

Reducing Sexual Orientation Discrimination: Experimental Evidence from Basic Information Treatments

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Abstract

We study basic information treatments regarding sexual orientation using randomized experiments in three countries with strong and widespread anti-gay attitudes: Serbia, Turkey, and Ukraine. Participants who received information about the economic costs to society of sexual orientation discrimination were significantly more likely than those in a control group to support equal employment opportunities based on sexual orientation. Information that the World Health Organization (WHO) does not regard homosexuality as a mental illness increased social acceptance of sexual minorities, but only for those who reported trust in the WHO. Our results have important implications for policymakers aiming to expand the rights of lesbian, gay, and bisexual people worldwide. © 2022 The Authors. Journal of Policy Analysis and Management published by Wiley Periodicals LLC on behalf of Association for Public Policy and Management.

INTRODUCTION

Recent advances in rights for lesbians, gay men, and bisexual (LGB) individuals have varied substantially across different parts of the world.¹ In the United States, for example, LGB rights have increased at a rapid pace in the past few decades: same-sex sexual activity was decriminalized in 2003, full legal access to same-sex marriage was granted in 2015, and nationwide employment discrimination protections on the basis of sexual orientation were granted in 2020, all through landmark U.S. Supreme Court rulings.² Outside the United States, India decriminalized same-sex sexual acts in 2018, and Taiwan granted legalization to same-sex marriage in 2019. In other parts of the world, however, LGB rights advanced more slowly or not at all. As of 2019, 70 United Nations member states (35 percent of all members) criminalize same-sex sexual conduct; in six UN member states, same-sex sexual activity is

¹ The same is true for transgender rights, but our focus in this paper will be on sexual (as opposed to gender) minority populations. We will sometimes refer to “LGBT” in the context of policy environments for and attitudes toward all sexual and gender minorities (lesbians, gay men, bisexual individuals, and transgender people) but in practice our study setting focuses only on sexual minorities (i.e., LGB people).

² Scholars have studied the effects of these and other LGBT-related policy reforms on outcomes. See, for example, Buchmueller and Carpenter (2012), Carpenter et al. (2021), and Delhomme and Hamermesh (2021) on the effects of legal relationship recognition.

punishable by death. Anti-LGB attitudes are particularly strong in Africa, the Middle East, and Eastern Europe, and anti-LGB policies have recently been adopted in Hungary, Poland, Russia, Tanzania, and Uganda (Mendos, 2019).

This paper provides new evidence on the determinants of support for sexual minorities in Serbia, Turkey, and Ukraine—countries with strong negative attitudes toward LGB people. These countries have highly restrictive LGBT equality laws and policies, scoring just 37, 4, and 19, respectively, on a scale where zero indicates *gross* human rights violations and 100 represents the greatest degree of equality under the law (ILGA-Europe, 2022). They also have some of the lowest rates of social acceptance of sexual minorities in Europe. Appendix Table A1 shows the share of respondents across 33 countries in the European Social Survey who agreed with the statement that gay men and lesbians should be free to live their lives as they wish.³ Serbia, Ukraine, and Turkey have the 29th, 30th, and 31st lowest shares of agreement, respectively (only respondents in Russia and Lithuania expressed more negative attitudes toward sexual minorities). In 2014, Amnesty International identified “a marked lack of will to tackle homophobia and transphobia” in Serbia, due to the frequent ban of pride marches by public authorities based on violent threats from homophobic groups (Amnesty International, 2014). Turkey’s President Erdogan has turned increasingly against granting LGB people equal rights over the course of his presidency and has repeatedly attacked the LGBT community via Twitter, while censoring gay TV characters (Beswick, 2020). In Ukraine, right-wing nationalist groups regularly disrupt LGBT events and publish electronic petitions that refer to adoption by same-sex couples as an “act of violence” against children.

What is the source of anti-gay sentiment in these countries, and can simple information treatments, at least to some extent, increase tolerance? We designed our experiment to test various underlying theories. First, we are interested in whether rational economic self-interest might overcome personal distaste for LGB people. Thus, in one arm of our experiment we inform people about the direct economic costs to their country from discrimination against sexual minorities. We hypothesize that this information might induce some self-interested individuals to set aside negative personal views to support LGB employment non-discrimination. Second, we are interested in understanding whether narratives about homosexuality being a mental illness might drive anti-gay sentiment. Thus, in another arm of our experiment we try to debunk this myth by informing individuals that the World Health Organization (WHO) does not consider homosexuality to be a mental disease. We hypothesize this information induces more favorable social views about homosexuality. We test these hypotheses through a controlled survey experiment where a third of respondents receive the “discrimination cost” information, a third of respondents receive the “myth debunking” information, and a third of respondents receive information unrelated to LGB people.

Our experiment yields four main results. First, we find that providing information about the economic cost of sexual orientation discrimination significantly increases support for measures to safeguard equal employment opportunities regardless of someone’s sexual orientation. Individuals who receive the discrimination cost treatment were 1.49 times more likely to support such equal opportunities compared with individuals randomly assigned to the control group. Second, we find this discrimination cost treatment spills over to support for equal employment opportunities based on ethnic origin, religious beliefs, nationality, gender, and disability. Each of the discrimination cost treatment effects in these other domains is quantitatively smaller than the effect for sexual orientation-based employment equality, but they

³ All appendices are available at the end of this article as it appears in JPAM online. Go to the publisher’s website and use the search engine to locate the article at <http://onlinelibrary.wiley.com>.

are all statistically significant. Third, the effect of the discrimination cost treatment does not spill over to LGB support in other aspects of life. After adjusting for false discovery rates, there are no effects on opinions about the moral acceptability and justifiability of homosexuality, whether sexual minorities should be able to live their lives freely, and whether sexual minorities bring shame on their families.

Fourth, we find that providing information that the WHO has stated that homosexuality is not a mental disease does not have a significant effect on support for equal employment opportunities but *is* associated with improved attitudes about sexual minorities in non-economic aspects of life. Specifically, the myth debunking treatment increases support regarding the moral acceptability and justifiability of homosexuality and that sexual minorities should be able to live their lives freely. It also reduces the likelihood that individuals report that a gay or lesbian relative would bring shame on their family. These effects of the myth debunking treatment on statements about the moral acceptability of homosexuality are also consistently smaller than the effects of the discrimination cost treatment on labor market discrimination views.

