Compulsory Military Service Reduces Affective Polarization:

Evidence from Conscription Reforms in Europe

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Abstract

Research on remedies for affective polarization has primarily focused on psychological interventions, and limited studies consider how state institutions might depolarize voters. I argue that compulsory military service—a central state institution—can depolarize voters because it prevents early partisan sorting and increases the likelihood of contact between partisans during their impressionable years. Leveraging the staggered abolition of mandatory conscription laws in fifteen European countries and employing a regression discontinuity design, I show that men exempt from mandatory conscription report higher levels of affective polarization than men who were subject to mass conscription. This effect is mainly driven by partisan parochialism among men exempt from service and is unrelated to ideological change. My findings emphasize the potential depolarizing effects of state institutions and illustrate how the abolition of mandatory service contributed to intensified patterns of affective polarization in Europe, contributing to the literature on the institutional origins of affective polarization.

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Partisan affective polarization—the gap between citizens' positive sentiment toward their own party and negative sentiment toward their opposing parties—is increasingly common around the world (Iyengar and Westwood 2015; Druckman and Levendusky 2019; Gidron, Adams and Horne 2020). Existing research on remedies for affective polarization has mostly focused on psychological interventions (Voelkel et al. 2022; Druckman et al. 2022), cross-party conversations (Levendusky and Stecula 2021; Fishkin et al. 2021; Kalla and Broockman 2022), and electoral institutions (Bassan-Nygate and Weiss 2022; Horne, Adams and Gidron 2022; Adams et al. 2022). Though it is commonly accepted that non-elected institutions such as militaries, schools, and civil society organizations have a profound influence on daily life in democratic contexts (Pepinsky, Pierskalla and Sacks 2017), and though popular accounts argue for the depolarizing effects of mandatory national service (Mason 2019; Carden 2021), scholars of affective polarization have yet to study whether and how compulsory military service improves partisan relations.

Building on studies of intergroup contact and cross-party conversations (Allport 1954; Weiss 2021; Levendusky and Stecula 2021; Santoro and Broockman 2022; Amsalem, Merkley and Loewen 2022), and studies on the effects of mass-conscription and military service on national identity and political preferences (Samii 2013; Grossman, Manekin and Miodownik 2015; Garcia 2015; Finseraas and Kotsadam 2017; Bove, Di Leo and Giani 2022; Ronconi and Ramos-Toro N.d.), I argue that compulsory military service can reduce affective polarization among voters because it prevents early partisan sorting (Mason 2015; Maxwell 2020), and increases the likelihood of intergroup contact between partisans during their impressionable years. To test the observable implication of this argument, I follow a novel research design proposed by Bove, Di Leo and Giani (2022), and leverage the staggered abolition of mandatory military conscription in fifteen European countries as well as data from the Comparative Study of Electoral Systems (CSES 2022). Employing an age-based regression discontinuity design (Samii 2013; Cavaille and Marshall 2019; Getmansky and Weiss 2022; Bove, Di Leo and Giani 2022), I show that men exempt from mandatory conscription report higher levels of affective polarization than men that were not exempt from conscription. I find no such pattern in placebo tests focusing on women who were not affected by the abolition of

compulsory military service.

To explore potential mechanisms, I analyze a series of additional outcomes relating to democratic norms, ideology, and levels of ingroup love and outgroup hate. I find no evidence that conscription affects ideology or support for democratic norms, suggesting that the effect of conscription on affective polarization is unlikely driven by ideological socialization. However, in line with theory and evidence regarding the potential of intergroup contact to reduce group-centric world views (Pettigrew 1998), and the consequences of social segregation and lack of contact for group favoritism (Scacco and Warren 2018), I provide suggestive evidence that my main effect—increased affective polarization among men exempt from military service—is driven by increased parochialism. Specifically, men exempt from military service report higher affective evaluations of their in-party, and military service appears to serve as an institution that reduces citizens' partisan attachments. These findings emphasize the depolarizing role of compulsory military service and suggest that the gradual abolition of mandatory military service throughout the 20th century contributed to intensified patterns of partisan affective polarization in Europe.

Conscription and Depolarization

A growing literature considers the effects of conscription and military service on a range of outcomes, including trust in institutions (Bove, Di Leo and Giani 2022), national identity (Ronconi and Ramos-Toro N.d.), ethnic prejudice (Samii 2013), and conflict-related preferences (Grossman, Manekin and Miodownik 2015). Theoretically, two central mechanisms may account for the link between conscription and changes in attitudes and preferences broadly defined. These mechanisms relate to socialization within military institutions and intergroup contact between soldiers' from different backgrounds (Rosman 2020).[1]

Taking both mechanisms into account, early theoretical work often alludes to the military as a nation-building institution, arguing that conscription can promote a strong national identity and social cohesion (Moskos et al. 1988; Huntington 1981). In line with these arguments, recent evidence demonstrates that military service increases national identification and promotes social integration

[1]Though see Krebs (2004) for a critique of these mechanisms in the realm of nation building.

(Ronconi and Ramos-Toro N.d.), but may have negative externalities for trust in institutions (Bove, Di Leo and Giani 2022). To date, existing research has yet to identify the effects of mass conscription on affective polarization. However, as implied by popular accounts (Mason 2019; Carden 2021), one might expect that military service—and specifically conscription—would have important consequences for partisan relations.

As a central state institution that guarantees that citizens from all walks of life serve in military forces, conscription can affect partisan affective polarization for several interrelated reasons. First, compulsory military service provides citizens with a meaningful experience of exposure to outgroups during their impressionable years. This experience entails equal status, cooperative, institutionally supported, and prolonged contact with other citizens holding different political and partisan preferences. Existing theory and evidence suggest that this type of contact can reduce intergroup animosity (Allport 1954; Samii 2013). Second, compulsory military service prevents common patterns of social and political sorting which can increase affective polarization (Mason 2015). Accordingly, my primary expectation is that citizens exempt from (exposed to) compulsory military service will report higher (lower) levels of affective polarization.

