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Military service and political behavior: Evidence from France

Etienne Fize[a,b], Charles Louis-Sidois[a,c,*]

[a] Sciences Po, Department of Economics, 28 rue des Saints-Pères, Paris 75007, France
[b] French Council of Economic Analysis, 20 avenue de Ségur, Paris 75007, France
[c] University of Mannheim, Collaborative Research Center 884 "Political Economy of Reforms", B6, 30-32, Mannheim 68131, Germany

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ABSTRACT

We investigate the impact of compulsory military service on turnout and political preferences. Exploiting the suspension of mandatory conscription for French men, we observe a significant and positive impact of military service on turnout. We estimate that the service increases turnout by approximately 7 percentage points. We also investigate the impact of conscription on political preferences. When we control for selection into the military service, we observe no support for a change in preferences of former conscripts.

1. Introduction

"[...] re-establishing a compulsory national service is absolutely necessary, not only to teach citizens how to adapt to the [terrorist] threat, but also to strengthen national cohesion."

Emmanuel Macron, April 18, 2017

A renewed interest in national services has been observed the past few years. For instance, Lithuania and Ukraine reintroduced military service in 2015. In France, the implementation of a new form of national service was a campaign promise of current President Emmanuel Macron. One of the main arguments in favor of the reintroduction of national service is the shaping of civic and political preferences. "The [2015] Paris attacks were attributed in some parts to a lack of intermixing between social milieus, with republican values no longer being promoted throughout society by way of the armed forces. According to opinion polls, 60–80 % of respondents would support the reintroduction of the draft." (Bieri, 2015). The fundamental question of the effects of service on political behavior has often been raised in public debate but according to our review of the literature, no in-depth analysis has been conducted. We use the suspension of the military service in France to identify the impact of mandatory military service on turnout and political preferences.

Many other institutions share the same characteristics as the French service. Some countries have maintained conscription while relying on a professional army for military purposes, which was the case of the French service during the period we study because no conscript participated in a conflict. Moreover, the new forms of national service being considered (e.g., in France) are compulsory, which reinforces the interest in studying men-mandatory conscription. Our paper is the first to

* Corresponding author at: University of Mannheim, Collaborative Research Center 884 "Political Economy of Reforms", B6, 30-32, Mannheim 68131, Germany.
E-mail addresses: etienne.fize@sciencespo.fr (E. Fize), charles.louissidois@uni-mannheim.de (C. Louis-Sidois).

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formally investigate the political consequences of such a national experiment. A few papers have considered the impact of veteran status on political attitudes in the United States (US),[1] but French conscripts differ in several dimensions. In particular, US veterans directly participated in conflicts, and the results could be influenced by combat exposure. Moreover, the United States relies on an all-volunteer military, implying that veterans are from a specific subsample of the population.

In the first part of the paper, we study the impact of military service on turnout. We use data on turnout collected during the presidential and legislative elections in 2002, 2012 and 2017. Our identification strategy exploits the variation in compliance with the military service across cohorts. We collected data from the archives of the Ministry of Defense and computed the share of individuals who completed the service for each year-of-birth cohort born after 1952. The military service was compulsory for males born before 1979; thus, younger cohorts did not have to serve. The variation of the rate of compliance with military service is mostly driven by exogenous political decisions, namely, the suspension of mandatory conscription announced in 1996. The main challenge of the identification strategy is disentangling the effect of military service from other cohort effects. We propose two identification strategies.

The first approach is restricting the sample to males and controlling for a linear function of year of birth in addition to the share of compliance with the military service. Because our data were collected for 3 different years, we can also introduce a linear trend of age. We can, therefore, identify the effect of military service from age and cohort effects, but this strategy relies on the assumption that cohort dynamics can be captured by a linear trend.

The second strategy is a difference-in-differences specification where we use women as a control group. In France, military service was only mandatory for males, and the rate of compliance for women is essentially zero. In this specification, we control for year-of-birth fixed effects to identify the impact of the rate of service from other generation effects. This method is more flexible and assumes that cohort effects do not differ across sexes.

With those two methods, we observe that a cohort where all individuals have complied with service would exhibit a higher turnout than a cohort where no one has. The point estimate of this impact is approximately 7 percentage points and roughly similar across elections. More precisely, we estimate the impact for the two rounds of the presidential elections of 2002, 2012, and 2017 and for the two rounds of the legislative elections for the same years. All estimations reveal an effect between 5 and 9 percentage points. Moreover, the results obtained with the two methods have the same magnitude; thus, we correctly identify the effect of the military service from other generation effects. To confirm our results, we propose multiple robustness checks including a placebo analysis where we specify another date for the suspension of the military service.

The second part of the paper investigates the effect of conscription on political preferences. We use a survey conducted in 2017 in France where we introduced a question on military service to identify former conscripts and observe that those individuals report more positive attitudes toward the traditional right-wing party, Les Républicains, and toward the Front National, the main national-populist party. This finding is confirmed by a series of surveys conducted between 1988 and 1991, where we observe that former conscripts are significantly more likely to report nationalist and conservative political preferences. However, we demonstrate that this correlation is at least partly driven by the selection into military service. Using different methods to account for the selection bias, we observe that the effect of military service on political preferences is no longer significant. In particular, we use the aggregate share of military service as an instrument for the individual service variable.

1.1. Literature review

Starting with Angrist (1990), a large body of literature has studied the impact of veteran status in the US on education and earnings. Using the natural experiment of the draft lottery during the Vietnam war, he demonstrates that veterans earn 15% less than comparable nonveterans. The same impact has been found in Angrist and Krueger, 1994 and, for the Netherlands, by Imbens and Van Der Klaauw (1995). Angrist et al., 2011 and Grenet et al. (2011) and demonstrated that this result erodes over time, suggesting that the earning gap is mainly due to reduced work experience. By contrast, in countries with lower education performance, military service appears to be a partial substitute for education. Service increases wages, at least for the least educated (see e.g. Card and Cardoso, 2012 for Portugal and Torun and Tumen, 2016 for Turkey).

A few papers have studied the link between military experience and other outcomes. For instance, Galiani et al. (2011) study the impact of military service on crime in Argentina and find it significantly increases post-service crime, which questions the civic virtues of conscription. Hjalmarsson and Lindquist, 2019 find the same positive relation in Sweden.

More similar to our topic, a few papers have investigated the impact of veteran status on political behavior. This literature has focused on professional soldiers in the United States and not on mandatory peace-time military service. Teigen (2006) and Leal and Teigen (2018) have investigated the impact of veteran status on voting turnout and find a positive impact in the United States. This result can be explained by the willingness of veterans to affect the policy in their favor and also by candidacies of other military veterans such as US Senator John McCain. Nesbit and Reingold (2011) demonstrate that veterans are more likely to volunteer in associations. Nevertheless, Bishin and Incantalupo (2008) find that veterans do not vote cohesively. Regarding political preferences, a higher likelihood of being drafted during the Vietnam War results

[1] See, for instance, Teigen (2006) and Leal and Teigen (2018).

in preferences that are more antiwar and liberal, according to Erikson and Stoker (2011), and increases the probability of voting for the Democrats. In France, Rouban, 2007,2013 documents that professional militaries are more conservative and more likely to support the right and far-right parties. Those aforementioned two studies are mostly descriptive: they do not account for selection bias and do not aim to identify a causal impact of the professional military status on political preferences.

Our paper is also related to the literature on voting and political participation. Following the seminal work of Wolfinger and Rosenstone (1980), a large body of literature has studied the socioeconomic characteristics of voting. In an updated survey, Leighley and Nagler (2013) explain the decision of voting by individual characteristics including race, education, income, sex, age, and marital status. Due to our identification strategy, the impact of gender is central in our study. Most of the literature, including Niel and Lincot, 2012 for France, Coffé and Bolzendahl (2010) for several Western democracies including France, Norris (2002), and Carreras and Castañeda-Angarita (2014), has concluded that women are more likely to vote than men but less likely to engage in political activities, such as volunteering with political parties. This pattern has been documented in most advanced democracies.

