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ABSTRACT

Reducing Sexual-Orientation Discrimination: Experimental Evidence from Basic Information Treatments*

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 policy makers aiming to expand the rights of lesbian, gay, and bisexual people worldwide.

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1. Introduction

Recent advances in rights for lesbians, gay men, and bisexual individuals (LGB) 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 US Supreme Court rulings. Across the globe, India decriminalized same-sex sexual acts in 2018, and Taiwan granted same-sex marriage in 2019. In other parts of the world, however, LGB rights have advanced more slowly or not at all. As of 2019, for example, 70 United Nations Member States (35% of all members) criminalize same-sex sexual conduct; in six UN Member States same-sex sexual activity is 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, three countries with very strong negative attitudes toward LGB people. These countries have highly restrictive LGBT equality laws and policies, scoring just 33, 4, and 18, respectively, on a scale where zero indicates *gross* human rights violations and 100 represents the greatest degree of equality under the law (ILGA, 2019). They also have some of the lowest rates of social acceptance of sexual minorities in Europe. Appendix Table 1 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.

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[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).

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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 on the basis of violent threats from homophobic groups. 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 (Euronews, 2020). And 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 nondiscrimination. 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 does not consider homosexuality to be a mental disease. We hypothesize that 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 from 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 that 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 are all statistically significant. Third, the effects 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 concerning the moral acceptability and justifiability of homosexuality, as well as 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 World Health Organization (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 the idea 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.

2. 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.[2] Recent papers build on this work to assess discrimination against LGB individuals (Badgett 1995, Aksoy et al. 2018, Aksoy et al. 2019). This literature, summarized in Badgett et al. (2021), generally finds that gay men earn lower wages than otherwise similar heterosexual men. A related literature demonstrates that anti-discrimination policies can help reduce the labor market penalty experienced by gay men (Klawitter and Flatt 1998, Burn 2018, Delhommer 2021). Further evidence on discrimination is provided by audit studies of fictitious

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[2] Altonji and Blank (1999) provide an early overview and discussion of this literature.

resumes sent to employers, which find that gay candidates receive fewer callbacks for interviews than heterosexual candidates (Tilcsik 2011).[3] Coffman, Coffman, and Marzilli Ericson (2017) use online experiments to show that anti-gay discrimination in the U.S. workplace, of both openly gay managers and of job applicants, may be more common than previously 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 on the basis of sexual orientation in environments hostile to LGB people.

Second, we contribute to a broader literature on the determinants of social attitudes towards 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 US (Flores and Barclay 2016, Sansone 2019) and in Europe (Abou-Chadi and 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 and 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

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[3] 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.

attitudes towards minority groups (Haaland, Roth and Wohlfart, forthcoming). 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 (forthcoming) 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. While 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 very strong and deeply engrained anti-LGB sentiments.

3. Survey and experimental design

3.1 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 with respect to age, gender and sub-national region (similar to states in the US). Once respondents agreed to take part in the survey, we directed them to the consent page and asked them initial screening questions to ensure that the quotas for age, gender, and geography would be met. All respondents in our sample fully completed the survey and received a baseline remuneration of about two euros.

The survey consisted of four sections: (i) initial screening questions on socio-demographic characteristics; (ii) a random split of respondents into three groups (two treatment groups and one control group); (iii) attitudinal outcome questions (such as views about discrimination on the basis of race, gender, age, religion, or sexual orientation, views about justifiability of homosexuality, and related questions); and (iv) 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).[4] We designed the original questionnaire in English. Professional translators then translated it into the major conversational language of each country.[5]

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

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[4] 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 available here: https://www.dropbox.com/s/wod48w5c5zq783h/Questionnaire_Online.pdf

[5] All surveys in Ukraine were translated into Ukrainian.

on the entire survey. This allows us to identify respondents who were exceptionally quick or slow in completing the survey.[6] In addition, 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.

3.2 Experimental design and the information treatments

After the screening questions on socio-demographic characteristics, we randomly assigned respondents in each country to one of three sub-samples. We refer to the first sub-sample as the Discrimination Cost Treatment group (T1), the second as the Myth Debunking Treatment group (T2) and the third as the control group. All sub-samples contain about 700-800 survey respondents per country. We pre-registered the experimental protocol with the American Economic Association (AEARCTR-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 because of such policies.[7] 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.

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[6] 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 8 and 9).

[7] 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).

In the Myth Debunking Treatment (T2), we informed individuals that the World Health Organization 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".[8] 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 towards employment nondiscrimination 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.[9]

Lastly, in the control (placebo treatment) 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. In all cases, we designed the basic information treatments to be short and neutrally framed, as suggested by Haaland, Roth, and Wohlfart (forthcoming).[10]

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[8] 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."

[9] 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.

[10] 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

3.3 Outcome measures

Regarding outcomes, 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 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, or disability. We randomized the order in which we presented these demographic groups across participants.[11] Individuals are coded as '1' if they were in favor of equal opportunity measures for the group in question.

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

In addition to LGB-related opinions, we elicited attitudes towards 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

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people exhibited heterogeneity across the distribution of prior beliefs regarding whether homosexuality is a mental disease. These are interesting questions for future work.

[11] 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.

as placebo outcomes, since we would not expect them to be affected by our information treatments.

4. Empirical strategy

We utilize the randomized, controlled nature of the experiment to estimate logistic regression models relating the treatments to outcomes. These models take the form:

$$Y_{ic} = \beta_0 + \text{LGB Information Treatments}_{ic}\, \beta_1 + X_{ic}\, \beta_2 + \varepsilon_{ic} \tag{1}$$

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. [12] 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). [13] Since we randomize at the individual level there is no need to cluster our (heteroscedasticity-robust) standard errors.

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[12] In the Appendix, 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.

[13] 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.

5. Results

5.1 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 are on average 35 years old, 46.6 percent are female, 63 percent of them have a tertiary education, 85 percent are religious, and 74.4 percent live in urban areas.[14]

5.2 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) described above. 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.[15] 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 increases in the

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[14] Appendix Table 2 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.

[15] In terms of interpretation, for example, a *q*-value of one percent means that one percent of significant results will reflect false positives.

