (ZCTAs) and the coordinates of the places of BLM protests in the ACLED (2021) dataset were calculated as the bird flies using the "geodist" R Package (Padgham et al., 2021) with the Haversine method.

- Indicator variables for above-median values of the former three types of variables.

4.B.3 Additional Tables and Analyses

In this section of the Appendix, summary statistics and regression tables are shown on which the exhibits from the main text are based.

Table 4.3: Summary Statistics: Trustlab Variables

Variable	Mean	SD	p25	p50	p75	Min	Max	N
Characteristics								
Female	0.53	0.50	0.00	1.00	1.00	0.00	1.00	2210
Age	46.69	15.24	34.00	48.00	59.00	17.98	80.00	2210
Age squared	2412.42	1433.56	1156.00	2304.00	3481.00	323.36	6400	2210
White	0.73	0.44	0.00	1.00	1.00	0.00	1.00	2210
African American	0.11	0.32	0.00	0.00	0.00	0.00	1.00	2210
Hispanic	0.10	0.30	0.00	0.00	0.00	0.00	1.00	2210
Asian	0.03	0.18	0.00	0.00	0.00	0.00	1.00	2210
Rural	0.23	0.42	0.00	0.00	0.00	0.00	1.00	2210
Town	0.21	0.41	0.00	0.00	0.00	0.00	1.00	2210
City	0.56	0.50	0.00	1.00	1.00	0.00	1.00	2210
Bottom income	0.23	0.42	0.00	0.00	0.00	0.00	1.00	2210
Top income	0.15	0.36	0.00	0.00	0.00	0.00	1.00	2210
Tertiary diploma	0.46	0.50	0.00	0.00	1.00	0.00	1.00	2210
High school or less	0.19	0.39	0.00	0.00	0.00	0.00	1.00	2210
Employed	0.52	0.50	0.00	1.00	1.00	0.00	1.00	2210
Self-employed	0.08	0.28	0.00	0.00	0.00	0.00	1.00	2210
Unemployed	0.14	0.35	0.00	0.00	0.00	0.00	1.00	2210
Inactive	0.26	0.44	0.00	0.00	1.00	0.00	1.00	2210
Survey and Experimental Variables								
Support BLM movement	6.44	3.37	4	7	10	0	10	1120
Racial Prejudice	5.49	3.02	3.00	5.50	8.00	0.00	10.00	2006
Left-wing	0.16	0.37	0.00	0.00	0.00	0.00	1.00	2210
Right-wing	0.35	0.48	0.00	0.00	1.00	0.00	1.00	2210
COVID Exposure	0.34	0.47	0.00	0.00	1.00	0.00	1.00	1120
Discriminates	0.16	0.37	0.00	0.00	0.00	0.00	1.00	2210
Prosociality	0.00	1.00	-0.60	-0.17	0.75	-2.17	1.83	2210
PGG Contribution	6.12	3.17	4.00	5.00	10.00	0.00	10.00	2210
DG Transfer	4.83	2.71	3.00	5.00	5.00	0.00	10.00	2210
DG Transfer (A.A.)	4.85	2.82	3.00	5.00	5.00	0.00	10.00	1120
Share donated	0.28	0.39	0.00	0.00	0.50	0.00	1.00	1452
Trust (Sender)	6.09	3.07	5.00	5.00	10.00	0.00	10.00	2210
Trustworthiness (Receiver)	9.08	5.51	5.00	8.50	11.55	0.00	25.00	2210

Notes: The table shows means, standard deviations, percentiles (25, 50, 75 percent), minimum and maximum values, and numbers of observations. Variables in the first block are binary except for Age and Age squared. Prosociality is the index based on transfers in the standard DG and contributions in the PGG (see design section above). Discriminates is equal to 1 if the participant discriminates against African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). DG Transfer and DG Transfer (A.A.) are the transfers in the standard DG and the interethnic DG towards a receiver of African American ethnicity. Share donated is the share of earnings donated to UNICEF. Trust (Sender) is the amount sent in the standard TG. Trustworthiness (Receiver) is the mean over all possible amounts returned (10, 13, ..., 40) for each possible amount sent (0, 1, ..., 10) by the sender.

Table 4.4: Summary Statistics: Local Variables

Variable	Mean	SD	Min	Max	N
ACS Variables (ZIP Code Level)					
Young Share	0.22	0.05	0.00	0.39	2195
Elderly Share	0.16	0.06	0.00	0.92	2195
Female Share	0.51	0.03	0.11	0.69	2195
Per-capita Income	35651.63	16297.57	11236	155789	2195
Unemployment rate	0.05	0.03	0.00	0.27	2195
Less than High school	0.11	0.08	0.00	0.53	2195
Bachelor or higher	0.34	0.18	0.00	0.90	2195
Gini Index	0.44	0.05	0.08	0.65	2194
Black Share	0.13	0.18	0.00	0.96	2195
Less than High school Black	0.12	0.12	0.00	1.00	2195
Bachelor or higher Black	0.26	0.19	0.00	1.00	2195
Unemployment Rate Black	0.08	0.10	0.00	1.00	2195
Per-capita Income Black	27433.53	15216.96	1701	286903	2045
Less than High school White	0.11	0.08	0.00	0.60	2195
Bachelor or higher White	0.36	0.19	0.00	1.00	2195
Unemployment Rate White	0.05	0.02	0.00	0.27	2195
Per-capita Income White	38957.03	17993.06	12003	168843	2195
Racial Gap Less than High school	-0.02	0.13	-0.95	0.46	2195
Racial Gap Bachelor or higher	0.10	0.18	-0.90	0.65	2195
Racial Gap Unemployment	-0.04	0.10	-0.97	0.27	2195
Racial Gap Per-capita Income	11239.33	16230.47	-244857	109799	2195
Local Racial Gaps (std)	0.00	1.35	-10.39	5.92	2195
County Level Variables					
ln(POP. ESTIMATE 2020)	13.03	1.55	12.00	13.28	2195
GOP16	0.41	0.49	0.00	0.00	2195
Percent Democrats 2016	0.51	0.18	0.37	0.51	2195
Percent Republicans 2016	0.45	0.18	0.33	0.43	2195
Cases per 100k	896.89	862.45	6.35	5875.15	1118
ln(Cases per 100k+1)	6.32	1.08	2.00	8.68	1118
Deaths per 100k	43.03	60.90	0.00	269.43	1118
ln(Deaths per 100k+1)	2.91	1.42	0.00	5.60	1118
BLM Protest Variables					
ln(Protests, inverse distances +1)	1.47	0.68	0.08	4.46	1119
ln(Protests within 50kms +1)	2.30	1.34	0.00	5.31	1119
Protests County	7.21	11.18	0.00	90	1119
Protests within 50kms	21.62	32.19	0.00	202	1119

Notes: The table shows means, standard deviations, minimum and maximum values, and numbers of observations for local variables. Young (Elderly) Share is the share of residents younger than 18 years (older than 65 years). ln(POP. ESTIMATE 2020) is the natural logarithm of the U.S. Census county population estimate from 2020. GOP16 is a dummy that is equal to 1 if the Republican party won in the 2016 general election in the county. Racial gap variables calculated based on the difference the value for white minus the value for African Americans. Local Racial Gaps is the PCA-Index based on the Racial Gap variables. BLM protests summary statistics for the number of protests in the time frame between the survey date and up to 14 days before.

Table 4.5: Regressions: Experimental Variables (Above-median Prosociality)

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice
Female		0.119**	0.052		-0.159****	-0.091**
		(0.060)	(0.057)		(0.044)	(0.039)
Age		-0.548***	-0.516***		0.572****	0.480****
		(0.184)	(0.173)		(0.142)	(0.128)
Age squared		0.427**	0.404**		-0.486****	-0.405***
		(0.188)	(0.178)		(0.143)	(0.131)
Bottom inc.		-0.077	-0.107		-0.106*	-0.069
		(0.078)	(0.074)		(0.058)	(0.052)
Top inc.		0.098	0.060		-0.024	-0.008
		(0.081)	(0.073)		(0.062)	(0.052)
High school or less		-0.092	-0.02		0.120*	0.025
		(0.087)	(0.085)		(0.064)	(0.058)
Tertiary diploma		0.180**	0.190***		-0.062	-0.113**
		(0.070)	(0.067)		(0.050)	(0.045)
Town		0.142	0.100		-0.084	-0.057
		(0.090)	(0.083)		(0.064)	(0.057)
City		0.185**	0.178**		0.012	0.001
		(0.082)	(0.075)		(0.057)	(0.051)
Self-employed		-0.055	-0.087		-0.144*	-0.116*
		(0.108)	(0.100)		(0.084)	(0.071)
Unemployed		0.003	-0.051		-0.043	-0.004
		(0.090)	(0.084)		(0.072)	(0.065)
Inactive		-0.112	-0.175**		-0.132**	-0.057
		(0.087)	(0.081)		(0.058)	(0.052)
African American	0.631****	0.635****	0.551****	-0.494****	-0.462****	-0.382****
	(0.074)	(0.079)	(0.078)	(0.070)	(0.071)	(0.065)
Hispanic	-0.037	-0.077	-0.090	-0.012	0.024	0.078
	(0.109)	(0.111)	(0.107)	(0.072)	(0.074)	(0.068)
Asian	0.046	-0.033	-0.114	-0.114	-0.023	0.054
	(0.145)	(0.141)	(0.134)	(0.094)	(0.093)	(0.085)
Right-wing			-0.407****			0.652****
			(0.069)			(0.045)
Left-Wing			0.537****			-0.716****
			(0.072)			(0.057)
Polit. Missing			-0.123			0.142*
			(0.104)			(0.078)
Wave 2				-0.079*	-0.047	-0.080**
				(0.043)	(0.044)	(0.039)
Prosocial	0.081***	0.067**	0.080***	0.021	0.022	-0.004
	(0.029)	(0.029)	(0.027)	(0.022)	(0.022)	(0.019)
Discriminates	-0.044	-0.115	0.029	0.595****	0.547****	0.313****
	(0.080)	(0.080)	(0.086)	(0.059)	(0.062)	(0.059)
Constant	-0.063*	-0.283***	-0.133	0.028	0.186***	0.052
	(0.037)	(0.100)	(0.101)	(0.035)	(0.070)	(0.066)
Obs.	1120	1120	1120	2006	2006	2006
Clusters						
R2	0.045	0.096	0.196	0.066	0.097	0.308
Adj. R2	0.041	0.082	0.181	0.063	0.089	0.301

Table 4.5: Regressions: Experimental Variables (Above-median Prosociality)

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice
Tests (p-values)						
Wave 2 = 0				0.068	0.287	0.042
Prosocial = 0	0.006	0.021	0.004	0.330	0.307	0.847
Discriminates = 0	0.576	0.149	0.731	0.000	0.000	0.000

Notes: The table shows OLS regression results. The regressions are those underlying the main results figure regarding behavior in the experiments. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-6) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The dummy "Wave 2" is equal to 1 for respondents from the second wave. The "Prosocial" dummy is equal to one if the prosociality index is above the median. The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.6: Regressions: Experimental Variables

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice
Female		0.121**	0.054		-0.159****	-0.091**
		(0.060)	(0.057)		(0.044)	(0.039)
Age		-0.575***	-0.548***		0.566****	0.481****
		(0.184)	(0.174)		(0.142)	(0.128)
Age squared		0.459**	0.441**		-0.480****	-0.405***
		(0.189)	(0.179)		(0.142)	(0.131)
Bottom inc.		-0.077	-0.107		-0.106*	-0.069
		(0.078)	(0.074)		(0.058)	(0.052)
Top inc.		0.098	0.060		-0.024	-0.008
		(0.081)	(0.073)		(0.062)	(0.052)
High school or less		-0.092	-0.020		0.119*	0.025
		(0.087)	(0.085)		(0.064)	(0.058)
Tertiary diploma		0.188***	0.200***		-0.062	-0.113**
		(0.070)	(0.067)		(0.050)	(0.045)
Town		0.150*	0.107		-0.082	-0.057
		(0.090)	(0.082)		(0.064)	(0.057)
City		0.184**	0.177**		0.012	0.001
		(0.082)	(0.075)		(0.057)	(0.051)
Self-employed		-0.054	-0.086		-0.145*	-0.116
		(0.107)	(0.100)		(0.083)	(0.070)
Unemployed		0.012	-0.042		-0.041	-0.003
		(0.090)	(0.084)		(0.072)	(0.065)
Inactive		-0.115	-0.178**		-0.132**	-0.057
		(0.087)	(0.081)		(0.058)	(0.052)
African American	0.644****	0.648****	0.565****	-0.489****	-0.458****	-0.380****
	(0.075)	(0.079)	(0.078)	(0.069)	(0.071)	(0.065)
Hispanic	-0.032	-0.072	-0.086	-0.008	0.027	0.079
	(0.108)	(0.111)	(0.107)	(0.072)	(0.074)	(0.068)
Asian	0.057	-0.024	-0.104	-0.111	-0.021	0.054
	(0.145)	(0.140)	(0.134)	(0.095)	(0.094)	(0.085)
Right-wing			-0.415****			0.651****
			(0.069)			(0.045)
Left-Wing			0.530****			-0.716****
			(0.072)			(0.057)
Polit. Missing			-0.128			0.143*
			(0.103)			(0.078)
Wave 2				-0.081*	-0.049	-0.080**
				(0.043)	(0.044)	(0.039)
Prosociality Index	0.117****	0.108****	0.118****	0.035	0.031	0.005
	(0.031)	(0.030)	(0.029)	(0.022)	(0.022)	(0.020)
Discriminates	-0.058	-0.127	0.019	0.593****	0.546****	0.313****
	(0.080)	(0.080)	(0.085)	(0.058)	(0.062)	(0.059)
Constant	-0.064*	-0.291***	-0.135	0.027	0.186***	0.051
	(0.037)	(0.100)	(0.101)	(0.035)	(0.070)	(0.066)
Obs.	1120	1120	1120	2006	2006	2006
Clusters						
R2	0.052	0.103	0.203	0.067	0.097	0.308
Adj. R2	0.048	0.089	0.189	0.064	0.089	0.301

Table 4.6: Regressions: Experimental Variables

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice
Tests (p-values)						
Wave 2 = 0				0.063	0.269	0.041
Prosociality Index = 0	0.000	0.000	0.000	0.117	0.163	0.806
Discriminates = 0	0.463	0.112	0.827	0.000	0.000	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-6) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The dummy "Wave 2" is equal to 1 for respondents from the second wave. The prosociality index is based on transfers in the standard DG and contributions in the PGG (see design section above). The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.7: Regressions: Donations as Proxy for Prosociality

	(1) Support	(2) Prejudice
Wave 2		-0.075
		(0.048)
Share donated	0.135****	-0.039
	(0.035)	(0.024)
Discriminates	0.074	0.333****
	(0.114)	(0.077)
Payoff	0.036	-0.010
	(0.037)	(0.024)
Constant	-0.185	0.069
	(0.126)	(0.079)
Covariates		
Demographics	x	x
Polit. Orient.	x	x
Obs.	685	1322
R2	0.247	0.315
Adj. R2	0.223	0.303
Tests (p-values)		
Wave 2 = 0		0.123
Prosociality (Donation) = 0	0.000	0.106
Discriminates = 0	0.518	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Support BLM movement / Equal respect urgent priority, (2) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The dummy "Wave 2" is equal to 1 for respondents from the second wave. Share donated is the share of earnings in the Trustlab voluntarily donated to UNICEF. The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). Payoff is the participants' earnings from the randomly chosen game to determine his or her payoff. Socio-demographic controls are race dummies, age, age-squared, sex, top and bottom income dummies, education dummies, urbanization dummies, and employment dummies. Political orientation dummies for right-wing, left-wing, and missing. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.8: Regressions: Prosociality and Generosity in the Interethnic DG (Wave 2)

	(1) Support	(2) Support	(3) Prejudice	(4) Prejudice	(5) Prejudice
Wave 2					-0.080**
					(0.039)
Prosociality Index	0.118****		0.052*		-0.055*
	(0.029)		(0.027)		(0.030)
Altruism African Americans		0.146****		0.024	
		(0.029)		(0.027)	
Wave 2 × Prosociality Index					0.105***
					(0.040)
Discriminates	0.019	0.064	0.286****	0.303****	0.301****
	(0.085)	(0.085)	(0.076)	(0.075)	(0.059)
Covariates					
Demographics	x	x	x	x	x
Polit. Orient.	x	x	x	x	x
Obs.	1120	1120	1036	1036	2006
R2	0.203	0.210	0.358	0.356	0.311
Adj. R2	0.189	0.196	0.345	0.343	0.303

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-2) Support BLM movement / Equal respect urgent priority, (3-5) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. Prosociality is the prosociality index based on the DG and PGG choices. Altruism African Americans is the DG transfer to African Americans in the interethnic DG (only available in the second wave). The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). Socio-demographic controls are race dummies, age, age-squared, sex, top and bottom income dummies, education dummies, urbanization dummies, and employment dummies. Political orientation dummies for right-wing, left-wing, and missing.

Table 4.9: Regressions: Local Exposure to Racial Gaps

	(1) Support	(2) Support	(3) Prejudice	(4) Prejudice
Wave 2			-0.052	-0.084**
			(0.049)	(0.041)
Prosociality Index	0.106****	0.114****	0.029	0.005
	(0.030)	(0.029)	(0.025)	(0.022)
Discriminates	-0.133*	0.013	0.547****	0.312****
	(0.080)	(0.086)	(0.062)	(0.059)
Share Black	0.013	0.035	0.064***	0.031
	(0.031)	(0.030)	(0.024)	(0.023)
Local Racial Gaps	0.065**	0.061**	0.002	0.002
	(0.030)	(0.030)	(0.026)	(0.022)
Constant	-0.281***	-0.114	0.218***	0.071
	(0.105)	(0.110)	(0.070)	(0.066)
Covariates				
Demographics	x	x	x	x
Polit. Orient.		x		x
Obs.	1119	1119	1996	1996
R2	0.107	0.208	0.101	0.309
Adj. R2	0.092	0.192	0.092	0.301
Tests (p-values)				
Wave 2 = 0			0.287	0.042
Prosociality = 0	0.000	0.000	0.251	0.819
Discriminates = 0	0.099	0.880	0.000	0.000
Share Black = 0	0.608	0.211	0.008	0.161
Local Racial Gap = 0	0.029	0.042	0.943	0.911

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-2) Support BLM movement / Equal respect urgent priority, (3-4) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The dummy "Wave 2" is equal to 1 for respondents from the second wave. The prosociality index is based on transfers in the standard DG and contributions in the PGG (see design section above). The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). Share Black is the zip code level share of African American residents. Local Racial Gap is the first principal component of the local (zip code level) racial gaps between Blacks and white people in the percentage of two levels of educational attainment (less than High School and Bachelor's degree or higher), their unemployment rates, and per-capita incomes. Socio-demographic controls are race dummies, age, age-squared, sex, top and bottom income dummies, education dummies, urbanization dummies, and employment dummies. Political orientation dummies for right-wing, left-wing, and missing. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.10: Experimental Measures: Heterogeneity Analysis (Above-median Prosociality)

	H = Right-wing (1)	H = A.A. (2)	H = Fem. (3)	H = Right-wing (4)	H = Wave 2 (5)	H = A.A. (6)	H = Fem. (7)
Wave 2				-0.137***	-0.156***	-0.087**	-0.033
				(0.051)	(0.052)	(0.041)	(0.056)
Wave 2 × H				0.155**		0.059	-0.086
				(0.075)		(0.127)	(0.076)
Prosocial	0.114*	0.168***	0.328****	-0.091*	-0.097*	-0.007	0.046
	(0.069)	(0.060)	(0.081)	(0.051)	(0.055)	(0.040)	(0.054)
Prosocial × H	0.081	-0.056	-0.306***	0.228***	0.174**	-0.002	-0.103
	(0.114)	(0.142)	(0.110)	(0.077)	(0.076)	(0.131)	(0.076)
Discriminates	-0.538****	0.052	0.075	0.469****	0.297****	0.330****	0.291****
	(0.136)	(0.091)	(0.112)	(0.098)	(0.089)	(0.061)	(0.075)
Discriminates × H	0.953****	-0.170	-0.120	-0.289**	0.020	-0.138	0.045
	(0.166)	(0.259)	(0.161)	(0.119)	(0.114)	(0.214)	(0.115)
Constant	-0.115	-0.211**	-0.268**	0.102	0.094	0.055	0.014
	(0.104)	(0.106)	(0.105)	(0.071)	(0.071)	(0.068)	(0.072)
Obs.	1120	1120	1120	2006	2006	2006	2006
R2	0.222	0.196	0.202	0.314	0.310	0.309	0.309
Adj. R2	0.206	0.18	0.186	0.306	0.302	0.300	0.301
Tests (p-values)							
Wave 2 (H = 0) = 0				0.007	0.047	0.034	0.551
Prosocial (H = 0) = 0	0.100	0.005	0.000	0.073	0.074	0.858	0.391
Discriminates (H = 0) = 0	0.000	0.566	0.500	0.000	0.001	0.000	0.000
Wave 2 × H = 0				0.040		0.643	0.254
Prosocial × H = 0	0.479	0.694	0.005	0.003	0.023	0.988	0.177
Discriminates × H = 0	0.000	0.511	0.455	0.015	0.864	0.520	0.698
Wave 2 (H = 1) = 0				0.762		0.820	0.025
Prosocial (H = 1) = 0	0.031	0.388	0.777	0.017	0.154	0.942	0.292
Discriminates (H = 1) = 0	0.000	0.627	0.714	0.010	0.000	0.350	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-7) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. H is the dummy variable for exploring the heterogeneity (either right-wing, African American ethnicity, female sex, or a second wave dummy). Below tests, p-values for the interaction terms with H are reported. The dummy "Prosocial" is equal to one for respondents with an above-median prosociality index. The prosociality index is based on transfers in the standard DG and contributions in the PGG (see design section above). The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive) and political orientation (right-wing, left-wing, missing) dummies. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.11: Experimental Measures: Heterogeneity Analysis

	H = Right-wing (1)	H = A.A. (2)	H = Fem. (3)	H = Right-wing (4)	H = Wave 2 (5)	H = A.A. (6)	H = Fem. (7)
Wave 2				-0.136***	-0.083**	-0.087**	-0.033
				(0.051)	(0.042)	(0.041)	(0.056)
Wave 2 × H				0.150**		0.057	-0.087
				(0.075)		(0.127)	(0.076)
Prosociality Index	0.088**	0.122****	0.176****	-0.057**	-0.055*	0.004	0.018
	(0.040)	(0.031)	(0.038)	(0.029)	(0.030)	(0.021)	(0.026)
Prosociality Index × H	0.042	-0.047	-0.130**	0.143****	0.105***	0.011	-0.028
	(0.058)	(0.091)	(0.059)	(0.039)	(0.040)	(0.070)	(0.040)
Discriminates	-0.539****	0.040	0.046	0.465****	0.290***	0.330****	0.292****
	(0.136)	(0.091)	(0.111)	(0.097)	(0.089)	(0.061)	(0.075)
Discriminates × H	0.937****	-0.158	-0.092	-0.290**	0.020	-0.141	0.042
	(0.166)	(0.259)	(0.161)	(0.118)	(0.114)	(0.215)	(0.116)
Constant	-0.071	-0.143	-0.126	0.065	0.053	0.052	0.034
	(0.099)	(0.101)	(0.100)	(0.067)	(0.066)	(0.067)	(0.068)
Obs.	1120	1120	1120	2006	2006	2006	2006
R2	0.232	0.204	0.208	0.324	0.311	0.309	0.309
Adj. R2	0.217	0.188	0.192	0.316	0.303	0.300	0.301
Tests (p-values)							
Wave 2 (H = 0) = 0				0.007	0.047	0.034	0.551
Prosociality Index (H = 0) = 0	0.028	0.000	0.000	0.046	0.063	0.849	0.501
Discriminates (H = 0) = 0	0.000	0.663	0.682	0.000	0.001	0.000	0.000
Wave 2 × H = 0				0.047		0.656	0.252
Prosociality Index × H = 0	0.467	0.607	0.027	0.000	0.008	0.875	0.491
Discriminates × H = 0	0.000	0.541	0.570	0.014	0.861	0.511	0.719
Wave 2 (H = 1) = 0				0.811		0.803	0.024
Prosociality Index (H = 1) = 0	0.002	0.380	0.310	0.001	0.061	0.823	0.745
Discriminates (H = 1) = 0	0.000	0.625	0.708	0.011	0.000	0.362	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-7) Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. H is the dummy variable for exploring the heterogeneity (either right-wing, African American ethnicity, female sex, or a second wave dummy). Below tests, p-values for the interaction terms with H are reported. The prosociality index is based on transfers in the standard DG and contributions in the PGG (see design section above). The Discriminates dummy is equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive) and political orientation (right-wing, left-wing, missing) dummies. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.12: Main Regressions: BLM Protests

