Discriminatory Attitudes Against the Unvaccinated During a Global Pandemic

Alexander Bor^{1,2,*}, Frederik Jørgensen¹, and Michael Bang Petersen^{1,3,*}

¹Department of Political Science, Aarhus University, Denmark

²Democracy Institute, Central European University, Hungary

³Centre for the Experimental-Philosophical Study of Discrimination, Aarhus University, Denmark

*Corresponding authors: bora@ceu.edu / michael@ps.au.dk

December 8, 2022

During the COVID-19 pandemic sizeable groups of unvaccinated minorities persist even in countries with high vaccine access¹. Consequently, vaccination became a controversial subject of debate and even protest². Here, we assess whether people express discriminatory attitudes in the form of negative affect, stereotypes and exclusionary attitudes in family and political settings across groups defined by COVID-19 vaccination status. We quantify discriminatory attitudes between vaccinated and unvaccinated citizens in 21 countries, covering a diverse set of cultures across the world. Across three conjoint experimental studies (N=15,233), we demonstrate that vaccinated people express discriminatory attitudes towards the unvaccinated, as high as the discriminatory attitudes suffered by common targets like immigrant and minority populations^{3;4;5}. In contrast, there is an absence of evidence that unvaccinated individuals display discriminatory attitudes towards vaccinated people, except for the presence of negative affect in Germany and United States. Discriminatory attitudes against the unvaccinated is cross-culturally pervasive but also more strongly expressed in cultures with strong cooperative norms. Prior research on the psychology of cooperation has shown that individuals react negatively against perceived free-riders^{6;7} including in the domain of vaccinations^{8;9}. Consistent with this, the present findings suggest that contributors to the public good of epidemic control (i.e., the vaccinated) react with discriminatory attitudes against perceived free-riders (i.e., the unvaccinated). Elites and the vaccinated general public appealed to moral obligations to increase COVID-19 vaccine uptake^{10;11} but the present findings suggest that discriminatory attitudes including support for the removal of fundamental rights simultaneously emerged.

Please cite the final version of this paper published in Nature at
<https://www.nature.com/articles/s41586-022-05607-y>.

In a historical feat of science, highly effective vaccines against SARS-CoV-2 were developed, tested, approved, and mass produced in less than a year¹². Soon, however, it became clear that achieving sufficiently high uptake of these vaccines was in itself a major challenge¹³. Despite targeted vaccine mandates, vaccine passports and massive information campaigns, sizeable groups in several countries across the world continued to refuse to get vaccinated against COVID-19, even where vaccines were widely available¹. At the same time, many countries continued to employ interventions to control infection spread, resulting in feelings of “pandemic fatigue”, waning support for restrictions, and dwindling trust in authorities^{14;15;16}.

Against this backdrop, public debates around COVID-19 have been heated. Some politicians have justified strict policies against the unvaccinated using highly moralistic rhetoric¹⁰. At the same time, disruptive public protests directed against vaccine mandates have taken place in several Western countries². Survey research shows that divisions based on vaccination status are also emerging among the general public^{17;18}. Individuals who comply with the advice of health authorities morally condemn the unvaccinated for violating a social contract in the midst of a crisis^{11;8;9}. Those who refuse vaccines report that they feel discriminated¹⁸ and pressured against their will¹⁹. Furthermore, vaccination status is consistently aligned with other political opinions such as trust in science and the authorities, and, in the case of the US, partisanship^{13;20;9}.

Prior research documents that political divides can poison everyday interactions between citizens by eliciting general antipathy in the form of prejudice²¹. Here, we provide a cross-cultural empirical investigation of the nature and level of prejudice across groups defined by COVID-19 vaccination status, covering in total 21 countries across all inhabited continents. We follow Crandall and Eshleman²² and define prejudice as “a negative evaluation of an individual that is significantly based on the individual’s group membership” (p. 414, see also^{23;24}). Prejudice can manifest itself in affective (e.g., negative emotions), cognitive (e.g., negative stereotypes) and attitudinal expressions (e.g., support for exclusion and discrimination) of prejudiced individuals²⁵. Here, we investigate all three dimensions in the context of groups defined by COVID-19 vaccination status.

Research on the psychology of vaccination decisions prior to the COVID-19 pandemic⁸ and prior to the implementation of COVID-19 vaccines⁹ have shown that generosity in two-player be-

havioral economic games is indeed affected by the vaccination status of the players. Specifically, vaccinated individuals are less generous towards unvaccinated individuals but, importantly, unvaccinated are not less generous towards those vaccinated. These findings are interpreted on the basis of the psychology of human cooperation⁸. Research on cooperation has provided strong evidence that people monitor cooperative situations for the existence of free-riders (i.e., individuals who benefit from the cooperation without paying appropriate costs)²⁶ and react negatively towards free-riders upon detection^{6;7}. Vaccinations contribute to the public good of epidemic control²⁷ and refusal to receive a vaccination is accordingly spontaneously perceived as an instance of free-riding, motivating contributors (i.e., the vaccinated) to withhold generosity from the unvaccinated⁸. As the spontaneous withholding of resources from the unvaccinated may incentivize vaccination, health communicators have been advised that “making the social contract explicit may help to increase vaccine uptake rates without relying on mandates”⁸. On most normative grounds, it is unproblematic if people – as shown in prior research – are generous only towards cooperators and withhold personal resources from strangers known to free-ride²⁸.

Yet, in highly polarized contexts such as vaccinations during the COVID-19 pandemic, it is possible that these psychological processes shift in multiple important ways beyond the findings of prior research on vaccination status and generosity. First, research on the psychology of cooperation suggests that two distinct psychological motivations are activated in the context of public goods provisions: Motivations to generously offer rewards to contributors and motivations to punitively impose costs on free-riders⁶. While prior research focused on the former, it is plausible that the polarized and moralized sentiments surrounding COVID-19 vaccination activate the latter, punitive motivations too. Thus, vaccinated people may not only suspend their generosity towards the unvaccinated, but may also express support for the imposition of costs on the unvaccinated by, for example, supporting their exclusion from social relationships or democratic rights and freedoms. Second, in this context, the unvaccinated may react with prejudice towards the vaccinated as well, grounded, for example, in perceived pressure and discrimination^{18;19}. Indeed, the first study examining generosity in two-player behavioral economic games after the implementation of COVID-19 vaccines found that the unvaccinated were also less generous towards the vaccinated, although ingroup favoritism was smaller than among

the vaccinated¹⁸. Third, the complexity of the debates surrounding COVID-19 vaccinations may fuel negative stereotypes beyond the dimensions most relevant to cooperative dilemmas. For example, research on impression formation documents that warmth is one major dimension of impression formation, which is directly related to cooperativeness²⁹. Consistent with this, research prior to the COVID-19 pandemic finds that vaccinated individuals perceive the unvaccinated as less warm⁸. Research on impression formation, however, also documents that impressions of competence constitutes another and independent evaluative dimension²⁹. In the context of COVID-19 vaccines, this other dimension may also be activated as, for example, the vaccinated may perceive the unvaccinated as being unintelligent and incompetent for believing false information regarding vaccinations³⁰. Discriminatory attitudes in the context of COVID-19 vaccines may, therefore, come to have a broader cognitive basis.

Figure 1: World map highlighting the countries included in Study 1. Countries are colored by the share of vaccinated citizens in the population on the first day of data collection (2021 December–2022 January).

To empirically examine these possibilities, we leverage large-scale cross-national data. Specifically, we conducted three experimental studies in 21 countries (Study 1; $N = 64,440$ observations from 10,740 respondents), six countries (Study 2; $N = 18,270$ observations from 3,045 respondents) and in the United States (Study 3; $N = 14,480$ observations from 1,448 respondents), respectively, studying the affective, cognitive and attitudinal dimensions of prejudice across groups defined by COVID-19 vaccination status. The data set measures discriminatory attitudes across a diverse set of cultures from all inhabited continents of the world (see Fig-

ure 1). As prior research on lack of generosity towards the unvaccinated has been limited to Western democratic contexts^{18;8;9}, this cross-cultural dataset sheds light on both the ubiquity of discriminatory attitudes against perceived free-riders as well as on the cross-cultural predictors of variation in the strength of such attitudes. If discriminatory attitudes against people not vaccinated against COVID-19 reflects the activation of anti-free-rider sentiments, such attitudes may be more strongly expressed in countries that have invested significantly in the public good of suppressing deaths from COVID-19 and, in particular, in cultures where citizens hold moral expectations that their fellow citizens support the provisions of such goods.

Exclusion from family in 21 countries

Our initial examination focuses on cross-cultural exclusionary attitudes in the context of family relationships and, specifically, the level of antipathy if a close relative was marrying an unvaccinated (versus fully vaccinated) person. Such discriminatory attitudes in family relationships has been a key focus in prior cross-national research on prejudice along racial, ethnic and partisan lines^{31;21}. Exclusion from family relationships are cross-culturally relevant, independently of the legal and democratic traditions of the country; discrimination on the basis of membership in politicized groups within families has also been shown to be highly disruptive for the families³²; and, finally, relative to other forms of discriminatory attitudes (e.g., support for state-sponsored discrimination), discrimination within families is something within the control of individuals and, hence, something that can take immediate effect.

We employ conjoint experiments where respondents evaluate fictitious target profiles simultaneously randomized on six attributes including their COVID-19 vaccination status. The conjoint experimental design yields causal traction, provides a cost-effective method for collecting large samples and allows us to examine a wide-range of responses covering affective, cognitive and attitudinal components of prejudice³³.

Given our ambition to study discriminatory attitudes rather than generosity, we depart from prior work that relied on incentivized economic games (e.g., the Dictator Game)^{18;8;9}. To help assess the validity of the conjoint experimental approach, we performed a number of tests. First, we show that people perceive measures focusing on social interactions as more ecologically valid than those focusing on monetary transactions captured by economic games

(paired sample t-test: $\Delta M = 0.15$, 95% CI = [0.14, 0.16], $t(1447)=24.6$, $p < 0.001$, see Extended Data Figure 1 and Supplementary Information [SI] Section K for details), making the present findings less vulnerable to criticisms regarding ecological validity. Second, to assuage potential concerns about social desirability bias from self-reported measures, we report experimental evidence indicating that people readily and openly admit their antipathy towards vaccination outgroups, even using a traditional, direct survey question ($m = 44\%$, 95%CI = [0.40,0.48]). Indeed, this estimate of antipathy is not statistically different from the estimate we get using the forced response technique, specifically designed to alleviate social desirability ($m = 39\%$, 95%CI = [0.35,0.43], $\chi^2(1, 1210) = 2.31$, $p = 0.13$, see Extended Data Figure 2 and SI Section L). Finally, despite the presumed advantages of incentivized behavioral measures, we demonstrate that ingroup bias in generosity across vaccination groups is identical whether estimated with incentivized measures replicating prior research ($M = 29$; 95%CI [26, 32], one-sample $t(724) = 19.4$, $p < 0.001$) or with non-incentivized, self-reported measures ($M = 30$; 95%CI [28, 33], one-sample $t(722) = 21.0$, $p < 0.001$; $\Delta M = -1.45$, 95%CI [-5.5, 2.6], statistically equivalent to 0, TOST two-samples t-test, $t(1445) = 2.16$, $p < 0.05$, see details in SI section M).

In the conjoint experiment for Study 1, we adapted a widely used instrument of exclusionary reactions in family relations³¹ and examine a specific set of discriminatory attitudes: How unhappy respondents would be if a close relative was marrying an unvaccinated versus vaccinated person. Furthermore, we assess the potential cognitive bases for discriminatory attitudes. First, we measure a reasonable basis for antipathy towards vaccination outgroups, namely fear of infection³⁴. (We do note that during the collection of these studies, the vaccine-evading Omicron variant was dominant³⁵, and vaccine-induced immunity against infection spread was waning³⁶ in most societies. This increased the chances of being infected by vaccinated people and thus decreased the risk of interacting with unvaccinated individuals relative to vaccinated individuals.) While fear of infection is likely more pronounced among the vaccinated, some unvaccinated individuals have been found to hold the misinformed belief that vaccinated people themselves spread COVID-19 through vaccine shedding³⁷. Second, we assess the two key negative trait impressions underlying prejudice according to research on impression formation and prejudice: perceptions of untrustworthiness and unintelligence²⁹.

With the help of YouGov and Ipsos survey agencies, we collected high quality, quota-

sampled, original survey data from 21 countries that had widespread access to vaccines against COVID-19 (Study 1: 64,440 observations from 10,740 respondents). The data were collected in a diverse set of cultures from all inhabited continents of the world. As detailed in the Methods section "Study 1 - Data and generalizability", the samples can be considered representative of the countries' online populations (except for India). This large, cross-cultural dataset not only allows us to quantify discriminatory attitudes in a wide range of countries but also to investigate sources of cross-cultural variation in its levels. Note that our pre-registered analyses in Study 1 focus on antipathy towards outgroups, pooling across respondent vaccination status. Given that we find large asymmetries by vaccination status, we report below estimates separately for vaccinated and unvaccinated respondents. However, pooled estimates – reported in Supplementary Information (SI) Section F – mirror these results very closely given the relatively small share of unvaccinated respondents.

Our results reveal that vaccinated respondents ($N = 54,054$) exhibit exclusionary attitudes towards unvaccinated individuals (see Figure 2, left panel). On average, they are 13 (AMCE 95% CI [12, 14], $z = 25.65$, $p < 0.001$) percentage points more unhappy when presented with an unvaccinated (versus fully vaccinated) target. Country-level estimates range from 1 to 36 percentage points. We can reject the null (at 5% alpha-level) in 19 of the 21 countries. Malaysia is an outlier with very high exclusionary attitudes (AMCE = 36% points, 95% CI [32, 41], $z = 15.3$, $p < 0.001$), while results in Romania (AMCE=1% point, 95% CI [-4, 6], $z = 0.39$, $p = 0.69$) and Hungary (AMCE=4% points, 95% CI [-1, 9], $z = 1.74$, $p = 0.08$) are inconclusive (see more discussion in SI Section G). Interaction models estimating conditional AMCEs indicate that while we observe exclusionary attitudes across all demographic groups, they are slightly larger among highly educated (AMCE difference of 5% points), female (by 4% points), more affluent (by 3.5% points), and older (by 2% points) respondents (all $ps < 0.05$, see Section D in the Supplementary Information for details).

Meanwhile, unvaccinated respondents ($N = 10,386$) exhibit negligible exclusionary attitudes towards vaccinated individuals (see Figure 2, right panel). Their unhappiness is largely independent of the target's vaccination status, with an average marginal component effect of only -2 percentage points (AMCE 95% CI [-4, 0], $z = -1.81$, $p = 0.07$; the AMCE difference between vaccinated and unvaccinated respondents is 15% points 95%CI [13, 18], $z = 13.33$, $p < 0.001$).

Figure 2: The average level of exclusionary attitudes in family relationships towards vaccination outgroups (i.e., towards the unvaccinated for vaccinated respondents and towards the vaccinated for unvaccinated respondents. Total $N = 64,440$). Exclusionary attitudes reflect being unhappy if a close relative married a person from the vaccination outgroup versus ingroup, with more positive coefficients indicating more exclusionary attitudes towards the outgroup relative to the ingroup. Purple and orange points denote country-level average marginal component effect estimates ($Ns > 3,000$) for vaccinated and unvaccinated respondents, respectively. Black points denote the pooled sample and include an estimate for exclusionary attitudes towards immigrants from the Middle East too. Error bars denote 90 and 95% confidence intervals. For more details, see Study 1 - Modeling under the Methods section.

Country-level estimates of exclusionary attitudes exhibited by unvaccinated respondents are noisy due to the small sample sizes ($90 < N < 1500$), ranging between -31 and 10% points. Indeed, unvaccinated individuals in Malaysia, Italy, and Russia even exhibit significant exclusionary reactions towards other unvaccinated individuals ($ps < 0.01$), highlighting how anti-free rider sentiments may take priority over sentiments related to ingroup favoritism⁸.

To help assess the substantive size of these effects, it is helpful to compare them to exclusionary attitudes towards a group battling high levels of discrimination in many Western countries: Immigrants from the Middle East³. Exclusionary attitudes towards the unvaccinated among vaccinated people (13 percentage points) is two and a half times larger than exclusionary attitudes towards Middle Eastern immigrants (5 percentage points, 95%CI [5, 6], $\chi^2(1, N=54,054) = 23.83$, $p < 0.001$). We do not suggest that the characteristics of these groups are comparable but

this finding nonetheless suggests that the substantive size of the exclusionary reactions facing the unvaccinated is high. Figures E.1 and E.2 juxtapose country-level estimates of exclusionary attitudes towards the two groups. Unvaccinated targets face significantly more exclusionary reactions than immigrants in 11 out of 21 countries, while immigrants do not face significantly more exclusionary reactions in any of the countries. Notably, exclusionary attitudes towards immigrants between the vaccinated and unvaccinated are substantively similar and not significantly different from 0 ($N = 64,440$, AMCE difference 1% point, 95%CI[-1, 3], $z = 0.88$, $p = 0.38$) implying that asymmetry in the domain of vaccination cannot be easily explained by omitted variables or design effects (Figure E.3). Nor do we find evidence that unvaccinated immigrants from the Middle East face disproportionate exclusionary attitudes compared to unvaccinated natives ($N = 64,440$, AMCE difference 1% point, 95%CI[0, 1], $z = 1.1$, $p = 0.27$, see also Figure E.4).

Stereotypes and exclusionary attitudes

Next, we ask if exclusionary attitudes merely reflect a heightened risk of infection or also activate more fundamental stereotypes. As displayed in Extended Data Figure 3.A, we find large experimental effects of vaccination status among vaccinated respondents on fear of infection ($N = 54,054$, 38 percentage points, 95% CI: [37, 40], $z = 65.99$, $p < 0.001$) and perceptions of untrustworthiness (13 percentage points, 95% CI: [12, 14], $z = 27.36$, $p < 0.001$). However, we also find an effect on incompetence (14 percentage points, 95% CI: [13, 15], $z = 29.00$, $p < 0.001$), suggesting that stereotypes of the unvaccinated extend beyond the domain of free-riding. As unvaccinated respondents ($N = 10,386$) exhibit insubstantial exclusionary reactions, it is not surprising that they also do not judge vaccinated respondents as untrustworthy (0 percentage points, 95% CI: [-2, 2], $z = 0.2$, $p = 0.84$) or as incompetent (0 percentage points, 95% CI: [-2, 2], $z = 0.37$, $p = 0.71$) either. If anything, they fear getting infected with corona by vaccinated people slightly *less* than by unvaccinated people (-2 percentage points, 95% CI: [-5, 0], $z = -2.1$, $p < 0.05$). For country level estimates of negative stereotypes against vaccination outgroups, see SI Section C.