Since affective polarization represents the gap between citizens' positive sentiment toward their own party and negative sentiment toward their opposing parties, increased (reduced) levels of affective polarization may be driven by shifts in ingroup love or outgroup hate (Bassan-Nygate and Weiss 2022). Theoretically, one might expect conscription to reduce affective polarization by encouraging conscripts to adopt favorable attitudes towards supporters of other parties as a result of intense and prolonged contact in the military. However, the experience of conscription that facilitates contact and inhibits partisan sorting may reduce affective polarization by reducing group parochialism (Pettigrew 1998). In other words, avoidance of "sorted" political spaces and exposure to soldiers holding opposing political preferences might lead soldiers to question their own preferences and attenuate their partisan attachments. These two processes are theoretically important and by no means mutually exclusive. I thus explore their plausibility as part of the empirical analyses I now turn to describe.

Research Design

Identifying the effects of compulsory military service on affective polarization is challenging because affective polarization—and intergroup relations more broadly—might be a *cause* or an *effect* of conscription policies (Krebs 2005). To overcome this challenge and isolate the effects of mass conscription on citizens' affective polarization, I build on Bove, Di Leo and Giani (2022) and adopt a research design that leverages the timing of conscription reforms across European countries and sharp age discontinuities in conscription eligibility. Doing so, I compare levels of affective polarization between male citizens subject to conscription and male citizens who were just young enough to be exempt from military service.

Data and Measurement

My empirical strategy, that builds on Bove, Di Leo and Giani (2022), requires three main sources of information: a measure of individual-level affective polarization (outcome), information about the date of conscription abolition in each country in my sample, and measures of a respondent's country, gender, and date of birth which determine whether a respondent was subject to, or exempt from, mass-conscription (treatment). To measure affective polarization, I use data from five waves of the CSES (1996-2020), in which respondents report like-dislike scores for political parties in their country. I follow recent advances in the measurement of affective polarization in multiparty systems Wagner (2021) to create an outcome representing the spread of like-dislike scores reported by respondent i for different parties in their country's electoral system using the following equation:

$$Polarization\ Spread_i = \sqrt{\frac{\sum_{p=1}^{p}(like_{ip} - \overline{like}_i)^2}{n_p}} \tag{1}$$

Where $like_{ip}$ represents respondent i's like-dislike score (0-10) for party p weighted by party p's relative electoral share (i.e., vote share in national elections), $\overline{like}_i$ represents the weighted average of a respondent's like-dislike scores for all parties in their country, and n_p represents the number of parties in the electoral system. Higher polarization scores represent a respondent for whom the like-dislike gap towards different parties in the electoral system is larger. I report

aggregate levels of affective polarization across countries in my sample Figure 1.

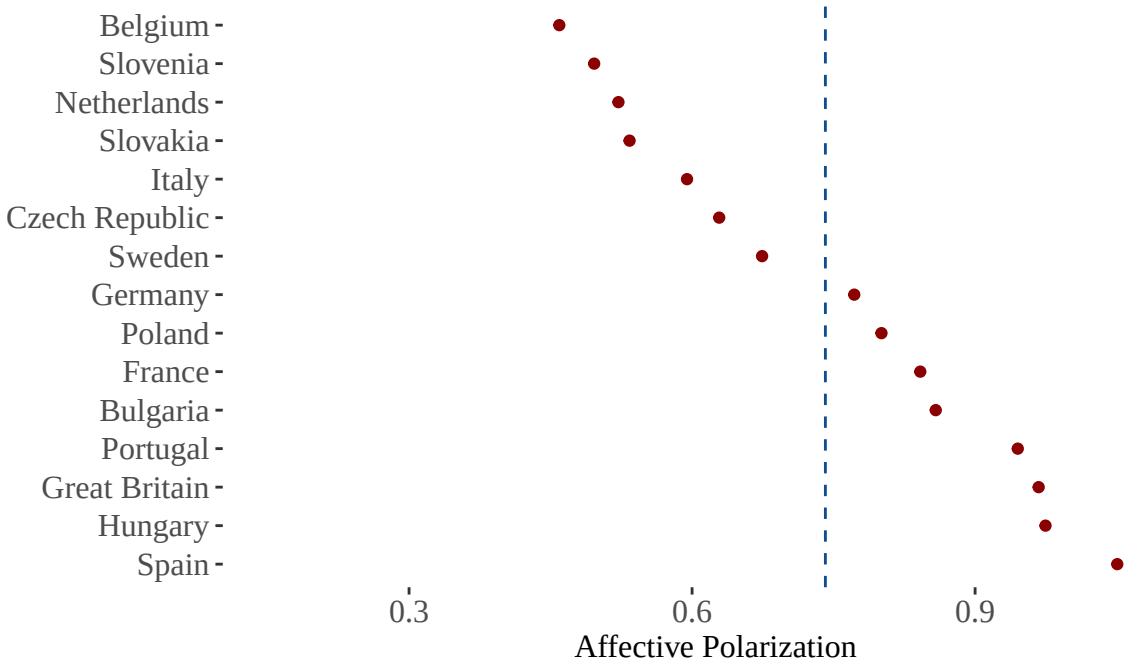

Figure 1: **Affective Polarization Across Countries** - This figure reports average levels of affective polarization by country using a measure of the weighted spread of like-dislike scores proposed by Wagner (2021).