Our results have two important implications for the expansion of LGB rights, particularly in parts of the world where anti-LGB attitudes are widely held and deeply ingrained. First, they clearly suggest that individuals in countries with strong views about the immorality of homosexuality can—when informed about the economic costs of sexual orientation discrimination—still voice support for non-discrimination policies. This indicates that advances in LGB rights in socially conservative places may be more effective if they appeal to the economic costs of anti-LGB discrimination instead of trying to change the underlying LGB-related views themselves. Second, our results also indicate that even views about the acceptability of homosexuality itself can be modestly affected by the provision of basic information, particularly when framed in the context of institutions that people trust.

RELATED LITERATURE AND CONTRIBUTIONS

Our paper contributes to three strands of the literature. First, we relate to the literature on taste-based labor market discrimination going back to Becker (1957). A key insight from that literature is that taste-based discrimination—whether by employers, co-workers, or clients—entails costs as unfettered prejudice means that hiring and promotion decisions are not solely based on whether an employee is the best fit for the job. A rich empirical literature has investigated the incidence and mechanisms of race and gender discrimination in the labor market.⁴ Recent papers build on this work to assess discrimination against LGB individuals (Aksoy et al., 2018, 2019; Badgett, 1995). This literature, summarized in Badgett et al. (2021), generally finds that gay men earn lower wages than otherwise similar heterosexual men.⁵ A related literature demonstrates that anti-discrimination policies can help reduce the labor market penalty experienced by gay men (Burn, 2018; Delhommer, 2021; Klawitter, 2011; Klawitter & Flatt, 1998). Further evidence on discrimination is provided by audit studies of fictitious resumes sent to employers, which find that gay candidates receive fewer callbacks for interviews than heterosexual candidates (Tilcsik, 2011).⁶ Coffman et al. (2017) use online experiments to show that anti-gay

⁴ Altonji and Blank (1999) provide an early overview and discussion of this literature.

⁵ Studies have also examined LGB discrimination in other settings, such as adoptions (Mackenzie-Liu et al., 2020).

⁶ Field experiments on housing discrimination confirm the presence of pervasive differential treatment of individuals presumed to be sexual minorities. For example, Koehler et al. (2018) conduct a “mystery shopper” rental housing study in Serbia where individuals sought apartments and differed only in whether they were in a same-sex or different-sex relationship.

discrimination in the U.S. workplace, of both openly gay managers and of job applicants, may be more common than thought. Aksoy et al. (2021) find in a lab experiment setting that women are less likely to signal a minority sexual orientation when they anticipate payoff-relevant discrimination. Our research contributes to this work by advancing our understanding of the nature of support (or lack of support) for equal employment opportunities based on sexual orientation in environments hostile to LGB people.

Second, we contribute to a broader literature on the determinants of social attitudes toward sexual and gender-minority people. For example, several studies examine the effects of the legal recognition of same-sex relationships. Some papers find that expanded legal recognition of same-sex relationships improved attitudes toward sexual minorities in the U.S. (Flores & Barclay, 2016; Sansone, 2019) and in Europe (Abou-Chadi & Finnigan, 2019; Aksoy et al., 2020). Others find evidence of backlash effects, particularly when judicial institutions adopt same-sex relationship recognition (Ofosu et al., 2019). Other research has demonstrated that face-to-face interviews by door-to-door canvassers can durably reduce transphobia (Broockman & Kalla, 2016). We contribute to this literature by studying whether simple information treatments in an online experiment can garner support for LGB people and for policies that promote equal employment opportunities based on sexual orientation.

Third, our work relates to an experimental literature that studies whether information treatments—which randomly vary the information set available to respondents—influence attitudes toward minority groups (Haaland et al., in press). For example, Haaland and Roth (2020) show that when survey respondents receive information on the lack of adverse labor market effects of immigration, they become more supportive of immigration. Bursztyn et al. (2020) demonstrate that randomly informing subjects about the underlying motivations for anti-immigrant activities causes improved views toward the individuals engaging in such activities. Hopkins et al. (2019) show that when survey respondents are randomly assigned to receive information about the true size of the foreign-born population in the United States (which most people significantly overestimate), they report more accurate guesses about the size of this population but do not report improved attitudes toward immigration. Haaland and Roth (in press) show that randomly assigned information treatments about the extent of racial discrimination in hiring led to convergence in beliefs about discrimination across the political spectrum but had no effects on support for pro-Black policies. In the context of LGB rights, Suhay and Garretson (2018) find that experimentally exposing respondents to scientific information on the origins of sexual orientation (i.e., whether homosexuality is a choice) did not affect support for gay rights in the United States. Harrison and Michelson (2017), in contrast, show that experimentally providing subjects with information that a leader in a group with a shared identity to the respondent supports LGB rights, significantly increases support from members of that group. Although we also focus on views toward LGB people and LGB rights, our contribution is to contrast explicitly the impact of different information treatments and to do so for a set of developing countries with strong and deeply engrained anti-LGB sentiments.

SURVEY AND EXPERIMENTAL DESIGN

Data Collection and Survey Structure

We conducted our surveys in three developing countries—Serbia, Turkey, and Ukraine—in early August 2020. The online survey was designed and distributed via the survey company Respondi. The samples were representative at the country-level regarding age and gender. Our final sample is more educated and more urban than the overall population. The reason for this is that, as with most online surveys, access

to the internet, internet speed, and other factors commonly result in samples that are more educated and more urban than the underlying population; this does not affect the internal validity of our experiment. Our main contribution is to provide new and internally valid evidence on LGB attitudes for countries not previously studied in this way. This offers an interesting counterpoint to most of the existing literature, which has studied the United States, the United Kingdom, and other Western European countries. Because of the limitations regarding representativeness, however, we do not make claims about generalizability.

Once respondents agreed to participate in the survey, we directed them to the consent page and asked them initial screening questions to ensure that the quotas for age and gender would be met. All respondents in our sample completed the survey and received a baseline remuneration of about two euros.

The survey consisted of four sections: (a) initial screening questions on sociodemographic characteristics; (b) a random split of respondents into three groups (two treatment groups and one control group); (c) attitudinal outcome questions (e.g., views about discrimination on the basis of race, gender, age, religion, or sexual orientation and views about justifiability of homosexuality); and (d) other questions relating to political orientation, urban/rural status, religion, labor market status, and trust in institutions (own government, own parliament, EU, WHO, UN and IMF/World Bank).⁷ We designed the original questionnaire in English. Professional translators then translated it into the major conversational language of each country.⁸

Our final sample includes about 6,600 respondents (2,200 per country) between 18 and 70 years of age. To safeguard data integrity, we kept track of the time spent by the respondent on the entire survey. This allows us to identify respondents who were exceptionally quick or slow in completing the survey.⁹ Throughout the survey, we randomized answer options to prevent order bias, which might arise when all respondents view the answer options in the same order.