Our study also replicates a well-known finding from the impression formation literature: Impressions of trustworthiness have the largest impact on overall exclusionary attitudes²⁹. Judging

from a linear regression with respondent fixed-effects, exclusionary attitudes are more closely associated with impressions of (un)trustworthiness ($\beta = 0.24$, 95%CI [0.23,0.25]) than with impressions of (in)competence ($\beta = 0.17$, 95%CI [0.16,0.18], Wald-test for equal effects: $\chi^2(1, N=64,440) = 62.6$, $p < 0.001$), or even infection concerns ($\beta = 0.16$, 95%CI [0.15,0.17], Wald-test for equal effects: $\chi^2(1, N=64,440) = 112$, $p < 0.001$, see also Extended Data Figure 3.B). While concerns about infection risks do shape exclusionary attitudes towards the unvaccinated, these findings suggest that negative stereotypes further enhance these attitudes.

Figure 3: The relationship between country-level indicators and cross-national levels of exclusionary attitudes among the vaccinated towards the unvaccinated. The country-level indicators are country-level deaths from COVID-19; the national proportion of people expressing trust towards fellow citizens; the national proportion vaccinated against COVID-19; and cultural tightness scores. Labeled dots denote countries, straight black lines denotes best fitting regression lines, and gray curves denote loess curves. Spearman's rank-order correlations across the four facets: deaths $\rho(21) = -0.62$, 95%CI [-0.83, -0.26]; social trust $\rho(21) = 0.57$, 95%CI [0.19, 0.81]; vaccination $\rho(21) = 0.38$, 95%CI [-0.06, 0.70]; tightness $\rho(16) = 0.62$, 95%CI [0.18, 0.85]. Total $N = 64,440$.

Culture and exclusionary attitudes

The results provide strong evidence that exclusionary attitudes against perceived free-riders in the domain of vaccinations emerge reliably across cultures, reflecting the deep-seated nature of the psychology of cooperation⁶. At the same time, it is clear that the strength of the observed exclusionary attitudes exhibits substantial cross-cultural variation (see SI Section O.1 for formal evidence). Figure 3 displays exclusionary attitudes towards the unvaccinated by vaccinated respondents against three pre-registered macro-indicators—COVID-19 deaths, vaccinations (both standardized to population size), and social trust—as well as an exploratory indicator, cultural tightness. Whereas COVID-19 deaths and vaccination rates indicate society-wide investments in the public good of suppressing the epidemic, social trust (i.e., the tendency to trust fellow citizens) and cultural tightness (i.e., the strength of social norms and the degree of sanctioning within societies,³⁸) are indicators of the moral expectations of fellow citizens and the willingness to sanction violations of these expectations. Countries that managed to keep the death toll of the coronavirus low show very strong exclusionary attitudes towards the unvaccinated at around 20 percentage points on average. In contrast, countries that struggled to mitigate the epidemic show much lower exclusionary attitudes. Spearman’s rank order correlation between death and prejudice is $\rho(21) = -0.62$, 95%CI [-0.83, -0.26]. At the same time, the association of exclusionary attitudes with actual vaccination levels is inconclusive $\rho(21) = 0.38$, 95%CI [-0.06, 0.70]. Although there is a tendency for highly vaccinated nations to display more exclusionary attitudes, and countries with lower compliance to display less, there is also considerable deviations from this trend, with outliers like Argentina (high vaccination, little prejudice) and South Africa (high prejudice, low vaccination). In the SI Section G, we furthermore analyse policy stringency that is a direct measure of how much national governments invested in suppressing infections. We find no reliable association between stringency and prejudice towards the unvaccinated ($\rho(21) = 0.23$ 95%CI [-0.22, 0.6], see Extended Data Figure 4).

Prior research has demonstrated that epidemic suppression hinges on citizens’ normative and moral expectations such that countries with higher social trust³⁹ and a tighter culture⁴⁰ suppressed the epidemic toll more effectively. As observed in Figure 3, these cultural differences are also associated with higher prejudice towards the unvaccinated. Specifically, exclusionary attitudes are higher in countries with higher social trust (Spearman’s $\rho(21) = 0.57$, 95%CI

[0.19, 0.81]). In countries where large majorities believe that “most people can be trusted”, the exclusionary reaction towards the unvaccinated is larger. Meanwhile, in countries where most believe that “you need to be very careful in dealing with people”, exclusionary attitudes are lower. Similarly, exclusionary attitudes are higher in countries with a tighter culture, oriented towards strong norms and the sanctioning of norm violations ($\rho(16) = 0.62$, 95%CI [0.18, 0.85]). These latter associations suggest that cultures that place stronger moral expectations on individuals not only more effectively produce the public good of epidemic control^{39;40} but also constitute a fertile ground for exclusionary attitudes against the unvaccinated, as they may be perceived to free-ride on the collective effort⁸. In SI Section [O.2](#), we provide robustness checks for these cross-cultural conclusions, addressing potential threats to the generalizability of data obtained via online surveys.

Antipathy across six countries

In Study 2, we focus on the affective component of prejudice. Specifically, we conducted a pre-registered, conceptual replication of Study 1 and, in the context of a conjoint experiment, asked participants to rate fictitious individuals that vary in terms of vaccination status (as well as other attributes) on a seven-point like-dislike scale.

Study 1 also showed that exclusionary attitudes are intertwined with a fear of infection. While fear of infection is a weaker correlate of exclusionary attitudes than trustworthiness impressions, the finding nonetheless raises the possibility that prejudice against the unvaccinated may be restricted to relationships characterized by physical interaction. The focus on pure antipathy in a neutral evaluation task allows us to examine this possibility. Furthermore, to gain perspective on the size of antipathy across vaccination groups, Study 2 also changed the benchmark group from Middle Eastern migrants to a more diverse set of four groups, which are also frequent targets of prejudice: Drug addicts, ex-convicts, people with mental illnesses, and atheists^{41;4;5}. These groups were chosen to offer some variance on how much perceived control people have in determining their group membership and how much of a threat they pose on members of society.

With the help of YouGov survey agency, Study 2 was fielded in six countries (Germany, India, Indonesia, Morocco, South Africa, United Kingdom) representing both Western affluent

Figure 4: The average level of antipathy towards vaccination outgroups (i.e., towards the unvaccinated for vaccinated respondents and towards the vaccinated for unvaccinated respondents. Total $N = 18,270$). Antipathy reflects disliking a person from the vaccination outgroup versus the ingroup, with more positive coefficients indicating higher relative antipathy for the outgroup. Purple and orange points denote country-level average marginal component effect estimates ($Ns > 3000$) for vaccinated and unvaccinated respondents, respectively. Black points denote the pooled sample and include estimates for antipathy towards various other common targets of prejudice. Error bars denote 90 and 95% confidence intervals. For more details, see Study 2 - Modeling under the Methods section.

and non-Western developing nations. We recruited about 500 respondents per country, quota sampled on age, gender and region, as well as education in Germany and the UK (see details in SI Section A). As before, each participant rated three pairs of target profiles (Study 2: 3,045 respondents, 18,270 observations). All analyses, unless otherwise noted, were preregistered (see Data Availability Section for details).

Our data shows that the vaccinated feel antipathy towards the unvaccinated, even in a neutral evaluation task without any indication that participants would physically meet the fictitious targets (see Figure 4). Across all six countries, we find that vaccinated respondents ($N = 15,966$) dislike unvaccinated targets more than vaccinated targets, on average by 14 percentage points (AMCE 95% CI [13, 15], $z = 25.94$, $p < 0.001$). Conversely, unvaccinated respondents ($N = 2,304$) on average do not dislike vaccinated targets significantly more than

unvaccinated targets (AMCE = 1 percentage points, 95% CI [-1, 4], $z = 1.01$, $p = 0.31$, although Germany is a significant outlier, AMCE = 8% points, 95% CI = [3, 13], $z = 3.12$, $p < 0.001$). We should also note that the substantive size of the prejudice expressed towards the unvaccinated remains high relative to the more diverse set of benchmarks. On average across the six countries, the unvaccinated are disliked as much as people who struggle with drug addiction (15 percentage points, 95%CI [13, 16], Wald-test for equal effects: $\chi^2(1, N=15,966) = 0.51$, $p = 0.47$), and significantly more so than people who have been in prison (10 percentage points, 95%CI [9, 11], $\chi^2(1, N=15,966) = 18.4$, $p < 0.001$), who are atheists (7 percentage points, 95%CI [6, 8], $\chi^2(1, N=15,966) = 67.5$, $p < 0.001$), or who suffer from mental illness (6 percentage points, 95%CI [5, 7], $\chi^2(1, N=15,966) = 87.9$, $p < 0.001$). For country level estimates of prejudice towards each of the four benchmarks, see SI Section I.

Study 2 included an additional test. The finding from Study 1 (i.e., widespread existence of exclusionary attitudes in personal relationships) may be less concerning, if members of the groups of vaccinated and unvaccinated are only weakly acquainted across group boundaries and if – consistent with intergroup contact theory – prejudice is high only among individuals with less intergroup contact⁴². Study 2 therefore measured how many relatives and friends respondents have who belong to the vaccination outgroup. Analyses demonstrate that while antipathy is indeed highest among people with no contact with the outgroups ($N = 18,270$, AMCE = 15% points, 95% CI = [13, 16], $z = 20.36$, $p < 0.001$), it is substantial across all contact levels (AMCEs = 5%–12% points, z s ≥ 2.4 , p s < 0.05 , see Extended Data Figure 5 and SI Section J).

Restriction of rights in United States

So far, the discriminatory attitudes we have investigated have only been demonstrated in the domain of private relationships. Study 3 therefore examines whether discriminatory attitudes extend into the domain of publicly-recognized rights. As the recognition of such rights differs across cultures, Study 3 was conducted in United States, a country with historical recognition of fundamental rights and freedoms⁴³. Study 3 is identical to Study 2, except that the study also included five new outcomes in addition to the measure of antipathy and that answers were obtained on binary scales. Specifically, respondents were asked to evaluate the target's freedom

of movement (“This person should be allowed to sit next to me in public transportation”), freedom of residence (“This person should be allowed to move into my neighborhood”), freedom of speech (“This person should be allowed to express their political views on social media freely, without fear of censorship”), access to citizenship (“This person should receive US citizenship, if they are eligible and apply for it”), access to unemployment benefits (“This person should receive unemployment benefits, if they are eligible and apply for it”). We collected data via YouGov from 1,448 US Americans quota sampled on age, gender, region, education and race. Each respondent evaluated five pairs of targets yielding a final sample size of 14,480 observations. All analyses, unless otherwise noted, were preregistered (see Data Availability Section for details). The survey also included the methodological studies discussed in relation to Study 1, and reported in detail in SI Sections [K-M](#).

Figure 5: Affective and attitudinal prejudice against vaccination outgroups in the USA (i.e., towards the unvaccinated for vaccinated respondents and towards the vaccinated for unvaccinated respondents. $N = 14,480$). Prejudice reflects relative antipathy towards and support for restricting the rights and freedoms of the outgroup relative to the ingroup. More positive coefficients indicate higher prejudice. Purple and orange points denote average marginal component effects among vaccinated and unvaccinated respondents, respectively. Error bars denote 90 and 95% confidence intervals. For more details, see Study 3 - Modeling under the Methods section.

The results are displayed in Figure 5 and demonstrate that exclusionary attitudes are not

restricted to the domain of private relationships. Vaccinated Americans not only feel greater antipathy towards unvaccinated Americans by 16 percentage points (95%CI [14, 19], $z = 13.09$, $p < 0.001$), they are also 28 percentage points less likely to respect their freedom of movement (95%CI [25, 31], $z = 19.4$, $p < 0.001$), 10 percentage points less likely to respect their freedom of residence (95%CI [8, 12], $z = 9.1$, $p < 0.001$), 8 percentage points less likely to support their application for citizenship (95%CI [6, 10], $z = 7.98$, $p < 0.001$), and 7 percentage points less likely both to respect their freedom of speech and to support their applications for welfare benefits (95%CIs [5, 9], $z = 7.23$ and 7.44 , respectively, $ps < 0.001$). Vaccinated respondents express significantly higher exclusionary attitudes towards the unvaccinated than against atheists on all six outcomes, than against mentally ill on five outcomes, and than against targets who have been in prison or struggle with drug addiction on three outcomes. Conversely, they do not express significantly higher exclusionary attitudes towards any of the benchmark groups on any of the outcomes than against the unvaccinated (see details on all Wald-tests in SI Section I).

Study 3 results also indicate that unvaccinated Americans also harbor some negative sentiment towards the vaccinated (4 percentage points, 95%CI [1, 7]) but unvaccinated Americans are neither more nor less likely to restrict their rights or freedoms. Finally, additional analyses (see SI Section N) indicate that vaccinated Americans' antipathy towards the unvaccinated is predictive of their support for restricting the rights of the unvaccinated (Spearman's rank order correlations $0.35 < \rho_{s(1448)} < 0.44$).

Discussion

Research on political polarization warns that if socio-political disagreement – even if based on legitimate grievances – permeates interactions between citizens, it can contribute to the entrenchment of conflict²¹. In this study, we have documented that individuals vaccinated against COVID-19 express negative attitudes against unvaccinated individuals in the form of antipathy, stereotypes, support for exclusion from family relationships and support for removal of political rights. In total, these four forms of discriminatory attitudes are consistent with the observation of prejudice according to standard definitions in social psychology. We examined and obtained evidence in support of all four reactions in United States. In the other countries, we only examined some but not all forms of discriminatory attitudes and found evidence in support

of the specific negative reactions examined. The only exceptions were Hungary and Romania, where we did not find evidence in support of discriminatory attitudes. Furthermore, we find that discriminatory attitudes towards the unvaccinated is as high or higher than discriminatory attitudes directed towards other common and diverse targets of prejudice including immigrants, drug-addicts and ex-convicts. At the same time, the results demonstrate that prejudice is mostly one-sided. Only in United States and Germany do we find that the unvaccinated feel some antipathy towards the vaccinated but even here we do not find statistical evidence in favor of negative stereotyping or exclusionary attitudes.

The finding that vaccinated individuals are prejudiced against the unvaccinated but that there is no evidence for the reverse is consistent with work on the psychology of cooperation^{6;7} and prior work on vaccinations: The cue that someone refuses to take up a vaccine activates psychological mechanisms designed to deter perceived free-riders among the vaccinated^{8;9}. Consistent with the deep-seated nature of anti-free-rider sentiments, the observation of substantial and culturally discriminatory attitudes including support for denial of fundamental rights suggests that negative reactions are easily triggered in the context of perceived public goods. At the same time, the results also reveal that some cultures are especially prone to react with prejudice. Consistent with an anti-free-rider perspective, vaccinated individuals in cultures with stronger cooperative norms react more negatively against the unvaccinated. Such norms are more reliably associated with cross-cultural differences in discriminatory attitudes than are actual country-level differences in government efforts to produce epidemic control. What seems to trigger discriminatory attitudes towards the unvaccinated is less governments' efforts to reduce deaths from COVID-19 and more how such efforts resonate with larger cultural norms and perceived obligations.

In this regard, it is relevant to note that the decision to refuse vaccination against COVID-19 may reflect many factors beyond a moral failure to appreciate collective goals. A recent review of almost one hundred empirical studies identified eighteen robust correlates of COVID-19 vaccine hesitancy in high-income countries⁴⁴. Even if negative stereotypes are statistically true, they are unlikely to adequately capture the full motivations of every individual. For example, an unvaccinated person may have medical conditions⁴⁵, immunity from prior infection⁴⁶, a history of mental health issues that may intensify fear of vaccinations⁴⁷, negative past experiences with

health authorities (especially as a minority)⁴⁸, concerns due to country-specific public health scandals⁴⁹, or ethical considerations about vaccine equity⁵⁰.

While moralistic communication of collective responsibilities may be an effective strategy to increase vaccination uptake⁸, such strategies may have unintended negative consequences in the form of eliciting prejudice⁵¹, especially in cultures with strong cooperative norms. Research on prejudice towards minority groups warns that experiences of prejudice and discrimination may have negative long-term effects, hurting well-being⁵², eroding identification with majority society⁵³, and breeding mistrust of the state, including health authorities⁵⁴. If the consequences of prejudice towards the unvaccinated resemble the consequences of prejudice against minority groups, they may exacerbate the mistrust and alienation that led to vaccine refusal in the first place^{13;20}.

In the short run, prejudice towards the unvaccinated may complicate pandemic management. In the long run, it may mean that societies leave the pandemic more divided than they entered it. Finally, our findings also offer a lesson for global challenges beyond the current pandemic. Large social crises – for example, the climate crisis – are often characterized by collective action dilemmas due to the need for substantial behavior change among the general public⁵⁵. To effectively manage such crises, the authorities should seek to avoid fuelling deep animosity between citizens. Indeed, as moral condemnation is often easily and spontaneously activated among the general public during a crisis¹¹, the authorities and politicians should consider tempering social animosities as an important part of their mandate, especially when societal conflict becomes more entrenched.