To measure whether respondents' were subject to (or exempt from) conscription, I employ data from Bove, Di Leo and Giani (2022) regarding the age of conscription and date of compulsory service reforms in European countries. I report this information in Table A1 of the appendix. Based on this data, I use survey respondents' age to create a running variable that takes a positive (negative) value if a respondent is from a country-cohort that is exempt (not exempt) from compulsory military service. Based on my running variable, I classify my main treatment as indicated in Equation 2.

$$Compulsory\ Conscription = \begin{cases} \text{No if running variable} \geq 0 \\ \text{Yes if running variable} < 0 \end{cases} \tag{2}$$

Identification and Estimation Strategies

To identify the effects of compulsory military service, and specifically the abolition of mass conscription on affective polarization, I employ a regression discontinuity design. My central identifying assumption relates to the continuity of conditional expectation of counterfactual outcomes in the running variable (McCrary 2008). Under this assumption, respondents just too old to be exempt from military service are comparable to younger respondents who were not subject to conscription. In Appendix C I report density, balance, and placebo tests that reduce concerns regarding endogenous sorting around the discontinuity and substantiate my main identifying assumption.

In my main specification, I estimate the regression reported in Equation 3 to identify my main local average treatment effect. In this model, y_{icw} is the level of affective polarization for respondent i, in country c, at time t, and $f(X_i)$ is a linear function of my running variable. This function, which is a local linear regression estimated with a triangular kernel and the optimal bandwidth selection method proposed by Cattaneo, Idrobo and Titiunik (2019), allows me to identify β – representing the effect of abolishing mass-conscription on respondents' affective polarization.

$$y_{ict} = \beta D_i + f(X_i) + e_{ict} \tag{3}$$

I estimate Equation 3 separately on samples of male and female respondents to the CSES. I expect to identify positive and precisely estimated point estimates for male respondents who were influenced by the abolition of compulsory military service. In contrast, like Bove, Di Leo and Giani (2022), for my female sample, which is a placebo sample, I expect to find small and imprecisely estimated point estimates since female respondents were not influenced by the abolition of compulsory military service. Notably, my cross-country setting, and my focus on male respondents, reduce concerns regarding alternative cohort explanations, which could confound any given main effect in a single-country.

Results

In columns 1-3 of Table 1, I estimate my baseline specification as well as additional regressions with country and survey wave controls, showing that male respondents just old enough to be exempt from compulsory military service report higher levels of affective polarization. In contrast, as I show in columns 4-6, when estimating regressions on my placebo female respondent sample, I find no such effect. In substantive terms, the main local average treatment effect I identify in column 1 of Table 1 is equivalent to almost a tenth of the cross-national difference between the least polarized country in my sample (Belgium) and the most polarized country in my sample (Spain).

Table 1: RD Estimates – Effects of Conscription Abolition on Polarization

	1	2	3	4	5	6
B	0.056	0.036	0.041	0.01	0.003	0.002
SE	(0.025)	(0.018)	(0.016)	(0.026)	(0.02)	(0.018)
P	0.025	0.048	0.011	0.706	0.877	0.907
Sample	Male	Male	Male	Female	Female	Female
Bandwith	6	10	11	6	8	9
Country FE	No	Yes	Yes	No	Yes	Yes
Wave FE	No	No	Yes	No	No	Yes
N	43546	43546	43546	47614	47614	47614
Sample Mean	0.719	0.719	0.719	0.741	0.741	0.741
Sample SD	0.367	0.367	0.367	0.388	0.388	0.388

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal.
Robust standard errors in parentheses.

In Table 2, I consider whether increases in affective polarization amongst Male respondents exempt from compulsory military service are driven by a rise in ingroup love or a reduction in outgroup hate.[2] To do so, I employ two additional outcomes measuring respondents' affect towards their most (least) liked parties.[3] In line with theoretical accounts regarding the ability of intergroup contact to reduce group parochialism (Pettigrew 1998), and the consequences of segregation and lack of contact for ingroup favoritism (Scacco and Warren 2018), I find that the depolarizing effect

[2] I report placebo regressions from my female sample in Section D of the appendix.

[3] Like in my main outcome, I weigh this score by the party's vote share to capture the party's relative prominence.

of compulsory military service is largely driven by changes in ingroup love. Specifically, compared with male respondents subject to conscription, male respondents exempt from military service report warmer attitudes toward their most liked party.[4] While the point estimate for the least liked party is negative, implying colder attitudes towards out-party supporters amongst respondents exempt from service, this point estimate is imprecisely estimated across all models. I interpret this pattern of findings to suggest that conscription reduces polarization by discouraging partisan parochialism.

Table 2: RD Estimates – Effects of Conscription Abolition on Ingroup Love and Outgroup Hate

	Most Liked	Most Liked	Most Liked	Least Liked	Least Liked	Least Liked
B	0.1	0.07	0.079	-0.012	-0.009	-0.015
SE	(0.061)	(0.046)	(0.038)	(0.016)	(0.013)	(0.013)
P	0.103	0.132	0.041	0.44	0.497	0.262
Sample	Male	Male	Male	Male	Male	Male
Bandwith	7	10	13	10	14	12
Country FE	No	Yes	Yes	No	Yes	Yes
Wave FE	No	No	Yes	No	No	Yes
N	43546	43546	43546	43546	43546	43546
Sample Mean	2.079	0.104	0.104	0.104	0.104	0.104
Sample SD	0.936	0.244	0.244	0.244	0.244	0.244

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal.
Robust standard errors in parentheses.

Finally, in Table 3, I consider whether mass conscription affects support for democratic norms and ideology. I measure support for democratic norms by creating an index of two questions in the fifth wave of the CSES, eliciting respondents' support for a strong leader that bends the rules in pursuit of their goals as well as their belief that political compromise represents a form of selling out one's principles. To measure ideology, I employ a standard 10-point left-right scale included in all waves of the CSES.

The first two columns in Table 3 suggest that decreased affective polarization has no downstream consequences for respondents' support for democratic norms.[5] These findings are in line

[4]Effects are precisely estimated at conventional levels ($p < 0.05$) when controlling for country and survey wave.

