Exclusionary Government Rhetoric and Migration Intentions*

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Abstract

We investigate whether exclusionary government rhetoric targeting a minority group affects residents' migration decisions. In 2019, almost 100 local governments in Poland voted to declare their localities "free from LGBTQ ideology," providing a unique setting in which government narratives suddenly changed, but the legal situation of targeted minorities remained the same. We study the impact of these declarations on migration intentions using novel data on domestic and international job search from a large global job site. Comparing counties with anti-LGBTQ resolutions to neighboring control counties in a difference-in-differences design, we find that the resolutions increased domestic out-of-county job search by 12 percent and international job search by 15 percent. Our results are likely driven by the shock to beliefs about local social norms, as we find the largest effects in counties with relatively low prior support for far-right parties. We also present suggestive evidence that the rise in job search translated into actual migration, with the treated counties losing nearly 1 percent of their young adult population.

Keywords: migration; migration intentions; job search; discrimination; LGBTQ
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1 Introduction

When studying the push factors of migration, researchers have traditionally focused on large-scale shocks such as war, poverty, or severe repression. While it is well established that such forces have historically triggered large waves of migration (Becker et al., 2022; Boustan, 2007; Hatton, 2020), less is known about whether government rhetoric alone can influence migration intentions. This question is particularly relevant today, given the growing prevalence of exclusionary political discourses targeting minorities in Europe and the United States (Bracco et al., 2022; Grosjean et al., 2023; Hoekstra & Orozco-Aleman, 2021; Müller & Schwarz, 2023) and rising polarization (Bertrand & Kamenica, 2023; Boxell et al., 2017).

We study the impact of exclusionary government rhetoric on migration intentions in the context of sexual orientation and gender identity using data from Poland, where nearly 100 local governments in 2019 adopted resolutions declaring their localities "free of LGBTQ ideology." These resolutions did not directly affect individuals' legal rights, because local governments in Poland lack the authority to change laws. However, they were very salient and employed discriminatory language (Commissioner for Human Rights, 2020) that portrayed LGBTQ individuals as a threat to society. The resolutions served primarily as symbolic statements, signaling local politicians' views or, more broadly, the prevailing local social norms.

To measure the migration intentions of local residents, we construct a novel revealed-preference indicator based on job search behavior. We use data from Indeed, the world's largest hiring platform, which captures the location of Polish job seekers and the location of the job postings they click on, both within Poland and abroad. Our dataset covers over 67 million job seeker clicks between 2016 and 2021. The use of this large-scale dataset distinguishes our study from research that has typically relied on stated migration preferences from surveys. Previous work by Mamertino and Sinclair (2019) shows that job search and migration are strongly correlated, validating the use of this data as a proxy for migration intentions.

To identify causal effects, we exploit variation in the adoption of anti-LGBTQ resolutions across counties in a difference-in-difference design with contiguous county-pair-by-year fixed effects to account for endogenous adoption and spatial concentration of the resolutions. In essence, we compare changes in job search patterns over time between the treated counties and their neighboring untreated counterparts.

We find that the adoption of anti-LGBTQ resolutions significantly increased outbound job search from the treated counties. Specifically, job seeker clicks on domestic out-of-county job postings increased by approximately 12 percent, while clicks on foreign postings increased by approximately 15 percent. These effects are driven by extensive margin responses: we observe no significant change in clicks per user, indicating that job search intensity did not change. The impact on internal migration intentions is entirely concentrated in destination counties without anti-LGBTQ resolutions, while the impact on international migration intentions is concentrated in European countries with legalized same-sex marriage. Importantly, the standards of LGBTQ rights in a country significantly predict the size of the relative effects on intentions to migrate to that country, even after accounting for geographical and economic factors. These findings highlight the relevance of destination characteristics in shaping migration intentions. In contrast, we find small and statistically insignificant effects on inbound job search into the treated counties.

The magnitude of the outbound search effects varies significantly depending on the local economic and political characteristics of the county of origin. The effects are strongest in counties with low pre-treatment support for far-right parties — that is, in areas where anti-LGBTQ resolutions were likely to be perceived as particularly surprising. This pattern suggests that the updating of beliefs about local social norms is a key mechanism behind the observed responses. We can rule out several alternative mechanisms, as we find no sizable effects of the resolutions on local labor demand, mortality, fertility, or public safety.

In addition, we provide suggestive evidence of the effects on actual migration. Since there is no high-quality annual data on migration flows in Poland, we use decennial census data on the counties' populations by birth cohort from 2011 and 2021, and we exploit the fact that only adults can make their own location decisions. We show that the treated counties witnessed a relative decline in their young adult population of approximately 0.8 percent compared to their control neighboring counties. Although these estimates are marginally insignificant for the entire population, we find statistically significant negative effects for women.

Our study contributes to three strands of literature. First, it extends the literature on the determinants of migration. Existing work recognizes that discrimination is a key push factor (Abramitzky & Boustan, 2017; Becker et al., 2024; Hatton, 2020), drawing on extreme

historical episodes such as the persecution of Jews in Nazi Germany (Buggle et al., 2023), Russian pogroms (Spitzer, 2021), or anti-German sentiments due to World War I casualties in the U.S. (Ferrara & Fishback, 2024). We add to this literature by examining the effects of a more subtle, symbolic change in government rhetoric. A further contribution is methodological: we capture migration intentions directly through revealed-preference measures of job search. This is important, because existing research shows that actual migration flows depend not only on the push factors in the origin country but also on the economic opportunities in potential destination countries (Boustan, 2007). By observing job seeker clicks, we measure migration interest even if individuals do not ultimately find a suitable job and migrate.

In addition, our paper contributes to the understanding of how narratives affect economic behavior (Shiller, 2017). Identifying causal effects of government rhetoric is difficult, as such rhetoric usually changes gradually, is accompanied by direct legal changes, and affects entire countries. Our setting allows us to address each of these challenges. First, the resolutions were adopted within a very short time period, triggered by an external and unanticipated event. Second, because laws in Poland are passed exclusively at the national level, the local government resolutions had no direct legal consequences for residents. This institutional feature allows us to isolate the effects of sudden changes in narratives from the effects of changes in the legal framework. Third, the adoption of resolutions by local governments creates spatial variation of government narratives across well-defined administrative boundaries. Recent research shows that exposure to exclusionary narratives by political leaders can increase the xenophobic behavior of majority groups (Bursztyn et al., 2020; Grosjean et al., 2023; Müller & Schwarz, 2023; Newman et al., 2021; Yanagizawa-Drott, 2014). We add to this literature by analyzing the responses of targeted groups to such narratives, showing that a change in exclusionary language is sufficient to increase migration intentions.

Finally, we contribute to the LGBTQ economics literature. Previous research has documented the overrepresentation of LGBTQ people among both international and internal migrants (Badgett et al., 2021; Badgett et al., 2024). Black et al. (2002) use cross-sectional data on U.S. cities to argue that gay men's location choices are explained primarily by differences in local amenities rather than in local attitudes. Subsequent studies, however, show that improvements in legal protections for LGBTQ individuals cause an increase in migration inflows (Baumle et al., 2023; Beaudin, 2017; Marcén & Morales, 2022). To our knowledge, we

are the first to examine the reverse scenario, i.e., the effects of actions hostile to the LGBTQ population. We show that residents respond to exclusionary rhetoric by selecting migration destinations based on the LGBTQ rights environment. This effect likely reflects the actions of both LGBTQ individuals themselves and others who disagree with the local government's stance. Although our analysis focuses on sexual orientation and gender identity, we view our findings as generalizable to other minority groups — such as ethnic or religious minorities — who face social, political, or institutional hostility (Glaeser, 2005).

2 Institutional background

In 2019, nearly 100 local governments (municipal councils, county councils, and provincial councils) voted in formal proceedings to adopt resolutions declaring their localities "free from LGBTQ ideology." The sudden wave of these resolutions was triggered by the perceived challenge to traditional family values prompted by a pro-LGBTQ declaration called the "LGBT+ Charter," signed by the mayor of Poland's capital city, Warsaw, in February 2019. This document proposed initiatives based on the European Commission's Diversity Charter, including support services for LGBTQ individuals and incorporating LGBTQ perspectives into anti-discrimination curricula in schools.[1]

Soon after the introduction of the Warsaw charter, municipal and county councils began adopting resolutions against what they termed "LGBTQ ideology" and in favor of protecting traditional family values that restricted marriage to a union between a man and a woman. While the resolutions had no direct legal consequences, the language used was stigmatizing and exclusionary, portraying LGBTQ people as a threat to families and framing advocacy for the rights of LGBTQ individuals as a harmful ideology aimed at corrupting children and destabilizing society.[2] As such, the resolutions signaled a hostile environment for LGBTQ individuals, and throughout this paper, we refer to them as anti-LGBTQ resolutions. The resolutions were adopted by the vote of council members elected in local elections.[3] In most

[1] See https://um.warszawa.pl/documents/39703/7003459/deklaracja_lgbt.pdf for the full text.

[2] The resolutions included emotionally loaded terms such as "LGBT ideology," "homopropaganda," "homoterror," "depravation," "demoralization," "sexualization' among others. See Appendix C for examples of the resolutions.

[3] Councils were elected in 2018, and anti-LGBTQ resolutions were not discussed in the electoral campaign. Subsequent local elections were held in 2024, well after our sample period.

cases, they were passed unanimously. In 2019, 52 of 380 counties witnessed the adoption of anti-LGBTQ resolutions, either by the county council or one of the municipal councils within the county. In 2020, the resolutions were adopted in four additional counties (Figure 1a).

The introduction of the resolutions was closely monitored by non-governmental organizations and was widely covered in the national media. The places that introduced them became commonly referred to as "LGBT-free zones." The wave of anti-LGBTQ resolutions was also covered by media outlets worldwide and sparked criticism from the international community. The *Factiva* database lists 878 newspaper articles in Polish and 681 international newspaper articles in English about anti-LGBTQ resolutions during our study period (Figure 1b).

In 2019, the European Parliament condemned these resolutions and urged the European Commission to ensure that EU funding was not used for discriminatory purposes.[4] Later, in 2021, it launched a formal procedure to verify whether anti-LGBTQ resolutions violated the EU Treaty and warned that the affected localities risked losing access to EU investment grants. Two years later, after the procedure was completed, a wave of local governments repealed their resolutions. The last anti-LGBTQ resolution was rescinded in April 2025.

The salience of anti-LGBTQ resolutions can be illustrated with data on the search intensity for the term "LGBT-free zone" on Google. While this search term was not used at all before 2019, its use peaked in July 2019 (in line with the peak of newspaper coverage), and it remained relatively popular throughout 2020 (see Figure 1c). Importantly, search interest was highest in the provinces with the highest incidence of anti-LGBTQ resolutions (Table A.1). This suggests that residents of the treated counties were actively looking for information about the resolutions. In places with anti-LGBTQ resolutions, negative effects on the mental health of the resident population (Meyerhoefer et al., 2025) and increased support for far-right political parties (Haas et al., 2025) have been observed.

3 Data

We use data on local governments' adoption of anti-LGBTQ resolutions from the *Atlas of Hate* (Pająk & Gawron, 2024). The dataset contains comprehensive information on the resolutions

[4]See https://www.europarl.europa.eu/news/en/press-room/20191212IPR68923/parliament-strongly-condemns-lgbti-free-zones-in-poland

passed by municipal, county, and provincial councils, including the dates of adoption, dates of repeal, voting results, links to the text of the resolutions, and minutes from the council sessions.

To capture migration intentions, we use unique data on job search from Indeed, the largest hiring platform globally. Our dataset tracks the job search activity of users located in Poland and includes over 67 million job seeker clicks on domestic and foreign job postings from 2016 to 2021.[5] For each click, we observe its date and the location of the user based on the IP address. For each job posting, we observe the advertised location of the workplace, the occupation, and the full text of the posting. We match job seeker information with the job posting information to calculate how often job seekers from county i clicked on postings in location j (country, county, municipality).

We interpret clicks on postings outside the job seeker's county as a revealed-preference measure of the intention to migrate. Job seekers are aware of the advertised location of the workplace before they click on a posting, because this information is displayed next to the job title in the search results. In addition, job seekers can filter the search results by destination. The Indeed platform can be accessed by all internet users from a desktop computer or mobile device (there is no registration requirement), so the main cost associated with online job search is the time that job seekers spend browsing postings. We exclude postings where the job location is listed as "fully remote" (representing only 0.2 percent of all Polish postings during our sample period), because in those cases the job seeker could be expressing interest in a job without intending to move. Clicks have previously been used to study individual behavior in the labor market (Adrjan et al., 2023; Le Barbanchon et al., 2023; Marinescu & Wolthoff, 2020) and in the product market (Gorodnichenko et al., 2018).

All occupational groups are represented in the data. Compared to overall employment, the occupational distribution of job postings is skewed toward high-wage occupations (Figure B.1). In contrast, the occupational composition of clicks on Indeed — our main measure of job interest — is not skewed toward high-wage occupations. If anything, elementary and clerical jobs appear to be overrepresented, which may reflect differences between the occupational composition of overall employment and that of active job seekers.

[5]Due to significant changes in Indeed's operations in Poland, the data from 2022 onward is not comparable to the data from previous years.

The key strength of the data is its ability to track job search not only within Poland but also internationally. Indeed has job sites in 60 countries, including in most of the world's largest labor markets, providing us with unique insights into cross-border job search activity. The top destination countries for clicks from Poland correspond closely with the list of countries with the highest stocks of Polish migrants (Table B.3), giving us confidence that the data is a meaningful signal of international migration intentions.

Using online job search data is associated with certain limitations. Since the platform is open to unregistered users, we have no information on the characteristics of job seekers except for their location, their online job search behavior, and their device identifier.[6] In particular, we do not have information about the sexual orientation or gender identity of the job seekers.

Another limitation is that we do not observe applications. Data on applications is not available until 2021, well into the post-treatment period. However, as Figure B.2 shows, clicks and applications in that year were highly correlated at the occupation level (in domestic search) and at the destination country level (in international search). Therefore, we believe that clicks accurately indicate job seekers' interest in working in a given location.

Online data is particularly relevant in our setting due to the widespread use of the internet for job search in Poland: 64 percent of unemployed individuals used online job search platforms in 2018 (above the European Union average of 59 percent).[7] In 2017, approximately 12 percent of individuals aged 16 to 74 used the internet to look for a job (Statistics Poland, 2020), more than double the unemployment rate of 5 percent. Moreover, while informal referrals likely play a large role in local job search, especially in rural areas, the types of job search we focus on – i.e., search for domestic out-of-county jobs and for job opportunities abroad – is likely to be dominated by online platforms.

