


Voter turnout and abstention pricing: quasi-experimental evidence on the effects of a marginal increase in the monetary enforcement of compulsory voting¹

Pascal Horni 

School of Management and Law, Zurich University of Applied Sciences, Institute of Public Management, Winterthur, Switzerland

ABSTRACT

In political science literature, it is often argued that compulsory voting can serve as a remedy against turnout inequality and thereby contribute to more egalitarian societies. However, the literature neglects differences between compulsory voting systems, and most studies consider the obligation to vote as a dichotomous variable. As a result, relatively little is known about the causal effects of particular stimuli and features of compulsory voting. By exploiting quasi-experimental conditions from direct-democratic decision-making in Switzerland, this research assesses the degree to which abstention fines account for a citizen's willingness to vote under a compulsory voting context. The findings reveal that a marginal increase in monetary sanctions for voter-abstention leads temporally to a sizeable increase in turnout. However, it does not necessarily contribute to the expression of preferences by those traditionally underrepresented in direct-democratic decision-making since a third of those motivated to vote by the threat of a heavier fine will cast an empty or invalid ballot. The effect on turnout is further found to be moderated by income.


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Introduction

One of the fundamental prerequisites for maintaining a well-functioning democracy is civic participation. However, many advanced democracies

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CONTACT Pascal Horni  pascal.horni@zhaw.ch

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face declining voter turnout (Hooghe and Kern 2017), and more and more citizens are not making use of their voice. Low voter turnout is considered harmful to modern societies and democracies since the lower socio-economic strata turn up less frequently at the ballot box (Hooghe and Pelleriaux 1998; Gallego 2010) and thus remain unheard. Consequently, and as famously argued by Lijphart (1997), electoral and policy preferences from that section of society do not feed into the political system. Against this background, compulsory voting is considered an adequate remedy to redress low turnout rates and turnout inequality (see Birch 2009; Dahl, 1989; Downs 1957; Hill 2006; Lacroix 2007; Schäfer 2011).^a In that respect, early work on compulsory voting provides extensive empirical evidence demonstrating its positive effects on turnout (Powell 1986; Jackman 1987; Jackman and Miller 1995; Blais and Dobrzynska 1998; Hooghe and Pelleriaux 1998).

More recent literature has shifted the spotlight from compulsory voting's mobilizing power towards the impact of compulsory voting on citizens' voting habits (Gaebler, Potrafke, and Roesel 2017; Bechtel, Hangartner, and Schmid 2018), political knowledge (Sheppard 2015), willingness to acquire information (Singh and Roy 2018), and on the channels by which the enforced mobilization of the lower socio-economic strata translates into politics or policies. The latter stream comprises studies on the impact of compulsory voting on electoral outcomes (Brunell and DiNardo 2004; Fowler 2013), the ideological range of governments (Jensen and Spoon 2011), the quality of votes cast by disinterested voters (Selb and Lachat 2009; Singh 2016; Hooghe and Stiers 2017), the outcomes of public referenda (Bechtel, Hangartner, and Schmid 2016), and government spending (Hoffman, León-Ciliotta, and Lombardi 2017). This newer wave of literature mainly speaks to the merits and limits of compulsory voting and engages with its remedial power to alleviate potential policy biases and legitimacy issues when a large number (or a given section of society) abstains and remains silent.

However, the existing literature frequently considers compulsory voting as a dichotomous variable, although comparative research (Birch 2009, 2016; Panagopoulos 2008) suggests that compulsory voting systems can take many different forms. As a result, relatively little is known about the causal effects of specific stimuli, features, and components associated with compulsory voting systems. With the exceptions of León (2017) and Gonzales, León-Ciliotta, and Martinez (2019), who studied the impact of changes in fines for abstaining in Peru, there have been few studies into the consequences and implications of (re-)configuring compulsory voting systems.

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This present study goes beyond analysing the aggregate effects of compulsory voting systems and, instead, focuses on monetary enforcement as a possible and relatively common feature (see Birch 2016). Arguably, the effects of compulsory voting unfold differently depending on the severity of monetary sanctioning and the level of the fine. In a similar vein as León (2017) and Gonzales, León-Ciliotta, and Martínez (2019), but for a European context, this study engages with the question of how much abstention fines account for citizen's willingness to vote in a compulsory voting context in isolation of other possible participation mechanisms. Empirically, this study exploits quasi-experimental conditions from Swiss direct-democratic decision-making. More specifically, this research studies the impact of a marginal increase in abstention fines on voter turnout and, therefore, offers relevant empirical insights concerning the configuration of compulsory voting systems and abstention pricing.

