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Electoral consequences of declining participation: A natural experiment in Austria



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ABSTRACT

Participation rates have declined sharply across developed democracies. But the precise impact of this decline on party systems has proven difficult to study due to endogeneity concerns. This paper seeks to address this issue by leveraging a natural experiment in Austrian parliamentary elections. By examining instances in which compulsory voting was gradually repealed in a federal setting, I isolate the causal relationship between turnout decline and subsequent shifts in party vote share. The findings suggest that turnout decline is not associated with a significant redistribution of votes between parties. The clearest visible effect is a consolidation of the party system, with a mild shift in votes from minor to mainstream parties. Evaluating the findings, the paper argues that characteristics of proportional representation systems insulate parties against the consequences of declining electoral participation.

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1. Introduction

Over the last thirty years, there has been a notable decline in electoral participation across developed European democracies. The recent spate of economic turmoil has not slowed this trend: in the latest round of parliamentary elections, France, Germany, and Italy all recorded historic lows. In response, many policymakers and academics have raised concerns of an emerging ‘democratic deficit’ in European politics. And while this critique is primarily normative, there may be reason to be concerned on practical grounds as well. Because decline in voter participation is often asymmetric with respect to demographic categories, declines in electoral turnout may be accompanied with shifts in the balance of political power.

At face value, the recent success of conservative parties in Europe lends some credence to this claim. However, the precise relationship between declining participation and

changes in party vote share remains unclear. Regressing participation rates on vote share – either cross-nationally or longitudinally – is unlikely to generate strong inferences because factors that affect participation are likely to influence party choice as well.¹ Recognizing this issue, the majority of studies that investigate the relationship between participation and party choice have focused on the identity of the ‘non-voter.’ By leveraging data on low turnout districts or analyzing survey responses, it is possible to investigate how turnout varies according to demographic, partisan and socioeconomic characteristics. But although these studies have reached some measure of consensus about which types of voters are likely to participate in elections, the electoral implications are less clear. For instance, depending on the particular mechanism through which preferences are translated into votes, high levels of participation could be viewed as likely to benefit

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¹ These approaches can still reveal useful correlations and trends related to turnout and party support; the issue at stake is whether the relationship is causal.

parties of the Left (Pacek and Radcliff, 1995; Lijphart, 1997; Herron, 1998; Mackerras and McAllister, 1999; Jackman, 1999; Sides et al., 2008), populist or centrist parties (Whiteley, 1977; Prior, 2007; Hidalgo, 2010),² or minor/protest parties (Belanger, 2004; Bernhagen and Marsh, 2007). Regardless, the outcomes in question remain hypothetical given that we cannot actually observe how non-voters would have voted in a realized setting.

This paper seeks to add new empirical evidence to this debate. I use the institution of compulsory voting law as a means to introduce variation in turnout while minimizing endogeneity concerns. Specifically, I draw upon municipal-level data and leverage subnational variation in compulsory voting within a single case: Austria in the early 1990s. With the exception of one Swiss canton (Vaud), Austria is the only modern democracy to have permitted subnational variation in compulsory voting law within national elections.³ The variation at the subnational level creates a natural experiment in which the effect of turnout decline on party vote share can be causally identified with a difference-in-differences design. While this approach implies a reduction in external validity, it allows for the robust analysis of electoral outcomes in a non-hypothetical setting.

The results suggest that the effect of decreased turnout on party vote share is minimal. Municipalities that experienced large declines in turnout caused by the repeal of compulsory voting were likely to vote similarly to control municipalities that did not experience such declines. This result is consistent across a variety of specifications and holds even for municipalities that experienced declines in excess of 15%. Indeed, the only clear effect of declining participation is a mild consolidation of the party system, with a redistribution of votes from minor parties to the mainstream Social Democratic Party. Although care should be taken when extending the findings to other settings,⁴ at minimum the results provide strong empirical support for the claim that researchers should be wary of assuming that declining turnout in developed democracies will necessarily translate into consequential shifts in party vote share (Lutz and Marsh, 2007; Van der Eijk and Van Egmond, 2007).

2. Compulsory voting as a natural experiment

By levying financial or administrative penalties, the institution of compulsory voting ‘encourages’ many individuals who would otherwise not vote to register their preferences at the polls. Since this institution is often orthogonal to factors that affect party choice, examining contexts in which compulsory voting has been

implemented provides a useful tool to study the effects of participation while minimizing endogeneity concerns.⁵

In this paper I focus on the *repeal* of compulsory voting within a developed democracy. Austria is one of the few European states to maintain compulsory voting well into the post-war period, with the practice finally being phased out in a series of legal steps between 1982 and 2004. However, unlike other instances in which compulsory voting law has been applied in developed countries, jurisdiction over voting law has frequently shifted between the state ($n = 9$) and the national level. As a result, there is significant subnational variation in the incidence of compulsory voting in national elections (see Fig. 1).

To provide a brief background, Austria possesses three main election types, all of which have utilized compulsory voting enforced by financial sanctions at different points in time (Gratschew, 2004). In the presidential and provincial elections, voting was compulsory in all states until 1982, when the decision was devolved to the state level. Although four states continued to practice compulsory voting after this decision, the law was eventually abolished in all states by 2004.

In contrast to the presidential and provincial elections, the constitution of the Second Austrian Republic granted states from the outset the authority to determine whether to implement compulsory voting in national parliamentary elections (Article 26/1). Three out of nine states implemented compulsory voting beginning in 1949: Styria, Tyrol, and Vorarlberg. A fourth, Carinthia, joined the ranks in 1986 despite previously high turnout levels in the province.⁶ However, in 1992 the constitutional court ruled that Article 26/1 was invalid and states no longer had the authority to enforce compulsory voting in national parliamentary elections (Bundes-Verfassungsgesetz-Novelle, BGBl. Nr. 4s70). As a result, all four states abruptly ended compulsory voting beginning in the 1994 election.

