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RESEARCH PAPER

Immigration restriction and the transfer of cultural norms over time and boundaries: the case of religiosity

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Abstract
We study the effect of an immigration ban on the self-selection of immigrants along cultural traits, and the transmission of these traits to the second generation. We show theoretically that restricting immigration incentivizes to settle abroad individuals with higher attachment to their origin culture, who, under free mobility, would rather choose circular migration. Once abroad, these individuals tend to convey their cultural traits to their children. As a consequence, restrictive immigration policies can foster the diffusion of cultural traits across boundaries and generations. We focus on religiosity, which is one of the most persistent and distinctive cultural traits, and exploit the 1973 immigration ban in West Germany (*Anwerbestopp*) as a natural experiment. Through a diff-in-diff analysis, we find that second generations born to parents treated by the *Anwerbestopp* show higher religiosity.

Keywords: Cultural transmission; immigration policy; religiosity; second-generation immigrants

JEL classification: D91; F22; J15; K37; Z13

1. Introduction

An increasing literature studies the transfer of cultural and institutional norms to the countries of origin of returning immigrants (Beine and Sekkat, 2013; Beine *et al.*, 2013; Tuccio and Wahba, 2018; Lodigiani and Salomone, 2020). However, the dissemination of norms and habits is a two-way process, which involves both the sending and receiving countries (Rapoport *et al.*, 2020). In this paper, we study the transfer of norms from the *sending* to the *receiving* country with the help of a major natural experiment. Specifically, we exploit the immigration ban in West Germany (henceforth Germany) following the 1973 oil shock to assess how immigration policies affect the transmission of religiosity across countries and between generations of immigrants.[1]

According to the Lucas critique, immigration policies —just like any policy— affect the individual behavior. Here, we are interested in the effect of immigration policies on the self-selection of the migrants along cultural characteristics. Self-selection is the crucial mechanism that makes migrants a non-random sample of the population. As a consequence, immigration policies introduce potential distortions into the very fundamental mechanism that determines the characteristics and the structure of the pool of immigrants, which, in turn, is central to the process of norms dissemination (Wahba, 2021).

For instance, in the literature it is known that restrictions on labor mobility alter the optimal duration of immigration. More precisely, authors like Vernon and Zimmermann (2021), Galli and Russo (2019), and Magris and Russo (2009) show theoretically and empirically that restrictions on international mobility incentivize individuals who would otherwise prefer circular or return migration to settle in the destination countries.[2] Settling permanently into a country entails reproduction and/or family reunion.[3] To the extent that these individuals show higher commitment to their origin culture, they are more inclined to convey their cultural traits to their children, spreading their culture across space and time. We describe this process through a simple, micro-founded theoretical model, in which the decision to migrate depends not only on the material benefits of emigration, but also on the cultural dimension, in the form of a preference for consuming in the origin country. Our model proceeds in two steps: (1) self-selection: restrictions on immigration force many individuals with higher commitment to their origin culture to settle in the destination country; (2) cultural transmission: these immigrants tend to convey their customs to the second generation.

This transmission mechanism is, in principle, testable. Unfortunately, finding causal evidence is hard, because it requires an exogenous source of variation in the immigration policy. The 1973 immigration ban in Germany (*Anwerbestopp*) provides us with a suitable natural experiment, which we use to study the transmission of religiosity to the second generation. In order to understand the functioning of the experiment, let us remark that, before 1973, Germany was very open to immigration. However, after the oil shock, concerns for mass unemployment among immigrants led the government to swiftly reverse this policy through the *Anwerbestopp*; namely, a ban on the recruitment of workers *who did not belong in the European Economic Community* (henceforth EEC).[4] Thus, EEC workers were still allowed free mobility in and out of Germany. This makes it possible to identify a treatment group and a control group, and treat the event as the quasi-experiment that is required to test our theory. Specifically, we expect that: (1) the treated group is more religious than the control group; (2) religiosity, in turn, is conveyed to the second generation, so that children born to treated immigrants show higher religiosity.

[1]Notice that, in our data, religiosity is a self-reported measure of *intensity* of religious devotion on a 1–10 scale.

[2]This mechanism is confirmed *a contrario* by Bazillier *et al.* (2023), who show that free labor mobility agreements incentivize returns to the origin countries.

[3]Obviously, unless one decides not to have children.

[4]The European Economic Community was the free-trade agreement replaced by the European Union in 1993.

In other words, we hypothesize a spillover of the *Anwerbestopp* treatment to the second generation. Notice that this application of our theoretical model requires that higher commitment to the origin culture is associated with higher religiosity. This happens if religious participation increases the benefits received by the local social network, and these benefits are lost after emigration. In this case, we should observe a negative relationship between the level of religiosity and the probability of migrating. This is confirmed in the literature. For instance, Docquier *et al.* (2020) find that in Middle East and North Africa countries people who intend to migrate exhibit significantly lower levels of religiosity than the rest of the population. Likewise, using data from the Arab Barometer, Falco and Rotondi (2016) find that radical Islam views are a deterrent to migration. As for the US, Myers (2000) highlights that frequent church attendance and involvement in the social aspects of one's religious organization discourage migration.[5]

In order to study the effect of the *Anwerbestopp* on the religiosity of second-generation immigrants, we use European Social Survey data; namely, the answer to the question "How religious are you", on a scale from one to ten. Our results display an increase in the level of religiosity for the treatment group, and hold under several robustness checks. Alternative measures of religiosity provide similar outcomes. These conclusions are in line with our prediction that the *Anwerbestopp* "forced" to settle in Germany many first-generation immigrants who, on their own, would have preferred circular or return migration.

Generally, our findings illustrate that immigration policies not only affect the share of permanent vs. return immigrants, but also that they influence the characteristics of the second generation. These long-run effects are crucial for the integration of the minorities, but can easily be overlooked by electorally-concerned, short-sighted governments.

Our paper is organized as follows: after this introduction, Section 2 presents our theoretical model; Section 3 reports the natural experiment; Section 4 describes our data; Section 5 discusses the identification strategy and the results; Section 6 is devoted to the robustness checks; Section 7 concludes.

2. Theory

Our theoretical approach is made of two steps. In the first step, we describe the self-selection into permanent or return migration, and illustrate how the immigration policy biases this choice. This part of the model is as in Galli and Russo (2019). In the second part, we improve on Galli and Russo (2019) by presenting a micro-founded model of intergenerational cultural transmission, which shows how the bias induced on the first generation can be conveyed to the second generation.

2.1 Self-selection

We use a simple two-period, two-country model with risk-neutral migrants. The model is extremely simplified in order to focus the attention only on the choice between permanent migration and return migration.[6] We consider two countries -Origin and

[5]Neudörfer and Dresdner (2014) exploit a natural experiment in Chile and find that Evangelical believers are less likely to migrate.

[6]In general, time spent abroad may vary: an individual may decide to migrate for a year, 10 years, or for her whole life; from this point of view, permanent migration is a corner solution. We refer to Hill (1987), Magris and Russo (2009), and Kirdar (2012) for more general models of circular migration.

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Destination- denoted as O and D, respectively. Individuals are endowed with one unit of labor they supply inelastically when young (namely, in the first period). In the second period, individuals are old and do not produce.[7] In D, one unit of labor produces one unit of a storable good. In O, production is normalized to zero. This shortcut is a convenient way to simplify the algebra without loss of generality, giving everybody an incentive to migrate for at least one period.[8] However, labor productivity is not the only difference between D and O. Destination and origin countries also differ in economic and political stability. Indeed, economic crises, political turmoils, and climate change are evermore important push factors (Naudé, 2009; Drabo and Linguère, 2015; Beine and Parsons, 2015; Coniglio and Pesce, 2015). We account for this effect by assuming that in O the state of the world is good with probability p, and bad with probability $(1 - p)$. The bad shock drives to zero the utility of staying in O and causes re-migration to D, where consumption of the stored good yields positive utility.[9] Thus, in our model, the incentive to migrate depends both on the productivity differential and the economic stability. However, entry to D is rationed and requires an entry permit, which is distributed through a lottery that grants a permit with probability $\pi \in (0,\ 1)$. Only one application per person is allowed. This concludes the description of the incentives to migrate.