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.[16] This suggests that a simple information treatment to de-bias individuals was not effective at increasing their support for sexual orientation-based nondiscrimination 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 one) than the associated estimate for equal opportunity based on sexual orientation.[17] 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

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[16] 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.

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

throughout (these coefficients are not shown but 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 life 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.[18] 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.

5.3 Heterogeneity

Appendix 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 1-4 of Table 3 that measure broad social

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[18] 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.

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 results indicate that the effect of our discrimination cost treatment T1 on stated support for equal employment opportunities on the basis of sexual orientation was broad-based. The adjusted odds ratios 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) is not statistically significant after we account for the false discovery rate with sharpened q-values. The impact of the treatment is equally large among men and women but appears somewhat stronger among older generations; the higher educated; and those who identify as non-religious, though these differences in treatment effects across groups are not statistically significant. We do find that the average treatment effect is significantly larger among urban residents, among those with higher incomes, and among those with left-of-center political views. In contrast, Appendix Table 5 shows that the effect of our myth debunking treatment T2 on the first principal component of the four outcomes shown in columns 1-4 of Table 3 was significantly stronger for younger people and those with more right political views.

We also directly explore heterogeneity in treatment effects with respect to respondents' stated trust in the WHO.[19] 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 4 shows that this is indeed the case: T2 has a stronger effect on individuals who trust the WHO. They are in particular more likely to consider homosexuality as morally acceptable and justifiable after the WHO-

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[19] 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 6 shows, trust in the WHO is not significantly related to our treatments.

based myth debunking treatment.[20] We did not find significant heterogeneity with respect to trust in the WHO for the discrimination cost treatment T1 (see Table 4 and Appendix Table 7).

6. Additional analyses and robustness checks

Additional analyses, reported in the Online Appendix, document the robustness of our findings. For example, Appendix Tables 8 and 9 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 10 and 11. Appendix Table 12 shows that ordered logit models for the outcomes in the non-economic domains also return qualitatively similar patterns. Finally, Appendix Table 13 shows that our information treatments are not systematically related to missing data on the outcomes studied in Table 3.

We also consider 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. First, we explicitly chose a brief online survey since research shows that this helps to mitigate experimenter demand effects (Mummolo and Peterson 2019, Haaland, Roth, and Wohlfart, forthcoming). 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. Second, the pattern where we find relatively weaker evidence in favor of the myth debunking (T2) treatment on social views relative to the discrimination cost (T1) treatment on support for nondiscrimination is also largely inconsistent with strong experimenter demand effects, since arguments about social

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[20] The Online Appendix also contains an analysis of heterogeneity using causal forest methods. That analysis generally supports the heterogeneity patterns described here.

desirability bias should plausibly be stronger for questions about social views than about economic views. Third, 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 unlikely that experimenter demand effects explain our results.

7. Conclusions

We describe 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 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 only observed 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 only measure the immediate effects of the information treatments; future work could examine if the effects persist. We are also unable to examine whether there are interactive effects of combining the two treatments and, as mentioned previously, we cannot 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 nondiscrimination 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 views about, say, moral objections to homosexuality from those 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 nondiscrimination 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, however, 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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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) p-value for equality of column 2 vs. column 4	(6) p-value for equality of column 3 vs. column 4
Serbia	0.349	0.347	0.351	0.349	0.902	0.867
	(0.006)	(0.010)	(0.010)	(0.010)		
Turkey	0.319	0.316	0.321	0.320	0.767	0.928
	(0.006)	(0.010)	(0.010)	(0.010)		
Ukraine	0.332	0.338	0.328	0.332	0.677	0.795
	(0.006)	(0.010)	(0.010)	(0.010)		
Age	35.021	34.922	35.027	35.112	0.612	0.821
	(0.153)	(0.262)	(0.265)	(0.268)		
N° Adults below 65	2.450	2.464	2.453	2.432	0.404	0.596
	(0.015)	(0.027)	(0.028)	(0.026)		
Secondary	0.251	0.259	0.263	0.250	0.512	0.661
	(0.006)	(0.010)	(0.010)	(0.010)		
Tertiary	0.630	0.611	0.642	0.637	0.073*	0.764
	(0.006)	(0.010)	(0.010)	(0.010)		
Female	0.466	0.467	0.459	0.472	0.744	0.385
	(0.006)	(0.011)	(0.011)	(0.011)		
Urban	0.744	0.743	0.753	0.737	0.663	0.233
	(0.005)	(0.009)	(0.009)	(0.009)		
Religious	0.850	0.852	0.851	0.848	0.746	0.823
	(0.004)	(0.008)	(0.008)	(0.008)		
Christian	0.549	0.550	0.547	0.550	0.999	0.852
	(0.006)	(0.011)	(0.011)	(0.011)		
Muslim	0.268	0.267	0.272	0.265	0.854	0.566
	(0.005)	(0.009)	(0.010)	(0.009)		
Single	0.448	0.445	0.445	0.456	0.467	0.467
	(0.006)	(0.011)	(0.011)	(0.011)		
Working for foreign firm	0.189	0.193	0.197	0.191	0.889	0.345
	(0.003)	(0.005)	(0.006)	(0.005)		
On temporary leave (Feb.)	0.065	0.061	0.071	0.062	0.878	0.243
	(0.003)	(0.005)	(0.006)	(0.005)		
Unemployed and looking for a job (Feb.)	0.135	0.139	0.135	0.130	0.420	0.675
	(0.004)	(0.007)	(0.007)	(0.007)		
On temporary leave (July)	0.052	0.049	0.056	0.051	0.751	0.460
	(0.003)	(0.005)	(0.005)	(0.005)		
Political views	5.056	5.013	5.116	5.039	0.722	0.290
	(0.030)	(0.052)	(0.052)	(0.051)		
Hh. Gross Income Decile (July 2020)	5.451	5.398	5.509	5.445	0.586	0.466
	(0.035)	(0.061)	(0.062)	(0.061)		
N	6,549	2,183	2,174	2,192		

Notes: Standard errors in parentheses. * $p<0.10$

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	1.487***	1.352***	1.341***	1.275***	1.334***	1.254***
	(0.1197)	(0.1068)	(0.1081)	(0.0920)	(0.0954)	(0.0794)
	[0.0010]	[0.0020]	[0.0040]	[0.0070]	[0.0010]	[0.0020]
Myth Debunking Treatment	1.145	1.100	1.205**	0.999	1.092	1.078
	(0.0956)	(0.0897)	(0.0982)	(0.0743)	(0.0795)	(0.0684)
	[0.2220]	[0.6150]	[0.0460]	[1.0000]	[0.3360]	[0.1130]
p-value on test of equality of treatments	0.0009	0.0084	0.1575	0.0008	0.0045	0.0169
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<0.10, ** p<0.05, *** p<0.01. 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.