Experimental Design and the Information Treatments

After the screening questions on sociodemographic characteristics, we randomly assigned respondents in each country to one of three subsamples. We call the first subsample the discrimination cost treatment group (T1), the second is the myth debunking treatment group (T2) and the third is the control group. All subsamples contain about 700 to 800 survey respondents per country. We pre-registered the experimental protocol with the American Economic Association (AECTR-0006189).

In the discrimination cost treatment (T1), we provided individuals with information about how much per capita income in their country could change in the medium term if policy makers would adopt non-discriminatory policies based on sexual orientation. Before receiving this information, we asked respondents in this treatment arm how much they themselves thought that per capita income could change

⁷ In addition to our experiment, respondents in Serbia and Turkey also took part in a series of trust and dictator games after our information treatments but before the questions about support for nondiscrimination and views about social and moral issues. This is useful context to keep in mind for interpreting our results. As we discuss below, we find results in Ukraine (where no one played the trust and dictator games and thus the outcome questions were asked immediately after we delivered the information treatments) as well as in the other countries where there was some time lag in between the delivery of the information treatments and the outcome questions. The questionnaire is provided in Appendix B.

⁸ All surveys in Ukraine were translated into Ukrainian.

⁹ As a robustness check, we drop respondents in the bottom and top five percent of the survey time distribution and find that our results remain robust (reported in Appendix Tables A8 and A9).

because of such policies.¹⁰ We hypothesize that informing people about the economic costs of sexual orientation discrimination will increase their support for equal employment opportunities based on sexual orientation.

In the Myth Debunking Treatment (T2), we informed individuals that the WHO does not consider homosexuality to be a mental disease (Cochrane et al., 2014). Before receiving this information, respondents in this treatment arm were asked whether they themselves agree with the statement that “homosexuality is a mental disease.”¹¹ To the extent that opinions concerning equal employment opportunities are determined by economic considerations, we would not expect T2 to have an impact on people’s attitudes toward employment discrimination with respect to sexual orientation. Instead, addressing people’s beliefs about the nature of homosexuality may make them more accepting in non-economic domains of life.¹²

Last, in the control group, we informed respondents about a neutral fact that should in principle not influence any of their later answers in the survey. Specifically, we asked individuals to guess their country’s population density, and we subsequently informed them about the correct answer. We always designed the basic information treatments to be short and neutrally framed, as suggested by Haaland et al. (in press).¹³

Outcome Measures

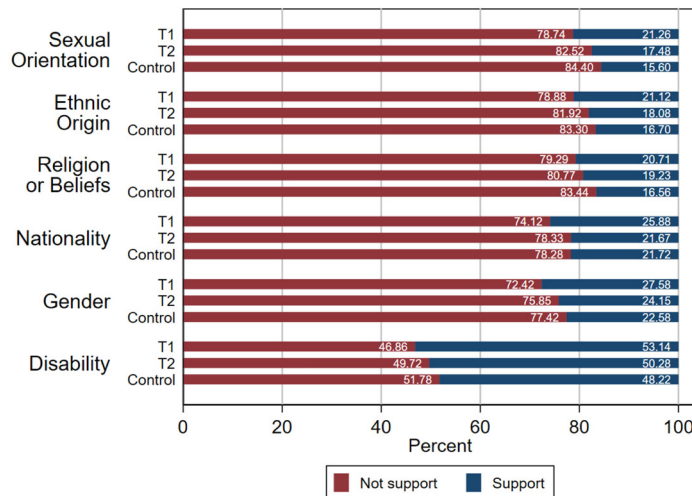
Our primary analysis gauges support for equal opportunity in employment based on sexual orientation and other characteristics. Specifically, respondents received the following introductory question: “Would you be in favor of or opposed to specific

¹⁰ Specifically, the question read: “Discrimination on the basis of sexual orientation (that is, being homosexual, gay, lesbian, or bisexual) can affect the incomes of individuals, businesses, and countries. By how much do you think the average per capita annual income in your country could change in the medium term if your country would adopt non-discriminatory policies on the basis of sexual orientation?” Response options were: decrease of more than \$1,000; decrease of between \$1,000–\$500; decrease of between \$500–\$100; Decrease of less than \$100; no change; increase of less than \$100; increase of between \$100–\$500; increase of between \$500–\$1,000; increase of more than \$1,000; Prefer not to say; I don’t know. The treatment read: “Research conducted in economics shows that protection against discrimination on the basis of sexual orientation is related to an increase in average per capita annual income of \$320 in the medium term.” We used estimates of these per capita income changes from Badgett et al. (2019).

¹¹ Specifically, the question read: “Homosexuality is having attraction to and/or preferring sexual relations with people of the same sex. To what extent do you agree or disagree with the following statement: ‘Homosexuality is a mental disease.’” Response options were: Agree strongly; Agree; Neither agree nor disagree; Disagree; Disagree strongly; Prefer not to say; I don’t know. The treatment read: “The World Health Organization and a large body of scientific evidence have stated that homosexuality is not a mental disorder.”

¹² However, the expected impact could be ambiguous in case some people who (incorrectly) believe that homosexuality is a mental disease might in fact hold relatively positive views about homosexuality because they consider diseases to be largely outside of a person’s control. Informing them that homosexuality is, in fact, not a mental disease could then lead these individuals to worsen their view of LGB people as they now hold them accountable for their sexual orientation.

¹³ We considered eliciting prior beliefs regarding the economic costs of LGB discrimination and about whether homosexuality is a mental disease from all study participants instead of just those in the T1 and T2 treatment arms, respectively. We decided against this to avoid concerns about priming and demand effects (e.g., simply asking people in the control group about their beliefs regarding sexual minorities may have affected their responses about LGB issues). A consequence of this choice is that we can only compare average treatment effects of T1 and T2 across the study population; we cannot explore questions such as whether the effect of T1 on support for LGB nondiscrimination exhibited heterogeneity across the distribution of prior beliefs regarding the economic costs of LGB discrimination. Similarly, we cannot explore whether the effect of T2 on social views about LGB people exhibited heterogeneity across the distribution of prior beliefs regarding whether homosexuality is a mental disease. These are interesting questions for future work.



Notes: The figure shows the distribution of responses across treatment and control groups. T1: Discrimination cost treatment. T2: Myth debunking treatment.

Figure 1. Information Treatments on Support for Equal Employment Opportunities.

