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Power to the People: Effects of Introducing Preference Voting in Swedish Municipalities

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Abstract: Sweden has had a semi-open list system since 1998. However, in 1994, a governmental pilot commenced, in which preference voting was introduced in seven municipalities. This natural experiment allows for a difference-in-differences framework. I look at the potentially causal effects of introducing preference voting on trust in politicians, voter turnout, political partisanship, the share of invalid votes, and voter support for Swedish political parties. I analyze individual-level survey data of Swedish voters from the Swedish National Election Studies Program (1982-1994) and aggregate-level data of Swedish elections from Statistics Sweden (1973-1994) using both static and dynamic difference-in-differences models. I find that introducing preference voting in pilot municipalities in 1994 caused an increase in trust in politicians; a decrease in partisanship; a decrease in the share of invalid votes; and a decrease in the share of votes for the Left Party. No effects were found on voter turnout or voter support for any other political party. These results are largely in line with the aims of the pilot. Not all of these results proved robust, though. Nonetheless, these findings complement the parsimonious governmental evaluation of the 1994 pilot, a large-scale government experiment, with a more thorough assessment that considers potential causality. The thesis also supports the notion that the electoral system can shape trust in politicians, which is a novel finding too, and fills a research gap on the causal relationship between partisanship and preference voting in Sweden.

Keywords: elections in Sweden, preference voting, semi-open list system, trust in politicians

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Contents

1 Introduction

A fundamental research question in political economics is the impact of institutions on political behavior. This thesis contributes to this overarching research question. More specifically, it investigates how the electoral system can strengthen the bond between voters and politicians, in terms of trust in politicians and political parties; voter turnout; political partisanship; invalid votes; and voter support for different parties. This is interesting for future policymaking, as there appears to be a general decline in trust in politicians and political institutions in contemporary Europe (Algan et al., 2017), and policymakers even refer to a European "trust crisis" (Murtin et al., 2018, p. 7). Furthermore, 2024 is "democracy's Super Bowl", Simon Tisdall wrote in *The Guardian* in December 2023. More than 40 countries, representing over 40 percent of the world's population, will have held elections in 2024 (Tisdall, 2023). The democracy Super Bowl of 2024 underscores the importance of election research.

The numerous elections of 2024 differ substantially when it comes to the opportunity to vote for individual people – in other words, to preference vote. There are namely two main electoral systems. For one, closed-list systems allow voters to vote for parties. In such systems, voters take the parties' candidate lists as given. For the other, open-list systems allow voters also to cross individual candidates on the parties' candidate lists. In open-list systems, the candidate with the most preference votes will automatically be allocated the first mandate. Then there is the intermediate of these main formats, the semi-open list system, in which voters can choose whether to vote for the party only and take their candidate list as is, or if they want to place a preference vote for someone on that candidate list. In semi-open list systems, candidates must gain a certain share of the total votes to circumvent the parties' ordering of candidates.

Sweden had a closed-list system until 1998, when a semi-open list system was implemented in the entire country for the general election. Already in the election year of 1994, however, seven Swedish municipalities were piloted for a semi-open list system in elections to the Municipal Councils. This serves as a natural experiment, allowing for a difference-in-differences framework. Given the design of this experiment, not much can be said about long-term effects – all municipalities were treated one year post-treatment, in the 1995 European Parliament election, and in the next general election of 1998. However, immediate causal effects are also of great interest.

In the 1990s, there was much discussion in Sweden on the declining political participation and trust in politicians (Holmberg et al., 1999; Möller, 1999; Petersson et al., 1998, 1999; Westerståhl, 1993; SOU 1993:21; SOU 1996:66). The goals of introducing preference voting in Swedish municipalities were to increase trust in politicians and the political system by strengthening the bond between voters and politicians, thereby also increasing voter turnout (Holmberg & Möller, 1999; SOU 1993:21; SOU 1996:66; SOU 1999:136). There are more outcomes of interest, though, given previous research, made possible to examine by the richness of the survey data I utilize. These include political partisanship, the share of invalid votes, and the share of votes for political parties in Sweden. Thus, I aim to answer the research question, *Did the 1994 pilot causally affect the intended outcomes, including trust in politicians and voter turnout, or related outcomes of interest, including partisanship, the share of invalid votes, and parties' share of votes?*