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	1.146 (0.0809) [0.1110]	1.072 (0.0727) [0.6150]	1.131 (0.0757) [0.1480]	0.958 (0.0640) [0.6930]	1.118 (0.0729) [0.1720]
Myth Debunking Treatment	1.155* (0.0817) [0.0920]	1.166* (0.0793) [0.0640]	1.173** (0.0789) [0.0450]	1.153* (0.0764) [0.0760]	1.170** (0.0763) [0.0420]
p-value on test of equality of treatments	0.9146	0.2116	0.5821	0.0050	0.4724
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<0.10, ** p<0.05, *** p<0.01. 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.

Table 4: 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	1.053 (0.1353) [1.0000]	0.968 (0.1205) [1.0000]	0.980 (0.1168) [0.8660]	0.799 (0.0968) [0.1840]
Myth Debunking Treatment	0.901 (0.1175) [1.0000]	0.917 (0.1145) [1.0000]	0.998 (0.1186) [0.8660]	0.965 (0.1163) [0.8700]
Trust WHO	1.088*** (0.0189) [0.0010]	1.058*** (0.0174) [0.0030]	1.042** (0.0165) [0.0260]	1.013 (0.0164) [0.4790]
T1 * Trust WHO	1.018 (0.0247) [1.0000]	1.021 (0.0235) [0.9060]	1.033 (0.0233) [0.3600]	1.040 (0.0235) [0.1870]
T2 * Trust WHO	1.057* (0.0260) [0.0650]	1.054* (0.0244) [0.0610]	1.038 (0.0232) [0.2610]	1.040 (0.0233) [0.1870]
P-value of $H_0: T_1 + T_1 * \text{TrustWHO} \geq T_2 + T_2 * \text{TrustWHO}$	0.8594	0.5871	0.4152	0.0332
N	5,238	5,936	6,279	5,920

Notes: Odds ratios are displayed. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. 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.

Online Appendix for
Reducing Sexual-Orientation Discrimination:
Experimental Evidence from Basic Information Treatments
January, 2022

Additional Results and Robustness Checks

In this section we report further analyses establishing the robustness of and exploring heterogeneity in our findings.

Heterogeneity Analysis using Causal Forest

While the heterogeneity analysis presented earlier returns suggestive patterns, applying regressions to split samples limits statistical power, especially when covariates such as income and education are strongly (though far from perfectly) correlated. We therefore complement this analysis with an honest causal forest algorithm (Athey and Imbens, 2016; Wager and Athey, 2018; Athey et al., 2019) to disentangle more precisely how individual respondent traits moderate the causal relationship between our discrimination cost treatment and support for LGB equal employment opportunities, as well as between our myth debunking treatment and general LGB attitudes.[21] The algorithm predicts *individual* treatment effects based on a broad set of respondent-level covariates.[22]

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[21] For simplicity, we again look at the first principal component from a principal component analysis on the four variables capturing general LGB attitudes from Table 3.

[22] Conceptually, the honest causal forest algorithm creates a random forest of causal trees. Each tree grows from a random (bootstrapped) subsample of training data, the root node. The tree then recursively splits into increasingly smaller nodes that share similar covariates until it arrives at a set of terminal nodes (called leaves). The algorithm makes splits that produce the biggest difference in treatment effects across leaves while still yielding an accurate estimate of the full treatment effect. If splitting a node would not result in an improved fit, that node is not split further and forms a final leaf. This approach is honest in the sense that for each training subsample (that is, for each tree) the observations are separated into a splitting sample (to determine where to place the splits) and an estimating sample (to estimate the within-leaf treatment effects).

This approach has two important advantages (Davis and Heller, 2017). First, causal forests can combine multiple explanatory variables in a data-driven, nonlinear but disciplined way. This gives us a more flexible and efficient, and hence statistically more powerful, tool to estimate heterogeneous treatment effects. In a setting like ours, with multiple dimensions of potential heterogeneity (which may also interact or have non-linear effects) this is especially useful. Second, the algorithm tells us how useful each respondent trait is in growing the forest from which we predict heterogeneous impacts. This allows us to gauge the relative importance of these traits as moderators of the causal effect between treatment and outcomes.

We use the generalized random forest grf package for R by Tibshirani et al. (2020) to estimate a forest with 20,000 trees based on a random training sample of 75 percent of the full dataset. To grow each tree, we split the training sample into a splitting and estimating sample of equal size. We repeat this step 20,000 times to grow the complete forest. In a final step, the 25 percent of the full dataset that was left aside is fed through all trees. For each observation, we determine to which leaf it belongs based on the respondent's various traits. We then assign it the predicted treatment effect of that particular leaf. The average prediction across all trees is then the predicted treatment effect at the respondent level.

Panel A of Appendix Figure 1 depicts the distribution of the predicted treatment effects of the discrimination cost treatment (T1) on LGB equal

employment attitudes. In the absence of treatment heterogeneity, this distribution would cluster tightly around the average treatment effect (ATE) (vertical dashed line). Instead, the causal forest reveals a relatively broad distribution of treatment effects underlying the ATE.

Panel B of Appendix Figure 1 ranks various respondent traits by their relative importance as moderators (drivers of treatment heterogeneity). We define a trait's relative importance as the weighted sum of the number of times it is used to split at each depth in the forest. The more a trait is used to split subsamples, the more predictive power it has. We find that respondents' annual household income, their age and, to a lesser extent, their trust in WHO and political orientation are by far the most important drivers of treatment heterogeneity. In sharp contrast, someone's religion, urban/rural and marital status, gender, and country of residence are far less important drivers of treatment heterogeneity.

