Learning About Diversity in Healthcare Institutions Reduces Prejudice: Evidence from the COVID-19 Crisis in Israel and the U.S.

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Abstract

Can information about ethnic diversity in public institutions shape intergroup relations? To answer this question, I develop a theory of prejudice reduction through institutional diversity. I suggest that learning about the presence of minorities in public institutions can reduce prejudice by providing majority group members with novel information regarding the out-group, and their role in society. To test my theory, I implemented a survey experiment in Israel, further replicated in the U.S., during the first outburst of the COVID-19 pandemic. In the experiment, treated respondents were informed about the share of minorities (Arab/Muslim) employed in healthcare institutions. Results from Israel suggest that information about minority representation in healthcare institutions reduces prejudice and promotes preferences for political inclusion in a similar magnitude to about a one-unit leftward-shift on a seven-point ideology scale. Similar, albeit more moderate patterns emerge from the U.S. These findings emphasize how institutions and the people embedded within them can shape intergroup relations.

Introduction

Does information regarding ethnic diversity and minority representation in public institutions shape majority group members' prejudicial attitudes?[1] Existing evidence suggests that diversity and representation in public institutions such as police forces, schools, and hospitals improves public goods provision, satisfaction, trust, and cooperation amongst minorities (Meier 1975; Riccucci, Van Ryzin and Lavena 2014; Kruk et al. 2017; Nanes 2018; Hill, Jones and Woodworth 2018; Alsan, Garrick and Graziani 2019; Greenwood et al. 2020; Ba et al. 2021). Yet, less is known about majority group members' responses to diversity in public institutions. Existing research suggests that diversity can facilitate intergroup contact between minority service providers and majority group members, and that such contact in turn reduces prejudice (Weiss 2021). However, it remains unclear whether even absent intergroup contact, mere information about the role of minorities in public institutions can shape mass intergroup attitudes.

To address this gap, I develop a theory of prejudice reduction through institutional representation. Building on theoretical frameworks in social and political psychology (Wittenbrink, Judd and Park 2001; Blair 2002; Ramasubramanian 2007; Williamson 2019; Lai et al. 2014; De Houwer, Thomas and Baeyens 2001; Gaertner et al. 2000), I suggest that information regarding diversity and minority representation in public institutions impacts majority group members perspectives regarding minorities and their role in society. In turn, this information can reduce majority group members' prejudice towards minorities. Therefore, I argue that beyond previously identified virtues of institutional diversity and representation for minority group members (Meier 1975; Nanes 2018; Ba et al. 2021), minority representation in public institutions can reduce majority group members' prejudice and facilitate more favorable intergroup relations.

To test my theory, I follow a broad community of social scientists focusing on healthcare provision (Cammett and Issar 2010; Chen and Cammett 2012; Cammett, Lynch and Bilev 2015; Kruk

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[1]Throughout this paper I adapt a colloquial understanding of institutions as *non-elected organizations that govern, educate, provide for, or organize citizens* (Tankard and Paluck 2017). In addition, I use the terms diversity and representation, to mean the presence of minorities in the ranks of public institutions (Meier 1975).

et al. 2017; Hill, Jones and Woodworth 2018; Michener 2018; Alsan, Garrick and Graziani 2019; Arriola and Grossman 2021; Weiss 2021). Doing so, I consider how information about minority representation in healthcare institutions–a central arena of public goods provision, which in OECD countries accounts for 15% of government expenditures (OECD 2019)–affects intergroup prejudice and preferences for political inclusion. Specifically, during the initial outbreak of COVID-19, I implemented a nationally representative survey experiment in Israel, which I further replicated in the U.S. In the Israeli (U.S) experiment, I exposed Jewish (non-Muslim) respondents to information regarding the share of Arab (Muslim-American) healthcare workers in public institutions responding to the COVID-19 crisis.

My evidence suggests that information about the share of Arabs (Muslim-Americans) in healthcare institutions, reduces prejudice, and in the Israeli case promotes preferences for political inclusion. The effects of information regarding minority representation, are similar in magnitude to well-powered interventions of prejudice reduction recently considered in a comprehensive meta-analysis of experimental interventions (Paluck et al. 2020). Indeed, despite the subtlety of information regarding minority representation in healthcare institutions, effect sizes are substantively significant, and in the Israeli case are equivalent to the effects of approximately a one-unit leftward-shift on a seven-point ideology scale. More so, it appears that effects are largely similar for both right- and left-wing (Republican and Democrat) respondents, and that information about minority representation does not result in attitudinal backlash amongst conservative subjects. This latter finding is important given the well documented degrees of partisan polarization in Israel (Bassan-Nygate and Weiss 2020, Forthcoming), and the U.S. (Iyengar et al. 2019).

I make two contributions to the literatures on public goods provision and prejudice reduction. First, I contribute to the prejudice reduction literature by following recent calls to consider structural and institutional perspectives on prejudice reduction (Paluck et al. 2020). Doing so, I demonstrate that information regarding the identity of agents operating within public institutions affects mass-attitudes. My theoretical approach diverges from recent bottom-up interventions for prejudice reduction (Ditlmann, Samii and Zeitzoff 2017; Scacco and Warren 2018; Mousa 2020),

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and emphasizes the ability of public non-elected institutions such as hospitals and clinics to influence intergroup relations from above, by providing novel information regarding diversity and minority representation.

Second, I extend recent findings which suggest that diversity and minority representation have beneficial effects on minority outcomes (Alsan, Garrick and Graziani 2019; Nanes 2018; Riccucci, Van Ryzin and Lavena 2014; Hill, Jones and Woodworth 2018; Greenwood et al. 2020). In turn, I demonstrate how mere information about institutional diversity reduces prejudice and promotes inclusivity amongst majority group members. It follows that diversifying institutions and facilitating minority descriptive representation can have multiple beneficial effects for minority (Alsan, Garrick and Graziani 2019; Nanes 2018), as well as majority group members. This insight regarding the positive externalities of minority representation for intergroup relations is in line with the literature which explores the externalities of electoral representation on intergroup attitudes (Beaman et al. 2009; Chauchard 2014, 2017). However, my focus on diversity in non-elected public institutions is notable given the far-reaching arms of such institutions, their ongoing direct engagement with the general public, and their ability to facilitate minority representation in many different positions and levels of power (Pepinsky, Pierskalla and Sacks 2017).

Diversity in Public Institutions

Dating back to Kingsley's early explorations of the British bureaucracy (1944), social scientists have emphasized the importance of minority representation in non-elected institutions. Indeed, theoretical frameworks in public policy suggest that diverse institutions, are better suited to provide public goods. The positive effects of minority representation in public institutions are theorized to be driven by three central mechanisms relating to: the behavior of minority civil servants, the responses of non-minority civil servants to the presence of minorities in their institutions, and the symbolic effect that information regarding minority civil servants has on minority clients (Mosher 1968; Rosenbloom and Featherstonhaugh 1977; Riccucci, Van Ryzin and Li 2016).

In line with these theoretical frameworks, scholars have linked diversity and minority representation in public institutions with improved policing (Nanes 2018; Ba et al. 2021), enhanced

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education (Keiser et al. 2002), beneficial health outcomes (Kruk et al. 2017; Hill, Jones and Woodworth 2018; Alsan, Garrick and Graziani 2019), organizational efficiency (Rasul and Rogger 2015; Fernandez, Koma and Lee 2018), reduced bias in voter registration processes (Neggers 2018), and more lenient court decisions (Grossman et al. 2016). More so, scholars have demonstrated that even when lacking action from a civil servant, information about the mere presence of an underrepresented group within an institution can foster trust, perceived legitimacy, and willingness to coproduce amongst citizens (Karim 2019; Theobald and Haider-Markel 2008; Riccucci, Van Ryzin and Lavena 2014; Riccucci, Van Ryzin and Li 2016).

Theory: How Learning about Institutional Diversity Reduces Prejudice

The existing literature has made important advances in identifying the effects of diversity in public institutions on public goods provision. However, far less attention has been directed to examining the externalities of diversity for intergroup relations. Recent evidence suggests that diversity in healthcare institutions can facilitate intergroup contact and reduce prejudice (Weiss 2021). However, it remains unclear whether in the absence of intergroup contact, information about the role of minorities in public institutions can shape mass intergroup attitudes.

This limited attention devoted to the links between institutional diversity and prejudice is rather surprising, since public non-elected institutions and the agents within them interact frequently and intimately with citizens (Karim 2020; Pepinsky, Pierskalla and Sacks 2017; Lipsky 1980). More so, civil servants are recruited by the state, often times based at least in part on their competence and qualifications (Bhavnani and Lee 2018, Forthcoming). To that extent, one may expect that information regarding the demographic make-up of meritocratic public institutions which impact citizens' daily lives may affect intergroup relations.

Studies linking electoral representation with citizens' prejudice and bias provide encouraging evidence to bolster my expectation that diversity in non-elected public institutions can reduce prejudice. Indeed, electoral representation of minorities has been theorized to promote tolerance by providing information, positioning minorities in a visible position of power, changing minorities' role within their social network, or facilitating intergroup contact between minority elected offi-

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cials and constituents (Hajnal 2001; Chauchard 2014, 2017). Building on this evidence, one may expect that information regarding minority representation in the ranks of schools, hospitals, police stations, and other institutions providing public goods, will promote tolerance amongst majority group members.

A host of theoretical frameworks in social and political psychology bolster this expectation. First, prejudice is often thought to be driven by limited information (Allport 1954), as well as negative stereotypes about minority group members (Ramasubramanian 2007; Ramasubramanian and Oliver 2007; Burns, Monteith and Parker 2017). Therefore, information regarding minority representation in public institutions, can increase majority group members' familiarity with, and knowledge about minorities. Such familiarity and knowledge can reasonably be expected to generate more tolerant attitudes. Indeed, information regarding minority representation may be especially powerful if it counters prevailing stereotypes (Ramasubramanian 2007; Ramasubramanian and Oliver 2007; Lai et al. 2014), or corrects misperceptions about out-groups (Williamson 2019).

Second, in line with psychological theories of associative conditioning (De Houwer, Thomas and Baeyens 2001), existing research suggests that the social roles in which individuals are embedded, impact how they are evaluated. According to such frameworks, prejudice towards an outgroup can be shaped by the context in which an outgroup is presented. In other words, contextual features such as an individual's occupation are expected to condition the extent to which they elicit positive or negative reactions from an out-group member (Wittenbrink, Judd and Park 2001; Blair 2002; Barden et al. 2004). To the extent that information regarding diversity and representation of minorities in public institution reinforces perceptions of group variability, and provides positive information about minority qualities (Wolsko et al. 2003), such representation could be expected to reduce prejudice towards the minority group as a whole.

Lastly, information about diversity in public institutions emphasizes that minorities are an integral part of society. In turn, such information may serve to create superordinate identities that serve to bridge, or obviate, the minority-majority cleavage, and reduce prejudice and polarization

([Gaertner et al. 2000](); [Brewer 2000](); [Bassan-Nygate and Weiss Forthcoming]()). In other words, once observing that minorities from group M in country C are an integral part of a reputable institution, members of the majority group may shift how they refer to minorities from group M, and focus on the identity which both groups share – identity C. In turn, the reorientation to focus on a shared identity (C, rather than M), can reduce prejudice and lead to more favorable intergroup relations.

Building on these insights, I suggest that information regarding diversity and minority representation in public institutions–especially when minorities are socially and politically excluded, and institutions are held in high-esteem–will reduce prejudice. Thus learning that minority group members are an integral part of a school, hospital, or appellate court, will motivate majority group members to update their beliefs, and adapt a more complex, or even favorable perspective regarding the qualities of the out-group.

Empirical Approach

Identifying how information regarding diversity and minority institutional representation shapes mass-prejudice is challenging, since individual- or community-level prejudice may be a cause, rather than an effect of diversity, and exposure to information about it. To sidestep this challenge and test my theoretical expectation, I adapt an experimental approach. Specifically, I focus on the attitudinal effects of information regarding diversity and minority representation in healthcare institutions, and consider whether learning about the presence of minorities in public institutions can reduce prejudice and promote more tolerant attitudes.

As noted above, minority representation in public institutions may affect attitudes through a host of mechanisms ([Chauchard 2017](), [2014]()). However, since my theoretical framework focuses on information as a central mechanism tying institutional diversity with prejudice reduction, I adapt a survey-experimental approach. This approach allows me to simply compare prejudice rates amongst respondents who are informed or not informed about the significant share of minorities within public institutions. My decision to construct a control condition which does not mention any information about minority representation, and avoid a deceptive control condition which mentions lower or no rates of representation, is driven by theoretical as well as ethical consideration.

Ethically, providing respondents with true information regarding representation as my treatment, allows me to avoid deception and provision of false information in my experimental design (For an elaborate description of the ethics of my studies, see Section A in the supplementary materials). Theoretically, with this design choice, I focus on my main theoretical construct of interest – true information regarding diversity and the share of minorities in healthcare institutions. Though the Israeli government provides aggregate statistics regarding minority employment in state institutions (Yaron 2020), for many respondents this information is novel. Consequentially, results from my studies can shed light on the general effects of information regarding minority representation, rather than the marginal effects of information regarding different levels of representation. That said, when comparing my experimental results from Israel and the U.S., which have varying levels of minority representation, I point to suggestive evidence regarding the possible impact of diverging levels of institutional representation on prejudice.

Institutional Focus: Healthcare Provision

My theory regarding institutional diversity and prejudice reduction is inspired by recent calls to consider structural and institutional approaches for improving intergroup relations (Paluck et al. 2020). Turing to non-elected institutions, and considering how their ethnic diversity shapes mass attitudes is sensible since "*Most citizens encounter government... not through letters to congressmen... but through their teachers... and through the policeman on the corner...*" (Lipsky 1980, p. 4). It follows that non-elected institutions, such as schools, hospitals and welfare agencies, play a central role in many citizens' lives (Pepinsky, Pierskalla and Sacks 2017), and have the potential to shape citizens' attitudes and preferences in important ways (Paglayan 2020).

My empirical focus on healthcare institutions, is motivated by four reasons. First, healthcare is a central area of public goods provision, which has received much attention by scholars of political science, public policy, and economics (Cammett and Issar 2010; Chen and Cammett 2012; Cammett, Lynch and Bilev 2015; Kruk et al. 2017; Hill, Jones and Woodworth 2018; Michener 2018; Alsan, Garrick and Graziani 2019; Weiss 2021; Arriola and Grossman 2021). Indeed, healthcare provision has been widely explored in the literature on institutional diversity and representation,

and there is evidence to suggest that diversity improves health outcomes for minority clients (Hill, Jones and Woodworth 2018; Alsan, Garrick and Graziani 2019; Greenwood et al. 2020). Thus, it is important to understand whether institutional diversity and minority representation has further societal benefits for intergroup relations.

