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Investigating how historical legacies of militarized violence can motivate COVID-19 vaccine hesitancy: Evidence from global dyadic survey

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A B S T R A C T

Background: In almost all countries, COVID-19 vaccines available for public use are produced *outside* of that country. Consistent with recent social science research, we hypothesize that legacies of violent conflict from vaccine-producing against vaccine-consuming countries may motivate vaccine hesitancy among people in targeted countries that purchase vaccines produced by the erstwhile aggressor.

Methods: Our analyses draw on data from the Correlates of War project and a large, representative survey of 18,291 adults that asked respondents in 16 countries to self-report their attitudes toward COVID-19 vaccines originating from 12 potential vaccine-producing countries in December 2020 (184 country-pairs, 208,422 ratings). For the main analysis, we used random-effect linear probability models and turned to Bayesian Model Averaging to probe the robustness of the main findings.

Results: We demonstrate that elevated levels of historical violence between vaccine-producing and vaccine-consuming countries are associated with increased negative feelings toward a COVID-19 vaccine produced by the vaccine producer.

Conclusion: Global vaccine hesitancy may result, at least in part, from public perceptions of historical conflict between vaccine-producing and vaccine-consuming countries. These results can help public health practitioners better preempt and adjust for cross-national vaccine resistance.

Keywords: Covid-19; violence, vaccine hesitancy; survey research; health behavior; vaccine uptake.

1. Introduction

In a global context, COVID-19 vaccines available to most people originate from a country that is not their own. Correspondingly, we might ask whether or not global vaccine hesitancy may result, in part, from legacies of violence between the vaccine-producing and vaccine-consuming countries.

Such nexus between violence and specific vaccine attitudes would not be a serious concern if vaccines were developed by a large number of countries, or only by countries with little legacy of interstate violence. Unfortunately, neither is the case. Vaccine development and production require enormous public and private investment; therefore, only firms in a small number of countries with high state capacity develop the vast majority of successful vaccines. At the same time, countries with higher state capacity are more active and aggressive in international politics (Clark et al., 2008; Palmer and Morgan, 2011), including the use of military weapons toward other countries. For example, the five countries that developed successful and widely-distributed vaccines against COVID-19—China, Germany, Russia, United Kingdom, United States, as of this writing, a tiny fraction consisting of about 3% of the countries—account for about 19% of all militarized actions of one country against another between 1950 and 2014 and 14% of all severe instances. This association between aggression and vaccine development makes our study all the more important.

We argue that a history of inter-country violence is one important mechanism through which people of one country develop animosities toward and distrust foreign countries and their regulatory systems and products, which, in turn, affect opinions and decisions about the consumption of a variety of products, including vaccines. A growing literature suggests that the legacies of aggression can have a long-lasting, even intergenerational impact on communities and a wide range of individual attitudes and behavior, including trust in people, governments, and companies in the erstwhile aggressors (Balcells, 2012; Besley and Reynal-Querol, 2014; DiGiuseppe and Barry; Homola et al., 2020; Klein, 2002; Klein et al., 1998; Lupu and Peisakhin, 2017; Rozenas et al., 2017). When a newly-developed vaccine is concerned, a key issue is over its quality and safety. In such situations, we argue that trust in government and scientific institutions—including attitudes toward the countries from which vaccines originate—play a particularly important role

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in one's willingness to consume products (Dror et al., 2021; Kreps et al., 2020; Lane et al., 2018; Motta, 2021; Stöckli et al., 2022). As such, we hypothesize that legacies of inter-country violence might negatively influence people's perceptions of specific foreign vaccines and—as a result—decrease vaccine uptake.

Studying how legacies of past conflict between countries can affect the willingness of citizens to take a particular vaccine has several important public health implications. First, and perhaps most importantly, our research contributes to the complex web of social and political factors found to underlie contemporary vaccine hesitancy. Past work primarily focuses on both contemporary and individual-level social, political, and psychological determinants of vaccine refusal in the United States (Callaghan et al. 2019, 2021; Dubé et al., 2013; Gadarian et al., 2021; Grossman et al., 2020; Martinelli and Veltri, 2021) and around the world (Barceló et al., 2022; Hornsey et al., 2018; Kobayashi et al., 2021; Lunz Trujillo and Motta, 2021; Murphy et al., 2021). Our work, in contrast, offers insights into how *historical geopolitical* factors might influence global vaccine hesitancy. In doing so, we not only build on and extend previous social science research suggesting that legacies of conflict can have important and long-standing effects on individuals' opinions and behavior (Besley and Reynal-Querol, 2014; Homola et al., 2020; Lupu and Peisakhin, 2017), but can help scholars better understand and preempt potential challenges to global vaccination efforts.

