Article

# Discriminatory attitudes against unvaccinated people during the pandemic

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Alexander Bor[1,2]✉, Frederik Jørgensen[1] & Michael Bang Petersen[1,3]✉

During the COVID-19 pandemic, sizeable groups of unvaccinated people persist even in countries with high vaccine access[1]. As a consequence, vaccination became a controversial subject of debate and even protest[2]. Here we assess whether people express discriminatory attitudes in the form of negative affectivity, 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 conjoined experimental studies ($n$ = 15,233), we demonstrate that vaccinated people express discriminatory attitudes towards unvaccinated individuals at a level as high as discriminatory attitudes that are commonly aimed at immigrant and minority populations[3–5]. By contrast, there is an absence of evidence that unvaccinated individuals display discriminatory attitudes towards vaccinated people, except for the presence of negative affectivity in Germany and the USA. We find evidence in support of discriminatory attitudes against unvaccinated individuals in all countries except for Hungary and Romania, and find that discriminatory attitudes are more strongly expressed in cultures with stronger cooperative norms. Previous 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, we find that contributors to the public good of epidemic control (that is, vaccinated individuals) react with discriminatory attitudes towards perceived free-riders (that is, unvaccinated individuals). National leaders and vaccinated members of the public appealed to moral obligations to increase COVID-19 vaccine uptake[10,11], but our findings suggest that discriminatory attitudes—including support for the removal of fundamental rights—simultaneously emerged.

In a historical feat of science, highly effective vaccines against SARS-CoV-2 were developed, tested, approved and mass produced in less than a year[12]. However, it soon became clear that achieving a sufficiently high uptake of these vaccines was in itself a major challenge[13]. 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 in regions in which vaccines were widely available[1]. At the same time, many countries continued to use interventions to control infection spread, resulting in feelings of pandemic fatigue, waning support for restrictions and dwindling trust in authorities[14–16].

Against this backdrop, public debates around COVID-19 have been heated. Some politicians have justified strict policies against unvaccinated individuals using highly moralistic rhetoric[10]. At the same time, disruptive public protests directed against vaccine mandates have taken place in several Western countries[2]. Survey research shows that divisions based on vaccination status are also emerging among the public[17,18]. Individuals who comply with the advice of health authorities morally condemn unvaccinated individuals for violating a social contract in the midst of a crisis[8,9,11]. Those who refuse vaccines report that they feel discriminated against[18] and pressured against their will[19]. Furthermore, vaccination status is consistently aligned with other political opinions such as trust in science and the authorities and, in the case of the USA, partisanship[9,13,20].

Previous research shows that political divides can harm everyday interactions between citizens by eliciting general antipathy in the form of prejudice[21]. 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 ref. [22] and define prejudice as "a negative evaluation of an individual that is significantly based on the individual's group membership" (see also refs. [23,24]). Prejudice can manifest itself in affective (for example, negative emotions), cognitive (for example, negative stereotypes) and attitudinal (for example, support for exclusion

[1]Department of Political Science, Aarhus University, Aarhus, Denmark. [2]Democracy Institute, Central European University, Budapest, Hungary. [3]Centre for the Experimental-Philosophical Study of Discrimination, Aarhus University, Aarhus, Denmark. ✉e-mail: bora@ceu.edu; michael@ps.au.dk

and discrimination) expressions of prejudiced individuals[25]. Here we investigate all three dimensions in the context of groups defined by COVID-19 vaccination status.

Research on the psychology of vaccination decisions before the COVID-19 pandemic[8] and before the implementation of COVID-19 vaccines[9] has shown that generosity in two-player behavioural economic games is indeed affected by the vaccination status of the players. Specifically, vaccinated individuals are less generous towards unvaccinated individuals but, importantly, unvaccinated individuals are not less generous towards vaccinated individuals. These findings are interpreted on the basis of the psychology of human cooperation[8]. Research on cooperation has provided strong evidence that people monitor cooperative situations for the existence of free-riders (that is, individuals who benefit from the cooperation without paying appropriate costs)[26] and react negatively towards free-riders after detection[6,7]. Vaccinations contribute to the public good of epidemic control[27], and refusal to receive a vaccination is accordingly spontaneously perceived as an instance of free-riding, motivating contributors (that is, vaccinated individuals) to withhold generosity from unvaccinated individuals[8]. As the spontaneous withholding of resources from unvaccinated individuals 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 previous studies—are generous only towards cooperators and withhold personal resources from strangers who are known to free-ride[28].

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 previous 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-riders[6]. Although previous 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 unvaccinated individuals, but may also express support for the imposition of costs on unvaccinated individuals by, for example, supporting their exclusion from social relationships or democratic rights and freedoms. Second, in this context, unvaccinated individuals may react with prejudice towards vaccinated individuals as well, grounded, for example, in perceived pressure and discrimination[18,19]. Indeed, a study examining generosity in two-player behavioural economic games after the implementation of COVID-19 vaccines found that unvaccinated individuals were also less generous towards vaccinated individuals, although ingroup favouritism was smaller than among vaccinated individuals[18]. 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 cooperativeness[29]. Consistent with this, research before the COVID-19 pandemic found that vaccinated individuals perceive unvaccinated individuals as less warm[8]. However, research on impression formation also documents that impressions of competence constitute another and independent evaluative dimension[29]. In the context of COVID-19 vaccines, this other dimension may also be activated as, for example, vaccinated individuals may perceive unvaccinated individuals as being unintelligent and incompetent for believing false information regarding vaccinations[30]. Discriminatory attitudes in the context of COVID-19 vaccines may therefore come to have a broader cognitive basis.

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 USA (study 3; $n$ = 14,480 observations from 1,448 respondents), studying the affective, cognitive and attitudinal dimensions of prejudice across groups defined by COVID-19 vaccination status. The dataset measures discriminatory attitudes across a diverse set of cultures from all inhabited continents of the world (Fig. 1). As previous research on the lack of generosity towards unvaccinated individuals has been limited to Western democratic contexts[8,9,18], 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 who are not vaccinated against COVID-19 reflect the activation of anti-free-rider sentiments, such attitudes may be more strongly expressed in countries that have invested substantially in the public good of suppressing deaths from COVID-19 and, in particular, in cultures in which citizens hold moral expectations that their fellow citizens support the provisions of such goods.

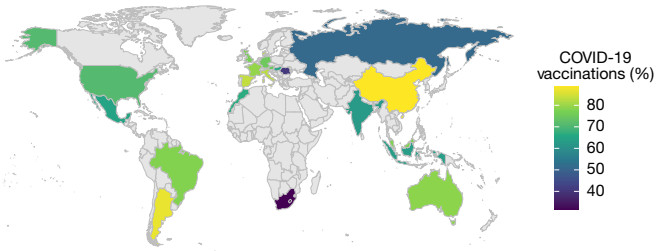


**Fig. 1 | World map highlighting the countries included in study 1.** Countries are coloured by the share of vaccinated citizens in the population on the first day of data collection (December 2021 to January 2022).

## 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 have been a key focus in previous cross-national research on prejudice along racial, ethnic and partisan lines[21,31]. Exclusion from family relationships is 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 families[32]; and, finally, relative to other forms of discriminatory attitudes (for example, support for state-sponsored discrimination), discrimination within families is something within the control of individuals and, therefore, something that can take immediate effect.

We used conjoint experiments in which respondents evaluated 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 enables us to examine a wide range of responses covering affective, cognitive and attitudinal components of prejudice[33].

Given our ambition to study discriminatory attitudes rather than generosity, we depart from previous work that relied on incentivized economic games (such as the dictator game)[8,9,18]. To help assess the validity of the conjoint experimental approach, we performed a number of tests. First, we show that people perceive that measures focusing on social interactions are more ecologically valid than those focusing on monetary transactions captured by economic games (paired sample $t$-test, $\Delta M$ = 0.15, 95% confidence interval = 0.14–0.16, $t_{1,447}$ = 24.6,

$P < 0.001$; Extended Data Fig. 1 and Supplementary Information K), 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% confidence interval = 0.40–0.48). Indeed, this estimate of antipathy is not statistically different from the estimate that we get using a forced-response technique, which was specifically designed to alleviate social desirability ($M = 39\%$, 95% confidence interval = 0.35–0.43, $\chi^2_{1,1,210} = 2.31$, $P = 0.13$; Extended Data Fig. 2 and Supplementary Information L). Finally, despite the presumed advantages of incentivized behavioural measures, we demonstrate that ingroup bias in generosity across vaccination groups is identical whether estimated with incentivized measures replicating previous research ($M = 29$; 95% confidence interval = 26–32, one-sample $t_{724} = 19.4$, $P < 0.001$) or with non-incentivized, self-reported measures ($M = 30$; 95% confidence interval = 28–33, one-sample $t_{722} = 21.0$, $P < 0.001$; $\Delta M = -1.45$, 95% confidence interval = −5.5–2.6, statistically equivalent to 0, TOST two-sample $t$-test, $t_{1,445} = 2.16$, $P < 0.05$; Supplementary Information M).

In the conjoint experiment for study 1, we adapted a widely used instrument of exclusionary reactions in family relationships[31] and examined a specific set of discriminatory attitudes: how unhappy would respondents be if a close relative was marrying an unvaccinated versus vaccinated person. Furthermore, we assessed the potential cognitive bases for discriminatory attitudes. First, we measured a reasonable basis for antipathy towards vaccination outgroups, namely, fear of infection[34] (note that, during the collection of these studies, the vaccine-evading Omicron variant was dominant[35], and vaccine-induced immunity against infection spread was waning[36] in most societies; this increased the chances of being infected by vaccinated people and therefore decreased the risk of interacting with unvaccinated individuals relative to vaccinated individuals). Although fear of infection is probably more pronounced among vaccinated individuals, some unvaccinated individuals have been found to hold the misinformed belief that vaccinated people themselves spread COVID-19 through vaccine shedding[37]. Second, we assessed the two key negative trait impressions underlying prejudice according to research on impression formation and prejudice—perceptions of untrustworthiness and unintelligence[29].

