Political institutions, society, and the functioning of democracy

Four essays in political economy

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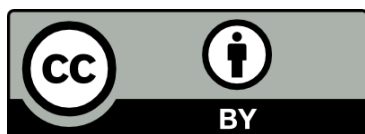
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Acknowledgments

Growing up in Switzerland, I have always been deeply grateful for the numerous opportunities available. From receiving a quality education to growing up in a peaceful environment, I have had the freedom to express my opinions, participate in politics, and even pursue a PhD. However, this stark contrast between Switzerland and many other countries, where populations suffer from war, oppressive regimes, limited rights, and a lack of economic opportunities, has always made me ponder the factors determining a nation's success or failure. It was through the lens of economics that everything started to make—at least a bit—more sense to me. I realized that political and economic institutions are at the core of every state and society. My interest in (democratic) institutions drove my decision to pursue a PhD in economics. Through my research, I aspired to contribute to a deeper understanding of how political institutions and societies interact, ultimately shedding light on the factors that drive the success or failure of nations.

These years at the University of Fribourg have passed quickly, which is generally a good sign indicating a fulfilling experience. As is always the case when something ends, many people deserve a big thank you, for without them, I would not be where I am today. First and foremost, I would like to express my sincere appreciation to my PhD thesis supervisor, Reiner Eichenberger. I recall him emphasizing early on in my academic journey that it was crucial to not only excel in scientific research but also to become a "good economist". Although I was and am still uncertain about what constitutes a "good economist," I was determined to become one. Today, I believe that I have moved closer to this goal. I have realized that the last few years have helped me question various ideas and concepts more critically, which I would have previously taken for granted. Or, as Yves' motivation card in our office puts it:

> "Whenever a theory appears to you as the only possible one, take this as a sign that you have neither understood the theory nor the problem which it was intended to solve." - Karl Popper

Recognizing that what appears obvious may not always be the best course of action is a valuable lesson. I owe this progress to my supervisor, who encouraged me to think outside the box and tackle controversial issues. I am profoundly grateful for his support, stimulating discussions, and motivation during these years.

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Introduction

> "Institutions are the humanly devised constraints that structure political, economic, and social interaction." (North, 1991, p. 97)

Institutions shape human behavior and, thus, are the focal aspect of political economy (North, 1991). Political institutions are known to affect various dimensions, such as economic policy (e.g., Persson, 2002), economic development (Acemoglu et al., 2019; Weingast, 1995), corruption (e.g., Alt and Lassen, 2003), debt crises (Van Rijckeghem and Weder, 2009), turnout (Jackman, 1987), happiness of individuals (Frey and Stutzer, 2000), rent-seeking of governmental members (Keefer and Knack, 2007), and trust (Ljunge, 2014). At the same time, however, scholars have acknowledged that political institutions are themselves endogenous and evolve over time (e.g., Aghion et al., 2004; Greif and Laitin, 2004). Therefore, in the field of political economy, it is critical to consider under what preconditions beneficial political institutions come to exist and under what circumstances they result in more or less beneficial outcomes. The present thesis contributes to a further understanding of this. Consisting of four independent empirical essays, this thesis sheds light on questions at the intersections and relations between political institutions, society, and the functioning of democracy.

The design of political institutions determines the extent of democratic rights in a state. Important elements of modern (representative) democracy are frequent, fair, and regular elections of officials; freedom of expression; alternative sources of information; associational autonomy; and inclusive citizenship (e.g., Dahl, 2005). Today, more countries qualify as democracies than nondemocracies, indicating that democratic rule has spread more than ever before (Mukand and Rodrik, 2020). Regarding the transition toward democracy, early scholars, such as Lipset (1959), point out that democracy can thrive only when certain cultural or societal preconditions exist. Mukand and Rodrik (2020), for instance, show that the evolution of liberal democracies—that ensure not only political rights but also the civil rights of minorities—relies heavily on the extent of societal cleavages.[1]

Not only do societal characteristics affect the transition toward having a democracy, but they also affect the functioning of democracy. Powell (1982) already noted that the political process works more successfully in some democracies than others. However, what makes a "good" democracy? Although challenging, Diamond and Morlino (2004)

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[1]Mukand and Rodrik (2020) introduce two societal cleavages in their framework: an inequality and an identity cleavage, which can arise from ethnicity, religion, language, ideology, or regional affiliations.

attempt to define the characteristics of "good" or "better" democracies and identify eight dimensions in which they differ in quality: the rule of law, participation, competition, vertical accountability, horizontal accountability, freedom, equality, and responsiveness. All these criteria are closely related, and some also conflict. The functioning of democracy on these dimensions can vary based on the society, even if they have the same political institutions in place. This has been shown for various political institutions, such as electoral rules (e.g., Eichenberger et al., 2021; Neto and Cox, 1997), or decentralization (e.g., Belmonte et al., 2018). However, rather than being exogenous, society and political institutions are themselves shaped by societal and institutional factors.

These considerations are summarized in the simplified framework in Figure 1, in which the functioning of democracy depends on an interplay of formal political institutions and society. Theoretically, there are five different links for investigation. The present thesis does not explicitly contribute to channel A, but the four independent essays focus on questions located in channels B to E in Figure 1. I understand political institutions broadly, including all the formal constraints that shape the behavior of political actors. This covers, for instance, the electoral system, the constitution, suffrage, direct-democratic instruments, and more. My understanding of the term society consists of, first, its structure–such as demography, culture, education, diversity, inequality, and more–and, in addition, what is often called "social institutions", such as social norms, rules, rituals, values, views, and others.[2] In the following, the links A to E (see Figure 1) are sorted into their respective strands of literature. I also discuss the contribution of the four independent essays in the present thesis to these strands of the literature.

First, societal preconditions can dynamically affect society (Channel A in Figure 1). Interest in these issues has grown because of globalization and international mobility, resulting in more diverse societies regarding ethnicity, language, and religion. This evolution can have ambiguous effects on countries, as scholars such as Alesina and La Ferrara (2005) note. For instance, ethnic diversity can negatively affect trust in society (for a meta-analysis, see Dinesen et al., 2020). However, societal imprints that originate in politics, such as the increase in affective polarization in many democracies (Boxell et al., 2022), also shape society. Affective polarization is positively related to social homophily (Iyengar et al., 2019) and negatively to social trust (Torcal and Thomson, 2023). Another example is the changing role of women in society, which has increased interest in its origins and its effects on society. For instance, attitudes toward gender roles may affect women's workforce participation (Fortin, 2015). These findings show that societal preconditions play a crucial role in shaping the future of society. The present thesis does not explicitly target this strand of literature, but it inevitably overlaps.

Second, society has an impact on the evolution and stability of political institutions (Channel B in Figure 1), as early scholars, such as Lipset (1959), point out.[3] A society's

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[2]The term society, thus, also includes what Casson et al. (2010) call informal institutions. The authors define informal institutions as *"[...] norms and customs regulating socio-economic life"* (Casson et al., 2010, p. 137).

[3]See also Casson et al. (2010) for a review of the literature on institutional change. The authors discuss the intersections and roles of formal and informal institutions.
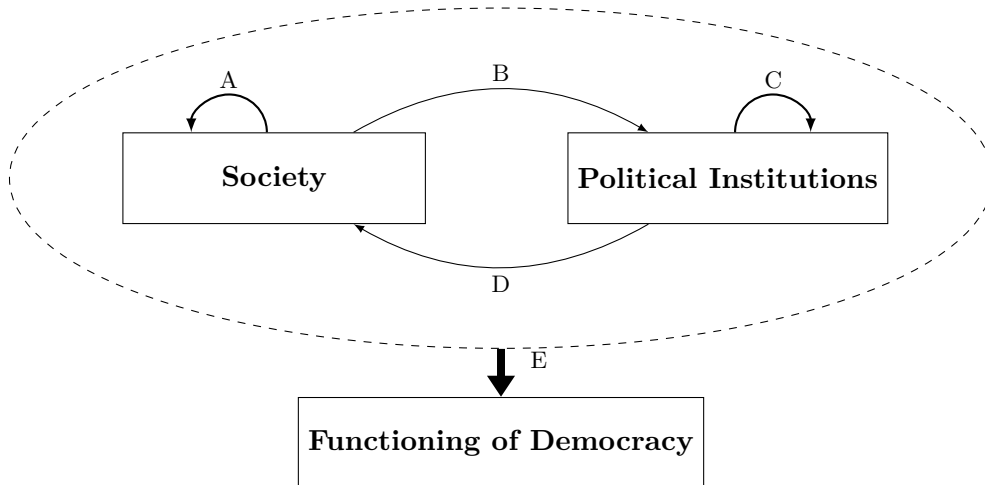
Figure 1: Society, political institutions, and the functioning of democracy.

background, for instance, in terms of socio-ethnic fractionalization (Aghion et al., 2004), affects what kind of political institutions are installed. As Alesina and Zhuravskaya (2011) show, higher segregation in democracies regarding ethnicity and language is negatively associated with the quality of government. An important factor in assessing the quality of political institutions is how inclusive they are and, thus, if political power is distributed widely in a pluralistic manner (Acemoğlu and Robinson, 2012). The inclusiveness of institutions has evolved enormously over the past few centuries, with many democracies installing universal suffrage. Some centuries ago, the right to participate in politics was mostly reserved for rich (white) male citizens because there were, for a long time, wealth or literacy requirements installed or people excluded because of gender or ethnicity (see, e.g., Engerman and Sokoloff, 2005). Regarding female enfranchisement, Braun and Kvasnicka (2013) provide evidence that the proportion of women in society affects the adoption of female suffrage. This emphasizes the importance of societal factors for the inclusiveness of democracy.

Chapter 1 of the present thesis contributes to the literature analyzing how society affects the emergence and stability of political institutions (Channel B in Figure 1). With an extensive data set on Swiss referendum votes about non-citizens' enfranchisement, Chapter 1 analyzes under what conditions the current electorate is willing to grant political rights to non-citizen residents. Hence, this chapter focuses on how growing ethnic diversity affects the degree of inclusiveness of political institutions and adds empirical evidence to two opposing theories: a growing non-citizen group in the society may, on the one hand, increase the cultural threat to the native population (Hainmueller and Hiscox, 2007; Kinder and Kam, 2009) and, therefore, increase natives' reluctance toward enfranchising non-citizens. On the other hand, a growing share of non-citizens can increase the contact between the old and potential new electorate and, hence, increase natives' willingness to enfranchise non-citizens (Allport, 1954; Paluck et al., 2019; Yehuda, 1998).

Thus, it remains an empirical question of how the societal background regarding ethnic diversity affects the inclusiveness of political institutions.

Third, not only societal factors, but also previously installed political institutions can affect the stability of political institutions or the emergence of new political institutions (Channel C in Figure 1). For example, the design of electoral systems is determined by the degree to which various groups have formal voting rights (e.g., Boix, 1999) or the electoral system can affect the stability of other institutions, such as the extent of decentralization (Eichenberger et al., 2021). Acemoğlu and Robinson (2001) famously elaborate a theory of democratization, where they explain suffrage extensions as a strategic decision by the elite to prevent revolution or social unrest. In the case of the enfranchisement of new groups, anecdotal evidence suggests that the institutional background plays a significant role. Switzerland, which is famous for its direct-democratic institutions, was not only among the last Western democracies to enfranchise women, but it was also one of the last democratic countries to rejuvenate suffrage to people who are 18 years old. Similarly, voters overruled the decisions of national parliaments in favor of suffrage extensions to the young in Denmark in 1969 (Nielsen, 1970) and in Luxembourg in 1969 (Dumont and Kies, 2016). Chapter 2 adds empirical evidence to these observations by analyzing the effect of direct-democratic institutions compared with representative democratic institutions on the extension of voting rights to the youth. Chapter 2, therefore, sheds light on whether direct democracy is harmful to outsiders (Donovan and Bowler, 1998; Gamble, 1997; Haider-Markel et al., 2007; Hainmueller and Hangartner, 2019). Moreover, it analyzes a potential trade-off between equipping the current electorate with strong participation rights and extending voting rights to new groups. Although strong participatory institutions are known to generate positive effects, such as fostering cooperation (Acemoğlu and Robinson, 2012), civic virtues (Frey, 1997), or trust (Rainer and Siedler, 2009), the lowering of the voting age can have positive effects as well, such as supporting the habit formation of political participation (Gidengil et al., 2016) or increasing political interest (Zeglovits and Zandonella, 2013).

Fourth, political institutions can affect societies in diverse ways (Channel D in Figure 1). For instance, constitutional design can affect the extent of civic virtue (Frey, 1997) or tax morale (Torgler, 2005) in society. Moreover, citizens are more politically informed and knowledgeable when they dispose of extensive participation rights (e.g. Benz and Stutzer, 2004; Tolbert et al., 2003). Further, the enfranchisement of women can affect their emancipation (Slotwinski and Stutzer, 2023), or voting on controversial topics—such as Brexit—can lead to subject-wise affective polarization (Hobolt et al., 2021). Chapter 3 contributes to these strands of the literature by analyzing the effect of direct-democratic decision-making on individual attitudes in society. In more detail, it empirically investigates the effect of face-veiling ban ballots in Switzerland on individual attitudes toward Islam. Therefore, by analyzing how democratic decision-making shapes societies, Chapter 3 adds to the literature on the effects of political institutions on society. These questions have recently gained increasing scholarly attention. For instance, scholars have shown that events in the political sphere can lead to rapid changes in people's values and beliefs (e.g., Bursztyn et al., 2020a; Funke et al., 2016; Giuliano and Spilim-

bergo, 2014). On the one hand, elections or referendums may update the perceived social norms (see, e.g., Bursztyn et al., 2020a; Gerling and Kellermann, 2022) and, therefore, social restrictions. On the other hand, an intensive democratic discourse can increase individuals' information about their own preferences or attitudes (e.g., Eichenberger and Serna, 1996; Habermas, 1979; Hirschman, 1989). This urges a deeper comprehension of how democratic decision-making impacts society, and the institutional background in Switzerland offers excellent opportunities to enhance this knowledge further.

Finally, the societal background may interact with the formal political institutions and lead to more or less "satisfying" outcomes in the quality of democracy. Therefore, the functioning of democracy relies on a combination of societal factors and political institutions (Channel E in Figure 1). The participation of citizens is often considered an important criterion for determining the quality of the democracy of a state (Diamond and Morlino, 2004); for a democracy to be considered "good," citizens must be able to exercise their formal rights and take part in the decision-making process through activities such as voting, organizing, protesting, and lobbying for their interests (Diamond and Morlino, 2004). Certain societal factors influence the level of political interest or participation of citizens, such as education (Blais et al., 2004), a sense of civic duty (François and Gergaud, 2019), and income (Kasara and Suryanarayan, 2015). Additionally, political institutions, such as electoral systems (Skorge, 2021; Teele, 2023) and extensive participation rights (Benz and Stutzer, 2004; Kim, 2019; Manatschal, 2021), play a key role in promoting citizen involvement.

Chapter 4 adds to these strands of the literature by exploiting historical gender-specific turnout data from Switzerland and analyzing how male and female turnout evolve after the enfranchisement of women. Hence, Chapter 4 focuses on the effect of institutional and societal backgrounds on the participation of the old and new electorates. Thus, the contribution of this chapter is twofold: First, it adds to the growing literature on the effects of the enlargement of the electorate (e.g., Aidt and Dallal, 2008; Aidt et al., 2006; Koukal and Portmann, 2020; Slotwinski and Stutzer, 2023). Second, the secret of the ballot usually prevents the analysis of turnout by different groups (e.g., Corder and Wolbrecht, 2006; Stockemer and Sundstrom, 2023). We overcome this challenge by making use of historical turnout data separated by gender, which helps contribute to further the understanding of how the turnout of the old and new electorates evolves after the enfranchisement of a new group (e.g., Kleppner, 1982; Morgan-Collins, 2021; Wolbrecht and Corder, 2020).

Even in the simplified framework shown in Figure 1, it is evident that there are numerous connections and interactions between political institutions, society, and the functioning of democracy. Hence, the four chapters of the present thesis do not aim to explain or explore all of these complex relationships. Instead, they focus on specific individual aspects that connect the different components within this framework. The abstracts below provide a succinct summary of each essay.

Chapter 1: Enfranchising non-citizens: What drives natives' willingness to share power?

Universal suffrage is a core element of democracy. However, in many democratic countries, a large portion of the inhabitants are foreigners without suffrage. This chapter analyzes the conditions under which domestic citizens are willing to extend suffrage to non-citizen residents. We explore a new panel data set (1992–2016) of Swiss referendums on the enfranchisement of non-citizens, concentrating on the size and composition of the foreign population and institutional context as determinants of non-citizens' enfranchisement. Our estimates show that a higher share of foreigners corresponds to a lower willingness of natives to enfranchise non-citizens. This effect seems driven by the cost of enfranchising non-citizens, which increases with the cultural distance between the foreign and native populations and the strength of direct democracy.

Chapter 2: Empowering the next generation: The role of direct democracy in youth enfranchisement

When are voters willing to enfranchise a new group? In this chapter, we analyze whether and how the extent of direct democracy affects the willingness of the electorate to extend suffrage to young people. We exploit a new municipality-level dataset from two Swiss federal referendums on lowering the voting age from 20 to 18. Based on a difference-in-differences (DiD) design, we demonstrate that support for lowering the voting age is lower within the context of direct democracy but that the negative effect disappears and even becomes positive if the youth population is already enfranchised at the local level at the time of the federal vote. This finding is consistent with the interpretation that voters react systematically to the expected power loss resulting from suffrage extensions, which is larger under a direct-democratic setting. However, once the new group is enfranchised on the local level, direct democracy can foster contact between the old and new electorate, increasing support for further suffrage extensions. In addition, we provide evidence of socio-demographic factors that affect the electorate's willingness to lower the voting age.

Chapter 3: Voting on minority rights – How veiling ban ballots affect attitudes toward Islam

How do political events shape society? Democratic decision-making is often seen as a means to aggregate individual preferences in society. However, elections or votes may also affect attitudes and perceived social norms. In this chapter, I analyze if and how votes on face-veiling bans affect reported individual attitudes toward Islam. Therefore, I make use of a unique setting in Switzerland, where two cantons voted on the introduction of a face-veiling ban, combining these events with individual survey data. Based on a multiple difference-in-differences (DiD) strategy in panels and repeated cross-sections, the estimates show that reported attitudes toward Islam were more negative

and polarized after voting on veiling bans. The results remain robust to a propensity score matching approach combined with DiD. My results suggest that the change in reported attitudes toward Islam can, on the one hand, be explained by citizens being better informed about their own attitudes and, on the other hand, by the change in social restrictions on expressing certain opinions.

Chapter 4: Male and female turnout after women's suffrage – Evidence from Switzerland

Many countries face two trends: On the one hand, a decrease in voter turnout and, on the other hand, an increase in discussions on voting rights extensions. However, how do the enlargement of the electorate and the evolution of turnout relate? This chapter systematically analyzes how the turnout of the old male and new female electorate evolves after the introduction of women's suffrage in Switzerland, using gender-specific turnout data and the staggered cantonal introduction of female suffrage. Our event study analysis provides evidence that male turnout in federal votes dropped by about five percentage points after the introduction of cantonal female suffrage. Importantly, our setting rules out that the decline comes from a change in the pivotality of the men's vote. Our findings are consistent with the interpretation that female enfranchisement can decrease the consumption value of politics for men. Moreover, with cantonal gender-specific turnout data, we show that the municipal background matters: in environments where reluctance toward female suffrage was larger, women turned out less, and the drop in male turnout was larger in the aftermath of the suffrage extension. Furthermore, the institutional context matters for the voters' short- and long-term political integration.

Note to the reader
All chapters tackle questions at the intersections of society, political institutions, and the functioning of democracy. However, they are stand-alone essays that can be read separately.

Chapter 1

Enfranchising non-citizens: What drives natives' willingness to share power?*

1.1 Introduction

International migration is on the rise, yet most democratic participation rights are still tied to traditional forms of citizenship. Therefore, in many democratic countries, a large and growing share of the population has no or only limited franchise.[1] An extreme case is Luxembourg, where 47% of the residents were non-citizens in 2017 and therefore excluded from full voting rights.[2] In countries such as Germany and Switzerland, this share was 12 and 24 percent, respectively. The numbers are even higher for specific age groups. Among Swiss residents aged 30 to 40, the share of foreigners is about 40 percent. Consequently, their democratic endowment is comparable to the situation that existed before the enfranchisement of women but is steadily shrinking.

Participatory political institutions are known to generate positive effects. They foster, for example, cooperation (Acemoğlu and Robinson, 2012), civic virtues (Frey, 1997), trust (Rainer and Siedler, 2009), and beneficial political outcomes (Ellis and Fender, 2010). Although these results refer to the potential for political integration through en-

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[1]In some countries, e.g., in the EU-member states, immigrants from specific countries have suffrage at the local level. See Arrighi and Bauböck (2017) for more information on local voting rights for non-citizens.

[2]Source: Eurostat.

franchisement, the literature has focused primarily on the determinants of naturalization and successful economic integration of foreigners.[3] We only know of three recent studies empirically evaluating the conditions for non-citizens' enfranchisement (Earnest, 2015; Kayran and Erdilmen, 2020; Stutzer and Slotwinski, 2021) and relied mainly on cross-sectional variation. However, little is known about the determinants of non-citizens' enfranchisement. Given the limited democratic legitimacy of decisions taken by only a fraction of the taxpayers and the integrative effects of political empowerment (Koukal, 2013; Koukal and Portmann, 2020; Slotwinski et al., 2017), it is important to understand the conditions under which political participation rights are transferred to non-citizens.

On the one hand, native citizens benefit from enfranchising foreigners through the said positive effects of increasing integration. On the other hand, they lose part of their political influence when sharing their political power with non-citizens. If the median voter positions of the new and established electorates differ, the majority of the old electorate loses by enlarging the franchise, as political outcomes may change. As both the native citizens' benefits and costs of enfranchising foreigners increase with regard to the number of foreigners to be enfranchised, it rests ambiguous whether immigration increases or decreases the willingness of natives to enfranchise foreigners. Thus, it is a priori unclear whether ongoing immigration induces the democratic endowment to fall or to rise.

In this chapter, we analyze the potential drivers of, and barriers to, non-citizens' enfranchisement in Switzerland during the 1992–2016 period and focus on the role of the size and composition of the foreign population. This chapter is among the first to consider this question and relies on a much richer data set than previous studies. We seek to contribute in at least two ways: First, this chapter is one of the few empirical analyses of non-citizens' enfranchisement and the first to use an extensive panel data set of natives' revealed preferences. Second, we contribute to the growing literature on the effects of immigrants' presence on natives' behavior by shedding light on how non-citizens' presence affects natives' willingness to share voting rights in favor or against.

Switzerland provides an ideal setting for analyzing the enfranchisement process for two reasons. First, the decision to enfranchise non-citizens is not taken centralized by the national parliament but by the native electorate through popular referendums. This institutional setting allows an analysis of voters' revealed preferences. Second, due to Swiss federalism, non-citizens' enfranchisement is not simultaneously introduced on all state levels but is a multilayered process at the municipal, cantonal, and federal levels. The extent of the franchise for cantonal and municipal matters is defined by the cantons, i.e., their respective voters. Therefore, the decision to hold a referendum vote is exogenous to the individual municipality. Thus, we exploit the within-municipality variations in the municipal acceptance to enfranchise non-citizens in 10 cantons during the 1992–2016 period.

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[3]For a broader discussion of the determinants of successful economic integration see Card (2005), Borjas (2014), or Card and Peri (2016). For naturalization, see Bloemraad et al. (2008), Hainmueller and Hangartner (2013), or Hainmueller et al. (2015).

Our results reveal that a larger share of foreigners does not foster natives' willingness to enfranchise non-citizens. On the contrary, natives' willingness to enfranchise non-citizens decreases in the share of foreigners in a municipality. Our estimates suggest that a one percent increase in the share of foreigners reduces natives' willingness to share political power by approximately 0.20 percentage points. We argue that this effect is likely to reflect the costs of enfranchising non-citizens for two reasons: First, the negative effect of an increase in foreigners' share on the enfranchisement of non-citizens is driven by the cultural distance between natives and foreigners, which plausibly results in a more pronounced preference heterogeneity and, thus, a higher cost of sharing political power. Whether discrimination also plays a role is a question that has to be left open, as we cannot clearly identify the various channels. Second, our results are also affected by institutional factors. We find that the negative effect of a larger foreign share is driven by the municipalities with strong direct-democratic instruments (town meetings). This suggests that the natives' individual loss of influence on politics increases their resistance to sharing political power with non-citizens.[4] The role of the increasing costs of power-sharing in the direct-democratic setting goes beyond the traditional explanations of the discriminatory role of direct democracy for minority rights (Donovan and Bowler, 1998; Gamble, 1997; Haider-Markel et al., 2007; Hainmueller and Hangartner, 2019).

By investigating how the size of the foreign community affects natives' willingness to enfranchise non-citizens, this chapter contributes to the literature on the impact of society on the evolution and stability of political institutions (see Channel B in Figure 1). It is organized as follows: The next section reviews the related literature. Section 1.3 introduces the institutional setting of Switzerland. Section 1.4 presents the theoretical considerations, and Section 1.5 describes our data and variables. In Section 1.6, we explain the empirical strategy, and the results are presented and discussed in Section 1.7. Section 1.8 summarizes our main results and an outlook on future research.

1.2   Related literature

Despite the large literature on franchise extensions for men and women, we only know of three recent papers that empirically study the conditions of non-citizens' enfranchisement. In a cross-country study over the 1975–2010 period, Earnest (2015) analyzes the conditions for the liberalization of citizenship laws, such as non-citizens' voting rights and the retraction of voting rights of non-citizen residents. The author explains the former with policy constraints and the latter as an interaction between policy constraints and national characteristics. In a recent cross-country comparison of 28 countries, Kayran and Erdilmen (2020) find that governments tend to (partially) delay the enfranchisement of non-citizens when their share in the total population is high. Furthermore, Stutzer and Slotwinski (2021) use data from two Swiss cantons to focus on the power dilution hypothesis for opting-in regimes to enfranchise non-citizens.[5]

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[4]For a related discussion about female enfranchisement, see Koukal and Eichenberger (2017).

[5]We make use of a much richer data set with all cantonal votes in Switzerland, while Stutzer and Slotwinski (2021) focus on two cantons (Grisons and Zurich). The largest part of their analysis relies

Unlike the literature on non-citizens' enfranchisement, the literature on franchise extension for native men and women is well established. Acemoğlu and Robinson (2001) prominently frame a theory of democratization that explains suffrage extensions as a strategic decision by the elite to prevent revolution or social unrest.[6] Wars (Hicks, 2013; Polishchuk and Syunyaev, 2015) and the strategic concerns of a divided elite (Lizzeri and Persico, 2004; Llavador and Oxoby, 2005) have also been considered potential drivers of suffrage extensions. Bertocchi (2011) finds that a smaller gender wage gap increases the likelihood of female enfranchisement in Europe. Engerman and Sokoloff (2005) empirically underlines this result with data from North and South America, providing evidence that greater homogeneity (in terms of socioeconomic or ethnic attributes) drives democratization.

Democratization via enfranchising more natives has also been explained by various price effects. Evidence shows that men's willingness to enfranchise women increases in their scarcity (Braun and Kvasnicka, 2013; Kenny, 1998) and decreases with the growing political influence of the actual (male) electorate (Koukal and Eichenberger, 2017). The impact of preference heterogeneity between the old and new electorate on suffrage extensions is less well explored. The few contributions about non-citizens we are aware of find for immigrants, compared to natives, lower status quo effects (Koukal, 2013), stronger preferences for increasing social services expenditures (Vernby, 2013), and a tendency to vote for left parties (Strijbis, 2014). Moreover, they indicate ethnicity as one of the main drivers of party choice (Tillie, 1998).

With increasing international mobility, interest in measuring the consequences of growing ethnic diversity is increasing (Hainmueller and Hopkins, 2014). There are two prominent opposing theories on the effect of outgroup size on ingroup attitudes toward the outgroup. On the one hand, the hypothesis of cultural threat suggests that natives fear immigrants because they threaten their cultural identity (Hainmueller and Hiscox, 2007; Kinder and Kam, 2009). Following this approach, the size of the foreign population has been shown to positively affect the support of far-right parties (Brunner and Kuhn, 2018; Edo et al., 2019; Halla et al., 2017) and to decrease the willingness to redistribute financially (Alesina et al., 2021; Luttmer, 2001; Tabellini, 2020).

Furthermore, restrictive naturalization rules are more prevalent within a growing foreign population (Bertocchi et al., 2010; Mariani, 2013). On the other hand, contact theory suggests that interactions between outgroup and ingroup members reduce information asymmetries, increase trust, reduce prejudices, and may moderate perceived threat (Allport, 1954; Paluck et al., 2019; Yehuda, 1998). Using French data, Jolly and DiGiusto (2014) find that xenophobic attitudes decrease with growing foreign population size, while for the Netherlands, Schlueter and Scheepers (2010) provide evidence for both threat and contact theory depending on the measures considered for the size of the foreign population. Semyonov et al. (2004) show that in Germany, perceived group

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on cross-municipality variation, whilst we can focus on within-municipality variation in all cantons of Switzerland that voted at least twice on foreigners' suffrage.

[6]See also Conley and Temimi (2001), Ellis and Fender (2010), Aidt and Jensen (2014), or Aidt and Franck (2015).

size drives anti-immigrant attitudes, while actual intergroup contact reduces perceived group threat.

In addition to the size of the foreign population, the cultural background and institutional context also matter in the naturalization process. Hainmueller and Hangartner (2013) provide evidence that the applicant's country of origin is the most critical determinant of naturalization decisions in Swiss municipalities. Moreover, once politicians rather than citizens decide on the naturalization applications, naturalization rates increase by about 60 percent (Hainmueller and Hangartner, 2019). Therefore, direct democracy might constitute a significant barrier to the broader integration of non-citizen residents in politics because it raises the price of political power-sharing or fosters the discrimination of minorities (Donovan and Bowler, 1998; Gamble, 1997; Haider-Markel et al., 2007; Hainmueller and Hangartner, 2019; Koukal and Eichenberger, 2017).

Beyond the impact of foreigners' population size, economic circumstances have also been shown to impact attitudes toward foreigners. In a cross-country study covering 12 countries, Quillian (1995) shows that anti-immigrant attitudes are more prevalent if economic conditions worsen. Most authors analyzing the economic threat hypothesis concentrate on the labor market situation. While some authors find evidence that anti-immigrant attitudes are positively affected by greater competition in the labor market (Hyll and Schneider, 2018; Mayda, 2006; Scheve and Slaughter, 2001), others question this result (Hainmueller and Hiscox, 2007; Sides and Citrin, 2007).

1.3 Institutional background

Across Europe, the enfranchisement of non-citizen residents has increased since the late 1970s.[7] In most countries, the national parliament decides on the enfranchisement of non-citizens. However, such a setting provides only limited information on the preferences of the actual electorate. In Switzerland, non-citizens' suffrage cannot be implemented by national or cantonal parliaments but only by the actual electorate through referendum votes. At the federal level, non-citizens' voting rights for federal matters have never been subject to a vote. In contrast, at the cantonal level, non-citizens' voting rights for cantonal and municipal matters have been subject to approximately 40 votes in 16 of the 26 cantons. The cantonal votes on non-citizens' enfranchisement result from different procedures listed in Table 1.A.1 for the votes in our data set. As voting rights are regulated at the constitutional level, the mechanisms are: (i) Citizens can start a popular initiative, i.e., they formulate a constitutional amendment and must collect a certain number of supportive signatures demanding a referendum vote on the respective proposal. The proposal is implemented if it gets a simple majority of votes. (ii) Cantonal parliaments can design a constitutional amendment. However, in all cantons, constitutional amendments are subject to a mandatory referendum. The amendment is only implemented if it gets a simple majority of the votes. (iii) Votes on non-citizens'

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[7]For more information on non-citizens' voting rights across Europe, see Groenendijk (2008) or Aleinikoff and Klusmeyer (2013).

enfranchisement sometimes result from total or partial revisions of the cantonal constitutions, which also need the consent of the citizens. In all these cases, the decision to hold a referendum vote is taken at the cantonal level and is, thus, exogenous to individual municipalities, which are our observation units.

Due to cantonal autonomy, various types of voting rights for non-citizens have been considered and partly installed. Table 1.A.1 in the appendix provides an overview of the different types of enfranchisement that have been voted on in the cantons of our data set. They range from active and passive voting rights at the cantonal level to optional voting rights at the municipal level, i.e., to allow municipalities to enfranchise non-citizens at the municipal level. Active voting rights give them the right to participate in the political process as voters, while passive voting rights allow them to run for office. Table 1.1 overviews the cantons that introduced non-citizens' suffrage. Analogously to the enfranchisement of women, French-speaking cantons were the first to make the move to enfranchise non-citizens. Currently, the cantons of Neuchâtel and Jura grant non-citizens the most extensive political rights, i.e., active voting rights at the cantonal level and active and passive voting rights at the municipal level. However, cantons also differ concerning the conditions under which non-citizens receive voting rights and, most importantly, their duration of stay.[8]

Table 1.1: Accepted referenda on non-citizens' suffrage in Swiss cantons.

Vote date	Yes share	Effective date	Canton	Suffrage type	Opt-in	# Munic.
20.03.1977	80%	01.01.1997	Jura	Active local + cantonal	NO	All
30.04.1995	nA	Opt-in	Appenzell A.R.	Full local	YES	4
24.09.2000	76.60%	01.01.2002	Neuchâtel	Active cantonal	NO	All
22.09.2002	55.90%	14.04.2003	Vaud	Full local	NO	All
18.05.2003	59.70%	Opt-in	Grison	Full local	YES	25
16.05.2004	58.00%	01.01.2005	Fribourg	Full local	NO	All
23.03.2005	76.50%	Opt-in	Basel-City	Full local	YES	0
24.04.2005	52.30%	24.04.2005	Geneva	Active cantonal	NO	All
17.06.2007	54.40%	17.06.2007	Neuchâtel	Passive local	NO	All
28.09.2014	54.00%	28.09.2014	Jura	Passive local	NO	All

Note: In the Canton of Appenzell Ausserrhoden, the cantonal assembly has voted on non-citizens' suffrage; thus, no data on the municipal level is available. Sources: Adler et al. (2015), cantonal chancelleries, cantonal constitutions.

To enfranchise non-citizens in a canton, at least 50 % of the participating voters at the cantonal level must agree. Table 1.1 overviews accepted referendum votes and the corresponding yes shares. The cantonal decisions are imposed on municipalities where only a minority of voters agree to enfranchise non-citizens (i.e., non-citizens are enfranchised at the municipal level against the will of the majority of municipal voters). In some cantons, an opt-in rule delegates the right to enfranchise non-citizens at the municipal level to the municipalities (indicated as opt-in YES in Table 1.1). Three

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[8]For instance, for cantonal voting rights, foreigners in Neuchâtel must have been canton residents for at least five years, whereas in Jura, foreigners are granted voting rights after ten years in Switzerland and one year in the canton.

cantons (Grison, Appenzell Ausserrhoden, and Basel City) have introduced opt-in rules for municipalities. Furthermore, some votes on enfranchising non-citizens are integrated into general constitutional revisions and are, therefore, part of a larger political package.[9]

1.4 Theoretical considerations

Without non-citizens' suffrage, the electorate consists of Swiss voters only. They decide on politics via direct-democratic instruments or by delegating their decision-making power to politicians. An enlargement of the group of voters by enfranchising non-citizens potentially generates costs and benefits for the current electorate. The following section discusses how these benefits and costs might evolve with a growing share of foreigners.

Enlarging the electorate has the potential of several *benefits*. Involving more and different people in the decision-making process increases the amount and quality of information on political issues, the legitimacy of political decisions, and the media's incentive to cover political topics.[10] Following the Condorcet jury theorem, an increase in the number of voters increases the quality of democratic decisions under uncertainty if their individual errors are independently distributed (i.e., if the heterogeneity of voters increases).[11] In addition, the political integration of non-citizens has been shown to have broader integrative effects as well (Koukal, 2013; Koukal and Portmann, 2020; Slotwinski et al., 2017). This is in line with the literature analyzing the role of participatory political institutions in fostering cooperation (Acemoğlu and Robinson, 2012), civic virtue (Frey, 1997), or trust (Rainer and Siedler, 2009). A growing share of foreign residents might affect how democratic institutions unfold their positive effects. Therefore, the need for political inclusion and resulting benefits will likely increase if the share of foreign residents grows. These considerations suggest that *natives' willingness to enfranchise non-citizens increases with the share of foreigners.*

Conversely, enlarging the electorate may also impose *costs* on the actual electorate. First, the individual influence of natives on political outcomes decreases with a larger electorate, as the probability of a vote affecting the outcomes decreases.[12] These costs increase in preference heterogeneity between the native and foreign populations and depend on the institutional setting. Previous literature (Koukal, 2013; Strijbis, 2014; Vernby, 2013) found evidence that the preferences of non-citizens and natives differ, and, hence, non-citizens' suffrage is likely to move the median voter and change political outcomes. Regarding the enfranchisement of women, Kenny (1998) and Braun and

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[9]Vote types are indicated in Table 1.A.1 in the Appendix. In our main estimation, we exclude these vote packages.

[10]See, for instance, Besley and Burgess (2002) for a model and application of the role of media in the political process.

[11]For a discussion of the Condorcet jury theorem and its application in politics, see Stadelmann et al. (2014).

[12]One's theoretical influence in the political decision-making process is $\frac{1}{n}$, and thus, the probability of affecting outcomes decreases based on the number of individuals with political rights. We, thus, ignore the paradox of voting—see, for instance, Aldrich (1997), Blais (2000), or Besley and Case (2003)–and assume that voters take into account the number of people with whom they share the right to vote.

Kvasnicka (2013) find that the scarcity of women, and, thus, their relatively small weight in democratic decisions, has a positive effect on female enfranchisement. In other words, the larger the increase in the size of the electorate, the larger the power loss of the current electorate. This effect is likely to be more pronounced if the institutional context grants the actual electorate more political influence, for instance, with more effective direct-democratic institutions (Koukal and Eichenberger, 2017). In addition, the literature also suggests that hostility against the outgroup increases when the size of the foreign population grows, as this threatens the native population in various dimensions, such as their cultural identity or their social and economic privileges (Brunner and Kuhn, 2018; Edo et al., 2019; Halla et al., 2017). Therefore, individual power loss and perceived threat suggest *that natives' willingness to enfranchise non-citizens decreases with the share of foreigners.* These costs are likely to be more pronounced if the preferences of the native and the foreign populations differ substantially from each other.

Ex-ante, the overall effect of extending the electorate to non-citizens may be positive or negative for the actual electorate, as this depends on the relative sizes of costs and benefits. In the following sections, we analyze how the presence and composition of non-citizens impact the willingness of natives to enfranchise non-citizens in different institutional settings.

1.5 Data and variables

Our empirical analysis relies on three data sources. (1) We collected and digitized data from 33 cantonal referendums on suffrage extension between 1992 and 2016. Table 1.A.1 in the Appendix provides an overview of the referendums in our data set. (2) We combined this information with various socio-demographic municipal characteristics acquired from the Swiss Statistical Office and the Federal Tax Administration. (3) For information on institutional municipal characteristics, we use the municipal survey data provided by Andreas Ladner. This results in a novel data set with approximately 3200 observations. We can exploit the within-municipality variation of 24 referenda stemming from 10 cantons that voted at least twice on non-citizens enfranchisement.[13] Table 1.2 provides the descriptive statistics of the outcome and the explanatory and control variables, including all vote types.

An empirical panel analysis at the municipality level exhibits several advantages. It allows the examination of the effect of the foreigners' share, characteristics of the foreigners, institutional features, and economic conditions on the approval of non-citizens' voting rights. Compared to cross-country data, municipal panel data enables the analysis of a richer variation in a more homogeneous context. Furthermore, the decision to conduct a referendum vote is exogenous to the individual municipality, as the requirement to launch such a vote is decided at the cantonal level. Like in many other OECD countries, in Switzerland, some municipalities have merged in our period of observation (Ladner, 2011; Steiner and Kaiser, 2017). As statistical offices provide most statistical

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[13]The panel is unbalanced, as different cantons voted with different frequencies. Table 1.A.1 in the Appendix lists the referendums in our data set.

information only for the newly merged unit but not for each former subunit, constructing a decent data set covering sufficient variables for the municipalities that merged later was not possible. Therefore, we excluded all municipalities that experienced an amalgamation in our observation period, allowing us to include a more extensive variety of control variables and work with a balanced panel.

Table 1.2: Descriptive statistics of the main sample of municipalities.

Variable	N	Mean	SD	Min	Max
yes share	2,476	27.29	12.86	0	83
population	2,476	3881.28	15741.34	38	384786
population (log)	2,476	7.14	1.32	3.64	12.86
foreigner (share)	2,476	12.45	10.03	0	61.71
MS-foreigner (share)	2,476	18.33	9.35	3.31	37.81
culturally distant (share)	2,476	29.8	19.67	0	100
former Yugoslavia (share)	2,476	13.58	14.94	0	88.24
not neighbor (share)	2,476	57.3	17.2	0	100
naturalization (share)	2,476	0.21	0.29	0	2.55
unemployment Swiss (share)	2,476	1.34	0.79	0	4.87
unemployment foreign (share)	2,476	3.71	3.7	0	50
parliament	2,101	0.18	0.38	0	1
agriculture (share)	2,476	2.67	2.77	0	19.51
pensioner (share)	2,476	15.37	4.24	2.45	37.1
Gini coefficient	2,476	43.75	6.69	29.8	90.8
Social Democratic Party (share)	2,476	18.52	7.92	0	56
mean income	2,476	59297.01	24023.21	27655	533312
mean income (log)	2,476	10.94	0.31	10.23	13.19

Note: These statistics include observations from our main sample. Votes embedded in partial or total revisions and singleton municipality observations are excluded. The number of observations is smaller for the variable parliament since this variable is not observed for all municipalities.

1.5.1 Dependent variable

Our outcome variable *yes share* captures the share of votes in favor of enfranchising non-citizens in a municipality. The outcome at the municipal level is observed in the cantonal referendum votes on suffrage extensions in the 1992–2016 period. Figure 1.A.1 in the Appendix illustrates the within-municipal variation between the first and last votes in our panel and suggests that the willingness to enfranchise non-citizens is not characterized by a positive time trend.

1.5.2 Explanatory variables

Our primary variable of interest is *foreigner* (measured as a share); it approximates the size of the affected group that is to be enfranchised in a municipality. Figure 1.A.2 in the Appendix provides an overview of the variation in the share of foreigners within a municipality between the first and last votes in our sample. Switzerland has been an immigration destination for decades. More than 80 percent of immigrants originate from European countries. While the period between the 1960s and 1970s was characterized by a wave of immigrants from Southern Europe (Italy, Portugal, and Spain), people from (former) Yugoslavia[14] constitute the largest new immigration group in our period of observation, accounting for around 24 percent of the foreign population in Switzerland in 2000 (Federal Statistical Office, 2020).

1.5.3 Measures of cultural distance

Cultural distance—which we understand as a proxy of preference heterogeneity between the native and the foreign population—can be measured by various approaches that vary widely between disciplines (Beugelsdijk et al., 2019). To meet this challenge, we follow two well-established approaches in the economic literature and compare the respective results. First, we apply the approach by Inglehart and Baker (2000), based on Word Values Survey (WVS) data. Second, we follow authors who emphasize the role of common language and geographical proximity as an essential indicator of common cultural traits (Bisin and Verdier, 2011; Guiso et al., 2009; Jasso, 2009).

Inglehart and Baker (2000) locate 65 countries on two central dimensions of cross-cultural variation, which explain around 70 percent of the variation in the applied WVS items.[15] The first dimension, "survival vs. self-expression," captures the importance of economic and physical integrity relative to personal self-fulfillment and self-expression. The second dimension, "traditional vs. secular-rational," considers the relative importance of traditional values that concentrate on the importance of family, religion, or respect for authorities compared to secular values emphasizing beliefs such as tolerance of human diversity or gender equality. We use the results of Inglehart and Baker (2000) to classify the countries of origin in our sample as culturally similar or distant to Switzerland. In the next step, we use this information to create an indicator of cultural distance between native and non-citizen residents on the Swiss municipal level. The variable *culturally distant* captures the share of foreigners classified as culturally distant to Switzerland among the total foreign population. The group of culturally similar countries contains countries with a value system similar to the Swiss one for the two dimensions explained above (survival vs. self-expression and traditional vs. secular-rational), such as historically Catholic and Protestant countries or English-speaking OECD states. A

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[14]The federal office of Statistics counts the following countries (or former countries) as belonging to this group: Bosnia-Herzegovina, Croatia, Kosovo, Montenegro, North Macedonia, Serbia, Slovenia, and (former) Yugoslavia.

[15]Theoretically they follow Huntington (1993), who describes eight cultural zones based on persistent cultural differences.

list of countries classified as culturally similar or culturally distant is provided in the Appendix in Table 1.A.2. To further ensure the reliability of our classification, we compared it with the composite bilateral measure of cultural distance by Kaasa et al. (2016). It strongly supports our categorization of European countries.[16] In the second step, we keep following Inglehart and Baker (2000) classification but focus on Switzerland's wave of immigrants from former Yugoslavia, which forms the most prominent immigration group in our observation period. Moreover, immigrants from former Yugoslavia got significant attention in the Swiss immigration debate (Hainmueller and Hangartner, 2013). The share of non-citizens from former Yugoslavia among the total foreign population is captured in the variable *former Yugoslavia*.

Our second approach, which emphasizes the importance of geographical and linguistic proximity, is implemented by categorizing immigrants from neighboring countries that share a common language with the Swiss population (Austria, France, Germany, Italy, and Liechtenstein) as culturally similar and foreigners from all other countries as culturally (more) distant. The variable *not neighbor* indicates the share of non-citizens among the total foreign population in a municipality not originally from a neighboring state of Switzerland. Table 1.A.2 in the Appendix lists the states classified as culturally distant and culturally similar in the three measures.

1.5.4 Measure of institutional price difference

Following Koukal and Eichenberger (2017), we hypothesize that more representative (instead of direct-democratic) instruments at the municipal level can—under specific conditions—foster the enfranchisement of non-citizens. The intuition behind this approach is that the existing electorate is more willing to share their political power with a new group of voters if their individual influence, and, thus, their influence loss, is smaller. One way of measuring the strength of representative democracy on the local level is by using the municipal legislative institutions, consisting of either a town meeting (direct democracy) or a parliament (representative democracy).[17] In direct-democratic municipalities, voters meet directly (one to four times a year) and discuss and decide on municipality topics. In contrast, in municipalities with a parliament, voters delegate some of their political power to politicians. We construct the dummy variable *parliament*, which is equal to zero when a municipality has a town meeting (direct democracy) and equal to one if the municipality has a parliament (representative democracy). We rely on survey data by Ladner to classify the municipal legislative institution. Note that changes in the municipal legislative institutions are possible over time but are relatively uncommon. We identify institutional switchers in our sample based on information pro-

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[16]Kaasa et al. (2016) use data from the European Value Survey and European Social Survey. As the database does not cover important European and non-European countries, this cultural distance index was not suitable for our analysis.

[17]For simplification, we denote municipalities with a local parliament as representative democratic. Strictly speaking, municipalities with a local parliament also have access to direct-democratic means on the cantonal and national levels.

vided by Funk and Litschig (2020) and the survey data by Ladner.[18] We treat the variable parliament as fully time-invariant by excluding the switchers from our analysis.

1.5.5 Control variables

We introduce a set of control variables that cover a broad range of economic and non-economic factors that might shape natives' willingness to enfranchise non-citizens. To account for the different sizes of municipalities, we introduce the variable *population* that constitutes the natural logarithm of the population in a municipality. A channel mentioned in the literature on how natives perceive immigrants is political ideology (Knoll et al., 2011; Mayda, 2006). The variable *Social Democratic Party* reflects the vote share of the largest left party in Switzerland in the most recent national elections and aims to proxy for the prevalence of left-leaning voters in a municipality. To further control the prevalence of (conservative) attitudes in the population, we introduce the variable *pensioner* that measures the share of the municipal population above 64 years and captures age-related preferences. Furthermore, we generate the variable *agriculture* as the number of farms relative to the population in a municipality. This variable captures the prevalence of traditional and conservative norms, the economic structure of a municipality, and the location on the rural-urban continuum, which has shown to be an essential determinant of voting behavior (Garcia and Davidson, 2013; Scala and Johnson, 2017). To control for the general openness toward foreigners in a municipality, we include *naturalization*, which is the number of naturalizations relative to the resident population in the year of observation. The Swiss Federal Statistical Office provides the information for the former four variables. We would have liked to analyze further how a municipality's stock of naturalized individuals affects the acceptance to enfranchise non-citizens. However, this information is not available at the municipality level. An important economic factor that has shown to influence native attitudes towards foreigners is labor market concerns (Hainmueller and Hiscox, 2007; Hainmueller and Hopkins, 2014; Mayda, 2006; Scheve and Slaughter, 2001). To consider this prominently discussed channel, we introduce the variables *unemployment foreign* and *unemployment Swiss*, which reflect the number of unemployed individuals relative to the foreign or domestic population in our set of control variables.[19] Besides economic competition in the labor market, the fiscal burden has been shown to impact native attitudes towards foreigners (Hanson et al., 2007). To ac-

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[18]The municipal surveys of Ladner are available at http://www.andreasladner.ch/uebersicht.htm. Municipalities can choose their legislative form (parliament or town meeting) in most cantons. Population thresholds exist for municipalities in the cantons of Vaud, Fribourg, Valais, and Zurich (Funk and Litschig, 2020). We include these cantons in our fixed-effect analysis, as the number of municipalities around these cut-offs is low, and the assignment rule is not stringent. Only the canton of Vaud used a sharp assignment rule and had a regulation change during our observation period. Thus, in Table 1.C.4, we perform a robustness check excluding the canton of Vaud from the analysis.