Second, the consideration of previous conflicts is also salient because countries that have the capacity to develop powerful vaccines also have the resources to be aggressive toward other countries (Clark et al., 2008; Palmer and Morgan, 2011). All countries that developed COVID-19 vaccines have not only the capacity but willingness to intervene internationally and militarily, both of which can have downstream effects on people's willingness to receive a vaccination. Correspondingly, our research can help public health practitioners better preempt vaccine resistance cross-nationally (MacDonald and the SAGE Working Group on Vaccine Hesitancy 2015). In so doing, our study answers the call for research into how country of origin may affect health care innovation diffusion (Harris et al., 2016) and more interdisciplinary research into how such determinants will impact the ability to mitigate the human and economic costs of pandemics (Latkin et al., 2021; Wen et al., 2021).

2. Methods

2.1. Survey data and outcome variables

On December 7–20, 2020, YouGov fielded a multi-country survey to understand how people in one country would evaluate COVID-19 vaccines developed in different countries. To our knowledge, it is the largest *dyadic* survey of vaccine preferences with 18,291 respondents and 208,422 vaccine evaluations across 16 countries (see Section A in SI for greater details). A "dyad" (p, c) refers to a pair of countries where p and c represent vaccine-producing country and vaccine-consuming country, respectively. YouGov's study design team deemed 12 countries as potential vaccine producers, all of which have produced vaccines that were either undergoing human clinical trials or are approved for public use, including Australia, Canada, China, France, Germany, India, Iran, Russia, Singapore, South Korea, the United Kingdom, and the United States. These countries account for 29% of all instances of militarized aggression against another country between 1950 and 2014 and 24% of all severe instances (calculated by the authors).

The survey was conducted in 16 vaccine-consuming countries: Australia, China, Denmark, France, Germany, Italy, India, Indonesia, Mexico, Poland, Singapore, Spain, Sweden, the United Arab Emirates, the United Kingdom, and the United States, all of which were again chosen by YouGov. Each sample was drawn from their online panel in a manner that reflects the national population in terms of the key demographics like age, gender, region, etc. Throughout our analysis, we use the weights provided by YouGov to adjust for over- and under-representation of different groups.

Respondents were asked whether they "would […] tend to think more positively or negatively about a COVID-19 vaccine if [they] saw that it was developed in each of the following countries, or would it make no difference?" They were shown the list of the aforementioned 12 potential vaccine developers and could indicate their vaccine preferences by choosing whether they would react to the specific hypothetical foreign vaccine "more positively," "more negatively," or that it made "no difference," or that they did not know.

In YouGov's survey, 8 countries were included as both vaccine-producing and consuming countries. These are Germany, UK, Australia, France, USA, Singapore, India, and China. The data clearly show a home bias—people universally prefer vaccines developed in their own countries to those developed abroad (see Section A in SI for detail; see also Smith (2021) and Barceló et al. (2022)). Given our interest in evaluations of vaccines produced abroad, we remove these 8 dyads—(p, c) where $p = c$—from our data. In total, our data include 184 dyads after dropping evaluations of vaccines made in one's country.

Figure A1in SI shows the net favorability ratings—i.e., the share of respondents who answered "more positively" minus the share of those who answered "more negatively"—for dyads of vaccine origins and targets. The outcomes of interest for our main analysis are two indicators for whether the respondent chose "more positively"/"more negatively" or not for a particular foreign vaccine.

2.2. Key explanatory variables

The primary explanatory variable in our study is a measure of each country's history of violent conflict with the vaccine-producing countries, which varies by the 184 dyads. We collect these data via reporting of incidents of (1) war or (2) militarized interstate disputes (MIDs) collected by the Correlates of War (COW) project. Since wars do not occur over the most-recent decades in our data set, we follow the rich literature on international conflict and use militarized actions to operationalize violent interstate actions. MIDs are defined as "cases of conflicts in which the threat, display or use of military force short of war by one member state is explicitly directed towards the government, official representatives, official forces, property, or territory of another state" (Jones et al., 1996,p. 163). As our interest is in directed actions in conflicts between two countries, we specifically draw on the most recent version of the Dyadic MID data (4.02) (Maoz et al., 2019).

For each of these conflict cases, the data include the highest hostility level from each of the belligerents in the dyad with 1 representing "no militarized action," 2 "threat to use force," 3 "display of force", 4 "use of force", and 5 "war" in an ordinal fashion. Using this information, we first build a data set of all 184 relevant dyads, capturing the annual number of cases with any action (hostility level 2 and up) and with severe actions (level 4 and up) that the vaccine-producing country perpetrated against the target and potential vaccine-consuming country.