Second, medical care is a near universal experience. Indeed, most citizens engage with healthcare institutions regardless of their age, partisanship, or ideological preferences. Third, healthcare institutions are a leading sector with regards to minority representation in Israel (Rosner 2016), and in other countries as well (Patel et al. 2018). Indeed, in the Israeli context, given stark patterns of segregation (Enos and Gidron 2018), hospitals and medical clinics serve as a central hub where Jews and Arabs interact.[2] More so, policy reports suggest that healthcare institutions serve to bridge gaps between minority and majority group members (Rosner 2016).

Lastly, since the outbreak of the COVID-19 pandemic, healthcare institutions and their workers have gained much salience amongst the general public. Indeed, health experts appear regularly on national TV. More so, doctors and nurses are at the spearhead of state responses to local and global health challenges. As demonstrated in a snapshot from a viral information campaign in Israel (Figure 1),[3] the role of Arabs in combatting COVID-19 has been a salient issue for the Israeli public. Similar campaigns emphasizing the role of immigrants in healthcare institutions were developed in the U.S.,[4] and in the UK prime-minister Boris Johnson gave a dramatic speech alluding to the role of minority-immigrant healthcare workers in saving his life (Booth and Adam 2020).

In the Israeli context, progressive social movements and public figures have explicitly linked diversity and Arab representation in healthcare provision with a call for more equitable intergroup

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[2]Hospitals in Israel employ a large share of Arab professionals, in relation to other government institutions. More specifically, the share of Arabs employed in the Israeli healthcare system is double that of their general employment rate in the Israeli labor market (Rosner 2016).

[3]This campaign was produced by an Israeli organization named "Have you seen the Horizon Lately?", and can be viewed through the following link: https://www.facebook.com/watch/?v=292639341723781, last checked on July 26, 2020.

[4]See https://twitter.com/AmericasVoice/status/1260332332701102080, last checked on June 26, 2020.

Figure 1: **Arab Healthcare Workers Informational Campaign** - This is a screenshot from a social media campaign in Israel with over 2,000,000 views. The campaign was titled in Hebrew and Arabic "Partners in Fate, Partners in Governance", and it depicts Arab doctors calling for social change and political inclusion.

relations and political arrangements (Hendrix 2020). However one may wonder how Jewish Israelis, and more generally majority group members, react to information regarding diversity and minority representation in healthcare provision, and whether information regarding the share of minority service providers in national healthcare institutions can reduce prejudice.

Study I: The Effects of Arab Healthcare Workers on Jewish Prejudice

To test my theory of prejudice reduction through institutional diversity, I implemented a survey experiment during the peak of the first COVID-19 outbreak in Israel (see Figure 2 for precise timing). The main goal of the experiment was to determine whether providing respondents with information regarding the share of Arabs working in Israeli hospitals shapes prejudice and preferences for political inclusion. In some ways, the timing of my experiment is rather favorable for detecting effects, given the centrality of healthcare professionals in combating the threat of COVID-19, and the positive valence associated with healthcare institutions at the time. However, other attributes of the COVID-19 pandemic warrant the timing of my experiment as a challenging instance to test

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a theory of prejudice reduction. This is since pandemics such as COVID-19 have been shown to increase hostility towards people from marginalized group (Dionne and Turkmen 2020).
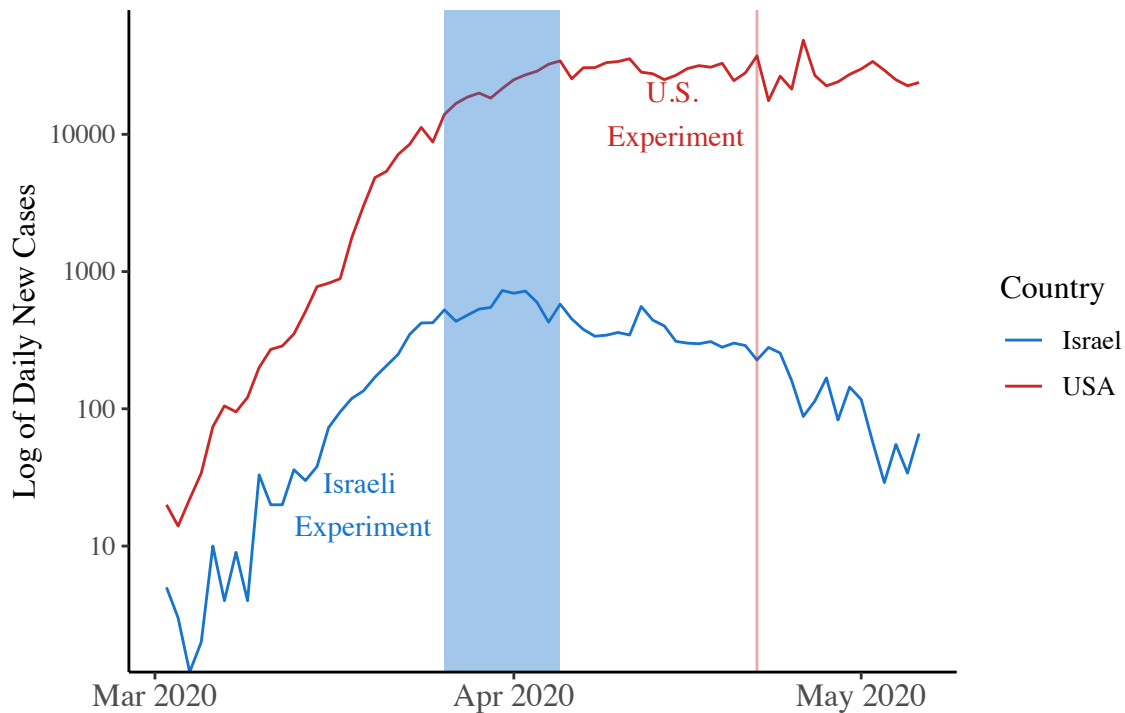
Figure 2: **Timeline** - This figure presents the log of daily new cases in Israel and in the U.S. Shaded regions represent the timing in which I implemented my original survey experiment in Israel (blue), and replication in the U.S. (red).

Prejudice towards the Arab Minorities in Israel

Due to its ethnic and religious diversity, Israel is often considered as a unique laboratory for social scientists seeking to better understand intergroup relations (Canetti-Nisim, Ariely and Halperin 2008; Porat, Halperin and Bar-Tal 2015; Halperin, Porat and Wohl 2013; Halperin et al. 2013). Indeed, Israel is entangled in a prolonged intractable conflict with the Palestinians (Bar-Tal 1998; Zeitzoff 2014, 2018). This conflict complicates intergroup relations between Jews and Arab citizens of Israel, which represent almost 20% of the Israeli population.

Arab citizens are a marginalized faction of Israeli society. This manifests itself in their low socio-economic standings, and limited participation in many segments of the labor market (Enos and Gidron 2018). The Inequality and marginalization of Arabs contribute to the widely docu-

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mented prejudice that many Jews hold towards Arab citizens in Israel. This prejudice translates into exclusionary behavior, in-group bias, threat perceptions, and discrimination (Enos and Gidron 2018; Shayo and Zussman 2011; Grossman et al. 2016; Smooha 2004; Zeitzoff 2014, 2018). The stark patterns of prejudice towards Arabs, combined with the central role that Arabs play in Israeli healthcare institutions, warrant Israel as a suitable site to test my theory of prejudice reduction through institutional representation.

Experimental Design

To implement my experiment I recruited a sample of 1,366 Jewish Israelis from IPanel – Israel's largest opt-in survey platform. I report descriptive statistics of my sample in Table S1 in the supplementary materials. In section A of the supplementary materials I discuss ethical issues relating to subject compensation and IRB approval.[5] As part of my experiment, I presented survey respondents with a brief vignette describing the initial outbreak of the COVID-19 pandemic in Israel. As depicted in Figure 3, my vignette included background information about the emerging pandemic, as well as three experimental treatments resulting in a fully-crossed 2*x*2*x*2 design.

My main treatment informed survey respondents that in Israel – 27% of doctors, 24% of nurses, and 47% of pharmacy workers are Arab citizens and residents of Israel (see blue text in Figure 3). This information is based on official statistics reported in Haaretz (a leading Israeli newspaper) during the time of my intervention (Yaron 2020). In addition, I experimentally embedded information regarding Knesset members (MKs) support for political exclusion (see red text in Figure 3), as well as information regarding the severity of the COVID-19 crisis (see green text in Figure 3), in order to determine the extent to which such factors (i.e. elite political exclusion and crisis severity), moderate the effects of information regarding institutional diversity on prejudice. In doing so, I follow recent experimental studies of prejudice reduction (Broockman and Kalla 2016; Williamson 2019; Kalla and Broockman 2020), and consider the possibility that competitive information envi-

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[5]IPanel provides researches access to samples of Israeli internet users which are representative of the population in terms of gender, age, religiosity, education, and geographical area. More so, IPanel is commonly used for social scientific research (e.g. Grossman, Manekin and Miodownik 2015; Yarhi-Milo, Kertzer and Renshon 2018; Bassan-Nygate and Weiss Forthcoming).

ronments might dampen (or reinforce) my main effects. As noted above, my experimental vignette was intentionally designed to avoid any form of deception.

After reading the experimental vignette, respondents where presented with several batteries of political and social questions, including five main outcomes of interest, measuring: i) intergroup affect, ii) preferences for social exclusion, iii) intergroup trust, iv) attitudes about intergroup peace, and v) preferences for political inclusion. These outcomes have been used in recent research on intergroup relations in Israel (Samooha 2013; Enos and Gidron 2018). More so, my measure of preferences for social exclusion (often referred to as a social distance scale), has been shown to be a potent determinant of Jewish respondents' discriminatory behaviors towards Arabs (Enos and Gidron 2018).

Recent methodological examinations of attitudinal responses to survey items and experiments suggest that social desirability and demand effects are unlikely to bias my main estimates. Indeed, comparisons of direct and indirect attitudinal measures of prejudice show that the extent to which survey respondents censor their self-reported prejudicial attitudes is limited (Blair, Coppock and Moor Forthcoming). I expect this pattern of limited self-censorship to be especially true in an anonymous survey implemented in Israel where survey respondents' remain un-identified to the researcher, and prejudicial statements by politicians and public figures across the political map are rather common (Zeitoun and Brot 2020; Jaffe-Hoffman 2020). In addition, I expect demand effects to be of limited concern in this case. Indeed, recent explorations of demand effects in survey experiments suggest that even incentivizing respondents' to confirm researchers' explicitly stated hypotheses, does not produce demand effects in survey experiments (Mummolo and Peterson 2019).

However, in designing my study I took two precautions which further mitigate concerns around social desirability and demand effects. First, I adapted a light-touch treatment which provides two very brief objective sentences regarding diversity and Arab representation in the middle of my experimental vignette. Second, I embedded questions about prejudice towards Arabs within more general batteries regarding attitudes towards multiple social groups in Israel (i.e. right- and left-

Please read the following paragraphs carefully:

In the past weeks the Coronavirus reached most countries around the world and created both health and economic crises. Officials in the Ministry of Health consider the virus to be a serious crisis which will have unprecedented adverse effects on public health and the economy in Israel. They expect the crisis to have real negative consequences on Israeli public health, and for that reason it is very important that Israeli healthcare systems prepare to deal with the consequences of the virus which does not have any treatment yet.

According to the Ministry of Health, the virus's main symptoms are:

- Fever
- Cough
- Sore throat
- Respiratory issues

According to official statistics from the Ministry of Health and the Central Bureau of Statistics, there are tens of thousands of Israeli citizens that work in health care system. 20.8% of healthcare workers in Israel are Arab. Specifically, 27% of doctors, 24% of nurses, and 47% of pharmacy workers are Israeli-Arab citizens.

In recent days, many MKs expressed their gratitude to all healthcare workers that are working around the clock in order to provide medical care for all Israeli citizens, and stated that they are eager to serve the public through intense work in the Knesset's committees. In addition, some MKs expressed their support in the political developments leading towards an emergency government / Jewish-Zionist emergency government which does not include the Joint Arab List. These MKs stated that it is not suitable to have members of the Joint Arab List leading the country.

Figure 3: **Experimental Vignette:** 2x2x2 Design. My main treatment regarding Arab representation in healthcare institutions is depicted in blue. My additional severity and exclusion treatments are depicted in green and red successively.

wing partisans). I present the wording of questions employed as my main outcomes in Table 1, and provide an elaborate description of my full survey in Section B.1 of my supplementary materials.

Estimation Strategy

Random assignment of respondents to treatment conditions allows for a simple estimation strategy, since in expectation treatment and control groups should be well balanced on all observable and unobservable social and demographic covariates.[6] For this reason, in my main analyses I adapt a pre-registered bivariate OLS model (see Equation 1), in which each prejudice related outcome, is regressed over my main treatment (Arab representation). However, due to a small (albeit statistically significant) gender difference between respondents assigned to my main treatment and control conditions, in section S1 of the appendix I provide additional results controlling for pre-treatment covariates, as well as additional treatment arms. Doing so, does not impact my findings. Throughout the following section, I report estimates of β_i from Equation 1, which represent the average treatment effect of my diversity informational treatment on five measures of intergroup relations.

$$y_i = \alpha + \beta_i + \varepsilon_i \tag{1}$$

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[6]In Table S3 of the appendix, I present balance tests for a set of central covariates (age, gender, education, ethnicity, and ideology).