We also contribute to the literature on the effects of the collective experience of voting. For instance, Madestam and Yanagizawa-Drott (2012) study the impact of attendance to Fourth of July celebrations in the United States. Using rainfall on July 4, as an instrument, they predict the number of celebrations attended in childhood and find that attending one additional celebration increases turnout by 0.88 percentage points at age 39. Moreover, they demonstrate that attending those celebrations increases the likelihood of voting for Republicans. Using discontinuities in the timing of the introduction of television in the United States, Gentzkow (2006) demonstrates that television led to a gradual decrease in turnout. Similarly, Falck et al. (2014) reveal that internet availability decreases turnout in Germany but find no evidence that it benefits specific parties. Other studies such as DellaVigna et al. (2016) or Gerber and Green (2000) have considered the impact of social pressure on voting behavior. For instance, Gerber and Green (2000) report an increase in turnout of 9 percentage points when individuals are contacted in person by a canvasser who reminds them to vote.

In the next section, we present the context and the data we use. We then discuss our main results: Section 3 analyzes the impact of military service on turnout, and Section 4 investigates the effects on political preferences. Section 5 concludes.

2. Context and data

In this section, we first briefly summarize the history of military service in France. We then describe the data on military service and discuss the selection process. Section 2.3 provides a description of French elections. Finally, we present the data on turnout and political preferences.

2.1. The French military service

Conscription has existed in France since 1798. Over the years, the form and the length of military service have fluctuated depending on the needs of the army. In our study, we focus on individuals born in 1952 or after, and no individual has complied with service before 1970. For the earliest cohorts we consider, the duration of service is 12 months, and the duration was shortened to 10 months in 1992. Nuclear weapons reduced the need for conscripts, and nonprofessional militaries were eventually a burden in a modern army. During the Gulf War, conscripts were not directly involved in the conflict. For the opponents to military service, this institution was costly and obsolete due to the evolution of warfare. President Jacques Chirac finally announced the suspension of military service in 1996. In October 1997, young men born after 1978 were officially released from military duties. Young men born before this date still had the obligation to do their service and did so up to 2001, although the rate of compliance is lower for the last cohorts.

However, the suspension of military service was far from a consensual decision. Individuals in favor of conscription, regardless of political affiliation, claimed it was a stepping stone in politics and civic education. Since the suspension, the reintroduction of national service has often been discussed, for instance, it was a campaign promise of current President Emmanuel Macron. Those projects are often less military oriented: other forms of national services, such as civic service, are considered.

2.2. Who are the conscripts?

We collected data from the archives of the office of the French Ministry of Defense in charge of military service (Direction du service national), including detailed yearly information on men who completed military service. We collected data on the age of conscripts from 1970 to 2001. We merged these data with the census to determine the share of each birth cohort that complied. Thus, we could compute the probability of compliance for males born in 1952 and after (Fig. 1).

For cohorts born before 1973, the share of men who completed their service is approximately 70%. This share declines for cohorts born in 1974 and later. After the announcement of the suspension of military service, the number of conscripts

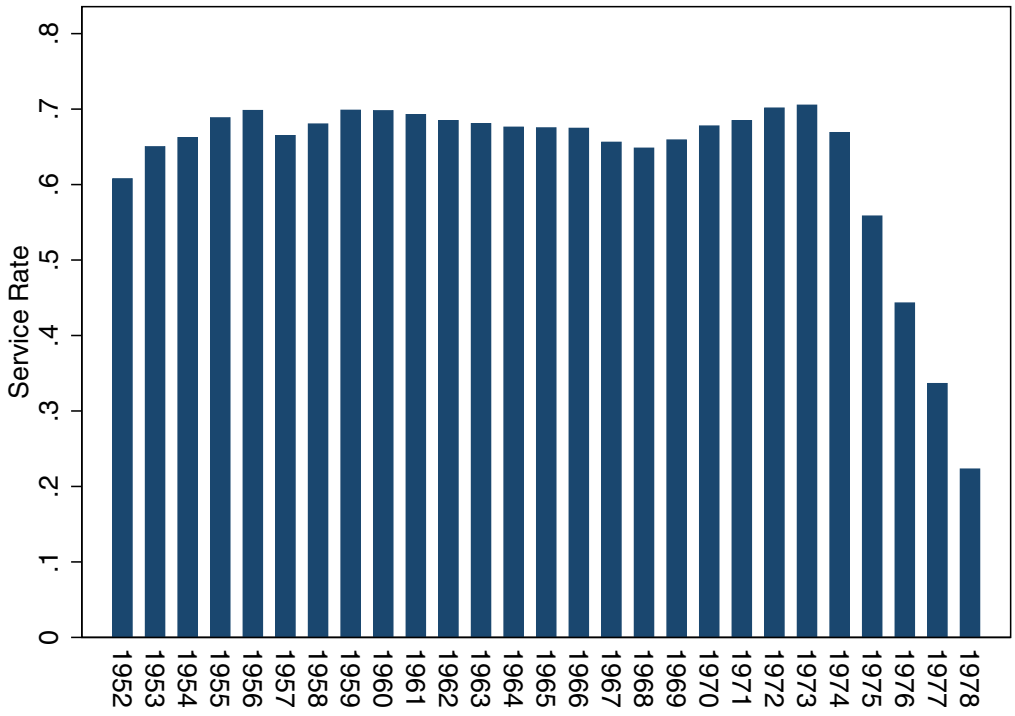

Fig. 1. Probability of compliance by birth cohort.

decreases although individuals born before 1979 theoretically had to serve. Finally, cohorts born in 1979 and after were not treated[2]

In principle, military service was mandatory for all French men. Nevertheless, young men could avoid military service for two reasons. First, medical exemptions were given to more than one fifth of each cohort. Before incorporation, young men were submitted to a medical examination and given six grades based on a set of criteria[3] The government set a threshold for each criterion, and men who had at least one grade below the threshold were exempted. This procedure allowed the Ministry to raise the threshold to reduce the number of incorporations when the army required fewer conscripts or for too-numerous cohorts.

Moreover, between 1972 and 1985, approximately 7% of conscripts were dispensated and not required to complete their military service.[4] The large majority of dispensations (more than 6% of a cohort) were granted to conscripts who provided the main financial support for their family. Another criterion for dispensation was double citizenship, in such a case, individuals could choose the country where they wanted to serve. Those dispensations applied to less than 1% of the population.[5] Other criteria, for example, living abroad or having a parent who died for the country, are used for exemption, but those cases represented, respectively, 1/1000 and 1/10,000 of the population.

In this paper, we jointly consider all forms of service. We discuss the allocation of conscripts in the supplementary material available online. Service conditions varied with conscripts' assignment, and we expect the effects of the service to depend on allocation. Unfortunately, our data do not allow us to disentangle the effects because individual assignments are not available.

Finally, we discuss early discharges. To compute the service rates, we considered the number of men who joined the army. However, some conscripts were discharged before the expected termination date due to a change in their personal situation. We only have information on it between 1975 and 1980. On average, 7% of conscripts did not complete their service. More precisely, 5% of conscripts were discharged less than 3 months after incorporation and an additional 2% after 3 months. Notably, early discharges cannot discard the results of our paper because the service rates computed in Fig. 1 overestimate the number of individuals who completed their military service. If we assume that the effect of conscription is increasing in the duration of the service, the effects we estimate in the following sections should be biased toward zero.

[2] Voluntary enrollment in the military is beyond the scope of our paper, and we do not consider it. We do not consider voluntary civic service either because the rate of enrollment is negligible for the period we consider.

[3] The "SIGYCOP" grading system (upper body, lower body, general state, eyes/vision, color blindness, hearing, and mental health).

[4] We have comprehensive data on dispensations between 1972 and 1985 and from 1996 to 2000.

[5] For example, the case of some French–Algerian citizens who served in Algeria. However, military service was often longer in other countries (e.g., 24 months in Algeria until 2002), and most men preferred to serve in France.

Table 1
French elections.