The temporal domain of our conflict data starts in 1950. While the Dyadic MID data set covers the 1816–2014 period, we arrived at 1950 as the cutoff for a few reasons. First, some countries included in our survey data simply did not exist as independent entities long before 1950 (e.g. the United Arab Emirates, Singapore, Indonesia). That would lead to severely uneven lengths of history for countries in which surveys were taking place. Second, we decided against including the World War II conflict on substantive grounds. The end of World War II rang in a new configuration of the world that is still recognizably the world we live in, suggesting that this is what we consider as the relevant past. Third, the practical effect of extending the time cutoff further into the past would be slim given the discounting of the past we apply to the conflict variable (which we will introduce below) and the relatively rare occurrences of militarized conflicts.

Since the latest Dyadic MID data only goes up to 2014, we augment our conflict data using a rich additional data source capturing all events between countries as reported by the BBC Monitoring's Summary of World Broadcasts. These data lack a long historical time span, but are

available for very recent years (Althaus et al., 2020). Using these data from 1980 to 2014, we train a random forest model to "translate" between the events data and the annualized dyadic MIDs so that we can use event data to predict the occurrences of MIDs for every dyad between 2014 and 2019. See Section C in SI for details. As a result, our data set of MIDs has a temporal domain between 1950 and 2019.

Our goal is to examine how the history of interstate conflicts is associated with the people's attitudes toward the (potential) erstwhile aggressor's vaccine. We treat the entire history of interstate violence of one country against another up to the date of the survey as one realization of historical violence. We do this (as opposed to focusing on more-refined temporal windows) for two reasons. First, this approach to treating the legacy of violence is useful in informing future policies, as opposed to looking at violence that occurred in a particular time period in the past. For future policies, it is of the utmost importance to know vaccine preferences *given* the history of violence. Second, our treatment of violent legacy blackboxes the intricate, dynamic processes of how one incidence of violent conflict begets further violence (Thompson and Dreyer, 2012; Vasquez and Leskiw, 2001), which are not of interest per se for a given upcoming vaccination campaign and would be difficult to identify given our survey data separately.

To aggregate the history of violence by one country against another, we assume that a more distant past matters less and rely on a simple discounted sum of MIDs across the years leading up to 2019. Let $\widetilde{X}_{p,c,t}$ be a count of militarized actions by vaccine-producing country (p) against a potential vaccine-consuming country c (i.e. the respondent's country) in year t. For the main analyses, we discount historical MID counts geometrically with a constant discount factor of 0.990 per year. That essentially means that we count a MID that occurred in 1950 as one-half of a MID in 2019. Formally, a measure of the history of interstate violence by p against c is:

$$X_{p,c} = \sum_{t=1950}^{2019} 0.990^{2020-t} \times \widetilde{X}_{p,c,t}. \quad (1)$$

For our main analyses, $\widetilde{X}_{p,c,t}$ includes either all MIDs involving at least a threat to use force (i.e. the hostility level 2 or higher) or only severe MIDs, in which force was at least used (i.e. the hostility level is 4 or greater), respectively. Figures A.2 and A.3 in SI provide an overview of the historical patterns of severe MIDs in our data as well as of $X_{p,c}$by dyad.

2.3. Statistical model

Our goal is to isolate the effect of historical violence on vaccine attitudes from other factors known to impact vaccine attitudes, such as the global scientific reputation of a vaccine producer. For this reason, we first use random-effect linear regression models in our main analyses. Specifically, the estimation equation is:

$$Y_{i,p} = \alpha + \xi_p + \mu_{c[i]} + D_i\beta + \gamma_1 \mathbb{1}\left(X_{p,c[i]} = 0\right) + \gamma_2 \mathbb{1}\left(X_{p,c[i]} > 0\right) \times log\left(X_{p,c[i]}\right) + \varepsilon_{i,p} \quad (2)$$

where $Y_{i,p}$is the binary variable of whether respondent i chose "more positively" ("more negatively") or not about vaccine from country p; to ease interpretation, we set the choice to 100 and the absence of a choice to zero. α is an global intercept term, and D_i contains demographic covariates (gender, age cohorts, whether one is working full-time, part-time, and is a college graduate).

We model the effect of the history of militarized aggression by first separating the many cases with no history of aggression from those with some $\left(\mathbb{1}\left(X_{p,c[i]} = 0\right)\right)$. $X_{p,c} = 0$ in about 78% (any MIDs). For those cases with at least one MID across the years, we then allow a more aggressive history to have a monotonically declining marginal effect with a logarithmic function $\left(\mathbb{1}\left(X_{p,c[i]} > 0\right) \times log\left(X_{p,c[i]}\right)\right)$.

