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ABSTRACT

Serving Countries, Shaping Views: Military Conscription and Attitude Towards Immigrants*

We study the long-term impact of compulsory military service, a powerful nation building tool, on attitudes toward immigrants. Using a regression discontinuity design, we compare cohorts of men required to serve with those exempted due to suspension of compulsory service in 21 European countries. We find that conscripts exhibit more negative attitudes towards immigrants, whereas this is not true for women in the same birth cohorts. The impact is more pronounced in countries with high levels of immigration, and when the military service was done during a left-wing government, and hence provided a stronger change in narrative during a crucial formative period.

JEL Classification: I28, Z13, F22, D71

Keywords: nation building, conscription, identity, immigration, discrimination

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1 Introduction

Following the French revolution, the establishment of a more centralized system of power prompted countries to implement policies, such as compulsory military service and school reforms, designed to help "build a nation". These policies aim to foster shared experiences between peers (Depetris-Chauvin et al., 2020). Previous research has shown that these policies have succeeded in creating a sense of nation in places previously with strong local identities.[1]

In this paper, we analyze whether national building policies have indirect consequences, in particular whether creating a national identity changes how individuals view "outsiders" (Alesina et al., 2020; Grosfeld & Zhuravskaya, 2023). Alesina et al. (2020) argue that nation-building policies may deliberately stress the difference with out-groups and enemies to strengthen patriotism and the national identity. Pursuing this intuition, we examine whether men who were required to serve in their country's military have more negative attitudes toward immigrants.

We identify the effect of compulsory military service by exploiting its suspension in 21 European countries following the collapse of the Soviet Union. Using eleven rounds of data from the European Social Survey (ESS), we implement a regression discontinuity design (RDD) where we compare cohorts of men who were obligated to serve (henceforth conscripts) to just younger cohorts of men who were exempted due to the suspension of the service. Importantly, women serve as a natural control group that allows us to test whether there are differences between birth cohorts before and after the end of mandatory conscription.

This research question is particularly relevant at the moment, as several European leaders have debated the implications of suspending mandatory conscription and have argued that its reinstatement will foster a stronger sense of civic duty and national identity.[2] Furthermore, the current situation in Ukraine has led multiple European

[1] Examples include Alesina et al. (2021), Bagues and Roth (2023), Bandiera et al. (2019), Bazzi et al. (2019), Cáceres-Delpiano et al. (2021), Cantoni et al. (2017), and Ronconi and Ramos-Toro (2025). Rohner and Zhuravskaya (2023) provide an extensive overview of the existing literature on nation building.

[2] This included French President Macron, former German Minister of Defense Boris Pistorius, and

nations to reinstate conscription. And, at the same time, anti-immigrant sentiment has been increasing in Europe (Bell et al., 2021; Vohra, 2023).

Why might military service cause men to have more negative attitudes toward immigrants? Conscription forces most young men to spend a number of months undergoing basic training in camps away from their hometowns. This period of service involves constant interaction between peers designed to foster shared experiences that help form a collective national identity. Experiences during these formative years have been argued to have an important role in shaping individuals' attitudes and beliefs in the long run (Cantoni et al., 2017; Ertola Navajas et al., 2022). Former conscripts may retain a sense of patriotism influencing attitudes related to national identity (Bagues & Roth, 2023; Ronconi & Ramos-Toro, 2025). They may perceive immigrants from diverse backgrounds as potential challenges to their national identity. As this is the identity that they care about and want to preserve, this may lead them to develop more unfavorable opinions towards immigrants.

We find that, among non-university graduates, conscripted cohorts of men have more negative attitudes toward immigrants than slightly younger men who were not forced to do military service. Controlling for country, year, and age fixed effects, cohorts without conscription have a 0.05 - 0.07 standard deviation more positive attitudes toward immigrants. When we examine the impact on women from the same birth cohorts, we find a precisely estimated zero effect. Our results are also robust to dropping cohorts for whom conscription was being phased out and are stronger when we focus on countries where i) the phase-out period was shorter and ii) fewer conscripted men avoided military service (typically, by doing an alternative type of public service). We also find similar, but smaller impacts on university graduates, which is unsurprising because in many countries they were able to delay or avoid military service.

To better understand the underlying mechanism, we explore the heterogeneity in our results. First, we examine whether the effect persists over time by testing variation across age-groups. Rather than fading, the impact strengthens with age, supporting the formative years hypothesis that early experiences leave lasting imprints. Second, we test

Italian Vice Prime Minister Matteo Salvini.

whether impacts depend on immigration salience. The effects are larger in countries and regions with higher immigrant populations, suggesting that conscripts in these locations perceive greater threats to national identity. Third, we also find stronger effects in linguistically fractionalized nations suggesting that when military service forges a unified identity by bridging internal divides—simultaneously it strengthening in-group cohesion while sharpening out-group distinctions. Finally, we examine variation by government ideology during service periods. Individuals who served under left-leaning governments show significantly stronger effects, consistent with the idea that impacts are stronger when the change in narrative is larger during a crucial formative period.

Our paper is the first to examine how nation-building tools can have unintended consequences on people's attitudes towards out-groups. We contribute to the literature on national building policies, which explores the role of education (Bandiera et al., 2019; Blanc & Kubo, 2024; Cantoni et al., 2017; Feir, 2016; Paglayan, 2022), and of shared intense experiences, including the military service, in shaping the national identity (Bagues & Roth, 2023; Cáceres-Delpiano et al., 2021; Depetris-Chauvin et al., 2020; Okunogbe, 2024; Ronconi & Ramos-Toro, 2025).[3] Our results nicely complement Okunogbe's: while she finds that temporary exposure to a different ethnic region in Africa's largest national youth service program reinforces both the national *and* the ethnic identity of participants, we document that a stronger national identity comes at the expenses of a less welcoming attitude towards immigrants. The difference in our findings may relate to the fact that national and ethnic identity are both personal characteristics of the same individual, and therefore more likely to co-exist. An alternative explanation is that youth service programs may place less emphasis on the conflictual dimension between cultures compared to military service, where patriotic rhetoric is prevalent.

Even more closely, we contribute to the small literature that has looked at the indirect or less intended consequences of national building policies, such as cultural backlash (Fouka, 2020), increased regional identity (Dehdari & Gehring, 2022) and larger support for local organized crime (Marciante, 2024). Our work is also closely related to the

[3]Nation-building theories of education include Darden and Grzymala-Busse (2006), Ernest (1983), Galor and Moav (2009), Scott (2020), and Weber (1976)

literature that studies how military service has impacted labor market outcomes and education (Angrist, 1990; Angrist & Chen, 2011; Bauer et al., 2012; Di Pietro, 2013; Greenberg et al., 2022), crime (Galiani et al., 2011; Hjalmarsson & Lindquist, 2019; Lyk-Jensen, 2018) and voting behavior (Fize & Louis-Sidois, 2020; Vlachos, 2022).

Most closely related to our paper, Bove et al. (2022) find that conscription in Europe reduced institutional trust and led to more homogeneous values among men, while Ertola Navajas et al. (2022) find that it caused individuals to have a stronger military mindset and to be more politically conservative. The findings in both of these papers are consistent with the hypothesis that military service reinforces national identity and shared values among conscripts, potentially influencing how they perceive out-groups such as immigrants.

2 Institutional Background

With the rise of the nation-state and more centralized form of power after the French revolution, mass conscription became widely adopted in Europe. This approach allowed countries to create substantial military forces. Generally, modern conscription consisted of several months of enlistment, during which conscripts were sent to a military base located away from their hometown to receive basic military training alongside similar-aged peers. Conscription typically lasted around 12 months. However, the duration of service varied based on the branch of the military and the year of enlistment. Generally, those in the navy and air force faced longer service periods than those in the regular army. Additionally, older groups often had to serve longer, sometimes up to 36 months in Hungary, while younger groups usually had shorter terms, often less than a year.

While the specifics varied by country and military branch, basic training typically involved a structured and rigorous set of activities designed to train civilians into disciplined soldiers, carried out in an immersive environment emphasizing patriotism, national identity and civic duty. These elements were deeply woven into the structure and culture of conscription programs and manifested through rituals such as flag ceremonies, often accompanied by the national anthem, oaths of allegiance (e.g., loyalty to the nation,

constitution or head of state), marching and parades as public displays of national pride during national holidays or military anniversaries, and celebration of national history, heroes, and values to instill pride and a sense of belonging. Wearing the national military uniform may also help foster unity and reinforce the sense of equality among citizens.