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice	(7) Police	(8) Police	(9) Police
Days since GF	0.087*	0.077*	0.070	-0.119**	-0.130**	-0.109**	-0.031	-0.024	-0.009
	(0.044)	(0.047)	(0.047)	(0.049)	(0.052)	(0.044)	(0.052)	(0.056)	(0.053)
BLM Protests	0.165****	0.148****	0.116***	-0.125***	-0.145***	-0.091**	-0.079*	-0.073	-0.030
	(0.039)	(0.044)	(0.042)	(0.048)	(0.050)	(0.040)	(0.042)	(0.046)	(0.043)
Constant	-0.371****	-0.337***	-0.150	0.344****	0.358***	-0.003	0.184*	0.183*	-0.078
	(0.104)	(0.111)	(0.117)	(0.099)	(0.109)	(0.105)	(0.103)	(0.109)	(0.108)
Obs.	1110	1110	1110	1026	1026	1026	1099	1099	1099
Clusters	504	504	504	479	479	479	502	502	502
R2	0.118	0.121	0.211	0.104	0.112	0.355	0.153	0.158	0.295
Adj. R2	0.103	0.101	0.19	0.087	0.089	0.337	0.138	0.138	0.277
Controls									
Demographics	x	x	x	x	x	x	x	x	x
Local Variables		x	x		x	x		x	x
Polit. Orient.			x			x			x
Tests (p-values)									
BLM Protests = 0	0.000	0.001	0.007	0.009	0.004	0.023	0.059	0.113	0.484

Notes: The table shows OLS regression results. The regressions are those underlying the main results figure regarding effects of BLM protests. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-6) Prejudice against African Americans, (7-9) Trust in the Police. The dependent and independent variables (except dummies) are standardized. Clustered (county level) standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. Days since GF is the linear time trend starting when George Floyd was murdered. BLM Protests is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 14 days before to the respondent's location. Regressions control for pandemic-related variables, i.e., the (natural logarithm of one plus the) total number of COVID-19 cases per 100,000 inhabitants at the county level and COVID Exposure which is a dummy equal to one if at least one of the conditions explained in the variables section is true. Socio-demographic controls are race dummies, age, age-squared, sex, top and bottom income dummies, education dummies, urbanization dummies, and employment dummies. Local variables are controls for local population characteristics (the share of residents younger than 18 years and older than 65 years, the share of female residents, and the share of African American residents), the principal component index of local racial gaps, and a dummy equal to one if the Republican party won the general election in 2016 in the respondent's county. Political orientation dummies for right-wing, left-wing, and missing. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.13: Regressions: Different Variables for BLM Protests

	(1) Support	(2) Support	(3) Support	(4) Prejudice	(5) Prejudice	(6) Prejudice	(7) Police	(8) Police	(9) Police
ln(Protests in County+1)	0.089***	0.076**	0.045	-0.101***	-0.111***	-0.062*	-0.086***	-0.073*	-0.037
	(0.032)	(0.034)	(0.031)	(0.037)	(0.041)	(0.035)	(0.032)	(0.037)	(0.034)
p-value	0.006	0.025	0.149	0.006	0.007	0.076	0.007	0.052	0.272
ln(Protests within 50 kilometers+1)	0.143****	0.138****	0.107***	-0.091**	-0.089*	-0.033	-0.092**	-0.081**	-0.046
	(0.038)	(0.039)	(0.038)	(0.042)	(0.047)	(0.042)	(0.037)	(0.040)	(0.038)
p-value	0	0	0.005	0.030	0.059	0.430	0.012	0.045	0.230
Above-median ln(Protests inverse distances+1)	0.087**	0.070*	0.042	-0.110***	-0.116***	-0.066**	-0.071**	-0.069*	-0.032
	(0.036)	(0.038)	(0.036)	(0.037)	(0.037)	(0.032)	(0.035)	(0.036)	(0.034)
p-value	0.017	0.068	0.235	0.003	0.002	0.041	0.041	0.056	0.347
Above-median ln(Protests within 50 kilometers+1)	0.117****	0.113***	0.073**	-0.102***	-0.106***	-0.033	-0.097***	-0.089**	-0.042
	(0.035)	(0.036)	(0.034)	(0.037)	(0.040)	(0.034)	(0.032)	(0.035)	(0.033)
p-value	0.001	0.002	0.033	0.006	0.007	0.336	0.003	0.010	0.199
Above-median ln(Protests in County+1)	0.088***	0.074**	0.057*	-0.104***	-0.113***	-0.083**	-0.089***	-0.080**	-0.059*
	(0.033)	(0.034)	(0.032)	(0.034)	(0.037)	(0.032)	(0.032)	(0.036)	(0.033)
p-value	0.007	0.031	0.074	0.003	0.003	0.010	0.006	0.026	0.074

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1-3) Support BLM movement / Equal respect urgent priority, (4-6) Prejudice against African Americans, (7-9) Trust in the Police. The dependent and independent variables (except dummies) are standardized. The construction of the protest variables is explained in the main text and in the Appendix above. The time frame is between the survey date and 14 days before the respondent took part. Clustered (county level) standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The sets of control variables are the same in each column as in the previous table.

Table 4.14: Protests: Support Heterogeneity Regressions

	H = Right-wing	H = High inc.	H = Female	H = A.A.	H = Prosoc.	H = Discrim.	H = GOP Gov
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Support	Support	Support	Support	Support	Support	Support
BLM	0.120**	0.128***	0.106**	0.118***	0.045	0.133***	0.112**
	(0.052)	(0.044)	(0.054)	(0.043)	(0.050)	(0.044)	(0.047)
BLM × H	-0.008	-0.077	0.015	-0.196**	0.124**	-0.136	0.010
	(0.057)	(0.080)	(0.051)	(0.099)	(0.049)	(0.084)	(0.053)
Obs.	1110	1110	1110	1110	1110	1110	1110
Clusters	504	504	504	504	504	504	504
R2	0.211	0.211	0.211	0.213	0.218	0.213	0.211
Adj. R2	0.189	0.19	0.189	0.192	0.196	0.191	0.189
Tests (p-values)							
BLM (H = 0) = 0	0.021	0.004	0.048	0.006	0.362	0.002	0.017
BLM × H = 0	0.888	0.335	0.767	0.049	0.011	0.105	0.872
BLM (H = 1) = 0	0.027	0.518	0.009	0.461	0.000	0.971	0.045

Notes: The table shows OLS regression results. The dependent variable is Support BLM movement / Equal respect urgent priority. The dependent and independent variables (except dummies) are standardized. BLM Protests is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 14 days before to the respondent's location. Clustered standard errors (county-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. H is the dummy variable for exploring the heterogeneity (dummy variables for right-wing, high income, female sex, African American ethnicity, an above-median prosociality index, discriminating in the interethnic TG, and living in a state with a Republican governor). Below tests, p-values for the interaction terms with H are reported. The H = 0 (H = 1) after a coefficient in brackets shows the test result (p-value) for the coefficient in the subgroup of the sample for which H is equal to zero (one). All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive), political orientation dummies, the dummy for self-reported exposure to COVID-19, the linear time trend since the death of George Floyd. Additionally, regressions control for the zip code level share of residents below 18 years and above 65 years, the female share, the share of African American residents, and the index for local racial gaps. At the county level, regressions contain a dummy equal to one if the Republicans won the 2016 general election and the natural logarithm of one plus the number of corona cases per 100k inhabitants up to the survey date. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.15: Protests: Prejudice Heterogeneity Regressions

	H = Right-wing	H = High inc.	H = Female	H = A.A.	H = Prosoc.	H = Discrim.	H = GOP Gov
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Prejudice	Prejudice	Prejudice	Prejudice	Prejudice	Prejudice	Prejudice
BLM	-0.138**	-0.069*	-0.105**	-0.090**	-0.106**	-0.105**	-0.112**
	(0.053)	(0.041)	(0.051)	(0.040)	(0.053)	(0.041)	(0.047)
BLM × H	0.096*	-0.134	0.023	-0.058	0.021	0.133**	0.063
	(0.053)	(0.084)	(0.059)	(0.100)	(0.052)	(0.065)	(0.059)
Obs.	1026	1026	1026	1026	1026	1026	1026
Clusters	479	479	479	479	479	479	479
R2	0.357	0.357	0.355	0.356	0.357	0.368	0.356
Adj. R2	0.339	0.339	0.337	0.337	0.338	0.349	0.337
Tests (p-values)							
BLM (H = 0) = 0	0.01	0.097	0.039	0.024	0.047	0.011	0.019
BLM × H = 0	0.071	0.111	0.702	0.561	0.690	0.041	0.290
BLM (H = 1) = 0	0.300	0.013	0.086	0.165	0.044	0.663	0.326

Notes: The table shows OLS regression results. The dependent variable is Prejudice against African Americans. The dependent and independent variables (except dummies) are standardized. BLM Protests is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 14 days before to the respondent's location. Clustered standard errors (county-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. H is the dummy variable for exploring the heterogeneity (dummy variables for right-wing, high income, female sex, African American ethnicity, an above-median prosociality index, discriminating in the interethnic TG, and living in a state with a Republican governor). Below tests, p-values for the interaction terms with H are reported. The H = 0 (H = 1) after a coefficient in brackets shows the test result (p-value) for the coefficient in the subgroup of the sample for which H is equal to zero (one). All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive), political orientation dummies, the dummy for self-reported exposure to COVID-19, the linear time trend since the death of George Floyd. Additionally, regressions control for the zip code level share of residents below 18 years and above 65 years, the female share, the share of African American residents, and the index for local racial gaps. At the county level, regressions contain a dummy equal to one if the Republicans won the 2016 general election and the natural logarithm of one plus the number of corona cases per 100k inhabitants up to the survey date. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.16: Protests: Trust Police Heterogeneity Regressions

	H = Right-wing	H = High inc.	H = Female	H = A.A.	H = Prosoc.	H = Discrim.	H = GOP Gov
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
	Police	Police	Police	Police	Police	Police	Police
BLM	-0.037	-0.016	-0.019	-0.029	0.008	-0.038	-0.025
	(0.053)	(0.044)	(0.056)	(0.043)	(0.058)	(0.043)	(0.049)
BLM × H	0.015	-0.087	-0.018	-0.161	-0.076	0.082	-0.014
	(0.055)	(0.070)	(0.054)	(0.106)	(0.059)	-0.069	(0.058)
Obs.	1099	1099	1099	1099	1099	1099	1099
Clusters	502	502	502	502	502	502	502
R2	0.293	0.294	0.293	0.295	0.296	0.299	0.293
Adj. R2	0.274	0.275	0.274	0.276	0.276	0.28	0.274
Tests (p-values)							
BLM (H = 0) = 0	0.483	0.714	0.735	0.505	0.891	0.373	0.603
BLM × H = 0	0.783	0.213	0.745	0.130	0.199	0.231	0.805
BLM (H = 1) = 0	0.648	0.166	0.431	0.105	0.144	0.539	0.474

Notes: The table shows OLS regression results. The dependent variables is Trust in the Police. The dependent and independent variables (except dummies) are standardized. BLM Protests is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 14 days before to the respondent's location. Clustered standard errors (county-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. H is the dummy variable for exploring the heterogeneity (dummy variables for right-wing, high income, female sex, African American ethnicity, an above-median prosociality index, discriminating in the interethnic TG, and living in a state with a Republican governor). Below tests, p-values for the interaction terms with H are reported. The H = 0 (H = 1) after a coefficient in brackets shows the test result (p-value) for the coefficient in the subgroup of the sample for which H is equal to zero (one). All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive), political orientation dummies, the dummy for self-reported exposure to COVID-19, the linear time trend since the death of George Floyd. Additionally, regressions control for the zip code level share of residents below 18 years and above 65 years, the female share, the share of African American residents, and the index for local racial gaps. At the county level, regressions contain a dummy equal to one if the Republicans won the 2016 general election and the natural logarithm of one plus the number of corona cases per 100k inhabitants up to the survey date. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.17: Robustness: Different Time Frames BLM Protests

	(1)	(2)	(3)	(4)	(5)
Dependent Variable: Support BLM	3 Days	5 Days	7 Days	10 Days	14 Days
BLM Protests	0.066**	0.100***	0.105***	0.111***	0.148****
	(0.033)	(0.037)	(0.040)	(0.040)	(0.044)
Obs.	1110	1110	1110	1110	1110
Clusters	504	504	504	504	504
R2	0.115	0.117	0.118	0.118	0.121
Adj. R2	0.094	0.097	0.097	0.097	0.101
BLM Protests (p-value)	0.042	0.008	0.008	0.005	0.001
Dependent Variable: Racial Prejudice	3 Days	5 Days	7 Days	10 Days	14 Days
BLM Protests	0.011	-0.060	-0.070	-0.084*	-0.145***
	(0.042)	(0.045)	(0.045)	(0.046)	(0.050)
Obs.	1026	1026	1026	1026	1026
Clusters	479	479	479	479	479
R2	0.103	0.105	0.105	0.106	0.112
Adj. R2	0.080	0.082	0.083	0.084	0.089
BLM Protests (p-value)	0.787	0.189	0.118	0.068	0.004
Dependent Variable: Trust in the Police	3 Days	5 Days	7 Days	10 Days	14 Days
BLM Protests	0.011	-0.032	-0.028	-0.044	-0.073
	(0.030)	(0.037)	(0.040)	(0.042)	(0.046)
Obs.	1099	1099	1099	1099	1099
Clusters	502	502	502	502	502
R2	0.154	0.155	0.154	0.155	0.156
Adj. R2	0.134	0.135	0.135	0.135	0.137
BLM Protests (p-value)	0.716	0.396	0.480	0.296	0.113

Notes: The table shows OLS regression results. The time frame for the protests contributing to the BLM protest variable (the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and several days before to the respondent's location) in the columns (1) to (5) is between the date of the survey and 3, 5, 7, 10, and 14 days before. The dependent and independent variables (except dummies) are standardized. Clustered standard errors (county-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. Below tests, p-values are reported. All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive), the dummy for self-reported exposure to COVID-19, and the linear time trend since the death of George Floyd. Additionally, regressions control for the zip code level share of residents below 18 years and above 65 years, the female share, the share of African American residents, and the index for local racial gaps. At the county level, regressions contain a dummy equal to one if the Republicans won the 2016 general election and the natural logarithm of one plus the number of corona cases per 100k inhabitants up to the survey date.

Table 4.18: BLM Protests: Mediation by Prosociality and Discrimination

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)	(10)	(11)	(12)
	Support	Support	Support	Support	Prejudice	Prejudice	Prejudice	Prejudice	Police	Police	Police	Police
BLM Protests	0.148****	0.141***	0.146****	0.139***	-0.145***	-0.153***	-0.138***	-0.145***	-0.073	-0.079*	-0.067	-0.071
	(0.044)	(0.044)	(0.044)	(0.044)	(0.050)	(0.049)	(0.048)	(0.047)	(0.046)	(0.046)	(0.044)	(0.044)
Prosociality Index		x		x		x		x		x		x
Discriminates			x	x			x	x			x	x
Obs.	1110	1110	1110	1110	1026	1026	1026	1026	1099	1099	1099	1099
Clusters	504	504	504	504	479	479	479	479	502	502	502	502
R2	0.121	0.128	0.122	0.13	0.112	0.121	0.142	0.149	0.156	0.162	0.173	0.178
Adj. R2	0.101	0.107	0.101	0.108	0.089	0.098	0.119	0.126	0.137	0.141	0.153	0.157
Tests (p-values)												
BLM Protests = 0	0.001	0.001	0.001	0.002	0.004	0.002	0.004	0.002	0.113	0.090	0.130	0.106

Notes: The table shows OLS regression results. Regressions in columns 2 and 6 contain the prosociality index based on choices in the DG and PGG. Regressions in columns 3 and 7 contain a dummy equal to 1 if the participant discriminates African Americans relative to white Americans in at least one of the interethnic TGs (with or without income information). Regressions in columns 1, 5, and 9 (4, 8, and 12) contain none (both) experimental variables. The dependent variables in the respective columns are (1-4) Support BLM movement / Equal respect urgent priority, (5-8) Prejudice against African Americans, and (9-12) Trust in the police. The dependent and independent variables (except dummies) are standardized. BLM Protests is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 14 days before to the respondent's location. Clustered standard errors (county-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. Below tests, p-values are reported. All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive), the dummy for self-reported exposure to COVID-19, and the linear time trend since the death of George Floyd. Additionally, regressions control for the zip code level share of residents below 18 years and above 65 years, the female share, the share of African American residents, and the index for local racial gaps. At the county level, regressions contain a dummy equal to one if the Republicans won the 2016 general election and the natural logarithm of one plus the number of corona cases per 100k inhabitants up to the survey date. p-values from hypothesis tests (Wald tests) for coefficients reported.

Table 4.19: Left-Right Divide: Mediation by Prosociality and Discrimination

	(1)	(2)	(3)	(4)	(5)	(6)
	Support	Support	Prejudice	Prejudice	Police	Police
Right-wing	-0.395****	-0.415****	0.677****	0.651****	0.488****	0.471****
	(0.068)	(0.069)	(0.045)	(0.045)	(0.046)	(0.046)
Left-wing	0.533****	0.530****	-0.738****	-0.716****	-0.432****	-0.426****
	(0.072)	(0.072)	(0.057)	(0.057)	(0.060)	(0.061)
Prosociality		0.118****		0.005		0.078****
		(0.029)		(0.020)		(0.020)
Discriminates		0.019		0.313****		0.082
		(0.085)		(0.059)		(0.062)
Wave 2			-0.069*	-0.080**	-0.141****	-0.149****
			(0.039)	(0.039)	(0.040)	(0.040)
Obs.	1120	1120	2006	2006	2191	2191
R2	0.190	0.203	0.298	0.308	0.219	0.225
Adj. R2	0.176	0.189	0.292	0.301	0.212	0.218
Diff. Left-Right (SD)	-0.928	-0.944	1.415	1.366	0.920	0.897
Tests (p-values)						
Prosociality = 0		0.000		0.806		0.000
Discriminates = 0		0.827		0.000		0.187
Right-wing = Left-wing	0.000	0.000	0.000	0.000	0.000	0.000
Wave 2 = 0			0.080	0.041	0.000	0.000

Notes: The table shows OLS regression results. Regressions in column 2, 4 and 6 contain the prosociality index based on choices in the DG and PGG and the discrimination dummy. The dependent variables in the respective columns are (1-2) Support BLM movement / Equal respect urgent priority, (3-4) Prejudice against African Americans, (5-6) Trust in the police. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The left-right divide is given in standard deviations of the dependent variable. Below tests, p-values are reported. All regressions contain the set of demographic dummies (race, age, age-squared, top and bottom quintile of household income dummies, High school or less, Tertiary diploma, town and city urbanization level dummies, dummies for self-employment, unemployment, and labor market inactive) and political orientation dummies. Regressions in the columns (3-6) contain a dummy for the second wave of the Trustlab. p-values from hypothesis tests (Wald tests) for coefficients reported.

4.B.4 Instrumental Variables Regression: BLM Protests

The results from the linear regressions in Section 4.3.2.2 suggest that more significant exposure to BLM protests is associated with a higher support for one of the main goals of the BLM movement (agreement that African Americans should be treated with equal respect by the police) and with weaker prejudice against African Americans. To some extent, the previous results also suggest a negative effect of protests on trust in the police, albeit only statistically significant in some specifications.

However, whether the OLS regressions deserve a causal interpretation can be questioned when the intensity of exposure to BLM protests is not truly exogenous concerning respondents' views on the studied issues. For instance, it might be that the proximity to BLM protests is partly related to respondents' attitudes. Respondents who live closer to BLM protests could, for instance, have chosen their location of residence in a neighborhood that fits their views on racial topics and attitudes. In the OLS regressions, the model specifications B and C in Panel B of Figure 4.3 control for local characteristics and election results at the county level to account for this supporting a (cautious) causal interpretation. Specification C also controlled for respondents' political orientations. The instrumental variables regressions in this section serve as an additional robustness check.

Unlike the main text's regressions, the BLM protest variable used in the instrumental variables regressions only comprises protests on the survey day and up to five days before. This reduction of the time frame is necessary to get sufficient explanatory power from the weather instruments on the BLM protest variable. However, as the OLS regression estimates show, the qualitative results are similar to the longer time frame.

Three variables serve as instruments for the BLM protest variable. The first is the natural logarithm of one plus local rainfall in millimeters on the survey day and the five days before. The second is the number of days with a comfortable temperature (ad hoc defined as a maximum temperature between 18 and 26 degrees celsius) within the time frame. The third instrument is the share of votes for the Democratic party in the general election of 2016 at the county level. The intuition for the instrumental variables' relevance in explaining the BLM protest variable is simple. Weather conditions are known to affect the formation of protests, and the objectives of the BLM movement are more related to the Democratic than to the Republican party (Collins and Margo, 2007; Madestam et al., 2013; Wasow, 2020; Teeselink and Melios, 2021). Weather around the day of the survey is plausibly exogenous to racial issues. However, general election results from 2016 at the county level are not exogeneous. Respondents are, for instance, more likely to be Democrats in Democratic counties, and the respondents' political orientations may be influenced by their environment. Because of this, the regressions in Table 4.21 condition on the respondent's political orientation to address this problem. Table 4.20 reports the results from the first-stage regressions.

Table 4.20: First-Stage Regressions

	(1)	(2)
Rain	-0.023***	-0.019**
	(0.008)	(0.009)
Temperature	0.048****	0.050****
	(0.006)	(0.006)
Democrats 2016	0.390****	0.356***
	(0.116)	(0.125)
Constant	0.540*	0.208
	(0.297)	(0.364)
Obs.	1109	1032
Clusters	504	453
Controls		
Time trend	x	x
Race	x	x
Political Orientation	x	x
Demographics	x	x
Local Population Characteristics	x	x
Local Economic Var. (General Pop. & Black)		x
Racial Gaps Index	x	

Notes: The table shows first-stage regression results for both specifications. Standard errors (clustered at the county level) are in parentheses. The dependent variable is the natural logarithm of one plus the sum of inverse distances of the protests in the time frame between the respondent-specific survey date and 5 days before to the respondent's location.

Table 4.21 shows results from OLS and 2SLS regressions. The first specification (for OLS and 2SLS) controls for ethnicity dummies, the complete set of socio-demographics (gender, age, age-squared, dummies for the first and last income quintiles, dummies for less than High school education and tertiary education, urbanization levels), employment status dummies, a linear time trend since the death of George Floyd, political orientation dummies, and the monthly normal of precipitation for the month when the respondent took part. It also contains variables for the natural logarithm of the county's total population estimate, the natural logarithm of one plus the number of corona cases per 100,000 inhabitants in the respondent's county, a dummy that is equal to one if the respondent reported personal exposure to COVID-19 (as defined above), the share of people below 18 years and above 65 years of age, the share of female residents, the share of Black residents at the zip code level, and the index of local racial gaps. The second specification contains all socio-demographic, ethnicity, and employment status controls from the first specification. Besides the local variables at the zip code level from the first specification, it also controls for the per-capita income of the general and Black population, the unemployment rates of the general and Black population, the share of residents having at least a Bachelor's degree of the general and Black population, and the local (ZCTA) Gini index.[37]

Overall, the instrumental variables regressions replicate the previous results. More significant exposure to BLM protests is associated with a more vital level of support for the BLM movement's primary goal. It is also associated with weaker prejudice. The slight negative effect on trust in the police cannot be replicated in the regressions. The relatively lower level of statistical significance is related to (a) the impreciseness of instrumental variables estimation and (b) the shorter time frame considered (necessary for avoiding instruments from becoming weak).

Table 4.21: Protests: IV and OLS Regressions

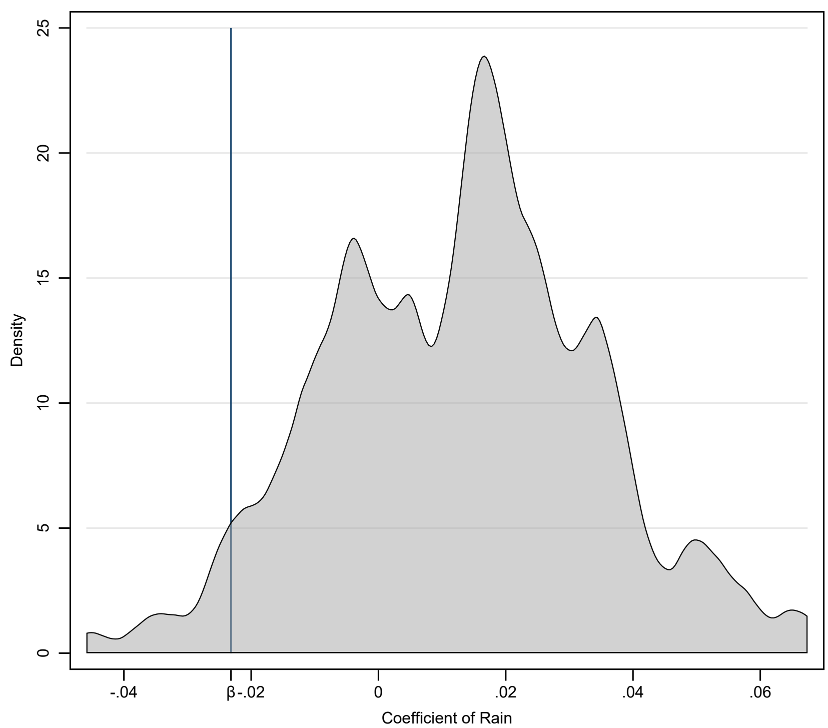
	Support OLS	**Support 2SLS**	**Prejudice OLS**	**Prejudice 2SLS**	**Police OLS**	**Police 2SLS**
Panel A: Model 1						
BLM Protests	0.081**	0.207**	-0.036	-0.200**	-0.018	-0.034
	(0.036)	(0.095)	(0.034)	(0.096)	(0.035)	(0.109)
Obs.	1109	1109	1025	1025	1098	1098
Clusters	504	504	479	479	502	502
R2	0.209	0.200	0.354	0.34	0.296	0.296
Adj. R2	0.187	0.179	0.336	0.321	0.277	0.277
Underidentification F		60.751		58.17		60.239
Weak Identification F		27.471		25.097		27.618
Hansen J Statistic		2.873		0.482		9.529
BLM Protests (p-value)	0.024	0.030	0.283	0.037	0.619	0.752
Panel B: Model 2						
BLM Protests	0.076**	0.197*	-0.055	-0.258**	-0.022	-0.063
	(0.037)	(0.102)	(0.035)	(0.104)	(0.037)	(0.117)
Obs.	1032	1032	952	952	1021	1021
Clusters	453	453	429	429	451	451
R2	0.203	0.195	0.359	0.338	0.306	0.305
Adj. R2	0.175	0.167	0.335	0.313	0.282	0.281
Underidentification F		58.109		54.956		57.581
Weak Identification F		29.804		27.829		29.975
Hansen J Statistic		1.759		0.076		5.421
BLM Protests (p-value)	0.042	0.053	0.116	0.013	0.558	0.593

Notes: Table shows results from OLS and 2SLS regressions. Estimation with the ivreg2 command. Standard errors (clustered at the county-level) in parentheses. Underidentification F is the Kleibergen-Paap rk LM statistic. Weak Identification F is the Kleibergen-Paap rk Wald F statistic. Hansen J statistic (overidentification test of all instruments) reported. p-value for the BLM coefficient reported in the last row of each panel.