References

- [1] Gianmarco Troiano and Alessandra Nardi. Vaccine hesitancy in the era of covid-19. *Public health*, 194:245–251, 2021.
- [2] Emilie Karafillakis, Pierre Van Damme, Greet Hendrickx, and Heidi J Larson. Covid-19 in europe: new challenges for addressing vaccine hesitancy. *The Lancet*, 399(10326):699–701, 2022.
- [3] Kirk Bansak, Jens Hainmueller, and Dominik Hangartner. How economic, humanitar-

- ian, and religious concerns shape european attitudes toward asylum seekers. *Science*, 354(6309):217–222, 2016.
- [4] Will M Gervais, Dimitris Xygalatas, Ryan T McKay, Michiel Van Elk, Emma E Buchtel, Mark Aveyard, Sarah R Schiavone, Ilan Dar-Nimrod, Annika M Svedholm-Häkkinen, Tapani Riekk, et al. Global evidence of extreme intuitive moral prejudice against atheists. *Nature Human Behaviour*, 1(8):1–6, 2017.
- [5] Bernice A Pescosolido, Tait R Medina, Jack K Martin, and J Scott Long. The “backbone” of stigma: identifying the global core of public prejudice associated with mental illness. *American Journal of Public Health*, 103(5):853–860, 2013.
- [6] Michael E Price, Leda Cosmides, and John Tooby. Punitive sentiment as an anti-free rider psychological device. *Evolution and Human Behavior*, 23(3):203–231, 2002.
- [7] Ernst Fehr and Simon Gächter. Altruistic punishment in humans. *Nature*, 415(6868):137–140, 2002.
- [8] Lars Korn, Robert Böhm, Nicolas W Meier, and Cornelia Betsch. Vaccination as a social contract. *Proceedings of the National Academy of Sciences*, 117(26):14890–14899, 2020.
- [9] Ori Weisel. Vaccination as a social contract: The case of covid-19 and us political partisanship. *Proceedings of the National Academy of Sciences*, 118(13):e2026745118, 2021.
- [10] Frederik Jørgensen, Alexander Bor, and Michael Bang Petersen. Increased pressure leads to decreased trust among the unvaccinated: Effects of the announcement of the re-introduction of covid passports in denmark. *PsyArXiv*, 2021.
- [11] Alexander Bor, Frederik Jørgensen, Marie Fly Lindholt, and Michael Bang Petersen. Moralizing the covid-19 pandemic: Self-interest predicts moral condemnation of other’s compliance, distancing and vaccination. *Political Psychology*, 2022.
- [12] Shivaji Kashte, Arvind Gulbake, Saadiq F El-Amin III, and Ashim Gupta. Covid-19 vaccines: rapid development, implications, challenges and future prospects. *Human cell*, 34(3):711–733, 2021.

- [13] Marie Fly Lindholt, Frederik Jørgensen, Alexander Bor, and Michael Bang Petersen. Public acceptance of covid-19 vaccines: cross-national evidence on levels and individual-level predictors using observational data. *BMJ open*, 11(6):e048172, 2021.
- [14] Frederik Jørgensen, Alexander Bor, Magnus S Rasmussen, Marie F Lindholt, and Michael Bang Petersen. “pandemic fatigue” fueled political discontent: Evidence from 8 western democracies over 11 months of the covid-19 pandemic. *PsyArXiv*, Oct 2021.
- [15] Alexander Bor, Frederik Jørgensen, and Michael Bang Petersen. The covid-19 pandemic eroded system support but not social solidarity. *PsyArXiv*, 2021.
- [16] Lau Lilleholt, Ingo Zettler, Cornelia Betsch, and Robert Böhm. Pandemic fatigue: Measurement, correlates, and consequences. *PsyArXiv*, 2020.
- [17] Julian Schuessler, Peter Thisted Dinesen, Søren Dinesen Østergaard, and Kim Mannemar Sønderskov. Public support for unequal treatment of unvaccinated citizens: Evidence from denmark. *Social Science & Medicine*, page 115101, 2022.
- [18] Luca Henkel, Philipp Sprengholz, Lars Korn, Cornelia Betsch, and Robert Böhm. Understanding the trouble spot: Does vaccination status identification fuel societal polarization?, Jan 2022.
- [19] Daniel L Rosenfeld and A Janet Tomiyama. Jab my arm, not my morality: Perceived moral reproach as a barrier to covid-19 vaccine uptake. *Social Science & Medicine*, page 114699, 2022.
- [20] Michael Bang Petersen, Alexander Bor, Frederik Jørgensen, and Marie Fly Lindholt. Transparent communication about negative features of covid-19 vaccines decreases acceptance but increases trust. *Proceedings of the National Academy of Sciences*, 118(29):e2024597118, 2021.
- [21] Shanto Iyengar, Yphtach Lelkes, Matthew Levendusky, Neil Malhotra, and Sean J Westwood. The origins and consequences of affective polarization in the united states. *Annual Review of Political Science*, 22:129–146, 2019.
- [22] Christian S Crandall and Amy Eshleman. A justification-suppression model of the expression and experience of prejudice. *Psychological Bulletin*, 129(3):414, 2003.

- [23] John Dixon, Mark Levine, Steve Reicher, and Kevin Durrheim. Beyond prejudice: Are negative evaluations the problem and is getting us to like one another more the solution? *Behavioral and Brain Sciences*, 35(6):411–425, 2012.
- [24] James L Gibson, Christopher Claassen, and Joan Barceló. Putting groups back into the study of political intolerance. In Eugene Borgida, Christopher M. Federico, and Joanne M. Miller, editors, *At the Forefront of Political Psychology: Essays in Honor of John L. Sullivan*, pages 55–78. Routledge, 2020.
- [25] Charles Stangor, Linda A Sullivan, and Thomas E Ford. Affective and cognitive determinants of prejudice. *Social cognition*, 9(4):359–380, 1991.
- [26] Leda Cosmides and John Tooby. Neurocognitive adaptations designed for social exchange. *The handbook of evolutionary psychology*, pages 584–627, 2015.
- [27] Rafael Ahlskog. *Essays on the collective action dilemma of vaccination*. PhD thesis, Acta Universitatis Upsaliensis, 2017.
- [28] Frederick Schauer. *Profiles, probabilities, and stereotypes*. Harvard University Press, 2006.
- [29] Susan T. Fiske, Amy J. C. Cuddy, and Peter Glick. Universal dimensions of social cognition: warmth and competence. *Trends in Cognitive Sciences*, 11(2):77–83, 2007.
- [30] Stephen R Neely, Christina Eldredge, Robin Ersing, and Christa Remington. Vaccine hesitancy and exposure to misinformation: a survey analysis. *Journal of General Internal Medicine*, 37(1):179–187, 2022.
- [31] Emory S Bogardus. Measurement of personal-group relations. *Sociometry*, 10(4):306–311, 1947.
- [32] Rachael A Hernandez and Colleen Colaner. “this is not the hill to die on. even if we literally could die on this hill”: Examining communication ecologies of uncertainty and family communication about covid-19. *American Behavioral Scientist*, 65(7):956–975, 2021.
- [33] Jens Hainmueller, Dominik Hangartner, and Teppei Yamamoto. Validating vignette and conjoint survey experiments against real-world behavior. *Proceedings of the National Academy of Sciences*, 112(8):2395–2400, 2015.

- [34] Arthur L Caplan. Stigma, vaccination, and moral accountability. *The Lancet*, 399(10325):626–627, 2022.
- [35] Frederik Plesner Lyngse, Laust Hvas Mortensen, Matthew J Denwood, Lasse Engbo Christiansen, Camilla Holten Møller, Robert Leo Skov, Katja Spiess, Anders Fomsgaard, Ria Lassauniere, Morten Rasmussen, et al. Sars-cov-2 omicron voc transmission in danish households. *medRxiv*, 2021.
- [36] Alexandra M Hogan, Sean L Wu, Patrick Doohan, Oliver John Watson, Peter Winskill, Giovanni Charles, Greg Barnsley, Eleanor M Riley, David S Khoury, Neil M Ferguson, et al. The value of vaccine booster doses to mitigate the global impact of the omicron sars-cov-2 variant. *medRxiv*, 2022.
- [37] Centers for Disease Control and Prevention. Myths and facts about covid-19 vaccines. <https://www.cdc.gov/coronavirus/2019-ncov/vaccines/facts.html>, 2021. Accessed: 2022-02-16.
- [38] Michele J Gelfand, Jana L Raver, Lisa Nishii, Lisa M Leslie, Janetta Lun, Beng Chong Lim, Lili Duan, Assaf Almaliach, Soon Ang, Jakobina Arnadottir, et al. Differences between tight and loose cultures: A 33-nation study. *Science*, 332(6033):1100–1104, 2011.
- [39] Thomas J Bollyky, Erin N Hulland, Ryan M Barber, James K Collins, Samantha Kiernan, Mark Moses, David M Pigott, Robert C Reiner Jr, Reed JD Sorensen, Cristiana Abbafati, et al. Pandemic preparedness and covid-19: an exploratory analysis of infection and fatality rates, and contextual factors associated with preparedness in 177 countries, from jan 1, 2020, to sept 30, 2021. *The Lancet*, 399(10334):1489–1512, 2022.
- [40] Michele J Gelfand, Joshua Conrad Jackson, Xinyue Pan, Dana Nau, Dylan Pieper, Emmy Denison, Munqith Dagher, Paul AM Van Lange, Chi-Yue Chiu, and Mo Wang. The relationship between cultural tightness–looseness and covid-19 cases and deaths: a global analysis. *The Lancet Planetary Health*, 5(3):e135–e144, 2021.
- [41] Christian S Crandall, Amy Eshleman, and Laurie O’Brien. Social norms and the expression and suppression of prejudice: the struggle for internalization. *Journal of Personality and Social Psychology*, 82(3):359–378, 2002.

- [42] Thomas F Pettigrew. Intergroup contact theory. *Annual Review of Psychology*, 49(1):65–85, 1998.
- [43] Freedom House. United states contry report. <https://freedomhouse.org/country/united-states/freedom-world/2022>, 2022. Accessed: 2022-11-01.
- [44] Junjie Aw, Jun Jie Benjamin Seng, Sharna Si Ying Seah, and Lian Leng Low. Covid-19 vaccine hesitancy—a scoping review of literature in high-income countries. *Vaccines*, 9(8):900, 2021.
- [45] Jill Harrison, Sarah Berry, Vince Mor, and David Gifford. “somebody like me”: understanding covid-19 vaccine hesitancy among staff in skilled nursing facilities. *Journal of the American Medical Directors Association*, 22(6):1133–1137, 2021.
- [46] Sundhedsstyrelsen. Statusreport 2021. aug. 23. – covid-19 vaccination [in danish]. https://www.sst.dk/-/media/Udgivelser/2021/Corona/Vaccination/Statusrapport/22_-statusraport-data-per-23_-aug.ashx, 2021. Accessed: 2022-02-16.
- [47] Jamie Murphy, Frédérique Vallières, Richard P Bentall, Mark Shevlin, Orla McBride, Todd K Hartman, Ryan McKay, Kate Bennett, Liam Mason, Jilly Gibson-Miller, et al. Psychological characteristics associated with covid-19 vaccine hesitancy and resistance in ireland and the united kingdom. *Nature Communications*, 12(1):1–15, 2021.
- [48] Mohammad S Razai, Tasnime Osama, Douglas GJ McKechnie, and Azeem Majeed. Covid-19 vaccine hesitancy among ethnic minority groups. *BMJ*, 372, 2021.
- [49] Shufang Sun, Danhua Lin, and Don Operario. Interest in covid-19 vaccine trials participation among young adults in china: Willingness, reasons for hesitancy, and demographic and psychosocial determinants. *Preventive Medicine Reports*, 22:101350, 2021.
- [50] Elaine Robertson, Kelly S Reeve, Claire L Niedzwiedz, Jamie Moore, Margaret Blake, Michael Green, Srinivasa Vittal Katikireddi, and Michaela J Benzeval. Predictors of covid-19 vaccine hesitancy in the uk household longitudinal study. *Brain, Behavior, and Immunity*, 94:41–50, 2021.
- [51] Günter Kampf. Covid-19: stigmatising the unvaccinated is not justified. *The Lancet*, 398(10314):1871, 2021.

- [52] Michael T Schmitt, Nyla R Branscombe, Tom Postmes, and Amber Garcia. The consequences of perceived discrimination for psychological well-being: a meta-analytic review. *Psychological Bulletin*, 140(4):921–948, 2014.
- [53] Nyla R Branscombe, Michael T Schmitt, and Richard D Harvey. Perceiving pervasive discrimination among african americans: Implications for group identification and well-being. *Journal of Personality and Social Psychology*, 77(1):135–149, 1999.
- [54] Lillie D Williamson, Marisa A Smith, and Cabral A Bigman. Does discrimination breed mistrust? examining the role of mediated and non-mediated discrimination experiences in medical mistrust. *Journal of health communication*, 24(10):791–799, 2019.
- [55] Mélusine Boon-Falleur, Aurore Grandin, Nicolas Baumard, and Coralie Chevallier. Leveraging social cognition to promote effective climate change mitigation. *Nature Climate Change*, 12(4):332–338, 2022.
- [56] Nic Newman, Richard Fletcher, Anne Schulz, Simge Andi, Craig T Robertson, and Rasmus Kleis Nielsen. Reuters institute digital news report 2021. *Reuters Institute for the study of Journalism*, 2021.

Methods

Study 1

Data and generalizability

We collected data between December 3, 2021 and January 28, 2022 from 21 countries: Argentina, Australia, Austria, Brazil, China, Denmark, France, Germany, Hungary, India, Indonesia, Italy, Malaysia, Mexico, Morocco, Romania, Russia, South Africa, Spain, the United Kingdom, and the United States. Data was collected through online panels by Ipsos in China, and by YouGov in all other countries. All participants provided informed consent and were reimbursed according to their standing agreements with the data provider. All studies (Studies 1-3) were exempt from formal ethical review by Danish law. As per section 14(2) of the Act on Research Ethics Review of Health Research Projects, “notification of questionnaire surveys ...

to the system of research ethics committee system is only required if the project involves human biological material.” The studies fully comply with Aarhus University’s Code of Conduct and with the ethical standards set by the Danish Code of Conduct for Research Integrity.

We sought to recruit 500 adult respondents from each country, quota sampling on age, gender, and region of residence, and – conditional on feasibility – also education (in Australia, Brazil, Denmark, France, Germany, Italy, Mexico, Russia, Spain, UK, and US) and race (in the US). See Extended Data Table 1 for demographic details. Quotas are always set to mimic the national population, except in Indonesia, Morocco, and Malaysia, where due to feasibility issues, they are set to the demographic characteristics of the online population and, in India, where they are set to the demographic characteristics of the national urban population. Respondents who failed a simple bot test were screened out at the beginning of the survey. For additional demographic information, as well as more details on the quotas set, see Section A in the OA. Questionnaires were translated to the official language of the country by professional translators employed by a translation agency (see deviations from this rule in SI Section B). Each translation was independently quality checked by another translator at the agency, and a native speaker recruited by the researchers.

Our samples cover a diverse set of cultures from all inhabited continents of the world. That said, our sample intentionally excludes the poorest countries where COVID-19 vaccines were not yet widely available to the public and where, accordingly, we would not expect vaccination status to lead to prejudice. Furthermore, despite the quotas set, our samples are not fully nationally representative as they exclude parts of society who have no internet access, or face other systemic disadvantages (e.g., are illiterate or do not speak the official language of the country). Prior research thus concludes that for results from cross-national data collected via YouGov’s online panels in low- and middle-income countries “it is better to think of [them] as representative of the online population.”⁵⁶. At the same time, during a pandemic, online surveys constitute a safe and efficient data collection method that allows voices from diverse cultures to be heard. Consistent with this, the World Health Organization refers to the use of online surveys as “the standard approach” for behavioral insights during the pandemic⁵⁷. Our main survey vendor, YouGov, aided Imperial College London to capture global behavioral dynamics during the pandemic⁵⁸.

Prior research suggests that cross-cultural differences can be reliably studied using online surveys^{59;60;61} and that studies using experimental designs (as we do) are particularly robust across a variety of sampling methods^{62;63}. Nonetheless, it is important to acknowledge and address limitations to generalizability of online surveys. The key threat in this regard is whether differences between online and national populations may endanger the robustness of the cross-cultural conclusions. To examine the consequences of this threat to inference, we report multiple robustness checks of the data and analyses. First, SI Section A compares the most relevant objective benchmark, actual vaccinations against COVID-19 in the adult national population, against those observed in the surveys, finding high correspondence. Second, SI Section D directly examines treatment heterogeneity in two of the key indicators associated with internet access, education and income, and finds very little treatment heterogeneity, even in low- and middle-income countries (see Extended Data Figure 6). Third, SI Section O.2 directly quantifies the potential threat to inference and examines the robustness of the cross-cultural conclusions to potential differences in prejudice between online and offline populations. It finds that the cross-cultural conclusions are robust to even the extreme assumption that offline population hold *zero* prejudice against the unvaccinated (see Extended Data Figure 7). Fourth, SI Section O.2 also reports a stress test, which examines the consequences of simultaneous violations of our two conjectures that (a) our samples represent the online populations and (b) that the represented and non-represented populations show similar prejudice. This test finds that the conclusion of cross-culturally pervasive prejudice would hold even if both of our conjectures were wrong. Overall, both prior work and extensive robustness analyses strongly suggest that our conclusions hold as stated.

Experimental design

Our design is a subtle, conjoint experimental implementation of Bogardus³¹ classic social distance scale. We presented participants with brief descriptions of a series of fictitious individuals and asked them to imagine that these are people whom one of their close relatives intends to marry. One of the six attributes describing these target individuals has been their COVID-19 vaccination status, randomly varying between “fully vaccinated” and “unvaccinated.” Importantly, this is a minimalist manipulation of vaccination status, simply labeling target individuals

with their group membership and thus offering no reason or justification for their choice.

We were interested whether participants would have higher exclusionary attitudes against unvaccinated individuals marrying into their families. To benchmark the size of this hypothesised prejudice, another attribute has been labeled “family background” and distinguished between people “born and raised in [the respondent’s country]” and people who “immigrated from the Middle East.” Middle Eastern immigrants serve as an excellent benchmark as widespread prejudice against them has been widely documented^{3,1}. The other four attributes (age, occupation, hobbies, and personality) were included to increase ecological validity and to reduce experimenter demand and social desirability. All in all, we collected data from 10,740 individuals.