Now, we introduce into the model a *cultural* motivation for return migration; namely, a preference for consuming at home, which we denote as "home-bias". This is done for taking into account the non-economic factors that affect migration decisions. Actually, if we consider wages alone, migration flows look surprisingly *low* with respect to what one would expect: given the existing wage differentials, there would be no reason for anyone to live and/or return in countries with lower ones. However, both economic and non-economic factors contribute to curb the outflows and incentivize returns.[10]

In our model, the material benefits of emigration are summarized by the productivity differential, while non-economic factors, like differences in language, culture, and, more generally, feelings of uprooting and homesickness are summarized in the concept of home-bias.[11,12,13] For simplicity, the future is not discounted and

[7]This assumption is only used for simplicity, with no loss of generality.

[8]For our results to hold, we only need labor productivity to be higher in D.

[9]Since the crisis drives the utility to zero, only returning migrants re-migrate (those who were unable to migrate in the first period are now old and do not produce; thus, their utility would be zero in both countries).

[10]Schewel (2020) argues that migration studies suffer from a "mobility bias", and that a systematic neglect of the causes and consequences of immobility hinders attempts to explain why, when, and how people migrate.

[11]Introducing a home-bias is common in the literature (see, for instance, Dustmann and Kirchkamp, 2002; Li, 2016; ?). The latter is simply an individual parameter $\theta_j \in [1,\ \theta_{\max}]$, which rescales the utility of consuming in O.

[12]In other words, consuming at home weakly dominates consuming abroad for any j. This is a convenient way to depict the commitment to the origin country customs. Notice that the material benefit of emigration (namely, the unit of good produced) is the same for all, while the home-bias is different. This simplification is quite useful to show the importance of the cultural factors in the decision of settling abroad: intuitively, for θ_j close to unity, there is no reason to return to O; however, when θ is sufficiently high, migrants are willing to return.

[13]We remark that, since labor in O is unproductive, everybody is willing to migrate for at least one period. Thus, the optimization problem boils down to compare permanent migration to return migration and finding the cutoff value of θ_j that makes an individual indifferent between migrating for one or two periods.

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returning migrants observe the shock in O only *after* their return.[14] The expected utility of a permanent migrant is given by

$$E[u_{PM}] = \pi + (1 - \pi)0 \tag{1}$$

(The migrant is successful with probability π. In that case, she produces and consumes one unit over her lifetime. If the migrant is unsuccessful, she spends both periods in O and consumes zero). The expected utility of returning to O and consuming at home the good produced in D is given by:

$$E[u_{RM}] = \pi[\underbrace{\theta_j p}_{\text{good shock}} + \underbrace{\pi(1-p)}_{\text{bad shock}}] \tag{2}$$

Equation (2) means that, for a successful migrant, if the shock is good, return migration makes it possible to enjoy home-bias augmented consumption. If the shock is bad, re-migration to D is uncertain. In this simplified world, the choice between permanent and return migration only depends on the home-bias θ_j.

2.1.1 Permanent and return migration

We compare the utility under return migration and permanent migration by solving the condition $E[u_{RM}] \geq E[u_{PM}]$ with respect to θ_j. This gives the cutoff value

$$\theta^* \equiv \frac{(1 - \pi(1-p))}{p}. \tag{3}$$

Thus, individuals for whom $\theta_j < \theta^*$ will be permanent migrants, and individuals for whom $\theta_j \geq \theta^*$ will be returning migrants.[15]

In other words, there exists a critical value of the home-bias θ^* that separates permanent migrants from returning migrants. It is crucial to note that the level of home-bias necessary to return depends on the immigration policy π. This happens because the probability of entering D affects the value of return migration through the probability of re-migrating in case of a shock. As we show in the comparative statics analysis below, this is the central finding of our theoretical analysis, and shows how the Lucas critique applies to this aspect of immigration policies. We now present the comparative statics results.

2.1.2 Comparative statics

In this section, we show the comparative statics properties of the model. Computing the derivatives is straightforward. We have

$$\frac{\partial \theta^*}{\partial \pi} < 0 \tag{4}$$

[14]This assumption is only used for simplicity, and can be dropped by using a three-period model, which would complicate the algebra without changing our results. The intuition is as follows: consider a three-period model, and suppose that a migrant wants to return after just one period. Such an immigrant exists because it is always possible to find θ_j high enough to prompt an individual to migrate for a single period. This immigrant observes a good state of the world in O and wants to return for the remaining two periods. However the possibility of a bad shock in the *third* period and the uncertainty about her ability to re-migrate will bias her decision exactly as it happens in the two-period model.

[15]We assume that the indifferent individual for whom $\theta_j \equiv \theta^*$ is willing to return.

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and

$$\frac{\partial \theta^*}{\partial p} < 0. \tag{5}$$

Derivative (4) shows that, as π grows, the share of returning migrants increases. This happens because freedom of immigration makes it easier to harbor abroad in the bad state; thus, return migration occurs at a lower θ. Put differently, *a restrictive immigration policy fosters permanent migration of immigrants with higher home-bias*. This application of the Lucas critique to the self-selection of immigrants is the basis of our empirical analysis.

Derivative (5) shows that improved economic conditions at home incentivize return migration. It is interesting to note that substituting $\pi = 1$ into θ^* is equivalent to setting $p = 1$. In both cases, we have $\theta^* = 1$. This means that, in presence of free mobility, our simplified model produces no permanent migration at all. This happens because labor mobility also provides insurance against the risk of a crisis in O.

2.2 Cultural transmission

It is very interesting to remark that attempts to curb immigration are mostly targeted at *permanent* immigration. Temporary immigration is rarely considered a source of concern. What is so special about permanent immigration? There are many obvious differences between temporary and permanent immigration. For instance, the net fiscal impact of immigration can be quite different in the short term than in the long term. However, for our purposes, we argue that what makes permanent immigration important is *procreation*. Procreation has crucial implications because families carry cultural and ethnic traits from one generation to another. The resilience of cultural traits across generations and the persistence of ethnic minorities are indeed some of the most common observations in heterogeneous societies (see Bisin and Verdier, 2010).[16] In the following, we exemplify a mechanism through which restrictive immigration policies can strengthen the persistence of the foreign culture in the second generation. Our idea is a simple one: according to equation (4), restrictions on immigration push individuals who are more committed to their native culture to settle in the destination country; then, they tend to convey their cultural preferences to their children.[17] In practice, this aim is pursued through educational and socialization effort, which will be the choice variable in our model of cultural transmission. In what follows, we show a simple example of such a mechanism.

Let us suppose that parents value cultural transmission to their children. Thus, we assume that cultural transmission (CT) is a good, which is produced through a Cobb-Douglas technology, with $\alpha \in (0, 1)$, whose inputs are educational effort ($e \geq 0$), and cultural capital, which is proxied by the home-bias θ_j. We consider a representative household that, in our case, is the household endowed with the average cultural capital of the permanent immigrants, denoted by $\overline{\theta}$. Notice that, by equation (4), $\overline{\theta}$ depends

[16]According to Borjas (1994) (p.1711), "the evidence suggests that the ethnic skill differentials will persist into the third generation and perhaps even into the fourth.[. . .] Ethnicity matters, and it seems to matter for a very long time".

[17]Notice that parents may rationally desire to convey their cultural values despite the costs their children may suffer from a weaker commitment to the majority culture. This is the "imperfect empathy" mechanism pointed out by Bisin and Verdier (2000).