The decision to vote and the price of abstention

Considerable academic interest has been devoted to what drives voter turnout and motivates citizens to participate in elections. When searching for determinants and motives of abstention and voting, two broad schools of thought have emerged.

The first of these supposes that individuals are primarily concerned about themselves and opt to maximize their personal utility by applying a cost-benefit calculus (Downs 1957; Tullock 1967). They adopt risk-averse behaviour to minimize any feelings of regret in the case of non-participation in closely fought elections (Ferejohn and Fiorina 1974) and seek psychological benefits through the fulfilment of duty (Riker and Ordeshook 1968) or when under social pressure (Knack 1992; Funk 2010; Gerber, Green, and Larimer 2008).

In contrast, the other school of thought argues that people are not primarily self-interested utility maximizers but consider collective interests when voting (in an antithesis to rational voter theory) (see, for example, Coleman 2004 or Blais and Achen 2019). Various recent experimental evidence suggests that prosocial stimuli positively affect voter turnout (Arceneaux and Nickerson 2009; Davenport et al. 2010; Gerber, Green, and Larimer 2008).

Arguably, the nexus between compulsory voting and turnout could be mediated by several mechanisms and individual-level determinants. An institutionalized duty to vote can arouse norm conformity and induce citizen internalization of a political participation norm (Bechtel, Hangartner, and Schmid 2018) – irrespective of how strict enforcement and sanctions are – simply because voting is an explicit civic duty (Funk, 2007). Hitherto, comparative literature suggests that the severity of sanctions positively correlates to voter turnout (Panagopoulos 2008; Singh 2011; 2015). However, a

potential pitfall of this literature is that it relies on exploiting cross-country differences in voting institutions whose diversity, however, is difficult to be captured and that their setting cannot establish a causal relationship.

Compulsory voting systems with monetary sanctions certainly play well into the cost calculus mechanism, which conceptualizes the decision to vote as the outcome of individual cost–benefit considerations and predicts participation in cases where the cost of voting is less than the personal benefits attached to voting (Downs 1957). Voting costs predominantly consist of time/opportunity costs (Downs 1957; Tullock 1967),² while abstention fines modify the cost–benefit structure and essentially attach a value to non-participation. Thus, citizens are confronted with the choice of either bearing the monetary cost of abstention or the time cost associated with voting. Compulsory voting systems counteract the potential negative outcome of an individual's cost–benefit calculus by making abstention less attractive and reducing the relative cost of voting. From a rational voter perspective, one would expect turnout rates to increase with higher penalties for abstention – while casting an invalid or blank vote might be considered as a rational strategy to reduce the time costs involved in making a considered choice *and* avoid the fine for non-participation. It, therefore, comes as no surprise that countries or regions with compulsory voting also experience higher rates of invalid balloting (e.g. Power and Roberts 1995).

However, behavioural economics taught us that monetary incentives not necessarily work in the standard economic, rational fashion and that fines can also be perceived as a price (Gneezy and Rustichini 2000; Gneezy, Meier, and Rey-Biel 2011) and crowd-out intrinsic motivation of prosocial behaviour (e.g. Ariely, Bracha, and Meier 2009). In a compulsory voting context, citizens may be tempted to consciously accept the abstention fine and thereby assuage their consciences resulting from freeriding and for not contributing to the maintenance of a well-functioning democracy. Several compulsory voting scholars (Panagopoulos 2013; Maldonado 2015; Blais and Achen 2019) raise the idea that abstention fines could also crowd out intrinsic motivation. Yet, this conjecture remains empirically untested, but the work of Maldonado (2015) reveals a slightly lower level of civic duty among Peruvians who were reminded of abstention fines. This leaves the direction and magnitude of abstention fines an open question.

Only two studies have engaged in researching the effect of abstention fines, and respective research is limited on a geographical scope, as both

²Voting costs are influenced by a series of factors and there is vast empirical evidence demonstrating a negative relationship between the cost of voting and actual turnout. Among other factors, the availability of postal voting (Luechinger, Rosinger, and Stutzer 2007; Funk 2010), e-voting (Germann and Serdült 2017; Germann 2021), and prepaid postage (Schelker and Schneiter 2017), the opening hours of polling stations (Potrafke and Roesel 2020), and the possibility of preregistration (Holbein and Hillygus 2016, 2017) have all been identified as relevant institutional variables.

studies are from the Peruvian context. Both studies were conducted in the context of a national reform implementing a differential reduction of abstention fines across districts in 2006. The first of those two studies (León 2017) provides results from a field experiment that randomly provided different information about the size of the reduction in abstention fines to voters in 29 Peruvian villages just before the general municipal elections. This revealed that depending on the created individual-level variation in perception of the reduction in abstention fines, turnout decreased with a cost elasticity of -0.22 [Δ turnout/ Δ abstention fine].