Date	Round	Turnout (%)	Main candidates with vote share	Allowed to vote
Presidential elections				
04/21/2002	1	71.60	Jacques Chirac (UMP) 19.88 Jean-Marie Le Pen (FN) 16.86 Lionel Jospin (PS) 16.18	04/20/1984
05/05/2002	2	79.71	Jacques Chirac (UMP) 82.21 Jean-Marie Le Pen (FN) 17.79	05/04/1984
04/22/2012	1	79.48	François Hollande (PS) 28.63 Nicolas Sarkozy (UMP) 27.18 Marine Le Pen (FN) 17.90 Jean-Luc Mélenchon (FG) 11.10	04/21/1994
05/06/2012	2	80.35	François Hollande (PS) 51.6 Nicolas Sarkozy (UMP) 48.36	05/05/1994
04/23/2017	1	77.77	Emmanuel Macron (EM) 24.01 Marine Le Pen (FN) 21.30 François Fillon (LR) 20.01 Jean-Luc Mélenchon (FI) 19.58	04/22/1999
05/07/2017	2	74.56	Emmanuel Macron (EM) 66.10 Marine Le Pen (FN) 33.90	05/06/1999
Date	Round	Turnout		
Legislative elections				
06/09/2002	1	64.41		
06/16/2002	2	60.31		
06/10/2012	1	57.22		
06/17/2012	2	55.40		
06/11/2017	1	48.71		
06/18/2017	2	42.64		

Note: Column 4 reports the candidates who received more than 10% of the vote and their vote share. Political parties are indicated between parentheses: UMP/LR (right wing), PS (left wing), FN (extreme right), FG/FI (extreme left), EM (center). Individuals born before the date reported in the last column are allowed to vote.

2.3. Elections

For our analysis, we focus on the presidential and legislative elections in France in 2002, 2012, and 2017. For all elections, French citizens aged more than 18 at the time of the election can vote. We summarized the main information of each election covered in our data, including date, turnout, and the candidates with the largest vote shares for the presidential elections (Table 1). Legislative and presidential elections use runoff electoral systems, and such elections are theoretically analyzed in Bouton (2013).

The president has the most critical role in the French Vth Republic, and the presidential election is the main event of the political cycle. Turnout is, thus, much higher than for other elections. For example, 80% of the French electorate voted in 2012 compared with 50% for local elections in 2015 ("départementales" and "régionales"). The two candidates who gather the greatest number of votes in the first round qualify for the runoff.

Legislative elections have two rounds and occur a few weeks after the presidential election. Each of the 577 constituencies elects one legislator who later sits in the lower chamber of the Parliament. The outcome of these elections determines the political orientation of the government. Turnout is, however, much lower for legislative elections than for the presidential election. If one candidate receives more than 50% of votes in the first round, she or he is directly elected. In those cases, no second round is held. In 2012, this case was observed in 36 districts. Otherwise, candidates supported by more than 12.5% of registered voters compete in a runoff, and the candidate who receives the greatest vote share is elected.

We had to exclude the presidential and legislative elections of 2007 from the analysis because the database does not include age or year of birth. Therefore, we could not infer who was impacted by military service.

Finally, we could not extend our analysis to other elections because individual data on turnout are not collected for local elections and the access to voting records is permitted only within 10 days of the election.

2.4. Turnout

Our data on turnout are from the datasets "Study on Electoral Participation" collected by the French Statistical Institution (INSEE) in 2002, 2012, and 2017.[6] For each study, a representative sample of 40,000 individuals is drawn from the census. The sample is renewed for each wave. We merged the data from the three bases and removed the individuals born before 1952 because we could not compute their probability of compliance with service. The sample collected was 82,266 individuals[7] (in the first part of the analysis, the number of observations is much smaller because we aggregated the data at the cohort level). Our data include information on individual characteristics from the census (e.g., sex, age, education, occupation, marital status) but not information related to military service. For each individual in the survey, the INSEE directly collected turnout in the voting records. Our data are therefore not biased by misreporting issues (see Harbaugh (1996) for a discussion on misreporting of political participation). Table 2 presents a summary of the variables we used in the analysis of turnout.

[6] INSEE (2002, 2012, 2017).

[7] We have 78,086 observations for the second round of the legislative elections because some candidates are directly elected in the first round.

Table 2
Descriptive statistics, turnout.

	Mean	S.d.	Obs.
Service rate			
Source: military statistics, "Direction du Service national"			
Probability of service, men only	0.44	0.31	40,600
Turnout			
Source: "Study on Electoral Participation", INSEE (2002, 2012, 2017)			
Presidential 1st round	0.79	0.41	82,266
Presidential 2nd round	0.80	0.40	82,266
Legislative 1st round	0.54	0.50	82,266
Legislative 2nd round	0.48	0.50	78,086
Individual controls available for all elections			
Source: "Study on Electoral Participation", INSEE (2002, 2012, 2017)			
Male	0.49	0.50	82,266
Year of birth	1971.59	12.33	82,266
Age	39.59	12.74	82,266
Region of origin: 27 dummy variables.			82,266
Individual controls available for 2012 and 2017			
Source: "Study on Electoral Participation", INSEE (2012, 2017)			
Level of education			
Highest diploma, 11 categories			59,251
Occupation			
Occupational dummies, 8 categories			59,261
Marital status			
Single, married, divorced or widow			59,261
Geographic controls			
Size of the city of residence, 9 categories			59,242

2.5. Political preferences

Very few surveys have included both data on political preferences and on military service. After the suspension, all sources collecting data on political behavior stopped including questions on military service. Therefore, we introduced a question on this topic in the French Electoral Survey conducted in 2017 by Gougou and Sauger (2017). This survey comprised 1830 face-to-face interviews and included a wide range of questions related to political preferences. Approximately one fourth of all respondents had been conscripts. We report the translation of all questions in the supplementary material available online.

We summarize the variables used for the analysis of political preferences in Table 3.[8]

3. Military service and turnout

In this section, we demonstrate that military service has a large and positive impact on turnout. Fig. 2 provides a graphical intuition of the main result of the paper. We pooled our three INSEE databases on turnout and computed the average participation of each individual for a given election year. As a result, our variable takes a range of values from 0 for individuals who abstained from all elections to 1 if they participated in the four elections[9] Next, we plotted the average of this variable by sex and birth cohort (Fig. 2). The red vertical line in Fig. 2 represents the last cohort for which men had to fulfill military obligations (1978 cohort). The greyed-out region indicates the cohorts from 1975 to 1978 for which we observed a graduate decrease in compliance in Fig. 1. Women, by contrast, had no mandatory military obligations to fulfill.

We observe no systematic difference in turnout between men and women that for cohorts born before 1975. At that time, men had to complete military service and approximately 70% of a birth cohort eventually did so. In the greyed-out region, where the service rate starts to decrease, the gender gap becomes more visible and males tend to participate less than women. For the later cohorts, not affected by military service, we observe a widening of the gap, indicating that men's turnout is much lower than for women of the same cohort. Moreover, this finding is not influenced by different age trends across sexes: in Fig. 2(a)–(d), in the supplementary material available online, we perform the same exercise on the 3 election

[8] Unlike the INSEE data on turnout, this information is based on a survey and is subject to misreporting. For example, the average reported turnout for the first round of the presidential election (85%) is larger than the official measurement of turnout (78%).

[9] We drop individuals who live in districts where a candidate was elected in the first round of the legislative elections to maintain only those who could vote four times. In the supplementary material available online, we observe the same pattern when we maintain the whole sample and exclude the second round of the legislative elections.

Table 3
Descriptive statistics, political preferences.