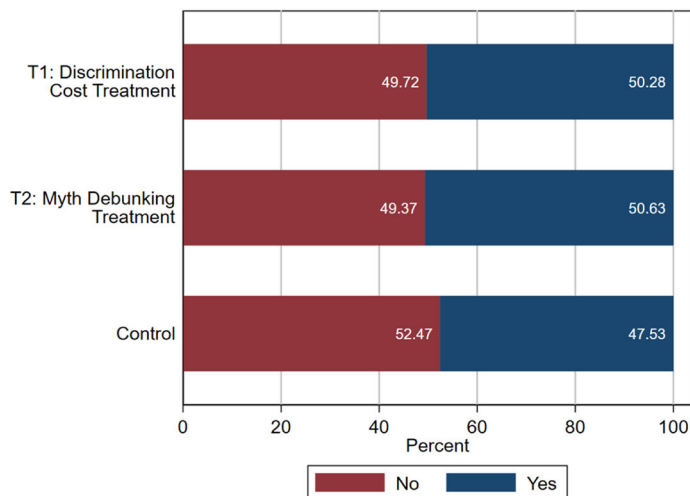
[Color figure can be viewed at wileyonlinelibrary.com]

measures being adopted to provide equal opportunities for everyone in the field of employment? Specific measures for people depending on their....” Individuals were then asked to respond separately regarding sexual orientation, ethnic origin, religion or beliefs, nationality, gender, and disability. We randomized the order in which we presented these demographic groups across participants.¹⁴ Individuals were coded as ‘1’ if they were for equal opportunity measures for the group in question.

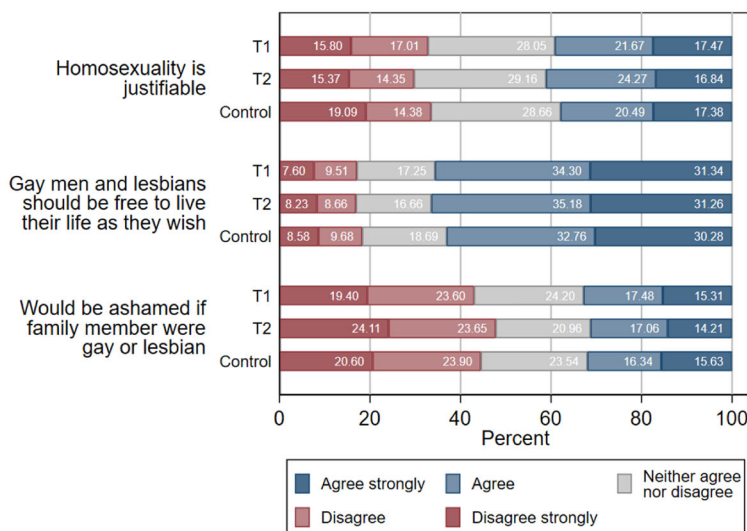
We asked a range of questions about LGB-related views in other aspects of life and define indicator variables accordingly. Respondents were asked whether they believed that homosexual acts are morally acceptable (1 = *yes*); whether they agreed that homosexuality is justifiable (1 = *agree or strongly agree*); whether they agreed that gay men and lesbians should be free to live their own life as they wish (1 = *agree or strongly agree*); and whether, if a close family member was gay or lesbian, the respondent would feel ashamed (1 = *disagree or strongly disagree*). To ease interpretation, we code outcomes so the indicator is 1 if the respondent reports more favorable LGB views. Figures 1 and 2 show the raw distribution of responses across treatment and control groups.

In addition to LGB-related opinions, we elicited attitudes toward other minorities (for instance immigrants and people of other races or religions) and ways of life (converting to other religions, having children out of wedlock, use of contraceptives, etc.). These questions serve as placebo outcomes, because we would not expect them to be affected by our information treatments.

¹⁴ We do not know the specific order in which each respondent saw each demographic characteristic, so we cannot control for this order in the regression models. We know, however, that the order in which the characteristics appeared to each respondent was random.



Panel (a) Do you personally believe that homosexual acts are morally acceptable?



Panel (b) Agree or disagree outcomes

Notes: The figures show the distribution of responses across treatment and control groups.

Figure 2(a)–(b). Information Treatments on LGB-Related Views in Non-Economic Domains.

[Color figure can be viewed at wileyonlinelibrary.com]

EMPIRICAL STRATEGY

We used the randomized, controlled nature of the experiment to estimate logistic regression models relating the treatments to outcomes. These models take the form expressed in equation (1):

$$Y_{ic} = \beta_0 + \text{LGB Information Treatments}_{ic}\beta_1 + X_{ic}\beta_2 + \varepsilon_{ic} \quad (1)$$

where Y_{ic} is a variable indicating positive attitudes toward sexual minorities for individual i in country c . We predict the odds of positive attitudes toward sexual minorities using indicator variables for the two information treatment arms and using the placebo condition as the excluded category.¹⁵ To increase precision, we also control for a vector of individual demographic characteristics, X_{ic} , which includes: age and its square, a male dummy, dummy variables for secondary and tertiary education, a dummy for being in any kind of partnership, a dummy for living in an urban area, survey date dummies, survey country dummies, number of adults above and below 65 in the household, religion dummies (Christianity, Islam, Judaism, Secular/no religion/atheist, or other), and labor market-related controls (foreign firm or international organization, and an unemployed dummy).¹⁶ Because we randomize at the individual level there is no need to cluster our (heteroskedasticity-robust) standard errors.

RESULTS

Balance Tests and Descriptive Characteristics

Table 1 presents the balance test for our sample overall and across the three experimental arms (discrimination cost treatment, myth debunking treatment, and the control group with a placebo treatment).

The sample is, by construction, approximately evenly split across the three countries as well as the three experimental arms. Columns (5) and (6) show that randomization was successful and that the three arms are well-balanced along many characteristics. Respondents were on average 35 years old, 46.6 percent female, 63 percent had a tertiary education, 85 percent were religious, and 74.4 percent lived in urban areas.¹⁷ Figures 1 and 2 show the distribution of responses across treatment and control groups.

Baseline Results

We present the main results on support for equal employment opportunities in Table 2. We report adjusted odd ratios based on the coefficients on the indicators for being in the discrimination cost treatment or in the myth debunking treatment. The models include all the other controls from equation (1). We report robust standard errors in parentheses below the adjusted odds ratios. We check whether our inference is robust to corrections that account for the testing of multiple hypotheses by adjusting the p -values using the “sharpened q -value approach” (Anderson, 2008) and report them in brackets.¹⁸ We also report the p -value on the test of equality of the T1 and T2 coefficient estimates.

The results in Table 2 provide strong evidence that informing people about the costs of sexual orientation discrimination causes sizable and statistically significant

¹⁵ In Appendix A, we show that our results are robust to using OLS models for dichotomous outcomes and to using ordered logit models for the social views outcomes.