[19]It would be optimal to calculate the unemployment rate with the number of unemployed individuals relative to the working population. Unfortunately, the number of the working population at the municipality level is often associated with high uncertainty (data from SECO). Hence, we decided to divide the number of unemployed by the total number of foreign and native residents in a municipality based on information from the Federal Statistical Office.

count for this factor, we proxy the financial situation within the municipality with data from the Federal Tax Administration and include the variable *mean income* (measured in logs), which reflects the mean income of natural persons in a municipality. Finally, we introduce the variable *Gini coefficient* to account for the prevalence of inequality in a municipality. We would have liked to include further variables in our analysis, such as information about educational attainment, income, crime rate, or religious affiliation, specifically for the native and the foreign population. Unfortunately, information on these variables is unavailable at the municipality level, limiting our analysis.

1.6   Empirical strategy

To gain a precise understanding of how the share of foreigners and other factors impact the willingness to enfranchise non-citizens, we need to account for the complex and multi-factorial setting. Therefore, a cross-sectional analysis is not suitable to answer our research question. Due to our rich panel dataset, we choose a model with municipality-fixed effects as our preferred option. Hence, we estimate the following model:

$$yes\ share_{mtr} = \alpha + \beta_1\ foreigner_{mt} + \theta\ X_{mt} + \delta_m + \gamma_r + \phi_t + \epsilon_{mtr} \qquad (1.1)$$

where X_{mt} denotes a vector of controls and δ, γ, and ϕ are fixed effects. By applying municipality fixed effects δ in our base model, we take into account the omitted variable biases stemming from the municipality level and control for time-invariant municipality characteristics, such as municipal institutions, general openness toward foreigners, culture, or geographical location. By including referenda fixed effects γ, we account for the different types of voting rights that have been debated and absorb cantonal-specific time effects. Additionally, by adding time-fixed effects ϕ, we control for general time effects across cantons.

Even after including municipality, time, and referendum fixed effects, consistent estimation of β_1 is only possible assuming that the *yes share* is uncorrelated with the error term. As discussed in Section 1.4, it is not clear ex ante if we expect $\beta_1 < 0$ (costs exceeding benefits) or $\beta_1 > 0$ (benefits exceeding costs). As is always true, our estimate of β_1 can be biased due to causality and simultaneity issues. The most plausible argument is that not only natives' preferences react to high foreigner shares (our thesis), but also foreigners react to the natives' preferences, that is, they migrate to municipalities where they are welcome, which is then also mirrored in natives' support for enfranchising foreigners. However, if $\beta_1 < 0$, this chain of reactions would not inflate but deflate our estimates, making them rather conservative and immune to being falsely interpreted as statistically significant.

Nevertheless, we try to control for potential endogeneity by an instrumental variable approach. One possible instrument is the 'shift-share' methodology, extensively used in labor economics (Card, 2001; Peri, 2012). However, the instrument has also been criticized (Goldsmith-Pinkham et al., 2020; Jaeger et al., 2018) and in the Swiss setting, shift-share instruments have, to the best of our knowledge, only been used to instrument

the share of foreigners at the regional levels (local labor markets or cantons).[20] There-fore, we follow Brunner and Kuhn (2018) and instrument the share of foreigners at the municipal level with the foreigner share at the MS-regional level (MS is the abbreviation of "mobilité-spatiale" or spatial mobility).[21] The boundaries of the 106 MS regions are determined by the Federal Statistical Office and follow the idea of small labor market areas, as the level of local economic activities classifies the regions. Figure A3 provides an overview of the distribution of MS regions.[22] Two identifying assumptions must be fulfilled for the MS foreigner share to be a valid instrument. First, the foreigner share in an MS region must be determined by factors other than natives' attitudes toward foreigners. A possible argument favoring this assumption is that immigrants settle in a specific region due to factors such as closeness to their family or employment possibili-ties. Second, the exclusion restriction requires that the share of foreigners in the broader region have no direct effect on the yes share to enfranchise non-citizens in a municipality. The second assumption may not be fulfilled if, for instance, an individual works in an-other municipality in the same MS region. In this case, the attitudes toward foreigners may not merely be driven by the share of foreigners in their living municipality but also by the share of foreigners in the workplace municipality.

Besides our base model 1.1, we estimate Models 1.2 and 1.3 to analyze the potential heterogeneous effects of foreigners on natives' willingness to enfranchise non-citizens. We investigate the interaction effect between the share of foreigners and the degree of cultural distance between natives and foreigners (Model 1.2), as well as the interaction effect of the share of foreigners and the institutional structure, measured by the variable parliament (Model 1.3). We hypothesize that (among others) the effect of foreigners on the willingness of natives to enfranchise non-citizens depends on the extent of preference heterogeneity between the two groups. To proxy the extent of preference heterogene-ity between the foreign and native populations, we rely on three measures: culturally distant, former Yugoslavia, and not neighbor. We estimate the following model:

$$
\begin{aligned}
yes\ share_{mtr} = \alpha &+ \beta_1\ culturally\ distant_{mt} + \beta_2\ foreigner_{mt} \\
&+ \beta_3\ culturally\ distant_{mt}\ foreigner_{mt} \\
&+ \theta\ X_{mt} + \delta_m + \gamma_r + \phi_t + \epsilon_{mtr}
\end{aligned}
\tag{1.2}
$$

where X_{mt} denotes a vector of controls and δ, γ, and ϕ are fixed effects. If preference heterogeneity, proxied by the share of culturally distant non-citizens, is a moderating factor, we expect β_3 to be negative. As mentioned in 1.5, information on other relevant

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[20]See, for instance, Favre (2011), Degen and Fischer (2017), or Basten and Siegenthaler (2019). A reason for the lack of shift-share instruments at the municipal level in Switzerland is the high degree of fractionalization of Swiss municipalities. In general, information on the nationality of the entire foreign population is used to construct shift-share instruments. As the median Swiss municipality counts 1335 inhabitants (in 2005) and 1066 in our sample, the individual nationalities of foreigners at the municipal level suffer from high variance over time. Therefore, the classical shift-share approach does not seem appropriate at the municipal level.

[21]This instrumental variable approach was first introduced in Dustmann and Preston (2001).

[22]Note that MS regions are not institutionally organized. In 2019, the conceptualization of MS regions was slightly changed, and the number of regions was reduced to 101.

characteristics of the foreign population—-such as education, income, crime rates, or language proficiency—-is unavailable at the municipality level. Therefore, we focus on cultural distance measures at the municipality level. Following Koukal and Eichenberger (2017), we further hypothesize that representative democracy, when compared to direct democracy, can–under specific conditions–lower the price to enfranchise non-citizens, as the individual power loss is less pronounced. We thus estimate the following model:

$$\begin{aligned} yes\ share_{mtr} = \alpha &+ \beta_1\ parliament_m + \beta_2\ foreigner_{mt} \\ &+ \beta_3\ parliament_m\ foreigner_{mt} \\ &+ \theta\ X_{mt} + \delta_m + \gamma_r + \phi_t + \epsilon_{mtr} \end{aligned} \qquad (1.3)$$

where X_{mt} denotes analogous to Model 1.1 and 1.2 a vector of controls and δ, γ, and ϕ are fixed effects. We expect the boosting effect of representative democracy (compared to direct democracy) to grow in the share of foreigners and therefore expect β_3 to be positive.

1.7 Results and discussion

This section presents and discusses the results of our main estimations and robustness checks.

1.7.1 Size of the foreign population

Table 1.3 reports the OLS estimates of the base model (1) with a list-wise introduction of control variables. Referendum votes that were part of constitutional revisions and, thus, embedded in a broader political package are excluded in Table 1.3. All specifications in Table 1.3 show a negative coefficient of *foreigner*, which remains statistically significant at the 1 percent level and robust in size. As we estimate a model with municipality-fixed effects, β_1 captures the within-municipality variation of the share of foreigners. Our estimates suggest that a one percent increase of foreigners in one's municipality is associated with a decreased willingness to share political power by approximately 0.20 percentage points.[23]

The results presented in Table 1.3 suggest that, overall, the benefits for citizens to enfranchise foreigners seem to be overcompensated for by the respective costs, for instance, the loss of political power (Braun and Kvasnicka, 2013). Another possible interpretation for $\beta_1 < 0$ is that increasing foreigner shares induce a feeling of threat among natives, which would be consistent with recent literature on the role of migration for the support for the far right (Brunner and Kuhn, 2018; Edo et al., 2019; Halla et al., 2017). Unfortunately, we cannot disentangle the threat hypothesis from other potential explanations. However, in the further analysis, we try to understand better

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[23]As shown in Table 1.A.1 in the Appendix, different cantons voted at different times on foreigners' suffrage. Thus, the years between a municipality's first and second observations may vary.

Table 1.3: Drivers of non-citizens' voting rights.

VARIABLES	(1) Yes Share	(2) Yes Share	(3) Yes Share	(4) Yes Share	(5) Yes Share
foreigner (share)	-0.196***	-0.215***	-0.171***	-0.170***	-0.169***
	(0.067)	(0.067)	(0.063)	(0.064)	(0.063)
population (log)	-1.865	-1.936	-2.487	-2.413	-2.676
	(1.829)	(1.832)	(1.703)	(1.769)	(1.730)
unemployment foreign (share)		0.058	0.036	0.032	0.027
		(0.061)	(0.058)	(0.058)	(0.059)
unemployment Swiss (share)		-1.196**	-1.327***	-1.319***	-1.373***
		(0.488)	(0.462)	(0.459)	(0.466)
Social Democratic Party (share)			0.319***	0.312***	0.308***
			(0.040)	(0.040)	(0.040)
pensioners (share)			-0.244***	-0.227***	-0.216***
			(0.065)	(0.066)	(0.070)
Gini coefficient				0.127*	0.130*
				(0.069)	(0.069)
mean income (log)				-0.415	-0.508
				(2.071)	(2.098)
agriculture (share)					-0.215
					(0.294)
naturalization (share)					0.300
					(0.779)
Municipal FE	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓
Observations	2,476	2,476	2,476	2,476	2,476
R-squared	0.904	0.905	0.912	0.912	0.912

Note: These statistics include observations from our main sample. Votes embedded in partial or total revisions and singleton municipality observations are excluded. The number of observations is smaller for the variable parliament since this variable is not observed for all municipalities.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

the conditions under which the negative relationship between granting non-citizens the right to vote and the share of foreigners is more pronounced.

In various robustness exercises, the negative coefficient of foreigners remains robust regarding size and significance. To address endogeneity concerns, we present IV estimates as discussed in Section 1.6. Table 1.B.1 presents an overview of our IV estimations' first and second-stage results, which confirm the negative association between foreigner share and natives' willingness to enfranchise non-citizens. However, the coefficient is larger. As a next step, we estimate our model using the entire sample, including all referendum votes, thus also those related to partial or total constitutional revisions (Table 1.C.1, Spec. 1).

As many Swiss municipalities are small, we control for the role of municipality size in the following ways: First, we estimate our model with a non-linear effect of the absolute size of population (1.C.1, Spec. 2). Second, we exclude the 5% smallest municipalities in our sample (1.C.1, Spec. 3), and third, we additionally exclude the five percent largest municipalities from our sample (1.C.1, Spec. 4). To consider the different scopes of referendums concerning the demanded franchise, we perform an analysis with a reduced sample of only the quasi-identical referendums from the canton of Zurich (1.C.1, Spec. 5). As we observe different cantons and regions in our sample, we want to rule out the possibility that specific regional time trends drive our coefficient of interest. Therefore, in Table 1.C.2, we provide robustness checks where we account for differential linear time trends among language regions (1.C.2, Spec. 1), MS-regions (1.C.2, Spec. 2), and cantons (1.C.2, Spec. 3). Again, the coefficient of foreigner remains robust to this exercise. Throughout all robustness checks, the effect of the foreign population on the yes share remains similar in size and statistical significance—only in the IV estimates is the coefficient somewhat larger compared to the OLS estimates. Although these estimations provide stable results, we cannot fully rule out the possibility that unobserved time-variant factors affect our results. Similar to the recent literature that analyzed the enfranchisement of non-citizens across countries (Kayran and Erdilmen, 2020) or by opt-in rules (Stutzer and Slotwinski, 2021), our estimations provide robust and convincing evidence from a large municipality-level panel that an increasing share of foreigners in a municipality decreases the willingness of natives to share political power with them.

Figure 1.C.1 in the Appendix depicts the development of the share of foreigners in the cantons of our sample and for the whole of Switzerland over the 1980–2018 period. During our observed period, the share of foreign residents in Switzerland increased by seven percentage points, from 17.43 to 24.60 percent. Thus, for a municipality that faced an average increase in foreign residents, the estimates of our base model point to a decrease in the willingness to share power by 1.40 percentage points over the 1992–2016 period. This effect is considerable, as the mean yes share is 29.33 percent, and some referendums ended in close decisions (see Table 1.A.1).

1.7.2 Composition of the foreign population

So far, we have only considered the average effect of the foreigners' share on natives' willingness to enfranchise non-citizens. In the following section, we analyze potential heterogeneous effects to understand better the results provided in Table 1.C.1. Following the preference and the cultural-threat hypotheses, larger cultural distance (and, hence, larger preference heterogeneity) between the foreign and the native population could lead to a decreasing willingness to enfranchise non-citizens. In Section 1.5, we introduced three cultural distance measures between the native and the foreign population. The measures account for the share of non-citizens among the total foreign population in a municipality that is (i) culturally distant based on Inglehart and Baker (2000), (ii) originally from former Yugoslavia, and (iii) not migrating from a neighboring country of Switzerland. We hypothesize that the negative effect of *foreigner* on natives' willingness to enfranchise foreigners is more pronounced if the cultural distance is larger. To test this, we interact the share of culturally distant foreigners with the total share of foreigners. In Table 1.4, the moderator variables are the shares of culturally distant foreigners (three measures) among the total foreign population in a municipality, where the share of foreigners is our main predictor.

Note that the base effect of *foreigner* is not negative and statistically significant in all specifications in Table 1.4 since it depicts the effect of foreigners in a municipality without culturally distant foreigners. The same accounts for the base effects of the three measures of cultural distance, which yield positive estimates but relate to municipalities without foreigners (which only applies to a neglectable number of municipalities). Our coefficient of interest is β_3, which reflects the interaction of the share of residents from culturally more distant countries with the total share of foreigners. β_3 is negative and significant throughout all specifications and all measures in Table 1.4. In terms of size, β_3 in Table 1.4 varies between -0.003 and -0.007, which seems small. However, considering the average increase of non-citizen share in Switzerland of seven percent in our period of observation (see Figure 1.B.1), a one percentage point increase of *culturally distant* would account for -0.028 (7 * -0.004) percentage points of the yes share. The sample mean of *culturally distant* is 29.80 percent. Hence, the cultural distance to the native population seems to be an important channel to explain the conditions under which the presence of immigrants negatively shapes the natives' willingness to enfranchise non-citizens.

Figure 1 provides a graphical illustration of this interaction term. The slopes of the yes share are computed as a reaction to the varying foreigner share while holding the value of the moderator variable (culturally distant) constant at values running from 0 to 100 percent. The interaction of culturally distant * foreigners is represented by the slopes, which are consistently steeper for municipalities with a larger share of culturally distant foreigners (Figure 1).

As 1.1 illustrates, the cultural composition of the immigrant population seems to be an important moderator on how the share of foreigners affects natives' willingness to enfranchise non-citizens. To further test the reliability of this finding, Table 1.C.3 in the Appendix yields the results of various robustness checks. We perform the estimations

Table 1.4: Support for non-citizens' suffrage based on cultural differences.

VARIABLES	(1) Yes Share	(2) Yes Share
1st Measure: Based on (Inglehart and Baker, 2000)		
culturally distant (share)	0.051**	0.052**
	(0.022)	(0.021)
foreigner (share)	-0.121	-0.084
	(0.084)	(0.077)
culturally distant (share) * foreigner (share)	-0.004**	-0.004**
	(0.002)	(0.002)
Observations	2,476	2,476
R-squared	0.905	0.913
2nd Measure: Origin Former Yugoslavia		
former Yugoslavia (share)	0.034	0.035
	(0.023)	(0.024)
foreigner (share)	-0.138*	-0.109
	(0.074)	(0.069)
former Yugoslavia (share) * foreigner (share)	-0.007***	-0.006***
	(0.002)	(0.002)
Observations	2,476	2,476
R-squared	0.904	0.912
3rd Measure: Origin Non-Neighbor State		
not neighbor (share)	0.044***	0.037**
	(0.017)	(0.017)
foreigner (share)	-0.005	0.025
	(0.117)	(0.111)
not neighbor (share) * foreigner (share)	-0.003**	-0.003**
	(0.002)	(0.002)
Observations	2,476	2,476
R-squared	0.905	0.912
Control variables		✓
Municipal FE	✓	✓
Vote FE	✓	✓
Time FE	✓	✓

Note: The dependent variable is the *yesshare_{mtr}* in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Control variables include population size (log), unemployment share, share of Social Democratic Party, pensioner share, agriculture share, naturalization share, Gini coefficient, and mean income of natural persons (log).

*** p < 0.01, ** p < 0.05, * p < 0.1

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with the total sample, including all referendums, also those related to partial or total constitutional revisions (Table 1.C.3, Spec. 1 to 3), with a nonlinear effect of the absolute size of the population (1.C.3, Spec. 4 to 6), and exclude population outliers (1.C.3, Spec. 7 to 9). The results remain robust in most specifications and provide convincing evidence that the negative effect of the share of foreigners is more pronounced with an increasing share of culturally distant foreigners. There are at least three explanations for this result, which cannot be disentangled in this chapter. First, cultural distance's hindering effect may result from larger preference heterogeneity between the native and the (potential) foreign electorate. Moreover, the negative coefficient of the *not neighbor * foreigner* interaction may also point to the importance of common languages and geographical proximity in fostering trust, thereby accelerating the speed of integration (Bisin and Verdier, 2011; Guiso et al., 2009). Second, $\beta_3 < 0$ may also be explained by the role of cultural threat (Hainmueller and Hiscox, 2007; Kinder and Kam, 2009). Third, we cannot rule out that $\beta_3 < 0$ also captures discrimination against culturally distant foreigners (Hainmueller and Hangartner, 2013).
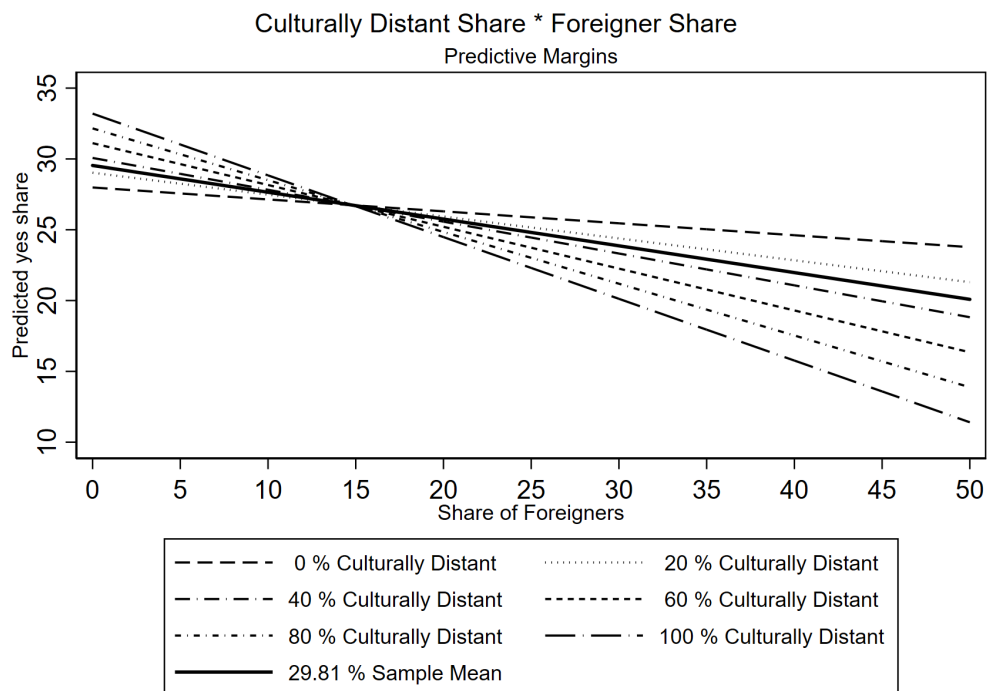
Figure 1.1: Non-citizens' suffrage & cultural differences - Predictive margins.

1.7.3 Institutional context

As elaborated in Section 1.6, we estimate Model 1.3 to account for the strength of representative democracy on the municipal level. We expect a higher willingness to enfranchise non-citizens in municipalities with a local parliament (as compared to a town meeting), as we assume the costs of political power sharing to be lower. Moreover, we assume this effect to grow in the share of foreigners, as the mentioned cost mechanism is more pronounced with a growing new electorate. If the costs of sharing political power are lower in representative democracy (parliament) than in direct democracy (town meeting), we expect *parliament * foreigner* (β_3) to be positive.

Table 1.5: Natives' support of non-citizens' suffrage based on institutions.

	No interaction	With interaction		
VARIABLES	(1) Yes Share	(2) Yes Share	(3) Yes Share	(4) Yes Share
parliament	0.928 (0.737)	-0.138 (1.201)		
foreigner (share)	-0.099*** (0.033)	-0.117*** (0.036)	-0.199** (0.079)	-0.180** (0.074)
parliament * foreigner (share)		0.057 (0.045)	0.187* (0.107)	0.229** (0.103)
Control variables	✓	✓		✓
Cantonal FE	✓	✓		
Municipal FE			✓	✓
Vote FE	✓	✓	✓	✓
Time FE	✓	✓	✓	✓
Observations	2,430	2,430	2,101	2,101
R-squared	0.756	0.756	0.905	0.912

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Control variables include population size (log), unemployment share, share of Social Democratic Party, pensioner share, naturalization share, Gini coefficient, and mean income (log). Votes embedded in a political package are excluded from the sample. The number of observations is smaller in Spec. (3) and (4) due to the introduction of municipality fixed effects, which drops singleton municipality observations.
*** p < 0.01, ** p < 0.05, * p < 0.1

As the municipal legislative institution is time-invariant, it is not possible to display the base effect of parliament when applying the municipality fixed effects in Table 1.5, Specifications (3) and (4). We start with a lean model considering cantonal fixed effects in Specifications (1) and (2) of Table 1.5. The introduction of a dummy for parliament in the cross-sectional analysis in Specification (1) does not display a significant effect on the willingness to enfranchise non-citizens. From Specification (2) onward, we introduce the interaction of the municipal legislative with the foreign share. When applying cantonal fixed effects in Specification (2), β_3 is positive but not statistically significant. A graphical illustration of Specification (2) can be found in Figure 1.C.2 in the Appendix. However, when municipality-fixed effects and further control variables are included in Specification (4), β_3 becomes positive and significant at the five percent level.

This result suggests that the share of foreign residents has a heterogeneous effect on the willingness to share power concerning the strength of representative democracy. An increase in the foreign share by one percentage point in municipalities with a parliament is associated with a 0.23 higher yes share than municipalities with a town meeting. Note that this effect stays positive and significant in most robustness exercises; these are presented in Table 1.C.4 in the appendix. The robustness tests in Table 1.C.4 include estimates for the whole sample (including total revisions), samples excluding outliers based on population size, a sample excluding the canton of Vaud (since they apply a sharp population threshold for the municipal legislative), a sample only of the canton of Zurich, and an estimation with cantonal time trends. The boosting effect of representative democracy, compared to direct democracy, can be explained by two mechanisms: first, by the lower price of sharing political influence in a representative democratic setting compared to a more direct-democratic environment (Koukal and Eichenberger, 2017) and second, by the controversially debated tendency of direct democracy to be harmful to minorities (Donovan and Bowler, 1998; Gamble, 1997; Haider-Markel et al., 2007; Hainmueller and Hangartner, 2019). Unfortunately, the data structure does not allow further disentangling of these two potential mechanisms but underlines the importance of local institutions in political power sharing.

1.7.4 Further discussion

This section discusses interesting results obtained for our control variables presented in Table 1.3. With the growth in municipality size, a citizen's vote is less likely to be decisive. Following the cost argument, population growth can translate into a higher willingness to share political rights with non-citizens in larger municipalities. However, when we look at the estimates in Table 1.3, we do not find evidence that population size significantly affects the approval of non-citizens' suffrage. Another factor that has gained much attention is the role of labor market competition. From Specification (2) onward, we introduce the municipal unemployment rate of the native and foreign populations as our proxy for economic competition. Throughout all specifications, the unemployment rate of natives exhibits a statistically significant negative effect on the citizens' willingness to enfranchise non-citizens. A one percent increase in the unemployment rate of natives is associated with a decrease in the willingness to enfranchise non-citizens by

approximately one percentage point. This aligns with the related literature, which explains anti-immigrant attitudes with increased labor market competition and economic downturn (Hainmueller and Hiscox, 2007; Hainmueller and Hopkins, 2014; Mayda, 2006; Scheve and Slaughter, 2001). The negative coefficient of *unemployment Swiss* hints at the explanation of economic threat, which may hinder political power sharing with non-citizens. In contrast, the unemployment of foreigners in one's municipality does not seem to impact the willingness of natives to enfranchise non-citizens.

Furthermore, in Table 1.3, from Specification 3 onward, we control for the vote shares of the Social Democratic Party, the most important left-wing party. As expected, the coefficient of the share of Social Democratic Party voters exhibits a positive and significant coefficient throughout all specifications in Table 1.3. Our estimates suggest that a one percent increase in the voters of the Social Democratic Party in a municipality increases the approval of non-citizens' suffrage by approximately 0.30 percentage points. There are multiple explanations for this relation. Given that foreigners have shown preferences for more leftist political positions (Strijbis, 2014; Vernby, 2013), the enfranchisement decision may be driven by a strategic calculus to shift the median voter to an income bracket that supports a political position (Meltzer and Richard, 1981). However, stronger support for the Social Democratic Party may also proxy for more leftist policy preferences within the native population, including the political integration of non-citizens.

In Table 1.3, the pensioners' share exhibits a robust negative and significant effect on the willingness to share political rights with non-citizens. Our estimates suggest that a one percent increase in pensioners in a municipality reduces the willingness to share political power with non-citizens by approximately 0.22 percentage points. A simple explanation for this observation constitutes the more conservative preferences of the elderly population. Furthermore, it can also point to the role of preference heterogeneity: If preference differences between elderly natives and foreigners (who are, on average, younger) are larger, enfranchising non-citizens will result in higher costs for elderly natives than for young natives.

1.8 Conclusion

In several developed countries, foreigners without political participation rights represent a large and growing percentage of the population. Therefore, the political integration of non-citizens is a major political challenge. Given the limited democratic legitimacy of decisions taken only by a fraction of the taxpayers and the potential positive economic and societal effects of political integration of non-citizens, it is important to understand the conditions under which native voters are willing to share political rights with non-citizens. While this chapter is among the first to consider this question, it relies on a much richer data set than previous studies. We explore a new municipality-level data set of Swiss cantonal referendums on the enfranchisement of non-citizens. The Swiss setting provides a unique laboratory for capturing the drivers of the enfranchisement of non-citizens, as it enables measuring the current electorate's revealed preferences. To

explain the willingness to enfranchise non-citizens, we focus on the role of the size and composition of the foreign population in different institutional settings.

Our estimates reveal that a growing share of foreigners in a municipality is negatively associated with the approval of non-citizens' voting rights. Thus, when confronted with a larger foreign population, the support to extend suffrage to non-citizens is lower. Our analysis reveals that this relationship is more pronounced in municipalities with strong direct democracy and the cultural distance between the native and foreign populations. There are at least three complementary explanations for this finding: First, the costs to enfranchise non-citizens are larger if the electorate loses more political influence (direct democracy and preference heterogeneity). Second, our results may also indicate a tendency for direct democracy to discriminate against outsiders. Third, attitudes-based mechanisms may also explain this result. The larger and more culturally distant the outgroup, the more threatened the natives may feel in their cultural identity, and the more prevalent the anti-immigrant attitudes may become. Furthermore, our results support the hypothesis that a tense situation in the labor market can negatively affect the enfranchisement of non-citizens.

Since many developed countries face an increasing share of foreign residents and cultural differences are likely to expand, our results imply that, in the future, citizens' willingness to enfranchise non-citizens may decline, and the lack of democratic legitimacy is not likely to be washed away automatically. Moreover, political integration via suffrage extension seems more likely in times of economic prosperity and phases of low unemployment. For future research on determinants of non-citizens' enfranchisement, individual data of the actual electorate might be helpful for a clearer distinction of instrumental power loss or discrimination channels.

Appendix

Chapter 1 Appendix

1.A Supplemental information on data and background
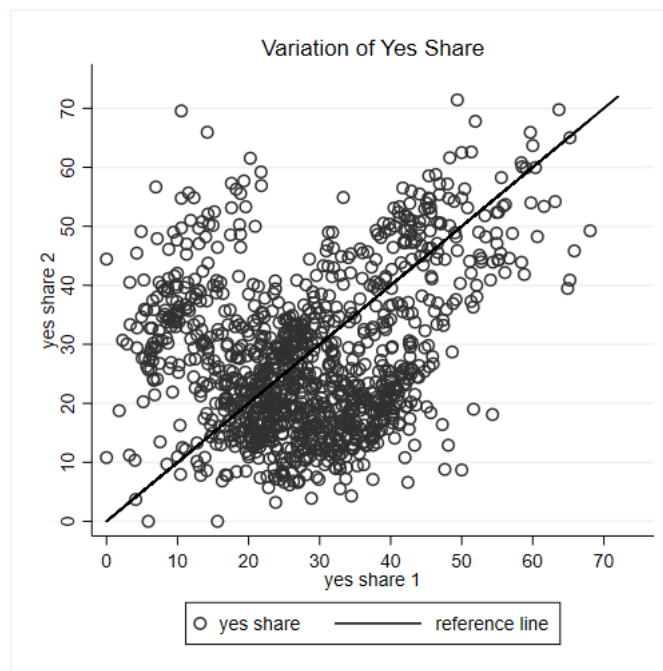
Figure 1.A.1: Yes shares in first and last cantonal vote on non-citizens' suffrage.

Note: Only the first and the last vote per canton is considered. Therefore, the variation is not identical to the variation used in our empirical analysis.

Table 1.A.1: Referenda on non-citizens' enfranchisement in our dataset.

Vote date	Canton	Suffrage	Yes Share	Vote type
27.09.1992	VD	Full local + full cantonal	26%	Initiative
06.06.1993	GE	Full local	29%	Initiative
28.11.1993	GE	Eligibility court of arbitration	45%	Counterproposal
26.09.1993	ZH	Full local opt-in	26%	Initiative
12.06.1994	BS	Full local + full cantonal	26%	Initiative
04.12.1994	BE	Full local + full cantonal	22%	Initiative
04.12.1994	BE	Full local opt-in	40%	Counterproposal
22.10.1995	UR	Active cantonal	16%	Initiative
10.03.1996	AG	Full local	16%	Initiative
09.06.1996	JU	Passive local opt-in	47%	Referendum
16.03.1997	FR	Full local	24%	Initiative
23.11.1997	SO	Full local + full cantonal	12%	Initiative
24.09.2000	NE	Active cantonal	77%	Complete revision
04.03.2001	GE	Full local	48%	Law revision
04.03.2001	SH	Active local + active cantonal	30%	Partial revision
22.09.2002	VD	Full local	56%	Complete revision
16.05.2004	FR	Full local	58%	Complete revision
30.10.2005	BS	Full local opt-in	77%	Complete revision
24.04.2005	GE	Full local	47%	Initiative
24.04.2005	GE	Active local + active cantonal	52%	Initiative
25.09.2005	SO	Full local opt-in	39%	Complete revision
17.06.2007	JU	Full local for executive	49%	Law revision
17.06.2007	NE	Passive local + passive cantonal	41%	Initiative
17.06.2007	NE	Passive local	54%	Counterproposal
26.09.2010	BS	Full cantonal	19%	Initiative
26.09.2010	BS	Active cantonal	39%	Counterproposal
26.09.2010	BE	Full local opt-in	28%	Initiative
04.09.2011	VD	Full cantonal	31%	Initiative
27.11.2011	LU	Active local opt-in	16%	Initiative
22.09.2013	ZH	Full local opt-in	25%	Initiative
28.09.2014	JU	Full local for executive	54%	Law revision
28.09.2014	SH	Full local + full cantonal	15%	Initiative
25.09.2016	NE	Passive cantonal	46%	Law revision
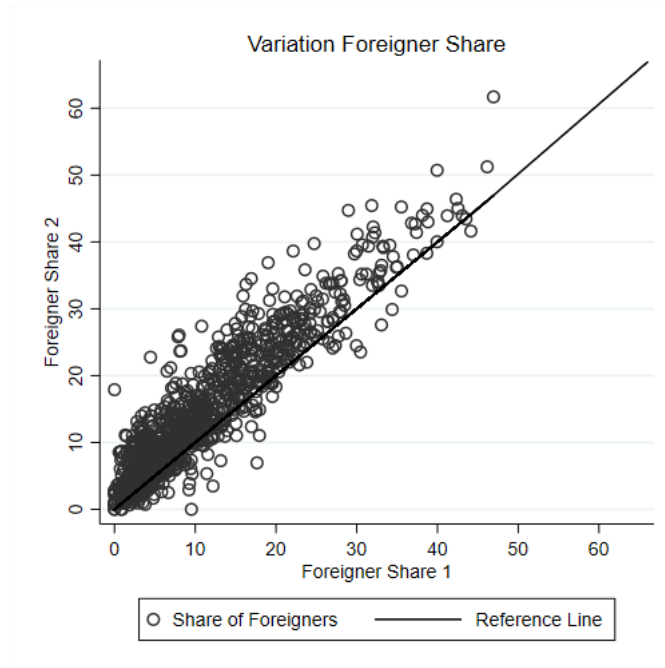
Figure 1.A.2: Foreigner shares in first and last cantonal vote on non-citizens' suffrage.

Note: Only the first and last votes per canton are considered. Therefore, the variation is not identical to the variation used in our empirical analysis.

Table 1.A.2: Classification of culturally similar and culturally distant states.

	1st **Measure:**	2nd **Measure:**	3rd **Measure:**
	Cultural distance based on Inglehart and Baker (2000)	Country of origin in former Yugoslavia	Country of origin in non-neighbor state
Classified as Culturally Similar	Australia Austria Belgium Canada Denmark Finland France Germany Great Britain Iceland Ireland Italy Liechtenstein Japan Netherlands New Zealand Norway Portugal South Korea Spain Sweden Taiwan United States	All remaining countries	Austria France Germany Italy Liechtenstein
Classified as Culturally Distant	All remaining countries	Bosnia-Herzegovina Croatia Kosovo Montenegro North Macedonia Serbia Slovenia (former) Yugoslavia	All remaining countries

Note: Liechtenstein does not participate in the WVS and was therefore not included in Inglehart and Baker's (2000) classifications. We still added Liechtenstein to the culturally similar countries since they are arguably culturally very close to the Swiss population (i.e., geographical proximity, language, Swiss francs, labor market, Swiss customs union).
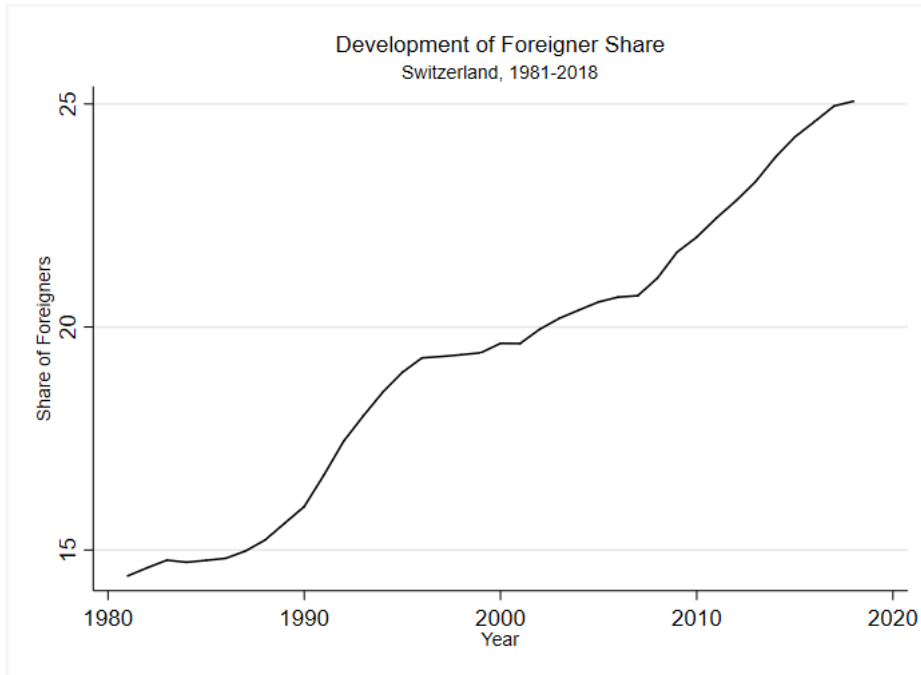
Figure 1.A.3: Share of non-citizens in Switzerland from 1980 to 2020.

Note: Federal statistical office.
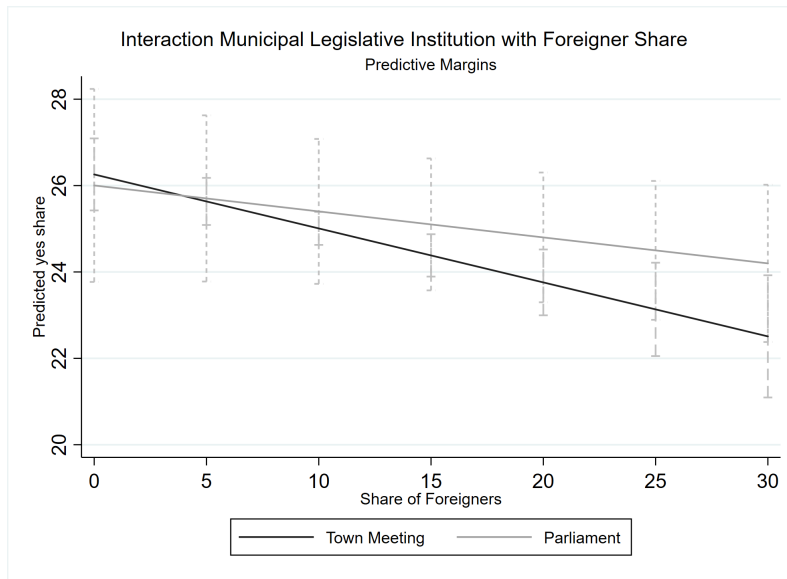
Figure 1.A.4: Non-citizens' suffrage & institutional background - Predictive margins.

Note: As base effects of time-invariant institutions cannot be displayed for our preferred speci-fication with municipality fixed effects, this figure reflects predicted margins for a specification with cantonal fixed effects.

1.B Instrumental variable approach
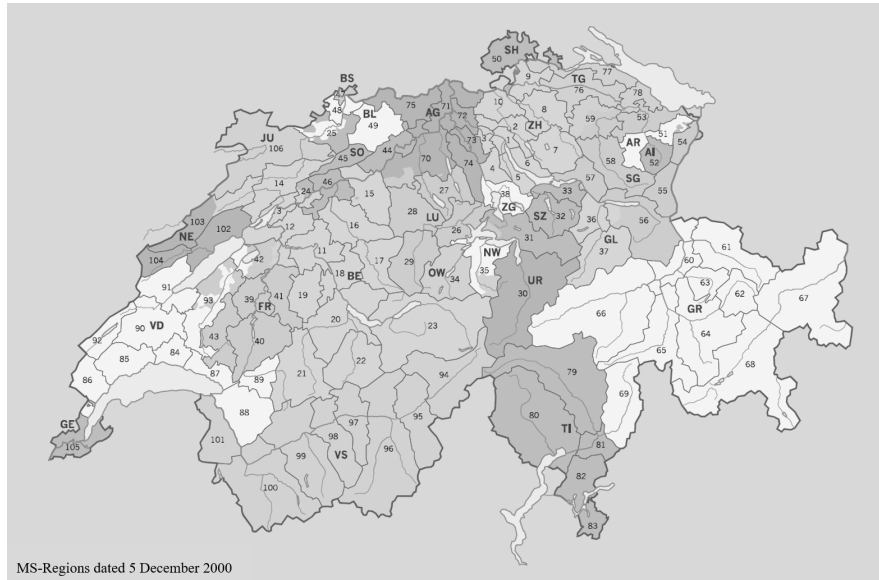
Figure 1.B.1: Map of MS-regions in Switzerland.

Source: Federal statistical office.

Table 1.B.1: IV estimations - non-citizen share in MS-region as an instrument.

IV - Second Stage: *Dependent variable: yes share*	(1) Incl. Total Revisions	(2) Excl. Total Revisions	(3) Incl. Total Revisions	(4) Excl. Total Revisions	(5) Excl. Pop. Outliers	(6) Excl. Outliers and Total Revisions
foreigner (share)	-0.749***	-0.587***	-0.665***	-0.420**	-0.652***	-0.459**
	(0.178)	(0.171)	(0.189)	(0.179)	(0.210)	(0.204)
Control variables			✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓	✓
Observations	2,971	2,476	2,971	2,476	2,631	2,194
Root MSE	4.526	4.049	4,399	3.86	4.081	3.603

First Stage: *Dependent variable: foreigner share in municipality*	(1)	(2)	(3)	(4)	(5)	(6)
foreigner MS-region (share)	0.767***	0.771***	0.753***	0.769***	0.714***	0.731***
	(0.057)	(0.061)	(0.059)	(0.064)	(0.059)	(0.066)
Observations	2,971	2,476	2,971	2,476	2,631	2,194
Clusters	1,074	984	1,074	984	957	871
Kleibergen-Paap F Stat	183.39	162.42	163.6	144.94	144.29	124.27

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. The foreigner share on the municipal level is instrumented by the foreigner share in the MS-region. Control variables include population size (log), unemployment shares, share of the Social Democratic Party, pensioner share, agriculture share, naturalization share, Gini coefficient, and mean income (log). In Spec. (5) and (6), the smallest and largest 5 % of the municipalities (in terms of population size) are excluded from the sample.
*** p < 0.01, ** p < 0.05, * p < 0.1

1.C   Robustness checks

Table 1.C.1: General robustness checks - Drivers of non-citizens' suffrage.

Dependent variable: yes share	(1) Incl. Total Revisions	(2) Population Linear, Square	(3) Excl. Pop. < 5%	(4) Excl. Pop. Outliers	(5) Only Zurich
foreigner (share)	-0.207***	-0.184***	-0.162***	-0.173***	-0.196**
	(0.068)	(0.063)	(0.061)	(0.064)	(0.082)
population (log)	-2.586		-4.472***	-3.498**	0.063
	(1.724)		(1.552)	(1.654)	(2.336)
unemployment foreign (share)	0.038	0.029	-0.072	-0.052	0.209
	(0.056)	(0.059)	(0.054)	(0.054)	(0.172)
unemployment Swiss (share)	-1.187***	-1.322***	-0.800*	-1.075**	-0.894
	(0.424)	(0.468)	(0.430)	(0.491)	(0.928)
Social Democratic Party (share)	0.253***	0.314***	0.325***	0.290***	0.278**
	(0.039)	(0.040)	(0.044)	(0.046)	(0.110)
pensioners (share)	-0.216***	-0.192***	-0.288***	-0.233***	-0.335***
	(0.070)	(0.070)	(0.065)	(0.071)	(0.089)
gini coefficient	0.075	0.136**	0.052	0.071	0.060
	(0.064)	(0.067)	(0.060)	(0.069)	(0.109)
mean income (log)	1.682	-1.130	2.586	2.888	1.351
	(2.171)	(2.008)	(2.246)	(2.393)	(3.220)
agriculture (share)	-0.110	-0.214	0.078	-0.336	1.833***
	(0.274)	(0.305)	(0.282)	(0.306)	(0.632)
naturalization (share)	0.482	0.152	0.585	0.576	1.201
	(0.639)	(0.781)	(0.587)	(0.628)	(1.324)
Municipal FE	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓
Time FE	✓	✓	✓	✓	✓
Observations	2,971	2,476	2,307	2,194	310
R-squared	0.925	0.912	0.925	0.920	0.879

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Votes embedded in a political package are included in Spec. (1). Spec. (2) controls for population^2 and population instead of population (log). In Spec. (3) the smallest 5 % of municipalities are excluded and in Spec. (4) also the largest 5 % are excluded. In Spec. (5) only the votes of the canton of Zurich are in the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 1.C.2: Robustness checks with time trends - Drivers of non-citizens' suffrage.

Dependent variable: yes share	(1) Language Region Trends	(3) MS Region Trends	(5) Cantonal Trends
foreigner (share)	-0.153**	-0.121*	-0.160**
	(0.062)	(0.069)	(0.063)
population (log)	-2.339	-1.846	-2.836
	(1.690)	(1.818)	(1.744)
unemployment foreign (share)	0.012	0.021	0.026
	(0.058)	(0.055)	(0.059)
unemployment Swiss (share)	-0.990**	-1.180**	-1.239***
	(0.439)	(0.474)	(0.411)
Social Democratic Party (share)	0.196***	0.173***	0.258***
	(0.041)	(0.040)	(0.039)
pensioners (share)	-0.187***	-0.223***	-0.218***
	(0.068)	(0.075)	(0.070)
Gini coefficient	0.110*	0.086	0.144**
	(0.064)	(0.063)	(0.063)
mean income (log)	-0.939	0.323	-0.928
	(2.008)	(2.133)	(2.090)
agriculture (share)	-0.354	-0.264	-0.233
	(0.297)	(0.323)	(0.290)
naturalization (share)	0.256	0.423	0.299
	(0.809)	(0.827)	(0.766)
Municipal FE	✓	✓	✓
Vote FE	✓	✓	
Observations	2,476	2,476	2,476
R-squared	0.917	0.921	0.775

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Votes embedded in a political package are excluded from the sample. Spec. (3) includes no vote FE as they are captured in the cantonal time trends.
*** p < 0.01, ** p < 0.05, * p < 0.1

Table 1.C.3: Robustness checks - Cultural distance and non-citizens' suffrage.

VARIABLES	Incl. Total Revisions			Pop. Linear and Squared			Excl. Population Outliers		
	(1) Yes Share	(2) Yes Share	(3) Yes Share	(4) Yes Share	(5) Yes Share	(6) Yes Share	(7) Yes Share	(8) Yes Share	(9) Yes Share
foreigner (share)	-0.129	-0.147**	0.091	-0.095	-0.126*	0.019	-0.067	-0.109	-0.038
	(0.087)	(0.074)	(0.133)	(0.078)	(0.069)	(0.111)	(0.082)	(0.072)	(0.126)
cult. distant (share)	0.060***			0.051**			0.050**		
	(0.021)			(0.020)			(0.020)		
cult. distant (share) * foreigner (share)	-0.004**			-0.004**			-0.004***		
	(0.002)			(0.002)			(0.002)		
former Yugo. (share)		0.030			0.029			0.042	
		(0.024)			(0.023)			(0.027)	
former Yugo. (share) * foreigner (share)		-0.006***			-0.006***			-0.006***	
		(0.002)			(0.002)			(0.002)	
not neighbor (share)			0.045**			0.036**			0.037**
			(0.018)			(0.017)			(0.018)
not neighbor (share) * foreigner (share)			-0.005***			-0.003**			-0.002
			(0.002)			(0.002)			(0.002)
Control variables	✓	✓	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓	✓	✓	✓
Observations	2,971	2,971	2,971	2,476	2,476	2,476	2,194	2,194	2,194
R-squared	0.926	0.925	0.925	0.913	0.912	0.912	0.921	0.920	0.921

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Spec. (1) to (3) include votes embedded in a political package. Spec. (4) to (6) include population (total) and population (squared) instead of population (log). In Spec. (7) to (9) the smallest and largest 5 % of the municipalities (in terms population size) are excluded from the sample. Control variables include population size (log), unemployment shares, share of Social Democratic Party, pensioner share, agriculture share, naturalization share, Gini coefficient, and mean income (log).
*** p < 0.01, ** p < 0.05, * p < 0.1

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Table 1.C.4: Robustness checks - Institutional background and non-citizens' suffrage.