Extended Data Table 2 displays the six attributes and their levels. Each target profile was sampled completely at random from the $2 \times 2 \times 6 \times 6 \times 6 \times 5 = 4,320$ unique combinations of attribute levels. Following best practices in the literature³³, we presented two targets at a time, side by side. Each respondent rated six random targets independently across three trials, yielding a total sample size of 64,440 observations. We also randomized between respondents the order in which the attributes appeared.

Measures

Respondents rated each target profile independently by indicating their agreement or disagreement with a series of four statements on a simple yes/no scale. Specifically, we measured respondents’ *exclusionary attitudes* with the statement, “I would be unhappy if this person married one of my close relatives”; *fear of infection* with the statement, “I would be afraid that this person infected me or my family with COVID-19”; perceptions of *intelligence* with “I think this person is unintelligent”; and *trustworthiness* with “I think this person is untrustworthy.”

We also collected background information on all respondents. Most importantly, prior to the treatment, we asked whether respondents themselves were vaccinated or not. We label all respondents who received at least one vaccine as “vaccinated,” and all other respondents, including those who refused to answer the question, as “unvaccinated.” We also rely on demographic

¹That said, this does not mean that all countries in our sample must be prejudiced against Middle Eastern immigrants. While it has been important for us that none of our samples come from the Middle East (as it would render the attribute meaningless), factors like a shared religious identity may temper prejudice against Middle Easterners. To minimize this latter effect, we deliberately avoided using the term “Muslim”.

data shared by the survey provider, which we dichotomized into male and female respondents, older and younger respondents (by splitting at the sample median in each country), respondents with and without a completed higher (tertiary) education, and finally, poor respondents with a gross household income below 70% of the national median and not poor respondents.

Finally, our analyses rely on a series of country-level predictors. We measure pandemic severity with the cumulative number of confirmed COVID-19 *deaths* per 100K people in the total population on the first day of data collection in the country as measured by Johns Hopkins University. We measure the *vaccination rate* of the country with the total number of people who received at least one vaccine dose per 100 people in the total population on the first day of data collection in the country as measured by Our World in Data. We measure *social trust* with the proportion of respondents who said “most people can be trusted” (versus “you need to be very careful in dealing with people”) in the latest World Values Survey data available for the country. Finally, we use cultural tightness-looseness scores from³⁸ as a (post-hoc) predictor of prejudice against the unvaccinated. Note that tightness scores are available for 16 out of our 21 countries, thus Denmark, France, Morocco, Romania and South Africa are omitted from these analyses.

Modeling

Following standard practices in the literature on conjoint experiments⁶⁴, we analyze our data with OLS regression models regressing one-by-one the four outcomes on the six categorical attributes. The models include post-stratification weights. We cluster standard errors on respondents. Our four hypotheses are evaluated on the average marginal component effect (AMCE) of vaccination attribute (scaled to indicate outgroups) on the four outcomes. AMCEs can be interpreted as the percentage point change in the proportion of respondents reporting exclusionary attitudes, perceived untrustworthiness, unintelligence, or fear of infection, *caused* by changing a target’s vaccination status from ingroup to outgroup. All significance tests reported in the manuscript – unless otherwise noted – are two-sided.

To estimate country-level effects, we rerun these models in each of the 21 countries separately. To estimate heterogeneities in prejudice between vaccinated and unvaccinated respondents, as well as between demographic groups, we rerun models on split samples. To estimate

country-level relationships between exclusionary attitudes and macro-level indicators, we rely on descriptive plots and Spearman’s rank order correlations.

Our identification strategy rests on the random assignment of vaccination status to target individuals⁶⁴. We report the standard diagnostic tests for conjoint experiments in SI Section O.6. We find little reason for concern, although we acknowledge that participants speeding through the experiment dilute the observed experimental effects (see Figure O.21) and that there are some carry-over effects for exclusionary attitudes but not the other three outcomes (see Figure O.24). We also note that insofar as some of our respondents falsely claim to be vaccinated, our estimates of prejudice towards the unvaccinated are likely to be too conservative.

Finally, in SI Section O, we report robustness tests. All our conclusions replicate when our regressions are implemented in a Bayesian multilevel framework, when we drop post-stratification weights, and when we exclude respondents claiming no prior opportunity to get vaccinated.

Study 2

Study 2 sought to conceptually replicate and extend the results of Study 1. First, it relies on an alternative, purely affective measure of prejudice, which cannot be confounded by concerns of infection risk. Second, it uses an alternative set of benchmark groups to get additional perspective on the substantive size of the prejudice faced by the unvaccinated. Third, it tests whether antipathy against vaccination outgroups is lower among people who have more contact with members of the outgroup. Finally, it conceptually replicates our findings in a period after the omicron wave has receded and when concerns about the pandemic were less outspoken.

Data and design

Our data was collected in May, 2022 from six countries: Germany, India, Indonesia, Morocco, South Africa, and the UK. As before, our data provider, YouGov, quota sampled minimum 500 respondents per country from online panels. All participants provided informed consent and were reimbursed according to their standing agreements with the data provider. The study was exempt from formal ethical review (see above under Study 1).

The design of Study 2 closely mirrors the conjoint experimental design described above for Study 1. For the sake of brevity, we therefore focus on deviations here. First, we omitted

the framing of the relationship between respondent and target as a prospective close family member. Instead, we simply presented target individuals to respondents, whom they were asked to evaluate on a standard like-dislike scale. Second, we replaced the family background attribute with a new one called personal information. Under this inconspicuous label, we included references to membership in one of four groups, which are well documented for facing (various levels of) prejudice: drug addicts, the mentally ill, convicts and atheists. As a neutral comparison, the attribute also had a control condition – “no additional information”. These four groups intentionally vary in the extent membership is conditional on personal choice versus luck, and whether they pose a danger on others. For the detailed description of all attributes (including two minor changes on background attributes), see SI Section B. Finally, beyond personal vaccination status, we also measure the personal experience of contact with vaccination outgroups and model the antipathy towards outgroups conditional on this variable.

As before, the target profiles are sampled completely at random from the $2 \times 5 \times 6 \times 5 \times 6 \times 5 = 9,000$ unique combinations of attribute levels (see details in Extended Data Table 3). Each respondent evaluated three pairs of targets. This yields a final sample size is 18,270 observations from 3,045 individuals. All hypotheses, materials and analyses were pre-registered at osf.io/a7hsu.

Measures

To measure contact with vaccination outgroup members we asked, pre-treatment, how many relatives and friends do [respondents] have who are [not] vaccinated against COVID-19? The response options were: None at all, 1-2, 3-5, 6-10, More than 10. The question always referred to the outgroup, based on a measure and categorization of personal vaccination status identical to the one used in Study 1.

General impressions of the targets were measured on a standard seven-point Likert scale from strongly dislike to strongly like. Respondents were prompted for each target to indicate, how much do [they] like or dislike person [A-F].

Modeling

We followed the same modeling strategy as in Study 1. We recoded the continuous dependent variables to the 0-1 range, with higher values indicating more dislike. To investigate if

respondents with more contact with outgroups express lower antipathy towards them, we pre-registered an interaction model, estimating antipathy conditional on contact levels, treated as a categorical variable with no contact as the reference category.

Study 3

The primary ambition of Study 3 was 1) to extend previous results relying on a wider range of outcome measures tapping into various forms of prejudice. Besides, it also included two additional experiments, relying on alternative paradigms for measuring prejudice and generosity across groups. Accordingly, 2) we tested whether people are less generous with unvaccinated others in an economic games both with and without monetary incentives^{8;9;18}. 3) We also measure prejudice using Bogardus' family context, both with a standard direct question and employing a forced response technique⁶⁵. Relying on these data, 4) we can investigate if social desirability biases the propensity to admit prejudice towards vaccination outgroups. Finally, 5) we collect data to understand if social interactions as a context for studying negative attitudes across vaccination outgroups are less vulnerable to criticism regarding ecological validity than standard economic games. All hypotheses, materials and analyses were pre-registered at osf.io/ypc6a.

Data and design

We collected data from 1,448 adults living in the USA in May, 2022 (simultaneously with Study 2). As before, respondents were recruited from YouGov's online panel using quota sampling on gender, age, region, education, and race. All participants provided informed consent and were reimbursed according to their standing agreements with the data provider. The study was exempt from formal ethical review (see above under Study 1).

The conjoint experimental design was identical to that of Study 2, except each participants rated five pairs of target profiles. This yielded a final sample size of 14,480 observations in the conjoint experiment. The study also included two additional experiments. First, replicating Henkel et al.¹⁸, respondents participated in a Dictator Game, where an allocator can give some of their 100 points endowment to another player, the recipient. All participants played in the role of the allocator, and were randomly matched with another respondent in the survey (post

hoc), about whom they only knew whether they are vaccinated or unvaccinated against COVID-19. We used the strategy method and elicited an allocation for both types of partners (in a random order). We experimentally manipulated between subjects, whether participants played for a monetary incentive. Specifically, we informed a random half of the participants that the points they divide in the game are worth money at a rate of 100 points = 250 YouGov points. We calibrated this to correspond to roughly \$0.20, an incentive equal^{8;9} or higher¹⁸ than those used in prior research.

Second, we also measured whether respondents agreed or disagreed with the statement “I would be unhappy if a person [not] vaccinated against COVID-19 married one of my close relatives.” The statement always referred to vaccination outgroups. Importantly, we manipulated between participants whether the question was asked directly, or embedded in a forced response design, which uses a randomization device to mask the responses of individual respondents, while retaining the ability to estimate the sample level agreement. Specifically, using a third-party random number generator, respondents “drew” an integer between 1 and 6. If they got 1 or 6, they were “forced” to respond “agree” or “disagree”, respectively. If they got anything in between, they answered freely, according to their true preference. This method is designed to remove social desirability bias from sensitive survey questions⁶⁵.

Measures

In the conjoint experiment, participants evaluated six statements for each target, indicating if they 1) like; if they support their applications for 2) citizenship and 3) unemployment benefits; and if they respect their 4) freedom of expression, 5) freedom of residence, and 6) freedom of movement.

Following a brief explanation of the rules of the dictator game (dubbed an “allocation task”), participants were asked, “How many points would [they] give to this vaccinated/unvaccinated person?”. We calculate the difference in points allocated to vaccination outgroups versus ingroups. Higher scores indicate more ingroup favoritism.

In the third experiment, we calculated the proportion of participants who indicated that they would be unhappy if someone from the vaccination outgroup would marry into their family. This is a simple proportion of “yes” answers to the direct question, but in the forced response

condition we must correct the counts to account for the fact that a third of all respondents are forced to respond one way or another. Accordingly, we subtract 1/6 of the total sample size both from the agree and the disagree responses. We test whether the proportion of prejudiced respondents is statistically significant from 0 and whether prejudice is higher or lower in the forced response condition, compared to the direct question condition.

For the measure on the best context to study discriminatory attitudes against vaccination outgroups, we operationalize ecological validity as the frequency with which people encounter situations similar to the one described in the study. Specifically, participants answer how often or rarely they encounter six situations, three of which describe social interactions (e.g. “I get upset when I think about interacting with all the people [not] vaccinated against COVID-19.”) and three describing monetary transactions (e.g. “I consider donating money to individuals [not] vaccinated against COVID-19.”).

Modeling

For the conjoint experiment, we follow the very same analysis strategy as described for Study 1. For the Dictator Game, we conduct simple t-tests to estimate if participants show significantly more generous towards their in-group members, and if the size of this in-group favoritism is affected by the incentives offered. For Bogardus’ measure of social distance, we estimate uncertainty of the estimates both with standard confidence intervals, but also perform a chi-squared test, to see if social desirability biases estimates compared to the direct question. Finally, the measures of ecological validity, again are compared with t-tests.

Data availability

All pre-registrations, data, materials, and computer code necessary to reproduce or replicate our analyses are available at <https://osf.io/7hszd>.

References

- [57] WHO Regional Office for Europe. Survey tool and guidance: rapid, simple, flexible behavioural insights on covid-19: 29 july 2020. Technical report, World Health Organization,

2020.

- [58] Imperial College London. Global covid-19 behaviour tracker. <https://www.imperial.ac.uk/global-health-innovation/what-we-do/our-response-to-covid-19/covid-19-behaviour-tracker/>. Accessed: 2022-20-09.
- [59] Antonio Alonso Arechar, Jennifer Nancy Lee Allen, Rocky Cole, Ziv Epstein, Kiran Garimella, Andrew Gully, Jackson G Lu, Robert M Ross, Michael Stagnaro, Jerry Zhang, et al. Understanding and reducing online misinformation across 16 countries on six continents. *PsyArXiv*, 2022.
- [60] Edmond Awad, Sohan Dsouza, Richard Kim, Jonathan Schulz, Joseph Henrich, Azim Shariff, Jean-François Bonnefon, and Iyad Rahwan. The moral machine experiment. *Nature*, 563(7729):59–64, 2018.
- [61] Simon Gächter and Jonathan F Schulz. Intrinsic honesty and the prevalence of rule violations across societies. *Nature*, 531(7595):496–499, 2016.
- [62] Alexander Coppock, Thomas J Leeper, and Kevin J Mullinix. Generalizability of heterogeneous treatment effect estimates across samples. *Proceedings of the National Academy of Sciences*, 115(49):12441–12446, 2018.
- [63] Kevin J Mullinix, Thomas J Leeper, James N Druckman, and Jeremy Freese. The generalizability of survey experiments. *Journal of Experimental Political Science*, 2(2):109–138, 2015.
- [64] Jens Hainmueller, Daniel J Hopkins, and Teppei Yamamoto. Causal inference in conjoint analysis: Understanding multidimensional choices via stated preference experiments. *Political Analysis*, 22(1):1–30, 2014.
- [65] Graeme Blair, Kosuke Imai, and Yang-Yang Zhou. Design and analysis of the randomized response technique. *Journal of the American Statistical Association*, 110(511):1304–1319, 2015.

Acknowledgements

This research has been supported by the Carlsberg Foundation’s grant CF20-0044 and the Danish National Research Foundation’s grant DRNF144. We benefited from research assistance from Frederikke Hyldgaard. We are indebted for insightful comments to Angelo Romano, Jesper Rasmussen, Gábor Simonovits, Dominik Hangartner, Sune Lehmann, Kasper Lippert-Rasmussen, Søren Flinch Midtgaard, Viki Møller Lyngby Pedersen, and Matthew Levendusky. We are grateful for foreign language assistance to Lea Pradella, Giuliana Spadaro, Ljudmila Sztankevics, Antoine Marie, Constantin Manuel Bosancianu, Oussama Ghajjou, Maria Carolina Rizzato and Bao-cun Zhang.

Author contributions

A.B., F.J. & M.B.P. designed the experiments; A.B. & M.B.P. conducted the experiments; A.B. analyzed data with inputs from F.J. & M.B.P.; A.B. and M.B.P. wrote the paper; All authors approved the final manuscript.

Competing interests

The authors declare no competing interests.

Additional information

Please address correspondence to Alexander Bor (bora@ceu.edu) or Michael Bang Petersen (michael@ps.au.dk).

Extended data

Extended Data Figure 1: Participants think about social interactions substantially more than about monetary transactions with vaccination outgroups. Dots denote means, errorbars denote 95% confidence intervals. $N = 1,448$. See more details in SI Section [K](#).

Extended Data Figure 2: Proportion indicating unhappiness if a vaccination out-group member married into their family. The plot contrasts a standard direct question (in red) to a question implemented with the forced response technique (in blue). Both methods indicate identical conclusions. Errorbars denote 95% confidence intervals. $N = 1,448$. See more details in SI Section [L](#).

A

B

Extended Data Figure 3: The role of the three proximate outcomes: fear of infection, untrustworthiness, and incompetence. Panel A displays average marginal component effects of target vaccination status on each of the three outcomes splitting on respondent vaccination status ($N_{vaccinated} = 54,054$ and $N_{unvaccinated} = 10,386$). Panel B displays the marginal regression coefficients from regressing prejudice on the three proximate variables simultaneously, while including respondent fixed effects. Errorbars denote 90% and 95% confidence intervals. See more details in SI Section C.

Extended Data Figure 4: Relationship between prejudice against the unvaccinated and policy stringency. Estimates of exclusionary attitudes (based on average marginal component effects). Blue line denotes best fitting linear regression line, and gray lines denotes a loess curve. (Total N = 64,440 observations in 21 countries). Policy stringency is based on the Oxford COVID-19 Government Response Tracker by Hale et al (2020). See more details in SI Section [G](#).

Extended Data Figure 5: Prejudice towards vaccination outgroups conditional on outgroup contact. The left panel shows average marginal component effects and demonstrates that prejudice is highest among respondents with no contact at all, and smallest among those with most contacts. The right panel displays marginal means to offer more nuance. Errorbars denote 95% confidence intervals. Total N = 18,270. See details in SI Section J.

Extended Data Table 1: Study 1 – Sample demographics by country

Country	N.obs	Median Age	Women	Higher Ed.	Income–Poor	Income–NA
Argentina	3090	38-42	0.48	0.19	0.41	0.28
Australia	3042	43-47	0.51	0.37	0.32	0.18
Austria	3054	48-52	0.52	0.34	0.33	0.22
Brazil	3036	38-42	0.52	0.21	0.77	0.15
China	3006	25-34	0.44	0.78	0.52	0.04
Denmark	3036	48-52	0.51	0.27	0.18	0.22
France	3054	48-52	0.52	0.31	0.44	0.12
Germany	3054	48-52	0.51	0.21	0.52	0.15
Hungary	3054	43-47	0.53	0.29	0.20	0.07
India	3174	33-37	0.50	0.78	0.22	0.28
Indonesia	3048	28-32	0.46	0.40	0.43	0.05
Italy	3042	48-52	0.52	0.16	0.42	0.22
Malaysia	3078	28-32	0.58	0.42	0.46	0.17
Mexico	3042	38-42	0.52	0.21	0.58	0.18
Morocco	3114	23-27	0.36	0.28	0.70	0.24
Romania	3054	43-47	0.52	0.47	0.71	0.18
Russia	3252	43-47	0.56	0.26	0.43	0.11
South Africa	3042	33-37	0.51	0.43	0.47	0.07
Spain	3036	48-52	0.51	0.40	0.38	0.15
United Kingdom	3108	48-52	0.52	0.33	0.27	0.21
United States	3024	48-52	0.52	0.35	0.38	0.14

Notes: **N** refers to the number of observations (not respondents). **Higher ed.** refers to the proportion of respondents who have completed higher education. **Poor** is defined as respondents indicating a gross household income less than 75% of the median. As many respondents refused to reveal their incomes, we included the share of missing data on this variable separately.