The second study by Gonzales, León-Ciliotta, and Martinez (2019) exploited the district-level variation in abstention fines stemming from the reform and studies the effect of the size of abstention fines on turnout as well as invalid and blank votes. The authors showed that larger fines positively impacted voter turnout with a cost elasticity of 0.03. Although both studies offer compelling causal evidence concerning the empirical question of the degree to which abstention fines account for a citizen's willingness to vote, they both essentially study the effect of a penalty reduction rather than an increase.

Against this background, the present study crafts empirical evidence on the effect of an increase in abstention fines in a west European context, allowing us to arbitrate between rational choice and behavioural-economic explanations. In so doing, it contributes to understanding the impact of abstention fines on a citizen's willingness to vote and exploits a natural experiment in the context of Switzerland's direct-democratic decision-making tradition.

Institutional background

Direct-democratic decision-making in Switzerland is an ideal laboratory for researching the effect of abstention fines on turnout, as citizens are quite frequently polled in public referenda, providing frequent opportunities to observe voting behaviour. Moreover, there is variation in voting laws between the subnational jurisdictions, the so-called cantons. Several cantons enforced mandatory voting in the past, but the last remaining canton to retain this practice is the German-speaking Schaffhausen, in the northeast of the country. An obligation to vote is anchored in its cantonal constitution, and while the law on elections operationalizes compulsory voting and specifies sanctions for non-compliance, there is also scope for exceptions. Here, mandatory voting includes elections and referenda at community, cantonal, and national level. Persons of 65 years and over and those who submit a valid excuse no later than three days after the ballot are exempt from any penalty. Acceptable reasons for missing a vote are vacations, professional and personal commitments, illness or injury, and military or civil protection service. Returning the ballot paper is also recognized as an excuse.

Consequently, anyone who fails to cast their vote, submit an excuse, or return their voting documents will be fined. Studies have shown that compulsory voting in the canton of Schaffhausen results in a significantly higher proportion of blank votes compared to other cantons but also that political interest and knowledge is higher among its voters (Schwegler 2009).

The abstention fine in Schaffhausen remained at three Swiss francs (CHF) between 1973 and 2014. Due to inflation, the penalty's real value fell by 140%, making it much less of a deterrent. Given the gradual increase of inflation-adjusted wages in Switzerland in recent decades, the monetary impact on a household budget further moderated over the years. As a result, the canton of Schaffhausen amended its voting legislation in 2014 and doubled the abstention fine to CHF 6 (State Chancellery of the Canton Schaffhausen 2014a). For comparison, this corresponds approximately to the price of one Big Mac[®] purchased in Switzerland or 1.28 Big Macs purchased in the United States.³ The doubled fine for abstainers was implemented on 1 January 2015 (State Chancellery of the Canton Schaffhausen 2014b). This move altered the substance of the decision to vote, as citizens are now confronted with a choice between a higher monetary penalty, if they abstain and take no action, or accepting time costs of voting or excusing themselves.

Conceptually, the increase in monetary sanctioning of abstention has changed the choice set available when deciding whether to vote. Irrespective of the properties of the cardinal utility function underlying an individual's personal voting decision, but dependent on the ordinal measure of on how citizens choose between the two options (technically only observed on an aggregate-level in voting records) allows us to question the impact of abstention fines on the willingness to vote. Based on the consideration that doubling abstention fines has changed the choice-set for citizens in the canton of Schaffhausen – while the situation in all other cantons has remained unchanged – a quasi-experimental setting for federal referendums emerges.

Research design, data and methodology

Empirically, this study aims to identify the causal effect of an increased monetary sanction for abstainers on voter turnout and the resulting percentage of blank and invalid casted votes. Municipal-level polling data for national referenda were collected from the official register of the Federal Statistical Office⁴ for the two years before and the year after the fine increase in Canton Schaffhausen – namely, January 2013 to December 2015. In these three years,

³The Big Mac Index is an indicator frequently used by economists to illustrate purchasing power. See <https://www.economist.com/big-mac-index>.

⁴See Appendix 1 for data sources and Appendix 2 for descriptive statistics.

voters were polled on 29 federal referenda held on 10 national referendum days, $t = \{1, \dots, 10\}$. The unit of analysis for this study are municipalities, $i = \{1, \dots, N\}$, which allows effect estimation at the lowest level for which vote data is available and at the smallest possible jurisdictional compound of voters.