Dependent variable yes share	(1) Incl. Total Revisions	(2) Population Linear, Square	(3) Excl. Pop. < 5%	(4) Excl. Pop. Outliers	(5) Excl. Vaud	(6) Only Zurich	(7) Cantonal Timetrends
foreigner (share)	-0.221***	-0.184**	-0.209***	-0.174**	-0.216**	-0.260***	-0.165**
	(0.078)	(0.074)	(0.073)	(0.075)	(0.084)	(0.086)	(0.074)
parliament * foreigner (share)	0.182	0.200*	0.266***	0.154	0.633***	0.368***	0.214**
	(0.113)	(0.111)	(0.102)	(0.132)	(0.138)	(0.090)	(0.102)
Control variables	✓	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓	✓	
Year FE	✓	✓	✓	✓	✓	✓	
Cantonal Time Trends							✓
Observations	2,455	2,101	1,951	1,877	1,731	306	2,103
R-squared	0.925	0.912	0.925	0.917	0.927	0.885	0.770

Note: The dependent variable is the $yesshare_{mtr}$ in votes on non-citizens' enfranchisement. Robust standard errors in parentheses are clustered at the municipal level. Control variables include population size (log), unemployment shares, share of Social Democratic Party, pensioner share, agriculture share, naturalization share, Gini coefficient, and mean income (log).
*** p < 0.01, ** p < 0.05, * p < 0.1

Chapter 2

Empowering the next generation: The role of direct democracy in youth enfranchisement*

2.1 Introduction

Over the past two centuries, suffrage has been gradually extended to all male citizens, all ethnic groups, all genders, and, more generally, all adult citizens. This evolution has engendered interest in studying the drivers of, and barriers to, the enfranchisement of new groups. Today, many democracies are home to ongoing debates about lowering the voting age and extending suffrage to younger citizens. While the effects of a lower voting age have gained scholarly attention (e.g. Bertocchi et al., 2020; Stockemer and Sundström, 2018; Wagner et al., 2012; Zeglovits and Zandonella, 2013), little is known about what conditions accelerate the enfranchisement of the young.

Starting in the late 1960s, many Western democracies lowered the voting age to 18. The United Kingdom was the first in 1969, followed by the United States, Canada, and many other countries in the ensuing years (Larsen et al., 2016). Strikingly, the timing of suffrage extensions between representative- and direct-democratic contexts has differed significantly. Switzerland, which is renowned for its direct-democratic institutions, was not only among the last Western democracies to enfranchise women but also was one of the last democratic countries to lower the age threshold to 18. While the Swiss parliament voted in favor of lowering the voting age to 18, the Swiss electorate overruled

this decision in 1979 in a referendum vote. Similar patterns were observed in Denmark in 1969 for a voting age of 18 (Nielsen, 1970) and in Luxembourg in 2015 for a voting age of 16 (Dumont and Kies, 2016).

This chapter sheds light on the conditions for extending suffrage to younger citizens by analyzing the effects of direct democracy on the electorate's willingness to lower the voting age. This chapter, therefore, adds to the literature on how political institutions can affect the stability or emergence of new political institutions (see Figure 1, Channel C). We exploit new municipality-level data from two Swiss federal referenda regarding lowering the voting age from 20 to 18 in 1979 and 1991. This setting is interesting because the role of institutions can be analyzed through the lens of three specific characteristics of Switzerland. First, suffrage extensions can only be granted by the current electorate via direct-democratic referendums. Second, Switzerland is known for its high degree of decentralization, which also applies to extending federal, cantonal, and municipal voting rights. Thus, lowering the voting age from 20 to 18 was a protracted process, involving a large number of cantonal referendum votes for extending the franchise at the cantonal level and two federal votes for extending the franchise at the federal level, the first of which was rejected in 1979 and the second of which was accepted in 1991. Third, Swiss municipalities differ with respect to the extent of direct-democratic instruments. The municipalities' primary legislative power relies upon either a municipal town meeting (direct democracy) or a municipal parliament (representative democracy).

The institutional variations and the staggered lowering of the voting age at the cantonal level enable us to disentangle the role of direct democracy in youth enfranchisement in a Difference-in-Differences framework. We analyze the effects of local direct democracy compared to local representative democracy on the electorate's willingness to lower the voting age at the national level in contexts with and without local youth suffrage. We contribute to the literature about extending suffrage, and we add to the debate about whether direct democracy is harmful to outsiders (e.g. Frey and Goette, 1998; Gamble, 1997; Hainmueller and Hangartner, 2019).

According to our results, voters within direct-democratic municipalities are more reluctant to extend suffrage to the youth compared to voters within representative-democratic municipalities if the youth is not yet enfranchised at the local level. This aligns with the idea that voters react systematically to the anticipated power loss associated with suffrage extensions, particularly in direct-democratic settings where citizens wield more power. Complementing previous studies about the stifling role of direct democracy in relation to extending suffrage to women and non-citizens (e.g. Koukal and Eichenberger, 2017; Stutzer and Slotwinski, 2021), this chapter hints at a more general effect of direct democracy. However, as soon as the youth population is enfranchised at the local level, voters from direct-democratic municipalities are more willing to lower the voting age at the federal level compared to voters from representative-democratic municipalities. This positive effect is more pronounced for municipalities that have experienced local youth suffrage for a longer period of time. The increased interaction between the established and new electorate in direct-democratic settings may explain this effect. Our estimates also shed light on socio-demographic variables that we used

as controls, reinforcing prior findings that older voters and those from rural areas tend to be more hesitant to lower the voting age (Birch et al., 2015; Svensson, 1979).

The remainder of this chapter is organized as follows: The next section summarizes the literature regarding catalysts behind suffrage extensions in general and in relation to the lowering of the voting age in particular, and it delineates our theoretical considerations. Section 2.3 discusses the institutional context and the process of youth enfranchisement in Switzerland. Section 2.4 describes our data and presents our descriptive statistics. Section 2.5 explains the empirical methodology. In Section 2.6, we present and discuss the results by focusing on the variable of interest and taking a glance at the effects of the controls. Section 2.7 summarizes our main findings and provides an outlook for future research.

2.2 Literature and theoretical considerations

Lowering the voting age can exert various effects, such as shifting the median voter's position and thus political outcomes (Funk and Gathmann, 2015), supporting the formation of habits related to the formal political participation of the young (Gidengil et al., 2016; Wagner et al., 2012), increasing interest in politics among the affected group (Zeglovits and Zandonella, 2013), or reducing the average age of the members of parliaments (Stockemer and Sundström, 2018). However, the (recent) empirical literature elucidating the conditions that influence the enfranchisement of young citizens is rather scarce.

The conditions of suffrage extensions to all men, women, and non-citizens have garnered substantial attention in the empirical and theoretical literature. Compared to females' or non-citizens' enfranchisement, at least two important differences arise when analyzing the conditions for lowering the voting age. First, the group to be enfranchised is smaller, and its members are less likely to make use of their political rights (Blais and Dobrzynska, 1998; Franklin, 2004; Rekker, 2022). Second, the mechanisms might vary if voters share their political rights with their own offspring.[1]

In the economic literature, a commonly discussed barrier impeding suffrage extensions is divergent preferences between the old and new electorate. If the new electorate shifts the median voter's position, political outcomes can change.[2] Preferences differ based on socio-demographic factors, such as gender, income, and nationality (Abrams and Settle, 1999; Aidt and Franck, 2015; Bertocchi, 2011; Conley and Temimi, 2001; Gonnot, 2022). The literature also highlights a generation gap in politics, as younger voters generally harbor more leftist political preferences and exhibit a tendency to vote for younger politicians (Fraga and Holbein, 2020; Leighley and Nagler, 2014; Rekker, 2022). Moreover, Bertocchi et al. (2020) demonstrate that the responsiveness of politicians to the needs of the youth (e.g., through investments in education) increases with their enfranchisement. Therefore, the more the preferences differ between young and old voters, the more the new electorate can shift the median voter's position and thereby al-

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[1] Rattsø and Sørensen (2010) or Kamijo et al. (2020) provide evidence for family and parental altruism.
[2] See for instance Meltzer and Richard (1981) or Jensen and Yntiso (2019) for a discussion about the redistributionist hypothesis.

ter political outcomes. In line with this notion, the willingness to lower the voting age is negatively associated with age (Bergh, 2013; Birch et al., 2015; Svensson, 1979), income (Birch et al., 2015), rurality (Svensson, 1979), and conservatism (Svensson, 1979).[3]

However, preference heterogeneity between the old and the new electorates can also be regarded as a contributor to suffrage extensions, as the lack of suffrage increases the risk of social unrest. In the literature, this is referred to as the "threat of revolution hypothesis" (Acemoğlu and Robinson, 2001; Aidt and Franck, 2013, 2015; Aidt and Jensen, 2014), according to which the mobilization of the masses has the potential to threaten the political elite (Acemoglu and Robinson, 2000; Collier, 1999). This hypothesis posits that the young can use protests, demonstrations, or riots to articulate their preferences when they do not feel represented in the formal political process. Today, the youth frequently use political demonstrations and school protests to express their preferences (Boulianne et al., 2020). However, whether the threat of revolution plays a role in lowering the voting age is unclear, as the group to be enfranchised is smaller compared to other groups that have benefited or could benefit from suffrage extensions. Moreover, the demands of the youth for formal voting rights might be missing (Beck and Jennings, 1969).[4]

The extent of the political power loss induced by suffrage extensions depends not only on preference heterogeneity but also on the institutional background. Anecdotal evidence suggests that direct-democratic instruments may hinder the enfranchisement of new groups (Dumont and Kies, 2016; Nielsen, 1970). While some scholars attribute this to discrimination against minorities under direct democracy (Gamble, 1997; Hainmueller and Hangartner, 2019), a political economy-based explanation would emphasize that the political power loss in the context of direct democracy is higher. In the case of Swiss women's enfranchisement, Koukal and Eichenberger (2017) demonstrate that local direct democracy, compared to local representative democracy, hinders franchise extension at the federal level, but only if a municipality has not introduced local female suffrage and if the present voters still fear losing power at the local level. A similar pattern has been uncovered in relation to the enfranchisement of foreign residents in Switzerland (Koukal et al., 2021). Whether the same mechanisms apply to the lowering of the voting age remains an empirical question.

2.3   Swiss political institutions and youth enfranchisement

In Switzerland, citizens can influence and overrule the policies and decisions of the parliament and government via referenda and initiatives. Popular votes are frequently held at all federal levels (i.e., several times per year) and complement representative democracy

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[3]Larsen et al. (2016) reveal a positive effect of mock elections in relation to introducing a lower voting age. Although the authors cannot disentangle a causal link, experience with the new group might play a role.

[4]Using survey data from U.S. high school seniors (12th graders), Beck and Jennings (1969) attribute the initial failure to lower the voting age in specific states to a lack of demand from the affected group. However, Wagner et al. (2012) find that 16 and 17-year-olds are not systematically less motivated or less able to participate in the political process in Austria compared to the current electorate.

in the broader universe of policy dimensions (Frey, 1994, 2005; Ladner, 2012). Therefore, in contrast to most other democratic countries, suffrage extensions are not decreed by the national parliament. Instead, the current electorate decides on concrete proposals for extending the franchise via popular votes. These proposals are initiated by the parliament or by a group of citizens who start a popular initiative by collecting a certain number of signatures (on the Swiss federal level, 100,000 signatures, which is about 2 percent of the electorate). All popular votes are carried out as secret ballots. Suffrage extensions are due to the federal structure instituted separately at the cantonal and federal levels. In each canton, the current electorate determines whether to enfranchise young adults at the cantonal and municipal levels. There were popular votes concerning lowering the voting age at the cantonal level, at the municipal level, at both levels (integral), and on delegating the decision to lower the voting age to the municipalities (opt-in). In all of these votes, the proposal to extend the franchise is accepted if the majority of eligible voters favors the extension.

2.3.1 Youth enfranchisement

We focus on two federal referendums to lower the voting age from 20 to 18. The first popular vote was conducted in 1979 and was narrowly rejected, with a yes share of 49.2%. The most important arguments forwarded by the opponents of youth enfranchisement were the following: (i) The resulting difference between the voting age and the legal age of maturity (20 years) was deemed to be problematic; (ii) the 18–to-20-year olds had no clear demand for being enfranchised; and (iii) the youth population neither had the time for nor interest in participating in politics.[5] The second vote was held in 1991 and was clearly accepted, with a majority of 72.7%. As a result, starting in 1991, young adults aged 18 and 19 could participate in federal elections and popular votes.

In contrast to the joint implementation of a lower voting age at the federal level in 1991, the situation on the cantonal level was different. Three cantons (Schwyz, Neuchâtel, and Jura) lowered the voting age for cantonal and municipal elections and referendums before the first federal vote in 1979; 13 cantons did so between the two federal votes (Berne, Basel-City, Basel-Country, Geneva, Glarus, Obwalden, Nidwalden, Schaffhausen, Ticino, Uri, Vaud, Zug, and Zurich), and ten cantons did so after the second federal vote (Appenzell Outer-Rhodes, Appenzell Inner-Rhodes, Argovia, Fribourg, Grisons, Lucerne, Solothurn, St. Gallen, Thurgovia, and Valais).[6] An illustration of the geographical and chronological distributions of youth enfranchisement is provided in Figure 2.3.1. All ten latecomer cantons lowered the voting age at the cantonal and local levels within one year after the second federal vote in 1991 (see Table 2.A.1).

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[5]For more information on pro and contra arguments, see the voting booklet on https://swissvotes.ch/vote/293.00.

[6]The information about enfranchisement at the cantonal level stems from the cantonal archives. Unfortunately, it was not possible to gather detailed information about the exact number of referendum votes in each canton. Opt-in rules that delegate decisions regarding enfranchising the 18- and 19-year-olds at the local level have been introduced in Lucerne (1986) and Grisons (1989).
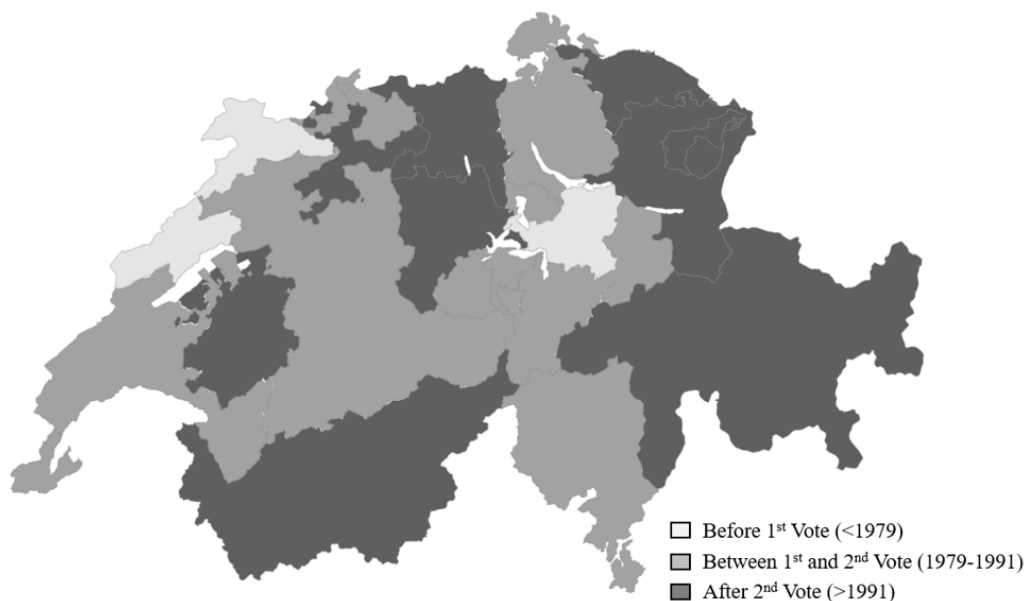
Figure 2.3.1: Chronology and geography of lowering the voting age to 18.

2.3.2 Institutions at the local level

The legislative branch of Swiss municipalities is either organized as a town meeting or as a parliament. In municipalities with a town meeting, all eligible voters meet one to four times per year to discuss and determine the most important municipality issues. In these town meetings, participants can participate in the public discourse, articulate the intensity of their preferences, establish the agenda by advancing new proposals, and observe both the discussion and voting behavior of other participants. In contrast, in municipalities with local parliaments, voters delegate part of their democratic rights to the politicians. Therefore, town meetings constitute a direct-democratic institution, and parliaments constitute a representative-democratic one. In 1988, approximately 17 percent (i.e., 493) of the municipalities organized their legislative as a parliament (Ladner, 2008). Table 2.A.2 in the Appendix indicates the distribution of municipalities with town meetings and with parliaments. Notably, citizens of municipalities with a parliament as their legislative branch also possess different direct-democratic instruments.

2.4 Data and variables

The empirical analysis is based on a panel of municipal results for two federal votes regarding lowering the voting age to 18 in 1979 and 1991. A salient advantage of this context is that the current electorate voted twice about the identical question.[7] We

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[7]This setting allows us to control for potential unobserved local preferences in relation to lowering the voting age that may have been prevalent in the first federal vote.

collected and digitized municipal-level voting data provided by the federal or cantonal statistical offices and matched it with federal decennial census data as well as data regarding municipal institutions provided by Ladner (1988). We only keep those municipalities in our dataset for which all control variables were observable, resulting in a fully balanced panel of 4,122 municipal observations stemming from 2,061 municipalities.[8] However, in our base estimations, we have excluded the first mover cantons, as they introduced local youth suffrage before the first federal vote. The descriptive statistics for the observations used in our base estimations for the full list of variables are listed in Table 2.4.1.

Table 2.4.1: Descriptive statistics

Variable	N	Mean	SD	Min.	Max.
yes share	3,840	58.38	17.17	0	100
direct democracy	3,840	0.89	0.31	0	1
local youth suffrage	3,840	0.24	0.43	0	1
experience	3,840	0.21	0.40	0	1
population	3,840	2726.65	11853.61	19	369522
population (log)	3,840	6.78	1.37	2.94	12.82
agriculture	3,840	16.29	14.98	0	96.88
foreigner	3,840	8.34	7.51	0	51.20
pensioner	3,840	13.80	4.53	0	39.81
young (<20)	3,840	28.32	4.77	3.85	64
women	3,840	49.15	2.51	32.56	63.35
married	3,840	48.17	4.02	25.64	62.50
Catholics	3,840	43.50	32.48	0	100
German	3,840	66.80	38.90	0	100
unemployed	3,840	1.07	1.16	0	21.57

Dependent variable

The endogenous variable *yes share* denotes the yes share in municipality m at time t in favor of lowering the voting age from 20 to 18. The two federal votes were held in 1979 and 1991, respectively.

Explanatory variables

We use data from the municipality survey by Ladner (1988) to operationalize the extent of direct democracy at the municipal level. The variable *direct democracy* indicates whether the legislative branch of a municipality consists of a direct-democratic town

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[8]Without data cleaning, we would end up with a dataset stemming from 2,561 municipalities. Most observations were lost because information about the institutional setting was missing. Since the data quality for the canton of Fribourg was poor, some observations for this canton had to be excluded from the sample. The canton of Ticino had to be dropped entirely from the sample, as no data was available.

meeting or a representative-democratic parliament. In our observation period, the municipal institutional setting was stable. Transitions from a town meeting to a parliament (or vice versa) were rare. Based on the information provided by Funk and Litschig (2020), we identified 20 municipalities that changed their institutional settings in the observation period, which is too little for a specific switcher analysis. Therefore, we excluded these observations, making the variable direct democracy a time-invariant variable.

The variable *local youth suffrage* captures whether the youth is already (*local youth suffrage = 1*) or not yet (*local youth suffrage = 0*) enfranchised in the municipality m at time t. As explained in Section 2.3, three cantons introduced local suffrage before the first vote, 13 cantons did so between the two federal votes, and ten cantons did so after the second vote. We provide graphical illustrations of the development of the yes shares separately for municipalities with town meetings or parliaments among (i) all cantons that lowered the voting age before 1979 or after 1991 (Figure 2.B.1) and (ii) all cantons that lowered the voting age between 1979 and 1991 (Figure 2.B.2). Moreover, we introduce a dummy variable *experience* that indicates, for the in-between mover cantons, whether they had introduced local youth suffrage shortly after the first federal vote in 1979 (experience = 1) or only shortly before the second federal vote in 1991 (experience = 0).

Control variables

We control for several socio-demographic variables, which are both mentioned in the economic and political literature about suffrage extensions and that are available at the municipality level. Following Svensson (1979), we account for the degree of rurality and urbanity by controlling for *population* size, the share of *agriculture*, and the share of *foreigners* present in a municipality. We also control for the demographic characteristics of the municipalities. Since the expected change in the size of the future electorate has been found to affect the timing of suffrage extensions (Braun and Kvasnicka, 2013; Koukal et al., 2021; Stutzer and Slotwinski, 2021), we control for the share of *young* residents (under 20 years old). We further include the *pensioner* share to control for the electorate's demographic composition, as the literature indicates a negative correlation between the age of the current electorate and the willingness to enfranchise the youth (Birch et al., 2015; Svensson, 1979). Furthermore, we control for further population characteristics, such as the share of women or married inhabitants. Moreover, we control for culture by introducing the share of *Catholics* and the share of *german* speakers. The *unemployment* rate reflects the economic situation in a municipality. As discussed in Section 3, preference heterogeneity between the current and the old electorate may affect the decision to enfranchise a new group. Some of these control variables, such as agriculture or pensioner share, are also proxies for the differences in the preferences between the old and the new electorates.

A look at the raw data

Table 2.4.2 provides a first descriptive picture of the yes share in the votes on lowering the voting age, separated for municipalities with local town meetings as opposed to local parliaments. The willingness to lower the voting age was smaller in municipalities

with town meetings in both federal referendums, which seemingly supports the hypothesis that direct democracy results in discrimination (Gamble, 1997). The average yes share increased from the first to the second federal vote by about two percentage points more in the direct-democratic municipalities compared to the representative-democratic municipalities.

Table 2.4.2: Mean Yes Shares in Federal Votes

Ø Mean Yes Share	Town Meeting	Parliament
vote 1979	44.68 %	50.19 %
vote 1991	72.61 %	76.09 %

Figure 2.4.1 illustrates the inter-municipal development of the yes share between the first and the second referendum within different institutional contexts. Overall, Figure 2.4.1 underscores a positive time trend: The willingness to lower the voting age increased over time both in municipalities with direct-democratic institutions and municipalities with representative-democratic institutions, as is also evident in the density plots in Figure 2.A.1 in the Appendix.
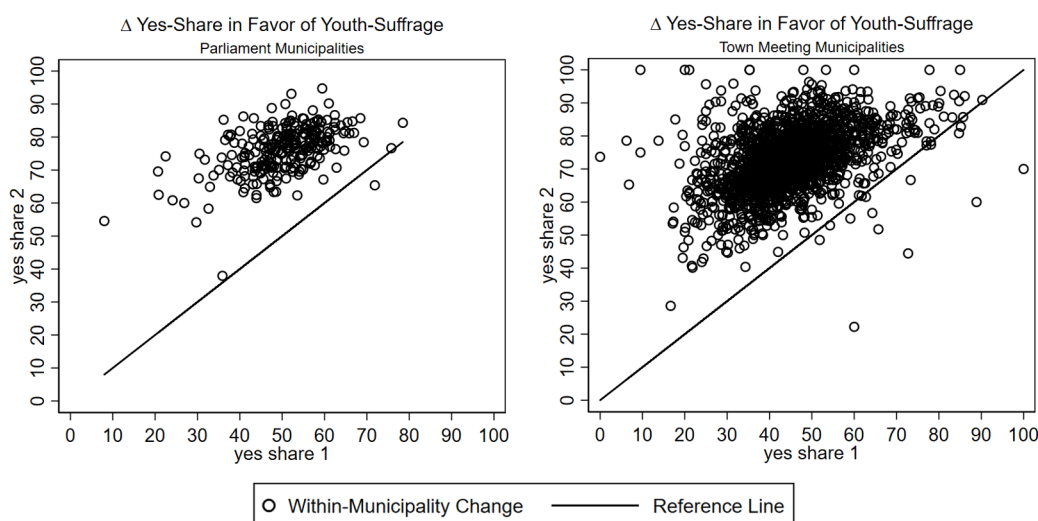
Figure 2.4.1: Development of yes share in town meeting and parliament municipalities.

2.5   Empirical strategy

Based on our theoretical considerations and previous empirical findings, we expect the institutional context and the extent of preference heterogeneity to affect the willingness of

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the current electorate to enfranchise the young. In this section, we explain how our data structure enables us to disentangle different mechanisms at play in direct-democratic environments as opposed to representative-democratic environments in the process of lowering the voting age.

Disentangling institutional channels

The Swiss context, characterized by its institutional variation and the staggered introduction of cantonal youth suffrage, allows the exploitation of the following differences:

- Municipalities with town meetings vs. municipalities with parliaments (i.e., direct-democratic vs. representative-democratic).

- Municipalities from cantons that lowered the voting age vs. municipalities from cantons that did not lower the voting age for municipal decisions between the vote in 1979 and the vote in 1991.

We observe the effects of local direct democracy compared to local representative democracy on voters' willingness to enfranchise the youth population at the federal level before and after local youth suffrage has been introduced. In cantons without youth suffrage at the local level, the (expected) power loss of the citizens when lowering the voting age at the federal level encompasses two elements:

i) They lose power at the federal level.

ii) They are likely to also lose power at the local level, as the lower voting age at the federal level increases the pressure on the cantons to reduce the voting age for municipal and cantonal issues. The fact that all cantons lowered their voting age within a year after the accepted federal vote in 1991 supports this assumption (see Table 2.A.1 in the Appendix).

While the first element of the expected loss of power is independent from the local institutions, the second element is dependent upon the local institutions. The voters from direct-democratic municipalities face a larger expected loss of power than those from representative-democratic municipalities. Therefore, we expect the behavior of voters in the second federal vote to depend upon whether their canton has decreased the voting age before this vote. If the canton has not done so, the voters endure a fear of a loss of power at the local level, which is more substantial if they are from municipalities with direct-democratic institutions. In contrast, in cantons that have lowered the voting age before the second federal vote, there is no reason for a differential effect between the two institutional types of municipalities. Consequently, we expect a negative effect of municipal direct democracy on the *yes share* for lowering the voting age at the federal level in cantons that have not lowered the voting age before the second federal vote. Regarding the effect of direct democracy in cantons that have already lowered the voting age before the second federal vote, we have no firm hypothesis. It could be positive, as local youth suffrage allows the electorate to gain experience with younger citizens in politics.

We observe the institutional variation of interest (direct- vs. representative-democratic) on the municipal level. In contrast, the decision to lower the voting age at the local level is made at the cantonal level and is therefore exogenous to the preferences mapped within an individual municipality.[9]

At the time of the first federal referendum in 1979, three Swiss cantons had already lowered the voting age on the local level.[10] To compare municipalities under similar enfranchisement conditions, we exclude these three pioneer cantons in our main estimations. Therefore, local youth suffrage is equal to zero for all municipalities in our data set at the time of the first federal referendum vote. At the time of the second federal referendum, 13 more cantons had already introduced local youth suffrage, whereas in the remaining 10 cantons, a lower voting age had not yet been introduced.

Empirical strategy

We analyze how *direct democracy* affects the willingness of the current electorate to lower the voting age when the youth is or is not yet enfranchised at the local level. Therefore, we estimate Model 2.1 and interact *direct democracy* and *local youth suffrage* and add cantonal and year-fixed effects:

$$yes\ share_{mt} = \beta_0 + \beta_1\ direct\ democracy_m + \beta_2\ local\ youth\ suffrage_{ct}$$
$$+\beta_3\ direct\ democracy_m * local\ youth\ suffrage_{ct} \qquad (2.1)$$
$$+\theta\ X_{mt} + \delta\ canton_c + \gamma\ year_t + \epsilon_{mt}$$

where X_{mt} is a vector of controls at the municipal level m in referendum year t. By examining the interaction between *direct democracy* and *local youth suffrage*, we shed light on the potential ambiguous effects of direct democracy in the process of enfranchising the youth population. Since *direct democracy* is a time-invariant variable, its base effect cannot be displayed when using municipal fixed effects. We therefore introduce cantonal fixed effects δ in our base model to better control for cantonal differences.[11] The introduction of cantonal fixed effects, however, limits the available variation as not all cantons dispose of the same institutional variation. To address this issue, we use municipal fixed effects in the robustness checks. By doing so, we can take advantage of the variation between different cantons.

For a causal interpretation, one must rely on the common trend assumption, meaning that in the absence of the treatment (local youth suffrage = 0), the difference in the outcome between the town meeting municipalities and the parliament municipalities

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[9]As a robustness check, we will exclude municipalities with "extreme" opinions from our sample (see Table 2.C.4). We perform robustness checks, in which we exclude the cantons that introduced opt-in rules, meaning that the decision to grant voting rights to the youth has been delegated to the individual municipalities (see Table 2.C.2).

[10]The three cantons were Schwyz, Jura, and Neuchâtel. See Section 2.3 for details about the enfranchisement process of the youth population in Switzerland.

[11]The introduction of *local youth suffrage* is, in most cases, determined at the cantonal and not the municipal level. Therefore, the variable *local youth suffrage* is exogenous to the differences between the individual municipalities within a canton. We exploit this difference when introducing cantonal fixed effects.

would remain constant over time. Unfortunately, this assumption cannot be formally tested, as it is not possible to observe the counterfactual trend in the yes shares. Nevertheless, we apply different approaches to assess the plausibility of the common trend assumption. As we use cantonal fixed effects in our base model, town meeting and parliament municipalities are embedded in the same geographical, economic, and linguistic contexts.

First, we look at municipalities that experienced no changes in their treatment status during our period of observation (see Section 2.3). This is the case for two groups of municipalities: (i) the municipalities of the three first-mover cantons, which lowered the voting age to 18 before 1979 (local youth suffrage always 1), and (ii) the municipalities of the ten latecomer cantons, which lowered the voting age at the local level after 1991 (local youth suffrage always 0). Graphical illustrations of the trends for town meeting and parliament municipalities, which have not undergone changes in the treatment status, are presented in the Appendix in Figure 2.B.1. The graphs support the notion of common trends for the two institutional types. In most cantons with no changes in the local voting age, the willingness to lower the voting age evolved similarly from 1979 to 1991. Figure 2.B.2 maps the time trend of our core treatment group, which consists of municipalities that granted local voting rights between the first and the second national votes, where we did not expect common trends between the two institutional types. Figure 2.B.2 indeed reveals different time trends for town meeting and parliament municipalities; slopes are steeper for municipalities with town meetings compared to municipalities with a parliament in the same canton. Two cantons (Grison and Lucerne) introduced opt-in rules between the first and second national votes. These opt-in rules delegate the decision to lower the voting age for municipal matters to the respective municipality. With such an opt-in rule in place, different municipalities of the same canton may be subject to different voting ages for local politics. Unfortunately, we could not obtain detailed information about which municipalities opted into the lower voting age.[12] However, we received the information from the cantonal chancellery of Lucerne that most municipalities applied the opt-in rule, which would shift them to the treatment group.

Second, we provide a graphical illustration of cantonal votes regarding the lowering of the voting age in the canton of Zurich. From an empirical perspective, Zurich is an insightful canton, since voters in Zurich voted twice about this issue at the cantonal level before the second federal vote (see Figure 2.B.3). Again, trends do not differ systematically between the town meeting and parliament municipalities.

Third, we analyze how the vote share of the Christian Democratic People's Party (CVP) evolved in the two different institutional types. We interpret this vote share as a measure of conservatism present in a municipality. As shown in Figure 2.B.4, there were no systematic differences in its development in the direct-democratic and representative-democratic municipalities.

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[12]As a robustness check, we will therefore exclude these two cantons from our analysis.

2.6 Results and discussion

2.6.1 The ambiguous role of direct democracy

Table 2.6.1 provides the estimations of Model 2.1. From Specifications (1) to (4), we insert cantonal and year-fixed effects and successively our control variables. The base effect of direct democracy is time-invariant and can only be displayed in estimations with cantonal fixed effects. The results remain robust when estimated in a model using municipal fixed effects (see Table 2.C.1).

The base effect of *direct democracy* reflects the differences in the yes shares between municipalities holding town meetings compared to municipalities with parliaments, given that the voting age is not lowered from 20 to 18 on the local level (*local youth suffrage*=0). The coefficient for *direct democracy* is negative, statistically significant, and robust at -1.329 to -1.575 percentage points in Specifications (1) to (4). While this effect is small in absolute terms, it is fairly sizeable compared to the effects of other variables, such as the *population (log)* or the *pensioner* share, which is an established barrier impeding youth enfranchisement (see e.g. Birch et al., 2015; Svensson, 1979).[13]

As discussed in Section 2.2, if the present electorate can more effectively affect politics in a direct-democratic setting compared to a representative-democratic setting, it also loses more power when sharing voting rights with new groups. Indeed, Table 2.6.1 indicates a negative effect of direct democracy when no local youth suffrage is installed, which is when the price of direct democracy matters. Theoretically, the additional power loss induced by direct democracy at the local level is eliminated when the young are enfranchised at the local level. Our estimates in Table 2.6.1 align with this notion: With local youth suffrage installed, the negative effect of direct democracy disappears and even transforms into a positive effect. To some extent, this interpretation discharges direct democracy from the claim of systematically resulting in the discrimination of outsiders (Gamble, 1997; Hainmueller and Hangartner, 2019; Koukal and Eichenberger, 2017), as the "discriminatory behavior" we observe might be related to a greater power loss. However, this power loss mechanism does not yet explain the positive coefficient of direct democracy when local youth suffrage is implemented.

As presented in Table 2.6.1, when the youth population is enfranchised at the local level, direct democracy seems to exert the opposite effect. Given that the voting age is already lowered at the local level, the voters of municipalities with town meetings (*direct democracy = 1*) are, on average, 1.300 percentage points more willing to enfranchise younger citizens at the federal level compared to municipalities with local parliaments (*direct democracy = 0*).[14] For a better understanding of the interaction, the predictive margins are plotted in Figure 2.C.1. In the specifications with municipal fixed effects (see Table 2.C.1), the coefficient for the interaction term remains robust but is somewhat

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[13]The variable *population (log)* has a coefficient of -0.758 and a standard deviation of 1.37. The *pensioner share* exhibits a coefficient of -0.211 and a standard deviation of 7.51.

[14]This effect results from extracting the base effect of *direct democracy* (-1.575) from the interaction effect (2.875). Joint significance tests of the base and the interaction effects reveal that the equality of the coefficients can be rejected at the 5 percent level.

larger than in the estimations with cantonal fixed effects. Potential mechanisms behind this finding will be discussed in the subsection 2.6.1.[15]

Robustness of the results

We perform several robustness exercises to test the robustness of our results.[16] Most importantly, we start with different sample restrictions, such as excluding opt-in cantons, population outliers, and extreme proponent and opponent municipalities. As the cantons of Lucerne and Grison introduced an opt-in rule for lowering the voting age between 1979 and 1991, municipalities in these cantons could independently opt to lower the voting age at the municipal level. As we do not dispose of precise information about all municipalities that have used this possibility, we exclude these cantons from our data set. The results remain robust in terms of size and significance (see Table 2.C.2 in the appendix). As our measure of direct democracy at the local level is correlated with municipality size, we exclude population outliers from the sample. In doing so, we ensure that we include only municipalities for which both types of institutional design are realistic options. We change the municipal thresholds for the exclusion of outlier municipalities (1%, 5%, and 10% smallest and largest municipalities were excluded) and find no meaningful variations in terms of size and significance (see Table 2.C.3). Furthermore, we aim to rule out the possibility that our results are driven by "extreme" municipalities, hence the ones with very high or very low acceptance rates in the first federal vote. We exclude the outlier municipalities in terms of yes shares in the first vote (5% and 10% lowest and highest yes shares) from our analysis (see Table 2.C.4). Again, the results remain robust in terms of size and significance. Interestingly, Specifications (1) and (2) in Table 2.C.4 now indicate a statistically significant positive coefficient for *local youth suffrage*. Therefore, installing local youth suffrage is also positively associated with the willingness of the current electorate to enfranchise the youth at the federal level in representative-democratic municipalities. However, the coefficient is still larger for direct-democratic municipalities compared to representative-democratic municipalities.

In further robustness tests (Table 2.C.5), we cluster our standard errors at the cantonal level (Spec. 1 in Table 2.C.5) and perform the same estimations without cantonal and municipal fixed effects (Spec. 2) and thus make use of the variations across different cantons. Furthermore, we include the three pioneer cantons in the analysis to evaluate whether the exclusion of early movers drives our results (Spec. 3). We add a control for the local party share in national elections (Spec. 4). This ensures that our results are not driven by changes in the local party environment. Finally, we restrict the sample to the cantons for which we dispose of at least ten observations (each) of parliament and town meeting municipalities (Spec. 5). This reveals that our findings are not driven by

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[15]See also (Koukal and Eichenberger, 2017) for a discussion of the boost effect in female enfranchisement.

[16]As shown in Table 2.C.1, the results remain robust when estimating Model 2.1 with municipal effects rather than cantonal fixed effects.

Table 2.6.1: Direct democracy and other conditions for youth enfranchisement.

VARIABLES	(1) yes share	(2) yes share	(3) yes share	(4) yes share
Explanatory variables				
direct democracy	-1.329*	-1.479*	-1.561**	-1.575**
	(0.774)	(0.767)	(0.709)	(0.708)
local youth suffrage	0.575	0.616	0.565	0.454
	(0.668)	(0.671)	(0.662)	(0.672)
direct democracy * local youth suffrage	2.531***	2.528***	2.767***	2.875***
	(0.655)	(0.652)	(0.630)	(0.640)
Urbanity				
population (log)	-0.482**	-0.570**	-0.765***	-0.758***
	(0.223)	(0.229)	(0.218)	(0.216)
agriculture	-0.178***	-0.156***	-0.156***	-0.155***
	(0.019)	(0.021)	(0.022)	(0.022)
foreigner	0.061**	0.041	0.127***	0.124***
	(0.030)	(0.031)	(0.033)	(0.033)
Population background				
pensioner		-0.241***	-0.212***	-0.211***
		(0.058)	(0.057)	(0.057)
young (<20)		-0.107	-0.123*	-0.119*
		(0.070)	(0.068)	(0.068)
women		0.156*	0.158*	0.157*
		(0.085)	(0.086)	(0.086)
married		-0.055	-0.011	-0.004
		(0.058)	(0.058)	(0.059)
Culture				
Catholics			0.009	0.009
			(0.012)	(0.012)
german			0.075***	0.075***
			(0.010)	(0.010)
Economic situation				
unemployed				0.150
				(0.196)
Cantonal FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	3,840	3,840	3,840	3,840
R-squared	0.766	0.768	0.776	0.776

Note: Robust standard errors in parentheses are clustered at the municipal level. The variable direct democracy is a dummy variable, the population is in logs, and the remaining variables are in shares of the municipality's total population.
*** p < 0.01, ** p < 0.05, *** p < 0.1

a few cantons with exceptional institutions.[17] A summary of these robustness tests is presented in Table 2.C.5; notably, all coefficients of interest remain robust.

Direct democracy: Enhancing contact?

Our estimation results suggest that direct democracy only negatively affects the willingness to enfranchise the young at the national level when the young are not yet enfranchised at the local level. In contrast, it positively affects power-sharing at the national level when the young are already enfranchised at the local level. Several mechanisms might explain these observations. In the remainder of this chapter, we discuss the possibility of collecting experience in politics with the newly enfranchised group.

One possible explanation for the positive interaction term is the potential for more intensive and more frequent contact between different groups in direct democracy contexts compared to representative democracy settings. In town meetings, the electorate discusses political topics and can better assess the political tastes, behaviors, and intentions of other voters. Hence, direct-democratic institutions increase the possibilities of contact and interaction with other voters. Contact theory suggests that increased contact can mitigate prejudices (Allport, 1954; Yehuda, 1998); therefore, interactions with young voters in politics on the local level could lead to a higher willingness to grant suffrage at the national level. This interpretation is underlined by the findings of Larsen et al. (2016), who attribute a positive effect of mock elections to the willingness of the electorate to lower the voting age. Moreover, social interactions in local politics between the new and the old electorates may increase the in-group feeling. The previous literature has also suggested that in-groups evolve quite rapidly (Goette et al., 2012).

If such an experience mechanism explains our results, we would expect that the longer a municipality has experienced local youth suffrage, the stronger the positive effect should be. We therefore estimate Model 2.1 for two different sub-samples: for municipalities that have already gained long experience with local youth suffrage and for municipalities that have introduced local youth suffrage only shortly before the second federal vote.[18] The results are presented in Table 2.6.2. Indeed, the positive effect of the interaction term is larger in terms of size for the sample of the cantons with extensive experience (Spec. 1 & 2 in Table 2.6.2). Moreover, the effect is statistically significant only for the sample with the lengthy experience, which supports the idea that joint democratic experiences of the old and the new electorates can explain parts of our findings.

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[17]As not all cantons exhibit the same distribution of institutions between the municipalities (see Table 2.A.2), we also made sure that some cantons with skewed distributions do not drive our findings. We therefore performed a jackknife analysis, where we excluded one canton after the other from the sample. We performed these estimations with cantonal and municipal fixed effects. The results remained robust in both exercises and are available upon request.

[18]The cantons with long experience are BL, GE, GL, VD, NW, OW, and ZG, and the cantons with short experience are BS, BE, UR, SH, TI, and ZH. The cantons with extensive experience introduced the lower voting age between 1979 and 1983, and the cantons with minimal experience did so between 1988 and 1991 (see Table 2.A.1).

We perform robustness checks in which we introduce municipal instead of cantonal fixed effects and interact *experience* directly with *direct democracy* and *local youth suffrage*. The results of these additional tests are depicted in Table 2.C.6. The estimates of the triple interaction support the notion of the split sample analysis; the coefficient of the triple interaction is positive and significant at the 10 percent level and indicates that *direct democracy* and *local youth suffrage* exert a more positive effect on the electorate's willingness to enfranchise the youth when the municipality has had extensive experience with the youth population at the local level. However, when introducing municipal fixed effects, the results do not remain robust; in the split sample analysis (see Table 2.C.6), both the more [Spec. (1)] and less [Spec. (2)] experienced municipalities exhibit positive and significant interaction terms, again pointing to the power-loss mechanism's importance.

Table 2.6.2: Experience as an explanation for the positive interaction term.

VARIABLES	Long experience		Short experience	
	(1) yes share	(2) yes share	(3) yes share	(4) yes share
direct democracy	-1.262	-1.391*	-1.468	-1.933**
	(0.906)	(0.809)	(1.052)	(0.945)
local youth suffrage	1.168	0.765	0.765	1.049
	(0.742)	(0.757)	(1.085)	(1.036)
direct democracy * local youth suffrage	3.133***	3.793***	1.498	1.493
	(0.844)	(0.824)	(1.079)	(1.022)
urbanity	✓	✓	✓	✓
population background		✓		✓
economic situation		✓		✓
culture		✓		✓
cantonal FE	✓	✓	✓	✓
year FE	✓	✓	✓	✓
Observations	2,772	2,772	3,028	3,028
R-squared	0.751	0.764	0.741	0.755

Note: Robust standard errors in parentheses are clustered at the municipal level. Spec. (1) and (2) look at the in-between movers with a long duration of local youth suffrage and Spec. (3) and (4) at the ones that only introduced it shortly before the 2nd federal vote. The experienced cantons are BL, GE, GL, NW, OW, VD, and ZG.   *** $p < 0.01$, ** $p < 0.05$, *** $p < 0.1$

Altogether, these findings provide support for local direct democracy to strengthen the contact between the former and the new parts of the electorate and to enhance the willingness of the older citizens to enfranchise the young also at the federal level, which inflates the size of the positive interaction term presented in Table 2.6.1.

2.6.2 Further conditions for youth enfranchisement

Our estimates in Table 2.6.1 also provide insights about the effects of our control variables, which prove to be important drivers of, and barriers to, youth enfranchisement. This section briefly discusses how the willingness to enfranchise the youth population relates to different municipal characteristics.

Urbanity. A more rural environment has been found to pose a barrier that impedes efforts to decrease the voting age. Table 2.6.1 depicts the coefficients for different measures of urbanity. We find that voters from areas with a larger share of people working in the agricultural sector are statistically significantly more reluctant to lower the voting age, which aligns with the results from previous studies (Birch et al., 2015; Svensson, 1979). A one percentage point larger share of people working in the agricultural sector corresponds to a 0.155 lower yes share for enfranchising the young. Concerning the size of a municipality, we expect larger municipalities to be more in favor of lowering the voting age, as municipality size can be viewed as a proxy for urbanity.[19] However, we find that a municipality with a one percent larger population size is associated with a statistically significantly 0.00758 percentage point lower willingness to enfranchise the youth population. A potential explanation for this finding is the smaller probability of interaction between the old and the potential new electorates in larger municipalities (Allport, 1954), which might decrease the willingness to share voting rights with this group. Another proxy for urbanity is the share of foreign people in a municipality, which is positively associated with the willingness to enfranchise younger citizens (Table 2.6.1). A one percentage point larger foreigner share corresponds to a statistically significantly 0.124 percentage point higher willingness to lower the voting age.

Population background. Table 2.6.1 further reveals insights about the relevance of the population backgrounds in the enfranchisement of the young. First, the age structure matters. The share of pensioners in a municipality is negatively associated with the revealed preferences for lowering the voting age to 18. The stable estimates of Table 2.6.1 indicate that a one percentage point larger pensioner share is associated with a 0.211 percentage point lower willingness to enfranchise the young, which corroborates the results by Birch et al. (2015) and Svensson (1979). This effect is consistent with the interpretation that larger preference heterogeneity between the new and the old electorates poses a barrier to their enfranchisement (see discussions in Section 2.2). Moreover, Table 2.6.1 also reveals that a one percentage point larger share of young people in a municipality relates to a statistically significantly 0.119 percentage point lower willingness of the actual electorate to share the franchise with the young. A potential explanation is the cost for the actual electorate to enfranchise the young, which increases with a larger group to be enfranchised.[20]

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[19]From a power-loss perspective, population size has no neat and simple effect. With an increasing number of citizens, the decision weight of an individual citizen decreases. However, at the same time, the size of the budget and the range of policies to decide on increase. Therefore, deriving a clear hypothesis from a power-loss perspective regarding population size is not possible.

[20]See, for instance, Koukal et al. (2021) who find a negative relationship between the number of non-citizens and the willingness of the electorate to enfranchise non-citizens.

In contrast to the findings of Birch et al. (2015), we find that a larger share of women in a municipality is associated with a higher willingness to lower the age threshold to become enfranchised. Our finding could be attributed to women behaving more altruistically than men (Eswaran and Kotwal, 2004; Simmons and Emanuele, 2007) or to the smaller preference heterogeneity between women and young people. While gender and the willingness to lower the voting age seem related, we do not find statistically significant relationships for the prevalence of married couples.

Culture. Cultural factors may play a role in decisions about suffrage extensions. For instance, Catholicism has been found to affect female voting rights (Bertocchi, 2011; Koukal and Eichenberger, 2017). However, the coefficient for the share of Catholics in Table 2.6.1 is close to zero and statistically insignificant. Another proxy for culture is the dominant language among the population (French vs. German). Specifications (3) and (4) in Table 2.6.1 reveal that a one percentage point larger share of German speakers relates to a statistically significant 0.075 percentage points higher willingness to lower the voting age. Although it is relatively small, the effect is interesting, as the French-speaking part of Switzerland was a first mover in enfranchising women and non-citizens but not in lowering the voting age.

Economic situation. In the case of non-citizen residents, it has been found that greater competition in the labor market is positively associated with anti-immigrant attitudes (Scheve and Slaughter, 2001) and negatively associated with natives' willingness to enfranchise foreigners (Koukal et al., 2021). In contrast, the unemployment rate does not appear to play a role in the electorate's decision to lower the voting age (see Table 2.6.1). However, the unemployment rate during our observation period was low, at least compared to the present, with a mean of 1.06 percentage points (see Table 2.4.1).

2.7 Conclusion

After the enfranchisement of women, lowering the voting age to 18 was the second significant extension of suffrage in the last century. Today, lowering the voting age further to 16 years is on the political agenda in many countries. To better understand why some states or jurisdictions are faster or slower in extending suffrage to the young, we can investigate historical cases. The Swiss case is particularly interesting since the citizens have revealed their preferences in many federal and cantonal votes concerning lowering the voting age from 20 to 18. Analyzing referenda data is especially insightful, as anecdotal evidence suggests that there are systematic differences between what parliaments decide and what voters desire with respect to the enfranchisement of new groups. We add to the literature about the catalysts behind youth enfranchisement by investigating how institutional variations in the extent of direct democracy relate to the willingness of the former electorate to share formal political voting rights with younger voters in Switzerland. Moreover, we provide insights into the effects of different socio-economic determinants of the enfranchisement process, such as demography, urbanity, culture, and the economy.

By exploiting a new dataset of two federal votes in Switzerland related to lowering the voting age from 20 to 18 years in 1979 and 1991, our DiD estimates provide evidence that the extent of direct democracy affects the decision to lower the voting age. We discern two effects of direct democracy on the acceptance of youth enfranchisement. First, when the voting age has not yet been lowered at the local level, local direct democracy has a negative effect on the current electorate's willingness to enfranchise younger people at the federal level compared to local representative democracy. This result is consistent with the former electorate fearing a loss of power (a cost or price), which increases with the extent of direct democracy. Second, when local youth suffrage is introduced, direct democracy may exert a positive effect on the willingness to grant voting rights to the youth at the federal level. Two mechanisms could explain these findings. First, the additional power loss due to direct democracy at the local level no longer matters as soon as youth enfranchisement is installed at the local level. As similar effects have been found for the enfranchisement of women and non-citizens, this chapter suggests a more general nature of this mechanism. Second, the contact between the old and new electorates may be more intensive within a more direct-democratic setting and may thus increase the willingness of the current electorate to enfranchise the youth. This interpretation receives empirical support since the positive effect of direct democracy is especially visible for municipalities with extensive experience with local youth suffrage. Additionally, we provide evidence that various socio-demographic variables are important for explaining the introduction of lower voting-age thresholds. In line with results from previous literature, we find the share of agriculture and the share of pensioners hinder the enfranchisement of the young, suggesting that the degree of preference heterogeneity between the old and the new electorates impacts the willingness to lower the voting age.