	Mean	S.d.	Obs.
Source: "French Electoral Survey", CEE 2017			
Political preferences			
Reported preferences on a scale from 1 (negative) to 10 (positive)			
Front National (FN)	2.27	3.31	1758
Les Republicains (REP)	3.49	2.81	1714
En Marche (LRM)	4.50	2.96	1730
Europe Ecologie Les Verts (ELV)	4.13	2.74	1725
Parti Socialiste (PS)	3.83	2.68	1723
France Insoumise (FI)	4.15	3.25	1710
(We sort political parties from right to left.)			
Turnout			
Presidential 1st round	0.85	0.36	1820
Military service			
Service, men only	0.51	0.50	853
(1 if respondent did the national service)			
Individual controls			
Age	50	18.82	1820
Male	0.47	0.50	1820
Net individual income (euros/months)	1365	1244	1820
At least one foreign parent	0.17	0.38	1820
Parental political preferences			
Reported on a 0–4 scale, high numbers indicate right-wing preferences			
Father politics	1.90	1.16	1228
Mother politics	1.88	1.08	1172

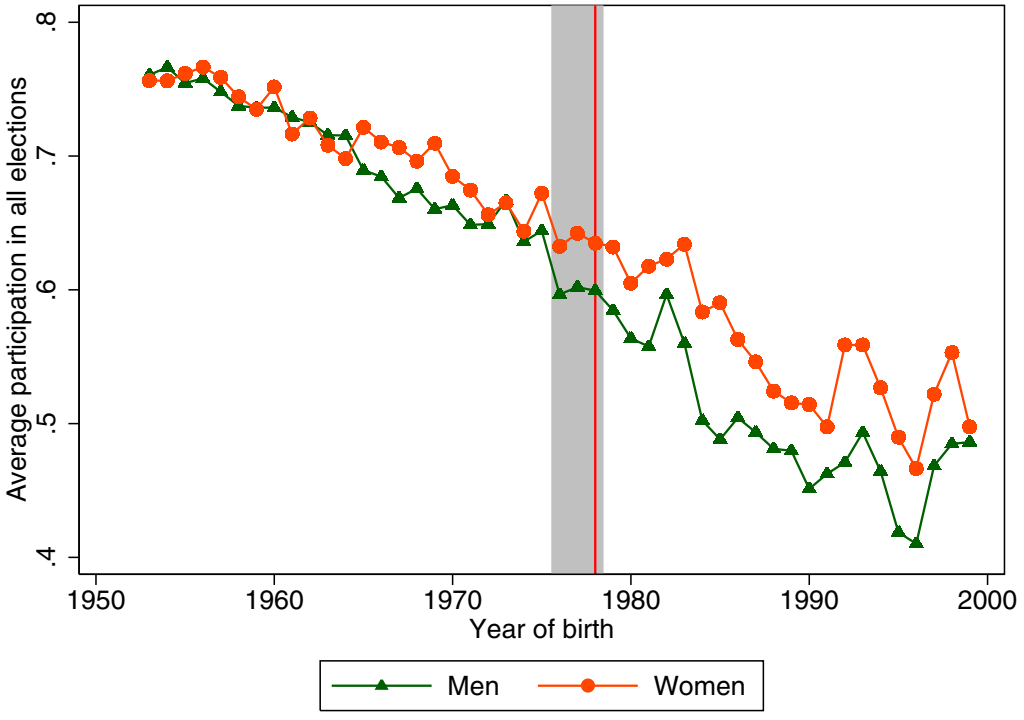

Fig. 2. Average turnout by birth cohort. The variable on the*x*-axis is the year of birth. The *y*-axis is the average number of votes cast by sex and by cohort. We only include individuals who could vote 4 times, we display the results for all individuals in (a) in the supplementary material available online.

years separately and observe a gender gap in turnout between the 1975 and the 1978 cohorts, demonstrating this pattern is influenced by a generation effect and not by age.

Our explanation for Fig. 2 is that women are on average more likely to vote (Carreras and Castañeda-Angarita, 2014; Coffé and Bolzendahl, 2010; Norris, 2002; Niel and Lincot, 2012) and that military service had a positive impact on men's turnout that compensated for this gender bias. In the following subsections, we test this graphical intuition formally.

Table 4
Main results.

	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
	Presidential		Legislative						
	First	Second	First	Second	Turnout	Turnout	Turnout	Turnout	Turnout
Service Rate	0.0543***	0.0859***	0.0759***	0.0625***	0.0691***	0.0397**		0.0413***	0.0384**
	(0.0195)	(0.0239)	(0.0228)	(0.0230)	(0.0201)	(0.0173)		(0.0130)	(0.0153)
Intention to Treat							0.0246***		
							(0.0079)		
Sample	Men	Men	Men	Men	Men	All	All	All	No 75–78
Age	Linear	Linear	Linear	Linear	Linear	Sex linear	Sex FE	Sex FE	Sex FE
Cohort	Linear	Linear	Linear	Linear	Linear	Cohort FE	Cohort FE	Cohort FE	Cohort FE
Cluster	Cohort	Cohort	Cohort	Cohort	Cohort	Cohort Sex	Cohort Sex	Cohort Sex	Cohort Sex
R2	0.79	0.78	0.92	0.91	0.89	0.92	0.97	0.98	0.99
Observations	123	123	123	123	123	246	495	236	212

Note: We estimate with an OLS Model (1): $\bar{Y}_{ct} = \alpha + \beta\bar{S}_c + \delta_1 Age_{ct} + \delta_2 Cohort + \delta_3 Election\_Year_{fe} + \epsilon_{ct}$ in Columns 1–5. In Column 6, we test Model (2) $\bar{Y}_{sct} = \alpha + \beta\bar{S}_{sc} + \delta_1 Age_{ct} + \delta_2 Male \times Age + \delta_3 Cohort_{fe} + \delta_4 Male + \delta_5 Election\_Year_{fe} + \epsilon_{sct}$ and in Column 7, we estimate a variation of Model (2) where we replace $\bar{S}_{sc}$ by the intention to treat, equal to 1 for male cohorts born before 1979 and 0 otherwise. The number of observations is larger in this specification because we do not use the service rates and keep the cohorts born before 1952. In the last two columns, we test Model (3) $\bar{Y}_{sct} = \alpha + \beta\bar{S}_{sc} + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \epsilon_{sct}$. The dependent variable is the average turnout by cohort, sex and election year. In the first four columns, we consider the four types of elections separately. In the next columns, Turnout is the average participation for the four elections of one given year. We drop individuals who live in districts where no second round was held for the legislative elections to compute the average. Service Rate is the proportion of males who did their military service per birth cohort, and the variable takes the value of 0 for women. In the last column, we exclude cohorts born between 1975 and 1978. Standard errors are in parentheses.

3.1. Estimating the effect of military service on turnout

The main challenge of the identification strategy is to control for cohort effects. The pattern that we observe in Fig. 2 suggests that male cohorts born before and after the suspension of military service exhibit different turnout rates. We must isolate the effect of this policy from potential confounding generation effects. We follow the methodology of Bedard and Deschênes (2006) who propose two approaches. The first approach comprises restricting the sample to males and introducing a smooth cohort trend. This first specification relies on the assumption that the effect of year of birth can be captured by a linear trend. The second approach is a difference- in-differences specification where women are a control group. In this case, we introduce year-of-birth fixed effects to control for cohort effects. This specification does not impose a specific trend in year of birth but assumes that cohort effects are the same for men and women.

We begin the analysis with the first identification strategy where we focus on males in the INSEE databases on turnout. We consider the average turnout computed at the cohort level for each election year (2002, 2012, and 2017) and estimate the following specification:

$$\bar{Y}_{ct} = \alpha + \beta\bar{S}_c + \delta_1 Age_{ct} + \delta_2 Cohort + \delta_3 Election\_Year_{fe} + \epsilon_{ct}. \quad (1)$$

In Model 1, the dependent variable is the average turnout of males in birth cohort $c \in \{1952, \ldots, 1999\}$ for election year $t \in \{2002, 2012, 2017\}$. In this aggregated approach, the parameter of interest β is the effect of the service rate $\bar{S}_c$ computed for each male cohort with the data of the army (Fig. 1). We control for cohort effects with a smooth function of year of birth. We also add a linear trend in age, which is identified because we have observations for three election years and election year fixed effects. In this specification, the cohort trend captures the changes in cohort characteristics (observable and unobservable) such as education or income.