We conduct our analysis at the level of the county (*powiat*), as this is a good approximation of a local labor market, and information on job seekers' locations may be imprecise for smaller areas. There are 380 counties in Poland. 66 of which are the so-called city counties consisting of one large municipality. We exclude the city counties from our baseline sample, as only

[6] We use the device identifier to study the effects on the intensive and extensive margins. Using the device identifier is useful for assessing the search intensity of a user over a short period of time. It is worth noting that one user may use more than one device and that they may reset their device identifiers. This makes it impossible to observe unique users over a long period (attrition rates are very high, and attrition likely does not occur at random).

[7] See Eurostat, 2023.

five of them introduced an anti-LGBTQ declaration, and they differ substantially from the remaining counties, the so-called land counties. These land counties consist of several urban and rural municipalities (usually one medium-sized town and a few smaller towns or villages). Our baseline search variable is expressed as the number of clicks per 1,000 inhabitants. We also use the information on unique device identifiers to calculate the number of job seekers per 1,000 inhabitants and the number of average clicks per job seeker. Finally, we calculate the number of postings per 1,000 inhabitants and measure the interest in these postings with average clicks per posting.

We use data on county characteristics to investigate the mechanisms behind the estimated effects. We construct geographic variables using official shapefiles and merge rich data on county-level characteristics compiled by Statistics Poland from administrative registers and surveys, as well as election results data from the National Electoral Commission. Unfortunately, there is no reliable data on migration in Poland, since the registration of residence changes is not enforced.[8] The most reliable data on population changes is collected in the decennial National Census.[9] We use census data to provide suggestive evidence of the effects on actual migration.

4 Empirical strategy

We classify a county as a treated county if the county council or at least one of the municipal councils in that county introduced an anti-LGBTQ declaration in 2019. Since almost all anti-LGBTQ resolutions were introduced in 2019, we use a standard difference-in-differences design to estimate their annual effects. In our baseline analysis, we exclude four counties that were not treated until 2020 from the sample.

[8] As a result, the scale of migration is severely underestimated in the administrative data (Wiśniowski, 2017).

[9] The Polish statistical office adjusts its annual population estimates backwards due to the discrepancies between the administrative data and the census data. This is mostly due to missing migration data. For example, in 2019, only 11,000 Polish citizens registered their departure from Poland, even though Germany alone recorded over 100,000 arrivals of Polish citizens. The data on births and deaths is considered to be reliable.

We investigate the effects using three specifications. In the first specification, we compare the treated counties to all other counties in Poland (see treated and control counties in Figure 2a). We do so by estimating the following equation:

$$y_{it} = \alpha + \theta \text{Treated}_i \times \text{PostTreatment}_t + \tau_t + \eta_i + \epsilon_{it} \tag{1}$$

where Treated_i is a binary variable equal to one for treated counties and zero for control counties, and PostTreatment_t is equal to one in 2019-2021 and zero in 2016-2018.[10] In addition, we control for time fixed effects (τ_t), and county fixed effects (η_i). The coefficient θ identifies the effects of the declarations.

Our identification rests on a key assumption that in the absence of the declarations, the treated counties would have followed trends identical to those in the control counties. While we cannot directly test this assumption, we show event study estimates to investigate deviations from parallel trends in the pre-treatment period.

The spatial concentration of "LGBT-free zones" raises doubts about the comparability of the treated counties to all other Polish counties. Counties on the western border that are less than an hour's drive from Berlin are unlikely to be a plausible control group for counties in Eastern Poland that are culturally and economically different. Accordingly, to compare areas that are most likely to experience similar shocks, we present two additional specifications based on a smaller sample of border counties. In our setting, a border county is a county that has at least one neighboring county with a treatment status that differs from its own. Specifically, the treatment group is restricted to treated counties that have at least one neighboring county that did not introduce an anti-LGBTQ declaration (all treated counties but one), and the control group consists of control counties that have at least one neighbor that introduced an anti-LGBTQ declaration (approximately 40 percent of control counties, see the map in Figure 2b). Thus, in our second specification, we estimate equation 1 while restricting the sample to border counties.

[10] In our baseline sample, four treated counties had repealed their resolutions by the end of our study period. Since repeals may have been endogenous to migration responses, we keep those counties in our sample.

Our third baseline specification exploits an even more conservative source of variation, namely the difference in the treatment status within each border county pair. This strategy has previously been used to study other policies with sharp discontinuities at county borders (Dube et al., 2010). We construct a new sample in which we include all 163 distinct county pairs. Each pair consists of two counties that share a border: one treated county and one control county. Hence, our dataset consists of 326 observations per year t. Since we consider all border county pairs, the number of occurrences of an individual treated county per year will be equal to the number of its control neighbors. Similarly, control counties may appear in the dataset more than once if they share a border with more than one treated county. We estimate the following equation:

$$y_{ipt} = \alpha + \theta \text{Treated}_i \times \text{PostTreatment}_t + \eta_i + \tau_{pt} + \epsilon_{ipt} \tag{2}$$

Thus, our baseline specification differs from equation 1 in two ways. First, p denotes the additional dimension of the dataset, a border county pair. Second, we include pair-specific time effects, τ_{pt}. Hence, we isolate the effects of all shocks that affect both members of a pair in a given year. The estimate of θ captures the average within-pair difference in differences in an outcome variable. Although this approach makes the parallel trends assumption more plausible, it requires a stronger SUTVA assumption, as cross-border spillovers could bias the results. Our estimates would be biased upward if the introduction of an anti-LGBTQ declaration in a neighboring county reduced migration intentions. This does not seem likely. Instead, it is more likely that a declaration in a neighboring county is interpreted as a more general shift in local social norms beyond administrative borders, and thus leads to an increase in intentions to emigrate. This would lead our estimates to be biased toward zero and thus against finding statistically significant results. In all specifications, we cluster standard errors at the level of the county.

Our empirical strategy does not require the treatment and control groups to be identical. In all our specifications, we control for county fixed effects, and our identification is based on variants of the parallel trends assumption. Nevertheless, it is useful to compare pre-treatment differences in observables between these two groups in each of our three specifications. Figure

3a shows that counties that introduced anti-LGBTQ resolutions differed from all remaining counties in Poland on several dimensions. In particular, as discussed above, anti-LGBTQ resolutions were concentrated in southeastern Poland. Counties with anti-LGBTQ resolutions were more socially conservative than the rest of the country, as indicated by their low divorce rates and high levels of support for the far-right *Law and Justice* party in the 2015 parliamentary elections. This suggests that, to a large extent, the decisions of local councils reflected local social norms. Moreover,the average wages in the treated counties were significantly lower than those in the control counties. Restricting the control group to border counties makes the control group much more similar to the treatment group, but the differences mentioned above remain significant (Figure 3b).

The pre-treatment differences between the two groups become much smaller after controlling for county pair fixed effects in the county-pair sample (Figure 3c). Unsurprisingly, the geographical differences are almost completely gone. The differences in support for the far right are no longer statistically significant. The treated counties recorded more births and fewer deaths and divorces than their control neighbors, but these differences are small. The difference in average wages is the only economically meaningful difference left. Thus, Figure 3 shows that our baseline specification largely reduces the differences in pre-treatment county-level characteristics caused by endogenous selection into treatment, making our difference-in-differences estimates more credible.

5 Results

5.1 Main effects on migration intentions

The introduction of anti-LGBTQ declarations by local governments significantly increased out-of-county job search (Table 1). The results from our preferred baseline specification with county pair-specific time fixed effects are shown in columns 3 and 6. Domestic postings attracted around 41 additional clicks per 1,000 inhabitants, which corresponds to 12 percent of the pre-treatment mean. The effects for foreign postings are much smaller in absolute terms, which suggests that individuals affected by anti-LGBTQ resolutions were more likely to opt for internal migration. In relative terms, the effects are similar: an additional 3.3 clicks

per 1,000 inhabitants implies a 15 percent increase. The estimates for alternative samples and specifications (columns 1-2 and 4-5), although less precisely estimated, are similar to our baseline estimates.

Event study estimates show no significant divergence in job search behavior during the pre-treatment period and sustained effects during the post-treatment period (Figure 4). Looking at quarterly data, the timing of the surge in search behavior in the third quarter of 2019 (Figure A.3) aligns with a spike in Google search intensity for "LGBT-free zones" and media coverage (Figure 1), further supporting the salience of the resolutions.

To investigate whether the increase in migration intentions was driven by new job seekers entering the search pool or by more intensive search activity among existing users, we decompose the treatment effect along the extensive and intensive margins. We use unique device identifiers to measure the number of users with at least one click (extensive margin) and the average number of clicks per user (intensive margin). Our findings indicate that the effect is driven by the extensive margin responses. Domestic job postings attracted an additional 5.1 job seekers per 1,000 residents (9 percent increase) while international job postings attracted an additional 0.5 job seekers per 1,000 residents (11 percent increase Table 2). We find precisely estimated zero effects on clicks per user, indicating the lack of a sizable impact on job search intensity. Taken together, these results suggest that anti-LGBTQ resolutions primarily affected residents who had not previously been searching for a job.

The group of new job seekers who began to consider migration due to the resolutions may have looked for different types of jobs and have had different wage expectations than existing job seekers. We decompose the effects by the occupation of the job posting to explore these differences. We classify postings into high-, middle-, and low-wage postings based on the 1-digit ISCO code for each posting and the average wage for that occupational group from the 2018 EU Structure of Earnings Survey. We then estimate the effects separately for each occupational category. We find statistically significant positive effects on job search, with point estimates of similar magnitudes, for all three categories (Table 3, columns 1-3 and 5-7).

We predict the log wage for each posting based on its ISCO 1-digit code to shed light on potential effects on wage expectations (wages are rarely posted directly in Polish job postings). We estimate a model with the log average predicted wage of the posting as the dependent

variable. We find a precisely estimated zero change in the average wage of clicked postings for both domestic and international job search (Table 3, columns 4 and 8). These results suggest that the migration intention response was broad-based and not confined to specific occupations or wage levels. This may reflect a lack of effect on the skill composition of job seekers. However, it is also consistent with a mechanism in which the affected individuals were relatively highly skilled, but were willing to consider lower-paying jobs as a trade-off for improved social and legal conditions.

5.2 Destination-specific patterns

We use data on the location of job postings to examine how anti-LGBTQ declarations affected intended migration destinations. The declarations likely influenced two main groups: LGBTQ individuals themselves and non-LGBTQ individuals who felt alienated by the hostile environment, such as friends and family of LGBTQ individuals or others whose social views conflicted with those expressed by the local government. Accordingly, we expect the strongest effects to be for destinations perceived to be open to LGBTQ individuals.

We obtain results consistent with this hypothesis for both domestic and international search. Within Poland, the increase in job search activity was concentrated in counties that did not introduce anti-LGBTQ resolutions (Figure 5a). Interestingly, the effect was not limited to urban centers. This suggests that distance or preferences for amenities typical of rural counties were also important for the destination choice. We find precisely estimated zero effects for postings in counties that introduced anti-LGBTQ declarations. This suggests that individuals were aware of the declarations in other counties and excluded those locations from their search.

The effects on international migration intentions were also concentrated in LGBTQ-friendly destinations. Specifically, the increase in job search activity was entirely driven by postings in European countries that had legalized same-sex marriage (Figure 5b). We find no effects for European countries that did not permit same-sex marriage. In other words, we find no effects on intentions to migrate to countries with low standards of protection of LGBTQ rights, including those in close proximity. We find no effect for non-European destinations, including the United States, regardless of their legal stances on same-sex marriage. These patterns

suggest that geographic proximity and institutional barriers to mobility outside the European single labor market also played a role.

To disentangle the influence of LGBTQ rights from other variables such as economic opportunities and geographic distance, we first run Poisson regressions to estimate the relative treatment effects for each of the top 35 destination countries.[11] We then regress the estimated coefficients on various measures of LGBTQ rights while controlling for each country's GDP per capita, unemployment rate, geographic distance from Poland, and membership in the European single labor market. The relative treatment effects remain positively and significantly correlated with LGBTQ rights, even conditional on these controls (Table A.2). The effects remain significantly larger for European countries, likely driven by the lower costs of migration due to geographical proximity and free access to labor markets in the European Economic Area.

5.3 Heterogeneity of the treatment effects

We examine the heterogeneity of the effects of anti-LGBTQ resolutions on domestic and international search with two objectives in mind. First, we assess the relevance of key migration drivers identified in the literature: distance, income differentials, and human capital. Second, we explore whether the resolutions were interpreted as a signal of local social norms. To this end, we investigate the heterogeneity of the effects depending on the pre-treatment political leanings and the level of the government at which the resolutions were passed.

We begin by residualizing the variables measuring dimensions of heterogeneity, as they are strongly correlated with each other. Specifically, for each heterogeneity dimension, we regress it on the other heterogeneity variables and keep the residuals. Next, for each dimension, we split our sample of county pairs into two halves based on the pre-treatment value of each residualized variable (smaller than the median and greater than or equal to the median).[12] The effects in the subgroups are less precisely estimated but provide suggestive evidence on the mechanisms underlying the job search responses (Table 4).

[11] Countries with at least 10,000 clicks recorded during our study period.

[12] Since a treated county may have several control neighbors, the number of observations in each half may be different.

The effect on international migration intentions is strongest in counties located closer to Poland's western border with Germany, suggesting that lower costs of migration driven by geographic distance may play a role. For domestic and international job search, the effects are most pronounced in low-wage counties, pointing to the importance of wage differentials between the origin and the destination for migration decisions.

Educational attainment also influences migration responses. Treated counties with above-median shares of college-educated residents exhibit stronger effects on both internal and international migration intentions. This may reflect better language skills, better access to information, or a broader set of job opportunities for educated individuals, all of which reduce the costs of migration. Hence, even though we observe no large changes in the occupational composition of the jobs that people searched for, the counties with relatively well-educated populations were affected the most. For Poland as a whole, this might suggest some degree of brain drain. These results are consistent with experimental studies showing that preferences for environmental, social, and governance practice amenities (ESG) are strongest among highly educated individuals (Colonnelli et al., 2025).

Next, we investigate the variation in the treatment effects depending on far-right voting to test whether the effects can be explained by the mechanism of updating beliefs about local social norms. In such a mechanism, the size of the effect would depend on the scale of the misperceptions. In other words, more unexpected resolutions should lead to larger migration responses. Consistent with this reasoning, we find large effects in counties with low levels of far-right voting. In these areas, the adoption of the resolutions likely came as a surprise, signaling an unexpected shift toward exclusionary norms and prompting a job search response. By contrast, counties with high levels of far-right voting experienced small and statistically insignificant responses, as residents in these areas likely would have been aware of strong local anti-LGBTQ sentiments before the resolutions were passed.