Although all three elections types provide an opportunity to leverage subnational variation in compulsory voting law, this paper focuses exclusively on the parliamentary elections (Nationalratswahl). Within the Austrian system, parliamentary elections are far more consequential than presidential elections, given that the president serves a largely symbolic role. Moreover, unlike the provincial elections, parliamentary elections are national in scope and are held on the same date across the country.

Most importantly, the fact that the 1992 law change occurred as a result of judicial review and not as the result of legislative politics sharply reduces the danger of selection

² The literature that highlights decreased levels of political sophistication under high turnout or compulsory voting would lead one to suspect gains for populist parties (Ackaert and De Winter, 1996; Hooghe and Pelleriaux, 1998; Rosema, 2007; Selb and Lachat, 2009), although this is not always claimed directly.

³ For an example of subnational compulsory voting in an early democracy, see Fowler 2013.

⁴ In particular, the results should not be expected to hold in developing democracies.

⁵ Given that compulsory voting raises turnout through an artificial process, some scholars have argued that compulsory voting systems differ in important ways from non-compulsory systems with high levels of political participation (Selb and Lachat, 2009; Jensen and Spoon, 2011). This is likely true, and researchers should be cautious about over-extrapolating results from compulsory systems. However, given the highly endogenous relationship between political participation and party support, compulsory voting remains one of the best available tools to study large shifts in turnout in consequential elections.

⁶ Carinthia's decision to implement compulsory turnout under conditions of high turnout underscores the fact compulsory voting was not introduced within these states as a remedy to correct low turnout; rather, it was largely an attempt to increase democratic legitimacy.

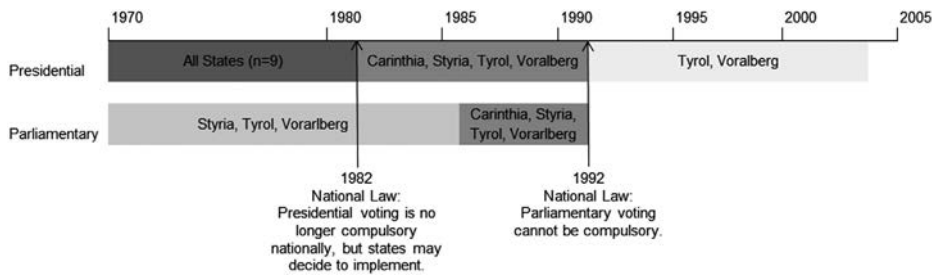


Fig. 1. Changes in Austrian compulsory voting law.

bias. Although compulsory voting is a useful tool to examine exogenously induced shifts in turnout, it is important to note that the introduction (and removal) of compulsory voting is often a highly political process (Helmke and Meguid, 2007). On the surface, the Austrian case is no different: several Austrian states in 1949 and 1983 voluntarily implemented compulsory voting. However, although the *introduction* of compulsory voting is non-random, the timing of the *transition* from compulsory to non-compulsory voting in 1994 as a result of a constitutional decision was non-voluntary from the perspective of the states.

As a result, the repeal of compulsory voting in Austria can be seen as a natural experiment following the nonequivalent control group design (Campbell et al., 1963). Given the common trends assumption that treated units (municipalities within compulsory voting states) and untreated units (municipalities in non-compulsory states) would follow similar trends in the absence of the treatment, the causal effect for the repeal of compulsory voting is identified by a simple difference-in-differences design:

$$\begin{aligned} \delta &= (E[X|D = 1, t = 1994] - E[X|D = 1, t = 1990]) - (E[X|D = 0, t = 1994] - E[X|D = 0, t = 1990]) \end{aligned} \quad (1)$$

where δ is the causal effect, X is vote share, D is the treatment indicator, and t is the election year.

In conclusion, the variation in compulsory voting within Austrian national parliamentary elections provides a 'best case' scenario to study the causal effect of declining turnout within an observational setting. By taking advantage of the 1992 constitutional decision and pre-treatment variation in the incidence of compulsory voting, plausible conclusions can be reached regarding the influence of decreased turnout on vote share within a developed democracy.

3. The effect of repealing compulsory voting in Austria

In order to analyze how the repeal of compulsory voting law affected turnout and party vote share, I digitized official election results published by the Austrian Interior Ministry (Bundesministerium für Inneres). After listwise deletion, the final dataset covers five parliamentary elections from 1983 to 1995, with results grouped by municipality ($n = 2288$), district ($n = 121$), and state ($n = 9$). The data were checked to verify district and municipality-level accuracy across election years and adjusted to compensate for

any changes in geographic borders during the period under consideration.

Fig. 2 displays the effect of repealing compulsory voting on the average turnout in Austrian municipalities. Throughout the period in which compulsory voting was in effect, turnout was higher in districts with compulsory voting laws (Fig. 2). Although at first glance the observed difference in turnout between the two groups of is not large, it is important to note that compulsory voting was implemented in Austria within the context of extant high turnout. In this context, the increase in turnout observed for compulsory municipalities is substantively significant, especially when considering that compulsory voting laws rarely achieve turnout rates in excess of 95% in comparative context (Singh, 2011). After the repeal of the compulsory voting provision in 1992, turnout in municipalities within former compulsory voting states declined by an average of 8.6%,⁷ with a sizable number of municipalities experiencing turnout declines in excess of 15% Table 1.