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on the immigration policy π, therefore we can write it as $\overline{\theta}(\pi)$, with, as we formally prove in the appendix,

$$\frac{\partial \overline{\theta}(\pi)}{\partial \pi} < 0. \tag{6}$$

Thus, cultural transmission is produced according to the following production function:

$$CT = \overline{\theta}^{a} e^{(1-a)} \tag{7}$$

With quadratic cost of effort, the representative household's utility can be written as

$$\overline{U} = \overline{\theta}^{a} e^{(1-a)} - \frac{e^2}{2} \tag{8}$$

Maximization yields the optimal effort:

$$\overline{e}^{*} = [(1-a)\overline{\theta}^{a}]^{1/(1+a)} \tag{9}$$

It is straightforward to verify that

$$\frac{\partial \overline{e}^{*}}{\partial \overline{\theta}} > 0. \tag{10}$$

In other words, the average equilibrium educational effort increases as the average home-bias increases. This is quite not surprising, as the inputs in the production function are complements. However, the closed solution (9) and the comparative statics result (10) provide a convenient way to summarize the transmission mechanism from the immigration policy to the cultural transmission, which is given by the derivative

$$\frac{\partial \overline{e}^{*}}{\partial \pi} = \frac{\partial \overline{e}^{*}}{\partial \overline{\theta}} \frac{\partial \overline{\theta}}{\partial \pi} < 0. \tag{11}$$

In other words, relaxing restrictions on immigration reduces the home-bias necessary to incentivize returns. In turn, this reduces the home-bias of the permanent immigrants, who are less committed to their origin, and put less effort in conveying their customs and traditions to the second generation. Of course, the mechanism is reversed in case of restriction. Equation (11) nicely summarizes the story we are telling about the *Anwerbestopp*.

When religion is part of one's cultural identity, its practice, considered as a set of beliefs and routines, is an important component of the family life. Consequently, it is a preeminent indicator of cultural transmission. If our model is correct, the intergenerational transmission of religiosity can be detected, provided that we find appropriate data and an exogenous source of variation. While data on religious affiliation and practice are available, finding a proper source of variation that ensures causality is harder. We found a suitable natural experiment in the 1973 immigration ban in Germany, which we describe in the next section.

3. The natural experiment

Currently, Germany is the top destination country in the EU for immigrants and for asylum seekers, and the second in the world. In January 2021, according to the Central Register of Foreign Nationals (AZR), around 11.4 million foreign nationals were living in Germany. In addition, 22.3 million people had a migration background, representing 27.2 per cent of the population. However, after World War II, (West) Germany was already the main destination in Europe. The economic boom (*Wirtschaftswunder*) generated a severe labor shortage, especially for low-skilled workers in construction and industry. At first, immigrants from East Germany and ethnic Germans from Eastern Europe (*Aussiedler*) were admitted.[18] The recruitment of foreign workers continued via agreements with several countries: first Italy (1953), then Spain, Greece (1960), and Turkey (1961). The foreign population rapidly increased, and, in 1964, the arrival of the millionth guest worker was even celebrated.[19] The building of the Berlin wall in 1961 stopped the flow of immigrants from East Germany, and led to further agreements: Morocco (1963), Portugal (1964), Tunisia (1965), and Yugoslavia (1968). During this period, immigration was considered a resource to support economic growth, albeit permanent settlements were preferably prevented. Recruitment programs were based on a rotation principle (*rotationprinzip*): migrants would enter Germany for a period of at most two years, returning to their origin country to make room for other guest workers. Between 1961 and 1973, about 14 million foreign workers entered Germany, of whom about 11 million left the country. The oil crisis of the 1970s marked a major change. It ended a period of unprecedented economic growth and opened the new era of stagflation. In order to preserve jobs for natives a halt on recruitment of non-EEC workers (*Anwerberstopp*) was declared on November 23, 1973. At that time, Germany had 2.6 million of foreign workers, with Turks (23%), Yugoslav (18%) and Italians (16%) representing the main minorities. However, it is important to note that family reunions (with spouses and children under sixteen years old) were still allowed (Heckmann, 1995). Against the government's expectations, the immigrants did not want to lose their jobs and, rather than leaving, they sought to sponsor the entry of their wives and children. As a consequence, the foreign population *rose* and its composition shifted from workers to dependents (Constant and Massey, 2002). This fostered the creation of a large second-generation. In terms of the theoretical model presented in section 2, the *Anwerberstopp* fostered permanent settlement, pushing immigrants with stronger commitment to their native culture to stay in Germany, and possibly convey their cultural traits to the second generation.[20] Galli and Russo (2019) find evidence of lower cultural integration of second-generation children whose parents were subject to the *Anwerberstopp*. We

[18]In particular, until the building of the Berlin Wall in 1961, around 3.5 million Germans coming from East Germany settled in the Federal Republic of Germany.

[19]This worker was Armando Rodrigues de Sás, a 38-year-old Portuguese carpenter. A bouquet of carnations, a certificate of honor and a Zündapp Sport Combinette moped awaited him at the Cologne-Deutz station.

[20]Notice that similar bans were enforced in Austria, France, Luxembourg, Switzerland, and the Nordic countries. These measures, together with the border controls that in the 1970s still restricted the movement of workers, minimize the risk of immigrants flowing from Germany to the neighboring countries, and that the immigrant pools in Germany before and after 1973 are affected by policies put in place by other countries.

now want to test if the *Anwerberstopp* has affected one of the most relevant aspects of the cultural identity, namely, the religiosity *of the second generation*.

4. Data

In order to assess the effect of the Anwerbestopp on the religiosity of second-generation immigrants, we use data from the European Social Survey, waves 2-10.[21] This research initiative, established in 2001, is committed to undertaking cross-national surveys through face-to-face interviews every two years,[22] with newly selected samples that represent diverse European populations aged 15 and above. To date, a total of 38 countries have participated in at least one round of the survey. This comprehensive survey addresses a wide spectrum of topics, encompassing living conditions, social structures, public opinions, attitudes, and behavioral tendencies. Furthermore, it collects variables related to ethnicity and religiosity, which are particularly relevant and useful for our analysis. In particular, individuals were asked to rate their religiosity ("How religious are you") on a scale of 1 to 10, where 1 represents the lowest level and 10 the highest level. Figure (A1) illustrates the methodology employed to generate our sample.

The ban imposed by Germany specifically targeted non-EEC (European Economic Community) immigrants, allowing EEC immigrants to freely travel between Germany and their home countries. Therefore, on the basis of father's country of origin, we distinguish a treatment and a control group. The control group includes Italy, France, United Kingdom, Austria, Netherlands, Belgium, Ireland, Denmark and Luxembourg.[23]

Figure A2 reports the major source countries of immigrants to Germany both in the overall sample and in the restricted time window 1963–1983 that we used in most estimations. Poland and Turkey are the countries with the largest number of immigrants. We can see that countries in the control group are substantially balanced before and after 1973, while a slight imbalance is present in the treatment. In particular, European countries like Poland or Czechoslovakia have a larger weight before the Anwerbestopp, while Turkey has a larger weight after. This is quite consistent with the historical evidence that movements from European countries started in the 50's, while Turkey signed a recruitment agreement only in 1961. In the robustness checks section below, we explored the robustness of our results by checking whether they hold when larger countries and when countries with sizeable German minorities are excluded from the sample. Individuals are assigned to the control or treatment group depending on the origin of their fathers. The descriptive statistics for our sample are included in Table (A1). Our proxy for the level of religiosity exhibits a mean value of 4.16. The standard deviation of 3.01 indicates a notable degree of variability in religiosity scores across the sample. The majority of individuals, on average 80%, consists of second-generation individuals with non-EEC

[21]The first wave does not report information on the origin of the parents.

[22]The COVID-19 pandemic disrupted face-to-face fieldwork for Round 10. Out of the 31 participating countries, 22 adhered to the conventional face-to-face methodology, while 9 countries, including Germany, transitioned to a self-completion approach that incorporated web-based and paper surveys.