With the help of the 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 described in the 'Study 1' (Data and generalizability) section of the Methods, the samples can be considered to be representative of the countries' online populations (except for India). This large, cross-cultural dataset enables us to not only quantify discriminatory attitudes in a wide range of countries, but also 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 found large asymmetries by vaccination status, we report estimates separately for vaccinated and unvaccinated respondents. However, pooled estimates—which are reported in Supplementary Information 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 (Fig. 2 (left)). On average, vaccinated respondents were 13 percentage points (average marginal component effects (AMCE) 95% confidence interval = 12–14, $z = 25.65$, $P < 0.001$) 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 the 5% alpha level) in 19 out of the 21 countries. Malaysia is an outlier with very high exclusionary attitudes (AMCE = 36 percentage points, 95% confidence interval = 32–41, $z = 15.3$, $P < 0.001$), whereas the results in Romania (AMCE = 1 percentage point, 95% confidence interval = −4–6, $z = 0.39$, $P = 0.69$) and Hungary (AMCE = 4 percentage points, 95% confidence interval = −1–9, $z = 1.74$, $P = 0.08$) are inconclusive (further discussion is provided in Supplementary Information G). Interaction models estimating conditional AMCEs indicate that, although we observed exclusionary attitudes across all demographic groups, they are slightly stronger among highly educated (AMCE difference of 5 percentage points), female (by 4 percentage points), more affluent (by 3.5 percentage points) and older (by 2 percentage points) respondents (all $P < 0.05$; Supplementary Information D).

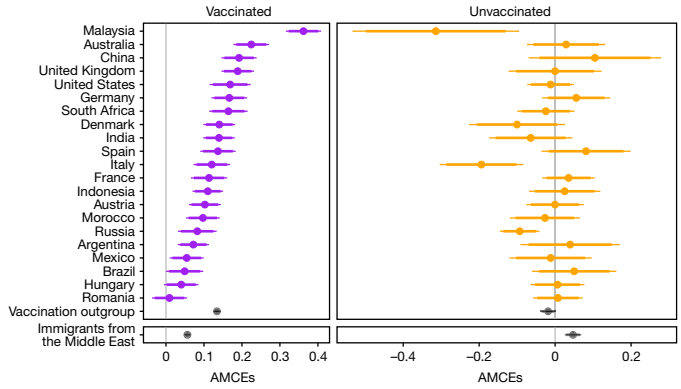


**Fig. 2 | Exclusionary attitudes in family relationships towards vaccination outgroups.** The average level of exclusionary attitudes in family relationships towards vaccination outgroups (that is, towards unvaccinated individuals for vaccinated respondents and towards vaccinated individuals for unvaccinated 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. The purple and orange points denote country-level AMCE estimates ($n > 3{,}000$) for vaccinated and unvaccinated respondents, respectively. The black points denote the pooled sample and include an estimate for exclusionary attitudes towards immigrants from the Middle East. The error bars denote the 90% and 95% confidence intervals. For more details, see the 'Study 1' (Modelling) section of the Methods.

Meanwhile, unvaccinated respondents ($n = 10{,}386$) exhibit negligible exclusionary attitudes towards vaccinated individuals (Fig. 2 (right)). Their unhappiness is largely independent of the target's vaccination status, with an AMCE of only −2 percentage points (AMCE 95% confidence interval = −4–0, $z = -1.81$, $P = 0.07$; the AMCE difference between vaccinated and unvaccinated respondents is 15 percentage points; 95% confidence interval = 13–18, $z = 13.33$, $P < 0.001$). Country-level estimates of exclusionary attitudes exhibited by unvaccinated respondents are noisy owing to the small sample sizes ($90 < n < 1{,}500$), ranging between −31 and 10 percentage points. Indeed, unvaccinated individuals in Malaysia, Italy and Russia even exhibit significant exclusionary reactions towards other unvaccinated individuals ($P < 0.01$), highlighting how anti-free-rider sentiments may take priority over sentiments related to ingroup favouritism[8].

To help assess the substantive size of these effects, it is helpful to compare them with exclusionary attitudes towards a group battling high levels of discrimination in many Western countries—immigrants from the Middle East[3]. Exclusionary attitudes towards unvaccinated individuals among vaccinated people (13 percentage points) is two and a half times greater than exclusionary attitudes towards Middle Eastern immigrants (5 percentage points, 95% confidence interval = 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 unvaccinated individuals is high. Supplementary Figs. E.1 and E.2 juxtapose country-level estimates of exclusionary attitudes towards the

two groups. Unvaccinated targets face significantly more exclusionary reactions compared with immigrants in 11 out of 21 countries, whereas immigrants do not face significantly more exclusionary reactions in any of the countries. Notably, exclusionary attitudes towards immigrants between vaccinated and unvaccinated individuals are substantively similar and not significantly different from 0 ($n$ = 64,440, AMCE difference 1 percentage point, 95% confidence interval = −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 (Supplementary Fig. E.3). Moreover, we did not find evidence that unvaccinated immigrants from the Middle East face disproportionate exclusionary attitudes compared with unvaccinated native individuals ($n$ = 64,440, AMCE difference 1 percentage point, 95% confidence interval = 0–1, $z$ = 1.1, $P$ = 0.27; Supplementary Fig. E.4).

## Stereotypes and exclusionary attitudes

Next, we examined whether exclusionary attitudes reflect a heightened risk of infection or also activate more fundamental stereotypes. As displayed in Extended Data Fig. 3a, we find large experimental effects of vaccination status among vaccinated respondents on fear of infection ($n$ = 54,054, 38 percentage points, 95% confidence interval = 37–40, $z$ = 65.99, $P$ < 0.001) and perceptions of untrustworthiness (13 percentage points, 95% confidence interval = 12–14, $z$ = 27.36 $P$ < 0.001). However, we also find an effect on incompetence (14 percentage points, 95% confidence interval = 13–15, $z$ = 29.00, $P$ < 0.001), suggesting that stereotypes of unvaccinated individuals 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% confidence interval = −2–2, $z$ = 0.2, $P$ = 0.84) or as incompetent (0 percentage points, 95% confidence interval = −2–2, $z$ = 0.37, $P$ = 0.71) either. If anything, they fear getting infected with SARS-CoV-2 by vaccinated people slightly less than by unvaccinated people (−2 percentage points, 95% confidence interval = −5–0, $z$ = −2.1, $P$ < 0.05). Country-level estimates of negative stereotypes against vaccination outgroups are shown in Supplementary Information C.

Our study also replicates a well-known finding from the impression-formation literature: impressions of trustworthiness have the greatest impact on overall exclusionary attitudes[29]. On the basis of a linear regression with respondent fixed effects, exclusionary attitudes are more closely associated with impressions of untrustworthiness ($\beta$ = 0.24, 95% confidence interval = 0.23–0.25) than with impressions of incompetence ($\beta$ = 0.17, 95% confidence interval = 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% confidence interval = 0.15–0.17, Wald test for equal effects: $\chi^2_1$ ($n$ = 64,440) = 112, $P$ < 0.001; Extended Data Fig. 3b). Although concerns about infection risks do shape exclusionary attitudes towards unvaccinated individuals, these findings suggest that negative stereotypes further enhance these attitudes.

## 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 cooperation[6]. At the same time, it is clear that the strength of the observed exclusionary attitudes exhibits substantial cross-cultural variation (formal evidence is provided in Supplementary Information O.1). Figure 3 shows exclusionary attitudes towards unvaccinated individuals by vaccinated respondents against three pre-registered macro-indicators—COVID-19 deaths and vaccinations (both standardized to population size) and social trust—as well as the 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 (that is, the tendency to trust fellow citizens) and cultural tightness (that is, 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 number of deaths due to COVID-19 low show strong exclusionary attitudes towards unvaccinated individuals at around 20 percentage points on average. By contrast, countries that struggled to mitigate the epidemic show much lower exclusionary attitudes. The Spearman's rank-order correlation between death and prejudice is $\rho_{21}$ = −0.62, 95% confidence interval = −0.83 to −0.26. At the same time, the association of exclusionary attitudes with actual vaccination levels is inconclusive $\rho_{21}$ = 0.38 (95% confidence interval = −0.06–0.70). Although there is a tendency for nations with a high level of vaccination to display more exclusionary attitudes, and countries with lower compliance to display less, there are also considerable deviations from this trend, with outliers such as Argentina (high vaccination, low prejudice) and South Africa (high prejudice, low vaccination). In Supplementary Information G, we analyse policy stringency, which is a direct measure of how much national governments invested in suppressing infections. We find no reliable association between stringency and prejudice towards unvaccinated individuals ($\rho_{21}$ = 0.23 95% confidence interval = −0.22–0.6; Extended Data Fig. 4).

Previous research demonstrated that epidemic suppression hinges on citizens' normative and moral expectations such that countries with higher social trust[39] and a tighter culture[40] suppressed the epidemic toll more effectively. As shown in Fig. 3, these cultural differences are also associated with higher prejudice towards unvaccinated individuals. Specifically, exclusionary attitudes are higher in countries with higher social trust (Spearman's $\rho_{21}$ = 0.57, 95% confidence interval = 0.19–0.81). In countries in which large majorities believe that 'most people can be trusted', the exclusionary reaction towards unvaccinated individuals is greater. Meanwhile, in countries in which most people 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% confidence interval = 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 unvaccinated individuals, as they may be perceived to free-ride on the collective effort[8]. In Supplementary Information O.2, we provide robustness checks for these cross-cultural conclusions, addressing potential threats to the generalizability of data obtained through online surveys.