Despite these nation-building functions, the end of the Cold War meant that geopolitical considerations no longer justified the cost of universal male conscription, leading most European countries to phase it out over time.[4] Appendix Table A.1 provides a comprehensive timeline outlining the abolition of compulsory military service in Europe for the countries included in our study. The United Kingdom was the first country to take this step, abolishing conscription in 1961. Belgium and the Netherlands followed suit in 1995 and 1997, respectively. From 2001 to 2009, the majority of countries in our sample discontinued conscription, with Germany being the last to abolish military service in 2012.[5]

3 Data

We use individual-level data from eleven rounds of the European Social Survey (ESS) conducted between 2002 and 2023. The ESS is a recurring cross-sectional survey administered every two years, although the participating countries may vary between rounds. Importantly, the ESS contains information on the gender and year of birth (i.e., cohort) of the respondent. In our main sample, we exclude individuals who currently are employed in the military, and non-citizens, who were never subject to mandatory military service.

Our main analysis focuses on men who do not have university degrees, as in many countries attending university either delayed military service or could be used to avoid it. Later, we examine whether there are also impacts on these individuals. We also only include individuals in cohorts born within fifteen years of the abolishment of military service. We opt for this bandwidth as it balances focusing on similar cohorts of individuals

[4] Austria, Denmark, Estonia, Finland, Greece, and Norway retain some form of compulsory military service.

[5] Recently, due to tensions between Russia and Ukraine, the situation has prompted a reassessment of conscription policies. In 2015, Lithuania reinstated military conscription, while Sweden reintroduced a highly selective conscription in 2017 including women, and Latvia followed suit in 2024.

and ensuring a sufficient amount of data to generate accurate estimates. We examine whether our results are robust to using alternative bandwidths. Our main sample includes 36,712 men. We also create a sample of women defined in the same way.

We measure attitudes toward immigrants by combining three variables from the ESS that capture economic and cultural attitudes, as well as general perceptions toward immigrants. Specifically, for economic and cultural attitudes, respondents rate, on a scale from 0 to 10, whether foreigners are bad (0) or good (10) for the country's economy or culture. Perceptions are captured by a question asking whether immigrants make the country a worse (0) or better (10) place to live. We then standardize each of these variables to have a mean of 0 and a standard deviation of 1 across the overall sample without any restrictions, and also create an average of the three variables as our main outcome measuring attitudes toward immigrants.[6]

For our heterogeneity analysis we make use of further data. First, we collect data on countries' foreign residents and immigrants inflows from the national statistical offices and Eurostat. The data comprises information on foreign residents without citizenship and on the inflow of foreigners from 2002 to 2020. In case of missing data we replaced them with the share of previous (subsequent) year plus (minus) the average growth rate of immigration over the sample period.[7] We then calculate the share of foreign residents relative to the total population. We also collect the immigration rate at the NUTS 1 level for the countries in our sample from various censuses. Subsequently, we combined the data with the nearest survey year.[8] Appendix Table A.2 provides more information on the share of foreign residents between 2002 and 2020.

Next, we gather data from Desmet et al. (2012) regarding linguistic fractionalization. Their index measures the probability that two randomly selected individuals belong to different linguistic groups. They compute these measures for different levels of linguistic aggregation using information on language trees. For instance, in Europe, the coarser level of aggregation corresponds to Indo-European and non-Indo-European languages,

[6]We also use principal component analysis to aggregate the three variables into one index. Our results are very similar using this approach versus using a simple average.

[7]We use data from 2015 as a proxy for 2020 in case of North Macedonia.

[8]We drop French data from after 2016 because of a change in how NUTS are defined.

while the finest corresponds to local dialects considered as different linguistic groups. Since most of the European languages are Indo-European, we chose the second level of aggregation which represents the principal branches of Indo-European languages, and reflects long-standing linguistic and (cultural) divides within the countries in our sample.[9]

Finally, we construct a time-varying measure of government ideology using data from the ParlGov dataset (Holger et al., 2022). For each country-year, we identify the cabinet(s) in office and compute the annual average ideological position as the mean across ideology scores of all governing parties. Precisely, our left-right variables ranges from 0 to 10, where 0 represents the most left-wing position and 10 represents the most right-wing position.[10]

4 Empirical Strategy

We exploit the abolishment of mandatory military service to identify the impact of conscription on attitudes towards immigrants. As this happened over a relatively quick time period in most countries, we can use a regression discontinuity design (RDD) to compare birth cohorts of men who were required to serve to those slightly younger who were no longer required to serve. As military service was abolished at different times in different countries, we can also control for country and year fixed effects that account for variation across countries and general changes over time in attitudes toward immigrants in Europe. Furthermore, as the ESS surveys a repeated cross-section of affected cohorts, we can also control for age fixed effects.

We define as treated men those whose birth cohort was not required to do military service. More formally,

$$T_{i,c} = \begin{cases} 1, & \text{if cohort} \geq \text{pivotal cohort.} \\ 0, & \text{otherwise.} \end{cases}$$

[9] Desmet et al. (2012) provide more detailed information on the construction of the index.

[10] In cases of government change within a year, we assign the cabinet to year t if its term begins before July 1 and to year $t+1$ otherwise.

where the pivotal cohort is defined as the first cohort without conscription. The running variable is then defined as the year of birth of an individual relative to the pivotal cohort in each country. We then estimate a RDD of the following form:

$$Y_{i,c} = \alpha + \beta\, T_{i,c} + \psi Z_i^q + \gamma\ T_{i,c} \times Z_i^q + X_{i,c,t} + \varepsilon_i \tag{1}$$

where $Y_{i,c}$ is the stated attitude towards immigrants for individual i in country c , $T_{i,c} = 1$ for *non-conscripted men* and $T_{i,c} = 0$ for *conscripts*. Z_i^q is the distance of cohort q from the "pivotal" cohort. $X_{i,c,t}$ is a vector of covariates that includes country, survey year and age fixed effects.

We estimate equation (1) using an OLS local linear regression with a uniform kernel. Standard errors are clustered at the country-year level.[11] The parameter of interest is β, which captures the causal effect of military service no longer being required for individuals in a particular birth cohort.

While it is highly unlikely, one could be concerned about potential manipulation of one's birth year to avoid military service. To address this concern, we use a McCrary density test, as depicted in Figure 1, to inspect selective sorting around the cutoff and to test the continuity assumption of the RD design (Hahn et al., 2001; Lee & Lemieux, 2010). As anticipated, we find no evidence of any discontinuity, indicating the absence of manipulation. We also examine whether there is a change in the composition of the sample over time. We do this by estimating equation (1) with pre-determined covariates as outcomes (age, father's and mother's education, and the year of observation). The results are presented in Table 1. We find that these characteristics are the same, on average, for cohorts with compulsory military service and those slightly younger.

A more relevant concern is that eligible individuals prior to the reform may have found strategies to avoid military service. Our empirical strategy treats all individuals subject to compulsory service based on their birth date as if they were effectively serving, irrespectively of whether they actually did–information we lack. In other words, our

[11] Kolesár and Rothe (2018) advise against clustering at the running variable level when having a discrete running variable, and suggest instead to cluster at the group level if the observations are not generated by independent sampling.

results should be interpreted in the spirit of an "intention to treat". This is likely to bias downwards our estimate as eligible individuals not serving prior to the reform are contaminating our control group. In section 5.2 we offer several complementary strategies to address this concern.

5 Results

5.1 Main Results

We begin by providing a graphical representation of the results in Figure 2. The x-axis shows the distance from the pivotal cohort, with positive values indicating individuals who were no longer drafted because of the reform implementation. Panel A reports the average standardized attitudes toward immigrants in a specification without any additional controls, where a higher number indicates that individuals have more positive attitudes toward immigrants. On the y-axis of Panel B we plot the residuals of the standardized outcome from a regression including country and year fixed effects. While no clear trends in attitudes toward immigrants over time in Europe seem to emerge, there is a visible improvement in attitudes among the first cohorts not subject to military service compared to just older cohorts with conscription.

Table 2 presents the results from estimating equation (1) using OLS without controls (Panel A) including only country and year fixed effects (Panel B), and finally also adding age fixed effects (Panel C). This final specification controls for age-specific heterogeneity, removing compositional differences in age structures and differential age-related patterns. We find robust evidence that ending conscription led to improved attitudes toward immigrants. Overall, the end of conscription is estimated to improve attitudes toward immigrants by 0.05 to 0.07 standard deviations (SD). Impacts are larger on economic and cultural attitudes, but there is also a statistically significant impact on perceptions toward immigrants. In the rest of the paper, we focus on specification C which includes country, year and age fixed effects.