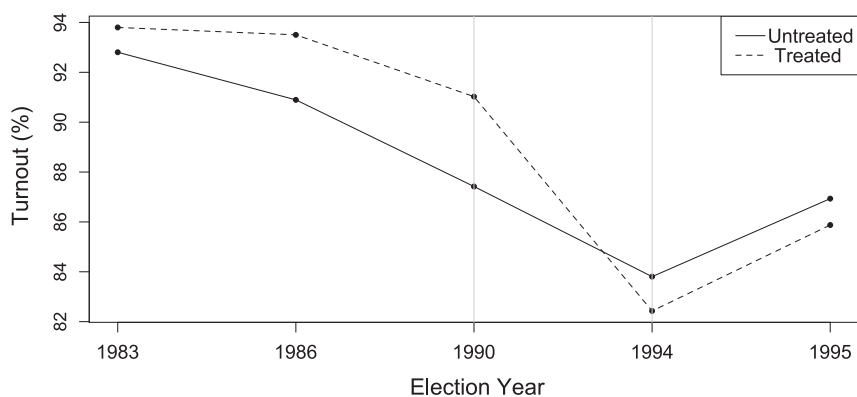
Table 1 displays the mean municipal-level vote share achieved by the four largest parties (the Austrian People's Party (ÖVP), the Social Democratic Party (SPÖ), the Freedom Party of Austria (FPÖ), and the Green party⁸) in 1994 versus vote share in 1990. Vote share for the remaining parties is summed and analyzed in aggregate under the designation 'Minor Parties'.⁹ The last two columns in Table 1 present difference-in-difference estimates between compulsory and non compulsory municipalities, with standard errors clustered by state. The observed effect of the repeal of compulsory voting is marginal. The results suggest that the only substantive change between treated and control groups was a 1.4% decline in vote share for minor parties, and a 1.5% increase in vote share for the Social Democratic Party (SPÖ). All other changes in vote share are substantively and statistically insignificant.¹⁰

⁷ The state of Styria is a significant outlier; without Styria the average decline in turnout among the treated group was 11.8%.

⁸ These parties are usually classified as Christian Democratic, Social Democratic, conservative/nationalist, and environmentalist, respectively.

⁹ These parties are: the Liberal Forum (LiF), the United Greens (VGO), the Centrist Greens (BGO), the Civic Action Group Against the Sale of Austria (NEIN), the Communist Party (KPO), Christian Democratic Party, the Christian Electoral Community (CWG), the Austrian National Law Party (ONP), Fritz Georg (FG), the Grey Austrians' Election Platform, the Alliance of Welfare Beneficiaries, and The Best Party (DBP).

¹⁰ As a placebo test, I conducted an identical analysis using a false threshold at the 1990 election results (see Appendix). All results were statistically insignificant.



The dashed line represents the average turnout across municipalities that practiced compulsory voting prior to 1994; the solid line represents municipalities that did not practice compulsory voting.

Fig. 2. Average Municipality Turnout in Parliamentary Elections.

Table 1
Difference-in-differences in municipal vote share, 1990–1994.

Mean vote share	Compulsory		Non-Compulsory		DID	t
	1994	Δ1990	1994	Δ1990		
OVP	0.380	−0.052	0.375	−0.054	−0.001	−0.04
SPO	0.286	−0.053	0.323	−0.066	0.015	3.66
FPO	0.232	0.061	0.196	0.062	−0.000	−0.05
Green	0.054	0.023	0.052	0.023	−0.000	−0.02
Minor parties	0.049	0.020	0.054	0.034	−0.014	−2.78

n = 1009 Compulsory, 1279 Non-compulsory municipalities. Compulsory voting no longer in effect in 1994. Standard errors clustered by state (n = 9).

4. Robustness checks

Before interpreting the results further, I address two possible concerns with the initial analysis: (a) that unobserved regional variation may have biased the results and (b) that the decline in turnout may have been too small to expect a significant redistribution of party vote share.

4.1. Addressing regional variation

The difference-in-differences design controls for unit-level and temporal variation, but relies on the assumption that compulsory and non-compulsory voting states would follow a common trend in the absence of compulsory voting. Given that the parliamentary elections are national in scope, this assumption is generally plausible, and indeed the turnout patterns of the two groups of states seem to follow a common trend after 1994 (Fig. 2). However, there are two concerns that could undermine the validity of this assumption. First, and most obviously, if party choice sets differed between compulsory and non-compulsory districts in 1990 or 1994, the parallel trends assumption would not hold. Fortunately, this concern is not particularly relevant in the Austrian case. Although parties within the Austrian system retain the ability to select the districts in which they will run, they do so at the state level, eliminating widespread variation across municipalities and districts.¹¹ Lastly,

a 4% threshold for political representation limits the viability of regional parties. The results of 1994 serve as an illustrative example: nine parties ran in every municipality in the country, while the remaining three parties (which only ran in some states) achieved only 14,000 total votes.

Another possible concern is that unobserved regional factors may have interacted with events occurring between 1990 and 1994, leading to non-parallel trends. While this concern is partially mitigated by Austria's relative regional homogeneity, it may not be valid to expect parallel trends between treated and control municipalities if the regions differ substantially.

In order to mitigate this potential source of bias, I employ two techniques. First, I fit a simple fixed effect model on data between 1986 and 1994 in order to control for unobserved unit-level heterogeneity:

$$y_{it} = \alpha_i + \lambda_t + \theta D_{it} + \beta X_{i,t-1} + \gamma \text{LogPopulation}_{i,t} \quad (2)$$

where y is the observed vote share for a given party, α is the fixed effect for each municipality, λ is a dummy for each year, D is a dummy indicating whether the municipality enforces compulsory voting in a given year, and X is a matrix containing lagged vote shares for the OVP, SPO, FPO, and Green parties. To control for changes in population size, the model also includes the log of the municipal population. The results are visible in Table 2.

Second, using 1994 data, I match treated and control units on a set of covariates in order to more precisely identify the effect of the repeal of compulsory voting. Specifically, I use entropy balancing to reweight a matrix of

¹¹ Moreover, if a given party chooses not to run within a state, the voters can opt to select from the national party list.

Table 2
Effect of compulsory voting on vote share: fixed effects model.