I analyze these outcomes with individual-level data from the Swedish National Election Studies Program (SNES, 1982-1994) and aggregate-level data from Statistics Sweden (1973-1994), using both static and dynamic difference-in-differences models. In sum, one of the main aims of the pilot to strengthen the bond between voters and individual politicians can be considered fulfilled, since the pilot seemed to have increased trust in politicians and decreased partisanship. Surprisingly, the share of invalid votes decreased as a consequence of introducing preference voting, despite the enhanced complexity of the voting process. This may have been due to fewer protest votes from enhanced satisfaction with the political system with preference voting being introduced, or perhaps the information on the new semi-open list system was either more expansive than regular election information, or more absorbable for certain voters, which might have caused a decrease in the share of unintentionally erroneous votes in general. Overall, the

pilot can thus be considered successfully implemented, although the goal of increased voter turnout did not seem to have been achieved. Moreover, opposing the implementation may not have come without political sacrifice, given that the share of votes for the Left Party, a vocal opponent of introducing preference voting, was on average lower in pilot municipalities in 1994 compared to control municipalities. These results are only indicative, though, as they do not hold up to robustness tests of various kinds. Hence, they should be interpreted with caution.

This thesis constitutes the most extensive evaluation of the 1994 pilot and its intended outcomes, to the best of my knowledge. Johansson et al. (1996) evaluated the pilot for SOU[1] 1996:66, but did not investigate causality. Neither did they fully examine the fulfillment of the main aim of the reform, to strengthen the bond between voters and politicians. Hence, the main contribution of this thesis is its complementation of the parsimonious evaluation of a large-scale government experiment. In broader terms, it thus adds to the often non-causal governmental evaluations of public policy. The results also support the notion that the electoral system can shape trust in politicians, which is a novel finding, too. Additionally, it fills a research gap on the causal relationship between preference voting and political partisanship in Sweden.

The rest of the thesis is structured as follows. Next, I set forth the institutionalia of Swedish elections, and the governmental process and aims of introducing preference voting in the 1990s. I end the background section by presenting the hypotheses. Section 3 summarizes the previous literature on which these hypotheses are based. Section 4 describes the data and the difference-in-differences methodology. Section 5 presents the results and their potential mechanisms. Section 6 goes over the internal and external validity of the study, and suggests further research. Section 7 concludes.

2 Background

In the election year of 1994, seven Swedish municipalities were piloted for a semi-open list system in elections to the Municipal Councils. These were Helsingborg, Linköping, Solna, Vaggeryd, Vänersborg, Östersund, and Övertorneå, so they have a relatively even geographical and demographic spread, depicted in Figure 1 in appendix A.1. In total, 13 municipalities announced their interest in participating in the pilot (also Danderyd, Jönköping, Ljusdal, Simrishamn, Stockholm, and Vellinge). Although the municipal decision to volunteer for the pilot was not unanimous in all municipalities, the government subsequently picked seven out of these 13 volunteers, considering the number of eligible voters in each municipality, the distribution of party sympathies, and other relevant factors, reportedly to strengthen the representativeness of the treatment group to the rest of Sweden's municipalities. (SOU 1996:66) Following this 1994 pilot, the new semi-open list system was implemented, with some alterations, in all Swedish municipalities in the first European Parliament (EP) election of 1995 and in the 1998 general election. This thesis will focus exclusively on the 1994 pilot in the Municipal Council elections. In the following sections, I will first review the institutionalia of Swedish elections (2.1). Then, I will go over the process of introducing preference voting in 1994, as well as the political context in which this occurred, and summarize the goals of reforming the electoral system (2.2). I will also go through the governmental evaluation of the pilot (2.3). Finally, I present the hypotheses (2.4).

2.1 Elections in Sweden

Sweden has a multi-party electoral system of proportional representation. Elections to the Swedish Parliament (national), County Councils (regional), and Municipal Councils (local) are held simultaneously on the second Sunday of September (1 Chapter, Section 3 of The Elections Act (2005:837)). These take place every four years as of the election of 1994, prior to which elections were held every three years (prop. 1993/94:115). In Swedish municipalities, which are the focus of the 1994 pilot and thus this thesis, the

[1] Statens Offentliga Utredningar (Swedish Government Official Reports)

Municipal Council (*kommunfullmäktige*), the local equivalent to the Parliament, appoints the Municipal Board (*kommunstyrelse*), the local equivalent to the government.