Overall, it is not astonishing that extending voting rights in direct democracies is characterized by seemingly contradictory observations. Although our results imply that direct democracy may initially hinder the lowering of the voting age, it does not appear to pose a general threat to the political integration of younger people. The comparatively late enfranchisement of the young is rather a consequence of the individual resistance to relinquish influence in the political process. Hence, our results suggest that there is a trade-off between equipping the current electorate with strong participation rights and extending voting rights to new groups. This potential conflict between the desirability of decision-making close to the people and the agility to reform institutions might fruitfully be tested more widely in the future.

Appendix

Chapter 2 Appendix

2.A   Supplemental information on institutions and data

Table 2.A.1: Introduction of voting age 18 in the cantons.

Canton	Year	Note
Schwyz	1848	
Jura	1979	
Neuchâtel	1979	
Basel-Country	1980	
Geneva	1980	
Glarus	1980	
Vaud	1980	
Zug	1981	Lowered from 19 to 18
Nidwalden	1982	
Obwalden	1983	
Basel-City	1988	
Berne	1989	
Uri	1989	
Schaffhausen	1990	
Ticino	1990	
Zurich	1991	Vote 1990, Introduction 1991
Argovia	1991	
Appenzell Outer Rhodes	1991	
Fribourg	1991	
Grison	1991	Opt-in Rule 1989
Lucerne	1991	Opt-in Rule 1986
Solothurn	1991	
Thurgovia	1991	
Valais	1991	
Appenzell Inner Rhodes	1992	
St. Gallen	1992	

Table 2.A.2: Municipal legislative institutions by cantons.

Canton	Parliament	Town Meeting
Appenzell Inner Rhodes	1	3
Appenzell Outer Rhodes	12	5
Argovia	11	183
Basel-City	2	1
Basel-Country	5	70
Berne	17	307
Fribourg	12	161
Geneva	28	0
Glarus	0	27
Grison	7	150
Jura	2	60
Lucerne	6	80
Neuchâtel	52	0
Nidwalden	0	11
Obwalden	0	7
Schaffhausen	2	28
Schwyz	0	27
Solothurn	1	103
St. Gallen	2	74
Thurgovia	4	45
Ticino	-	-
Uri	0	17
Valais	7	113
Vaud	78	171
Zug	1	8
Zurich	11	149

Note: The columns parliament and town meeting report the number of municipalities in the cantons making use of this type of municipal legislative in our full sample. As data for the canton of Ticino was not available, this canton had to be dropped.
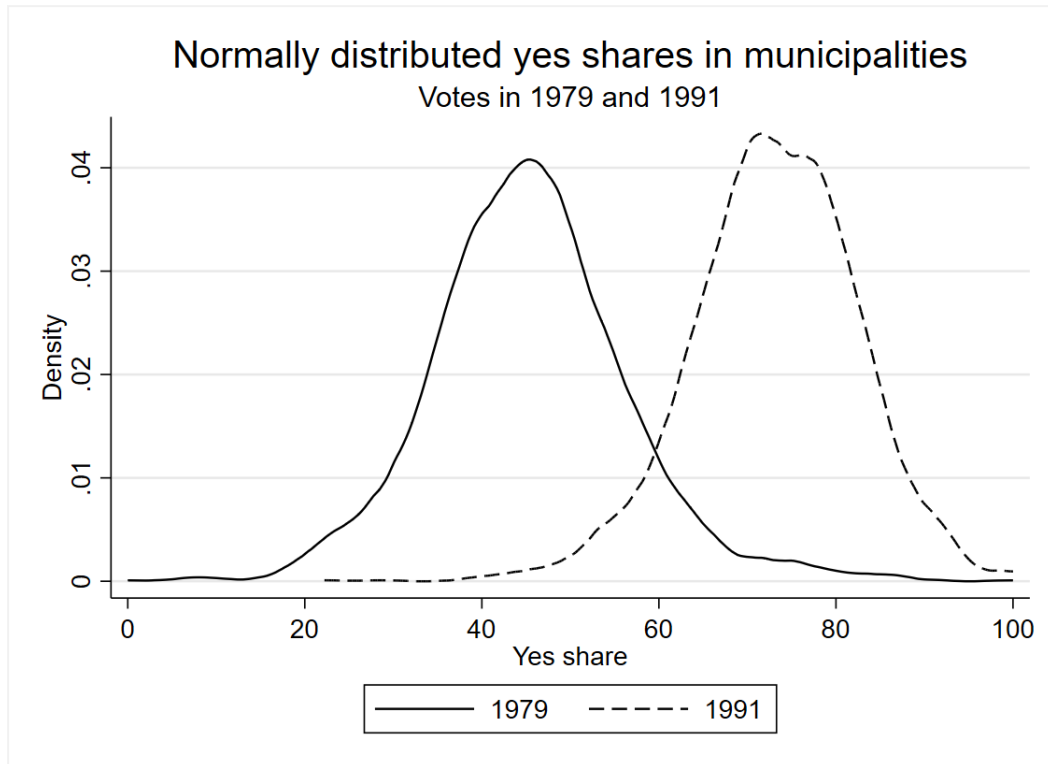
Figure 2.A.1: Acceptance of voting age 18 - Kernel density plots (1979 & 1991).

2.B   Graphical illustrations for parallel trend assumption

Figure 2.B.1: Acceptance of voting age 18 in untreated cantons (1979 & 1991).
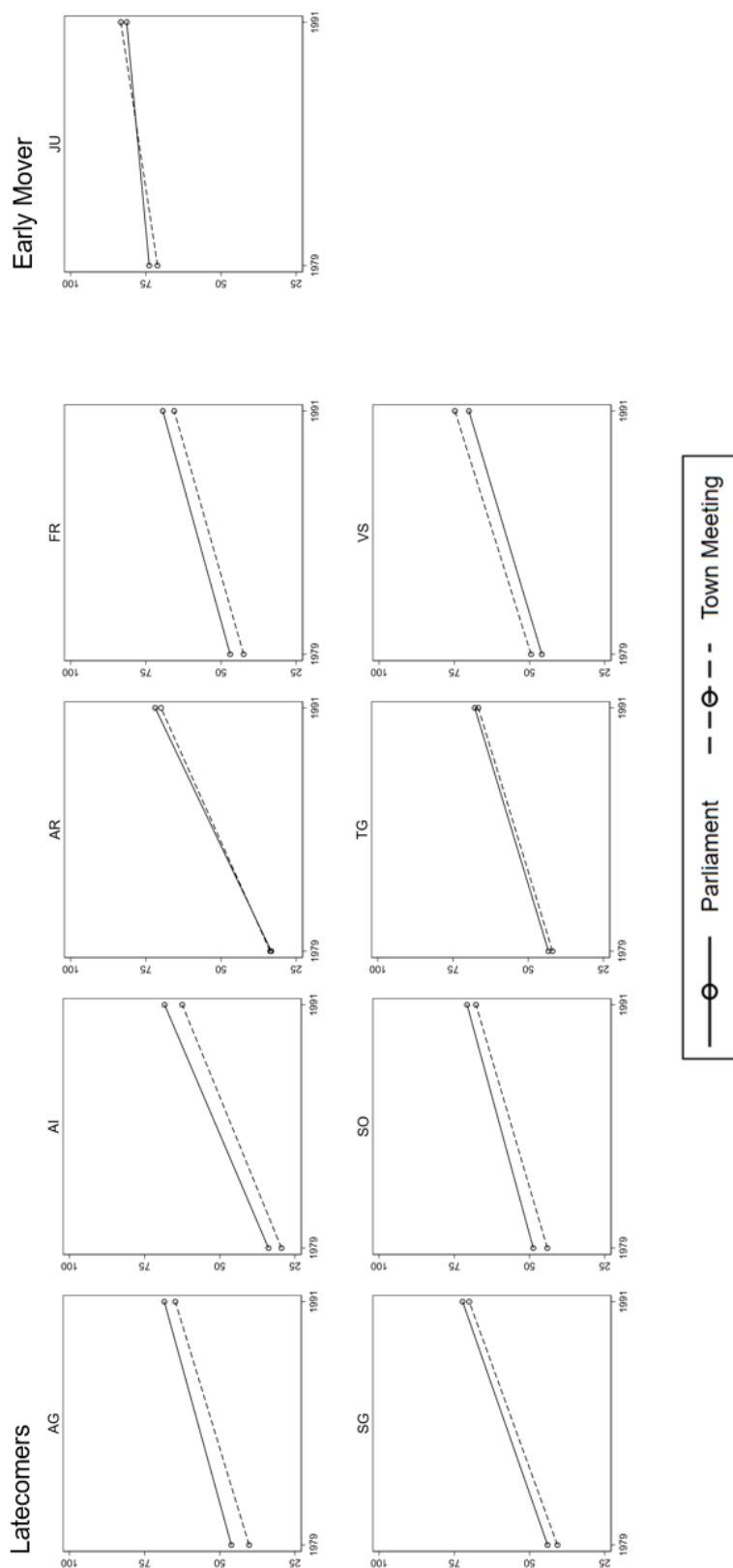
Note: Latecomers are the cantons that introduced voting age 18 on the local level only after the second federal vote (1991) and early mover before the first federal vote (1979). Nidwald, Neuchâtel, and Schwyz are excluded as they have no variation in the municipal legislative institutions.

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Figure 2.B.2: Acceptance of voting age 18 in treated cantons (1979 & 1991).
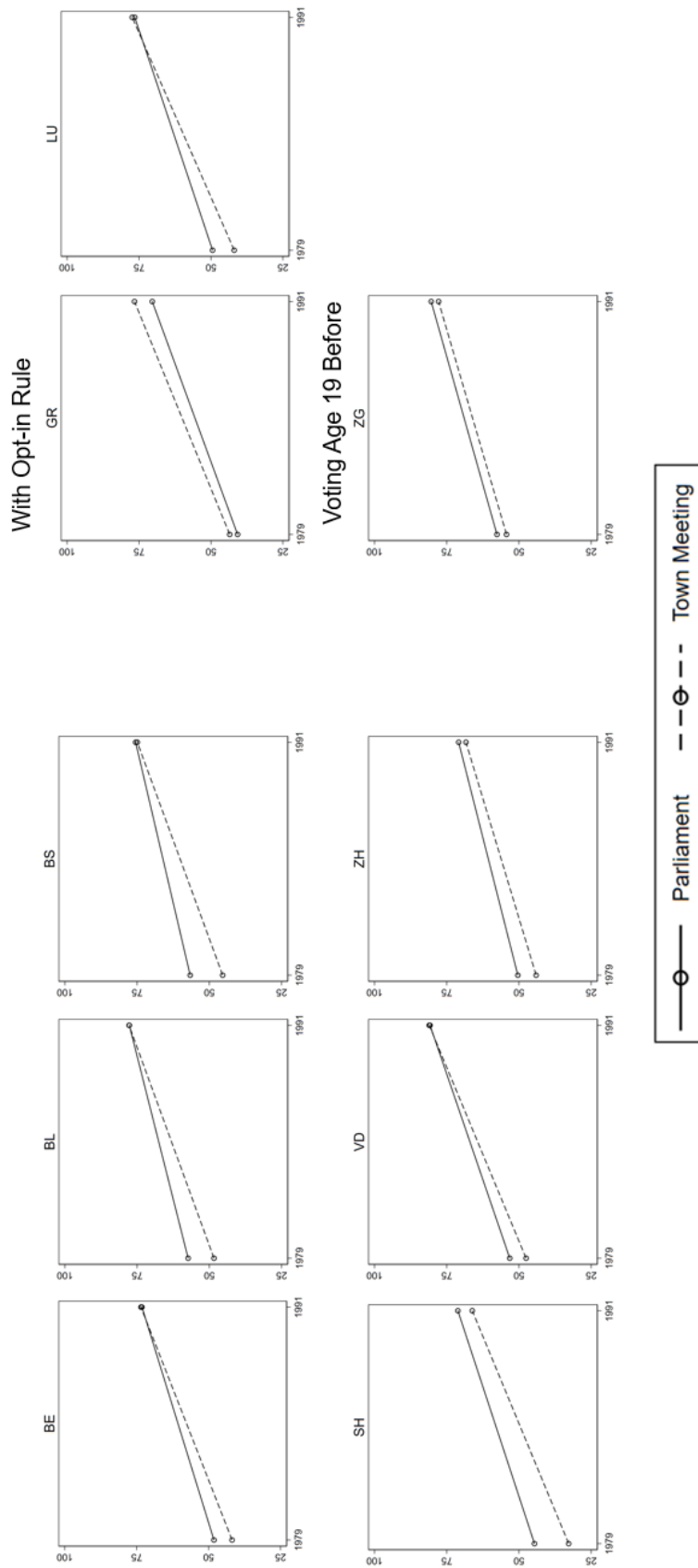
Note: In-between movers are the cantons that introduced voting age 18 on the local level between the first (1979) and the second (1991) federal vote. Grison and Lucerne introduced opt-in rules, where municipalities could lower the voting age on the local level. Geneva, Glarus, Obwalden, and Uri are excluded since they have no variation in their municipal legislative institutions.

Figure 2.B.3: Acceptance of voting age 18 in cantonal votes in Zurich (1974 & 1990).
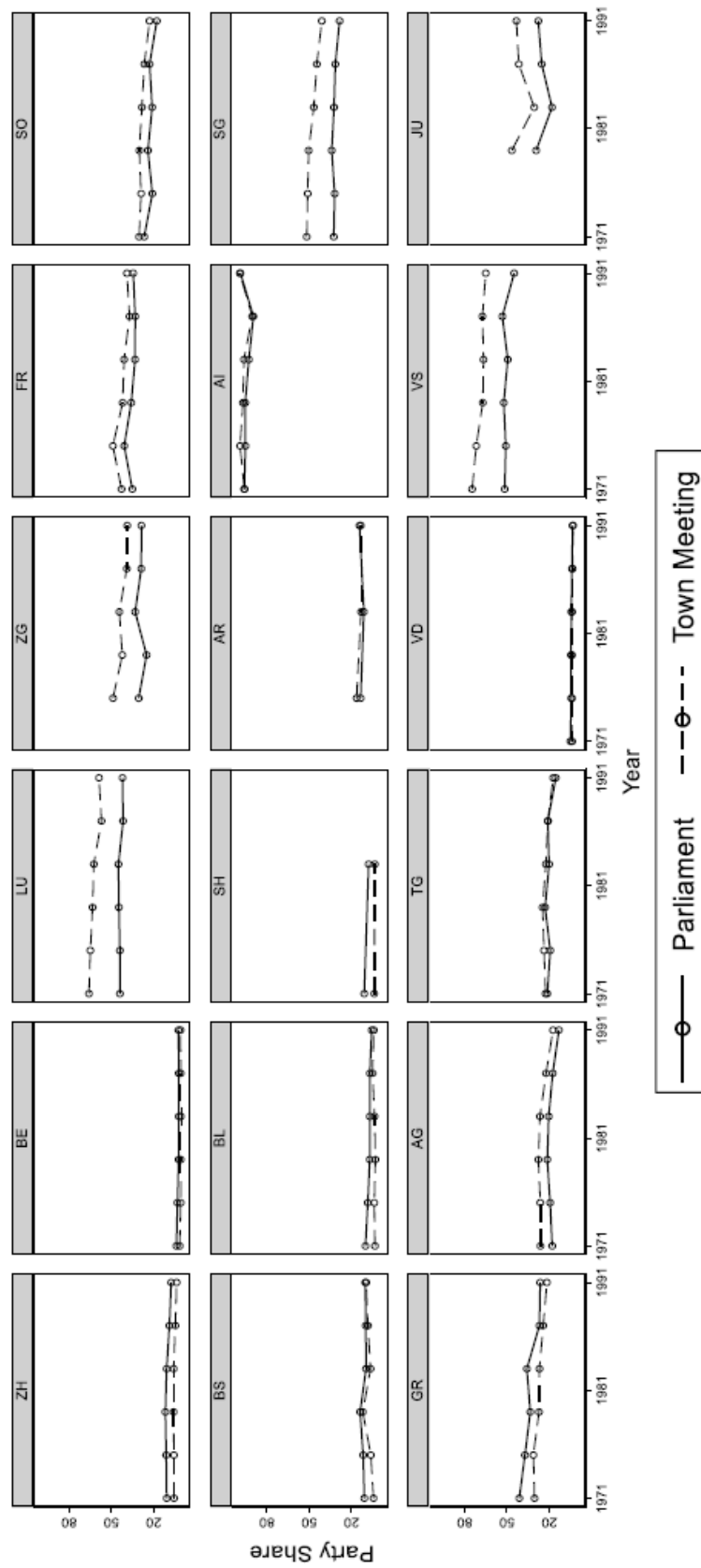
Figure 2.B.4: Vote share of the Christian Democratic Party (CVP) in national elections (1971-1991).

2.C   Robustness checks and further results
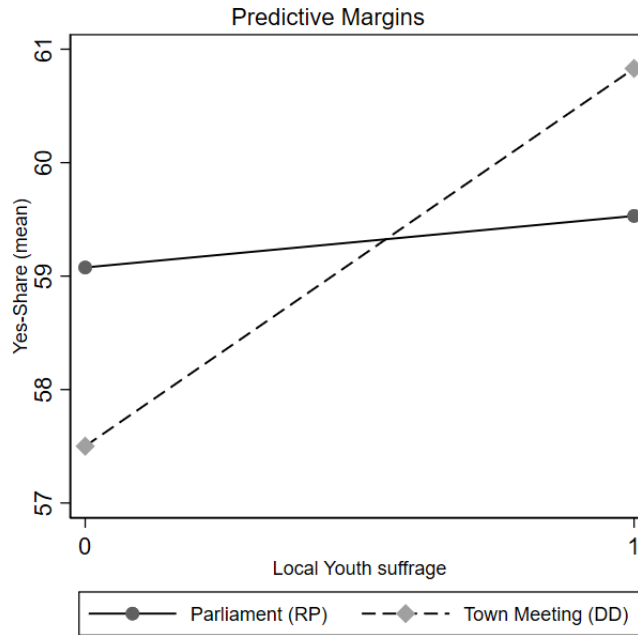
Figure 2.C.1: Interaction of direct democracy and local youth suffrage - Marginsplot.

Table 2.C.1: Base estimation with municipal fixed effects.

VARIABLES	(1) yes share	(2) yes share	(3) yes share	(4) yes share
direct democracy	-	-	-	-
local youth suffrage	0.124	-0.588	-0.576	-0.685
	(0.631)	(0.684)	(0.740)	(0.772)
direct democracy * local youth suffrage	3.146***	3.445***	3.401***	3.487***
	(0.638)	(0.647)	(0.648)	(0.655)
urbanity	✓	✓	✓	✓
population background		✓	✓	✓
culture			✓	✓
economic situation				✓
Municipal FE	✓	✓	✓	✓
Year FE	✓	✓	✓	✓
Observations	3,840	3,840	3,840	3,840
R-squared	0.888	0.890	0.891	0.891
Number of Municipalities	1,920	1,920	1,920	1,920

Note: Robust standard errors in parentheses are clustered at the municipal level. The variables *direct democracy* and *local youth suffrage* are dummy variables. The full set of control variables includes: pop(log), agriculture, youth, foreigner, women, pensioner, Catholics, german speaking, unemployment.
*** $p < 0.01$, ** $p < 0.05$, *** $p < 0.1$

Table 2.C.2: Robustness when excluding GR and LU from the sample.

VARIABLES	(1) yes share	(2) yes share	(3) yes share	(4) yes share	(5) yes share	(6) yes share
direct democracy	1.336**	1.296*	1.611**	-1.566**	-1.625**	-1.601**
	(0.667)	(0.727)	(0.749)	(0.682)	(0.637)	(0.639)
local youth suffrage				2.083***	1.991***	2.140***
				(0.639)	(0.628)	(0.646)
direct democracy * local youth suffrage	3.696***	3.660***	3.415***	3.198***	3.452***	3.300***
	(0.644)	(0.656)	(0.670)	(0.620)	(0.599)	(0.610)
urbanity	✓	✓	✓	✓	✓	✓
population background	✓	✓	✓	✓	✓	✓
culture		✓	✓		✓	✓
economic situation			✓			✓
Cantonal FE	✓	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓			
Year FE	✓	✓	✓	✓	✓	✓
Observations	3,354	3,354	3,354	3,354	3,354	3,354
R-squared	0.905	0.905	0.905	0.787	0.795	0.795
Number of Municipalities	1,677	1,677	1,677			

Note: Robust standard errors in parentheses are clustered at the municipal level. The variables *direct democracy* and *local youth suffrage* are dummy variables. The full set of control variables includes: pop(log), agriculture, youth, foreigner, women, pensioner, Catholics, german speaking, unemployment.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

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Table 2.C.3: Robustness when excluding population outliers from the sample.

Dependent variable: yes share	(1) Excl. 1 % population outliers	(2) Excl. 5 % population outliers	(3) Excl. 10 % population outliers	(4) Excl. 1 % population outliers	(5) Excl. 5 % population outliers	(6) Excl. 10 % population outliers
VARIABLES						
direct democracy	-1.416**	-0.756	-0.132			
	(0.693)	(0.752)	(0.850)			
local youth suffrage	0.933	1.561**	2.199***	0.285	1.458*	1.836**
	(0.667)	(0.743)	(0.778)	(0.773)	(0.821)	(0.842)
direct democracy * local youth suffrage	2.335***	1.865**	1.524**	2.696***	1.527**	1.518***
	(0.647)	(0.730)	(0.775)	(0.670)	(0.733)	(0.757)
urbanity	✓	✓	✓	✓	✓	✓
population background	✓	✓	✓	✓	✓	✓
culture	✓	✓	✓	✓	✓	✓
economic situation	✓	✓	✓	✓	✓	✓
Cantonal FE	✓	✓	✓			
Municipal FE				✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	3,763	3,454	3,074	3,763	3,454	3,074
R-squared	0.783	0.807	0.818	0.900	0.913	0.919
Number of municipalities	1,889	1,750	1,573	1,889	1,750	1,573

Note: Robust standard errors in parentheses are clustered at the municipal level. The variables *direct democracy* and *local youth suffrage* are dummy variables. The full set of control variables includes: pop(log), agriculture, youth, foreigner, women, pensioner, Catholics, german speaking, unemployment.
*** p < 0.01, ** p < 0.05, *** p < 0.1

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Table 2.C.4: Robustness when excluding "extreme" municipalities from the sample.

Dependent variable: yes share	(1) Excl. 5% yes share outliers	(2) Excl. 10% yes share outliers	(3) Excl. 5% yes share outliers	(4) Excl. 10% yes share outliers
VARIABLES				
direct democracy	-1.522***	-1.108**		
	(0.564)	(0.508)		
local youth suffrage	1.646***	2.648***	0.654	1.019
	(0.623)	(0.614)	(0.711)	(0.723)
direct democracy * local youth suffrage	2.173***	1.418**	2.512***	1.820***
	(0.603)	(0.602)	(0.628)	(0.647)
urbanity	✓	✓	✓	✓
population background	✓	✓	✓	✓
culture	✓	✓	✓	✓
economic situation	✓	✓	✓	✓
Cantonal FE	✓	✓		
Municipal FE			✓	✓
Year FE	✓	✓	✓	✓
Observations	3,602	3,239	3,602	3,239
R-squared	0.806	0.823	0.916	0.926
Number of municipalities			1,827	1,646

Note: Robust standard errors in parantheses are clustered at the municipality level. In Spec. (1) and (3) the municipalities with the lowest and largest 5% yes shares in the 1979 vote are excluded (28.08 < yes share < 63.73). In Spec. (2) and (4) the 10% lowest and largest yes shares of the 1979 vote are excluded (32.61 < yes share < 58.51).
*** p < 0.01, ** p < 0.05, *** p < 0.1

Table 2.C.5: General robustness checks.

Dependent variable: yes share	(1) SE clustered at cantonal level	(2) No fixed effects	(3) Incl. First Mover Cantons	(4) Control Party Share (CVP)	(5) Large institutional variation
VARIABLES					
direct democracy	-1.575**	-3.601***	-1.426**	-1.876**	-1.344*
	(0.667)	(0.753)	(0.694)	(0.750)	(0.726)
local youth suffrage	0.454	3.490***	1.534**	0.672	3.461***
	(1.490)	(0.708)	(0.657)	(0.690)	(0.824)
direct democracy * local youth suffrage	2.875***	1.628**	2.923***	2.700***	3.468***
	(0.916)	(0.745)	(0.635)	(0.666)	(0.707)
urbanity	✓	✓	✓	✓	✓
population background	✓	✓	✓	✓	✓
culture	✓	✓	✓	✓	✓
economic situation	✓	✓	✓	✓	✓
Cantonal FE	✓	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓
Observations	3,840	3,840	4,122	3,448	2,200
R-squared	0.776	0.723	0.775	0.777	0.787

Note: Robust standard errors in parentheses are in Spec. (1) clustered at the cantonal level and in (3)-(5) at the municipal level. In Spec. (2), neither cantonal nor municipal FE are included. Spec. (3) includes the first mover cantons (SZ, NE, JU) in the sample. Spec. (4) includes a control for the local party share (CVP), and Spec. (5) restricts the sample to the cantons with at least ten parliaments & ten town meetings. The same robustness checks are performed with municipal fixed effects. Results remain robust and are available upon request.

*** $p < 0.01$, ** $p < 0.05$, *** $p < 0.1$

Table 2.C.6: Robustness checks for the role of experience.

Dependent variable: yes share VARIABLES	(1) Long experience	(2) Short experience	(3) Triple int. cantonal FE	(4) Triple int. cantonal FE	(5) Triple int. municipal FE	(6) Triple int. municipal FE
direct democracy			-1.236 (0.997)	-1.650* (0.902)		
local youth suffrage	-0.288 (0.889)	-3.012*** (0.993)	0.618 (1.072)	0.884 (1.025)	-2.181** (0.925)	-2.949*** (0.977)
direct democracy * local youth suffrage	4.251*** (0.900)	5.303*** (0.961)	1.600 (1.070)	1.565 (1.010)	4.702*** (0.929)	5.131*** (0.945)
experience (=omitted)			-	-		
direct democracy * experience			-1.234 (1.239)	-0.318 (1.199)		
local youth suffrage * experience			-0.135 (1.183)	-0.469 (1.156)	2.938*** (1.042)	3.018*** (1.067)
direct democracy * local youth suffrage * experience			2.554* (1.384)	2.839** (1.353)	-0.926 (1.258)	-1.233 (1.279)
urbanity	✓	✓	✓	✓	✓	✓
population background	✓	✓	✓	✓	✓	✓
culture	✓	✓	✓	✓	✓	✓
economic situation	✓	✓	✓	✓	✓	✓
Cantonal FE			✓	✓		
Municipal FE	✓	✓			✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	2,772	3,028	3,840	3,840	3,840	3,840
R-squared	0.878	0.887	0.766	0.776	0.889	0.891
	1,386	1,514			1,920	1,920

Note: Robust standard errors in parentheses are clustered at the municipal level. The variables direct democracy, local youth suffrage, and experience are dummy variables. The full set of control variables includes: pop (log), agriculture, youth, foreigner, women, pensioner, unemployment, Catholics, and german speaking. *** p < 0.01, ** p < 0.05, *** p < 0.1

Chapter 3

Voting on minority rights – How veiling ban ballots affect attitudes toward Islam*

3.1 Introduction

What happens to society when individuals are confronted with political events, such as elections or referendum votes? In political economy, for a long time, it was standard to analyze how individual preferences aggregate in a democratic process. However, many economists have started to relax the assumption of stable, exogenous preferences because the environment itself may systematically affect individuals' preferences (e.g., Becker and Mulligan, 1997; Bernheim et al., 2021; Palacios-Huerta and Santos, 2004). Therefore, events in the political sphere can also lead to rapid changes in people's values and beliefs (e.g., Bursztyn et al., 2020a; Funke et al., 2016; Giuliano and Spilimbergo, 2014). On the one hand, elections or referendums may update the perceived social norms (see, e.g. Bursztyn et al., 2020a; Gerling and Kellermann, 2022) and, therefore, social restrictions. On the other hand, the democratic discourse increases the individuals' information about their own preferences or attitudes (e.g., Eichenberger and Serna, 1996; Habermas, 1979; Hirschman, 1989). This urges a deeper comprehension of how democratic decision-making impacts society.

In this chapter, I analyze whether direct-democratic votes on face-veiling bans affect the reported individual attitudes toward Islam. I use the Swiss setting, where two cantons (Ticino and St. Gallen) have voted on—and accepted—face-veiling bans on the cantonal level. I match these events with individual survey data from the Swiss Household Panel (SHP), where the respondents indicate their attitudes toward Islam. This allows applying a difference-in-differences strategy (DiD) with multiple treatment

points in a panel and repeated cross-sections. The setting offers great opportunities to analyze how democratic decision-making affects individual attitudes and to dig deeper into potential channels.

Individuals living in cantons with face-veiling ban ballots altered their reported attitudes toward Islam. Estimates from the DiD strategy with multiple treatment periods (Callaway and Sant'Anna, 2021) provide evidence that, after the face-veiling ban ballots, people indicate, on average, 0.328 points more negative attitudes toward Islam (on a scale from 0 to 10). This result is in line with the findings of a positive effect of the election of populists on xenophobic views, which has, for instance, been explained by a change in social norms (e.g., Bursztyn et al., 2020a). Moreover, although less stable, I find that the polarization of the answers is affected by the votes on veiling bans. Individuals from the treated cantons indicate, on average, around 0.201 points more polarized attitudes toward Islam (on a scale from 0 to 5) after the votes. This result further stresses the importance of understanding how societies coevolve with political decisions.

In addition, my findings highlight the importance of the *social restrictions* and *information* mechanisms under democratic decision-making. The negative effect of face-veiling ban ballots on attitudes toward Islam seems especially prevalent in areas with high acceptance, indicating a highly restrictive social norm toward Islam. Moreover, the effect is stronger for self-indicated left-wing voters. For them, the vote outcome might pose a more substantial update to their perceived social norm. Furthermore, it appears that those who previously had little opinion on the issue drive the effects. This is in line with the idea that the democratic discourse increases the individuals' awareness of their own preferences (e.g. Eichenberger and Serna, 1996; Habermas, 1979).

So far, the empirical literature on endogenous preferences in democracies has primarily focused on how elections affect social norms, individual behavior, or vote intentions (e.g., Bursztyn et al., 2020a; Gerling and Kellermann, 2022; Giani and Méon, 2021; Hagemeister, 2022; Romarri, 2020). In contrast, the literature on the effects of direct-democratic votes on individual attitudes and social norms is relatively scarce. The focus of the empirical literature has primarily been on independence votes: Dinas et al. (2024) show that public displays of nationalism increased after the referendum on Catalan independence, and the authors attribute this change to a shift in social norms. For Scotland, Fieldhouse and Prosser (2018) find a decrease in support for the labor party in the elections following the independence vote of 2014, and the Brexit referendum is shown to induce opinion-based affective polarization (Hobolt et al., 2021). In their analysis of the referendum on legalizing abolition in Ireland, Jung and Tavits (2021) find an effect on the perceived social norm but not on individual attitudes.

By analyzing the impact of cantonal veiling ban ballots on individual attitudes toward Islam, I offer three significant contributions: First, because of extensive federalism and decentralization in Switzerland, important referendum votes do not only take place nationally but also on the subnational level. Therefore, in contrast to the previous literature (Fieldhouse and Prosser, 2018; Hobolt et al., 2021; Jung and Tavits, 2021), the setting naturally provides a control group in the empirical analysis. Moreover, extensive Swiss federalism allows a closer focus on an individual's environment. Second, previ-

ous studies on the effects of referendum votes on individual attitudes share the focus on states that are not frequently facing direct-democratic votes (i.e., Ireland, Great Britain, and Spain). Thus, a referendum vote in these contexts constitutes a significant "shock" that likely impacts society. Analyzing referendum votes in the context of Switzerland provides insights into its effects in an established direct-democratic system, and, thus, in equilibrium. Third, although there is extensive literature focusing on the effect of direct democracy on minority rights (see, e.g., Gamble, 1997; Hainmueller and Hangartner, 2013), indirect effects on the position of minorities in society through direct-democratic decision-making have, to the best of my knowledge, not been analyzed in detail. Studying how individual attitudes toward a religious minority are affected by direct-democratic votes on their civil rights holds significant value as international mobility increases religious diversity.

Therefore, this chapter adds to the literature on the effects of political institutions on society (see Channel D in Figure 1). The chapter is organized as follows: In Section 3.2, I begin by reviewing the related literature and providing a short overview of the institutional background in Section 3.3. In Section 3.4, I then provide a conceptual background on how votes on veiling bans theoretically affect reported individual attitudes toward Islam. Section 3.5 presents the data used in this chapter, and the empirical strategy is discussed in Section 3.6. Section 3.7 provides a discussion of the results, and Section 3.8 closes with a summary and outlook.

3.2 Literature

It is often assumed that preferences with deep roots evolve slowly over time (Alesina and Giuliano, 2015; Alesina et al., 2013; Voigtländer and Voth, 2012). Therefore, for a long time, it was standard to analyze how different preferences aggregate in a democratic process. However, scholars have started to endogenize preferences (e.g., Becker and Mulligan, 1997; Bernheim et al., 2021; Palacios-Huerta and Santos, 2004), as some events can lead to rapid changes in people's beliefs and values (e.g., Bursztyn et al., 2020a; Funke et al., 2016; Giuliano and Spilimbergo, 2014). Therefore, also democratic decision-making can lead to swift changes.

The literature has primarily focused on the impact of elections on society. The rise of populist parties in democracies has sparked scholarly interest in their effect on social norms and individual behavior. For instance, Bursztyn et al. (2020a) provide experimental evidence showing that a rise in Donald Trump's popularity increased people's willingness to express xenophobic views. A channel over which democratic outcomes may affect people's attitudes is by providing information—and correcting misperceptions—of what fellow human beings believe (Bursztyn et al., 2020a,b).[1] Gerling and Kellermann (2022) find that an information shock by the unexpected elections of a populist party in Germany increases the party's reported support through less reputational concerns.

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[1]For instance, in Saudi Arabia, men underestimate support by male fellows for women working outside of their homes; by correcting these beliefs, men became more likely to support their wives in applying and searching for jobs (Bursztyn et al., 2020b).

For Sweden, Carlsson et al. (2021) show that increased political representation of the far-right party decreases negative attitudes toward immigration, supporting a backlash effect. Furthermore, Giani and Méon (2021) provide evidence by analyzing Trump's election, showing that the effect of elections on opinions must not be restricted to the respective country but that social norms can diffuse globally. Moreover, the electoral success of far-right parties has been shown to increase the probability of hate crimes against immigrants (Romarri, 2020), the within-job-spell injury of the foreign workforce (D'Ambrosio et al., 2021), and the probability of far-right protest (Hagemeister, 2022). In addition, Doerr et al. (2021) find that diversity in local Austrian football teams decreases when populists are in power.

The literature on the effects of direct-democratic votes on social norms or individual attitudes is—to the best of my knowledge—relatively scarce. It has mainly been studied in the context of independence votes: Dinas et al. (2024) show that public displays of nationalism increased after the referendum on Catalan independence, attributing this change to a shift in social norms. For Scotland, Fieldhouse and Prosser (2018) find a decrease in support for the labor party in the elections following the independence vote of 2014, and the Brexit referendum is shown to induce opinion-based affective polarization (Hobolt et al., 2021). In their analysis of the referendum on legalizing abolition in Ireland, Jung and Tavits (2021) find an effect on the perceived social norm but not on individual attitudes. It is no surprise that studies have mainly focused on referendums at the national level because that is where the most significant referendums occur in most democracies. However, by examining Switzerland, I can concentrate on highly debated referendum votes at the cantonal levels, allowing for the use of a control group within the same country.

Moreover, I contribute to a further understanding of the effects of direct-democratic votes on minority groups. Although there is a significant strand of the literature studying whether the civil rights of minorities are restricted under direct democracy (see, e.g., Frey and Goette, 1998; Gerber et al., 2008; Hainmueller and Hiscox, 2007), I am not aware of studies that analyze how minorities who are subject to a vote are affected beyond the instrumental outcomes. Thematically, what is most closely linked to my research is a study by Helbling and Traunmüller (2016), where the authors argue that "[...] governments play a considerable role in shaping citizens' attitudes toward Muslim immigrants though the way they regulate religion" (Helbling and Traunmüller, 2016, p. 391). Their theory focuses on political institutions that regulate religion, suggesting that these institutional contexts determine the extent to which Muslims' religious practices and demands for religious rights are viewed as a threat and the level of resentment toward Muslim immigrants. Thus, the authors also add to the literature on cultural threat (e.g., Hainmueller and Hiscox, 2007; Kinder and Kam, 2009). Additionally, Helbling and Traunmüller (2016) present empirical evidence at the subnational level in Switzerland, where they show that, in cantons with strong religious traditions in politics, society, and culture, citizens tend to perceive Muslim immigrants as more of a threat. However, the authors must rely on cross-sectional variance, while I can analyze changes over time.

These findings align with the expressive power of laws, which posits that laws, besides having a deterrent effect, also influence values and behavior (Chen and Yeh, 2014; Funk, 2007; Sunstein, 1996). The expressive power of laws has often been studied empirically in the context of sexual minorities. Studies have shown that legalizing same-sex marriage can increase same-sex couples' joint probabilities of being employed (Sansone, 2019). Moreover, by banning employment discrimination for LGBT individuals in certain US states, unfavorable attitudes toward the LGBT community have decreased (Deal, 2022). Aksoy et al. (2020) discover that recognizing same-sex relationships can significantly improve attitudes toward sexual minorities.[2] Whether similar effects are at play in the case of religious minorities after a direct-democratic decision-making process on their civil rights remains an open question.

3.3   Institutional background

Switzerland is famous for its degree of direct democracy and federalism. The Swiss confederation comprises 26 cantons and approximately 2200 municipalities with high autonomy compared with other countries. The electorate frequently votes at all government levels on various issues, which usually take place as secret ballots.[3] Every eligible voter receives the voting material sent home some weeks before voting day. Voters can choose how they want to cast their vote; they can go to the polls on voting day or vote by mail before the vote.

Popular votes either result from optional or mandatory referendums or initiatives directly initiated by (a part of) the electorate. Some examples of controversial Swiss ballots that have gained international media attention over the last 20 years have been votes on restrictions to immigration laws, environmental policies, or the civil rights of minorities. Regarding the latter, the Swiss electorate voted on issues such as same-sex marriage, naturalization laws for second- or third-generation non-citizens, or a ban on building minarets or wearing a burqa.

Because of the significant level of federalism and decentralization, it is not uncommon for new policies to be implemented in certain cantons before being adopted at the national level. Famous examples were the introduction of female suffrage, the lowering

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[2]A growing strand of the theoretical literature tries to model these interactions between laws and social norms. In their research, Benabou and Tirole (2011) discuss a theoretical model that explains how laws interact with norms and values. Acemoglu and Jackson (2017) study the relationship between social norms and law enforcement and demonstrate that laws that contrast starkly with the prevailing social norms may have unintended consequences. Blasco et al. (2022) provide a dyadic model in which individuals learn about the social norm conveyed by the law through social ties, thus analyzing an indirect effect of the change in the law. Furthermore, experimental evidence supports the view that laws have often—but not always— an influence on norms (Lane et al., 2023). Moreover, Apffelstaedt et al. (2022) provide experimental evidence that elected rules can affect what is perceived as socially appropriate behavior.

[3]For one canton (Glarus) and most municipalities, direct-democratic decisions are not made through secret ballots but in town meetings. All eligible voters are invited to these town meetings one to four times a year to discuss and decide on communal or cantonal topics. Unfortunately, the canton of Glarus belongs to the smallest cantons population-wise and does not provide enough observations for analysis.

of the voting age from 20 to 18, and the introduction of a debt brake. Therefore, because of the high cantonal autonomy, controversial ballots that receive much attention occur not only on the federal level but also regularly on the cantonal level. Regarding the controversial face-veiling ban, the cantons of St. Gallen and Ticino accepted it at the cantonal level before the national vote occurred. Table 3.3.1 shows the chronology of votes on Islam-related topics in Switzerland.

Table 3.3.1: Chronology of votes tackling Muslim rights – Switzerland, 2009–2021.

Date	Topic	Level	Secret ballot	Acceptance
November 29, 2009	Minaret ban	Federal	Yes	57.50%
September 22, 2013	Veiling ban	Cantonal - Ticino	Yes	66.20%
May 07, 2017	Veiling ban	Cantonal - Glarus	No	≈ 33.33 %
September 23, 2018	Veiling ban	Cantonal - St. Gallen	Yes	66.65%
March 07, 2021	Veiling ban	Federal	Yes	51.20%

Note: The canton of Glarus uses a town meeting instead of secret ballots for direct-democratic decision-making at the cantonal level. Therefore, the acceptance rate is only approximately given.

As shown in Table 3.3.1, the Swiss electorate voted and accepted two federal votes on the civil rights of Muslims. In 2009, the ban on building minarets and, in 2021, the face-veiling ban was accepted. In between these two federal ballots, the canton of Ticino (2013) and the canton of St. Gallen (2018) voted on and accepted veiling bans on the cantonal level.[4] Although the laws contain no direct mention of Islam (see additional information in Appendix 3.A), the discussion beforehand mainly was about the face veiling of women under Islam, and the unofficial name of the federal initiative was the "burqa initiative." This strong connection is supported by the pro- and contra-arguments mentioned in the voting information booklet sent to Swiss voters before the federal vote.

On the one hand, opponents argued that the face veil is only a marginal phenomenon, and a ban thereof does not help the suppressed women. On the other hand, proponents argued that the face veil is a sign of the oppression of women and goes against the principles of liberal coexistence (Schweizerische Eidgenossenschaft, 2021). Such controversial votes usually lead to intensive debates beforehand, where the streets are full of posters of supporters and opponents.[5] Figure 3.C.1 in the Appendix provides an example of publicity favoring the veiling ban in the federal vote in 2021.

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[4]See also Figure 3.5.1 for a timeline of the votes. The canton of Glarus decided on the face-veiling ban in a town meeting. Because I do not have the municipal vote outcome data (no secret ballot) and the survey data does not provide many observations for this canton (small population size), I excluded this canton from the analysis.

[5]In Switzerland, it is allowed to make political advertisements on public and private grounds and in newspapers. However, political advertisements on television and radio are prohibited.

Emergence of the cantonal votes

Cantonal votes can follow out of compulsory or facultative referendums and initiatives. In the case of Ticino, an individual started the collection of signatures for the initiative of a cantonal veiling ban. The initiative committee, led by Giorgio Ghiringhelli, delivered 11,767 valid signatures.[6] This amounts to around 5% of the electorate of Ticino at that time. For the canton of St. Gallen, in contrast, the cantonal parliament wanted to change the Offences Penal Code and add a veiling ban to it. Different political parties disagreed with that and raised a referendum against it. In the end, 4,273 voters signed the referendum, which amounts to around 1% of the electorate of St. Gallen at that time.[7] Thus, the votes on veiling bans emerged once from direct democracy and once from representative democracy.

3.4 Conceptual background

In the following, I discuss how direct-democratic votes on veiling bans theoretically affect the reported attitudes toward Islam through different channels. As shown in Figure 3.4.1, (reported) individual attitudes are seen as endogenous to democratic decision-making.
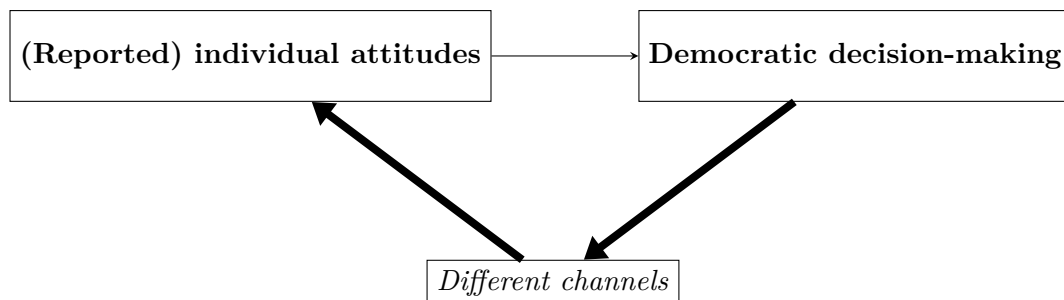
Figure 3.4.1: Links between individual attitudes and democratic decision-making.

A rational individual reports his attitudes toward a given topic by maximizing its utility under some constraints. A direct-democratic vote, on the one hand, affects an individual's *information* about a topic. On the other hand, the public information on the vote outcome changes the *social restrictions* and, thereby, the constraints. Hence, the reported attitudes toward Islam can be affected by votes on veiling bans (at least) through the following channels:

Information. The paradox of voting leads to the situation that rational voters have poor incentives to collect costly information on a topic (Downs, 1957). When direct-

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[6]For more information, please see the homepage of the canton of Ticino `https://www4.ti.ch/generale/diritti-politici/votazioni/archivio/archivio-votazioni-cantonali-federali-e-comunali` (Accessed: 30 August 2023).

[7]More information is provided on the homepage of the canton of St. Gallen `https://wab.sg.ch/` (Accessed: 30 August 2023).

democratic votes occur, fellow citizens expect individuals to have an opinion on the discussed issue, turning it into a private good. Thus, having an opinion is a benefit *per se* (Frey, 1994; Hirschman, 1989). This is shown, for instance, by the fact that voters are more informed and politically knowledgeable under direct-democratic settings (e.g., Benz and Stutzer, 2007; Tolbert et al., 2003).

At the same time, intensive discussions before direct-democratic votes help people collect information about a topic. Habermas (1979) understands democracy as a set of institutions where legitimacy relies on collective will-formation through discourse. Discursive theorists see a direct-democratic process as a social process in which a particular topic is intensively discussed, which allows citizens to form opinions and learn what might be best for society.[8] This direct-democratic process generates clean information that makes individuals more knowledgeable and decreases their estimation error on their individual attitude (Eichenberger and Serna, 1996). Moreover, not only do individuals learn more about a topic, but also the potential "victims" of the vote are identified. This may increase other-regarding behavior (see, e.g., Bohnet and Frey, 1999).

Social restrictions. Aggregators of private opinions in society, like outcomes of elections or referendums, reveal new information to the public about the prevailing social norm (Apffelstaedt et al., 2022; Bursztyn et al., 2020a; Jung and Tavits, 2021). Observers may sanction norm-breaching behavior (Alvarez-Benjumea and Valentim, 2022; Bursztyn et al., 2020a), such that the change in the social norm can impact the costs and benefits of expressing certain opinions.[9] If a new controversial policy is introduced or a populist party is elected, the social restriction to raise these opinions weakens. This phenomenon is known as the legitimization effect. It obtains, for instance, empirical support from studies that find that populists in power increase xenophobic views in the population (e.g., Bursztyn et al., 2020a; D'Ambrosio et al., 2021; Hagemeister, 2022). The literature shows how individuals hide their (political) preferences until new public information in favor of their individual preferences becomes available (e.g., Kuran, 1989). Moreover, the legitimization effect is closely linked to the expressive function of laws. In addition to the deterrent effect of laws, there might exist an expressive power that is supposed to influence values and behavior (e.g. Funk, 2007; Sunstein, 1996).[10]

To sum up, first, the *information* effect may lead to the fact that people are better informed about their own attitude and have a more precise opinion on the discussed

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[8]See for instance Habermas (1979) theoretical framework of how democratic settings may affect individual capacities necessary for democracy. One of these capacities is the discovery of one's preferences (Warren, 1993).

[9]When looking at anonymous survey data, sanctions by observers may not occur. However, because of the well-known *social desirability bias*, individuals may falsify their true attitudes if they believe their opinions are not socially acceptable (see, e.g., Gerling and Kellermann, 2022). For instance, individuals systematically underreport their right-wing party preferences in interviews (DellaVigna et al., 2012; Hainmueller and Hangartner, 2013).

[10]Another theory discusses how new policies or the election of populists lead to a *backlash effect* because individuals with other opinions might try to counterbalance the outcome (e.g., Bischof and Wagner, 2019; Bishin et al., 2016). This effect should mostly occur in situations where the law contradicts the prevailing social norm (e.g., Acemoglu and Jackson, 2017). This should not be the case in referendums that are decided by majority voting.

issue. Second, individuals are better informed about the prevailing public opinion, which signals a new social norm. Because individuals are known to adapt to social norms, this may change their utility of raising certain opinions and affect their *social restrictions.* These considerations suggest that a democratic process with votes on veiling bans affects citizens' attitudes toward Islam. However, it remains an empirical question of which mechanisms are at play and outweigh the others.

3.5   Setting and data

In this section, I present the data used and provide descriptive statistics for the treated and control groups. To answer the research question, I make use of two data sources:

- Individual survey data from the Swiss Household Panel (SHP) with reported attitudes toward Islam (and other religions)

- Information on vote outcomes at the municipal level (from cantonal or federal archives)

This results in 32,807 observations of reported attitudes toward Islam in the 2013–2022 period. Of these, 3,117 stem from the treated cantons (TI and SG), and 29,690 stem from the untreated cantons (see Table 3.5.1).[11]

The SHP is a general survey of the Swiss population in which the key objective is to observe social change. It is a joint project run by the Swiss Center of Expertise in Social Science (FORS), the Swiss National Science Foundation (SNSF), and the Swiss Federal Statistical Office (FSO). This survey of individuals takes place yearly, but not all questions enter the survey yearly.[12] The timeline of the Islam-related votes and SHP waves with the question(s) of interest are depicted in Figure 3.5.1. The federal vote for a nationwide ban on veiling took place before the latest survey observations, as shown in Figure 3.5.1. I will drop the observations for this survey in the robustness section because it is unclear whether the federal vote affects the attitudes toward Islam in the cantons with a cantonal vote on the topic before and in the cantons without such a vote in the same manner.[13] Because the treatment occurs at the cantonal levels and I am interested in the environment in which individuals live, I dropped individuals who have moved between districts during my observation period (2012–2021).

