

Military Culture and Institutional Trust: Evidence from Conscription Reforms in Europe

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Abstract: *Does military conscription reduce the distance between the ordinary citizen and the state? Decades after its abolition, numerous European policy makers from across the political spectrum advocate the reintroduction of conscription to foster civic virtues, despite a lack of empirical evidence in this respect. Leveraging quasi-random variation in conscription reforms across 15 European countries, we find that cohorts of men drafted just before its abolition display significantly and substantially lower institutional trust than cohorts of men who were just exempted. At the same time, ending conscription had no effect on institutional trust among women from comparable cohorts. Results are neither driven by more favorable attitudes toward the government, nor by educational choices. Instead, this civil–military gap unfolds through the formation of a homogeneous community with uniform values. We argue that reintroducing a compulsory military service may not produce the effects anticipated by its advocates.*

Verification Materials: The data and materials required to verify the computational reproducibility of the results, procedures, and analyzes in this article are available on the *American Journal of Political Science* Dataverse within the Harvard Dataverse Network, at: <https://doi.org/10.7910/DVN/WI7WN0>.

As the collapse of the Soviet Union and the emergence of international terrorism deterritorialized the military threat from the continent, a majority of European countries replaced obsolete conscripted armies with highly technological, all-volunteer forces. Yet, as the debate around the merits of such policy change seemed settled, an increasing number of policy makers from across the political spectrum begun to advocate a U-turn on military labor policies. Discontinuing universal military conscription, they claim, has contributed to widening the distance between the ordinary citizen and the state. Several French commentators and politicians have, for example, lamented the loss of the integrative and didactic function of the *service national*. “Histori-

cally, military conscription was a mean for the state to pass on the Nation’s values” claimed French sociologist Sébastien Jakubowski, so that “[...] to end the military service was to tell young citizens that the Nation doesn’t need them.”¹ Senior figures in the French socialist party explicitly included the return of universal conscription in their platform for the 2017 party primaries.² In the debate that followed, Emmanuel Macron vowed to reinstitute a “service national universel” during the 2017 presidential campaign, with the specific aim to promote a sense of civic duty and national unity (Zaret-sky 2018). Macron’s legislative proposal became law in 2018. In Germany, both mainstream and populist parties have set off debates on reintroducing conscription to

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²Available at: <https://bit.ly/3pUIE9A> and <https://bit.ly/31X1B38>.

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tackle the progressive deterioration of social cohesion and national values. The former commissioner for the armed forces—and SPD member—Eva Högl spurred a heated debate by affirming that “ending conscription was a big mistake,” in alignment with members of the CDU, as well as with the majority of members of the main German populist party (AFD).³ Similarly, a U.K. government-commissioned report by military historian Sir Hew Strachan in 2020 concludes that returning to a compulsory national service would address a lack of “mature public engagement.”⁴ The belief that reinstating military conscription would inculcate positive attitudes toward national institutions in millions of young citizens can be traced in many countries that discontinued military conscription.⁵

At first sight, the enthusiasm of those politicians and experts leading the current debate on labor military policies seems to be grounded. Taking place at a very sensitive time of one’s life, and isolating young citizens from society for several months, military conscription does have the potential to shape attitudes toward the state. However, to date, there exists neither theoretical consensus nor empirical validation to ascribe current patterns of decreased institutional trust to the abolition of universal military conscription. This article investigates whether ending military service shaped individuals’ confidence in the political institutions that conscripts are instructed to serve.

Scholarly work provides only some support to the hypothesis that military conscription would reduce the widening gap between the citizen and the state. Its advocates argue that conscription would “reinstill the sense of shared national mission and community that is at present allegedly absent” (Krebs 2004, 89), leading young individuals to develop an appreciation of the “civic whole” (Moskos et al. 1988). By introducing conscripts to other segments of the population and transmitting them a state civic identity (see, e.g., Levi 1997), conscription is expected to cement loyalties to the polity (de Juan, Haass, and Pierskalla 2021; George and Teigen 2008; Leander 2004). If subscribing to this argument, we should expect ending military conscription to have decreased institutional trust. Yet, a substantial number of scholars in the field of civic–military relationships are rather skeptical of the romantic idea that the military can serve as a “school

for the nation” (Krebs 2004). Instead of aligning the citizen with state institutions, conscription may produce the opposite effect, as soldiers “define their identities and loyalties with reference to the military as a distinct institution” (Kadercan 2013, 126), prioritizing parochial interests over broad, national ones (Rhodes 1995; Rosen 1995). If this is the case, we should expect that ending military conscription has increased, rather than decreased, institutional trust.

Against this background, this article explores the effect of discontinuing military conscription on institutional trust in European countries. This is an open, nontrivial, policy-relevant question, which has implications for the ongoing debate over whether reintroducing military conscription might help fighting the decades-long erosion in trust toward representative institutions. We address this question using a three-step empirical design that seeks to safeguard at the same time internal validity, threatened by unobserved heterogeneities in military recruitment, and external validity, which would be limited if focusing on a specific conscription system. We first assemble a dataset on conscription policies across 15 European countries, and identify the “pivotal” cohort for each of them, that is, the first cohort of citizens affected by the reform ending conscription, based on their year of birth. Although ending conscription is a political choice taken at the country level, and so endogenous to an evolving political context, policy variations are *as good as random* at the individual level. We then leverage individual-level data on attitudes toward institutions from the European Social Survey (ESS). Using a regression discontinuity (RD) design, we compare the attitudes of individuals born just before the pivotal cohort—the “conscripts,” our control group—against those of individuals born just after the pivotal cohort—the “civilians,” our treatment group. Given the presence of male-only conscription, we also look at the effect of the conscription reforms on men vis-à-vis their effect on women within the same cohort, expecting to detect no impact on the institutional trust of the latter group. Finally, to account for country-specific unobservable characteristics and for time-varying dynamics of institutional trust, we include country and ESS-wave fixed effects.

A key result emerges: Cohorts of men who reached the drafting age just before the abolition of conscription exhibit lower trust than comparable cohorts of men who were just exempted, whereas no effect is found among women within the same cohorts. In particular, ending military conscription significantly increases trust in the country’s legal system, parliament, political parties, and politicians by, respectively, 5.1%, 6.47%, 6.25%, and 4.15%, relative to the unconditional mean. Treatment effects are stronger in postsocialist countries, where the

³Available at: <https://bit.ly/3msNZBP>, <https://bit.ly/3fHksEt>, <https://bit.ly/3ykgJUD> and <https://bit.ly/3GvDGXM>.

⁴Available at: <https://bit.ly/2HZckjY>.

⁵See, for example, <https://bit.ly/3nOqqnG>, <https://bit.ly/2TN2AMd>, and <https://bit.ly/361dFjM>. Note that this debate is not unique to Europe. For example, Gulf states are increasingly relying on conscription to enhance national identities (Ardemagni 2018).

level of democratization was lower and conscription was abolished more recently, compared to Western European states. Our findings, which are robust to a wide array of robustness checks, hence show that ending conscription *did* affect institutional trust, but not in the direction lamented by those advocating its reintroduction.