2.2 The 1993 Proposal of a 1994 Pilot

Introducing preference voting had been up for discussion on several occasions ever since 1918. The current system, however, was officially proposed by a 1993 governmental report, SOU 1993:21. Its Nomination and Constituency Committee (henceforth 'the committee') suggested an introduction of preference voting with a pilot preceding full-scale implementation. The Swedish Parliament subsequently decided that a pilot of preference voting would commence in 1994.

While no government report is overly clear about the exact goals of reforming the electoral system, I narrow down the aims of the reform to increase (1) trust in politicians and the political system and (2) voter turnout. The committee wanted to *strengthen the bond between voters and politicians* with a reformed electoral system, such that voters were empowered to alter the allocation of mandates to individual politicians (SOU 1993:21, SOU 1996:66; SOU 1999:136). There seemed to be a need for "a more personally oriented electoral system" (SOU 1993:21, p. 202) to reinforce political participation and engagement. Petersson et al. (1998) analyzed the state of Swedish democracy in the 1990s and concluded that, apart from a decline in political participation, politics had indeed been personalized in the sense that political parties seemed to have lost their significance to personal communications. Möller (1999) summarized the government's view on the state of democracy at the time: "the foremost expression of concern about democracy may be found in the long list of government-sponsored studies of the issue" (p. 274), including those reviewed here. Above all, there was much public debate on the lack of trust in politicians at the time (SOU 1993:31; SOU 1993:90; SOU 1996:66; SOU 1999:136; Möller 1999). Möller (1998) specifically emphasized the need to further investigate the causal determinants of trust in politicians in Sweden.

The SOU 1993:21 committee also discussed politician knowledge extensively. It is unclear, however, whether this was an explicit goal of the reform. It seems, rather, that politician knowledge was assessed to serve as a prerequisite for, and perhaps also a long-term result of, preference voting. The committee wrote that, while there was no robust evidence for an introduction of preference voting strengthening the relationship between voters and politicians, they referred to Holmberg (1993) in SOU 1993:63, who found that voters with higher politician knowledge tend to exhibit higher trust in politicians. The committee thus concluded that the reform's "general personification of politics" (p. 54, my translation) would likely work in its favor. They also argued that introducing preference voting would signal increased transparency in the election of candidates, which might increase satisfaction with the electoral system as a whole. Hence, I leave politician knowledge as the mechanism proposed by the committee through which the introduction of preference voting would contribute to said aims.

2.2.1 The Pilot Design

The "modified Danish model" proposed in SOU 1993:21 was followed to the letter in the pilot. Parties would still present their lists of nominated candidates in order of preference, and voters would still be able to cross over or add candidates on the ballot, or in other ways alter the ordering of names. These voter liberties were a direct result of the so-called free right to nominate (*den fria nomineringsrätten*), which meant that the nomination of candidates to parties' candidate lists was free of regulations. The free right to nominate thus meant that there was, technically, a small element of personal voting already before, but according to several governmental evaluations (Holmberg & Möller, 1999; SOU 1993:21; SOU 1996:66; SOU 1999:136), the adding or crossing of names on the ballot had not previously led to voters circumventing the parties' ordering of candidates. That would have required a vast mobilization of ballot alterations, and the general use of this possibility was considerably small. Essentially, this was the reason for reform – to assign people more power to affect the election outcome in terms of the elected individual politicians. Thus, the Swedish electoral system was, in general, regarded as a closed-list system at the time,

and Sweden is considered to have switched to a semi-open list system in 1998 (see for example Berg et al., 2015; Dehdari et al., 2021; Möller, 1999).

In the pilot, voters could now mark *one* specific candidate on the ballot, using the checkboxes that were added next to each candidate, by which the voter had placed a preference vote. This was not mandatory, though. Voters could still support the parties' candidate lists as they were and not cast a preference vote, or only use the party ballot without a candidate list. However, each party ballot now also included an empty line for voters to nominate another candidate of their own. SOU 1993:21 presented examples of what the new ballots could look like[2] (see Figure II and Figure III in appendix A.2). The intention was for the ballots to change as little as possible, to reduce the risk of voters casting more invalid votes as a consequence of the pilot. Still, the risk of a temporary increase in invalid votes was inevitable, according to the committee.

Preference votes would circumvent the parties' ordering of candidates only if their share of total valid votes within a constituency crossed a specific threshold (five percent for the Municipal Council elections). This was to avoid preference votes reducing the parties' influence on the final allocation of mandates, and it was the deciding compromise for the Social Democratic Party and the Left Party who wanted the parties to remain in control of their candidates (Holmberg & Möller, 1999; Möller, 1998; Petersson et al., 1999; SOU 1993:21; SOU 1996:66; SOU 1999:136). They expressed concern that Swedish elections would become much too Americanized with an increased focus on individual candidates, and feared that success in candidate campaigns would come to depend on financial resources. Hence, the modified Danish model did not alter the distribution of mandates between parties, only the distribution of mandates *within* parties, such that the electoral system remained a party system.