[5]Note that I do not include a model with wave fixed effects because democratic norm items

with recent experimental studies questioning the link between interpersonal affective polarization and support for democratic norms (Broockman, Kalla and Westwood 2020), but should be taken with a grain of salt given their restricted sample size. Turning to my measure of ideology, which was elicited in all CSES waves, I find similar results by which the abolition of conscription does not appear to affect respondents' ideology. I construe the result in Tables 2-3, as suggestive evidence that socialization and ideological change are unlikely the mechanisms linking compulsory military service with shifts in affective polarization. Instead, I suggest that conscription reduces polarization because it encourages exposure to citizens with different partisan preferences and leads citizens to reevaluate their group attachments and, in turn, depolarize.

Table 3: RD Estimates – Effects of Conscription Abolition on Democratic Norms and Ideology

	Dem Index	Dem Index	Ideology	Ideology	Ideology
B	-0.096	-0.061	-0.043	-0.073	-0.043
SE	(0.101)	(0.097)	(0.144)	(0.13)	(0.123)
P	0.341	0.529	0.767	0.576	0.724
Sample	Male	Male	Male	Male	Male
Bandwith	12	13	10	12	13
Country FE	No	Yes	No	No	No
Wave FE	No	No	No	No	No
N	7388	7388	38843	38843	38843
Sample Mean	2.986	2.986	5.141	5.141	5.141
Sample SD	0.991	0.991	2.429	2.429	2.429

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal. Robust standard errors in parentheses.

Conclusion

I contribute to two lines of research. First, I join recent advances in the comparative study of affective polarization, which focus on the institutional causes and remedies of partisan hostility (Bassan-Nygate and Weiss 2022; Horne, Adams and Gidron 2022; Adams et al. 2022), and show that conscription can depolarize voters. Second, I contribute to a growing literature examining the effects of conscription and military service on a range of social and political outcomes (Bove, Di Leo and Giani 2022; Samii 2013; Horowitz and Stam 2014; Grossman, Manekin and

were only included in wave 5 of the CSES.

Miodownik 2015). Building on arguments regarding the potential of the military as a total institution to unite citizens from different social backgrounds (Moskos et al. 1988), and employing a rigorous identification strategy developed by (Bove, Di Leo and Giani 2022), I identify the effects of conscription on a timely and important outcome — partisan affective polarization.

Despite these contributions, my study is subject to two central limitations. First, though I theorize about a specific intergroup contact mechanism and provide suggestive evidence supporting its observable implications, I do not obtain a direct measure of respondents' contact in the military. Second, though I focus on a central validated measure of affective polarization in multiparty systems (Wagner 2021), I do not measure other (potentially correlated) measures of partisan animosity relating to social distance and discrimination. Building on the evidence from this study which spans fifteen European countries, future research can leverage richer country-specific surveys to improve the measurement of mechanisms and outcomes and further establish the link between conscription and affective polarization.

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Online Appendix

A Information on Conscription Reforms

The primary treatment of this study is exposure to conscription, which has been abolished in a host of European countries since World War II. To measure whether respondents' were subject to conscription in their country, I make use of data collected by (Bove, Di Leo and Giani 2022) (building on Toronto (2007)). These data provide information about the year in which conscription was abolished in each of the fifteen European countries under investigation.

I report conscription reform years, as well as country-specific conscription age and average service duration in Table A1. Using this information, as well as CSES respondents' self-reported age and year of birth, I create my running variable, which indicates whether a respondent was above or below the age of conscription when the draft was abolished in their country. I report descriptive statistics of my running variable, as well as all other variables employed in my analyses in Table C.

Table A1: Compulsory Military Service Reforms (Bove, Di Leo and Giani 2022)

Country	**End of Compulsory Service**	**Service Duration (Months)**	**Conscription Age**
Belgium	1995	5	18
Bulgaria	2008	15	18
Czech Republic	2005	12	18
France	2002	15	18
Germany	2012	6	17
Hungary	2005	21	18
Italy	2005	15	18
Netherlands	1997	16	17
Poland	2009	16	18
Portugal	2005	13	18
Slovakia	2006	4	18
Slovenia	2004	7	18
Spain	2002	9	18
Sweden	2011	7	18
Great Britain	1961	22	18

B Measurement and Descriptive Statistics

In Table A2 I report descriptive statistics of all variables employed in my main analyses and robustness checks. As noted in the main text, my outcome is based on measures of partisan like-dislike scores collected in all five waves of the CSES. The text for the like-dislike survey question asks respondents:

‘‘I’d like to know what you think about each of our political parties. After I read the name of a political party, please rate it on a scale from 0 to 10, where 0 means you strongly dislike that party and 10 means that you strongly like that party. If I come to a party you haven’t heard of or you feel you do not know enough about, just say so."

As described in the main text, my measure of polarization is based on respondents’ reported affect towards all parties in their country. I also use specific measures of respondents’ affect towards their most (least) liked political party in order to measure ingroup love and outgroup hate. In my exploration of mechanisms, I measure respondents’ ideology through a survey measure included in all waves of the CSES, which asked respondents:

‘‘In politics people sometimes talk of left and right. Where would you place yourself on a scale from 0 to 10 where 0 means the left and 10 means the right?"

To measure respondents’ support for democratic norms, I create an additive scale based on two survey items included in wave 5 of the CSES. These questions ask respondents:

- Do you strongly agree, somewhat agree, neither agree nor disagree, somewhat disagree, or strongly disagree with the following statement?)
  - What people call compromise in politics is really just selling out on one’s principles.
  - Having a strong leader in government is good for [Country] even if the leader bends the rules to get things done.