Identification of the causal effect of increased monetary sanctions would ideally require a comparison of turnout of the same units (/municipalities) at the same time (/referendum date) under the enhanced fining regime as well as the status quo. Given that it is only possible to observe an outcome under one condition at a time – either the status quo or with the harsher sanction – causal identification must rely on a counterfactual imputation. Consequently, this study employs a difference-in-differences (DID) identification strategy. More specifically, it exploits the quasi-experimental conditions from the above-mentioned policy change and associated spatial variation in exposure to a changed choice-set.

The increased monetary sanction for voting abstainers in the canton of Schaffhausen has altered the choice sets there, but the situation has remained unchanged everywhere else. A binary variable indicating exposure to the treatment [D] (i.e. the change in the choice set) was considered. It is coded one for municipalities within the jurisdictional borders of the canton of Schaffhausen and 0 otherwise. In our quasi-experimental setting, municipalities within the canton of Schaffhausen make up the treatment group ($D = 1$), while the two neighbouring Swiss cantons – Thurgau and Zurich – serve as a comparison group for baseline imputation ($D = 0$). The rationale for constructing the baseline based on geographical proximity is to reduce possible variation of unobserved factors such as differences in culture, which are arguably fewer among neighbouring cantons. To differentiate the time periods before and after increased monetary sanction, a respective time indicator [P] (taking the value 1 for the post-treatment period) was included. Following the DID causal inference approach, this study estimates the average treatment effect (ATE) within a municipality that materializes depending on exposure to the treatment ($D = 1$) and temporally after the implementation ($P = 1$), by using the in-time difference in expected outcomes of the untreated group [$E(Y^{P=1}|D=0) - E(Y^{P=0}|D=0)$] as counterfactual of how the outcomes would have developed in the treatment group in the absence of the increase in monetary sanctioning. Accordingly, as typical of the DID approach, it is assumed that in absence of the treatment, the time trend of the treatment group would have developed parallel to the time trend of the comparison group. It is supposed that the outcomes under study would have followed the same time trend in any given municipality irrespective of its subnational territorial belonging determining exposure to the treatment. Turnout rates and the percentage of blank and invalid casted ballots should have developed similarly over time in municipalities

within the canton of Schaffhausen and those within the two neighbouring cantons (as presented in the section on robustness checks, this assumption holds).

Identification of municipal-level average treatment effects is achieved by comparing the in-time differences of the treatment and comparison group $[(E(Y^{P=1}|D=1) - E(Y^{P=0}|D=1)) - (E(Y^{P=1}|D=0) - E(Y^{P=0}|D=0))]$.

To estimate the effect, the following fixed effects regression is applied:

$$Y_{it} = \alpha + \beta X_{it} + \delta(D_i \cdot P_t) + \mu_i + \gamma_t + \varepsilon_{it};$$

where α is the constant term, X_{it} a vector of covariates varying in time and across municipalities, δ the coefficient capturing ATE, μ_i a municipal-level fixed effect, γ_t a referendum day fixed effect, and ε as an idiosyncratic error term. The covariate vector X_{it} ⁵ comprises the percentage of women, percentage of young people, percentage of people at retirement age or above, percentage of low-income citizens⁶, and a subnational election dummy taking account of cantonal-level elections that coincide in time with the referendum dates.⁷

However, considering that municipalities within the treatment group could be different from those considered as baseline, heterogeneity is reduced by pairwise matching (Cf. Rosenbaum and Rubin, 1983) of one treated and one untreated municipality based on relevant covariates (i.e. the percentage of women, percentage of young citizens aged 30 or less, percentage of citizens aged 65 or over, percentage of citizens with low incomes, vote share for left-wing parties, and size of the electorate). Accordingly, the average treatment effects on both outcomes of interest are estimated for the full panel of municipalities ($N=268$) and the matched sample ($N=52$). This further allows for relaxing dependence on modelling assumptions (see Ho, Imai, King and Stuart, 2007).

To account for autocorrelation and heteroscedasticity, robust standard errors were computed as suggested by Bertrand, Duflo, and Mullainathan (2004) and two-way clustering was implemented as encouraged by Cameron, Gelbach, and Miller (2011). Standard errors are clustered on municipality and referendum day level to account for dependencies of standard error variance. In consideration of possible concerns related to the small

⁵Regarding the identification of covariates varying across time and units, it should be noted that education, age, and gender are the main turnout determinants of voter participation in Switzerland (Lutz, 2016). This is why turnout research in the context of Swiss direct-democratic decision-making usually controls for these variables (see, for example, Germann and Serdült 2017).