2.3 The 1996 Evaluation of the 1994 Pilot

In a study conducted at the request of the government, included in SOU 1996:66, Johansson et al. (1996) evaluated the 1994 pilot by comparing three pilot municipalities with one representative control municipality each (Linköping-Halmstad; Vänersborg-Lidköping; Vaggeryd-Tranemo). They surveyed these three pairs of municipalities, as well as all seven pilot municipalities. This method has its drawbacks, mainly with not all municipalities being surveyed, but it is, to the best of my knowledge, the only evaluation of the pilot available. Johansson et al. (1996) concluded that those with greater trust in the political system were more positive about the introduction of preference voting. They predicted that the introduction of preference voting would not become a way of expressing discontent, but rather a way for politically active and interested voters to exercise greater influence on an aspect of the election they could not affect before. Johansson et al. (1996) also argued that "preference voting likely works best in small municipalities, where preference voting is not really needed" (p. 223, my translation). In smaller municipalities, local name recognition is greater; the election material is generally more candidate-focused; information on local politics among the population is greater; and personal contact is more important for distributing information to voters, they conclude. They also found that voters in smaller municipalities were generally more positive about introducing preference voting than in larger municipalities.

One main conclusion of SOU 1996:66 was the lack of information on the pilot, especially from the Municipality Councils themselves, to the extent that the pilot did not reach full effect[3], although there were substantial differences between municipalities. It was thus noted that a larger information

[2] While SOU 1996:66 does not display the design of ballots in the 1994 pilot, the SOU 1993:21 examples seem to match the descriptions of the pilot ballots in SOU 1996:66 as well as the regulations on ballot design in the new law enabling the pilot (Section 2 and Section 4 Subsection 2, *Lag* (1994:285) *om försök i vissa kommunner där väljarna skall kunna avge en särskild personröst vid de allmänna valen år 1994*).

[3] A little less than one-tenth of surveyed voters said they had *not* heard of the semi-open list pilot before the election. This share was higher in the larger municipalities. (Johansson et al., 1996)

campaign on preference voting could be beneficial for the upcoming 1998 election. However, Johansson et al. (1996) dialed down this critique, underlining that mass media played a bigger role in information distribution anyway, and that the person-oriented change made information on the pilot more receivable to those otherwise not very interested in politics. That is, people who are less interested in politics tend to see politics more in terms of people rather than parties, which might have motivated them to preference vote. They also underlined that the main reason for not preference voting was lack of candidate knowledge, not lack of information on the pilot itself[4]. On the other hand, Johansson et al. (1996) concluded that the information to the parties about the pilot may have come too late for the pilot to have full effect on their nomination of candidates.

The governmental evaluation of SOU 1996:66, with all due credit, does not investigate the causal effects of the pilot. Neither does it investigate whether voters in pilot municipalities came to exhibit greater trust in politicians or parties as a consequence of the introduction of preference voting. They do not discuss general voter turnout either, only preference voter turnout[5]. Admittedly, this was likely a consequence of the terms of reference (*kommittédirektiv*) that "[t]he evaluation should according to the government take aim at the technical, economic, organizational and practical aspects" of the pilot (Dir. 1995:7, my translation). That is, these evaluations have been crucial for the practical development of Sweden's semi-open list system. Indeed, several regulatory alterations were made in the 1998 full-scale implementation following the SOU 1996:66 recommendations, after which the system was regulated even further following SOU 1999:136 recommendations. Additionally, the government reports evaluating the implementation of Sweden's semi-open list system (Holmberg & Möller, 1999; SOU 1993:21; SOU 1996:66; SOU 1999:136) focused on the occurrence of preference voting across socioeconomic groups as well as the socioeconomic spread among politicians receiving preference votes (I return to this issue in 4.4). However, the causal effects of the pilot on the intended outcomes remain unexamined, which motivates this thesis to investigate them.