We explore three potential mechanisms through which *not* being conscripted may increase institutional trust later in life. Theoretical arguments that conceive conscription as an institution with the potential to influence recruits' long-run attitudinal patterns emphasize the role of military culture in creating a cohesive, homogeneous community with uniform values and attitudes, coalescing young men around the primacy of the military over mistrusted democratic institutions (see, e.g., de Juan, Haass, and Pierskalla 2021; Krebs 2004). Following these arguments, we should expect attitudes toward institutions to be more homogeneous among conscripts, and more polarized among nonconscripts. We find robust evidence supporting this claim. Ending conscription may also lower the opportunity cost of skill development, so higher trust among nonconscripts may also reflect endogenous changes in schooling choices (see, e.g., Di Pietro 2013). Our findings, however, suggest that this socioeconomic mechanism is unlikely to play. Finally, we show that results are neither channeled by context-specific evaluations of the executive in charge at the time of the interview, nor by the political leaning of the government discontinuing conscription in the first place. In sum, ending conscription likely increased institutional trust via the transmission of military culture, rather than by shaping educational choices or government popularity.

Our contribution bridges the literature on military conscription with that on the long-run determinants of institutional trust. Existing empirical papers on conscription and civil–military relations rely on specific drafting systems (e.g., the Argentinian lottery, the Vietnam draft) to study the “civil–military” gap in partisan orientations, political participation and authoritarianism, resulting in strikingly different conclusions (Erikson and Stoker 2011; Green, Davenport, and Hanson 2019; Horowitz, Simpson, and Stam 2011; Navajas et al. 2022). We focus instead on attitudes toward those national institutions that the military is instructed to serve, and build our inference on 15 different countries. Although proponents of conscription in the absence of territorial threats motivate it as a tool to foster civic sense, we show that its reintroduction would most likely yield the opposite effect, contributing to the erosion of trust toward institutions. This finding adds to the ever-expanding literature on the long-run determinants of trust

(Anderson and Paskeviciute 2006; Dinesen, Schaefer, and Sønderskov 2020; Keele 2007).

Conscription and Trust

In the absence of territorial threats, the rationale behind military conscription is to incorporate the ordinary citizen—specifically, the ordinary young male—in the high-politics domain of state coercion. Conscription represents a cornerstone in the political socialization of men: at a critical stage of life, they are immersed in an extensive, long-lasting program, in which their “civilian status is broken down and the new identity of military recruit is forged” (Jackson et al. 2012, 271).

The military experience affects the recruits' personality traits, beliefs, and behavior (Horowitz and Starn 2014; Jackson et al. 2012; Navajas et al. 2022), and can shape the attitudes and voting patterns of politicians, particularly if they served in higher military ranks (Stadelmann, Portmann, and Eichenberger 2018). It can also generate a “civil–military gap,” that is, a distance in the sociopolitical beliefs and values held by military personnel relative to civilians (see Brooks 2019, for an extensive overview). In the short run, the “civil–military gap” can affect the public scrutiny over—and accountability of—the armed forces (Fordham 2001). In the long run, it can redefine the relationship between the military and the state more broadly (see, e.g., Feaver and Gelpi 2005).

How does military conscription—and its abolition—affect the “*institutional* civil–military gap”? In this section, we develop two competing perspectives.

Promoting Civic Virtues

One strand of the literature in civil–military relationship holds that, by “bonding citizens [...] and providing civic skills” (George and Teigen 2008, 342), and thus reinvigorating “the civic-mindedness that they believe characterized earlier generations” (Krebs 2004, 89), conscription forms “loyal and virtuous citizens” (Leander 2004, 576). Scholars in this tradition believe conscription to have served a democratizing function, helping to integrate the military into newly formed nation-states (Dier 2010), and welcome the bond of loyalty between armed forces and democratic institutions enshrined in a number of modern constitutions.

The expectation that conscription promotes transferable civic virtues builds on an optimistic assessment of two cornerstones of the military experience: the

top-down transmission of national values from the elite to the draftees, and the bottom-up convergence emerging from the interaction among draftees of different socioeconomic background. The first mechanism envisages conscription as a “a great national school in which the officer would be an educator in the grand style, a shaper of the people’s mind” (Krebs 2004, 92). As a “total institution,” conscription isolates young individuals with highly unstable political opinions from society at large, disciplining their behavior by enforcing military norms via both formal ordinance and informal praxes. Due to the increased awareness of one’s bonds with the country, and to the ultimate sacrifice military service may demand of the conscript (Grossman, Manekin, and Miodownik 2015), military training inculcates in the youth values of loyalty, patriotism, and respect for the law (Huntington 1981).

The second mechanism is expected to shape draftees’ attitudes via intergroup contact. Drawing recruits from virtually all social groups, conscription instills a collective sense of duty from which no one is exempted (Choulis, Bakaki, and Böhmelt 2021; Poutvaara and Wagener 2011b). Prominent proponents of military conscription like Janowitz (1983) regard the military as generating intense interactions between individuals from varied backgrounds during their “impressionable” years, often to execute cooperative tasks. By forging closer ties across diverse subpopulations, and exposing conscripts to the national community, military service demands that “members of a polity be loyal to the community and to its values, rather than to the traditional values of family or clan” (Leander 2004, 576). Conscription forces individuals to reconsider their identity, their personal attachments and their definition of the political community, and in doing so, it develops a civic identity and loyalty toward the institutions.

Military training and the political socialization thereof are believed to boost civic obligations toward the state and its institutions, and thus abolishing conscription could lead to a deterioration in civic “mindedness.” In line with this argument, we seek to test the following hypothesis:

H1a: Ending military conscription *decreased* long-run institutional trust.

Opposing Checks and Balances

Whereas one of the most conventional and popular justifications for the reintroduction of compulsory military service is derived from its role in forming polities and

producing virtuous citizens, many contemporary scholars take issue with such “myth,” emphasizing the lack of consistent evidence (see e.g., Krebs 2004; Leander 2004).

Particularly relevant for our study, although the military’s first task is to protect the citizen, its coercive power makes it a potential threat for democracy. This leads to a civil–military relations paradox: “[...] a variant of the basic problem of governance that lies at the core of political science: making the government strong enough to protect the citizens but not so strong as to become tyrannical” (Feaver 1999, 214). The introduction of a mandatory military service can be used as a powerful instrument for mass indoctrination, with the specific aim of bolstering regime resilience (de Juan, Haass, and Pierskalla 2021). As Poutvaara and Wagener (2011b, 165) point out, “not only were conscript forces used by totalitarian regimes (Nazi-Germany, the Soviet Union, or Fascist Italy) without noticeable resistance from within the army, but also democratic countries [...] used conscription at the time of their military coups.” The threat of a tyrannical military has been traditionally minimized by clustering soldiers away from competing societal organizations (Rosen 1995). On the one hand, this reduces the risk of subversive alliances between the military and part of society. On the other, isolating soldiers fosters their loyalty toward and identification with the armed forces, at the expenses of their allegiance to those institutions they are instructed to protect (Rosen 1995).

Whereas the extent to which military education inculcates *civic* virtues is disputable, there is little doubt about it instilling traditional *military* virtues (Finer 2002; Leander 2004). The development of a sectarian military culture is not solely the unintended consequence of geographic segregation during the conscription period, but is deliberately strengthened by the military organization itself, through both behavioral practices and rational arguments. The extensive training and social ritual recruits must endure shape their civic identity along military norms (Finer 2002; Varin 2014). In the words of Kier (1995, 69), “[f]ew organizations devote as many resources to the assimilation of their members. The emphasis on ceremony and tradition, and the development of a common language and *esprit de corps*, testify to the strength of the military’s organizational culture.” Obviously, young men join the military with heterogeneous *ex ante* beliefs. In some cases, those beliefs could be strong enough, despite the young age, to “resist” the military culture, but the emphasis on comradeship around military norms and social cohesion are likely to lead to beliefs’ homogeneity. This powerful assimilation process coalesces soldiers around a set of focal norms placing armed forces above political

institutions (Kier 1995). It does not come as a surprise that, although the military represents a highly trusted institution in most democracies (Choulis, Bakaki, and Böhmelt 2021), its personnel often exhibits disparaging attitudes toward civil society (Feaver and Kohn 2001; Stadelmann, Portmann, and Eichenberger 2018).