5.2 Placebo Test and Robustness

We next examine the various assumptions underlying our main estimates. First, we estimate our main model using comparable cohorts of women who were exempted from military service in all countries in our sample. This allows us to test whether there is something particular about the cohorts in each country around the time that conscription was abolished. Figure 3 shows that there was no change in the average attitudes toward immigrants for women in these same cohorts. We estimate a β very close to zero. This bolsters our confidence in the interpretation of our main results. Any alternative explanation behind our main result should have affected only the male population around the asynchronous reforms removing the compulsory military service across 21 European countries.[12]

We next examine whether our results are robust to focusing only on cohorts closer to the time of reform in each country in our estimation. Figure 4 shows how the coefficients on the average attitudes toward immigrants (and the corresponding 95% confidence interval) change as we use smaller windows of data. Our findings become less significant as we decrease the estimation bandwidth, but do not change qualitatively. In red, we flag the MSE-optimal bandwidth proposed by Calonico et al. (2014).

While reducing the bandwidth does not change the qualitative result, it does reduce the quantitative effect significantly. One potential concern is that the cohorts immediately preceding the reform implementation may have already benefited from it. In other words, once the reform is discussed and ratifies by the legislator, many individuals may have already been able to avoid military service prior to conscription being officially eliminated. Indeed, during the phase-out period, there were fewer conscripts due to governments' lack of interest in mobilizing eligible men, as shown by Bove et al. (2022) and also in Appendix Figure A.1. If that is the case, by focusing on the latest cohorts preceding the reform our results are potentially biased downward. We address this concern in several ways. First, in Table 3 we examine whether our results are robust to excluding cohorts in the last years of conscription (Panel A to C) or for the entire country-specific phase-out period

[12] In Appendix Table A.3 we provide the complete results mirroring Table 2 on the corresponding sample of women.

(all years between the reform ratification by the legislator and its implementation - Panel D). In other words, in the spirit of a "doughnut hole" approach we estimate our model by excluding particular cohorts. The results are strengthened in line with the expectation.

We complement this by examining, in Panel E of Table 3, whether effects are larger in countries with short phase-out periods (one year or less), where we expect our RDD approach to work best, since the drop in exposure to the military service has been more rapid in the immediate neighborhood of the reform. As expected, all coefficients are larger than their corresponding estimates in Table 2.

A final and complementary approach is to replicate our analysis restricting the attention to countries where in the years preceding the reform, a significant fraction of the eligible young male population was still effectively conscripted. To do this, we calculate the share of 18-year-olds serving in each year by dividing the number of conscripts by the number of men aged 18 in each country. Figure A.1 shows the evolution of young men who served before the reform took place.[13] We calculate the average share of service by country in the five years preceding the reform and replicate our analysis focusing on the countries with relatively higher (above median) share of conscripts preceding the reform.

Notice that one potential reason for a relatively low share of conscripts before the reform comes from the ability of individuals to be conscientious objectors and opt instead for civil service, an alternative form of service to one's own country, typically involving unpaid activities such as social assistance, environmental protection, and cultural heritage promotion - all activities potentially attracting a selected population and having very different potential effects on attitudes toward migrants. This was sometimes discouraged by making the service period longer and was not common (well under 10 percent) except in Germany, Spain, and Italy.[14] The results of this exercise, reported in Panel F, once more imply a higher average effect than the corresponding estimates in Table 2, as expected.

In Appendix Table A.4 we present some additional robustness checks. In Panel

[13]In Bulgaria, certain years exhibit a share exceeding one. This happens because the duration of service in Bulgaria was 18 months, resulting in the overlap of multiple cohorts. We have adjusted those years equal one. We were unable to find these data for Albania, so they are excluded from this analysis.

[14]The alternative, more radical approach, of excluding these three countries from our sample does not affect our main results.

A we estimate model 1 with the use of triangular kernel which puts more weight on observations close to the cutoff. Unsurprisingly, in light of the above discussion concerning the phase-out period preceding the implementation of the reform in many countries, we obtain smaller and less precisely estimated effects.

In Panel B, we control for the trust of each of the respondents in the parliament, as conscription has been shown to negatively impact institutional trust (Bove et al., 2022). This reduces our estimated impact by around 20 percent, but it is still strongly significant. In Panel C we also control for individual trust in other people, to ensure that our results are not driven by a broader erosion of interpersonal trust. The results remain robust. All previous results were generated in the sample of young males excluding individuals who currently are employed in the military, with the rationale of keeping out a potentially very selected population, for instance in terms of political or nationalist attitude, clearly related to the attitude towards immigrants. It is possible, however, that the population of individuals choosing to work in the military may change after the reform. For instance, absent the military conscription, relative nationalistic individuals may be more likely to opt for a military professional career. If this is the case, then our previous analysis may overestimate the effect of the reform by changing the composition of professional military personnel. The results in Panel D, in which we include individual employed in the military in our sample, are identical to our main estimates.

Finally, in Panel E, we include individuals with a university degree in the sample. Since in many countries attending university either delayed military service or could be used to avoid it, by including these individuals we increase noise. More specifically, we are likely to assign to conscription many individuals who were either delaying their military service, possibly beyond the reform date, or avoiding the service altogether. Not surprising, the estimated effect is reduced, but we continue to find that, in general, ending conscription leads to more positive attitudes toward immigrants.

We also examine in Appendix Figure A.2 whether there is a particular country driving our results. We do this by running the same specification and leaving out the observations of each country one at the time. We find that our estimates are not driven by any specific

country.

Our last set of tests verify whether the change in attitudes toward immigrants aligns with the timing of the reforms. We adopt four alternative strategies. First, we report in the top figure in Appendix Figure A.3 the coefficients from estimating model 1 setting the discontinuity from six years before the reform until six years after it. Coefficients for the four years preceding the reform are significant and of similar size as the one estimated at the reform date. This is clearly a consequence of the phase-out period (on average four years among countries which delayed the implementation beyond one year from the ratification), during which, as already observed, many eligible young males were no longer conscripted.

Following the recent RDD literature, we therefore propose an alternative strategy immune to this sort of contamination. More specifically, we first split our sample into conscripted *vs.* non-conscripted individuals. Within each subsample we then run several placebo tests, by estimating the impact of "imposing" a discontinuity at 10 to 5 year distance from either side of the reform and then pool the estimates. These results presented in the bottom figure in in Appendix Figure A.3 are indeed more reassuring and indicate that no interesting dynamics occur around all these years relatively far from the reforms' decision and implementation dates.

A complementary strategy is to randomly assign the reform dates across the 21 countries and use model 1 to estimate the corresponding coefficient. We iterate this procedure one thousand times and report in increasing order the estimated coefficients (and 95% confidence intervals) in Appendix Figure A.4. Only about 5% of the coefficients are statistically significant, entirely consistent with the random nature of the procedure. For reference, we also report as a green dot the coefficient of estimating the model with the actual reform dates. Indeed, it is very unlikely for our baseline estimate to be the results of multiple spurious correlations.

Finally, in Appendix Table A.5, we test whether cohorts effects are driving our results by estimating equation 1 for countries that still maintain some form of conscription. Specifically, we construct a parallel sample of men in Greece, Austria, Switzerland,

Norway, Finland, Denmark and Estonia. In the absence of a pivotal cohort, we establish it as the average of the pivotal cohorts from neighboring countries that have eliminated conscription. For instance, in Switzerland, the pivotal cohort is 1988, which represents the "average" pivotal cohort of Italy (1986), France (1983), and Germany (1994). Reassuringly, we find no impact of our fake reforms.

5.3 Impacts on National Identity

The results in the previous sections document a robust negative effect of conscription on general attitudes toward immigrants, which we interpret as a side effect of strengthening national identity at the expense of individuals perceived as outsiders and potentially not aligned with its features. While an ultimate test of this interpretation is not feasible, the ESS collects in a subset of surveys (4 out of the 11 waves we use) information on respondents' emotional attachment to their own country and to Europe. As with our main dependent variables, respondents rate their emotional attachment on a scale from 0 to 10. We use the response to the first question as an admittedly imperfect measure of the strength of one's national identity, which should be affected by conscription if the negative effect on attitude towards immigrants is indeed working via this mechanism. And we use the response to the second question, focusing on the European identity, as a falsification test: military service should normally not boost the European (supra-national) identity.[15]

We report the results in Table 4 from estimating model 1 using this alternative outcome variables both in continuous form and, to capture the potential non-linearity of the effect, as dummy variables for individuals revealing a strong emotional attachment, defined for value larger than 7 (Panel B). The results in Panel A indicate the ending conscription resulted in a reduction of 0.4 standard deviation in emotional attachment to the country among men, although the coefficient is not statistically significant, potentially due to the severe decrease in the sample. The corresponding coefficient for Europe reveals a zero-effect. While the average effect is not significant, the results in Panel B indicate that conscription increases (by 4 percentage points) the number of individual having a

[15]Both outcomes are standardized.

strong national identity. This is in line with the interpretation offered in the previous section and the existing literature (Bagues & Roth, 2023; Ronconi & Ramos-Toro, 2025). Again, we find no impact on European identity.