ξ_p and μ_c are random intercepts for the vaccine producer/potential aggressor p and for the vaccine consumer and potential target c, respectively, both of which play an important role in our model. They capture all between-country variations on either side of the dyad. On the one hand, ξ_p accounts for the global reputation of the vaccine-producing country (p), such as perceptions of the country's pharmaceutical industry and the quality of government oversight and goods produced in the country. On the other hand, μ_c captures the average willingness to take any vaccine in the vaccine-consuming country (c) as well as perceptions of its government competency in rolling out vaccines and religious/cultural/political orientations at the country level that might be correlated with vaccine update. With these random intercepts, our results stem purely from variation across the (directed-)dyads.

Last, the model is estimated via *lme4* (Bates et al., 2015) using a cluster-bootstrap to account for intra-respondent correlations of the errors, $\varepsilon_{i,p}$, since each respondent evaluated 12 vaccine origins (Harden, 2011).

3. Results

3.1. Main analysis

Table 1 gives the results for the full sample, with each column showing the estimates for one combination of response type (viewing

Table 1
Linear probability models; all respondents. Each model gives the summary of the coefficients and the standard errors for one model. For the MID-related logarithmic variable, the share of times for which the estimated t-statistic is larger (and in the hypothesized direction) compared to t-statistic based on data with MID-variables reshuffled by dyad.

	More negatively		More positively	
	Any	Severe	Any	Severe
Any MID history (99%), none	−3.4 [-4.3; -2.7]		2.1 [1.2; 2.9]	
Any MID history (99%), log	4.3 [3.8; 4.9] (1.00)		−1.1 [-1.6; -0.7] (0.87)	
Severe MID history (99%), none		−6.5 [-7.4; -5.6]		3.9 [3.0; 5.0]
Severe MID history (99%), log		3.1 [2.4; 3.9] (0.91)		0.1 [-0.6; 0.9] (0.47)
Gender, male	2.5 [1.6; 3.3]	2.6 [1.8; 3.3]	3.5 [2.6; 4.5]	3.6 [2.7; 4.5]
Age, 25-34	1.0 [-0.4; 2.2]	0.9 [-0.5; 2.1]	−1.8 [-3.6; 0.0]	−1.8 [-3.6; -0.1]
Age, 35-44	0.6 [-0.6; 1.9]	0.5 [-0.9; 1.8]	−1.4 [-3.2; 0.3]	−1.4 [-3.2; 0.4]
Age, 45-54	1.1 [-0.1; 2.4]	1.0 [-0.6; 2.4]	−0.7 [-2.4; 1.0]	−0.7 [-2.5; 1.0]
Age, 55+	3.6 [2.4; 4.7]	3.4 [2.1; 4.7]	0.8 [-0.9; 2.4]	0.9 [-0.7; 2.5]
Education, graduate	1.2 [0.5; 2.0]	1.3 [0.6; 2.1]	3.8 [2.8; 4.9]	3.8 [2.7; 4.8]
Work, full-time	1.5 [0.7; 2.4]	1.5 [0.5; 2.5]	2.0 [1.0; 3.0]	2.0 [1.1; 3.1]
Work, part-time	1.8 [0.6; 2.9]	1.7 [0.5; 3.0]	1.8 [0.3; 3.4]	1.9 [0.5; 3.2]
Intercept	17.1 [10.3; 23.9]	20.7 [13.4; 27.6]	18.4 [10.9; 25.3]	16.5 [9.5; 23.7]
RE SE, origin	5.1	5.2	8.6	8.7
RE SE, target	11.2	11.6	9.9	10.0
Residual SE	37.5	37.5	40.3	40.3

vaccines more negatively, more positively) and type of MID (any, severe). The ranges in brackets give the corresponding 95% confidence intervals. (The numbers underneath the bracketed quantities are discussed later.) Collectively, the results suggest that legacies of historical conflict are indeed associated with vaccine hesitancy.

Consider the coefficient estimate on the indicator of no historical militarized violence (column 2), which is negative and statistically significant ($\beta = -3.4$, 95% CI [−4.3, −2.7]). This means that an absence of violent history is associated with a lower probability of a negative assessment of a vaccine developed in the country of origin. However, the estimated coefficient on the log-transformed history of violence is positive and statistically significant, meaning that an increasing number of any MIDs is associated with a higher probability of a negative assessment of the foreign vaccine ($\beta = 4.3$, 95% CI [3.8, 4.9]). The results for severe MIDs (column 3) show an analogous pattern.