Extended Data Figure 6: Heterogeneities in exclusionary attitudes by education and income in each country. Average marginal component effects (AMCEs) for exclusionary attitudes against the unvaccinated by country across lower and higher educated respondents (left panel), and poor and not poor respondents (right panel). Estimates are based on Bayesian multilevel regression models. Error bars denote 90 and 95% credible intervals. Total N = 64,440. See more details in SI Section D.

Extended Data Figure 7: Simulations show that even if no offline citizen shows any exclusionary attitudes our main conclusions remain unchanged. Original average marginal component effect estimates of exclusionary attitudes against the unvaccinated (orange dots) and simulations estimating the same under maximal bias from non-online populations (purple dots). Errorbars display 90% and 95% confidence intervals. Total N = 54,054. See more details in SI Section [O.2](#).

Extended Data Table 2: Study 1 – Attributes and levels in the conjoint experiment

Attributes	Levels
Vaccination against COVID-19 (2)	Fully Vaccinated, Unvaccinated
Family Background (2)	Born and raised in {...}, Immigrated from the Middle East
Age (6)	21, 27, 33, 39, 45, 51
Occupation (6)	Lawyer, High school teacher, Construction inspector, Factory worker, Web developer, Retail salesperson
Hobbies (6)	Movies and TV series, Running and hiking, Reading books, Traveling, Cooking and gastronomy, Music and concerts
Personality (5)	Extrovert and sociable, Has a vivid imagination, Thorough and meticulous, Kind and considerate, Good at staying cool under stress

Note: Numbers in parentheses denote the number of levels. {...} was replaced with the country of the respondent.

Extended Data Table 3: Studies 2&3 – Attributes and levels in the conjoint experiment

Attributes	Levels
Vaccination against COVID-19 (2)	Fully Vaccinated, Unvaccinated
Personal Information (5)	No additional information, Struggles with drug addiction, Suffers from mental illness, Is an atheist, Has been in prison
Age (6)	27, 33, 39, 45, 51, 57
Occupation (5)	Administrator, Construction inspector, Factory worker, Web developer, Retail salesperson
Hobbies (6)	Movies and TV series, Running and hiking, Reading books, Traveling, Cooking and gastronomy, Music and concerts
Personality (5)	Extrovert and sociable, Has a vivid imagination, Thorough and meticulous, Kind and considerate, Good at staying cool under stress

Note: Numbers in parentheses denote the number of levels. {...} was replaced with the country of the respondent.

Supplementary Information for

Discriminatory Attitudes Against the Unvaccinated During a Global Pandemic

Contents

A Studies 1-3 – Sample demographics	8
B Studies 1-3 – Conjoint experimental design details	12
C Study 1 – The effects of vaccination status on proximate outcomes	13
D Study 1 – Heterogeneity in exclusionary attitudes against unvaccinated across demographic groups	14
E Study 1 – Antipathy towards immigration and vaccination outgroups	17
F Study 1 – Analyses pooling across vaccinated and unvaccinated respondents	23
G Study 1 – More on the country-level predictors of prejudice	28
G.1 Leave-one-out robustness test	28
G.2 Stringency as a predictor of prejudice	30
G.3 Social polarization and prejudice against disadvantaged groups	30
H Studies 1 & 3 – Vaccination prejudice and partisan identities	33
I Studies 2 and 3 – Prejudice across all groups by country	36
J Study 2 – Intergroup contact theory and vaccination prejudice	39
K Study 3 – Ecological validity	42
L Study 3 – Forced-response design	44
M Study 3 – Incentivized vs non-incentivized dictator games	44

N Study 3 – Affective prejudice, behavioral prejudice and ingroup bias in generosity	45
O Studies 1-3 – Robustness tests and diagnostics	49
O.1 Study 1 – Multilevel models for macro correlations	49
O.2 Simulations to assess generalizability of our results	54
O.3 Post-stratification weights	58
O.4 Dropping participants with no opportunity to get vaccinated	62
O.5 Marginal means across studies and attributes	62
O.6 Conjoint diagnostic tests	67

List of Figures

A.1 Study 1 – Vaccination rates in the population versus sample per country. N = 17.	10
A.2 Detailed responses to vaccination status question in each country. Total N = 64,440.	11
C.1 Negative stereotypes against vaccination outgroups by country and re- spondent vaccination status. N = 64,440. Dots denote average marginal component effects. Higher values indicate more negative stereotypes. Er- rorbars denote 90 and 95% confidence intervals.	13
D.1 Average marginal component effects (AMCEs) and marginal means (MM) by demographic groups. Errorbars denote 95% confidence intervals. Total N = 64,440.	14
E.1 Juxtaposing antipathy towards vaccination outgroups and immigrants from the Middle East by country. Dots denote country-level average marginal component effects (Total N = 64,440 observations in 21 countries). The black dashed line denotes the diagonal, whereas the gray dashed lines de- note thresholds where antipathy towards one group is twice (/ half) that of the other.	19

E.2	Prejudice against the unvaccinated and immigrants in Western and non-Western countries. We see no systematic differences across the two regions. Errorbars denote 95% confidence intervals. Total N = 64,440, left panel is based on vaccinated respondents only (N = 54,054).	20
E.3	No asymmetry in antipathy towards immigrants between vaccinated and unvaccinated respondents. Points denote average marginal component effects in panels 1 and 2, and difference in average marginal component effects in panel 3. Errorbars are 95% confidence intervals. Total N = 64,440.	21
E.4	No interaction between immigration and vaccination status. Unvaccinated immigrants from the Middle East suffer from as much prejudice as unvaccinated natives. Points denote average marginal component effects in panels 1 and 2, and difference in average marginal component effects in panel 3. Errorbars are 95% confidence intervals. Total N = 64,440.	22
F.1	Average Marginal Component Effects on all four outcomes in all 21 countries (with black) and the pooled sample (with red). In most cases, results closely resemble the patterns observed among vaccinated respondents. Total N = 64,440. Errorbars denote 90% and 95% confidence intervals. . . .	24
F.2	Benchmarking country-level antipathy towards vaccination outgroups versus antipathy towards immigrants from the Middle East. Dots denote country-level average marginal component effects (Total N = 64,440 observations in 21 countries).	25
F.3	Asymmetries in antipathy towards vaccination outgroups by demographic groups. Dots denote country-level average marginal component effects (Total N = 64,440). Errorbars are 95% confidence intervals.	26
F.4	Relationship between macro-indicators (death, trust, and vaccination) and country-level estimates of exclusionary attitudes (based on average marginal component effects). Blue lines denote best fitting linear regression line, and gray lines denote loess curves. (Total N = 64,440 observations in 21 countries).	27

G.1	Leave-one-out robustness test for the relationship between macro-indicators (death, trust, and vaccination) and country-level prejudice. Each line denotes the best fitting linear regression line when one of the 21 countries is dropped from the analysis.	29
G.2	Relationship between prejudice against the unvaccinated and prejudice against disadvantaged group (left) or polarization of society (right). Blue lines denote best fitting linear regression line, and gray lines denote loess curves. Tightness values are taken from V-Dem Institute ⁶⁶ , country N = 19. Prejudice is based country averages from the World Values Survey, country N = 21.	32
H.1	Average marginal component effects (AMCEs) and marginal means (MM) by voting behavior. The third column of facets “vaccination” reports proportion vaccinated within each subcategory. Errorbars denote 95% confidence intervals. N = 9,174 observations across 3 countries.	34
H.2	Average marginal component effects (amce) and marginal means (mm) by respondent vaccination (rows) and voting behavior (y-axis). Errorbars denote 95% confidence intervals. N = 14,480.	35
I.1	Study 2 – Antipathy towards vaccination outgroups and the benchmark groups conditional on country. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 18,270.	36
I.2	Study 3 – Prejudice towards vaccination outgroups and the benchmark groups conditional on outcome variable. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 14,480.	37
J.1	Prejudice towards vaccination outgroups conditional on outgroup contact and on vaccination status. The leftmost panel displays the relative frequency of contact patterns among the unvaccinated and the vaccinated, respectively. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 18,270.	40

J.2	Prejudice towards vaccination outgroups conditional on country. We split between people with no contact and those who have some contact. Dots denote average marginal component effects (left panel) or marginal means (right panel). Errorbars denote 95% confidence intervals. Total N = 18,270.	41
N.1	Study 3 – Correlation matrix across the conjoint outcome variables. Affective prejudice (antipathy) shows sizeable correlation with behavioral prejudice. N = 14,480.	47
N.2	Study 3 – Correlation matrix across individual level estimates ingroup generosity bias and prejudice. The top panel shows the pooled sample with sizable correlations across the prejudice items, but much lower with generosity gap. The middle panel paints a similar picture using only vaccinated respondents. The bottom panel shows that among the unvaccinated estimates of prejudice remain highly correlated, but they are largely independent of ingroup bias in generosity. Total N = 1,448.	48
O.1	Splitting AMCEs for each of the 6 profiles. We find some carry-over effects for prejudice. Errorbars denote 95% confidence or credible intervals. Total N = 64,440.	50
O.2	Estimating the relationship between macro-predictors and prejudice in a Bayesian setting – Pooled sample. Errorbands denote 90% and 95% credible intervals. Total N = 64,440.	51
O.3	Estimating the relationship between macro-predictors and prejudice in a Bayesian setting – Vaccinated respondents. Errorbands denote 90% and 95% credible intervals. Total N = 64,440.	52
O.4	Original average marginal component estimates of prejudice against the vaccinated (orange dots) and simulations estimating prejudice under maximal bias from non-online populations (purple dots). Even if offline unvaccinated citizens express considerable prejudice (equivalent to the cross-country mean prejudice of vaccinated respondents towards the unvaccinated) our main conclusions remain unchanged. Horizontal lines display 90% and 95% confidence intervals. Total N = 10,386.	55

O.5	Estimates of prejudice against the unvaccinated at different levels of sampling bias. The black lines are the observed estimates and the grey shaded areas are 95 % confidence intervals around it. Dashed lines are the smallest effect size of interest (5% points). The x-axis denotes the percentage of the online population reflected in the observed estimate. The colored lines denote the percentage of observed prejudice that unrepresented individuals are assumed to hold.	57
O.6	AMCEs among vaccinated respondents from models without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 64,440.	58
O.7	AMCEs in the pooled sample from models without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 64,440.	59
O.8	Study 2–AMCE of vaccination attribute in the pooled sample from models with and without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 18,270.	60
O.9	Study 3–AMCE of vaccination attribute in the pooled sample from models with and without weights. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480. . .	61
O.10	AMCEs in the sample excluding respondents who had no opportunity to get vaccinated yet. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 61,848. . . .	63
O.11	Marginal means (dots) across all attribute levels in Study 1 (pooling across countries). Errorbars denote 95% confidence intervals. Total N = 64,440. . . .	64
O.12	Marginal means (dots) across all attribute levels in Study 2 (pooling across countries). Errorbars denote 95% confidence intervals. Total N = 18,270. . . .	65
O.13	Marginal means across all attribute levels in Study 3 across each of the six outcome variables. Errorbars denote 95% confidence intervals. Total N = 14,480.	66

O.14 Study 1–Regressing respondents’ vaccination status on the vaccination and immigration attributes to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 64,440. . . .	68
O.15 Study 2–Regressing respondents’ vaccination status on the vaccination attribute to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 18,270.	69
O.16 Study 3–Regressing respondents’ vaccination status on the vaccination attribute to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 14,480.	69
O.17 Study 1 – Displaying the frequency of all attribute levels in our conjoint experiment for the first 12 countries. Total N = 64,440.	70
O.18 Study 1 – Displaying the frequency of all attribute levels in our conjoint experiment for the last 9 countries. Total N = 64,440.	71
O.19 Study 2 – Displaying the frequency of all attribute levels in our conjoint experiment. Total N = 18,270.	72
O.20 Study 3 – Displaying the frequency of all attribute levels in our conjoint experiment. Total N = 14,480.	73
O.21 Study 1 –Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents appear to be less sensitive to target vaccination manipulation. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 64,440.	74
O.22 Study 2 – Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents appear to be less sensitive to target vaccination manipulation. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 18,270.	74

O.23 Study 3 – Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents are less sensitive to target vaccination manipulation, but only when it comes to disliking and support the freedom of movement of outgroup. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480.	75
O.24 Study 1 – Splitting AMCEs for each of the 6 profiles. We find small carry-over effects for prejudice. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 64,440.	76
O.25 Study 2 – Splitting AMCEs for each of the 6 profiles. We find no carry-over effects. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 18,270.	77
O.26 Study 3 – Splitting AMCEs for each of the 10 profiles. We find no carry-over effects. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480.	78

List of Tables

A.1 Study 1 – Information on panels and sampling by country	8
A.2 Studies 2 & 3 – Sample demographics by country	9
E.1 Is antipathy against unvaccinated larger than against immigrants among vaccinated respondents?	18
I.1 Do vaccinated respondents (N = 10570) exhibit more exclusionary attitudes against the unvaccinated than against the other benchmark groups? Wald-tests for equal effects for all 24 combinations of outcomes and benchmark groups. Note: all χ^2 tests have 1 degree of freedom.	38
O.1 Bayesian MLM coefficient estimates for antipathy towards the unvaccinated	53
O.2 Bayesian MLM coefficient estimates for antipathy towards vaccination outgroups	53

A Studies 1-3 – Sample demographics

Extended Data Table 1 and Table A.2 display the key demographic variables for each of the countries using the weighted samples from Studies 1-3. Following best practices by YouGov to ensure high quality data the quotas in Morocco, Indonesia and Malaysia are set to online (and not national) populations. This explains the slight demographic imbalances, e.g., on gender. We share detailed weighting schemes provided by YouGov at the paper’s OSF repository, <https://osf.io/7hszd>. Table A.1 displays information on panels and sampling for each country in Study 1. Note that Studies 2 and 3 relied on the same panels for the respective six countries.

Table A.1: Study 1 – Information on panels and sampling by country

Country	Panel	Population Benchmark	Quotas
Argentina	YouGov	National	Age, Gender, Geography
Australia	YouGov	National	Age, Gender, Geography, Education
Austria	YouGov	National	Age, Gender, Geography
Brazil	YouGov	National	Age, Gender, Geography, Education
China	Ipsos	National	Age, Gender, Geography
Denmark	YouGov	National	Age, Gender, Geography, Education
France	YouGov	National	Age, Gender, Geography, Education
Germany	YouGov	National	Age, Gender, Geography, Education
Hungary	YouGov	National	Age, Gender, Geography
India	YouGov	National Urban	Age, Gender, Geography
Indonesia	YouGov	Online	Age, Gender, Geography
Italy	YouGov	National	Age, Gender, Geography, Education
Malaysia	YouGov	Online	Age, Gender, Geography
Mexico	YouGov	National	Age, Gender, Geography, Education
Morocco	YouGov	Online	Age, Gender, Geography
Romania	YouGov	National	Age, Gender, Geography
Russia	YouGov - OMI	National	Age, Gender, Geography, Education
South Africa	YouGov - KLA	National	Age, Gender, Geography
Spain	YouGov	National	Age, Gender, Geography, Education
United Kingdom	YouGov	National	Age, Gender, Geography, Education
United States	YouGov	National	Age, Gender, Geography, Edu., Race

Table A.2: Studies 2 & 3 – Sample demographics by country

Country	N.obs	Median Age	Women	Higher Ed.	Income–Poor	Income–NA
Germany	3042	48-52	0.52	0.22	0.53	0.18
India	3066	33-37	0.5	0.75	0.3	0.13
Indonesia	3048	28-32	0.53	0.42	0.37	0.06
Morocco	3036	23-27	0.37	0.29	0.71	0.22
South Africa	3036	33-37	0.5	0.4	0.46	0.08
United Kingdom	3042	43-47	0.49	0.32	0.27	0.24
USA	14480	48-52	0.48	0.38	0.42	0.11

Notes: **N** refers to the number of observations (not respondents). **Higher ed.** refers to the proportion of respondents who have completed higher education. **Poor** is defined as respondents indicating a gross household income less than 75% of the median. As many respondents refused to reveal their incomes, we included the share of missing data on this variable separately.

Figure A.1 displays the share of vaccinated individuals within each country (on the x-axis) against the share of vaccinated individuals in our sample. As cross-country data on vaccination levels among *adults* (and not the total population) are not openly available to the best of our knowledge, we assembled these data ourselves, mostly using national press releases and COVID-19 dashboards. Data for Argentina, Brazil, China, and Morocco are not available; hence, they are omitted from the figure. With most points around the diagonal, our samples resemble the respective populations well. That said, samples from South Africa and Romania have too many vaccinated, whereas samples from France and, to a lesser extent, US and Russia have too many unvaccinated respondents.

Figure A.1: Study 1 – Vaccination rates in the population versus sample per country. $N = 17$.

Finally, Figure A.2 displays detailed information on respondents’ answers to the vaccination status question. Most importantly, it demonstrates that only 4% of respondents said that they have not had an opportunity to get vaccinated yet. This number is reasonably low even in countries with the highest share of respondents indicating no prior opportunity: Russia (17%) and South Africa (12%). In the case of Russia, this likely reflects the country’s controversial decision to not approve any non-Russian vaccines and scandals over the approval of domestic ones.¹ In the case of South Africa, two factors have contributed to low vaccination levels, namely widespread vaccine hesitancy and difficulties in reaching remote rural areas. Accordingly, South Africa had to delay COVID-19 vaccine deliveries already by November 2021 as supply surpassed demand in the country.² At any rate, we consider these data to corroborate that our strategy to field the experiment in countries with widespread access to COVID-19 vaccines has been successful. We offer additional evidence that excluding respondents claiming no access does not substantively change our estimate of antipathy (see Section F).

Figure A.2: Detailed responses to vaccination status question in each country. Total N = 64,440.