6 Heterogeneity Analysis

So far we documented a robust negative effect of conscription on general attitudes toward immigrants, at least partially explained by the strengthening of national identity among conscripts. We next investigate several factors which may moderate or increase the impact of conscription. Formally, we augment model 1 to include a interaction term between a particular factor M (defined at the individual, regional or country level, depending on the specific test implemented) and the treatment T (Becker et al., 2013):

$$Y_{i,c} = \alpha + \beta\, T_{i,c} + \psi Z_i^q + \gamma\; T_{i,c} \times Z_i^q + \lambda\; T_{i,c} \times M_{i,c,t} + X_{i,c,t} + \varepsilon_i \tag{2}$$

where interaction coefficient λ captures how the treatment effect varies with M. As before, $X_{i,c,t}$ includes the full set of country, survey year, and age fixed effects to control for unobserved heterogeneity. All results are reported in Table 5.

6.1 Age

Prior research has shown that experiences during formative years can shape individuals' preferences and beliefs in persistent ways (Cantoni et al., 2017; Ertola Navajas et al., 2022; Feir, 2016). These effects may diminish over time as former conscripts age, or even reinforce due to a more conservative leaning among older people.

We test whether military service effects persist across the life course by exploiting individual-level variation in age at the time of survey response. Formally, M in model 2 is replaced by the standardized age of the individual.[16] A negative sign for λ indicates that

[16]Standardizing age allows for more meaningful interpretation of the interaction term λ: it reflects how the treatment effect varies with age in standard deviation units. This also makes λ more directly comparable to other heterogeneity coefficients using standardized moderators. The main effect β captures the treatment effect for an individual of average age (33 years).

military service effects decay over time, while a positive coefficient suggests strengthening effects with age. We show the results in Panel A of Table 5. All the outcomes display a positive and statistically significant interaction coefficient, suggesting that the effect does not fade away, but actually is long-lasting. This trend aligns with the formative years hypothesis: experiences during the crucial phase of identity development seem to instigate profound attitudinal shifts that reinforce rather than diminish over time, implying that military service in young adulthood has a lasting impact on preferences.

An alternative interpretation concerns the different environment characterizing the military service across time. Older respondents were drafted further back in the past, when the emphasis on national identity during conscription was arguably stronger compared to more recent years. In other words, positive λ may reflect a more nationalistic rhetoric of European military services further in the past.

6.2 Immigration Salience

Given the variation in immigration levels and their salience across countries, we aim to explore the effect of conscription between high- and low-immigration countries, as well as high- and low-immigration regions within the same country. Former conscripts may perceive a greater threat to national identity in countries or regions with higher levels of immigration due to the presence of immigrants with diverse cultures and backgrounds. Alternatively, a higher exposure to the immigrant population for conscripts could increase their positive attitudes because of the contact-theory hypothesis.

More specifically, we exploit the variation in immigration levels over time across countries by estimating equation 2 where M is replaced by the standardized share of immigrants (foreigners relative to total population) of country (or NUTS 1) c during ESS wave t. Focusing on variation in country-level immigration rates (Panel B of Table 5), we find that impacts are larger in countries with higher immigration rates. Specifically, for each standard deviation increase in the share of foreign residents, the effect of the ending conscription on attitudes toward migrants increases by 0.047 standard deviations. This is true for economic, cultural and perception attitudes. In Panel C, we examine

within-country variation in immigration finding similar results.[17]

6.3 Ethno-Linguistic Fractonalization

Military service compels individuals from different backgrounds to engage within a cohesive national framework. This collective experience characterized by a regular nationalistic rhetoric may contribute more to the strengthening of a unified national identity in ethno-linguistically fragmented countries, as compared to more homogeneous nations. On the other hand, forced coexistence between different ethnic/linguistic groups can also reinforce atavistic divisions.

To explore this dimension, we estimate equation 2, where we interact the linguistic fractionalization index with the treatment dummy. The results in Panel D of Table 5 highlight that the effect is larger in countries with a high level of linguistic fractionalization, suggesting that military service reinforces a broader, constructed national identity by bridging internal cleavages — an identity that, once solidified, may sharpen distinctions between nationals and perceived outsiders.

6.4 Government Ideology at Time of Service

Finally, we examine whether the ideological orientation of the government in office during military service shapes long-term attitudes. There are two alternative mechanisms which could operate simultaneously. On the one hand, nationalistic and patriotic themes may be particularly emphasized in military service under a right-wing government. If so, we expect a stronger negative effect on the attitude towards immigrants. On the other hand, under left-wing governments, the military environment serves as an *institutional counterweight* to the prevailing civilian discourse. Regardless of whether military rhetoric itself intensifies, the military environment may contrast sharply with left-wing governments likely promotion of multiculturalism and diversity.

We match individuals to the government in office during their year of military service

[17] Results are also similar when examining heterogeneity by the flow rather than stock of migrants, see Appendix Table A.6.

(or corresponding age for the non-conscripted individuals). We then standardize our 0-10 left-right variable (where 0 represents the most left-wing and 10 the most right-wing position) and assign it to all governments in our sample. This measure is then interacted with the treatment dummy while estimating equation 2. As shown in Panel E of Table 5, the estimated effect is stronger among individuals who served under more left-wing oriented governments. This is consistent with the idea that patriotic values are promoted by the military irrespective of the current government's position and that a contrast between the two strengthens the impact of military service on identity formation.

7 Conclusions

In this paper, we examine how compulsory military conscription, a powerful nation-building tool, impacts attitudes toward immigrants. We do this by leveraging the suspension of military service in 21 European countries and comparing cohorts of men who were drafted to those who were no longer required to serve. Employing a RD design, we find that former conscripts develop a stronger emotional attachment to their own country and have more negative attitudes toward immigrants relative to non-conscripts.

Our heterogeneity analyses reveal important insights about when and why military service shapes attitudes most strongly. They tend to persist—and even grow—over time, which supports the idea that early adulthood is a key period for shaping long-term views. The effects are stronger in areas with more immigrants, suggesting that perceived threats to national identity make military service more consequential for attitudes toward outsiders. We also find larger effects in linguistically diverse countries, where the military may play a stronger role in building a shared national identity. Finally, the political environment during service matters: those who served under left-leaning governments show stronger responses, in line with the larger distance between the rhetoric experienced during the military service and the one prevalent in the wider society under more progressive governments.

This paper contributes additional insights to the ongoing debate about reinstating

military service, particularly within Europe. These findings suggest that policymakers considering the reinstatement of military service should account for its potential effects on social cohesion and inter-group attitudes.