Outcome variables. The variable in which I am interested is "Attitudes toward Islam," which has been indicated on a scale from 0 (very negative) to 10 (very posi-

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[11]For the yearly observations of the canton of TI, the canton of SG, and the untreated cantons, please see Table 3.B.1.

[12]So far, the panel is based on four different samples. It started in 1999 (5,074 households + 12,931 individuals) and added new samples in 2004 (2,538 households + 6,569 individuals), in 2013 (4,093 households + 9,945 individuals), and in 2020 (4,380 households and 7,557 individuals).

[13]Theoretically, it is possible that the federal vote (i) affects all individuals the same, (ii) exerts a more minor effect on the individuals from the treated cantons (because they already experienced the social discourse on the topic), or (iii) exerts a more significant effect on the individuals from the treated cantons (because they already have updated their social norm before).

tive).[14] This question was asked in the 2013, 2016, 2019, and 2022 surveys.[15] I look at the variable in the following terms: First, the variable *AttIslam* reflects the attitudes indicated on a scale from 0 to 10. Second, I use a polarization measure called *AttIslamPolar*. This variable is constructed in the following way: it measures in absolute terms how far away an indicated attitude is from the middle, neutral opinion (=5), and takes values from 0 (neutral) to 5 (very extreme). Thus, if a respondent indicates the value 0 or 10, this is coded as a 5, and if a respondent indicates the value 5, this is coded as a 0. The variables of interest are depicted in Table 3.5.1–the variable *AttIslam* is only slightly lower in the treated cantons.
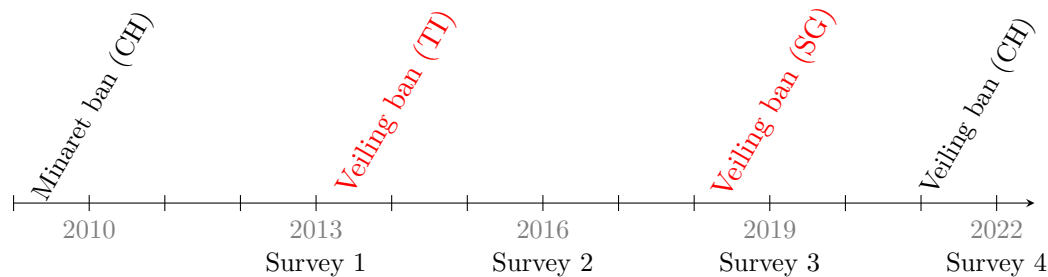
Figure 3.5.1: Timeline of the votes and surveys (2009–2022).

Notes: The surveys of the SHP take place over a period from the fall of the previous year until the spring of the indicated year. Hence, the federal vote on a veiling ban occurred before Survey 4. I run robustness checks where I exclude this survey from the sample.

Municipal backgrounds. Because the survey data can be matched with municipal data, I also dispose of information on the respondents' backgrounds, allowing for an analysis of the environment in which an individual lives. I report descriptive statistics on the municipal level for the treated and untreated groups in the Appendix of Table 3.B.2. Notably, the information on an individual's municipal background allows for delving deeper into potential channels (see Section 3.7.5). Moreover, municipal information can help in controlling for potential confounders. For instance, I dispose of the information on the yes share in the federal vote on a minaret ban in 2009 (*YesMinaret*), which proxies well for pretreatment attitudes toward Islam. Further municipal controls are the population size (*PopTot*) or share of non-citizen residents (*ForeignShare*) of the municipality in which an individual lives. This may be an indicator of the urban or rural environment. Moreover, to control for the conservatism of an environment, I

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[14]The exact question was the following: *"What is your personal attitude toward the following religions and worldviews, if 0 means "very negative" and 10 "very positive"?".*

[15]Please note that the survey years here do not refer to the official survey year dates of the SHP. In official terms, I use the 2012, 2015, 2018, and 2021 surveys. However, because the surveys only start in the fall of the respective year and usually take place until the following year's spring, it is easier to indicate the timelines with the following year as the survey year (see Figure 3.5.1).

control for the share of married inhabitants (*MarriedShare*) and inhabitants over 65 years (*PensionerShare*).

Table 3.5.1: Descriptive statistics on the individual level for (un)treated cantons.

| | Treated cantons | | | | | Untreated cantons | | | | |
	Obs.	Mean.	Std.dev.	Min.	Max.	Obs.	Mean.	Std.dev.	Min.	Max.
Variables of interest										
AttIslam	3,117	4.14	2.48	0	10	29,690	4.15	2.38	0	10
AttBudd	2,858	5.80	2.32	0	10	27,554	5.70	2.29	0	10
AttChrist	3,089	6.60	2.34	0	10	29,494	6.28	2.30	0	10
AttHind	2,732	5.25	2.29	0	10	26,436	5.16	2.20	0	10
AttJuda	2,810	5.02	2.32	0	10	28,107	4.97	2.21	0	10
AttAth	2,856	4.61	2.75	0	10	27,468	4.76	2.67	0	10
AttIslamPolarized	3,117	1.93	1.78	0	5	29,690	1.83	1.75	0	5
Individual characteristics										
sex	3,117	1.53	0.50	1	2	29,689	1.52	0.50	1	2
age	3,117	50.10	18.97	14	94	29,690	50.50	19.06	14	100
education	3,116	5.37	3.08	0	10	29,681	5.62	3.14	0	10
Swiss	3,117	0.91	0.29	0	1	29,690	0.93	0.25	0	1
PolitInterest	3,002	5.38	2.93	0	10	28,861	5.71	2.72	0	10
PolitPosition	2,552	5.05	2.15	0	10	26,027	4.90	2.15	0	10
Protestant	3,087	0.13	0.33	0	1	29,522	0.36	0.48	0	1
Catholic	3,087	0.65	0.48	0	1	29,522	0.38	0.49	0	1
Muslim	3,087	0.01	0.10	0	1	29,522	0.01	0.12	0	1

Individual backgrounds. The SHP survey data also provide information on the individual level. The descriptive statistics on the individual level for the treated (TI and SG) and untreated individuals are shown in Table 3.5.1. The variables of interest and individual characteristics are fairly balanced between the treated units (TI and SG) and the untreated units (all other cantons). Greater differences exist in the variables *Education*, *PolitInterest*, and *PolitPosition*, with the treated units being slightly less educated, less politically interested, and positioned more to the right in the political spectrum.

First look at the data

The two cantons of Ticino and St. Gallen voted on the cantonal level on veiling bans (2013 and 2018). Figure 3.5.2 depicts the development of the cantonal means in indicated attitudes toward Islam of the two treated cantons and the control cantons (divided into German- and French-speaking regions). Interestingly, the means of the treated cantons seemed to move reasonably differently from the others after the cantonal votes took place. This shift in the direction of negative attitudes toward Islam looks especially strong for the canton of Ticino (see Figure 3.5.2). Figure 3.5.2 further points to the importance of making use of different empirical procedures to make sure that parallel trends hold (see discussion in Section 3.6.1).

Because the outcome variable is indicated on a scale of 0 to 10, further interesting insights can be gained when examining potential shifts in the categorical values. In the Appendix, Figure 3.C.2 and 3.C.3 respectively, depict histograms of the distribution of attitudes toward Islam before and after the cantonal votes on face-veiling bans for the treated cantons and their neighbors. Indeed, for Ticino, the attitudes shifted more toward the extreme values after the votes than toward their neighbors (see Figure 3.C.2).
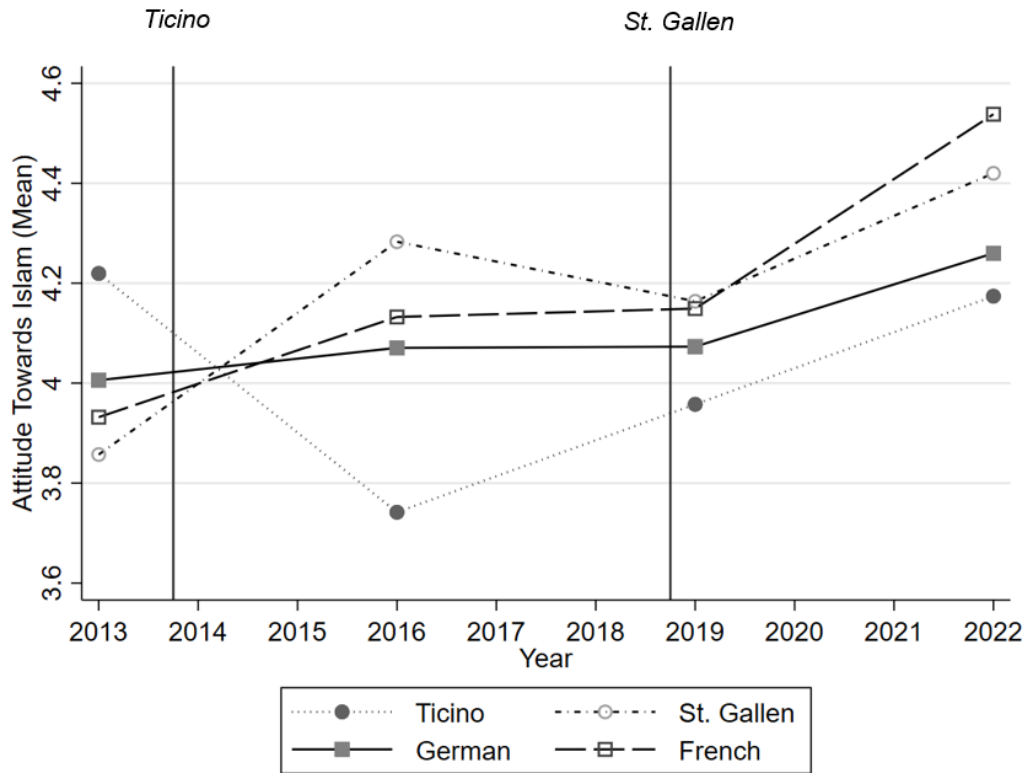
Figure 3.5.2: Cantonal means of individual attitudes toward Islam.

Notes: Data are taken from the individual-level survey from the SHP. The surveys usually take place from the fall of the previous year to the spring of the indicated year.

3.6 Empirical strategy

The data structure allows for the use of a DiD strategy with multiple time periods to identify the average treatment effect of the treated (ATT) in repeated cross-section and

panel analyses.[16] The treated individuals are citizens living in Ticino or St. Gallen after the cantonal vote on veiling bans, thus from 2013 or 2018, respectively.[17] I dispose of an unbalanced panel[18], thus giving the possibility to estimate the ATT in repeated cross-sections and the panel.

In my baseline estimates, I follow Callaway and Sant'Anna (2021) to estimate the $ATT(g,t)$, which is identified by comparing the expected change in attitudes toward Islam for the group g between periods $g-1$ and t with that for a control group. The model, where I take the never-treated cantons as a control group and do not introduce covariates, looks as follows:

$$ATT(g,t) = [EY(g)_t - EY(NT)_t] - [EY(g)_{t-1} - EY(NT)_{t-1}] \qquad (3.1)$$

where NT stands for "never treated." In the first parenthesis, the difference in outcomes at time t is calculated, while in the second parenthesis, the outcome at time $g-1$ is calculated, which is the period before the treatment occurs. Under this specification, the $ATT(g,t)$ is identified if parallel trends hold (unconditional on covariates) and if there are no anticipation effects. In the baseline estimations, I cluster standard errors at the district level because standard errors are likely to be correlated in this unit.[19] Although in the baseline, I use all never-treated cantons as the control groups, I restrict the samples to individuals from more similar environments regarding prior voting behavior in Islam-related topics or geographic closeness in the robustness section. Moreover, I include the observations of the four survey waves (2013, 2016, 2019, 2022) in the baseline sample but exclude the 2022 survey as a robustness test because the federal veiling ban ballot took place before this survey.[20]

As shown in Section 3.5, not all variables are fully balanced among the treatment and control groups. In the panel estimations, I absorb those characteristics that do

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[16]See Figure 3.5.1 for the timeline of the votes and surveys. Please note that the vote on the veiling ban in St. Gallen occurred at the beginning of Survey 4 in the data set. Theoretically, this allows for analyzing the vote's effect on citizens' reported attitudes in a regression discontinuity design (RDD). Unfortunately, there are insufficient observations around the voting date, and an RDD is underpowered. Figure 3.C.6 depicts the indicated attitudes toward Islam in the weeks before and after the vote—it shows an insignificant negative trend. In the DiD estimates, I drop the observations of 2018 before the vote in SG.

[17]So far, I have assumed that there are no spillover effects between the different cantons. In the future, it would be interesting to analyze to what extent the discussions or information on the result in one canton spill over to other cantons.

[18]As mentioned in Section 3.5, some households and individuals dropped out of the sample after a while, and new samples were added at three different points in time, making it an unbalanced panel.

[19]The majority of the 26 cantons are subdivided into districts, which are made up of a varying number of municipalities (see https://www.bfs.admin.ch/bfs/de/home/statistiken/kataloge-datenbanken.assetdetail.24065857.html (Accessed: 31 July 2023). With a few exceptions, these districts are mainly administrative and judicial entities or electoral districts and have no independent legal status. However, individuals from the same districts often share similar cultural, linguistic, or traditional backgrounds. I cluster standard errors at the cantonal or municipal level in the robustness sections.

[20]The federal vote on a veiling ban took place in March 2021, whereas Survey 4 started in September 2021. Under the assumption that this vote affects all individuals equally, independent of their pre-vote experience, responses from Survey 4 can be used as posttreatment observations.

not vary over time, such as sex, education, religion, or stable municipal characteristics. Moreover, I include different individual and municipal controls in the robustness section (see Section 3.5). Additionally, I use a matching procedure combined with the multiple DiD to balance the covariates.

The estimation procedure of Callaway and Sant'Anna (2021) is used because it has been shown that, under a standard dynamic two-way-fixed effect (TWFE), the estimates are not reliable measures of "dynamic treatment effects" because of weighting problems (e.g., Sun and Abraham, 2021).[21] Intuitively, the idea behind the Callaway and Sant'Anna (2021) estimator is to rely on never-treated (or not-yet-treated) units as controls to obtain consistent estimators for the ATTs. Otherwise, heterogeneous treatment effects would violate the parallel trend assumption, and the estimations of the effects could be severely biased. Callaway and Sant'Anna (2021) propose a transparent way to analyze DiD frameworks with multiple time periods; this method estimates every feasible 2x2 DiD design available in the sample. I use the default option so that the estimator is based on the doubly robust DiD estimator based on stabilized inverse probability weighting and ordinary least squares.

3.6.1 Parallel trends

The DiD approach requires relying on the assumption of parallel trends. Therefore, I have to assume that, in the absence of the treatment, the treated group would have evolved similarly to the control group during the treatment period. This assumption cannot formally be tested. However, in some situations, parallel trends are more or less likely. In the following, I discuss potential arguments favoring the parallel trend assumption.

First, the cantons of Ticino and St. Gallen are embedded in the same economy, national laws, and regulations (e.g., immigration laws) as the other Swiss cantons. Therefore, they are likely to face common shocks during the observation period. For instance, the percentage of people relating themselves to Islam seems to evolve similarly over time in the treated cantons and their neighboring cantons (see Figure 3.C.4 in the Appendix).

Second, the parallel trend assumption is more plausible if the outcome variable has evolved similarly in the pre-treatment period. However, data collection for the outcome variable did not start far enough before the treatment. For St. Gallen, I have two observation periods in the pre-treatment period and only one for Ticino. Therefore, when checking for parallel pre-trends (see Table 3.B.3), these estimations only rely on St. Gallen as the treatment group. Table 3.B.3 provides the estimates for the evolution of trends before the vote on veiling bans. I do not find statistically significant different trends in the outcome variable in the pre-treatment period for the treated and untreated cantons (see Table 3.B.3). To make the common trend assumption more plausible, I will rely on different control groups as robustness checks and implement a matching

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[21]See Callaway and Sant'Anna (2021) for a discussion on the issues of standard dynamic two-way fixed effect models.

procedure in the robustness section.[22] Furthermore, I check for parallel pre-trends in those variables correlated with the outcome variable and that are available more regularly in the pre-treatment period. For instance, the indicated individuals' left-right positions on the political spectrum of the individuals have evolved similarly in the treated and untreated cantons before the votes on veiling bans (see Table 3.B.4).

Third, to increase the plausibility of the parallel trend assumption, it is important that the level of the outcome variable is not too distinct between the treatment and control groups before treatment. The outcome variable in Table 3.5.1 supports this. Additionally, Ticino and St. Gallen did not deviate from the other cantons when examining the yes shares of the vote prohibiting minarets building in 2009, as shown in Table 3.B.5. Moreover, the indicated religious tolerance toward covering heads in the 1999 SHP survey does not indicate that the two treated cantons are outliers (see Table 3.B.6).

3.6.2 Exogeneity of the treatment

As discussed in Section 3.3, the cantons that voted on veiling bans were not randomly selected for treatment. Although this is often the case when some jurisdictions introduce new policies, the potential selection of treatment poses an empirical challenge. Although in the canton of St. Gallen the vote resulted from a facultative referendum, in the canton of Ticino, it followed out of a popular initiative. In both cases, however, only a minor number of voters' signatures was needed (see Section 3.3). Moreover, the treated cantons were no exceptional outliers in the minaret vote (Table 3.B.5) or in the 1999 survey about religious tolerance (Table 3.B.6), and there was no systematic violation of parallel pre-trends detected for the self-indicated political position (Table 3.B.4).

Although I estimate the ATTs in multiple DiD frameworks in my baseline estimations, I complement these analyses with a propensity score-matching approach in the robustness section to ensure that I compare the treated individuals only to individuals as likely to be treated. Therefore, I balance the sample in terms of pre-treatment characteristics of the individual's environment, such as the municipal yes share in the minaret vote 2009, the population size of the municipality, and share of foreigners. Moreover, in the robustness section, I focus on different control samples, where I only include individuals as controls from municipalities with similar preconditions.[23] If the results remain stable under these alternative estimations, this somewhat relaxes the endogeneity concerns.[24]

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[22]Another possibility to ensure parallel pre-trends are fulfilled would be to aggregate the reported attitudes on the cantonal levels and introduce a synthetic control method. Unfortunately, this is not possible in this setting because the synthetic control method needs a long pre-treatment period to construct the donor pool (e.g., Abadie, 2021).

[23]I restrict the control sample to municipalities that have accepted the vote on minaret bans in 2009 and exclude outliers regarding population size or foreigner shares.

[24]A further possibility that could be tested in the future is to use a Bartik (shift-share) instrument to make use of a more exogenous variation (e.g., Borusyak et al., 2022). For instance, one could introduce the foreigner shares in a municipality as exposure share weights.

3.7 Results and discussion

This section provides the results of the DiD estimations and further discusses the potential underlying mechanisms. As explained in the previous section, I use the DiD for multiple treatment periods in a panel and repeated cross-section provided by Callaway and Sant'Anna (2021).

3.7.1 Attitudes toward Islam - Level

Democratic decision-making may affect individual attitudes via different mechanisms (see Section 3.4). In particular, the *information* and *social restrictions* mechanisms can be important in the evolution of reported attitudes toward Islam after the votes on face-veiling bans. The overall effect of veiling ban votes on the level of reported attitudes toward Islam is not apparent from a theoretical perspective.

To obtain an overview of how the reported attitudes toward Islam evolved after voting on a face-veiling ban, I estimate the multiple DiD model (3.1) on *AttIslam*. The results for the repeated cross-section and panel estimates are depicted in Figure 3.7.1, and the underlying period estimates are provided in the Appendix in Table 3.B.7. Both estimates are unconditional on the control variables, and standard errors are clustered at the district level. I will discuss several robustness checks in subsection 3.7.3.

| (a) | (b) |

Figure 3.7.1: Voting on veiling bans & attitudes toward Islam – Level.

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The continuous outcome variable indicates attitudes toward Islam on a scale of 0 (very negative) to 10 (very positive). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; thus, the canton of GL has been excluded.

Both panels (a) and (b) in Figure 3.7.1 provide a similar picture: the estimated coefficient for the period -1 is not statistically significant, thus supporting the parallel pre-trends. However, after the vote on the face-veiling bans (from period 0 on), the coefficients for the reported attitudes toward Islam become negative. These estimates are statistically significant for most periods.

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After the votes on veiling bans, citizens report on average 0.328 (panel) or 0.347 (repeated cross-section) points more negative attitudes toward Islam (see ATTs in Table 3.7.1 Spec. 1 & 3). These results are statistically significant at the 1% level and, therefore, are similar in size and statistical significance when estimated in repeated cross-sections or a panel. This effect is sizable, given the mean of 4.15 points in the untreated sample (see Table 3.5.1).

These findings show that, on average, the reported attitudes toward Islam evolved more negatively in the cantons that have voted on face-veiling bans. This provides first evidence that the discussed mechanisms of Section 3.4 (*information* & *social restrictions* channels) might be at play. Vote outcomes can act as public opinion aggregators, which may update people's beliefs about the prevailing social norm. At the same time, the social discourse increases individuals' knowledge about a topic and their personal preferences (see, e.g. Eichenberger and Serna, 1996; Frey, 1994). Potential channels will be more closely discussed in Section 3.7.5.

Table 3.7.1: ATT and period estimates of the baseline DiD.

	Panel		Repeated cross-section	
	(1) AttIslam	(2) AttIslamPolar	(3) AttIslam	(4) AttIslamPolar
ATT	-0.328***	0.201	-0.347***	0.197**
	(0.124)	(0.123)	(0.110)	(0.092)
N. obs.	22053	22053	31911	31911

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The variable AttIslam ranges from 0 (very negative) to 10 (very positive), and the variable AttIslamPolar from 0 (middle option) to 5 (extreme option). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; thus, the canton of GL has been excluded. *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.7.2 Attitudes toward Islam - Polarization

Both of the discussed mechanisms of Section 3.4—the *social restrictions* and *information* channels—can lead to more polarization in the reported attitudes toward Islam after the votes on face-veiling bans. On the one hand, the new public information available (un)stigmatizes certain opinions and allows for expressing more "extreme" attitudes. On the other hand, social discourse allows individuals to form clearer opinions on the discussed issue, which may lead individuals to indicate less the middle option (5) and instead more extreme views.

Hence, I estimate the baseline Model 3.1 on the outcome variable *AttIslamPolar*, which consists of values from 0 to 5, where it takes the value 0 when individuals report the middle option (5) and value 5 when individuals report an opinion at the margin (0

or 10) in their attitudes toward Islam. Both plots in Figure 3.7.3 point to a positive but not thorough statistically significant effect, and the underlying period estimates are presented in the Appendix in Table 3.B.7. This is also reflected in the ATTs reported in Table 3.7.1. The coefficients are 0.201 (panel) and 0.197 (repeated cross-section), respectively, and the coefficient is only statistically significant in the cross-section estimations at the 5% level.
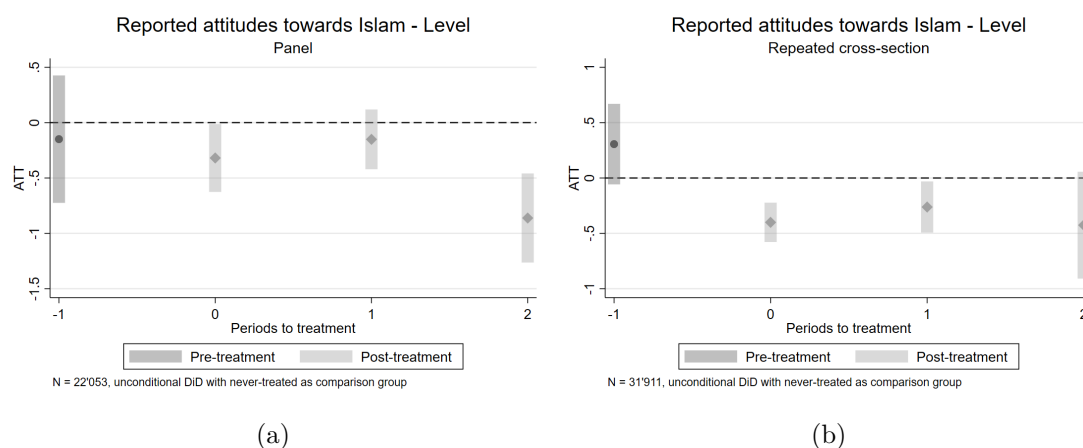
(a)

(b)
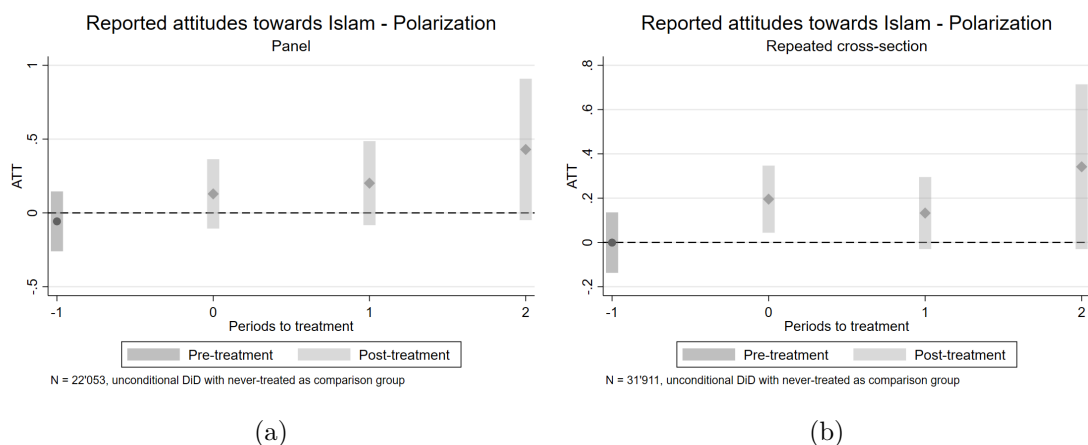
Figure 3.7.3: Voting on veiling bans & attitudes toward Islam – Polarization.

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The continuous outcome variable refers to the absolute distance from the middle option (5) in indicated attitudes toward Islam. The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; thus, the canton of GL has been excluded.

Thus far, the estimations point to more negative and polarized attitudes toward Islam in the treated cantons after the votes on face-veiling bans. These results remain robust and are (not statistically significant) larger when excluding the 2021 survey (see Table 3.B.8 in the Appendix). In the following subsection, I perform further robustness checks to relax the omitted variables or selection bias concerns to some extent (see Section 3.7.3). Moreover, I run placebo tests to rule out that the findings result from general changes toward other religions and that initial attitudes toward Islam drive the results (see Section 3.7.4).

3.7.3 Robustness tests

Although I provide some evidence favoring the parallel trend assumption (see Section 3.6), the main empirical challenge remains the potential selection bias because of selection into treatment status. To address this concern, I check whether the findings are robust to introducing control variables. I also utilize a matching procedure to balance the treatment and control groups based on pre-treatment characteristics. Furthermore, I narrow the sample to ensure that the treatment and control groups are comparable.

I do not include control variables in the baseline estimations because I rely on the unconditional parallel trend assumption on the control variables. However, if characteristics between the treated and control groups differ significantly or evolve significantly differently, this would make the unconditional parallel trend assumption less plausible. To address this, a possibility is relying on the parallel trend assumption conditional on covariates. Therefore, I estimate the multiple DiD coefficients by introducing different covariates in the panel and repeated cross-section estimations. Although in the panel estimations, the individual fixed effects should absorb time-invariant differences between the treated and control groups, there might be a need to include time-variant covariates. In the repeated cross-section, I introduce individual controls and time-invariant characteristics at the municipal level. The event study plots of the estimations, here with the control variables, are depicted in Table 3.C.7, and they look similar to the baseline estimations.

Moreover, I perform a matching procedure, balancing the sample between the treated and control groups regarding pre-treatment characteristics. Based on the following pre-treatment variables, I estimate the propensity score and, thus, the likelihood that an individual gets "selected" into the treatment (*MinaretYes, ForeignShare, PopSize, Religion, PolitPosition*). As Figure 3.C.9 shows, the matching procedure successfully balances the covariates between the treatment and control groups better than in the baseline estimations. This makes the parallel trend assumption more credible than under unconditional DiD estimates. In addition, in these estimations, the results for the level of reported attitudes remain robust, as shown in the event study plots in Figure 3.C.10 in the Appendix. However, the estimations for the polarization are not statistically significant anymore, but in terms of size and sign, they point in the same direction as the baseline estimations.

Furthermore, I run the baseline estimations with different sample restrictions (see Table 3.B.9) to ensure that the treatment and comparison groups are comparable regarding background. Therefore, I restrict the sample to municipalities that voted yes in the federal minaret vote and exclude French-speaking cantons from the model. Moreover, I exclude observations from individuals who live in municipalities that are outliers regarding municipality size or foreign share of the resident population. The results remain (mostly) robust in terms of size and significance; only the coefficients for the *AttIslamPolar* in the panel estimations lose statistical significance. Moreover, I run the baseline estimations when clustering the standard errors on different levels—also here, the results remain mostly robust (see Table 3.B.10 in the Appendix).

3.7.4 Placebo tests

Thus far, I have found fairly stable estimates for the effect of face-veiling bans on reported attitudes toward Islam. The results also remain robust over a series of robustness checks (see Section 3.7.3). In the following, I perform placebo tests, in which I interchange the outcome variables and the treatment group.

I want to make sure that the change in attitudes reported toward Islam in the respective cantons cannot be attributed to general changes in attitudes toward religion.

In the SHP survey, individuals do not only indicate their attitudes toward Islam but also toward other religions (see Table 3.5.1). Therefore, I perform the same estimations but replace the outcome variable with attitudes toward Christianism, Hinduism, Buddhism, Judaism, and Atheism. The ATTs on the attitudes toward other religions are shown in Table 3.7.2. For most religions, Table 3.7.2 depicts no sizable or significant coefficient. Only for the reported attitudes toward Buddhism do I find a somewhat sizable and significant effect (see Spec. 4 in Table 3.7.2) in the repeated cross-section; however, this coefficient is in contrast to the reported attitudes toward Islam positive and does not stay robust under the panel estimations. In addition, no systematic significant results can be found on the polarization of attitudes toward other religions (see Table 3.B.11). Hence, attitudes toward other religions have not evolved differently during the treatment period in the treated and untreated cantons, making the assumption of a common trend in general religious attitudes more plausible.

Table 3.7.2: ATT placebo outcomes - attitudes toward other religions.

	Repeated cross-section					
Level attitudes	(1) Islam	(2) Christianism	(3) Hinduism	(4) Buddhism	(5) Judaism	(6) Atheism
ATT	-0.347***	-0.057	0.148	0.168*	-0.044	0.126
	(0.110)	(0.104)	(0.117)	(0.100)	(0.123)	(0.095)
N. obs.	31911	33424	28603	30065	30497	30263
	Panel					
Level attitudes	(1) Islam	(2) Christianism	(3) Hinduism	(4) Buddhism	(5) Judaism	(6) Atheism
ATT	-0.328***	-0.116	0.052	0.042	-0.051	0.060
	(0.124)	(0.096)	(0.077)	(0.114)	(0.105)	(0.168)
N. obs.	22053	23329	19046	20371	20930	20691

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The continuous outcome variables indicate attitudes toward the respective religion on a scale of 0 (very negative) to 10 (very positive). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; thus, the canton of GL has been excluded.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Another concern might be that the shifts in attitudes toward Islam follow from a general shift toward more right-wing or xenophobic views in the respective cantons. Therefore, I provide the trends of the vote shares for right-wing parties in national elections in the treated and untreated cantons (see Figure 3.C.5 in the Appendix). The vote shares for right-wing parties do not evolve systematically differently, giving more certainty that the effects found do not merely come from a change in political attitudes.

Furthermore, I perform placebo tests in which I introduce other cantons as treatment cantons. Therefore, I specify the cantons with similar voting behavior in the federal initiative on minaret bans (see Table 3.B.5) as the placebo-treated cantons. Another

exercise uses the cantons with similar indicated attitudes toward head coverings in the 1999 survey as the treated cantons as the placebo-treated cantons (see Table 3.B.6). With these approaches, I want to rule out that the initial attitudes toward Islam would play a role in explaining the evolution in the reported attitudes in the aftermath of the cantonal veiling ban ballots.

The results are depicted in Table 3.7.3 and do not provide the same findings as in the baseline estimations. Only for the minaret sample is the negative effect in the level of reported attitudes negative and statistically significant in the repeated cross-sections (Spec. 1 in Table 3.7.3). The coefficient is, however, smaller in terms of size (-0.347 vs. -0.134) and does not hold for the panel estimations in Specification 5 (see Table 3.7.1 for the coefficients of the baseline).

Table 3.7.3: Placebo tests - substituting the treatment groups.

	Repeated cross-section			
	Level		Polarization	
AttIslam & AttIslamPolar	(1)	(2)	(3)	(4)
Placebo - treated group	Minaret	CoverHead	Minaret	CoverHead
ATT	-0.134*	-0.050	0.009	-0.045
	(0.077)	(0.071)	(0.046)	(0.051)
N. obs.	29382	29173	29382	29173
	Panel			
	Level		Polarization	
AttIslam & AttIslamPolar	(5)	(6)	(7)	(8)
Placebo - treated group	Minaret	CoverHead	Minaret	CoverHead
ATT	-0.007	-0.012	0.019	-0.061
	(0.063)	(0.060)	(0.055)	(0.052)
N. obs.	20566	20194	20566	20194

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. In these placebo tests, I first substituted the treated cantons with cantons that showed a similar voting behavior in the minaret initiative as the treated cantons (see Table 3.B.5). For TI: AI, GL, TG / For SG: AG, SO, SZ. Second, I substituted the treated cantons with cantons that indicated similar attitudes toward head coverings in the 1999 SHP survey (see Table 3.B.6). For TI: JU, VS, FR / For SG: SO, SZ, AG.
*** p < 0.01, ** p < 0.05, * p < 0.1.

These placebo tests (interchanging the outcome variable and the treated) bolster the evidence that the change in attitudes toward Islam in the treated cantons result—at least in some part—from the votes on face-veiling bans. The approaches to disentangling potential mechanisms will be discussed below.

3.7.5 Exploring channels

So far, I have looked at the ATTs and have found relatively stable results in that the reported attitudes toward Islam were more negative and more polarized after the votes on veiling bans. However, different mechanisms can explain these findings, as discussed in Section 3.4. Below, I try to disentangle the potential channels that lead to the results.

Social restrictions – Are individuals adapting to the new social norm?

Vote outcomes provide new public information on the prevailing social norms (e.g. Bursztyn et al., 2020a). Vote outcomes can, therefore, lead to an update of the social norm (see Section 3.4) and legitimize the expression of certain attitudes. In the case of democratically accepted face-veiling bans, the costs of raising a negative attitude toward Islam may decrease.

One way to understand the importance of the *social restrictions* channel is to examine whether there are heterogeneous effects based on the level of acceptance in an individual's environment, as measured by the *municipal yes shares* of the cantonal veiling ban ballots. If the ban got accepted with a high percentage of the votes, the information on the prevailing social norm was stronger—which may enhance the legitimization effect. To examine this, I divide the sample into two groups: individuals from municipalities with high acceptance rates (over the median) and those from municipalities with low acceptance rates (under the median). The results of the estimations for the samples with high and low acceptance rates are provided in Table 3.7.4.

Table 3.7.4: Heterogeneous effects based on acceptance rate in cantonal burqa votes.

Level Attitudes	Repeated cross-section		Panel	
	(1) high yes	(2) low yes	(3) high yes	(4) low yes
ATT	-0.404***	-0.301*	-0.421***	-0.202
	(0.133)	(0.181)	(0.136)	(0.149)
N. Obs.	30588	30526	21331	21289

Notes: The estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The outcome variable *AttIslam* is the reported attitudes toward Islam on a scale from 0 (very negative) to 10 (very positive). In the sample with high acceptance rates, I include individuals with a higher than median acceptance rate; in the sample with low acceptance rates, I include individuals from environments with a lower than median acceptance rate.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

As depicted in these estimations, the negative effect on the overall reported attitudes toward Islam is larger in the sample with high acceptance rates (Spec. 1 & 3) compared

with those with low acceptance rates (Spec. 2 & 4). However, the difference is not statistically significant. For the samples with a high acceptance rate and, thus, a "strong signal," the ATTs amount to -0.421 in the panel estimations and are statistically significant at the 1% level (see Spec. 3 in Table 3.7.4). Therefore, the ATT on the reported attitudes toward Islam seems largely driven by individuals from municipalities where the veiling ban was relatively highly accepted—that is, over the median yes share. This aligns with Arnesen et al. (2019), who provide experimental evidence that the extent to which a referendum is accepted affects the individual's perceived legitimacy of the political decision.

Of course, the acceptance rate at the municipal level is not an entirely exogenous signal to the individuals. Still, it provides some first evidence that the strength of the signal matters and that the *social restrictions* mechanism plays an essential role in the aftermath of the veiling ban ballots.

Moreover, the *social restrictions* mechanism would not only suggest heterogeneous effects based on the signal received but also based on the individuals. The same signal may change the *social restrictions* for some individuals more strongly than others. Therefore, more insights into the potential channels driving my results can be gained when dividing the sample into right- and left-wing individuals. Individuals indicate their political position in the survey on a scale from 0 (left) to 10 (right), so I construct a sample in which only individuals with left-wing (< 5) or right-wing (> 5) views are included in the treated and control groups.

Table 3.7.5: Heterogeneous effects based on the political backgrounds of individuals.

	Repeated cross-section		Panel	
	(1)	(2)	(3)	(4)
Level	right	left	right	left
Attitudes	wing	wing	wing	wing
ATT	-0.201	-0.526***	-0.279	-0.452**
	(0.181)	(0.168)	(0.242)	(0.186)
N. Obs.	13534	10402	7304	6367

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The outcome variable *AttIslam* is reported attitudes toward Islam on a scale from 0 (very negative) to 10 (very positive). Spec. 1 & 3 include only individuals in the sample that indicate themselves as politically right wing; Spec. 2 & 4 only include individuals in the sample that indicate themselves as politically left wing.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Please note that under these restrictions, the sample used shrinks a lot. However, Table 3.7.5 provides some interesting insights. When restricting the sample to self-indicated right-wing individuals (Spec. 1 & 3), the negative effect of the votes on the reported

attitudes toward Islam becomes smaller, losing statistical significance compared with the baseline estimations. However, when restricting the sample to self-indicated left-wing individuals (Spec. 2 & 4), the effect remains statistically significant and becomes larger than the baseline estimates. Please note that this difference is not statistically significant. This is consistent with the interpretation that left-wing individuals face stronger barriers to expressing an unfavorable opinion toward Islam before the vote. For this group, the pre-vote social restrictions were presumably larger. These findings, therefore, provide more evidence that the *social restrictions* mechanism in the aftermath of the controversial votes plays an essential role in the reported attitudes of the individuals.

Another possibility to learn more about the *social restrictions* effects is to look at potential answers for hiding one's preferences. Individuals may choose this option when their attitude conflicts with the perceived social norm (see, e.g., Gerling and Kellermann, 2022). A strategy for hiding the attitude is to indicate "don't know" or report the neutral middle option (=5) in the survey. Therefore, I change the outcome variable in the multiple DiD estimations to the variable *NeutralAtt* which indicates whether an individual has reported "don't know" or the middle option on their attitude toward Islam. I perform these estimations for the whole sample, the left-wing sample, and the right-wing sample. The reporting of *NeutralAtt* should be especially relevant again for the individuals who faced presumably strong *social restrictions*, that is, the left-wing voters.

All coefficients in Table 3.B.12 show negative signs. However, only for the self-indicated left-wing individuals (Spec. 6) is the coefficient statistically significant at the 5% level and larger in size. After the vote, left-wing individuals from the treated cantons reported a neutral attitude toward Islam with an approximately 17 percentage points lower probability than left-wing individuals from control cantons. However, the coefficient is not statistically significantly larger than the estimated coefficients for the other individuals. Please note again that, under this sample restriction, the sample becomes relatively small. The fact that there are heterogeneous results based on political positions gives some more evidence in favor of a *social restrictions* effect. However, it cannot be disentangled entirely from the *information* channel, which will be discussed in the following section.

Information − Are individuals more aware of their own attitudes?

The change in the reported attitudes toward Islam after the votes on face-veiling bans can be driven by social discourse, which increases individuals' *information* on a topic. People learn about their preferences and become more knowledgeable (see Section 3.4).[25] On

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[25]I further planned to look at the opinion polls from GFS, which were conducted before the federal vote on the face-veiling ban. This would allow for checking the vote intentions of individuals from cantons who have already voted on this topic compared with individuals who have not voted on the topic before. It would be interesting to examine the vote intentions at the start of the federal discussion of individuals from the before-treated cantons compared with the before-untreated cantons—maybe the individuals who already voted on the topic on the cantonal level have a clearer opinion on it. Unfortunately, I could not receive the data with the cantonal or municipal information on the individuals from GFS.

the one hand, it could be that, through social discourse, attitudes in society move closer together and are less polarized. On the other hand, direct democracy has the potential to make voters better informed and knowledgeable (e.g. Benz and Stutzer, 2007; Tolbert et al., 2003). Therefore, individuals who did not have clear opinions before the vote on veiling bans might learn more about their own attitudes and, therefore, move away from the more neutral opinions—thus leading to more polarized answers.

One straightforward test to better understand these channels would be to examine the evolution of the reported neutral options (*NeutralAtt*) in the survey. However, individuals' answers may not be sincere but instead adapted to the new social restriction, as discussed in the previous section. Moreover, because I found heterogeneous results for the outcome *NeutralAtt* by the political backgrounds (see Table 3.B.12), this does not seem to be the sole *information* mechanism.

Therefore, one can look at environments and individuals potentially treated differently by the discussions of direct-democratic votes. A potential measure of the intensity of the discourse in an individual's background provides the *municipal turnout* of the cantonal veiling ban ballots. This provides the possibility of analyzing subsamples because individuals from environments with high or low discussion intensity are assumed to be treated differently by the direct-democratic vote. The results of the estimations, where I split the sample into *high turnout* and *low turnout* groups are depicted in Table 3.B.13 in the Appendix. I do not find systematic size or statistical significance differences between the two municipal characteristics.

Another test focuses on individual characteristics that may lead to heterogeneous effects by the *information* mechanism. Who are the individuals not informed before but who might be informed more after the vote on veiling bans? The ones who should increase their information about the topic the most are, in general, politically uninterested individuals. Therefore, I check whether there are heterogeneous effects by the degree of political interest that individuals indicate in the survey on a scale from 0 (not at all interested) to 10 (very interested). I divide the sample into three: the highly politically interested (> 5), the medium politically interested ($=5$), and the lowly politically interested individuals (< 5).

The results for the baseline multiple DiD estimations in the subpopulations by political interest are shown in Table 3.7.6. Interestingly, the coefficients align with the expectations: the ATT of the face-veiling ban votes on the reported attitudes toward Islam are more pronounced for individuals with low political interest than for those with high political interest. There are at least two explanations for this finding. First, the highly interested individuals might already have had a clearer opinion on the issue before the vote—therefore, the social discourse affects them less. Second, individuals with a low political interest may be more easily influenced through extreme campaigns (see, e.g., Figure 3.C.1)—because this may, for instance, increase their perceived cultural threat (e.g., Hainmueller and Hiscox, 2007; Helbling and Traunmüller, 2016).

Table 3.7.6: Heterogeneous effects based on the political interest of individuals.

	Repeated cross-section			Panel		
	(1)	(2)	(3)	(4)	(5)	(6)
Level	high	medium	low	high	medium	low
Attitudes	interest	interest	interest	interest	interest	interest
ATT	-0.284**	-0.488*	-0.508***	-0.300**	-0.694	-0.454**
	(0.111)	(0.292)	(0.182)	(0.119)	(0.492)	(0.208)
N. Obs.	23438	5648	14121	15432	1720	7724

Notes: Estimations are based on Sant'Anna and Zhao's (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The outcome variable *AttIslam* is reported attitudes toward Islam on a scale from 0 (very negative) to 10 (very positive). Samples are based on the indicated political interest on a scale from 0 (very low) to 10 (very high) of the respondents: high interest (> 5), medium interest ($= 5$), and low interest (< 5).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.8 Conclusion

Democratic decision-making is often seen as a means to aggregate individual preferences in society. However, elections or votes may also affect the attitudes and perceived social norms in a society (e.g., Bursztyn et al., 2020a; Jung and Tavits, 2021). An important question in today's diverse societies is how voting and introducing laws that restrict the civil rights of minorities—whether it is sexual, ethnic, or religious—affects society's view of these groups. In this chapter, I analyzed whether direct-democratic ballots on face-veiling bans affect citizens' attitudes toward Islam. To do so, I made use of a unique setting: in Switzerland, individuals from two cantons voted on veiling bans on the cantonal level. In contrast to previous studies, I analyze variations at the subnational level, which naturally provides comparison groups. Combining the events of referendum votes with survey data on attitudes toward Islam allows for the use of a multiple DiD strategy (Callaway and Sant'Anna, 2021).

I provide evidence showing that individuals from jurisdictions voting on and accepting face-veiling bans reported systematically different attitudes toward Islam. The treated individuals indicated (i) more negative attitudes and (ii) more polarized attitudes toward Islam. To tackle potential endogeneity problems, I performed several robustness checks, in addition to the baseline multiple DiD estimates and a matching procedure combined with the DiD strategy. The results remain mostly robust in size and significance and suggest that the votes on face-veiling bans led people to report more negative and polarized opinions toward Islam. The reported stronger negative attitudes toward Islam are in line with the findings from the positive effects of elections of populists and xenophobic views, which have, for instance, been explained by a shift of social norms (e.g., Bursztyn et al., 2020a). My results suggest that the change in attitudes toward Islam after the votes on veiling bans can, on the one hand, be driven by people who became more informed in the democratic discourse and individuals adapting to the "new" social norm.

These findings add insights to the discussion on how democracy affects societies—and minorities—beyond the instrumental outcomes. Previous studies have mainly analyzed the effects of elections (e.g., Bursztyn et al., 2020a; Gerling and Kellermann, 2022) and the literature on attitudes in the aftermath of referendums is relatively scarce (e.g., Dinas et al., 2024; Jung and Tavits, 2021). Moreover, to the best of my knowledge, previous literature has focused on countries where direct-democratic instruments are not well established. My research adds the insight that, also in direct-democratic equilibrium, the votes themselves further shape societies.

In the future, it would be promising to further analyze the different mechanisms behind the effects of democratic decision-making on individual attitudes. Are people genuinely changing their attitudes because of social discourse, or are they becoming more aware of their preferences? Are they adapting their responses to fit the new social norm? Another avenue worth exploring is the differentiation between the short- and long-term effects of democratic decisions on society. Do the effects persist over time, and what happens when individuals are confronted with the same issues multiple times? By gaining more insights into the persistence and the dynamics of these effects, one can also better understand how political actors, such as governments or parties, can use democratic instruments not only to shape societies but also their chances of (re)election. Finding answers to these questions will pave the way for promising research in the future.

Appendix

Chapter 3 Appendix

3.A   Additional information

Popular initiative in Ticino, 2013
In the canton of Ticino, a popular initiative has led to the vote on a veiling ban. The electorate in Ticino voted on the following legislation:[1]

> ***Art. 9a*** - Divieto di dissimulazione del proprio viso
> [1] Nessuno può dissimulare o nascondere il proprio viso nelle vie pubbliche e nei luoghi aperti al pubblico (ad eccezione dei luoghi di culto) o destinati ad offrire un servizio pubblico.
> [2] Nessuno può obbligare una persona a dissimulare il viso in ragione del suo sesso.
> [3] Le eccezioni al primo capoverso e le sanzioni sono stabilite dalla legge.

Optional referenda in St. Gallen, 2018
In the canton of St. Gallen, an optional referendum has led to the vote on a veiling ban. The electorate in St. Gallen voted on the following legislation:[2]

> ***Art. 12ter (neu)*** - Gesichtsverhüllungsverbot
> [1] Wer sich im öffentlichen Raum sowie an Orten, die öffentlich zugänglich sind, durch Verhüllung des Gesichts unkenntlich macht und dadurch die öffentliche Sicherheit oder den religiösen oder gesellschaflichen Frieden bedroht oder gefährdet, wird mit Busse bestraft.

Hence, in both votes, the legislation at hand was about a ban on face covering in public places. However, in the media and also in the political discussion beforehand, the focus has mostly been set on the veiling ban. The media called the new laws mostly the "Burqa-Bans" and, therefore, directly targeted the group of Muslims.

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[1] For more information on the vote see `https://www4.ti.ch/generale/diritti-politici/votazioni/archivio/archivio-votazioni-cantonali-federali-e-comunali`. (Last accessed: 16 February 2024)

[2] For more information on the vote see `https://wab.sg.ch/vote/iii-nachtrag-zum-ubertretungsstrafgesetz/entities`. (Last accessed: 16 February 2024)

3.B   Additional tables

Table 3.B.1: Yearly observations of the treated and untreated cantons.

# obs.	**2013**	**2016**	**2019**	**2022**
St. Gallen	315	523	415	562
Ticino	205	360	283	454
Others	5448	8229	6597	9416

Note: The surveys take place between the fall prior and spring after the indicated year.