## Antipathy across six countries

In study 2, we focused 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 the 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. Although fear of infection is a weaker correlate of exclusionary attitudes compared with trustworthiness impressions, the finding nonetheless raises the possibility that prejudice against unvaccinated individuals may be restricted to relationships characterized by physical interaction. The focus on pure antipathy in a neutral evaluation task enabled us to examine this possibility. Furthermore, to gain perspective on the level of antipathy across vaccination groups, in study 2, we also changed the benchmark group from Middle Eastern migrants to a more diverse set of four groups that are also frequent targets of prejudice: drug addicts, ex-convicts, people with mental illnesses and atheists[4,5,41]. These groups were chosen to offer some

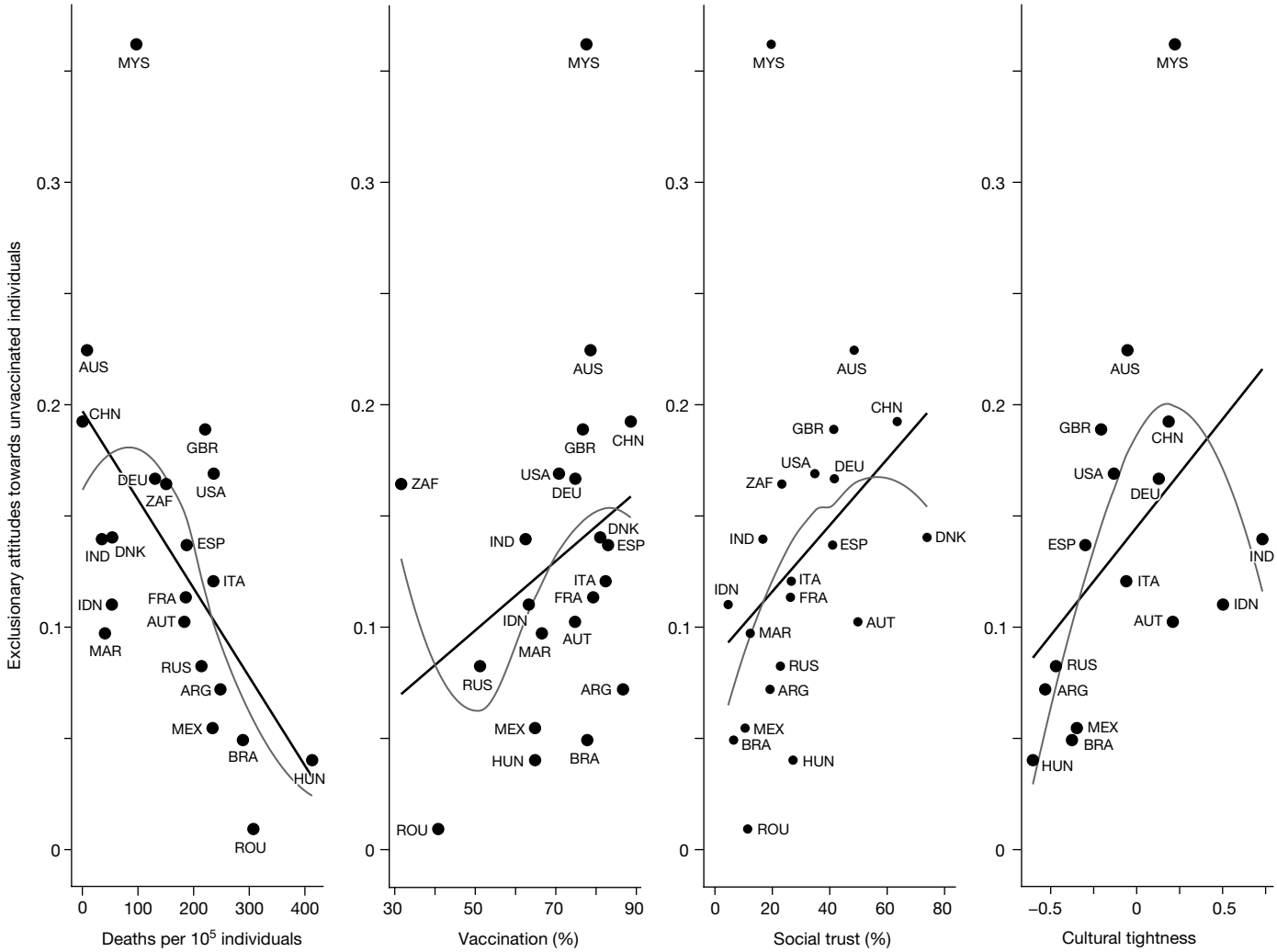


**Fig. 3 | The relationship between country-level indicators and cross-national levels of exclusionary attitudes among vaccinated individuals towards unvaccinated individuals.** The country-level indicators are country-level deaths from COVID-19; the national proportion of people expressing trust towards fellow citizens; the national proportion vaccinated against COVID-19; and cultural tightness scores. The labelled dots denote countries; the straight black lines denote best-fitting regression lines; and the grey curves denote loess curves. Spearman's rank-order correlations across the four facets: deaths, $\rho_{21} = -0.62$, 95% confidence interval = −0.83 to −0.26; social trust, $\rho_{21} = 0.57$, 95% confidence interval = 0.19–0.81; vaccination, $\rho_{21} = 0.38$, 95% confidence interval = −0.06–0.70; tightness, $\rho_{16} = 0.62$, 95% confidence interval = 0.18–0.85. Total $n = 64{,}440$.

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 the YouGov survey agency, study 2 was fielded in six countries (Germany, India, Indonesia, Morocco, South Africa and the UK), representing both Western affluent 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 Supplementary Information 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 pre-registered (see Data Availability).

Our analyses show that vaccinated individuals feel antipathy towards unvaccinated individuals, even in a neutral evaluation task without any indication that the participants would physically meet the fictitious targets (Fig. 4). Across all six countries, we found that vaccinated respondents ($n = 15{,}966$) dislike unvaccinated targets more than vaccinated targets, on average by 14 percentage points (AMCE 95% confidence interval = 13–15, $z = 25.94$, $P < 0.001$). By contrast, unvaccinated respondents ($n = 2{,}304$) on average do not dislike vaccinated targets significantly more than unvaccinated targets (AMCE = 1 percentage points, 95% confidence interval = −1–4, $z = 1.01$, $P = 0.31$, although Germany is a significant outlier, AMCE = 8 percentage points, 95% confidence interval = 3–13, $z = 3.12$, $P < 0.001$). Note also that the substantive size of the antipathy expressed towards unvaccinated individuals remains high relative to the more diverse set of benchmarks. On average across the six countries, unvaccinated individuals are disliked as much as people who struggle with drug addiction (15 percentage points, 95% confidence interval = 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% confidence interval = 9–11, $\chi^2_1$ ($n = 15{,}966$) = 18.4, $P < 0.001$), who are atheists (7 percentage points, 95% confidence interval = 6–8, $\chi^2_1$ ($n = 15{,}966$) = 67.5, $P < 0.001$) or who have a mental health illness (6 percentage points, 95% confidence interval = 5–7, $\chi^2_1$ ($n = 15{,}966$) = 87.9, $P < 0.001$). Country-level estimates of antipathy towards each of the four benchmarks are provided in Supplementary Information I.

Study 2 included an additional test. The finding from study 1 (that is, the widespread existence of exclusionary attitudes in personal relationships) may be less concerning if members of the groups of vaccinated and unvaccinated individuals are only weakly acquainted across group boundaries and if—consistent with intergroup contact

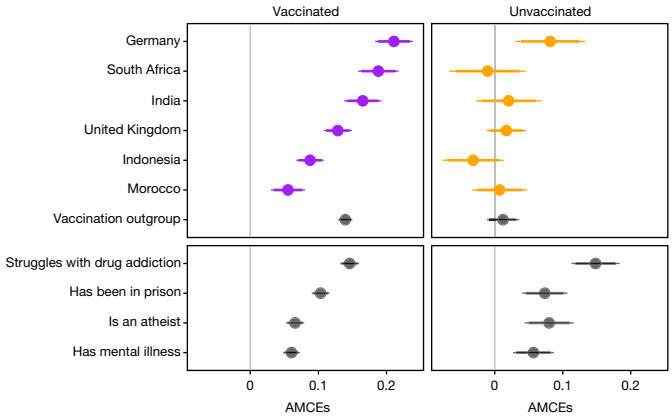


**Fig. 4 | Antipathy towards vaccination outgroups.** The average level of antipathy towards vaccination outgroups (that is, towards unvaccinated individuals for vaccinated respondents and towards vaccinated individuals for unvaccinated respondents; total $n = 18{,}270$). Antipathy reflects disliking a person from the vaccination outgroup versus the ingroup, with more positive coefficients indicating higher relative antipathy for the outgroup. The purple and orange points denote country-level AMCE estimates ($n > 3{,}000$) for vaccinated and unvaccinated respondents, respectively. The black points denote the pooled sample and include estimates for antipathy towards various other common targets of prejudice. The error bars denote the 90% and 95% confidence intervals. For more details, see the 'Study 2' (Modelling) section of the Methods.

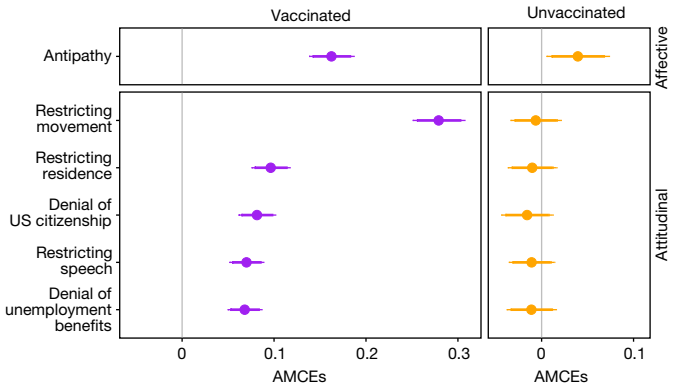


**Fig. 5 | Affective and attitudinal prejudice against vaccination outgroups.** Affective and attitudinal prejudice against vaccination outgroups in the USA (that is, towards unvaccinated individuals for vaccinated respondents and towards vaccinated individuals for unvaccinated respondents; $n = 14{,}480$). 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. The purple and orange points denote AMCEs among vaccinated and unvaccinated respondents, respectively. The error bars denote the 90% and 95% confidence intervals. For more details, see the 'Study 3' (Modelling) section of the Methods.

theory—prejudice is high only among individuals with less intergroup contact[42]. Study 2 therefore measured how many relatives and friends respondents have who belong to the vaccination outgroup. The analyses demonstrate that, although antipathy is indeed highest among people with no contact with the outgroups ($n = 18{,}270$, AMCE = 15 percentage points, 95% confidence interval = 13–16, $z = 20.36$, $P < 0.001$), it is substantial across all contact levels (AMCEs = 5–12 percentage points, $z \geq 2.4$, $P < 0.05$; Extended Data Fig. 5 and Supplementary Information J).