8 Figures and Tables

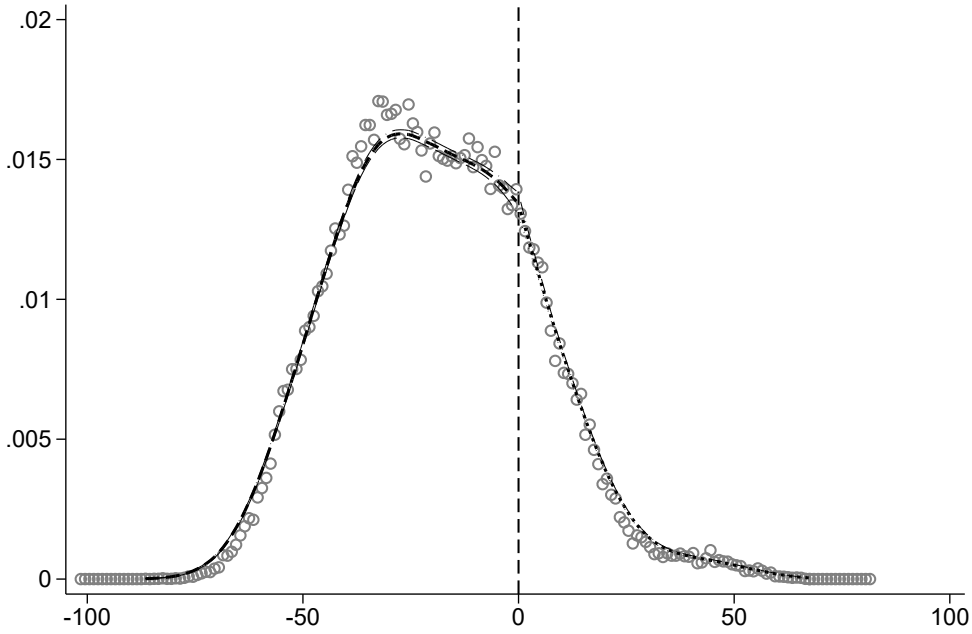

Figure 1: McCrary Density Test

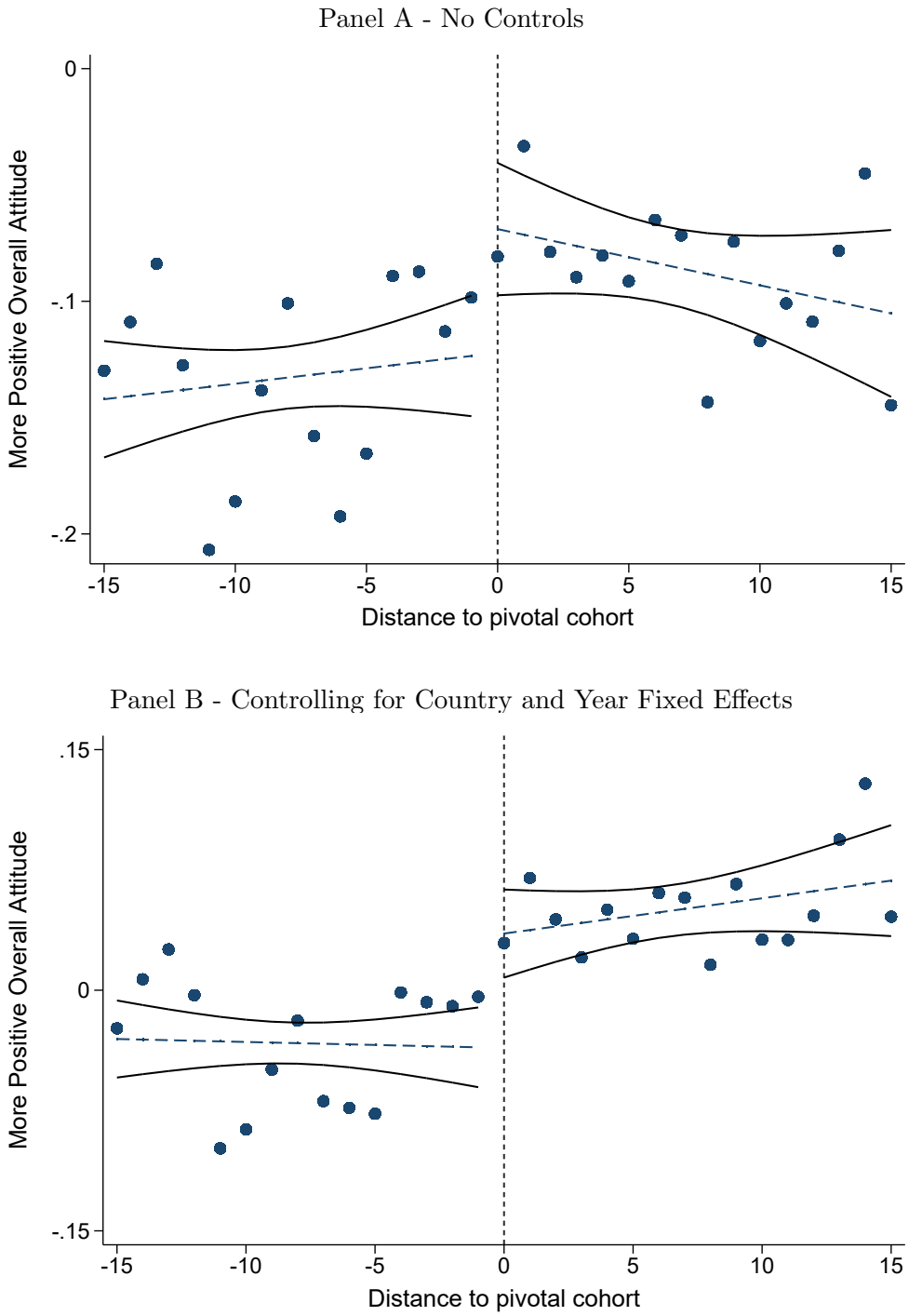

Figure 2: The Impact of Military Conscription on Attitudes Toward Immigrants

Notes: The graph limits the observations to individuals whose cohort falls within a 15-year range from the pivotal cohort. In Panel A we plot the raw mean of the overall attitude towards immigrants while in Panel B we plot residuals from a regression of the overall attitude towards immigrants on country and survey year fixed effects.

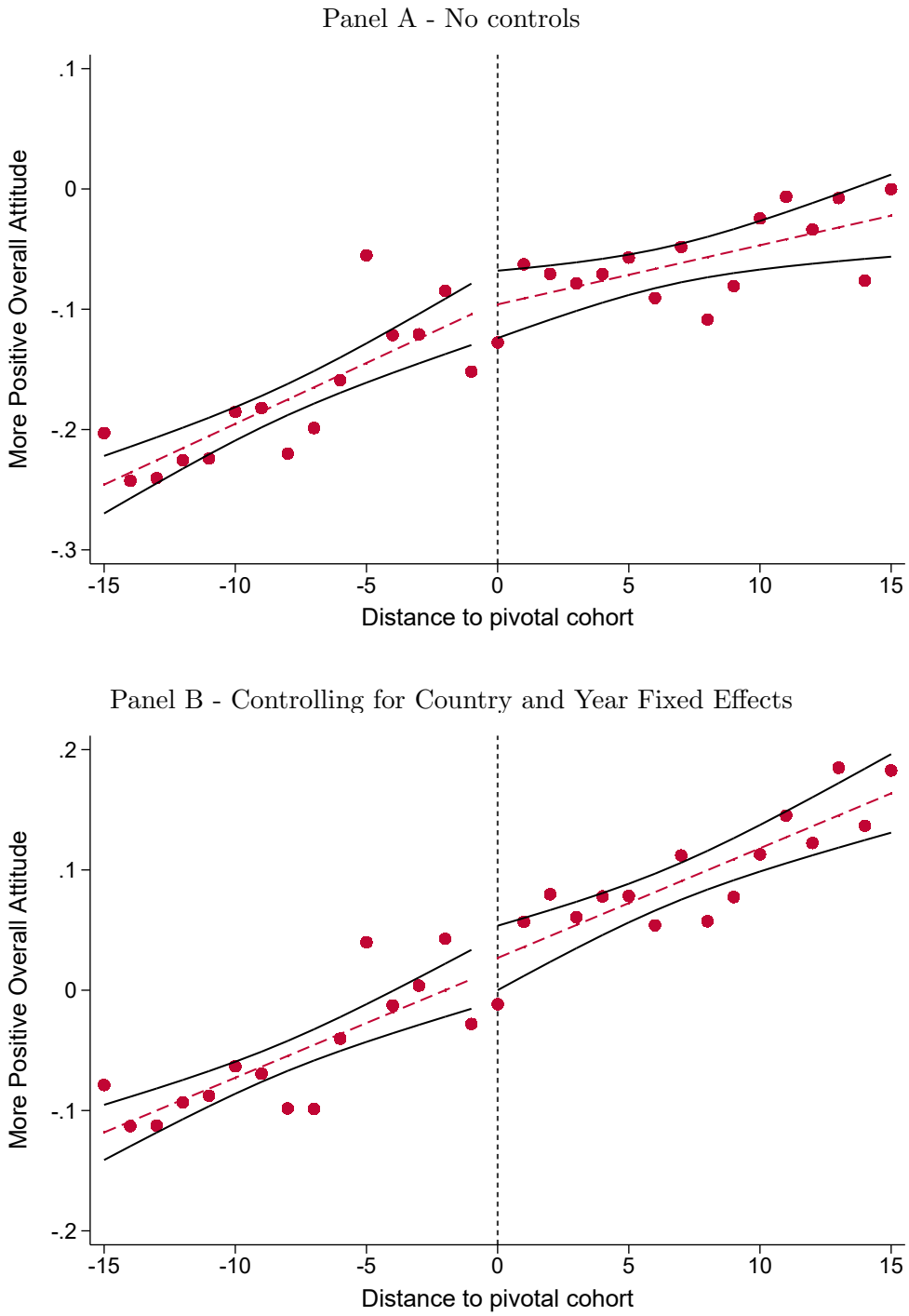

Figure 3: Placebo Test - Impact on Women

Notes: This figure limits the observations to women whose cohort falls within a 15-year range from the (male) pivotal cohort. In Panel A we plot the raw mean of the overall attitude towards immigrants while in Panel B we plot residuals from a regression of the overall attitude towards immigrants on country and survey year fixed effects.