Table 3.B.2: Descriptive statistics on the municipal level.

| | Treated Cantons | | | | | Untreated Cantons | | | | |
	Obs.	Mean	SD	Min	Max	Obs.	Mean	SD	Min	Max
Variables										
YesMinaret	2,798	66.46	7.47	51.07	82.08	28,911	56.32	11.71	28.40	93.00
TurnoutMinaret	2,798	52.08	4.35	40.22	65.93	28,911	54.22	5.51	34.50	80.00
YesBurka	3,055	55.83	7.70	37.34	71.91	29,457	51.12	9.31	27.15	89.09
TurnoutBurka	3,055	47.93	3.99	34.49	61.11	29,457	52.61	6.87	26.84	81.39
PopTot	3,053	18346	23775	167	76778	29,448	39353.92	88134	92	426753
ForeignShare	3,053	24.37	9.21	4.51	58.68	29,448	23.85	10.84	0.00	63.19
MarriedShare	3,053	43.59	2.96	34.95	51.73	29,448	42.47	4.40	26.33	55.00
PensionerShare	3,053	19.58	3.37	11.15	39.67	29,448	18.09	3.21	6.34	35.20

Note: The number of observations is not the same for all variables due to municipal mergers that took place during the observation period.

Table 3.B.3: Test for parallel pre-trends of the reported attitudes toward Islam.

	Repeated cross-section			
	Level		Polarization	
	(1)	(2)	(3)	(4)
	Baseline	Controls	Baseline	Controls
chi2	2.721	0.203	0.000	0.572
p-value	0.099	0.6523	0.991	0.449
Number of Observations	31911	27193	31911	27193
	Panel			
	Level		Polarization	
	(5)	(6)	(7)	(8)
	Baseline	Controls	Baseline	Controls
chi2	0.258	0.328	0.306	0.197
p-value	0.612	0.567	0.580	0.658
Number of Observations	22053	21760	22053	21760

Table 3.B.4: Test for parallel pre-trends of the indicated political position.

	Cross-section		Panel	
	Level	Polarization	Level	Polarization
chi2	30.0263	18.8918	21.8491	27.1569
p-value	0.3130	0.8739	0.7450	0.4554
Number of Observations	53'201	53'201	45'282	45'282

Table 3.B.5: Acceptance levels of the vote on a minaret ban (2009).

Canton	Eligible voters	Turnout	Yes Share
Geneva	235'497	57.7	40.3
Vaud	401'595	52.8	46.9
Basel-City	113'740	58.0	48.4
Neuchâtel	108'652	53.9	49.3
Jura	50'030	49.8	51.2
Zurich	864'730	54.9	51.8
Fribourg	179'912	51.5	55.9
Zug	70'903	61.9	56.7
Valais	200'822	61.1	58.0
Grisons	133'871	46.7	58.6
Basel-Country	185'704	52.1	59.9
Berne	707'025	52.0	60.8
Lucerne	253'818	54.1	61.2
Obwald	24'680	61.0	62.4
Nidwald	30'006	57.0	62.8
Schaffhausen	49'000	70.2	63.5
Appenzell Outer-Rhodes	37'387	58.2	63.7
Uri	25'949	52.0	63.8
Solothurn	170'721	56.3	64.0
Argovia	389'964	52.0	64.0
St. Gall	305'694	53.8	65.9
Schwyz	96'397	51.6	66.3
Thurgovia	157'273	54.0	67.7
Ticino	209'452	49.4	68.3
Glarus	25'750	46.9	68.8
Appenzell Inner-Rhodes	11'138	49.7	71.4

Table 3.B.6: Indicated religious tolerance towards covering the head (1999).

Canton	# Respondents	Avg.	Median	SD
Neuchâtel	508	3.73	3	3.89
Geneva	374	3.97	3.5	4.08
Vaud	698	4.16	4	4.12
Jura	3	4.67	4	5.03
Ticino	314	4.75	5	4.34
Valais	270	4.77	5	4.15
Fribourg	297	5.10	5	3.97
St. Gallen	441	5.45	6	3.94
Solothurn	283	5.58	6	3.91
Schwyz	111	5.58	6	4.08
Argovia	651	5.74	6	3.94
Appenzell Inner-Rhodes	4	5.75	6.5	5.06
Thurgovia	215	5.75	6	4.02
Zurich	1231	5.82	7	4.05
Appenzell Outer-Rhodes	54	5.83	6.5	3.79
Zug	89	5.96	7	3.90
Basel-Country	278	6.01	7	4.00
Schaffhausen	76	6.11	7	3.84
Berne	868	6.12	8	3.99
Basel-City	201	6.17	8	4.18
Lucerne	389	6.20	8	3.95
Grisons	166	6.49	8	3.86
Glarus	37	6.84	8	3.71
Nidwald	23	7.09	10	4.13
Uri	24	7.13	8.5	3.72
Obwald	39	7.28	8	3.40

Table 3.B.7: ATT and period estimates of the baseline DiD.

	Panel		Repeated cross-section	
	(1) AttIslam	(2) AttIslamPolar	(3) AttIslam	(4) AttIslamPolar
ATT	-0.328***	0.201	-0.347***	0.197**
	(0.124)	(0.123)	(0.110)	(0.092)
Pre_avg	-0.149	-0.057	0.306*	-0.001
	(0.294)	(0.103)	(0.186)	(0.070)
Post_avg	-0.444***	0.253*	-0.363***	0.223**
	(0.111)	(0.140)	(0.125)	(0.106)
Tm1	-0.149	-0.057	0.306*	-0.001
	(0.294)	(0.103)	(0.186)	(0.070)
Tp0	-0.319**	0.129	-0.400***	0.195**
	(0.156)	(0.120)	(0.091)	(0.077)
Tp1	-0.151	0.201	-0.262**	0.133
	(0.138)	(0.145)	(0.118)	(0.083)
Tp2	-0.861***	0.430*	-0.426*	0.342*
	(0.205)	(0.244)	(0.246)	(0.190)
N. obs.	22053	22053	31911	31911

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The variable AttIslam ranges from 0 (very negative) to 10 (very positive) and the variable AttIslamPolar from 0 (middle option) to 5 (extreme option). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; the canton of GL has thus been excluded.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.B.8: ATTs robust to the exclusion of the 2021 survey.

	Repeated cross-section		Panel	
Attitudes Islam	(1) AttIslam excl. 2021	(2) AttIslamPolar excl. 2021	(3) AttIslam excl. 2021	(4) AttIslamPolar excl. 2021
ATT	-0.411***	0.223**	-0.363***	0.212
	(0.101)	(0.106)	(0.122)	(0.134)
N. Obs.	21830	21830	17182	17182

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The continuous outcome variables refer to indicated attitudes toward Islam on a scale 0 (very negative) to 10 (very positive) and to the polarization of answers on a scale from 0 (middle attitude) to 5 (extreme attitude). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; the canton of GL has thus been excluded. The survey of the year 2021 is excluded from the sample.
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.B.11: ATT placebo outcomes - polarization other religions.

	Cross-section					
Polarization Attitudes	(1) Islam	(2) Christianism	(3) Hinduism	(4) Buddhism	(5) Judaism	(6) Atheism
ATT	0.197**	-0.089	0.103	0.212**	0.079	0.036
	(0.092)	(0.091)	(0.087)	(0.096)	(0.083)	(0.070)
N. obs.	31911	33424	28603	30065	30497	30263
	Panel					
Polarization Attitudes	(1) Islam	(2) Christianism	(3) Hinduism	(4) Buddhism	(5) Judaism	(6) Atheism
ATT	0.201	-0.059	0.089	0.106	0.075	-0.053
	(0.123)	(0.075)	(0.072)	(0.098)	(0.079)	(0.084)
N. obs.	22053	23329	19046	20371	20930	20691

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The continuous outcome variables range from 0 (middle option) to 5 (extreme option) in attitudes toward different religions. The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; the canton of GL has thus been excluded.
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.B.9: General robustness checks.

Repeated cross-section

Attitudes	Level				Polarization			
	(1) Minaret Yes	(2) Excl. French	(3) Excl. Foreign Outliers	(4) Excl. Pop. Outliers	(5) Minaret Yes	(6) Excl. French	(7) Excl. Foreign Outliers	(8) Excl. Pop. Outliers
ATT	-0.342***	-0.298***	-0.280***	-0.325***	0.195**	0.163*	0.173*	0.176*
	(0.108)	(0.108)	(0.090)	(0.108)	(0.093)	(0.093)	(0.104)	(0.097)
N. obs.	22193	23650	26970	27170	22193	23650	26970	27170

Panel

Attitudes	Level				Polarization			
	(1) Minaret Yes	(2) Excl. French	(3) Excl. Foreign Outliers	(4) Excl. Pop. Outliers	(5) Minaret Yes	(6) Excl. French	(7) Excl. Foreign Outliers	(8) Excl. Pop. Outliers
ATT	-0.338***	-0.319**	-0.293**	-0.269*	0.195	0.175	0.162	0.192
	(0.123)	(0.125)	(0.136)	(0.138)	(0.122)	(0.123)	(0.141)	(0.139)
N. obs	15038	16326	18815	18971	15038	16326	18815	18971

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The variable AttIslam ranges from 0 (very negative) to 10 (very positive) and the variable AttIslamPolar from 0 (middle option) to 5 (extreme option). The robustness checks include: Restricting the sample to the municipalities that voted yes in the minaret vote in 2009 (Minaret Yes), excluding the french-speaking cantons (Excl. French), excluding the municipalities with the highest and lowest 5 % foreign shares (Excl. Foreign Outliers), excl. the 5% largest and smallest municipalities (Excl. Pop. Outliers).
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.B.10: Different clustering of the standard errors.

Repeated cross-section

Attitudes	Level			Polarization		
	(1) district level	(2) cantonal level	(3) no clustering	(4) district level	(5) cantonal level	(6) no clustering
ATT	-0.347***	-0.294***	-0.294**	0.197**	0.176**	0.176**
	(0.110)	(0.111)	(0.118)	(0.092)	(0.086)	(0.087)
N. obs.	31911	32705	32705	31911	32705	32705

Panel

Attitudes	Level			Polarization		
	(1) district level	(2) cantonal level	(3) individual level	(4) district level	(5) cantonal level	(6) individual level
ATT	-0.328***	-0.281	-0.281***	0.201	0.207	0.207**
	(0.124)	(0.213)	(0.103)	(0.123)	(0.134)	(0.087)
N. obs	22053	22487	22487	22053	22487	22487

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the cantonal, district, or individual level. The variable AttIslam ranges from 0 (very negative) to 10 (very positive) and the variable AttIslamPolar from 0 (middle option) to 5 (extreme option).
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 3.B.12: Reporting neutral attitudes and political position.

	Repeated cross-section			Panel		
Neutral Attitudes	(1) whole sample	(2) right wing	(3) left wing	(4) whole sample	(5) right wing	(6) left wing
ATT	-0.041	-0.046	-0.075	-0.041	-0.013	-0.168**
	(0.028)	(0.035)	(0.049)	(0.035)	(0.062)	(0.072)
N. Obs.	34007	14674	10877	23714	7993	6690

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The dummy outcome variable *NeutralAtt* indicates if individuals reported "don't know" or the neutral middle option in the survey (=1). The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; the canton of GL has thus been excluded.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 3.B.13: Reported attitudes toward Islam and turnout in the cantonal vote.

	Repeated cross-section			
	Level		Polarization	
AttIslam & AttIslamPolar	(1) high turnout	(2) low turnout	(3) high turnout	(4) low turnout
ATT	-0.339**	-0.388**	0.223**	0.212*
	(0.146)	(0.181)	(0.103)	(0.118)
N. obs.	30662	30430	30662	30430
	Panel			
	Level		Polarization	
AttIslam & AttIslamPolar	(5) high turnout	(6) low turnout	(7) high turnout	(8) low turnout
ATT	-0.305**	-0.357*	0.185*	0.187
	(0.125)	(0.205)	(0.185)	(0.201)
N. obs.	21410	21217	21410	21217

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The sample is split into two groups: High turnout refers to the municipalities with a higher than median turnout rate and low turnout refers to the municipalities with a lower than median turnout rate. The treated cantons are TI (2013) and SG (2018). The comparison group includes all cantons without cantonal votes on veiling bans; the canton of GL has thus been excluded.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

3.C   Additional figures

Figure 3.C.1: Public poster for the veiling ban in the federal vote (2021).
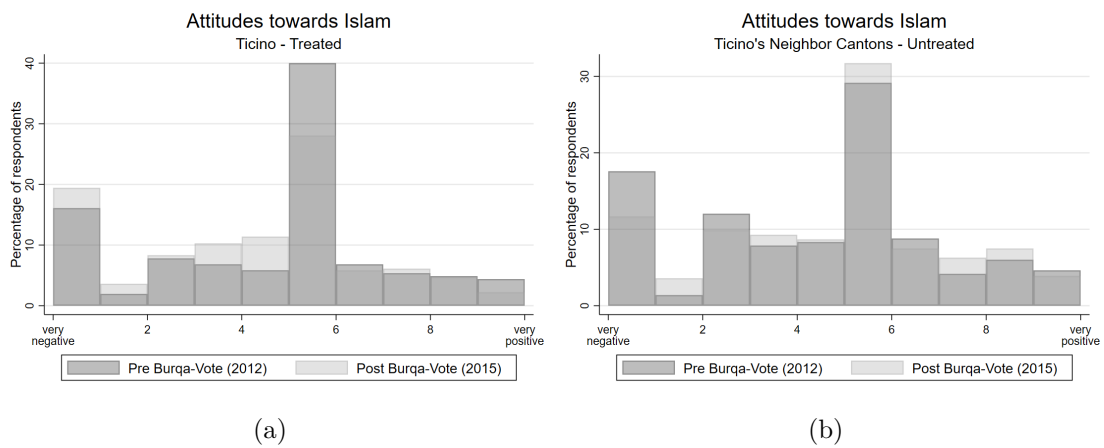

(a)
(b)

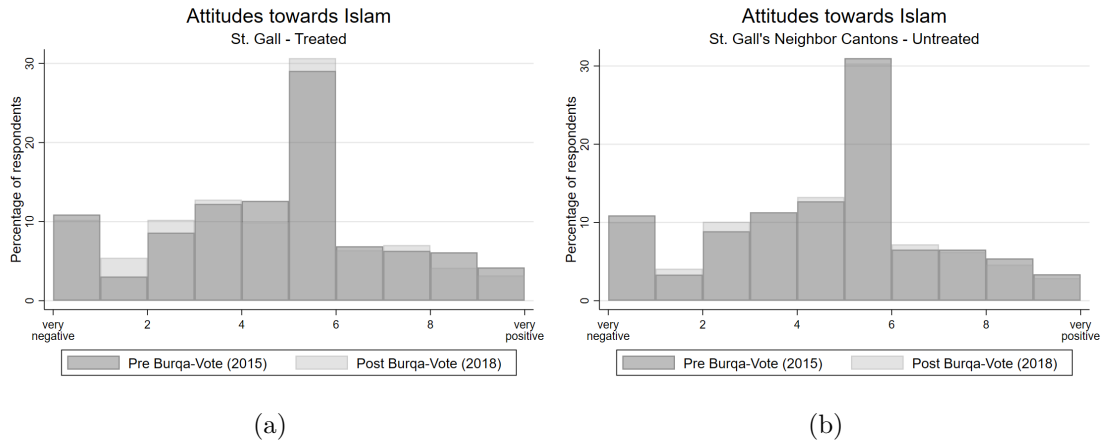Figure 3.C.2: Histograms of reported attitudes toward Islam - TI and neighbors.

Figure 3.C.3: Histograms of reported attitudes toward Islam - SG and neighbors.
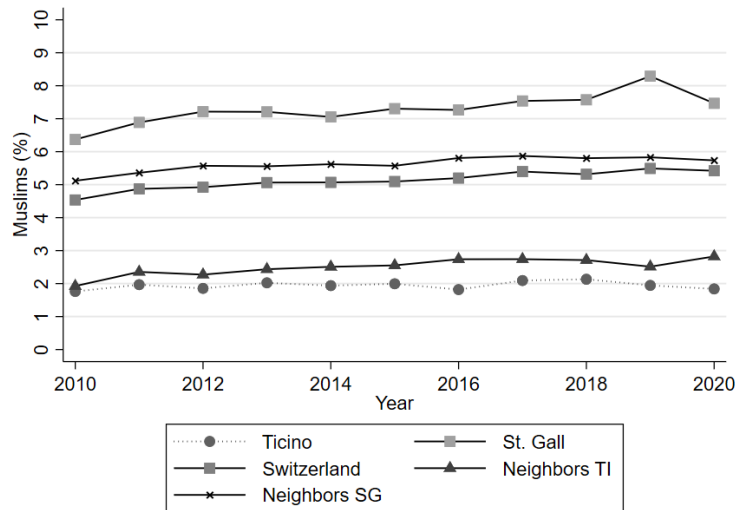
Figure 3.C.4: Evolvement of the Muslim share in the population over 15 years old.

Note: Data is taken from the Federal Statistical Office. The numbers have to be interpreted with caution since they come from a random sample survey and have large confidence intervals.
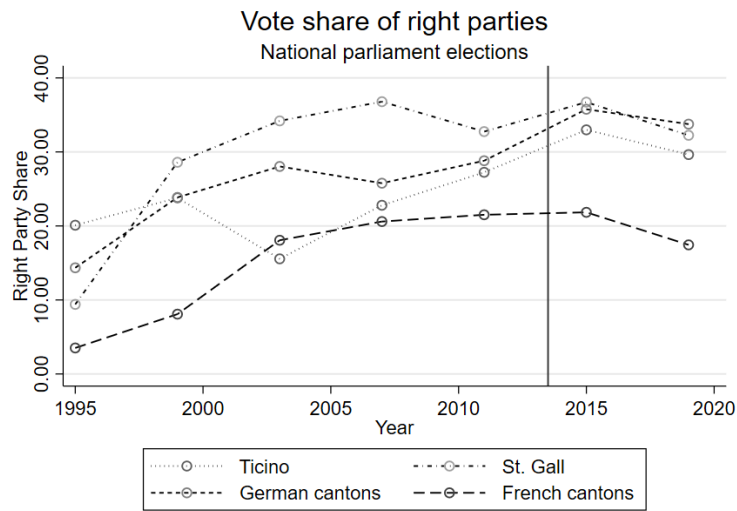
Figure 3.C.5: Evolvement of the right-wing vote share in national elections.

Note: Data is taken from the Federal Statistical Office. The right-wing party-share consists of the vote shares for the Swiss Peoples Party (SVP), the Lega, the MCG, and the EDU.
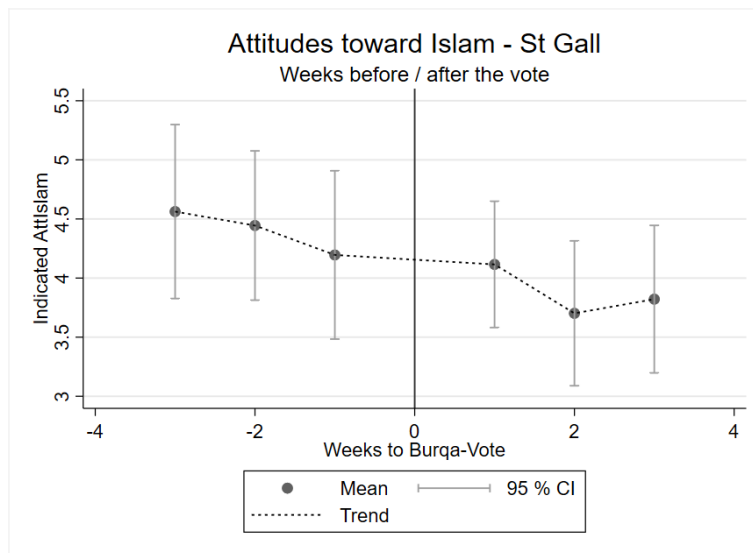

Figure 3.C.6: Development of indicated attitudes toward Islam in St. Gallen (pre/post).

Reported attitudes towards Islam - Level — Panel

(a)

Reported attitudes towards Islam - Level — Repeated cross-section

(b)

Reported attitudes towards Islam - Polarization — Panel

(c)

Reported attitudes towards Islam - Polarization — Repeated cross-section

(d)
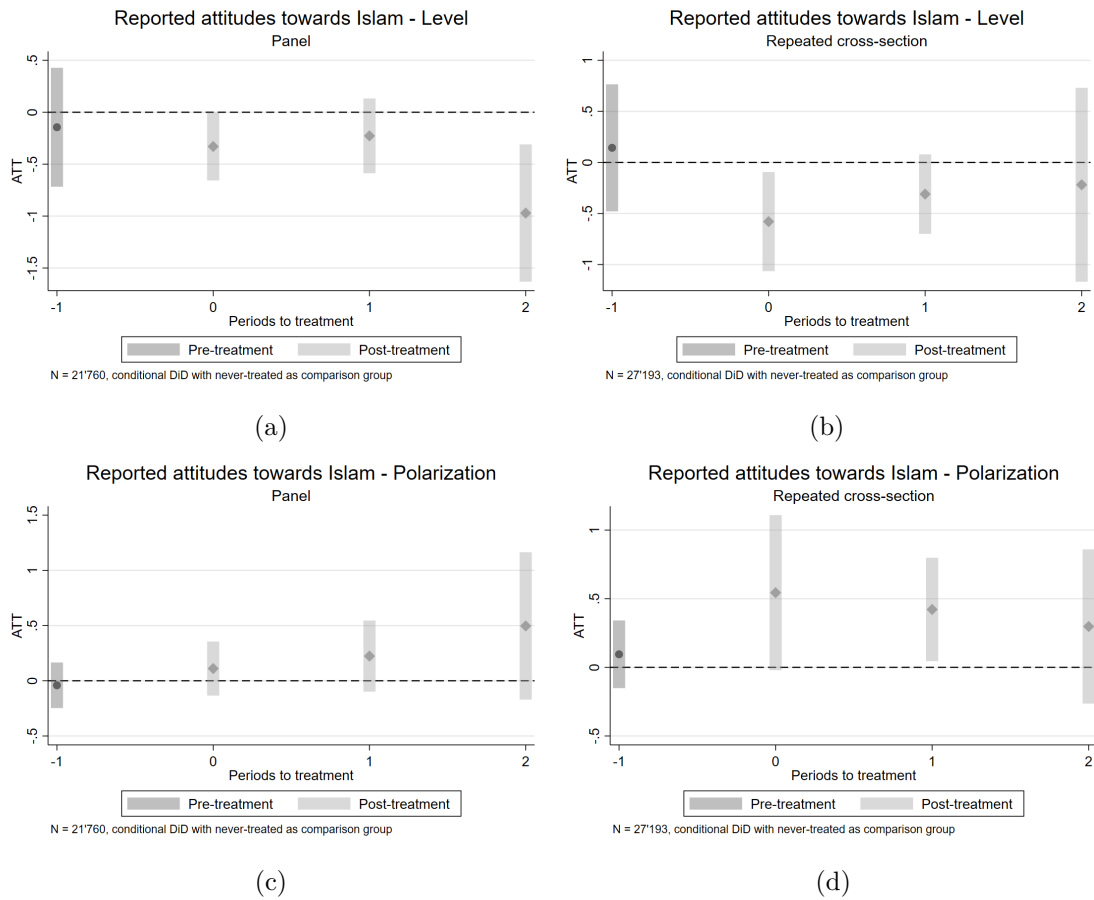
Figure 3.C.7: Event study on reported attitudes toward Islam with control variables.

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The treated cantons are TI (2013) and SG (2018). In the panel estimations, time-variant controls at the municipal level are introduced (PopTot, ForeignShare, MarriedShare, PensionerShare). The repeated cross-section estimations include additional control on the municipal (MinaretYes, MinaretTurnout, BurkaYes, BurkaTurnout) and on the individual (sex, age, education, Swiss, PolitInterest, PolitPosition, Protestant, Catholic, Muslim).
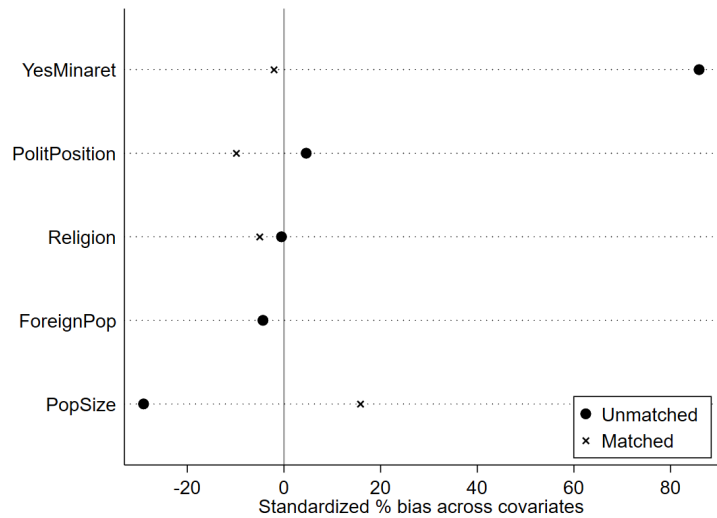
Figure 3.C.9: Balance of the matched compared to the unmatched sample.

Note: The sample has been matched on the pre-treatment variables *MinaretYes, PolitPosition, Religion, ForeignShare*, and *PopSize*.

(a)

(b)

(c)

(d)

120
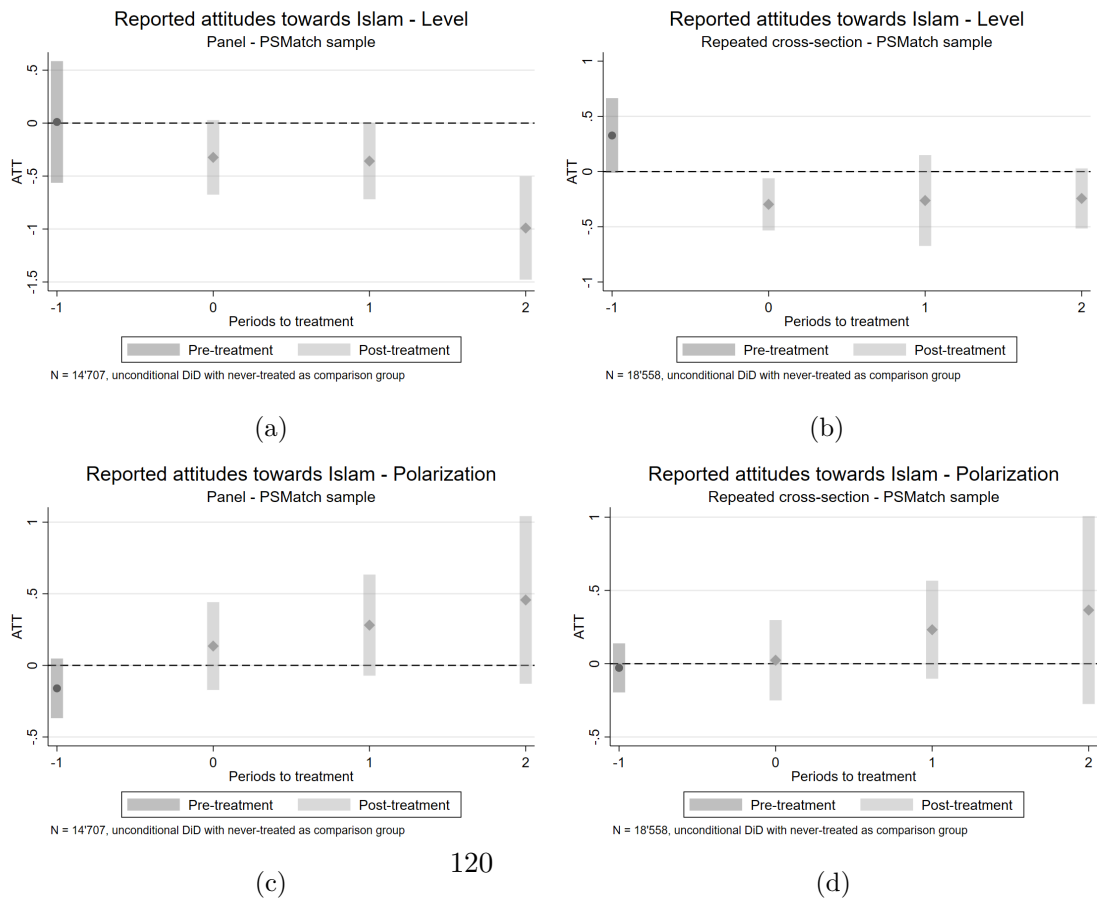
Figure 3.C.10: Reported attitudes toward Islam with a propensity score balanced sample.

Notes: Estimations are based on the Sant'Anna and Zhao (2020) improved doubly robust DiD estimator. Standard errors are clustered at the district level. The treated cantons are TI (2013) and SG (2018). The sample is restricted to observations based on the propensity scores on the following variables: MinaretYes, PopTot (log), ForeignShare, religion, PolitPosition.

Chapter 4

Male and female turnout after women's suffrage – Evidence from Switzerland*

4.1 Introduction

Many democracies are confronted with two trends: on the one hand, a decline in voter turnout and, on the other hand, ongoing discussions about the enlargement of the electorate. However, how do turnout and suffrage extensions interact? To answer this question, it is essential to distinguish between the turnout of the old and new electorates. While the new electorate represents an inexperienced group of voters who first have to get accustomed to the voting habit (e.g., Coppock and Green, 2016), the old electorate is affected as well; besides the decreasing chance that they are casting the decisive vote (e.g., Downs, 1957), the political sphere changes with the enlargement of the electorate. This chapter seeks to answer how the turnout rates of the old (male) and new (female) electorates evolve after the enfranchisement of women and how that differs by societal and institutional backgrounds.

Because turnout data is not typically separated by different subsets of voters in most countries, these questions are often difficult to answer. In this chapter, we overcome this problem by using rich gender-specific turnout data from Switzerland after the enfranchisement of women. The Swiss setting, characterized by its federal and direct-democratic institutions, provides interesting variations in time and between jurisdictions. First, Swiss women's suffrage came relatively late (in 1971) at the national level, but different cantons introduced it earlier (9 out of 25). The canton of Vaud was the first to

introduce female suffrage in 1959. Overnight, the pure male sphere changed to a mixed-gender sphere for different cantons and at different points in time. Second, because of the extensive federalism and decentralization in Switzerland, there are differences in the societal and institutional backgrounds faced by the new (female) and old (male) electorates. Municipalities use different political institutions—either a direct-democratic town meeting or representative democratic parliament—as their legislative. Moreover, we have information on the preferences of the male electorate of a municipality toward female enfranchisement because the yes shares on a vote on female suffrage in 1959 are available for all Swiss municipalities. The staggered introduction of female suffrage and the municipal differences allow us to analyze how institutional and societal factors affect the turnout of the old and new electorates.

We find that male turnout at the federal level dropped after the introduction of female suffrage at the cantonal level. The effect is sizable and amounts to around five percentage points. Importantly, in our setting, the chance that a voter casts the decisive vote (e.g., Downs, 1957) on the federal level is not touched by the cantonal introduction of female suffrage. Hence, alternative mechanisms need to be examined. Because we find that the turnout drop is more pronounced in municipalities where men's opposition against female suffrage was initially stronger, one possible mechanism is that the turnout decline results from a change in the consumption value that men draw from politics. The shift in politics from a "pure male sphere" to a "mixed-gender sphere" can affect men's identity benefits that they receive from participating in politics (e.g., Akerlof and Kranton, 2000; Rogers et al., 2013). Moreover, male turnout decreased more in municipalities with town meetings, where the electorate meets two to four times a year to decide on municipal topics. We find that the turnout of newly enfranchised women differs regarding their municipal backgrounds as well. Shortly after female enfranchisement, women turned out less in contexts where men's opposition toward female suffrage was more considerable. The same is the case for the direct-democratic town meeting backgrounds. This effect, however, flips over time. This provides evidence that participation-enhancing institutions—such as, in our case, the direct-democratic background—may exert differential effects on the short- and long-term turnout of the old and new electorate.

In the past century, in many democracies, voting was extended to all male or female citizens passing an age threshold. Today, suffrage extensions—especially to non-citizens or younger citizens—are on the political agenda in many countries. This led scholars to exploit the conditions for sharing political power (Acemoglu and Robinson, 2000; Aidt and Franck, 2015; Aidt and Jensen, 2014; Bertocchi, 2011; Braun and Kvasnicka, 2013; Kayran and Erdilmen, 2020; Koukal et al., 2021; Stutzer and Slotwinski, 2021). Moreover, its effects on outcomes such as social spending (e.g., Aidt and Dallal, 2008), fiscal outcomes (e.g., Aidt et al., 2006), the emancipation of the enfranchised group (Slotwinski and Stutzer, 2023), attitudes of the established electorate (Koukal and Portmann, 2020), naturalization propensity (Slotwinski et al., 2023), and many others have been analyzed. However, how do suffrage extensions and turnout relate?

The decision to turn out can be explained with instrumental and expressive costs and benefits. From an instrumental point of view, the inflow of new voters into the electorate decreases the probability that the established electorate is pivotal in changing the outcome. For new voters, the costs of participating might be higher because they must first get used to the habit of voting (e.g. Coppock and Green, 2016). However, the paradox of voting (Downs, 1957) states that, for a rational, self-interested individual, the costs of voting will usually exceed the instrumental benefits. This has led scholars to discover other reasons for the turnout of citizens (e.g., Riker and Ordeshook, 1968). An important explanation has since been attributed to the expressive utility (or consumption value) of voting; voters care not only about the instrumental but also about the expressive utility of their vote (see, e.g., Brennan and Hamlin, 2000; Fujiwara et al., 2016; Hillman, 2010). One way of expressive utility voters can derive from voting is "[...] what the act displays about their identities." (Rogers et al., 2013, p. 99).[1] Suffrage extensions can lead to a swift change in the identity attached to politics, hence affecting the consumption value of voting.

Turnout in the aftermath of female enfranchisement has been analyzed in different countries and democratic institutions. However, scholars frequently encounter the problem that gender-specific turnout data are not available (e.g., Corder and Wolbrecht, 2006) and, therefore, must rely on (potentially biased) survey data (see, e.g., Stockemer and Sundstrom, 2023) or make use of ecological inference procedures (Corder and Wolbrecht, 2006). For Scandinavian states, gender-specific turnout data are available. Different factors, such as the electoral system (e.g., Skorge, 2021; Teele, 2023), the extent of direct democracy (Kim, 2019), or electoral competition (Morgan-Collins, 2023) are relevant in explaining the turnout of newly enfranchised women. Moreover, for the US, Morgan-Collins (2021) finds that social movements play an essential role, and Wolbrecht and Corder (2020) stress the importance of state legislation in bringing freshly enfranchised women to the polls. In sum, the focus of the literature has mostly been on the turnout of newly enfranchised women. In contrast, we also systematically analyze the evolution of male turnout after introducing female enfranchisement, which has received less scholarly attention. For the US, Kleppner (1982) questions how male turnout evolved after female enfranchisement. The author looks at the staggered introduction of female suffrage in US states and performs a before–after comparison of turnout values at the county level—however, because Kleppner (1982) cannot disentangle turnout by gender, he does not explicitly study the influence of female enfranchisement on male turnout. For Scandinavia, scholars dispose of gender-specific turnout data, but the effect of the female enfranchisement *per se* on male turnout has not been analyzed because the enlargement usually happens simultaneously for all regions in a country. To the best of our knowledge, we are the first to analyze in detail how suffrage extensions affect the turnout of the old (male) electorate.

Hence, the contribution of this chapter is twofold. First, we contribute to the literature on enfranchisement and democratization by examining the impact of suffrage ex-

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[1]See also Akerlof and Kranton (2000), who discuss the role of identity in an individual's decision-making and its externalities on other individuals.

tensions on the established electorate. A clearer understanding of how the enlargement of the electorate may impact current voters is essential, particularly when considering potential suffrage extensions in the future. Second, we contribute insights from newly collected gender-specific turnout data, enabling an analysis of how different institutional and societal backgrounds influence the turnout of established and new voters. Therefore, we add to the literature on determinants of turnout and the gender turnout gap.

This chapter delves into the interactions of societal backgrounds and political institutions and their effect on the functioning of democracy (see Channel E in Figure 1). The chapter proceeds as follows: Section 4.2 provides an overview of the Swiss institutional background and the history of female enfranchisement. Section 4.3 offers a theoretical background on the development of male and female turnout after introducing women's suffrage. We explain our data in Section 4.4 and the empirical strategies in Section 4.5. In Section 4.6, we discuss the results, and in Section 4.7, we conclude and provide an outlook.

4.2 Institutional background

Switzerland offers an ideal case for studying the female enfranchisement process and its effects on the turnout rates of the old and new electorates. This is because of the strong degree of federalism and direct democracy, which led to a staggered introduction of female suffrage and interesting (institutional) variations between jurisdictions.

4.2.1 Female suffrage in Switzerland

Swiss women were enfranchised substantially later compared with women in other European countries, such as Germany (where women were enfranchised in 1918), Austria (1919), or France (1944). In Switzerland, women's suffrage was granted in 1971 by the existing electorate—Swiss men above 20—while in most other countries, the national parliament or executive decided on the enfranchisement.

Moreover, because of the Swiss federalist structure, women were enfranchised at the different levels of government (federal, cantonal, and municipal) separately. Although federal female suffrage had to be implemented by a national vote, cantons could independently determine whether they wanted to enfranchise women on the cantonal and municipal levels or if they wanted to delegate the decision to enfranchise women to the electorate of individual municipalities (opt-in rule). As a result, approximately 100 referendum votes on female enfranchisement took place at different levels of government between 1919 and 1990 in Switzerland. Two of these referendum votes were held on the national level. The first of them, held in 1959, was rejected with 66.9% of the votes. In 1971, Swiss women were enfranchised on the national level as the second federal vote was accepted by 65.7% of the (all-male) Swiss electorate (for a chronology of the votes in the different cantons, see 4.A.2 in the Appendix).

Because of this structure, the timing of women's enfranchisement substantially differs between cantons and municipalities. The first mover canton was the canton of Vaud,

where women could participate in municipal and cantonal political matters from 1959 on. The last canton was forced to enfranchise its women by the Swiss Federal Court in 1990. This staggered introduction, as shown in Figure 4.2.1, of female suffrage in Swiss cantons (see also Table 4.A.1) is one of the main variations we exploit in our empirical strategy, which is explained in Section 4.5.[2] The staggered cantonal introductions allow us to analyze the impact on male turnout in federal votes and elections, which is rare because, usually, after female enfranchisement, turnout data comprise an inseparable combination of male and female turnout. Another advantage of this setup is that men do not lose political influence in national referendum votes and national elections because, on the national level, women could only participate after 1971. Therefore, the size and composition of the respective electorate are not affected by female enfranchisement at the cantonal and local levels, and the probability that any voting man is pivotal is not changed (see Section 4.3 and Figure 4.3.1). Compared with cross-country analyses, the staggered introduction of female suffrage in the Swiss cantons offers advantages: the different cantons are embedded in the same national environment.

Indirectly, this staggered introduction of cantonal female suffrage also allows us to explore cantonal turnout data by gender, as in the canton of Vaud, the data were collected separately by gender from the introduction of female suffrage in the canton (1959) until its introduction on the federal level (1971). This offers the opportunity to track the participation rates of women and men in elections and referendums during the first decade of female suffrage.[3] Such data are rarely ever available, and we exploit these rich data set in our empirical work (see Section 4.4).

4.2.2 Local political institutions

Switzerland's extensive federalism has also led to another particularity: various political institutions can be observed on the cantonal and municipal levels. A noteworthy aspect to consider for our research question is the varying organization of the legislative branch among Swiss municipalities. The legislative institution on the local level is organized by a town meeting (direct democracy) or a parliament (representative democracy).

In some cantons, municipalities can choose freely whether to install a town meeting or parliament; in some cantons, only one or the other form is used; and in other cantons, a population threshold externally decides which institution must be installed. In 2013, around 80% of Swiss municipalities used a town meeting (Ladner, 2016). Here, a town meeting refers to organizing citizen assemblies, where all eligible voters of a municipality are invited once to four times a year to discuss and decide on municipal topics. Because the electorate can meet directly and share their views, this institution is often referred to as the prototype of direct democracy (e.g., Koukal and Eichenberger, 2017; Mueller, 2003). Parliaments, in contrast, are representative democratic institutions because voters

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[2] For a general overview of the path to female enfranchisement in Switzerland, see Ruckstuhl (1986).

[3] We were able to compile the municipal turnout data for the canton of Vaud in a reasonable amount of time. To the best of our knowledge, gender-specific data are unavailable or not easily accessible around the time of female enfranchisement for other cantons.
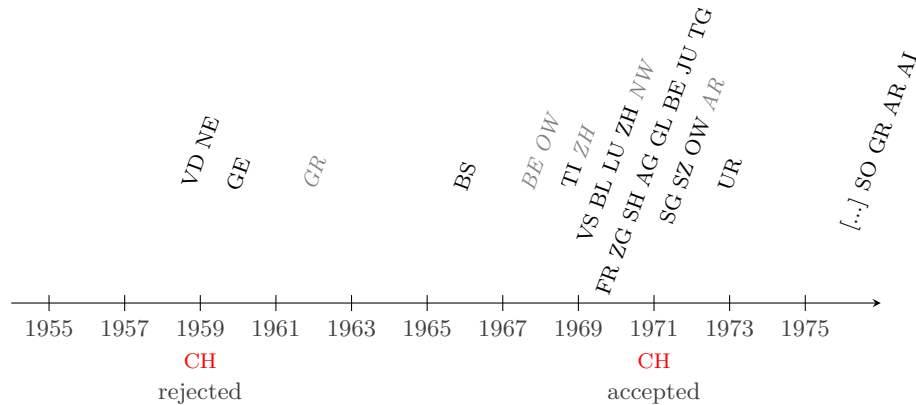
Figure 4.2.1: Staggered introduction of female suffrage in Swiss cantons.

Note: The timeline is based on Ruckstuhl (1986). The cantons in light gray / italics refer to the introduction of municipal voting rights (for more details, see Table 4.A.1).

elect politicians to the parliaments and, thus, delegate some of their political power. These town meetings are usually held in restaurants, town halls, or churches. It is common for participants to be offered drinks after the meeting, offering room for political discussion. Anecdotal evidence from the canton of Bern suggests that the town meetings were seen as a male-political arena: although on the municipal level in the canton of Bern, women were politically formally on equal terms with men since 1833,[4] they were banned from participating in the town meetings and had to send a male representative in their place. As mentioned in a parliamentary debate in 1958, "men wanted to be among themselves back then" (Parlamentsdienste, 1958, p. 254).

As discussed in Section 4.2.1, the relatively early introduction of female suffrage in the canton of Vaud allows us to exploit gender-specific turnout data. Moreover, in this canton, a population threshold defines the mandatory appointment of a parliament as their local legislative branch. Before 1960, the threshold was equal to 600 inhabitants. A constitutional amendment (approved by referendum) in 1960 increased this threshold from 600 to 800 inhabitants, and since the enactment of the Municipalities Act in 2005, it was raised to 1,000 inhabitants. We exploit the variations in institutional backgrounds in our empirical strategy, as explained in Section 4.5.

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[4]The first municipality law of the canton of Bern, introduced in 1833, included female suffrage for municipal matters. These voting rights, however, were only granted under the same conditions as for men, thus only if women possessed certain assets or were liable to pay taxes (Parlamentsdienste, 1958). These conditions were hardly ever met for women. Hence, this could not be considered general female suffrage.

4.3 Theoretical considerations and setting

Before the introduction of female suffrage, the electorate consisted of male voters only. Men decided on politics by delegating their decision-making power to politicians or via direct legislation. The electorate was doubled overnight with the introduction of female suffrage.[5] This raises (at least) two questions: How does the turnout of the established electorate evolve after suffrage extensions? And how does the turnout of the new voters develop?

Calculus of voting. As discussed in Section 4.1, the paradox of voting[6] urged scholars to analyze the determinants of turnout. One famous solution has been proposed by Riker and Ordeshook (1968), who add an additional term D to the previous work on the calculus of voting (Downs, 1957). This led to the equation, where an individual votes if:

$$Bp - C + D > 0 \tag{4.1}$$

The decision to vote thus depends on the benefit if an outcome is chosen against an alternative (B) times the probability that a voter is decisive (p) and a potential utility an individual receives from participating that is independent of the electoral outcome (D)—compared with the cost of voting (C). In the following, we theoretically discuss the potential factors that affect the calculus of voting in the context of suffrage extensions for the old (male) and new (female) electorate.

Notably, in our setting, we can exclude the explanation stating that an increase in the electorate reduces the chance of a voter being decisive (p) (see Figure 4.3.1). This is the case because the variations we are interested in occur on a lower governmental level than where turnout is observed. We can analyze how (i) cantonal female enfranchisement affects the turnout of men in federal votes and elections and (ii) how the institutional and societal background on the municipal level affects the turnout of men and women in cantonal votes and elections. This allows us to focus on costs (C) and benefits (D) of voting other than the likelihood of being decisive.[7]

Male turnout. The act of showing that one belongs to a group and confirming one's identity is an important turnout determinant (see, e.g., Rogers et al., 2013), which affects D in the calculus of voting. Bryan et al. (2011), for instance, have shown that enacting a personal identity—"being a voter" instead of "to vote"—can increase turnout. Enlarging the electorate changes the prevailing *political identity* in politics. To the male voters, the suffrage extension to women constitutes a feminization of politics overnight. On the one hand, this can lower the expressive benefits of voting for men. From an identity economics perspective, the enfranchisement of women can exert a negative externality

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[5]Today, most countries are not discussing suffrage extensions that would double the electorate. However, for some countries, equipping non-citizens with voting rights would increase the number of voters in significant numbers as well.

[6]See Downs (1957) for a theoretical discussion of the paradox of voting.

[7]For an overview of expressive and instrumental theories of voting, see Dhillon and Peralta (2002).
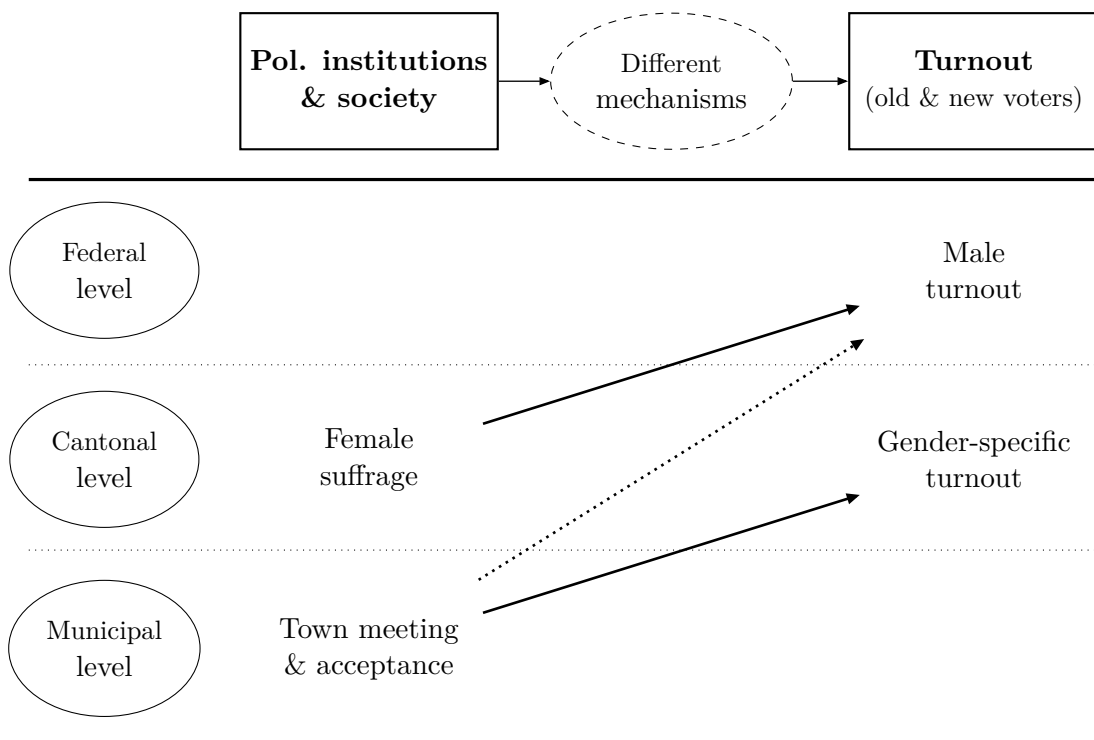
127

Figure 4.3.1: Municipal gender-specific turnout – Empirical setting.

on men's value within the realm of politics (see, e.g., Akerlof and Kranton, 2000). The authors provide the following example: "[...] if a man wears a dress, this may threaten the identity of other men." (Akerlof and Kranton, 2000, p. 717)–the same effects can be prevalent when politics turns from a male to mixed-gender sphere. The negative externality can, thus, be more substantial under backgrounds where the political sphere is strongly male-dominated.[8] These arguments support the consideration that the introduction of female suffrage may negatively affect male turnout. On the other hand, men may also better identify with politics after female enfranchisement, for example, because they now perceive politics as fairer and gain more procedural utility (e.g., Frey et al., 2004). This would increase the consumption value D of participating in politics for men, and we would expect them to turn out more.

A further important turnout determinant that changes with the enfranchisement of women is the extent to which participating in politics is seen as a *civic duty*. Voter participation can be seen as a collective action problem, and one way of overcoming it is by strong civic or societal norms (e.g., François and Gergaud, 2019; Knack, 1992). In many democracies around the world, there exists a public norm that says that voting is a civic duty (e.g., Blais and Achen, 2019). If a new "non-politicized" group gets the right to vote, this may decrease the extent to which voting is seen as a *civic duty*.