[23]The countries in the control group were members of the EEC in 1973, with the exception of Austria. However, in 1973, Austria became a member of the European Free Trade Association (EFTA), which promoted unrestricted movement of workers (article 20).

fathers. Approximately 44% of the individuals were born after the policy year. Gender distribution is relatively balanced, with an average of 49% males.

5. Empirical evidence

5.1 Identification strategy

To capture the effect of the immigration policy on the second-generation immigrants, we use a diff-in-diff model[24]:

$$Y_{it} = \alpha + \gamma * BornAfter_{it} + \delta * extraEEC_{it} + \beta * Treatment_{it} + \theta * X_{it} + \epsilon_{it} \qquad (12)$$

Where Y_{it} is our proxy of religiosity, "Born after" refers to being born after 1973, "extraEEC" indicates the treated group, "Treatment" is the interaction between "Born after" and "extraEEC", thus the parameter of interest is β. The specification is completed by ϵ_{it}, the error term, and a vector of exogenous controls X_{it}; namely, gender, year of birth of respondents,[25] and dummies for the round of ESS in which individuals were interviewed. Given that the identification relies on the exogeneity of the policy, in the main regressions presented in Table A2, we decided to be conservative and avoid all possible controls that could theoretically be correlated with the treatment. A richer set of hopefully exogenous controls is proposed in the regressions of Table A5.

To establish support for the parallel trends assumption, we examine the average religiosity based on the year of birth in Figure A3, where the averages of religiosity for EEC and extra EEC countries are interpolated nonparametrically using Loess.[26] Before the policy year, both groups exhibit parallel trends.[27] After 1973, the trend of the treatment (extra EEC) group is reversed, supporting our identification. The observed declining trend in the control (EEC) group aligns with the overall decrease in religiosity observed in all countries represented in our sample. In Figure A4, we illustrate the average religiosity of the natives, along with the corresponding fitted linear trends, for the main countries included in the analysis. The increasing trend in the religiosity of the treatment group contrasts starkly with the pervasive decline we can observe everywhere else. This is consistent with the theoretical framework we have outlined and suggests that the effects of the *Anwerbestopp* are indeed visible in the second generation of immigrants.

5.2 Results

For our analysis, our favorite time window is 1963–83. We also ran several checks for symmetric and asymmetric windows around 1973. However, we think that the 1963–83

[24]For simplicity, we present the linear specification in equation (12). An ordered probit model is presented hereafter.

[25]This variable can be interpreted as a linear trend, whose purpose is to control for integration effects due to time-varying confounders, such as longer exposure to German culture or different skill levels in immigrant cohorts.

[26]We used the default smoothing parameter $\alpha = 0.75$ of the Loess function in R.

[27]A possible change in the trend is observable around 1967–68. It is hard to figure out the cause of this change. An hypothesis could be that the economic recession of 1967 or the student movement of 1968 could have played some role. However, since the change seems to affect both the treatment and the control groups, parallelism looks preserved.

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window gives the best trade-off in terms of closeness to the policy year and sample size (namely, 545 observations). The results of our favorite estimation of equation (12) are presented in table (2). In column (1), we employ an ordered probit model, while in column (2), we use ordinary least squares for a more straightforward interpretation. The reported results include standard errors clustered by the father's country of origin.[28] For reference, the same estimations with classic and robust standard errors are presented in Table A11. In both estimates, the treatment demonstrates a positive and statistically significant impact at the 5% level. In the linear model, we observe that belonging in the second generation born to non-EEC fathers implies a marginal effect of approximately 1.5 points on the level of religiosity.[29] This outcome is in line with our prediction, namely, the *Anwerbestopp* "forced" to settle in Germany many first-generation immigrants who would have preferred circular or return migration based on their own cultural tastes. These immigrants were characterized by a higher religiosity, which was conveyed to the second generation.

Interestingly, the OLS results provide an opportunity to explore the magnitude of our estimates. In particular, being part of the non-EEC second generation born after 1973 implies a 1.5-point increase in religiosity. With the mean religiosity score standing at 4.16 (Table 1), this point estimate implies a more than 35 percent increase in the levels of religiosity, which indicates a sizable effect. This coefficient might seem large, yet it must be considered that religiosity displays a high variance (Table 1), and a 1.5 point variation would amount to roughly half a standard deviation. Moreover, the small sample size generates a relatively large confidence interval for the causal effect: the 95% bootstrap-computed interval ranges from 0.28 to 2.83, so the true effect could be from five times smaller to twice as large than the estimated value.

Thus, according to our results, an immigration ban in 1973 has contributed to create a population whose religiosity, after half a century, goes definitely against the trend prevailing in both the origin and the destination countries. This finding shows that immigration policies may have far-reaching effects on the structure of the receiving societies, and they should be carefully used for achieving short-term objectives.

Finally, there is a caveat: our results might be subject to a kind of *intertemporal* external validity problem. In other words, it must be acknowledged that the story we are telling dates back to half a century ago, when communications were difficult and expensive, border controls were enforced even within Europe, there was no Internet, neither low-cost flights or satellite TV. Even English was far from being the popular *lingua franca* it is today. This background possibly contributed to exacerbate the difficulty of adaptation to the host society, generating ethnic separation and social marginalization. For this reason, it is difficult to argue that *current* immigration restrictions would yield the *same* effect. Today, in a sense, the problem is reversed: it is the ease of communication and transporting cultural consumption across borders that may delay the integration. To be more precise, we do not know the net effect of the ease of traveling and communicating on the incentives to integration. This is an open question both theoretically and empirically, which we are eager to address in future research.

[28]Due to the sizeable asymmetry in the dimensions of the clusters we used, we followed the suggestion of MacKinnon and Webb (2017) and used the wild cluster bootstrap of Cameron *et al.* (2008), in its score version proposed by Kline and Santos (2012) for ordered probit estimations.

[29]It worth to remark that males exhibit significantly lower levels of religiosity compared to females.

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6. Robustness

6.1 Alternative time frames

To ensure the robustness of our findings and demonstrate their independence from the chosen time frame, we estimate Equation (12) applying different window configurations (symmetric and asymmetric) around 1973. Again, we use our baseline regression model, incorporating clustered standard errors based on the father's country of origin. Table (A3) shows a matrix with symmetric windows positioned along the main diagonal. Each cell presents the parameter of interest, sample size, and standard errors. We employ a color-coding system to indicate the significance level (1%, 5%, 10%). It is worth noting that, with a few exceptions, the magnitudes of these parameters remain consistently stable. Most of them exhibit high or medium statistical significance, as indicated by the blue shading.

6.2 Placebo regressions

To assess the significance of the policy year, we conduct placebo regressions by changing the policy year. The results are presented in Table (A4). In column (1) we employ a symmetric window of ±10 years around the counterfactual policy year 1963. In Column (2), we restrict our sample to focus on the period before 1973 and use a symmetrical window spanning ±6 years around 1967. This year provides a suitable alternative, because it corresponds to a recession, which caused a notable decrease in arrivals to Germany (Jennissen, 2014). The estimates on the coefficients of interest are not significant; generally, signs are even negative for the two counterfactual policy years before 1973.

6.3 Religious affiliation and contextual effects

As an additional robustness check, we estimate Equation (12) by including variables for the region of residence (länder), religious affiliation, and their interactions with the policy year. The classification of the treatment and control groups remains based on the father's country of origin. The findings for the coefficient of interest, as presented in Table (A5), consistently align with those in Table (2).

In Column (1), we introduce region-specific interview dummies to account for unobserved regional heterogeneity. Notably, the coefficient associated with the treatment closely resembles our baseline regression in both sign and statistical significance (5%). Column (2) presents the coefficients for religious affiliations, with "non-religious" as the reference category. Remarkably, all coefficients of religious affiliations are positive and statistically significant at the 1% level.

In Column (3), we add region of residence dummies to the religious affiliations. The results remain consistent in terms of sign and significance, keeping significance at the 1% level.

Column (4) examines the interaction between religious affiliation and dummy variables indicating birth before and after 1973. All coefficients are positive and statistically significant at the 1% level, except for 'other religion", which is insignificant.