The results for a positive assessment are mixed. Considering any MIDs, we see that increases in any MIDs are associated with a lower probability of a positive assessment ($\beta = -1.1$, 95% CI [−1.6, −0.7]). However, the association with the severe MIDs is statistically insignificant ($\beta = 0.1$, 95% CI [−0.6, 0.9]).

Due to the convenient mathematical function, we can provide a simple illustration of these patterns. For example, compared to a scenario in which the vaccine-producing country only initiated one any MID 40 years ago, just one additional MID of any type ten years ago increases the "more negatively" responses by 3.7 [3.2, 4.2] percentage-points and decreases the "more positively" responses by 0.9 [0.6, 1.4] percentage-points.

As contracting COVID-19 poses particularly strong health risks to older people, examining the results for older survey-takers is important. Correspondingly, Table 2 shows results repeating the analysis with the data subset to those respondents aged 55 and older, dropping the age-cohort indicators from D_i. The same relationships between the two types of MID histories and vaccine preferences hold for people 55 and older when focusing on the negative assessments. Increasing any and severe MIDs lead to a higher probability of more negative views ($\beta = 5.5$, 95% CI [3.9, 7.1]; $\beta = 2.9$, 95% CI [0.6, 5.1]). Any MID types are statistically significantly associated with the positive assessments for people 55 and older ($\beta = -2.1$, 95% CI [−3.4, −0.8]), but severe MIDs are not ($\beta = -0.2$, 95% CI [−2.4, 1.9]). Together, these findings suggest that an increased health benefit from vaccination for older people does not overshadow the negative effect of a militarized legacy on vaccine attitudes.

Table 2
Linear probability models; respondents aged 55 and above. Each model gives the summary of the coefficients and the standard errors for one model. For the MID-related logarithmic variable, the share of times for which the estimated t-statistic is larger (and in the hypothesized direction) compared to t-statistic based on data with MID-variables reshuffled by dyad.

	More negatively		More positively	
	Any	Severe	Any	Severe
Any MID history (99%), none	−2.0 [-4.5; 0.5]		−2.4 [-5.0; 0.2]	
Any MID history (99%), log	5.5 [3.9; 7.1] (1.00)		−2.1 [-3.4; -0.8] (0.97)	
Severe MID history (99%), none		−9.3 [-12.4; -6.3]		2.2 [-0.9; 5.3]
Severe MID history (99%), log		2.9 [0.6; 5.1] (0.91)		−0.2 [-2.4; 1.9] (0.52)
Gender, male	0.6 [-1.8; 2.9]	0.5 [-1.7; 2.8]	5.5 [3.0; 7.9]	5.3 [2.8; 7.9]
Education, graduate	0.5 [-2.0; 2.8]	0.3 [-2.3; 2.9]	4.8 [2.0; 7.5]	4.9 [2.3; 7.5]
Work, full-time	−0.8 [-3.7; 2.1]	−0.7 [-3.4; 2.0]	−1.0 [-4.0; 1.9]	−0.9 [-4.6; 2.7]
Work, part-time	1.7 [-2.4; 5.9]	2.0 [-1.5; 5.7]	1.1 [-2.8; 5.0]	1.0 [-3.6; 5.2]
Intercept	21.1 [11.5; 30.4]	28.7 [18.1; 38.2]	23.3 [13.4; 33.0]	19.0 [8.8; 29.0]
RE SE, origin	7.1	7.5	12.8	12.8
RE SE, target	14.4	14.9	12.0	12.2
Residual SE	37.8	37.8	37.8	37.9

While we modeled each side of the vaccine-producing and vaccine-consuming dyads with random intercepts, the dyadic nature of our data is not fully captured. We risk our results being overconfident due to (artificially) small standard errors. We, therefore, complement the confidence intervals for the key coefficient (γ_2) on the log-positive part of $X_{p,c}$ with results from randomization inference (RI) (Erikson et al., 2014). RI randomly reshuffles $X_{p,c}$ between dyads *en bloc*, maintaining the coherence of the directed-dyadic structures.

Re-estimating the model, we obtain a new t statistic for γ_2 that was created under the designed (null) assumption that there is no relationship between $X_{p,c}$ and the outcome. Repeating this process 1,000 times, we calculate the share of t statistics for γ_2 under observed data that is larger than under the repeatedly reshuffled data for "more negatively" outcomes; for "more positively", we are interested in the share that is smaller. These shares are the numbers in parentheses in Tables 1 and 2 The RI results confirm that the "more negatively" results exceed the null in the 1,000 resampling iterations in a vast share of simulations, 0.99 and 1.00 for any MIDs in the pooled and older samples, respectively, and 0.91 for each of severe MIDs. In contrast, the already slightly noisier results for the positive assessments show an absence of a statistically significant pattern with RI shares between 0.46 and 0.82 in the expected direction. The exception is that results for the older survey-takers and any MIDs remain statistically significant.