Table 1: **Outcome Measures for Israel and U.S. Experiments**

Item	Question Israeli Study	Question U.S Study	Possible Responses
Feeling Thermometer	Please place the following social groups on a feeling thermometer (Left Wing Supporters / Right Wing Supporters / Arabs)	Please place the following social groups on a feeling thermometer (Democrats / Republicans / Hispanics / Muslims / African Americans / Asians)	1-100 Scale
Social Distance	What is the closest level of proximity which you would accept with (Left Wing Supporters / Right Wing Supporters / Arabs)	What is the closest level of proximity which you would accept with (Democrats / Republicans / Hispanics / Muslims / African Americans / Asians)	1. Not accept in my country 2. Accept as guest in my country 3. Accept as citizen in my country 4. Accept as co-worker 5. Accept as neighbor 6. Accept as close friend 7. Accept as family through marriage
Peace	Do you agree with the following statement? "Most Arabs want to live in peace"	Do you agree with the following statement? "Most Muslims in the U.S. want to live in peace"	1:7 Disagree - Agree Scale
Trust	Do you agree with the following statement? "Most Arabs in Israel can be trusted"	Do you agree with the following statement? "Most Muslims in the U.S. can be trusted"	1:7 Disagree - Agree Scale
Political Inclusion	Do you agree with the following statement? "Arab MKs should lead Knesset committees"	Do you agree with the following statement? "Muslim elected officials should be leading congressional committees"	1:7 Disagree - Agree Scale

Results

In Figure 4, I report the average treatment effects of information regarding institutional representation of Arabs in Israeli healthcare institutions on five different attitudinal measures. Positive point estimates resemble a move towards more accepting and inclusionary attitudes. For ease of interpretation I standardize all outcomes ($\mu = 0$ and $\sigma^2 = 1$). In line with my pre-registered theoretical expectation, Jewish Israelis who learn about the role of Arabs in Israeli healthcare institutions report more favorable attitudes. Specifically, effect sizes range between almost a quarter and a fifteenth of a standard deviation – an effect size similar to average effects of well-powered interventions recently considered in a meta-analysis of experimental studies of prejudice reduction (Paluck et al. 2020).
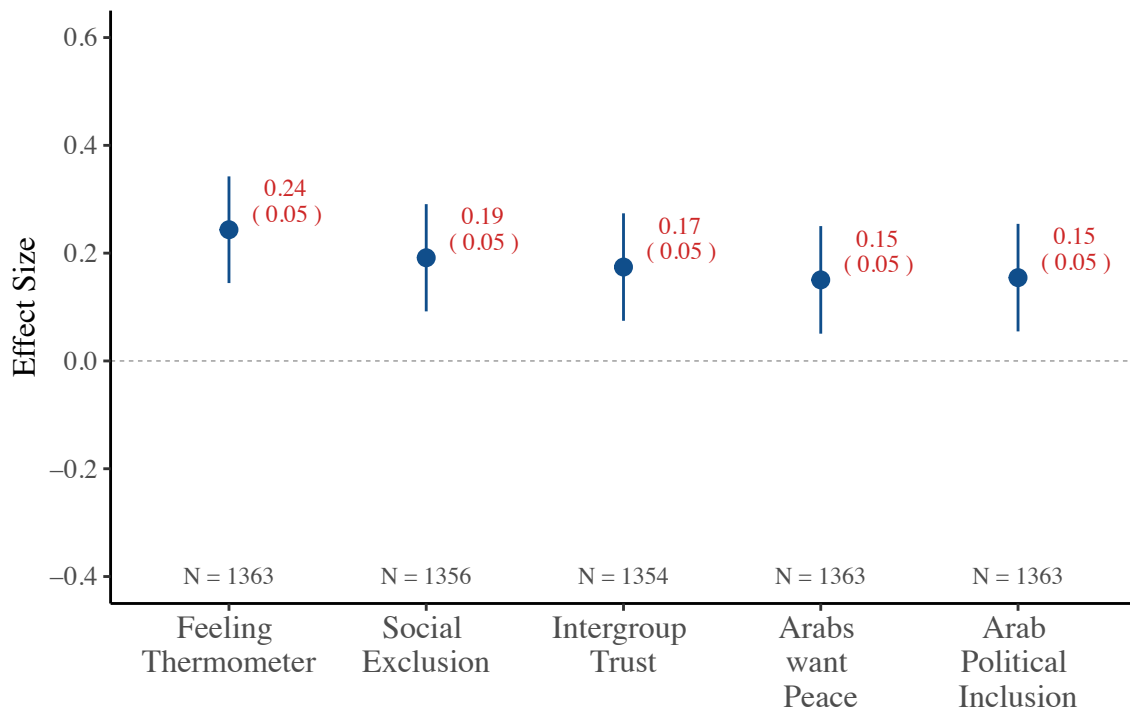
Figure 4: **Information about Arab healthcare workers reduces prejudice amongst Jewish Israelis** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding representation on prejudice attitudes and preferences for political exclusion. Point estimates and standard errors (in parentheses), are depicted in red, and observations for each model are reported in gray at the bottom region of the plot.

Substantively, the average treatment effect of information regarding diversity and Arab representation on Jewish respondents affect towards Arabs (measured through a feeling thermometer) is equivalent to the impact of close to a one unit leftward shift on a seven-point ideology scale. Effect magnitudes for all other outcomes are slightly smaller, but are larger than the impact of a half-unit leftward shift on a seven-point ideology scale. These effects are substantively significant, as ideology is a variable which accounts for a quarter of the variation in preferences for Arab social exclusion.[7] More generally, these effects are remarkable given the subtle nature of my informational treatment, the deeply rooted nature of prejudice amongst many Israelis (Bar-Tal 1998), the hostile environment towards Arab inclusion during my intervention, and the strong links between symbolic prejudicial attitudes and discriminatory behaviors (Enos and Gidron 2018).

Most notable from Figure 4 is that the effects of information regarding Arab representation in Israeli healthcare institutions, shapes not only symbolic prejudicial attitudes, but also political preferences. Indeed, respondents in the treatment condition are more accepting of the idea that Knesset committees will be led by Arab MKs from the Joint Arab List. This is notable given the delegitimization of Arab politicians by mainstream center-left and right-wing politicians, who have referred to Arab MKs in the past as terrorists in suits, or other derogatory names (Zeitoun and Brot 2020; Jaffe-Hoffman 2020).

Does Ideology Moderate Identified Effects?

The evidence above suggests that providing Israelis with information about the share of Arabs working in healthcare institutions shapes prejudice and preferences for political inclusion. However, one may wonder whether these effects are moderated by respondents partisan ideology – a variable which accounts for more than a quarter of the variation in preferences for Arab social exclusion. Put differently, given the strong link between respondents' partisan ideology and prejudicial outcomes, one may expect that identified effects are largely driven by positive reactions from less-prejudiced left-wingers, and that right-wing respondents react differently, and perhaps in

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[7]When regressing my social exclusion measure over responses to a seven-point ideology scale, the regression $R^2 = 0.27$.

a hostile fashion, to information regarding diversity and Arab representation in Israeli healthcare institutions. Such an expectation is quite plausible given recent evidence regarding partisan polarization in Israel (Tsfati and Nir 2017; Bassan-Nygate and Weiss 2020, Forthcoming). Alternatively, it is possible that information about representation is only effective amongst those holding very exclusionary attitudes (Williamson 2019), whereas more tolerant individuals may not be sensitive to information regarding representation due to ceiling effects.

I consider the possibility of heterogenous treatment effects conditional on partisan ideology in Table 2, by interacting my main treatment with a 7-point ideology scale. Since ideology correlates with a host of individual level characteristics, I further control for a set of pre-treatment covariates, including: age, gender, ethnicity, religiosity, locality of origin, and education. To increase efficiency I further account for my two other treatment arms relating to the severity of the COVID-19 crisis, and politicians' support for political exclusion.

Across all outcomes, I do not find support for any moderating effect of ideology on my treatment. Indeed, the point estimates of my interaction effect (`Representation*Ideology`) are both substantively small, and statistically insignificant. In Tables S7-S10 of the appendix, I demonstrate that this result holds across several other specification, including a fully saturated model, and models which focus on "strong" partisans.

These null findings suggest that both right- and left-wing respondents react similarly to information regarding diversity in healthcare provision. More generally, this result is insightful and surprising in light of recent research which documents heightened levels of partisan polarization in Israel (Bassan-Nygate and Weiss 2020). Despite such polarization – both left- and right-wing supporters report more tolerant attitudes when informed about the share of Arabs represented in Israeli healthcare institutions.

Additional Analyses and Robustness Checks

In the appendix, I further consider the moderating effects of my two additional treatment arms relating to the severity of the COVID-19 pandemic, and Israeli politicians exclusionary statements. My pre-registered expectation was that respondents' who are exposed to information that empha-

Table 2: Heterogenous Treatment Effect of Arab Representation Conditional on Ideology

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.181	0.110	0.170	0.183	0.056
	(0.093)	(0.090)	(0.092)	(0.093)	(0.082)
Ideology	0.244	0.255	0.288	0.274	0.317
	(0.020)	(0.020)	(0.020)	(0.020)	(0.018)
Representation*Ideology	0.017	0.031	0.001	−0.011	0.032
	(0.027)	(0.026)	(0.027)	(0.027)	(0.024)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.

sizes the severity of the COVID-19 crises would report higher degrees of prejudice reduction. This is since I expected the severity of the crises to emphasize the importance and contribution of minority healthcare workers. In contrast, building on recent findings regarding the dampening effects of competing informational environments (Williamson 2019), I expected exclusionary statements to attenuate prejudice reduction, as such statements reinforce institutional legitimacy for intolerant attitudes. Nonetheless, as depicted in Tables S4-S5 in the appendix, I do not find support for these expectations.[8]

In section B.2 of the appendix I demonstrate the stability of my results to a host of alternative specifications in which I control for an unbalanced covariate (gender), as well as additional treatment arms. I also demonstrate that respondents' age, which in the context of COVID-19 proxies vulnerability and medical risk, does not moderate treatment effects (Table S6). Lastly, in Figure S2 of the appendix, I examine the spillover effects of my treatment, on attitudes towards other social groups (i.e. left and right-wing partisans). I find that my treatment does not affect attitudes

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[8]I present several manipulation checks which suggest that unlike the diversity and severity treatments, my exclusion treatment was not effective in shifting respondents' perceptions regarding political elites' preferences for exclusion. Therefore, as I further discuss in section B.2 of the appendix, the null moderating effect of exclusion is likely driven by the limited effectiveness of this specific treatment arm.

towards right-wing partisans, but has a slight imprecisely estimated effect on attitudes towards left-wing partisans. Together, these additional analyses further strengthen my confidence in the identified effects of my first experiment, which provide strong support in favor of my theory of prejudice reduction through institutional diversity.

Study II: The Effects of Muslim Healthcare Workers on Non-Muslim American Prejudice

Thus far I have provided evidence in support of my theory from Israel – An important case for scholars of prejudice reduction (Canetti-Nisim, Ariely and Halperin 2008). However, it is unclear whether such evidence generalizes to additional contexts. Since generalizability is primarily achieved through replication of similar studies in different sites (McDermott 2011), I now turn to test my theory in an alternative case - The U.S.

Like in Israel, where intergroup relations are shaped by a host of ethnic, religious, and partisan cleavages (Canetti-Nisim, Ariely and Halperin 2008; Enos and Gidron 2016, 2018; Bassan-Nygate and Weiss 2020), multiple minority groups in American society suffer from negative stereotypes and prejudice (Sides and Gross 2013; Williamson 2019; Lajevardi Forthcoming). However, in my U.S. study, I focus on Muslim representation in healthcare institutions, and its effects on majority group attitudes towards Muslim Americans.[9] My focus on the U.S., and specifically on prejudice towards Muslims, is informed by four similarities linking the American context with Israel.

Cross-Case Similarities

First, previous studies suggest that like Arab healthcare workers in Israel, Muslim physicians are an integral part of U.S. healthcare institutions. Indeed, though Muslims comprise only 1% of the American population, estimates suggest that 5% of all U.S. physicians are Muslim (Padela et al. 2016). Second, despite the over-representation of Muslims in healthcare institutions relative to their size in the American population (like in the Israeli context), Muslim Americans still suffer from a great deal of negative stereotypes and prejudice (Sides and Gross 2013; Williamson 2019;

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[9]Given recent findings which demonstrate that Americans draw little distinctions between "Muslims", and "Muslim-Americans" – I use both terms interchangeably to refer to Muslims residing in the U.S.

Lajevardi and Abrajano 2019; Lajevardi Forthcoming). Indeed, some accounts suggest that Muslims are one of the social groups suffering most severely from hostile attitudes in the U.S. (Kalkan, Layman and Uslaner 2009). Similarly other scholars emphasize that "*Prejudice appears to be alive and well with respect to Muslims...*" and that most Americans are unlikely to view Muslims as anything other than enemies (Sides and Gross 2013). Such grim accounts emphasize the challenge and importance entailed in studying institutional approaches for prejudice reduction towards Muslims in the U.S.

Third, like prejudice towards Arabs in Israel, prejudice towards Muslims in the U.S. is linked with stereotypes relating to violence, (lack of) trustworthiness, and security threats. More so, such negative stereotypes and attitudes have been identified as a cause of support for exclusionary politicians and policies (Sides and Gross 2013; Lajevardi and Abrajano 2019). Lastly, in a similar manner to the Israeli case, negative sentiment towards Muslims is not confined to mass-public opinion. Indeed, such sentiment manifests itself in a host of exclusionary policies relating to immigration bans, and proposed attempts to surveil mosques, and keep updated registries of Muslim U.S. residents for security purposes (Lajevardi and Abrajano 2019).

Cross-Case Differences

Despite these similarities, three important structural factors differentiate between Israel and the U.S., warranting my additional empirical investigations noteworthy. First, the relative share of Muslims in the U.S. population, and the proportion of Muslim physicians in American healthcare institutions is substantially smaller than in Israel. These differences allow me to consider the effectiveness of institutional diversity in shaping intergroup relations, in an instance where the minority group is smaller (i.e. 1% rather than 20%), and its relative role within institutions is more modest (i.e. 5% rather that 20%).

Second, American Muslims differ from Arabs in Israel, in that they are for the most part either immigrants or descendants of immigrants. More so, Muslim Americans are far more ethnically diverse (Lajevardi and Abrajano 2019), when compared with Arabs in Israel. Therefore, by focusing on prejudice in the U.S., I am able to consider the extent to which minority representation is

effective in reducing prejudice towards a far more heterogenous social group.

Lastly, unlike the Israeli case, the U.S. is not embedded in an ongoing intractable conflict (Bar-Tal 1998). More so, unlike many Arabs in Israel which have Palestinian national aspirations, Muslim Americans do not have distinct national aspirations. Therefore, by turning to the U.S., I am able to consider the extent to which identified effects generalize to ethnically diverse societies which are not entangled in an ongoing national conflict.