Because the algorithm provides us with a complete distribution of the treatment effects, we can also plot the value of these traits against the predicted treatment effect at the level of individual respondents. Appendix Figure 2 does so for the three most important respondent traits. We fit smooth local polynomial functions in each scatterplot. The patterns are striking. Panel A shows how the predicted treatment effect increases linearly with household income. The treatment effect in the highest income decile is more than three times that in the bottom decile. Panel B shows that treatment effects also vary strongly with age, but in a less linear

3

way. In particular, treatment effects increase until the age of 33 after which the impact of our discrimination cost treatment stabilizes. Lastly, Panel C shows how treatment effects are somewhat stronger among those who hold relatively liberal political views.

Concerning the effect of our myth debunking treatment (T2) on general LGB attitudes, Panel B of Appendix Figure 3 shows that the drivers of treatment heterogeneity are the same as for the discrimination cost (T1) treatment effect on LGB equal employment opportunities, with household income, age and political orientation top of the list. The effect is again stronger with income, but treatment heterogeneity goes in the opposite direction concerning age and political orientation: younger and more right-wing people are impacted more (see Appendix Figure 4).

Are the Results Driven by Respondents at the Extreme Ends of the Survey Completion Time Distribution?

One might worry that our findings are driven by respondents who spend very little or a particularly long time in answering the survey. We checked for sensitivity to these outliers by excluding respondents in the bottom and top 5 percent of the sample based on survey time distribution.[23] Doing so does not substantively change

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[23] Conditional on not interrupting the survey, the fastest 5% took on average 7.16 minutes to complete the survey, while the slowest 5% took 64.21 minutes on average. The sample excluding the top and bottom 5% took 20.23 minutes on average.

the point estimates for our variables of interest (Appendix Tables 8 and 9).

Are the Results Robust to Alternative Estimation Methods?

We also show that our results are robust to using OLS models in Appendix Tables 10 and 11 for the employment nondiscrimination outcomes and social views, respectively. In Appendix Table 12 we show that the effects of T2 on social views are also robust to estimating ordered logit models instead of dichotomizing the outcomes.

Are the Results Driven by Systematic Non-Responses?

In Appendix Table 13 we show that our information treatments are not systematically related to non-response to the questions studied in Table 3 of the main paper.

Are there Relationships with LGB-Related Questions about Perceptions of the Status Quo?

We examined three outcomes designed to capture respondents' perceptions of the status quo, including with respect to sexual minorities. For example, we asked individuals: "*For each of the following types of discrimination, could you please tell me whether, in your opinion, it is very widespread, fairly widespread, fairly rare, or very rare in your country? Discrimination on the basis of...*", and then

individuals were asked to respond separately regarding each of the same demographic characteristics listed above in the question about equal opportunities. We again randomized the order of the various demographic groups. We code an outcome as '1' if the individuals answered that discrimination against the relevant group was either 'very widespread' or 'fairly widespread' (and zero otherwise).

We then asked individuals: "*In your country, when a company wants to hire someone and has the choice between two candidates with equal skills and qualifications, which of the following criteria may, in your opinion, put a candidate at a disadvantage?*" This question comes from the Eurobarometer and has been used in many studies to assess perceptions of the extent of hiring discrimination based on demographic characteristics. We then asked individuals about each of the same criteria as in the prior two questions while again randomizing the order of categories across participants. We code an indicator outcome equal to '1' if the respondent reported that the specific characteristic in question would put a job applicant at a disadvantage.

As a final measure of respondent perceptions of the status quo, we asked individuals whether the city or area where they live is a good place to live for gay and lesbian people or not. We code an indicator equal to '1' if the respondent reported it as a good place. We consider the set of three LGB-related outcomes regarding respondent perceptions of the status quo – whether they think discrimination on the basis of sexual orientation is widespread, whether they think

sexual orientation puts a person at a hiring disadvantage, and whether they think their city is a good place to live for gays and lesbians – to be placebo tests that inform the interpretation of our findings regarding support for equal employment opportunities.

We present the results from similarly specified models for these outcomes in Appendix Table 14. In all cases, neither the discrimination cost treatment nor the myth debunking treatment were significantly related to these outcomes. The adjusted odds ratios are all close to one and statistically insignificant.

Appendix Figure 1: Conditional Treatment Effects (T1) and Variable Importance Ranking from a Causal Forest Model - *Outcome: Support for Equal Employment Opportunities Based on Sexual Orientation*

Panel A: Distribution of Conditional Treatment Effects
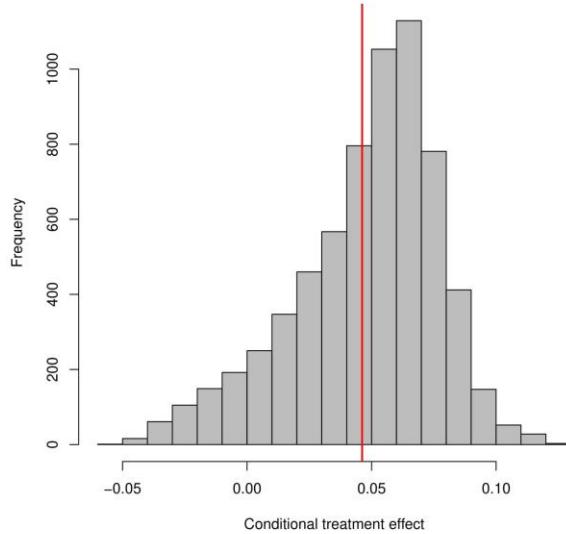
Panel B: Variable Importance Graph
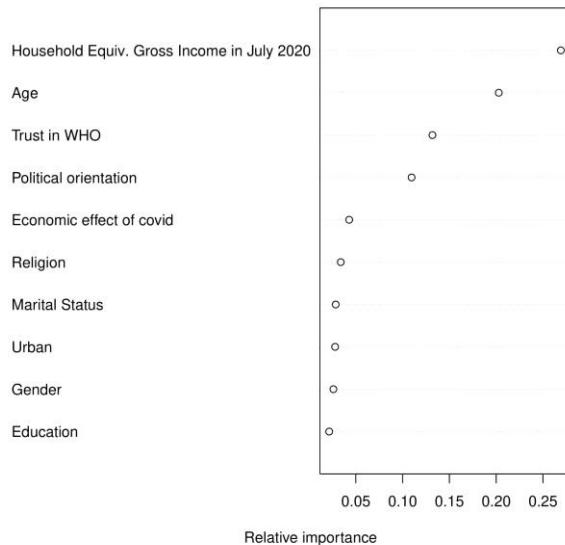
Appendix Figure 2: Predicted Treatment Effects (T1), by Household income, Age, Trust in WHO and Political views – *Outcome: Support for Equal Employment Opportunities Based on Sexual Orientation*