¹⁶ We considered asking questions about the respondent's own sexual orientation, but the survey company recommended against including it. Given the size of our experiment, we also decided it would be unlikely that we would identify enough sexual minorities for meaningful heterogeneity analyses.

¹⁷ Appendix Table A2 provides further support that the randomization was successful. When we separately regress indicators for being in each of the two information treatment arms on observable characteristics, only 2 out of 38 coefficients are statistically significant at the 1 percent level, indicating that almost all respondent characteristics are uncorrelated with treatment assignment.

¹⁸ In terms of interpretation, for example, a q -value of 1 percent means that 1 percent of significant results will reflect false positives.

Table 1. Descriptive statistics and treatment-control balance.

	(1) Full sample	(2) Discrimination cost treatment	(3) Myth debunking treatment	(4) Control condition/placebo treatment	(5) <i>p</i> -value for equality of column 2 vs. column 4	(6) <i>p</i> -value for equality of column 3 vs. column 4
Serbia	0.349 (0.006)	0.347 (0.010)	0.351 (0.010)	0.349 (0.010)	.902	.867
Turkey	0.319 (0.006)	0.316 (0.010)	0.321 (0.010)	0.320 (0.010)	.767	.928
Ukraine	0.332 (0.006)	0.338 (0.010)	0.328 (0.010)	0.332 (0.010)	.677	.795
Age	35.021 (0.153)	34.922 (0.262)	35.027 (0.265)	35.112 (0.268)	.612	.821
N° adults below 65	2.450 (0.015)	2.464 (0.027)	2.453 (0.028)	2.432 (0.026)	.404	.596
Secondary	0.251 (0.006)	0.259 (0.010)	0.263 (0.010)	0.250 (0.010)	.512	.661
Tertiary	0.630 (0.006)	0.611 (0.010)	0.642 (0.010)	0.637 (0.010)	.073*	.764
Female	0.466 (0.006)	0.467 (0.011)	0.459 (0.011)	0.472 (0.011)	.744	.385
Urban	0.744 (0.005)	0.743 (0.009)	0.753 (0.009)	0.737 (0.009)	.663	.233
Religious	0.850 (0.004)	0.852 (0.008)	0.851 (0.008)	0.848 (0.008)	.746	.823
Christian	0.549 (0.006)	0.550 (0.011)	0.547 (0.011)	0.550 (0.011)	.999	.852
Muslim	0.268 (0.005)	0.267 (0.009)	0.272 (0.010)	0.265 (0.009)	.854	.566
Single	0.448 (0.006)	0.445 (0.011)	0.445 (0.011)	0.456 (0.011)	.467	.467

Table 1. (Continued).

	(1) Full sample	(2) Discrimination cost treatment	(3) Myth debunking treatment	(4) Control condition/placebo treatment	(5) <i>p</i> -value for equality of column 2 vs. column 4	(6) <i>p</i> -value for equality of column 3 vs. column 4
Working for foreign firm	0.189 (0.003)	0.193 (0.005)	0.197 (0.006)	0.191 (0.005)	.889	.345
On temporary leave (Feb.)	0.065 (0.003)	0.061 (0.005)	0.071 (0.006)	0.062 (0.005)	.878	.243
Unemployed and looking for a job (Feb.)	0.135 (0.004)	0.139 (0.007)	0.135 (0.007)	0.130 (0.007)	.420	.675
On temporary leave (July)	0.052 (0.003)	0.049 (0.005)	0.056 (0.005)	0.051 (0.005)	.751	.460
Political views	5.056 (0.030)	5.013 (0.052)	5.116 (0.052)	5.039 (0.051)	.722	.290
Hh. gross income decile (July 2020)	5.451 (0.035)	5.398 (0.061)	5.509 (0.062)	5.445 (0.061)	.586	.466
<i>N</i>	6,549	2,183	2,174	2,192		

Notes: Standard errors in parentheses. * $p < .10$

Table 2. Effects of information treatments on support for equal employment opportunities.

Support for Equal Employment Opportunities on the Basis of →	(1) Sexual orientation	(2) Ethnic origin	(3) Religion or beliefs	(4) Nationality	(5) Gender	(6) Disability
Discrimination cost treatment (T1)	1.487*** (0.1197) [0.0010]	1.350*** (0.1067) [0.0020]	1.341*** (0.1085) [0.0040]	1.276*** (0.0921) [0.0100]	1.339*** (0.0959) [0.0010]	1.255*** (0.0797) [0.0020]
Myth debunking treatment (T2)	1.137 (0.0949) [0.2970]	1.094 (0.0891) [0.6820]	1.203*** (0.0981) [0.0420]	0.994 (0.0739) [1.0000]	1.093 (0.0795) [0.2660]	1.077 (0.0684) [0.1010]
p-value on test of equality of treatments	.0006	.0071	.1689	.0006	.0040	.0152
Mean of outcome in placebo group	15.6	16.7	16.6	21.7	22.6	48.2
N	6,547	6,532	6,532	6,547	6,549	6,549

Notes: Odds ratios are displayed. * $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors robust to heteroscedasticity are reported in parentheses. All models include controls for: age and its square; a male dummy, dummy variables for tertiary education, secondary education; a dummy for being in any kind of partnership; a dummy variable for living in an urban area; survey date dummies; survey country dummies; number of adults above and below 65; religion dummies (Catholic, Muslim, Orthodox, other religion) and labor market-related controls (whether individual works at a state-owned enterprise, foreign firm or international organization, and unemployed dummy). Sharpened q-values, which report the expected “positive false discovery rate” obtained by rejecting the null hypothesis for any result with an equal or smaller q-value, are reported in brackets.

increases in the likelihood of support for equal employment opportunities based on sexual orientation (column 1). Individuals in the discrimination cost treatment arm have 1.49 times increased odds of voicing agreement with the sexual orientation-based equal opportunities statement. This estimate remains statistically significant at the one percent level after accounting for multiple hypothesis testing. Importantly, we find no impact of the myth debunking treatment on agreement with the statement about equal employment opportunities based on sexual orientation.¹⁹ This suggests that a simple information treatment to de-bias individuals was not effective at increasing their support for sexual orientation-based non-discrimination policies. The *p*-value on the test of equality of the T1 and T2 coefficients confirms that they are significantly different from each other.

Interestingly, we also find spillover effects from the Discrimination Cost Treatment to support for measures to promote equal opportunity based on ethnic origin, religion or beliefs, nationality, gender, and disability. In each case, we find statistically significant increases in the likelihood of voicing support for such measures, though all adjusted odds ratios are smaller (that is, closer to 1) than the associated estimate for equal opportunity based on sexual orientation.²⁰ One interpretation of this pattern is that informing individuals about the costs of sexual orientation discrimination makes them aware of the cost of labor discrimination generally and thus has spillover effects to support for equal employment opportunities for other minority groups as well.