Clearly, there are some stark similarities, as well as notable differences between both of my empirical cases. Since cross-site differences between my two experimental sites cannot be reduced to one variable, attributing contextual moderation in identified effects to one factor alone would not be credible – and doing so is not the objective of my second study. However, by turning to the U.S., and examining the replicability of my findings in a different context, I seek to bolster the credibility of my theory, and provide evidence for its generalizability beyond Israel.

Experimental Design

I fielded my second experiment through Lucid, amongst a sample of 1,216 U.S. survey respondents (See figure 2 for precise timing).[10] I report descriptive statistics of my sample in Table S11 in the supplementary materials. In section A of the supplementary materials I discuss ethical issues relating to subject compensation and IRB approval. After filling out a battery of pre-treatment demographics, respondents were exposed to a fully crossed $2x2x2$ experimental vignette which described the crisis around the COVID-19 pandemic in the U.S. As depicted in the appendix (Figure S4), this vignette was very similar to the one I previously employed in the Israeli context.

Specifically, my main treatment informed respondents that in many localities there is a sizable proportion of Muslim healthcare workers, and that more generally there are over 50,000 Muslim

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[10]Lucid is a platform which provides access to online survey respondents. Recent research demonstrates the suitability of the Lucid platform for evaluating social scientific theories (Coppock and McClellan 2019). Platforms like Lucid grew in popularity during the first stages of the COVID-19 pandemic, and a series of replication studies suggests that the "pandemic does not pose a fundamental threat to the generalizability of online experiments to other time periods." However, effect magnitudes of pandemic period experiments yield smaller average treatment effects, and this may be a result of lower attentiveness amongst online subjects (Peyton, Huber and Coppock 2020).

doctors and many more nurses working in American hospitals. These statistics were based on previous studies of diversity in American healthcare institutions (Abu-Ras, Laird and Sensai 2012; Padela et al. 2016).[11] In addition, like in the Israeli experiment, I embedded treatments regarding political elite's active support for exclusion (alluding to Muslim immigration bans), as well as information regarding the severity of the COVID-19 crisis.[12] Like in my first experiment, all information provided in the vignette was based on verified true information to avoid deception of subjects.

After reading the vignette respondents were presented with several batteries of questions regarding the extent to which they follow social distancing guidelines, as well as their social and political attitudes and preferences. Most importantly, these batteries included the five outcome measures explored in the Israeli experiment. The precise wording of questions used as outcome measures is depicted in Table 1.[13]

Estimation Strategy

I follow similar estimation procedures when analyzing data from the U.S. Specifically, in my main analyses I regress five measures of intergroup attitudes over my main treatment to identify the

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[11]One notable difference between the treatment in the Israeli and U.S. studies relates to the type of statistics provided. Whereas in the Israeli case I was able to provide official numbers regarding Arab doctors, nurses, and pharmacists, to the best of my knowledge such statistics regarding Muslim healthcare workers do not exist in the U.S. Therefore, in my U.S. experiment I rely on more general estimations from recent policy reports (Abu-Ras, Laird and Sensai 2012).

[12]To correct for the slight imbalance in gender across treated and controlled groups in the Israeli experiment, I implemented a simple block randomization procedure in the U.S. experiment. Specifically, I block randomized subjects by partisanship (Democrat, Republican, Independent, Other) and gender (Male, Female, Other), resulting in twelve potential cells in which randomization occurs. However, since only Democrats self-identified as Other, in practice randomization was implemented in nine cells.

[13]Recent advances in American politics have developed unique indices to study attitudes towards Muslims in the U.S. (Lajevardi and Abrajano 2019; Lajevardi Forthcoming). However, in order to enable a simple and more direct comparison between the the Israeli and U.S. experiments, I employ common survey questions relating to: i) intergroup affect, ii) preferences for social exclusion, iii) intergroup trust, iv) attitudes about intergroup peace, and v) preferences for political inclusion. These measures have been used widely by social scientists across different contexts in order to capture majority group members' prejudice towards minorities.

effects of information regarding Muslim representation in healthcare provision. In the results re-
ported below, I deviate from my pre-analysis plan, and include block-fixed effect to improve the
precision of my estimates. A formal representation of my model is reported in Equation 2. My
main parameter of interest in the following analyses is β_i, which represents the average treatment
effect of my main diversity treatment (randomized within blocks), on attitudes towards muslims
(y_{ib}). In Figure S5 of the appendix, I demonstrate the robustness of my results to the inclusion
of pre-treatment covariates, as well as my additional treatment arms relating to the severity of
COVID-19, and politicians preferences for Muslim Exclusion.

$$y_{ib} = \beta_i + \gamma_b + \varepsilon_{ib} \tag{2}$$

Results

In Figure 5, I report the main effects from my U.S. experiment. As indicated by the positive
point estimates, information regarding the share of Muslim healthcare workers in U.S. healthcare
institutions improves intergroup attitudes. However, such information does not appear to have a
statistically significant effect on preferences for political inclusion.

Notably, the effects of information regarding Muslim representation in healthcare institutions
are smaller in the U.S. context, when compared with Israel. Indeed, treated respondents who
learn about the share of Muslim doctors and nurses in American healthcare institutions, report
slightly more than a tenth of a standard deviation increase in affect towards Muslims (measured
through a commonly used feeling thermometer), as opposed to almost a quarter of a standard
deviation increase in the Israeli case. Substantively, the magnitude of treatment effects amongst
my U.S. sample, is equivalent to over two-thirds of the impact of shifting from strong to moderate
Republican views.

What might explain the smaller effect size in the U.S. experiment, when compared with results
from Israel? One theoretical explanation could relate to the varying strength of treatments across
both studies. Specifically, the Israeli treatment provided more explicit information regarding the
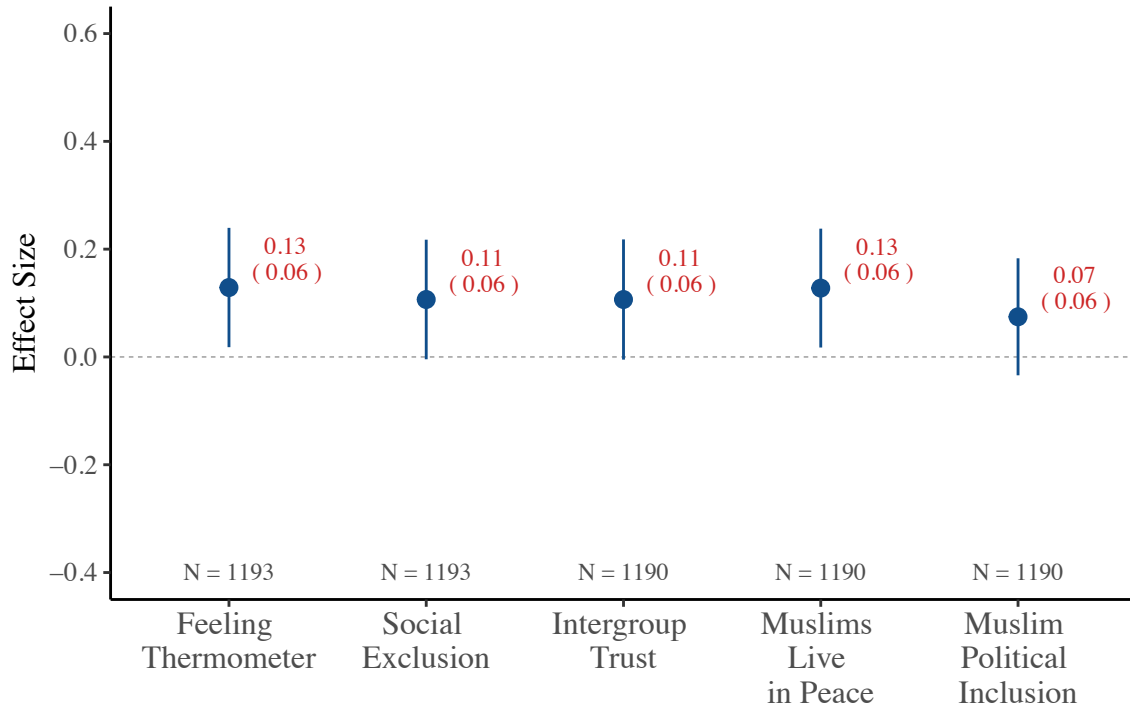
Figure 5: **Information about Muslim healthcare workers reduces prejudice amongst non-Muslim Americans** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Muslim representation on prejudice attitudes and preferences for social exclusion. Point estimates and standard errors (in parentheses), are depicted in red, and observations for each model are reported in gray at the bottom region of the plot.

precise share of doctors, nurses, and pharmacists in Israeli healthcare institutions, as opposed to the U.S. treatment which provided general information regarding diversity and Muslim representation. More so, the share of Arab workers in Israeli healthcare institutions is larger (both in absolute and relative terms), than the share of Muslim workers in American healthcare institutions. It is possible that the variation in treatment strength, which was necessitated in part by the state of institutional representation in both countries, and in part by the lack of formal statistics on Muslim representation in U.S. healthcare institutions, accounts for variation in effect sizes across both studies.

Alternatively, variation in effect sizes might relate to baseline attitudes towards minorities in both contexts. Specifically, when considering feeling thermometers in both contexts, it appears

that hostility towards Arabs is substantially more severe in Israel, than hostility towards Muslims in the U.S (See Figure 6). Indeed, the average of my intergroup affect measure in Israel is 42.4 ($\sigma^2 = 24.5$), whereas the identical measure in the U.S. equals 58.4 ($\sigma^2 = 29.9$). Similarly, the Israeli average on my political inclusion variable is almost a full unit lower ($\mu = 3.5$, $\sigma^2 = 1.9$) than in the U.S. ($\mu = 4.4$, $\sigma^2 = 1.7$). Therefore, it is possible that there is much more "room" to move Israelis with regards to prejudice towards minorities, when compared with American survey respondents. The said, despite the smaller effect sizes, it still appears that American respondents who learn about the share of Muslim doctors and nurses in U.S. healthcare institutions report more tolerant attitudes.[14]

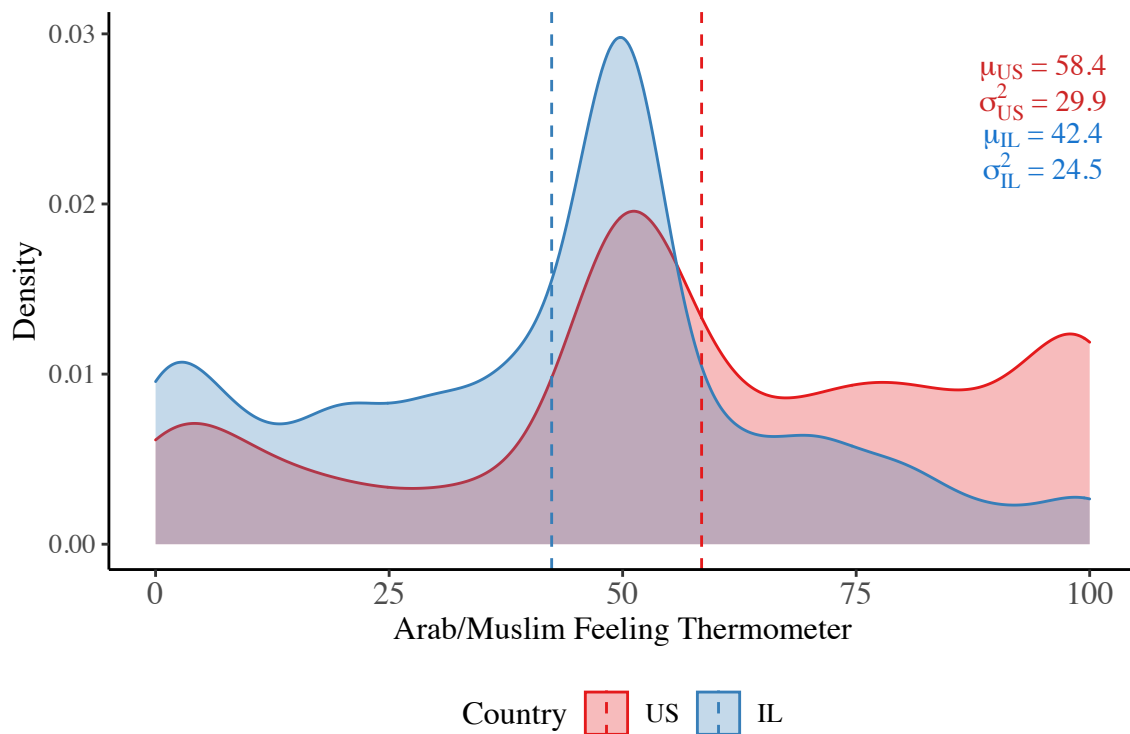Figure 6: **Comparison of Intergroup Affect in Israel and the U.S.** – Dotted lines denote country means.

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[14]A final more technical explanation for the smaller effect size in the U.S. experiment, might relate to subject attentiveness on the Lucid platform during the first stages of the COVID-19 pandemic (Peyton, Huber and Coppock 2020), which might have been lower than attentiveness in the Israeli IPanel sample.

Does Partisanship Moderate Identified Effects?

Given the centrality of partisan polarization in the U.S. (Iyengar et al. 2019), one may expect that identified treatment effects vary significantly by partisanship. Specifically, one may worry that information regarding the share of Muslims in American healthcare institutions reduces prejudice amongst Democrats and Independents, but results in backlash effects amongst Republicans. Alternatively, it is possible that Republicans that are on average more prejudicial towards Muslims, will report higher degrees of prejudice reduction, as suggested to some extent by Williamson (2019). I consider these possibilities in Tables 3-4 below.

Specifically, to consider the possibility of polarized reactions to information regarding Muslim representation in U.S. healthcare institutions, I interact my main treatment with a partisanship indicator. To account for pre-treatment covariates which correlate with partisanship, I further control for a set of covariates, including: respondents' race, religiosity, education, and age.[15] To improve the efficiency of my model I further control for other treatment arms. Table 3 (4) compares the effect of my treatment conditional on Democratic (Republican) party identification.

Table 3: Heterogenous Treatment Effect of Muslim Representation Conditional on Party ID – Democrats (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.091	0.123	0.129	0.196	0.058
	(0.071)	(0.071)	(0.071)	(0.070)	(0.068)
Democrat	0.192	0.108	0.504	0.598	0.483
	(0.225)	(0.228)	(0.226)	(0.224)	(0.218)
Representation*Democrat	0.071	−0.051	−0.069	−0.192	−0.004
	(0.117)	(0.116)	(0.115)	(0.114)	(0.111)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

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[15]Block randomization was administered according to gender and partisanship. Thus my block fixed effects account for these covariates.