Panel A: Household income (July 2020)
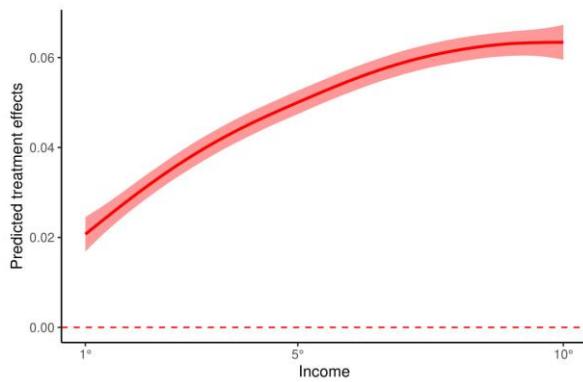
Panel B: Age
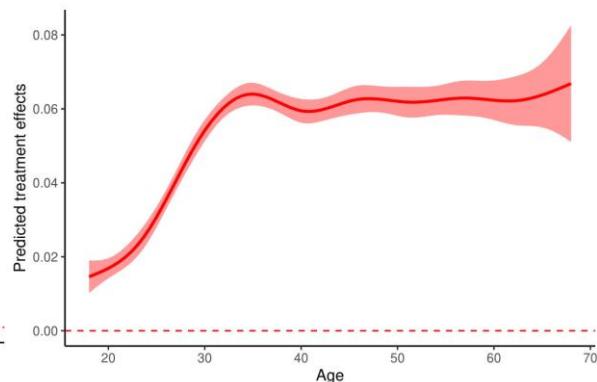
Panel C: Trust in WHO
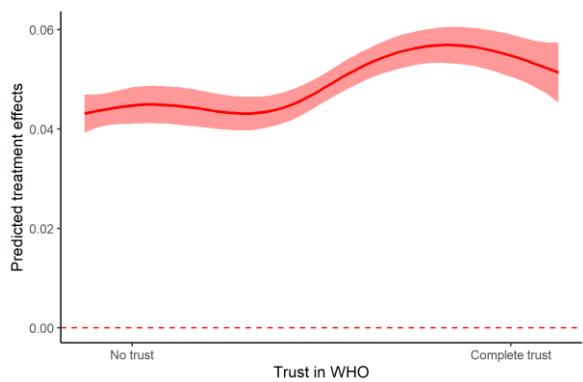
Panel D: Political views
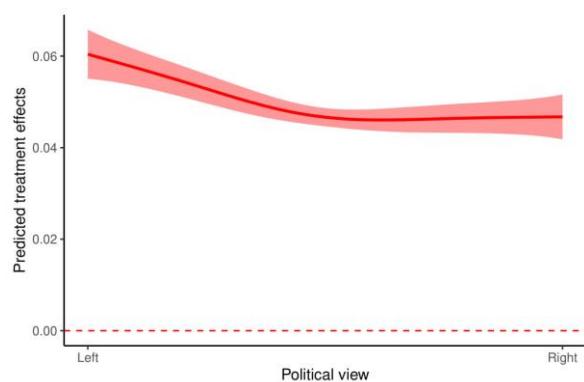
Appendix Figure 3: Conditional Treatment Effects (T2) and Variable Importance Ranking from Causal Forest Model - *Outcome: First Principal Component of LGB Attitudes*

**Panel A: Distribution of Conditional Treatment Effects**
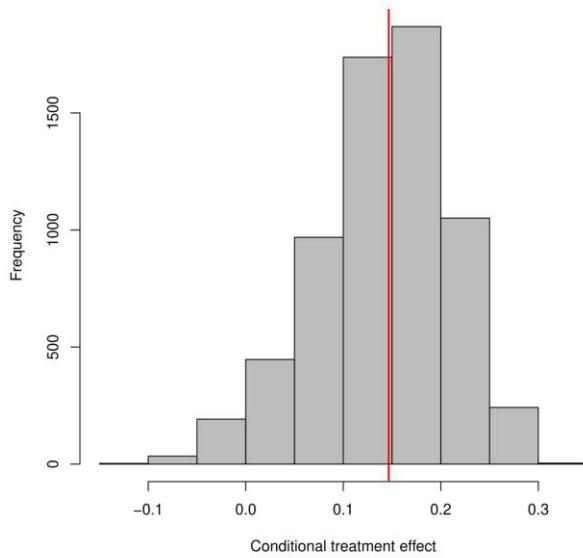

Panel B: Variable Importance Graph
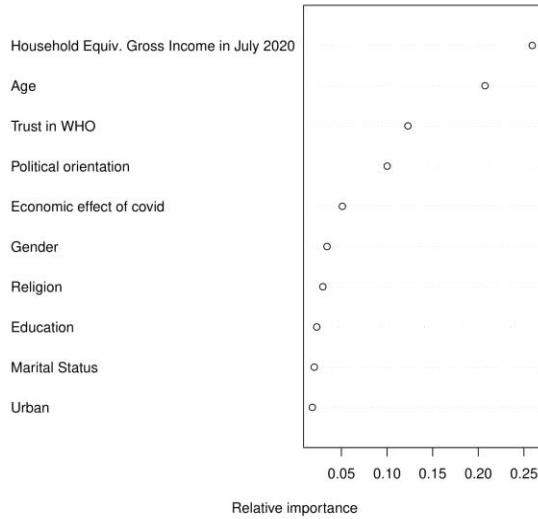
Appendix Figure 4: Predicted Treatment Effects (T2), by Household income, Age, Trust in WHO and Political views – *Outcome: First Principal Component of LGB Attitudes*