⁶The covariate 'percentage of citizens on low incomes' (PoLI) is operationalized as a municipalities' share of citizens with an annual net-income below CHF 40'000. This threshold is set with reference to the statutory minimum wages, which some, but not all, Swiss cantons have anchored. The respective data is sourced from the Swiss Federal Tax Administration.

⁷Cantons and municipalities sometimes schedule subnational votes on a national referendum day. However, subnational votes other than cantonal-level elections are rarely considered drivers of voter turnout. Consequently, this should not bias the results.

number of clusters for consistently estimating the standard errors (see, for example, Cameron, Gelbach, and Miller 2008), clustered wild bootstrapped p -values were calculated complementary for the full sample models (see Appendix 5). However, the wild-bootstrap p -values were similar to the p -values found using the clustered standard errors.

Results: empirical evidence on the effect of a marginal change in abstention fines

Effect estimates

Table 1 summarizes the municipal-level average treatment effects on turnout rate (Models 1–3) and the percentage of blank and invalid casted ballots (Models 4–6) for both the full sample with all municipalities in the two neighbouring cantons ($n = 268$) serving as baseline, and for the matched sample ($n = 52$). The increase in abstention fines and therewith associated change in citizens' voting decision choice-set increased turnout rates by 3.9 percentage points compared to both groups serving as baseline (i.e. the comparison group consisting of all municipalities in the two neighbouring cantons and the matched group). This equates to a cost elasticity of ~ 0.039 (respectively ~ 0.057 using the midpoint method). The effect on blank and invalid casted ballots is also positive but smaller than the effect on turnout. Similar estimates result when the pre-treatment period ($P = 0$) is set to one year (see Appendix 7) and when running the fixed-effects regression without covariates (see Appendix 4).

Since abstention fines are in most municipalities issued cumulative once a year, it could be possible that citizen's reaction is lagged if they did not take notice of the increase and just realize the it once they receive the bill. To control for this possibility and to assess sustainability of effects, fixed effect regressions were also run for a post-treatment period of two years (see Appendix 8). However, results indicate that the effect holds only temporarily and does not last for the subsequent year (see Appendix 9).

Generally, (quasi-)experimental research is helpful for revealing causal effects but does not necessarily shed light on the underlying causal mechanisms (Deaton 2009). Since this study's quasi-experimental design builds on municipal-level aggregate data, we cannot observe reactions to the change in choice-set on an individual level nor fully reveal the decision-making mechanisms at work when deciding whether to vote. As mentioned earlier, this study captures the aggregate discrete choices. The channels over which the stricter monetary sanctioning translates into the decision to participate thus remain coated. Theoretically, the increase in turnout could be explained by the rational choice motif of fine avoidance as well as stimulation of norm

Table 1. DID effect estimates.

	<i>Dependent variable:</i>					
	Turnout [in %]			Blank and invalid [in %]		
	(1)	(2)	(3)	(4)	(5)	(6)
DID	3.919*** (0.519)	3.987*** (0.672)	3.987*** (1.153)	0.772*** (0.207)	0.917*** (0.221)	0.917*** (0.272)
PoW	-0.173 (0.226)	-0.26 (0.467)	-0.26 (0.404)	-0.007 (0.073)	-0.024 (0.185)	-0.024 (0.163)
PoRP	0.058 (0.185)	-0.06 (0.242)	-0.06 (0.247)	0.018 (0.067)	-0.0005 (0.165)	-0.0005 (0.168)
PoY	-0.174*** (0.056)	-0.244 (0.206)	-0.244*** (0.091)	0.022 (0.027)	0.163 (0.127)	0.163 (0.121)
PoLI	0.048 (0.070)	0.067 (0.075)	0.067 (0.066)	-0.029 (0.020)	-0.068*** (0.024)	-0.068*** (0.026)
Subnational elect.	2.998*** (0.341)	3.568*** (1.109)	3.568 (2.641)	0.344*** (0.074)	0.458** (0.213)	0.458*** (0.028)
Municipality FEs	Yes	Yes	Yes	Yes	Yes	Yes
Referendum day FEs	Yes	Yes	Yes	Yes	Yes	Yes
Observations	2680	520	520	2680	520	520

Note: This table presents the results from two-way fixed effects regressions estimating difference-in-differences (DID) treatment effects on direct-democratic referendum turnout (Models 1–3) and the percentage of blank and invalid ballot papers (Models 4–6), including the covariates. The models were run for the full sample (Models 1 and 4) and the matched sample (Models 2–3 and 5–6). Standard errors clustered at the municipality level (Models 1 and 2, 4 and 5) and both municipality and referendum day level (Models 3 and 6) are given in parentheses. The covariates are the following: percentage of women (PoW), percentage of citizens at retirement age or above (PoRP), percentage of young voters (between 18 and 30), and percentage of citizens on low incomes (PoLI).