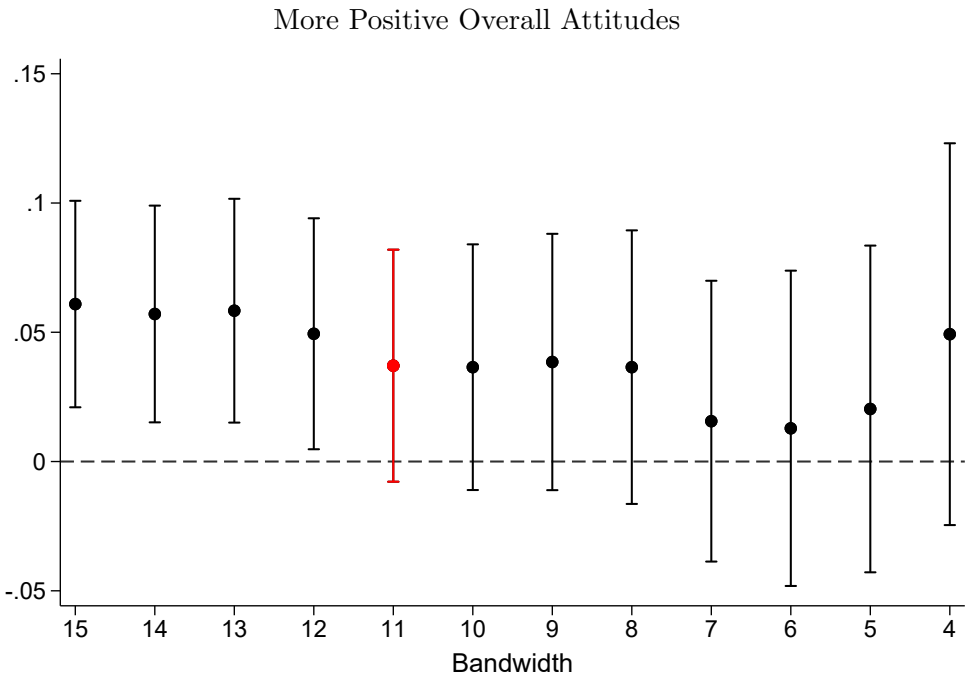

Figure 4: Robustness to Smaller Bandwidths

Notes: This graph shows the estimates from equation 1 for increasingly smaller bandwidths with 95 percent confidence intervals. Bandwidth refers to the range of data points around the cutoff used in the estimation. The red line indicates the optimal bandwidth, as calculated using the *rdbwselect* command from Calonico et al. (2014).

Table 1: Testing for Covariates Balance

Dep. Variable	Discontinuity	SE	p-value	N.
Age	0.017	0.020	0.413	36712
Father's Education	0.028	0.038	0.473	10498
Mother's Education	0.058	0.040	0.149	10693
Survey Year	0.040	0.043	0.352	36712
Bandwidth	15	15	15	15
Country FE	Yes	Yes	Yes	Yes

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We test covariate balance by estimating equation 1 using standardized individual covariates and the probability of ESS participation as dependent variables. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth, including country fixed effects. The sample is restricted to individuals whose birth cohort falls within 15 years of the pivotal cohort.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 2: The Impact of Military Conscription on Attitudes Toward Immigrants

	(1) More Positive Overall Attitudes	(2) More Positive Economic Attitudes	(3) More Positive Cultural Attitudes	(4) More Positive Perceptions
Panel A: No Controls				
$Non-Conscripted\ Cohort$	0.053^{**}	0.057^{**}	0.048^{**}	0.035
	(0.024)	(0.024)	(0.024)	(0.025)
Panel B: Controlling for Country and Year Fixed Effects				
$Non-Conscripted\ Cohort$	0.072^{***}	0.075^{***}	0.064^{***}	0.051^{**}
	(0.020)	(0.022)	(0.019)	(0.021)
Panel C: Controlling for Country, Year and Age Fixed Effects				
$Non-Conscripted\ Cohort$	0.061^{***}	0.064^{***}	0.057^{***}	0.037^{*}
	(0.020)	(0.022)	(0.020)	(0.022)
Observations	36712	36712	36712	36712
Bandwidth	15	15	15	15

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. Panel A presents the results relative to equation 1 without controls. Panel B shows the results of the same specification with the addition of country and survey year fixed effects. In Panel C we show results that account for country, survey year and age fixed effects.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 3: Does the Phase-out Period Matter?

	(1) More Positive Overall Attitudes	(2) More Positive Economic Attitudes	(3) More Positive Cultural Attitudes	(4) More Positive Perceptions
Panel A: Removing Year -1				
$Non-Conscripted\ Cohort$	0.069***	0.077***	0.060***	0.044*
	(0.022)	(0.023)	(0.021)	(0.024)
Panel B: Removing Year -2				
$Non-Conscripted\ Cohort$	0.083***	0.093***	0.071***	0.053**
	(0.024)	(0.026)	(0.022)	(0.027)
Panel C: Removing Year -3				
$Non-Conscripted\ Cohort$	0.101***	0.113***	0.088***	0.065**
	(0.027)	(0.029)	(0.026)	(0.029)
Panel D: Removing All Phase-out Years				
$Non-Conscripted\ Cohort$	0.079***	0.084***	0.067***	0.057**
	(0.024)	(0.025)	(0.025)	(0.026)
Panel E: Focusing on Countries with a Phase-out Period One Year or Less				
$Non-Conscripted\ Cohort$	0.081**	0.089***	0.075**	0.047
	(0.031)	(0.033)	(0.030)	(0.033)
Panel F: Focusing on Countries with a High Share of Conscripts Preceding the Reform				
$Non-Conscripted\ Cohort$	0.065*	0.046	0.075**	0.050
	(0.034)	(0.036)	(0.030)	(0.034)

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. All panels control for country, survey year, and age fixed effects and follow equation 1. In Panel A, we exclude cohorts with running variable equal to -1. In Panels B and C, we exclude cohorts between -2 and -1, and between -3 and 1, respectively. In Panel D, we exclude all country-specific cohorts that were expected to serve during the phase-out period. In Panel E, we focus on countries with short phase-out periods (one year or less), and in Panel F on countries with high shares of conscripts.

$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 4: Impact on Attachment to Own Country and Europe

	(1) Attachment to Country	(2) Attachment to Europe
Panel A: Continuous Outcome		
$Non-Conscripted\ Cohorts$	-0.043	-0.007
	(0.038)	(0.040)
Panel B: Dummy Variable for Strong Attachment		
$Non-Conscripted\ Cohorts$	-0.040**	-0.018
	(0.016)	(0.018)
Observations	14694	14694
Bandwidth	15	15

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. All panels have country, survey year and age fixed effects. In this table, we estimate equation 1 using as outcomes the country and Europe attachment of the respondent from the ESS. This question was asked only from wave number 8. In Panel A, we use the original (standardized) variable, while in Panel B, we transform the variable into a dummy due to its heavy right skew. Specifically, the variable equals one if the original value (ranging from 0 to 10) is above 7.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table 5: Heterogeneity

	(1) More Positive Overall Attitudes	(2) More Positive Economic Attitudes	(3) More Positive Cultural Attitudes	(4) More Positive Perceptions
Panel A: Age				
$Non-Conscripted\ Cohort$	0.079***	0.076***	0.077***	0.054**
	(0.022)	(0.023)	(0.021)	(0.023)
$T \times Age$	0.100***	0.061**	0.110***	0.090***
	(0.027)	(0.029)	(0.023)	(0.025)
Panel B: Country-level Immigration Rate				
$Non-Conscripted\ Cohort$	0.057***	0.062***	0.053***	0.034
	(0.020)	(0.022)	(0.020)	(0.022)
$T \times ImmRate$	0.047***	0.029**	0.047***	0.049***
	(0.015)	(0.013)	(0.014)	(0.016)
Panel C: Regional(NUTS1)-level Immigration Rate				
$Non-Conscripted\ Cohort$	0.053**	0.058***	0.052**	0.029
	(0.021)	(0.022)	(0.021)	(0.022)
$T \times ImmRate$	0.043***	0.030**	0.036***	0.049***
	(0.012)	(0.012)	(0.012)	(0.012)
Panel D: Desmet et al (2003) Linguistic Index (ELF)				
$Non-Conscripted\ Cohort$	0.059***	0.064***	0.055***	0.036*
	(0.020)	(0.022)	(0.019)	(0.022)
$T \times ELF$	0.031***	0.018	0.037***	0.025**
	(0.012)	(0.012)	(0.011)	(0.011)
Panel E: Serving under a Right-wing Government				
$Non-Conscripted\ Cohort$	0.057***	0.061***	0.052**	0.036
	(0.021)	(0.023)	(0.020)	(0.022)
$T \times Right-wing\ Government$	-0.029**	-0.030**	-0.022*	-0.023*
	(0.013)	(0.014)	(0.013)	(0.013)