Table A2: Descriptive Statistics

Statistic	N	Mean	St. Dev.	Min	Max
Male	91,520	0.477	0.499	0	1
Religious Domination	91,520	3.195	4.568	0	13
Maritial Status	91,520	1.867	1.248	0	4
Race	91,520	0.126	0.357	0	6
Ethnicity	91,520	0.182	0.472	0	15
Urban	91,520	2.022	1.327	0	4
Education	91,520	2.968	1.196	0	5
Age	91,299	48.635	17.355	16	100
Running Variable	91,299	−26.169	21.061	−95	56
Affective Polarization (Main)	91,520	0.730	0.378	0.000	2.464
Affective Polarization (Alt)	91,520	1.550	0.839	0.000	4.407
Outparty Hate	91,520	0.113	0.270	0.000	4.927
Inparty Love	91,520	2.112	0.952	0.000	4.927
Ideology	79,141	5.097	2.437	0.000	10.000
Democratic Norms Index	15,408	2.973	0.983	1.000	5.000

C RD Diagnostics

In this section, I report several diagnostic tests to bolster confidence in my empirical strategy. First, in Figure A1, I report a McCray density test to rule out the possibility of sorting around the cutoff of my discontinuity. As one might expect in this context where the running variable is based on respondents' year of birth, the result reported visually in Figure A1, confirms that my running variable is continuous around the discontinuity.

Second, I consider the balance of several covariates around the discontinuity in Figure A2. Specifically, I examine whether, for my male sample, treated subjects (i.e., running variable is great than 0) are different from control subjects (i.e., running variable is smaller than 0) with regards to their rural/urban status, religion, race, marital status, and ethnicity. To do so, I estimate my main RD specification from the main text with covariates as outcomes.

Across all covariates, I cannot reject the null hypotheses of similarity with conventional levels of statistical significance. I interpret the small and imprecisely estimated point estimates in Figure A2 as evidence bolstering the credibility of my identification strategy. In other words, my

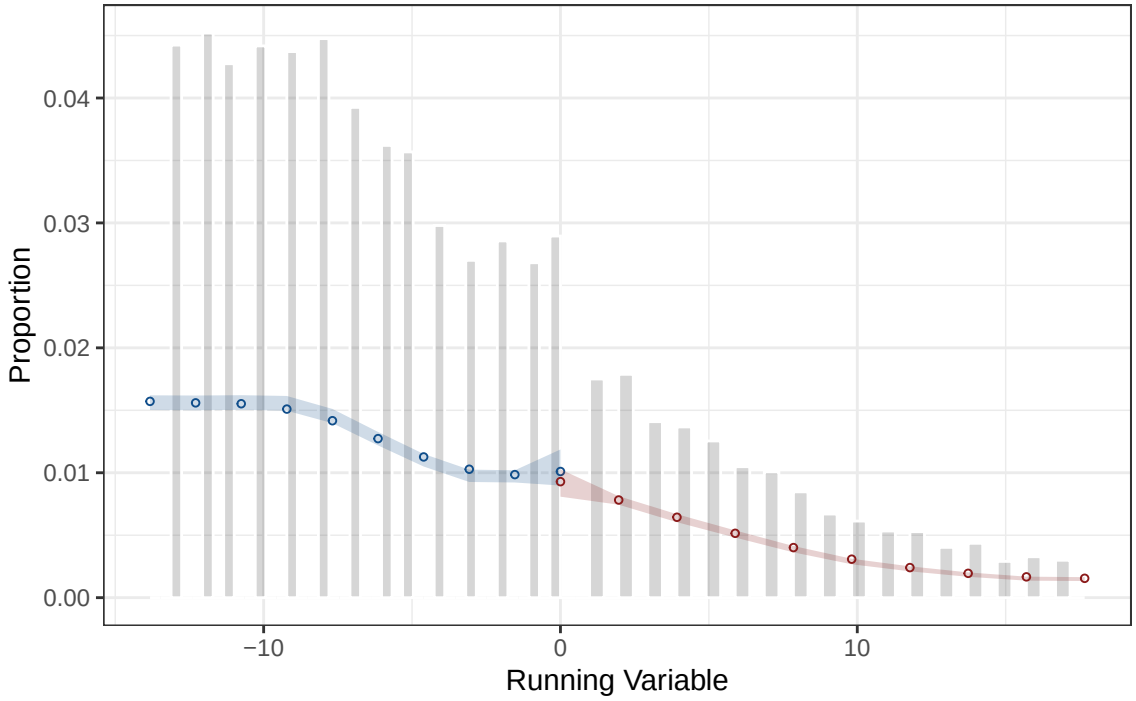

Figure A1: **McCrary Density Test** - This figure and its associated test suggest that there is no sorting around the cutoff of the running variable.

balance test substantiates the assumption that male respondents' just young enough to be exempt from military service are comparable to men who were just too old to be exempt from service.

Finally, I also consider a series of placebo tests reported in Figure A3. To do so, I estimate a series of regressions using my main specification but employing alternative cutoffs for my male and female samples. Notably, across 21 tests, employing alternative RD cutoffs for my male and female sample yields small and imprecise estimates on affective polarization. The only instance in which I reject the null hypothesis is when using my true cutoff ($x = 0$) on the male sample.