Table 3 presents estimates of the effect of our treatments on responses to general questions about homosexuality, not in the context of employment or economic opportunities. Each column reflects a separate regression, and we control for all our standard covariates throughout (these coefficients are not shown but are available upon request). The results in Table 3 suggest that informing people about the economic costs of sexual orientation discrimination—despite increasing support for equal employment opportunity based on sexual orientation—had no meaningful impact on a range of other LGB-related views in non-economic domains of life. Specifically, we estimate that the discrimination cost treatment did not affect the likelihood of agreement with the statement that homosexual acts are morally acceptable (column 1), that homosexuality is justifiable (column 2), or that gay men and lesbians should be free to live their lives as they wish (column 3). The discrimination cost treatment also did not influence the likelihood that the respondent disagreed with the statement that they would be ashamed if a family member were gay or lesbian (column 4).

In contrast, the myth debunking treatment T2 positively affected LGB support in all these non-economic realms. Column 5 shows that the effect of T2 on broad views about homosexuality is robust to considering the first principal component of these four outcomes.²¹ These effects of T2 on social views are smaller than the effects of T1 on labor market discrimination views, however, and we cannot reject that the coefficients on T1 and T2 for the social views outcomes are equal except for the “ashamed if a family member were gay or lesbian” outcome in column 4.

¹⁹ There is evidence in column 3 of Table 2 that the myth debunking treatment was related to a significantly higher likelihood of support for equal employment opportunities on the basis of religion, but it is the only characteristic (out of six) where this relationship holds.

²⁰ As noted above, we randomized the order in which we presented the demographic groups across participants.

²¹ Principal component analysis allows us to summarize the information content in several variables by means of creating a “summary index” that can be more easily analyzed. More specifically, it is a technique for reducing the dimensionality while increasing interpretability and minimizing information loss.

Table 3. Effects of information treatments on LGB-related views in non-economic domains.

	(1) Homosexual acts are morally acceptable	(2) Agree or strongly agree that homosexuality is justifiable	(3) Agree or strongly agree that gay men and lesbians should be free to live their life as they wish	(4) Disagree or strongly disagree that would be ashamed if family member were gay or lesbian	(5) The first principal component of the four outcomes
Discrimination cost treatment (T1)	1.152 (0.0827) [0.1110]	1.072 (0.0732) [0.5870]	1.137 (0.0765) [0.1170]	0.953 (0.0646) [0.6150]	1.128 (0.0740) [0.1880]
Myth debunking treatment (T2)	1.160* (0.0833) [0.0620]	1.170* (0.0801) [0.0540]	1.179** (0.0794) [0.0400]	1.164* (0.0784) [0.0610]	1.170** (0.0767) [0.0380]
p-value on test of equality of treatments	.9201	.2023	.5933	.0032	.5669
Mean of outcome in placebo group	47.5	37.9	63.1	44.5	—
N	5,237	5,936	6,277	5,918	4,664

Notes: Odds ratios are displayed. * $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors robust to heteroscedasticity are reported in parentheses. All models include controls for: age and its square; a male dummy, dummy variables for tertiary education, secondary education; a dummy for being in any kind of partnership; a dummy variable for living in an urban area; survey date dummies; survey country dummies; number of adults above and below 65; religion dummies (Catholic, Muslim, Orthodox, other religion) and labor market-related controls (whether individual works at a state-owned enterprise, foreign firm or international organization, and unemployed dummy). Sharpened q-values, which report the expected “positive false discovery rate” obtained by rejecting the null hypothesis for any result with an equal or smaller q-value, are reported in brackets.

Heterogeneity

Tables 4 and 5 investigate heterogeneity in the effects of the information treatments on stated support for equal employment opportunities based on sexual orientation, and the first principal component of the outcomes in columns 1 to 4 of Table 3 that measure broad social acceptance of LGB people in non-economic domains of life, respectively. We report the coefficient on the treatment indicator separately for various subsamples in each row.

The adjusted odds ratios in Table 4 indicate that the discrimination cost treatment was effective in each of the three countries we study, though the estimate for Turkey (the country in our sample with the strongest anti-LGBT attitudes as measured by Rainbow Europe see ILGA-Europe, 2022) is not statistically significant after we account for the false discovery rate with sharpened q -values. This somewhat weaker impact of the discrimination cost treatment in Turkey, as compared with Serbia and Ukraine, might reflect differences in existing legislation and hence people's experience with and knowledge of legislation intended to curb labor market discrimination. More specifically, in Serbia, Article 18 of the Labor Law explicitly prohibits direct and indirect discrimination against employees and job seekers. Likewise, in Ukraine, Article 2 of the Code of Labor Laws includes sexual orientation as a protected ground. No such legislative protection exists for the LGB population in Turkey. This means that Turkish respondents may have been less familiar with even the idea of non-discriminatory policies on the basis of sexual orientation.

Moreover, we also observed smaller and statistically insignificant effects of T1 on stated support for equal employment opportunities on the basis of sexual orientation for individuals who reported that homosexual acts were not morally justifiable and for individuals who agreed or strongly agreed with the statement that they would be ashamed to have a gay or lesbian family member.

Apart from this, we find that the effect of our discrimination cost treatment (T1) on stated support for equal employment opportunities on the basis of sexual orientation was very broad-based. For example, the impact of the treatment is equally large among men and women but appears somewhat stronger among older generations; the higher educated; urban residents; those with higher incomes; and those who identify as non-religious, though these differences in treatment effects across groups are not statistically significant. It is notable that the odds ratios suggest larger effects among demographic groups that would seem likely to have divergent views on social issues (e.g., older and highly educated individuals). This is worthy of additional research.

Table 5 shows that the effect of our myth debunking treatment T2 on the first principal component of the four outcomes shown in columns 1 through 4 of Table 3 was significantly stronger for those with more right-of-center political views. It is possible that these individuals had more margin to be affected than individuals with more left-of-center political views. Apart from this, we do not find significant differences in the effect of T2 across demographic groups.

We directly explore heterogeneity in treatment effects with respect to respondents' stated trust in the WHO.²² As our myth debunking treatment T2 specifically refers to the WHO, we would expect those respondents who exhibit higher trust in this institution to react more strongly to our treatment than those who distrust the WHO. Table 6 shows that this is indeed the case: T2 has a stronger effect on individuals who trust the WHO. They are more likely to consider homosexuality as morally

22 Since we elicited trust in the WHO only after our information treatments, this variable could theoretically be affected by them. Exploring treatment effect heterogeneity with respect to this variable could thus pose endogeneity problems. However, as Appendix Table A4 shows, trust in the WHO is not significantly related to our treatments.