2.4 Hypotheses

In sum, the main aims of the 1994 pilot were to increase trust in politicians and the political system and voter turnout. There are more outcomes of interest, though, given previous research, made possible to examine by the richness of the survey data (see 4.1), including political partisanship which I elaborate on in 3.3. below. The share of invalid votes is an outcome of interest too, which SOU 1996:66 mainly discussed in regard to the 1995 EP election. Additionally, in view of the political debate that preceded the introduction of preference voting (more on this in 3.5), the parties' share of votes is the final outcome of interest for this study. I thus hypothesize that the pilot had an effect on the following outcomes: (1) trust in politicians and the political system (2) voter turnout (3) partisanship (4) the share of invalid votes (5) the share of votes for any established Swedish political party. I return to the formal hypothesis test in 4.4. Furthermore, I expect the effect of introducing preference voting in 1994 on trust to be positive; the effect on voter turnout to be positive; the effect on partisanship to be negative; a positive effect on the share of invalid votes; a negative effect on the share of votes for parties of the anti-preference voting bloc (the Social Democratic Party, the Left Party and the Green Party), and a positive effect on the share of votes for parties of the pro-reference voting bloc (the Moderate Party, the Center Party, the Liberal Party, and the Christian Democrats). In the literature review below, I motivate the reasoning behind these hypotheses.

[4] Johansson et al. (1996) both asked respondents directly about their reason for not preference voting and tested their candidate knowledge.

[5] On average, 26-38 percent of voters in the pilot municipalities preference voted, depending on way of measurement. There were, however, major differences between municipalities. 19 percent of the elected candidates got their mandates via preference votes, and some of them were already ranked high on the party list. (Johansson et al., 1996)

3 Theoretical Considerations

3.1 Trust in Politicians and Political Parties

Trust in politicians and parties was one of the main aims of introducing preference voting in Sweden in the 1990s. There is much research on the connection between trust and democracy (for example, Hooghe & Stolle, 2003; Newton, 2001; Paxton, 2002). The SOU 1993:21 committee proposed that the mechanism of introducing preference voting enhancing trust in politicians was the ”personification” of politics. That is, voters who know about the candidates likely trust them and other politicians more (SOU 1993:21; Holmberg 1993). Westerståhl (1993) further concluded from a 1991 survey of 27 municipalities that a majority of those surveyed believed that trust in the local politician *as an individual* was more important than her or him representing a certain party. The SOU 1993:21 committee seems to have regarded this finding as support for their proposal. The SOU 1993:21 committee also believed that the increased transparency in the election of candidates would increase satisfaction with the electoral system as a whole. This is supported by empirical studies. For instance, Farrell and McAllister (2006) conclude from cross-country data that preferential voting systems “promote a greater sense of fairness about election outcomes among citizens, which in turn is a major component of the public’s satisfaction with the democratic system” (p. 742). Bosch and Orriols (2014) find similar effects, but only among knowledgeable, well-informed, and politically interested voters. All in all, there seems to be support in the literature for the hypothesis that introducing preference voting may have increased trust in politicians and the political system[6]. Importantly, however, it is notoriously hard to measure. I return to this aspect in 4.1.1.

3.2 Voter Turnout

Political participation was declining in 1990s Sweden (Möller, 1999; Petersson et al., 1998), which motivated modifying the electoral system. Voter turnout is generally “regarded as an indicator of the health of democracy” (Lundell & Högström, 2021, p. 280). Downs (1957) proposed a rational choice model to explain why people choose to vote – or not to vote – which was further adapted by Riker and Ordeshook (1968). Their calculus of voting has been the base of much subsequent research on voting behavior, and is presented in eq. (1). A rational individual i will vote if the utility of voting exceeds the cost of voting. Formally, i will vote if the following relationship holds:

$$pB + D > C \tag{1}$$

where p is the probability that i’s vote affects the election outcome; B is i’s benefit of her preferred party or candidate being elected; D is the additional intrinsic utility that i receives from the act of voting, such as fulfilling a civic duty; and C is i’s cost of voting. p could be affected by introducing a semi-open list system via the parties’ nomination processes. In much of the political economy literature, parties are assumed to act strategically to gain votes and win elections, for example by assigning influential posts to politicians with strong voter support (Blumenau et al., 2017; Crisp et al., 2013; Shugart & Valdini, 2005; Tavits, 2010). Indeed, Folke et al. (2016) find evidence in Swedish municipalities of what they call ‘the primary effect’, which is the causal effect of winning the most preference votes on the probability of political promotion