Vote share	OVP	SPO	FPO	Green	Minor
Estimate	0.003	-0.010	-0.002	0.001	0.009
<i>t</i> (clustered by municipality)	(1.75)	(-5.89)	(-1.67)	(1.61)	(8.48)
<i>t</i> (clustered by state)	(0.32)	(-2.82)	(-0.25)	(0.29)	(1.74)

Estimates are the coefficients on the compulsory voting dummy, and represent the estimated change in vote share attributable to compulsory voting (Thus signs are inverted relative to Table 1). The model includes year dummies, lagged vote share for each party, the log of the municipal population, and fixed effects for each municipality. 1986–1994 data.

control observations (Hainmueller, 2012). By weighting control units to maximize comparability with treated units, reliable inferences are possible even when treated and control groups do not possess pre-treatment equivalence (Campbell et al., 1963; Ho et al., 2007; Sekhon, 2009). Table 3 displays the average effect of the treatment on compulsory municipalities (upper half of the table) after matching on a set of covariates (lower half of table). Municipalities are matched primarily on the basis of fully saturated election results for prior years. In addition to the electoral controls, several models include variables from the 1991 national census.

Across both approaches, the results remain consistent with the findings from the difference-in-differences model, and suggest that the repeal of compulsory voting is associated with slight increase in vote share for the Social Democratic Party (significant at the 95% confidence level) and decreased vote share for minor parties (significant at the 90% confidence level). The effects remain substantively small: in the matching analysis, vote share for minor parties in former compulsory-voting municipalities declined on average 0.9–1.0%, while the Social Democratic Party gained 0.6–0.8%.¹²

4.2. Variation in turnout decline

A final concern worth addressing is whether the observed decline in turnout was large enough to expect corresponding shifts in vote share. To shed further light on this question, I exploit the heterogeneity in turnout decline across municipalities (see Fig. 3) and investigate changes in vote share experienced by municipalities with relatively large declines in turnout. In order to maximize comparability between treated and control units, I build on the existing matching results. Selecting the model with the smallest maximum weight (Model 1), I fit the following on 1994 weighted data:

$$y = \beta_0 + \beta_1 D*(Z - Z_{t-1}) + \beta_2 D*(Z - Z_{t-1})^2 + \beta_3 X \quad (3)$$

where *D* is the treatment indicator, *Z* is % turnout, *t* indicates election year, and *X* is a matrix of controls corresponding to Model 1 in Table 3.

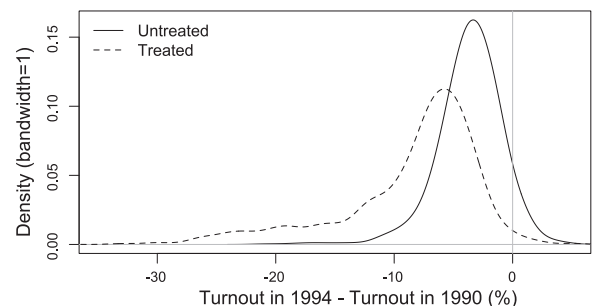
The results (Fig. 4) suggest that the effect of repealing compulsory voting remains quite small, even at very high

Table 3
Difference in 1994 vote share after matching (municipalities).

Vote share	1	2	3	4	5
OVP	0.001 (0.16)	0.001 (0.08)	-0.000 (-0.07)	-0.000 (-0.01)	-0.000 (-0.07)
SPO	0.008 (3.28)	0.007 (1.57)	0.007 (2.66)	0.007 (1.95)	0.006 (1.93)
FPO	0.003 (0.44)	0.004 (0.48)	0.004 (0.62)	0.004 (0.86)	0.006 (0.86)
Green	-0.003 (-1.13)	-0.003 (-0.87)	-0.002 (-0.85)	-0.002 (-0.69)	-0.002 (-0.84)
Minor	-0.009 (-1.77)	-0.009 (-1.82)	-0.009 (-1.63)	-0.009 (-1.78)	-0.010 (-1.75)
Matching covariates					
Lag OVP	x	x	x	x	x
Lag SPO	x	x	x	x	x
Lag FPO	x	x	x	x	x
Lag Green	x	x	x	x	x
Lag 2 OVP	x	x	x	x	x
Lag 2 SPO	x	x	x	x	x
Lag 2 FPO	x	x	x	x	x
Lag 2 Green	x	x	x	x	x
Log % unskilled 1991		x		x	x
Log population 1991			x	x	x
Log % inactive 1991					x
Max weight	2.89	3.48	3.72	3.94	4.04

1009 treated, 1279 control municipalities. Standard errors clustered by state (*n* = 9), with *t*-statistics in parentheses. Estimates represent the difference in mean vote share for each party, between treated (compulsory voting) and control (non-compulsory voting) municipalities after matching. Each column indicates a separate model matched on the covariates indicated with an 'x' in the lower half of the table. All models use the Entropy balancing matching algorithm (Hainmueller, 2012). Due to entropy balancing's use of weights and the number of observations available, post-match differences in covariate means are negligible, with *t*-statistics less than 0.01 in all cases (not shown). The maximum weight listed at the bottom of each column indicates the largest observed weight assigned to a municipality in the matching process.