Conscripts are exposed not only to the identity of the military, but also to its organizational interests (Kadercan 2013). Among these there are the preservation of domestic political power, as well as the acquisition of additional institutional autonomy and influence over a range of issues, from the organization of national security to the appointment of key institutional figures (Bove, Rivera, and Ruffa 2020; Huntington 1981). As political actors subordinate the military to their democratic authority, the latter often claims institutional prerogatives that can lead to disputing such authority, particularly within specific policy domains that bear on the military itself (Brooks 2019). Being the unique responsible of the “management of violence,” the military represents an exceptional body within democratic societies, whose prerogatives clearly differentiate it from other institutional actors. In sum, the military experience may foster distrust around institutions of civilian oversight, as they represent an undesirable constraint to its organizational scope of action. Altogether, these arguments lead to the formulation of a second, competing hypothesis:

H1b: Ending military conscription *increased* long-run institutional trust.

Empirical Analysis Identification Strategy

For our analysis, we leverage information on 15 reforms that discontinued compulsory military service in Europe throughout the second half of the twentieth and twenty-first century. We then use individual-level data to compare institutional trust in cohorts of men just young enough to avoid the compulsory military service to institutional trust in cohorts of men born not quite late enough to be affected by the reform.⁶

Denote by $T_{x,c}$ the treatment variable for individuals born in cohort x and country c , taking value 1 if individual i was affected by the reform ending conscription, else 0. Also, denote by $r_{x,c}$ the “running variable,” given by the absolute distance in years from

⁶Such approach has been widely used in the domain of compulsory schooling reforms (e.g., Cavaille and Marshall 2019; Marshall 2016).

the “pivotal” cohort. We then use an RD design to test:

$$y_{i,x,c,w} = \alpha + \beta T_{x,c} + f(r_{x,c}) + \theta_c + \mu_w + \epsilon_{i,x,c,w}, \quad (1)$$

where $y_{i,x,c,w}$ is the institutional trust of individual i born in cohort x and country c , interviewed during the w th ESS wave. Our main coefficient of interest, β , captures the local causal effect of the treatment on institutional trust. In the main specification, β is estimated using a local linear regression, a first-order local polynomial to construct the bias correction, a triangular kernel function to construct the local-polynomial estimator, and the standard mean squared error optimal data-driven bandwidth selector (Calonico et al. 2017). As trust may exhibit idiosyncratic patterns over space and time, we apply both country (θ_c) and ESS wave (μ_w) fixed effects. Because in each of the sampled countries only men were conscripted, we test our main hypotheses through Equation (1) among self-reported men, and compare our findings with those obtained using the subsample of self-reported women.

We expect any discontinuity around the end of conscription to be more discernible among men. Note that although this comparison strengthens the reliability of our inference, we cannot assume that women’s time-varying institutional trust could be taken as a missing outcome, and thus that, in the absence of conscription, men in the treatment group would have followed the same trend in institutional attitudes as women. Several studies show that, alike ideological attitudes (Erzeel and Celis 2016), trust toward society in general and institutions in particular (see, e.g., Ulbig 2007) are affected by gender, for example, through the gender gap in political representation. To account for this, our RD strategy allows time bandwidths to be chosen separately for the two subgroups. As such, we do not make any assumption about the potentially gendered time trends in institutional trust (see Dinas and Stoker 2014, for an extended discussion).

Dependent Variable: Institutional Trust at the Individual Level Data

Information about individual-level trust toward the country’s institutions is taken from the nine rounds of the ESS, running every two years between 2002 and 2018 and covering 33 European countries. We use the following question:

Do you trust the legal system (parliament/parties/politicians)?

0: Not at all ; ... ; 10: Completely.

The chosen items are arguably the most frequently used markers of institutional trust (see e.g., Citrin and Stoker 2018). Our main dependent variable, labeled “institutional trust,” is based on the principal component analysis (hereafter, PCA) of the survey items presented above (see, e.g., West 2017, for a similar approach). Institutional trust is the first component of the PCA (normalized between 0 and 1 to facilitate the interpretation of results), and increases in each single item. Because answers to the four questions are highly correlated with each other,⁷ the first component of the PCA yields similar, positive weights for each trust item, and captures a high fraction of the overall variance.⁸ Our proposed metric thus suitably captures institutional trust, while allowing us to keep the presentation of findings concise. We provide results separately for each single trust item in the Supporting Information (SI) Section C.4 (p. 11).

We remove five subgroups of respondents from the pool of ESS observations, as their inclusion might be problematic for our estimation strategy. Specifically, we drop (i) individuals who have opted for a professional military career, due to self-selection issues; (ii) individuals who might not have been conscripted, due to their citizenship status; (iii) individuals born in Sweden after 1999, hence affected by the 2017 reactivation of conscription; (iv) individuals from Croatia and Luxembourg, due to limited data;⁹ (v) individuals with a university degree, due to the possibility to postpone conscription in the majority of our case studies.¹⁰ This leaves us with 161,623 respondents from 15 European countries, which have abolished conscription between 1961 and 2012. Importantly, none of these sampling

⁷Not surprisingly, the correlation between trust in parties and politicians is the highest one (87.1%). Trust in parliament is also highly correlated with both trust in parties (70.0%) and politicians (72.4%). Trust in the legal system is less collinear with the other proxies, but even in this case correlations remain quite high: 62.5% with trust in parliament, 56.4% with trust in parties, 58.1% with trust in politicians.

⁸Specifically, the weights are 0.5022, 0.5183, 0.5267, and 0.4491 for, respectively, trust in parliament, parties, politicians, and the legal system, capturing 76.43% of the overall variation.

⁹These countries have only been sampled twice by the ESS.

¹⁰Pursuing tertiary education allowed conscripts to delay the start of military service in many countries, hence shifting the formal eligibility criterion away (Stolwijk 2005). Therefore, those with tertiary education who reached the eligible age before the approval of the legislation that ended conscription may be incorrectly included in the control group. In our main estimation, we exclude individuals with a university degree from the analysis. Although their inclusion lowers the magnitude of our main estimates (see SI Section C.2, p. 9), this sampling choice helps closing the gap between eligibility and treatment status, thereby minimizing attribution issues, at the expense of representativeness.

choices would change our results. In the Codebook, which is part of the replication material, we provide a detailed description of our subsampling strategies, present the summary statistics for our data, and discuss our choices on conscription reforms when confronted with discrepancies between the different sources employed.