¹https://en.wikipedia.org/wiki/COVID-19_vaccination_in_Russia

²<https://www.reuters.com/world/africa/exclusive-south-africa-delays-covid-vaccine-deliveries-inoculations-slow-2021-11-24/>

B Studies 1-3 – Conjoint experimental design details

Extended Data Table 2 displays the design of Study 1. Extended Data Table 3 displays the design employed in Studies 2 and 3. Beyond the important substantial change in the benchmark groups in the latter two studies, we also adjusted the age and the potential occupations of the targets to avoid unusual targets (e.g., a lawyer who has been in prison; a teacher who struggles with drug addiction).

We also note that while questionnaires in general were translated to the original language of the given country, there are a few exceptions: the questionnaires were not translated to Hindi (India) and Tamazight (Morocco). At the same time, following best practices of the survey provider, the surveys were also offered in English in Indonesia, Malaysia, and Morocco; in Chinese in Malaysia; and French in Morocco as many panelists are more proficient in these non-official languages.

C Study 1 – The effects of vaccination status on proximate outcomes

Figure C.1: Negative stereotypes against vaccination outgroups by country and respondent vaccination status. $N = 64,440$. Dots denote average marginal component effects. Higher values indicate more negative stereotypes. Errorbars denote 90 and 95% confidence intervals.

D Study 1 – Heterogeneity in exclusionary attitudes against unvaccinated across demographic groups

Our first analysis of heterogeneities relies on the pooled sample averaging across countries. As displayed in Figure D.1, exclusionary attitudes against the unvaccinated are present across all demographic groups we investigate. Yet, there are also slight and, given our large sample size, very precisely estimated heterogeneities across groups. Specifically, respondents who completed higher education (AMCE difference of 5% points), women (AMCE difference of 4% points) and respondents who report an income above 75% of the country median (AMCE difference of 3.5% points) express larger antipathy (p s < 0.001). We see a smaller, although still statistically significant, difference between younger and older respondents (AMCE difference of 2% points, $p < 0.05$).

Figure D.1: Average marginal component effects (AMCEs) and marginal means (MM) by demographic groups. Errorbars denote 95% confidence intervals. Total N = 64,440.

Given that we rely on online samples, and therefore necessarily exclude all groups of

society without an internet access, we are particularly interested in heterogeneities across income and education—two demographic variables that are highly predictive of internet access, including in low- and middle-income countries⁶⁷. While the pooled analyses showed small differences across these groups, it is plausible that they mask large cross-country variance in heterogeneities. Therefore, next, we estimate heterogeneities in exclusionary attitudes across income and education levels within each country. To account for the fact that our sample sizes offer limited power to detect these heterogeneities and that naive models without any pooling are known to overfit to the data, we employ multilevel models with varying (also known as “random”) slopes at the level of countries. These models assume that the country-level variation in prejudice across groups follows a normal distribution with a mean and standard deviation estimated from the data itself. The practical consequence of this assumption is that the model partially pools information across groups. It “borrows” information from all countries when trying to estimate prejudice across groups within one country. Thus, estimates are pulled towards the grand mean, proportionally to how much information they are based on (smaller groups are pulled more) and to how surprising they are (more extreme outliers are pulled more). Meanwhile, standard errors are reduced compared to naive estimates. Multilevel models with partial pooling are known to have better out-of-sample predictive qualities compared to naive models with no pooling (for an accessible introduction, see e.g.⁶⁸). We fit our models in a Bayesian framework to help model convergence. We use weakly informative priors that do not meaningfully affect coefficient estimates and default MCMC settings from the `rstanarm` package (for details please refer to the computer code in the OSF repository). Given that our primary interest is exclusionary attitudes towards the unvaccinated, we limit our data to vaccinated respondents.

Extended Data Figure 6 displays our findings. Our main conclusion is that country level variation in demographic heterogeneities is substantively small. We find somewhat more variation across countries for education than for income, where the largest difference is observed for the US at 8 percentage points, meaning that the exclusionary attitude exhibited by those who did not complete higher education (14% points) was about 2/3 of the size of those who did complete higher education (22% points). Put differently, even in

the country with most heterogeneity across higher education, the difference (8% points) is less than double the average difference across countries (5% points). For income, the differences are smaller, and arguably inconsequential. Indeed, even the largest differences observed, in France, at 4% points and in China at -2% points are so close to the pooled estimate (which this model puts at 2% points) that the data and model are consistent with the statement that there is no variation in heterogeneity by income across countries.

E Study 1 – Antipathy towards immigration and vaccination outgroups

Figure E.1 displays the country-level estimates of antipathy towards immigrants from the Middle East (x-axis) versus antipathy towards unvaccinated people among the vaccinated (y-axis). The black dashed line outlines the diagonal where the two levels are equal. Points above this diagonal signal higher antipathy towards the unvaccinated. Gray dashed lines denote cutoff for twice (above diagonal) and half (below diagonal) difference. Figure E.2 displays the same estimates but separating Western and non-Western countries. We see no systematic differences across the two sets of countries on vaccination prejudice, although Westerners appear to be slightly more consistently prejudiced against migrants from the Middle East. At any rate, the plot indicates that our results are not driven solely by either group of countries.

To judge whether antipathy against the unvaccinated is significantly higher than against immigrants, we rerun our regression models regressing the outcome on the six conjoint attributes, and including respondent fixed effects, as well as survey weights. (These estimates very closely replicate our main results, but include insubstantial differences). Next, we compare the size of average marginal component effects using the `car` R package's `linearHypothesis` command. Results indicate that we can reject with very high confidence the null hypothesis that the two estimates are equal on the pooled sample. $\chi^2(1, N=54054) = 23.83, p < 0.001$. Table E.1 below repeats these tests for each country separately. The second column displays whether the coefficient is descriptively larger for the unvaccinated than for the immigrant (indicated by "TRUE"), whereas the last column provides a significant test for the difference between the two coefficients. Although slicing up the data leads to a loss of precision, antipathy against unvaccinated targets is significantly higher in 11 out of 21 countries (Argentina, Australia, Germany, India, Italy, Malaysia, Morocco, South Africa, Spain, United Kingdom, United States) than antipathy against immigrants. The table also demonstrates that immigrants do not face significantly larger antipathy than unvaccinated targets in any of our countries.

Table E.1: Is antipathy against unvaccinated larger than against immigrants among vaccinated respondents?

Country	Unvaxxed > Immigrant	DF	N	Chisq	p
Argentina	TRUE	1	2784	9.296	0.002
Australia	TRUE	1	2766	80.785	0
Austria	TRUE	1	2508	3.289	0.07
Brazil	TRUE	1	2730	1.237	0.266
China	TRUE	1	2916	1.217	0.27
Denmark	FALSE	1	2778	1.905	0.167
France	FALSE	1	2310	0.753	0.385
Germany	TRUE	1	2580	36.552	0
Hungary	FALSE	1	2244	0.859	0.354
India	TRUE	1	2850	15.094	0
Indonesia	TRUE	1	2778	1.222	0.269
Italy	TRUE	1	2688	20.775	0
Malaysia	TRUE	1	2982	142.076	0
Mexico	TRUE	1	2676	3.315	0.069
Morocco	TRUE	1	2610	19.885	0
Romania	TRUE	1	2052	0.244	0.621
Russia	TRUE	1	1752	0.008	0.929
South Africa	TRUE	1	2214	15.271	0
Spain	TRUE	1	2808	21.365	0
United Kingdom	TRUE	1	2862	53.836	0
United States	TRUE	1	2166	23.832	0

Figure E.1: Juxtaposing antipathy towards vaccination outgroups and immigrants from the Middle East by country. Dots denote country-level average marginal component effects (Total $N = 64,440$ observations in 21 countries). The black dashed line denotes the diagonal, whereas the gray dashed lines denote thresholds where antipathy towards one group is twice (/ half) that of the other.

Figure E.2: Prejudice against the unvaccinated and immigrants in Western and non-Western countries. We see no systematic differences across the two regions. Errorbars denote 95% confidence intervals. Total $N = 64,440$, left panel is based on vaccinated respondents only ($N = 54,054$).

Next, Figure E.3 demonstrates that unvaccinated (top panel) versus vaccinated respondents (middle panel) both feel antipathy towards immigrants from the Middle East. The bottom panel subtracts these two estimates (or put differently, depicts the interaction) and demonstrates that the difference is close to 0. In other words, we have no reason to believe that the asymmetry between vaccination groups in antipathy towards each other is an artefact of unvaccinated respondents being less prejudiced against everyone.

Figure E.3: No asymmetry in antipathy towards immigrants between vaccinated and unvaccinated respondents. Points denote average marginal component effects in panels 1 and 2, and difference in average marginal component effects in panel 3. Errorbars are 95% confidence intervals. Total N = 64,440.

Finally, Figure E.4 tests if unvaccinated immigrants from the Middle East face prejudice above and beyond the prejudice faced by unvaccinated and immigrant targets. While tangential, this is a substantively interesting question as vaccination levels among immigrant communities lag behind majority levels in many countries – potentially contributing to ethnic discrimination. Yet, consistent with prior research^{69;70}, we find no evidence for an interaction between these two factors.

Figure E.4: No interaction between immigration and vaccination status. Unvaccinated immigrants from the Middle East suffer from as much prejudice as unvaccinated natives. Points denote average marginal component effects in panels 1 and 2, and difference in average marginal component effects in panel 3. Errorbars are 95% confidence intervals. Total N = 64,440.

F Study 1 – Analyses pooling across vaccinated and unvaccinated respondents

In this section, we report our pre-registered hypothesis tests on the pooled sample – i.e. not splitting on respondents’ vaccination status. Figure F.1 displays the pooled experimental effects (technically, AMCEs) on all four outcomes. The pooled AMCE of vaccination outgroup on prejudice is 0.11. 95% CI = [0.1; 0.12]. Country-level estimates range between 0, 0.34. The most notable difference is that, on average, Russians do not show outgroup prejudice because both vaccinated and (the relatively large group of) unvaccinated respondents are prejudiced against unvaccinated people. The pooled AMCE of vaccination outgroup on fear of infection is 0.32. 95% CI = [0.31; 0.33]. The pooled AMCE of vaccination outgroup on incompetence (aka unintelligent) is 0.12. 95% CI = [0.11; 0.12], and on untrustworthiness, it is 0.11. 95% CI = [0.1; 0.12]. Benchmarking outgroup antipathy to antipathy towards immigrants from the Middle East, we again find that in 16 out of 21 countries, vaccination status causes larger prejudices (see Figure F.2). The pooled effect size is two times larger than the pooled effect size for immigration. We also demonstrate in Figure F.3 that women, those with higher education and income, and older respondents display somewhat higher prejudice in the pooled analyses.

Finally, we replicate the associations between country-level prejudice and macro-indicators in Figure F.4. Spearman’s rank order correlations between antipathy towards vaccine outgroups and deaths is $\rho = -0.68$, 95%CI [-0.86, -0.36]; vaccination levels is $\rho = 0.42$, 95%CI [-0.01, 0.72]; and social trust is $\rho = 0.53$, 95%CI [0.13, 0.78]. The only notable difference is that the correlation with vaccination levels grew and the confidence interval almost excludes 0, but this is an artefact of countries with lower vaccination levels having more unvaccinated respondents showing little to no prejudice, thereby suppressing the overall antipathy towards vaccination outgroups.

Overall, the results resemble the prejudice estimates observed among vaccinated respondents.

Figure F.1: Average Marginal Component Effects on all four outcomes in all 21 countries (with black) and the pooled sample (with red). In most cases, results closely resemble the patterns observed among vaccinated respondents. Total N = 64,440. Errorbars denote 90% and 95% confidence intervals.

Figure F.2: Benchmarking country-level antipathy towards vaccination outgroups versus antipathy towards immigrants from the Middle East. Dots denote country-level average marginal component effects (Total N = 64,440 observations in 21 countries).

Figure F.3: Asymmetries in antipathy towards vaccination outgroups by demographic groups. Dots denote country-level average marginal component effects (Total N = 64,440). Errorbars are 95% confidence intervals.

Figure F.4: Relationship between macro-indicators (death, trust, and vaccination) and country-level estimates of exclusionary attitudes (based on average marginal component effects). Blue lines denote best fitting linear regression line, and gray lines denote loess curves. (Total N = 64,440 observations in 21 countries).

G Study 1 – More on the country-level predictors of prejudice

G.1 Leave-one-out robustness test

Our tests of the country-level predictors of vaccination prejudice are limited to the 21 countries we survey. Accordingly, these estimates are imprecisely estimated (i.e., confidence intervals are wide). Case in point, Malaysia stands out as an outlier. On the one hand, this could reflect substantive country-specific characteristics, for example that Malaysia managed to keep deaths low and vaccinations very high (see Figure 3). As such, vaccinations may relate to strong norms that fuel very high antipathy towards those few who refuse to receive the vaccine. As evidence of such strong norms, school teachers not wanting to get vaccinated were threatened with disciplinary actions; that the spread of misinformation about the vaccines was sanctioned with fines; as well as sanctions against sellers of false vaccination certificates ³

Is it possible that our macro-results are driven by an outlier like Malaysia? As an additional test of the robustness of our analyses, we estimate each correlation while leaving out countries one-by-one. Figure G.1 shows the best fitting regression lines across the three macro-predictors from these analyses. We also calculate Spearman’s rank-order correlations for each set of 20 countries. We find that the associations with our two strongest predictors – deaths and social trust – are reasonably robust. The regression lines cluster together and the rank-order correlations cover a relatively short range (deaths $\rho = [-.70, -.56]$; social trust $\rho = [.53, .63]$). Meanwhile, the associations with vaccination levels are a bit more volatile both in terms of the regression lines and the range of correlations ($\rho = [.30, .49]$).

³https://en.wikipedia.org/wiki/COVID-19_vaccination_in_Malaysia Accessed: 2022 July 12.

Figure G.1: Leave-one-out robustness test for the relationship between macro-indicators (death, trust, and vaccination) and country-level prejudice. Each line denotes the best fitting linear regression line when one of the 21 countries is dropped from the analysis.

G.2 Stringency as a predictor of prejudice

Our pre-registered analyses found that death per capita is the strongest country-level correlate of prejudice against the unvaccinated: countries with the least amounts of deaths display the highest prejudice. Here, we run an exploratory analyses to explore the foundations of this relationship. Specifically, we test if countries which implemented stricter public and social measures against the coronavirus avoided deaths, but (perhaps as a byproduct) also instilled prejudice among the vaccinated against the unvaccinated. We rely on the Oxford COVID-19 Government Response Tracker⁷¹ data to measure the strictness of public and social measures. Specifically, we average the (daily) Government Stringency Index scores between January 1, 2020 and December 31, 2021 for each country in our data.

Extended Data Figure 4 displays the relationships between prejudice against the unvaccinated and these two macro-predictors. It demonstrates that cultural tightness is a much stronger predictor of prejudice ($\rho = 0.62$ 95%CI [0.18, 0.85]) than stringency ($\rho = 0.23$ 95%CI [-0.22, 0.6]).

G.3 Social polarization and prejudice against disadvantaged groups

We also explore two additional (post-hoc) predictors of country-level prejudice against the unvaccinated: social polarization and prejudice against disadvantaged groups. While we argue that the rift across vaccination is a novel cleavage in societies, which emerged due to the pandemic, it is plausible that this rift is larger in countries which were polarized at the onset of the pandemic. For example, the United States has suffered from major (political, racial, religious) polarization even before the pandemic, which may have contributed to the severe tensions around COVID-19 vaccinations, leading among other things to a sizable partisan gap in vaccination levels. To test if more polarized societies also display more prejudice against the unvaccinated, we turn to the V-Dem Institute’s expert ratings⁶⁶. Specifically, we rely on the “polarization of society” measure, which asks coders to “characterize the differences of opinions on major political issues” on a five point scale from “No polarization. There are differences in opinions but there is

a general agreement on the direction for key political issues.” to “Serious polarization. There are serious differences in opinions in society on almost all key political issues, which result in major clashes of views.”

We also explore whether countries that are generally more prejudiced against disadvantaged groups are also more prejudiced against the unvaccinated. To do so, we exploit the fact that the latest wave (7) of the World Values Survey included a version of Bogardus’ classic social distance measure to tap into prejudice against various disadvantaged groups. Specifically, respondents received a list with various groups of people and were asked to “mention any that [they] would not like to have as neighbors”. For 19 out of 21 countries in our data (all, except India and South Africa), we therefore have data on how much people are prejudiced against drug addicts, heavy drinkers, homosexuals, people of a different race, and immigrants/foreign workers. For our analyses, we simply calculate an average prejudice score (across the five groups) for each respondent and then average the average prejudice scores by country.

Figure G.2 displays the associations across these two predictors. It shows, first, that countries which are more prejudiced against disadvantaged groups are not more prejudiced against the unvaccinated on average ($\rho(19) = 0.12$ 95%CI [-0.35, 0.54]). It also demonstrates that if anything, the more polarized societies were (pre-pandemic), the *less* prejudice they display against the unvaccinated ($\rho(21) = -0.41$ 95%CI [-0.72, 0.02]). We believe these results underscore the novelty of the conflict across vaccination groups.

Figure G.2: Relationship between prejudice against the unvaccinated and prejudice against disadvantaged group (left) or polarization of society (right). Blue lines denote best fitting linear regression line, and gray lines denote loess curves. Tightness values are taken from V-Dem Institute⁶⁶, country N = 19. Prejudice is based country averages from the World Values Survey, country N = 21.

H Studies 1 & 3 – Vaccination prejudice and partisan identities

Next, we turn our attention to the role of partisan identities. In some countries, most notably the US, the large gap in vaccination levels across partisan groups received a lot of attention. Is it possible that partisan animosities fuel or, at least, contribute to vaccination prejudice across the World? While our experiment was not designed to address this question head on, we report exploratory (post-hoc) analyses investigating observable implications of this possibility.