## Restriction of rights in the USA

So far, the discriminatory attitudes we have investigated have been demonstrated only in the domain of private relationships. Thus, study 3 examined 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 the USA—a country with historical recognition of fundamental rights and freedoms[43]. Study 3 was identical to study 2, except that the study also included five new outcomes in addition to the measure of antipathy, and the 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 neighbourhood"), 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") and access to unemployment benefits ("This person should receive unemployment benefits, if they are eligible and apply for it"). We collected data through 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 pre-registered (see Data availability). The survey also included the methodological studies discussed in relation to study 1, which are reported in detail in Supplementary Information K–M.

The results (Fig. 5) 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% confidence interval = 14–19, $z = 13.09$, $P < 0.001$), but they are also 28 percentage points less likely to respect their freedom of movement (95% confidence interval = 25–31, $z = 19.4$, $P < 0.001$), 10 percentage points less likely to respect their freedom of residence (95% confidence interval = 8–12, $z = 9.1$, $P < 0.001$), 8 percentage points less likely to support their application for citizenship (95% confidence interval = 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% confidence interval = 5–9, $z = 7.23$ and 7.44, respectively, $P < 0.001$). Vaccinated respondents expressed significantly higher exclusionary attitudes towards unvaccinated individuals than against atheists on all six outcomes, against mentally ill on five outcomes, and against individuals who have been in prison or struggle with drug addiction on three outcomes. By contrast, they do not express significantly higher exclusionary attitudes towards any of the benchmark groups on any of the outcomes compared with unvaccinated individuals (details on all Wald tests are provided in Supplementary Information I).

The results of study 3 also indicate that unvaccinated Americans also have some negative sentiment towards vaccinated individuals (4 percentage points, 95% confidence interval = 1–7), but unvaccinated Americans are neither more nor less likely to restrict their rights or freedoms. Finally, additional analyses (Supplementary Information N) indicate that the antipathy of vaccinated Americans towards unvaccinated individuals is predictive of their support for restricting the rights of unvaccinated individuals (Spearman's rank order correlations $0.35 < \rho_{1,448} < 0.44$).

## Discussion

Research on political polarization warns that, if sociopolitical disagreement—even if based on legitimate grievances—permeates interactions between citizens, it can contribute to the entrenchment of conflict[21]. Here we show that individuals who are 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 the USA. In the other countries, we examined only 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, in which we did not find evidence in support of discriminatory attitudes. Furthermore, we found that discriminatory attitudes towards unvaccinated individuals 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 the USA and Germany did we find that unvaccinated individuals feel some antipathy towards vaccinated individuals but even here we did not find statistical evidence in favour of negative stereotyping or exclusionary attitudes.

The finding that vaccinated individuals are prejudiced against unvaccinated individuals but that there is no evidence for the reverse is consistent with studies on the psychology of cooperation[6,7] and previous research on vaccinations. The cue that someone refuses to take up a vaccine activates psychological mechanisms designed to deter perceived free-riders among vaccinated individuals[8,9]. Consistent with the deep-seated nature of anti-free-rider sentiments, the observation of substantial and culturally widespread 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 unvaccinated individuals. Such norms are more reliably associated with cross-cultural differences in discriminatory attitudes than actual country-level differences in government efforts to produce epidemic control. What seems to trigger discriminatory attitudes towards unvaccinated individuals 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, 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 100 empirical studies identified 18 robust correlates of COVID-19 vaccine hesitancy in high-income countries[44]. 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 conditions[45], immunity from previous infection[46], a history of mental health issues that may intensify fear of vaccinations[47], negative past experiences with health authorities (especially as members of a minority group)[48], concerns due to country-specific public health scandals[49] or ethical considerations about vaccine equity[50].

Although moralistic communication of collective responsibilities may be an effective strategy to increase vaccination uptake[8], such strategies may have unintended negative consequences in the form of eliciting prejudice[51], 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-being[52], eroding identification with majority society[53] and driving mistrust of the state, including health authorities[54]. If the consequences of prejudice towards unvaccinated individuals 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 unvaccinated individuals 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 behaviour change among the public[55]. 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 public during a crisis[11], the authorities and politicians should consider tempering social animosities as an important part of their mandate, especially when societal conflict becomes more entrenched.

## Online content

Any methods, additional references, Nature Portfolio reporting summaries, source data, extended data, supplementary information, acknowledgements, peer review information; details of author contributions and competing interests; and statements of data and code availability are available at https://doi.org/10.1038/s41586-022-05607-y.


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# Methods

## Study 1

**Data and generalizability.** We collected data between 3 December 2021 and 28 January 2022 from the following 21 countries: Argentina, Australia, Austria, Brazil, China, Denmark, France, Germany, Hungary, India, Indonesia, Italy, Malaysia, Mexico, Morocco, Romania, Russia, South Africa, Spain, the UK and the USA. Data were collected through online panels by Ipsos in China, and by YouGov in all other countries. All of the participants provided informed consent and were reimbursed according to their standing agreements with the data provider. All of the 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. All hypotheses, materials and analyses were pre-registered at the OSF (https://osf.io/6teug).

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, the UK and the USA) and race (in the USA). Demographic details are provided in Extended Data Table 1. Quotas were always set to mimic the national population, except in Indonesia, Morocco and Malaysia, where, due to feasibility issues, they were set to the demographic characteristics of the online population and, in India, where they were 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 Supplementary Information A. Questionnaires were translated to the official language of the country by professional translators employed by a translation agency (deviations from this rule are described in Supplementary Information 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 low-income countries in which 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 members of society who have no internet access, or face other systemic disadvantages (for example, are illiterate or do not speak the official language of the country). Previous research concludes that, for results from cross-national data collected through 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 enables 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 behavioural insights during the pandemic[57]. Our main survey vendor, YouGov, enabled Imperial College London to capture global behavioural dynamics during the pandemic (https://www.imperial.ac.uk/global-health-innovation/what-we-do/our-response-to-covid-19/covid-19-behaviour-tracker/).

Previous research suggests that cross-cultural differences can be reliably studied using online surveys[58–60] and that studies using experimental designs (as we do) are particularly robust across a variety of sampling methods[61,62]. Nonetheless, it is important to acknowledge and address limitations to the 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, Supplementary Information 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, Supplementary Information 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 (Extended Data Fig. 6). Third, Supplementary Information 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 the offline population holds zero prejudice against unvaccinated individuals (Extended Data Fig. 7). Fourth, Supplementary Information O.2 also reports a stress test that examines the consequences of simultaneous violations of our two conjectures that (1) our samples represent the online populations and (2) 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 previous research and extensive robustness analyses strongly suggest that our conclusions hold as stated.

**Experimental design.** Our design is a subtle, conjoint experimental implementation of E. S. Bogardus' classic social distance scale[3] (that said, this does not mean that all of the countries in our sample must be prejudiced against Middle Eastern immigrants; although 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'). 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 labelling target individuals with their group membership and therefore offering no reason or justification for their choice.

We were interested in whether participants would have higher exclusionary attitudes against unvaccinated individuals marrying into their families. To benchmark the magnitude of this hypothesized prejudice, another attribute, labelled family background, distinguished between people who were "born and raised in [the respondent's country]" and people who "immigrated from the Middle East." Middle Eastern immigrants are an excellent benchmark, as widespread prejudice against them has been widely documented[3]. 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. We report in the pre-registration a sensitivity analysis justifying our sample size.

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 literature[33], 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 of the respondents. Importantly, before the treatment, we asked whether respondents themselves were vaccinated or not. We labelled 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 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, low-income respondents with a gross household income of less than 70% of the national median and respondents above that threshold.

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 100,000 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 from ref. [38] as a post hoc predictor of prejudice against unvaccinated individuals. Note that tightness scores were available for 16 out of our 21 countries, and Denmark, France, Morocco, Romania and South Africa are omitted from these analyses.

**Modelling.** According to standard practices in the literature on conjoint experiments[63], we analysed our data using ordinary least squares 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 were evaluated on the 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—were 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 relied on descriptive plots and Spearman's rank order correlations.

Our identification strategy rests on the random assignment of vaccination status to target individuals[63]. We report the standard diagnostic tests for conjoint experiments in Supplementary Information O.6. We find little reason for concern, although we acknowledge that participants speeding through the experiment dilute the observed experimental effects (Supplementary Fig. O.21) and that there are some carry-over effects for exclusionary attitudes but not the other three outcomes (Supplementary Fig. O.24). We also note that insofar as some of our respondents falsely claim to be vaccinated, our estimates of prejudice towards unvaccinated individuals are likely to be too conservative.

Finally, in Supplementary Information 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 excluded respondents who claimed that they had no previous 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 unvaccinated individuals. 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 had receded and when concerns about the pandemic were less outspoken.

**Data and design.** Our data were collected in May 2022 from six countries: Germany, India, Indonesia, Morocco, South Africa and the UK. As before, our data provider, YouGov, quota sampled a minimum of 500 respondents per country from online panels. All of the 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 the 'Study 1' section above).