[37] The per-capita income, unemployment rates, educational attainment and Gini index variables "replace" the index of local racial gaps which is based on these variables.

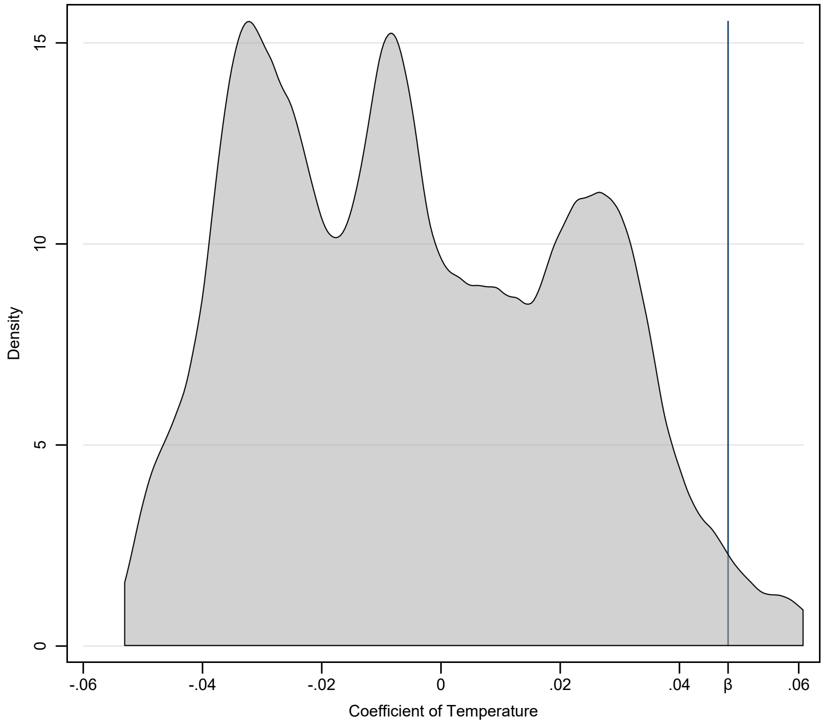
Placebo analysis. — I used weather data from the 370 days preceding the survey dates to calculate the first-stage regression coefficients of placebo weather variables. This approach leads to a total of 366 estimated coefficients spanning over the time frame of six days. Although some extreme values occur, the correlation of weather in the "original" time frame up to five days before the survey with the BLM protests outcome variable is remarkably strong. More precisely, 95.6 percent of the estimated rainfall coefficients and 98.1 percent of the coefficients for the number of days with a comfortable temperature are larger or smaller, respectively, than the estimated first-stage coefficient. The Figures 4.20 and 4.21 depict the distribution (kernel density estimation) of estimated coefficients on the BLM protests outcome variable, where beta is the coefficient in the actual time frame around the survey using one year of weather data. This finding suggests that the weather instruments indeed explain variation in the intensity of BLM protests and do not only capture a random coincidence.

Figure 4.20: Placebo: Rainfall



Notes: The figure shows the distribution of 366 estimated coefficients of the (placebo) natural logarithm of one plus the rainfall over the time frame. Biweight kernel density estimation. Beta is the coefficient using the time frame from the survey day to five days before the survey. The weather data ranges from the day of the survey to 370 days before the survey to cover a year of weather data.

Figure 4.21: Placebo: Days with Comfortable Temperature



Notes: The figure shows the distribution of 366 estimated coefficients of the (placebo) number of days with comfortable temperature. Biweight kernel density estimation. Beta is the coefficient using the time frame from the survey day to five days before the survey. The weather data ranges from the day of the survey to 370 days before the survey to cover a year of weather data.

Chapter 5

The politicized pandemic: Ideological polarization and the behavioral response to COVID-19

Gianluca Grimalda[a], Fabrice Murtin[b],
David Pipke[c], Louis Putterman[d], Matthias Sutter[e]

An earlier version of this paper is available at http://hdl.handle.net/10419/250080 (permalink) as Grimalda, G., Murtin, F., Pipke, D., Putterman, L., & Sutter, M. (2022). The politicized pandemic: Ideological polarization and the behavioral response to COVID-19, Kiel Working Paper, No. 2207, Kiel Institute for the World Economy (IfW Kiel), Kiel.

Financial support from the University of Cologne (through the Hans Kelsen Prize), the Max Planck Society and Deutsche Forschungsgemeinschaft (DFG, German Research Foundation) under Germany's Excellence Strategy – EXC 2126/1–390838866 is gratefully acknowledged.

[a]Kiel Institute for the World Economy (gianluca.grimalda@ifw-kiel.de)
[b]OECD Statistics and Data Directorate (fabrice.murtin@oecd.org)
[c]Kiel Institute for the World Economy (david.pipke@ifw-kiel.de)
[d]Brown University (louis_putterman@brown.edu)
[e]Sutter: Max Planck Institute for Research on Collective Goods, University of Cologne, University of Innsbruck, and IZA (matthias.sutter@coll.mpg.de)

Abstract

Ideological polarization is associated with specific traits that characterize liberals and conservatives differently. We focus on a trait that has received relatively little attention, i.e., prosociality. We investigate how prosociality and ideology interact in their relationship with health-protecting behavior vis-à-vis the COVID-19 pandemic and trust in government to handle the COVID-19 crisis. Our hypotheses are: (1) Conservatives are less compliant with COVID-19-related behavioral restrictions than liberals. We argue that protective behavior ultimately rests on prosocial preferences; therefore, (2) we expect a positive relationship between our experimental measure of prosociality and protective behavior. (3) We also expect that one's ideology mediates the evaluation of the government's handling of the crisis. While previous studies analyzed the effect of prosociality and ideology on protective behavior separately, (4) we test whether prosociality mediates the effect of ideology. We test these hypotheses in a representative sample of the U.S. population during the first summer of the pandemic. We find confirmation for hypotheses (1)-(3). Contrary to (4), prosociality and ideology are independent of each other in their relationship with health-protective behavior. We also find that behavioral differences between liberals and conservatives are up to 4.4 times smaller than their differences in judging the government's crisis management. This result suggests that Americans were more polarized in their political views than in their acceptance of public health advice.

JEL Codes: D01, D72, D91, I12, I18, H11, H12

Keywords: Polarization, Ideology, Trust in politicians, COVID-19, Prosociality, Health behavior, Worries

5.1 Introduction

Political polarization is on the rise in the U.S. as well as Western democracies (Waller and Anderson, 2021; Pew Research Center, 2019) and is considered a disruptive force for democracies worldwide (Iyengar et al., 2019; Svolik, 2019; Gidron et al., 2020; Foa and Mounk, 2017, 2021; McCoy and Somer, 2021). The share of World Value Survey respondents thinking that a political system with "a strong leader who does not have to bother with parliament and elections" is "very good" or "fairly good" has been growing in most Western countries (see Table 5.19 in the Online Appendix). In the U.S., the share achieved an all-time high of 37.1% in 2017, close to the share found in Russia (39.4%).[1]

Political polarization has also been evident during the COVID-19 pandemic (Bobba and Hubé, 2021; Bruine de Bruin et al., 2020; Kerr et al., 2021), in spite of calls not to politicize the virus.[2] Political leaders and partisan media have spread conflicting messages and misinformation about the virus threat, which have likely affected their followers' views and their behavior (Simonov et al., 2022; Bursztyn et al., 2020). Political polarization has then been blamed for impeding efforts to fight the pandemic (Allcott et al., 2020; Gollwitzer et al., 2020). The reason is that adherence to public health policies to control the virus - such as wearing face masks or obeying stay-at-home policies - is fundamentally a large-scale cooperation problem (van Bavel et al., 2020; Nielsen and Lindvall, 2021; Campos-Mercade et al., 2021b). Both reduced trust in politicians and reduced prosociality may thus negatively affect the capacity to fight the COVID-19 pandemic (Bargain and Aminjonov, 2020). In this study, we examine, on the one hand, the interplay between ideology, prosociality, and protective behavior during the COVID-19 pandemic and, on the other hand, the evolution of trust in political institutions, in particular how the political management of the pandemic has been assessed. We present data from a representative sample of the U.S. population.

To understand the root causes of polarization, we need to analyze the psychological motivations of supporters of opposing political views. In the last two decades, research in political psychology has shown that conservative and liberal ideology rests on fundamentally different psychological traits associated with the management of threat and

[1]The share of respondents agreeing that a system with a strong leader is good for one's country ranges from 72.6% in Romania to 14.6% in Norway (see Table 5.19). Again, compared with other highly developed countries, the U.S. stands out for its high share of respondents accepting the idea of a strong leader. For instance, in Germany and Spain, such shares are 20.9% and 22.9%, respectively.

[2]Tedros Ghebreyesus, Director-General of the WHO, said in April 2020 "Please don't politicize this virus. It exploits the differences you have at the national level. [...] The unity of your country will be very important to defeat this dangerous virus." (WHO, 2020c).

uncertainty on the one hand and the degree of acceptance of inequality and hierarchy on the other (Jost et al., 2003; Jost, 2017).[3]

Another trait relevant for political ideology, albeit little investigated thus far, is prosociality, i.e., the willingness to sacrifice personal interests for collective interests. Existing research suggests that liberals have stronger prosocial attitudes than conservatives (van Lange et al., 2012; Solon, 2014; Osborne and Weiner, 2015; van Bavel et al., 2020; Romano et al., 2021b; Grünhage and Reuter, 2022), although the effect sizes tend to be small in a large-scale study involving 42 countries (Romano et al., 2021b). As adherence to COVID-19-related restrictive measures involves a disposition to prosociality, one could conjecture that conservatives are less likely to comply with such restrictions because of their lower levels of prosociality.

Besides potential behavioral differences along the ideological spectrum, another relevant aspect of political cleavages concerns the degree of support for public authorities during COVID-19. Conservatives tend to trust governments less than liberals. However, trust is responsive to the so-called "President-in-Power" effect, that is, the tendency to trust political institutions more when the political party supported by an individual is in power. For instance, Morsi et al. (2019) found that conservatives were (a) 14% more likely to favor reductions in government services and spending, (b) 16% more likely to agree that "less government is better," and (c) 19% more likely to say that the "government is too involved in things" when a Democrat rather than a Republican was in power.

This study contributes to understanding both aspects, (1) the interplay between ideology, prosociality, and compliance with COVID-19-related behavioral restrictions, and (2) the evolution of trust in political institutions - drawing on an online experiment run on a representative sample of the U.S. population (N=1,120). The sample is representative with respect to the targeted variables of gender, age, and income but also accurately mirrors the distribution of ideology preferences on the conservative-liberal spectrum (see Table 5.3 in the Appendix). The survey was conducted during the summer of 2020, when cases and deaths in the U.S. reached a second peak.

[3]In particular, conservatives are significantly less tolerant of ambiguity and uncertainty (Kohn, 1974; Lytwyn, 2012; Malka et al., 2014), more likely to engage in automatic/intuitive ("System-1") rather than deliberative/analytical ("System-2") thinking (Deary et al., 2008; Kemmelmeier, 2010), more likely to adopt self-deception heuristics (Onraet et al., 2011), more prone to believe that political or religious actors are threatening (Landau et al., 2004; Akrami et al., 2009; Jugert and Duckitt, 2009), more likely to fall into false consensus effects (Rabinowitz et al., 2016), more valuing of ingroup social cohesion (Caprara et al., 2006), less likely to engage in cross-ideological dissemination of both political and nonpolitical information on social media (Barberá et al., 2015), more dogmatic and cognitively rigid (Pettigrew, 1958), and more in need of order and structure, than liberals (van Hiel and Mervielde, 2003). Such psychological differences are even reflected in neurological differences, as Kanai et al. (2011) found that political conservatism is associated with a larger amygdala size. Instead, liberalism was associated with a higher volume of grey matter in the anterior cingulate cortex, an area related to empathy and decision-making.

Consistently with our hypotheses, we find that conservatives, on average, worried less about the spread of the virus and reported lower levels of self-quarantining and wearing face masks than liberals. Moreover, an experimental measure of prosociality obtained through standard dictator games and public goods games correlates positively with protective behavior and worry about the local spread, confirming the cooperative nature of complying with health-protective restrictions. Surprisingly, we found no mediation effect of prosociality in the relationship between ideology and compliance with either COVID-19-related restrictions or worry about the local spread of the virus. Prosociality and political ideology are virtually independent in affecting health-related protective behavior. This finding suggests that liberals' higher compliance with COVID-19 regulations is not due to their different degrees of prosociality. Instead, more prosocial people tend to comply with regulations more strongly independently of their political ideology.

As for the second aspect, we confirm the "President in Power" effect, as conservatives assess the government's crisis management much more positively than liberals at both the state and national levels. As a result, the reported evolution of trust in the government for handling the pandemic is much more positive among conservatives than liberals. Remarkably, polarization across ideological camps is several orders of magnitude higher for government support than for behavioral differences in health protection measures. For example, differences between conservatives and liberals in judging the political control of the pandemic are up to five times as large as differences in self-reported worries and behavioral measures. This result suggests that political polarization is considerably larger than behavioral polarization.

Our paper advances the literature in various essential directions. Previous studies run during the COVID-19 pandemic analyzed the impact of either ideological orientation or prosociality on protective behavior separately. Studies focusing on ideology found polarized engagement in protective behavior in line with our results. Several studies use aggregated data comparing the development of cases and deaths in geographical areas differing in their average political preferences (Gollwitzer et al., 2020; Grossman et al., 2020; Allcott et al., 2020). For example, Gollwitzer et al. (2020) use geo-tracking data of 14 million smartphones finding that counties voting for Donald Trump over Hillary Clinton in the 2016's election engage in less physical distancing. Similarly, Grossman et al. (2020) found that state governments' leaders' recommendations to stay at home to reduce mobility were more effective in Democratic-leaning counties. Interestingly, stay-at-home recommendations by Republican governors reduced mobility in Democratic-leaning counties relatively more strongly than recommendations by a Democratic governor (see also Allcott et al. (2020) and Painter and Qiu (2021)). Gadarian et al. (2021) analyzed survey data from the early days of the pandemic in

March 2020, finding that Republicans were less likely to follow health guidelines, were less worried, but yet supported presidential proclamations to limit entry to the United States to a more considerable extent than Democrats. Pennycook et al. (2022) show that political conservatism is related to lower perceived risks of the virus (see also Bruine de Bruin et al. (2020) and Druckman et al. (2021a)), weaker adherence to mitigation behavior, and weaker vaccination intentions. Kerr et al. (2021) provide further evidence that liberals engaged in a more significant number of health-protective behaviors than conservatives and are more critical about the response by the government. Overall, our investigation of differences across political ideology confirms these results, offering evidence for polarized engagement in protective behavior and, to an even more significant extent, in the assessment of political crisis management. Thereby, effects of political ideology on the outcome variables are relatively independent of prosociality as both mediate the other only to a minor extent.

Studies focusing on the effect of prosociality on health protective behavior also find a positive and significant correlation, in line with our study (Campos-Mercade et al., 2021b; Müller and Rau, 2021; Syropoulos and Markowitz, 2021; Huynh, 2020; Cappelen et al., 2021; Chavarría et al., 2021; Jordan et al., 2021; Romano et al., 2021a; Thunström et al., 2021).[4] Campos-Mercade et al. (2021b) show that an experimental measure of prosociality can explain several dimensions of COVID-relevant health behavior (physical distancing, following stay home requirements, and face mask buying). Their measure is based on a game where other people can be put at risk for personal benefit, thus resembling the individual decision situation whether one follows public health guidelines that aim to reduce the spread of COVID-19. Based on another index of prosociality from a survey conducted two years before on a subgroup of the same broadly representative sample from Sweden, they find that prosociality is a stable long-term predictor of this behavior. Concordantly, Müller and Rau (2021) study whether non-monetarily incentivized survey measures of pre-crisis economic preferences and social responsibility in a student sample of 185 subjects can predict pandemic behavior and compliance with COVID-19 containment policies. They find that risk preferences are negatively related to physical distancing and panic buying while finding no significant association of protective behavior with measures of trust and honesty. However, a measure of social responsibility is positively related to physical distancing (Müller and Rau, 2021). More

[4]Chavarría et al. (2021) do not find predictive power of trust, risk, and time preferences on protective behavior, namely physical distancing, hygiene rules, and wearing of face masks, in Indonesia. Thunström et al. (2021) conduct a survey experiment to investigate COVID-19 testing behavior finding that people who have more contacts – potential supers-spreaders – are more inclined to do a costless test. A treatment increasing potential private costs of testing (due to an obligatory quarantine away from home in case of a positive result) does not affect testing behavior. The authors conclude that COVID-19 testing is a largely selfless behavior. Cappelen et al. (2021) show that priming respondents with information about the COVID-19 crisis affects preferences for redistribution.

generally, our study contributes to the strand of literature examining the ramifications of social preferences on real-life behavior (Levitt and List, 2007; Franzen and Pointner, 2013; Galizzi and Navarro-Martinez, 2019) and the relevance of polarization for a wide field of economic outcomes (Kuziemko et al., 2015; Alesina et al., 2021a). Our study is the first, to the best of our knowledge, to analyze the interplay between prosociality and ideology in their relationship with health-related behavior. We can thus assess the dependence of such factors and quantify the relative magnitude of behavioral and ideological differences in participants' responses to the pandemic.

Moreover, our use of a nationally representative sample innovates on existing research because previous studies on the determinants of health-protective behavior during COVID-19 used aggregated (mainly at the county-level) data (Allcott et al., 2020; Gollwitzer et al., 2020; Grossman et al., 2020; Jiang et al., 2020; Painter and Qiu, 2021) or non-representative samples (Pennycook et al., 2022; Jordan et al., 2021). Noteworthy exceptions are Bruine de Bruin et al. (2020) and Kerr et al. (2021), who study political polarization in representative samples but without using experimental measures of (prosocial) behavior. Moreover, studies linking social preferences to pandemic-related outcomes are mostly based on non-representative samples (Müller and Rau, 2021), solely rely on survey measures (Bruine de Bruin et al., 2020), or both (Huynh, 2020; Chavarría et al., 2021; Syropoulos and Markowitz, 2021). To the best of our knowledge, the only contribution utilizing experimentally-measured prosociality in a representative sample (of Sweden) and linking it to health behavior is Campos-Mercade et al. (2021b), who, however, do not investigate political polarization and perceptions of political crisis management, as the present study does. Hence, the joint investigation of political polarization and prosociality as determinants of several health-related and policy-relevant outcome measures connects two, so far detached, strands of the literature. The existence of prosocial individuals across the political spectrum and the responsiveness of such individuals to appropriately pitched public health messages can help inform future policy, provided that the environment is not too politicized for such messages to be heard. We return to policy relevance in the final section.

The remainder of this paper is structured as follows. Section 5.2 explains the design of the study and lays out our hypotheses. Section 5.3 outlines the results. Section 5.4 discusses the findings and concludes.

5.2 Study design and hypotheses

5.2.1 Design and data

Our analysis primarily draws from data of the second wave of the Trustlab initiative conducted in the United States (Murtin et al., 2018). This initiative combines large-scale incentivized economic experiments with a survey on a broad range of questions on the determinants of trust. The data collection of the second wave of the Trustlab started on the 12th of June 2020, when Corona cases and deaths in the U.S. were quickly growing and was completed on the 7th of September in the same year. The questionnaire of this second wave of the Trustlab captured a set of questions related to the COVID-19 pandemic which constitute our main variables of interest, ranging from self-reported (protective) behavior over worries about the spread in the local community to opinions about the political management of the crisis.[5]

The sample contains 1,120 participants and is approximately representative of the U.S. adult population in terms of the targeted dimensions of age, gender, and income. The other non-targeted characteristics shown in Table 5.3 in the Appendix are relatively close to the population values as well, including the political ideology dimension.[6] The Trustlab thus overcomes a frequent criticism of experimental approaches relying on, e.g., student samples (Cappelen et al., 2015). We retrieved additional data from several sources to control for variables related to the pandemic intensity and the political environment. To control for local and temporal infection rates, we matched the data from the Trustlab based on participants' ZIP codes[7] with COVID-19 statistics on cases and deaths from the New York Times[8] at the level of counties (where the Trustlab participants live) using the R package "covdata" (Healy, 2020).[9] We also matched the Trustlab data with data on general election results at the county level compiled

[5]See the Online Appendix Section 5.B.6 for details.

[6]We attempted to reach representativeness with respect to the U.S. adult population based on age, gender, and income criteria. As a result, the sample only marginally deviates from the U.S. population in the targeted dimensions. It contains slightly more females (55% vs. 52%), more persons in the age group between 45 and 64 years (37% vs. 33%), more people from the low-income bracket (43% vs. 40%), and fewer people from the high-income bracket (35% vs. 40%) than in the adult population. See Table 5.3 in the Appendix for details.

[7]Seven participants in the second wave of the Trustlab did not enter a valid ZIP code. We were able to recover the location of 6 of them by using an IP-based geolocation tool. The remaining participant is excluded from analyses controlling for geographical variables.

[8]The New York Times provides data on cumulative coronavirus cases and deaths at the county level. The five boroughs of New York City (the counties New York, Kings, Queens, Bronx, and Richmond) are aggregated to an artificial county, which we have to follow.

[9]The counties where participants live were obtained via the crosswalk between 5-digit ZIP codes and counties provided by the R package "zipcodeR" (Rozzi, 2021). We retrieved data on counties' total numbers of population which allows us to compute the number of cases and deaths per 100,000 inhabitants, and data on population density (population per square mile) from the United States Census Bureau (2021).

Table 5.1: Sample Characteristics

Variable	Mean	SD	Min	Max	N
Demographics					
Female	0.55	0.50	0	1	1120
Age	47.96	16.50	18	80	1120
Income					
Low income category	0.43	0.50	0	1	1120
Medium income category	0.22	0.42	0	1	1120
High income category	0.35	0.48	0	1	1120

Notes: Table shows means, standard deviations, minimum and maximum values, and number of observations for the Trustlab (second wave) sample characteristics. Female is a dummy for female sex. Age is the age in years. The medium income category is the third quintile. Low (high) income category refers to the two bottom (top) income quintiles.

by McGovern et al. (2020) and added indicators for whether the state‘s governor was a Democrat or a Republican at the time of the survey. Summary statistics for demographic characteristics are provided in Table 5.1.

Outcome variables. — This paper focuses on the correlational analysis of a set of COVID-19 related variables. More specifically, we explore the domain of protective behavior by two questions that asked participants whether they engaged in self-quarantine and how often they wore a face mask when going out. Another item asks whether they were worried about the spread in their local community. Finally, three questions on a 0-to-10 Likert scale focused on assessing the policy response to the pandemic. We asked participants to state whether the provision of adequate relief has been timely and efficient, where 0 was "Not at all timely and efficient" and 10 "Extremely timely and efficient." In two questions, we asked how respondents' trust in politicians evolved for handling the crisis, both at the state and the national level, where respondents could place their views between 0 ("Decreased"), 5 ("Stayed stable"), and 10 ("Increased").

Our main explanatory variables are an index of prosociality measured by economic games and self-reported political ideology. Furthermore, we control for a broad set of demographic and environmental variables such as gender and the local and temporal intensity of the COVID-19 pandemic measured by the reported number of deaths per 100,000 inhabitants at the level of counties between the day of the survey and seven days before the survey.