Finally, we investigate whether the size of the effect varies depending on the level of government that adopted the resolutions (Table 5). We find that the effects on migration intentions are large and significant in counties with municipal council resolutions. By contrast, the effects in counties where county councils introduced anti-LGBTQ resolutions are small and statistically insignificant. This may reflect the greater salience of municipal-level decisions,

which are more visible to local residents. Municipal councils have much larger budgets and are responsible for a wider range of policies than county councils, so their actions are more closely followed by the citizens.[13] Moreover, their actions may be interpreted as a more precise signal of local social norms.

5.4 Robustness

Our results are robust to a number of sensitivity tests. First, one concern is that users may manipulate their location using Virtual Private Networks (VPN). Some internet bots may pass through Indeed's filtering mechanisms, potentially introducing noise. We perform a leave-one-out test by sequentially excluding all county pairs that include a given treated county from the sample to make sure that our results are not affected by outliers. The estimated treatment effects remain stable across iterations, indicating that no single county is driving our results (Figure A.4).

Second, clicks on job postings located in counties that directly neighbor the county of residence may reflect commuting intentions rather than migration intentions, affecting our interpretation of the results. Moreover, the county of residence may be incorrectly identified for users living near a county border due to the limited precision of geolocation, resulting in the misclassification of clicks as out-of-county clicks. To address these concerns, we distinguish between clicks on job postings located in counties that are direct neighbors of the county of residence and those located in non-neighboring counties. Table A.4 shows that the effects are driven by destination counties that are not direct neighbors of the counties of residence of the users, and the impact on job search in neighboring counties is small and statistically insignificant.

Third, we test the sensitivity of our standard errors by applying alternative clustering levels. Our baseline clustering at the county level yields the most conservative estimates, and the results remain robust under alternative specifications (Table A.5).

Fourth, we test the sensitivity of our estimates to population weighting and alternative sample restrictions (Table A.6). In the baseline, we abstract from differences in county sizes. Weighting observations by the pre-treatment population increases the estimated coefficients

[13]The total revenue of municipal councils is over four times larger than the total revenue of county councils.

slightly. Including city counties reduces the international search effect – likely due to the presence of large urban control counties like the capital city of Warsaw – but does not affect the results for domestic search. If instead we only add the treated city counties, the effects on international search are identical to our baseline estimates. Including the four counties where the resolutions were not adopted until 2020 leads to estimates that are slightly smaller but still statistically significant. This is expected since these additional counties were treated for a shorter period. Finally, omitting counties on Poland's national border does not alter the estimates. This mitigates concerns about measurement error from IP geolocation around borders or disproportionate international migration from border areas.

5.5 Effects on other local outcomes

Finally, to explore whether other demographic, social, or economic channels might explain the increase in migration intentions, we estimate treatment effects on alternative outcomes. For comparability, all variables are standardized with a mean of zero and a standard deviation of one.

First, we investigate effects on demographic outcomes. We find no significant effects on births, deaths, or marriage formation (Figure 6, panel A). In particular, the lack of effect on the death rate is reassuring, as it implies that the Covid-related mortality patterns were very similar in the treated and the control counties. This suggests that the Covid-19 pandemic did not influence our estimates. We find a statistically significant increase in divorce rates. Controlling for endogenous divorce rates does not impact our estimates, so it is unlikely to be a major driver of the effects on migration intentions (see Table A.7).

Overall, deteriorating local labor market conditions do not appear to explain the observed increase in outbound job search. We find no statistically significant effects on unemployment rates or the number of job postings, suggesting that local labor demand remained stable (panel B). Similarly, there is no impact on the presence of foreign firms. While local anti-LGBTQ declarations may have influenced the relocation decisions of foreign corporations with inclusive policies, the cost of violating internal standards may have been lower than the cost of relocation – at least in the short term, although we cannot rule out long-term effects. We

do find small, marginally significant increases in average employee wages, which may reflect efforts to attract replacement workers for those who emigrated.

Interestingly, we see modest increases in public investment and EU funds received by the treated counties and municipalities. These increases may reflect anticipatory responses by local authorities concerned about potential future funding restrictions from the European Commission. As early as 2019, EU institutions began warning that anti-LGBTQ policies could jeopardize access to EU grants. In 2021, the European Commission initiated formal procedures against the Polish government and cautioned local councils that EU investment funds would be frozen if the declarations were not repealed. The short processing time of the applications for EU funds may have facilitated an anticipatory response from local governments. As the additional EU funds were not sufficient to cover increased investment expenditure, we also see that the budget deficits of the treated local governments widened significantly. This is likely due to the requirement that local governments cover a large portion of the costs for EU-funded projects using their own resources.

We believe that increased public investment is unlikely to explain the effects on migration intentions. We find no effects on unemployment or vacancies, which suggests that fiscal changes did not fundamentally alter economic opportunities, at least in the short run. If new investments affected the perception of future economic opportunities, we would expect our estimates of the effects on migration intentions to be biased toward zero. As suggestive evidence, we show that controlling for measures of government investment does not change our estimates of the effects on migration intentions (Table A.7).

Last, we investigate the effects on public safety. The introduction of anti-LGBTQ declarations may have changed the perceptions of social approval of violent attacks against LGBTQ people or, more broadly, against people seen as "different." These declarations may have also adversely affected the mental health of LGBTQ individuals, potentially leading to self-harm or deaths of despair. However, the effects on crime, fire accidents, car accidents, deaths due to external causes, and suicides are small and statistically insignificant. While this does not rule out the possibility that public safety was impacted, it suggests that the declarations did not have substantial short-term effects on the most severe potential outcomes.

5.6 Effects on inbound job search

In addition to increasing emigration intentions, anti-LGBTQ resolutions may have influenced intentions to immigrate to the treated counties. First, the loss of workforce might have caused general equilibrium effects, attracting job seekers from other counties through higher wages. Second, these resolutions may have made the treated counties more appealing to anti-LGBTQ individuals from other areas. On the other hand, anti-LGBTQ resolutions might have also discouraged the immigration of LGBTQ individuals or progressive individuals who would have considered moving to the treated counties in the absence of such resolutions. To assess whether anti-LGBTQ declarations might have attracted migrants, we construct a measure of inbound job search. For each county, we calculate the number of clicks on postings of jobs that were located in that county, but were made by users located in another county or outside Poland. This is directly comparable to our measure of outbound search.

We find a small and statistically insignificant effect on overall inbound search (column 1 in Table 6). The point estimate is twice as small as the effect on outbound migration intentions (the sum of columns 3 and 6 in Table 1). In relative terms, this implies a 4 percent increase in inbound migration intentions as opposed to a 12 percent increase in overall outbound migration intentions. Although the effects on net migration intentions are negative (i.e., the effects on outbound migration intentions exceed the effects on inbound migration intentions), we lack the precision to establish statistical significance (Table A.3). Nevertheless, it appears that the effects on net migration intentions are smaller than the effects on outbound migration intentions due to a partially offsetting rise in inbound search.

Similarly to outbound search, we decompose the effects on inbound search by occupational group to shed light on the preferences of additional job seekers. Unlike in the outbound search, the effects on inbound search are concentrated in a single occupational group. The only statistically significant positive effect appears for middle-skilled job postings, but it is still smaller than the corresponding outbound search effect (sum of columns 2 and 6 in Table 3) in both absolute and relative terms.

6 Effects on population changes in census data

Due to the data limitations mentioned above, we cannot test directly whether there was a rise in actual outbound migration from the areas where local governments made anti-LGBTQ declarations. Instead, we use decennial Polish census data (2011 and 2021) and provide suggestive evidence on actual net migration by investigating cohort-specific population changes in our county pair dataset.

Instead of time, our third dimension (in addition to county and county pair) is a birth cohort denoted by g and indicating the age of the cohort in 2021. We investigate population changes for individuals who were 1-17 years old in 2011 since their initial place of residence is the outcome of the decision of their parents and not their own decision. We estimate the following event-study equation:

$$\Delta^{2021-2011} y_{ipg} = \alpha + \rho y_{ipg}^{2011} + \sum_{\substack{g=11 \\ g \neq 17}}^{27} \theta_g \text{Treated}_i \times C_g + \eta_i + \tau_{pg} + \epsilon_{ipg} \tag{3}$$

where $\Delta^{2011-2021} y_{ipg}$ is the 2011-2021 difference in the log population of cohort g in county i in county-pair p. Our reference category is the cohort of individuals who were 17 years old in 2021 and 7 years old in 2011. We control for county fixed effects and pair-specific cohort fixed effects. Hence, the coefficients θ_g measure the effects relative to the reference cohort while accounting for overall population changes in a given county and cohort-specific population changes in a given county pair.

Underage individuals in Poland cannot make their own migration decisions, so we treat the cohorts aged 11-17 as placebo cohorts. By contrast, young adults are the most mobile group. They face relatively low costs of migration, because they often have not yet formed a family, and they are at the beginning of their career. Around the age of 19, they choose whether to go to university, which often requires moving to a university town. Finally, after graduation, they can decide whether to return to their hometown, to stay in the university town, or to move somewhere else seeking better opportunities. Population changes in these cohorts likely indicate net migration (both internal and international) due to their very low mortality.

Importantly, surveys consistently find that the prevalence of LGBTQ identification is highest among young cohorts. Therefore, we expect to observe the largest effects on migration for these cohorts.

A major limitation of this approach is that we cannot distinguish between population changes in the pre-treatment period (2011-2018) and the post-treatment period (2019-2021). By attributing these event study estimates to the effects of anti-LGBTQ resolutions, we make the relatively strong assumption that there are zero effects in the years 2011-2018. This would not be true if, within county pairs, there were some cohort-specific differences in moving patterns between the control counties and the treated neighbors (e.g., if the treated counties always experienced large outflows of young adults). We address this by estimating the event study equation for population changes between the censuses in 2002 and 2011.

The second important limitation is a potential violation of the SUTVA assumption. While underage individuals cannot make their own migration decisions, anti-LGBTQ resolutions may incentivize their parents to migrate (e.g., to protect their children from a hostile environment). This would bias our estimates toward zero. On the other hand, our estimates would be biased in the direction of observing significant negative results if anti-LGBTQ resolutions caused the immigration of conservative parents from other counties. The fact that children almost always move with their parents is another reason for not including older cohorts in our study.[14] An additional violation of SUTVA would occur if individuals deciding to leave the treated counties left for the neighboring control county. Such individuals would be double-counted and hence would inflate the estimates (increase the absolute value of the negative coefficient).

We find imprecise negative effects of the resolutions on population changes among young adults (Figure 7). We observe the largest effects on the cohorts that recently graduated from university (22-26 years old). Relative to the reference cohort, the treated counties witnessed a population decline in these cohorts of approximately 1.5 percent. The average effect for the young adult cohorts is equal to -0.8 log points and is marginally insignificant. We find zero effects for the cohorts younger than 17 years old, which suggests that the parallel trend assumption likely holds.

[14] In Poland, most adults aged 28-39 years old have at least one child, so population changes in these cohorts are mechanically linked to population changes in the cohorts of their children aged 11-17 years old.

The point estimates for the cohorts 22-26 years old are economically large,as the anti-LGBTQ resolutions led to a net emigration of approximately 1.5 percent among this group. How do these effects compare to the share of the population that identifies as LGBTQ? First, it is important to note that affected individuals were 12-16 years old in the 2011 census. Hence, the initial population shares should be identical to the shares for the general population. This would not be true for adults, as existing studies show that LGBTQ individuals tend to move to large cities and locations with progressive social norms. Unfortunately, there are no estimates of the population shares of LGBTQ individuals in Poland. Badgett et al. (2021) present the population shares for adults in the U.S. in 2014-2018 using representative survey data. They find that 3.9 percent of people identify as bisexual or homosexual, and 0.5 percent of people identify as transgender. Assuming that the share of people who identify as LGBTQ in the studied cohorts is equal to 4.4 percent and that the effects are entirely driven by the migration of LGBTQ individuals, our estimates would imply that more than one-third of LGBTQ individuals emigrated from the treated counties. This is a very large effect. For comparison, approximately 54 percent of German Jews left Nazi Germany between 1933 and 1939. We offer two explanations for the magnitude of the effects. First, the estimated effects include both internal and international migration. Moving to another Polish town is much less costly than moving abroad. Second, the effects of anti-LGBTQ resolutions may extend to peers of LGBTQ people, members of other marginalized groups, and those who are dissatisfied with the dissonance between their own beliefs and the majority views reflected in these resolutions.

We find that the effects are particularly pronounced among women (Figure A.5). The negative impact on population changes among young adult women amounts to approximately 1.1 log points, and these effects are statistically significant. These stronger responses of women may be due to the perception that anti-LGBTQ norms are correlated with traditional gender roles, and due to young women having significantly more socially progressive views than men (Langsæther & Knutsen, 2025).

Finally, we conduct a placebo test using census data from 2002 to 2011 to rule out pre-existing differential cohort-specific migration patterns. We find no significant effects on population changes between 2002 and 2011, which strengthens the credibility of our interpretation of the 2011-2021 results (Figure A.6).

7 Conclusion

This paper shows that exclusionary government rhetoric influences migration decisions. Anti-LGBTQ resolutions of local governments had no direct legal consequences, and yet they led to a large increase in migration intentions. Heavily publicized, the resolutions likely served as a signal of local social norms, affecting expectations about future discrimination against targeted minorities. Consistent with this mechanism, we show that the effects were particularly large in places with the lowest levels of prior support for far-right political parties.

In absolute terms, the effects on domestic job search were much larger than the effects on international job search. For each additional user interested in international job postings, there were approximately 11 additional users interested in domestic out-of-county job postings. Hence, internal migration to counties without anti-LGBTQ resolutions was the most common choice for the affected individuals. However, in relative terms, the effects on international migration intentions were substantial relative to the pre-treatment mean. Poland's membership in the European single market facilitated international migration responses, as the effects were entirely due to interest in job postings in LGBTQ-friendly destination countries in the European Union, even when accounting for economic and geographic factors. Our results are thus particularly relevant in the context of EU countries and federal states with single labor markets and varying local policies.

The overall impact on demographic and economic outcomes in the treated counties was more complex. While we observe an increase in outbound migration intentions, this effect appears to have been partially offset by statistically insignificant increases in inbound search. These increases might have been driven by anti-LGBTQ individuals attracted by the resolutions or modest general equilibrium effects, such as slight increases in local wages. Nevertheless, suggestive evidence based on census data indicates that net population impacts were likely negative, driven by young adult women.