Finally, Column (5) replicates the estimations from Column (4) while incorporating residence dummies. Once again, sign and significance of the coefficients are preserved.

6.4 Alternative rules of inclusion in treatment and control groups

In table A6, we introduce alternative criteria for defining the treatment and control groups. In Column 1, we distinguish second-generation immigrants on the basis of

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the mother's country of origin. The coefficient of interest is very similar to the one obtained by using the father's country of origin and is strongly significant. In this estimation we clustered on the mother's country of origin.

In Column 2, we further refine our analysis by restricting the sample to households with both parents coming from the same country. Despite a considerable reduction in the sample size, the results are confirmed, though significant at the 10% level. Remarkably, the estimated coefficient is almost twice as large than in the cases when only one parent is considered, suggesting, as one can expect, that cultural transmission is more effective when both parents share the same background.

Finally, in Column 3, we include in our sample all the individuals with at least one parent of foreign origin.[30] The sample is larger than the one used in the main estimations. The coefficient of interest does not change substantially and is highly significant.

6.5 Ethnic Germans

In Column 4 of table A6, we run an estimation excluding countries with sizeable German minorities[31] to check whether our results depend on the repatriation of ethnic Germans. The estimated effect is slightly reduced, but significant at the 5% level.

6.6 Pre-treatment effect

A possible source of bias in the estimated causal effect of the Anwerbestopp could be due to some sort of "pre-treatment" effect in our data.[32] It is in fact possible that some immigrants whose children were born before 1973 were planning to return to their origin country, but changed their mind after the Anwerbestopp. Due to this phenomenon, though the immigrants' decision was actually affected by the policy in 1973, our identification strategy, based on the year of birth of their offspring, could lead to observe a change in the response up to a few years before.

In principle, this possibility does not invalidate our strategy, which only requires that the Anwerbestopp affects the average home-bias of the treatment group. However, this pre-treatment effect could introduce some downward bias in our estimates, as some treated units are coded *before* the policy year.

It is difficult to assess how important this problem is in practice. In the first place, we notice that, as mentioned in section 3, the guest workers were initially subject to the rotation principle, under which they were assigned renewable yearly work permits tied to a specific job and employer. At the beginning, these workers were mostly unaccompanied young men. In the 60's, as the labor shortage became permanent, the *rotationprinzip* was basically dismissed. Work permits were extended, and those who wanted to settle permanently raised their families in Germany, attesting their integration into the German society (Constant and Massey, 2002). However, just because mobility was free, it is still possible that some families who were planning to return were locked in by the Anwerbestopp.

[30]In this estimation, we clustered on the country of the foreign parent if only one is not native, and on the father's if both parents were born abroad

[31]We removed individual whose fathers were born in Austria, Russia, Ukraine, Belarus, Latvia, Lithuania, Czechoslovakia, Poland, Kazakhstan, United States, South Africa, Argentina, and Brazil

[32]We thank an anonymous referee for this remark.

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If we assume that families with children older than 6–7 had decided to settle in Germany many years before, we can guess that this effect mostly concerned families with children no older than 5–6 years in 1973. Thus, we ran an estimation excluding individuals born from 1968 to 1973. This robustness check should hopefully eliminate most of the pre-treatment effect due to this mismatch. Results are shown in column 5 of table A6. The estimated treatment effect is very similar to the one resulting from the full sample suggesting that this pre-treatment effect either is of limited magnitude or not present at all.

6.7 Effects of single countries

As shown in figure A2, the distribution of origin countries in our sample is uneven, with a few countries representing a large proportion of the individuals. To ensure that our results are not influenced by the immigration from a single major country, we removed the six most frequent origin countries one at the time in the ordered probit regressions of table A7. Though of course sample sizes and estimates display some variation, the sign of the estimated effect is unchanged and there does not seem to be a single country that drives our results, which look robust from this point of view.

6.8 Alternative measures of religiosity

Finally, we used questions about participation in religious events and the frequency of pray to obtain two alternative measures of religiosity. On a scale ranging from 1 to 7, individuals were asked two questions: "how often do you attend religious services?" and "how often do you pray?". The response options are: never (1); less often (2); only on special holy days (3); at least once a month (4); once a week (5); more than once a week (6); every day (7). The alternative measures are named, respectively, "Participation" and "Pray".

In Table (A8), we present the correlation coefficients (Spearman's correlation) among the three indicators of religiosity. There exists a moderate positive relationship between personal religiosity and the frequency of participation in religious events, with a correlation coefficient of 0.633. Furthermore, the correlation between religiosity and the frequency of prayer is even stronger, measuring 0.740.

Table (A9) presents estimations of equation (11) with "Participation" and "Pray" as dependent variables, using the same format as in the baseline estimation (Table A2). The coefficients of interest show positive and statistically significant results at the 5% level for both variables.

6.9 Different clustering variables

In most of our estimations, we decided to cluster on the father origin because we assumed that the unobservables which drive both religiosity and the intensity of cultural transmission can strongly depend on the cultural background of the immigrant. The country effect based on the origin of the father seems a suitable proxy for this cultural background. In table A10, we cluster on other possibly relevant sources of group correlation in the residuals, such as the mother's country of origin, the religious affiliation of respondents, their year and decade of birth and the ESS round. Results of course vary, but the significance of the treatment estimates is comparable. A possible exception is given by the clustering performed on birth year and ESS round. Here, we obtained suspiciously low p-values for the significance

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of the treatment. Though we present these results anyway, we think they are unreliable, possibly due to a failure or the bootstrap algorythm.

7. Conclusions

The socioeconomic integration of the second generations is crucial for supporting the welfare state, the economic growth, and the long-term social cohesion of the multi-cultural societies of most OECD countries. In this view, cultural integration is not only a necessary complement of economic integration, but even a prerequisite. What is the effect of immigration policies on this process? The cultural integration of foreign-born children crucially depends on parental decisions. By a straightforward application of the Lucas critique, we showed that immigration policies affect the size and the characteristics of the pool of immigrants. In particular, theoretical considerations and empirical findings suggest that restrictions on immigration push individuals with a stronger commitment to their native culture to settle and reproduce abroad. To the extent that these individuals convey their culture to the second generation, restrictive immigration policies foster the diffusion of cultural traits and bias the intergenerational process of cultural integration. We exploited the major natural experiment occurred in Germany in 1973, which provides a unique opportunity to investigate the consequences of immigration restriction. We found that the descendants of treated immigrants show higher religiosity. This adds further evidence to the initial results in Galli and Russo (2019) and is important for two reasons: (1) in the literature, religiosity is associated with lower propensity to migrate (Docquier *et al.*, 2020; Falco and Rotondi, 2016; Myers, 2000). Higher religiosity in the second generation confirms that first-generation immigrants who suddenly lost the possibility of moving back and forth were somewhat “forced” to settle in Germany. (2) Religiosity is a major component of the cultural identity, and its transmission confirms the immigrants’ will to preserve the origin culture. Though this does not imply any antagonism with the receiving society, it shows a distortionary long-term effect of restrictive immigration policies.

We are aware that the social and economic conditions in 1973 were quite different than today —not to mention the ease of movement and communication- and that our results cannot be generalized to the present times. However, at least they show that immigration policies can have very far-reaching effects, which can be unexpected to short-sighted policymakers, and show up after decades. In a globalized labor market, more open immigration can reduce distortions in the self-selection of immigrants and, possibly, in the intergenerational integration process. Similar policies were successfully used in the past, and include for instance, free movement agreements between countries,[33] dual citizenship, and permanent or indefinitely renewable residence permits.

Supplementary material. The supplementary material for this article can be found at https://doi.org/10.1017/dem.2024.17

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Competing interests. None.

[33]See Constant *et al.* (2013) for a survey of major mobility partnerships worldwide.