Panel A: Household income (July 2020)
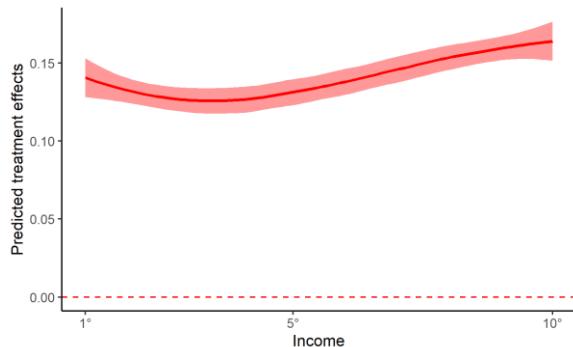
Panel B: Age
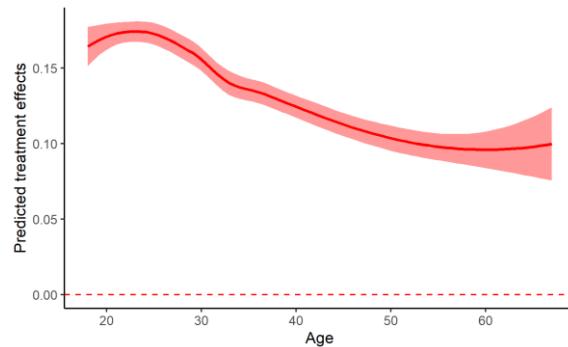
Panel C: Trust in WHO:
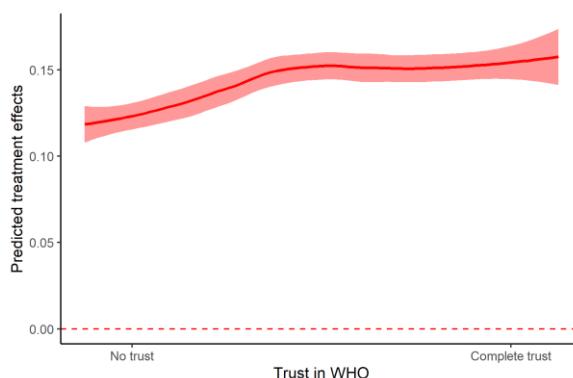
Panel D: Political views
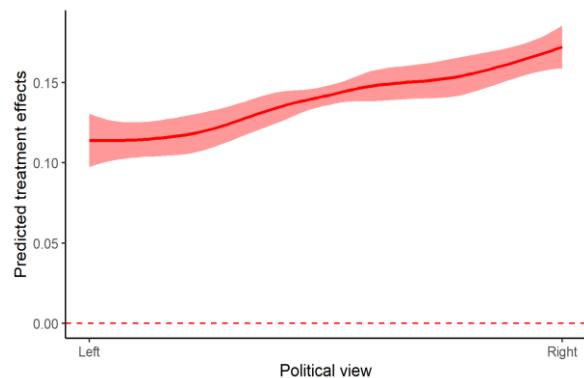
Appendix Table 1: Share of respondents that agree or strongly agree that "Gay men and lesbians should be free to live their own life as they wish", European Social Survey, 2002-2019

Country	Average
Iceland	93.03
Netherlands	91.76
Denmark	89.87
Sweden	87.52
Belgium	84.54
Ireland	83.91
Norway	83.34
France	82.62
United Kingdom	82.28
Luxembourg	81.49
Switzerland	81.33
Germany	81.07
Spain	79.23
Austria	72.65
Finland	71.54
Italy	70.02
Portugal	65.97
Israel	62.09
Czech Republic	61.55
Slovenia	58.56
Cyprus	52.99
Greece	51.89
Poland	50.64
Bulgaria	50.24
Estonia	45.23
Hungary	45.17
Slovakia	41.12
Croatia	39.06
Serbia	**35.08**
Ukraine	**30.38**
Turkey	**27.2**
Russia	25.65
Lithuania	19.5

Source: European Social Survey. Note: Weighted means.

Appendix Table 2: Omnibus Test

	Treatment 1 & Control		Treatment 2 & Control	
	Coef.	S.E.	Coef.	S.E.
Turkey	-0.016	(0.039)	-0.024	(0.038)
Ukraine	0.009	(0.020)	-0.005	(0.020)
Age	-0.000	(0.001)	-0.000	(0.001)
Male	-0.000	(0.001)	-0.000	(0.002)
N° adults above 65	-0.001	(0.007)	-0.011	(0.007)
N° adults below 65	0.004	(0.006)	0.005	(0.006)
Tertiary education	-0.029*	(0.016)	0.004	(0.016)
Christian	0.003	(0.029)	-0.015	(0.029)
Muslim	0.024	(0.039)	0.013	(0.038)
Single	-0.019	(0.017)	-0.009	(0.017)
On temporary leave (Feb.)	-0.007	(0.033)	0.041	(0.031)
State-owned bank (Feb.)	-0.110	(0.126)	-0.142	(0.127)
International Organizations (Feb.)	0.078	(0.156)	0.027	(0.181)
State-owned enterprise (July)	-0.017	(0.026)	-0.024	(0.027)
Unemployed and looking for a job (Feb.)	0.033	(0.030)	0.048	(0.031)
Foreign firm (Feb.)	-0.031	(0.058)	-0.070	(0.059)
On temporary unpaid leave (July)	-0.023	(0.036)	0.020	(0.035)
Public sector or government (July)	-0.009	(0.021)	-0.015	(0.021)
Unemployed and looking for a job (July)	-0.032	(0.029)	-0.052*	(0.030)
In paid work, self-employed (Feb.)	-0.006	(0.020)	0.005	(0.020)
N	4,375		4,366	
R-squared	0.004		0.004	
P-value for joint significance	0.688		0.755	

Notes: Odds ratios are displayed. Robust standard errors in parentheses: * $p<0.10$, ** $p<0.05$, *** $p<0.01$.