* $p < .05$.

** $p < .01$.

*** $p < .001$.

conformity, as the fine increase reminds citizens that voting is an explicit civic duty.

Given that the identified effect on turnout rate is accompanied by an increase in the percentage of blank and invalid casted ballots, it appears that a small fraction of those additionally pushed to the polls is perfectly utility-maximizing. These citizens opt to avoid the monetary cost of abstention fines and reduce the time cost of casting their vote by not engaging in the cognitive preference-building and decision-making process. An alternative explanation for the increase in blank and invalid ballots is that those additionally pushed to the polls are politically less sophisticated and unable to process complex information, or they are protest voters expressing their general discontent through blank votes (Moral 2016). Knowing from the Peruvian context that politically uninformed and less interested citizens perceive a change in abstention fines more keenly (León 2017), it is likely that increased abstention fines push the uninterested to the polls. If this is the case, the increase in blank and invalid ballot papers could result from this indifference. In sum, and given that this study identifies only municipal-level aggregate effects, utility-maximization and political disinterest of a minority pushed to vote could be conceived as explanatory mechanisms for the increase in blank and invalid ballot papers.

When controlling for interaction effects between the treatment and a municipality's share of low-income citizens (see Appendix 6), the main effects are offset in favour of the interaction term. A municipality's share of citizens on low-income moderates the magnitude of the effect on turnout, but not the effect on blank and invalid votes. Consequently, it seems like the increased abstention fines primarily trigger low-income citizens, which is logical from a rational voter perspective and in line with León's (2017) findings on abstention fines. However, we should consider such an interpretation with caution, as the effect on turnout is observed on municipal-level aggregate. Based on aggregates it is always difficult to draw inferences on behaviour of individuals (i.e. ecological fallacy). Figure 1 below plots the relationship between the effect on voter turnout and a municipalities' share of citizens on low incomes.

Cross-country effect comparison and external validity

The quasi-experimental conditions exploited by this study and the institutional context of Swiss direct democracy with its rich tradition of public referenda are unique, so this study's external validity might be limited. Nonetheless, the effect findings identified by this study are mostly consistent with previous work on abstention fines (i.e. León 2017; Gonzales, León-Ciliotta, and Martinez 2019), although these studies also have their geographical (and other) limitations. Other than here and in the Peruvian research, the effects

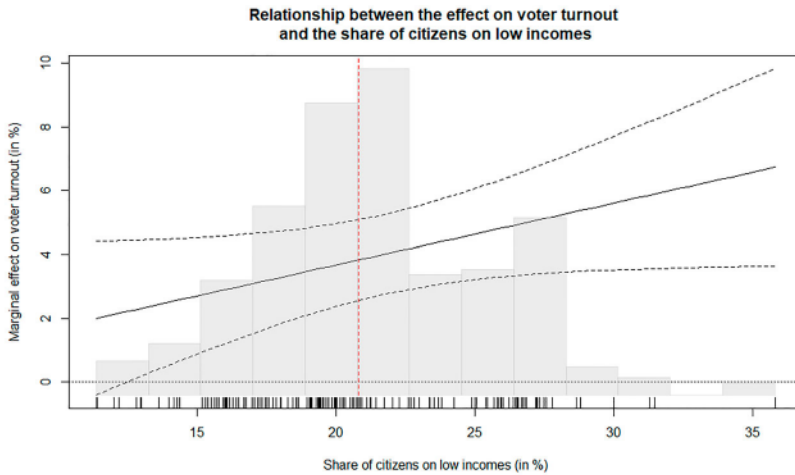


Figure 1. Marginal effect of a municipalities' share of low-income citizens. Note: The above-depicted plot shows the marginal effect of the abstention fine increase on voter turnout (including 95% confidence interval bounds) for the matched sample moderated by a municipalities share of citizens on low incomes. The histogram in light-grey colour summarizes the observed-levels of citizens on low incomes among the matched municipalities, whereby the dotted vertical line indicates the median.⁸

of stricter monetary penalties for abstainers under compulsory voting regulations have yet to be investigated. Hence, a comparison of effect sizes with other countries is not possible. Interestingly, Gonzales, León-Ciliotta, and Martinez (2019) found that an USD 3 (/10 Peruvian Sol) higher abstention fine leads to a 0.5% higher turnout rate. This Swiss study also found the effect of an increase in abstention fines equal to USD 3 produced a higher turnout, albeit temporary. It is surprising that in a high-income country like Switzerland, the impact of a USD 3 higher fine was more than seven times greater than for an upper-middle-income country such as Peru. However, the Peruvian study was crafted against the background of a differential reduction in abstention fines. This study, in contrast, looked at the effect of a fine increase. Hence, differences in effect magnitude of a USD 3 higher abstention fine between the two countries might be attributed to the signal sent with the policy change. A fine reduction arguably sends a different signal than a fine increase.⁸