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Appendix A

The average cultural capital is given by

$$\overline{\theta}(\pi) = \frac{\int_1^{\theta^*(\pi)} \theta_j f(\theta_j)\, d\theta_j}{\int_1^{\theta^*(\pi)} f(\theta_j)\, d\theta_j}. \tag{A.1}$$

The derivative of (A.1) with respect to the immigration policy π is

$$\frac{\partial \overline{\theta}}{\partial \pi} = \frac{f(\theta^*(\pi))\theta^{*\prime}(\pi)\Big(\int_1^{\theta^*(\pi)} \theta^*(\pi) f(\theta_j)\, d\theta_j - \int_1^{\theta^*(\pi)} \theta_j f(\theta_j)\, d\theta_j\Big)}{\Big(\int_1^{\theta^*(\pi)} f(\theta_j)\, d\theta_j\Big)^2} < 0 \tag{A.2}$$

where

$$\theta^{*\prime}(\pi) \equiv \frac{\partial \theta^*}{\partial \pi}. \tag{A.3}$$

To prove that (A.2) is negative, notice that, in the numerator, the term $f(\theta^*(\pi))\theta^{*\prime}(\pi)$ is negative by equation (4), and the term $\Big(\int_1^{\theta^*(\pi)} \theta^*(\pi) f(\theta_j)\, d\theta_j - \int_1^{\theta^*(\pi)} \theta_j f(\theta_j)\, d\theta_j\Big)$ is positive because, by construction, $\theta^*(\pi) > \theta_j$ for any $j \in [1, \theta^*)$.

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Table A1. Descriptive statistics, window 1963–1983

	Mean	Std dev	Min	Max
Religiosity	4.16	3.01	0	10
Born after	0.44	0.497	0	1
Extra EEC	0.80	0.403	0	1
Male	0.49	0.500	0	1
Year of birth	1972.70	6.153	1963	1983
Round 2	0.07	0.264	0	1
Round 3	0.07	0.252	0	1
Round 4	0.09	0.299	0	1
Round 5	0.12	0.326	0	1
Round 6	0.09	0.283	0	1
Round 7	0.12	0.319	0	1
Round 8	0.12	0.319	0	1
Round 9	0.07	0.251	0	1
Round 10	0.25	0.444	0	1
N	545			

Source: Authors' elaborations on ESS, waves 2 to 10.

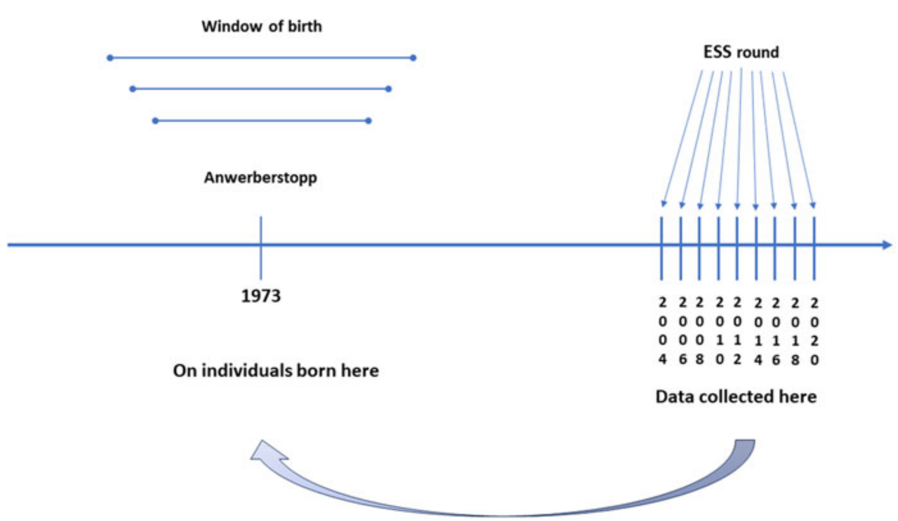

Figure A1. Timeline of natural experiment and data collection.

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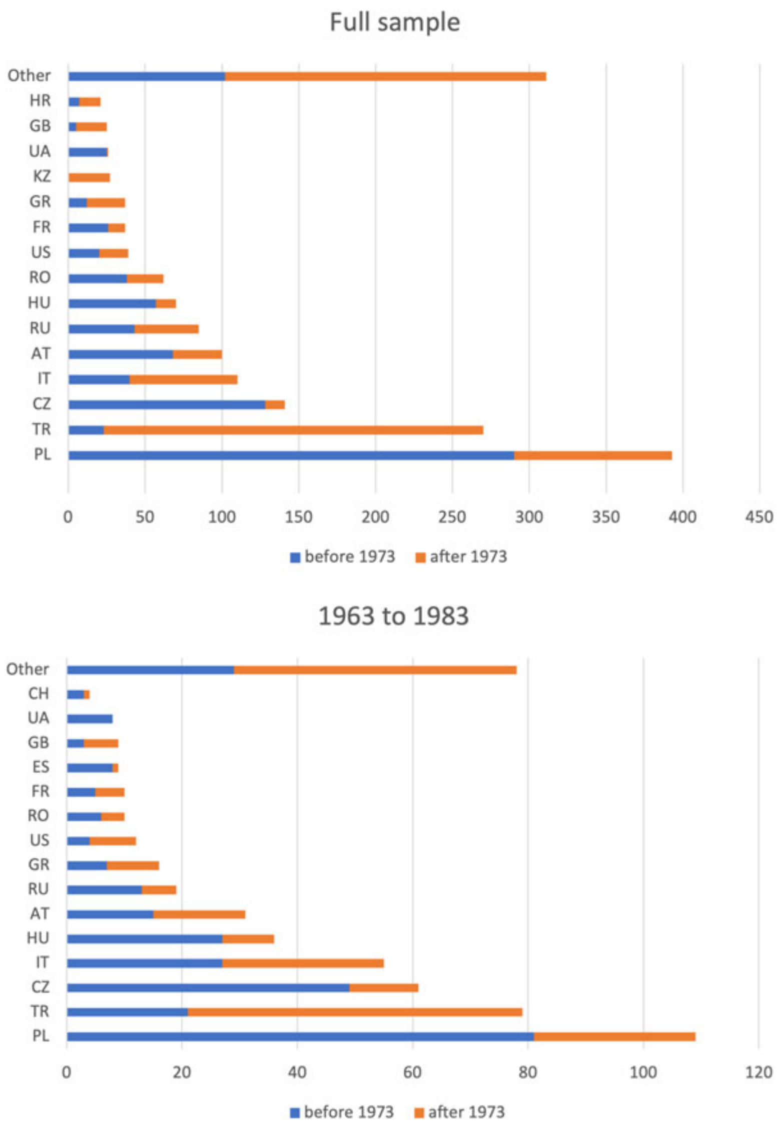

Figure A2. Father's country of origin, full sample and sample used for estimations.

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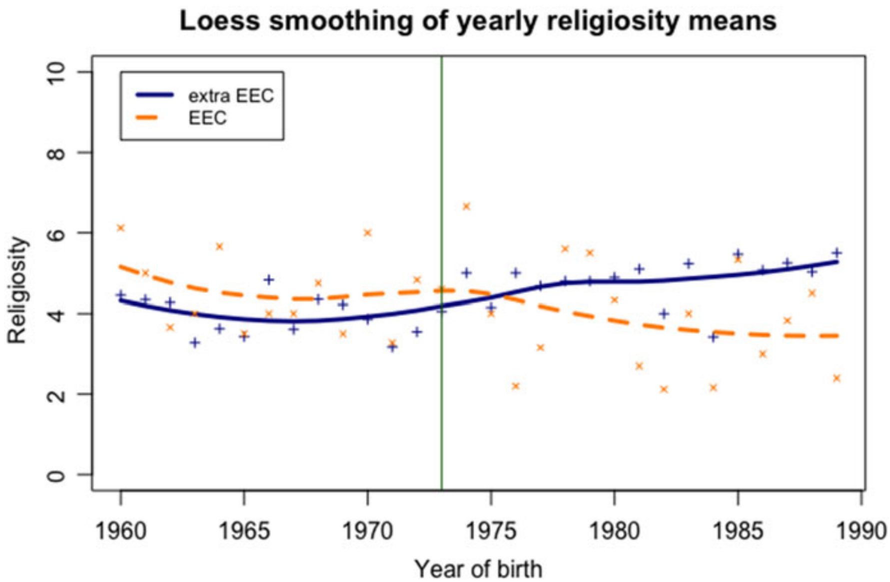

Figure A3. Loess smoothing of mean religiosity by year of birth, window 1960–1989 (Source: ESS, waves 2–10). Control group: orange, dashed, ×. Treatment group: blue, solid, +.