The validity of this approach relies on the exogeneity of $\bar{S}_c$. Indeed, the change in compliance rate across cohorts is influenced by exogenous political decisions. Those decisions could only have affected turnout through the change in the rate of participation in military service. The main variation in rate of compliance is due to the suspension of military service, which led to a gradual decrease in the rate of conscription for cohorts born between 1975 and 1978. Before this date, the share of young men who entered conscription each year depended on the needs of the army and on the number of males in that cohort. The Ministry of Defense adjusted the medical criteria required to enter the army to regulate the number of admissions.[10] This process was eventually used to match the number of incorporations with the human and financial requirements. As a result, we considered the variation in the aggregate share of enrollment in military service exogenous.

We report the estimation of Model 1 in Table 4. In the first four columns, we consider separately the different elections. For instance, the dependent variable in the first column is the average turnout for the first round of the presidential elections computed at the cohort level for the 3 election years. For all elections, the coefficient of the service rate is positive and significant at the 1% level. Moreover, the point estimates are of the same magnitude and range from 5.4 to 8.6 percentage

[10] A report of the Army (*Rapport sur les conditions d'exécution du service militaire, Ministère de la Défense,1989*) stated that "exemptions were used to get rid of unfit individuals, but also, in case of excess human resource, adjust the supply to meet the budget constraint." The same report claims that such adjustments were performed at least in 1975, 1978, and 1983.

points for the first and second round of the presidential elections, respectively. On average, we infer that a cohort where all individuals have been conscripted would exhibit a turnout rate 7% higher than a cohort where no one has served.

In Column 5, the dependent variable is the average participation of a cohort for an election year and can take values between 0 and 1. Unsurprisingly, we observe that doing the service increases turnout by 7 percentage points. Because the effect of service is roughly similar across elections, we focus on the average turnout for the remainder of this section.

The specification proposed in (1) assumes a linear trend in cohorts and focuses on males. Next, we discuss the second approach proposed in Bedard and Deschênes (2006), a difference-in-differences strategy that exploits the data on women to flexibly control for cohort effects:

$$\bar{Y}_{sct} = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_{ct} + \delta_2 Male \times Age + \delta_3 Cohort_{fe} + \delta_4 Male + \delta_5 Election\_Year_{fe} + \epsilon_{sct}. \quad (2)$$

The dependent variable is the average turnout computed by sex $s \in \{Male, Female\}$ and cohort $c \in \{1952, \ldots, 1999\}$ for each election year $t \in \{2002, 2012, 2017\}$. We control for a linear trend of age that we also interact with a male dummy. This interaction allows the effect of age to differ across sexes. This specification formally tests whether the widening of the gender gap in turnout is explained by age affecting men and women differently. Moreover, we now introduce cohort fixed effects defined at the year-of-birth level. Cohort effects do not vary across sexes and capture the potentially confounding generation effects. The implicit assumption is that cohort effects impact males and females similarly. Finally, because age, cohort, and election years are related, we introduce only one election year fixed effect to avoid collinearity.

This specification is a difference-in-differences with women as a control group. The variable *Male* is a treatment-group dummy because only this group was affected by military service. Cohort effects capture the information pre- and post-treatment because the treated cohorts are $c \leq 1978$. Finally, the service rate $\bar{S}_{sc}$ is equal to 0 for women and, thus, indicates which cohorts have received the treatment. It, therefore, captures the interaction between Treatment $\times$ Treated, and the estimate of β in (2) is the treatment effect in a difference-in-differences model with women as a control group.[11] In our main specification, $\bar{S}_{sc}$ are the service rates reported in Fig. 1 for males and $\bar{S}_{sc} = 0$ for females. We also test a simplified version where we consider the intention to treat, namely, where $\bar{S}_{sc} = 1$ for male cohorts born in 1978 or before and 0 otherwise.

We estimate Model 2 in Column 6 of Table 4. The effect of military service on turnout remains positive and significant at the 5% level, although smaller than that in Column 5. The estimation of this specification by election (not reported) provides positive and significant estimates for the two rounds of the presidential elections and the second round of the legislative elections. The point estimate is positive for the second round of the legislative elections but not significant.

In Column 7, we estimate an alternative version of Model 2 where $\bar{S}_{sc}$ is the intention to treat instead of actual service probabilities ($\bar{S}_{sc} = 1$ for males born in 1978 or before and 0 otherwise). The result is essentially similar. The point estimate is slightly smaller because the intention to treat specification assumes that all males born before the suspension of military service were treated, although some did not serve.

We estimate the following refinement of Model 2 in Column 8 of Table 4:

$$\bar{Y}_{sct} = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \epsilon_{sct}. \quad (3)$$

This is more demanding than Model 2 because we introduce age by sex fixed effects instead of assuming linear trends. Since we have data for three election years, we can simultaneously introduce those effects and cohort fixed effects. Consequently, we need to drop the male variable which is now captured by the age by sex dummies. As in (2), we also have to drop an election year dummy. In Column 8, we estimate Model 3 and find an effect similar to what we estimated in Column 6. This effect is even significant at the 1% level.

Finally, one could be worried about the gradual decrease in the service rates observed in Fig. 1 for the cohorts born between 1975 and 1978. The lower conscription rates could induce two possible issues: first, there could be a selection bias if the last conscripts exhibit specific characteristics that affect the impact of the military service on their political participation. Second, conscripts' affectations changed after the announcement of the suspension and the last cohorts served in different conditions, as documented in the supplementary material available online. For instance, at the end of the 1990's, the share of nonmilitary oriented services reached 15% while it was below 5% before 1990. In the last column of Table 4, we estimate Model 3 excluding the cohorts born between 1975 and 1978. The estimate is very similar to what we found in Column 6 and still significant at the 5% level, which shows that the results are not sensible to the evolution of the military service during the last years before the suspension.

3.2. Robustness checks

We now propose a series of robustness checks to validate our result on turnout. We first test the parallel trend assumption. Moreover, we perform a placebo analysis when we vary the date of the suspension of military service. Finally, we move from the aggregated approach and use the individual-level data to account for the change in the composition of cohorts.

Because our identification strategy is equivalent to a difference-in-differences, we test the parallel trend assumption and report the results in Table 5. In our analysis, women can be considered a valid control group if the cohort trends do not

[11] Imbens and Van Der Klaauw (1995) and Bedard and Deschênes (2006) have demonstrated that Model 2 is also equivalent to the instrumental variable specification in the individual-level model where the service rates by birth cohorts are used as an instrument for individual service compliance (that we do not observe). A complete demonstration is in Imbens and Van Der Klaauw (1995).

Table 5
Test of parallel trend assumption.

	(1) Turnout	(2) Turnout	(3) Turnout
Cohort	−0.0040***	−0.0064***	−0.0060***
	(0.0011)	(0.0016)	(0.0004)
Male × cohort	−0.0010	−0.0001	−0.0017***
	(0.0012)	(0.0022)	(0.0005)
Sample	Pre-1975	Post-1979	All
R2	0.43	0.42	0.76
Observations	138	78	246

We estimate with an OLS $\bar{Y}_{sct} = \alpha + \delta_1 Cohort + \delta_2 Male + \delta_3 Male \times Cohort + \delta_4 Election\_Year_{fe} + \epsilon_{sct}$. The dependent variable is the average turnout by cohort, sex and election year. *Cohort* is a linear effect of year of birth. We also interact this trend with *Male*. Standard errors are in parentheses.

differ across sexes for year-of-birth cohorts not affected by a change in the treatment. We regress turnout aggregated at the cohort by sex level on a linear trend of year of birth. We also interact this trend with a male dummy to test the parallel trend assumption. To validate our approach, the coefficient of this interaction should be zero for cohorts born before and after the treatment. We estimate this specification for cohorts born before 1975 in the first column. The coefficient of this interaction is not significantly different from zero, and the point estimate is very small. Similarly, the effect is close to 0 when we restrict the sample to cohorts born after 1979. Finally, the interaction is negative and significant on the whole sample, including treated and untreated cohorts. Those findings support the parallel trend assumption.