Female turnout. The length of time a person has held the right to vote is an essential factor for their political participation (see, e.g., Corder and Wolbrecht, 2006; Tawfik et al., 2012). Because women were excluded from politics, they could not acquire a habit of voting in their early years of adulthood. Moreover, the *social environment* plays a vital role in turnout: First-time voters turn out less when they are not living in their parent's household anymore (e.g., Bhatti et al., 2012; Zeglovits and Zandonella, 2013). However, as, for instance, Carreras (2018) points out, women may have a higher sense for *civic duty* and, therefore, receive more utility from voting than men.

Moreover, social pressure and social desirability are essential for the turnout of voters (e.g., Funk, 2010; Gerber et al., 2008; Jones and Hudson, 2000; Karp and Brockington, 2005). Women were, for a long time, not welcomed in politics, meaning that the *social desirability* for them to vote was not given. Gender norms in society are important to female turnout; if the political arena is perceived as a male domain, this limits women's participation in politics (Inglehart and Norris, 2003; Jennings, 1983).

From an *identity* economics perspective, women can exert a negative externality on men's value of politics if these men have strongly identified with the male political sphere.

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[8]We can also borrow insights from labor market research to understand the potential *devaluation* effect. On average, men earn higher wages in more prestigious jobs than women (e.g., Blinder, 1973; Oaxaca, 1973), and the gender wage gap persists even if conventional human capital variables (such as education and skills) and demographic characteristics are controlled for. One nonexclusive explanation to this observation is that employers—or the society at large—assign a lower value to typically female tasks (see, e.g., Levanon et al., 2009). This *devaluation hypothesis* states that a higher proportion of women in an occupation lowers the wages of that occupation (Blau and Kahn, 2000; Magnusson, 2013, p. 229)–if the same effects are at play in politics, we would expect female enfranchisement to lower male turnout.

This can lead to unwelcoming behavior from the male side and, thus, increase the cost C or decrease the benefit D of voting.[9]

In sum, there are many potential mechanisms to explain the turnout of men and women after female enfranchisement. Therefore, the following empirical question remains: Which mechanisms are at play and outweigh the others? Our setting, as depicted in Figure 4.3.1, allows for digging deeper into potential mechanisms by using variations in institutional and societal factors.

4.4 Data and variables

In this chapter, we use newly collected turnout data before and around the enfranchisement of women in Switzerland. We use two different data sets of municipal turnout: first, a data set with municipal male turnout in federal votes and elections covering the 1945 to 1970 period and, second, a data set with municipal gender-specific turnout data of the canton of Vaud covering the 1945 to 1970 period. Until the cantonal female enfranchisement in 1959, this data set only includes male turnout. We use the standard definition of voter turnout: voter turnout (female and male) describes the share of registered voters casting valid (including blank votes) or invalid ballots. These data sets allow us to use two types of variation in the political and societal backgrounds: on the one hand, a time-wise variation because women were enfranchised in different cantons at different times and, on the other hand, variations between municipalities because they have different political and societal backgrounds.[10]

4.4.1 Outcome variables

Federal male turnout. The data set on municipal male turnout in national votes and elections covers the 1945 to 1970 period–the period of observation ends in 1970 because this is the last year in which only men were entitled to vote on the national level. We newly collected and digitized (male) turnout data on the municipality level for the following votes and elections:

- Federal votes: We use the turnout rates on 67 national referendum days between 1945 and 1970 and combine these with more general information about the referendum (e.g., topic). These data is taken from Swissvotes, an online platform hosted by the University of Bern.[11]

- Federal parliamentary elections: We look at the turnout for the elections of the National Council, the larger of the two parliamentary chambers (elected by proportional rule). These elections occurred in 1947, 1951, 1955, 1959, 1963, and

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[9]See, e.g., Akerlof and Kranton (2000) who summarize examples of how women working in typical "men's jobs" face unkind acts against themselves since men want to affirm their masculinity.

[10]Importantly, both data sets allow us to analyze the outcome on a higher governmental level than the variation is located. Therefore, we can disentangle the effects of expressive motives on turnout from the direct instrumental effects. For more details, please see Section 4.5 and Figure 4.3.1.

[11]See www.swissvotes.ch for further information.

1967. The data were drawn from official notices or releases published by cantons in the aftermath of an election to the National Council.[12]

Note that by looking at turnout in *national* elections and referendum votes, we only look at *male turnout*, even if we look at the period after 1959 when cantonal female enfranchisement started to be implemented in Switzerland (see Sections 4.2 & 4.3). On the national level, women gained suffrage only in 1971. We cannot extend our observation period because women started participating in national votes and elections in 1971. Therefore, after 1971, it was no longer possible to disentangle male and female turnout because federal turnout data are anonymous and not separated by gender.

Both outcome variables—turnout in national elections and turnout in national referendums—have different advantages for our analyses.[13] Although the political context can starkly change from election to election, the question is (relatively) comparable from one election to another. In contrast, the questions asked in referendum votes vary strongly—resulting in disparities in debate, attention, interest, and, thus, turnout. However, a shortcoming of national elections is that they only occur every four years. Moreover, we are confronted with missing data for some elections in some cantons: because of processing problems, the turnout data of the cantons of Bern, Basel City, and Thurgovia are not included in our data set.[14] Furthermore, in certain small cantons, elections were held tacitly because of an exact match between the number of candidates and number of seats to be filled, hence lowering the number of observations.[15] In contrast to elections, referendum votes take place more frequently (67 referendum vote days in our observation period compared with six election days), which is an advantage compared with the election data. Because turnout data are noisy, it is important to control for the timing of the election or of the referendum.[16]

In total, for our baseline estimations, we end up with 93,567 (male) municipal turnout observations in federal votes and 7,164 (male) municipal turnout observations in federal elections.[17]

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[12]As our period of observation consists of the period before 1970, we had to collect and digitize our data from a variety of sources because national election outcomes were not yet centrally collected.

[13]Note the caveat that elections to the second chamber of parliament, where women could vote in enfranchised cantons, took place on the same day as the elections to the National Council. Similarly, some cantons hold cantonal referendum votes simultaneously as a national referendum vote takes place. Regarding the referendum votes, we can control for a cantonal referendum vote on the same day.

[14]For the canton of Bern, the data were not available because the data were lost by the district offices. We received the data from the official gazette for the canton of Thurgovia, but because of a large municipal reorganization, the use of these data was not possible. For the canton of Basel City, according to the state archive, it is unclear whether the data exist.

[15]This is the case for the cantons of Appenzell Outer Rhodes (national elections of 1947, 1959, 1963, 1967), Glarus (all the national elections between 1947 and 1967), Schaffhausen (1951, 1955), Schwyz (1967), and Zug (1963).

[16]Fujiwara et al. (2016), e.g., show with US data that even the weather in past elections can influence voter turnout. For Switzerland, Meier et al. (2019) show that rain can influence the outcome of a referendum.

[17]In our baseline estimations, we exclude all cantons with compulsory voting installed during our observation period. In the robustness checks, we extend our sample to cantons that had compulsory voting rules installed but experienced no change in this rule during our observation period.

Cantonal male & female turnout. We collect the municipal turnout data of the cantonal elections and referendums for the canton of Vaud. We have chosen this canton because, from its cantonal introduction of women's suffrage in 1959 to the introduction of women's suffrage at the federal level in 1970, the reports issued by the prefectures of Vaud explicitly distinguish between the number of female voters and male voters (see Figure 4.A.1).[18] This allows for looking at municipal *gender-specific turnout* data after 1959 and *male turnout* for the 1945 to 1970 period. Moreover, in the canton of Vaud, the local legislative institution either consists of a town meeting or a parliament.[19] We dispose of turnout data for the following elections and votes in our data set for the canton of Vaud:

- Federal parliamentary elections: We dispose of gender-specific turnout data for three elections to the Council of States–the upper house of the Federal Assembly– between 1959 and 1970.[20]

- Cantonal government elections: We collected municipal male (before 1959) or gender-specific (after 1959) turnout data for 10 cantonal government elections. Five elections took place after 1959.

- Cantonal votes: We collected municipal male (before 1959) and gender-specific (after 1959) turnout data for 26 cantonal votes. Eleven votes took place after 1959.[21]

An overview of the votes and elections of which we dispose of the gender-specific turnout for the canton of Vaud is depicted in Table 4.A.3. In total, we end up with 7,296 observations of municipal female and male turnout, where 4,224 stem from cantonal votes and 3,072 from cantonal or federal elections (see Table 4.4.1). We look at *female turnout*, *male turnout*, and the *gender turnout gap*. The latter is constructed as the difference between male and female participation.

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[18]It was possible to compile these data with a reasonable amount of time. To the best of our knowledge, gender-specific data are not available or not easily accessible for other cantons.

[19]For more information on the institutional backgrounds, see Section 4.2.

[20]In Switzerland, the legislative body at the federal level (the Federal Assembly) is divided into two organs: the National Council, which has 200 members and represents the Swiss people, and the Council of States, which has 46 members (2 per canton and 1 per semi-canton) and represents the Swiss cantons. Against this background, the election for the Council of States is open to the entire electorate of each canton, including female voters in the case of Vaud between 1959 and 1970, even if women's suffrage at the federal level had not existed. For more information about the Federal Assembly, see https://www.parlament.ch/en/organe-home.

[21]For each referendum, our data set also includes the legal form (popular initiative, compulsory referendum, or counter-proposal) and the political topic of the referendum. Concerning the latter point, we use the classification proposed by Swissvotes, an online platform hosted by the University of Bern. This classification includes the following items: state order, foreign policy, security policy, national economy, agriculture, public finance, energy policy, transport and infrastructure, environmental policy, social policy, education and research, and culture-religion-media. Furthermore, from this platform, we draw the dates of the federal referendums that were held between 1945 and 1970, with the turnout for a given cantonal referendum being potentially affected by the holding (the same day) of a referendum at the federal level.

Table 4.4.1: Descriptive statistics of municipal gender-specific turnout data.

Variable	Obs.	Mean	Std. dev.	Min	Max	Obs.	Mean	Std. dev.	Min	Max
	FEDERAL TURNOUT - VOTES					*FEDERAL TURNOUT - ELECTIONS*				
male turnout	93,567	43.27	17.52	0.00	100.00	7,164	68.86	16.04	18.18	100.00
acceptance 1959	93,567	27.90	15.51	0.00	95.52	7,158	27.81	14.95	0.00	83.30
population	93,567	1726.83	7018.30	20.00	176183	7,164	1519.20	6546.42	22.10	175414
agriculture	93,567	14.98	9.30	0.08	75.00	7,163	15.98	9.35	0.05	65.16
pensioner	93,567	10.42	3.26	0.00	34.62	7,164	10.19	3.37	1.10	29.52
women	93,567	48.71	3.07	20.80	75.36	7,164	48.45	3.30	22.05	73.91
foreigner	93,567	5.00	5.83	0.00	55.19	7,164	5.11	6.24	0.00	51.05
unemployed	93,567	0.01	0.05	0.00	1.40	7,164	0.01	0.05	0.00	1.63
working women	93,567	20.37	6.86	0.00	75.00	7,163	19.40	7.12	0.00	55.63
German	93,567	59.00	42.92	0.00	100.00	7,164	45.00	43.55	0.00	100.00
married	93,567	42.65	6.00	20.25	69.23	7,164	41.38	6.53	20.25	65.49
Catholic	93,567	40.09	39.02	0.00	100.00	7,164	58.45	39.04	0.00	100.00
	CANTONAL TURNOUT - VOTES					*CANTONAL TURNOUT - ELECTIONS*				
male turnout	4,224	34.84	16.92	0.00	100.00	3,072	57.92	15.00	16.31	100.00
female turnout	4,224	14.36	11.82	0.00	93.33	3,072	30.07	15.24	0.00	100.00
gender turnout gap	4,224	20.49	13.24	-20.83	87.50	3,072	27.85	11.73	-43.75	88.89
town meeting	4,224	0.78	0.41	0.00	1.00	3,072	0.78	0.41	0.00	1.00
acceptance 1959	4,224	38.02	12.98	0.00	66.67	3,072	38.02	12.98	0.00	66.67
population	4,224	1198.44	6960.73	20	137383	3,072	1232.08	7062.80	20	137383
agriculture	4,224	17.97	9.83	0.38	75.00	3,072	17.44	9.99	0.38	75.00
pensioner	4,224	13.36	3.97	0.00	34.62	3,072	13.68	4.08	1.10	34.62
women	4,224	48.13	3.60	33.33	75.36	3,072	48.26	3.51	34.25	75.36
foreigner	4,224	8.80	7.88	0.00	53.65	3,072	9.36	8.40	0.00	51.05
unemployed	4,224	0.02	0.09	0.00	1.40	3,072	0.03	0.11	0.00	1.40
working women	4,224	21.24	7.85	0.00	75.00	3,072	22.99	7.78	0.00	75.00
married	4,224	47.67	4.74	23.75	69.23	3,072	48.35	4.65	23.75	69.23
Catholic	4,224	17.29	13.54	0.00	96.43	3,072	17.85	13.80	0.00	96.43

4.4.2 Explanatory variables

We rely on different explanatory variables to better understand the determinants of turnout of the old (male) and new (female) electorate in the aftermath of female enfranchisement. On the one hand, we exploit the staggered cantonal introduction of female suffrage, which changes politics from a "pure male sphere" to a "mixed-gender sphere" overnight, on male turnout. On the other hand, we exploit the effects of different municipal backgrounds on male and female turnout.

Cantonal female suffrage. In Switzerland, the cantons granted voting rights to women on the cantonal level in a staggered way (see Table 4.A.1). Hence, we analyze how the introduction of female suffrage in a canton affects male turnout on the federal level. In our base estimation, *cantonal female suffrage* acts as a treatment variable.[22]

Town meeting. Female voters entered diverse political contexts. One dimension that differs is the institutional background on the municipal level; there are municipalities with a town meeting (*town meeting* = 1) and municipalities that install a parliament (*town meeting* = 0) as their legislative institution (see Section 4.2.2). The *town meeting* constitutes a direct-democratic and parliament a representative democratic institution.

Acceptance 1959. Under the Swiss direct-democratic system, suffrage extensions are subject to a direct-democratic vote. This allows us to observe the acceptance rate in the federal vote of 1959 for all Swiss municipalities. For each municipality, we know what percentage of the male electorate voted for (and against) female federal enfranchisement, which refers to our variable *acceptance 1959*. The acceptance rate of a municipality proxies for the prevailing gender norms in a society.

4.4.3 Control variables and further information

Because other time-variant variables at the municipal level might be important to explain turnout, we control (when available) for the following municipal characteristics:[23]

Rurality. A finding in the literature points to a negative association of population size and population concentration with turnout (e.g., Cancela and Geys, 2016). Thus, we control for the *population* size of a municipality. Moreover, we control for the proportion of the labor force working in the agricultural sector (in the following referred to as the *agriculture*), which controls for the rurality of a municipality and, at the same time, proxies for the population density in a municipality.

Demography. The age structure of the electorate has been shown to affect turnout decisions (e.g., Bhatti et al., 2012). Thus, we include the *pensioner* share to control for

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[22]As shown in Table 4.A.1 in the Appendix, some cantons have only introduced (optional) municipal female suffrage. In the robustness checks, we analyze if the results hold when adding municipalities from these cantons to the treatment group.

[23]These variables stem from decennial census data (from 1950, 1960, and 1970). Since election and referendum years differ from census years, we calculate a weighted average to derive the values of the control variables for each election and referendum year.

the share of elderly people. Moreover, Braun and Kvasnicka (2013) show that the share of women played an important role in the enfranchisement process in the US. Although there are no large variations in sex ratios in Swiss municipalities, we add the share of *women* in a municipality to our list of control variables. Furthermore, we include the share of *foreigners* to control for one part of the population that cannot participate in politics. The share of foreigners without formal voting rights in the neighborhood is negatively related to the natives' political participation (Bellettini et al., 2016).

Economy. Economic performance has not only been shown to affect the choice of voters but also the decision to participate in politics (e.g., Burden and Wichowsky, 2014). Therefore, we include the *unemployment* share as a control variable. Moreover, we also control for the share of *working women.*

Culture. Switzerland is a multilingual and multireligious country with four official languages and two main religions (Catholics and Protestants). The largest language groups in Switzerland are German ($\approx 60\%$), French ($\approx 25\%$), Italian ($< 10\%$), and Rhaeto-Romanic.[24] Because the language borders are not fully congruent with cantonal borders, we control for the share of the *German-speaking* population on the municipality level in our nationwide analysis. A country's gender culture is important for the political interest and participation of women (Dassonneville and Kostelka, 2021)—thus, we control for the share of *married* in a municipality because this might proxy for traditionalism and the degree of emancipation. Moreover, we can control for the share of *Catholics* in a municipality, which has been shown to play a role in the enfranchisement process of women (Koukal, 2019).

We harmonize the census data and election data with the list of municipalities published by the Swiss FSO for the year 2000. More specifically, the census data of 1950 and 1960, as well as the data of the six national elections considered, are adjusted to take into account the mergers of municipalities that occurred between 1950 and 2000,[25] such that 2,256 municipalities remain in our federal data set. For the canton of Vaud, we can observe 386 municipalities. Table 4.4.1 provides the descriptive statistics for all the variables in our data sets.

4.5 Empirical strategies

This chapter exploits how variations on the cantonal and municipal level affect the turnout of men and women in the aftermath of female enfranchisement. Figure 4.3.1 summarizes the setting. First, we observe the staggered introduction of female suffrage on the cantonal level and how this affects male turnout on the national level. Second, we obtain more insights by analyzing how different municipal backgrounds affect male and female turnout on the cantonal level and male turnout on the federal level. There-

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[24]In the considered time period, six (out of 25) cantons officially speak French (Geneva, Neuchatel, Vaud) or are bilingual (Bern, Fribourg, Valais). The French-speaking cantons were pioneers in the enfranchisement process.

[25]We have taken into account 87 mergers of municipalities between 1950 and 2000.

fore, the variations are located on a lower state level (municipal or cantonal) than the observed turnout (cantonal or federal). The explanatory variables do, hence, not affect the probability of being pivotal (see Section 4.3). In the following, we explain how we proceed in analyzing the staggered introduction of female suffrage in the cantons and the (institutional) variations on the municipal level.

4.5.1 Variations in cantons over time

The staggered cantonal introduction of female suffrage enables us to exploit the variations in enfranchisement between cantons over time. To understand whether and how female enfranchisement affects male turnout, we apply an event study design by Clarke and Tapia-Schythe (2021) to exploit the staggered introduction of women's suffrage. This flexible approach allows for different enfranchisement dates and flexible tests for differential selection into treatment groups in periods before the treatment.

We observe a panel of male turnout ($turnout_{mt}$) in municipalities m over the time period t. We consider a municipality m to be treated as soon as female suffrage is installed in the respective canton—thus, the enfranchisement of women refers to $Event_m$. The panel event study specification can be written as follows:

$$\text{turnout}_{mt} = \alpha + \sum_{j=2}^{J} \beta_j (\text{Lag } j)_{mt} + \sum_{k=1}^{K} \gamma_k (\text{Lead } k)_{mt} + \mu_m + \lambda_t + X'_{mt}\theta + \varepsilon_{mt}, \quad (4.2)$$

where μ_m and λ_t are municipality and election or referendum fixed effects. Our model includes socio-demographic control variables $X'_{mt}\theta$, and ε_{mt} is an unobserved error term. Lags and leads to the event of interest are defined as follows in 4.2:

$$
\begin{aligned}
(Lag\,J)_{mt} &= \mathbb{1}\left[t \leq Event_m - J\right], \\
(Lag\,j)_{mt} &= \mathbb{1}\left[t = Event_m - j\right] for\, j \in \{1, ..., J-1\}, \\
(Lead\,k)_{mt} &= \mathbb{1}\left[t = Event_m + k\right] for\, k \in \{1, ..., K-1\}, \\
(Lead\,K)_{mt} &= \mathbb{1}\left[t \geq Event_m + K\right].
\end{aligned}
\quad (4.3)
$$

The leads and lags are binary variables; they indicate that a given municipality m was a given number of periods away from female suffrage ($Event_m$) in the respective time period t. For example, if in a municipality m the event occurred at time $t = 3$, then $(Lag\,0)_{m3} = 1$, while $(Lag\,1)_{m3} = 0$. The variables $(Lead\,k)_{mt} \ldots (Lead\,K)_{mt}$ indicate in the same manner the leads; thus, for instance, if $t = 3$, then $(Lead\,1)_{m2} = 1$. The leads and lags, that indicate how far away a vote or election is from our event (= introduction of *cantonal female suffrage*), are either constructed in vote days or years. Those municipalities where female suffrage was not introduced during our observation period serve as our controls with 0s in all lead and lag terms.[26] Because in these analyses, a

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[26]For future research, it would be interesting to analyze if there are spillover effects from one region to the other. For instance, cantons could be affected by the introduction of female suffrage in neighboring cantons.

standard inference concern is related to serial correlation in the outcome variable over time, we allow for within-cluster autocorrelation at the district level. Most of the 26 cantons are subdivided into various districts comprising varying numbers of municipalities. With a few exceptions, these districts are mainly administrative and judicial entities or electoral districts and have no independent legal status.[27][28]

Unbiased estimation of the treatment effects relies heavily on the parallel trends assumption. We have to assume that, in the absence of the treatment (no female suffrage on the cantonal level), treated and control municipalities would have maintained similar differences as in the baseline period (see also Clarke and Tapia-Schythe, 2021). This assumption seems more probable when the treated and control municipalities follow parallel pre-trends, which will be discussed in Section 4.6.

Besides the common trend assumption, we have to assume that the timing of the treatment is statistically independent of potential outcome distributions (conditional on the municipality and time-fixed effects). Our units of observation are the municipalities, and the introduction of female suffrage is decided on the cantonal level; thus, we can claim that the introduction of female suffrage is, to some extent, exogenous to the individual municipalities. However, we will also provide robustness tests where we exclude municipalities favoring female suffrage in 1959, which makes the treatment and control groups more similar.

According to Bechtel et al. (2016), 15 out of 25 Swiss cantons perform some form of compulsory voting during our observation period, of which seven enforce compulsory voting by financial sanctions. Because our dependent variable is turnout and compulsory voting rules impact turnout, we estimate our models for different samples. In our baseline sample, we exclude all cantons with any form of enforced compulsory voting during the observation period.[29] In less restrictive estimates, we only exclude the cantons that experienced a change in compulsory voting rules during our period of observation, here under the assumption that constant effects of compulsory voting are absorbed by municipality fixed effects in the estimates.[30]

4.5.2 Variations between municipalities

We derive further insights on the drivers of male and female turnout after female enfranchisement by focusing on turnout in one single canton and its municipalities' backgrounds

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[27]For an overview of the districts in Switzerland, see `https://www.bfs.admin.ch/bfs/de/home/statistiken/kataloge-datenbanken.assetdetail.24065857.html` (Last accessed: 31 July 2023).

[28]See also Schmidheiny and Siegloch (2023) for a more formal description of the setup. The authors discuss (i) how important the choice of the *effect window* is, (ii) that event study designs with binned endpoints are equivalent to distributed-lag models, and (iii) that a generalization of simple event study designs to account for multiple treatments of different signs and varying intensities is possible. We perform several robustness checks for the event study in different samples or under different specifications.

[29]The following cantons are excluded in this specification: AG, AI, BE, GL, GR, LU, OW, SH, SO, SG, TG, ZG, ZH, UR, and VD.

[30]Depending on the period of observation used in an estimate, the cantons belonging to the different categories can vary. The cantons that experienced a change in their compulsory voting rules were GL, GR, SG, and ZH.

instead of the staggered introduction of the cantons. We decided to collect data from the canton of Vaud for this matter. From a data perspective, the canton of Vaud has three major advantages. First, we have detailed information on the political institutions on the local level. Second, it was possible to digitize gender-specific participation in cantonal elections and referendums.[31] Third, the canton of Vaud was the first to enfranchise women in 1959, allowing us to observe the canton for 10 years, up to 1971.

We want to provide general insights on how *town meeting* and *acceptance 1959* relate to the turnout of freshly enfranchised women and the established male electorate in the aftermath of female enfranchisement. The variables reflect the institutional and societal backgrounds of the municipalities (see Section 4.4). We estimate the following ordinary least squares (OLS) model:

$$turnout_{mt} = \beta_0 + \beta_1 town\,meeting_m + \beta_2 acceptance\,1959_m + \beta_3 X_{my} + \gamma_t + \epsilon_{mty}, \quad (4.4)$$

where γ_r are day-fixed effects and X_{my} denotes a vector of control variables, including socio-demographic variables. Because the variables of interest are time-invariant on the municipal level, using municipality fixed effects is not possible in this model. Therefore, omitted-variable bias or endogeneity concerns might be at play in Model 4.4.

As explained in Section 4.2.2, the institutional background on the municipal level differs between the municipalities, and for the canton of Vaud, a population threshold is used for the appointment of the legislative branch at the municipality level, which theoretically allows us to analyze the effect of the institution on gender-specific turnout in RDD and solve some of the potential concerns of Model 4.4.

The key identifying assumption of the RDD is that the treatment (or probability of the treatment) changes abruptly at the known threshold (see, e.g., Cattaneo and Titiunik, 2022). Therefore, if we want to learn more about the causal effect of the institutional background on male and female turnout, the municipalities must be randomly distributed around the cutoff, and we need enough observations close to the cutoff. Although the first requirement is fulfilled because the municipalities do not seem to sort around the threshold of 600 inhabitants (see Figure 4.C.1), the second requirement is more challenging. Because there are only a few observations close to the cutoff, the optimal bandwidth that must be selected has to be relatively large, making the key identifying assumption more unrealistic. Still, the setting and some results of the RDD are presented in Appendix 4.C.

4.6   Results & discussion

In this section, we present and discuss our findings. In Section 4.6.1, we exploit how the cantonal introduction of female suffrage affects male turnout at the federal level. To provide further insights into the evolution of men's and women's turnout after female enfranchisement, Section 4.6.2 presents the results of exploitation of gender-specific

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[31]We were able to compile the municipal turnout data for the canton of Vaud in a reasonable amount of time. To the best of our knowledge, gender-specific data around the time of female enfranchisement are not available or easily accessible for other cantons.

turnout data from the canton of Vaud. In Section 4.6.3, we explore the different channels (see Section 4.3) that may explain our findings.

4.6.1 Staggered introduction of cantonal female suffrage - male turnout

Starting in 1959, the cantons started to introduce female suffrage on the cantonal (and municipal) level. This staggered introduction allows us to analyze the influence of the inflow of women into politics on male turnout on the federal level. To provide a first flavor of our data, Figure 4.6.1 plots the turnout of male voters in federal votes from our treatment cantons before and after the introduction of cantonal female suffrage. The distance to cantonal suffrage (x-axis) describes how many federal votes before or after the cantonal introduction of female suffrage the turnout is observed. Note that the time between two given referendum days differs because referendum days take place irregularly. Compared with election data, referendum data provide the advantage that multiple referendum days are usually observed yearly. Although male turnout seems to follow a negative time trend over the whole observation period, we observe a slight jump in the trend lines, hence pointing to a lower male turnout after the introduction of cantonal female suffrage.

The setting allows running an event study (see Section 4.5 and Model 4.2). The results of these base estimations are plotted in Figure 4.6.2, whereas subplot 4.6.2a refers to the estimates based on votes as the running variable and subplot 4.6.2b on the years as the running variable. In both estimations, we control for the complete set of controls (see Section 4.4), including controls for a municipality's rurality, demography, economy, and culture. Moreover, we introduce referendum day and municipality fixed effects and cluster the standard errors at the district level (see Section 4.5).

The assumption of parallel trends for the treatment and control units after the treatment is more plausible if the two groups show parallel pre-trends. This requirement is not met during the entire pre-treatment period because we observe some significant variations. However, the relevant votes and years to look at for parallel trends are the ones just before the cantonal introduction of female suffrage. For these votes and years, plots (4.6.2 (a) & (b)) do not show significant differences in male turnout between the treatment and control units.

Both plots point to decreased federal male turnout after the introduction of cantonal female suffrage. Because women could only vote for cantonal and municipal matters (see Section 4.3), men's weight in the decision of the federal votes was not affected. However, the turnout decline did not start instantly but only some votes after introducing female suffrage. This might be because men's reactions to the introduction of female suffrage took some time—the potential channels will be discussed in Section 4.6.3. In terms of size, the drop in male turnout amounts to approximately -5 percentage points. This is a sizable effect because the mean male turnout in federal referendum votes in our observation period was 43.27% (see Table 4.4.1).

Figure 4.B.1 in the Appendix provides evidence that this result is very robust: it persists if we only use population size as a control (Panel (a) of Figure 4.B.1), if we also
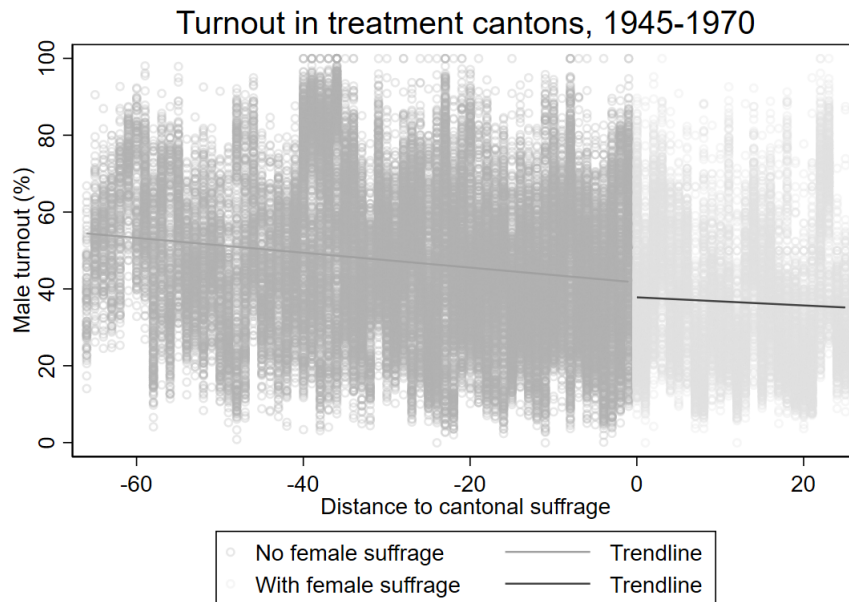
Figure 4.6.1: Male turnout in federal votes before and after cantonal female suffrage.

include municipalities from cantons that only allowed for municipal female suffrage as treated (b), if we enlarge the sample to municipalities with compulsory voting (but no change in this regulation) (c), if we take only the first-movers (VD, GE, NE) as treated and the relatively early movers as controls (< 1972)(d), if we exclude the latecomers (> 1971) from the control group (e), if we exclude the highest 25 % acceptance rates in the 1959 vote (f), if we exclude the canton of ZH[32] (g), or if we change the reference period to two votes before the first treatment period (h). These robustness checks are based on the referendum day basis; we perform a similar set of robustness checks based on the year, which are provided in Figure 4.B.2 in the Appendix. The results remain robust under this specification.

In addition, we perform several placebo tests to check whether we find this pattern only for our treated municipalities at the actual treatment point. A summary of these placebo tests on a referendums and year basis is provided in Figure 4.B.3 in the Appendix. To make sure that we do not just observe general time trends for our treatment compared with our control cantons, we advance or delay their treatment moment (10 votes or 5 years prior / after the actual treatment - subplots (a)-(d) in Figure 4.B.3). In another test, we take other cantons with (the most) similar voting behavior in the 1959 vote on federal female suffrage as the treated cantons (subplots (e) & (f) in Figure 4.B.3). Furthermore, we perform random placebo tests in which the cantons are alphabetically

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[32]The introduction of female suffrage in the canton of Zurich occurred only some months before the federal vote in 1959.

attributed to the treatment points. In all these placebo tests, there are no systematic effects of *cantonal female suffrage* on *federal male turnout.*
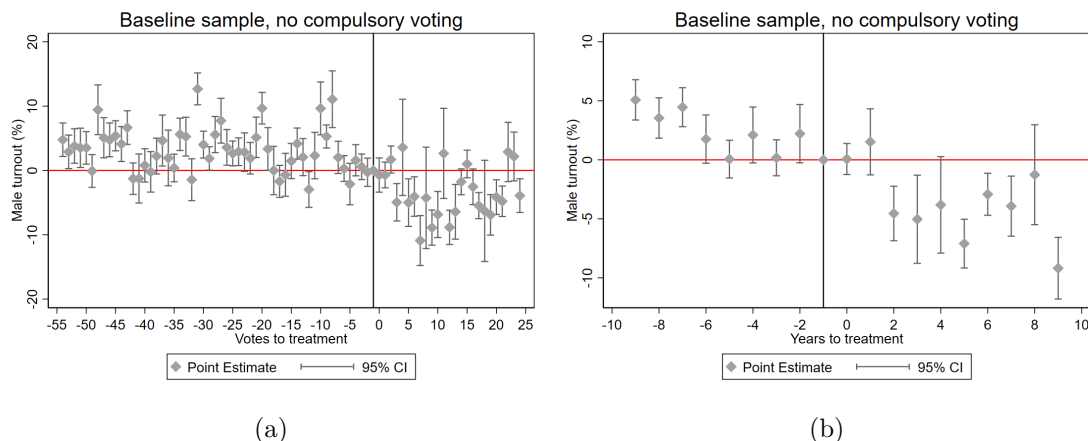

Figure 4.6.2: Female suffrage & male turnout - Event study plots.

Notes: In panel (a), the estimations are based on how many *referendum days* the observation is away from cantonal female suffrage. In panel (b), the estimations are based on how many *years* the observation is away from cantonal female suffrage.

As discussed in Section 4.4, we can also run an event study with the turnout in federal elections as the outcome variable. These results are plotted in Figure 4.B.4– again, we include municipality and election day-fixed effects, and the standard errors are clustered at the district level. For the pre-treatment periods, the turnout differences between the treated and untreated municipalities are not systematically different in any of the specifications, hence supporting the parallel trend assumption. For the post-enfranchisement period, the estimates from the election sample point again to a "delayed" decline in turnout in the treated cantons. Although the turnout drop is not visible in the first elections after the introduction of female suffrage, it appears in the second and third elections after the introduction. In terms of size, the estimates from the election sample (see Figure 4.B.4) yield around -5 percentage points, which is similar to the estimates from the referendum votes sample (see Figure 4.6.2). Therefore, the election sample supports the finding that male turnout on the federal level dropped with a "delay" after the enfranchisement of women on the cantonal level.

4.6.2   Different municipal backgrounds - male & female turnout

As elaborated in Section 4.3, we expect the societal and political backgrounds to affect male and female turnout in the aftermath of female enfranchisement. Although the introduction of female suffrage *per se* seems to affect male turnout beyond the change in their political weight, the effect may be heterogeneous in different environments. Women's barriers to entering politics may also depend on their environment (see Section 4.3). As

explained in Section 4.4, we can proxy for the societal and institutional background with the variables *acceptance 1959* and *town meeting*.

Table 4.6.1: Municipal background & gender-specific turnout – VD, 1959–1970.

Cantonal votes & elections (VD)	(1) female turnout	(2) female turnout	(3) male turnout	(4) male turnout	(5) gender turnout gap	(6) gender turnout gap
town meeting	-1.530***	-1.485***	0.363	0.388	1.896***	1.872***
	(0.460)	(0.452)	(0.476)	(0.471)	(0.426)	(0.426)
acceptance 1959	0.102***	0.112***	0.075***	0.087***	-0.031***	-0.025**
	(0.011)	(0.011)	(0.011)	(0.011)	(0.010)	(0.010)
Rurality	✓	✓	✓	✓	✓	✓
Demography	✓	✓	✓	✓	✓	✓
Economy		✓		✓		✓
Culture		✓		✓		✓
Day FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Observations	7,296	7,296	7,296	7,296	7,296	7,296
R-squared	0.575	0.592	0.722	0.729	0.496	0.499

Note: Robust standard errors in parentheses. Town meeting is a dummy variable indicating whether the municipality has installed a town meeting (=1) or parliament (=0). Acceptance 1959 is a continuous variable that captures the share of yes votes in the 1959 federal vote on female suffrage. The outcome variables include turnout for elections and cantonal referendums. The controls include population size, agriculture, women, foreigner, pensioner, unemployed, working women, married, and Catholics (in shares). *** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4.6.1 depicts the estimates from Model 4.4 on female turnout, male turnout, and the gender turnout gap. Hence, we use our gender-specific cantonal data from Vaud in these estimations. Table 4.6.1 depicts the estimations, which can be summarized as follows: First, women seem to turn out less in *town meeting* municipalities (Spec. 1 & 2), and the gender turnout gap is larger in town meeting municipalities (Spec. 5 & 6). This means that, over the 10 years after female enfranchisement, the gender turnout gap *ceteris paribus* was almost 2 percentage points larger in municipalities holding *town meetings* compared to municipalities having parliaments. Second, when the *acceptance 1959* is larger, men and women turn out more (Spec. 1 - 4). However, because the *acceptance 1959* is more positively related to female than to male turnout, a one percentage point larger acceptance rate relates to a 0.025 percentage point lower gender turnout gap (Spec. 5 & 6). The relationships mostly remain stable regarding size and significance over different robustness checks (see Table 4.B.3 in the Appendix), where we exclude population outliers and outliers in terms of the acceptance rate in 1959.

The findings of Table 4.6.1 give some first hints that municipal backgrounds play a role in the turnout behavior of men and women after female enfranchisement and support some of the discussed theoretical expectations (see Section 4.3): women turn out

more when they are accepted in politics, maybe because of fewer social sanctions when participating. Moreover, the start for participating in politics seemed to be more challenging for women under a more direct- compared to a more representative-democratic background—this may be due to the potentially stronger male identity in *town meeting* municipalities (Akerlof and Kranton, 2000). Moreover, men from municipalities with a higher acceptance rate turn out more. Men from municipalities with more liberal gender roles may better identify with politics after the enfranchisement of women. This potential channel will also be tested in the federal votes' sample in Section 4.6.3.

As discussed in Section 4.5, the setting in the canton of Vaud theoretically allows us to explore the effect of the institutional background (town meeting vs. parliament) on the municipal level in an RDD. However, there are unfortunately not many observations close to the cutoff, which makes the use of an RDD challenging (see 4.5.2). Still, the empirical strategy and some results are presented in Appendix 4.C. A descriptive picture of how the turnout of men and women after female enfranchisement as related to the population of a municipality is shown in Figure 4.6.4. The potential channels behind these findings will be discussed in Section 4.6.3.
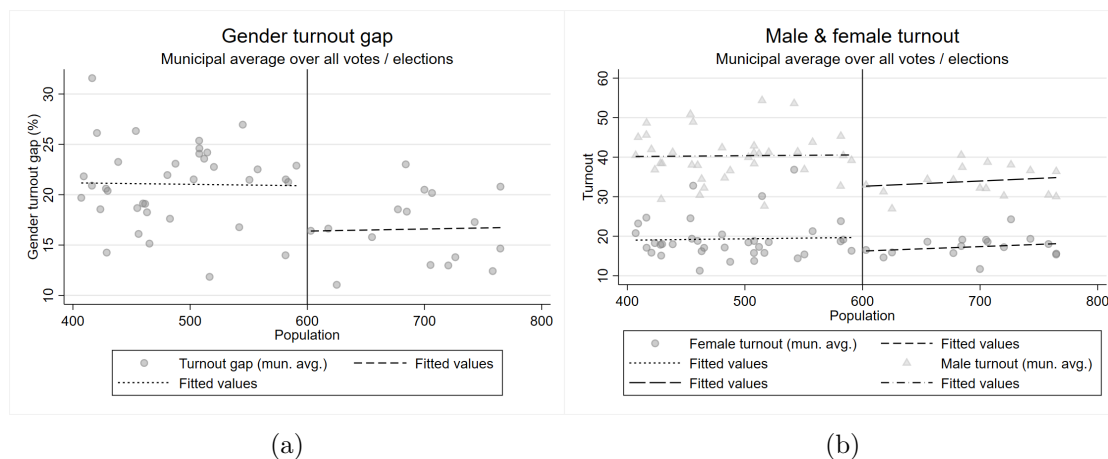

Figure 4.6.4: Gender-specific turnout based on population size – VD, 1959–1970.

As depicted in Figure 4.6.4a, the gender turnout gap seems to be larger in municipalities below the cutoff of 600 inhabitants, which means that more direct-democratic municipalities are associated with a larger turnout gap. When grasping the raw turnout data of the male and female electorate (see Figure 4.6.4b), we gain some more insights. Turnout seems to be higher for both men and women in the municipalities below the cutoff. Interestingly, it does not seem to be "only" a population size effect because the fitted values are reasonably flat. This descriptive picture shows that the larger gender gap in the *town meeting* municipalities cannot be attributed only to the low turnout of women but rather to the high turnout of men. One potential way to interpret this finding is that the direct-democratic environment exerted an integrative effect on the old electorate—this potential channel will be discussed in Section 4.6.3. The scatterplots of

Figure 4.6.4 and the OLS estimates of Table 4.6.1 suggest that the societal and institutional environments matter for the turnout decisions of the new (female) and old (male) electorate.

4.6.3 Potential mechanisms

So far, our results point to the following exciting correlations: federal male turnout dropped after the cantonal introduction of female suffrage. The proxied liberal gender norms in society positively correlate with the turnout decisions of men and women after the introduction of female suffrage. Moreover, the gender turnout gap was larger in town meeting (compared with parliament) municipalities in the aftermath of female suffrage. However, this mostly seems to be driven by men from direct-democratic backgrounds participating strongly. How can these findings be explained? In the following, we discuss the potential mechanisms.

Devaluation of politics for men

Because of the empirical setting (see Figure 4.3.1), the decrease in municipal male turnout on the federal level cannot be explained by a lower weight in the decision of the men's votes cast. To explore the possibility that the inflow of women lowered men's consumption value of politics, we examine whether there are divergent effects on male turnout in municipalities based on their initial reluctance toward female suffrage. If these municipalities placed significant value on the political sphere being exclusive to males before the introduction of female suffrage, the decrease in turnout should be more pronounced for them after implementing female suffrage.

Table 4.6.2 provides the estimates for an interaction of *acceptance 1959* and *cantonal female suffrage* in our federal data set. We use the acceptance in 1959 as a continuous and dummy variable that indicates whether (or by how many percent) a municipality has accepted federal female suffrage in 1959. For both the male turnout in federal referendums and elections, we find similar patterns: when interacting the *enfranchisement* with the dummy variable of *acceptance 1959* (Spec. 1 & 3), the base effect is negative and statistically significant, indicating that male turnout was lower in municipalities that enfranchised women and have not accepted female federal suffrage in 1959. The federal male turnout after their cantonal enfranchisement was 5.854 (referendums) or 1.567 (elections) percentage points lower. However, when a municipality accepted female suffrage in 1959, the drop in turnout after the cantonal enfranchisement is statistically significantly smaller for the referendum sample (+1.804). We provide several robustness checks for the estimations of the referendum sample in Appendix Table 4.B.1, where we, for instance, absorb cantonal time trends in turnout or exclude those cantons from the control group that introduced female suffrage only after the second federal vote. Moreover, we perform the same estimations for the sample, where we also allow for compulsory voting when there was no regulation change during our observation period (see Table 4.B.2 in the Appendix). The results remain robust regarding the size and statistical significance of these exercises.

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These findings align with the interpretation that the enfranchisement of women touches the consumption value in terms of identity benefits of the male electorate. Suppose men were content with the pre-female suffrage political climate, hence resulting in a low *acceptance 1959*. In that case, they may struggle to identify with the new mixed-gender political identity (see Section 4.3). Lower identity benefits might then lead to lower male turnout (see e.g. Hillman, 2010). The estimations with data from the canton of Vaud in Table 4.6.1 support this finding because the acceptance rate of 1959 is positively correlated with male voter turnout after female enfranchisement.

Table 4.6.2: Male turnout by initial acceptance of female suffrage – CH, 1960–1970.

	Referendums		Elections	
Federal votes	(1)	(2)	(3)	(4)
& elections	(male)	(male)	(male)	(male)
	turnout	turnout	turnout	turnout
enfranchisement	-5.854***	-7.506***	-1.567***	-3.196***
	(0.168)	(0.379)	(0.438)	(0.991)
enfranchisement * acceptance 1959 (=1)	1.804***		0.686	
	(0.311)		(0.756)	
enfranchisement * acceptance 1959 (cont.)		0.054***		0.048**
		(0.009)		(0.024)
Municipal FE	✓	✓	✓	✓
Vote FE	✓	✓		
Year FE			✓	✓
Control variables	✓	✓	✓	✓
Observations	93,567	93,567	7,163	7,157
R-squared	0.578	0.578	0.050	0.051
Number of municipalities	1,478	1,478	1,263	1,262

Note: Robust standard errors in parentheses. The base effect of acceptance 1959 is omitted because it is a time-invariant variable. Control variables include population (log), pensioner, women, agriculture, foreigner, pensioner, unemployed, working women, German-speaking, married, and Catholics (in shares). In the referendums sample, we introduce a dummy variable to control for cantonal votes taking place on the same day. Cantons with compulsory voting are excluded from the sample.
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Participation-enhancing institutions

For the canton of Vaud, we have found that turnout in the aftermath of female enfranchisement differed based on the institutional background (town meeting vs. parliament municipalities). For women, turnout was lower in *town meeting* municipalities (see Table 4.6.1). This, to some extent, contradicts the literature showing that extensive participation rights can increase civic virtue and duty, perception of individual political efficacy, and political interest, hence leading to higher levels of civic engagement (e.g. Benz and Stutzer, 2004; Bowler and Donovan, 2002; Frey, 1997; Smith and Tolbert, 2007). These potential positive effects, however, may need time to evolve. Therefore, we conduct a

closer analysis of the old (male) and new (female) turnout based on the institutional background and over time.

For the period after the cantonal enfranchisement in 1959 in Vaud until the introduction of federal female suffrage (1971), we dispose of data on the *gender-specific turnout* of the municipalities in the canton of Vaud (see Section 4.4). Moreover, we dispose of information on the institutional background of the municipalities (see Section 4.2). We can, therefore, look at the turnout evolution of the old (male) and new (female) electorate in the years after female enfranchisement. A graphical illustration of average municipal turnout from 1959 to 1970 by gender and institutional background in the canton of Vaud is provided in Figure 4.6.5.
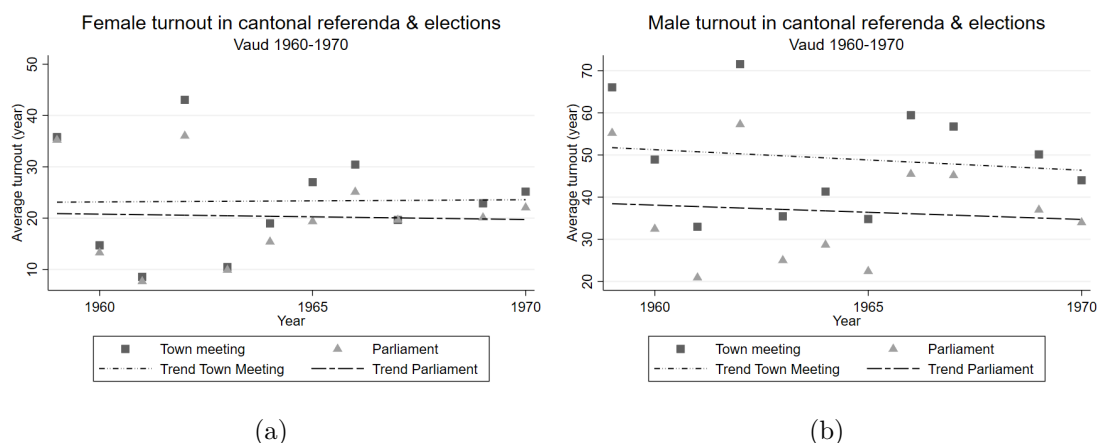

(a)

(b)

Figure 4.6.5: Female & male turnout by institutional background – VD, 1959–1970.

The trends of Figure 4.6.5 offer at least two additional insights: First, the evolution of female turnout seemed to be more positive under *town meeting* compared to *parliament* municipalities. Second, the turnout of the male electorate seemed to drop stronger in *town meeting* compared to *parliament* municipalities after the enfranchisement of women. How can these observations be interpreted? One potential explanation for the evolution of female turnout refers to direct democratic institutions unfolding a participation-enhancing effect over time. The integrative force of extensive participation rights for new voters is, for instance, shown by Kim (2019) for female participation in Sweden and by Manatschal (2021) for some groups of non-citizens in Switzerland. For the potentially stronger male turnout drop under direct democratic municipalities, a potential explanation could be the stronger devaluation effect as *town meetings* can be seen as a sphere with a strong male identity (see also Section 4.3).[33]

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[33]Another potential channel that could explain this observation is the difficulty of parents to participate in town meetings simultaneously. Therefore, some men may participate less in town meetings after female enfranchisement. Hence, the participation-enhancing effect of this direct-democratic institution might vanish.

For the old (male) electorate of the canton of Vaud, we further dispose of turnout observations in cantonal and federal votes and elections *before* and *after* the enfranchisement of women. This allows us to examine the relationship between male turnout and the institutional background separately in a male-only and a mixed-gender political sphere. We estimate an OLS regression with day-fixed effects and a reduced (Spec. 1 & 3) and the full set of controls (Spec. 2 & 4). Because the institutional background *town meeting* is a time-invariant variable, it is not possible to run the estimations with municipality fixed effects.

Table 4.6.3: Male turnout by institutional background – VD, 1945–1970.