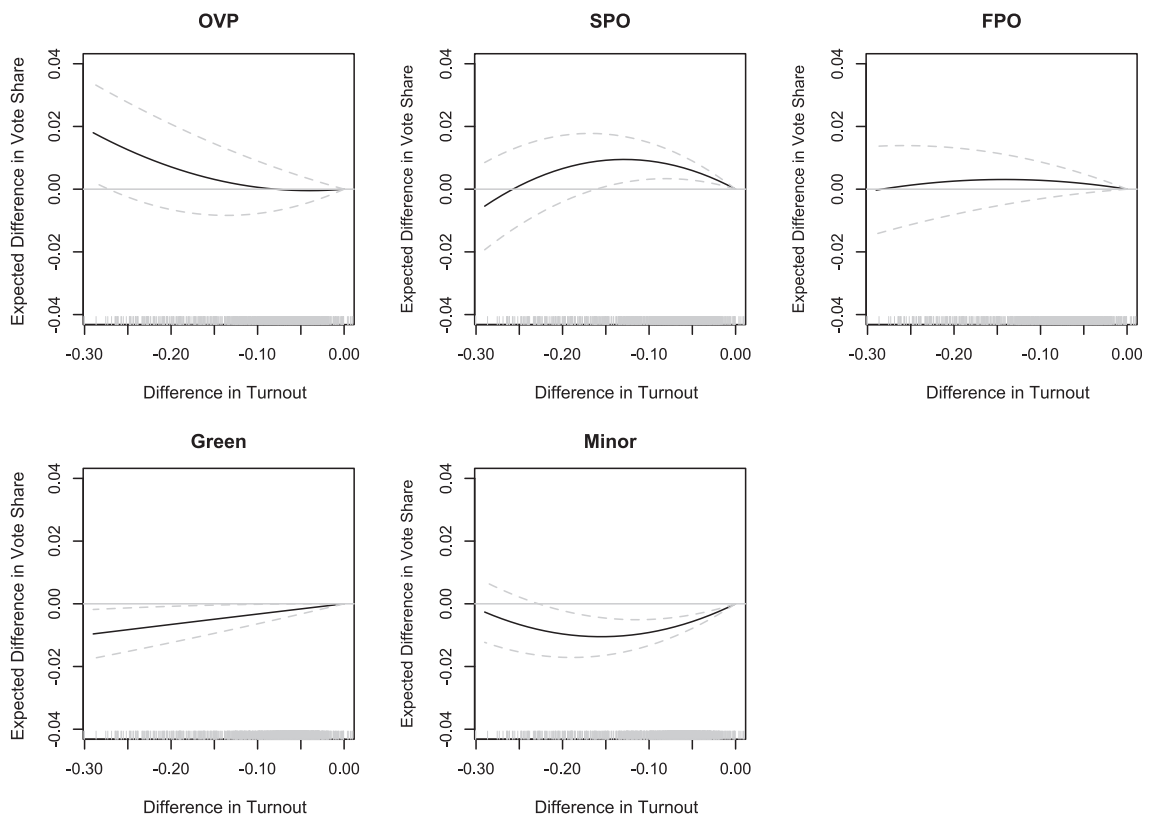
levels of turnout loss (>15%). The initial finding appears quite robust. In municipalities that experienced turnout decline as a result of the repeal of compulsory voting, a small redistribution of votes occurred between minor parties and the mainstream Social Democratic party. At higher levels of turnout decline, the redistribution of votes appears to shift towards the Austrian People's Party, although the estimates remain statistically insignificant at the 95% level.



The dashed line is a density plot of the change in turnout in municipalities that practiced compulsory voting prior to 1994; the solid line plots municipalities that did not practice compulsory voting.

Fig. 3. Difference in Turnout, All Municipalities, 1990–1994.

¹² Results are consistent at the district level with additional covariates; they are also robust to alternate matching techniques (see Appendix).



Figures show the expected change in vote share at the corresponding level of turnout loss within treated municipalities, based on post-matched data (Model 1). Gray dotted lines indicate 95% confidence intervals, with robust standard errors. The rug indicates the distribution of observed change in turnout between 1990 and 1994.

Fig. 4. Relative effect of Turnout Decline.

5. Discussion

The results of this study suggest that even dramatic declines in electoral participation are not associated with large changes in party vote share. What explains this electoral stasis? Perhaps the most obvious possibility concerns the degree of similarity between voters and non-voters. If the two groups are functionally similar – perhaps driven by Austria’s comparatively high ethnic homogeneity¹³ or low GINI coefficient – then it might be reasonable to expect that the population of voters will remain representative of the population as a whole even as electoral participation declines.

Similar arguments have been used to explain null findings within the cross-national literature on turnout decline (Van der Eijk and Van Egmond, 2007; Petterson and Rose, 2007; Rubensen et al. 2007). But although it may be reasonable to assume that preferences of voters and non-voters do not meaningfully diverge in other contexts, it does not appear to be true in the Austrian case. As the

following analysis will demonstrate, the population of voters and non-voters in Austria differs along a number of relevant dimensions. Taking this puzzle as a starting point, this paper will argue that in order to understand the apparent lack of relationship between turnout decline and vote share, it is necessary to examine the degree to which competitive electoral systems insulate parties against the effect of declining turnout.

5.1. Assessing differences between voters and non-voters

To test the extent to which declines in turnout are correlated with sociodemographic characteristics, I gathered a set of covariates at the municipality and district level. Unfortunately, regional data during the 1980s and 1990s are extremely limited and are for the most part only available for census years (1981, 1991). Accordingly, the dataset includes three pre-treatment covariates from 1991 at the municipality level: total population, the percentage of unskilled workers,¹⁴ and the percentage of residents

¹³ Austria has a large immigrant population, but citizenship remains difficult to acquire for immigrants.

¹⁴ Defined as the percentage of workers with no tertiary education or a vocational degree.

aged 15–65 who are economically inactive. At the district level, the dataset contains the percentage of residents with a secondary-school education or lower, the percentage of retired workers in the population, and the percentage of workers employed in the agricultural/forestry sectors, respectively.¹⁵ Trimming the sample to municipalities and districts that practiced compulsory-voting in 1990, I regressed the observed change in turnout between 1990 and 1994 against a set of covariates from the 1991 census (Table 4). The results suggest that regions with a less educated populace or a higher share of manual workers were more likely to experience a drop in electoral participation. In contrast, rural areas with greater numbers of inactive or retired voters were the least likely to experience large declines.¹⁶

Table 4
Factors correlated with turnout decline.

Municipalities	Estimate	t value
% Unskilled workers 1991	-0.102	-3.54
% Economically inactive population 1991	0.026	2.93
Log population 1991	-0.001	-0.29
Districts		
% Secondary schooling only 1991	-0.401	-2.16
% Retired 1991	0.196	1.45
% Ag-forestry employment 1991	0.624	4.29
Log population 1991	-0.002	-0.14

OLS. Constant omitted. The dependent variable is (Turnout in 1990 – Turnout in 1994). Observations are restricted to municipalities that practiced compulsory voting in 1990. For municipalities, $n = 1009$. For districts, $n = 40$.