Robustness checks

Various robustness checks were performed such as calculation of two-way clustered standard errors, bootstrapping (see Appendix 5) and inclusion of

⁸This plot is based on Anton Strezhev's R code implementation of Stata's "marginsplot" command.

two comparison groups. As noted previously, the exposure of municipalities to the treatment [*D*] correlates with subnational territorial borders. Indeed, municipalities in the canton of Schaffhausen could be completely different from those in the two neighbouring cantons serving as baseline. Municipality/unit-specific time-varying confounding variables could thus bias the results. For this reason, relevant covariates were included in the regression model and to reduce heterogeneity between treated and comparison units/municipalities, pairwise matching was conducted based on propensity scores and the nearest neighbour method. [Figure 2](#) compares propensity scores of the full, unmatched panel ($n = 268$) with the sample making up the matched panel ($n = 52$). As we can see, propensity score matching could produce more (but not entirely) balanced panels concerning the covariates compared to raw data (/full sample).

To test the validity of the primary identifying assumption of parallel trends, the development of the unconditional means was plotted. [Figure 3](#) plots the data and time trends in turnout (upper panels) and for municipalities within the canton Schaffhausen (the treatment group), municipalities within the two neighbouring cantons (Comparison Group A), and municipalities matched to the treated units (Comparison Group B).

Visual inspection lends credibility to the assumption that parallel trends are present for the development of turnout. However, one could raise doubts about whether parallel trends are present in the pre-intervention period for the percentage of blank and invalid votes (as these are not perfectly parallel). We should thus consider effect estimates on blank and invalid votes with caution. In addition, an in-time placebo test (see Angrist

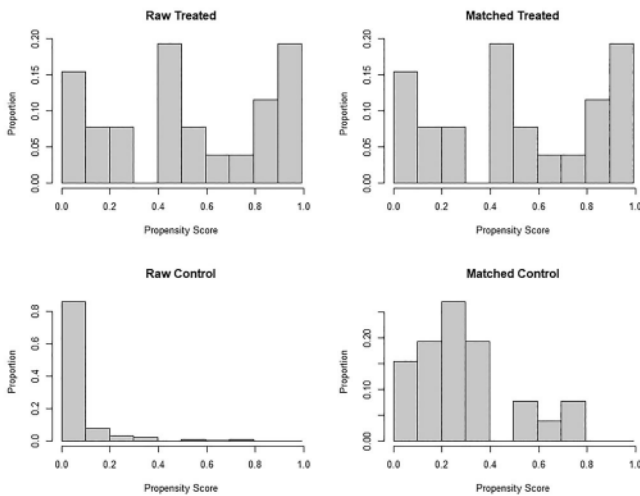


Figure 2. Propensity scores of matched and unmatched data.

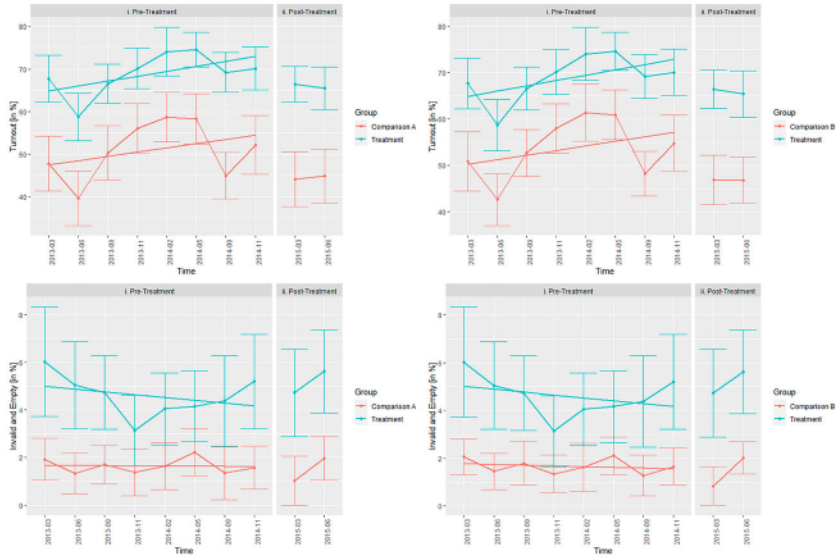


Figure 3. Pre- and post-treatment trends. Note: Upper panels show unconditional average turnout rates (with 0.95 confidence envelopes) for municipalities whose citizens' choice-sets were exogenously affected by the policy change (i.e. the treatment group) and for those unaffected (i.e. the comparison group) for national direct-democratic referendums between January 2013 and December 2015. Lower panels show unconditional averages of the percentage of blank and invalid votes (with 0.95 confidence envelopes). Left panels comprise the comparison group (A) consisting of all municipalities within the two neighbouring cantons, whereas panels on the right consider the matched comparison group (B).