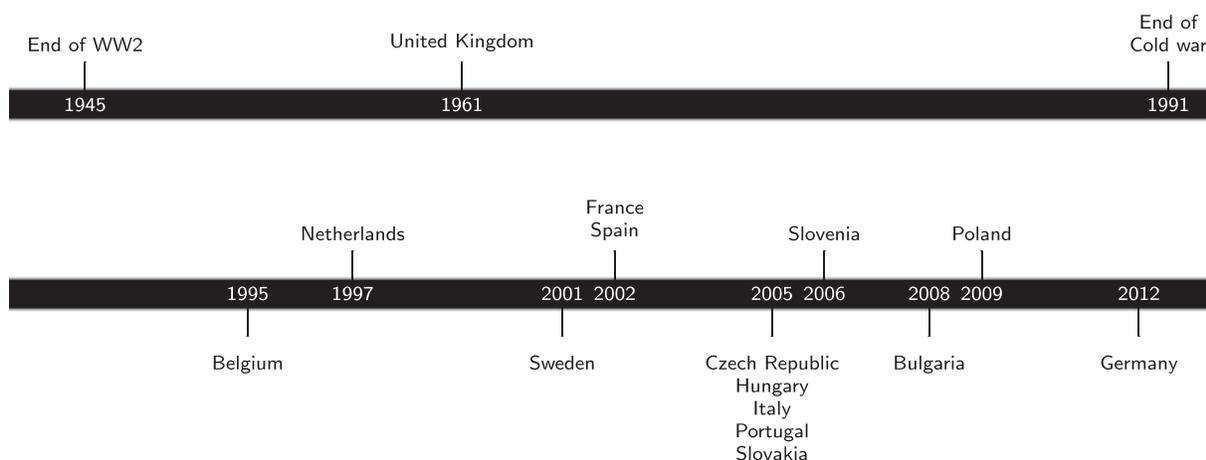
Independent Variable: Conscription Reforms at the Country Level

The popularity of universal, compulsory military service experienced historical highs and lows. Although active in Mesopotamia as far back as 1750 B.C., the modern revival of the draft in Europe originated in the aftermath of the French Revolution. At the time of writing, about 60 countries worldwide keep a program drafting people in their military (Desilver 2019). After being widely used throughout the twentieth century, the European countries we analyze discontinued conscription from 1995 onward, with the notable exception of the United Kingdom, where it was abolished in 1961 (see timeline in Figure 1).

Although the European countries that decided to replace conscription with an all-volunteer force (AVF) undoubtedly did so following a domestic debate, defense scholars point toward a common, overarching incentive: The need to replace obsolescent mass armies as the end of the Cold War dramatically reduced the risk of large-scale interstate conflicts in Europe.¹¹

Volunteer militaries were also better suited for the new generation of high technology, expeditionary, and multinational NATO and UN missions characterizing the post-Cold War world, compared to poorly equipped mass armies (Haltiner and Tresch 2008; Poutvaara and Wagener 2011a). Divorcing warfare activities from border defense dramatically weakened the policy rationale behind military conscription. In the words of Haltiner and Tresch (2008, p. 172): “citizen-soldiers were formally and traditionally considered ideal defenders of their national territory; they are, however, not considered suited to the new kind of multinational military missions abroad. No European people would be ready to legitimize the compulsory employment of

¹¹Military conscription is often a response to deteriorating international security. Most of the countries that did not discontinue conscription, such as Finland, Greece, or Norway, are still involved in territorial disputes. Amid rising regional tensions, Sweden and Lithuania decided to reintroduce a military draft; available at: <https://bit.ly/339XKxH> and <https://bbc.in/3IwhiiD>.

FIGURE 1 Timing of Reforms across Europe

Notes: Based on an update of Toronto's (2007) military recruitment dataset. The Codebook, which is part of the replication material, provides extensive summary statistics and some contextual description for each reform.

young conscripts in missions out of national or alliance territory.”¹²

In line with their tradition of pioneering liberal change, Belgium (in 1995) and the Netherlands (1997) were among the first countries to abolish conscription, leading to the first wave of reforms—lasting until 2005—that involved all of the sampled Western European countries but Germany. The second wave, taking place between 2004 and 2009, saw Central and Eastern European countries moving from conscription to AVFs (Bove and Cavatorta 2012). The relative delay with respect to Western countries has to do with the broader transformation former USSR allies experienced with the collapse of the Soviet Union, which led to an agenda of increasing integration between Western and Eastern Europe. Ten former members of the so-called Warsaw Pact, in fact, chose, upon their independence, to move from the Soviet to the Western security umbrella, joining NATO.¹³ By 2005, six of them had already adopted AVFs. Abolishing conscription was such an important reform for countries such as Hungary and Czechia, that they completed the transfor-

mation to an AVF prior to the deadline that had been set initially (George and Teigen 2008).

For our RD strategy, the two key pieces of information that jointly identify the “pivotal cohort”—the first one to be exempted from conscription—are (a) the date in which the reform ending conscription was implemented in each country, and (b) the age at which young men were required to serve. The former information is retrieved from the “Military recruitment dataset” (Toronto 2007; see also Asal, Conrad, and Toronto 2017, for a recent application), which we extend to include seven countries where conscription was suspended after 2007, using data from *War Resisters' International*, a global network of grassroots pacifist groups.¹⁴ The latter is based on the CIA World Factbook (CIA 2020), an archive providing the required ages for voluntary or mandatory military service and the length of service obligation for each of the countries in our sample.

Results

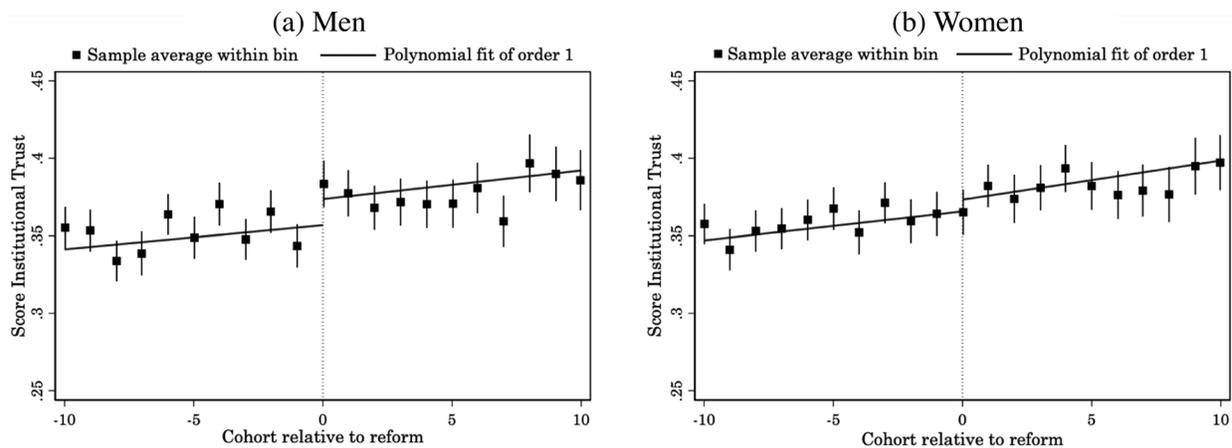
Figure 2 displays the effect of ending conscription on institutional trust. Institutional trust is captured by a single composite index obtained through the first component of a PCA that combines trust in legal system, parliament, parties, and politicians. We begin by looking at

¹²In addition to strategic considerations, economists converge in describing military conscription as “inefficient” compared to voluntary service, which exhibits relatively longer tenures, minimizing turnover costs, and does not involve any coercive recruiting mechanism. Conscription also disregards individuals' relative skills, violating established principles of labor market productivity via specialization (Hall and Tarabar 2015). Similar negative views about military conscription arise when considering issues of intergenerational equity (Poutvaara and Wagener 2007, 2011b).

¹³Whereas Western countries, except Sweden, had been already NATO members for decades when conscription was halted, every Central and Eastern European country in our sample joined NATO just a few years before discontinuing military conscription.

¹⁴We retrieved information for the following countries (first year without conscription in parentheses): Bulgaria (2008), Poland (2009), and Sweden (2011). Conscription in Sweden was then reactivated in 2017. See <https://wri-irg.org/en/> for detailed country reports.