Prosociality. — The index of prosociality is based on the decisions in standard versions of the dictator game (DG) and the public goods game (PGG). In the DG, the participants had an initial endowment of 10 USD, of which they could transfer any share in multiples of 1 USD to another participant from the U.S. In the PGG, participants had an endowment of 10 USD and were informed that they played with three other participants who could transfer any share of their endowment into a joint project. The total amount of money transferred to the joint project would be multiplied by 1.6 and split equally between all 4 group members independent of their contribution. To construct the index of prosociality, we standardized both original variables, took the average, and standardized again. A Cronbach's alpha (calculated based on both standardized components) of 0.652 indicates appropriate index reliability (average interitem correlation = 0.483).

Political ideology. — Political ideology was measured by the question "In political matters, people often talk of 'Liberal' and 'Conservative.' Generally speaking, how would you place your views on this scale?" where participants could place themselves between 0 ("very liberal") and 10 ("very conservative").[10] To ensure a straightforward interpretation, we dichotomized the ideology scale. We categorized respondents who placed their ideology below or equal to 3 as "Liberals". Participants with a score of 7 or above were labeled as "Conservatives" and the rest as "Moderates."

The economic games were placed before the survey questions. There was no mention of the COVID-19 pandemic before the last module of the survey to minimize any repercussions of the COVID-19 crisis on the measurement of social preferences. The survey instruments and experiment instructions are available at https://osf.io/ebnm8.

5.2.2 Hypotheses

The data analyzed in this paper are part of a project aiming to compare prosociality and ingroup bias before and during the COVID-19 pandemic. We pre-registered hypotheses relative to this project at the AEA repository (AEARCTR-0005995). The hypotheses relative to the present paper only refer to the second wave of this project and have not been pre-registered. They are, however, straightforward inferences from existing theory and empirical evidence.

From the beginning of the pandemic, protective measures such as self-quarantining and wearing a face mask were linked to prosocial behavior (Betsch et al., 2020; van Bavel et al., 2020) as public messaging about them emphasized the protection they offered to others as well as to oneself (WHO, 2020a; CDC, 2021).[11] These measures, if

[10] Our results are equivalent using the scale "In political matters, people often talk of "the left" and "the right." How would you place your views on this scale, generally speaking?" where 0 is "left" and 10 is "right". However, this variable contains a larger number of missing values. Corresponding results using this alternative question are available upon request.

[11] Early WHO guidelines even advised against using masks for the general public because they could provide a false sense of security, whereas later guidelines recommended their use primarily to protect others (WHO, 2020a,b).

carefully followed, have the potential to strongly decrease the spread of the virus in a society (Mitze et al., 2020; Wellenius et al., 2021; Howard et al., 2021) but, undoubtedly, also imposed costs on individuals, like discomfort when wearing a face mask, and selfish people may be inclined to free-riding on other people's selfless behavior. The connection of prosociality with worrying about the virus' spread in the local community is likely multi-faceted. On the one hand, these worries can be purely selfish as one wants to protect one's health. On the other hand, we expect that this measure also captures motives related to caring about other people. Therefore, we expect that prosociality is positively associated with protective behavior and worrying about the spread in the local community. However, we do not predict the effect of prosociality on how respondents assess the political management of the crisis.

- *Hypothesis 1: Stronger prosociality is positively correlated with protective behavior and worries about the spread in participants' local communities.*

Political polarization in the United States has deepened over the last decades (Boxell et al., 2020; Pew Research Center, 2014). Ideological differences are remarkable over a wide range of (socio-economic) topics (Sterling et al., 2019) as well as in faith in science (Pittinsky, 2015; Jost et al., 2018). Deepened affective polarization has increased distrust between parties and hostility between political opponents (Iyengar et al., 2019). In the COVID-19 pandemic, Democrats and Republicans often use different information channels, likely resulting in diverging beliefs about its threat (Simonov et al., 2022). For instance, recent studies found strong polarization in how newspapers in the U.S. covered the COVID-19 pandemic (Hart et al., 2020; Motta et al., 2020). In addition, political leaders of both parties sent conflicting messages, likely influencing their supporters (Grossman et al., 2020). For instance, former President Donald Trump repeatedly downplayed the riskiness of COVID-19 (Yamey and Gonsalves, 2020). This attitude is consistent with previous research findings that Republicans perceive lower health risks from COVID-19 than Democrats (Dryhurst et al., 2020; Bruine de Bruin et al., 2020; Kerr et al., 2021). As such, we expect that political ideology is an important predictor of (protective) behavioral variables, worries about the spread in the local community, and the assessment of political crisis management.

- *Hypothesis 2a: Conservatives report lower adherence to self-quarantine and face mask wearing guidance than liberals.*

- *Hypothesis 2b: Conservatives worry less about the spread of the coronavirus in their local community than liberals.*

- *Hypothesis 2c: Due to the President-in-Power effect, conservatives assess the political management and relief provision by the government more positively than* *liberals, in particular at the national level and in states with Republican governors in 2020.*

5.3 Results

5.3.1 Descriptive statistics

Table 5.2 in the Appendix presents descriptive statistics for dependent and independent variables in our regression analysis. Histograms are provided in Section 5.B.3 of the Online Appendix.

A brief glimpse at our dependent variables indicates that the COVID-19 pandemic had a high impact on people's lives at the time of the survey. 79.9% of the participants reported to have engaged in self-quarantine to at least a limited extent and 81.6% stated that they often or always wear their face masks. 54.3% worried most of the time or always about the spread of COVID-19 in their local community. From the whole sample perspective, the assessment of how the political elite managed the crisis seems to be relatively neutral on average. The mean score of the 0-to-10 Likert scale variable for the assessment of the relief provided by the government lies slightly to the left of the center with a mean value of 4.81 (sd = 3.2). On average, participants stated a higher score in the question asking for the evolution of trust in politicians (whether it increased, stayed stable, or decreased for the handling of the crisis) at the state-level (mean = 5.3, sd = 2.9) than at the national level (mean = 4.3, sd = 3.1) ($p < 0.001$, two-sided t-test).

On average, participants sent almost half of their 10 USD endowment in the DG (mean = 4.9, sd = 2.9) and contributed roughly 61 percent of their endowment to the common project in the PGG (mean = 6.1, sd = 3.2). In both games, sending half of the endowment is the modal choice, and only a small share of people keep everything for themselves (9.1 percent in the DG and 5.1 percent in the PGG).

A potential concern is that prosociality itself might have been significantly affected by the COVID-19 pandemic (Campos-Mercade et al., 2021b; Cappelen et al., 2021; Grimalda et al., 2021; Terrier et al., 2021). We address this concern by comparing the prosociality index in the second wave to the index based on the same experimental decisions from the first wave of the Trustlab conducted in 2017. We find that the prosociality index is only marginally larger in the second wave than in the first (pre-COVID) wave ($p = 0.265$, two-sided t-test). Furthermore, measures of the objective pandemic intensity at the county level as the total number of cases and deaths up to the survey and the contemporary intensity around the survey do not significantly correlate with the prosociality index, nor its components based on behavior in the experimental games (see Table 5.17 in the Online Appendix).

Reported income changes because of COVID-19 and expectations about the financial situation of the participants' households indicate a considerable level of economic instability affecting the survey respondents' lives. 38.8% of the participants report that they lost income during the COVID-19 pandemic whereas 53.3% of the sample report that their income stayed stable. On average, expectations about the household income in the next year are worse "now that the COVID-19 pandemic has arrived" than the value when asking them for their expectations "prior to the COVID-19 pandemic" ($p < 0.001$, two-sided t-test). According to the political ideology scale, our sample mean is slightly leaning toward conservatism (mean = 5.6, sd = 2.9).[12] Applying the dichotomization to simplify the interpretation of results, 24.5 (40.7) percent of the sample are counted as liberals (conservatives).

5.3.2 Main results

We address our hypotheses through a linear regression model having as dependent variable each of the outcome variables described in Section 5.2.1. Each regression includes the prosociality, liberal, and conservative ideology variables, a set of control variables, and a constant. We control for the participant's age in years, the age-squared, a dummy for the female gender, dummies for ethnic groups (African Americans, Hispanics, and other ethnicities, relative to white ethnicity) dummies for the high (low) income categories defined as the top (bottom) two quintiles of household income, dummies for medium (vocational education or community college degree) and high education (University degree), two dummies for the highest education level attained by participants' parents, a dummy for parents being immigrants, two dummies for urbanization categories (town and city, relative to rural), and the date of the survey. We further control for the natural logarithm of one plus the number of deaths per 100,000 inhabitants between the day of the survey and 7 days before the survey in the participant's county as a measure of the current intensity of the pandemic, the natural logarithm of the county's total population, and the natural logarithm of the population density of the respondent's county of residence (people per square mile).

Figure 5.1 depicts OLS regression coefficients and their 95 percent confidence intervals for the prosociality index, liberal political ideology, and conservative political ideology. The dependent and explanatory variables are standardized, except for the political ideology indicator variables, which distinguish "conservatives" and "liberals"

[12]Similarly, the alternative scale of political orientation between 0 (left) and 10 (right) has a mean value of 6.06 with a standard deviation of 2.92.

from one another and the omitted "moderates" category. In the following, coefficients named b (β) indicate standardization of the dependent (and explanatory) variable. Standard errors are clustered at the state level.

Effects of prosociality. — Beginning with the prosociality index, we note a statistically significant, positive association with all the dependent variables. A one standard deviation increase in prosociality is associated with a 0.11 standard deviation increase in engagement in self-quarantine ($\beta = 0.11$, $p = 0.001$). The effect magnitudes are comparable for wearing face masks ($\beta = 0.09$, $p = 0.004$) and worrying about the virus' spread in the local community ($\beta = 0.12$, $p = 0.001$), thus supporting our first hypothesis. There is also a positive and statistically significant correlation of prosociality with the assessment of political crisis management, i.e., respondents who are more prosocial, ceteris paribus, report higher satisfaction with official crisis response (see Table 5.8 for the underlying regressions).

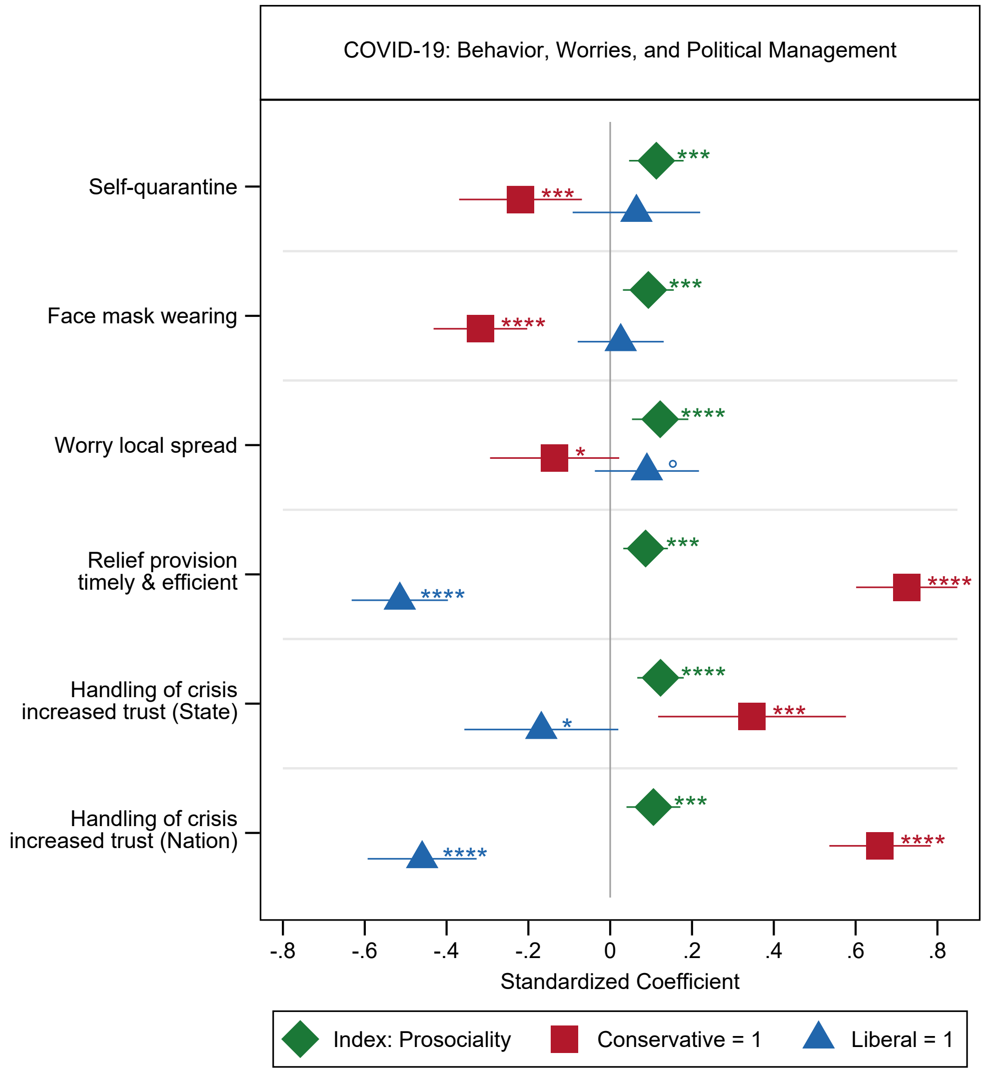
- Result 1: Prosociality is positively associated with protective behavior and worrying about the local spread of COVID-19 (Hypothesis 1 confirmed).

Effects of political ideology. — Along the political ideology scale, we observe strong signs of polarization. Consistent with Hypotheses 2a, conservatives report significantly lower engagement in self-quarantine ($b = -0.22$, $p = 0.005$) and wearing of face masks ($b = -0.32$, $p < 0.001$) than moderates, with the differences relative to liberals being also statistically significant ($p < 0.001$ for both dependent variables, Wald tests). The effect of conservative ideology on worrying about the local spread only reaches marginal statistical significance relative to moderates ($b = -0.14$, $p = 0.090$) but statistical significance at the 5 percent level relative to liberals ($p = 0.023$, Wald test), hence overall supporting Hypothesis 2b. Liberal ideology does not reveal any significant effect relative to moderate ideology for the three items.

- Result 2: Conservatives report lower levels of protective behavior (self-quarantine, wearing of face maks) and worry less about the spread in their local community than liberals (Hypotheses 2a and 2b confirmed).

The differences between political camps are most strongly pronounced in the realm of questions related to the political management of the crisis. Liberals assess the relief provided by the government as being significantly less timely and efficient than moderates ($b = -0.51$, $p < 0.001$). Conservatives assess the relief as significantly more timely and efficient than moderates ($b = 0.73$, $p < 0.001$). The difference of 1.2 standard deviations between liberals and conservatives is highly significant ($p < 0.001$, Wald test). The results are very similar concerning the evolution of trust in politicians (whether it increased, decreased, or stayed stable) at the national level. Liberals (conservatives)

Figure 5.1: Regression Coefficients of Core Variables



Notes: This figure shows OLS regression coefficients and 95 percent confidence intervals (standard errors clustered at the state level) for the set of explanatory variables listed in the legend. The dependent variables are indicated on the y-axis. All regressions include individual-level controls for age, sex, ethnicity, education, income, and urbanization level at the respondents' places of residence. Environmental and situational controls for the natural logarithm of the county population and population density (people per square mile) where the respondent resides, survey date, and the natural logarithm of one plus the number of deaths due to COVID-19 per 100k inhabitants between the day of the survey and 7 days before the survey at the county level. (****, ***, **, *, °) indicate two-sided p-values below 0.001, 0.01, 0.05, 0.1, and 0.2, respectively.

score significantly lower (higher) than moderates (b = -0.46, $p < 0.001$, b = 0.66, $p < 0.001$, respectively).

These results mean that differences between liberals and conservatives are up to roughly four (five) times as large when assessing the government's crisis management as when reporting their behavior (worry) in response to the crisis. Differences in the dependent variables between conservatives and liberals are 0.28 of a standard deviation for self-quarantine, 0.23 for worries about the local spread, and 1.24 in assessing the relief provided by the government (see Table 5.8). In other words, liberals and conservatives appear much closer in their behavior than in their opinions about the government.[13]

- Result 3: Conservatives and liberals differ strongly in their assessment of political crisis management, with the conservatives being more positive (Hypothesis 2c confirmed). Differences between conservatives and liberals in their assessment of politics are larger than in the measures of protective behavior and worrying.[14]

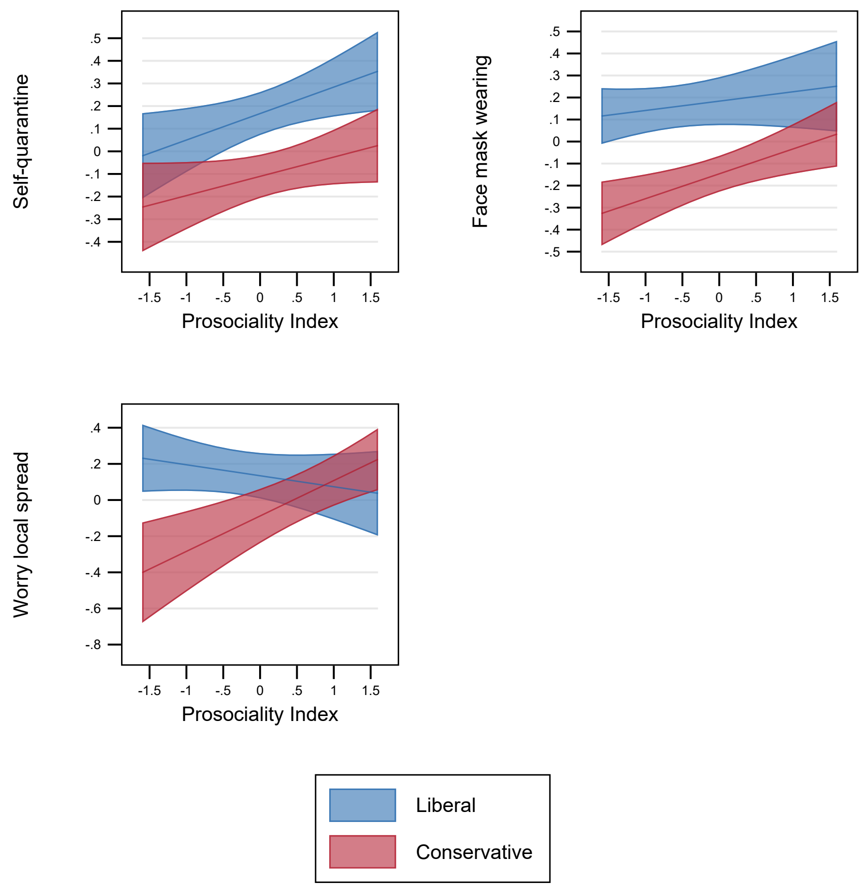
We further show (in Table 5.15 in the Online Appendix) that the effects of prosociality and political ideology are each only marginally mediated by the presence of the other in the regression model, suggesting that both are relatively independent in their explanatory power on the outcome variables. In particular, the differences in protective behavior between liberals and conservatives do not seem to be driven by differences in prosociality. We also tested for moderation effects (see Figure 5.2 and Table 5.16 in the Online Appendix) by adding interactions of the prosociality index with ideology dummies. The analyses do not reveal statistically significant differences between ideologies in the effect of prosociality on protective behavior. The only statistically significant heterogeneity concerning our main outcome variables is that conservatives entirely drive the positive effect of prosociality on worrying about the local spread (b = -0.06 vs. b = 0.20, for liberals and conservatives, respectively, p = 0.004 for the difference between liberals and conservatives).

As a matter of fact, conservatives turn out to be more prosocial than liberals (see Table 5.18 in the Online Appendix). This unexpected result seems to be a persistent characteristic of the Trustlab samples in the two waves run in the US. The first wave was

[13] As shown in Online Appendix Table 5.13, conservatives report a significantly better evolution of trust in politicians at the state level than liberals when the state's governor in 2020 was from the Republican party and also report a more positive assessment of the relief provided by the government, further supporting Hypothesis 2c. Furthermore, liberals (conservatives) living in a state ruled by a Republican governor assessed the trust development at the state level much more negatively (positively) than liberals (conservatives) in a Democratic-ruled state.

[14] The third result also holds for the evolution of trust in politicians at the state level. Conservatives report a significantly more positive evolution of trust in politicians than moderates (b = 0.35, p = 0.004). Liberals report a slightly more negative score than moderates (b = -0.17, p = 0.078), with the difference relative to conservatives being highly significant (p = 0.003, Wald test).

Figure 5.2: Effect of Prosociality by Political Ideology



Notes: This figure illustrates the (absence) of significant impact of political ideology on the effect of prosociality on self-quarantine, face mask wearing, and worrying about the local spread of the virus. Depicted is the linear prediction of the outcome variables related to protective behavior and worries for different values of the prosociality index, separately for liberal and conservative ideologies. The regression specification underlying the prediction is the same as in the main results figure.

run in 2017 on a representative sample of the US population along the same variables targeted in the second wave, i.e., age, gender, and income. Because the question on ideology was only part of the second wave, we cannot compare the two waves on the ideology spectrum. However, we can use a question on political orientation which is available in both waves. Political orientation was measured through a 0-to-10 Likert scale, asking people how close they were to the "left" (0) or "right" (10) of the political spectrum, where we classified respondents scoring three or below (7 or above) as left-wing (right-wing). We find that this "left/right" variable correlates strongly with the "liberal/conservative" variable in the second wave (Pearson's $r = 0.81$, $p < 0.001$). It turns out that right-wing respondents were more prosocial than left-wing respondents in both waves (see Table 5.18 in the Online Appendix). This result contrasts with previous research (van Lange et al., 2012; Solon, 2014; Grünhage and Reuter, 2021; Romano et al., 2021b), suggesting that the relationship between political or ideological orientation and prosociality may not be as clearcut as expected.

5.3.3 Robustness checks

Our main results are robust to different model specifications and estimators.[15] We show that our results are qualitatively equivalent in terms of the direction of effects and their statistical significance applying ordered logit regressions (see Online Appendix Table 5.14) as well as when only the core variables prosociality and political ideology are included in the regression model (see Online Appendix Table 5.9). Figures 5.10 and 5.11 in the Online Appendix depict the means of the dependent variables for each quartile of the core explanatory variables alongside a linear fit of the dependent variables on the explanatory variables, illustrating the correlational relationship, e.g. that higher levels of prosociality are associated with more self-reported engagement in self-quarantining. Table 5.6 in the Online Appendix further shows that our sample still closely resembles the characteristics of the U.S. adult population when restricted to complete observations for the variables considered in the regression analyses.

The data allows us to show that our results are stable using an alternative measure of prosociality. Participants in the Trustlab were asked whether they would like to donate any part of their earnings to UNICEF. As earnings are not equal across participants, we use the share of earnings that participants chose to donate. The average share donated was 0.31 (sd = 0.41, N = 685).[16] Consistent with our previous results, voluntary donations show positive correlations that are statistically significant on self-quarantining

[15] In the Online Appendix, we also show in Section 5.B.1 that potential experimenter demand effects are unlikely to have had an influence on our results.

[16] The voluntary donation variable has 485 missing values. Results are equivalent and remain statistically significant coding missing values as zero donations.

($\beta = 0.07$, $p = 0.025$), wearing of face masks ($\beta = 0.12$, $p = 0.003$), and worries about the local spread ($\beta = 0.15$, $p < 0.001$).

We also show that the effect of prosociality on self-quarantining, wearing of face masks, and worries about the local spread is partially mediated when controlling for contextual factors, i.e. self-reported vulnerability and worrying about getting infected (see Table 5.12). Self-reported vulnerability to COVID-19 is strongly correlated with self-quarantine engagement ($\beta = 0.19$, $p < 0.001$), wearing of face masks ($\beta = 0.22$, $p < 0.001$), and worries about the local spread ($\beta = 0.20$, $p < 0.001$). While worries about getting infected show no statistically significant effect on self-quarantine behavior ($\beta = 0.03$, $p = 0.442$), it strongly correlates with wearing of face masks ($\beta = 0.26$, $p < 0.001$) and worries about the local spread ($\beta = 0.66$, $p < 0.001$). Nevertheless, even after controlling for both items, prosociality remains a (largely) statistically significant predictor for the outcome variables of self-quarantine behavior, wearing of face masks, and worries about the local spread ($p = 0.007$, $p = 0.122$, and $p = 0.012$, respectively). Similarly, differences between liberals and conservatives in all outcome variables remain statistically significant ($p < 0.01$ for all outcome variables, see Table 5.12) after controlling for self-reported vulnerability and worrying about getting infected. This finding further strengthens the interpretation of differences resulting from an underlying profound ideological polarization.

As we are only reporting correlations which do not allow us to uncover causal pathways between our measures, it is beyond the scope of this paper to explain the interdependencies of prosociality with worries about infection and vulnerability. However, the fact that prosociality is itself positively correlated with self-reported vulnerability (Pearson's $r = 0.11$, $p < 0.001$) and worries about getting infected with COVID-19 (Pearson's $r = 0.10$, $p < 0.001$) may indicate that prosocial individuals have higher levels of health awareness. Prosociality may potentially be related to fears about getting infected due to considering the risk of transmitting the virus to other people.