The major implication of these findings for policy makers is that government rhetoric alone is sufficient to influence major economic decisions. While the impact of messaging has long been recognized in the context of monetary policy (Blinder et al., 2008), we show that even when the government cannot credibly commit to fulfilling its promises (in our case, due to the lack of legal tools), its narratives affect residents' location preferences, likely through

updating their beliefs about the prevailing social norms. Consequently, exclusionary rhetoric is a powerful tool that may be used to drive targeted groups away and improve the electoral performance of anti-minority politicians. Indeed, this may explain why Haas et al. (2025) find that the treated locations recorded an increase in far-right voting following the introduction of anti-LGBTQ resolutions. Future studies could test for the asymmetry of these effects by investigating the impact of inclusive narratives.

In addition, our findings have important implications for researchers exploiting spatial variation in exclusionary narratives or discriminatory policies. The effects of such policies on the outcomes of minority groups may be severely underestimated if they fail to account for selective migration responses. The most affected individuals are the most likely to leave, and we show that even subtle changes in government rhetoric may induce relatively large migration responses.

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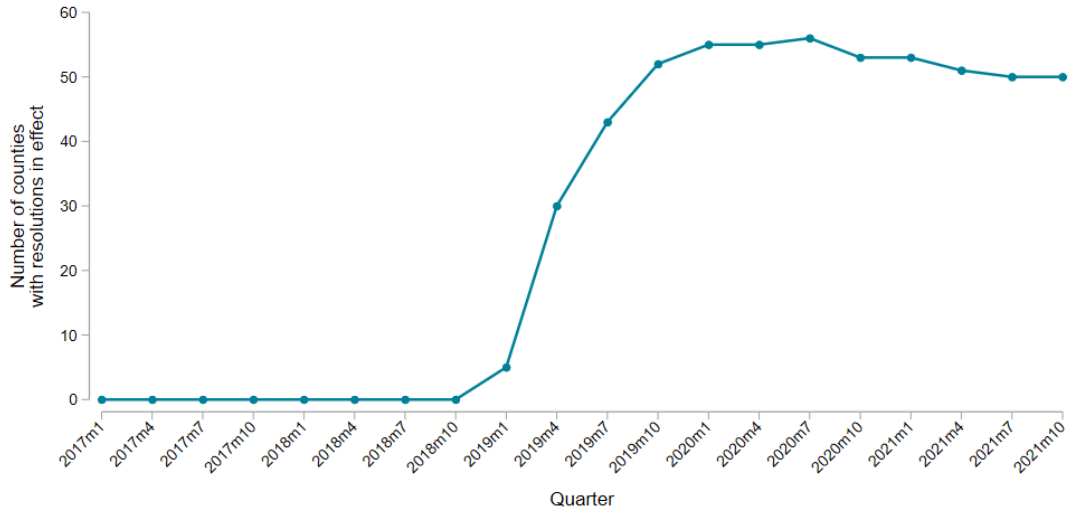
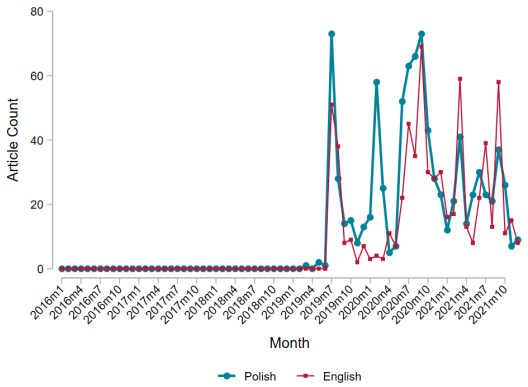
Figures

(a) Number of counties with enacted anti-LGBTQ resolutions



(b) Newspaper articles



(c) Google search interest

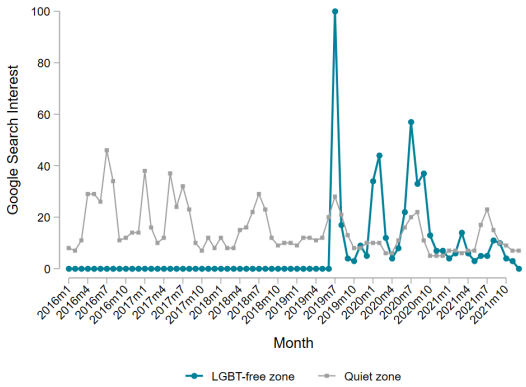
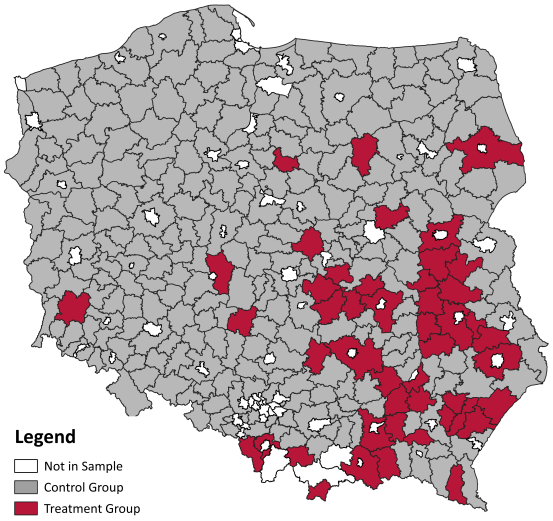


Figure 1: Adoption and salience of anti-LGBTQ resolutions

Notes: Figure 1a shows the number of counties in which the county council or at least one municipality council has introduced an anti-LGBTQ resolution that has not yet been repealed. Figure 1b shows the number of articles in the Factiva database that contain the phrase "LGBT-free zone" in Polish and English (in English, we additionally required an article to contain the word "Poland". Figure 1c shows the evolution of search intensity for the search term "LGBT-free zone" ("strefa wolna od LGBT"). For comparison, we show search interest for an alternative search term "quiet zone" ("strefa ciszy"), which refers to areas designated as quiet zones in train carriages. Search interest is represented on a scale from 0 to 100, where 100 corresponds to the maximum search interest for the term within Poland between 2016 and 2021.

(a) All-county sample



(b) Border county sample

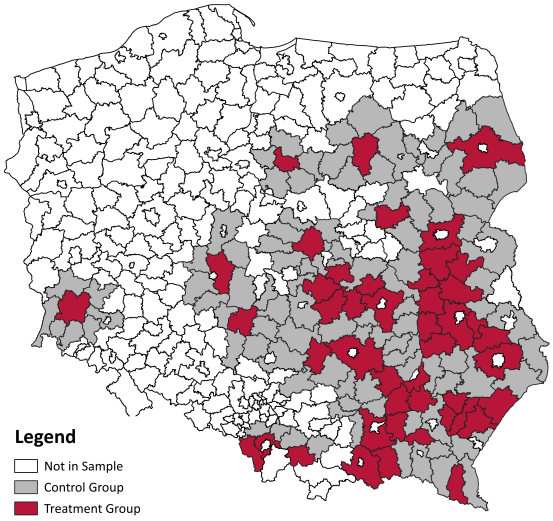


Figure 2: Treatment and control counties

Notes: The map in Figure 2a shows treated and control counties in the all-county sample. City counties and counties that became treated in 2020 are excluded from the sample. The map in Figure 2b shows treated and control counties in the border county sample. City counties, counties that became treated in 2020, and control counties that have no treated neighbors are excluded from the sample.

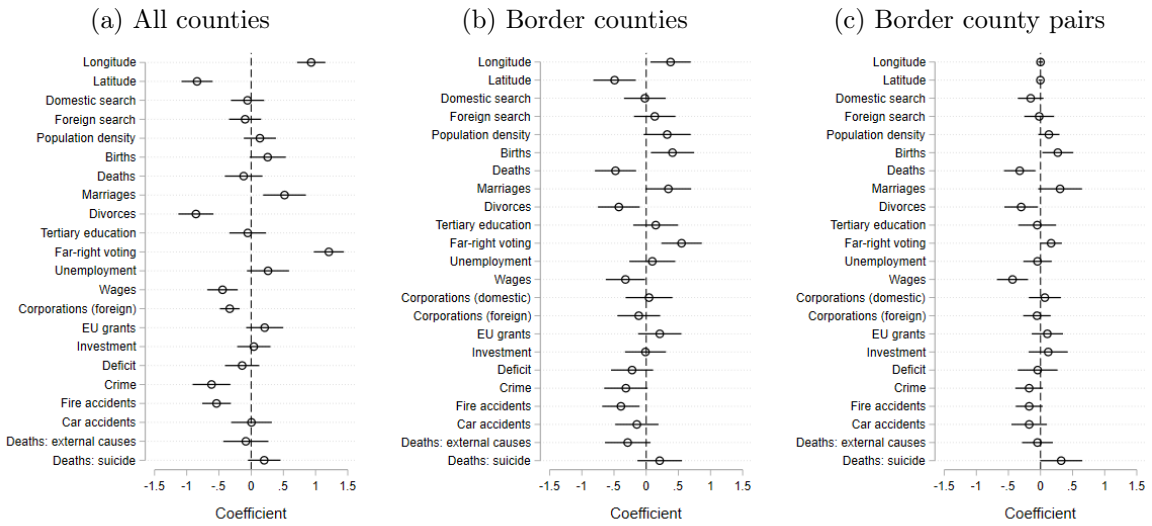


Figure 3: Correlation between the treatment variable and pre-treatment county characteristics.

Notes: The figure shows coefficients and 95% confidence intervals from regressions of selected county characteristics on the treatment group binary variable for three specifications. Figure 3a shows the results for the sample consisting of all counties and a regression with a single independent variable: the treatment group binary variable. Figure 3b shows the results for the sample consisting of border counties and a regression with a single independent variable: the treatment group binary variable. Figure 3c shows the results for the sample consisting of border county pairs and a regression that includes the treatment group variable and county pair fixed effects. All outcomes are measured in 2018. The two exceptions are tertiary education (measured in the 2002 census) and far-right voting (vote shares of *Law and Justice* in the 2015 parliamentary elections). All outcomes are standardized with a zero mean and a standard deviation of one. Standard errors are clustered at the county level.

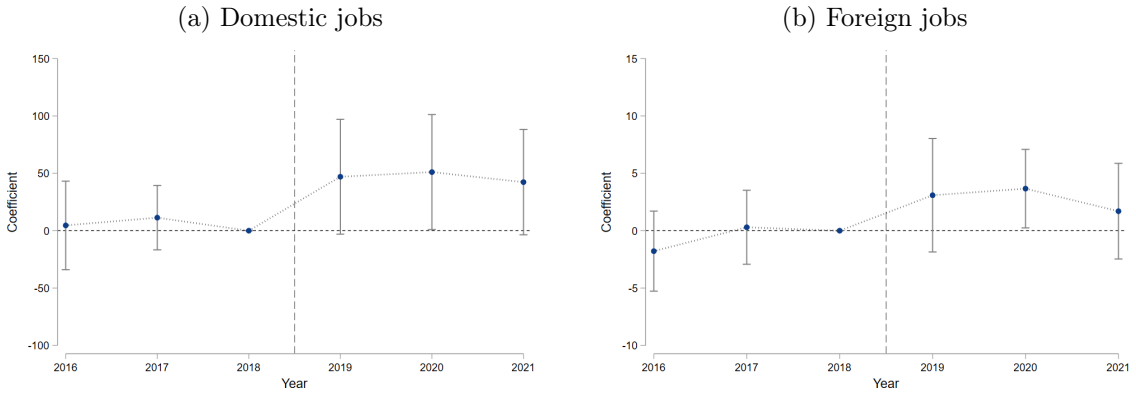
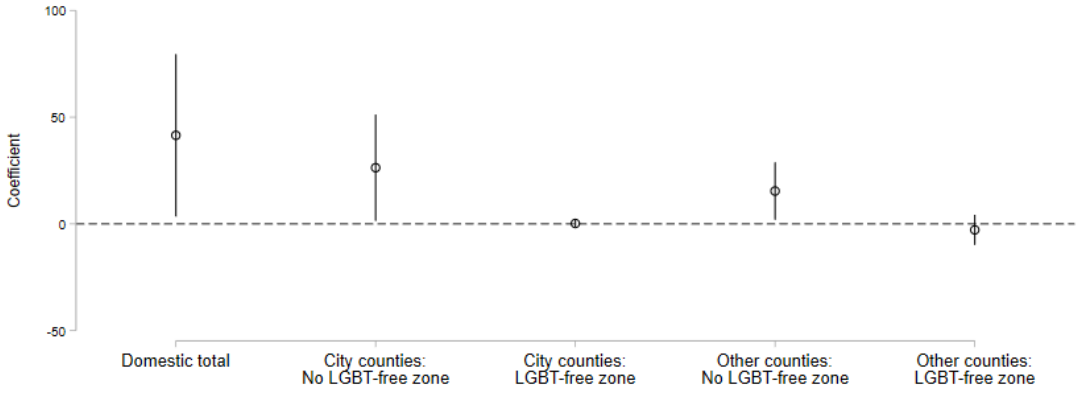


Figure 4: Effects on migration intentions: Event study

Notes: The figure shows the event-study coefficients and 95% confidence intervals from regressions of the out-of-county job search on the interaction of year fixed effects and the treatment group binary variable. In Figure 4a, the outcome variable is the number of clicks on job postings located in Poland (outside of the county of residence) per 1,000 inhabitants. In Figure 4b, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. Regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county. Figures A.1-A.2 show the results for alternative samples and specifications.

(a) Domestic jobs



(b) Foreign jobs

Figure 5: Effects on migration intentions by job posting location

Notes: The figure shows the event-study coefficients and 95% confidence intervals measuring the effects of the anti-LGBTQ declarations on job search activity depending on the location of the job posting. Job search activity is measured as the number of clicks on job postings per 1,000 inhabitants. Figure 5a shows the decomposition for domestic job search by county type (city, county or land county) and the anti-LGBTQ resolution status. Figure 5b shows the decomposition for international job search by continent (Europe vs. other continents) and same-sex marriage legislation. Regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.