Cantonal and federal votes & elections (VD)	Before 1959		After 1959	
	(1) male turnout	(2) male turnout	(3) male turnout	(4) male turnout
town meeting	2.413***	2.443***	0.687**	0.585*
	(0.282)	(0.282)	(0.340)	(0.337)
Rurality	✓	✓	✓	✓
Demography		✓		✓
Economy		✓		✓
Culture		✓		✓
Day FE	✓	✓	✓	✓
Observations	18,269	18,269	12,712	12,712
R-squared	0.741	0.743	0.660	0.671

Note: Robust standard errors in parentheses. Control variables include population (log), pensioner, women, agriculture, foreigner, pensioner, unemployed, working women, married, and Catholics (in shares).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4.6.3 depicts how the institutional background relates to male turnout for a split sample in the pre- and post-female suffrage period.[34] The findings in Table 4.6.3 provide interesting insights: the level of male turnout is—after controlling for different municipal backgrounds—in the periods with and without female suffrage higher under *town meeting* compared with parliament municipalities. This provides evidence that extensive participation rights are indeed related to a higher turnout of an experienced electorate (see also Figure 4.6.4). However, the coefficient is larger for the period before 1959. In the robustness checks in Table 4.B.4, we exclude outliers in terms of *population* or *acceptance 1959*. The coefficient for the pre-period remains stable in size and signif-

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[34]Please note that these estimations are based on a combination of the data set on federal votes and elections and the data set of cantonal votes and elections for the male electorate of the municipalities of Vaud. Therefore, it provides more observations than the gender-specific turnout data used for the estimations in Table 4.6.1.

icance, and the coefficient for the post-period becomes smaller in terms of size, losing statistical significance.

These results provide evidence that the positive relationship between *town meeting* and *male turnout* was stronger in the period without female suffrage. To more closely look at the trends in the period before and after female enfranchisement, we further perform an OLS regression for the two split samples where we interact the variable *town meeting* with that of *year*. The variable *year* indicates for how many years women have been granted cantonal voting rights. These estimations, where we introduce municipal and day-fixed effects, are provided in the Appendix in Table 4.B.5. In the period before female suffrage, the coefficients point to a smaller male turnout decline in *town meeting* municipalities compared with parliament municipalities. After female enfranchisement, Table 4.B.5 shows a faster turnout drop for *town meeting* municipalities than parliament municipalities (Spec. 3 & 4). Please note that these interaction terms may not only cover the effect of the institutional background over time but also of potential confounders that are correlated with the institutional background and evolve over time. However, these findings provide additional evidence that the institutional background matters for the integration of the electorate into politics and that it interacts with the enfranchisement of a new group.

4.7 Conclusion

Over the past century, many democracies have experienced suffrage extensions being given to new groups. This trend continues today with the demand for voting rights extensions to non-citizens or youth. Therefore, scholars have extensively analyzed the conditions for expanding the electorate (see, e.g., Acemoglu and Robinson, 2000; Bertocchi, 2011; Braun and Kvasnicka, 2013; Koukal et al., 2021). Studies have also looked at the effects of suffrage extensions on political outcomes (see, e.g., Aidt and Dallal, 2008) and on the newly enfranchised group (see, e.g., Slotwinski et al., 2023). However, what may seem like a simple question has not been thoroughly explored: How does voter turnout of both the existing and new electorate evolve after the extension of suffrage? The difficulty in answering this question is that, in many situations, we lack gender-specific turnout data or a reliable control group.

This chapter contributed to this question by analyzing Swiss gender-specific turnout data before and after female enfranchisement. Because of the extensive federalism and direct democracy in Switzerland, we can observe rich gender-specific turnout data under different institutional and societal backgrounds around female enfranchisement. We have focused on how male turnout in federal votes and elections (1945–1970) has evolved after the enfranchisement of women by analyzing the staggered introduction of female suffrage on the cantonal level. Additionally, with gender-specific turnout data and information on the municipal backgrounds of one canton, we have gained more insights into the developments and determinants of male and female voter turnout after women gained the right to vote. Importantly, we observe turnout on a higher governmental level than the variations we are interested in and, hence, do not capture changes in the pivotality

of the voters. Therefore, in our empirical setting, we can rule out that the change in the probability of a voter being decisive (e.g., Downs, 1957) affects the observed gender-specific turnout.

In our event study analysis, we find that (federal) male turnout dropped by about five percentage points after the introduction of cantonal female suffrage. Because our estimates suggest that the turnout drop is more pronounced in municipalities where men's opposition against female suffrage was initially stronger, a lower consumption value is one possible mechanism to explain the male turnout decline. Men's identity benefits from participating in politics can be affected by the shift of the political arena from a "pure male sphere" to a "mixed-gender sphere" (e.g., Akerlof and Kranton, 2000; Rogers et al., 2013). Moreover, shortly after their enfranchisement, women turned out less in contexts where men's opposition toward female suffrage was larger. This can be explained by lower social incentives for turning out (e.g., Funk, 2010; Gerber et al., 2008), which decreases women's net utility to turn out.

Furthermore, also the institutional background is associated with male and female turnout after women's enfranchisement. After female suffrage, male turnout decreased more in municipalities with more extensive direct-democratic rights. Shortly after their enfranchisement, also women from direct-democratic municipalities turned out less compared to women from representative-democratic municipalities. This negative relationship, however, seems to reverse over time. This provides evidence that although participation-enhancing institutions—such as, in our case, direct democracy—can positively affect turnout, a swift change in the electorate can create a backlash in the short term. The turnout drop for the established electorate can be larger, and the entry into politics for new groups can be more difficult. These findings also go in line with evidence from Chapter 1 and Chapter 2, which demonstrate a greater reluctance among the electorate to enfranchise new groups under a more direct democratic background unless they are enfranchised at the local level.

Suffrage extensions are discussed in many countries and are thought to make democracies more inclusive. However, to fully understand its effects on democracies, the current and new electorates must be separately analyzed. Suffrage extensions swiftly change the political arena, which is likely to affect the old electorate—in a positive way, if they are happy with the enlargement of the electorate, or in a negative way, if they were happy with how it was before. Therefore, suffrage extensions need broad support to avoid the unintended effects on the old electorate. For the successful integration of the new electorate into politics, understanding the effects of different institutional backgrounds is important. Extensive participation rights may be one possibility to promote political integration in the long term.

For future research, it is promising to analyze if there are heterogeneous effects regarding the group to be enfranchised. The political arena may not change similarly when enfranchising women, non-citizens, or the youth. Moreover, we have focused on turnout as an outcome variable. However, suffrage extensions may affect not only the turnout decision of the old electorate but also their vote choice. It is promising to analyze the extent to which the old electorate incorporated the preferences of the future

new electorate in their vote choice before the suffrage extension in order to prevent social unrest. Another exciting research avenue is digging deeper into potential mechanisms using individual-level data. For historical suffrage extensions, large-scale surveys were not available. Nowadays, they are common in most countries and can be used to analyze suffrage extensions to youth or non-citizens. Although individuals are likely to over-report turnout in survey data, it can still offer valuable insights into what motivates the old and new electorates to turn out.

Appendix

Chapter 4 Appendix

4.A   Background information on the data

Table 4.A.1: Chronology of female suffrage in the cantons.

Acceptance date	canton	level	optional
February 1, 1959	Vaud	integral	-
September 27, 1959	Neuchatel	integral	-
March 6, 1960	Geneva	integral	-
October 7, 1962	Grison	municipal	yes
June 26, 1966	Basel City	integral	-
February 18, 1968	Bern	municipal	yes
May 19, 1968	Obwalden	municipal	yes
September 14, 1969	Zurich	municipal	yes
October 19, 1969	Ticino	integral	-
April 12, 1970	Valais	integral	-
April 26, 1970	Nidwalden	municipal	no
September 27, 1970	Basel County	integral	-
October 25, 1970	Lucerne	integral	-
November 15, 1970	Zurich	integral	-
February 7, 1971	all cantons	federal	-
February 7, 1971	Fribourg	integral	-
February 7, 1971	Zug	integral	-
February 7, 1971	Schaffhausen	integral	-
February 7, 1971	Aargau	integral	-
May 5, 1971	Glarus	integral	-
December 12, 1971	Bern/ Jura	integral	-
December 12, 1971	Thurgau	integral	-
January 23, 1972	St. Gallen	integral	-
April 30, 1972	Appenzell A.Rh.	municipal	no
September 24, 1972	Obwalden	integral	-
March 5, 1972	Schwyz	integral	-
March 5, 1973	Uri	integral	-
March 2, 1980	Solothurn	integral	-
February 27, 1983	Grison	integral	-
April 30, 1989	Appenzell A.Rh.	integral	-
November 27, 1990	Appenzell I.Rh.	integral	-

Note: The chronology is based on Ruckstuhl (1986).

Table 4.A.2: Overview of enfranchisement referendums in Swiss cantons, 1919-1983.

vote ID	year	date	canton	level	optional	yes share	vote ID	year	date	canton	level	optional	yes share
2	1919	29.06.1919	NE	integral	0	30,8	52	1968	18.02.1968	SO	cantonal	0	42,5
3	1920	08.02.1920	BS	integral	0	35,0	53	1968	18.02.1968	SO	municipal	0	47,3
5	1920	08.02.1920	ZH	integral	0	19,6	54	1968	19.05.1968	OW	municipal	1	78,7
4	1921	06.09.1921	SG	integral	0	31,6	55	1968	23.06.1968	BL	cantonal	0	68,1
7	1921	16.10.1921	GE	integral	0	31,9	56	1968	20.10.1968	GR	integral	0	39,0
9	1926	11.07.1926	BL	school issues	0	48,7	57	1969	26.01.1969	TG	school issues	0	50,8
10	1927	15.05.1927	BS	integral	0	29,2	59	1969	14.09.1969	SH	integral	0	47,2
13	1946	16.06.1946	BS	integral	0	37,1	60	1969	14.09.1969	ZH	municipal	1	57,9
14	1946	07.07.1946	BL	integral	0	26,5	61	1969	19.10.1969	TI	integral	0	63,0
15	1946	29.09.1946	GE	integral	0	43,7	62	1969	16.11.1969	FR	fundamental approval	1	71,1
16	1946	08.11.1946	TI	integral	0	22,8	63	1970	12.04.1970	VS	integral	0	72,6
17	1947	30.11.1947	ZH	integral	0	22,5	68	1970	27.09.1970	BL	municipal	0	81,1
19	1948	14.03.1948	NE	municipal	0	32,8	69	1970	27.09.1970	SG	municipal	1	47,3
20	1948	14.11.1948	SO	municipal	0	49,5	70	1970	25.10.1970	LU	integral	0	63,0
21	1951	25.02.1951	VD	municipal	1	39,2	71	1970	15.11.1970	SO	municipal	1	65,9
23	1953	07.06.1953	GE	integral	0	42,8	72	1970	15.11.1970	ZH	integral	0	67,0
25	1954	05.12.1954	BS	integral	0	45,1	73	1971	07.02.1971	ALL	federal	0	65,7
26	1954	05.12.1954	ZH	integral	0	28,7	74	1971	07.02.1971	FR	integral	0	73,8
27	1955	15.05.1955	BL	stepwise	1	43,7	75	1971	07.02.1971	ZG	integral	0	62,5
33	1956	04.03.1956	BE	municipal	1	45,6	76	1971	07.02.1971	SH	integral	0	57,1
34	1957	03.11.1957	BS	municipal	1	59,7	77	1971	07.02.1971	AG	integral	0	51,7
35	1959	01.02.1959	ALL	federal	0	33,1	78	1971	07.02.1971	SZ	integral	1	47,0
36	1959	01.02.1959	VD	integral	0	52,6	81	1971	06.06.1971	SO	cantonal	0	79,5
37	1959	27.09.1959	NE	integral	0	53,6	82	1971	12.12.1971	BE	cantonal	0	82,8
38	1960	06.03.1960	GE	integral	0	55,4	84	1971	12.12.1971	TG	integral	0	62,7
39	1960	04.12.1960	LU	municipal	1	24,5	85	1972	23.01.1972	SG	integral	0	65,3
42	1962	07.10.1962	GR	municipal	1	59,0	86	1972	30.01.1972	UR	integral	1	57,1
44	1966	13.03.1966	BL	stepwise	1	57,3	87	1972	05.03.1972	SZ	integral	0	68,2
45	1966	24.04.1966	TI	integral	0	48,3	88	1972	05.03.1972	UR	integral	0	62,9
46	1966	26.06.1966	BS	integral	0	60,0	89	1972	05.03.1972	GR	cantonal	0	72,2
47	1966	20.11.1966	ZH	integral	0	46,4	94	1972	24.09.1972	OW	cantonal	0	58,7
49	1967	28.05.1967	SH	integral	0	45,0	99	1980	02.03.1980	SO	municipal	0	65,4
50	1967	04.06.1967	BL	stepwise	1	63,9	101	1983	27.02.1983	GR	municipal	0	62,9
51	1968	18.02.1968	BE	municipal	1	52,1							

Notes: Listed are only those votes which are part of the dataset. Gaps in the data can be explained either by lack of data in general or lack of municipality information. Missing are votes held in cantons with cantonal assemblies for cantonal and municipal matters. In those cantons municipal voting data is only available for the two federal votes. Main source of this list is Ruckstuhl (1986).

Figure 4.A.1: Municipal gender-specific turnout – VD, 1959-1970.

Table 4.A.3: Votes & elections with gender-specific turnout data – VD, 1959-1970.

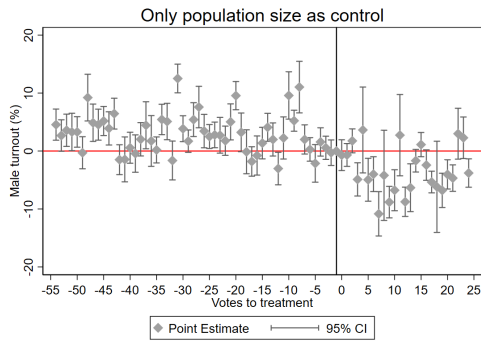| Year	Date	Level	Type	Detail	Fem. turnout	Male turnout
1959	10/27/1959	Fed.	Election	Council of States, 1st Round	37.70%	55.70%
1960	03/27/1960	Cant.	Vote	Mandatory Referendum	10.50%	22.40%
1960	12/04/1960	Cant.	Vote	Popular Initiative	18.10%	41.60%
1961	04/16/1961	Cant.	Vote	Popular Initiative	12.90%	27.00%
1961	06/11/1961	Cant.	Vote	Mandatory Referendum	3.10%	9.30%
1961	10/22/1961	Cant.	Vote	Direct Counterproposal	8.60%	25.70%
1962	03/04/1962	Cant.	Election	Cantonal government, 1st Round	34.30%	54.10%
1963	06/30/1963	Cant.	Vote	Mandatory Referendum	2.60%	8.20%
1963	10/30/1963	Fed.	Election	Council of States, 1st Round	18.40%	42.80%
1964	06/28/1964	Cant.	Vote	Popular Initiative	16.40%	29.70%
1965	10/03/1965	Cant.	Vote	Mandatory Referendum	17.50%	21.20%
1966	01/30/1966	Cant.	Vote	Mandatory Referendum	23.40%	44.00%
1966	03/06/1966	Cant.	Election	Cantonal government, 1st Round	30.90%	51.50%
1966	03/27/1966	Cant.	Election	Cantonal government, 2nd Round	23.60%	42.90%
1967	10/29/1967	Fed.	Election	Council of States, 1st Round	21.40%	46.10%
1969	01/26/1969	Cant.	Election	Cantonal government, Substitute	19.20%	35.40%
1970	03/01/1970	Cant.	Election	Cantonal government, 1st Round	35.80%	52.30%
1970	05/10/1970	Cant.	Vote	Mandatory Referendum	21.20%	30.00%
1970	10/18/1970	Cant.	Vote	Popular Initiative	9.50%	21.20%

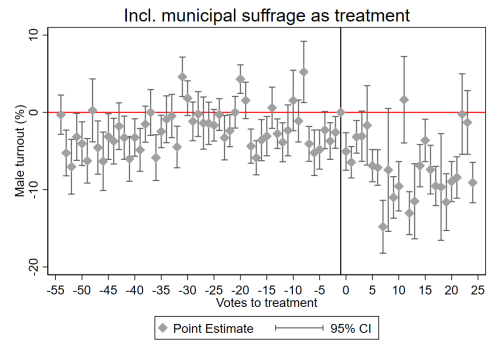## 4.B Robustness and placebo tests

Event study design - robustness

We provide several robustness tests for the event study design of our referenda data in Figure 4.B.1. If nothing is specified here, the estimation procedure (control variables, fixed effects, clustering of standard errors, treatment of compulsory voting...) is described as in Model 4.2.

In subplot 4.B.1a, we only include population size as a control variable. In subplot 4.B.1b, all municipalities where female suffrage was granted on the municipal or cantonal level are considered as treated; this is in contrast to our base estimations where we only take the cantons with cantonal female suffrage as treated (see Table 4.A.1). In subplot 4.B.1c we enlarge the sample to cantons that had compulsory voting installed but had no change during our period of observation. As in all estimations, municipality fixed effects are included, which should absorb the additional turnout from compulsory voting. Although the decision for cantonal female suffrage is decided on the cantonal level (and we observe municipalities), the treatment might still not be randomly distributed. To make the treated and control groups more similar, we restrict the sample of the treated cantons to the early introducers (VD, NE, GE) and the control group to the cantons that have introduced female suffrage before 1971 in subplot 4.B.1d. In subplot 4.B.1e, we exclude the latecomer cantons (>1971) from the sample. Moreover, in subplot 4.B.1f, we exclude the municipalities with the 25% highest acceptance rates in the 1959 vote. This reassures us that our results are not driven by outliers. In subplot 4.B.1g, we exclude the canton of Zurich from the sample, which introduced cantonal female suffrage just a few months before the federal vote in 1971. Finally, in subplot 4.B.1h, we change the reference period (which is set to -1 by the default mode) to 2 votes before the cantonal female suffrage was introduced.
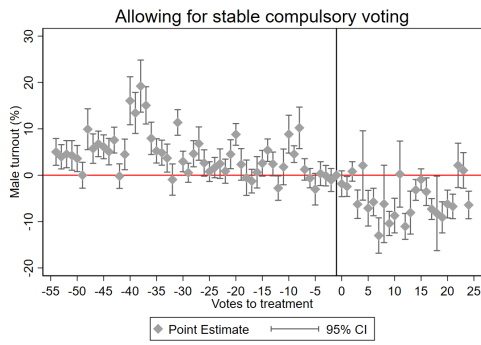
Moreover, we run the event study design also with a year-distance variable as the running variable. Here, the distance is not measured in how many votes a municipality is away from the introduction of female suffrage but in how many years from the first vote when women were allowed to participate. We still use referenda and municipality-fixed effects, but the votes are now pooled within one calendar year. Figure 4.B.2 provides the robustness checks to the event study design in Figure 4.6.2. Subplot 4.B.2a only includes population size as control, subplot 4.B.2b again treats also the municipalities with cantons with municipal suffrage as treated, subplot 4.B.2c enlarges the sample to municipalities with compulsory voting (but no change in this regulation), subplot 4.B.2d takes the first-movers (VD, GE, NE) as treated and the relatively early movers as controls (< 1972), subplot 4.B.2e excludes the latecomers (> 1971) from the control group, subplot 4.B.2f excludes the highest 25 % acceptance rates in the 1959 vote, subplot 4.B.2g excludes the canton of ZH. In subplot 4.B.2h, we change the first treatment point to the first full year where women were allowed to participate (in the baseline, the year where women could participate in their first vote indicated the first treatment point).
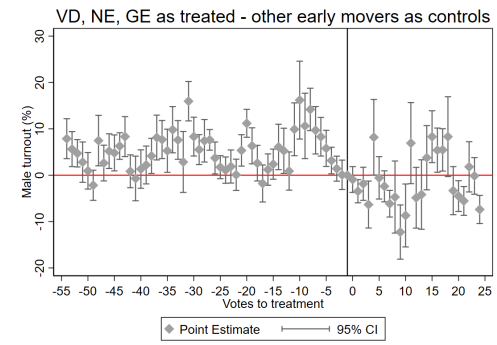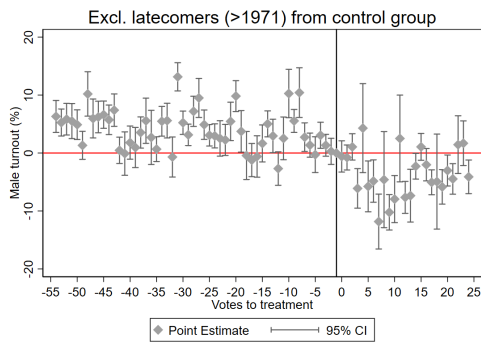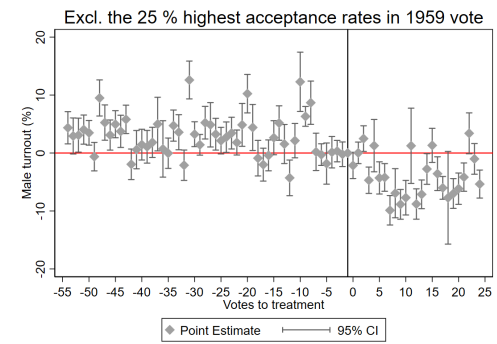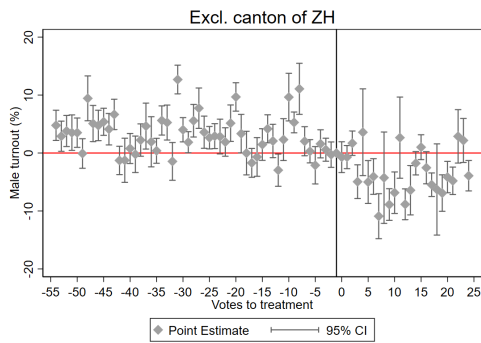
155

Only population size as control

Incl. municipal suffrage as treatment

(a)

(b)

Allowing for stable compulsory voting

VD, NE, GE as treated - other early movers as controls

(c)

(d)

Excl. latecomers (>1971) from control group

Excl. the 25 % highest acceptance rates in 1959 vote

(e)

(f)

Excl. canton of ZH
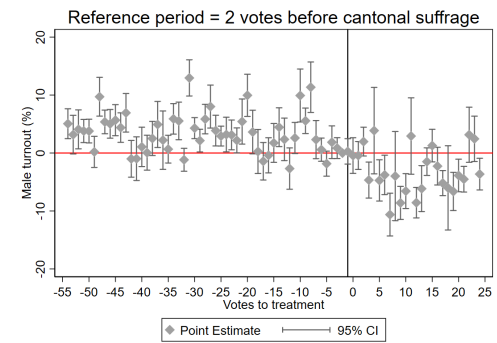
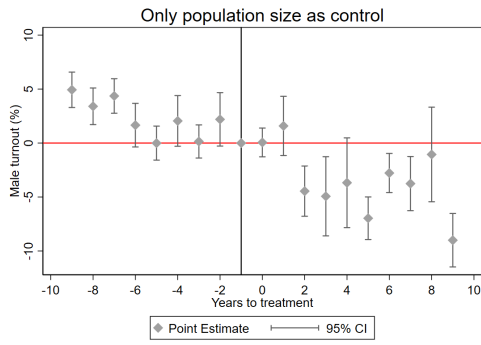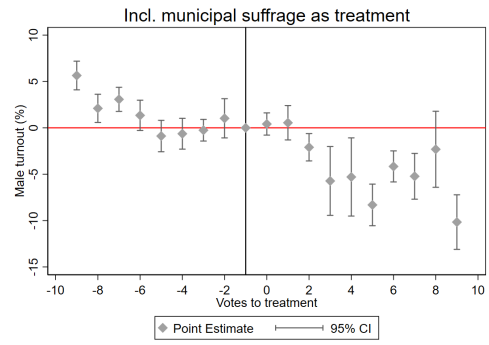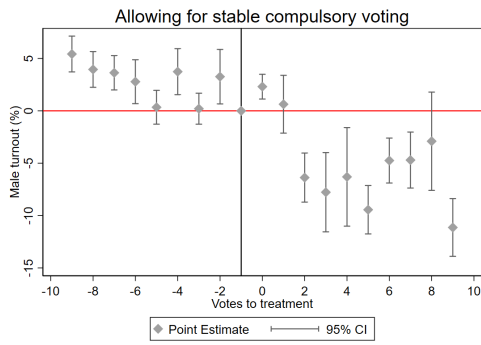Reference period = 2 votes before cantonal suffrage

(g)

156

(h)

Figure 4.B.1: Male turnout – Robustness for the referendum-day event study.
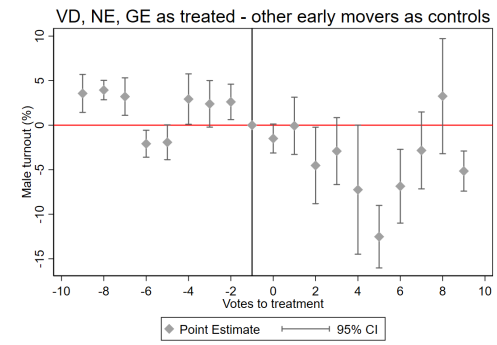
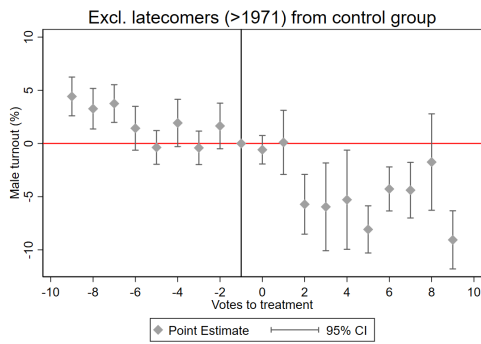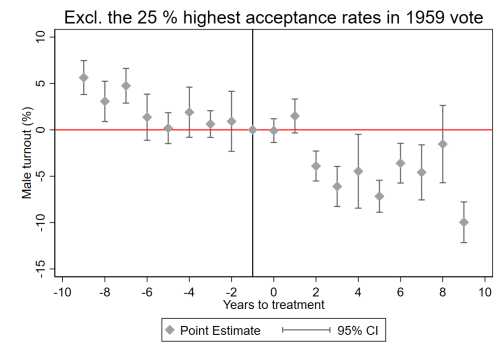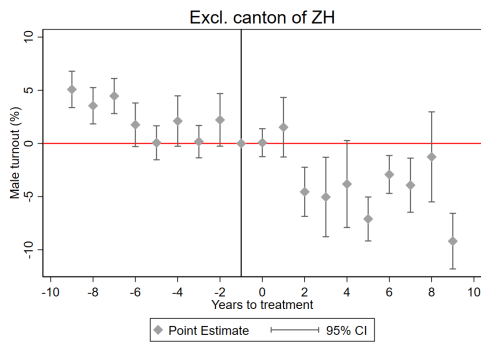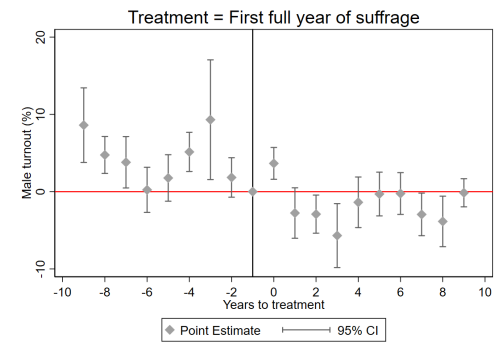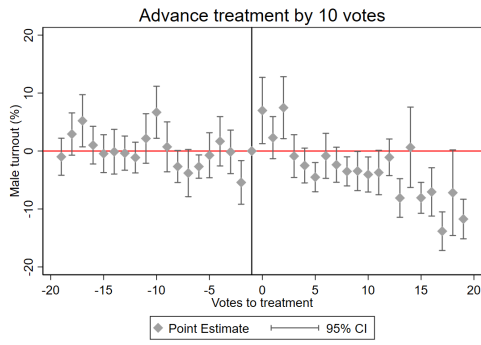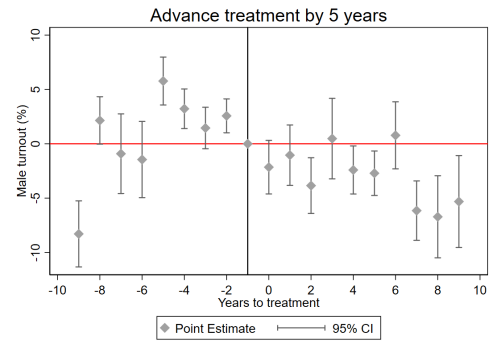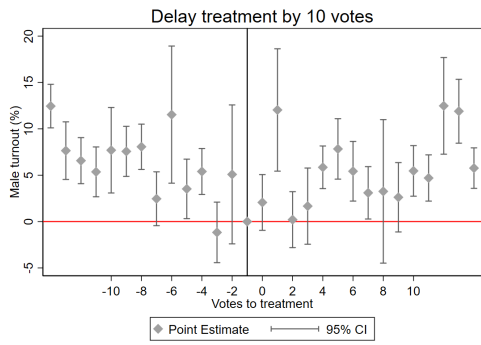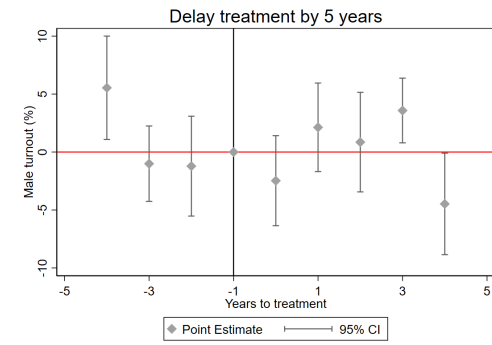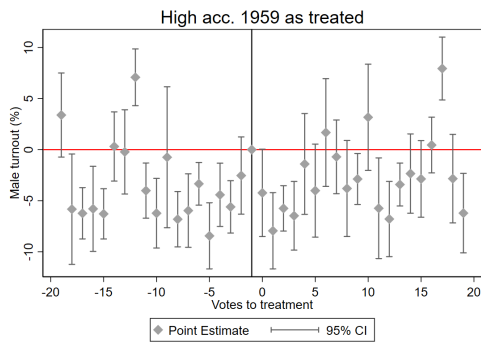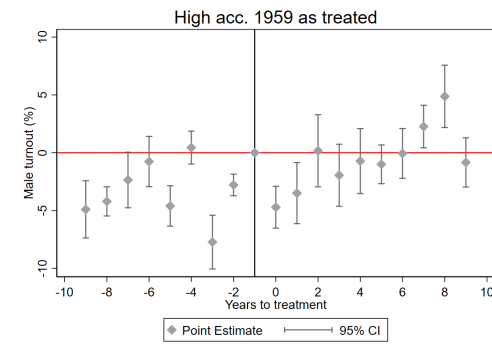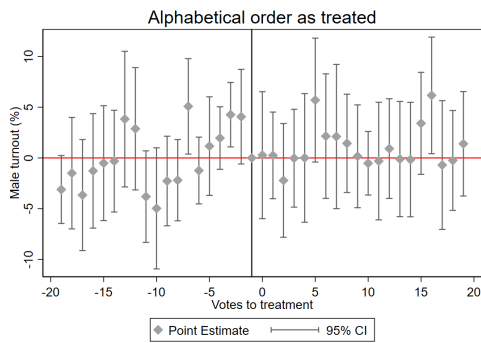Figure 4.B.2: Male turnout – Robustness for the referendum-year event study.
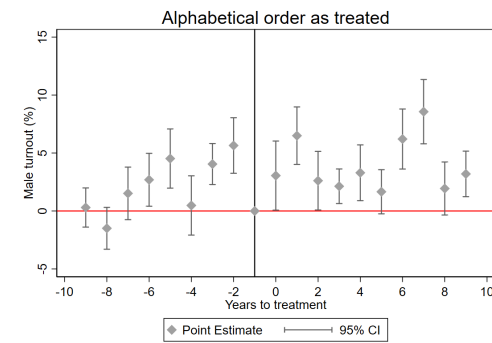
Figure 4.B.3: Male turnout – Placebo tests for the referendum event study.
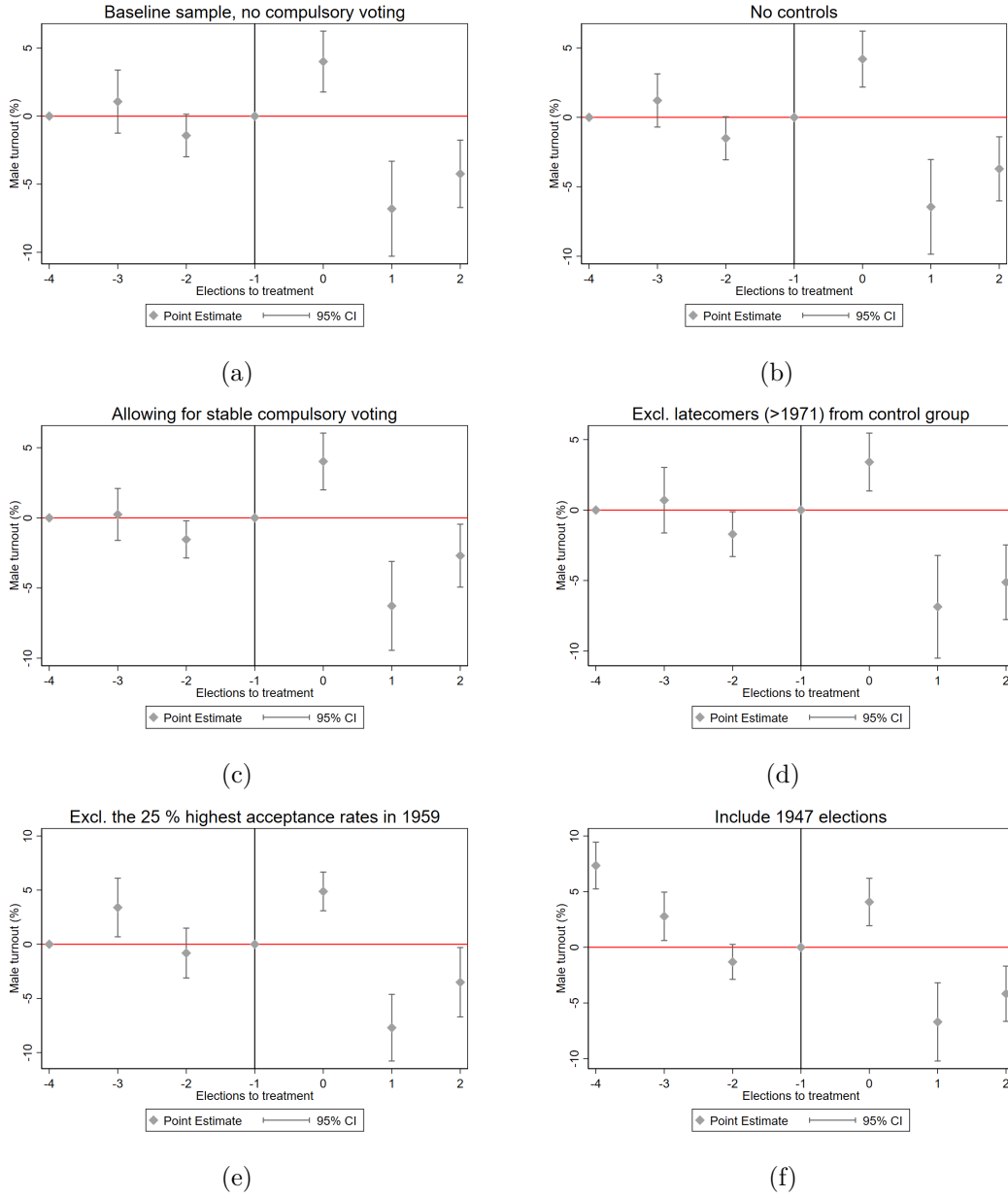
Figure 4.B.4: Male turnout – Robustness for the election-day event study.

Table 4.B.2: Male turnout & acceptance – CH, 1950-1970 (with compulsory voting).

Federal votes & elections	Referenda		Elections	
	(1) (male) turnout	(2) (male) turnout	(3) (male) turnout	(4) (male) turnout
VARIABLES				
enfranchisement	-4.983***	-6.862***	-0.448***	-2.277***
	(0.168)	(0.382)	(0.398)	(0.931)
enfranchisement * acceptance 1959 (=1)	2.056***		0.775	
	(0.314)		(0.717)	
enfranchisement * acceptance 1959 (cont.)		0.061***		0.053**
		(0.009)		(0.023)
Municipal FE	✓	✓	✓	✓
Vote FE	✓	✓		
Year FE			✓	✓
Control variables	✓	✓	✓	✓
Observations	90,173	90,173	7,991	7,081
R-squared	0.530	0.530	0.061	0.062
Number of municipalities	1,602	1,602	1,618	1,616

Note: Robust standard errors in parentheses. The base effect of acceptance 1959 is omitted, as it is a time-invariant variable. Control variables include population (log), pensioner, women, agriculture, foreigner, pensioner, unemployed, working women, German-speaking, married, and Catholics (in shares). In the referenda sample, we introduce a dummy variable to control for cantonal votes taking place on the same day. Cantons with compulsory voting are included in the sample if there was no change in regulation over our period of observation (sample restricted to the years 1950-1970).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$

Table 4.B.1: Male turnout & acceptance (Robustness) – CH, 1945-1970.

Outcome: male turnout in federal referenda	(1) no controls	(2) cantonal time trends	(3) no late comer	(4) compulsory allowed	(5) no controls	(6) cantonal time trends	(7) no late comer	(8) compulsory allowed
enfranchisement	-6.268***	-4.641***	-4.990***	-6.862***	-5.707***	-3.065***	-3.559***	-4.983***
	(0.363)	(0.419)	(0.421)	(0.382)	(0.156)	(0.255)	(0.262)	(0.168)
enfranchisement * acceptance 1959 (cont.)	0.019**	0.052***	0.048***	0.061***				
	(0.009)	(0.009)	(0.009)	(0.009)				
enfranchisement * acceptance 1959 (=1)					0.773***	1.664***	1.580***	2.056***
					(0.299)	(0.314)	(0.314)	(0.314)
Municipal FE	✓	✓	✓	✓	✓	✓	✓	✓
Vote FE	✓	✓	✓	✓	✓	✓	✓	✓
Cantonal time trends		✓	✓	✓	✓	✓	✓	✓
Control variables			✓	✓	✓	✓	✓	✓
Observations	93,567	93,567	80,269	90,173	93,567	93,567	80,269	90,173
R-squared	0.576	0.583	0.595	0.530	0.576	0.583	0.595	0.530
Number of municipalities	1,478	1,478	1,279	1,602	1,478	1,478	1,279	1,602

Note: Standard errors in parentheses. The base effect of acceptance 1959 is omitted because it is a time-invariant variable. In Spec. 3 & 6 we exclude the cantons that introduced female suffrage late (after the second federal vote) from the sample. In Spec. 4 & 8 we also include cantons with compulsory voting in our sample if the regulation remained stable over our period of observation. The full set of controls includes population (log), agriculture, pensioner, women, foreigner, unemployed, women working, German speaking, married, Catholics (in shares). We further introduce a dummy variable that indicates whether a cantonal vote took place on the same day.

*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 4.B.3: Mun. background & gender-specific turnout (Robust.) – VD, 1959-1970.

	female turnout		male turnout		gender turnout gap	
Cantonal votes	(1)	(2)	(3)	(4)	(5)	(6)
& elections	excl.	excl.	excl.	excl.	excl.	excl.
VARIABLES	pop outliers	acc. 1959	pop outliers	acc. 1959	pop outliers	acc. 1959
town meeting	-1.533***	-1.816***	-0.274	0.650	1.257***	2.463***
	(0.466)	(0.536)	(0.486)	(0.558)	(0.434)	(0.502)
acc. 1959 (cont.)	0.110***	0.083***	0.086***	0.069***	-0.025**	-0.014
	(0.011)	(0.015)	(0.011)	(0.016)	(0.010)	(0.014)
Day FE	✓	✓	✓	✓	✓	✓
Year FE	✓	✓	✓	✓	✓	✓
Control variables	✓	✓	✓	✓	✓	✓
Observations	7,135	5,738	7,135	5,738	7,135	5,738
R-squared	0.595	0.595	0.732	0.725	0.511	0.490

Note: Standard errors in parentheses. In Spec. 1 & 3 & 5 population outliers (smallest and largest 1%) are excl. from the sample. In Spec. 2 & 4 & 6 we exclude municipalities that accepted federal female suffrage in 1959 from the sample. The full set of controls includes population (log), agriculture, pensioner, women, foreigner, unemployed, women working, married, Catholics (in shares).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4.B.4: Male turnout & institutional background (Robustness) — VD, 1945-1970.

Outcome: Male turnout	**Before 1959**		**After 1959**	
Cantonal and federal	(1)	(2)	(3)	(4)
votes & elections	excl.	excl.	excl.	excl.
	pop 5%	acc. 1959	pop 5%	acc. 1959
town meeting	1.642***	1.568***	-0.717*	-0.387
	(0.311)	(0.320)	(0.373)	(0.377)
Day FE	✓	✓	✓	✓
Control variables	✓	✓	✓	✓
Observations	16,387	15,092	10,981	10,506
R-squared	0.756	0.741	0.687	0.675

Note: Standard errors in parentheses. In Spec. 1 & 3, population outliers (smallest and largest 5%) are excl. from the sample. In Spec. 2 & 4, municipalities that have accepted federal female suffrage in 1959 are excl. from the sample. The full set of controls includes population (log), agriculture, pensioner, women, foreigner, unemployed, women working, married, Catholics (in shares).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

Table 4.B.5: Trends in male turnout & institutional background – VD, 1945-1970.

	Before 1959		After 1959	
Cantonal and federal votes & elections (VD)	(1) male turnout	(2) male turnout	(3) male turnout	(4) male turnout
town meeting	-	-	-	-
year (cont.)	-1.161***	-1.075***	-0.696***	-0.274***
	(0.051)	(0.056)	(0.066)	(0.083)
town meeting (=1) * year (cont.)	0.307***	0.237***	-0.097**	-0.201***
	(0.035)	(0.038)	(0.046)	(0.056)
Rurality		✓		✓
Demography		✓		✓
Economy		✓		✓
Culture		✓		✓
Day FE	✓	✓	✓	✓
Municipal FE	✓	✓	✓	✓
Observations	18,269	18,269	12,712	12,712
R-squared	0.809	0.810	0.734	0.736
Number of municipalities	374	374	374	374

Note: Robust standard errors in parentheses. The base effect of town meeting is omitted, as it is a time-invariant variable. Control variables include population (log), pensioner, women, agriculture, foreigner, pensioner, unemployed, working women, married, and Catholics (in shares).
*** $p < 0.01$, ** $p < 0.05$, * $p < 0.1$.

4.C   RDD in the canton of Vaud

The canton of Vaud uses a population threshold for appointing a parliament as the local legislative institution. This allows us theoretically to analyze the effect of direct relative to representative democracy on female turnout, male turnout, and the gender turnout gap in a regression discontinuity design.

The population in a municipality m thus serves as our running variable X_m. Note that the mandatory threshold for the appointment of a parliament in our period of observation (1960-1970) referred to 800 inhabitants, the relevant cutoff $\bar{x}$ for our RD-analysis, however, is at 600 inhabitants. This is the case since, until 1960, all municipalities with at least 600 inhabitants were obliged to install a parliament, and returning to a town meeting after the change of the threshold was not common. The treatment status is determined as $T_i = \mathbb{1}(X_i \leq \bar{x})$ for the cutoff $\bar{x}$ at 600 inhabitants.

We are, therefore able to estimate the RD treatment effect at the cutoff:

$$\tau = \tau(\bar{x}) = \mathbb{E}\{turnout_m(1) - turnout_m(0)|X_m = \bar{x}\} \tag{4.5}$$

As outcome variables serve as before female turnout, male turnout, and the turnout gap on the municipal level.

The key identifying assumption of the regression-discontinuity-design is that the treatment (or probability of the treatment) changes abruptly at the known threshold (see e.g. Cattaneo and Titiunik, 2022). Therefore, if we want to learn more about the local causal effect of the institutional background on male and female turnout, municipalities mustn't sort around this population threshold. This allows the use of units barely below the cutoff as comparison groups to the treatment groups barely above the cutoff. We perform the McCrary density test to analyze whether municipalities used manipulation to sort into treatment–there do not seem to be jumps in the density around the cutoff of 600 inhabitants (see Figure 4.C.1). Moreover, another important indicator for randomization around the cutoff is balanced covariates. Falsification tests are shown in Table 4.C.2. As there are not enough observations close to the cutoff the RDD is underpowered if the bandwidth is not chosen large enough. Therefore, the optimal bandwidth selection procedure rather ends in a relatively large bandwidth, which does not end in a well-balanced sample around the cutoff of 600 inhabitants. Especially for the Catholic and foreigner share, we find significant effects—unfortunately, due to missing statistical power, we are not able to control for these variables.

The coefficients from the regression discontinuity for the gender turnout gap (Spec. 5 & 6) point in the same direction as the OLS estimations from Table 4.6.1–with a parliament as the institutional background, the gender turnout gap seems to be smaller than with a town meeting. However, especially the estimated effect of having installed a parliament at the local level on male turnout (Table 4.C.1 Spec. 3 & 4) is negative and large in terms of size and statistically significant at the one percent level. Also, for the female turnout (Spec. 1 & 2), we find a bit of a lower but negative coefficient.
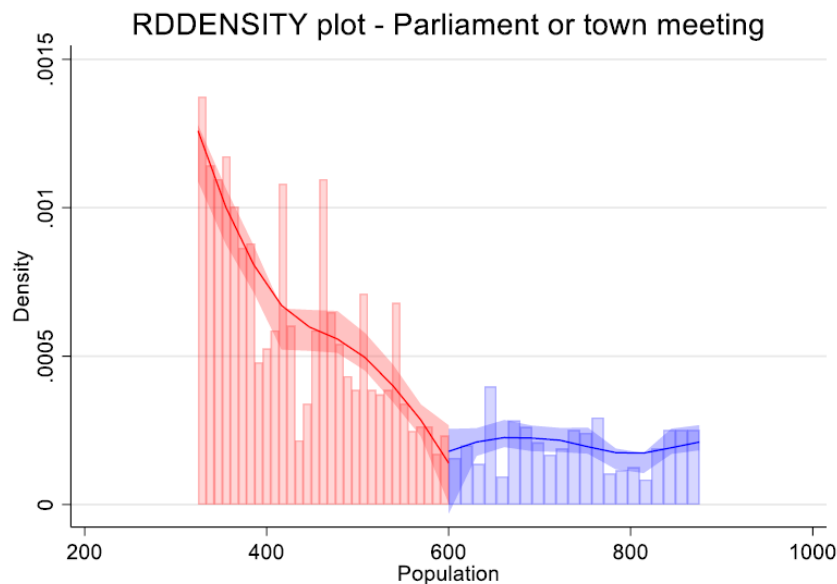
Figure 4.C.1: McCrary density plot - Institutional background in Vaud.

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Table 4.C.1: Town meeting & gender-specific turnout (RDD) – VD, 1959-1970.

VARIABLES	(1) Female turnout	(2) Female turnout	(3) Male turnout	(4) Male turnout	(5) Gender turnout gap	(6) Gender turnout gap
Parliament	-4.066*	-3.963*	-8.938***	-8.837***	-4.577**	-5.218**
	(2.160)	(2.384)	(2.710)	(2.862)	(2.048)	(2.151)
Obs.	384	384	384	384	384	384
Covariates	NO	NO	NO	NO	NO	NO
Bandwidth	243.1	388.1	235	449.4	258.4	480.4
Order polyn.	1	2	1	2	1	2

Note: Robust standard errors in parentheses. The relevant cutoff for the implementation of a *parliament* instead of a *town meeting* was 600 inhabitants.
*** p < 0.01, ** p < 0.05, * p < 0.1.

Table 4.C.2: Falsification tests for the RDD in the canton of Vaud.

VARIABLES	(1) agriculture	(2) agriculture	(3) women	(4) women	(5) pensioner	(6) pensioner	(7) unemployed	(8) unemployed
RD_Estimate	-5.699**	-6.747**	-3.032	-3.461	-2.497	-2.278	0	0
	(2.669)	(2.938)	(2.599)	(2.932)	(1.697)	(1.865)	(0)	(0)
Observations	384	384	384	384	384	384	384	384
Covariates	NO	NO	NO	NO	NO	NO	NO	NO
Bandwidth_left	252.7	422.2	296.4	477.4	287.3	496	123776	123776
Bandwidth_right	252.7	422.2	296.4	477.4	287.3	496	123776	123776
Order polyn.	1	2	1	2	1	2	1	2

VARIABLES	(9) women working	(10) women working	(11) foreigner	(12) foreigner	(13) married	(14) married	(15) Catholics	(16) Catholics
RD_Estimate	3.126	3.930	8.948**	10.06**	3.553	4.240	14.42***	15.42***
	(3.366)	(3.845)	(4.361)	(4.771)	(2.923)	(3.439)	(4.514)	(4.895)
Observations	384	384	384	384	384	384	384	384
Covariates	NO	NO	NO	NO	NO	NO	NO	NO
Bandwidth_left	313.7	491.9	301.7	529.6	317.1	502.6	227.8	375
Bandwidth_right	313.7	491.9	301.7	529.6	317.1	502.6	227.8	375
Order polyn.	1	2	1	2	1	2	1	2

*** p < 0.01, ** p < 0.05, * p < 0.1.

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