We now perform a placebo analysis specifying different dates for the suspension of military service. As in Column 7 of Table 4, we simplify the information on the service and focus on the intention to treat. We define a sequence of variables $Placebo_k$ that take the value 1 for males born in cohort k or earlier and 0 for younger cohorts. $Placebo_k$ is equal to 0 for all women. For $k \in \{1960, \ldots, 1997\}$, we consider an adapted version of Model (3):

$$\bar{Y}_{sct} = \alpha + \beta Placebo_k + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \epsilon_{sct}. \quad (4)$$

We estimate this model for all cohorts born between 1960 and 1997 and report the results for even-numbered years of birth in Fig. 3.[12] Odd-numbered years are presented in the supplementary material available online. Ideally, we expect to obtain a positive and significant effect of $Placebo_{1978}$, which corresponds to the true suspension of military service and is what we estimated in Column 7 of Table 4, and no result for other years. However, the estimate of $Placebo_{1976}$ and $Placebo_{1980}$ are also significant at the 1% level, as are the estimates for 1975 and 1977 (see the supplementary material available online). This pattern is first explained by the gradual decrease in conscription rate observed in Fig. 1 for cohorts born between 1975 and 1978. Moreover, even if military service was the only explanation for the widening of the gender gap in turnout, the estimation of (4) would yield false-positive results for cohorts close to the suspension. To mitigate this contamination effect, we restrict the sample to cohorts born between $k-10$ and $k+10$ for each treatment $Placebo_k$. All placebos before 1975 and after 1980 are not statistically different from zero, and the point estimate of the 1978 placebo is the largest, which reinforces our interpretation. Notably, in the placebo analysis, we implicitly assume that all males in the supposedly treated cohorts have performed the service, instead of less than 70% in reality. This explains why we observe a smaller point estimate for $Placebo_{1978}$ than in Table 4 (except in Column 7 when we actually test the same specification).[13]

The validity of the estimations performed thus far relies on the assumption that we control adequately for generation effects other than military service. Even in the most demanding specifications, we cannot account for the characteristics of males and females possibly evolving differently with year of birth, explaining the widening gap in turnout. In particular, changes that would have occurred for cohorts born between 1975 and 1978 could interfere with our results. Among the potential confounding explanations, we can imagine that the cohorts who experienced the suspension of military service are also affected by a change in women's education compared with men. Accounting for these effects would require controlling for year of birth by sex fixed effects, but the impact of military service cannot be identified in such a specification.

Next, we depart from the estimation strategy of Bedard and Deschênes (2006) and Imbens and Van Der Klaauw (1995) and exploit the individual structure of our data to control for those alternative explanations. We estimate the following model:

$$y_i = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \delta_5 X_i + \epsilon_i. \quad (5)$$

Instead of aggregating the data at the cohort level, we consider individual turnout as the independent variable to control for X_i, the individual characteristics available in the INSEE databases. The 2002 election dataset does not contain most

[12] For the latest cohorts, estimates are noisy because individuals were too young to vote for all elections covered in our data.

[13] Instead of the intention to treat, we can also perform a placebo analysis where we shift the actual service probabilities and suppose that the suspension happened in year k. This increases the point estimates as well as the standard errors and the estimated treatment is significant for the same year-of-birth cohorts.

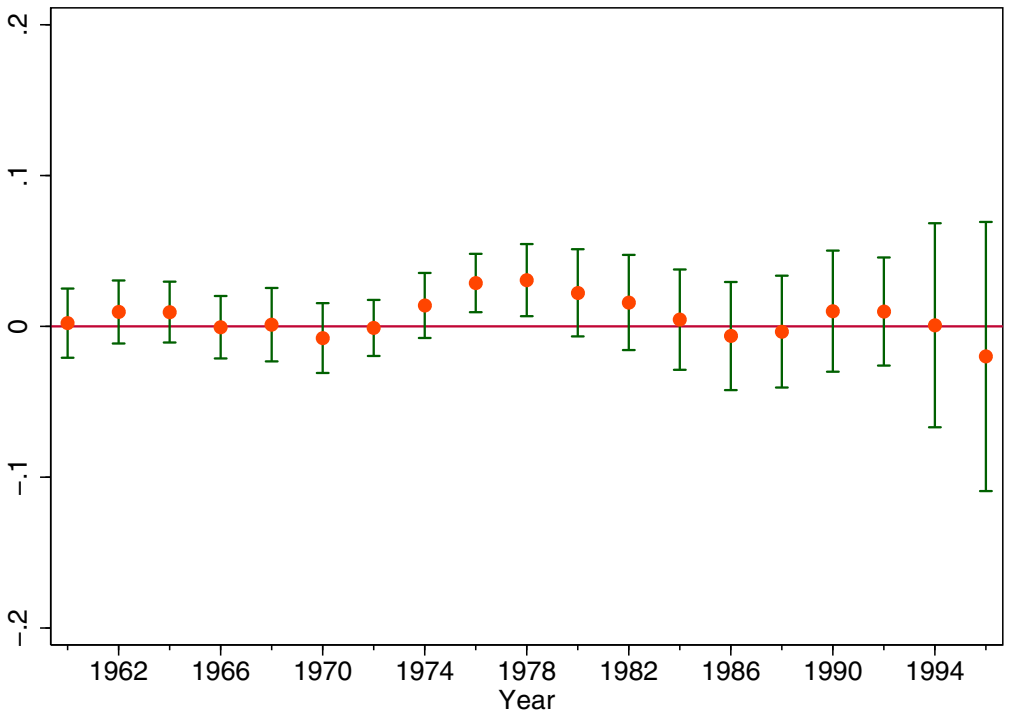

Fig. 3. Placebo analysis. We estimate with an OLS Model (4): $\bar{Y}_{sct} = \alpha + \beta Placebo_k + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \epsilon_{sct}$ for $k \in \{1960, \ldots, 1997\}$, where the dependent variable is the average turnout by birth-cohort, sex, and year of election. We report the point estimates for even-numbered k, and odd-numbered years are in (a) in the online appendix. The vertical axis indicates the increase in turnout for the supposedly treated cohorts, namely, male cohorts born in year k or before. A point estimate of 0.1, therefore, indicates that the turnout for the supposedly treated is larger by 10 percentage points. We report the point estimates and the 1% confidence intervals. For each treatment k, we restrict the sample to cohorts born between $k-10$ and $k+10$. Standard errors are clustered at the cohort sex level.

individual controls and had to be dropped for the remaining of the analysis. As in Model (3), we also control for age by sex fixed effects, cohort fixed effects, and election year fixed effects. Because we do not have individual information on military service and to solve for the selection problem, we continue to use the aggregate service rate $\bar{S}_{sc}$ as a regressor. We, thus, regress individual-level data on a group average, which generally biases standard errors toward zero (Moulton 1986, Garrett 2003). To account for this concern, all standard errors are clustered at the cohort by sex level, as suggested in Angrist and Pischke (2008).[14]

In the first column of Table 6, we adopt a conservative approach and include only male and region of origin dummies in X_i. According to Angrist and Pischke (2008), additional variables directly affected by military service are bad controls. For the case of France, Maurin and Xenogiani (2007) and Mouganie (2020) have demonstrated that military service affects education and wages. As a result, those variables are also outcomes of military service, and their inclusion could bias the estimation. The individual controls we include in the first column of Table 6 do not present this problem because they were already fixed when the individuals passed the examination for the service. The estimate of the effect of military service on turnout is very similar to what is in Table 4.

Next, we introduce further controls in Column 2. Those controls include marital status, education, profession dummies, and the size of the city of residence, which are the usual controls in the literature on voting (Wolfinger and Rosenstone 1980, Alvarez et al. 2011 Burden et al. 2014). We observe that the effect of conscription remains significant and that the point estimate is very similar. This finding indicates that our result is not influenced by a relative change across sexes in education or in participation in the labor market.

Thus far, we have been imposing the effect of military service on turnout such that it is the same in any year. We relax this restriction in the third column of Table 6 and allow the effect to vary across election years. The difference between the effects in 2012 and 2017 is not statistically different and it is also the case when we consider the different elections separately, but we do not report the results. It demonstrates that the effect of military service is actually persistent over time.