Although the analysis at the regional level is based on the actual turnout observed in 1994, the results may be misleading due to the ecological inference bias. Accordingly, I also analyze survey evidence from a series of nationally representative surveys conducted in the year immediately following the October 1994 election (Eurobarometers 43.1, 44.0, and 44.1).¹⁷ Leveraging a question that asked respondents to report their electoral participation in the previous election, Table 5 reveals the difference in means between self-reported voters and non-voters in the 1994 election.¹⁸

Although the Eurobarometer only provides a limited number of socio-demographic variables,¹⁹ the results suggest that voters and non-voters differ in a number of important respects. Compared to reported voters, non-voters are more likely to be male, younger, less educated,

Table 5
Differences between reported voters and non-voters.

	Diff in means	t
Sex	-0.073	-2.00
Age	-10.098	-7.31
Education 16 + years	-0.083	-2.24
Frequent political discussion	-0.078	-3.12
Unemployed	0.071	3.33
Manual workers	0.159	4.65
Support EU	-0.105	-3.11
Support common EU currency	-0.098	-1.94
Priority: drugs & crime	-0.117	-2.52
Priority: human rights	0.107	1.72
Priority: education	-0.018	-0.45
Priority: healthcare	0.015	0.31
Priority: environment	0.174	2.70
Priority: unemployment	-0.019	-0.31

The difference-in-means represent pooled and reweighted results from Eurobarometers 43.1, 44.0, and 44.1, except for the questions concerning support for a common currency and questions on priorities. The former is from 44.0 to 44.1, while the latter set of questions is from 44.1 only. Variable numbers can be found in the Appendix.

and either unemployed or engaged in manual labor. Importantly, these sociodemographic differences appear to translate to different policy preferences as well. For instance, non-voters were less likely to support a common European currency and less supportive of the EU in general. When asked to list policy priorities, they were comparatively more concerned than voters about the environment and human rights, and less concerned about policies combatting drugs and crime.²⁰ While conclusions from this survey evidence are necessarily limited by data availability and the constraints of the questions posed, the results suggest that the null effect of declining turnout in the Austrian case is not driven by a fundamental congruity between the voting and the non-voting population.

5.2. Investigating the relationship between turnout decline and vote loss

In this section, I argue that the limited effect of declining turnout on vote share in Austria can be traced to a competitive electoral system that indirectly protects parties from the consequences of turnout decline.

Although the relationship between turnout and vote share initially seems straightforward, it is reliant on two distinct factors: the magnitude of the decline, and the skew between the preferences of voters and non-voters. As I will demonstrate via a stylized model, the interaction of these two factors in fact creates a stringent set of conditions under which one might expect to observe a consequential shift in election outcomes – conditions which are particularly unlikely to be met in a proportional electoral system.

To model the causal relationship between turnout decline and changes in party vote share, consider the following scenario. A fully proportional electoral district is comprised of two similar municipalities, A and B. Voters in

¹⁵ All data is drawn from Statistik Austria's reported 1991 census results.

¹⁶ Data on relative income levels is available for districts in 1995. Since this variable is measured after the treatment it was not included in the analysis. However, there is a correlation between income and turnout; poorer districts were more likely to experience declines.

¹⁷ Eurobarometer represents the best available data source. Austria did not conduct a national electoral survey at the time of the 1994 election; other nationally representative surveys from the period do not ask about retrospective voting behavior.

¹⁸ Respondents below the voting age, non-citizens, and respondents who indicated that they spoiled their ballot were dropped from the analysis. All weights were adjusted to account for pooling.

¹⁹ Notably, no objective measure of income is available for the 1995 surveys.

²⁰ Although the Eurobarometer asks questions concerning transnational issues, these positions are revealing for domestic preferences as well, especially given the salience of Austria's accession to the EU in June 1994.

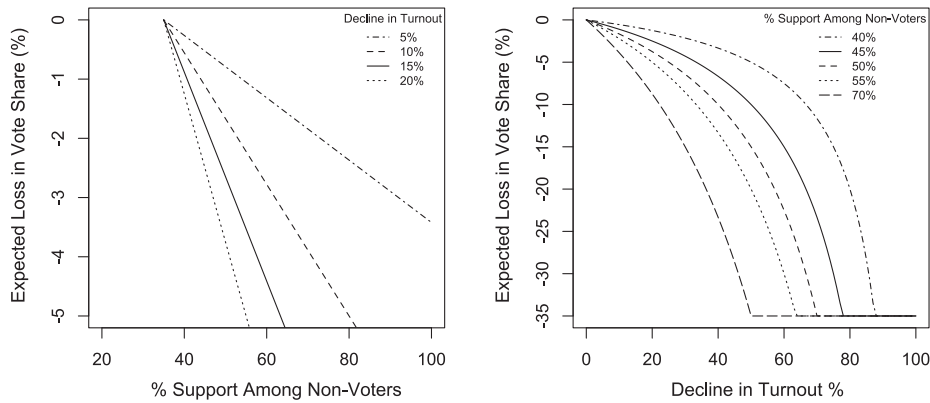


Fig. 5. Modeling the effect of turnout decline.

each municipality have identical and stable party preferences, and the population of eligible voters is the same in each municipality and does not change across elections. In this setting, the expected number of votes received by Party Z in each municipality during Election $t + 1$ can be modeled as follows:

$$\begin{aligned} V_{t+1,m} &= V_{t,m} - (\omega^*(1 - T_{t+1,m}) * N) \\ Y_{t+1,m} &= V_{t+1,m} / (N * T_{t+1,m}) \end{aligned} \tag{4}$$

Where V is the number of votes for Party Z, Y is vote share, T is % turnout, N is the number of eligible voters, and ω is the percentage of abstaining voters in Election $t + 1$ who supported Party Z in the previous election (t).²¹

Now consider the case in which turnout declines in municipality B but remains constant in municipality A.²² The effect of turnout decline on Party Z's vote share can be shown to be:

$$Y_{t+1,A} - Y_{t+1,B} = \frac{V_{t,m}(1 - T_{t+1,A}) - \omega N(1 - T_{t+1,A})}{N * T_{t+1,A}} \tag{5}$$

Fig. 5 plots this model for a hypothetical party that received 35% of votes in Election t .²³ The left-hand side of the figure varies the decline in turnout ($1 - T$) and solves for ω , while the right-hand plot varies ω and solves for ($1 - T$).