FIGURE 2 End of Conscription and Institutional Trust



Notes: Figures are obtained using the `rdrobust` package developed by Calonico et al. (2017), based on IMSE-optimal binning and triangular dummy weights; 95% confidence interval. In subpart (a) on the left, we estimate $\hat{\beta} = 0.026$, significant at 1%, with $SE = 0.008$ and a 7-years bandwidth around the treatment including all males eligible for serving in the army ($n = 12,523$). In subpart (b) on the right, the effect of ending conscription on institutional trust ($\hat{\beta} = 0.008$, $SE = 0.007$), with a bandwidth of 9 years and a sample of 14,792 women. Country and ESS-wave fixed effects apply.

the subsample of men in Figure 2a, where we estimate $\hat{\beta} = 0.026$, significant at 1%. Ending conscription did not lead to distrust toward the state, as those proposing its reintroduction would implicitly suggest. Quite on the contrary, we detect a mild but nonetheless meaningful increase in institutional trust, later in life, by about seven percentage points on the respondents' scale.

As expected, Figure 2b shows that the end of conscription did not entail any effect on women from the same cohorts. Comparing the z -scores of RD coefficients with a standard t -test reveals that the effect of ending conscription on men's trust is significantly higher than the effect on women's for each of the four trust items.¹⁵ As shown in SI Section C.4 (p. 11), the main finding holds across each of the individual trust items.¹⁶

Although all our sampled countries discontinued conscription under a democratic regime, 6 of 15 countries (Bulgaria, Czechia, Hungary, Poland, Slovakia, and Slovenia) were members of the Warsaw Pact until the collapse of the Soviet Union. The political and historical contexts within which reforms ending conscription took

place across the postsocialist block in our sample differ in two important ways.

First, despite the enactment of important reforms, in the years of the transition to AVFs (between 2004 and 2009), former members of the "Warsaw Pact" lagged behind Western democracies with respect to several democracy indexes, and particularly to the pervasiveness of military and political corruption. For example, the *V-Dem* corruption index was on average more than four times higher in the six postsocialist countries in our sample, compared to the nine Western ones,¹⁷ whereas the "Global Corruption Barometer" computed by *Transparency International*¹⁸ reports that perception of corruption of the military to be 17% higher in ex-Warsaw pact republics. In a similar vein, all postsocialist countries are labeled as "flawed democracies" by the index developed by the *Economist Intelligence Unit*.¹⁹

Second, labor military policies in postsocialist countries differ significantly from those in Western countries: in the former, in fact, the abolition of military conscription was part of a broader reorganization of defense policies, and occurred later in time. After the end of the Cold War, postsocialist countries had to profoundly revisit institutional checks and balances as part of their democratization process, which involved also the military

¹⁵Specifically, the z -scores are 2.03 for the legal system, 1.90 for parliament, 2.27 for parties, and 1.32 for politicians.

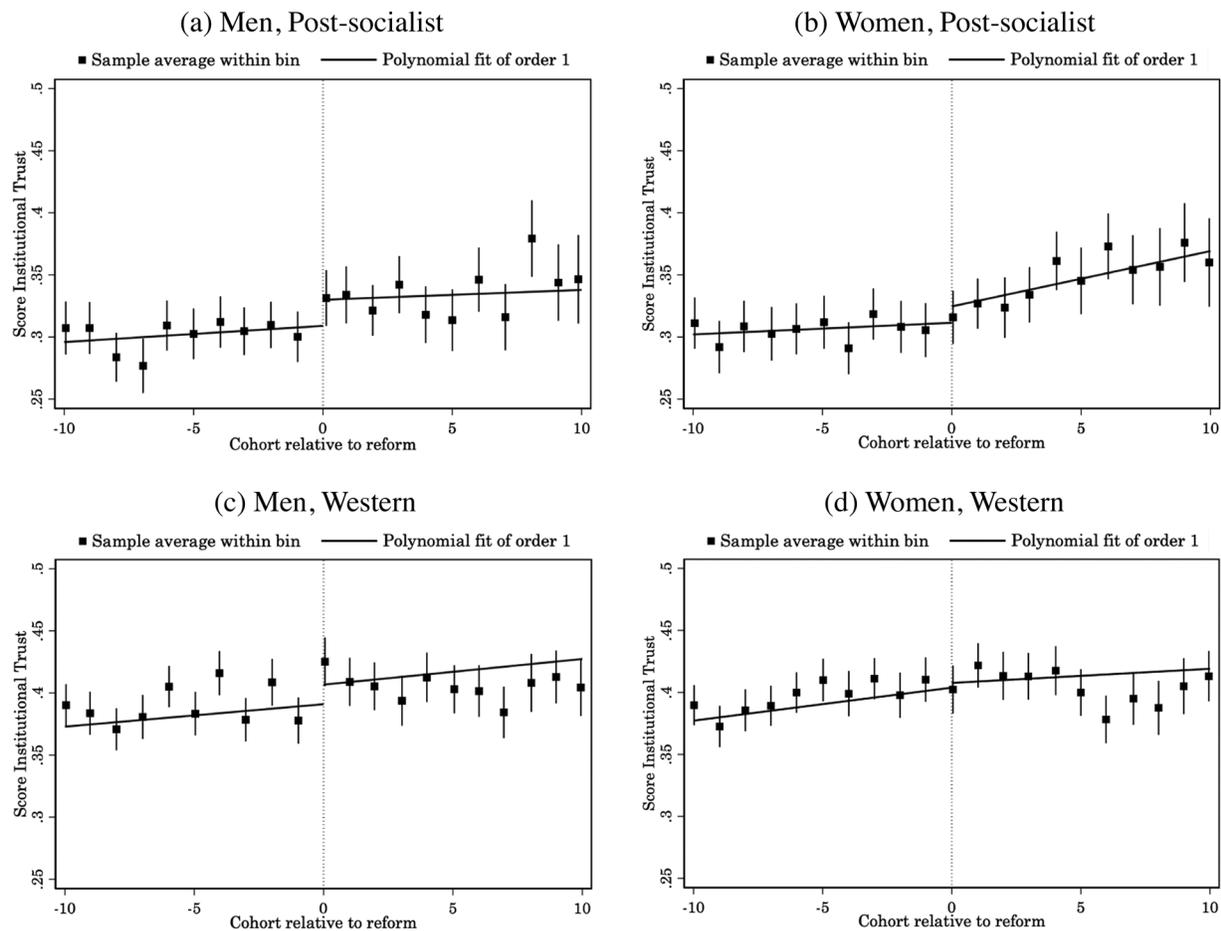
¹⁶We do not detect any informative heterogeneity when we analyze each of the 15 countries in our sample individually. We find strong evidence in support for H1b in Czechia, Germany, Italy, Portugal, and the United Kingdom. In the remaining countries the effect has the expected sign but is largely insignificant, given the rather limited statistical power due to a lower number of observations within each country.

¹⁷Available at: <https://www.v-dem.net/en/>.

¹⁸Available at: <https://www.transparency.org/en/gcb>. Authors' calculations are provided in the replication material.

¹⁹Available at: <https://www.eiu.com/n/>.

FIGURE 3 Postsocialist versus Western Countries



Notes: Figures are obtained using the *rdrobust* package developed by Calonico et al. (2017), based on IMSE-optimal binning and triangular dummy weights; 95% confidence interval. In subpart (a) on the top left, we estimate $\hat{\beta} = 0.030$ (significant at 5%), $SE = 0.014$, bandwidths of 6 years, $n = 4713$ men. In subpart (b) on the top right, we estimate $\hat{\beta} = 0.010$ (nonsignificant), $SE = 0.011$, bandwidths of 8 years, $n = 5526$ women. In subpart (c) on the bottom left, we estimate $\hat{\beta} = 0.019$ (significant at 5%), $SE = 0.009$, bandwidths of 9 years, $n = 9019$ men. In subpart (d) on the bottom right, we estimate $\hat{\beta} = 0.006$ (nonsignificant), $SE = 0.008$, bandwidths of 9 years, $n = 8740$ women. Country and ESS-wave fixed effects apply.