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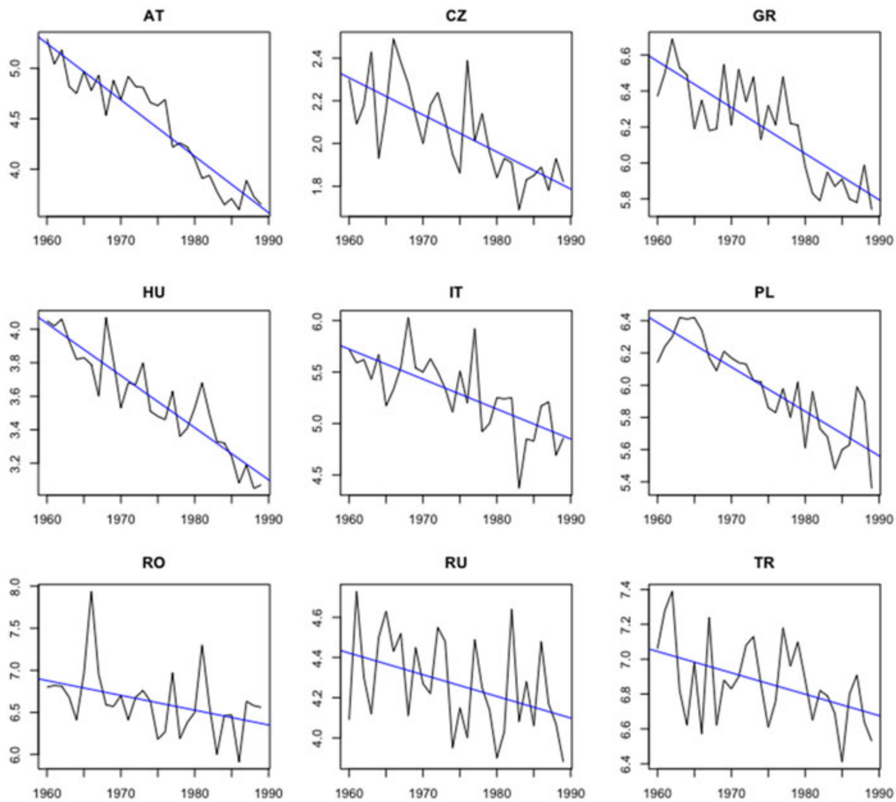

Figure A4. Average religiosity of natives of different origin countries, by year of birth, window 1960–1989 (Source: ESS, waves 2–10). Linear trend in blue.

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Table A2. DID analysis

	Religiosity	
	(1)	(2)
	oprobit clustered SE	ols clustered SE
Treatment	**0.545****	**1.484****
	(0.016)	(0.028)
Born after	−0.143	−0.369
	(0.526)	(0.554)
Extra EEC	−0.140	−0.433
	(0.486)	(0.438)
Male	−0.323***	−0.869***
	(0.000)	(0.002)
Year of birth −0.0110	−0.00501	−0.0110
	(0.724)	(0.776)
Intercept		25.81
Round	Yes	Yes
N	545	545
Pseudo R^2 / R^2	0.0159	0.0677

Dependent variable: Religiosity. Time window: 1963–1983. Estimation methods: Ordered probit and ordinary least squares with wild bootstrap clustered standard errors on father's country of origin.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A3. DID analysis

	1975	1977	1979	1981	**1983**	1985	1987
	0.144	0.798	0.566	0.671	0.816	0.841	0.856
1971	0.758	0.038	0.056	0.030	0.012	0.004	0.004
	125	159	208	275	321	372	427
	−0.016	0.630	0.41	0.519	0.633	0.671	0.695
1969	0.972	0.050	0.098	0.022	0.020	0.006	0.004
	180	214	263	330	376	427	482
	−0.074	0.614	0.390	0.495	0.603	0.641	0.660
1967	0.942	0.058	0.148	0.064	0.006	0.006	0.022
	234	268	317	384	430	481	536
	−0.142	0.507	0.295	0.412	0.508	0.548	0.570
1965	0.770	0.114	0.270	0.124	0.022	0.016	0.034
	291	325	374	441	487	538	593
	−0.121	0.535	0.324	0.45	**0.545**	0.592	0.609
1963	0.790	0.098	0.220	0.720	**0.016**	0.012	0.008
	349	383	432	499	**545**	596	651
	−0.222	0.475	0.285	0.407	0.498	0.542	0.557
1961	0.790	0.128	0.340	0.124	0.014	0.028	0.012
	417	451	500	567	613	664	719
	−0.088	0.472	0.297	0.422	0.514	0.563	0.589
1959	0.852	0.124	0.254	0.062	0.010	0.008	0.004
	483	517	566	633	679	730	785

Dependent Variable: Religiosity. Symmetric and asymmetric time windows centered around 1973. Treatment group based on the father's country of origin. Ordered probit with standard errors wild bootstrap clustered on father's country of origin.
Cell color: * $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.
Contents: Coefficient, *p*-value, sample size.

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Table A4. Placebo regressions

	Religiosity	
	(1)	(2)
Window	1953–1973	1961–1973
Counterfactual year	1963	1967
Treatment	−0.0603	−0.223
	(0.726)	(0.372)
Born after	−0.000617	0.342
	(0.948)	(0.210)
Extra EEC	−0.0697	0.0172
	(0.588)	(0.988)
Male	−0.126*	−0.155
	(0.052)	(0.150)
Year of birth	−0.00232	−0.0250
	(0.796)	(0.404)
Round	Yes	Yes
N	584	374
Pseudo R^2	0.00745	0.00931

Dependent Variable: Religiosity. Counterfactual years: 1963, 1967. Ordered probit with wild bootstrap clustered standard errors based on father's country of origin.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A5. DID analysis

	Religiosity					
	(1) Region	(2) Religion	(3) Religion Land	(4) Interaction Land	(5) Interaction Land Mother country	(6) Interaction Land Mother country Parents' education
Treatment	**0.495****	**0.512****	**0.607****	**0.670****	**0.772****	**0.972****
	(0.018)	(0.012)	(0.028)	(0.014)	(0.014)	(0.012)
Born after	−0.0982	−0.111	−0.108	−0.575***	−0.774***	−0.651
	(0.720)	(0.472)	(0.558)	(0.002)	(0.002)	(0.136)
Extra EEC	−0.0835	−0.131	−0.177	−0.196	−0.160	−0.376*
	(0.672)	(0.208)	(0.250)	(0.190)	(0.342)	(0.094)
Male	−0.339***	−0.285***	−0.258***	−0.273***	−0.292**	−0.584***
	(0.000)	(0.002)	(0.000)	(0.000)	(0.000)	(0.000)
Year of birth	−0.0196	−0.0123	−0.0256	−0.0118	0.00635	0.0063
	(0.266)	(0.362)	(0.140)	(0.458)	(0.704)	(0.760)
Christian		1.399***	1.363***			
		(0.000)	(0.000)			
Protestant		1.323***	1.318***			
		(0.000)	(0.000)			

Orthodox	1.538***	1.694***	
	(0.000)	(0.000)	
Jewish	2.222**	2.162**	
	(0.012)	(0.046)	
Islam	1.583***	1.490***	
	(0.000)	(0.000)	
Other	0.732	0.582	
	(0.140)	(0.192)	
Christian after			1.591***
			(0.000)
Christian before			1.144***
			(0.000)
Protestant after			1.633***
			(0.000)
Protestant before			1.027***
			(0.000)
Orthodox after			1.312**
			(0.006)
Orthodox before			2.390**
			(0.038)

(*Continued*)

Table A5. (Continued.)