Finally, the number of reported COVID-19 deaths per 100,000 inhabitants at the county level during the days around the survey does not significantly affect any of our outcome variables (see regressions in the Online Appendix Table 5.10). The same holds for using the number of cases during the last seven days and for the total number of cases or deaths per 100,000 inhabitants at the county level up to the survey date.[17]

5.3.4 Further results on gender and ethnicity

Previous research found that the adherence to protective behavior and perceptions of the COVID-19 pandemic differed along the line of demographic characteristics, e.g.

[17] The regression results using these alternative variables are available upon request.

between males and females (Pedersen and Favero, 2020; Zickfeld et al., 2020; Alsharawy et al., 2021; Zettler et al., 2022). In regressions with only demographic and county-level variables included, we find that women engaged in more self-quarantine (b = 0.11, p = 0.084) and stated more frequently wearing their face masks (b = 0.15, p = 0.028) than men (see Table 5.9). Interestingly, the gender difference becomes insignificant when adding the indicators for liberal and conservative political ideology, suggesting that a substantial part of the difference may be attributable to ideology. In fact, women in our survey are significantly less conservative than men as measured on the 0-to-10 Likert scale (MD = -0.87, $p < 0.001$, N = 1041).

When we add controls for prosociality and political ideology, there is likewise no gender difference in self-quarantine, wearing a face mask and the degree to which women or men worry about the spread in the local community. However, even controlling for prosociality and political ideology, the assessment of the political performance by female respondents remains statistically significantly less positive than that of male respondents (b = -0.15, p = 0.002 for the question on relief provision; b = -0.16, p = 0.022 for the evolution of trust at the state-level; and b = -0.25, $p < 0.001$ at the national level, respectively, see Table 5.8). The finding of a significant gender difference in the assessment of political performance but an insignificant one for behavior like self-quarantine and mask wearing runs parallel to our result that Americans from opposed ideological camps differed more in political views than in their protective behavior concerning the pandemic.

Furthermore, African American participants report (relative to Whites) being less engaged in self-quarantine (b = -0.29, p = 0.054) but wearing their face masks more often (b = 0.27, p = 0.006). This result may be due to African Americans being more likely to be active in occupations that cannot be conducted from home (Almagro and Orane-Hutchinson, 2020). African Americans report a lower degree of satisfaction with the relief provided by the government than do Whites (b = -0.25, p = 0.005) and report a less positive evolution of trust in politicians due to the crisis management ($p < 0.01$) (see Table 5.8).[18]

5.4 Concluding remarks

This study uses data from a large-scale online experiment run on a representative sample of the U.S. population during the summer of 2020. We examined differences between those on opposite ends of the ideological spectrum alongside correlations of prosociality

[18] All ethnicity-related results we report are from regressions underlying Figure 5.1 controlling for prosociality and political ideology.

measured in experimental economic games with COVID-19-related behavior, worries about the pandemic, and assessing the pandemic‘s political management.

We document profound polarization, which has been trending upward in the U.S. for a long time (Iyengar et al., 2019), between liberals and conservatives. By looking simultaneously at attitudes towards the political handling of the COVID-19 crisis and the behavioral response, we are the first to show that both extremes of the ideological spectrum are substantially more polarized in their judgments of the political efforts to manage the crisis than in their reported behavior and worries. Respondents who report being relatively more conservative on the political ideology scale indicate that they are less engaged in self-quarantine, wear their face masks less often than liberals and worry less about the local spread of the virus. While differences between liberals and conservatives range between 0.2 standard deviations and 0.35 standard deviations for the variables measuring protective behavior and worries about the virus's local spread, the polarization is considerably more substantial when assessing political crisis management. E.g., the difference between liberals and conservatives concerning the question of whether trust in politicians at the national level increased for their handling of the crisis is roughly 1.1 standard deviations. This finding may suggest that Donald Trump's divisive communication style, not intended to foster solidarity above party lines (Hatcher, 2020), had more potent polarizing effects on political opinions than on actual behavior during the first summer of the COVID-19 pandemic.

Consistent with previous studies, we find that prosociality is positively correlated with protective behavior and worrying about the virus' spread in respondents' local communities, suggesting that prosocial behavior in economic games is related to caring more about others in the real world (Levitt and List, 2007). An important difference to previous studies on the topic of COVID-19 related behavior and prosociality (Campos-Mercade et al., 2021b; Müller and Rau, 2021; Dinić and Bodroža, 2021a) is that we use standard versions of experimental games to construct the index of prosciality. We can thus pin down real-life prosocial behavior to standard measures of prosociality, ruling out additional effects related to risk tolerance. Although the standard dictator and public goods games come without a possibility of imposing risks on others or any reference to health issues, we nevertheless find a statistically significant correlation with behavior and worries about the COVID-19 pandemic, supporting findings from previous studies. Our result also holds for end-of-survey charitable donation decisions.

An essential contribution of our study is that our data enable us to investigate the role of political ideology and prosociality jointly. In contrast, previous studies largely focused on one factor at a time, were often based on non-representative samples, or relied on geographically-aggregated data (see the discussion of the literature in the introduction). While we replicate the patterns of existing research, we show that each factor correlates with protective behavior in its own way and that the effect of neither significantly mediates that of the other. This finding suggests profound differences between liberal and conservative ideologies concerning following behavioral guidelines intended to control the pandemic, which cannot be attributed to differences in prosocial behavior. This result is underpinned by the fact that we do not observe any significant difference in the way prosociality affects protective behavior in liberals versus in conservatives.

We believe our results can help inform the political debate on current troubles in fighting the ongoing pandemic. A recent GALLUP poll finds that one-third of Americans think the pandemic is over, about the same share as one year ago (GALLUP, 2022). Interestingly, the partisan gap is also virtually constant compared to 2021, as two-thirds of Republicans believe the pandemic is over, whereas only one out of ten Democrats thinks so. Despite effective vaccines being available in the industrialized countries, cases of COVID-19 are on very high levels again in the U.S. and many other countries, with weekly death tolls in summer 2022 so far topping those from 2021 (Our World in Data, 2022).

Apart from more transmissible virus variants, plateaued vaccine uptake and relatively careless behavior by large parts of the population have contributed to the new surges.[19] Recent studies suggest that prosociality not only strengthens people's adherence to protective behavior but is also related to COVID-19 vaccination intentions (Campos-Mercade et al., 2021a; Yu et al., 2021; Lindholt et al., 2021; Jørgensen et al., 2021), which further amplifies the need to promote prosocial behavior by calling on people's prosociality, promoting altruistic and cooperative behavior (van Bavel et al., 2020).[20] Political polarization, however, impedes the efforts to suppress the virus' spread. Hence, the documented polarization, which we demonstrate to be somewhat independent of prosociality, might present unexploited potential that should be used to improve the response to the pandemic in the U.S. For this, policymakers must find strategies complementing the calls on people's prosociality to attenuate partisan gaps. Valuable starting points could be priming American national identity (Levendusky, 2018), encouraging inter-partisan dialogues fostering mutual acceptance (Warner and Villamil, 2017; Wojcieszak and Warner, 2020), correcting highly prevalent misperceptions of the other party (Ahler and Sood, 2018; Druckman et al., 2022), making the benefits of cooperation between liberals and conservatives more salient - for instance, by fostering the sense of a shared fate or common values (Gaertner et al., 1993; Charness et al., 2007; Gelfand et al., 2011; Theiss-Morse et al., 2018; Carothers and O'Donohue,

[19] Andersson et al. (2021) argue that anticipation of vaccine availability even led to lower adherence to protective behavior, increasing the spread of COVD-19.

[20] See Böhm and Betsch (2021) for a review on the topic of prosocial vaccination.

2019) as well as efforts to increase faith in science and health protection agencies (Algan et al., 2021).

Promoting protective behavior will likely remain necessary for a longer time to avoid stress on the healthcare system, given that substantial shares of the population are not willing to be (repeatedly) vaccinated, the difficulty of updating vaccines, or even developing pan-coronavirus vaccines with universal protection against (upcoming) variants not least due to approval processes not suited for swift reactions during a pandemic (Callaway, 2022; Launay et al., 2022; Sun et al., 2022), and the patchiness of protective immunity acquired from natural infections (Cacciapaglia et al., 2021; Reynolds et al., 2022). Suppressing the virus' spread is also needed to protect the vulnerable. These include people who either cannot be vaccinated or for which the vaccines do not offer the same level of protection, e.g., immunocompromised people and ones with other underlying health conditions. Furthermore, growing evidence suggests that post-infectious sequelae such as organ damage and immunological disorders are not rare and affect people presenting with seemingly benign courses of acute COVID-19 (Lopez-Leon et al., 2021; Hamdy and Leonardi, 2022; Mehandru and Merad, 2022; Phetsouphanh et al., 2022; Xie et al., 2022). This might increase the necessity to maintain or even reintroduce non-pharmaceutical interventions in the future. Our findings may prove helpful in understanding where and when policy interventions can tap into prosociality despite the presence of ideological polarization.

References

Ahler, Douglas J. and Gaurav Sood. 2018. "The parties in our heads: Misperceptions about party composition and their consequences." *The Journal of Politics*, 80 (3): 964–981.

Akrami, Nazar, Bo Ekehammar, Robin Bergh, Elisabet Dahlstrand, and Sanna Malmsten. 2009. "Prejudice: The person in the situation." *Journal of Research in Personality*, 43 (5): 890–897.

Alesina, Alberto, Matteo Ferroni, and Stefanie Stantcheva. 2021a. "Perceptions of racial gaps, their causes, and ways to reduce them." *NBER Working Paper*, 29245.

Algan, Yann, Daniel Cohen, Eva Davoine, Martial Foucault, and Stefanie Stantcheva. 2021. "Trust in scientists in times of pandemic: Panel evidence from 12 countries." *Proceedings of the National Academy of Sciences of the United States of America*, 118 (40): e2108576118.

Allcott, Hunt, Levi Boxell, Jacob Conway, Matthew Gentzkow, Michael Thaler, and David Yang. 2020. "Polarization and public health: Partisan differences in social distancing during the coronavirus pandemic." *Journal of Public Economics*, 191 (0): 104254.

Almagro, Milena and Angelo Orane-Hutchinson. 2020. "JUE Insight: The determinants of the differential exposure to COVID-19 in New York city and their evolution over time." *Journal of Urban Economics*, 107 (3): 103293.

Alsharawy, Abdelaziz, Ross Spoon, Alec Smith, and Sheryl Ball. 2021. "Gender Differences in Fear and Risk Perception During the COVID-19 Pandemic." *Frontiers in Psychology*, 12 (0): 689467.

Andersson, Ola, Pol Campos-Mercade, Armando N. Meier, and Erik Wengström. 2021. "Anticipation of COVID-19 vaccines reduces willingness to socially distance." *Journal of Health Economics*, 86 (11): 102530.

Barberá, Pablo, John T. Jost, Jonathan Nagler, Joshua A. Tucker, and Richard Bonneau. 2015. "Tweeting from left to right: Is online political communication more than an echo chamber?" *Psychological Science*, 26 (10): 1531–1542.

Bargain, Olivier and Ulugbek Aminjonov. 2020. "Trust and compliance to public health policies in times of COVID-19." *Journal of Public Economics*, 192 104316.

Betsch, Cornelia, Lars Korn, Philipp Sprengholz, Lisa Felgendreff, Sarah Eitze, Philipp Schmid, and Robert Böhm. 2020. "Social and behavioral consequences of mask policies during the COVID-19 pandemic." *Proceedings of the National Academy of Sciences of the United States of America*, 117 (36): 21851–21853.

Bobba, Giuliano and Nicolas Hubé. eds. 2021. *Populism and the politicization of the COVID-19 crisis in Europe*. Cham: Springer International Publishing.

Böhm, Robert and Cornelia Betsch. 2021. "Prosocial vaccination." *Current Opinion in Psychology*, 43 (0): 307–311.

Boxell, Levi, Matthew Gentzkow, and Jesse Shapiro. 2020. "Cross-country trends in affective polarization." *NBER Working Paper*, 26669.

Bruine de Bruin, Wändi, Htay-Wah Saw, and Dana P. Goldman. 2020. "Political polarization in US residents' COVID-19 risk perceptions, policy preferences, and protective behaviors." *Journal of Risk and Uncertainty*, 61 (2): 177–194.

Bursztyn, Leonardo, Aakaash Rao, Christopher Roth, and David Yanagizawa-Drott. 2020. “Misinformation during a pandemic.” *NBER Working Paper*, 27417.

Cacciapaglia, Giacomo, Corentin Cot, and Francesco Sannino. 2021. “Multiwave pandemic dynamics explained: how to tame the next wave of infectious diseases.” *Scientific Reports*, 11 (1): 6638.

Callaway, Ewen. 2022. “Fast-evolving COVID variants complicate vaccine updates.” *Nature*, 607 (7917): 18–19.

Campos-Mercade, Pol, Armando N. Meier, Florian H. Schneider, Stephan Meier, Devin Pope, and Erik Wengström. 2021a. “Monetary incentives increase COVID-19 vaccinations.” *Science (New York, N.Y.)*, 374 (6569): 879–882.

Campos-Mercade, Pol, Armando N. Meier, Florian H. Schneider, and Erik Wengström. 2021b. “Prosociality predicts health behaviors during the COVID-19 pandemic.” *Journal of Public Economics*, 195 (0): 104367.

Cappelen, Alexander W., Ranveig Falch, Erik Ø. Sørensen, and Bertil Tungodden. 2021. “Solidarity and fairness in times of crisis.” *Journal of Economic Behavior & Organization*, 186 (2): 1–11.

Cappelen, Alexander W., Knut Nygaard, Erik Ø. Sørensen, and Bertil Tungodden. 2015. “Social preferences in the lab: A comparison of students and a representative population.” *The Scandinavian Journal of Economics*, 117 (4): 1306–1326.

Caprara, Gian Vittorio, Shalom Schwartz, Cristina Capanna, Michele Vecchione, and Claudio Barbaranelli. 2006. “Personality and politics: Values, traits, and political choice.” *Political Psychology*, 27 (1): 1–28.

Carothers, Thomas and Andrew O’Donohue. 2019. *Democracies divided: The global challenge of political polarization*. Washington, D.C: Brookings Institution Press.

CDC. 2021. “Guidance for wearing masks: Help slow the spread of COVID-19.” `https://www.cdc.gov/coronavirus/2019-ncov/prevent-getting-sick/cloth-face-cover-guidance.html`.

Charness, Gary, Luca Rigotti, and Aldo Rustichini. 2007. “Individual behavior and group membership.” *American Economic Review*, 97 (4): 1340–1352.

Chavarría, Eliana, Farah Diba, Maja E. Marcus, Marthoenis Marthoenis, Anna Reuter, Lisa Rogge, and Sebastian Vollmer. 2021. "Knowing versus doing: Protective health behaviour against COVID-19 in Aceh, Indonesia." *The Journal of Development Studies*, 57 (8): 1245–1266.

Deary, Ian J., G. David Batty, and Catharine R. Gale. 2008. "Bright children become enlightened adults." *Psychological Science*, 19 (1): 1–6.

Dinić, Bojana M. and Bojana Bodroža. 2021a. "COVID-19 protective behaviors are forms of prosocial and unselfish behaviors." *Frontiers in Psychology*, 12 (0): 647710.

Druckman, James N., Samara Klar, Yanna Krupnikov, Matthew Levendusky, and John Barry Ryan. 2021a. "Affective polarization, local contexts and public opinion in America." *Nature Human Behaviour*, 5 (1): 28–38.

Druckman, James N., Samara Klar, Yanna Krupnikov, Matthew Levendusky, and John Barry Ryan. 2022. "(Mis)estimating affective polarization." *The Journal of Politics*, 84 (2): 1106–1117.

Dryhurst, Sarah, Claudia R. Schneider, John Kerr, Alexandra L. J. Freeman, Gabriel Recchia, Anne Marthe van der Bles, David Spiegelhalter, and Sander van der Linden. 2020. "Risk perceptions of COVID-19 around the world." *Journal of Risk Research*, 23 (7-8): 994–1006.

Falco, Paolo and Sarah Zaccagni. 2021. "Promoting social distancing in a pandemic: Beyond good intentions." *PloS one*, 16 (12): e0260457.

Foa, Roberto Stefan and Yascha Mounk. 2017. "The signs of deconsolidation." *Journal of Democracy*, 28 (1): 5–15.

Foa, Roberto Stefan and Yascha Mounk. 2021. "America after Trump: From "clean" to "dirty" democracy?" *Policy Studies*, 42 (5-6): 455–472.

Franzen, Axel and Sonja Pointner. 2013. "The external validity of giving in the dictator game." *Experimental Economics*, 16 (2): 155–169.

Gadarian, Shana Kushner, Sara W. Goodman, and Thomas B. Pepinsky. 2021. "Partisanship, health behavior, and policy attitudes in the early stages of the COVID-19 pandemic." *PloS one*, 16 (4): e0249596.

Gaertner, Samuel L., John F. Dovidio, Phyllis A. Anastasio, Betty A. Bachman, and Mary C. Rust. 1993. "The common ingroup identity model:

Recategorization and the reduction of intergroup bias." *European Review of Social Psychology*, 4 (1): 1–26.

Galizzi, Matteo M. and Daniel Navarro-Martinez. 2019. "On the external validity of social preference games: A systematic lab-field study." *Management Science*, 65 (3): 976–1002.

GALLUP. 2022. "Pandemic outlook recovers as U.S. leaders' messaging falters." https://news.gallup.com/poll/404696/pandemic-outlook-recovers-leaders-messaging-falters.aspx.

Gelfand, Michele J., Jana L. Raver, Lisa Nishii, Lisa M. Leslie, Janetta Lun, Beng Chong Lim, Lili Duan, Assaf Almaliach, Soon Ang, Jakobina Arnadottir, Zeynep Aycan, Klaus Boehnke, Pawel Boski, Rosa Cabecinhas, Darius Chan, Jagdeep Chhokar, Alessia D'Amato, Montse Ferrer, Iris C. Fischlmayr, Ronald Fischer et al. 2011. "Differences between tight and loose cultures: a 33-nation study." *Science*, 332 (6033): 1100–1104.

Gidron, Noam, James Adams, and Will Horne. 2020. *American affective polarization in comparative perspective.* Cambridge University Press.

Gollwitzer, Anton, Cameron Martel, William J. Brady, Philip Pärnamets, Isaac G. Freedman, Eric D. Knowles, and Jay J. van Bavel. 2020. "Partisan differences in physical distancing are linked to health outcomes during the COVID-19 pandemic." *Nature Human Behaviour*, 4 (11): 1186–1197.

Grimalda, Gianluca, Nancy R. Buchan, Orgul D. Ozturk, Adriana C. Pinate, Giulia Urso, and Marilynn B. Brewer. 2021. "Exposure to COVID-19 is associated with increased altruism, particularly at the local level." *Scientific Reports*, 11 (1): 18950.

Grossman, Guy, Soojong Kim, Jonah M. Rexer, and Harsha Thirumurthy. 2020. "Political partisanship influences behavioral responses to governors' recommendations for COVID-19 prevention in the United States." *Proceedings of the National Academy of Sciences of the United States of America*, 117 (39): 24144–24153.

Grünhage, Thomas and Martin Reuter. 2021. "Tell me who you vote for, and I'll tell you who you are? The associations of political orientation with personality and prosocial behavior and the plausibility of evolutionary approaches." *Frontiers in Psychology*, 12 (2): 656725.

Grünhage, Thomas and Martin Reuter. 2022. "Political orientation is associated with behavior in public-goods- and trust-games." *Political Behavior*, 44 (1): 23–48.

Hamdy, Adam and Anthony Leonardi. 2022. "Superantigens and SARS-CoV-2." *Pathogens (Basel, Switzerland)*, 11 (4): 1–11.

Hart, P. Sol, Sedona Chinn, and Stuart Soroka. 2020. "Politicization and polarization in COVID-19 news coverage." *Science Communication*, 42 (5): 679–697.

Hatcher, William. 2020. "A failure of political communication not a failure of nureaucracy: The danger of presidential misinformation during the COVID-19 pandemic." *The American Review of Public Administration*, 50 (6-7): 614–620.

Healy, Kieran. 2020. "covdata: COVID-19 case and mortality time series." `http://kjhealy.github.io/covdata`.

Howard, Jeremy, Austin Huang, Zhiyuan Li, Zeynep Tufekci, Vladimir Zdimal, Helene-Mari van der Westhuizen, Arne von Delft, Amy Price, Lex Fridman, Lei-Han Tang, Viola Tang, Gregory L. Watson, Christina E. Bax, Reshama Shaikh, Frederik Questier, Danny Hernandez, Larry F. Chu, Christina M. Ramirez, and Anne W. Rimoin. 2021. "An evidence review of face masks against COVID-19." *Proceedings of the National Academy of Sciences of the United States of America*, 118 (4): 1–12.

Huynh, Toan L. D. 2020. ""If you wear a mask, then you must know how to use it and dispose of it properly!": A survey study in Vietnam." *Review of Behavioral Economics*, 7 (2): 145–158.

Inglehart, R., C. Haerpfer, A. Morena, Christian Welzel, K. Kizilova, H. Diez-Medrano, M. Lagos, P. Norris, E. Ponarin, and B. Puranten. 2021. "World Values Survey (WVS): Rounds 3-7 (1995-2020)." `https://www.worldvaluessurvey.org/WVSDocumentationWVL.jsp`.

Iyengar, Shanto, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J. Westwood. 2019. "The origins and consequences of affective polarization in the United States." *Annual Review of Political Science*, 22 (1): 129–146.

Jiang, Julie, Emily Chen, Kristina Lerman, and Emilio Ferrara. 2020. "Political polarization drives online conversations about COVID-19 in the United States." *Human Behavior and Emerging Technologies*, 2 (3): 200–211.

Jordan, Jillian J., Erez Yoeli, and David G. Rand. 2021. "Don't get it or don't spread it: Comparing self-interested versus prosocial motivations for COVID-19 prevention behaviors." *Scientific Reports*, 11 (1): 20222.

Jørgensen, Frederik, Alexander Bor, Marie Fly Lindholt, and Michael Bang Petersen. 2021. "Public support for government responses against COVID-19: Assessing levels and predictors in eight Western democracies during 2020." *West European Politics*, 44 (5-6): 1129–1158.

Jost, John T. 2017. "Ideological asymmetries and the essence of political psychology." *Political Psychology*, 38 (2): 167–208.

Jost, John T., Jack Glaser, Arie W. Kruglanski, and Frank J. Sulloway. 2003. "Political conservatism as motivated social cognition." *Psychological Bulletin*, 129 (3): 339–375.

Jost, John T., Sander van der Linden, Costas Panagopoulos, and Curtis D. Hardin. 2018. "Ideological asymmetries in conformity, desire for shared reality, and the spread of misinformation." *Current Opinion in Psychology*, 23 (0): 77–83.

Jugert, Philipp and John Duckitt. 2009. "A motivational model of authoritarianism: Integrating personal and situational determinants." *Political Psychology*, 30 (5): 693–719.

Kanai, Ryota, Tom Feilden, Colin Firth, and Geraint Rees. 2011. "Political orientations are correlated with brain structure in young adults." *Current Biology : CB*, 21 (8): 677–680.

Kemmelmeier, Markus. 2010. "Authoritarianism and its relationship with intuitive-experiential cognitive style and heuristic processing." *Personality and Individual Differences*, 48 (1): 44–48.

Kerr, John, Costas Panagopoulos, and Sander van der Linden. 2021. "Political polarization on COVID-19 pandemic response in the United States." *Personality and Individual Differences*, 179 (5): 110892.

Kohn, P. M. 1974. "Authoritarianism, rebelliousness, and their correlates among British undergraduates." *The British Journal of Social and Clinical Psychology*, 13 (3): 245–255.

Kuziemko, Ilyana, Michael I. Norton, Emmanuel Saez, and Stefanie Stantcheva. 2015. "How elastic are preferences for redistribution? Evidence from randomized survey experiments." *American Economic Review*, 105 (4): 1478–1508.

Landau, Mark J., Sheldon Solomon, Jeff Greenberg, Florette Cohen, Tom Pyszczynski, Jamie Arndt, Claude H. Miller, Daniel M. Ogilvie, and Alison Cook. 2004. "Deliver us from evil: the effects of mortality salience and reminders of 9/11 on support for President George W. Bush." *Personality and Social Psychology Bulletin*, 30 (9): 1136–1150.

Launay, Odile, Marine Cachanado, Liem B. Luong Nguyen, Laetitia Ninove, Marie Lachâtre, Inès Ben Ghezala, Marc Bardou, Catherine Schmidt-Mutter, Karine Lacombe, Fabrice Laine, Jean-Sébastien Allain, Elisabeth Botelho-Nevers, Marie-Pierre Tavolacci, Christian Chidiac, Patricia Pavese, Bertrand Dussol, Stéphane Priet, Dominique Deplanque, Amel Touati, Laureen Curci et al. 2022. "Immunogenicity and Safety of Beta-Adjuvanted Recombinant Booster Vaccine." *The New England Journal of Medicine*, 387 (4): 374–376.

Levendusky, Matthew S. 2018. "Americans, not partisans: Can priming American national identity reduce affective polarization? l." *The Journal of Politics*, 80 (1): 59–70.