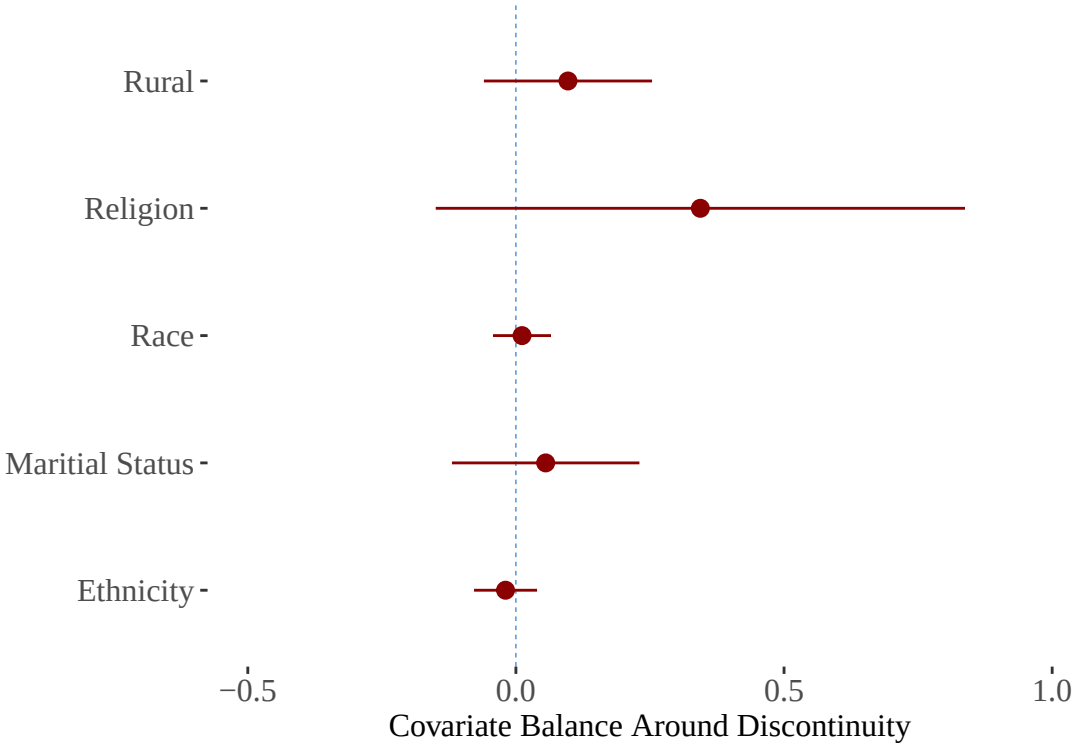

Figure A2: **Covariate Balance** - This figure demonstrates that key pre-treatment covariates are continuous around the discontinuity.

D Additional Analyses

In this section, I report additional analyses to examine the sensitivity of my main results to alternative modeling specifications and measurement approaches. First, in Figure A4, I demonstrate that my main results are robust to alternative bandwidth selection approaches. Though some approaches yield imprecise estimates, the point estimates I report in Figure A4, using a range of alternative bandwidth selection procedures, are for the most part, consistent with the results reported in the main text.

Second, in Figure A5, I consider the robustness of my main result to alternative polynomial specifications. Specifically, I report point estimates of my main specification, employing first, second, third, and fourth-order polynomials. The results reported in Figure A5 suggest that my main results are largely robust to alternative polynomial selections.

In Table A3, I consider the robustness of my findings to an alternative measure of affective

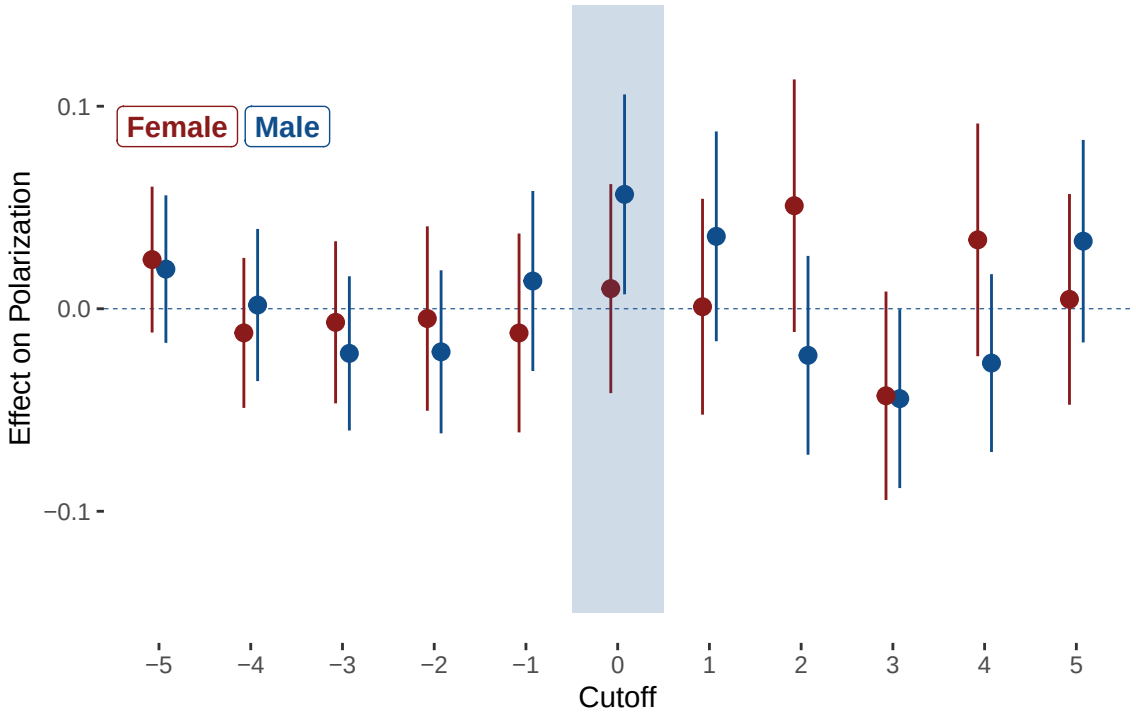

Figure A3: **Placebo Test** - This figure demonstrates no effect on affective polarization in alternative cutoffs and in the female sample. All models follow the main specification reported in the main text.

polarization proposed by Wagner (2021). My alternative measure—the mean distance measure—captures how much a respondent dislikes other parties compared to their own *most liked party*, rather than their average affect to all parties. To construct this measure, I follow equation A1. The primary difference between Equation A1 and Equation 1 in the main text relates to the value from which respondent i's affect towards party p is subtracted. In my primary measure—based on Wagner (2021)'s spread of scores indicator— I subtract respondent i's weighted affect towards party p from their average weighted affect towards all parties ($\overline{like}_i$). In my alternative measurement presented below, I subtract respondent i's weighted affect towards party p from their weighted affect towards their most liked party ($like_{max,i}$).

$$Polarization\ Distance_i = \sqrt{\frac{\sum_{p=1}^{p}(like_{ip} - like_{max,i})^2}{n_p}} \tag{A1}$$

Using this alternative outcome measure, I report additional analyses in Table A3. Like in the