Table 4: Heterogenous Treatment Effect of Muslim Representation Conditional on Party ID – Republicans (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.081	0.054	0.027	0.050	0.023
	(0.069)	(0.069)	(0.068)	(0.067)	(0.066)
Republican	−0.499	−0.411	−0.591	−0.656	−0.793
	(0.118)	(0.117)	(0.117)	(0.115)	(0.113)
Representation*Republican	0.110	0.151	0.228	0.223	0.101
	(0.119)	(0.118)	(0.117)	(0.116)	(0.113)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

For the most part, these additional analyses do not provide strong support for the expectation that Republicans and Democrats react differently to information regarding Muslim representation in healthcare institutions. Indeed, in eight out of ten models in Tables 3-4, the interaction term `Representation*Republican/Democrat` is statistically insignificant. Only in two models (Model 3-4, Table 4) does this interaction approach conventional levels of statistical significance ($p < 0.1$). In Tables S19-S20 of the appendix, I demonstrate that this pattern of limited polarization in treatment effects remains stable when focusing only on strongly identifying Democrats and Republicans. I construe this, together with results from the Israeli case, as evidence for the rather uniform effect of minority institutional diversity on intergroup attitudes.

Additional Analyses and Robustness Checks

In Section C.2.3 of the appendix, I consider a battery of additional pre-registered heterogenous treatment effects. First, I explore the possibility that my severity and political exclusion treatments moderate my main average treatment effects. However, like in the Israeli context I find limited support for such moderation. To further consider the possibility that the severity of the COVID-19 crisis moderates my main effects, I leverage two pre-treatment covariates: `age`, and `pre-existing medical conditions`. These variables take a value of 1 for respondents who are more vulnerable

to COVID-19 due to their age (65+), or medical condition. In Tables S17-S18 of the appendix, I report interaction models which provide further support for my finding that the severity of the crisis does not moderate the main effects of diversity on prejudice.

In Figure S5 of the the appendix, I demonstrate the robustness of my results to additional model specifications where I control for pre-treatment covariates and additional treatment arms. In addition, like in my Israeli study, I consider the extent to which information regarding Muslim representation in healthcare institutions shapes attitudes towards other social groups (Figure S6). Specifically, I focus on attitudes towards Democrats, Republicans, Asian Americans, African Americans, and Hispanics. Results from Figure S6 suggest that in addition to the consistent effects of my treatment on attitudes towards Muslims, my treatment has some inconsistent positive effects on attitudes towards Democrats and Black Americans, and no discernible effect on attitudes towards Asian Americans, Hispanics, and Republicans.

Lastly, one may worry that prejudice reduction caused by institutional diversity, might come at the cost of majority group members' willingness to follow guidelines put forward by institutions where minorities are well represented. In Table S14 of the appendix, I attempt to address these concerns, by considering the extent to which information about the share of Muslims in healthcare institutions affects citizens' adherence to public health guidelines aimed to limit the spread of COVID-19. Encouragingly, I do not find support for this concern in my additional analyses. Indeed, my main treatment does not affect respondents' self-reported likelihood of wearing a mask, leaving their home, or visiting friends and family.

Conclusion

In this paper, I provide an answer for a consequential yet under-explored question, namely: how does information regarding diversity and minority representation in non-elected institutions shape intergroup relations? To do so, I build on several frameworks in social and political psychology (Wittenbrink, Judd and Park 2001; Blair 2002; Ramasubramanian 2007; Williamson 2019), and develop a theory of prejudice reduction through institutional representation. Specifically, I argue that information about the share of minorities in non-elected institutions reduces prejudice and

promotes preferences for political inclusion.

My theory is inspired by recent calls to consider structural and institutional perspectives on prejudice reduction (Paluck et al. 2020). Indeed, the theoretical argument I lay out diverges from recent grass-root interventions to improve intergroup relations (Ditlmann, Samii and Zeitzoff 2017; Scacco and Warren 2018; Mousa 2020). Instead, I suggest an approach which promotes prejudice reduction by leveraging the widespread nature and influence that state institutions can have on citizens daily lives, attitudes, and preferences.

To test my theory, I focus on healthcare institutions and the share of minority workers within them. Through a series of survey experiments implemented during the hight of the first outbreak of COVID-19, I demonstrate that information regarding the share of Arab (Muslims) healthcare workers in Israeli (American) hospitals reduces prejudicial attitudes towards Arabs (Muslims). More so in the Israeli case, these effects extend beyond social measures of prejudice such as intergroup affect and trust, as information about diversity and representation in healthcare provision promotes more inclusive political preferences.

Interestingly, despite the diversionary consequences of polarization documented in both the Israeli and American contexts (Iyengar et al. 2019; Bassan-Nygate and Weiss 2020), I do not find strong evidence that the effects of information regarding representation are systematically moderated by respondents' partisan identity. Indeed right- and left-wing (Democrat and Republican) respondents react rather similarly to information regarding minority representation. More so, I find no support for backlash effects amongst more conservative respondents, or negative externalities around adherence to public health guidelines. Taken together, the rather uniform effects from Israel and the U.S. provide strong support for my theory of prejudice reduction through institutional diversity, suggesting that providing citizens with information regarding the demographic make-up of diverse public institutions can have psychological effects, which promote more favorable intergroup relations.

However, this study is not without limitations. Like many other studies of diversity and representation in public institutions (Keiser et al. 2002; Riccucci, Van Ryzin and Lavena 2014; Riccucci,

Van Ryzin and Li 2016; Nanes 2018), my empirical focus is on one institutional environment: healthcare provision. This in turn limits my ability to confidently generalize my findings to other types of institutions such as schools, tax collection agencies, or police forces.

My institutional focus was motivated by the centrality of healthcare institutions in developed countries (OECD 2019), as well as by previous studies which demonstrate the positive effects of minority representation on minority health outcomes (Hill, Jones and Woodworth 2018; Alsan, Garrick and Graziani 2019; Greenwood et al. 2020). In many regards healthcare institutions are a most likely institutional context for my theory of prejudice reduction through institutional diversity, since diversity and minority representation in such institutions provides positive information regarding out-group members' competence, and general role in society. However, my theoretical framework should apply to additional institutional environments such as schools or welfare agencies, in which minority representation provides majority group members with positive information about out-group members. Testing this expectation, and identifying whether and how institutional environments moderate the effects of minority representation on prejudice is a promising avenue for future research.

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Learning About Diversity in Healthcare Institutions Reduces Prejudice

Supplementary Information

A Discussion of Ethics in Experimental Designs

Both experiments included in this paper were evaluated by the Institutional Review Board of the authors home institution, and were determined to meet the criteria for exempt human subjects research. More generally, the research included in this paper complies with the with Principles and Guidance for Human Subjects Research outlined by the American Political Science Association. The risks to subjects in both studies were evaluated to be minimal and the researcher set several procedures to ensure that any potentially identifying information provided by respondents was protected and omitted prior to making the data available for analysis and replication. In practice no such identifying information was received or collected by the author. In addition, there were no conflicts of interest identified for the researcher implementing this study. The data for replication, which does not include any identifiable information, will be made available when the manuscript is published via dataverse. In addition, the pre-registration materials are already posted on OSF (https://osf.io/bmxr4/).

Both experiments began with respondents completing a standard online consent form. Through this form respondents were informed that they were being asked to participate in a voluntary study that had been reviewed by the University of Wisconsin-Madison institutional review board. The consent form provided respondents with information that if they choose to participate in the study, they would be asked a variety of questions about intergroup relations in their country. More so, to promote full transparency regarding the research process, the consent form emphasized that some questions may make some respondents feel uncomfortable, and explained that if a respondent feels uncomfortable with answering a particular question they may skip it, or leave the survey. The consent form also included the estimated length of the survey, the contact information for the investigator, and that the study was deemed to be of minimal risk by the Univeristy of Wisconsin-Madison IRB. Respondents could select "I hereby give my consent to be the subject of your research" or "I do NOT consent to be the subject of this research." If the later was selected, the survey was terminated.

Respondents were recruited by commonly used surveying firms in Israel (IPanel) and the U.S.

(Lucid). All respondents in the both studies voluntarily selected to be part of respondent pools for survey research. Compensation for participation in each study was administered by survey firms and not by the author of this article. The compensation provided by each survey vendor, could include cash, gift cards, and loyalty reward points. The respondents in both studies were all based in Israel (study I) or the United States (study II), and received necessary information allowing them to contact the researcher regarding any concerns about compensation or the survey itself. In addition, respondents received contact information for the author's IRB office. As of now, the author or the IRB office did not receive any queries from subjects participating in this research.

As noted in the main text both experiments avoid any use of deception or "incomplete disclosure" of information. In addition, with regard to Principal 10 on the impact of the research on the political processes, it is highly unlikely that the experiments presented in this paper would have had an impact on elections or policy creation.

B   Israeli Study

My Israeli experiment was embedded within a brief public opinion survey distributed amongst sample of 1,366 Jewish Israelis. I report descriptive statistics of all variables employed in my analysis in Table S1. In addition, I provide an elaborate description of my survey in section B.1 below.

B.1   Survey Instrument

My survey included four main sections: i) pre-treatment demographic questions, ii) an experimental vignette (see Figure 3 in the main text), iii) outcome measures, and iv) a series of manipulation checks. In this section I outline all variables collected as part of my survey.

- **Informed Consent**

- **Demographic Questions**

    - Gender

    - Age Group

- Ethnicity

- Religiosity

- Locality

- Education

- Political Ideology

- Exact Age

- **Experimental Vignette**

- **Outcome Measures**

  - Feeling Thermometers

    * Left-Wing Supporters

    * Right-Wing Supporters

    * Arabs

  - Social Exclusion

    * Left-Wing Supporters

    * Right-Wing Supporters

    * Arabs

  - Additional Measures of Intergroup Relations – Do you agree that:

    * Most Arabs can be trusted

    * Most Arabs want to live in peace

    * Arabs should serve as leaders of Knesset committees

  - Social Norms – Do you agree that:

    * Most Israelis would be willing to receive treatment from Arab doctors

    * Most Israelis would be willing to be friends with Arabs

* Most Israelis would be willing to work for an Arab Boss

* Most Israelis would support the idea that Arab MKs serve as leaders of Knesset committees

- **Manipulation Checks**

  - Do you agree that:

    * The effects of the virus on Israeli society will be severe

    * Arab doctors are in the forefront of combatting the coronavirus

    * There are many MKs that oppose including members of the Arab Joint list in key positions within the government

B.2 Additional Analyses

B.2.1 Manipulation Checks

In Table S2 I assess the effectiveness of my treatment by leveraging three manipulation check questions. Specifically, to consider the extent to which my treatments impacted respondents' perspectives regarding i) Arab representation in healthcare institutions, ii) Crisis severity, and iii) Israeli politicians exclusionary statements, I regressed responses to the last three question in my survey (see section above), over treatment indicators.

Results from Table S2 suggest that my Arab representation treatment increased respondents' perception that Arab healthcare workers are at the forefront of combatting the coronavirus (column 1). In addition, it appears that respondents who were provided additional information regarding the severity of the COVID-19 crises were more likely to evaluate the COVID-19 crisis as a severe threat for Israeli society (column 2). That said, the null effects in column 3, suggest that my political exclusion treatment was ineffective in shaping respondents perspectives regarding the prevalence of preferences for exclusion amongst politicians. This may be driven by the salience of such preferences, which are common knowledge to many survey respondents.

Table S1: Descriptive Statistics - Survey Respondents (Israel)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Male	2,508	0.467	0.499	0.000	0.000	1.000	1.000
Age	1,564	41.304	14.643	18.000	28.000	53.000	73.000
Secular	1,611	0.483	0.500	0.000	0.000	1.000	1.000
Traditional	1,611	0.357	0.479	0.000	0.000	1.000	1.000
Religious	1,611	0.132	0.339	0.000	0.000	0.000	1.000
Ultra-Orthodox	1,611	0.028	0.165	0.000	0.000	0.000	1.000
Jerusalem Area	1,611	0.113	0.317	0.000	0.000	0.000	1.000
Tel-Aviv	1,611	0.317	0.466	0.000	0.000	1.000	1.000
Haifa and North	1,611	0.250	0.433	0.000	0.000	0.000	1.000
South	1,611	0.226	0.418	0.000	0.000	0.000	1.000
Sharon	1,611	0.094	0.292	0.000	0.000	0.000	1.000
Less than HS	1,609	0.011	0.102	0.000	0.000	0.000	1.000
HS	1,609	0.238	0.426	0.000	0.000	0.000	1.000
Partial Academic	1,609	0.070	0.255	0.000	0.000	0.000	1.000
Non-Academic Degree	1,609	0.223	0.416	0.000	0.000	0.000	1.000
BA	1,609	0.297	0.457	0.000	0.000	1.000	1.000
MA+	1,609	0.162	0.368	0.000	0.000	0.000	1.000
LR Scale	1,566	3.002	1.623	1.000	2.000	4.000	7.000
Therm Arabs	1,550	42.180	24.596	0.000	24.000	52.000	100.000
Exclusion Arabs	1,542	4.154	1.933	1.000	3.000	6.000	7.000
Trust Arabs	1,538	4.325	1.707	1.000	3.000	6.000	7.000
Peace Arabs	1,538	4.720	1.615	1.000	4.000	6.000	7.000
Pol Inc Arabs	1,538	3.570	1.957	1.000	2.000	5.000	7.000
Manip - Doctor	1,532	5.106	1.541	1.000	4.000	6.000	7.000
Manip - Severity	1,532	5.616	1.406	1.000	5.000	7.000	7.000
Manip - Exclusion	1,532	5.632	1.336	1.000	5.000	7.000	7.000

Table S2: Manipulation Check

	Doctor Role	Severity of Crisis	Arab Exclusion
	(1)	(2)	(3)
Arab Treatment	0.176		
	(0.079)		
Severity Treatment		0.151	
		(0.072)	
Exclusion Treatment			0.043
			(0.068)
N	1,532	1,532	1,532

B.2.2 Balance and Robustness Checks

In table S3 I present results from balance tests comparing respondents across the two conditions of my main treatment relating to diversity and Arab representation in Israeli healthcare institutions. As noted in the main text, despite randomization there is a small albeit statistically significant difference in gender across conditions. To ensure that my identified effects are not confounded by gender, in Figure S1 I present a set of additional analyses, with alternative specification to my main pre-registered model.