Levitt, Steven D. and John A. List. 2007. "What do laboratory experiments measuring social preferences reveal about the real world?" *Journal of Economic Perspectives*, 21 (2): 153–174.

Lindholt, Marie Fly, Frederik Jørgensen, Alexander Bor, and Michael Bang Petersen. 2021. "Public acceptance of COVID-19 vaccines: Cross-national evidence on levels and individual-level predictors using observational data." *BMJ open*, 11 (6): e048172.

Lopez-Leon, Sandra, Talia Wegman-Ostrosky, Carol Perelman, Rosalinda Sepulveda, Paulina A. Rebolledo, Angelica Cuapio, and Sonia Villapol. 2021. "More than 50 long-term effects of COVID-19: a systematic review and meta-analysis." *Scientific Reports*, 11 (1): 16144.

Lytwyn, Tracy. 2012. "The personality of policy preferences: Analyzing the relationship between Myers-Briggs personality types and political views." *Res Publica - Journal of Undergraduate Research*, 17 (1): 99–119.

Malka, Ariel, Christopher J. Soto, Michael Inzlicht, and Yphtach Lelkes. 2014. "Do needs for security and certainty predict cultural and economic conservatism? A cross-national analysis." *Journal of Personality and Social Psychology*, 106 (6): 1031–1051.

McCoy, Jennifer and Murat Somer. 2021. "Overcoming polarization." *Journal of Democracy*, 32 (1): 6–21.

McGovern, Tony, Stephen Larson, Bill Morris, and Matt Hodges. 2020. "United States General Election presidential results by county from 2008 to 2020." `https://github.com/tonmcg/US_County_Level_Election_Results_08-20`.

Mehandru, Saurabh and Miriam Merad. 2022. "Pathological sequelae of long-haul COVID." *Nature Immunology*, 23 (2): 194–202.

Mitze, Timo, Reinhold Kosfeld, Johannes Rode, and Klaus Wälde. 2020. "Face masks considerably reduce COVID-19 cases in Germany." *Proceedings of the National Academy of Sciences of the United States of America*, 117 (51): 32293–32301.

Morsi, Davide, John T. Jost, and Vishal Singh. 2019. "An asymmetrical "President-in-power" effect." *American Political Science Review*, 113 (2): 614–620.

Motta, Matt, Dominik Stecula, and Christina Farhart. 2020. "How right-leaning media coverage of COVID-19 facilitated the spread of misinformation in the early stages of the pandemic in the U.S." *Canadian Journal of Political Science*, 53 (2): 335–342.

Müller, Stephan and Holger A. Rau. 2021. "Economic preferences and compliance in the social stress test of the COVID-19 crisis." *Journal of Public Economics*, 194 (5): 104322.

Murtin, Fabrice, Lara Fleischer, Vincent Siegerink, Arnstein Aassve, Yann Algan, Romina Boarini, Santiago González, Zsuzsanna Lonti, Gianluca Grimalda, Rafael Hortala Vallve, Soonhe Kim, David Lee, Louis Putterman, and Conal Smith. 2018. "Trust and its determinants." *OECD Statistics Working Papers*, 2018/02.

Nielsen, Julie H. and Johannes Lindvall. 2021. "Trust in government in Sweden and Denmark during the COVID-19 epidemic." *West European Politics*, 44 (5-6): 1180–1204.

Onraet, Emma, Alain van Hiel, Arne Roets, and Ilse Cornelis. 2011. "The closed mind: 'Experience' and 'cognition' aspects of openness to experience and need for closure as psychological bases for right–wing attitudes." *European Journal of Personality*, 25 (3): 184–197.

Osborne, Danny and Bernard Weiner. 2015. “A latent profile analysis of attributions for poverty: Identifying response patterns underlying people’s willingness to help the poor.” *Personality and Individual Differences*, 85 (0): 149–154.

Our World in Data. 2022. “Coronavirus (COVID-19) Deaths.” `https://ourworldindata.org/covid-deaths`.

Painter, Marcus and Tian Qiu. 2021. “Political beliefs affect compliance with government mandates.” *Journal of Economic Behavior & Organization*, 185 (6392): 688–701.

Pedersen, Mogens Jin and Nathan Favero. 2020. “Social distancing during the COVID-19 pandemic: Who are the present and future non-compliers?” *Public Administration Review*, 80 (5): 805–814.

Pennycook, Gordon, Jonathon McPhetres, Bence Bago, and David G. Rand. 2022. “Beliefs About COVID-19 in Canada, the United Kingdom, and the United States: A Novel Test of Political Polarization and Motivated Reasoning: A novel test of political polarization and motivated reasoning.” *Personality and Social Psychology Bulletin*, 48 (5): 750–765.

Pettigrew, Thomas F. 1958. “The measurement and correlates of category width as a cognitive variable1.” *Journal of Personality*, 26 (4): 532–544.

Pew Research Center. 2014. “Political polarization in the American public.” `https://www.pewresearch.org/politics/2014/06/12/political-polarization-in-the-american-public/`.

Pew Research Center. 2019. “Partisan antipathy: More intense, more personal.” `https://www.pewresearch.org/politics/2019/10/10/partisan-antipathy-more-intense-more-personal/`.

Phetsouphanh, Chansavath, David R. Darley, Daniel B. Wilson, Annett Howe, C. Mee Ling Munier, Sheila K. Patel, Jennifer A. Juno, Louise M. Burrell, Stephen J. Kent, Gregory J. Dore, Anthony D. Kelleher, and Gail V. Matthews. 2022. “Immunological dysfunction persists for 8 months following initial mild-to-moderate SARS-CoV-2 infection.” *Nature Immunology*, 23 (2): 210–216.

Pittinsky, Todd L. 2015. “America’s crisis of faith in science.” *Science*, 348 (6234): 511–512.

de Quidt, Jonathan, Johannes Haushofer, and Christopher Roth. 2018. "Measuring and bounding experimenter demand." *American Economic Review*, 108 (11): 3266–3302.

Rabinowitz, Mitchell, Lauren Latella, Chadly Stern, and John T. Jost. 2016. "Beliefs about childhood vaccination in the United States: Political ideology, false consensus, and the illusion of uniqueness." *PloS one*, 11 (7): e0158382.

Reynolds, Catherine J., Corinna Pade, Joseph M. Gibbons, Ashley D. Otter, Kai-Min Lin, Diana Muñoz Sandoval, Franziska P. Pieper, David K. Butler, Siyi Liu, George Joy, Nasim Forooghi, Thomas A. Treibel, Charlotte Manisty, James C. Moon, Amanda Semper, Tim Brooks, Áine McKnight, Daniel M. Altmann, Rosemary J. Boyton, Hakam Abbass et al. 2022. "Immune boosting by B.1.1.529 (Omicron) depends on previous SARS-CoV-2 exposure." *Science (New York, N.Y.)*, 377 (6603): eabq1841.

Romano, Angelo, Giuliana Spadaro, Daniel Balliet, Jeff Joireman, Caspar van Lissa, Shuxian Jin, Maximilian Agostini, Jocelyn J. Bélanger, Ben Gützkow, Jannis Kreienkamp, N. Pontus Leander, and PsyCorona Collaboration. 2021a. "Cooperation and trust across societies during the COVID-19 pandemic." *Journal of Cross-Cultural Psychology*, 52 (7): 622–642.

Romano, Angelo, Matthias Sutter, James H. Liu, and Daniel Balliet. 2021b. "Political ideology, cooperation and national parochialism across 42 nations." *Philosophical transactions of the Royal Society of London. Series B, Biological sciences*, 376 (1822): 20200146.

Rozzi, Gavin C. 2021. "zipcodeR: Advancing the analysis of spatial data at the ZIP code level in R." *Software Impacts*, 9 (0): 100099.

Simonov, Andrey, Szymon Sacher, Jean-Pierre Dubé, and Shirsho Biswas. 2022. "The persuasive effect of Fox News: Noncompliance with social distancing during the COVID-19 pandemic." *Marketing Science*, 41 (2): 230–242.

Solon, Idan S. 2014. "How intelligence mediates liberalism and prosociality." *Intelligence*, 47 (0): 44–53.

Sterling, Joanna, John T. Jost, and Curtis D. Hardin. 2019. "Liberal and conservative representations of the good society: A (social) structural topic modeling approach." *SAGE Open*, 9 (2): 215824401984621.

Sun, Xiaoyu, Chunyan Yi, Yuanfei Zhu, Longfei Ding, Shuai Xia, Xingchen Chen, Mu Liu, Chenjian Gu, Xiao Lu, Yadong Fu, Shuangfeng Chen, Tianlong Zhang, Yaguang Zhang, Zhuo Yang, Liyan Ma, Wangpeng Gu, Gaowei Hu, Shujuan Du, Renhong Yan, Weihui Fu et al. 2022. "Neutralization mechanism of a human antibody with pan-coronavirus reactivity including SARS-CoV-2." *Nature Microbiology*, 7 (7): 1063–1074.

Svolik, Milan W. 2019. "Polarization versus democracy." *Journal of Democracy*, 30 (3): 20–32.

Syropoulos, Stylianos and Ezra M. Markowitz. 2021. "Prosocial responses to COVID-19: Examining the role of gratitude, fairness and legacy motives." *Personality and Individual Differences*, 171 (5): 110488.

Terrier, Camille, Daniel L. Chen, and Matthias Sutter. 2021. "COVID-19 within families amplifies the prosociality gap between adolescents of high and low socioeconomic status." *Proceedings of the National Academy of Sciences of the United States of America*, 118 (46): e2110891118.

Theiss-Morse, Elizabeth A., Michael W. Wagner, William H. Flanigan, and Nancy H. Zingale. 2018. *Political behavior of the American electorate: Fourteenth edition.* London and Thousand Oaks, California: Sage and CQ Press.

Thunström, Linda, Madison Ashworth, Jason F. Shogren, Stephen Newbold, and David Finoff. 2021. "Testing for COVID-19: Willful ignorance or selfless behavior?" *Behavioural Public Policy*, 5 (2): 135–152.

United States Census Bureau. 2021. "County population totals: 2010-2020." `https://www2.census.gov/programs-surveys/popest/datasets/`.

van Bavel, Jay J., Katherine Baicker, Paulo S. Boggio, Valerio Capraro, Aleksandra Cichocka, Mina Cikara, Molly J. Crockett, Alia J. Crum, Karen M. Douglas, James N. Druckman, John Drury, Oeindrila Dube, Naomi Ellemers, Eli J. Finkel, James H. Fowler, Michele J. Gelfand, Shihui Han, S. Alexander Haslam, Jolanda Jetten, Shinobu Kitayama et al. 2020. "Using social and behavioural science to support COVID-19 pandemic response." *Nature Human Behaviour*, 4 (5): 460–471.

van Hiel, Alain and Ivan Mervielde. 2003. "The measurement of cognitive complexity and its relationship with political extremism." *Political Psychology*, 24 (4): 781–801.

van Lange, Paul A. M., René Bekkers, Antonio Chirumbolo, and Luigi Leone. 2012. "Are conservatives less likely to be prosocial than liberals? From games to ideology, political preferences and voting." *European Journal of Personality*, 26 (5): 461–473.

Waller, Isaac and Ashton Anderson. 2021. "Quantifying social organization and political polarization in online platforms." *Nature*, 600 (7888): 264–268.

Warner, Benjamin R. and Astrid Villamil. 2017. "A test of imagined contact as a means to improve cross-partisan feelings and reduce attribution of malevolence and acceptance of political violence." *Communication Monographs*, 84 (4): 447–465.

Wellenius, Gregory A., Swapnil Vispute, Valeria Espinosa, Alex Fabrikant, Thomas C. Tsai, Jonathan Hennessy, Andrew Dai, Brian Williams, Krishna Gadepalli, Adam Boulanger, Adam Pearce, Chaitanya Kamath, Arran Schlosberg, Catherine Bendebury, Chinmoy Mandayam, Charlotte Stanton, Shailesh Bavadekar, Christopher Pluntke, Damien Desfontaines, Benjamin H. Jacobson et al. 2021. "Impacts of social distancing policies on mobility and COVID-19 case growth in the US." *Nature Communications*, 12 (1): 3118.

WHO. 2020a. "Advice on the use of masks in the context of COVID-19: Interim guidance 5 June 2020." `https://apps.who.int/iris/handle/10665/332293`.

WHO. 2020b. "Advice on the use of masks in the context of COVID-19: Interim guidance 6 April 2020." `https://apps.who.int/iris/handle/10665/331693`.

WHO. 2020c. "COVID-19 virtual press conference: Transcript, 8 April, 2020." `https://www.who.int/docs/default-source/coronaviruse/transcripts/who-audio-emergencies-coronavirus-press-conference-full-08apr2020.pdf`.

Wojcieszak, Magdalena and Benjamin R. Warner. 2020. "Can interparty contact reduce affective polarization? A systematic test of different forms of intergroup contact." *Political Communication*, 37 (6): 789–811.

Xie, Yan, Evan Xu, Benjamin Bowe, and Ziyad Al-Aly. 2022. "Long-term cardiovascular outcomes of COVID-19." *Nature Medicine*, 28 (3): 583–590.

Yamey, Gavin and Gregg Gonsalves. 2020. "Donald Trump: A political determinant of covid-19." *BMJ (Clinical research ed.)*, 369 (0): m1643.

Yu, Yanqiu, Sitong Luo, Phoenix Kit-Han Mo, Suhua Wang, Junfeng Zhao, Guohua Zhang, Lijuan Li, Liping Li, and Joseph Tak-Fai Lau. 2021. “Prosociality and social responsibility were associated with intention of COVID-19 vaccination among university students in China.” *International Journal of Health Policy and Management*, 11 (8): 1562–1569.

Zettler, Ingo, Christoph Schild, Lau Lilleholt, Lara Kroencke, Till Utesch, Morten Moshagen, Robert Böhm, Mitja D. Back, and Katharina Geukes. 2022. “The Role of Personality in COVID-19-Related Perceptions, Evaluations, and Behaviors: Findings Across Five Samples, Nine Traits, and 17 Criteria.” *Social Psychological and Personality Science*, 13 (1): 299–310.

Zickfeld, Janis H., Thomas W. Schubert, Anders Kuvaas Herting, Jon Grahe, and Kate Faasse. 2020. “Correlates of health-protective behavior during the initial days of the COVID-19 outbreak in Norway.” *Frontiers in Psychology*, 11 (0): 564083.

Appendices

5.A Appendix

5.A.1 Descriptive Statistics

Table 5.2: Descriptive Statistics

	Mean	SD	Min	Max	N
Dependent Variables					
Self-quarantine	1.360	0.795	0	2	1120
Face mask wearing	3.273	1.124	0	4	1120
Worry local spread	2.463	1.348	0	4	1120
Relief timely and efficient	4.808	3.214	0	10	1061
Handling of crisis increased trust (State)	5.320	2.938	0	10	1072
Handling of crisis increased trust (Nation)	4.348	3.051	0	10	1069
Explanatory variables					
Index: Prosociality	0.00	1	-2.10	1.72	1120
Altruism (DG)	4.97	2.86	0	10	1120
Cooperation (PGG)	6.09	3.24	0	10	1120
Index: Econ. Affectedness	0.00	1	-3.26	3.38	1039
Income Loss/Gain	3.23	1.62	0	8	1120
HH Expectations pre-COVID	6.18	2.38	0	10	1067
HH Expectations during COVID	5.15	2.53	0	10	1048
Political ideology (Conservatism)	5.60	2.89	0	10	1041
Liberal (Conservatism ≤ 3)	0.24	0.43	0	1	1041
Conservative (Conservatism ≥ 7)	0.41	0.49	0	1	1041
ln(pop. density)	6.509	1.690	1.065	11.149	1119
ln(county pop.)	13.051	1.493	8.399	16.112	1119
ln(deaths per 100k around survey +1)	0.730	0.673	0.00	3.5817	1118

Notes: The table shows number of observations, means, standard deviations, and minimum and maximum values for the variables.

5.A.2 Sample Characteristics and Corresponding Population Values

Table 5.3: Sample Characteristics

	Sample mean	Standard deviation	Population mean
Targeted characteristics			
Female	0.55	(0.50)	0.52
Age	47.96	(16.50)	
Age 18-20	0.04	(0.19)	0.05
Age 21-44	0.40	(0.49)	0.41
Age 45-64	0.37	(0.48)	0.33
Age 65 and above	0.19	(0.39)	0.21
Low income	0.43	(0.50)	0.40
Medium income	0.22	(0.42)	0.20
High income	0.35	(0.48)	0.40
Non-targeted characteristics			
Some college	0.33	(0.47)	0.28
Tertiary diploma	0.50	(0.50)	0.35
Employed	0.49	(0.50)	0.53
Self-employed	0.09	(0.29)	0.04
Unemployed	0.16	(0.37)	0.05
Out of the labor force	0.26	(0.44)	0.38
White	0.74	(0.44)	0.60
African-American	0.11	(0.32)	0.13
Hispanic	0.09	(0.28)	0.16
Asian American	0.03	(0.16)	0.06
Other race	0.01	(0.09)	0.04
Liberal	0.24	(0.43)	0.25
Conservative	0.41	(0.49)	0.36
Obs.	1120		

Notes: Table shows means and standard deviations of the Trustlab sample characteristics (column title "Sample mean") and respective population values (column title "Population mean"). All variables except Age and Age (median) are binary. The aim was to achieve a nationally representative sample in terms of gender, age, and income (targeted dimensions). (*) Gender and population age group shares refer to the U.S. population aged 18 years and over. Labor force population statistics from U.S. Bureau of Labor Statistics (https://www.bls.gov/cps/cpsaat01.htm). Ethnicity statistics from United States Census Bureau (https://www.census.gov/quickfacts/fact/table/US/PST045219). Gender and age statistics from the CIA World Factbook (https://www.cia.gov/the-world-factbook/countries/united-states/) and the United States Census Bureau (https://www.census.gov/topics/population/age-and-sex/data/tables.html). Population ideological views from GALLUP (https://news.gallup.com/poll/328367/americans-political-ideology-held-steady-2020.aspx).

5.B Online Appendix

Supplementary Online Material for

The politicized pandemic: Ideological polarization and the behavioral response to COVID-19

Gianluca Grimalda, Fabrice Murtin, David Pipke, Louis Putterman, Matthias Sutter

5.B.1 Experimenter demand effects

The results of this study are based on self-reported behavior rather than real-life observations. In addition to self-reported behavior being possibly inconsistent with actual actions (Falco and Zaccagni, 2021), self-reported measures of behavior are prone to experimenter demand effects. That is, participants may feel urged to report behavior that aligns with what they perceive to be the researchers‘ expectations (de Quidt et al., 2018). This problem arises mainly when the survey enquires about behavior that may affect other people and is highly politicized. Therefore, we tried to measure experimenter demand effects through a question placed at the end of the survey, following the approach by de Quidt et al. (2018).

The question asked participants whether they believed that researchers had a preference on their choices in the experimental module about interethnic relationships. Even if this question does not pertain to any outcome variables in the present paper, it may be taken as a general proxy to identify those participants who thought that researchers had expectations over their answers to the survey. Therefore, we construct a desirability dummy equal to one (labeled "Desirability" in Table 5.4) for participants answering that the researchers had certain expectations on their behavior. Participants who believed that researchers preferred specific allocations are more likely to be conservatives and less likely to be Liberals (see Table 5.4). They also worry more about the local spread of the virus and are more likely to state that their trust in national politicians increased because of their crisis management. In addition, such participants are somewhat more likely to adhere to self-quarantine. However, they do not differ significantly from others in terms of their prosociality score, face mask wearing, and the variables related to political crisis management. In Table 5.5, we introduce the desirability dummy alongside our core explanatory variables. The mediation of the political ideology and prosociality coefficients on the outcome variables is negligible. This result suggests that experimenter demand effects may have had no crucial impact on our results.

Table 5.4: Effect of Experimenter Demand on Prosociality, Ideology, and Outcomes

	(1) Prosociality	(2) Self-quarantine	(3) Face mask wearing	(4) Worry local spread	(5) Relief timely and efficient	(6) Trust increased (State)	(7) Trust increased (Nation)	(8) Liberal	(9) Conservative
Desirability	-0.013	0.127*	0.127	0.163***	0.032	-0.019	0.120**	-0.252****	0.317****
	(0.086)	(0.064)	(0.080)	(0.055)	(0.081)	(0.067)	(0.054)	(0.055)	(0.060)
Obs.	1039	1039	1039	1039	1005	1013	1011	1039	1039
Clusters	50	50	50	50	50	50	50	50	50
R2	0.056	0.073	0.138	0.080	0.336	0.134	0.342	0	0
Adj. R2	0.035	0.053	0.120	0.060	0.321	0.114	0.327	0.042	0.067

Notes: The table shows regression results. The dependent variables in the respective columns are (1) Prosociality index (2) Self-quarantine, (3) Face mask wearing, (4) Worry local spread, (5) Relief timely and efficient, (6) Handling of crisis increased trust (State), (7) Handling of crisis increased trust (Nation), (8) Liberal ideology dummy, (9) Conservative ideology dummy. Variables (except dummies) are standardized. Each regression includes controls for age, age-squared, sex, ethnicities, income categories, education categories (respondent's and parents' attainment), dummy for parents immigrated, population, density, coronavirus deaths per 100k between the day of the survey and 7 days before the date of survey, date of survey. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.5: Mediation: Experimenter Demand

Without Desirability Question	Prosociality		Liberal		Conservative	
Self-quarantine	0.113***	(0.03)	0.064	(0.08)	-0.219***	(0.08)
Face mask wearing	0.093***	(0.03)	0.026	(0.05)	-0.317****	(0.06)
Worry local spread	0.122****	(0.03)	0.090	(0.06)	-0.136*	(0.08)
Relief timely and efficient	0.087***	(0.03)	-0.514****	(0.06)	0.725****	(0.06)
Handling of crisis increased trust (State)	0.123****	(0.03)	-0.168*	(0.09)	0.347***	(0.11)
Handling of crisis increased trust (Nation)	0.106***	(0.03)	-0.460****	(0.07)	0.660****	(0.06)
With Desirability Question	**Prosociality**		**Liberal**		**Conservative**	
Self-quarantine	0.113***	(0.03)	0.073	(0.08)	-0.234***	(0.07)
Face mask wearing	0.094***	(0.03)	0.034	(0.05)	-0.332****	(0.06)
Worry local spread	0.123****	(0.03)	0.101	(0.06)	-0.155*	(0.08)
Relief timely and efficient	0.087***	(0.03)	-0.512****	(0.06)	0.721****	(0.06)
Handling of crisis increased trust (State)	0.123****	(0.03)	-0.170*	(0.09)	0.349***	(0.11)
Handling of crisis increased trust (Nation)	0.106***	(0.03)	-0.452****	(0.07)	0.645****	(0.06)

Notes: The table shows regression coefficients of the variables in the heading row on the dependent variable indicated in the first column. The first 6 rows after the heading show coefficients from regressions where we do not control the experimenter demand dummy (1 = Respondent thinks that the researchers had a preference on what they should transfer in the interethnic games). The last 6 rows show coefficients from regressions with a dummy for the experimenter demand question included. All regressions include the same control variables as the main regressions. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

5.B.2 Sample Characteristics (Restricted to complete observations)

The Table 5.6 shows that the subset of the sample without missing value in any of the variables used in the main regression analysis (see Figure 5.1 and Online Appendix Table 5.8) very closely resembles the characteristics of the sample as described in Tables 5.2 and 5.A.2.

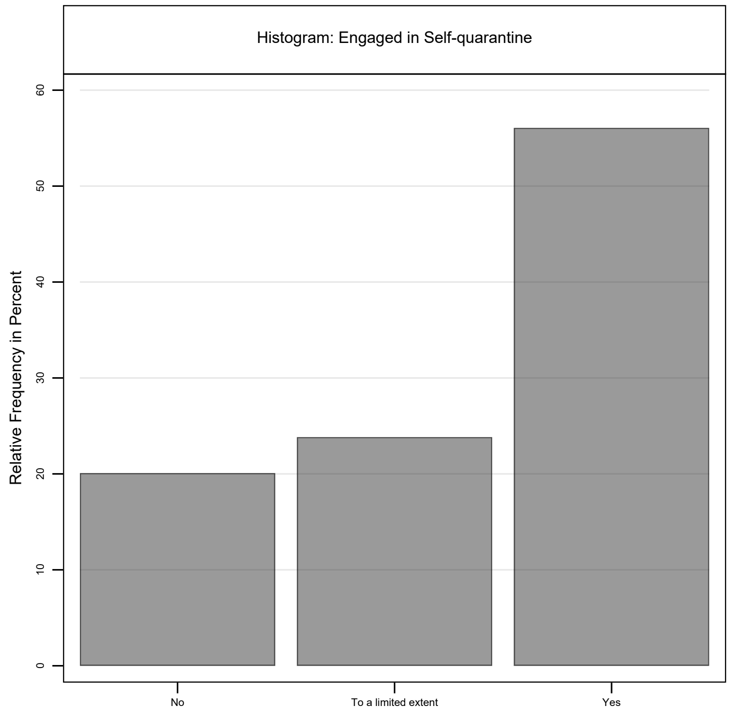
Table 5.6: Sample Characteristics (Observations without any missing values)

	Mean	SD	Min	Max	N
White	0.75	0.43	0	1	986
African-American	0.11	0.31	0	1	986
Hispanic	0.09	0.29	0	1	986
Asian American	0.03	0.17	0	1	986
Other race	0.01	0.07	0	1	986
Female	0.54	0.50	0	1	986
Age	48.42	16.45	18	80	986
Age 18-20	0.03	0.18	0	1	986
Age 21-44	0.39	0.49	0	1	986
Age 45-64	0.38	0.48	0	1	986
Age 65 and above	0.20	0.40	0	1	986
Low income	0.40	0.49	0	1	986
Med income	0.23	0.42	0	1	986
High income	0.37	0.48	0	1	986
High-school or less	0.16	0.37	0	1	986
Some college	0.32	0.47	0	1	986
Tertiary diploma	0.52	0.50	0	1	986
Employed	0.51	0.50	0	1	986
Self-employed	0.09	0.29	0	1	986
Unemployed	0.15	0.36	0	1	986
Out of the labor force	0.25	0.43	0	1	986
Liberal (<= 3)	0.25	0.43	0	1	986
Conservative (=> 7)	0.41	0.49	0	1	986

Notes: The table shows number of observations, means, standard deviations, and minimum and maximum values for the variables. The sample is restricted to those observations without missing values in any of the variables used as outcome or control variables in the main regression analysis.