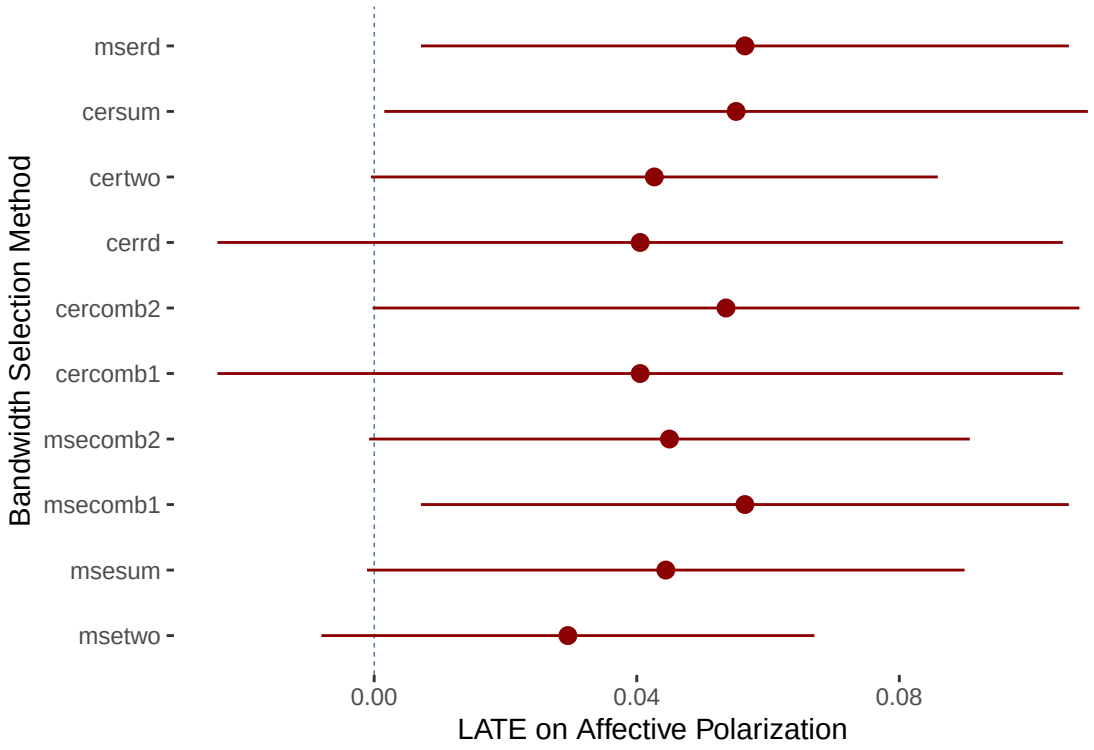

Figure A4: **Alternative Bandwidth Selection Method** - This figure demonstrates the robustness of the main LATE to alternative bandwidth selection methods.

main analyses, point estimates for the male sample are positive and relatively similar in magnitude (columns 1-3). However, for the male sample, point estimates are precisely estimated only when including country or country and wave controls. Similarly, along with the pattern of results in the main text, point estimates of all female models (columns 4-6) are small and imprecisely estimated. I interpret this pattern of results as realatively consistent with the main analyses reported in Table 1 of the main text.

In Tables A4-A5 I report female sample placebo models the complement the regressions reported in Tables 2-3 which investigate mechanisms focusing on male respondents. As expected, point estimates of these additional regressions are small and imprecisely estimated. Finally, in Figure A6, I report an alternative specification of my main model reported Table 1, omitting a single country at a time. Despite reductions in sample size, point estimates remain rather stable when omitting specific countries from my sample.

Table A3: RD Estimates – Effects of Conscription Abolition on Polarization (Alternative Measure)

	1	2	3	4	5	6
B	0.071	0.068	0.07	0.014	0.02	0.006
SE	(0.049)	(0.039)	(0.035)	(0.053)	(0.045)	(0.041)
P	0.145	0.081	0.046	0.785	0.651	0.879
Sample	Male	Male	Male	Female	Female	Female
Bandwith	8	11	12	7	8	9
Country FE	No	Yes	Yes	No	Yes	Yes
Wave FE	No	No	Yes	No	No	Yes
N	43546	43546	43546	47614	47614	47614
Sample Mean	1.53	1.53	1.53	1.567	1.567	1.567
Sample SD	0.821	0.821	0.821	0.854	0.854	0.854

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal.
Robust standard errors in parentheses.

Table A4: RD Estimates – Effects of Conscription Abolition on Ingroup Love and Outgroup Hate (Female Sample)

	Most Liked	Most Liked	Most Liked	Least Liked	Least Liked	Least Liked
B	0.01	0.018	0.004	0.001	0.009	0.005
SE	(0.065)	(0.051)	(0.043)	(0.015)	(0.013)	(0.014)
P	0.878	0.724	0.932	0.96	0.524	0.741
Sample	Female	Female	Female	Female	Female	Female
Bandwith	6	8	10	11	13	11
Country FE	No	Yes	Yes	No	Yes	Yes
Wave FE	No	No	Yes	No	No	Yes
N	47614	47614	47614	47614	47614	47614
Sample Mean	2.142	0.121	0.121	0.121	0.121	0.121
Sample SD	0.966	0.292	0.292	0.292	0.292	0.292

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal.
Robust standard errors in parentheses.