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 attribute 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, 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 that membership is conditional on personal choice versus luck, and whether they pose a danger on others. A detailed description of all of the attributes (including two minor changes on background attributes) is provided in Supplementary Information B. Finally, beyond personal vaccination status, we also measured the personal experience of contact with vaccination outgroups and modelled the antipathy towards outgroups conditional on this variable.

As before, the target profiles were sampled completely at random from the $2 \times 5 \times 6 \times 5 \times 6 \times 5 = 9{,}000$ unique combinations of attribute levels (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 the OSF (https://osf.io/a7hsu).

**Measures.** To measure contact with vaccination outgroup members, we asked, before 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 and 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]".

**Modelling.** We followed the same modelling strategy as in study 1. We recoded the continuous dependent variables to the 0–1 range, with higher values indicating more dislike. To investigate whether the 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. Moreover, 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 individuals in an economic game both with and without monetary incentives[8,9,18]. (3) We also measured prejudice using the Bogardus' family context, both with a standard direct question and using a forced response technique[64]. Relying on these data, (4) we can investigate whether social desirability biases the propensity to admit prejudice towards vaccination outgroups. Finally, (5) we collect data to understand whether 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 the OSF (https://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 of the 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 the 'Study 1' section above).

The conjoint experimental design was identical to that of study 2, except each of the 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 ref. [18], the respondents participated in a dictator game, in which an allocator can give some of their 100 points endowment to another player, the recipient. All of the 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 SARS-CoV-2. We used the strategy method and elicited an allocation for both types of partners (in a random order). We experimentally manipulated between participants whether the 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 higher[18] than those used in previous 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 questions[64].

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

After a brief explanation of the rules of the dictator game (referred to as an allocation task), the participants were asked "How many points would [they] give to this vaccinated/unvaccinated person?" We calculated the difference in points allocated to vaccination outgroups versus ingroups. Higher scores indicate more ingroup favouritism.

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 with 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 that are similar to the one described in the study. Specifically, the participants answered how often or rarely they encounter six situations, three of which describe social interactions (for example, "I get upset when I think about interacting with all the people [not] vaccinated against COVID-19.") and three describing monetary transactions (for example, "I consider donating money to individuals [not] vaccinated against COVID-19.").

**Modelling.** For the conjoint experiment, we followed the very same analysis strategy as described for study 1. For the dictator game, we conducted simple *t*-tests to estimate whether the participants were significantly more generous towards their in-group members, and whether the size of this in-group favouritism was affected by the incentives offered. For Bogardus' measure of social distance, we estimated the uncertainty of the estimates both using standard CIs and also using a $\chi^2$ test to determine whether social desirability biases estimates compared with the direct question.

Finally, the measures of ecological validity were compared using *t*-tests.

### Reporting summary

Further information on research design is available in the Nature Portfolio Reporting Summary linked to this article.

## Data availability

All pre-registrations, data and materials necessary to reproduce or replicate our analyses are available online (https://osf.io/7hszd).

## Code availability

All computer code necessary to reproduce or replicate our analyses is available online (https://osf.io/7hszd).


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**Author contributions** A.B., F.J. and M.B.P. designed the experiments. A.B. and M.B.P. conducted the experiments. A.B. analysed data with input from F.J. and M.B.P.; A.B. and M.B.P. wrote the paper. All of the authors approved the final manuscript.

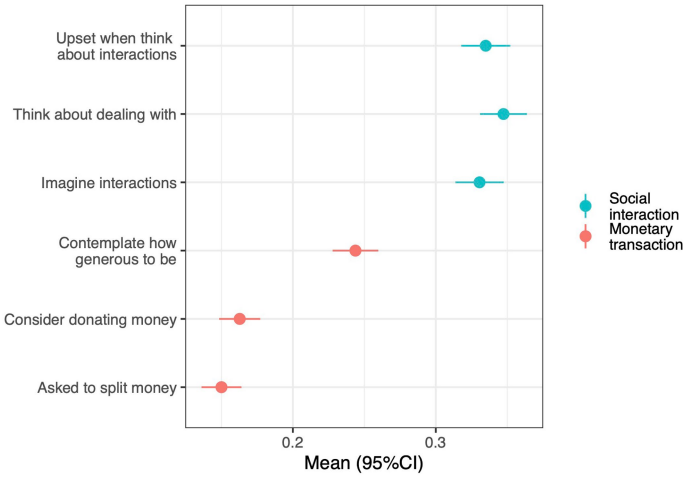
**Competing interests** The authors declare no competing interests.

**Additional information**

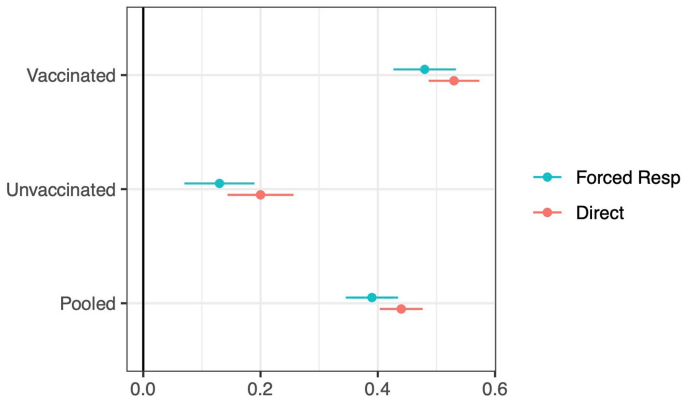
**Supplementary information** The online version contains supplementary material available at https://doi.org/10.1038/s41586-022-05607-y.

**Correspondence and requests for materials** should be addressed to Alexander Bor or Michael Bang Petersen.

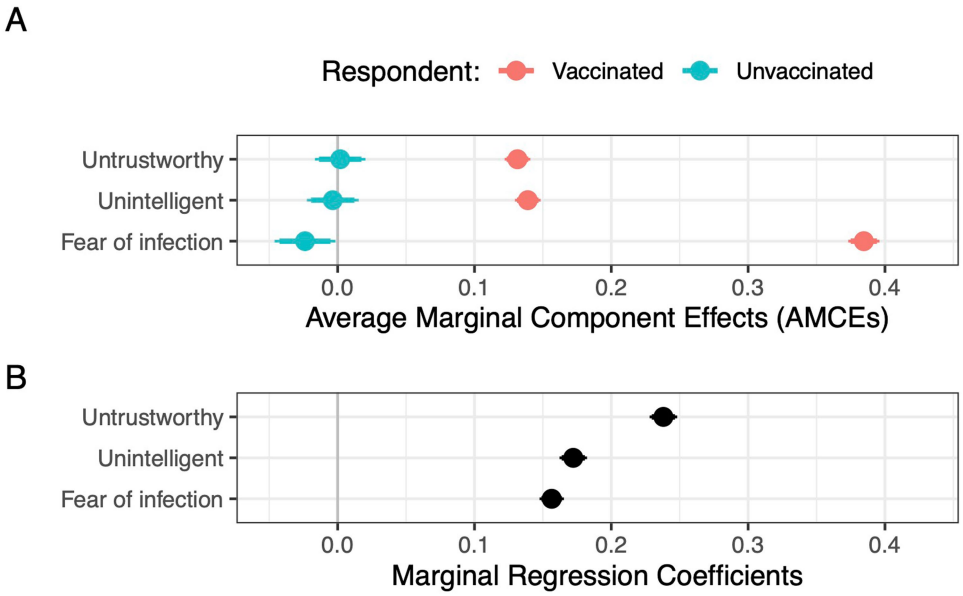
**Peer review information** *Nature* thanks Christian Crandall, John Roozenbeek and the other, anonymous, reviewer(s) for their contribution to the peer review of this work. Peer reviewer reports are available.



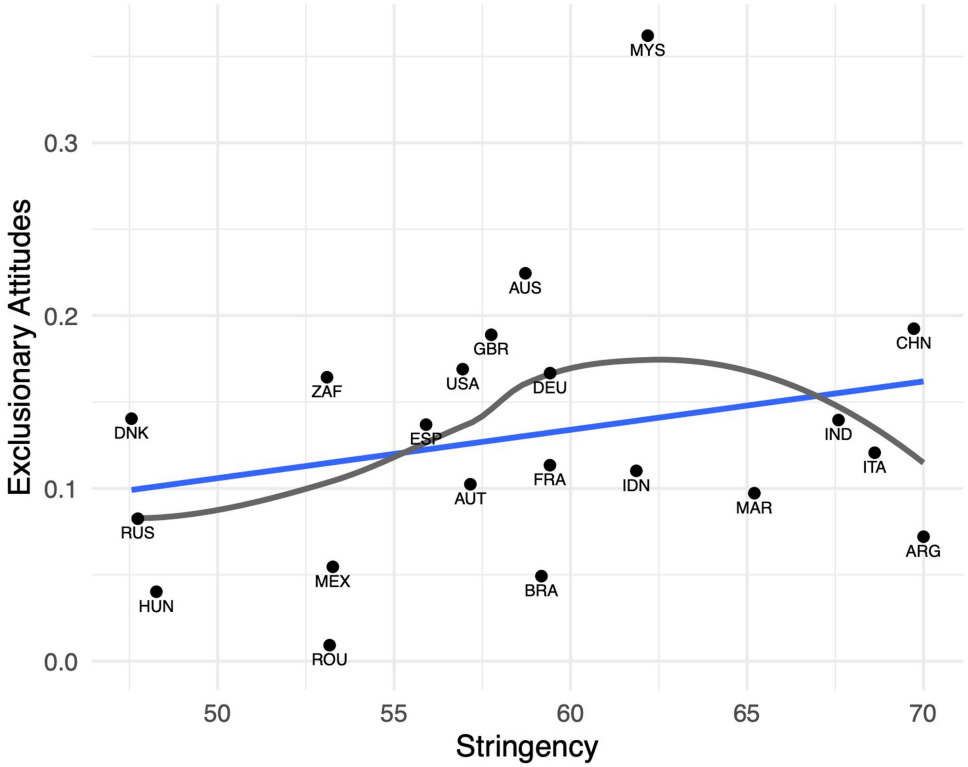

**Extended Data Fig. 1 | Participants think about social interactions substantially more than about monetary transactions with vaccination outgroups.** Dots denote means, errorbars denote 95% confidence intervals. N = 1,448. See more details in Supplementary Information K.