3.2. Robustness checks and sensitivity analyses

The main analyses show that histories of violent conflict can encourage vaccine hesitancy. This sub-section probes the robustness of our main findings by examining alternative annual discounting factors, 97.5% and 99.9%, to calculate $X_{p,c}$ and by examining all age-cohorts separately. We also give a more 'realistic' demonstration of our results by comparing actual (observed) histories of violence in our data set.

In Section E, we provide all details and thus will only summarize the analysis and results here. We use 97.5% and 99.9% annual discount factors in addition to the 99.0% from before. For each combination of the outcome variable ("more positively", "negatively") and age-cohort (18–24, 25–34, 35–44, 45–54, 55+), we estimate six models using each of three discounting factors and each of the two types of MIDs (any and severe). For each, we simulate predicted vaccine attitudes based on synthetic observations which hold demographics constant while varying histories of violence. Specifically, we build the synthetic observations by taking 10,000 randomly selected demographic profiles (work, gender, age-cohorts, education) and pairing them with random draws from the three groups of observed histories of MIDs between vaccine-producing and vaccine-consuming countries. The three violence history groups are:

"No violence." All versions $X_{p,c}$ are zero because no militarized dispute has taken place since the 1950s. There are 146 unique such directed-dyads in the data.

"Some violence." We set the MID-related features to the observed values for dyads which had more than zero and less than two 99.0%-discounted severe MIDs since 1950s. There are 17 dyads in the data with this history of violence, including U.S.–Indonesia (first is origin/aggressor), Russia–Italy, Russia–Poland, Iran–India, Iran–-France, Canada–Spain, China–Indonesia, Australia–Indonesia, France–Spain, among others.