In Column 4, we exclude the cohorts born between 1975 and 1978 in order to account for the gradual decrease in the rate of conscription. As in the last column of Table 4, this exercise does not alter our results.

[14] Another solution to the Moulton problem proposed in Angrist and Pischke (2008) is aggregating observations at the cohort level, which is precisely what we have done in Table 4.

Table 6
Individual-level analysis.

	(1) Turnout	(2) Turnout	(3) Turnout	(4) Turnout	(5) Turnout
Service rate	0.0588***	0.0558***		0.0711***	0.0717***
	(0.0092)	(0.0101)		(0.0067)	(0.0111)
Service rate × 2012			0.0570***		
			(0.0100)		
Service rate × 2017			0.0635***		
			(0.0102)		
Sample	All	All	All	No 75–78	All
Controls-1	Yes	Yes	Yes	Yes	Yes
Controls-2	No	Yes	Yes	Yes	No
Registration	No	No	No	No	Yes
R2	0.12	0.16	0.16	0.17	0.16
Observations	57,524	57,495	57,495	52,919	57,495

We estimate with an OLS Model (5): $y_i = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \delta_5 X_i + \epsilon_i$. In the third column, we interact $\bar{S}_{sc}$ with election years. The dependent variable is average individual turnout. We restrict the sample to individuals who could have voted to all elections – i.e who lived in districts where a second round was held for the legislative elections – and consider their average participation. *Service Rate* is the proportion of male individuals who did their military service per birth cohort, it is equal to 0 for women. The first set of controls is limited to male and region of origin dummies. In the second set, we include level of education defined as the highest diploma obtained, occupation dummies (8 in total), marital status and the size of the city of residence. These controls are not available in the 2002 database and we drop the observations collected in 2002. Standard errors are in parentheses and clustered at cohort by sex level.

Table 7
Extensive/intensive margin.

	(1) Extensive	(2) Intensive
Service rate	0.0559***	0.0822***
	(0.0053)	(0.0077)
Cohorts FE	Yes	Yes
Election FE	Yes	Yes
Cluster	Birth-sex	Birth-sex
R2	0.04	0.08
Observations	78,086	78,086

We estimate with an OLS a variation of Model (5): $y_i = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_{fe} + \delta_2 Male \times Age_{fe} + \delta_3 Cohort_{fe} + \delta_4 Election\_Year_{fe} + \delta_5 X_i + \epsilon_i$ where we adjust the dependent variable to separate the intensive and extensive margin effects. In the first column, the outcome is a dummy variable with value 1 if the individual voted once or more. In the second column, we consider the probability to cast at least 3 ballots for a given election year. We control for male and region of origin dummies. Standard errors are in parentheses and clustered at cohort sex level.

Moreover, we now discuss the potential effect of registration on voter lists. Before 1997, voters had to register on voter lists before the election to be allowed to vote. During military service, conscripts could have been encouraged to do so, which would explain why conscription bridges the gender gap in turnout. In 1997, a reform that made registration automatic for both sexes was enacted. The effect of military service could thus be partly influenced by the difference in registration rate for treated cohorts. We isolate this effect in the last column. Data on registration have been collected in a companion database to the data that we use in 2012 and 2017. We can therefore compute the average registration rate by cohort and by sex at the time of the election. In the last column, we control for the registration rate and find that it does not affect the effect of the service, demonstrating that registration on voter lists is not the impetus for the result on turnout.

Before we consider the effect on other political behavior, we test in Table 7 whether conscription increases the probability of never voters to turn out at least once, or if it increases the probability of occasional voters to vote more. We refer to the first effect as the extensive margin and the second effect as the intensive margin. In the first column, we consider the sum of ballots cast by each individual and generate a dummy variable equal to 1 if the individual voted at least once. In the

Table 8
Political preferences, correlation analysis.

	(1) Turnout	(2) FI	(3) PS	(4) ELV	(5) LRM	(6) REP	(7) FN
Service	0.091***	−0.053	−0.059	−0.290	−0.083	0.578**	0.495*
	(0.028)	(0.252)	(0.216)	(0.217)	(0.235)	(0.227)	(0.267)
N	1711	1608	1624	1624	1628	1615	1653

We estimate with an OLS Model (6) $Y_i = \alpha + \beta S_i + \delta_1 Age_i + \delta_2 X_i + \epsilon_i$. In the first column, the dependent variable is the reported turnout for the first round of the presidential election. In the next columns, we study the reported preference for the main political parties, indicated on a scale from 0 to 10, where 10 is the most positive opinion. A translation of the questions are in the online appendix. In addition to age, age squared, and a male dummy, we control for income and a dummy equal to 1 if at least one parent was not born French. We also control for the average preference for political parties in all columns except for Column 1. Standard errors are in parentheses.

second column, we test the effect of military service on the probability to cast 3 ballots or more for one given election year. We observe a larger effect in the second column, indicating that military service increased the turnout of occasional voters but was less efficient in inducing nonvoters to vote. The difference between the two coefficients is significant at the 1% level. We conclude that military service affects mostly the intensive margin.

The magnitude of the effect that we have found in this section is comparable to the impact of door-to-door canvassing documented in the United States by Gerber and Green (2000). They report an increase in turnout of 9 percentage points when individuals are contacted in person by a canvasser who reminds them to vote. Also in the United States, Madestam and Yanagizawa-Drott (2012) find that attending one additional Fourth of July celebration increases turnout by 0.88 percentage points at age 39 years. In addition to the effect on turnout, we naturally wonder if those experiments affect political preferences. Madestam and Yanagizawa-Drott (2012) demonstrate that attending celebrations increases individuals' support for the Republicans. Falck et al. (2014) also investigates both questions. They first demonstrate that internet penetration decreases turnout in Germany. They also consider the impact on vote shares but find no evidence that the internet benefits specific parties. In Section 4, we analyze the consequences of military service on political preferences.

4. Political preferences

To investigate the question of political preferences, we exploit the French Electoral Study conducted in 2017 by Gougou and Sauger (2017). This study comprises 1830 face-to-face interviews. Respondents had to answer a long list of political questions. According to our review of the literature, this survey is the only dataset in France released after the suspension of military service that includes a question on conscription status. In the sample, 436 respondents have performed military service.

Because we have individual data on political preferences and military service, a first attempt to assess the effect of conscription is to estimate the following specification:

$$Y_i = \alpha + \beta S_i + \delta_1 Age_i + \delta_2 X_i + \epsilon_i. \tag{6}$$

Where Y_i is the reported political preference of individual *i*, S_i is a dummy variable equal to 1 if the individual declares that he has done military service, and X_i is a set of control variables. This specification has two main differences from what we performed in Section 3. First, the estimate of β is potentially subject to selection bias. We can imagine, for instance, that individuals self-select into military service based on characteristics that also affect their political preferences. In Table 8, we estimate Model (6) without correcting for this potential bias but propose three methods to account for this concern in Table 9. Moreover, the data are now cross-sectional, implying that we cannot simultaneously control for age and cohort effects. We can therefore not properly disentangle the effect of military service from other generation effects.

To validate the results of the previous section, our first variable of interest is (reported) turnout. We focus on the first round of the presidential election in 2017 because the interviews were either conducted between the two rounds of the presidential election or between the presidential and the legislative elections.[15] We also consider the reported preferences for the six main political parties[16] Respondents were asked to indicate their feelings on a scale from 0 to 10, where 10 is the most positive opinion. We report the results in Table 8.

First, we observe in Column 1 of Table 8 that the correlation between military service and turnout is significant and the point estimate is of the same magnitude we found in Section 3. Moreover, having performed service is correlated with the likelihood to report positive attitudes toward right-wing political parties. In particular, former conscripts are more likely to be in favor of the party "Les Républicains," the traditional right-wing party, and the "Front National," the main far-right

[15] Results are similar when we consider prospective turnout for the other elections, but we posit that this information is less reliable.