First, we explore if vaccination prejudice is moderated by partisan identities. If prejudice is fueled by partisan conflicts over vaccination compliance, we would observe that vaccination prejudice is driven by one partisan group (the vaccinated), who dislike another partisan group (the unvaccinated). To test this, we use background information on self-reported voting behavior at the last national elections for respondents in the US, UK and Australia. Figure H.1 demonstrates that estimating prejudice against vaccination outgroups conditional on vote choice shows little evidence of substantial heterogeneities, especially outside the US. The third column of Figure H.1 also serves as a reminder that vaccination status varies little across partisan groups outside the US⁷², and even here, over 60% of Republicans are vaccinated (vs 90% among Democrats). Put differently, the partisan composition of unvaccinated Americans (judging from our samples) is around 16% Democrat, 44% Republican and 40% non-partisan – a group too politically diverse to be equated with a single partisan entity.

However, a potential limitation of the analyses reported above is that they do not allow to split respondents both by their vote and vaccination status. We address this shortcoming with data from the larger Study 3, relying on the affective prejudice (like/dislike) outcome. Figure H.2 displays some interesting patterns: vaccinated Biden voters express the highest antipathy, whereas the few (63) unvaccinated Democrats show no antipathy. Both vaccinated and unvaccinated Trump voters express antipathy against their outgroup, although it is markedly lower than among vaccinated Democrats. Meanwhile, among respondents who voted for neither of the two forerunners, only the vaccinated show antipathy.

Figure H.1: Average marginal component effects (AMCEs) and marginal means (MM) by voting behavior. The third column of facets “vaccination” reports proportion vaccinated within each subcategory. Errorbars denote 95% confidence intervals. $N = 9,174$ observations across 3 countries.

In short, even in the country with the highest political polarization in our sample – the US – antipathy is not limited to people with partisan affiliations, nor is it consistently congruent with partisan stereotypes: vaccinated Democrats do dislike the unvaccinated (who they may think are mostly Republicans), but vaccinated Republicans also dislike the unvaccinated (who they may perceive as fellow Republicans, see also ⁷⁰).

Finally, note that consistent with these individual-level analyses, we report that countries with higher polarization in society actually display *less* prejudice against the unvaccinated (see Section G for details).

Figure H.2: Average marginal component effects (amce) and marginal means (mm) by respondent vaccination (rows) and voting behavior (y-axis). Errorbars denote 95% confidence intervals. $N = 14,480$.

I Studies 2 and 3 – Prejudice across all groups by country

Figure I.1 displays prejudice levels for each country separately. It demonstrates that vaccination status is the most divisive group marker in four of the six countries (Germany, India, South Africa and the UK). In Indonesia, drug addicts face much higher prejudice (indeed, the highest observed across all countries and groups), while the other three benchmark groups face prejudice roughly at similar levels. Morocco is an outlier in that the small (but significant) prejudice toward vaccination outgroups is surpassed by all four benchmark groups. Figure I.1 also increases our trust in the quality of our data, by showing heterogeneities in the prejudice patterns with high face validity. For example, while we find little prejudice against atheists in the two secular, Western countries (Germany and the UK), it is very high in the most religious country in our sample, Morocco. The aforementioned extraordinary high prejudice towards drug addicts in Indonesia is reasonable in light of the country’s very tough anti-drug policies.

Figure I.1: Study 2 – Antipathy towards vaccination outgroups and the benchmark groups conditional on country. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 18,270.

Figure I.2 displays prejudice levels from Study 3, conducted in the US. Here, instead of countries, each panel refers to one of the six outcome variables. The main conclusion

is that exclusionary attitudes towards the unvaccinated (purple) at least equal, but often greatly surpass the exclusionary attitudes held towards more traditional victims of prejudice.

Table I.1 displays Wald-tests for equal effects for all 6 (outcomes) $\times$ 4 (benchmark groups) = 24 contrasts using data from vaccinated respondents only. The “Diff” column quantifies the difference between exclusionary attitudes towards the unvaccinated and the benchmarks. Larger values indicate more exclusion towards the unvaccinated. The table demonstrates that in none of the cases we see higher exclusion against any benchmark on any outcome than against the unvaccinated. Meanwhile, in 17 out of 24 cases the unvaccinated face significantly higher exclusionary attitudes than the various benchmark groups.

Figure I.2: Study 3 – Prejudice towards vaccination outgroups and the benchmark groups conditional on outcome variable. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 14,480.

Table I.1: Do vaccinated respondents (N = 10570) exhibit more exclusionary attitudes against the unvaccinated than against the other benchmark groups? Wald-tests for equal effects for all 24 combinations of outcomes and benchmark groups. Note: all χ^2 tests have 1 degree of freedom.

DV	Benchmark	Diff	Chisq	p
Like	Struggles with drug addiction	0.06	7.73	0.005
Like	Has been in prison	0.07	14.44	<0.001
Like	Is an atheist	0.13	43.76	<0.001
Like	Suffers from mental illness	0.12	36.81	<0.001
Citizen	Struggles with drug addiction	0.01	0.57	0.449
Citizen	Has been in prison	-0.01	0.26	0.611
Citizen	Is an atheist	0.07	19.47	<0.001
Citizen	Suffers from mental illness	0.05	10.67	0.001
Welfare	Struggles with drug addiction	0.01	0.14	0.704
Welfare	Has been in prison	0.03	3.33	0.068
Welfare	Is an atheist	0.05	10.54	0.001
Welfare	Suffers from mental illness	0.06	16.51	<0.001
Speech	Struggles with drug addiction	0.04	7.53	0.006
Speech	Has been in prison	0.04	7.38	0.007
Speech	Is an atheist	0.06	14.11	<0.001
Speech	Suffers from mental illness	0.03	3.68	0.055
Residence	Struggles with drug addiction	0.01	0.28	0.598
Residence	Has been in prison	0.02	0.91	0.34
Residence	Is an atheist	0.08	22.31	<0.001
Residence	Suffers from mental illness	0.04	6.17	0.013
Movement	Struggles with drug addiction	0.23	142.78	<0.001
Movement	Has been in prison	0.25	149.28	<0.001
Movement	Is an atheist	0.26	175.13	<0.001
Movement	Suffers from mental illness	0.24	145.07	<0.001

J Study 2 – Intergroup contact theory and vaccination prejudice

Extended Data Figure 5 displays our pre-registered model displaying prejudice towards vaccination outgroups conditional on the level of outgroup contact. Consistent with intergroup contact theory, prejudice is generally higher among people with less intergroup contact. At the same time, the figure also demonstrates that significant prejudice is present across all contact levels. In other words, contact could reduce but does not eliminate prejudice.

Figure J.1 offers a more detailed, exploratory (post-hoc) look at the same results, by splitting vaccinated and unvaccinated respondents (vertical panels). It also displays the frequency of intergroup contact patterns conditional on vaccination status. Consistent with the intuition that in societies with (sometimes large) majorities vaccinated, unvaccinated respondents have much more intergroup contact on average than vaccinated respondents. While less than 15% of unvaccinated respondents report no contact with vaccinated people at all, more than half of vaccinated respondents lack intergroup contact. This plot also reminds us that given the fewer unvaccinated respondents in the sample, our estimates of the attitudes of unvaccinated towards vaccinated are noisier. Still, we find some evidence that the few unvaccinated individuals across the 6 countries, who have no contact with vaccinated people dislike them more than their fellow unvaccinated targets. Yet, even those vaccinated people with most intergroup contact harbor more prejudice towards the unvaccinated than the unvaccinated people with least contact towards the vaccinated.

Finally, Figure J.2 offers an alternative (post-hoc) view on the contact patterns, this time disaggregated by country. Given that we have much smaller samples within each country, we lump together all respondents who have some intergroup contact. The results indicate that respondents who have some contact never display more prejudice than those who have none. The interaction reaches statistical significance in four of the six countries surveyed (United Kingdom, Germany, India, South Africa).

Figure J.1: Prejudice towards vaccination outgroups conditional on outgroup contact and on vaccination status. The leftmost panel displays the relative frequency of contact patterns among the unvaccinated and the vaccinated, respectively. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total $N = 18,270$.

Figure J.2: Prejudice towards vaccination outgroups conditional on country. We split between people with no contact and those who have some contact. Dots denote average marginal component effects (left panel) or marginal means (right panel). Errorbars denote 95% confidence intervals. Total N = 18,270.

K Study 3 – Ecological validity

Incentivized behavioral economic games constitute important tools for revealing preferences using a neutral unit (i.e., money). At the same time, behavioral economic games—such as those used in prior research on generosity and vaccination status^{69;70;73}—are often met with criticisms regarding lack of ecological validity⁷⁴, i.e., that they “fail to reflect important features of the real world”⁷⁵ and therefore that behavior elicited in these games do not correspond to real-world behavior⁷⁶. In this regard, the stylized nature of behavioral economic games often make it difficult to adapt them to reflect the complexity of real-world decisions. This is especially the case when studying complex psychological and social phenomenon such as discrimination, tolerance and prejudice that goes beyond mere sympathy or antipathy for others. For example, with relevance to Study 3, political tolerance is often defined as “something one expresses towards persons or groups that one does not care for”⁷⁷. In such regards, the flexibility of self-reported measures are often key as they allow for a more ecologically valid approach, despite other drawbacks. Across the three studies, our self-reported measures focus on social interactions. To measure whether social interactions more broadly offer an ecologically valid context for studying intergroup relations between vaccinated and unvaccinated people, we designed a novel battery of six items. Three of the items tapped into (contemplating) social interactions, while the other three tapped into contemplating monetary transactions. We asked respondents how often or rarely they engage in each of the activities described on a five-point scale from “never” to “all the time”. For our pre-registered analysis, we average across the two sets of three items. As predicted, we find that people think more about social interactions ($m = 0.34$) than monetary transactions ($m = 0.19$) with vaccination outgroups (paired sample t-test, $\Delta M = 0.15$, 95% CI = [0.14, 0.16], $t(1447)=24.6$, $p < 0.001$). Extended Data Figure 1 displays the sample means for each item separately. It shows that while respondents report engaging in the three types of social interaction in roughly equal measures, there is a gap between thinking about generosity in broad terms, and actually considering donating outgroups money. The item describing a dictator game (e.g., used by⁷³) in most accurate terms (“I am asked to split money or other valuables between myself and another person,

who is [not] vaccinated against COVID-19”) received the lowest ratings. As consequence, the measures employed in the current study are less likely to be the subject of criticism regarding ecological validity than those employed in prior research^{69;70;73}.

L Study 3 – Forced-response design

Extended Data Figure 2 displays the proportion of respondents who would be unhappy if a vaccination outgroup member married one of their close relatives. We pre-registered the analyses on the pooled sample, but also report exploratory analyses splitting on respondents' vaccination status. Consistent with our main results, we see that about 4 out of 10 respondents express prejudice against vaccination outgroups, both with the direct question (44%, 95%CI = [0.40,0.48]), and the forced response technique (39%, 95%CI = [0.35,0.43]). As before, we see that vaccinated respondents display much more prejudice (at 50% agreement) than unvaccinated respondents (below 20% agreement). Yet, it is still noteworthy that unlike most of our experimental findings, around 1 in 5 or 6 unvaccinated respondents say they would be unhappy if a vaccinated person married their close relatives.

At the same time, our results also show that direct self-reports and estimates from the forced response design are neither statistically, nor substantially different either in the pooled sample ($\chi^2(1, 1210) = 2.31$, $p = 0.13$) nor among either of the subsamples (vaccinated respondents: $\chi^2(1, 883) = 1.36$, $p = 0.24$, unvaccinated respondents: $\chi^2(1, 327) = 2.20$, $p = 0.14$). Accordingly, we conclude that even direct self-reported measures of vaccination prejudice appear robust to desirability bias or demand effects.

M Study 3 – Incentivized vs non-incentivized dictator games

A potential objection against our conjoint experimental approach for studying prejudice across vaccination groups is that it relies on self-reported data, which may be subject to desirability bias or demand effects. Motivated by these concerns, prior research on generosity across vaccination groups relied on incentivized economic experiments to estimate individual level ingroup bias in generosity⁶⁹. Here, we replicate one of the most recent of these studies and test the assumption that the monetary incentives significantly affect average estimates of the generosity gap⁷³. Specifically, a randomly selected half of participants simply participated in a dictator game, where they had to split an endowment between themselves and a randomly matched recipient as if it was another self-reported

measure without incentives. The other half of the participants were informed that their allocation decisions will influence the number of bonus points they receive for participation. We anchored the incentive to be roughly equivalent to the incentive used in Korn et al⁶⁹. This incentive is over 10 times larger than the one used in by Henkel et al⁷³, where instead of a fixed (but modest) incentive paid out to everyone, participants enrolled at a lottery for 100EUR price. At 5,305 participants, this translates to a stake just below 2 eurocents, in expectation.

The experiment was embedded in Study 3 and thus was implemented on a sample of 1.4K US American adults. The results show that participants were significantly more generous towards their ingroup than their outgroups in both the incentivized ($\Delta M = 29$; 95%CI [26, 32], one-sample $t(724) = 19.4$, $p < 0.001$) and non-incentivized dictator game ($\Delta M = 30$; 95%CI [28, 33], one-sample $t(722) = 21.0$, $p < 0.001$). At the same time, the estimated level of ingroup-favoritism is very similar across the two conditions and the observed difference of 1.4 points is statistically insignificant (95%CI [-5.5, 2.6], $p = .49$), and statistically equivalent with 0, TOST independent samples t-test, $t(1445) = 2.16$, $p < 0.05$, with equivalence bounds set to the minimum detectable effect at 80% power and 5% alpha, Cohen's $d = +/- 0.15$.

N Study 3 – Affective prejudice, behavioral prejudice and ingroup bias in generosity

A potential limitation of our studies is that we bring evidence of exclusionary attitudes in the political domain only in Study 3, from a single country, the United States. Meanwhile, we have evidence of exclusionary attitudes in family relationships and stereotypes from twenty other countries. Should we assume that denial of political rights could be present in these other countries too?

Figure [N.1](#) displays a correlation matrix based on raw responses to the six outcome variable in Study 3's conjoint experiment. It indicates that affective prejudice (i.e., antipathy) shows sizable correlation with all five measures of exclusionary political attitudes, ranging from $r=0.35$ (access to unemployment benefits and freedom of speech) to $r=0.44$

(freedom of movement). However, admittedly, some of these correlations may be inflated if some respondents are systematically responding in similar ways across all items and profiles.

Accordingly, we also rely on Bayesian multilevel models to estimate individual level prejudice across all six measures. These are models which allow the AMCE for vaccination outgroups to vary between respondents, providing an opportunity to extract random effects for each individual. We use weakly informative priors and default MCMC settings from the `rstanarm` package (for details please refer to the computer code in the OSF repository). These models have the advantage over a naive mean difference between ingroup and outgroup profiles that they take into account the other attributes of the given profile. In other words, they should adjust for noise due to imbalances across the ten profiles on the other attributes. The top panel of Figure N.2 demonstrates that some of the correlation estimates are slightly smaller (Spearman’s $\rho_s = 0.25$), yet remain sizable.

Estimating prejudice at the individual level also has the advantage of allowing comparisons to responses in the dictator game. Is previous research demonstrating that people are less generous with unvaccinated partners than with vaccinated partners offer evidence of prejudice? Unsurprisingly, there are some correlations between the generosity gap and measures of prejudice. Antipathy ($\rho = 0.30$) and freedom of movement ($\rho = 0.37$) stand out the most. At the same time, freedom of residence is only weakly correlated ($\rho = 0.15$) and the remaining behavioral prejudice measures do not correlate with biased generosity at all. The middle panel demonstrates that the pooled correlations are driven primarily by vaccinated respondents. Meanwhile, the bottom panel shows that all correlations between ingroup bias in generosity and prejudice disappear if we zoom in on unvaccinated respondents. This is not so surprising if we consider that unvaccinated respondents displayed considerable ingroup bias themselves (i.e., transferring more money to unvaccinated than to vaccinated others), but very little prejudice. In short, it appears that people who are prejudiced are probably also less generous with an outgroup. But just because someone is more generous with their ingroup, they are not necessarily prejudiced against the outgroup. These results dovetail with cooperation research in social

psychology arguing that intergroup bias in dictator games is driven primarily by ingroup favoritism and not by outgroup discrimination⁷⁸.

Figure N.1: Study 3 – Correlation matrix across the conjoint outcome variables. Affective prejudice (antipathy) shows sizeable correlation with behavioral prejudice. N = 14,480.

Figure N.2: Study 3 – Correlation matrix across individual level estimates ingroup generosity bias and prejudice. The top panel shows the pooled sample with sizable correlations across the prejudice items, but much lower with generosity gap. The middle panel paints a similar picture using only vaccinated respondents. The bottom panel shows that among the unvaccinated estimates of prejudice remain highly correlated, but they are largely independent of ingroup bias in generosity. Total N = 1,448.

O Studies 1-3 – Robustness tests and diagnostics

O.1 Study 1 – Multilevel models for macro correlations

Our main analyses follow the convention in the conjoint experimental literature and analyse our samples by either pooling all data completely, i.e., ignoring variation by countries, or without any pooling at all, i.e., rerunning models in each country separately. These models are sometimes called fixed effects models. An alternative approach to analyzing our data is to rely on multilevel (or hierarchical) regression models, which partially pool across countries. Partial pooling has been shown to improve the precision of group-level (here, country-level) estimates and reduce bias due to overfitting.

We rerun our models of prejudice in a Bayesian multilevel setting relying on `rstanarm` library in R. We ran models both on the pooled sample and on vaccinated respondents only. We specified weakly informative priors, which help convergence but do not influence estimates. We relied on Leave-One-Out Cross-Validation to estimate model fit. Our best fitting model had varying intercepts for individuals (to account for multiple observations per respondents), varying intercepts for countries, and varying slopes for vaccination outgroup. The model explicitly quantifies the size of cross-country variation in prejudice against the unvaccinated at $SD = 0.08$, corresponding to 60% of the main effect. A neat feature of a Bayesian approach is that it quantifies the uncertainty around this variance parameter, and the 95% credible interval [0.06-0.11] does not include 0. In other words, both the model fit statistics and the variance parameter strongly imply that there is sizable cross-country variation in prejudice against the unvaccinated.