(Čížik 2021; Dunay 2005; Kříž 2007; Latawski 2005; Malešič and Vegič 2009). In many postsocialist democracies, citizens saw the conscription system as an emblematic relic of the corrupted, predemocratic era.²⁰

To the extent to which their shared postsocialist (or communist) history intensifies the effect of discontinuing conscription, we should recover differences in the intensity of the treatment effect between the six ex-Warsaw Pact and the nine Western European countries in our sample. We evaluate the effect of ending conscription on trust across the two blocks in Figure 3. Our findings are rather reassuring: Although the treatment effect is indeed

higher among postsocialist countries, it remains positive and significant among men—while nonsignificant among women—in both blocks.

Threats to Identification

In the SI, we assess the validity of our RD design (SI Section A, pp. 2–4). Performing standard density and continuity tests confirms the absence of sorting around the discontinuity (SI Sections A.1 and A.2, pp. 2–3). We also look into the possibility that educational, fiscal and labor market policies implemented in the same year as the conscription reforms, and

²⁰In Russia, where conscription remains active to this date, it has been reported that, whereas men from the rural reaches of the country are unlikely to avoid conscription, affluent fami-

lies often press officials to exempt their children; available at: <https://econ.st/2ZRpVTD>.

potentially exerting a long-run gendered impact on institutional trust, may act as cofounders (SI Section A.3, p. 4), finding no support for this hypothesis. Second, we test the robustness of our analysis to different modeling choices (SI Section B, pp. 6–7), demonstrating that the reported findings are robust to the use of, respectively, alternative bandwidths, polynomial orders, and RD weights (SI Sections B.1 and B.2, pp. 6–7). Results are robust to alternative country sampling (i.e., dropping one country at a time, SI Section C.1, p. 8), to alternative unit sampling (i.e., including respondents who completed tertiary education, SI Section C.2, p. 9), and to placebo reform dates (SI Section C.3, p. 10). As mentioned above, we also show that results hold up well to considering each of the four trust items composing our index separately (SI Section C.4, p. 11). Altogether, these tests give us confidence that our estimates are unlikely to be spuriously driven by sorting around the treatment, underlying time trends, outlier countries or specific trust items.²¹

Finally, SI Section D (pp. 12–19) explores the heterogeneity of the treatment both in terms of policy graduality (SI Section D.1, p. 12), reform date (SI Section D.2, p. 17), socioeconomic backgrounds (SI Section D.3, p. 18), and political views of the government approving the reform (SI Section D.4, p. 19), recovering larger coefficients for reforms enacted sharply and more recently, and for individuals with higher socioeconomic background. It is worth pausing on this last battery of robustness checks. Because the treatment status is based on formal eligibility, rather than participation status, our estimates must be understood as an intention to treat. We explain here how *policy graduality* may drive the discrepancy between de jure and de facto treatment status. Overall, these threats to identification make false negatives more likely relative to a fully randomized experiment.

We account for the possibility that the actual *implementation* of the policy, which is the threshold upon which individuals are assigned (or not) to the treatment group in our analysis, might have occurred later than the *approval* of the legislation discontinuing conscription. Particularly in, in countries opting for a gradual phaseout period, governments' effort to mobilize conscripts might have weakened following the approval date (Stolwijk

²¹ Findings are also robust to controlling for the duration of enlistment each respondent faced. Country fixed effects cannot in fact account for the possibility that the length of conscription may have varied over time. We retrieve yearly information on terms of service for each country from Toronto (2007), merge it with our survey using CIA (2020) data on conscription ages, and find that our estimates remain largely unchanged when controlling for months of enlistment. Because of space limitations, these tables are not included, but can be reproduced using the replication material provided.

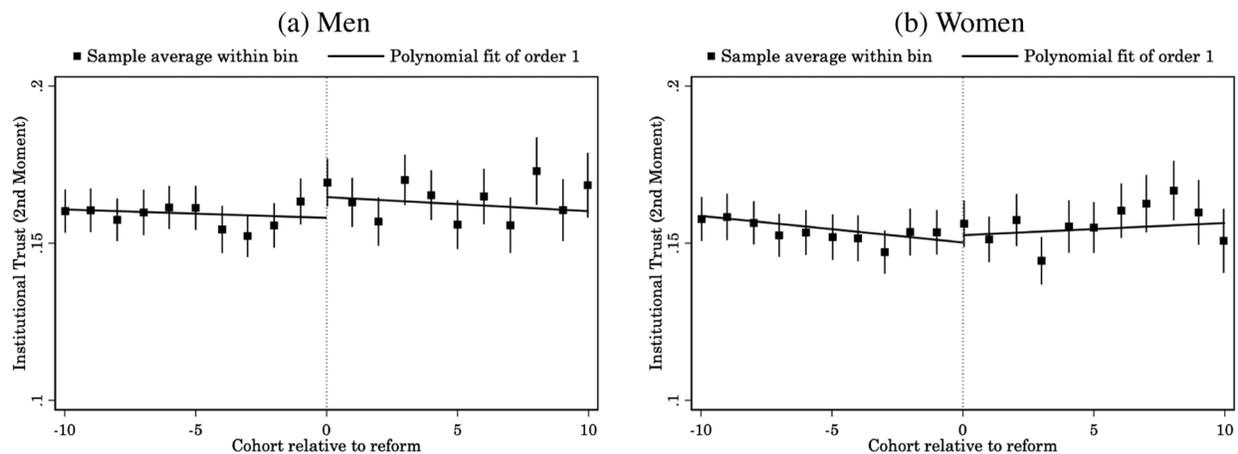
2005), decreasing the number of conscripted individuals and, therefore, possibly biasing our estimated effect toward zero. To assess the extent of the measurement error arising from our eligibility-based intention to treat, we collect qualitative information about the date of approval of each reform, as well as quantitative administrative and survey data about the number of conscripts in each of the sampled countries, in the years before the formal end of conscription. Results displayed in SI Section D.1 (p. 12) show how countries in which the distance between approval and completion of the conscription reform was one year or less display a 14.3% higher treatment effect, compared to those adopting a longer phaseout period. The smaller effect we retrieve in countries where implementation took longer might be due laxer policy enforcement in the phaseout years, which made draft avoidance relatively easier. As a result, our coefficient, that is, an intention to treat, is more likely to misclassify as conscripted (i.e., members of the control group), individuals who avoided the military service (i.e., the treatment group). If anything, this exerts a *downward* bias on our aggregate estimated treatment effect.²²

How Conscription Affects Institutional Trust

Military Culture

We have shown that ending conscription increased institutional trust. Common to the theoretical arguments

²² An additional reason why some individuals in conscripted cohorts—our control group—may not have received military training is due to the fact that, in most democracies, conscripted individuals are given the possibility to exert *conscientious objection* to the military, and apply for alternative services. We argue that the edge between intention to treat and treatment status is however small. Except for Slovenia, in all sampled countries the compulsory period of alternative services was always longer than that of military conscription, in order to discourage draftees from resorting to conscientious objection. For a similar reason, governments typically granted a lower compensation to objectors, relative to their military counterparts (for an extensive review, see Smith 2004). As a consequence, the share of young men choosing to apply for alternative service has generally been very small, and steadily below 5% in Belgium, France, Portugal, and the United Kingdom (Smith 2004). Similar figures most likely apply to Bulgaria, Hungary, Poland, and Slovakia, even though in these cases, available estimates come from experts' reports (Stolwijk 2005). Figures are slightly higher in the Netherlands, were less than 8% among conscripted men applied for civilian service between 1982 and 1991, with 80% of these being accepted. Similarly, in Sweden, the share of draftees applying for conscientious objection reached 10% in the final years in which conscription was in place. Finally, Germany, Italy, and Spain represent the outliers among the countries under scrutiny, exhibiting substantially higher shares of objectors among their conscripts (Stolwijk 2005).