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. All panels control for country (or NUTS 1 in Panel C), survey year, and age fixed effects and follow equation 2. All mediating factors are standardized. For time-varying mediating factors, we add the non-interacted mediating factors to equation 2. In Panels B and C, immigrants are defined as individuals residing in a country without citizenship, expressed as a share of total population. In Panel D, we use the second-level linguistic fractionalization index from Desmet et al., 2012. In Panel E, we use the continuous 0-10 left-right government ideological scale as a mediating factor.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

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A Appendix

Table A.1: Timeline military service suspension

Country	Year of reform	Pivotal Cohort
Albania	2010	1991
Belgium	1995	1976
Bulgaria	2008	1989
Croatia	2008	1990
Czech Republic	2005	1986
France	2002	1983
Germany	2012	1994
Hungary	2005	1986
Italy	2005	1986
Latvia	2006	1988
Luxembourg	1967	1948
North Macedonia	2006	1988
Netherlands	1997	1979
Poland	2009	1990
Portugal	2005	1986
Romania	2006	1988
Slovak Republic	2006	1987
Slovenia	2004	1985
Spain	2002	1983
Sweden	2011	1992
United Kingdom	1961	1942

Source: Bove et al., 2022

Table A.2: Summary Statistics on Immigrants

	% Foreign Residents		Population
	Mean (1)	Std.Dev (2)	Mean (3)
Albania	0.30	0.21	2,922,605
Belgium	10.41	1.72	10,968,024
Bulgaria	0.89	0.60	7,305,043
Croatia	0.49	0.36	4,164,123
Czech Republic	4.50	2.15	10,472,077
France	9.08	1.43	45,497,945
Germany	10.05	1.92	82,120,963
Hungary	1.65	0.34	9,909,941
Italy	6.34	2.24	59,046,261
Latvia	13.63	3.72	2,081,553
Luxembourg	43.65	3.58	535,170
North Macedonia	6.27	0.06	1,940,551
Netherlands	5.18	1.17	16,770,063
Poland	0.41	0.38	37,921,147
Portugal	4.32	1.83	10,467,201
Romania	0.44	0.27	20,115,713
Slovak Republic	1.01	0.37	5,416,111
Slovenia	4.58	2.12	2,047,280
Spain	9.61	2.38	45,497,936
Sweden	7.07	1.40	9,654,502
United Kingdom	7.32	1.69	63,582,591

Source: Countries' Statistical Office and Eurostat

Table A.3: Placebo Test - Impacts on Women

	(1) More Positive Overall Attitudes	(2) More Positive Economic Attitudes	(3) More Positive Cultural Attitudes	(4) More Positive Perceptions
Panel A: No Controls				
$Non-Conscripted\ Cohort$	-0.002	0.001	-0.003	-0.002
	(0.029)	(0.028)	(0.029)	(0.027)
Panel B: Including Country and Year Fixed Effects				
$Non-Conscripted\ Cohort$	0.010	0.009	0.009	0.008
	(0.023)	(0.024)	(0.023)	(0.020)
Panel C: Including Country, Year and Age Fixed Effects				
$Non-Conscripted\ Cohort$	-0.006	-0.006	-0.002	-0.007
	(0.022)	(0.024)	(0.022)	(0.020)
Observations	37097	37097	37097	37097
Bandwidth	15	15	15	15

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth for the woman sample. The dependent variables are standardized to have mean 0 and standard deviation 1. Panel A presents the results relative to equation 1 without controls. Panel B shows the results of the same specification with the addition of country and survey year fixed effects. In Panel C we show results that account country, survey year and age fixed effects.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.4: Robustness Tests

	(1) More Positive Overall Attitude	(2) More Positive Economic Attitude	(3) More Positive Cultural Attitude	(4) More Positive Perception Attitude
Panel A: Triangular kernel				
$Non-Conscripted\ Cohort$	0.044^{**}	0.042^{*}	0.047^{**}	0.026
	(0.022)	(0.024)	(0.021)	(0.024)
Panel B: Controlling for Parliament Trust				
$Non-Conscripted\ Cohort$	0.045^{**}	0.047^{**}	0.046^{**}	0.024
	(0.032)	(0.035)	(0.028)	(0.033)
Panel C: Controlling for People Trust				
$Non-Conscripted\ Cohort$	0.056^{***}	0.060^{***}	0.053^{***}	0.033
	(0.032)	(0.035)	(0.028)	(0.033)
Panel D: Military individuals included				
$Non-Conscripted\ Cohort$	0.061^{***}	0.063^{***}	0.058^{***}	0.039^{*}
	(0.020)	(0.022)	(0.020)	(0.022)
Panel E: University graduated included				
$Non-Conscripted\ Cohort$	0.037^{**}	0.045^{**}	0.028	0.025
	(0.018)	(0.019)	(0.017)	(0.020)

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel (with the exception of Panel A) with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. All panels control for country, survey year, and age fixed effects and follow equation 1. In Panel A, we make use of the triangular kernel. In Panels B and C, we control for individual trust in parliament and trust in people. In Panel D, we include military professionals. In Panel E, we also include individuals with university degrees.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.5: Placebo Test on Countries Still With Conscription

	(1) More Positive Overall Attitudes	(2) More Positive Economic Attitudes	(3) More Positive Cultural Attitudes	(4) More Positive Perceptions
$Non-Conscripted\ Cohort$	0.002	-0.008	-0.003	0.019
	(0.036)	(0.038)	(0.034)	(0.034)
Bandwidth	15	15	15	15
Observations	11930	11930	11930	11930

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. All panels control for country, survey year, and age fixed effects and follow equation 1. In this table, we construct a sample of countries with conscription (Greece, Austria, Switzerland, Norway, Finland, Denmark, and Estonia). We establish the first cohort without conscription as the average of the pivotal cohorts from neighboring countries that have suspended the policy.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

Table A.6: Heterogeneous Effects - Immigration Flows

	More Positive AVG Attitudes	More Positive Economic Attitudes	More Positive Cultural Attitudes	More Positive Perceptions
$Non-Conscripted\ Cohort$	0.053^{***}	0.057^{***}	0.050^{**}	0.031
	(0.020)	(0.022)	(0.020)	(0.022)
T × ImmFlow	0.029^{**}	0.011	0.033^{***}	0.031^{**}
	(0.012)	(0.013)	(0.011)	(0.012)
Bandwidth	15	15	15	15
Observations	36712	36712	36712	36712

Notes: Standard errors (in parentheses) are clustered at the country-survey year level. We employ a uniform kernel with a first-order polynomial and a 15-cohort bandwidth. The dependent variables are standardized to have mean 0 and standard deviation 1. This specification controls for country survey year, and age fixed effects and follow equation 2. All mediating factors are standardized. For time-varying mediating factors, we add the non-interacted mediating factors to equation 2. Immigrants are defined as the flow of incoming foreigners relative to the total population.
$^{***}p < 0.01$, $^{**}p < 0.05$, $^{*}p < 0.1$

A.1 Share of conscripts

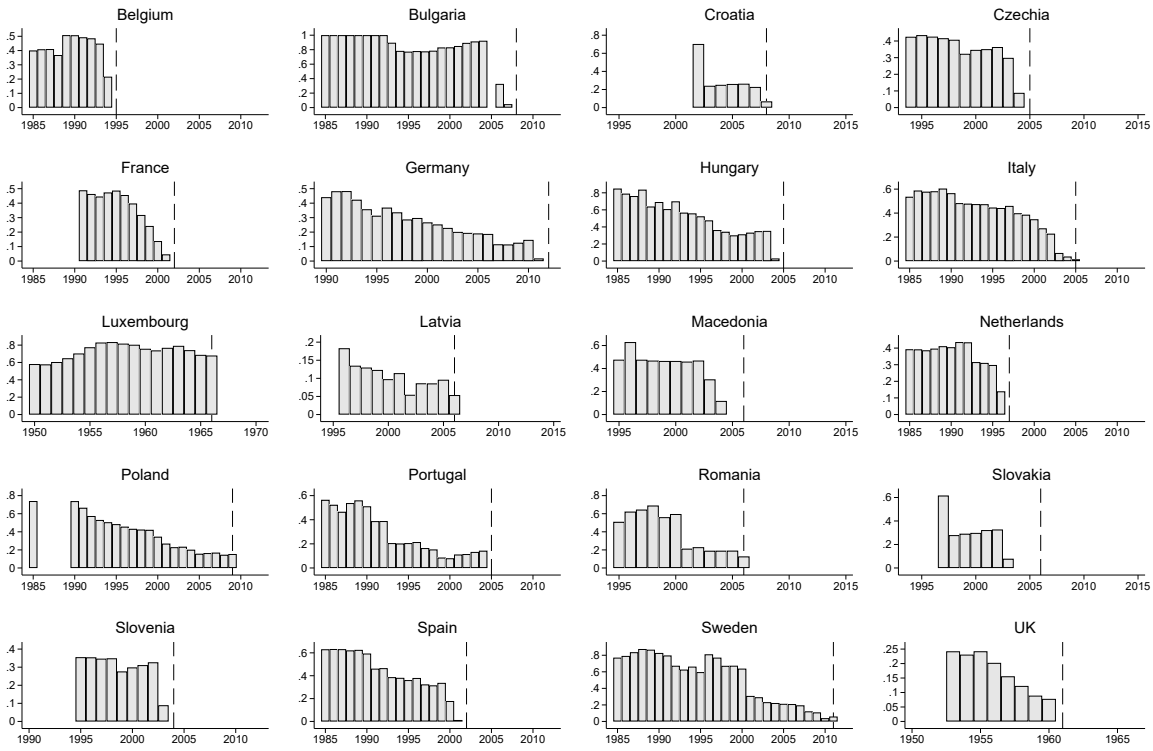

Figure A.1: Conscripts relative to baseline

Notes: The graph show the mean share of conscripts for each country with data from Military Balance and EU-LFS provided by Bove et al., 2022.