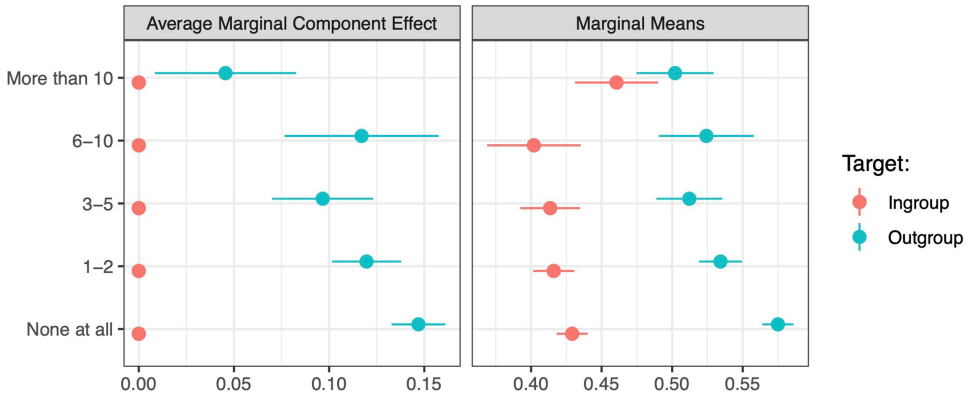

**Extended Data Fig. 2 | Proportion indicating unhappiness if a vaccination outgroup member married into their family.** The plot contrasts a standard direct question (in red) to a question implemented with the forced response technique (in blue). Both methods indicate identical conclusions. Errorbars denote 95% confidence intervals. N = 1,448. See more details in Supplementary Information L.



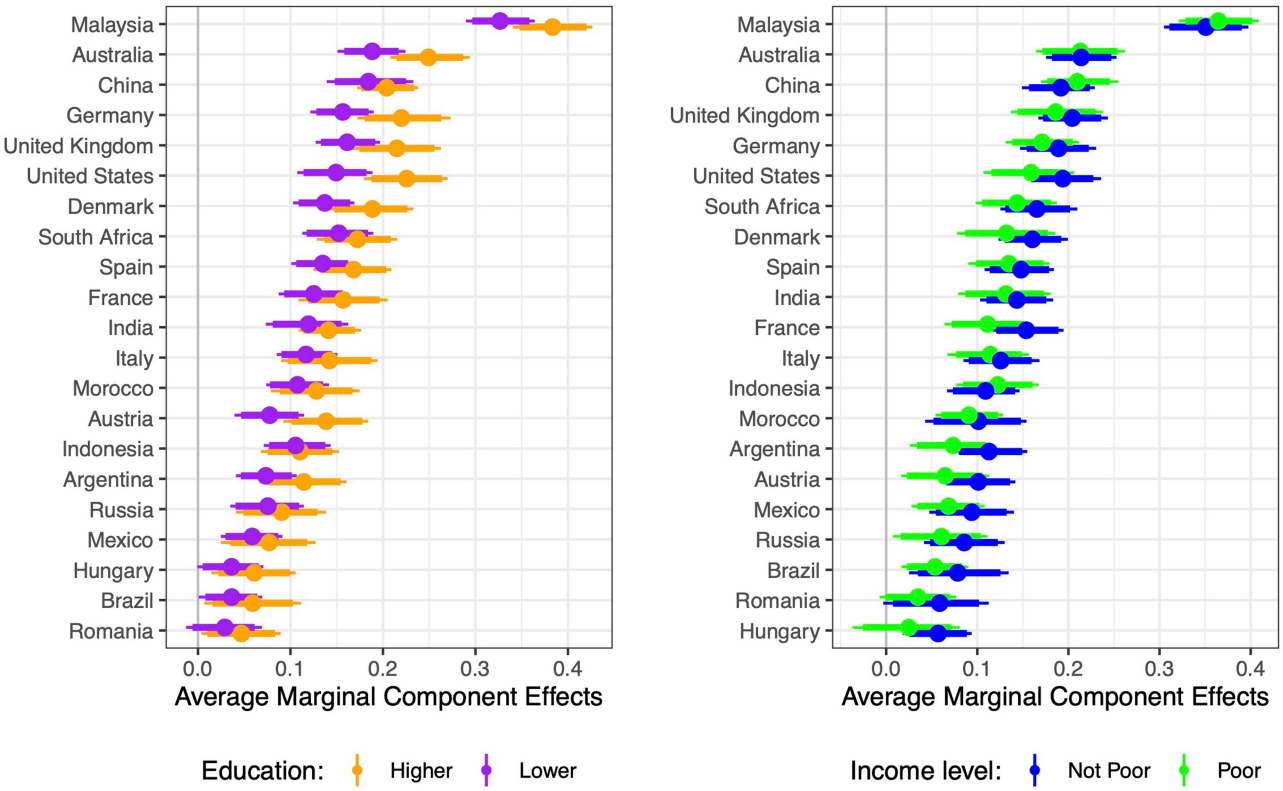

**Extended Data Fig. 3 | The role of the three proximate outcomes: fear of infection, untrustworthiness, and incompetence.** Panel A displays average marginal component effects of target vaccination status on each of the three outcomes splitting on respondent vaccination status ($N_{vaccinated}$ = 54,054 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 Supplementary Information C.



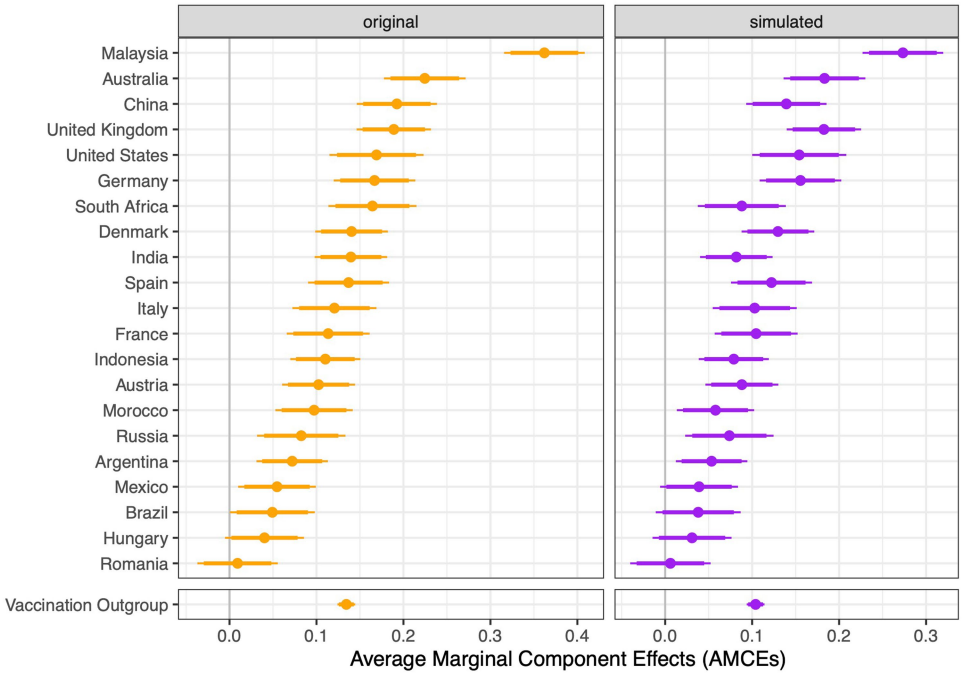

**Extended Data Fig. 4 | Relationship between prejudice against unvaccinated individuals and policy stringency.** Estimates of exclusionary attitudes (based on average marginal component effects). Blue line denotes best fitting linear regression line, and grey lines denotes a loess curve. (Total N = 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 Supplementary Information G.




**Extended Data Fig. 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 Supplementary Information J.




**Extended Data Fig. 6 | Heterogeneities in exclusionary attitudes by education and income in each country.** Average marginal component effects (AMCEs) for exclusionary attitudes against unvaccinated individuals 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 Supplementary Information D.




**Extended Data Fig. 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 unvaccinated individuals (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 Supplementary Information O.2.

**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 Table 2 | Study 1 – Attributes and levels in the conjoint experimenta**

| 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.

nature portfolio

Corresponding author(s): Alexander Bor & Michael Bang Petersen

Last updated by author(s): Nov 14, 2022

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|---|---|
| ☐ | ☒ The exact sample size (*n*) for each experimental group/condition, given as a discrete number and unit of measurement |
| ☐ | ☒ A statement on whether measurements were taken from distinct samples or whether the same sample was measured repeatedly |
| ☐ | ☒ The statistical test(s) used AND whether they are one- or two-sided<br>*Only common tests should be described solely by name; describe more complex techniques in the Methods section.* |
| ☒ | ☐ A description of all covariates tested |
| ☐ | ☒ A description of any assumptions or corrections, such as tests of normality and adjustment for multiple comparisons |
| ☐ | ☒ A full description of the statistical parameters including central tendency (e.g. means) or other basic estimates (e.g. regression coefficient) AND variation (e.g. standard deviation) or associated estimates of uncertainty (e.g. confidence intervals) |
| ☐ | ☒ For null hypothesis testing, the test statistic (e.g. *F*, *t*, *r*) with confidence intervals, effect sizes, degrees of freedom and *P* value noted<br>*Give P values as exact values whenever suitable.* |
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| ☒ | ☐ For hierarchical and complex designs, identification of the appropriate level for tests and full reporting of outcomes |
| ☒ | ☐ Estimates of effect sizes (e.g. Cohen's *d*, Pearson's *r*), indicating how they were calculated |

*Our web collection on statistics for biologists contains articles on many of the points above.*

## Software and code

Policy information about availability of computer code

Data collection: Data was collected by YouGov survey agency with their in house platform in all cases, except for Chinese data in S1, which was collected by Ipsos.