Table S3: Balance on Covariates (Israeli Sample)

	Variable	Treatment N	Control N	Treatment Mean	Control Mean	p. Value
1	Age	612	599	47.79	48.77	0.30
2	Male	612	599	0.45	0.45	0.90
3	Education	612	599	4.48	4.61	0.23
4	White	611	599	0.76	0.73	0.23
5	Ideology	612	599	1.97	1.92	0.53

Figure S1 demonstrates that my main results remain robust when controlling for gender (see blue-triangle coefficients), additional treatment arms (see green-square coefficients), and an exhaustive set of individual-level covariates (see purple-cross coefficients). These additional models strengthen my confidence in the identified effects presented in the main text.

In Figure S2 I present results from additional analyses in which I consider whether my Arab representation treatment shifted attitudes towards other social groups. In large, my main treatment did not affect general affect, or attitudes of social exclusion towards Right-Wing partisans. In addition, I do not detect a treatment effect on respondents' preferences of social exclusion towards left-wing supporters. That said, I do find a small effect, which approaches conventional levels of statistical significance when considering general affect towards Left-Wing partisans. This may be driven by the fact that respondents link between Arabs, Arab doctors, and left-wing partisans. However, more generally, I construe these findings as supportive of the idea, that my treatment facilitates updating with regards to the represented minority group (i.e. Arabs).
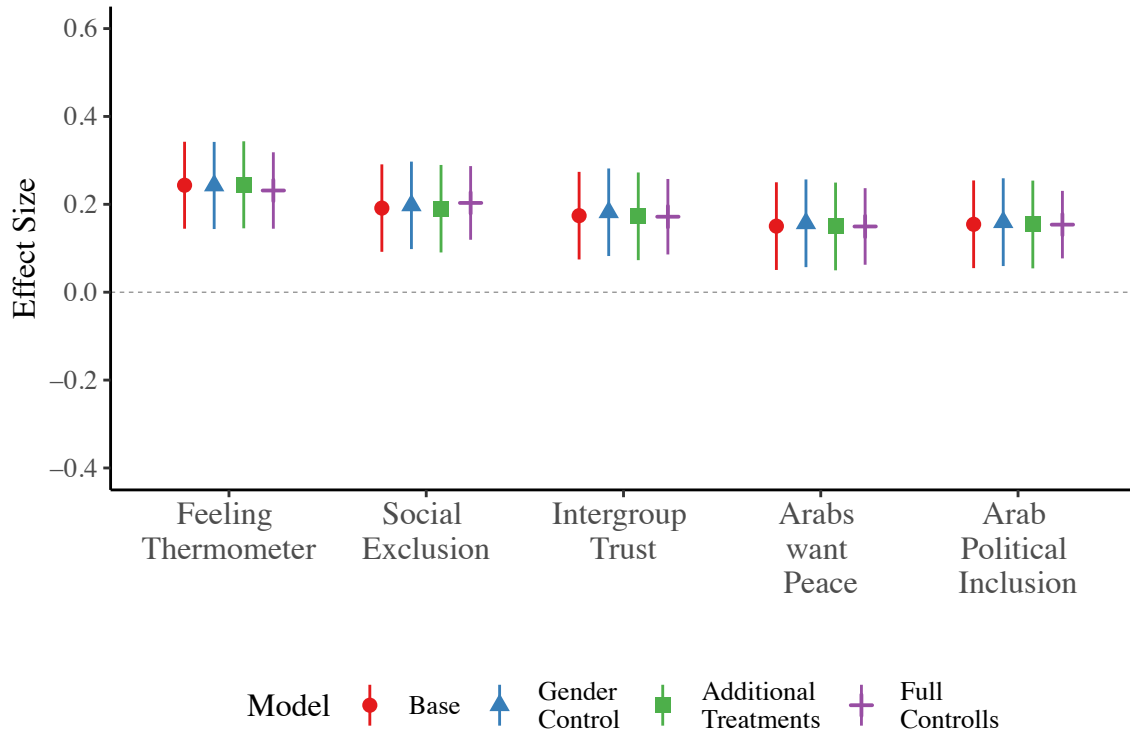
Figure S1: **Israeli Experiment Robustness to Alternative Specifications** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Arab representation on prejudice attitudes and preferences for political exclusion. The full control model include the following covariates: `age`, `gender`, `ethnicity`, `religiosity`, `locality`, and `education`.

B.2.3 Heterogenous Treatment Effects

In this section, I report a series of pre-registered heterogenous treatment effects. In my original pre-analysis plan, I expected that emphasizing the severity of the COVID-19 crisis would amplify the average treatment effects of information regarding diversity, whereas emphasizing politicians' preferences for political exclusion of Arabs would dampen average treatment effects. In Tables S4-S5 I consider these possibilities by interacting my representation treatment with a binary variable taking the value of one for respondents primed with the severity or exclusion treatments.

As demonstrated in these tables, I do not find support for these expectations, as the effects of representation are not moderated by the severity of the COVID-19 crisis. However, my ability to speak about the moderating effects of politicians' exclusionary statements is rather limited,
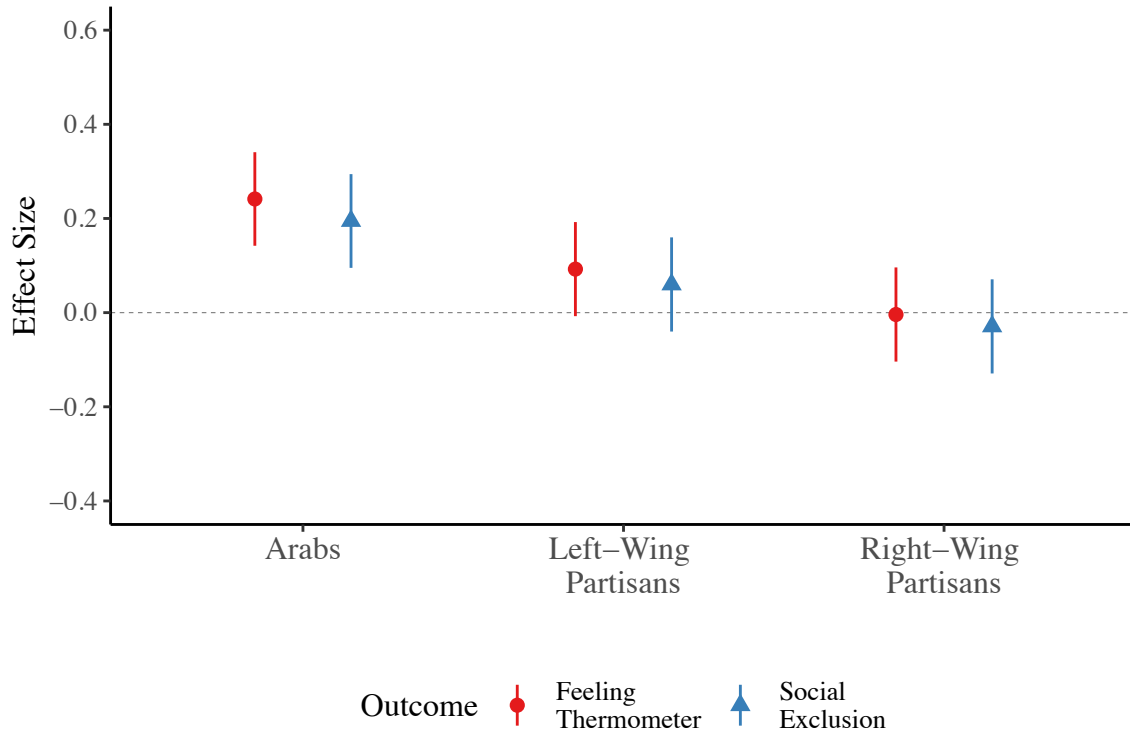
Figure S2: **Treatment Effects on Other Social Groups** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Arab representation on prejudice attitudes towards Arabs, Left-Wing Partisans, and Right-Wing Partisans.

since the manipulation checks presented in Table S2 suggest that unlike my representation and severity treatments, the exclusion treatment was unsuccessful in shaping respondents' perceptions regarding the prevalence of political exclusion.

Table S4: Heterogenous Treatment Effect of Arab Representation Conditional on Severity Treatment

	Therm	Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.291	0.239	0.223	0.200	0.275
	(0.071)	(0.072)	(0.072)	(0.072)	(0.072)
Severity	0.088	0.036	0.055	0.093	0.156
	(0.071)	(0.072)	(0.072)	(0.072)	(0.072)
Representation*Severity	−0.094	−0.095	−0.097	−0.100	−0.240
	(0.101)	(0.101)	(0.102)	(0.102)	(0.102)
N	1,550	1,542	1,538	1,538	1,538

Table S5: Heterogenous Treatment Effect of Arab Representation Conditional on Exclusion Treatment

	Therm	Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.215	0.139	0.106	0.129	0.116
	(0.071)	(0.072)	(0.072)	(0.072)	(0.072)
Exclusion	−0.052	−0.008	−0.019	0.015	−0.014
	(0.071)	(0.072)	(0.072)	(0.072)	(0.072)
Representation*Exclusion	0.058	0.102	0.134	0.041	0.077
	(0.101)	(0.101)	(0.102)	(0.102)	(0.102)
N	1,550	1,542	1,538	1,538	1,538

In Table S6 I consider the possibility that older high-risk respondents' report stronger treatment effects. The motivation for this expectation is similar to the motivation around the moderating effects of crisis severity. Specifically, one may expect that respondents which are more threatened by the pandemic, may appreciate the role of Arab healthcare workers to a greater extent, and thus report higher degrees of prejudice reduction. To consider this possibility, I created a binary indicator taking a value of one for respondents ages 65 and up,[16] which I interacted with my main treatment regarding Arab representation. Results from Table S6 suggest that older respondents who are more vulnerable to COVID-19, do not report different reaction to my treatment.

In Tables S7-S10 I provide a series of additional robustness checks relating to the (lack of) moderating effects of partisanship on my main treatment – information regarding Arab representation. Specifically, expanding on Table 2 of the main text, where I interact a seven point ideology scale with my main treatment, I further consider a fully saturated model where I divide the seven-point scale into binary indicators, which are interacted with my main treatment. Doing so, I do not find any support that ideology moderates treatment effects.

In addition, I consider the extent to which "strong" partisans or centrists react differently to my treatment. To do so, I created three binary indicators: Left – taking a value of 1 for respondents scoring 6-7 on the ideology scale, Right – taking a value of 1 for respondents scoring 1-2 on the

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[16]Citizens above the age of 65 were identified by the Israeli Ministry of Health as "at risk".

Table S6: Heterogenous Treatment Effect of Arab Representation Conditional on Vulnerable Age

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.249	0.230	0.197	0.165	0.195
	(0.050)	(0.049)	(0.050)	(0.050)	(0.047)
65+	0.166	0.256	0.234	0.183	0.306
	(0.140)	(0.137)	(0.141)	(0.141)	(0.134)
Representation*65+	0.079	0.002	0.014	0.163	−0.203
	(0.199)	(0.194)	(0.200)	(0.200)	(0.190)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.

ideology scale, and `Center` – taking a value of 1 for respondents scoring 4 on the ideology scale. When interacting these indicators with my main treatment in Tables S8-S10, I find no evidence that strong partisans or centrists respond to my treatment in a unique fashion. Indeed, these additional analyses bolster my confidence that information regarding Arab representation in Israeli healthcare institutions has a rather uniform effect on Israeli Jewish citizens.

B.2.4 *Treatment Effects on Perceived Social Norms*

In Figure S3, I report additional pre-registered analyses, which consider treatment effects on a set of perceived social norms. As revealed from the small yet imprecisely estimated effects, it appears that information about diversity in healthcare institutions does not shape perceived social norms. These null effects may be driven by the subtle nature of my treatment, and the pervasiveness of social norms relating to intergroup relations in Israel.

C   U.S. Study

My U.S experiment was embedded within a brief public opinion survey distributed amongst a sample of 1216 non-Musilim U.S. survey respondents. I report descriptive statistics of all variables employed in my analysis in Table S11. In addition, I provide an elaborate description of my survey in section C.1 below.

Table S7: Heterogenous Treatment Effect of Arab Representation Conditional on Ideology (Fully Saturated Model)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.235	0.283	0.221	0.153	0.197
	(0.088)	(0.084)	(0.087)	(0.088)	(0.077)
Center-Left	−0.168	−0.122	−0.096	−0.170	−0.290
	(0.102)	(0.098)	(0.101)	(0.102)	(0.090)
Center-Right	0.437	0.567	0.601	0.366	0.686
	(0.122)	(0.118)	(0.121)	(0.123)	(0.108)
Left	−0.338	−0.500	−0.452	−0.492	−0.621
	(0.101)	(0.096)	(0.099)	(0.101)	(0.088)
Right	−0.604	−0.782	−0.764	−0.885	−0.825
	(0.091)	(0.087)	(0.089)	(0.091)	(0.080)
Strong Left	0.762	0.258	0.732	0.477	0.838
	(0.182)	(0.174)	(0.179)	(0.182)	(0.159)
Strong Right	0.110	−0.023	0.006	−0.010	0.094
	(0.142)	(0.136)	(0.140)	(0.142)	(0.124)
Representation*Center-Left	−0.028	−0.327	−0.195	−0.092	−0.108
	(0.177)	(0.170)	(0.175)	(0.178)	(0.156)
Representation*Center-Right	−0.002	−0.056	−0.166	−0.114	−0.193
	(0.136)	(0.130)	(0.135)	(0.137)	(0.120)
Representation*Left	−0.206	−0.120	−0.244	−0.152	0.006
	(0.227)	(0.217)	(0.223)	(0.227)	(0.199)
Representation*Right	−0.088	−0.210	−0.013	0.088	−0.071
	(0.125)	(0.120)	(0.123)	(0.126)	(0.110)
Representation*Strong Left	0.242	0.274	0.201	0.208	0.169
	(0.279)	(0.266)	(0.277)	(0.282)	(0.247)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.

Table S8: Heterogenous Treatment Effect of Arab Representation Conditional on Strong Left Ideology

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.244	0.206	0.187	0.164	0.161
	(0.048)	(0.048)	(0.049)	(0.049)	(0.046)
Left	0.947	0.580	0.936	0.747	0.995
	(0.130)	(0.129)	(0.131)	(0.132)	(0.122)
Representation*Left	−0.044	0.151	−0.043	−0.023	0.095
	(0.176)	(0.175)	(0.178)	(0.180)	(0.167)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.