	Religiosity					
	(1) Region	(2) Religion	(3) Religion Land	(4) Interaction Land	(5) Interaction Land Mother country	(6) Interaction Land Mother country Parents' education
Jewish after				2.038		
				(0.122)		
Jewish before				2.223		
				(0.164)		
Islam after				1.607***		
				(0.000)		
Islam before				1.354		
				(0.136)		
Other after				0.219		
				(0.752)		
Other before				0.531		
				(0.412)		
Round	Yes	Yes	Yes	Yes	Yes	Yes
Region	Yes	No	Yes	Yes	Yes	Yes

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Mother country	No	No	No	No	Yes	Yes
Interactions	No	No	No	Above	Yes	Yes
Parents educ. dummies	No	No	No	No	No	Yes
N	413	545	413	413	413	230
Pseudo R^2	0.0273	0.0998	0.1016	0.1019	0.1306	0.1785

Dependent variable: Religiosity. Ordered probit with standard errors wild bootstrap clustered on father's country of origin. Different combination of region, religion affiliation, religion affiliation interacted with pre- and post-1973 dummy variables, mother country of origin and dummies for the level of parent education.
T-test p-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A6. DID analysis

	Religiosity				
	(1)	(2)	(3)	(4)	(5)
	Mother	Both parents	At least One parent	No German Minorities	Born 1968–73 Excluded
Treatment	**0.524*****	**0.996***	**0.493*****	**0.437****	**0.524****
	(0.006)	(0.082)	(0.008)	(0.048)	(0.022)
Born after	−0.372**	−0.529	−0.135	−0.161	−0.063
	(0.010)	(0.066)	(0.142)	(0.422)	(0.866)
Extra EEC	−0.417	−0.627	−0.138	−0.085	−0.104
	(0.536)	(0.138)	(0.126)	(0.584)	(0.640)
Male	−0.387***	−0.322***	−0.335***	−0.332***	−0.385
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Year of birth	0.0225*	0.0125	−0.009	−0.013	−0.007
	(0.066)	(0.556)	(0.560)	(0.314)	(0.654)
Round	Yes	Yes	Yes	Yes	Yes
N	410	170	663	355	398
Pseudo R^2	0.0188	0.0326	0.0142	0.0268	0.0179

Dependent variable: Religiosity. Time window: 1963–1983. Ordered probit. Different rules of inclusion into the control and treatment groups: column (1): Mother's birth country, wild bootstrap clustering on mother's origin. Column (2): both parents from the same country, w.b. clustering on parent's origin. Column(3): at least one parent born abroad, w.b. clustering on the immigrant parent or on father. Column (4): excluding countries with German minorities from the sample (Austria, Russia, Ukraine, Belarus, Latvia, Lithuania, Czechoslovakia, Poland, Kazakhstan, United States, South Africa, Argentina and Brazil), w.b. clustering on father's origin.
T-test p-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A7. DID analysis

	Religiosity					
	(1) Without Poland	(2) Without Turkey	(3) Without Italy	(4) Without Czechoslovakia	(5) Without Austria	(6) Without Hungary
Treatment	**0.480****	**0.390****	**0.674***	**0.521****	**0.388***	**0.581****
	(0.048)	(0.050)	(0.068)	(0.018)	(0.064)	(0.014)
Born after	−0.217	−0.145	−0.272	−0.079	−0.006	−0.062
	(0.266)	(0.544)	(0.378)	(0.732)	(0.978)	(0.792)
Extra EEC	−0.0559	−0.156	−0.192	−0.135	0.051	−0.123
	(0.730)	(0.418)	(0.550)	(0.522)	(0.748)	(0.558)
Male	−0.350***	−0.387***	−0.305***	−0.299***	−0.338***	−0.328***
	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)	(0.000)
Year of birth	0.00235	−0.00471	−0.004	−0.011	−0.003	−0.013
	(0.830)	(0.752)	(0.736)	(0.444)	(0.848)	(0.256)
Round	Yes	Yes	Yes	Yes	Yes	Yes
N	436	466	491	484	514	509
Pseudo R^2	0.0192	0.0178	0.0168	0.0168	0.0166	0.0163

Dependent variable: Religiosity. Time window: 1963–1983. Ordered probit model with standard errors wild bootstrap clustered by father's country of origin, excluding data from Poland, Turkey, Italy, Czechoslovakia, Austria, Hungary.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A8. Spearman's correlation: religiosity, participation and Pray

	Religiosity	Participation	Pray
Religiosity	1		
Participation	0.6339	1	
Pray	0.7405	0.6387	1

.

Table A9. DID analysis for the 1963–1983 period

	Participation	Pray
	(1)	(2)
Treatment	**0.536****	**0.571****
	(0.026)	(0.010)
Born after	−0.258	−0.247
	(0.356)	(0.392)
Extra EEC	−0.00363	−0.336**
	(0.988)	(0.048)
Male	−0.135	−0.347***
	(0.300)	(0.000)
Year of birth	−0.00278	−0.00585
	(0.776)	(0.666)
Round	Yes	Yes
N	545	540
Pseudo R^2	0.0233	0.0204

Dependent Variables: Participation and Pray. Ordered probit with standard errors wild bootstrap clustered on father's origin.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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Table A10. DID analysis

	Religiosity					
	(1)	(2)	(3)	(4)	(5)	(6)
	Clustering variable					
	Father birth country	Mother birth country	Religion denomination	Birth year	Birth decade	ESS round
Treatment	**0.545****	**0.545****	**0.545****	**0.545*****	**0.545****	**0.545*****
	(0.016)	(0.028)	(0.018)	(0.000)	(0.044)	(0.000)
Born after	−0.143	−0.143	−0.143	−0.143	−0.143**	−0.143
	(0.526)	(0.250)	(0.290)	(0.544)	(0.044)	(0.490)
Extra EEC	−0.140	−0.140	−0.140	−0.140	−0.140	−0.140
	(0.486)	(0.180)	(0.486)	(0.238)	(0.454)	(0.246)
Male	−0.323**	−0.323***	−0.323***	−0.323***	−0.323***	−0.323***
	(0.000)	(0.000)	(0.000)	(0.002)	(0.002)	(0.002)
Year of birth	−0.00501	0.00501	−0.00501	−0.00501	−0.00501	−0.00501
	(0.724)	(0.620)	(0.478)	(0.678)	(0.702)	(0.712)
Round	Yes	Yes	Yes	Yes	Yes	Yes

Dependent variable: Religiosity. Time window: 1963-1983. Estimation methods: ordered probit with wild bootstrap clustered standard errors on father's country of origin, mother's country of origin, religion denomination, year of birth, decade of birth, ESS round.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

Table A11. DID analysis

	Religiosity	
	(1)	(2)
	oprobit	ols
	classic SE	robust SE
Treatment	**0.545****	**1.484****
	(0.014)	(0.014)
Born after	−0.143	−0.369
	(0.574)	(0.587)
Extra EEC	−0.140	−0.433
	(0.365)	(0.311)
Male	−0.323***	−0.869***
	(0.000)	(0.001)
Year of birth	−0.00501	−0.0110
	(0.744)	(0.796)
Intercept		25.81
		(0.757)
Round	Yes	Yes
N	545	545
Pseudo R^2 / R^2	0.0159	0.0677

Dependent variable: Religiosity. Time window: 1963–1983. Estimation methods: Ordered probit and ordinary least squares with classic and robust standard errors.
T-test *p*-values in parentheses.
* $p < 0.1$, ** $p < 0.05$, *** $p < 0.01$.

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