[6] The importance of trust is also discussed in the economic literature. Indeed, the connection between social capital and the economy as a whole is well-researched (see for example Beugelsdijk et al. 2004; Knack & Keefer 1997; Zak and Knack 2001). The proposed mechanisms for the connection between trust and economic outcomes, such as economic growth (Algan and Cahuc 2010 & 2013) include social capital as lubricant for the development of market economies (Arrow, 1972); human capital accumulation (Coleman, 1988); entrepreneurship (Guiso et al. 2016); and international trade (Guiso et al. 2009). Most interestingly for this study of policy implementation, Knack (2002) finds that trust is associated with enhanced governmental performance. These findings consider trust as a general concept, though, particularly as social capital, and not trust in politicians or parties specifically.

within the party. Meriläinen and Tukiainen (2018) find similar rank effects in Finnish municipalities. However, Johansson et al. (1996) suggest that the upcoming pilot was announced too late for the pilot to fully affect on the parties' nomination process. Furthermore, Blais et al. (2000, 2019) argue that D is the most important parameter for explaining voter turnout. D is also likely the parameter of greatest relevance when studying local elections, as D might be higher for individuals who are more engaged with their community (Feddersen, 2004; Leighley, 1996). This aspect might be relevant to the 1994 pilot's effect on B as well, although Enos and Fowler (2014) suggest that B is not very large. Lastly, the cost of voting, C, includes not only registration requirements (Brady et al., 2011; Filer et al., 1991), transportation costs to the polling place (Brady et al., 2011) and election day weather (Gomez et al., 2007), but also information costs. With limited information on parties or candidates, the cost of voting increases (Andersen et al., 2014; Dehdari et al., 2021; Feddersen & Pesendorfer, 1996; Ghirardato & Katz, 2006; Matsusaka, 1995; Palfrey & Poole, 1987). Indeed, Johansson (1996) concluded that lack of information on the candidates was the main reason for not preference voting in pilot municipalities. This held true also in the 1998 full-scale implementation (Holmberg, 1999; Johansson, 1999).

Summing up the literature just reviewed, p is likely not affected already in the pilot (although the primary effect occurred later on); B and D likely increase; and C probably consists of a surge in information costs for browsing candidates. Hence, the pilot's overall effect remains uncertain in terms of the voter calculus. However, the voter calculus is rather simplistic. One important nuance it lacks is that the introduction increases the transparency of allocating mandates to politicians, which may enhance the perceived fairness of the electoral system (see 3.1). This, in turn, may increase voter turnout (see for example Grönlund & Setälä, 2007). However, the causal direction between satisfaction with democracy and voter turnout may also be the opposite, and political scientists have long debated the relationship between satisfaction with democracy and voter turnout (Kostelka & Blais, 2018). Nonetheless, the pilot aimed to increase voter turnout, so it makes sense to hypothesize that the pilot increased voter turnout in order to evaluate the government experiment properly.

3.3 Partisanship

The SOU 1993:21 committee did not explicitly aim for less partisan voters, as they emphasized the importance of maintaining a party-based electoral system. However, their definite goal of strengthening the bond between voters and individual politicians might imply decreased partisanship. Hence, to evaluate the pilot's intended aims, I hypothesized in 2.4 that the 1994 pilot decreased partisanship. The committee had also identified a mass sway of voter interest from party to person, which suggests a general trend of weakening partisanship in Sweden at the time (this is also how Berg et al. 2015 interpreted the aims of the reform in terms of partisanship). It seems the committee wanted to appeal to this trend to engage voters to turn up to the polling station.

There is a rich body of research on partisanship serving as a shortcut for deciding how to vote, primarily among voters that are less politically interested, less politically informed, and less educated (Arceneaux, 2008; Boudreau, 2009; Boudreau & MacKenzie, 2014; Colombo & Kriesi, 2017; Dancey & Sheagley, 2013; Lupia, 1994; Simon, 1985). Partisanship has thus been suggested to increase voter turnout in general, by for instance Krishna and Sokolova (2017) and (Schaffner et al., 2001). The committee, however, believed in the opposite mechanism, namely that voter turnout would increase by bringing individual politicians closer to the voters, since voters were becoming more engaged by individual politicians and less with parties. Moreover, Walder and Strijbis (2023) find that "whether high or low knowledge voters make more use of the party cue is conditional on the information environment" (p. 2), in other words that voters with both high and low political knowledge may utilize partisanship as a decision-making shortcut. Interestingly, they find similar (although weaker) effects when they replace political knowledge with education. I will return to this aspect in the discussion. Lastly, there seems to be a lack of research on the

causal relationship between partisanship and preference voting, not least in the Swedish context, to the best of my knowledge. This further motivates investigating the partisanship hypothesis.