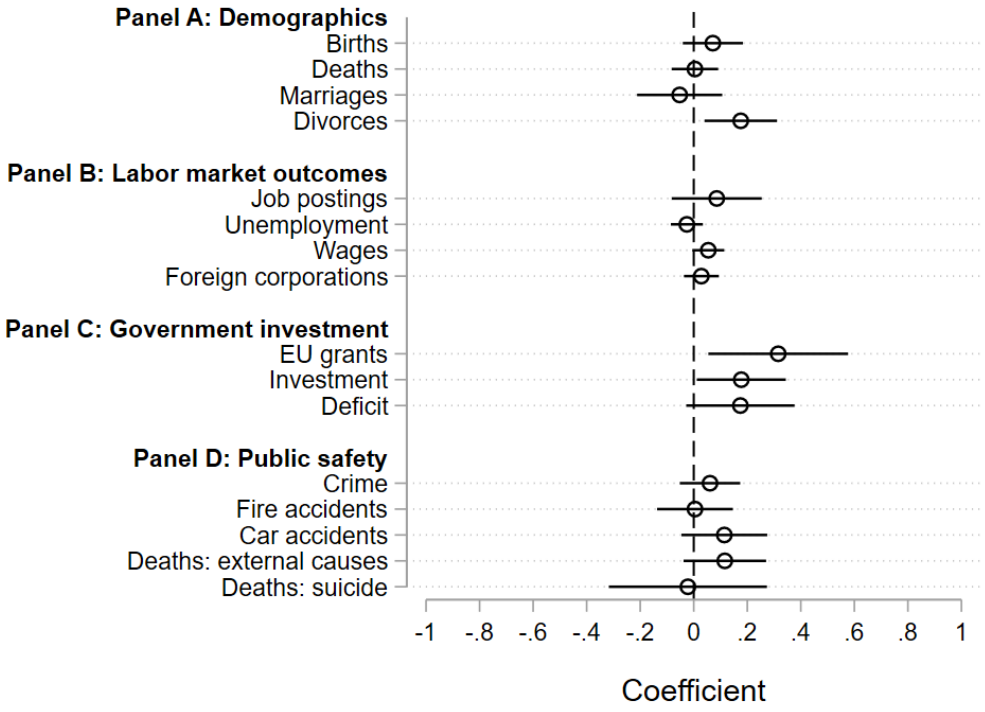


Figure 6: Alternative explanations

Notes: The figure shows the treatment effects for alternative outcome variables. The figure shows the point estimates of the treatment effects and 95% confidence intervals. For ease of interpretation, the outcome variables are first divided by population (except for average wages) and then standardized with a zero mean and a standard deviation of one. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.

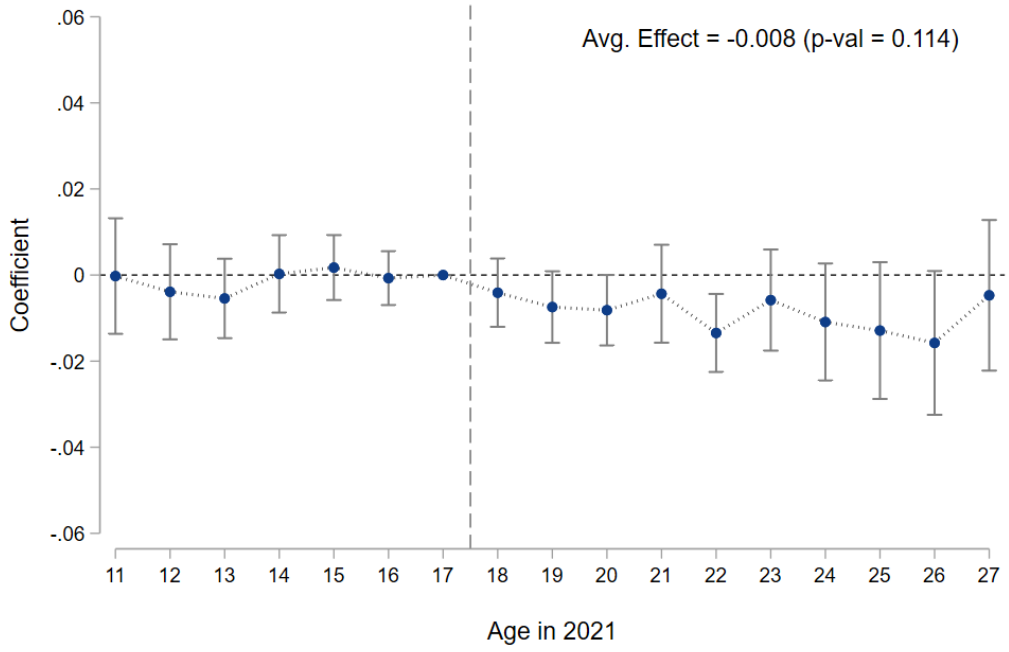


Figure 7: Effects on population change: Birth cohort event study

Notes: The figure shows the event-study coefficients and 95% confidence intervals from regressions of log population changes (2011-2021) on the interaction of birth cohort fixed effects and the treatment group binary variable. Regressions are estimated using the county pair sample. We control for log population in 2011, county fixed effects and pair-specific birth cohort fixed effects. Standard errors are clustered at the level of the county.

Tables

Table 1: Effects on migration intentions

	Domestic job postings			Foreign job postings		
	All counties	Border counties	Border county pairs	All counties	Border counties	Border county pairs
	(1)	(2)	(3)	(4)	(5)	(6)
LGBT-free zone × Post-treatment	50.509**	52.080*	41.494**	2.864	3.539	3.313**
	(24.990)	(27.261)	(19.407)	(2.125)	(2.309)	(1.462)
Mean of outcome	376.00	362.83	352.64	25.39	22.75	21.63
Observations	1,860	870	1,956	1,860	870	1,956
County FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓		✓	✓	
County pair FE x Year FE			✓			✓

Note: The table shows the effects of the anti-LGBTQ declarations on out-of-county job search activity. Job search activity is measured by the number of clicks on job postings per 1,000 inhabitants. In columns 1-3, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In columns 4-6, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. Standard errors are clustered at the county level.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 2: Effects on migration intentions: Extensive and intensive margins

	Domestic job postings		Foreign job postings	
	Users per 1,000 inhabitants	Clicks per user	Users per 1,000 inhabitants	Clicks per user
	(1)	(2)	(3)	(4)
LGBT-free zone × Post-treatment	5.068*	0.091	0.478**	0.194
	(2.596)	(0.119)	(0.215)	(0.248)
Mean of outcome	56.47	5.96	4.38	4.76
Observations	1,956	1,956	1,956	1,954
County FE	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on out-of-county job search on the extensive and intensive margins. Columns 1-2 show the effects for domestic out-of-county search and columns 3-4 show the effects for foreign postings. Columns 1 and 3 show the effects on the extensive margin: the outcome variable is defined as the number of unique users with at least one click divided by the population (in thousands). Columns 2 and 4 show the effects on the intensive margin: the outcome variable is defined as the number of clicks per user. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 3: Effects on migration intentions: Occupations and predicted wages

	Domestic Job Postings				Foreign Job Postings			
	High-wage	Middle-Wage	Low-Wage	Average Wage	High-wage	Middle-Wage	Low-Wage	Average Wage
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LGBT-free zone × Post-treatment	11.142*	17.114**	13.068*	-0.003	1.276*	0.891**	1.108**	-0.009
	(5.744)	(6.662)	(7.451)	(0.003)	(0.725)	(0.439)	(0.426)	(0.008)
Mean of outcome	108.98	132.66	108.62	1.68	9.34	6.19	5.79	1.79
Observations	1,956	1,956	1,956	1,956	1,956	1,956	1,956	1,954
County FE	✓	✓	✓	✓	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on out-of-county job search. Columns 1 and 5 show the results for job postings for high-skilled occupations (ISCO 1-3), columns 2 and 6 show the results for job postings for middle-skilled occupations (ISCO 4, 7, 8), and columns 3 and 7 show the results for job postings for low-skilled occupations (ISCO 5, 6, 9). In columns 4 and 8, the dependent variable is the log average wage of postings clicked on by individuals proxied by the postings' ISCO 1-digit occupational groups (using data on wages from the 2018 EU Structure of Earnings Survey). We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 4: Heterogeneity of the effects

	Longitude		Average wage		Tertiary education		Far-right voting	
	Below median	Above median	Below median	Above median	Below median	Above median	Below median	Above median
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
				Panel A. Domestic jobs				
LGBT-free zone × Post-treatment	30.585	53.981**	56.671**	24.119	17.574	66.927**	69.930**	12.706
	(26.973)	(26.538)	(27.915)	(24.604)	(22.186)	(31.068)	(29.648)	(23.340)
Mean of outcome	404.39	293.39	369.93	332.84	319.56	387.81	356.01	349.22
Observations	1,044	912	1,044	912	1,008	948	984	972
				Panel B. Foreign jobs				
LGBT-free zone × Post-treatment	4.333**	2.146	4.138*	2.369	0.824	5.960**	4.452*	2.160
	(1.892)	(2.243)	(2.197)	(1.817)	(1.612)	(2.356)	(2.328)	(1.696)
Mean of outcome	24.58	18.25	20.94	22.41	17.76	25.74	21.03	22.23
Observations	1,044	912	1,044	912	1,008	948	984	972
County FE	✓	✓	✓	✓	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on job search. Longitude is time-invariant. In the case of the average wage, tertiary education, and far-right voting, we take the most recent pre-treatment value. Variables measuring dimensions of the heterogeneity are strongly correlated. Therefore, for each variable, we calculate residuals from a regression of this variable on other dimensions of heterogeneity and show the effects for county pairs with residuals below and above the median (based on residuals for the treated county in the pair). We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 5: Effects on job search: Municipal vs. county council resolutions

	Domestic job postings		Foreign job postings	
	Municipal	County	Municipal	County
	(1)	(2)	(3)	(4)
LGBT-free zone × Post-treatment	64.040***	21.304	5.779***	0.093
	(23.472)	(21.200)	(1.660)	(1.510)
Mean of outcome	350.01	362.77	22.12	21.35
Observations	1,464	1,224	1,464	1,224
County FE	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on job search depending on the level of the government that introduced the anti-LGBTQ declarations. Columns 1-2 show the effects for domestic out-of-county search, and columns 3-4 show the effects for foreign postings. Columns 1 and 3 show the effects for counties with at least one municipal-level declaration. Columns 2 and 4 show the effects for counties where the county council introduced an anti-LGBTQ declaration. The intersection of the two groups is not an empty set, as there are counties where both county councils and municipal councils introduced anti-LGBTQ resolutions. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table 6: Effects on inbound job search

	Total	High-wage	Middle-wage	Low-wage
	(1)	(2)	(3)	(4)
LGBT-free zone × Post-treatment	19.130	6.689	15.611**	-3.079
	(15.077)	(4.067)	(7.170)	(9.168)
Mean of outcome	490.02	123.88	190.65	173.56
Observations	1,956	1,956	1,956	1,956
County FE	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on inbound search (immigration intentions). The inbound search activity measures the interest in job postings in a given county by job seekers from other Polish counties and other countries. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Appendix A Additional Figures and Tables

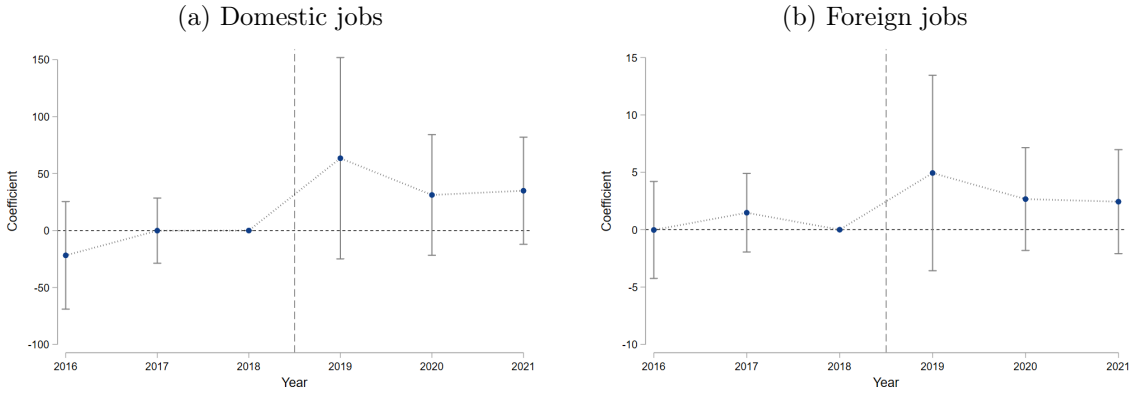


Figure A.1: Effects on sob search: All counties

Notes: The figure shows the event study coefficients and 95% confidence intervals from regressions of job search on the interaction of year fixed effects and the treatment group binary variable. In Figure A.1a, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In Figure A.1b, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. The regressions are estimated using the all counties sample. We control for county fixed effects and year fixed effects. Standard errors are clustered at the level of the county.

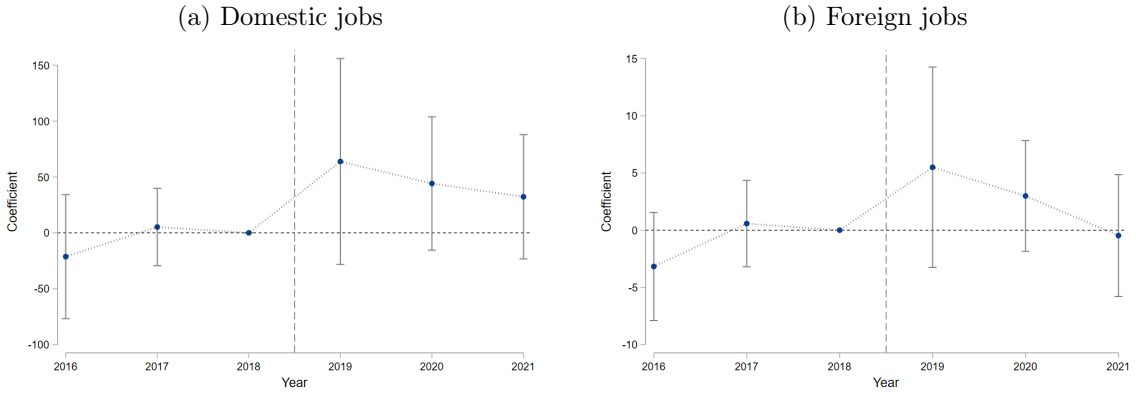


Figure A.2: Effects on job search: Border counties

Notes: The figure shows the event study coefficients and 95% confidence intervals from regressions of job search on the interaction of year fixed effects and the treatment group binary variable. In Figure A.2a, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In Figure A.2b, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. The regressions are estimated using the border counties sample. We control for county fixed effects and year fixed effects. Standard errors are clustered at the level of the county.