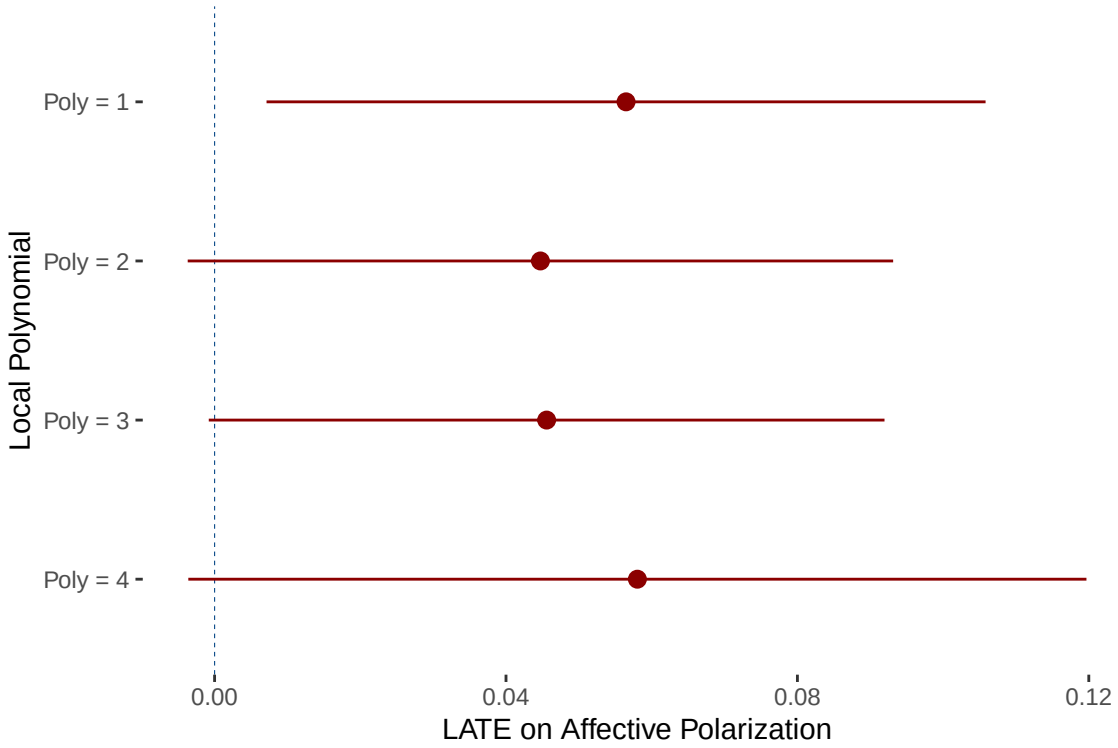

Figure A5: **Alternative Local Polynomials** - This figure demonstrates the robustness of the main LATE to alternative local polynomial selections.

Table A5: RD Estimates – Effects of Conscription Abolition on Democratic Norms and Ideology (Female Sample)

	Dem Index	Dem Index	Ideology	Ideology	Ideology
B	-0.047	-0.034	0.001	0.009	0.005
SE	(0.086)	(0.079)	(0.015)	(0.013)	(0.014)
P	0.584	0.663	0.96	0.524	0.741
Sample	Male	Male	Male	Male	Male
Bandwith	16	19	11	13	11
Country FE	No	Yes	No	No	No
Wave FE	No	No	No	No	No
N	7907	7907	47614	47614	47614
Sample Mean	2.956	2.956	5.053	5.053	5.053
Sample SD	0.97	0.97	2.445	2.445	2.445

Regression discontinuity models with MSE optimal, bandwidths, and a triangular kernal. Robust standard errors in parentheses.

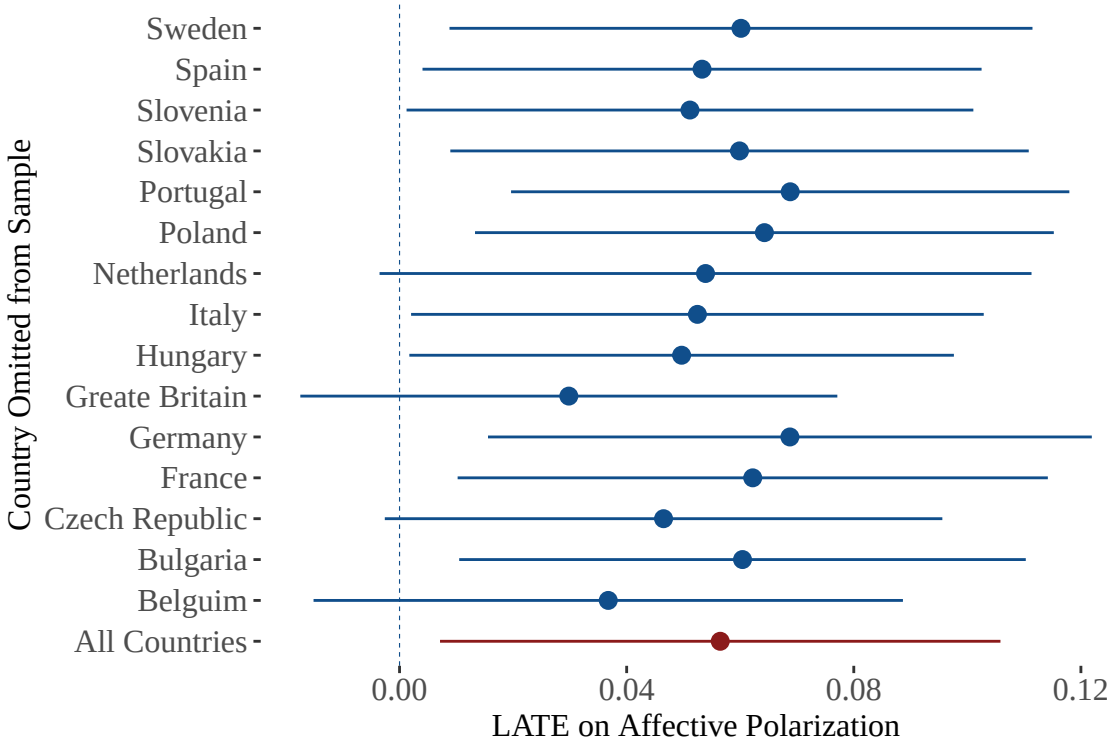

Figure A6: **Robustness to Country Deletion** - This figure demonstrates the stability of point estimates when omitting individual countries from the sample.