[16] France Insoumise (FI), far left; Parti Socialiste (PS), left; Europe Ecologie – les Verts (ELV), green party; La République en Marche (LRM), center; Les Républicains (REP), right; Front National (FN), far-right.

Table 9
Political preferences controlling for selection.

	(1) Turnout	(2) REP	(3) FN	(4) Turnout	(5) REP	(6) FN	(7) Turnout	(8) REP	(9) FN
Service	0.121***	0.461*	0.149				0.199***	0.290	0.356
	(0.033)	(0.271)	(0.312)				(0.053)	(0.408)	(0.514)
Service rate				0.225***	0.329	0.401			
				(0.059)	(0.465)	(0.582)			
Father politics	0.024*	0.509***	−0.036						
	(0.014)	(0.113)	(0.130)						
Mother politics	−0.007	0.511***	0.684***						
	(0.015)	(0.122)	(0.141)						
N	1014	995	1001	1252	1182	1211	1252	1182	1211

Note: OLS regressions. In Columns 1, 4 and 7 the dependent variable is the reported turnout for the first round of the presidential election. In Columns 2, 5, and 8 we consider the reported preferences for the right-wing party *Les Républicains*. We consider the preference for the *Front National* in Columns 3, 6, and 9. In addition to age, age squared and a male dummy, we control for income and a dummy equal to 1 if at least one parent was not born French in all columns. We also control for the average preference for political parties in the columns where we test political preferences. In Columns 1–3, we estimate a variation of Model (6) $Y_i = \alpha + \beta S_i + \delta_1 Age_i + \delta_2 X_i + \epsilon_i$ where we also control for parental political preferences when the respondent was 10. For each parent, political preferences are indicated on a scale from 0 to 4 where large numbers indicate right-wing preferences. In Columns 4–6, we control for the service rates computed in Fig. 1 instead of the individual service observations in Model (6): $Y_i = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_i + \delta_2 X_i + \epsilon_i$. Columns 7–9 report the results of a 2SLS where the first stage consists in regressing the individual service variable on the service rate: $S_i = \alpha + \beta \bar{S}_{sc} + \delta_1 Age_i + \delta_2 X_i + \epsilon_i$. We display the result of the first stage in Column 13 of Table in the online appendix. In the second stage, we estimate Model (6) with the values predicted in the first stage $\hat{S}_i$: $Y_i = \alpha + \beta \hat{S}_i + \delta_1 Age_i + \delta_2 X_i + \epsilon_i$. The translation of the questions are in the online appendix. Standard errors are in parentheses.

party. In the online appendix, we observe a similar result using data collected between 1988 and 1991: individuals who have performed the service are more likely to report right-wing preferences and exhibit other values traditionally associated with the right. Our data do not allow us to formally investigate the mechanisms through which this potential ideological shift could have occurred. One possible explanation is the exposure of young men to professional militaries, known to be conservative and nationalist (Rouban, 2007,2013), during their service. The interactions between conscripts and professional militaries could have influenced the shaping of political preferences of young men.

However, the validity of those results relies on the hypothesis that the selection of military service was not influenced by political preferences before incorporation in military service. This assumption is strong: We can, for instance, posit that left-wing individuals are more opposed to the military and therefore manage to avoid the draft in larger proportions. For turnout, we can imagine that individuals who are socially well integrated are at the same time more likely to perform their service and to vote. This would induce selection bias. In Table 9, we propose three methods to account for this concern. We focus on turnout and preferences for the right-wing parties – Les Républicains (REP) and the Front National (FN) – because those are the outcomes for which we found a significant impact in Table 8. We report the results for all parties in Table 10 of the online appendix.

We want to emphasize that the selection bias could not affect the result on turnout found in Section 3 because we were using aggregated data for military service. Even when we used individual-level data on turnout in Model (5), the regressor was the aggregate rate of compliance with military service computed at the cohort level. Given that the share of conscripts by cohort was mostly determined by exogenous political decisions, we reasonably assumed that pre-military service characteristics of individual *i* did not affect the conscription rate of his cohort. The selection bias could be an issue in the current section because the regressor of interest in Model (6) is an individual-level service dummy.

In the first three columns of Table 9, we test a specification similar to Model (6) where we add parental political preferences as a control. We posit that this specification accounts for self-selection into military service based on political preferences. For instance, we could imagine that individuals whose parents had right-wing political preferences are simultaneously more likely to do their service and to report right-wing political preferences. In this case, the positive correlation between military service and the likelihood to report right-wing political preferences in Table 8 could not be interpreted as a causal impact of military service. Under the assumption that parental political preferences reflect individuals' preferences before military service, which is supported, for instance, by Jennings et al. (2009), adding this variable as a control would mitigate the selection bias.

Respondents had to indicate the political preferences of their father and mother on a scale from 1 to 5, from "extreme leftist" to "extreme rightist." When we control for this variable, the point estimates for political preferences are smaller than in Table 8 and only the sympathy for the party LR remains significant at the 10% level. The estimated effect for the FN is divided by four and is no longer significant. The effect of turnout remains very significant and appears slightly larger. Moreover, in Table 10 (online appendix), we continue to observe that military service has no significant effect on the preference for nonright parties.

Another approach to correcting the selection bias is ignoring the individual data on service and estimating a specification similar to Model 6 where the regressor of interest is the service rate by cohort indicated in Fig. 1 instead of the individual

service information. This approach is essentially the approach we followed in Model (5) of the turnout section. However, the data are now cross-sectional, and we cannot control simultaneously for age and cohort. This identification strategy is, therefore, weaker than in the section on turnout: We cannot account convincingly for other generation effects. We report the results in Columns 4–6 of Table 9. The impact on military service on political preferences is not significant anymore. However, the effect on turnout remains significant at the 1% level.

Finally, we use the service rate at the cohort level as an instrument for the individual service variable. This approach is the same as Bedard and Deschênes (2006), when they estimate the effect of veteran status on tobacco smoking: as in our paper, they rely on individual-level data to investigate the channels influencing the impact on health they document. In the first stage, we regress the individual-level information on military service S_i on the service rate by cohort. We then estimate Model (6) with the values predicted in the first stage instead of S_i as a regressor. We report the results of the 2SLS in the last three columns of Table 9. The effect on turnout remains very high and even larger than in the uninstrumented regression. However, the impact on political preferences is not significant.

Those three methods suggest that the correlations between military service and political preferences in Table 8 were influenced by a selection effect. All the point estimates for political preferences in Table 9, where we account for the selection bias, are smaller than in Table 8, and only one is significant at the 10% level. This finding could reflect that individuals with right-wing political preferences were more likely to do their service. However, even if not significant, all the estimates in Table 9 are positive and our data do not allow us to conclude that military service had no effect on political preferences. Finally, the impact on turnout is positive, very robust, and significant at the 1% level in all specifications, confirming the results of Section 3.

5. Conclusion

This paper is a first step in understanding the impact of military service on political behavior. This topic has received surprisingly little attention from scholars, despite being in the limelight in the political arena.

We exploit the natural experiment of the suspension of military service in France at the end of the 1990s to estimate the impact of conscription on turnout and political preferences. We use the difference in treatment between men and women and between men of different cohorts to identify our effect.

Regarding turnout, we demonstrate that doing military service increases participation by approximately 7 percentage points. This result is robust to various specifications and is observed to be valid for all presidential and legislative elections in 2002, 2012, and 2017.

We then investigate the impact of military service on political preferences. We demonstrate that former conscripts are more conservative and nationalist, but this result is not significant when we account for selection bias. However, due to the sample size and to the cross-sectional nature of the data, this is not sufficient to conclude that there is no effect. To improve this point, a question on the military service could be added in a larger scale study during the next elections.[17]

Finally, we have considered indifferently the various types of military service. However, as pointed out in the online appendix, conscripts performed different types of military service and the effects could be heterogenous. Thus, further research could identify more accurately the patterns that shape political behavior. This would imply to collect data on individual assignments to analyze the mechanisms driving our results.

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Supplementary material

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[17] For instance, the CEVIPOF French Electoral Study included 17,000 individuals in 2017.

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