FIGURE 4 Polarization of Institutional Trust

Notes: Figures are obtained using the *rdrobust* package developed by Calonico et al. (2017), based on IMSE-optimal binning and triangular dummy weights; 95% confidence interval. In subpart (a) on the left, we estimate $\hat{\beta} = 0.007$ (significant at 10%), SE = 0.004, bandwidth of 10 years, $n = 16,899$ men. In subpart (b) on the right, we estimate $\hat{\beta} = 0.001$, SE = 0.003, bandwidth of 19 years, $n = 28,235$ women. Country and ESS-wave fixed effects apply.

presented in the second section of the article is the belief that conscription drives recruits' long-run attitudinal patterns by transmitting a set of core military values. The resulting, gradual adjustment of draftees to the dominant military culture should ultimately produce a rather homogeneous community with uniform values and attitudes (see e.g., de Juan, Haass, and Pierskalla 2021; Krebs 2004). This military culture mechanism yields an important testable prediction: attitudes toward institutions should be more homogeneous among conscripts, and more polarized among nonconscripts.

We analyze the causal effect of ending conscription on the standard deviation of the distribution of institutional trust among men and women. We begin by computing the country-wave average institutional trust. Then, we take the squared difference between each individual's score and the corresponding country-wave average (both normalized between 0 and 1). Finally, we test the effect of our treatment—ending conscription—on such index, using the same estimation strategy of the main analysis. Positive treatment effects would thus indicate that ending conscription increases the diversity of opinions, and vice versa.

We find evidence in support of this “uniformity mechanism.” Figure 4 shows that ending conscription exerted a positive and significant impact on the polarization of institutional trust among men (a), with no comparable effect among women (b). The increased polarization in men's institutional trust supports the idea that military conscription prompted more uniformed, negative views of civil society among draftees, at a very sen-

sitive stage of their life. Further analysis show that such effect is driven, as one would expect, by those countries in which the policies ending conscription were implemented more abruptly (see SI Section D.1, p. 12).

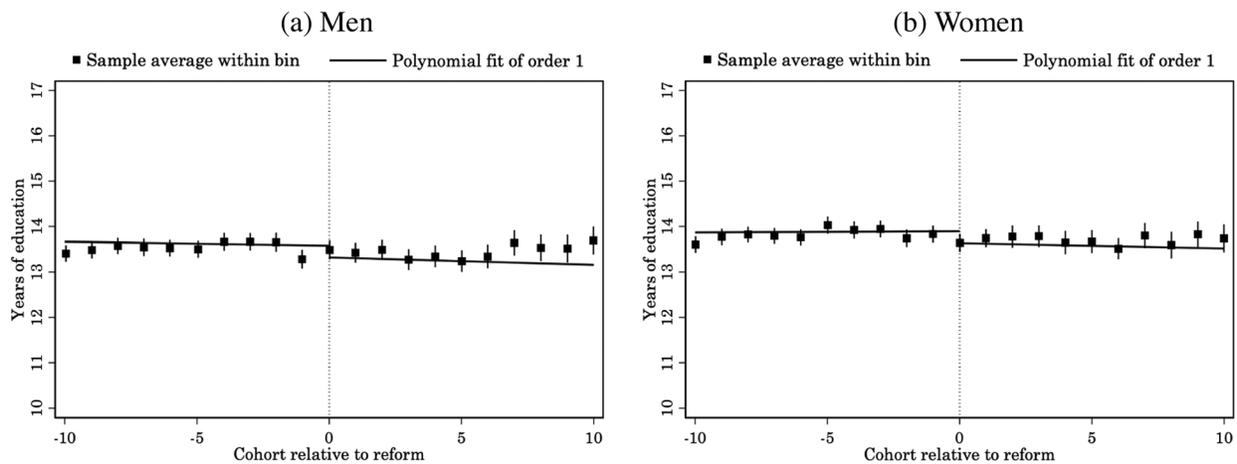
Educational Choices

Conscription and the end thereof may affect trust formation indirectly by altering educational choices. The abolition of conscription might in fact exert an ambivalent effect on college enrollment rates: positive, on the one side, as it possibly entails an opportunity cost for skill development (e.g., Cipollone and Rosolia 2007); negative, as it could reduce the incentive to enroll as a way to postpone (or avoid) the military (e.g., Card and Lemieux 2001). The net effect of these countervailing forces is often small, and sometimes indistinguishable from zero (Di Pietro 2013; Imbens and Klaauw 1995). Yet, if the first effect dominates, discontinuing conscription would increase educational attainments, hence likely improve individual socioeconomic conditions and, in turn, institutional trust (see, e.g., Gelepithis and Giani 2020), thereby partly channeling our findings.

We do not find evidence in support of the opportunity cost mechanism just presented, which is consistent with some empirical evidence (Di Pietro 2013). The abolition of conscription entails a nonsignificant effect on either men's or women's number of years of completed education (see Figure 5a,b).²³ Such finding replicates when

²³For this analysis, we reinclude in the sample individuals with tertiary education, but discard respondents who are studying at the

FIGURE 5 Educational Choices



Notes: Figures are obtained using the `rdrobust` package developed by Calonico et al. (2017), based on IMSE-optimal binning and triangular dummy weights; 95% confidence interval. In subpart (a) we estimate $\hat{\beta} = 0.015$, $SE = 0.115$, bandwidth of 7 years, $n = 15,292$ men. In subpart (b) we estimate $\hat{\beta} = -0.118$, $SE = 0.104$, bandwidth of 7 years, $n = 16,192$ women. The estimated effects are statistically insignificant for both men and women. Country and ESS-wave fixed effects apply.

considering different proxies for long-run skill development, including occupational skill specificity, perceived household income and a different operationalization of the education variable (i.e., an indicator taking value 1 if the respondent completed tertiary education or above), all measured at the time of the survey.²⁴ Although our dataset is not equipped to estimate the effect of ending conscription on subsequent career decisions, this analysis suggests that the opportunity-cost channel is unlikely to be the main mechanism through which military service affects institutional trust.

Government Popularity

Our design builds on the idea that the lower institutional trust observed among former servicemen captures a stable, “diffuse” mistrust, rather than a time-varying, “specific” evaluation of the government in charge at the time of the interview (Citrin and Stoker 2018; Hetherington 1998; Keele 2007; Zmerli and Newton 2008). The fact that point estimates and standard errors are very similar across different trust items is reassuring: it suggests that our metric is unlikely to capture contextual dissatisfaction for specific institutional branches at the time of the interview. Yet the documented difference in institu-

time of the interview. Although this subgroup cannot be exactly identified in the ESS, we exclude individuals younger than 24 for whom no ISCO occupational code is reported in the ESS dataset.

²⁴We provide the coding for these additional analyses in the replication material.