The plot provides a visual illustration of the extent to which the influence of declining turnout on vote share depends on the interaction between the magnitude of the decline and the skew in non-voter preferences. For instance, consider a case in which support for Party Z among non-voters is skewed by +15% (that is, 50% of the

abstaining population in Election $t + 1$ are supporters of Party Z). As seen in the right-hand side of Fig. 5, given this set of conditions, turnout would have to decline by approximately 25% in order to produce a loss in vote share for Party Z of only 5%. Alternately, consider a case in which turnout declines by 10%. As shown in the left-hand side of the figure, Party Z would experience a 5% loss in vote share only if 82% of the abstaining voters would have otherwise voted for Party Z.

As the model demonstrates, in order for there to be a meaningful loss in party vote share, there must simultaneously be a large decline in turnout between elections, as well as a large skew in preferences between the voting and non-voting population. This combination of factors is particularly unlikely to hold in proportional electoral systems. First, given that proportional representation generally stimulates turnout, declines in participation are unlikely to be sufficient to affect electoral outcomes in either the short or long run. For instance, in the EU-15 from 1990 to 2013, the largest observed decline in turnout between legislative elections was a mere 11.3%, while average turnout in the most recent round of elections was 75%.²⁴

Second, the presence of an increased number of effective parties in proportional systems reduces the possibility that the preferences of non-voters will be skewed to the extent necessary to influence an election outcome. While it is possible to envisage a scenario in which 80% of non-voters support a given party in a majoritarian system, this degree of electoral skew is much harder to achieve in proportional systems, in which party vote share rarely exceeds 40%.²⁵ The issue may be further exacerbated in Europe by the presence of numerous cross-cutting cleavages, such as immigration, which encourage right-wing parties to target appeals to the traditional constituencies of the Left.

In sum, the volume of turnout decline and the degree of electoral skew are likely to remain somewhat muted in systems characterized by several viable parties. Although additional cross-national investigation is needed to identify

²¹ The 'skew' in preferences between voters and non-voters in Election $t + 1$ is thus $\omega - V_{t,m}/N$.

²² This setup is analogous to a causal design in which a unit is exposed to a treatment (turnout decline), and the resulting effect on vote share is identified by assessing a counterfactual in which the treatment did not take place. Rather than referring to municipalities A and B, the model could be expressed as of $\delta = X_D = 1 - X_D = 0$, where X represents a municipality and D represents the treatment.

²³ Choosing a different vote share for a party in Election t modifies the x-intercept.

²⁴ In Portugal. The UK, Luxembourg, and France are excluded.

²⁵ As of 2013, the average size of the largest party in EU-28 proportional electoral systems was approximately 33%.

the precise manner in which aspects of the party system insulate or expose parties to the consequences of changes in turnout, at a minimum the model suggests that researchers need not assume that non-voter preferences are irrelevant in order to explain the lack of significant turnout effects in developed countries. As suggested by the Austrian case, the dampening effect of political competition may ensure that a significant redistribution of votes does not occur even in contexts in which the sociodemographic characteristics and preferences of the non-voting and voting population are clearly not aligned.

6. Conclusion

Common sense dictates that “who votes, and who doesn’t, has important consequences for who gets elected and for the content of public policies” (Lijphart, 1997). While the latter may be true, this paper provides strong evidence that large shifts in turnout within developed democracies do not necessarily affect the electoral fortunes of mainstream parties (Lutz and Marsh, 2007; Sides et al., 2008; Kohler and Rose, 2010). And while declining participation does appear to result in a loss of support for fringe parties (Belanger, 2004; Bernhagen and Marsh, 2007; Birch, 2009; Jensen and Spoon, 2011), the total effect remains quite small.

The finding that changes in turnout do not necessarily induce meaningful shifts in the political landscape is certainly not new. Nevertheless, the paper moves the existing literature forward in two ways. First, the paper utilizes a strong methodological approach that minimizes the possibility that the ‘null result’ (in this case the lack of an apparent relationship between turnout and vote share) is the result of a biased test. Unlike simulations, a natural experiment deals with outcomes in a realized and consequential setting. Perhaps more importantly, the use of a natural experiment provides a solution to the endogeneity concerns that are endemic to the relationship between participation rates and party choice. Given the research design, any relationship between electoral participation and party vote share should be easily detectable. The fact that no meaningful effect is detected even at dramatic levels of turnout loss, suggests that the null finding is quite robust.²⁶

Second, the paper advances a new argument to explain the lack of relationship between turnout and vote share. Rather than relying on the possibility that voters are representative of the population as a whole or that sociodemographic characteristics of non-voters are ‘lost in translation’ when transformed into concrete party preferences, this paper argues that the electoral system plays a role in buffering the effects of turnout loss. While further comparative research is necessary to fully develop this argument, it provides a possible explanation for why significant turnout effects are occasionally uncovered in majoritarian or local elections²⁷ (e.g. Hansford and

Gomez, 2010; Hajnal and Trounstein, 2005), but are found much less commonly in proportional systems such as Austria.