3.4 Invalid Votes

In the 1994 pilot, voters were presented with new ballots, with new instructions on how to preference vote. The SOU 1993:21 committee thus figured the risk of a temporary increase in invalid votes was inevitable. There is a plethora of research on invalid votes, too, which further motivates analyzing the effects of the pilot on the share of invalid votes. One strand of research regards the heightened propensity to make unintentional errors when voting in a complex electoral system or with a complex ballot design (Carman et al., 2008; Darcy & McAllister, 1990; Fossati & Martinez i Coma, 2020; Hanmer et al., 2010; Herrnson et al., 2012; Herron & Sekhon, 2003; Kimball & Kropf, 2005; Kouba & Lysek, 2016, 2019; Leib & Dittmer, 2002; Lundell & Högström, 2021; Martinez i Coma & Werner, 2019; McAllister & Makkai, 1993; Niemi & Herrnson, 2003; Pachón et al., 2017; Power & Garand, 2007; Power & Roberts, 1995; Taylor, 2012). This propensity may interact with the voter's lack of skill and competence to cast a ballot correctly, which in turn often stems from socioeconomic factors such as illiteracy or education, or from social marginalization (Hill & Young, 2007; Hooghe & Stolle, 2003; McAllister & Makkai, 1993; Power & Garand, 2007; Power & Roberts, 1995; Reynolds & Steenbergen, 2006; Uggla, 2008). This underscores the importance of controlling for socioeconomic variables when running regressions on invalid votes (see 4.4). Voters may also simply lack information. Stiefbold (1965) was the first to classify different types of invalid voters. He distinguished "apathetic individuals" from "highly politicized individuals", where the former turn up to vote from a sense of civic duty (in the voter calculus framework, a high D), while the latter use the invalid vote as a political protest. Kouba and Lysek (2019) summarize 54 studies on invalid votes and further categorize invalid votes into (1) unintentionally erroneous votes, and different types of intentionally invalid votes cast as (2) acts of dissatisfaction with democracy or distrust with the authorities (3) protests against their political powerlessness and aversion to politicians and/or the political system (4) expressions of little interest in politics and low perceived p, but high D, meaning individuals turn up to vote but are indifferent to the outcome.

In Sweden, the average share of blank votes in the election to Parliament has been close to one percent from 1952 to 2022. Blank votes count towards the voter turnout, but do not affect the allocation of seats. (Swedish National Election Studies Program, 2022) Until 2006, blank votes and other types of invalid votes were not differentiated in the official statistics (Swedish National Election Studies Program, 2021). Uggla (2008) and Moral (2016) both conclude that invalid votes occur as acts of protest especially in elections of lower political competition. Kouba and Lysek (2019) also raise low political competition as an important predictor of invalid votes. While Sweden has a multi-party electoral system, the Social Democratic Party has dominated contemporary politics (in government 1932-1976, 1982-1991, 1994-2006, and 2014-2022). The introduction of preference voting essentially adds an element of political competition to the Swedish environment of arguably low political competition. Although mandates *between* parties were not affected by preference votes, they could affect the mandate allocation *within* parties. Hence, the share of invalid votes may have been affected in pilot municipalities both by the enhanced complexity of the electoral system and by the added political competition. Still, I expect the overall effect of the pilot to be positive on the share of invalid votes, considering that the average of blank votes and thus potential protest votes in Sweden is about one percent. That is, I anticipate that the increase in invalid votes from the enhanced complexity of the electoral system will outweigh the potential decrease in protest votes from the enhanced competitiveness of the electoral environment.

3.5 Parties' Share of Votes

There was disagreement between the political parties on whether introducing preference voting was desirable. While this was only subtly discussed in the governmental evaluations, this political fragmentation

is interesting to examine further. The effect of introducing preference voting on parties' share of votes may illustrate the potential voter support for carrying it through. Indeed, major electoral reforms in Western democracies are rare and often require political compromise (Lundell, 2008; Shugart, 1992).