Data analysis:

```
R version 4.2.1 (2022-06-23)
Platform: x86_64-apple-darwin17.0 (64-bit)
Running under: macOS Monterey 12.6.1

Matrix products: default
LAPACK: /Library/Frameworks/R.framework/Versions/4.2/Resources/lib/libRlapack.dylib

locale:
[1] en_US.UTF-8/en_US.UTF-8/en_US.UTF-8/C/en_US.UTF-8/en_US.UTF-8

attached base packages:
[1] stats     graphics  grDevices utils     datasets  methods
[7] base

other attached packages:
 [1] psych_2.2.9       TOSTER_0.4.2      Hmisc_4.7-1
 [4] Formula_1.2-4     survival_3.3-1    lattice_0.20-45
 [7] tidybayes_3.0.2   matrixStats_0.62.0 DescTools_0.99.47
[10] viridis_0.6.2     viridisLite_0.4.1  here_1.0.1
[13] cregg_0.4.0       patchwork_1.1.2    car_3.1-1
[16] carData_3.0-5     ggforce_0.4.1      labelled_2.10.0
```

```
[19] haven_2.5.1      forcats_0.5.2     stringr_1.4.1
[22] dplyr_1.0.10     purrr_0.3.5       readr_2.1.3
[25] tidyr_1.2.1      tibble_3.1.8      ggplot2_3.3.6
[28] tidyverse_1.3.2  rio_0.5.29
```

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

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# Life sciences study design

All studies must disclose on these points even when the disclosure is negative.

| | |
|---|---|
| Sample size | *Describe how sample size was determined, detailing any statistical methods used to predetermine sample size OR if no sample-size calculation was performed, describe how sample sizes were chosen and provide a rationale for why these sample sizes are sufficient.* |
| Data exclusions | *Describe any data exclusions. If no data were excluded from the analyses, state so OR if data were excluded, describe the exclusions and the rationale behind them, indicating whether exclusion criteria were pre-established.* |
| Replication | *Describe the measures taken to verify the reproducibility of the experimental findings. If all attempts at replication were successful, confirm this OR if there are any findings that were not replicated or cannot be reproduced, note this and describe why.* |
| Randomization | *Describe how samples/organisms/participants were allocated into experimental groups. If allocation was not random, describe how covariates were controlled OR if this is not relevant to your study, explain why.* |
| Blinding | *Describe whether the investigators were blinded to group allocation during data collection and/or analysis. If blinding was not possible, describe why OR explain why blinding was not relevant to your study.* |

# Behavioural & social sciences study design

All studies must disclose on these points even when the disclosure is negative.

| | |
|---|---|
| Study description | Quantitative conjoint experiments implemented in online surveys. |
| Research sample | Participants were recruited from large online panels maintained by YouGov (for 20/21 countries) and Ipsos (in China) in S1. In S2 and S3, all data was from YouGov. We recruited at least 500 adult respondents from each country. In our pre-registration, we report a detailed power analysis. It demonstrates that 500 respondents (3,000 observations) per country yields 80% power to detect a main effect of 5 percentage points, and 95% power to detect an effect of 6 points. We judged 5 percentage point as the minimal effect size of interest. Studies 2-3 followed same strategy, although S3 was deliberately overpowered. The data collection used quota sampling on age, gender, and region of residence to ensure representativeness on these variables, and – conditional on feasibility – also education (in Australia, Brazil, Denmark, France, Germany, Italy, Mexico, Russia, Spain, UK, and US) and race (in the US). Quotas were 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. We provide detailed demographic information in Extended Data Table 1 for S1 and Supplementary Section A for S2 and S3. |
| Sampling strategy | The survey providers employed quota to ensure that the sample composition is "representative" of the wider population. Specifically, the data collection used 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). Quotas were 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. We recruited at least 500 adult respondents from each country. In our pre-registration, we report a detailed power analysis. It demonstrates that 500 respondents (3,000 observations) per country yields 80% power to detect a main effect of 5 percentage points, and 95% power to detect an effect of 6 points. We judged 5 percentage point as the minimal effect size of interest. Studies 2-3 followed same strategy, although S3 was deliberately overpowered.

| | |
|---|---|
| Data collection | The surveys were administered by third party companies, YouGov and Ipsos. As such, data was collected exclusively through double blind online surveys. |
| Timing | We collected data between December 3, 2021 and January 28, 2022 for Study 1. Studies 2-3 were collected simultaneously in May 2022. |
| Data exclusions | No data was excluded from the analyses, but only participants who passed a simple screener (weeding out bots) were allowed to participate in the experiment. |
| Non-participation | Study specific response rates were not shared by the survey providers (YouGov and Ipsos). |
| Randomization | The target profiles which participants rated in our experiment were generated completely at random. However, all participants read and responded to all questions. |

# Ecological, evolutionary & environmental sciences study design

All studies must disclose on these points even when the disclosure is negative.

| | |
|---|---|
| Study description | *Briefly describe the study. For quantitative data include treatment factors and interactions, design structure (e.g. factorial, nested, hierarchical), nature and number of experimental units and replicates.* |
| Research sample | *Describe the research sample (e.g. a group of tagged Passer domesticus, all Stenocereus thurberi within Organ Pipe Cactus National Monument), and provide a rationale for the sample choice. When relevant, describe the organism taxa, source, sex, age range and any manipulations. State what population the sample is meant to represent when applicable. For studies involving existing datasets, describe the data and its source.* |
| Sampling strategy | *Note the sampling procedure. Describe the statistical methods that were used to predetermine sample size OR if no sample-size calculation was performed, describe how sample sizes were chosen and provide a rationale for why these sample sizes are sufficient.* |
| Data collection | *Describe the data collection procedure, including who recorded the data and how.* |
| Timing and spatial scale | *Indicate the start and stop dates of data collection, noting the frequency and periodicity of sampling and providing a rationale for these choices. If there is a gap between collection periods, state the dates for each sample cohort. Specify the spatial scale from which the data are taken* |
| Data exclusions | *If no data were excluded from the analyses, state so OR if data were excluded, describe the exclusions and the rationale behind them, indicating whether exclusion criteria were pre-established.* |
| Reproducibility | *Describe the measures taken to verify the reproducibility of experimental findings. For each experiment, note whether any attempts to repeat the experiment failed OR state that all attempts to repeat the experiment were successful.* |
| Randomization | *Describe how samples/organisms/participants were allocated into groups. If allocation was not random, describe how covariates were controlled. If this is not relevant to your study, explain why.* |
| Blinding | *Describe the extent of blinding used during data acquisition and analysis. If blinding was not possible, describe why OR explain why blinding was not relevant to your study.* |

Did the study involve field work? ☐ Yes ☐ No

## Field work, collection and transport

| | |
|---|---|
| Field conditions | *Describe the study conditions for field work, providing relevant parameters (e.g. temperature, rainfall).* |
| Location | *State the location of the sampling or experiment, providing relevant parameters (e.g. latitude and longitude, elevation, water depth).* |
| Access & import/export | *Describe the efforts you have made to access habitats and to collect and import/export your samples in a responsible manner and in compliance with local, national and international laws, noting any permits that were obtained (give the name of the issuing authority, the date of issue, and any identifying information).* |
| Disturbance | *Describe any disturbance caused by the study and how it was minimized.* |

# Reporting for specific materials, systems and methods

We require information from authors about some types of materials, experimental systems and methods used in many studies. Here, indicate whether each material, system or method listed is relevant to your study. If you are not sure if a list item applies to your research, read the appropriate section before selecting a response.

## Materials & experimental systems

| n/a | Involved in the study |
|---|---|
| ☒ | ☐ Antibodies |
| ☒ | ☐ Eukaryotic cell lines |
| ☒ | ☐ Palaeontology and archaeology |
| ☒ | ☐ Animals and other organisms |
| ☐ | ☒ Human research participants |
| ☒ | ☐ Clinical data |
| ☒ | ☐ Dual use research of concern |

## Methods

| n/a | Involved in the study |
|---|---|
| ☒ | ☐ ChIP-seq |
| ☒ | ☐ Flow cytometry |
| ☒ | ☐ MRI-based neuroimaging |

# Antibodies

Antibodies used — *Describe all antibodies used in the study; as applicable, provide supplier name, catalog number, clone name, and lot number.*

Validation — *Describe the validation of each primary antibody for the species and application, noting any validation statements on the manufacturer's website, relevant citations, antibody profiles in online databases, or data provided in the manuscript.*

# Eukaryotic cell lines

Policy information about cell lines

Cell line source(s) — *State the source of each cell line used.*

Authentication — *Describe the authentication procedures for each cell line used OR declare that none of the cell lines used were authenticated.*

Mycoplasma contamination — *Confirm that all cell lines tested negative for mycoplasma contamination OR describe the results of the testing for mycoplasma contamination OR declare that the cell lines were not tested for mycoplasma contamination.*

Commonly misidentified lines (See ICLAC register) — *Name any commonly misidentified cell lines used in the study and provide a rationale for their use.*

# Palaeontology and Archaeology

Specimen provenance — *Provide provenance information for specimens and describe permits that were obtained for the work (including the name of the issuing authority, the date of issue, and any identifying information). Permits should encompass collection and, where applicable, export.*

Specimen deposition — *Indicate where the specimens have been deposited to permit free access by other researchers.*

Dating methods — *If new dates are provided, describe how they were obtained (e.g. collection, storage, sample pretreatment and measurement), where they were obtained (i.e. lab name), the calibration program and the protocol for quality assurance OR state that no new dates are provided.*

☐ Tick this box to confirm that the raw and calibrated dates are available in the paper or in Supplementary Information.