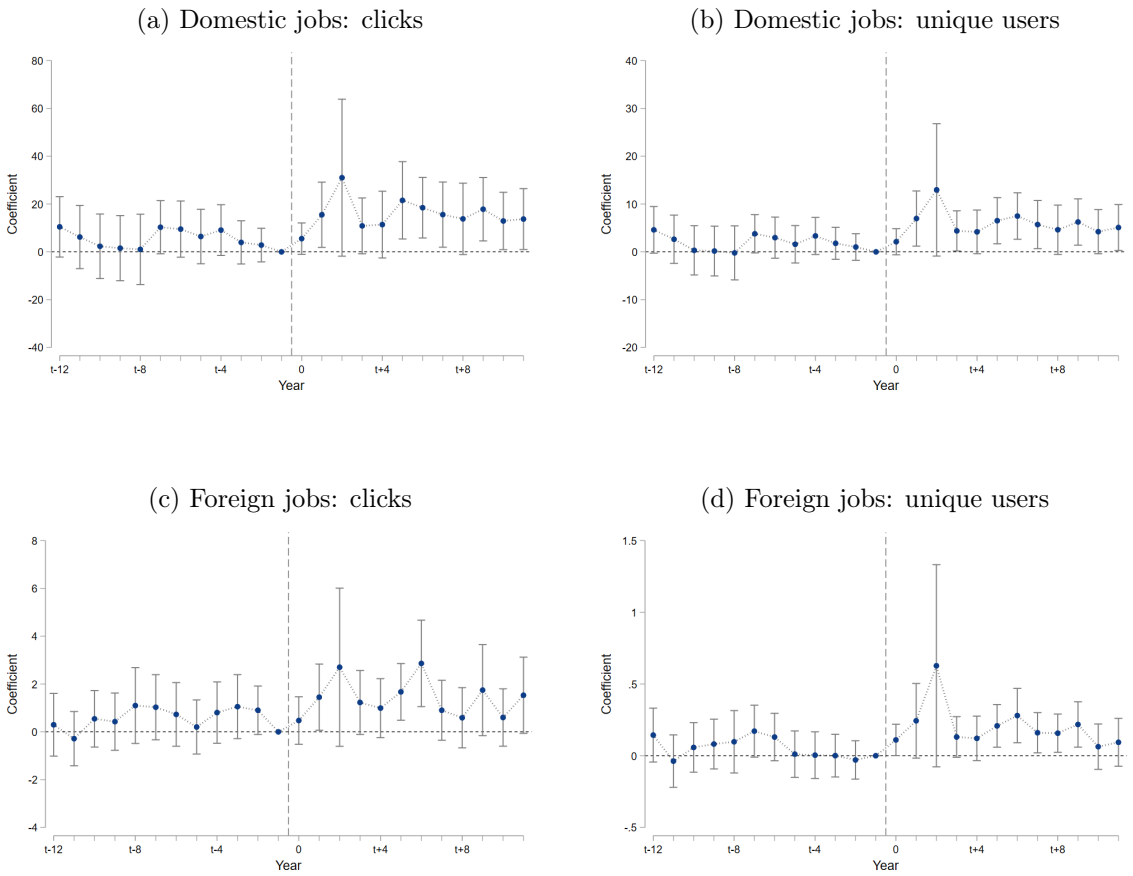
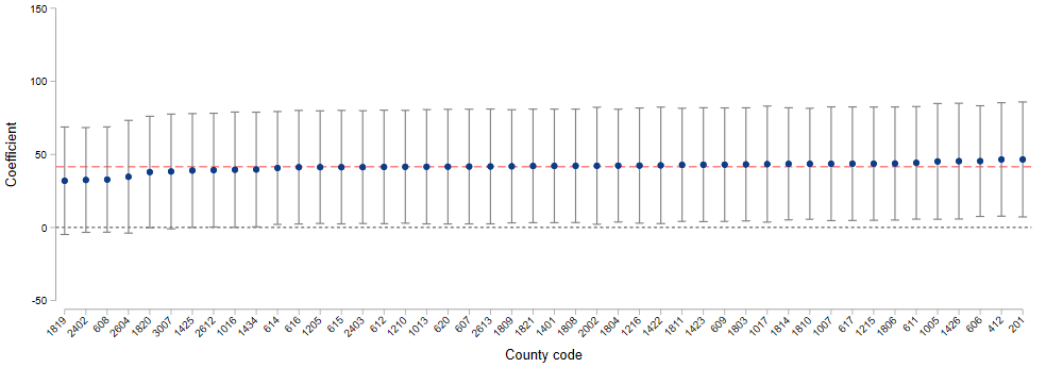


Figure A.3: Effects on job search: Quarterly data

Notes: The figure shows the event study coefficients and 95% confidence intervals from regressions of the job search on the interaction of quarter fixed effects and the treatment group binary variable. In Figure A.3a, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In Figure A.3b, the outcome variable is the number of unique users clicking on job postings located in Poland per 1,000 inhabitants. In Figure A.3c, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. In Figure A.3d, the outcome variable is the number of unique users clicking on job postings located in other countries per 1,000 inhabitants. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific quarter fixed effects. Standard errors are clustered at the level of the county.

(a) Domestic jobs



(b) Foreign jobs

Figure A.4: Effects on job search: Leave-one-out test

Notes: The figure shows the point estimates and 95% confidence intervals of the effects of the the anti-LGBTQ declarations on domestic and foreign job search. The regressions are estimated using the county pair sample. County codes correspond to TERYT administrative codes. In each regression, we remove pairs that include a given treated county. The red dashed line denotes the baseline estimate. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.

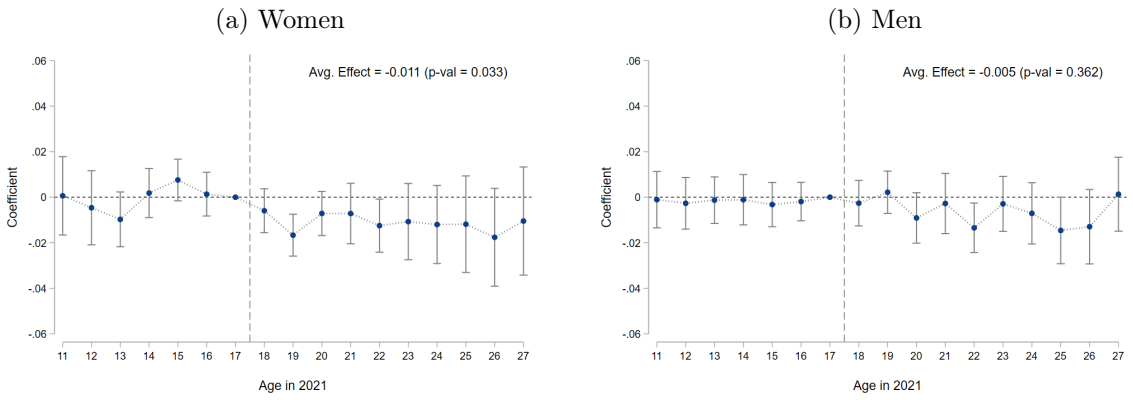


Figure A.5: Effects on population change by gender: Birth cohort event study

Notes: The figure shows the event study coefficients and 95% confidence intervals from regressions of log population changes (2011-2021) on the interaction of birth cohort fixed effects and the treatment group binary variable. The regressions are estimated using the county pair sample. We control for log population in 2011, county fixed effects, and pair-specific birth cohort fixed effects. Standard errors are clustered at the level of the county.

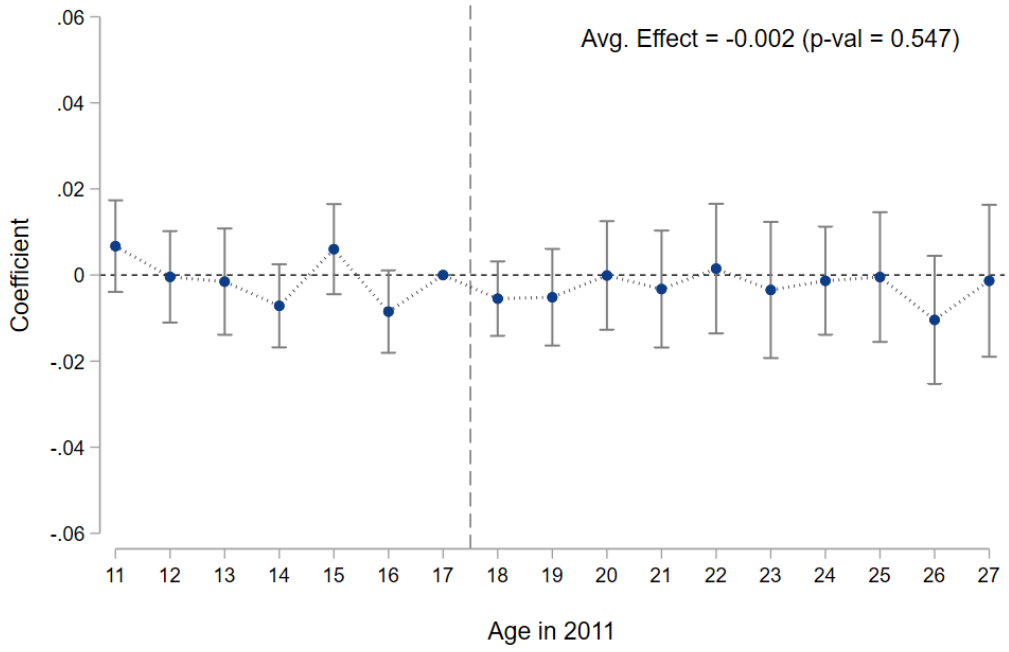


Figure A.6: Placebo effects on population change (2002-2011): Birth cohort event study

Notes: The figure shows the event study coefficients and 95% confidence intervals from regressions of log population changes (2002-2011) on the interaction of birth cohort fixed effects and the treatment group binary variable. The regressions are estimated using the county-pair sample. We control for log population in 2002, county fixed effects, and pair-specific birth cohort fixed effects. Standard errors are clustered at the level of the county.

Table A.1: Google search interest by province

Province	Anti-LGBTQ resolutions (share of population)	Google search interest: LGBT-free zone
Lublin	0.53	100
Podkarpackie	0.45	75
Swietokrzyskie	0.26	87
Lesser Poland	0.22	87
Łódź	0.18	62
Podlaskie	0.13	62
Masovian	0.11	68
Silesian	0.08	62
Lower Silesian	0.03	62
Greater Poland	0.02	56
Kuyavian-Pomeranian	0.02	62
Lubusz	0.00	62
Pomeranian	0.00	68
Warmian-Masurian	0.00	56
Opole	0.00	75
West Pomeranian	0.00	56

Notes: The table shows the percentage of the population in counties with anti-LGBTQ resolutions and the Google search interest for the search term "LGBT-free zone" ("strefa wolna od LGBT"). Google search interest data was downloaded from Google Trends. Search interest is represented on a scale from 0 to 100, where 100 corresponds to the search interest in the province with the highest search interest for the term "LGBT-free zone" in Poland (2016-2021). The province is the most granular administrative level for which Google Trends provides data.

Table A.2: Correlates of country-specific treatment effects

	All countries	EEA countries			Non-EEA countries
	(1)	(2)	(3)	(4)	(5)
Same-sex marriage	0.060*	0.070**			0.189*
	(0.034)	(0.032)			(0.095)
ILGA score (log)			0.055		
			(0.067)		
ILGA score > 50				0.074**	
				(0.032)	
GDP per capita (log)	-0.062	0.055	0.046	0.057	-0.314**
	(0.068)	(0.099)	(0.116)	(0.098)	(0.105)
Unemployment rate (log)	-0.055	0.035	0.021	0.037	-0.156*
	(0.042)	(0.125)	(0.138)	(0.124)	(0.076)
Geographic distance (log)	-0.006	-0.090	-0.089	-0.091	0.050*
	(0.016)	(0.103)	(0.113)	(0.102)	(0.023)
EEA country	0.154***				
	(0.026)				
Observations	35	18	18	18	17

Note: The table shows the results of an OLS regression of country-specific treatment effects on the selected characteristics. The unit of observation is a destination country (country of a job posting). The country-specific treatment effects are coefficients from Poisson regressions of the equation 2 where the dependent variable is the number of clicks on job postings located in a given country divided by the county population. All variables are measured in 2018. Same-sex marriage is a dummy variable that is equal to one for countries with legalized same-sex marriage and is equal to zero otherwise. A more comprehensive ILGA index is available for European countries. ILGA index measures the human rights situation of LGBTQ people on a 0-100 scale. The variable "ILGA score > 50" is a dummy variable that is equal to one for countries with an ILGA score greater than or equal to 50 and is equal to zero otherwise. EEA countries are countries that belong to the European Economic Area, which is a single market with no legal barriers for Polish workers. In the sample, we include countries with at least 10,000 clicks recorded during our study period. We weight the observations by pre-treatment levels of the search variable divided by the standard errors of the treatment effects from the country-specific Poisson regressions. Robust standard errors are reported in parentheses.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table A.3: Effects on net migration intentions

	Total	High-Wage	Middle-Wage	Low-Wage
	(1)	(2)	(3)	(4)
LGBT-free zone × Post-treatment	-25.677	-5.729	-2.394	-17.255
	(26.254)	(7.417)	(9.564)	(12.713)
Mean of outcome	115.76	5.57	51.80	59.15
Observations	1,956	1,956	1,956	1,956
County FE	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on net migration intentions (inbound job search - outbound job search). Inbound job search activity is measured through clicks on job postings in a given county by job seekers from other counties and countries. Outbound job search activity is measured through clicks by job seekers in a given county on postings in other counties and countries (i.e., both international and internal migration intentions as defined in other tables in the paper). The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table A.4: Effects on job search: non-neighboring and neighboring destination counties

	Non-neighboring (1)	Neighboring (2)
LGBT-free zone × Post-treatment	32.496**	8.998
	(15.796)	(10.111)
Mean of outcome	226.59	126.05
Observations	1,956	1,956
County FE	✓	✓
County pair FE x Year FE	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on out-of-county job search activity. In column 1, the outcome variable is the number of clicks per 1,000 inhabitants on job postings located in Polish counties that are not direct neighbors of the county of residence. In column 2, the outcome variable is the number of clicks per 1,000 inhabitants on job postings located in Polish counties that are direct neighbors of the county of residence. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table A.5: Effects on job search: Alternative standard errors