Table 4. Discrimination Cost Treatment effects on support for equal employment opportunities on the basis of sexual orientation, by group.

	(1)
Full sample (<i>N</i> = 6,547)	1.487*** (0.1197) [0.0010]
Serbia (<i>N</i> = 2,272)	1.783** (0.3131) [0.0160]
Turkey (<i>N</i> = 2,087)	1.385 (0.1738) [0.1530]
Ukraine (<i>N</i> = 2,151)	1.545** (0.2112) [0.0240]
Men (<i>N</i> = 3,477)	1.492*** (0.1619) [0.0050]
Women (<i>N</i> = 3,045)	1.504*** (0.1849) [0.0080]
Above median age (<i>N</i> = 3,207)	1.634*** (0.1972) [0.0010]
Below median age (<i>N</i> = 3,312)	1.369** (0.1502) [0.0280]
Tertiary education (<i>N</i> = 4,124)	1.549*** (0.1532) [0.0010]
Secondary education (<i>N</i> = 2,411)	1.429 (0.2141) [0.1580]
Religious (<i>N</i> = 5,566)	1.422*** (0.1273) [0.0010]
Not religious (<i>N</i> = 977)	1.932** (0.3719) [0.010]
Urban (<i>N</i> = 4,871)	1.573*** (0.1424) [0.0010]
Rural (<i>N</i> = 1,667)	1.151 (0.2073) [0.7390]
Respondents who said homosexual acts are not morally acceptable (<i>N</i> = 2629)	1.141 (0.1547) [1.0000]
Respondents who agreed or strongly agreed they would be ashamed if family member were gay or lesbian (<i>N</i> = 1883)	0.973 (0.1467) [1.0000]

Table 4. (Continued).

	(1)
Low income (<i>N</i> = 2,049)	1.205 (0.1786) [0.2780]
Middle income (<i>N</i> = 2,557)	1.630*** (0.2136) [0.0020]
High income (<i>N</i> = 1,911)	1.681** (0.2433) [0.0110]
Left political views (<i>N</i> = 872)	1.735 (0.3715) [0.4450]
Center political views (<i>N</i> = 4,714)	1.437*** (0.1408) [0.0030]
Right political views (<i>N</i> = 931)	1.525 (0.3135) [0.5600]

Notes: Odds ratios are displayed. * *p* < .10; ** *p* < .05; *** *p* < .01. Standard errors robust to heteroscedasticity are reported in parentheses. All models include controls for: age and its square; a male dummy; dummy variables for tertiary education, secondary education, a dummy for being in any kind of partnership; a dummy variable for living in an urban area; survey date dummies; survey country dummies; number of adults above and below 65; religion dummies (Catholic, Muslim, Orthodox, other religion) and labor market-related controls (whether individual works at a state-owned enterprise, foreign firm or international organization, and unemployed dummy). Sharpened *q*-values, which report the expected “positive false discovery rate” obtained by rejecting the null hypothesis for any result with an equal or smaller *q*-value, are reported in brackets. ^a indicates a statistically significant difference in each group of estimates at *p* < .05.

Table 5. Myth Debunking Treatment effects on first principal component of LGB attitudes, by group.

	(1)
Full sample (<i>N</i> = 4,664)	1.170** (0.0767) [0.0380]
Serbia (<i>N</i> = 1,576)	1.134 (0.1327) [0.4470]
Turkey (<i>N</i> = 1,571)	1.251 (0.1445) [0.1590]
Ukraine (<i>N</i> = 1,514)	1.153 (0.1310) [0.3190]
Men (<i>N</i> = 2,482)	1.236* (0.1124) [0.0910]
Women (<i>N</i> = 2,173)	1.125 (0.1098) [0.4170]
Above median age (<i>N</i> = 2,369)	1.082 (0.0987) [0.5960]
Below median age (<i>N</i> = 2,295)	1.274** (0.1226) [0.0300]
Tertiary education (<i>N</i> = 3,019)	1.193* (0.0966) [0.0630]
Secondary education (<i>N</i> = 1,498)	1.117 (0.1337) [0.3770]
Religious (<i>N</i> = 3,969)	1.171* (0.0824) [0.0670]
Not religious (<i>N</i> = 695)	1.105 (0.2172) [1.0000]
Urban (<i>N</i> = 3,530)	1.185* (0.0893) [0.0730]
Rural (<i>N</i> = 1,134)	1.126 (0.1552) [0.3520]
Respondents who said homosexual acts are not morally acceptable (<i>N</i> = 2,305)	1.343** (0.1305) [0.0410]
Respondents who agreed or strongly agreed they would be ashamed if family member were gay or lesbian (<i>N</i> = 1,596)	1.302 (0.1592) [0.1370]

Table 5. (Continued).

	(1)
Low income ($N = 1,345$)	1.227 (0.1560) [0.4730]
Middle income ($N = 1,852$)	1.099 (0.1169) [0.4300]
High income ($N = 1,467$)	1.264 (0.1516) [0.1780]
Left political views ($N = 645$)	0.963 (0.2005) [0.8040] ^a
Center political views ($N = 3,298$)	1.129 (0.0875) [0.2000] ^a
Right political views ($N = 721$)	1.690** (0.2972) [0.0160] ^a

Notes: Odds ratios are displayed. * $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors robust to heteroscedasticity are reported in parentheses. All models include controls for: age and its square; a male dummy, dummy variables for tertiary education, secondary education, a dummy for being in any kind of partnership; a dummy variable for living in an urban area; survey date dummies; survey country dummies; number of adults above and below 65; religion dummies (Catholic, Muslim, Orthodox, other religion) and labor market-related controls (whether individual works at a state-owned enterprise, foreign firm or international organization, and unemployed dummy). Sharpened q-values, which report the expected “positive false discovery rate” obtained by rejecting the null hypothesis for any result with an equal or smaller q-value, are reported in brackets. ^a indicates a statistically significant difference in each group of estimates at $p < .05$.

Table 6. Effects of Myth Debunking Treatment only observed for those who trust the WHO.