Ethics oversight — *Identify the organization(s) that approved or provided guidance on the study protocol, OR state that no ethical approval or guidance was required and explain why not.*

Note that full information on the approval of the study protocol must also be provided in the manuscript.

# Animals and other organisms

Policy information about studies involving animals; ARRIVE guidelines recommended for reporting animal research

Laboratory animals — *For laboratory animals, report species, strain, sex and age OR state that the study did not involve laboratory animals.*

Wild animals — *Provide details on animals observed in or captured in the field; report species, sex and age where possible. Describe how animals were caught and transported and what happened to captive animals after the study (if killed, explain why and describe method; if released, say where and when) OR state that the study did not involve wild animals.*

Field-collected samples — *For laboratory work with field-collected samples, describe all relevant parameters such as housing, maintenance, temperature, photoperiod and end-of-experiment protocol OR state that the study did not involve samples collected from the field.*

Ethics oversight — *Identify the organization(s) that approved or provided guidance on the study protocol, OR state that no ethical approval or guidance was required and explain why not.*

Note that full information on the approval of the study protocol must also be provided in the manuscript.

## Human research participants

Policy information about studies involving human research participants

| | |
|---|---|
| Population characteristics | See above. |
| Recruitment | Third party survey companies, YouGov and Ipsos, recruited samples as described above on the sampling strategy. With samples recruited online, asymmetry in prejudice between online and offline populations could be a potential source of bias. That said, because the share of offline populations is small and declining in virtually all countries, and because we have no theoretical reasons to expect any such asymmetries in discriminatory attitudes we consider this bias to be minimal (see more details in SI Section O2). Insofar as some of our respondents falsely claim to be vaccinated, our estimates of prejudice towards the unvaccinated are likely to be too conservative. |
| Ethics oversight | This study fully complies with Aarhus University's Code of Conduct and with the ethical standards set by the Danish Code of Conduct for Research Integrity. 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." All participants provided informed consent and were reimbursed according to their standing agreements with the data provider. |

Note that full information on the approval of the study protocol must also be provided in the manuscript.

## Clinical data

Policy information about clinical studies

All manuscripts should comply with the ICMJE guidelines for publication of clinical research and a completed CONSORT checklist must be included with all submissions.

| | |
|---|---|
| Clinical trial registration | *Provide the trial registration number from ClinicalTrials.gov or an equivalent agency.* |
| Study protocol | *Note where the full trial protocol can be accessed OR if not available, explain why.* |
| Data collection | *Describe the settings and locales of data collection, noting the time periods of recruitment and data collection.* |
| Outcomes | *Describe how you pre-defined primary and secondary outcome measures and how you assessed these measures.* |

## Dual use research of concern

Policy information about dual use research of concern

### Hazards

Could the accidental, deliberate or reckless misuse of agents or technologies generated in the work, or the application of information presented in the manuscript, pose a threat to:

| No | Yes | |
|---|---|---|
| ☒ | ☐ | Public health |
| ☒ | ☐ | National security |
| ☒ | ☐ | Crops and/or livestock |
| ☒ | ☐ | Ecosystems |
| ☒ | ☐ | Any other significant area |

### Experiments of concern

Does the work involve any of these experiments of concern:

| No | Yes | |
|---|---|---|
| ☒ | ☐ | Demonstrate how to render a vaccine ineffective |
| ☒ | ☐ | Confer resistance to therapeutically useful antibiotics or antiviral agents |
| ☒ | ☐ | Enhance the virulence of a pathogen or render a nonpathogen virulent |
| ☒ | ☐ | Increase transmissibility of a pathogen |
| ☒ | ☐ | Alter the host range of a pathogen |
| ☒ | ☐ | Enable evasion of diagnostic/detection modalities |
| ☒ | ☐ | Enable the weaponization of a biological agent or toxin |
| ☒ | ☐ | Any other potentially harmful combination of experiments and agents |

## ChIP-seq

### Data deposition

☐ Confirm that both raw and final processed data have been deposited in a public database such as GEO.

☐ Confirm that you have deposited or provided access to graph files (e.g. BED files) for the called peaks.

| | |
|---|---|
| Data access links<br>*May remain private before publication.* | *For "Initial submission" or "Revised version" documents, provide reviewer access links. For your "Final submission" document, provide a link to the deposited data.* |
| Files in database submission | *Provide a list of all files available in the database submission.* |
| Genome browser session<br>(e.g. UCSC) | *Provide a link to an anonymized genome browser session for "Initial submission" and "Revised version" documents only, to enable peer review. Write "no longer applicable" for "Final submission" documents.* |

### Methodology

| | |
|---|---|
| Replicates | *Describe the experimental replicates, specifying number, type and replicate agreement.* |
| Sequencing depth | *Describe the sequencing depth for each experiment, providing the total number of reads, uniquely mapped reads, length of reads and whether they were paired- or single-end.* |
| Antibodies | *Describe the antibodies used for the ChIP-seq experiments; as applicable, provide supplier name, catalog number, clone name, and lot number.* |
| Peak calling parameters | *Specify the command line program and parameters used for read mapping and peak calling, including the ChIP, control and index files used.* |
| Data quality | *Describe the methods used to ensure data quality in full detail, including how many peaks are at FDR 5% and above 5-fold enrichment.* |
| Software | *Describe the software used to collect and analyze the ChIP-seq data. For custom code that has been deposited into a community repository, provide accession details.* |

## Flow Cytometry

### Plots

Confirm that:

☐ The axis labels state the marker and fluorochrome used (e.g. CD4-FITC).

☐ The axis scales are clearly visible. Include numbers along axes only for bottom left plot of group (a 'group' is an analysis of identical markers).

☐ All plots are contour plots with outliers or pseudocolor plots.

☐ A numerical value for number of cells or percentage (with statistics) is provided.

### Methodology

| | |
|---|---|
| Sample preparation | *Describe the sample preparation, detailing the biological source of the cells and any tissue processing steps used.* |
| Instrument | *Identify the instrument used for data collection, specifying make and model number.* |
| Software | *Describe the software used to collect and analyze the flow cytometry data. For custom code that has been deposited into a community repository, provide accession details.* |
| Cell population abundance | *Describe the abundance of the relevant cell populations within post-sort fractions, providing details on the purity of the samples and how it was determined.* |
| Gating strategy | *Describe the gating strategy used for all relevant experiments, specifying the preliminary FSC/SSC gates of the starting cell population, indicating where boundaries between "positive" and "negative" staining cell populations are defined.* |

☐ Tick this box to confirm that a figure exemplifying the gating strategy is provided in the Supplementary Information.

## Magnetic resonance imaging

### Experimental design

| | |
|---|---|
| Design type | *Indicate task or resting state; event-related or block design.* |

Design specifications

*Specify the number of blocks, trials or experimental units per session and/or subject, and specify the length of each trial or block (if trials are blocked) and interval between trials.*

Behavioral performance measures

*State number and/or type of variables recorded (e.g. correct button press, response time) and what statistics were used to establish that the subjects were performing the task as expected (e.g. mean, range, and/or standard deviation across subjects).*

## Acquisition

Imaging type(s)

*Specify: functional, structural, diffusion, perfusion.*

Field strength

*Specify in Tesla*

Sequence & imaging parameters

*Specify the pulse sequence type (gradient echo, spin echo, etc.), imaging type (EPI, spiral, etc.), field of view, matrix size, slice thickness, orientation and TE/TR/flip angle.*

Area of acquisition

*State whether a whole brain scan was used OR define the area of acquisition, describing how the region was determined.*

Diffusion MRI ☐ Used ☐ Not used

## Preprocessing

Preprocessing software

*Provide detail on software version and revision number and on specific parameters (model/functions, brain extraction, segmentation, smoothing kernel size, etc.).*

Normalization

*If data were normalized/standardized, describe the approach(es): specify linear or non-linear and define image types used for transformation OR indicate that data were not normalized and explain rationale for lack of normalization.*

Normalization template

*Describe the template used for normalization/transformation, specifying subject space or group standardized space (e.g. original Talairach, MNI305, ICBM152) OR indicate that the data were not normalized.*

Noise and artifact removal

*Describe your procedure(s) for artifact and structured noise removal, specifying motion parameters, tissue signals and physiological signals (heart rate, respiration).*

Volume censoring

*Define your software and/or method and criteria for volume censoring, and state the extent of such censoring.*

## Statistical modeling & inference

Model type and settings

*Specify type (mass univariate, multivariate, RSA, predictive, etc.) and describe essential details of the model at the first and second levels (e.g. fixed, random or mixed effects; drift or auto-correlation).*

Effect(s) tested

*Define precise effect in terms of the task or stimulus conditions instead of psychological concepts and indicate whether ANOVA or factorial designs were used.*

Specify type of analysis: ☐ Whole brain ☐ ROI-based ☐ Both

Statistic type for inference
(See Eklund et al. 2016)

*Specify voxel-wise or cluster-wise and report all relevant parameters for cluster-wise methods.*

Correction

*Describe the type of correction and how it is obtained for multiple comparisons (e.g. FWE, FDR, permutation or Monte Carlo).*

## Models & analysis

| n/a | Involved in the study |
|---|---|
| ☐ | ☐ Functional and/or effective connectivity |
| ☐ | ☐ Graph analysis |
| ☐ | ☐ Multivariate modeling or predictive analysis |

Functional and/or effective connectivity

*Report the measures of dependence used and the model details (e.g. Pearson correlation, partial correlation, mutual information).*

Graph analysis

*Report the dependent variable and connectivity measure, specifying weighted graph or binarized graph, subject- or group-level, and the global and/or node summaries used (e.g. clustering coefficient, efficiency, etc.).*

Multivariate modeling and predictive analysis

*Specify independent variables, features extraction and dimension reduction, model, training and evaluation metrics.*