Appendix Table 3: No Effects on Placebo Outcomes

	(1) Allow migrants from majority ethnic group	(2) Allow migrants from minority ethnic group	(3) Morally acceptable to convert to another religion	(4) Morally acceptable to use contraceptives	(5) Morally acceptable to have a child outside of marriage	(6) Morally acceptable that a married man has an affair	(7) Morally acceptable to live on unemployment benefits
Discrimination Cost Treatment	0.975	1.103	0.898	1.068	1.143	1.057	0.935
	(0.0735)	(0.0771)	(0.0643)	(0.1077)	(0.0998)	(0.0937)	(0.0631)
	[0.6720]	[0.5670]	[0.3480]	[0.2360]	[0.4530]	[0.6260]	[0.7810]
Myth Debunking Treatment	0.951	1.053	0.908	0.869	0.921	1.084	0.922
	(0.0711)	(0.0731)	(0.0656)	(0.0857)	(0.0770)	(0.0949)	(0.0627)
	[0.5580]	[1.0000]	[0.4280]	[0.1570]	[0.9480]	[0.5940]	[0.7810]
N	5,590	5,631	5,247	5,809	5,854	5,807	5,366

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 4: Discrimination Cost Treatment Effects on Support for Equal Employment Opportunities on the Basis of Sexual Orientation, by Group

	(1)
Full sample (N=6,547)	1.487*** (0.1197) [0.0010]
Serbia (N=2,272)	1.734** (0.3003) [0.0220]
Turkey (N=2,087)	1.395 (0.1748) [0.1230]
Ukraine (N=2,151)	1.544** (0.2120) [0.0240]
Men (N=3,477)	1.496*** (0.1619) [0.0040]
Women (N=3,045)	1.502*** (0.1843) [0.0080]
Above median age (N=3,207)	1.640*** (0.1966) [0.0010]
Below median age (N=3,312)	1.369** (0.1501) [0.0180]
Tertiary education (N=4,124)	1.553*** (0.1536) [0.0010]
Secondary education (N=2,411)	1.392* (0.1983) [0.0960]
Religious (N=5,380)	1.422*** (0.1273) [0.0010]
Not religious (N=1,160)	1.932** (0.3719) [0.010]
Urban (N=4,871)	1.571*** (0.1423) [0.0010] [a]
Rural (N=1,667)	1.169 (0.2093) [1.0000] [a]
Respondents who said homosexual acts are not morally acceptable (N=2629)	1.135 (0.1542) [0.9910]
Respondents who agreed or strongly agreed they would be ashamed if family member were gay or lesbian (N=1878)	0.948 (0.1426) [1.0000]
Low income (N=2,049)	1.191 (0.1763) [0.3550]
Middle income (N=2,557)	1.405*** (0.2101) [0.0040]
High income (N=1,911)	1.711*** (0.2489) [0.0090] [a]
Left political views (N=869)	1.796* (0.3806) [0.0910] [a]
Center political views (N=4,714)	1.331*** (0.1402) [0.0030]
Right political views (N=946)	1.513 (0.3101) [0.352]

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 5: Myth Debunking Treatment Effects on First Principal Component of LGB Attitudes, by Group

	(1)
Full sample (N=4,664)	1.170** (0.0763) [0.0420]
Serbia (N=1,576)	1.141 (0.1331) [0.1560]
Turkey (N=1,571)	1.277 (0.1455) [0.1420]
Ukraine (N=1,514)	1.124 (0.1275) [0.1780]
Men (N=2,482)	1.239* (0.1126) [0.0630]
Women (N=2,173)	1.137 (0.1099) [0.4410]
Above median age (N=2,369)	1.085 (0.0983) [0.9810]
Below median age (N=2,295)	1.286** (0.1222) [0.0270] [a]
Tertiary education (N=3,019)	1.184 (0.0954) [0.1090]
Secondary education (N=1,645)	1.101 (0.1247) [0.4220]
Religious (N=3,854)	1.161 (0.0824) [0.1660]
Not religious (N=810)	1.138 (0.1963) [0.7710]
Urban (N=3,530)	1.184* (0.0884) [0.0540]
Rural (N=1,134)	1.094 (0.1520) [0.4510]
Respondents who said homosexual acts are not morally acceptable (N=2,305)	1.355** (0.1317) [0.0330]
Respondents who agreed or strongly agreed they would be ashamed if family member were gay or lesbian (N=1,596)	1.319 (0.1612) [0.1140]
Low income (N=1,345)	1.213 (0.1511) [0.5360]
Middle income (N=1,852)	1.090 (0.1150) [0.8760]
High income (N=1,467)	1.284 (0.1547) [0.1160]
Left political views (N=645)	1.019 (0.2033) [1.000]
Center political views (N=3,298)	1.114 (0.0859) [0.3500]
Right political views (N=721)	1.701** (0.2992) [0.0160] [a]

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 6: No Effect of T1 and T2 on Trust in the WHO

	Trust in the WHO
Equal Employment Opportunities	
Discrimination Cost Treatment	1.025
	(0.0547)
	[0.5730]
Myth Debunking Treatment	1.033
	(0.0546)
	[0.5410]
N	6,549

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 7: Measures to Ensure Equal Employment Opportunity - Interactions with Trust in WHO