Table S9: Heterogenous Treatment Effect of Arab Representation Conditional on Strong Right Ideology

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.255	0.233	0.181	0.130	0.207
	(0.061)	(0.058)	(0.060)	(0.060)	(0.055)
Right	−0.578	−0.741	−0.763	−0.770	−0.813
	(0.070)	(0.067)	(0.069)	(0.070)	(0.064)
Representation*Right	−0.053	−0.073	−0.023	0.043	−0.125
	(0.093)	(0.088)	(0.092)	(0.092)	(0.084)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.

Table S10: Heterogenous Treatment Effect of Arab Representation Conditional on Center Ideology

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.245	0.194	0.172	0.165	0.159
	(0.056)	(0.054)	(0.056)	(0.056)	(0.053)
Center	0.087	0.193	0.120	0.253	0.189
	(0.079)	(0.077)	(0.080)	(0.079)	(0.075)
Representation*Center	0.016	0.119	0.081	0.016	0.074
	(0.110)	(0.107)	(0.111)	(0.111)	(0.105)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,549	1,541	1,537	1,537	1,537

Notes: Controls include: age, gender, ethnicity, religiosity, education, and indicators for exclusion and severity treatments.
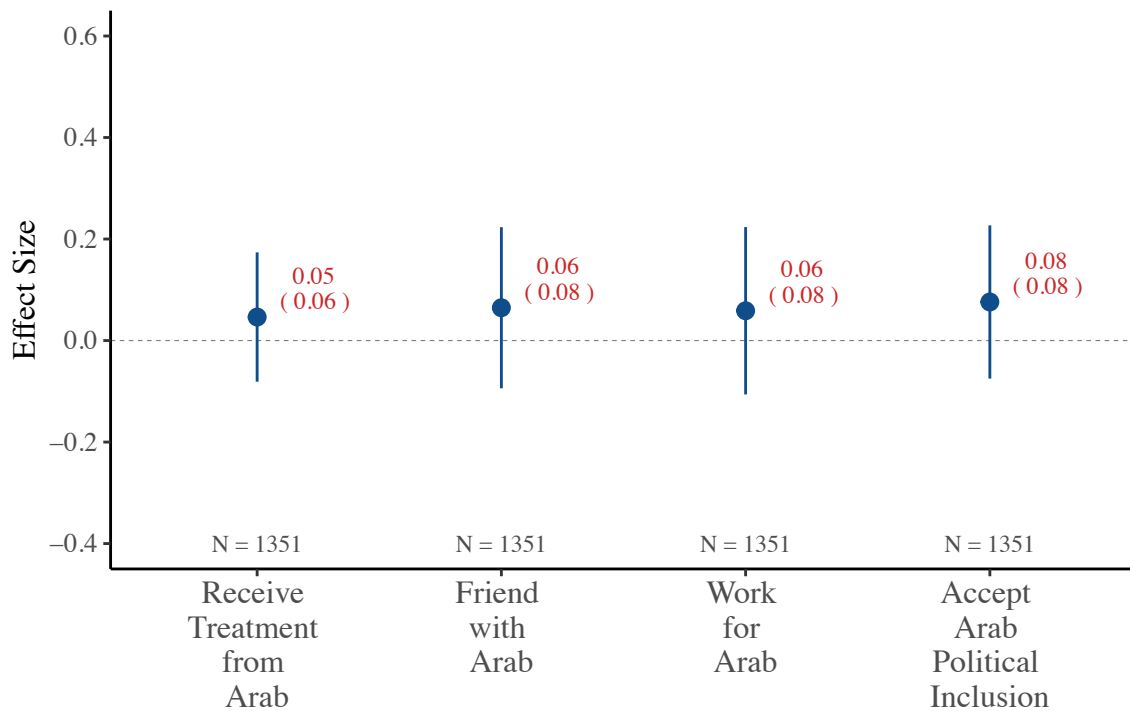

Figure S3: **Treatment Effects on Perceived Social Norms** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Arab representation on perceived social norms. Each outcome measure asks respondents whether most Israelis would engage in a given behavior.

Table S11: Descriptive Statistics - Survey Respondents (US)

Statistic	N	Mean	St. Dev.	Min	Pctl(25)	Pctl(75)	Max
Male	1,211	0.453	0.498	0	0	1	1
Age	1,211	48.277	16.505	16	35	62	95
White	1,210	0.745	0.436	0.000	0.000	1.000	1.000
Hispanic	1,210	0.076	0.265	0.000	0.000	0.000	1.000
African American	1,210	0.108	0.311	0.000	0.000	0.000	1.000
Asian	1,210	0.044	0.205	0.000	0.000	0.000	1.000
Protestant	1,211	0.268	0.443	0	0	1	1
Catholic	1,211	0.293	0.455	0	0	1	1
Atheist	1,211	0.128	0.334	0	0	0	1
Jewish	1,211	0.027	0.163	0	0	0	1
Buddhist	1,211	0.014	0.118	0	0	0	1
Hindu	1,211	0.007	0.081	0	0	0	1
Education	1,211	4.540	1.908	1	2	6	8
Democrat	1,211	0.372	0.483	0	0	1	1
Republican	1,211	0.336	0.473	0	0	1	1
Idenpendent	1,211	0.261	0.439	0	0	1	1
Other Party	1,211	0.031	0.174	0	0	0	1
COVID Medical Risk	1,211	0.336	0.473	0	0	1	1
Thermometer (Muslims)	1,193	58.453	29.928	0.000	47.000	83.000	100.000
Social Exclusion (Muslims)	1,188	4.889	1.884	1.000	4.000	7.000	7.000
Trust (Muslims)	1,186	5.031	1.670	1.000	4.000	6.000	7.000
Peace (Muslims)	1,186	5.363	1.544	1.000	5.000	7.000	7.000
Political Inclusion (Muslims)	1,186	4.474	1.776	1.000	4.000	6.000	7.000
Doctor Manipulation	1,176	4.776	1.500	1.000	4.000	6.000	7.000
Severity Manipulation	1,176	5.864	1.317	1.000	5.000	7.000	7.000
Exclusion Manipulation	1,176	4.960	1.495	1.000	4.000	6.000	7.000

C.1 Survey Instrument

My survey included five main sections: i) pre-treatment demographic questions, ii) an experimental vignette (see Figure S4 below), iii) questions regarding social distancing practices, iv) outcome measures, and v) a series of manipulation checks. In this section, I outline all variables collected as part of my survey.

- **Informed Consent**

- **Demographic Questions**

    - Gender

    - Race

    - Religion

    - State

    - Education

    - Political Ideology

    - Partisanship

        * If response above == "Republican" or "Democrat": Are you a strong or not very strong Republican/Democrat

    - Age

    - Medical Conditions which pose vulnerability with regards to COVID-19 (Obesity / Diabetes / Kidney Disease / Liver Disease / Medical Condition which Compromises the Immune System / Chronic Heat Illness / I Do Not Have Any of these Conditions)

- **Experimental Vignette**

- **Social Distancing Practices**

    - How likely are you to adapt the following behaviors?

- ∗ Avoid visiting friends and family

- ∗ Limit time spent out of your house

- ∗ Wear a face mask

– Feeling thermometer

  - ∗ Democrats

  - ∗ Republicans

  - ∗ Hispanics

  - ∗ Muslims

  - ∗ African Americans

  - ∗ Asians

– Social Exclusion

  - ∗ Democrats

  - ∗ Republicans

  - ∗ Hispanics

  - ∗ Muslims

  - ∗ African Americans

  - ∗ Asians

– Additional Measures of Intergroup Relations – Do you agree that:

  - ∗ Most Muslims in the U.S. can be trusted

  - ∗ Most Muslims in the U.S want to live in peace

  - ∗ Muslim elected officials should be leading congressional committees

– Immigration Questions – Do you agree that:

  - ∗ Muslim Immigrants are generally good for American economy

  - ∗ America's culture is generally harmed by Muslim immigrants

* Muslim immigrants take away jobs from American citizens

– Norms Questions – Do you agree that:

* Most Americans would be willing to receive treatment from Muslim doctors

* Most Americans would be willing to be friends with a Muslim

* Most Americans would be willing to work for a Muslim Boss

* Most Americans support having Muslim elected officials leading congressional committees

– Integration Questions – Do you agree that:

* Most Muslims would like to be friends with non-Muslim Americans

* Most Muslims would like to work for a non-Muslim boss

* Most Muslims would be willing to receive treatment from a non-Muslim doctor

* Most Muslims work hard to integrate into American society

• **Manipulation Checks**

– Do you agree that:

* The impact of COVID-19 on American society will be extremely severe

* In the U.S., Muslim doctors are at the forefront of combatting the Coronavirus

* Over the past few years, some politicians have promoted or supported policy aiming to ban immigration from Muslim countries

C.2   Additional Analyses

C.2.1   Manipulation Checks

In Table S12 I assess the effectiveness of my treatment by leveraging three manipulation check questions. Specifically, to consider the extent to which my treatments impacted respondents' perspectives regarding: i) Muslim representation in healthcare institutions, ii) Crisis severity, and iii)

Please read the following paragraphs carefully:

In the past weeks, the Coronavirus (COVID-19) has hit many countries, causing serious health and economic consequences. Officials from the CDC and NIH emphasize that COVID-19 poses an unprecedented challenge which will have major detrimental effects on the American people's public health and economy. For that reason, they argue that it is extremely important that health institutions in the U.S. seriously prepare for treating patients with COVID-19, for which a cure has yet to be found.

According to the Center for Disease Control and Prevention (CDC), the virus' main symptoms include:

- Fever

- Cough

- Shortness of breath

Official statistics suggest that there are over a million healthcare workers providing services in hospitals and clinics across the United States. Recent studies show that in many localities across the United States, a sizable proportion of healthcare workers are Muslim. Specifically, over 50,000 doctors and many more nurses working in American hospitals are Muslim. These Muslim healthcare workers are taking care of American citizens in urban as well as rural communities all across the United States.

Indeed, doctors, nurses, paramedics, pharmacists, and other healthcare employees are working around the clock to ensure that the American people will successfully and safely prevail over the Coronavirus.

In recent days, many Congress-members have expressed their gratitude towards healthcare workers in the United States, who are working around the clock in order to provide medical services and care for American patients. They further emphasized that they seek to serve the public, by promoting legislation which will aid the American people.

Over that past several years, American elected officials have supported and promoted policies in different areas, relating to: healthcare reform, international trade, and education policy. **/** , education policy, and restrictions on immigration from Muslim countries.

Figure S4: **Experimental Vignette:** 2x2x2 Design, severity treatment depicted in green, representation treatment depicted in blue, and political exclusion treatment depicted in red.

politicians exclusionary preferences towards Muslims, I regressed responses to the last three question in my survey (see section above), over my main treatment indicators (employing the same specification outlined in Equation 2 of the main text).

Table S12: Manipulation Check (US)

	Doctor Role	Severity of Crisis	Muslim Exclusion
	(1)	(2)	(3)
Muslim Treatment	0.279		
	(0.085)		
Severity Treatment		0.017	
		(0.076)	
Exclusion Treatment			0.055
			(0.087)
N	1,176	1,176	1,176

Results from Table S12 suggest that my Muslim representation treatment increased respondents' perception that Muslim healthcare workers are at the forefront of combatting the coronavirus (column 1). However, the null effects in column 2-3 of Table S12, suggest that my severity and political exclusion statement were ineffective in shaping respondents perspectives regarding the severity of the COVID-19 pandemic, and the prevalence of preferences for exclusion amongst politicians.

C.2.2 Balance and Robustness Checks

In Table S13 I report results from simple balance tests, in which I compare the demographics of respondents assigned to my main treatment and control condition relating to Muslim representation. Overall, it appears that respondents' are well balanced across conditions. Regardless, in Figure S5, I demonstrate that my results remain robust when controlling for additional treatment arms and pre-treatment covariates. Lastly, In Figure S6 I consider additional analyses, where I regress attitudes towards a host of social groups (Democrats, Republicans, Muslims, Hispanics, African Americans, and Asian Americans), over my main treatment regarding Muslim representation. Overall, it seems that my treatment mainly impacted prejudice towards Muslims. I do

identify some inconsistent effects on attitudes towards Democrats and Black Americans (which may be associated with Muslim minorities), but no effect on attitudes towards other social groups.

Table S13: Balance on Covariates (US Sample)

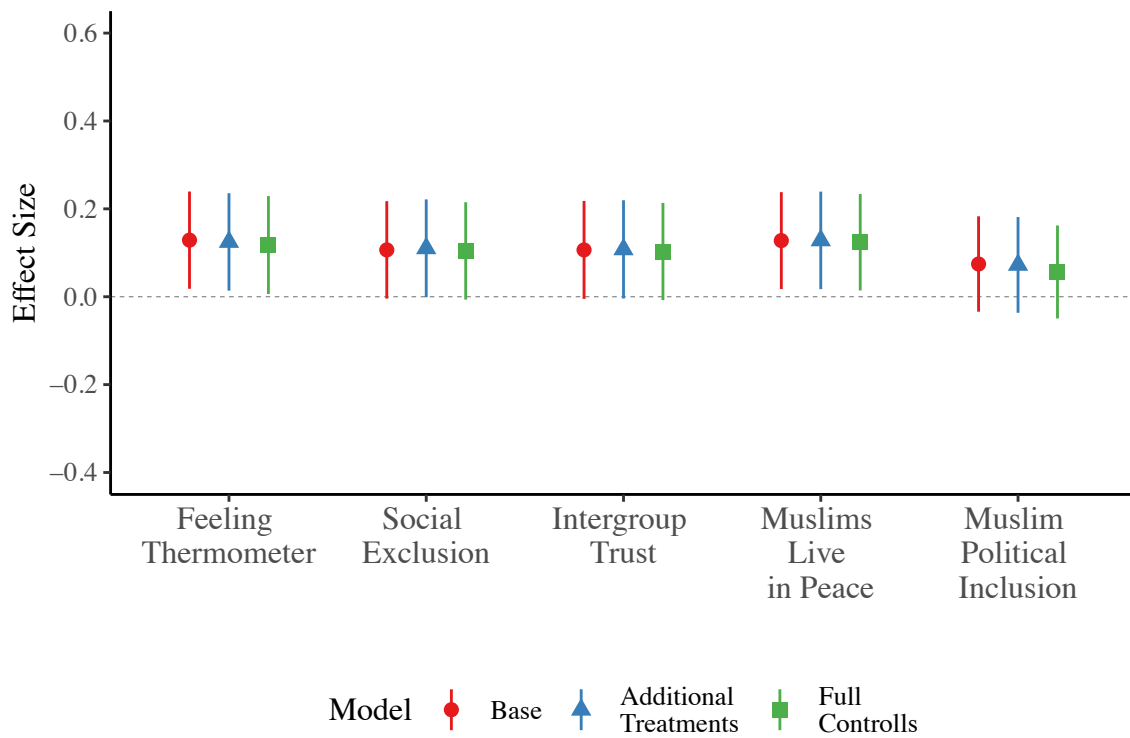|	Variable	N Treatment	N Control	Treatment Mean	Control Mean	p. Value
1	Age	612	599	47.79	48.77	0.30
2	Male	612	599	0.45	0.45	0.90
3	Education	612	599	4.48	4.61	0.23
4	White	611	599	0.76	0.73	0.23
5	Ideology	612	599	1.97	1.92	0.53

Figure S5: **U.S. Experiment Robustness to Alternative Specifications** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Muslim representation on prejudice attitudes and preferences for political exclusion. The full control model include the following covariates: `age`, `race`, `religion`, `education`, and `experimental bloc`.