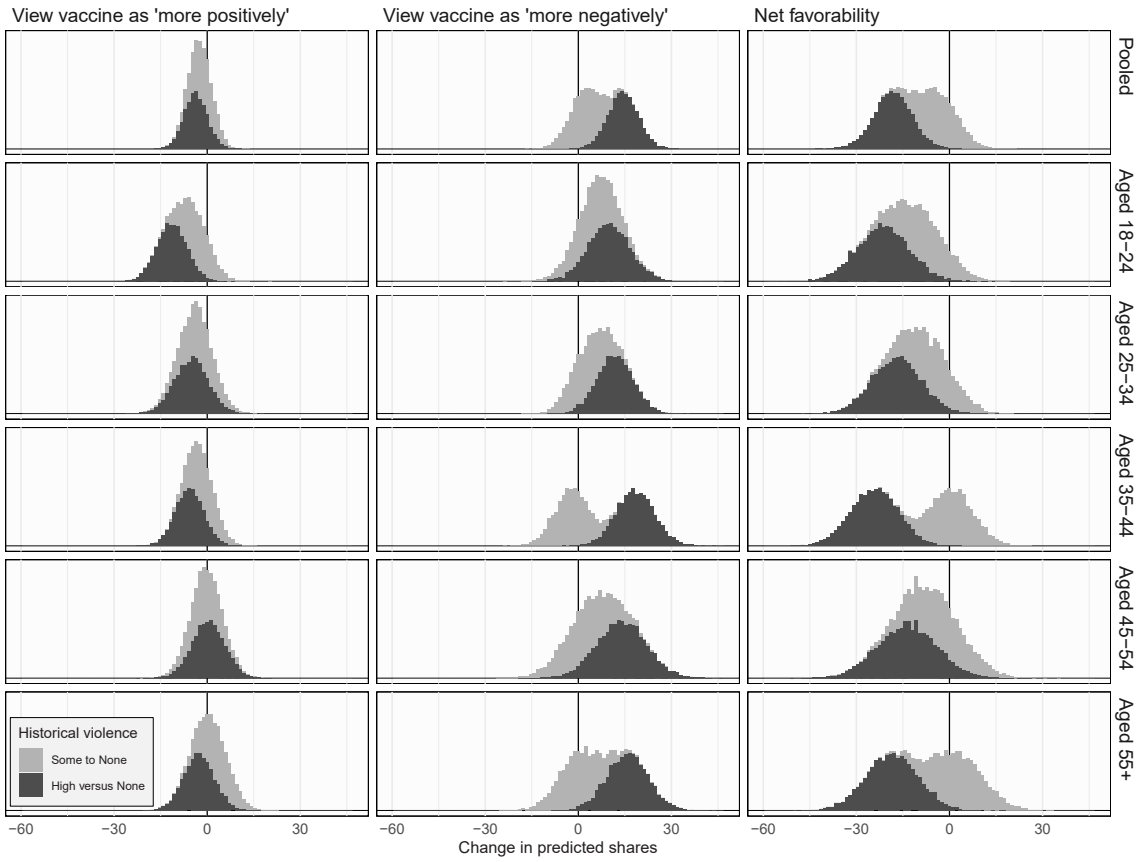

Fig. 1. Changes to shares of people viewing vaccine more negatively and positively as well relatively so by history of violence and age-cohort. Each panel shows the predicted change in when comparing "high" (black) and "some" (gray) violence to "no" violence. The columns of panels give the outcome quantity of interest and the rows the (sub)samples.

"High violence." For the cases with the most violent histories, namely five or more severe MIDs over the years (at 99.0% discount). There are 10 unique dyads, including China–U.K., China–India, Russia–U.S., China–Australia, Iran–U.S., India–China, and Australia–China.

For each outcome and age-cohort sample, predictions are averaged first across the six models using BIC-approximations of Bayesian Model Averaging weights and second across the 10,000 synthetic observations within the each violence grouping. To highlight the changes, we subtract the predictions for "no violence" from the "high" and "some" violence predictions, and then additionally difference them again for a change in net-favorability rating. The undifferenced predictions are shown in Figure A.4 in SI, including the additional outcomes ("don't know", "no difference"); further, SI also shows the estimates prior to applying BMA weights. Parametric bootstrapping allows us to account for sampling uncertainty (King et al., 2000).

Fig. 1 gives the results. We see that for all age-cohort samples, a more violent history leads to increased shares of "more negatively" views (histogram is to the right of the zero line), whereas the "more positively" shares decline or cluster around zero. Combining the more positively/negatively effects leads to systematic and sizable negative shifts in the net-favorability ratings in one country toward a foreign vaccine. Depending on the sample, the changes are about ≈ -22 percentage points for the "high violence" cases. Violence begets increased vaccine hesitancy.

In an additional robustness check, we repeat the subset analysis using gender and educational background as demographic subsets, following a procedure analogous to the age-cohorts. Cross-nationally, previous research has observed demographic asymmetries in vaccine confidence attributable to gender and educational attainment (e.g. De Figueiredo et al., 2020; Hornsey et al., 2018; Larson et al., 2016). The coefficient estimates are shown in tables in Section F, and the substantive simulations in Figures A.4 and A.5 in SI. We find no large differences by education or gender.

4. Discussion

Our work suggests that when people in vaccine-consuming countries are faced with the prospect of receiving COVID-19 vaccines produced by countries with whom their home country has a history of militarized violence, they are more likely to hold negative views toward vaccinating. This is especially true for individuals in vaccine-consuming countries that have severe legacies of violence with potential vaccine producers. In addition to being large in substantive size, these effects hold across a diverse set of alternate modeling strategies, different subsets of data (by age-cohorts, genders, and educational backgrounds), and standard error estimation procedures.

Our research offers at least two important lessons for global public health. First, in addition to contributing to a growing scholarly literature on vaccine uptake "country of origin" effects, our work suggests that health practitioners around the globe ought to seriously account for the possibility of vaccine refusal motivated by legacies of interstate violence. Health officials in national governments, for example, may wish to consider available alternatives (should any exist) when faced with the prospect of creating vaccine-purchasing agreements between

countries with whom they have a history of militarized violence. They might also consider taking action to diversify the national profiles of their vaccine purchasing portfolios.

Additionally, we believe that our research can help health agencies better preempt public vaccine refusal. When faced with the prospect of providing that country's residents with vaccines from an actively or formerly-hostile nation, our work suggests that health officials should anticipate at least some amount of vaccine refusal. Making regular efforts to surveil public awareness of vaccines' national origins—at least in nations that regularly track public vaccine sentiment—might further facilitate these efforts.

Our work, of course, is not without important limitations. First, our outcome variable is a measure of vaccine-related *attitudes* and not behavioral intentions. Negative views toward vaccinating do not necessarily indicate that respondents will forego vaccination. Still, consistent with the theory of planned behavior (Ajzen, 1991; Ajzen and Fishbein, 1977), research documents a strong link between self-reported vaccine attitudes and behavioral intentions (Martin and Petrie, 2017; Xiao and Wong, 2020) as well as a high degree of correspondence between self-reported behavior and actual vaccine update (Brewer et al., 2007; Roberts et al., 2015; Smith et al., 2017). Correspondingly, insights gleaned from this study likely have important implications for vaccine uptake.