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Appendix. Robustness Checks and Secondary Tables

Table A1
Eurobarometer Variable Numbers

	43.1	44.0	44.1	Notes
Sex	v441	v583	v1007	
Age	v442	v584	v1008	
Education 16 + years	v438	v580	v1004	Recoded to binary
Frequent political discussion	v39	v34	v65	Recoded to binary
Unemployed	v459	v601	v1025	Recoded to binary
Manual workers	v459	v601	v1025	Recoded to binary
Support EU	v50	v61	v67	Recoded to binary
Support common EU currency		v62	v71	Recoded to binary
Priority: drugs & crime			v369	
Priority: human rights			v370	
Priority: education			v371	
Priority: healthcare			v372	
Priority: environment			v373	
Priority: unemployment			v374	

Table A2
Replication of Table 4 with Coarsened Exact Matching

	1	2	3	4	5
OVP	−0.006 (−0.96)	−0.012 (−1.40)	−0.008 (−1.09)	−0.012 (−1.23)	−0.012 (−1.24)
SPO	0.003 (0.97)	0.006 (2.05)	0.012 (2.94)	0.013 (4.31)	0.016 (5.40)
FPO	0.013 (2.10)	0.014 (1.78)	0.011 (1.54)	0.009 (1.15)	0.007 (0.97)
Green	−0.003 (−1.51)	−0.001 (−0.68)	−0.004 (−2.59)	−0.004 (−1.83)	−0.004 (−1.65)
Minor	−0.008 (−1.70)	−0.008 (−2.15)	−0.011 (−2.49)	−0.007 (−1.69)	−0.007 (−1.81)
Balance					
Lag OVP	0.001 (0.11)	0.002 (0.16)	0.004 (0.24)	0.000 (0.01)	0.000 (0.01)
Lag SPO	−0.000 (−0.01)	−0.002 (−0.18)	−0.001 (−0.05)	0.002 (0.09)	0.002 (0.08)
Lag FPO	0.003 (1.01)	0.004 (0.95)	0.002 (0.26)	0.002 (0.20)	0.002 (0.21)
Lag 2 OVP	0.003 (0.24)	0.005 (0.29)	0.002 (0.12)	0.005 (0.17)	0.005 (0.18)
Lag 2 SPO	−0.003 (−0.24)	−0.003 (−0.22)	−0.001 (−0.07)	−0.005 (−0.18)	−0.006 (−0.22)
Lag 2 FPO	0.000 (0.02)	−0.000 (−0.12)	−0.002 (−0.29)	−0.001 (−0.13)	−0.000 (−0.02)
Lag Green	−0.000 (−0.36)	−0.001 (−0.70)	0.000 (0.02)	−0.001 (−0.41)	−0.001 (−0.41)
Lag 2 Green	−0.002 (−1.37)	−0.002 (−1.33)	−0.001 (−0.33)	0.000 (0.10)	0.000 (0.09)

²⁶ Note that the null result is not driven by party pooling (see Finseraas and Vernby, 2014 for a relevant critique).

²⁷ In which year-over-year changes in turnout are likely to be higher and vote preferences may be more highly skewed.

Table A2 (continued)

	1	2	3	4	5
Log % unskilled 1991		0.037	0.002		0.050
		(1.13)	(0.03)		(0.75)
Log pop 1991			-0.049	-0.035	-0.044
			(-0.66)	(-0.35)	(-0.42)
Log % inactive 1991					-0.008
					(-0.26)

1009 treated, 1279 control municipalities. Standard errors clustered by state ($n = 9$), with t -statistics in parentheses. Estimates represent the difference in mean vote share for each party in 1994, between treated (compulsory voting) and control (non-compulsory voting) municipalities after matching. Each column indicates a separate model matched on the covariates indicated in the lower half of the table.

As a robustness check, I use Coarsened Exact Matching (Iacus et al., 2012), as well as entropy balancing at the district level in order to take advantage of additional covariates. The results are generally analogous to the entropy balancing results, albeit with reduced covariate balance between the treated and control groups.²⁸

Table A3
Difference in 1994 Vote Share after Matching (Districts)

Vote share	1	2
OVP	-0.004 (-0.059)	0.001 (0.16)
SPO	0.016 (2.13)	0.016 (3.71)
FPO	0.000 (0.04)	-0.003 (-0.91)
Green	-0.005 (-1.11)	-0.003 (-1.12)
Minor parties	-0.008 (2.02)	-0.010 (3.51)
Post-match balance		
Lag OVP	0.003 (0.14)	0.004 (0.16)
Lag SPO	-0.010 (-0.50)	-0.010 (-0.52)
Lag FPO	0.017 (1.57)	0.017 (1.60)
Lag Green	-0.003 (-1.08)	-0.004 (-0.10)
Lag 2 OVP	-0.001 (-0.04)	
Lag 2 SPO	-0.012 (-0.57)	
Lag 2 FPO	0.015 (1.49)	
Lag 2 Green	-0.001 (-0.20)	
% Secondary schooling only 1991	0.005 (0.55)	0.005 (0.60)
% Manufacturing employment 1991	0.013 (0.76)	0.013 (0.86)

²⁸ In models 1 and 2, a significant effect is detected for FPO vote share, but this dissipates when matching on additional covariates and may be the result of poor initial balance. No statistically significant effect for the FPO is detectable at the district level.

Table A3 (continued)

Vote share	1	2
% Ag forestry employment 1991	0.003 (0.35)	0.003 (0.36)
% Retired 1991	-0.008 (-0.97)	-0.008 (-0.97)

$n = 40$ Treated, 81 Control. Weighted t -statistics in parentheses, clustered by state ($n = 9$). All models use the Ebalance matching algorithm (Hainmueller, 2012).

Table A4

Placebo – Difference in Avg Turnout and Vote Share, 1986 – 1990. As a robustness check, I test whether any statistically significant effects can be detected via the difference-in-differences design, using 1990 as a false threshold.

	Compulsory		Non-Compulsory		DID	t
	1994	Δ 1990	1994	Δ 1990		
Mean turnout	0.910	-0.025	0.874	-0.035	0.010	1.05
Mean vote share: OVP	0.432	-0.093	0.428	-0.080	-0.013	-0.77
Mean vote share: SPO	0.339	0.010	0.339	0.015	-0.005	-0.54
Mean vote share: FPO	0.171	0.062	0.134	0.051	0.011	1.25
Mean vote share: Green	0.030	-0.003	0.029	-0.002	-0.001	-0.24
Mean vote share: minor parties	0.029	0.022	0.020	0.015	0.007	0.48

Percentages. $N = 1009$ Treated, 1279 control municipalities. Difference-in-difference standard errors clustered by state ($n = 9$).

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