	Domestic job postings				Foreign job postings			
	Robust	Cluster: County pair	Cluster: County	Cluster: County & county pair	Robust	Cluster: County pair	Cluster: County	Cluster: County & county pair
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
LGBT-free zone × Post-treatment	41.494***	41.494***	41.494**	41.494**	3.313***	3.313***	3.313**	3.313**
	(10.381)	(15.239)	(19.407)	(17.205)	(0.968)	(1.206)	(1.462)	(1.320)
Observations	1,956	1,956	1,956	1,956	1,956	1,956	1,956	1,956
County FE	✓	✓	✓	✓	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on job search. In columns 1-4, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In columns 5-8, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. In columns 1 and 5, we calculate heteroskedasticity-robust standard errors. In columns 2 and 6, standard errors are clustered at the county pair level. In columns 3 and 7, standard errors are clustered at the county level (our baseline choice). In columns 4 and 8, we use two-way clustered standard errors by county and county pair.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table A.6: Effects on job search: Robustness checks

	Weighted by population (1)	Incl. city counties treated & control (2)	Incl. city counties Treated only (3)	Incl. 2020 resolutions (4)	Excl. counties on national border (5)
			Panel A. Domestic jobs		
LGBT-free zone × Post-treatment	53.963**	43.789**	39.745**	36.417*	44.836**
	(23.188)	(21.068)	(19.009)	(18.695)	(22.014)
Observations	1,956	2,196	2,016	2,088	1,548
			Panel B. Foreign jobs		
LGBT-free zone × Post-treatment	4.098**	1.947	3.255**	3.109**	3.706**
	(1.637)	(1.816)	(1.429)	(1.382)	(1.668)
Observations	1,956	2,196	2,016	2,088	1,548
County FE	✓	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓	✓

Note: The table shows the effects of the anti-LGBTQ declarations on job search. In Panel A, the outcome variable is the number of clicks on job postings located in Poland per 1,000 inhabitants. In Panel B, the outcome variable is the number of clicks on job postings located in other countries per 1,000 inhabitants. In column 1, we weight observations by their populations in 2018. In column 2, we include city counties (both treated and control). In column 3, we include treated city counties only. In column 4, we add pairs that include counties that became treated in 2020. In column 5, we exclude counties on Poland's state border. The regressions are estimated using the county pair sample. We control for county fixed effects and pair-specific year fixed effects. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Table A.7: Effects on job search: Controlling for alternative mechanisms

	(1)	(2)	(3)	(4)
		Panel A. Domestic jobs		
LGBT-free zone × Post-treatment	41.171**	41.749**	42.987**	41.790**
	(19.118)	(19.138)	(19.414)	(19.166)
Observations	1,956	1,956	1,956	1,956
		Panel B. Foreign jobs		
LGBT-free zone × Post-treatment	3.198**	2.991**	3.304**	3.361**
	(1.436)	(1.431)	(1.463)	(1.430)
Observations	1,956	1,956	1,956	1,956
County FE	✓	✓	✓	✓
County pair FE x Year FE	✓	✓	✓	✓
Divorces	✓			
EU grants		✓		
Investment			✓	
Deficit				✓

Note: The table shows the effects of the anti-LGBTQ declarations on job search. The regressions are estimated using the county pair sample. In each regression, we control for county fixed effects and pair-specific year fixed effects. We additionally control for selected time-varying variables measuring alternative mechanisms. Standard errors are clustered at the level of the county.
* $p < .10$; ** $p < .05$; *** $p < .01$

Appendix B Data Appendix

Table B.1: Variable descriptions (i.)

Variable	Description	Source
Treatment variables		
Anti-LGBTQ resolution	A binary variable which is equal to one for counties with at least one anti-LGBTQ resolutions introduced by the county council or municipality councils in 2019	Atlas of Hate
Job search variables		
Domestic job search	The number of clicks made by users located in a given county on postings of jobs located in other Polish counties divided by county population in 2018 (multiplied by 1000)	Indeed
Foreign job search	The number of clicks made by users located in a given county on postings of jobs located in other countries divided by county population in 2018 (multiplied by 1000)	Indeed
Foreign job search, Same-sex marriage: Europe	Foreign job search but limited to the following countries: Austria, Belgium, Denmark, Finland, France, Germany, Iceland, Ireland, Luxembourg, Malta, Netherlands, Norway, Portugal, Spain, Sweden, United Kingdom	Indeed
Foreign job search, Same-sex marriage: Other continents	Foreign job search but limited to the following countries: Argentina, Australia, Brazil, Canada, Colombia, Ecuador, New Zealand, South Africa, Taiwan, United States, Uruguay.	Indeed
Foreign job search, No same-sex marriage: Europe	Foreign job search but limited to the following countries: Albania, Andorra, Armenia, Azerbaijan, Belarus, Bosnia and Herzegovina, Bulgaria, Croatia, Cyprus, Czech Republic, Estonia, Georgia, Greece, Hungary, Italy, Kosovo, Latvia, Liechtenstein, Lithuania, Moldova, Monaco, Montenegro, North Macedonia, Romania, Russia, San Marino, Serbia, Slovakia, Slovenia, Switzerland, Ukraine, Vatican City.	Indeed
Foreign job search, No same-sex marriage: Other continents	Foreign job search but limited to the countries not listed in the description of the three variables above.	Indeed
Job search: users	The number of unique device identifiers with at least one click on postings of jobs located in the given destination (domestic or foreign) divided by county population in 2018 (multiplied by 1000)	Indeed
Job search: clicks per user	The number of clicks divided by the number of unique device identifiers with at least one click on postings of jobs located in the given destination (domestic or foreign)	Indeed
Inbound job search	The number of clicks on postings of jobs located in the given county made by users located in other counties divided by county population in 2018 (multiplied by 1000)	Indeed
Net interest	Inbound job search - (Domestic job search + Foreign job search)	Indeed

Notes: Description of variables used in the analysis. Data on county population is taken from 2018, because it is the most recent pre-treatment year.

Table B.2: Variable descriptions (ii.)

Variable	Description	Source
Other variables		
Cohort-specific population change	the difference in the number of residents aged x years old in the 2021 census and $x-10$ years old in the 2011 Census	Statistics Poland (National Census)
Longitude	Longitude of the county's centroid	Own calculations based on Head Office of Geodesy and Cartography shapefiles.
Latitude	Latitude of the county's centroid	Own calculations based on Head Office of Geodesy and Cartography shapefiles.
Population density	Population per square kilometre	Statistics Poland
Births	The number of births divided by county population in 2018	Statistics Poland
Deaths	The number of deaths divided by county population in 2018	Statistics Poland
Marriages	The number of marriages divided by county population in 2018	Statistics Poland
Tertiary education	The number of individuals with tertiary education in the 2002 census divided by county population in 2018	Statistics Poland (National Census)
Far-right voting	The number of votes received by Law and Justice in the 2015 parliamentary elections divided by the number of valid votes	National Electoral Commission
Unemployment	The number of registered unemployed individuals divided by county population in 2018	Statistics Poland
Wages	Average earnings in medium and large firms (firms with at least 10 employees)	Statistics Poland
Corporations (domestic)	The number of registered corporations divided by county population in 2018	Statistics Poland
Corporations (foreign)	The number of registered corporations with foreign capital divided by county population in 2018	Statistics Poland
EU grants	EU funds received by local governments (municipalities and county) divided by county population in 2018	Statistics Poland
Investment	Local governments' investment spending (municipalities and county) divided by county population in 2018	Statistics Poland
Deficit	The difference between the total spending and the total revenue of local governments (municipalities and county) divided by county population in 2018	Statistics Poland
Crime	The number of crimes ascertained by police divided by county population in 2018	Statistics Poland
Fire accidents	The number of fire accidents reported by fire departments divided by county population in 2018	Statistics Poland
Car accidents	The number of road accidents reported by the police divided by county population in 2018	Statistics Poland
Deaths: external causes	The number of deaths due to external causes (ICD-10 V,W,X,Y) divided by county population in 2018	Statistics Poland
Deaths: suicide	The number of deaths due to intentional suicide divided by county population in 2018	Statistics Poland

Notes: Description of variables used in the analysis. Data on county population is taken from 2018, because it is the most recent pre-treatment year.

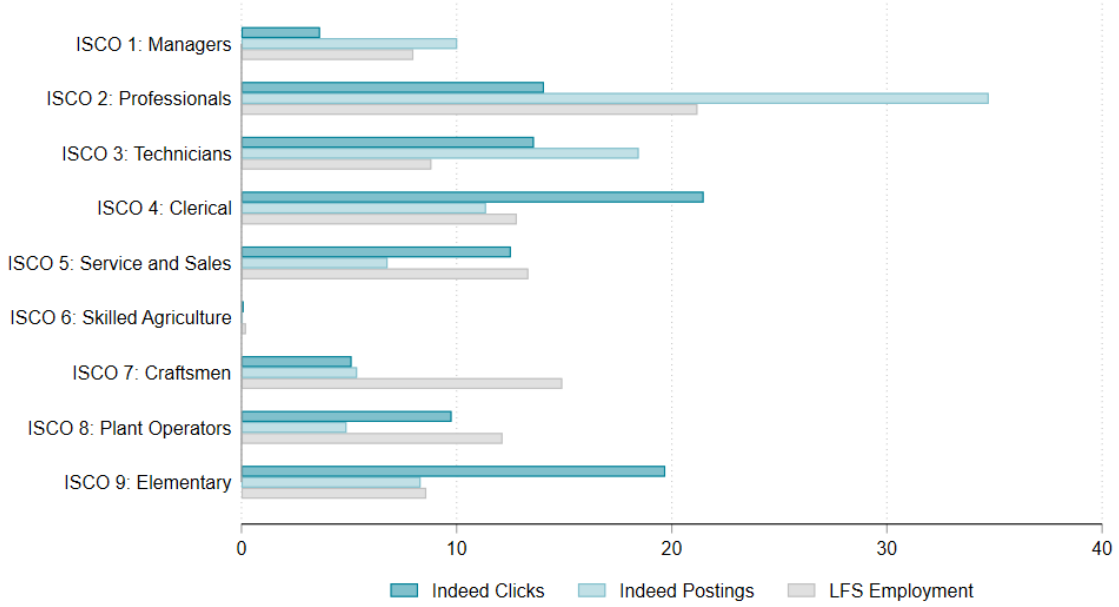


Figure B.1: Indeed vs. LFS data: Clicks, postings, employment shares

Notes: The figure shows the occupational shares (ISCO 1-digit groups) of Indeed clicks, Indeed postings, and employment measured by the Polish Labour Force Survey (https://stat.gov.pl/obszary-tematyczne/rynek-pracy/popyt-na-prace/popyt-na-prace-w-2018-roku,1,14.html). We use the data from 2018, the last year of the pre-treatment period.

(a) Job titles

(b) Destination countries

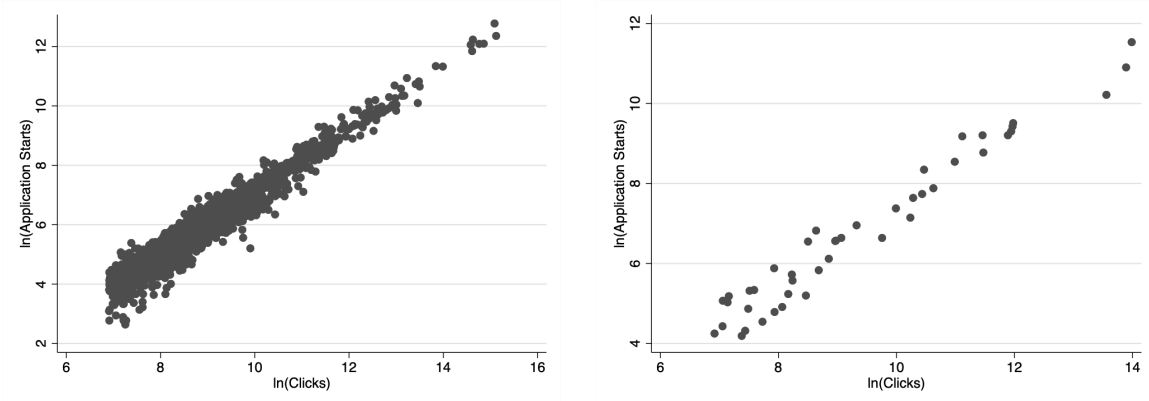


Figure B.2: Application starts and clicks by job title and destination country in 2021

Notes: The figure shows application starts and clicks for each job title with at least 1,000 clicks in the case of domestic job search (Figure B.2a), and for each destination country other than Poland with at least 1,000 clicks in the case of international job search (Figure B.2b). The correlation coefficients are 0.97 and 0.98, respectively. Data is for job seekers with a Polish IP address in 2021, the first full year for which data on application starts is available. An application start represents a job seeker action of clicking on "apply" after viewing the complete job description. All values are expressed in logs.

Table B.3: Migration and job search destination countries

International migration stock, 2020 (Country of origin: Poland)		Clicks on job postings on Indeed, 2018 (Job seeker IP address: Poland)	
Rank	Country	Rank	Country
1	Germany	1	United Kingdom
2	United Kingdom	2	Germany
3	United States	3	United States
4	Canada	4	Ireland
5	Netherlands	5	Netherlands
6	Ireland	6	France
7	Italy	7	Canada
8	Norway	8	Switzerland
9	Sweden	9	Italy
10	France	10	Spain

Notes: The table shows the ten countries with the highest stocks of Polish-born migrants in 2020 from the United Nations International Migrant Stock 2020 dataset on the left-hand side and the ten countries with the highest number of clicks on job postings by users with a Polish IP address in 2018 on the right-hand side.

Appendix C Examples of Anti-LGBTQ Declarations

In the wake of the ideological war unleashed by some politicians, the Świdnik County Council is adopting the declaration, "Świdnik County Free of LGBT Ideology."
Radicals seeking a cultural revolution in Poland are attacking the freedom of speech, the innocence of children, the authority of family and school, and the freedom of entrepreneurs. That is why we will steadfastly defend our local community!

Świdnik County Free of "LGBT" Ideology

For the sake of life, family and freedom, we declare that the local government we represent – in accordance with our centuries-old culture and society – will not interfere in the private lives of Polish women and men. We will not let the exaggerated problems and artificial conflicts brought by "LGBT" ideology be imposed on us.
We will not agree to the illegal embedding of political correctness officers in schools (so-called "lighthouse keepers"). We will uphold the right to raise children in accordance with the beliefs of their parents!
We will do everything possible to ensure that corruptors interested in the early sexualization of Polish children according to the so-called standards of the World Health Organization (WHO) are kept out of schools. We will protect students, ensuring that parents, with the help of educators, can responsibly convey to them the beauty of human love!
We will not permit administrative pressures to apply political correctness (sometimes rightly and simply called "homopropaganda") in selected professions. We will protect, among others, teachers and entrepreneurs from the imposition of unprofessional criteria, for example, in their educational work or in the hiring of employees or contractors!
We declare that Świdnik County will be faithful to the national and state traditions while carrying out of its public duties, honoring the 1050 years that have passed since the Baptism of Poland, 100 years since the restoration of Polish independence and 29 years since the restoration of self-government of Polish women and men.