Figure [O.1](#) displays the fixed effects results reported in the main text and Bayesian multilevel estimates side by side. As expected, we find that Bayesian estimates of country-level effects have more narrow confidence/credible intervals and are pulled towards the grand mean. Meanwhile, while the point estimates for the main (across-country) effect is similar in both paradigms, the multilevel estimate has a much wider 95% confidence/credible interval, ranging from 10 to 17 points among the vaccinated and from 8 to 15 points in the pooled sample (recall that the width of the 95% confidence interval in the fixed effects estimate is just 2 points, from 12 to 14 points). This added uncertainty

is due to treating the 21 countries in our data as random draws from the population of all countries.

Figure O.1: Splitting AMCEs for each of the 6 profiles. We find some carry-over effects for prejudice. Errorbars denote 95% confidence or credible intervals. Total N = 64,440.

We also extend these analyses by incorporating a cross-level interaction between the indicator for targets from vaccination outgroups and macro-predictors. Figures O.2 and O.3 display these relationships for the pooled sample and for vaccinated respondents, respectively, along with 90 and 95% credible intervals for the entire range of the observed macro-indicators (rescaled by centering and dividing by two for computational reasons). These estimates corroborate that death is the strongest predictor, followed by social trust and vaccination – although for the latter two, we cannot rule out that the true relationship is 0.

Figure O.2: Estimating the relationship between macro-predictors and prejudice in a Bayesian setting – Pooled sample. Errorbands denote 90% and 95% credible intervals. Total $N = 64,440$.

Figure O.3: Estimating the relationship between macro-predictors and prejudice in a Bayesian setting – Vaccinated respondents. Errorbands denote 90% and 95% credible intervals. Total N = 64,440.

Table O.1: Bayesian MLM coefficient estimates for antipathy towards the unvaccinated

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	0.26 [0.22; 0.3]	0.26 [0.22; 0.3]	0.26 [0.22; 0.3]	0.26 [0.23; 0.29]	0.26 [0.23; 0.3]
Unvaccinated	0.13 [0.1; 0.17]	0.13 [0.11; 0.16]	0.13 [0.1; 0.17]	0.14 [0.1; 0.17]	0.13 [0.1; 0.16]
Deaths		0.04 [-0.04; 0.11]			0 [-0.06; 0.06]
Unvaccinated $\times$ Deaths		-0.08 [-0.14; -0.02]			-0.08 [-0.14; -0.01]
Vaccination			-0.06 [-0.13; 0.01]		-0.02 [-0.08; 0.05]
Unvaccinated $\times$ Vaccination			0.04 [-0.03; 0.11]		0.02 [-0.05; 0.09]
Social Trust				-0.11 [-0.16; -0.05]	-0.1 [-0.17; -0.03]
Unvaccinated $\times$ Social Trust				0.06 [-0.01; 0.13]	0.02 [-0.05; 0.1]

Table O.2: Bayesian MLM coefficient estimates for antipathy towards vaccination outgroups

	Model 1	Model 2	Model 3	Model 4	Model 5
Intercept	0.27 [0.24; 0.31]	0.27 [0.24; 0.31]	0.27 [0.24; 0.31]	0.27 [0.25; 0.3]	0.27 [0.24; 0.3]
Unvaccinated	0.11 [0.08; 0.15]	0.11 [0.08; 0.14]	0.11 [0.08; 0.15]	0.11 [0.08; 0.15]	0.11 [0.08; 0.14]
Deaths		0.04 [-0.04; 0.11]			0 [-0.06; 0.06]
Unvaccinated $\times$ Deaths		-0.08 [-0.14; -0.02]			-0.08 [-0.14; -0.01]
Vaccination			-0.06 [-0.13; 0]		-0.02 [-0.08; 0.04]
Unvaccinated $\times$ Vaccination			0.05 [-0.01; 0.12]		0.04 [-0.02; 0.11]
Social Trust				-0.11 [-0.16; -0.06]	-0.1 [-0.16; -0.03]
Unvaccinated $\times$ Social Trust				0.06 [-0.01; 0.12]	0.01 [-0.05; 0.08]

O.2 Simulations to assess generalizability of our results

In this section, we present simple simulations to probe the robustness of our conclusions on the cross-cultural prevalence of prejudice against the unvaccinated. Given that our data have been collected online, they necessarily exclude from our sample citizens who have no internet access. Is it possible that these groups exhibit so different attitudes from the surveyed population that they jeopardize our main conclusions? Given the evidence we present in SI Section D, we believe there is little reason to expect wildly different attitudes among offline populations. Yet, this assumption is untestable with the present data, therefore caution is warranted. Here, we exploit the fact that country-level internet penetration rates are well-documented, and hence, we can estimate the maximum potential bias in the most extreme scenario: where there is no prejudice against the unvaccinated among those vaccinated individuals who have no internet connection in a given country. To assess the sensitivity of our results, we ran the following simulation: for each country, we calculated prejudice as the weighted average of internet users (whose prejudice we know from our study 1) and non-internet users (assumed to have 0 prejudice). We use Wikipedia’s estimates of internet penetration per country.⁴ The figure below shows that even these most conservative prejudice estimates support the conclusions as they are stated in the main text: Across countries there is sizeable prejudice against the unvaccinated including in low- and middle-income countries as well (see Extended Data Figure 7). Also, we note that the significant cross-cultural correlations are robust to these small deviations with COVID-19 deaths per capita ($\rho = -0.51$, $p < 0.05$), social trust ($\rho = 0.67$, $p=0.001$) and cultural tightness ($\rho = 0.53$, $p < 0.05$) remaining robust predictors of the level of prejudice against the unvaccinated.

We repeat the same exercise with prejudice towards vaccinated from unvaccinated respondents. Here, our estimates would be biased if – unlike our respondents – offline unvaccinated citizens showed large positive prejudice towards the vaccinated. We assume the size of this prejudice is equal to the average prejudice towards the unvaccinated in the

⁴https://en.wikipedia.org/wiki/List_of_countries_by_number_of_Internet_users. We are mindful of the fact that this estimate is too lenient for the Indian (national urban) sample as it includes rural internet users excluded by our sampling, yet for the sake of transparency, we report results for India too.

pooled sample, or 13% percentage points. Again, Figure O.4 indicates that our results change very little and the substantive conclusion that we see no systematic prejudice against the vaccinated appears robust.

Figure O.4: Original average marginal component estimates of prejudice against the vaccinated (orange dots) and simulations estimating prejudice under maximal bias from non-online populations (purple dots). Even if offline unvaccinated citizens express considerable prejudice (equivalent to the cross-country mean prejudice of vaccinated respondents towards the unvaccinated) our main conclusions remain unchanged. Horizontal lines display 90% and 95% confidence intervals. Total $N = 10,386$.

As our final analysis, we relax the assumption that our samples are representative of the online population. We repeat the simulation above with two modifications: we assume that our sample is representative of $X\%$ of the online population, where X is set to vary between 10 and 100% percent. Next, we assume that the proportion of the population not represented by our data, i.e. $\%$ of people without internet access + $(100 - X)$ shows a prejudice that is between 25% and 100% of the country specific prejudice estimate. Figure O.5 reports these simulations. A number of important patterns emerge. First, the black

lines demonstrate that when the unobserved prejudice is assumed to be equal to (100% of) the observed prejudice, the simulated population prejudice is constant at the level we report in the main text. This is a direct consequence of the fact that bias in our estimates is the product of unrepresented population size *and* asymmetries between populations. We use a grey shaded area around these original estimates to convey a sense of the 95% confidence intervals around our estimates. We also draw a dashed line at 5% points, which is the minimum detectable effect (or smallest effect size of interest) as specified by our pre-registration. The figure demonstrates that when the unobserved prejudice is assumed to be smaller than the observed – colorful lines – the simulated population estimates diverge from the reported estimates. Yet, these divergences are relatively small and often within the 95% confidence intervals of the original estimates. Indeed, the most dramatic changes happen in Malaysia, Australia, and China, because the observed prejudice here is the largest. Still even here, the most extreme assumptions (i.e., that our data represents only 10% of the population, and that the observed prejudice is 4 times larger than the unobserved prejudice) still yield a prejudice above the 5% threshold. Replicating a pattern found in our heterogeneity estimates (SI Section [D](#)), countries with lower (observed) average prejudice also necessarily have smaller heterogeneities too and the simulated lines slowly converge to the main estimates. To sum up, unless we simultaneously assume that our data is informative of only a very small segment of the online population (which goes against prior research on YouGov panels⁷⁹, *and* that unobserved population segments show drastically different patterns of prejudice (which goes against the theory underlying the present study and empirical estimates of treatment effect heterogeneity), we can be reasonably certain that our results are robust. This makes sense because, if our data were noisy, our pre-registered predictors of cross-country prejudice would not correlate with our estimates.

Figure O.5: Estimates of prejudice against the unvaccinated at different levels of sampling bias. The black lines are the observed estimates and the grey shaded areas are 95 % confidence intervals around it. Dashed lines are the smallest effect size of interest (5% points). The x-axis denotes the percentage of the online population reflected in the observed estimate. The colored lines denote the percentage of observed prejudice that unrepresented individuals are assumed to hold.

O.3 Post-stratification weights

Figures O.6, O.7, O.8, and O.9 demonstrate that the effects of the vaccination out-group attribute are substantively similar across all outcomes if we drop post-stratification weights from our models.

Figure O.6: AMCEs among vaccinated respondents from models without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total $N = 64,440$.

Figure O.7: AMCEs in the pooled sample from models without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 64,440.

Figure O.8: Study 2–AMCE of vaccination attribute in the pooled sample from models with and without weights. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 18,270.

Figure O.9: Study 3–AMCE of vaccination attribute in the pooled sample from models with and without weights. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480.

O.4 Dropping participants with no opportunity to get vaccinated

As we mention above, we sought to field our experiment in countries with widespread access to COVID-19 vaccines. That said, one of our answer options allowed respondents to indicate if they had “no opportunity to get vaccinated yet.” This is an intentionally broad and vague category, partly seeking to reduce potential social desirability bias, wherein respondents feel they must lie about their vaccination levels. Yet, it is plausible that some respondents who picked this option had genuine difficulty accessing a COVID-19 vaccine they trust. Thus, unfairly lumping them together with vaccine hesitant respondents could bias our estimates. Figure [O.10](#) demonstrates that even if this is the case, any bias is likely to be very small as our results substantively mirror those in the main text.

O.5 Marginal means across studies and attributes

For the sake of brevity, most of our analyses focus on the average marginal component effect of the vaccination status attribute. However, conjoint experiments also allow to investigate the marginal mean level of antipathy or prejudice across all groups. Accordingly, below in Figures [O.11](#), [O.12](#) and [O.13](#) we report marginal means for all conjoint attribute levels from our three studies.

Figure O.10: AMCEs in the sample excluding respondents who had no opportunity to get vaccinated yet. Dots denote average marginal component effects. Errorbars denote 90% and 95% confidence intervals. Total N = 61,848.

Figure O.11: Marginal means (dots) across all attribute levels in Study 1 (pooling across countries). Errorbars denote 95% confidence intervals. Total N = 64,440.

Figure O.12: Marginal means (dots) across all attribute levels in Study 2 (pooling across countries). Errorbars denote 95% confidence intervals. Total N = 18,270.

Figure O.13: Marginal means across all attribute levels in Study 3 across each of the six outcome variables. Errorbars denote 95% confidence intervals. Total N = 14,480.

O.6 Conjoint diagnostic tests

Judging from Figure O.14, O.15 and O.16, the samples across all studies appear well balanced. Especially looking at samples pooling across countries for Studies 1 and 2, vaccinated and unvaccinated respondents were exposed to essentially the same amount of vaccinated and unvaccinated target profiles. Indeed, the asymmetry hovers around 0 and is small in magnitude in every single country, with deviations never more than 4 percentage points (in Russia, Study 1).

Next, in Figures O.17, O.18, O.19, and O.20 we display the frequency of each attribute level within each country. These figures increase our confidence that the randomization of the conjoint table has been implemented successfully in each country across all studies.

Figures O.21, O.22, and O.23 in turn split the sample on response time. Participants who complete the survey faster than the median respondent in their country are labeled as “faster,” and participants who complete it slower are labeled as “slower.” In Study 1, find that faster respondents are somewhat less sensitive to the manipulation, which is likely an artefact of inattention. This difference is statistically significant, although substantively small across all outcomes in Study 1. The differences are mostly insignificant in Study 2. Meanwhile, in Study 3, the difference is significant for “freedom of movement”-outcome, and for antipathy, but much smaller for the other four outcomes.

Finally, Figures O.24, O.25, and O.26 display the AMCEs for vaccination outgroups split by the profile number (A-F). In Study 1, the effects appear independent of the order of profiles for fear, trustworthiness, and intelligence. At the same time, we find some carry-over effects in that by the second and third trials (profiles C-F), the penalty for vaccination outgroups increases from 7 percentage points to about 13 points in the pooled sample, and from 9 percentage points to 16 points among vaccinated respondents. We take this as a sign that participants “learn” that in this experimental setting, vaccination status is the most important cue of their prejudice and lean on them more heavily after the first trial. While this is technically a slight violation of the identifying assumptions, we note that the prejudice identified in the first trial is already large enough to warrant attention.

Interestingly, we find no similar learning effects in Study 2. Meanwhile, in Study 3, we find the opposite effect, wherein the more profiles participants had seen, the less affected they are to the treatments, perhaps due to fatigue. At the same time, these differences are substantively small and hover on the edge of significance.

Figure O.14: Study 1—Regressing respondents' vaccination status on the vaccination and immigration attributes to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 64,440.

Figure O.15: Study 2—Regressing respondents' vaccination status on the vaccination attribute to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 18,270.

Figure O.16: Study 3—Regressing respondents' vaccination status on the vaccination attribute to assess balance. Dots denote regression coefficients. Errorbars denote 95% confidence intervals. Total N = 14,480.

Figure O.17: Study 1 – Displaying the frequency of all attribute levels in our conjoint experiment for the first 12 countries. Total N = 64,440.

Figure O.18: Study 1 – Displaying the frequency of all attribute levels in our conjoint experiment for the last 9 countries. Total N = 64,440.

Figure O.19: Study 2 – Displaying the frequency of all attribute levels in our conjoint experiment. Total N = 18,270.

Figure O.20: Study 3 – Displaying the frequency of all attribute levels in our conjoint experiment. Total N = 14,480.

Figure O.21: Study 1 –Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents appear to be less sensitive to target vaccination manipulation. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 64,440.

Figure O.22: Study 2 – Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents appear to be less sensitive to target vaccination manipulation. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 18,270.

Figure O.23: Study 3 – Splitting sample on respondents completing the survey above and below the median time in their country. Faster respondents are less sensitive to target vaccination manipulation, but only when it comes to disliking and support the freedom of movement of outgroup. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480.

Figure O.24: Study 1 – Splitting AMCEs for each of the 6 profiles. We find small carry-over effects for prejudice. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total $N = 64,440$.

Figure O.25: Study 2 – Splitting AMCEs for each of the 6 profiles. We find no carry-over effects. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 18,270.

Figure O.26: Study 3 – Splitting AMCEs for each of the 10 profiles. We find no carry-over effects. Dots denote average marginal component effects. Errorbars denote 95% confidence intervals. Total N = 14,480.

References

- [66] Michael Coppedge, John Gerring, Carl Henrik Knutsen, ... Oskar Rydén. *V-Dem Dataset 2022*. Varieties of Democracy (V-Dem) Project, 2022.
- [67] Farooq Mubarak, Reima Suomi, and Satu-Päivi Kantola. Confirming the links between socio-economic variables and digitalization worldwide: the unsettled debate on digital divide. *Journal of Information, Communication and Ethics in Society*, 18(3):415–430, 2020.
- [68] Andrew Gelman and Jennifer Hill. *Data analysis using regression and multi-level/hierarchical models*. Cambridge university press, 2006.
- [69] Lars Korn, Robert Böhm, Nicolas W Meier, and Cornelia Betsch. Vaccination as a social contract. *Proceedings of the National Academy of Sciences*, 117(26):14890–14899, 2020.
- [70] Ori Weisel. Vaccination as a social contract: The case of COVID-19 and US political partisanship. *Proceedings of the National Academy of Sciences*, 118(13), March 2021.
- [71] Thomas Hale, Noam Angrist, Rafael Goldszmidt, Beatriz Kira, Anna Petherick, Toby Phillips, Samuel Webster, Emily Cameron-Blake, Laura Hallas, Saptarshi Majumdar, et al. A global panel database of pandemic policies (oxford covid-19 government response tracker). *Nature human behaviour*, 5(4):529–538, 2021.
- [72] Marie Fly Lindholt, Frederik Jørgensen, Alexander Bor, and Michael Bang Petersen. Public acceptance of covid-19 vaccines: cross-national evidence on levels and individual-level predictors using observational data. *BMJ open*, 11(6):e048172, 2021.
- [73] Luca Henkel, Philipp Sprengholz, Lars Korn, Cornelia Betsch, and Robert Böhm. The association between vaccination status identification and societal polarization. *Nature Human Behaviour*, November 2022.

- [74] Nicole Naar. Gaming anthropology: The problem of external validity and the challenge of interpreting experimental games. *American Anthropologist*, 122(4):784–798, 2020.
- [75] Anne C Pisor, Matthew M Gervais, Benjamin G Purzycki, and Cody T Ross. Preferences and constraints: the value of economic games for studying human behaviour. *Royal Society open science*, 7(6):192090, 2020.
- [76] Jeffrey Winking and Nicholas Mizer. Natural-field dictator game shows no altruistic giving. *Evolution and Human Behavior*, 34(4):288–293, 2013.
- [77] Michael Petersen, Rune Slothuus, Rune Stubager, and Lise Togeby. Freedom for all? the strength and limits of political tolerance. *British Journal of Political Science*, 41(3):581–597, 2011.
- [78] Daniel Balliet, Junhui Wu, and Carsten K. W. De Dreu. Ingroup favoritism in cooperation: A meta-analysis. *Psychological Bulletin*, 140(6):1556–1581, 2014.
- [79] Nic Newman, Richard Fletcher, Anne Schulz, Simge Andi, Craig T Robertson, and Rasmus Kleis Nielsen. Reuters institute digital news report 2021. *Reuters Institute for the study of Journalism*, 2021.