and Pischke 2009) was performed for the matched data. A fixed-effects regression was run with the same model specification for the pre-treatment period ($P = 0$) – 2013 and 2014 – but pretending the penalty increase was imposed in January 2014. This in-time placebo test did not show robust significant results and, therefore, supports the plausibility of the prevalence of parallel trends formally (see Appendix 10).

Conclusion

This study set out to examine how much abstention fines account for citizen's willingness to vote in isolation of other features of compulsory voting systems and to estimate the effect of a 100% increase in abstention fines. It thereby complements existing literature on electoral reform and compulsory voting institutions. Based on quasi-experimental conditions from direct-democratic decision-making in Switzerland, this study presents evidence that a marginal increase of abstention fines (from three to six Swiss Francs / \approx USD) can temporally have a sizeable effect on turnout (i.e. with 95%

confidence between 2.84% and 4.99%). The study captured the municipal-level aggregates of discrete choices made by citizens regarding participation in public referenda before and after a 100% increase in abstention fines in comparison to municipalities whose citizens' choice sets remained unchanged.

In line with previous studies researching the effect of a decrease in abstention fines (León 2017; and Gonzales, León-Ciliotta, and Martinez 2019), this study finds stricter monetary sanctioning of abstention has a positive effect on turnout, although the effect lasts no longer than one year. The effect on turnout tends to be larger in municipalities (electoral districts) with a higher share of citizens on low-income. With an increased turnout, the percentage of blank and invalid casted ballot papers also rises, though only by a third of the increase in turnout, suggesting that this small proportion of "new" voters are utility-maximizing. This study thus finds some grounds in favour of rational voter theory. However, attributing the identified effects purely to the rational choice mechanism might fall short. First, the motivation of those casting an empty or invalid ballot in response to the fine increase remains unknown and it cannot a priori be laid out as utility maximization. Null voting in public referendums could also be the result of political disinterest or the failure to process complex information. Second, the identified increase in turnout does not necessarily have to be a rational reaction to avoid the fine but could rather also evolve from norm conformity (Cf. Bechtel, Hangartner, and Schmid 2018). It is conceivable that the increase of abstention fines stimulated norm-conformity, as it reminded people that voting is an explicit civic duty. In light of the low value of the fine and the transient nature of the effect, it could be inferred that instead or in addition to fine avoidance a norm conformity mechanism is at work. However, based on the presented evidence (and by the design of the study), the channel over which the stricter sanctioning stimuli translates into the decision to participate cannot ultimately be pinned down to one of the explanations. Future research is thus invited to join this article in arbitrating between rational voter theory and possible behavioural alternatives and generally in studying the effects of features associated with compulsory voting systems.

Since the empirical evidence presented in this study is crafted from quasi-experimental conditions, a few words of caution should be issued. One of the main limitations underlying the inferences drawn is that the baseline is not a perfect counterfactual of the treatment group. The comparison group is composed of municipalities whose citizens' choice set remained unchanged, but which have not been under a compulsory voting regime. As shown with a series of robustness checks, the comparison group is capable of mimicking the basic time trend, but it should be noted that an ideal comparison group would have consisted of untreated municipalities that were under the same compulsory voting regime. In addition, the external validity of

this study might be constrained, as it is crafted in the institutional context of the Swiss direct-democratic decision-making tradition.

Although this study finds that a marginal increase in abstention fines can be a substantial stimuli for voter turnout and indeed motivate two thirds of those additionally pushed to the polls to effectively express their preferences, the findings of this study do not suggest that stricter monetary enforcement of compulsory voting is a panacea for fixing turnout inequality. In view of the transient nature of the effects as well as the possibility that a norm conformity mechanism is at work and that citizen's reaction to the fine increase is possibly only partially driven by monetary incentives, other stimuli, such as arousing norm conformity, should be considered equally in the (re-)configuration of compulsory voting systems.

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ORCID

Pascal Horni  <http://orcid.org/0000-0002-4012-6754>

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