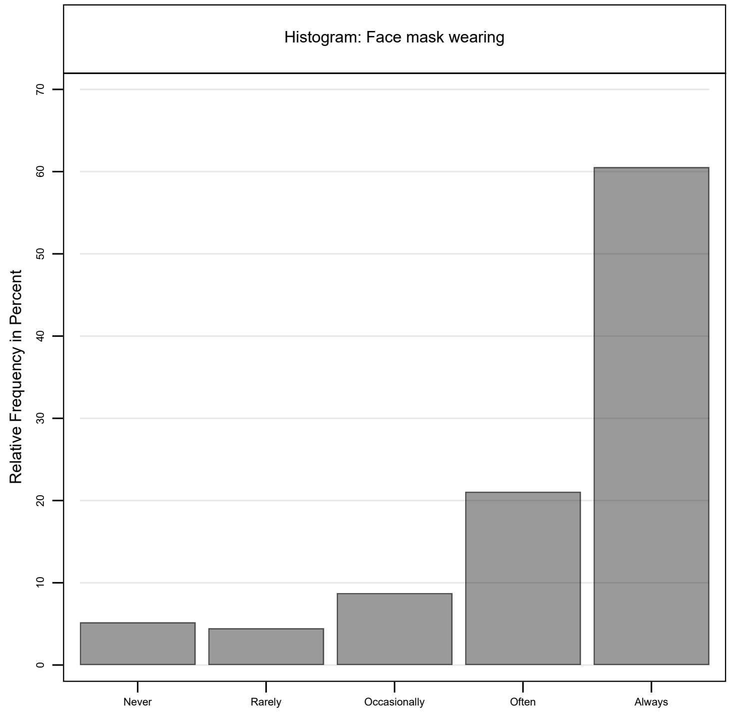
5.B.3 Histograms of selected variables

Figure 5.3: Self-quarantine



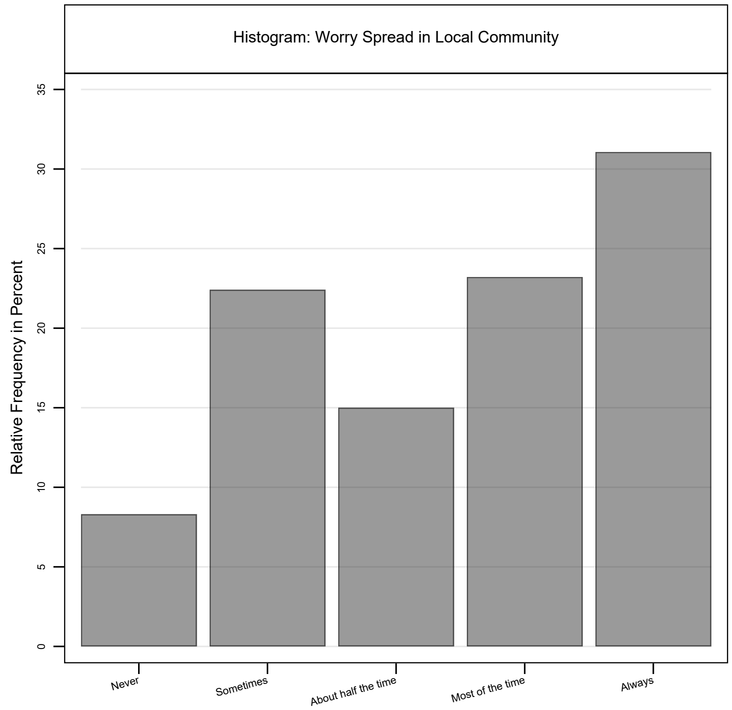
Notes: This figure shows the histogram referring to the question "Did you (perhaps with family or roommates) self-quarantine or self-isolate for a week or longer during the COVID-19 pandemic?". Possible answers: "Yes, I (we) self-quarantined", "To a limited extent, only" "No, I (we) engaged in no self-quarantine".

Figure 5.4: Face Mask Wearing



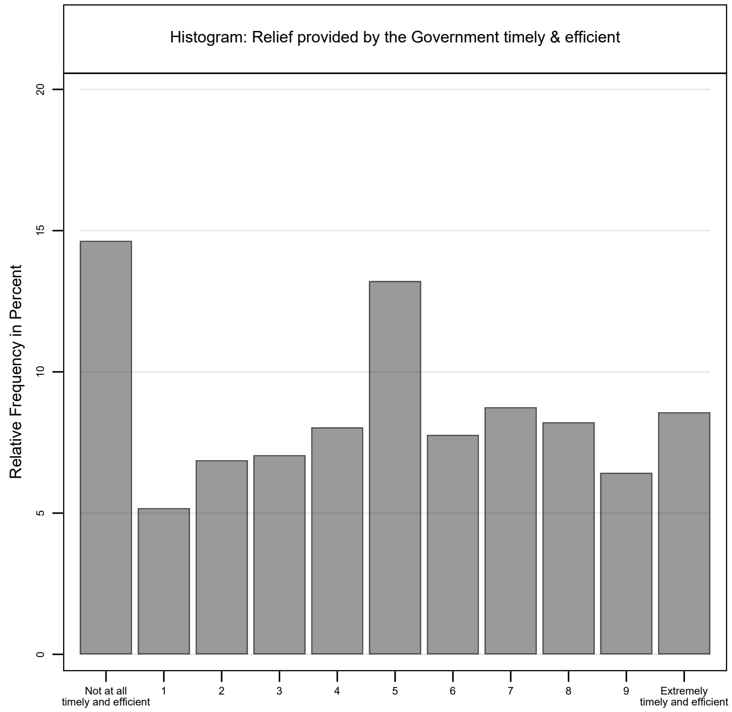
Notes: This figure shows the histogram referring to the question "Do you or did you wear a face mask when going out because of the COVID-19 pandemic?". Possible answers: "Always", "Often", "Occasionally", "Rarely", "Never".

Figure 5.5: Worry Local Spread



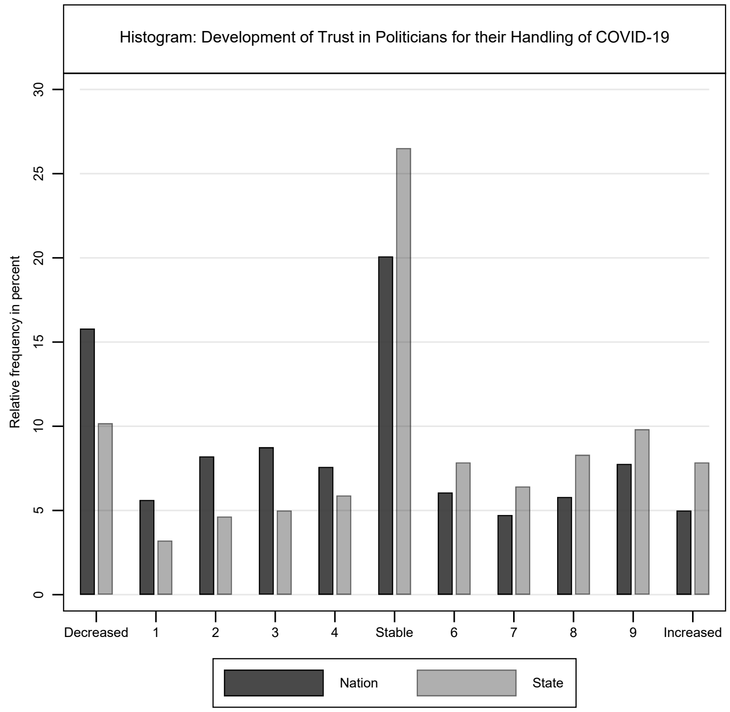
Notes: This figure shows the histogram referring to the question "I worry about Covid-19 spreading in my local community". Possible answers: "Always", "Most of the time", "About half the time", "Sometimes", "Never".

Figure 5.6: Relief Timely and Efficient



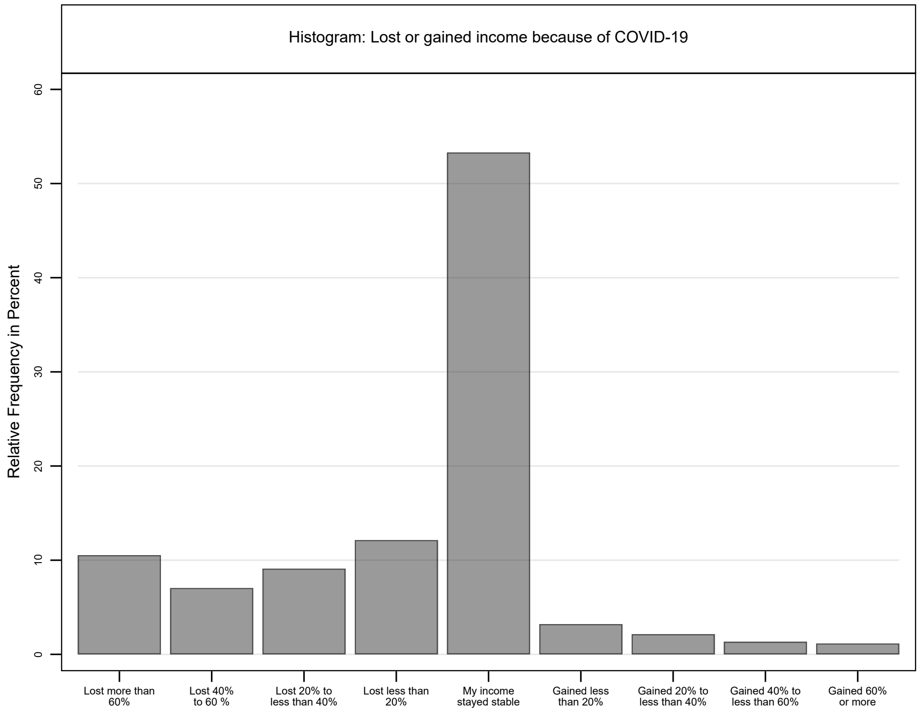
Notes: This figure shows the histogram referring to the question "Now that the COVID-19 epidemic has occurred, do you think that the provision by government of adequate relief has been timely and efficient ?". Possible answers between 0 "Not at all timely and efficient" and 10 "Extremely timely and efficient".

Figure 5.7: Trust Development Politicians (State and Nation)



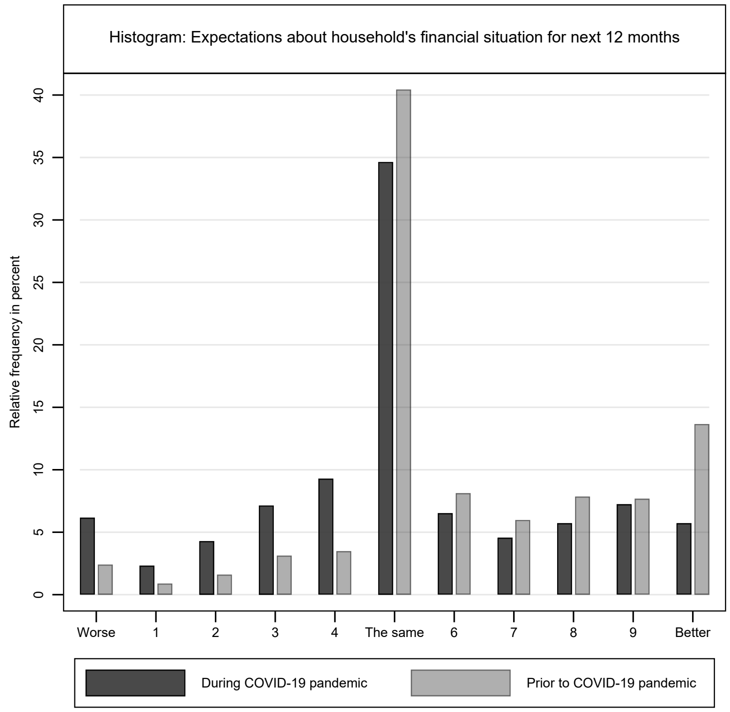
Notes: This figure shows the histogram referring to the questions "Has your trust in politicians in your state (nationally) increased, decreased, or stayed stable for their handling of COVID-19?". Possible answers between 0 "Decreased", 5 "Stable", and 10 "Increased".

Figure 5.8: Income Change because of COVID-19



Notes: This figure shows the histogram referring to the question "Have you lost income or gained income because of COVID-19, or has your income stayed stable? Please check the option that best describes your situation."

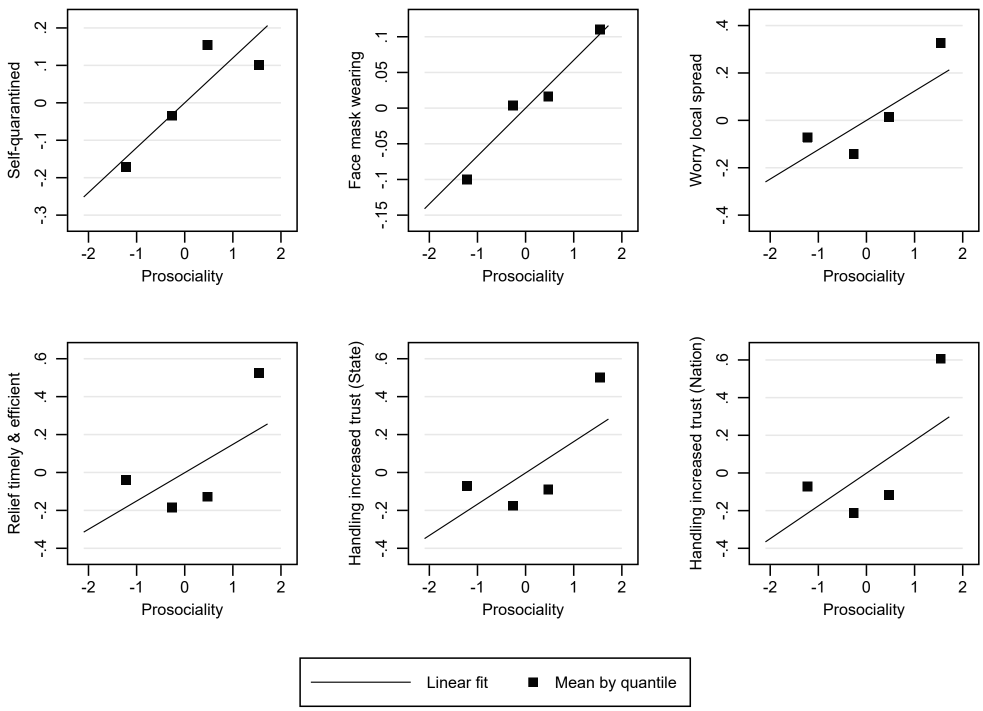
Figure 5.9: Expectations about the Financial Situation of the Household



Notes: This figure shows the histogram referring to the questions "Prior to the COVID-19 pandemic, when it came to the financial situation of your household, what were your expectations for the 12 months to come? Were you then expecting that the next 12 months be: better, worse, or the same?" and "Now that the COVID-19 pandemic has arrived, when it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?".

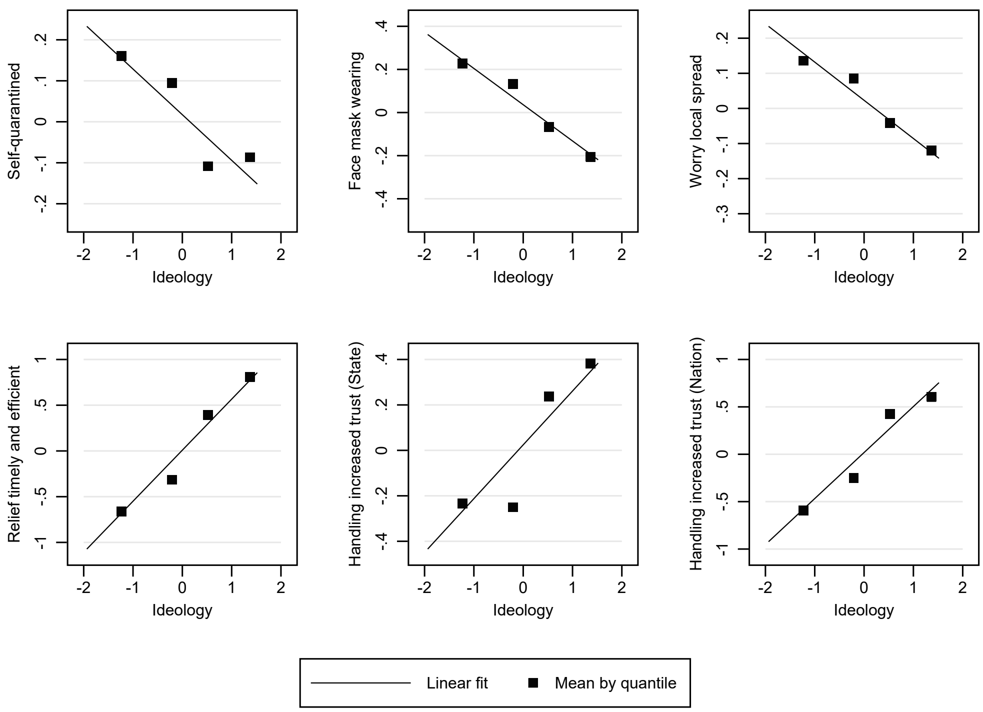
5.B.4 Scatterplots

Figure 5.10: Prosociality Index



Notes: This figure shows a linear regression fit of the standardized outcome variable on the y-axis on the standardized explanatory variable on the x-axis. Dots mark the means of the standardized outcome variable for each quartile of the standardized explanatory variable.

Figure 5.11: Political Ideology



Notes: This figure shows a linear regression fit of the standardized outcome variable on the y-axis on the standardized explanatory variable on the x-axis. Dots mark the means of the standardized outcome variable for each quartile of the standardized explanatory variable.

5.B.5 Group differences (political ideology and gender) in explanatory variables

Table 5.7: Group differences in Means of Explanatory Variables

	Con.	Lib.	N	p-val.	Male	Fem.	N	p-val.
High income	0.406	0.341	679	0.094	0.438	0.269	1120	0.000
Medium income	0.248	0.227	679	0.552	0.250	0.200	1120	0.044
Low income	0.347	0.431	679	0.028	0.312	0.531	1120	0.000
Low education	0.165	0.110	679	0.047	0.135	0.203	1120	0.003
Medium education	0.262	0.337	679	0.036	0.240	0.398	1120	0.000
High education	0.573	0.553	679	0.608	0.625	0.399	1120	0.000
Rural	0.226	0.180	679	0.154	0.183	0.255	1120	0.004
Town	0.186	0.227	679	0.196	0.181	0.244	1120	0.011
City	0.587	0.592	679	0.900	0.637	0.502	1120	0.000
Political ideology	8.450	1.584	679	0.000	6.076	5.206	1041	0.000
Index: Prosociality	0.146	-0.060	679	0.012	0.066	-0.054	1120	0.045
Index: Economic affectedness	-0.184	0.170	642	0.000	-0.117	0.099	1039	0.001

Notes: The table shows means, number of observations, and p-values from two-sided t-tests of the null hypothesis of equal means for the contrasted groups of conservatives vs. liberals and males vs. females.

5.B.6 Selected questionnaire items

The complete questionnaire from the second wave of the Trustlab has been deposited under https://osf.io/ebnm8.

- In political matters, people often talk of 'Liberal' and 'Conservative.' Generally speaking, how would you place your views on this scale?
  - Very Liberal - 0 1 2 3 4 5 6 7 8 9 10 - Very Conservative
- Did you (perhaps with family or roommates) self-quarantine or self-isolate for a week or longer during the COVID-19 pandemic?
  - • Yes, I (we) self-quarantined • To a limited extent, only • No, I (we) engaged in no self-quarantine
- Do you or did you wear a face mask when going out because of the COVID-19 pandemic?
  - • Always • Often • Occasionally • Rarely • Never
- I worry about getting infected with Covid-19:
  - • Always • Most of the time • About half the time • Sometimes • Never
- I feel vulnerable to Covid-19 infection:
  - • Strongly agree • Agree • Neither agree nor disagree • Somewhat disagree • Strongly disagree
- I worry about Covid-19 spreading in my local community.
  - • Always • Most of the time • About half the time • Sometimes • Never
- Have you lost income or gained income because of COVID-19, or has your income stayed stable? Please check the option that best describes your situation.
  - • Lost more than 60% • Lost 40% to 60% • Lost 20% to less than 40% • Lost less than 20% • My income has stayed stable. • Gained less than 20% • Gained 20% to less than 40% • Gained 40% to less than 60% • Gained 60% or more
- Prior to the COVID-19 pandemic, when it came to the financial situation of your household, what were your expectations for the 12 months to come? Were you then expecting that the next 12 months be: better, worse, or the same?
  - Worse - 0 1 2 3 4 5 - The same 6 7 8 9 10 - Better Don't know
- Now that the COVID-19 pandemic has arrived, when it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?