	(1) Sexual orientation	(2) Ethnic Origin	(3) Religion or Beliefs	(4) Nationality	(5) Gender	(6) Disability
Equal Employment Opportunities						
Discrimination Cost Treatment	1.224	1.311	1.240	1.284	1.243	1.387***
	(0.1882)	(0.1956)	(0.1933)	(0.1733)	(0.1622)	(0.1583)
	[0.4910]	[0.2450]	[0.1550]	[0.2520]	[0.3200]	[0.0100]
Myth Debunking Treatment	1.027	0.962	1.268	0.982	0.930	0.980
	(0.1632)	(0.1515)	(0.1973)	(0.1378)	(0.1243)	(0.1113)
	[1.0000]	[1.0000]	[0.1240]	[1.0000]	[0.4940]	[0.3010]
Trust WHO	1.037	1.053*	1.044*	1.041	1.020	1.058***
	(0.0211)	(0.0206)	(0.0211)	(0.0186)	(0.0178)	(0.0164)
	[0.2140]	[0.0670]	[0.0790]	[0.1360]	[0.4290]	[0.0020]
T1 * Trust WHO	1.041	1.006	1.016	0.998	1.015	0.977
	(0.0281)	(0.0264)	(0.0278)	(0.0243)	(0.0240)	(0.0208)
	[0.3550]	[1.0000]	[0.3550]	[1.0000]	[0.4910]	[0.1430]
T2 * Trust WHO	1.022	1.026	0.989	1.003	1.034	1.021
	(0.0284)	(0.0280)	(0.0270)	(0.0252)	(0.0247)	(0.0217)
	[0.7080]	[0.8370]	[0.3610]	[1.0000]	[0.3320]	[0.1670]
P-value of $H_0: T_1 + T_1 *$ TrustWHO $\geq T_2 + T_2 *$ TrustWHO	0.9311	0.9874	0.5144	0.9883	0.9923	0.9992
N	6,549	6,549	6,549	6,549	6,549	6,549

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 8: Equal Employment Opportunities – Robustness to Removing Respondents in the Bottom and Top 5% of the Survey Time Distribution

	(1) Sexual orientation	(2) Ethnic Origin	(3) Religion or Beliefs	(4) Nationality	(5) Gender	(6) Disability
Equal Employment Opportunities						
Discrimination Cost Treatment	1.543***	1.411***	1.464***	1.322***	1.332***	1.277***
	(0.1318)	(0.1177)	(0.1251)	(0.1008)	(0.1005)	(0.0849)
	[0.0010]	[0.0010]	[0.0010]	[0.0030]	[0.0010]	[0.0010]
Myth Debunking Treatment	1.185	1.136	1.306**	1.047	1.135	1.107*
	(0.1053)	(0.0981)	(0.1133)	(0.0823)	(0.0870)	(0.0736)
	[0.1840]	[0.3890]	[0.0150]	[1.0000]	[0.3800]	[0.0930]
N	5,904	5,891	5,891	5,904	5,906	5,906

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 9: LGB Attitudes - Robustness to Removing Respondents in the Bottom and Top 5% of the Survey Time Distribution

	(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	1.146	1.075	1.143	0.979
	(0.0847)	(0.0766)	(0.0812)	(0.0686)
	[0.1480]	[0.6900]	[0.1500]	[1.0000]
Myth Debunking Treatment	1.138	1.127	1.171*	1.180*
	(0.0845)	(0.0810)	(0.0834)	(0.0822)
	[0.1780]	[0.2740]	[0.0820]	[0.0510]
N	4,754	5,380	5,671	5,382

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 10: Equal Employment Opportunities – OLS

	(1) Sexual orientation	(2) Ethnic Origin	(3) Religion or Beliefs	(4) Nationality	(5) Gender	(6) Disability
Equal Employment Opportunities						
Discrimination Cost Treatment	0.056***	0.044***	0.041***	0.043***	0.052***	0.052***
	(0.0115)	(0.0116)	(0.0114)	(0.0128)	(0.0129)	(0.0146)
	[0.0010]	[0.0020]	[0.0030]	[0.0080]	[0.0010]	[0.0020]
Myth Debunking Treatment	0.018	0.013	0.025**	-0.000	0.015	0.017
	(0.0111)	(0.0113)	(0.0112)	(0.0124)	(0.0126)	(0.0146)
	[0.2120]	[0.5560]	[0.0480]	[1.0000]	[0.3160]	[0.1090]
N	6,549	6,549	6,549	6,549	6,549	6,549

Notes: * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 11: LGB Attitudes - OLS

	(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	0.032	0.015	0.027	-0.010	0.082
	(0.0164)	(0.0149)	(0.0144)	(0.0154)	(0.0539)
	[0.1100]	[0.6400]	[0.1430]	[0.6740]	[0.2630]
Myth Debunking Treatment	0.033*	0.034*	0.034**	0.033*	0.129**
	(0.0164)	(0.0150)	(0.0144)	(0.0154)	(0.0540)
	[0.0980]	[0.0560]	[0.0440]	[0.0670]	[0.0400]
N	5,238	5,936	6,279	5,920	4,664

Notes: * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 12: LGB Attitudes – Ordered Logit

	(1) Homosexuality is justifiable	(2) Gay men and lesbians should be free to live their life as they wish	(3) Would be ashamed if family member were gay or lesbian
Discrimination Cost Treatment	1.078	1.091	0.970
	(0.0621)	(0.0615)	(0.0549)
	[0.4160]	[0.3340]	[1.0000]
Myth Debunking Treatment	1.160**	1.098	1.136*
	(0.0667)	(0.0619)	(0.0655)
	[0.0270]	[0.2790]	[0.0570]
N	5,936	6,277	5,918

Notes: * p<0.10, ** p<0.05, *** p<0.01. 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.

Appendix Table 13: Missing Responses Not Systematically Related to the Information Treatments – Outcomes Related to Non-Economic Domains

Outcome (1/0) is missing responses for ➜	(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	0.847 (0.0991)	1.073 (0.112)	0.949 (0.143)	0.919 (0.0939)
Myth Debunking Treatment	0.932 (0.0707)	1.083 (0.113)	0.898 (0.137)	0.979 (0.0988)
N	6,549	6,549	6,549	6,549

Notes: Odds ratios are displayed. * p<0.10, ** p<0.05, *** p<0.01. 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).

Appendix Table 14: Information Treatments Had No Relationship to Statements Describing the Status Quo in the Context of Sexual Orientation

	(1) Discrimination on the basis of sexual orientation is widespread	(2) Sexual orientation puts a person at a disadvantage in hiring	(3) Area where you live is a good place for gays and lesbians
Discrimination Cost Treatment	0.994 (0.0708) [1.0000]	1.067 (0.0702) [1.0000]	1.099 (0.0844) [1.0000]
Myth Debunking Treatment	1.014 (0.0722) [1.0000]	0.952 (0.0633) [1.0000]	1.050 (0.0808) [1.0000]
N	5,860	6,549	4,439

Notes: Odds ratios are displayed. * $p<0.10$, ** $p<0.05$, *** $p<0.01$. 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.