	(1) Homosexual acts are morally acceptable	(2) Agree or strongly agree that homosexuality is justifiable	(3) Agree or strongly agree that gay men and lesbians should be free to live their life as they wish	(4) Disagree or strongly disagree that would be ashamed if family member were gay or lesbian
Discrimination cost treatment (T1)	1.063 (0.1391) [0.9290]	0.973 (0.1217) [1.0000]	0.990 (0.1185) [0.6530]	0.808* (0.0995) [0.2170]
Myth debunking treatment (T2)	0.903 (0.1191) [0.6770]	0.925 (0.1162) [1.0000]	1.012 (0.1207) [0.6530]	0.984 (0.1196) [0.8850]
Trust WHO	1.085*** (0.0193) [0.0010]	1.055*** (0.0175) [0.0060]	1.041** (0.0166) [0.0390]	1.010 (0.0165) [0.7530]
T1 * Trust WHO	1.016 (0.0251) [0.7660]	1.020 (0.0236) [0.7930]	1.032 (0.0234) [0.3260]	1.037 (0.0238) [0.2420]
T2 * Trust WHO	1.057** (0.0264) [0.0450]	1.053* (0.0245) [0.0830]	1.035 (0.0233) [0.3100]	1.038 (0.0236) [0.2380]
P-value of $H_0 : T_1 + T_1 * TrustWHO \geq T_2 + T_2 * TrustWHO$	0.8665	0.5726	0.4058	0.0292
N	5,238	5,936	6,279	5,920

Notes: Odds ratios are displayed. * $p < .10$; ** $p < .05$; *** $p < .01$. Standard errors robust to heteroscedasticity are reported in parentheses. All models include controls for: age and its square; a male dummy, dummy variables for tertiary education, secondary education; a dummy for being in any kind of partnership; a dummy variable for living in an urban area, survey date dummies; survey country dummies; number of adults above and below 65; religion dummies (Catholic, Muslim, Orthodox, other religion) and labor market-related controls (whether individual works at a state-owned enterprise, foreign firm or international organization, and unemployed dummy). Sharpened q-values, which report the expected “positive false discovery rate” obtained by rejecting the null hypothesis for any result with an equal or smaller q-value, are reported in brackets.

acceptable and justifiable after the WHO-based myth debunking treatment.²³ We did not find significant heterogeneity with respect to trust in the WHO for the discrimination cost treatment T1 (see Appendix Table A5).

Additional Analyses and Robustness Checks

Additional analyses, reported in Appendix A, document the robustness of our findings. Appendix Tables A6 and A7 show that excluding respondents in the bottom and top 5 percent of the sample based on survey completion time does not change our main findings. Our results are also robust to estimating models using OLS as shown in Appendix Tables A8 and A9. Appendix Table A10 shows that ordered logit models for the outcomes in the non-economic domains also return qualitatively similar patterns. Finally, Appendix Table A11 shows that our information treatments are not systematically related to missing data on the outcomes studied in Table 3.

We considered whether our results might reflect experimenter demand effects, whereby respondents are reporting not their true beliefs but instead the beliefs they think we (the researchers) want to observe based on cues inherent in the questions that precede the information treatments. We make several points in this regard. We explicitly chose a brief online survey because research shows that this helps to mitigate experimenter demand effects (Haaland et al., in press; Mummolo & Peterson, 2019). We repeatedly stressed that participation was voluntary, that the survey responses were anonymous, and that no information would be linked back to any individual's identity. In addition, the pattern where we found relatively weaker evidence in favor of the myth debunking (T2) treatment on social views relative to the discrimination cost (T1) treatment on support for non-discrimination is also largely inconsistent with strong experimenter demand effects, because arguments about social desirability bias should plausibly be stronger for questions about social views than about economic views. We emphasize that the countries we are studying are places where people freely and openly report negative views and attitudes toward sexual minorities. This is perhaps most plainly evidenced by the large fraction of respondents who willingly report that they think homosexuality is not justifiable or that they would be ashamed if a family member were gay or lesbian, even in the control condition where there was no LGB-related introductory questioning. Finally, we note that there is no relationship between our treatments and a range of placebo outcomes such as views on other social issues (e.g., abortion, contraception) or views about other minority groups, which one might expect also to be affected if our main results would reflect experimenter demand effects. For these reasons, we think it is unlikely that experimenter demand effects explain our results.

CONCLUSIONS

This paper presents the results of a novel information provision experiment designed to understand the determinants of support for sexual minorities in employment and other domains of life in Serbia, Turkey, and Ukraine—three developing countries with very strong anti-LGB attitudes. We find that randomly providing individuals with information on the costs to their country of sexual orientation discrimination causes statistically and economically meaningful increases in stated support for policies to promote equal employment opportunity based on not only sexual orientation but also gender, disability status, ethnic origin, nationality, and religion. We also find

²³ Appendix A also contains an analysis of heterogeneity using causal forest methods. That analysis generally supports the heterogeneity patterns described here.

evidence that a myth debunking treatment—which informed people that the WHO does not consider homosexuality to be a mental disease—caused improved attitudes toward LGB people in other non-economic domains of life. For example, individuals randomly assigned to myth debunking were significantly more likely to agree with the statement that gay men and lesbians should be free to live their life as they wish. Interestingly, these improvements in social views about LGB people were observed only for respondents in the myth-debunking treatment arm who reported trust in the WHO. Neither information treatment affected views about other socially controversial groups (e.g., immigrants) or issues (e.g., contraception).

Our study is subject to some limitations that could be addressed in future work. For example, we measure only the immediate effects of the information treatments; future work could examine if the effects persist. We were also unable to examine whether there are interactive effects of combining the two treatments and, as mentioned previously, we could not directly estimate treatment effect heterogeneity across the distribution of prior beliefs in the full sample.

Despite these caveats, our results shed new light on the global movement for non-discrimination protection in a part of the world that is especially relevant for LGB rights. Efforts to advance LGB inclusion in Eastern Europe continue to face significant challenges. The 2021 Pride march in Georgia was cancelled after LGB activists and journalists suffered a violent attack from far-right groups in the country. Hungary's new anti-LGB law bans schools from using materials that “promote homosexuality,” while in Poland several municipalities and towns have declared themselves to be “LGBT-ideology free zones.” As recently as July 2021, the European Commission has taken action against Hungary and Poland for these policies that it deems inconsistent with its Charter of Fundamental Rights.

On the one hand, our results show quite clearly that individuals in countries with negative attitudes toward sexual minorities can separate their moral objections to homosexuality from objections regarding equal employment. Our results also show, however, that moving the needle on more fundamentally held beliefs about homosexuality is difficult: our myth debunking treatment had statistically weaker and smaller effects at improving views about homosexuality than our discrimination cost treatment had on support for equal employment opportunity. Together, our findings suggest that political actors wanting to achieve the policy goal of expanding non-discrimination employment protections should consider information campaigns that stress the costs of discrimination as opposed to trying to change more fundamental views about homosexuality. On the other hand, our results also indicate that changing those more fundamental views is not beyond the scope of information campaigns, particularly when framed in the context of trusted institutions.

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