- Worse - 0 1 2 3 4 5 - The same 6 7 8 9 10 - Better Don't know

- Now that the COVID-19 epidemic has occurred, do you think that the provision by government of adequate relief has been timely and efficient ?
  - Not at all timely and efficient - 0 1 2 3 4 5 6 7 8 9 10 - Extremely timely and efficient Don't know
- Has your trust in politicians in your state increased, decreased, or stayed stable for their handling of COVID-19?
  - Decreased - 0 1 2 3 4 5 - Stable 6 7 8 9 10 - Increased Don't know
- Has your trust in politicians nationally increased, decreased, or stayed stable for their handling of COVID-19?
  - Decreased - 0 1 2 3 4 5 - Stable 6 7 8 9 10 - Increased Don't know
- Do you think that the researchers had any preference on how you should transfer money to some groups – among non-Hispanic Whites, African Americans, and Hispanics, in comparison to others?
  - Yes No

5.B.7 Regression Tables

Table 5.8: Regressions (OLS): Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
Prosociality	0.113***	0.093***	0.122****	0.087***	0.123****	0.106***
	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)	(0.03)
Liberal	0.064	0.026	0.090	-0.514****	-0.168*	-0.460****
	(0.08)	(0.05)	(0.06)	(0.06)	(0.09)	(0.07)
Conservative	-0.219***	-0.317****	-0.136*	0.725****	0.347***	0.660****
	(0.08)	(0.06)	(0.08)	(0.06)	(0.11)	(0.06)
Female	0.057	0.113*	-0.007	-0.146***	-0.160**	-0.243****
	(0.06)	(0.07)	(0.08)	(0.05)	(0.07)	(0.06)
Age	-0.688****	0.115	0.195	0.121	-0.117	-0.185
	(0.20)	(0.19)	(0.27)	(0.16)	(0.19)	(0.21)
Age squared	0.582***	-0.068	-0.255	-0.213	0.054	0.041
	(0.18)	(0.17)	(0.26)	(0.14)	(0.19)	(0.21)
High income	0.053	0.005	-0.017	0.002	-0.060	-0.115
	(0.07)	(0.08)	(0.12)	(0.06)	(0.06)	(0.09)
Low income	-0.035	-0.153*	-0.067	-0.085	-0.064	-0.066
	(0.08)	(0.09)	(0.10)	(0.08)	(0.09)	(0.08)
Med. Educ.	0.101	-0.025	-0.070	-0.114	-0.077	-0.129*
	(0.09)	(0.09)	(0.10)	(0.09)	(0.07)	(0.07)
High Educ.	0.065	0.050	-0.089	-0.061	0.106	-0.102
	(0.11)	(0.11)	(0.08)	(0.11)	(0.09)	(0.09)
Parents med.	-0.124*	-0.016	-0.075	0.107	0.022	0.157**
	(0.07)	(0.10)	(0.11)	(0.07)	(0.07)	(0.06)
Parents high	-0.010	0.015	0.131	0.082	0.061	0.160*
	(0.09)	(0.11)	(0.10)	(0.08)	(0.07)	(0.09)
Parents imm.	-0.101	0.241****	0.111	-0.019	-0.033	-0.063
	(0.07)	(0.06)	(0.08)	(0.06)	(0.09)	(0.08)
Town	0.102	0.196*	0.115	0.073	0.124	0.023
	(0.11)	(0.11)	(0.08)	(0.10)	(0.08)	(0.10)
City	0.187	0.255**	0.205*	0.197*	0.220***	0.211**
	(0.12)	(0.11)	(0.10)	(0.10)	(0.08)	(0.09)
ln(Deaths per 100k)	-0.033	-0.027	0.046	0.004	-0.019	-0.024
	(0.04)	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)
ln(county pop.)	0.038	0.081	0.066	0.029	-0.010	0.034

Table 5.8: Regressions (OLS): Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
	(0.05)	(0.06)	(0.06)	(0.06)	(0.05)	(0.05)
ln(pop. den.)	0.033	0.103	-0.008	-0.042	0.092	0.058
	(0.04)	(0.06)	(0.06)	(0.04)	(0.06)	(0.05)
Date survey	-0.019	0.085***	0.105***	-0.001	0.003	0.002
	(0.05)	(0.02)	(0.03)	(0.03)	(0.03)	(0.02)
African American	-0.286*	0.265***	0.042	-0.245***	-0.198*	-0.257***
	(0.15)	(0.09)	(0.10)	(0.08)	(0.12)	(0.08)
Hispanic	-0.198	-0.010	-0.035	-0.030	-0.144	-0.049
	(0.15)	(0.13)	(0.12)	(0.13)	(0.14)	(0.10)
Other Ethnicities	0.005	-0.052	0.082	-0.211*	-0.274*	-0.224**
	(0.11)	(0.13)	(0.20)	(0.11)	(0.16)	(0.09)
Constant	-0.055	-0.155	-0.059	-0.136	-0.126	-0.034
	(0.13)	(0.15)	(0.15)	(0.11)	(0.15)	(0.12)
Obs.	1039	1039	1039	1005	1013	1011
No. Clusters	50	50	50	50	50	50
R2	0.082	0.143	0.088	0.343	0.148	0.350
Adj. R2	0.062	0.125	0.069	0.328	0.129	0.335
Tests (p-values)						
Lib. = Con.	0.000	0.000	0.023	0.000	0.003	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.9: Regressions (OLS): Control Variables Only

	(1)	(2)	(3)	(4)	(5)	(6)
Female	0.105*	0.153**	0.042	-0.289****	-0.228****	-0.360****
	(0.06)	(0.07)	(0.08)	(0.07)	(0.06)	(0.08)
Age	-0.718****	0.107	0.223	0.109	-0.056	-0.110
	(0.17)	(0.18)	(0.24)	(0.20)	(0.19)	(0.24)
Age squared	0.611****	-0.060	-0.283	-0.182	0.002	-0.013
	(0.16)	(0.17)	(0.23)	(0.19)	(0.19)	(0.25)
High income	0.041	0.014	-0.009	0.006	-0.045	-0.104
	(0.07)	(0.07)	(0.12)	(0.07)	(0.06)	(0.10)
Low income	-0.057	-0.174**	-0.078	-0.162*	-0.089	-0.123
	(0.09)	(0.08)	(0.09)	(0.09)	(0.10)	(0.10)
Med. Educ.	0.146*	0.032	0.008	-0.222**	-0.126**	-0.215***
	(0.08)	(0.08)	(0.10)	(0.08)	(0.06)	(0.07)
High Educ.	0.083	0.068	-0.027	-0.159	0.075	-0.171*
	(0.10)	(0.11)	(0.09)	(0.10)	(0.08)	(0.10)
Parents med.	-0.056	-0.012	-0.045	0.111	0.038	0.174**
	(0.07)	(0.09)	(0.10)	(0.08)	(0.07)	(0.07)
Parents high.	0.060	0.006	0.129	0.110	0.080	0.191**
	(0.09)	(0.11)	(0.11)	(0.09)	(0.06)	(0.08)
Parents imm.	-0.084	0.273****	0.127*	0.035	0.000	-0.003
	(0.08)	(0.05)	(0.07)	(0.09)	(0.09)	(0.10)
Town	0.110	0.128	0.096	0.033	0.111	-0.011
	(0.11)	(0.12)	(0.09)	(0.10)	(0.09)	(0.10)
City	0.215*	0.188	0.169	0.209**	0.256****	0.238**
	(0.11)	(0.12)	(0.11)	(0.08)	(0.07)	(0.09)
ln(Deaths per 100k)	-0.025	-0.016	0.054*	0.034	0.012	0.011
	(0.03)	(0.03)	(0.03)	(0.05)	(0.03)	(0.04)
ln(county pop.)	0.016	0.077	0.060	0.007	-0.019	0.013
	(0.05)	(0.06)	(0.06)	(0.06)	(0.06)	(0.06)
ln(population density)	0.043	0.110*	0.003	-0.064	0.073	0.037
	(0.05)	(0.06)	(0.07)	(0.04)	(0.06)	(0.06)
Date survey	-0.037	0.064*	0.073**	0.001	0.003	0.003
	(0.05)	(0.04)	(0.03)	(0.03)	(0.03)	(0.03)
African American	-0.244*	0.281***	0.101	-0.474****	-0.332**	-0.481****
	(0.13)	(0.10)	(0.09)	(0.10)	(0.13)	(0.10)
Hispanic	-0.120	0.069	0.057	-0.183	-0.224	-0.188*

Table 5.9: Regressions (OLS): Control Variables Only

	(1)	(2)	(3)	(4)	(5)	(6)
	(0.15)	(0.13)	(0.11)	(0.11)	(0.14)	(0.11)
Other Ethnicities	0.034	-0.046	0.061	-0.357**	-0.351**	-0.360***
	(0.13)	(0.15)	(0.19)	(0.14)	(0.16)	(0.13)
Constant	-0.238*	-0.274*	-0.170	0.259**	0.044	0.279**
	(0.13)	(0.15)	(0.15)	(0.11)	(0.14)	(0.13)
Obs.	1118	1118	1118	1059	1070	1067
No. Clusters	50	50	50	50	50	50
R2	0.063	0.108	0.061	0.093	0.091	0.136
Adj. R2	0.047	0.092	0.044	0.076	0.074	0.12

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.10: Regressions (OLS): Only Core Variables

	(1)	(2)	(3)	(4)	(5)	(6)
Prosociality	0.127****	0.085**	0.133****	0.112****	0.143****	0.142****
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.04)
Liberal	0.100	0.097	0.119*	-0.515****	-0.131	-0.435****
	(0.08)	(0.06)	(0.06)	(0.07)	(0.10)	(0.07)
Conservative	-0.186**	-0.315****	-0.136*	0.776****	0.423****	0.718****
	(0.08)	(0.06)	(0.08)	(0.06)	(0.11)	(0.08)
Constant	0.051	0.104**	0.026	-0.193****	-0.140**	-0.184****
	(0.05)	(0.05)	(0.04)	(0.05)	(0.07)	(0.05)
Controls	1040	1040	1040	1006	1014	1012
Obs.	50	50	50	50	50	50
No. Clusters	0.027	0.036	0.025	0.299	0.085	0.256
R2	0.024	0.033	0.023	0.297	0.082	0.253
Adj. R2						
Tests (p-values)						
Lib. = Con.	0.000	0.000	0.008	0.000	0.001	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.11: Regressions (OLS): Economic Affectedness

	(1)	(2)	(3)	(4)	(5)	(6)
Prosociality	0.097***	0.101***	0.120***	0.092***	0.134****	0.116****
	(0.03)	(0.03)	(0.04)	(0.03)	(0.03)	(0.03)
I: Econ. aff.	0.129***	0.086***	0.079**	-0.107****	-0.106***	-0.127****
	(0.04)	(0.03)	(0.03)	(0.02)	(0.03)	(0.02)
Liberal	0.032	-0.020	0.056	-0.510****	-0.143	-0.418****
	(0.08)	(0.06)	(0.07)	(0.06)	(0.10)	(0.07)
Conservative	-0.204***	-0.335****	-0.118	0.688****	0.306**	0.621****
	(0.07)	(0.06)	(0.09)	(0.07)	(0.13)	(0.06)
Constant	-0.050	-0.118	-0.069	-0.091	-0.092	-0.027
	(0.13)	(0.16)	(0.16)	(0.12)	(0.16)	(0.12)
Controls	yes	yes	yes	yes	yes	yes
Obs.	974	974	974	949	960	953
No. Clusters	50	50	50	50	50	50
R2	0.096	0.156	0.091	0.357	0.164	0.368
Adj. R2	0.075	0.136	0.069	0.341	0.143	0.353
Tests (p-values)						
Lib. = Con.	0.000	0.001	0.120	0.000	0.014	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. The index for economic affectednes is based on three survey items related to the respondents' financial situation. The first of them asks whether respondents experienced changes of their income because of COVID-19 ("Have you lost income or gained income because of COVID-19, or has your income stayed stable?"), allowing for the answers "Lost more than 60 percent", "Lost 40 percent to 60 percent", "Lost 20 percent to less than 40 percent", "Lost less than 20 percent", "My income has stayed stable.", "Gained less than 20 percent", "Gained 20 percent to less than 40 percent", "Gained 40 percent to less than 60 percent", and "Gained 60 percent or more". Two further questions focus on the expectations (prior to the COVID-19 pandemic and currently) about the financial situation of the respondents' household for the next year ("Prior to the COVID-19 pandemic, when it came to the financial situation of your household, what were your expectations for the 12 months to come? Were you then expecting that the next 12 months be: better, worse, or the same?" and the second question "Now that the COVID-19 pandemic has arrived, when it comes to the financial situation of your household, what are your expectations for the 12 months to come, will the next 12 months be better, worse, or the same?"). Respondents could place their answer between 0 "Worse", 5 "The same", and 10 "Better". We calculated the difference between the expectations prior and during the pandemic for each individual in the sample, such that larger numbers correspond to worsened expectations. We multiplied the question on income changes by (-1) to align the meaning with the change in expectations, i.e. that larger numbers are associated with a worse economic situation. To build the index, we standardized the difference in expectations as well as the inversed score in the question on income changes, took the average of both standardized measures and standardized again. Due to 81 missing values in this variable, we do not add it in our main specification. However, results are equivalent as shown in the table.

Table 5.12: Regressions (OLS): Vulnerability and Worries about Infection

	(1)	(2)	(3)	(4)	(5)	(6)
Prosociality	0.089***	0.044	0.036**	0.083***	0.098****	0.095***
	(0.03)	(0.03)	(0.01)	(0.03)	(0.03)	(0.03)
Liberal	0.059	0.020	0.083*	-0.513****	-0.168*	-0.457****
	(0.08)	(0.05)	(0.04)	(0.06)	(0.10)	(0.07)
Conservative	-0.194**	-0.272****	-0.063	0.727****	0.371***	0.668****
	(0.07)	(0.05)	(0.05)	(0.06)	(0.11)	(0.06)
Vulnerability	0.189****	0.221****	0.205****	-0.034	0.113**	-0.001
	(0.04)	(0.04)	(0.03)	(0.04)	(0.06)	(0.04)
Worry Infection	0.032	0.257****	0.656****	0.074*	0.120**	0.102***
	(0.04)	(0.04)	(0.02)	(0.04)	(0.05)	(0.04)
Controls	yes	yes	yes	yes	yes	yes
Obs.	1039	1039	1039	1005	1013	1011
No. Clusters	50	50	50	50	50	50
R2	0.125	0.326	0.702	0.346	0.191	0.359
Adj. R2	0.104	0.310	0.695	0.330	0.172	0.344
Tests (p-values)						
Lib. = Con.	0.000	0.000	0.003	0.000	0.002	0.000

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

There is substantial heterogeneity concerning the governor's party affiliation at the time of the survey concerning trust in politicians at the state level. To investigate the heterogeneity, we interacted the political ideology indicators with a dummy equal to one when the governor was from the Republican Party in 2020. First, there is no significant difference in the reported evolution of trust in politicians at the state level between liberals and conservatives when the current governor is from the Democratic party (Lib. = Con.). Second, when the state's governor is from the Republican party, conservatives report a significantly better evolution of trust in politicians at the state level than liberals and are more likely to say that the relief has been timely and efficient (GOP + Con + Con $\times$ GOP = GOP + Lib + Lib $\times$ GOP Con + Con $\times$ GOP = Lib + Lib $\times$ GOP). Third, liberals living in a state where the governor is from the Republicans assess the trust development at the state level much more negatively than liberals in a Democratic-ruled state (GOP + Lib $\times$ GOP = 0). Fourth, conservatives living in a state with a Republican governor also assess the trust development at the state level more positively than conservatives in a Democratic-ruled state (GOP + Con $\times$ GOP = 0).

Table 5.13: Regressions (OLS): Heterogeneity State Governor Party Affiliation

	(1) Relief timely and efficient	(2) Trust increased (State)	(3) Trust increased (Nation)
Prosociality	0.085***	0.120****	0.106***
	(0.03)	(0.03)	(0.03)
Liberal	-0.566****	0.024	-0.462****
	(0.07)	(0.09)	(0.07)
Conservative	0.613****	0.024	0.627****
	(0.09)	(0.10)	(0.09)
GOP Governor	-0.115	-0.322***	-0.060
	(0.10)	(0.10)	(0.08)
Liberal × GOP Governor	0.111	-0.406**	0.007
	(0.12)	(0.16)	(0.14)
Conservative × GOP Governor	0.240**	0.691****	0.072
	(0.11)	(0.14)	(0.12)
Constant	-0.090	-0.003	-0.013
	(0.11)	(0.16)	(0.13)
Controls	yes	yes	yes
Obs.	1005	1013	1011
No. Clusters	50	50	50
R2	0.346	0.201	0.35
Adj. R2	0.329	0.181	0.334
Tests (p-values)			
Lib. = Con.	0.000	0.995	0.000
Lib. × GOP = 0	0.343	0.014	0.961
Con.× GOP = 0	0.036	0.000	0.546
GOP + Con. × GOP = 0	0.217	0.001	0.900
GOP + Lib. × GOP = 0	0.965	0.000	0.636
Lib. + Lib. × GOP = Con. + Con. × GOP	0.000	0.000	0.000
GOP = 0	0.239	0.002	0.453

Notes: The table shows OLS regression results. The dependent variables in the respective columns are (1) Relief timely and efficient, (2) Handling of crisis increased trust (State), (3) Handling of crisis increased trust (Nation). The dependent and independent variables (except dummies) are standardized. Each regression includes controls for age, age-squared, sex, ethnicities, income categories, education categories (respondent's and parents' attainment), dummy for parents immigrated, population, density, coronavirus deaths per 100k between the day of the survey and 7 days before the date of survey, date of survey. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.14: Ordered Logit Regressions: Main Results

	(1)	(2)	(3)	(4)	(5)	(6)
Prosociality	1.249***	1.226***	1.265****	1.170***	1.272****	1.274***
	(0.09)	(0.09)	(0.09)	(0.07)	(0.07)	(0.10)
Liberal	1.184	1.028	1.234*	0.321****	0.720*	0.353****
	(0.20)	(0.15)	(0.16)	(0.04)	(0.13)	(0.05)
Conservative	0.647***	0.498****	0.787*	4.823****	2.043****	4.106****
	(0.10)	(0.06)	(0.11)	(0.67)	(0.42)	(0.55)
Female	1.134	1.250	0.983	0.720***	0.681***	0.582****
	(0.13)	(0.18)	(0.16)	(0.07)	(0.09)	(0.07)
Controls	yes	yes	yes	yes	yes	yes
Obs.	1039	1039	1039	1005	1013	1011
No. Clusters	50	50	50	50	50	50
Tests (p-values)						
Lib. = Con.	0.000	0.000	0.017	0.000	0.001	0.000

Notes: The table shows ordered logit regression results. Odds ratios reported. The dependent variables in the respective columns are (1) Self-quarantine, (2) Face mask wearing, (3) Worry local spread, (4) Relief timely and efficient, (5) Handling of crisis increased trust (State), (6) Handling of crisis increased trust (Nation). The independent variables (except dummies) are standardized. Each regression includes controls for age, age-squared, sex, ethnicities, income categories, education categories (respondent's and parents' attainment), dummy for parents immigrated, population, density, coronavirus deaths per 100k between the day of the survey and 7 days before the date of survey, date of survey. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.15: Mediation: Ideology and Prosociality

Separately	Prosociality		Liberal		Conservative	
Self-quarantine	0.108****	(0.03)	0.069	(0.08)	-0.196**	(0.07)
Face mask wearing	0.081***	(0.03)	0.03	(0.05)	-0.299****	(0.06)
Worry local spread	0.121****	(0.03)	0.095	(0.06)	-0.111	(0.08)
Relief timely and efficient	0.114****	(0.03)	-0.511****	(0.06)	0.742****	(0.06)
Handling of crisis increased trust (State)	0.140****	(0.03)	-0.164*	(0.10)	0.371***	(0.11)
Handling of crisis increased trust (Nation)	0.129***	(0.04)	-0.454****	(0.07)	0.679****	(0.06)
Jointly	Prosociality		Liberal		Conservative	
Self-quarantine	0.113***	(0.03)	0.064	(0.08)	-0.219***	(0.08)
Face mask wearing	0.093***	(0.03)	0.026	(0.05)	-0.317****	(0.06)
Worry local spread	0.122****	(0.03)	0.09	(0.06)	-0.136*	(0.08)
Relief timely and efficient	0.087***	(0.03)	-0.514****	(0.06)	0.725****	(0.06)
Handling of crisis increased trust (State)	0.123****	(0.03)	-0.168*	(0.09)	0.347***	(0.11)
Handling of crisis increased trust (Nation)	0.106***	(0.03)	-0.460****	(0.07)	0.660****	(0.06)

Notes: The table shows regression coefficients of the variables in the heading row on the dependent variable indicated in the first column. The first 6 rows after the heading show coefficients from regressions where either only the prosociality index or only the ideology dummies are included. The last 6 rows show coefficients from regressions with both, the prosociality index and the ideology dummies, included. All regressions include the same control variables as the main regressions. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.16: Heterogeneity of Prosociality w.r.t. Ideology

	(1) Self-quarantine	(2) Face mask wearing	(3) Worry local spread
Prosociality	0.155***	0.081	0.101*
	(0.05)	(0.05)	(0.05)
Liberal × Prosociality	-0.036	-0.037	-0.163**
	(0.08)	(0.07)	(0.07)
Conservative × Prosociality	-0.068	0.037	0.097
	(0.07)	(0.05)	(0.07)
Liberal	0.060	0.024	0.080
	(0.08)	(0.06)	(0.06)
Conservative	-0.221***	-0.319****	-0.142*
	(0.08)	(0.06)	(0.08)
Controls	yes	yes	yes
Obs.	1039	1039	1039
No. Clusters	50	50	50
Tests (p-values)			
Prosociality: Con vs. Lib	0.641	0.206	0.004

Notes: The table shows regressions where the prosociality index is interacted with the dummies for liberal and conservative ideology. Below tests we report p-values of the tests against the null hypothesis that the effect of prosociality is equal for liberals and conservatives. All regressions include the same control variables as the main regressions. Standard errors (clustered at the state-level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively.

Table 5.17: Prosociality and Pandemic Intensity

	(1)	(2)	(3)	(4)
Dependent variable: Prosociality index				
Intensity	0.015	0.058	0.026	0.039
	(0.032)	(0.038)	(0.033)	(0.030)
Obs.	1118	1118	1118	1118
p-value	0.638	0.129	0.434	0.195
Dependent variable: Altruism (DG Transfer)				
Intensity	0.019	0.050	0.044	0.038
	(0.034)	(0.035)	(0.033)	(0.029)
Obs.	1118	1118	1118	1118
p-value	0.574	0.159	0.195	0.200
Dependent variable: Cooperation (PGG Contribution)				
Intensity	0.007	0.050	0.001	0.029
	(0.032)	(0.034)	(0.030)	(0.026)
Obs.	1118	1118	1118	1118
p-value	0.820	0.155	0.983	0.275
Obs.	1118	1118	1118	1118
p-value	0.820	0.155	0.983	0.275

Notes: The table shows OLS regression results. The dependent and independent variables are standardized. Standard errors (clustered at the state level) in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. All regressions control for age, age-squared, sex, income categories, education categories (respondent's and parents' attainment), and a dummy for parents immigrated. The pandemic intensity measure differs by column. Intensity is the number of coronavirus cases (deaths) per 100,000 inhabitants at the county level between the day of the survey and seven days before in column (1) ((2)). Intensity is the total number of coronavirus cases (deaths) per 100,000 inhabitants at the county level up to the survey date in column (3) ((4)). p-value is from a hypothesis test against the effect of pandemic intensity on the outcome being equal to zero.

Table 5.18: Differences in Prosociality between Political Camps

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
Wave	Wave 2	Wave 2	Wave 2	Wave 2	Wave 1	Wave 2	Wave 1	Wave 2	Wave 1
Dep.	Prosoc.	DG	PGG	Prosoc.	Prosoc.	DG	DG	PGG	PGG
Liberal	0.038	0.009	0.057						
	(0.079)	(0.080)	(0.081)						
Conservative	0.227***	0.297****	0.089						
	(0.072)	(0.073)	(0.071)						
Left-wing				0.026	-0.055	0.016	-0.064	0.029	-0.029
				(0.083)	(0.087)	(0.080)	(0.085)	(0.088)	(0.092)
Right-wing				0.155**	0.154**	0.226***	0.188**	0.038	0.074
				(0.076)	(0.076)	(0.074)	(0.078)	(0.077)	(0.073)
Obs.	1040	1040	1040	978	958	978	958	978	958
R2	0.040	0.060	0.019	0.034	0.016	0.051	0.013	0.021	0.021
Adj. R2	0.026	0.048	0.006	0.020	0.002	0.037	-0.001	0.007	0.007
p-value (Diff.)	0.017	0.000	0.699	0.131	0.032	0.016	0.007	0.917	0.317
Diff.	0.189	0.288	0.033	0.129	0.209	0.210	0.253	0.009	0.102

Notes: The table shows OLS regression results. The dependent and independent variables (except dummies) are standardized. Robust standard errors in parentheses. (****, ***, **, *) indicate two-sided p-values below 0.001, 0.01, 0.05, and 0.1 respectively. Each regression includes controls for age, age-squared, sex, income categories, education categories (respondent's and parents' attainment), and a dummy for parents who immigrated. "Diff." is the difference in SD of the dependent variable between conservatives and liberals (columns 1-3) and right-wing and left-wing respondents (columns 4-9). The dependent variable are the prosociality index in columns 1,4, and 5; the amount sent in the dictator game (altruism) in columns 2,6, and 7; the transfer in the public goods game in columns 3,8, and 9.

Table 5.19: WVS: Strong Leader Question

Country	Wave 3	Wave 7	Diff.	Rel. Diff.
United States	23.7	37.1	13.4	0.57
Albania	34.8	22.7	-12.1	-0.35
Belarus	48.5	61.6	13.1	0.27
Bulgaria	48.2	51.6	3.4	0.07
Croatia	28.8	38.2	9.4	0.33
Czech Rep.	14.8	24.7	9.9	0.67
Finland	25.8	14.6	-11.2	-0.43
Germany	13.4	20.9	7.5	0.56
United Kingdom	25.1	27.6	2.5	0.10
Hungary	17	21.1	4.1	0.24
Lithuania	57	50.5	-6.5	-0.11
Montenegro	21.7	66.9	45.2	2.08
Norway	13.8	14.6	0.8	0.06
Romania	40	72.6	32.6	0.82
Russia	42.6	39.4	-3.2	-0.08
Serbia	27.7	52	24.3	0.88
Slovakia	17.7	26.2	8.5	0.48
Slovenia	23.5	28.1	4.6	0.20
Spain	25.3	22.9	-2.4	-0.09
Sweden	25.7	18.9	-6.8	-0.26
Switzerland	26.2	20.9	-5.3	-0.20
Turkey	35.8	49.4	13.6	0.38
Ukraine	38.6	60.4	21.8	0.56

Notes: The table shows the share of respondents answering "Very good" or "Fairly good" to the question "I'm going to describe various types of political systems and ask what you think about each as a way of governing this country. For each one, would you say it is a very good, fairly good, fairly bad, or very bad way of governing this country?" by country and wave of the WVS as well as the difference between both waves (Diff.) and the difference relative to the share in the third wave (Rel. Diff.). The type of political system to be evaluated was "Having a strong leader who does not have to bother with parliament and elections". The third (seventh) wave of the WVS was conducted between 1995 and 1998 (2017 and 2020). Apart from the U.S., we included only European countries who were part of both the third and the seventh wave of the WVS.

Erklärung zum selbstständigen Verfassen der Arbeit

Ich erklare hiermit, dass ich meine Doktorarbeit *Essays on social cohesion, inequality, preferences for redistribution, and prosocial behavior* selbstständig und ohne fremde Hilfe angefertigt habe und dass ich als Koautor maßgeblich zu den weiteren Fachartikeln beigetragen habe. Alle von anderen Autoren wörtlich übernommenen Stellen, wie auch die sich an die Gedanken anderer Autoren eng anlehnenden Ausführungen der aufgeführten Beitrage wurden besonders gekennzeichnet und die Quellen nach den mir angegebenen Richtlinien zitiert.

Ort, Datum