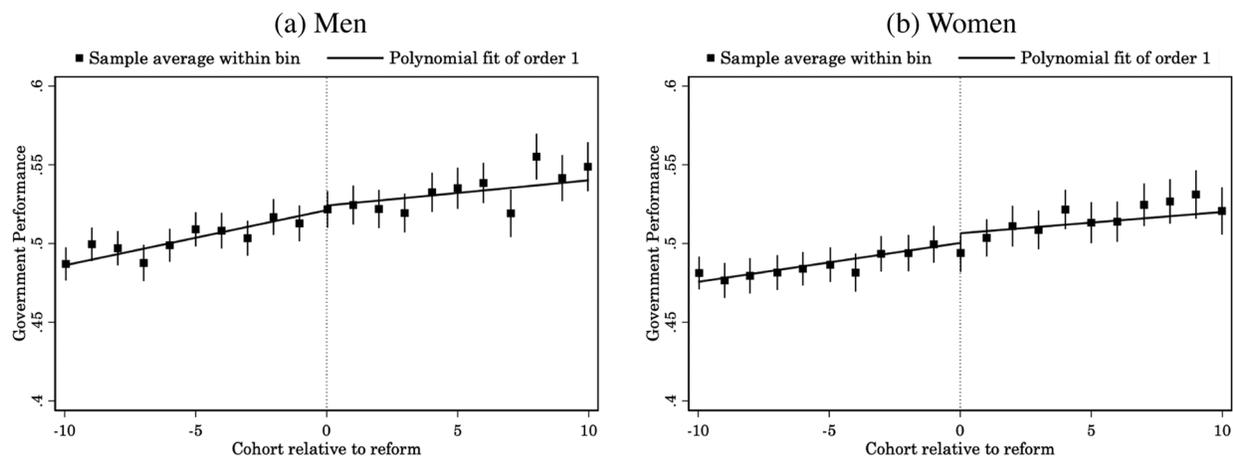
tional trust between conscripts and nonconscripts may also partly reflect differences in support for the executive.

We investigate whether military conscription affects *specific* support, captured by the contextual evaluation of government performance on three key issues: economy, education, and health.²⁵ To ease the presentation, our dependent variable is again the (normalized) first component of a PCA between the three survey items. Figure 6 shows that ending conscription had indeed no effect on either men’s (a) or women’s (b) assessment of the performance of the government at the time of the interview. These additional analyses help ruling out the possibility that our findings are spuriously driven by attitudes toward the government in charge at the time of the interview, or by potential, concurrent events, such as political scandals.

Our effect may also be capturing long-lasting patterns of support for the government that adopted the reform. Conscription might have been perceived as an unfair gender-specific burden when it was abolished. As such, the last cohorts of conscripts could still display resentful evaluations toward the political leaning of the government in charge at the time, whereas the first cohorts of exempted young men could still be grateful to that same government. If this was the case, then the magnitude of our estimates should be larger when the government at the time of the interview shares the

²⁵Respondents were asked the following question: “How do you rate the state of the economy/education/healthcare in your country nowadays?”

FIGURE 6 Specific Government Performance



Notes: Figures are obtained using the *rdrobust* package developed by Calonico et al. (2017), based on IMSE-optimal binning and triangular dummy weights; 95% confidence interval. In subpart (a) on the left, we estimate $\hat{\beta} = -0.000$, $SE = 0.006$, bandwidth of 9 years, $n = 16,577$ men. In subpart (b) on the right, $\hat{\beta} = 0.001$, $SE = 0.005$, bandwidth of 10 years, $n = 17,799$ women. The estimated effects are statistically insignificant for both men and women. Country and ESS-wave fixed effects apply.

same political leanings of the government adopting the reform. Respondents may in fact be more likely to incorporate, in these cases, *specific* mistrust toward the executive in their responses, affecting in turn their *diffuse* levels of trust toward the institutional system.

We explore this possibility by leveraging the interview date of each respondent, as well as information from *ParlGov* (Döring and Manow 2021) about the ideological orientation of the government in charge, both at the time of the reform adoption *and* at the time of each interview. We do not find support for this mechanism, as treatment effects are unaffected by whether the government in charge when the interview takes place shares (or not) the political leaning of the government adopting the reform (SI Section D.4, p. 19 and Codebook B.2, p. 7).²⁶

Conclusion

The debate around the possibility of reintroducing a compulsory military service has been particularly lively in recent years. Politicians advocating in its favor have

²⁶In SI Section D.4, p. 19, we also show how results are robust when differentiating between reforms *adopted* by, respectively, right-wing and left-wing executives. In the replication material, we show how estimates remain the same when removing from the sample the interviews occurring in the years around the reform adoption ($N = 13,030$). Unfortunately, only 370 men aged 17–19, from nine countries, were interviewed in the 365 days around the reform adoption date, making any inference on this subset of respondents problematic.

long argued that reintroducing conscription is an effective mean to bring the citizen closer to the nation. In this article, we contribute to such discussion by asking whether ending military conscription has affected institutional trust in European countries. The answer to this question is not clear-cut *ex ante*, as conscription exposes individuals to socialization processes with seemingly complementary objectives. On the one side, it is intended as an experience boosting values of service and civic obligations toward the state; on the other, the same socialization processes, taking place in highly structured organizations, distant from society, shape civic identity along military norms. This creates a strong identification with the armed forces, possibly clashing with loyalty toward democratic institutions.

Building on a quasi-experimental design, we demonstrate that reintroducing military conscription as a way to foster civic sense would likely yield the opposite effect. We find that cohorts just exempted from conscription did not develop more negative attitudes toward the state, but actually exhibit higher levels of institutional trust than cohorts who served compulsory time as soldiers, several years after doing so. We demonstrate that the documented decrease in institutional trust is neither the by-product of contextual government evaluations nor the result of a change in educational choices driven by the abolition of conscription. Instead, we show that the creation of homogeneous communities with uniform views is likely at the root of lower diffuse trust later in life.

Our article pieces together two strands of the literature, analyzing, respectively, the dynamics of the “civil–military gap” and the long-run determinants of institutional trust. We contribute both substantively and methodologically to the debate on the existence of a “civil–military gap” between those who served in the military and those who did not. Substantively, whereas previous research focused on personality traits (Jackson et al. 2012), ideological orientations (Erikson and Stoker 2011; Green, Davenport, and Hanson 2019; Navajas et al. 2022), or attitudes toward the use of violence (Horowitz, Simpson, and Stam 2011; Navajas et al. 2022), we focus on institutional trust. The documented attitudinal divide we uncover is of key relevance, as it can undermine the stability of the relationship between military institutions and society at large (see, e.g., Brooks 2019).

This article also contributes to the literature that seeks to understand the determinants of trust. Margaret Levi qualifies trust as “a holding word for a variety of phenomena that enable individuals to take risks in dealing with others, that solve collective action problems, or that promote willingness to act in ways that seem contrary to standard definitions of self-interest” (Levi 1996, p. 1). In the realm of politics, trust confers legitimacy to institutions, fostering compliance with the rule of law among citizens. It is thus crucial “for the effectiveness and durability of democratic governments, regimes that many today view as increasingly fragile” (Rathbun 2011, p. 50). It is hence by no surprise that diminishing institutional trust is conceived as a major issue in liberal democracies (Citrin and Stoker 2018), prompting scholars to investigate its long-run determinants. We add to this literature by focusing on the domain of high politics. That military conscription decreases institutional trust highlights a policy paradox, warning against the somewhat popular idea that reintroducing military conscription may solidify the relationship between the citizen and the state.

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Supporting Information

Additional supporting information may be found online in the Supporting Information section at the end of the article.

- Appendix A:** Validity of RD specification
Appendix B: Robustness to modelling choices
Appendix C: Robustness to sampling choices
Appendix D: Analysis of Heterogeneity