Figure C.1: Świdnik County Council's anti-LGBTQ declaration of March 26, 2019

Notes: The figure shows our translation into English of the first anti-LGBTQ declaration by a local government in Poland recorded by the Atlas of Hate (Pająk & Gawron, 2024), approved by the council of Świdnik County in southeastern Poland on March 26, 2019. While the original text in Polish has since been removed from the county's official website, it is available as part of a court judgment that declared it illegal in its entirety on January 11, 2024: https://bip.brpo.gov.pl/sites/default/files/2024-02/WSA_skarga_uchwala_antylgbt_Swidnik_wyrok_uzasadnienie_11.01.2024.pdf (last accessed on October 12, 2024). Quotation marks and exclamation marks are as in the original text. Many other local governments used identical or very similar wording (Pająk & Gawron, 2024).

In response to aggressive homosexual propaganda conducted as part of an ideological war by left-liberal political circles and "LGBT" organizations, which threatens our fundamental principles and values of social and national life, the Niedrzwica Duża Municipality Council adopts the following declaration:

The Niedrzwica Duża Municipality is free from gender and "LGBT" ideology

In defense of children, youth, families, and Polish schools against sexual depravity and indoctrination, which lead to numerous pathologies already observed in Western countries—such as the acceptance of pornography, abortion, sexual crimes, the crisis of the family, and many others—we declare our position and determination to halt "homo-terror" and the sexualization of social life. We will not allow marginal and minority groups to impose upon us gender and "LGBT" ideologies, imported from the West, which are alien to our centuries-old tradition, values, and Christian morality based on the Decalogue and respect for human dignity. We aim to defend the right to raise children and youth in accordance with the convictions of their parents.

Therefore, the Niedrzwica Duża Municipality Council opposes: 1. The promotion of homosexuality and sexual education in schools by LGBT groups and their educators and enforcers of political correctness, as expressed in the so-called LGBT+ Declaration. We consider this declaration to be harmful and dangerous, not only for Polish schools and families but for the entire sphere of social life and the future of our nation. 2. The premature sexualization of children in schools under the so-called standards of the World Health Organization (WHO). We will protect students in our schools by ensuring that parents, with the assistance of educators, can responsibly convey content about human love and the beauty of procreation. 3. The imposition of cultural and political correctness ("homopropaganda") in many areas of social, political, and economic life. We will support teachers, local government officials, entrepreneurs, and representatives of other professions in resisting this ideological pressure and the "homo-terror" being applied. 4. The acceptance of methods and sociotechnical manipulations employed by "LGBT" activists who, under the guise of noble slogans—such as freedom, tolerance, "anti-discrimination," combating violence, and so-called hate speech—impose programs and ideologies that deprave children, youth, and adults, leading to the complete collapse of moral norms and the destruction of the natural and healthy model of family, school, and society.

We declare that: The Niedrzwica Duża Municipality will uphold our national principles, which are based on fidelity to national and state traditions, attachment to Christian values, and the ideals of freedom. We act with pride in our 1,000-year history, whose source is the act of the Baptism of Poland, the sacrifices of many generations of Poles in building cultural and national identity, acts of independence, and the legacy of our greatest compatriots.

Figure C.2: Niedrzwica Duża Municipality Council's anti-LGBTQ declaration of May 28, 2019

Notes: The figure shows our translation into English of the bulk of the anti-LGBTQ declaration approved by the council of Niedrzwica Duża Municipality on May 28, 2019, The original is available at https://ugniedrzwicaduza.bip.lubelskie.pl/upload/pliki//Stanowisko_w_sprawie_powstrzymania_ideologii_gender_i_LGBT.pdf (last accessed on June 19, 2025).

In recent months, there have been attempts made at the local government level to undermine the constitutionally guaranteed rights of families, including the rights of parents and children, as well as the legal nature and institutional status of marriage. The LGBT+ Declaration signed by the Mayor of Warsaw has generated the firmest opposition. It contains not only a number of ideological statements that violate the standard of ideological neutrality for the public authorities but also the announcement of the introduction of permissive sex education classes in schools according to WHO standards that raise fundamental doubts from the standpoint of the constitutional principle of protecting children from demoralization. Similar solutions have already been introduced with a narrower scope in Słupsk and Gdańsk, among others. In October, Polish schools were used for the third time to carry out the "Rainbow Friday" propaganda campaign.

After extensive public consultations, together with a number of parent and pro-family organizations, the Local Government Charter on the Rights of the Family was presented as an affirmation of the most important constitutional principles and rights. Co-authors of the Charter are the Ordo Iuris Institute, the Center for Life and Family, CitizenGo Poland, the Mom and Dad Foundation, the March Fourth Movement, the Association of Families with Multiple Children "Happy Home" in Gdańsk, the Association "Gift of Life," the Association of Educators "Natan," the Association "Parents Protect Children." Adoption of the Charter by local authorities will ensure respect for the primacy and autonomy of parents in the upbringing of their children, guarantee the openness of schools' cooperation with outside organizations and the need to obtain parental consent for their children's participation in controversial extracurricular activities.

Under these circumstances, it is considered necessary to support measures aimed at protecting the values certified in the Constitution of the Republic of Poland - the definition of marriage as a union between a man and a woman, family and parenthood (Article 18), the right to protection of family life (Article 47) the right of parents to raise their children in accordance with their own beliefs (Article 48(1)) and the right of the child to protection from demoralization (Article 72(1)). This can be done by implementing concrete solutions proposed in the Local Government Charter on the Rights of the Family, which is attached to this resolution.

Figure C.3: Łowicz County Council's anti-LGBTQ declaration of April 26, 2019

Notes: The figure shows our translation into English of the anti-LGBTQ declaration approved by the council of Łowicz County on April 26, 2019. The original is available at https://www.bip.powiat.lowicz.pl/plik,21570,uchwala-nr-viii-54-2019-rpl.pdf (page 3; last accessed on October 12, 2024). This declaration accompanied the approval of the Local Government Charter of the Rights of the Family, the full text of which can be found in Figure C.4. Quotation marks are as in the original text. Many other local governments used identical or very similar wording when approving the Charter (Pająk & Gawron, 2024).

LOCAL GOVERNMENT CHARTER
OF THE RIGHTS OF THE FAMILY

The foundation of social order and the basic social unit, family is an optimal environment for people to develop. It provides the indispensable support for all of its members, especially the youngest and oldest ones. No community is able to pursue common good without the involvement of families. Our future depends on their proper functioning.

Local government is the foundation for the rule of law. We are categorically against any attempts to undermine the constitutional rights of families at a local government level, including the rights of parents and children and the legal status of marriage.

We stand by the values stipulated in the Constitution of the Republic of Poland, i.e. family, marriage as a relationship between a woman and a man, parenthood and motherhood (Article 18), the right to protect family life (Article 47), the right of parents to raise their children in accordance with their own beliefs (Article 48(1)), and the right of a child to be protected against demoralisation (Article 72(1)). Public authorities are obligated to develop their social and economic policy taking into account the best interest of the family (Article 71(1) of the Constitution).

We urge everyone – families, associations and local governments – to join this Charter by defending the values it stands for and ensuring they are respected through enactments and measure s that implementf the solutions defined herein.

I. RIGHTS OF PARENTS AND THE BEST INTEREST OF THE CHILD AT SCHOOL AND PRESCHOOL

When applied to the family, i.e. the basic community of citizens and the primary social unit, the constitutional principle of subsidiarity, which "strengthens the rights of citizens and their communities," is realised in the constitutional right of parents to raise their children in accordance with their own beliefs. The task of educational institutions is to support the educatio nal role of families without violating the constitutional rights of parents (Article 1(2) of the Polish Education Law 1). The role of the education system is not to replace family upbringing. Unfortunately, in practice, even the statutory rights of parents are often ignored in the course of school and preschool education.

In view of the above, local governments must adopt a "Code of Good Practice" that would lay down the model solutions showing how to organise the educational work of schools based on the principle of their educational subsidiarity to the family. Even though such a document will not be binding, it may

Figure C.4: Local Government Charter of the Rights of the Family (page 1 of 4)

Notes: The figure shows the full text of the Charter published in English by Ordo Iuris at https://en.ordoiuris.pl/local-government-charter-rights-family (last accessed on October 11, 2024). The Charter is clear in its affirmation that marriage should be restricted to a union of a man and a woman. Local government resolutions to adopt the Charter was often accompanied by explicit written declarations against "LGBT ideology" or Warsaw's "LGBT+ Charter," like the example shown in Figure C.3.

substantially influence the structure of the organisational culture of schools managed by the local government.
Local government schools must respect the statutory rights of parents, especially the rights of the parents' board to adopt the education and prevention programme and to approve any partnership with NGOs, and they must each time secure the parent's consent to the child's participation in any extracurricular activities. Furthermore, it is a good practice to give parents an opportunity to become actively involved in those processes, also when it comes to the content taught.
Parents should be allowed to verify any external organisations operating on the premises of the school and any materials they use during non -compulsory classes dually: both individually and collectively – through the parents' board. It is a good practice to present information including not only the name but also the programme and profile of the organisation to each parent separately, in a way allowing them to become familiar with the content thereof before enrolment. A similar mechanism should be applied to any other teaching and educational activities pursued by schools or institutions that go beyond the curriculum or that involve issues covered by the curriculum of Family Life Education, including those financed from public grants.
It is also a good practice for the school to inform the parents about their rights even if this is not directly required by statute.
The local government managing the school should also make the information about any partnerships between schools and NGOs available to the public by posting it on the Public Information Bulletin (BIP) and on the local government's website. The notice should specify at least the names of the organisations approved by the headmaster and the nature of the ir activity. This will give the parents insight into the functioning of the school before they decide to entrust it with any tasks related to the upbringing of their child.
Such solutions should be supplemented by workshops for parents to develop their upbringing competences and for children – for supporting the upbringing role of the family in accordance with integral upbringing model. Local government should initiate and support teacher training that addresses those issues and focuses on relations with parents. Schools that develop and follow good practices related to the family rights should be supported and rewarded by the local government.

II. FAMILY RIGHTS IN THE LOCAL GOVERNMENT'S SOCIAL POLICY

The context of the family rights, autonomy and identity should be included in the development and implementation of any instruments of the local government's social policy.
Programmes of partnerships with community organisations should respect the principle of strengthening the family and marriage and rule out the funding of any projects harmful to these values. It is especially crucial to exclude any chance of allocating public funds and public property for projects that undermine the constitutional identity of marriage as a relationship between a man and a woman or the autonomy of the family. The terms and conditions of local government competitions for community organisations should be supplemented with standards that support the family and marriage and

Figure C.5: Local Government Charter of the Rights of the Family (page 2 of 4)

exclude the earmarking of funds for activities undermining the constitutional foundations of the family law or prejudicing the rights of citizens.
Local government programmes aimed at preventing violence and supporting its victims, preventing alcohol and drug abuse and supporting health prophylaxis should respect the integrity of the family, which may only be waived in special situations, such as threat to the life or health of its members. It is the only way to avoid pathological situations of disproportionate, unfounded interference in the life of families, up to taking the children away.
It is important that the preventive and awareness programmes pursued by the local government address the key challenges that families face today, including procreational health promotion and measures to support the durability of marriages.
Local government should support families that raise children, including large families. It is also necessary to exclude legal discrimination of marriages and the children they are raising from social policy. Situations where the fact that the parents are married has negative consequences for the child happen way too often. This applies in particular to access to services and benefits offered by the local government which require a declaration on a single parent status. The situation can be managed through the introduction of a requirement, already known in the Polish legal system, for a single parent to present a judgement awarding child maintenance – to eliminate the abuse of that privilege by those who are not eligible and to substantially limit the scale of the discrimination of marriages.
Another important element of the social policy of a local government should be the creation of solutions that allow parents to choose between various forms of childcare for the youngest children. Local authorities should implement mechanisms to allow the parents of the youngest children to choose between home care, institutional collective care and other forms of childcare to meet the diverse needs of various groups of parents and children. Such solutions would be also convenient for those parents whose children cannot use collective care.
The actual activity of local authorities depends not only on the applicable legislation but also on the knowledge of the clerks and on how well they are prepared. This is why it is important to train local government workers, including those in charge of social welfare, on the autonomy and legal identity
of the family, on the rights of parents and on the best interest of the child. The training participants should receive reliable and ideology-free information about the existing legislation and effective methods of preventing any undesired phenomena, such as violence, addictions and other dysfunctions that may take place in households and public space. Even though empirical studies clearly confirm that strong family bonds effectively protect against violence, this basic knowledge has been undermined at many seminars delivered thus far.

Figure C.6: Local Government Charter of the Rights of the Family (page 3 of 4)

Local government should also attempt to optimally adapt social services to the needs of families and, where possible, provide solutions to make them available to families with children . Sports and leisure infrastructure should offer possibilities for families to spend time together.
It is also important to promote the significance of family, marriage and parenthood within the activity of the local government's cultural institutions.

IV. PROMOTION OF GOOD PRACTICES REGARDING THE RIGHTS OF FAMILIES IN BUSINESS

Good practices regarding the rights of families should also be promoted in the business sector.
Local government should support certification programmes addressed to businesses that have adopted family-friendly solutions. This may be a programme for certifying solutions addressed to consumers, such as discounts for families or amenities for families with children, or a programme of good practices regarding the situation of employees who are parents.

V. MONITORING AND ENFORCEMENT OF FAMILY RIGHTS

Exercise of family rights requires effective mechanisms for their enforcement. Families must also be aware of their rights.
Proper exercise of the rights of families at local government level requires appointing a Speaker for Family Rights and establishing a local system for reporting violations of family rights. The Speaker should monitor if local government institutions, including schools, respect family rights and intervene whenever they are breached. The Speaker may hold a helpline or a contact mail box to receive complaints. They should also keep parents informed about their rights.

VI. ENACTMENT OF FAMILY-FRIENDLY LAWS

The significance of family rights is not limited to the solutions included in this Charter. The situation of families is also directly shaped by many legal acts whose primary focus is on other issues.
This is why the enactment of every local law should be preceded by a determination of whether it will affect the situation of families and the scope of their rights, including the rights of parents and children, in accordance with the family mainstreaming principle. If it will, the law must be each time subjected to comprehensive assessment in this respect. It is particularly unacceptable for local laws to limit the constitutional and statutory rights of families and their members.

Figure C.7: Local Government Charter of the Rights of the Family (page 4 of 4)