Second, and relatedly, one concern about our finding may be about how much people actually know about where vaccines are developed when faced with the decision of actually vaccinating. On the one hand, the lack of people's knowledge of where a vaccine originates does not invalidate our survey results. In the survey, respondents are told where vaccines are from, prior to being asked to provide judgment about them. Consequently, we are "standardizing knowledge," and we view the lack of knowledge as not an issue in the survey. On the other hand, the lack of knowledge may pose a threat to the external validity of our findings. If people generally knew little about country of origin, histories of interstate violence would matter little in the actual use of foreign vaccines. During the COVID-19 pandemic, we know that country of origin (e.g. Russia, U.K., China) was salient in media and public discourses in many countries. But, we also know that country of origin is not very salient in the use of vaccines like influenza vaccines. We think that the applicability of our finding would depend crucially on how salient vaccines and their country of origin are in media and elite discourses in future pandemics.

Third, the data made available to us via YouGov lacks information about the extent to which respondents are aware of prior histories of interstate conflict. Although we document strong and robust associations between historical violence and vaccine attitudes, efforts to collect such data could help to validate our findings further. Measures like that could also serve as useful moderators of the relationship identified in this study—e.g., if individuals aware of violence in a particular context may be especially likely to hold negative views toward COVID-19 vaccines. However, it is worth noting that one study reports an absence of moderation effects related to general feelings toward the vaccine origin (Kobayashi et al., 2021).

Fourth, we recognize that this study provides insights from only a single cross-sectional survey of a select group of countries and conducted at a single point in time about vaccines targeting one disease. We, therefore, cannot rule out the possibility that the patterns documented in this paper might be weaker or stronger in countries not under investigation. While our data cover many different regions, certain important regions like those in Africa, Central Asia, South America, are omitted. This issue is important as some of these omitted regions or countries have more complicated histories with some of the potential vaccine producers, like the United States, Russia, and China. For example, people in countries in Central Asia regularly consume and therefore are very familiar with medicines produced in Russia, which is a direct result of their colonization history. Such high familiarity with Russian products may counteract or amplify the effect of the historical legacy of violence. We are of course not able to address such questions given the data we have, but we see addressing such questions as promising next steps for future research.

We also lack the ability to determine how legacies of violence influence vaccine attitude over time at different points in the COVID-19 pandemic. We, therefore, encourage global public health scholars to continue to consider how legacies of interstate violence might impact survey-based estimates of vaccine hesitancy—both with respect to the COVID-19 pandemic and with respect to other communicable diseases—in studies that vary both national geography and (ideally) have the opportunity to interview the same individuals over time.

Finally, our evidence does not tell us the precise mechanisms by which legacies of violence impact attitudes toward the vaccine developed by aggressors. While our goal in this study has been to document the robust relationships between violent legacies and vaccine attitudes, we believe that a better understanding of the mechanisms behind these relationships would be key to addressing the country-of-origin effect emanating from histories of violent conflicts. We have argued that the key mechanism is through general animosities toward and distrust of the aggressors and potential vaccine producers. This line of argument implies that other factors could counteract the negative impact of legacies of violence. For example, some monadic, aggressor-specific factors (e.g. global reputation in scientific competency or regulations) and dyadic factors (e.g. differences in political/economic systems or cultures, trade relationships between the vaccine consumer and producer) could counteract or even interact with the effect of distrust and historical violence. Indeed, some research suggests that distrust was a serious concern for some vaccine producers like Russia and China and was a key driver in their decisions to move vaccine productions or transfer vaccine technology abroad (Suzuki and Yang). Marketing researchers find evidence that localization strategies, such as joint ventures, a foreign subsidiary with partial local ownership, and co-branding, can be effective in ameliorating distrust of products made by foreign countries (e.g. Fong et al., 2014; Lee et al., 2013). Some evidence gives support for the idea that a locally-manufactured vaccine is more favored even when the technology is developed in a foreign country (Barceló et al., 2022). It would be useful to know more about whether such strategies are effective in reducing distrust of foreign vaccines.

Credit author statement

YK, **CH**, and **TH** were responsible for initial project conceptualization, manuscript write-up, and critical revisions. **TH** handled the statistical analysis. **MM** was responsible for elements of project conceptualization, manuscript write-up, and critical revisions. All authors approved the final version of the manuscript for submission.

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Declaration of competing interest

The authors have no conflicts of interests to declare.

Data availability

Data will be made available on Heinrich's personal website.

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Appendix A. Supplementary data

Supplementary data to this article can be found online at https://doi.org/10.1016/j.socscimed.2022.115346.

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