Lastly, in Table S14, I consider the effects of my main treatment on respondents willingness to abide by public health guidance devised to limit the spread of COVID-19. Specifically, I consider the respondents' likelihood of wearing masks, visiting friends and family, and leaving their home.
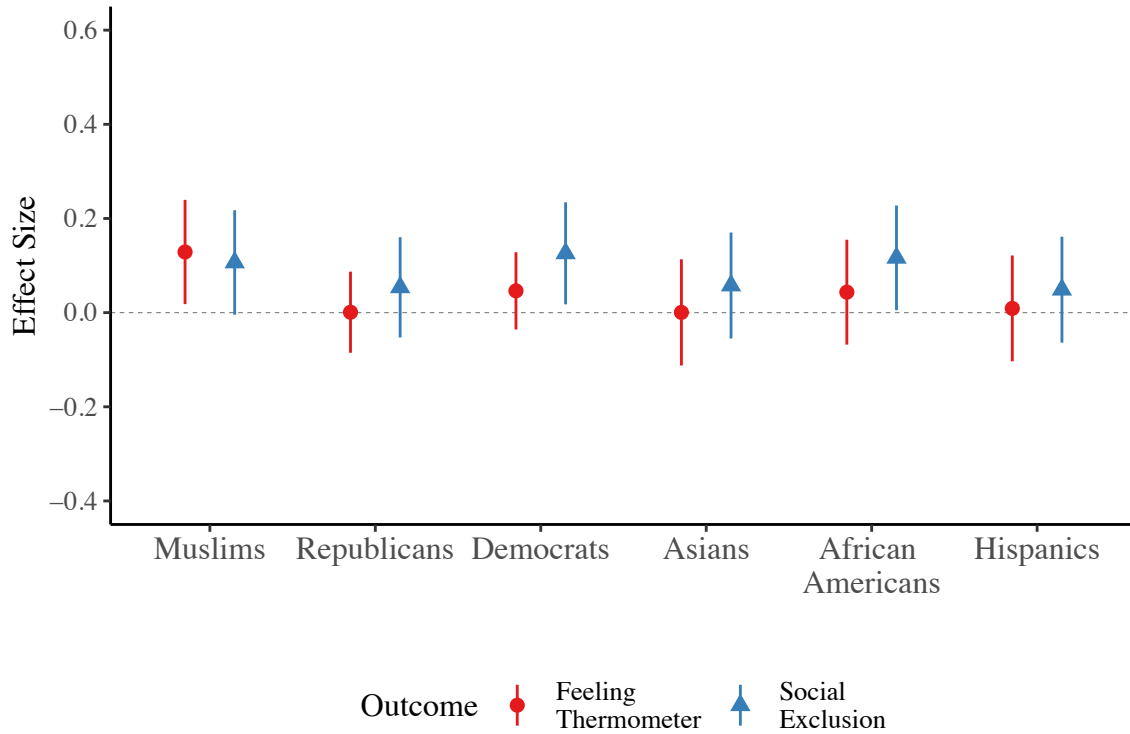
Figure S6: **Treatment Effects on Other Social Groups** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Muslim representation on prejudice attitudes towards Muslims, Republicans, Democrats, Asians, African Americans, and Hispanics

Results from Table S14 suggest that my treatment does not affect these outcomes. Indeed, even when considering heterogenous treatment effects amongst Republican respondents, I still find no evidence for a negative externality of representation on majority group members' social distancing behavior.

C.2.3 Heterogenous Treatment Effects

In this section, I report a series of pre-registered heterogenous treatment effects. Despite finding limited support for the moderating effects of crisis severity and politician's preferences for exclusion in the Israeli context, I further consider these moderators in my U.S. experiment. In Tables S15-S16 I consider these possibilities by interacting my main diversity treatment with a binary variable taking the value of one for respondents primed with the severity or exclusion treatments. As demonstrated in these tables, I find no support for my expectation that the severity of

Table S14: Effect of Muslim Representation on Social Distancing (US)

	Mask		Visit Friends		Leave Home	
	(1)	(2)	(3)	(4)	(5)	(6)
Representation	0.062	0.063	0.026	0.042	−0.012	0.023
	(0.057)	(0.069)	(0.057)	(0.070)	(0.057)	(0.070)
Republican		−0.288		−0.086		−0.248
		(0.117)		(0.119)		(0.118)
Representation*Republican		0.014		−0.033		−0.050
		(0.118)		(0.120)		(0.120)
Controls	No	Yes	No	Yes	No	Yes
N	1,202	1,201	1,202	1,201	1,202	1,201

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

the COVID-19 crisis, or politicians' exclusionary behavior moderates my main average treatment effects. These findings are in line with the null results from my Israeli experiment reported in Tables S4-S5.

Table S15: Heterogenous Treatment Effect of Muslim Representation Conditional on Severity (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.291	0.134	0.161	0.169	0.164
	(0.083)	(0.081)	(0.081)	(0.080)	(0.079)
Severity	0.154	0.040	0.068	0.052	0.064
	(0.082)	(0.081)	(0.081)	(0.080)	(0.079)
Representation*Severity	−0.343	−0.053	−0.105	−0.080	−0.178
	(0.115)	(0.114)	(0.114)	(0.113)	(0.111)
N	1,193	1,188	1,186	1,186	1,186

I further evaluate the extent to which the perceived severity of COVID-19 impacts reactions to my main treatment (i.e. information regarding Muslim representation) in Tables S17-S18. Specifically, I leverage a pre-treatment question in which I presented respondents with a series of health conditions, and asked them to select any condition which they have. The conditions I presented to respondents' were those which the CDC has originally identified as conditions which increase

Table S16: Heterogenous Treatment Effect of Muslim Representation Conditional on Exclusion (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.128	0.112	0.159	0.196	0.123
	(0.079)	(0.079)	(0.079)	(0.079)	(0.077)
Exclusion	−0.055	0.053	0.066	0.073	0.030
	(0.080)	(0.080)	(0.081)	(0.080)	(0.078)
Representation*Exclusion	−0.005	−0.005	−0.105	−0.140	−0.101
	(0.113)	(0.114)	(0.114)	(0.112)	(0.111)
N	1,193	1,188	1,186	1,186	1,186

peoples' risk to suffer severely from COVID-19. Based on responses to this question, I created a binary variable taking the value of one for any respondent suffering from at least one pre-existing condition. In Table S17, I interact my main treatment with this variable and do not find any support for a moderating effect of medical vulnerability on my main treatment.

Table S17: Heterogenous Treatment Effect of Muslim Representation Conditional on Pre-Existing Condition (US)

	Therm	Social Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.083	0.053	0.123	0.119	0.047
	(0.069)	(0.069)	(0.069)	(0.068)	(0.066)
Medical Condition	−0.115	−0.138	0.001	−0.051	0.032
	(0.085)	(0.084)	(0.083)	(0.082)	(0.080)
Representation*Medical Condition	0.095	0.145	−0.062	0.012	0.031
	(0.119)	(0.119)	(0.118)	(0.117)	(0.114)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

In Table S18, I consider a similar exercise focusing on the moderating effects of age, or specifically "vulnerable age" on my main treatment. To do so, I interact my treatment with a binary variable taking the value of 1 for respondents which are 65 or older. Results from Table S18 sug-

gest that being in a vulnerable age group does not moderate the average treatment effect of Muslim representation on prejudice.

Table S18: Heterogenous Treatment Effect of Muslim Representation Conditional on Vulnerable Age (US)

	Therm	Social Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.137	0.121	0.107	0.147	0.059
	(0.064)	(0.063)	(0.063)	(0.062)	(0.061)
65+	0.143	0.094	0.097	0.191	0.048
	(0.120)	(0.119)	(0.118)	(0.117)	(0.114)
Representation*65+	−0.098	−0.083	−0.026	−0.120	−0.013
	(0.140)	(0.139)	(0.138)	(0.136)	(0.133)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

As reported in Tables 3-4 of the main text, I do not find strong evidence that the average treatment effect of Muslim representation is moderated by partisanship. However, to further consider the possibility that partisanship moderates my main effects, I employ a strong-partisan variable taking the value of one for respondents who strongly identify as Democrats or Republicans. I interact this variable with my main treatment, as an alternative approach to identifying the moderating effect of partisanship on my main treatment. Results from Tables S19-S20 are consistent with the patterns reported in the main text. Indeed, it does not appear that strong partisans (either Democrats or Republicans) respond differently to my main treatment.

C.2.4   *Treatment Effects on Perceived Social Norms*

In Figure S7, I report additional pre-registered analyses, which consider treatment effects on a set of perceived social norms. Interestingly, unlike in the Israeli experiment, I identify precisely estimated positive treatment effects on respondents' perceived social norms. This suggests that learning about diversity in healthcare institutions, shapes the extent to which respondents' believe that their fellow Americans are willing to engage with Muslims, and accept them in political life. The different result emerging from the U.S. case, may be driven by social norms of inclusion and

Table S19: Heterogenous Treatment Effect of Muslim Representation Conditional on Party ID – Strong Democrats (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.081	0.091	0.102	0.149	0.016
	(0.065)	(0.064)	(0.064)	(0.063)	(0.062)
Strong Democrat	0.039	−0.076	0.175	0.105	0.097
	(0.117)	(0.116)	(0.115)	(0.114)	(0.111)
Representation*Strong Democrat	0.151	0.057	−0.002	−0.108	0.165
	(0.133)	(0.132)	(0.131)	(0.130)	(0.126)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

Table S20: Heterogenous Treatment Effect of Muslim Representation Conditional on Party ID – Strong Republicans (US)

	Therm	Soc Exclusion	Trust	Peace	Pol Inc
	(1)	(2)	(3)	(4)	(5)
Representation	0.085	0.083	0.064	0.071	0.017
	(0.064)	(0.063)	(0.063)	(0.062)	(0.060)
Strong Republican	−0.238	−0.282	−0.264	−0.337	−0.368
	(0.125)	(0.124)	(0.123)	(0.122)	(0.119)
Representation*Strong Republican	0.162	0.114	0.193	0.262	0.205
	(0.137)	(0.136)	(0.135)	(0.134)	(0.130)
Controls	Yes	Yes	Yes	Yes	Yes
N	1,192	1,187	1,185	1,185	1,185

Notes: Controls include: age, race, religion, education, block, and indicators for exclusion and severity treatments.

exclusion which are less salient in the U.S., and thus easier to affect with a light-touch treatment.
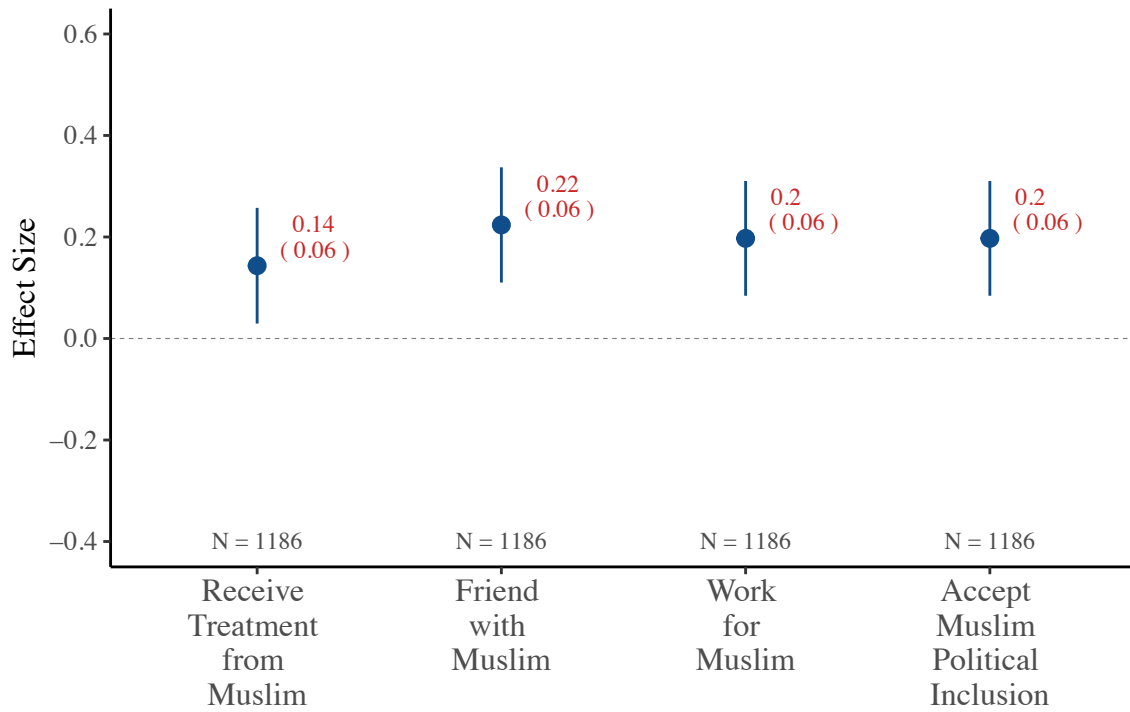

Figure S7: **Treatment Effects on Perceived Social Norms** - OLS point estimates and their corresponding confidence intervals represent the average treatment effect of information regarding Muslim representation on perceived social norms. Each outcome measure asks respondents whether most Americans would engage in a given behavior.