A.2 Country deletion

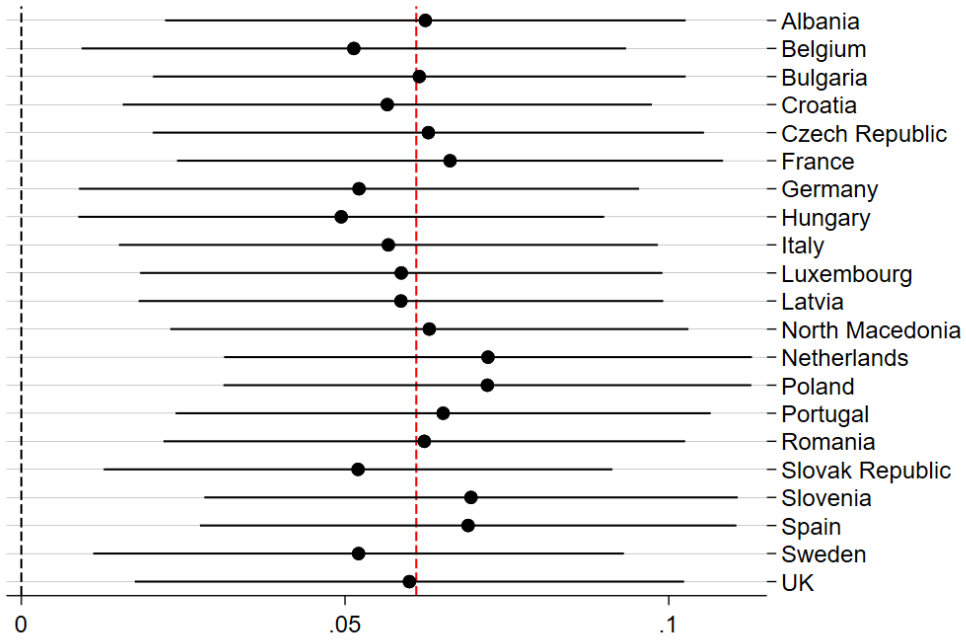

Figure A.2: Leave-out one country test

Notes: The graphs show one-country deletion test. We run equation 1 15 times by excluding one country at a time. The vertical red line indicates the main estimate from Table 2. The country dropped is shown on the right of each panel. Estimates include age, country, survey and age fixed effects.

A.3 Placebo reform dates

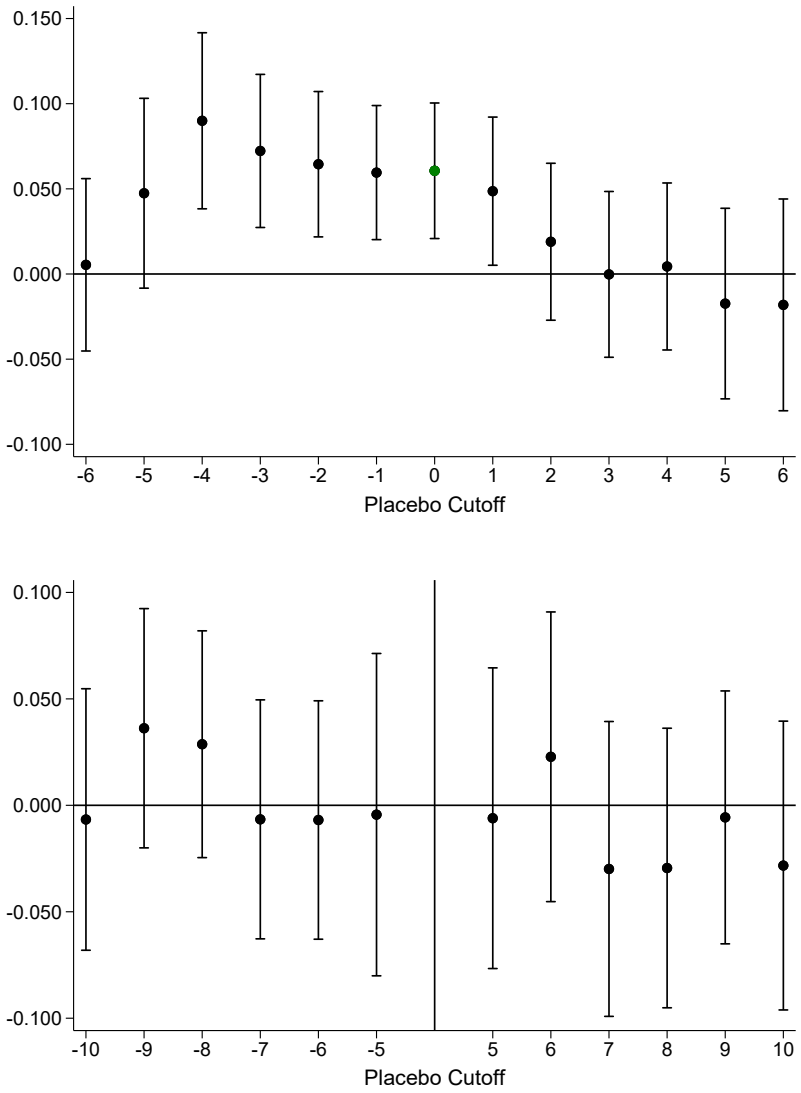

Figure A.3: Placebo Cutoffs

Notes: Figure (a) shows the coefficients and 95% confidence intervals of a placebo test setting the cutoffs from six years before to six year after the actual reform. Figure (b) replicates a similar test on the sub-samples of conscripted and non conscripted individuals separately, setting the cutoffs at 10 to 5 years from the actual reform on both sides.

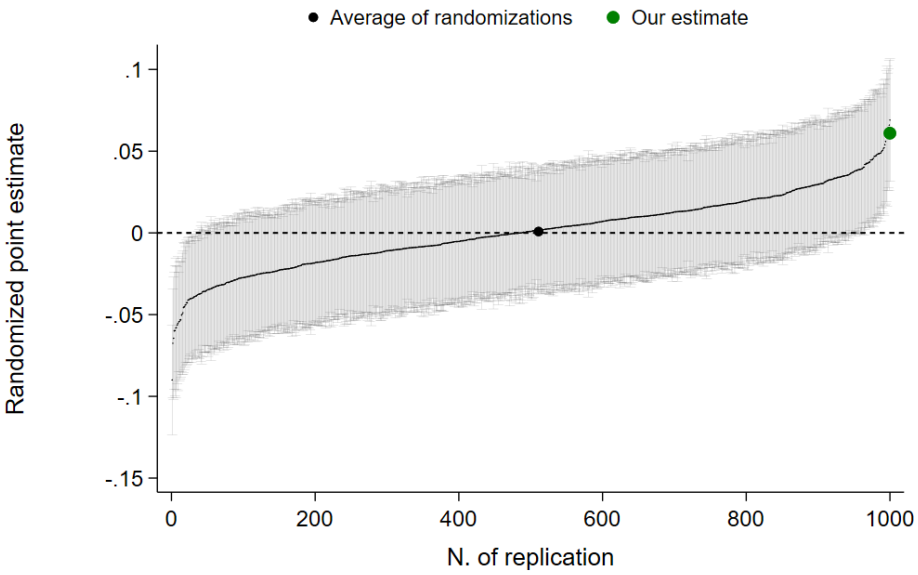

Figure A.4: Randomization country's reform date

Notes: The graph show (in increasing order of magnitude) the point estimate and the 95% confidence interval obtained randomizing the date of each country's reform 1000 times. For reference we report in green our baseline estimate in Column 1 of Panel C in Table 2 (green circle).