The 1994 preference voting pilot and its preceding governmental evaluations were initiated by the right-block cabinet of Carl Bildt, then leader of the Moderate Party. His 1991-1994 cabinet was a coalition government between the Moderate Party, the Center Party, the Liberal Party, and the Christian Democrats. They had come to power after four consecutive Social Democratic governments. The Moderates and the Liberals were in favor of introducing preference voting, while the opposing bloc of the Social Democrats and the Left Party were against it (Holmberg & Möller, 1999; Möller, 1999; SOU 1996:66; SOU 1999:136) – the Green Party were not represented in Parliament following the 1991 election. Hence, it would be interesting to see whether the voter support for either party was affected by this debate. For example, if a pilot municipality voter was going to vote for the Social Democrats in 1994, but wanted the opportunity to preference vote, did the debate on introducing preference voting alter which party they voted for in the 1994 election? That is, did the share of votes for any party increase or decrease as an effect of the 1994 preference voting pilot?

Theoretically, if the voters were aware of the debate preceding the pilot, and if they viewed the introduction of preference voting positively, then voter support for the proponents might have increased, and decreased for the opponents. Indeed, Johansson et al. (1996) emphasized that voters were generally positive about the introduction of preference voting. While the most articulated advocates were the Moderates and the Liberals, voters might have perceived the Center Party and the Christian Democrats as advocates too, considering they were part of the same coalition government to launch the pilot[7]. Importantly, these party differences applied to the municipality level as well, with the addition of the Green Party as a naysayer, among the municipalities surveyed by Johansson et al. (1996). The Green Party's aversion to preference voting in the 1998 general election is brought up in Petersson et al. (1999) too. Hence, I hypothesized a decrease in the share of votes also for the Green Party, despite them not being in Parliament in 1991-1994 and thus not represented in the SOU 1993:21 committee. Additionally, Johansson et al. (1996) concluded that local parties were distinctively pro-preference voting – which is not surprising given the body of research suggesting amplified effects at the local level (see for example Holmberg 1993 and Westersthåhl 1993 on trust; Feddersen 2004 and Leighly 1996 on voter turnout; Johansson et al. 1996 on local name recognition and personal contacts being greater in local elections). Unfortunately, such local parties are aggregated into a residual called "Other parties" in the data I use to analyze the share of votes for each party. I describe this data in more detail in the next section.

4 Data and Methods

In this section, I lay out the empirical strategy for examining the potential causal effects of the 1994 pilot. I first describe the individual-level data from the Swedish National Election Studies Program (4.1), and the aggregate-level data from Statistics Sweden (4.2). In 4.3, I set forth and motivate the difference-in-differences framework. Finally, in 4.4, I present the model specifications and the assumptions required to interpret the results causally.

[7] Brothén (1999) in Holmberg and Möller (1999) subsequently concluded that in the full-scale implementation of preference voting in 1998, differences also within the pro-preference voting bloc protruded. Members of Parliament (MEPs) for the Moderate Party and the Liberal Party wanted to enhance the element of preference voting even further, while MEPs of the Center Party and the Christian Democrats wanted the semi-open list system to remain as it was in 1998. These differences did not seem to have materialized already in 1994, though, which is why I do not base my hypothesis on parties' share of votes on this particular finding.

4.1 Data from the Swedish National Election Studies Program

The Swedish National Election Studies Program (SNES) data consists of Swedish election surveys of eligible voters. These surveys have been conducted at every election since 1956, making it the world's next largest time-series election data set after its U.S. equivalent. The SNES encompasses about 3,000-4,000 respondents each year. A rolling two-step panel was introduced in 1973, where half of those surveyed in 1973 were re-surveyed for the next election of 1976, whereas the other half was replaced with new respondents, and so on. This was done to obtain balanced panel data while maintaining high response rates over time, according to the SNES descriptives. To further uphold high response rates, some survey questions reappear in slightly different formats over the years, which unfortunately limits measuring certain aspects of political behavior reliably over time. Furthermore, the municipality variable is only available from 1982 onwards, so the analysis is restrained to the elections of 1982, 1985, 1988, 1991, and 1994, amounting to almost 17,900 respondents, most of whom were surveyed in their own homes[8]. The SNES sample is said to be representative of the electorate as a whole each election year. I present some descriptive statistics for *my* SNES sample in appendix A.3. I discuss these further when presenting the model specifications in 4.4, but overall, the relevant background characteristics of the respondents are mostly balanced across treated municipalities and controls over the years.

All data are anonymized by removing respondent identifiers and dropping all unnecessary variables to prevent backtracking to individual respondents. Moreover, most variables had been coded differently depending on whether respondents did not *want* to answer, did not know *how* to answer, or the answer had not been recorded for some other reason. To ease interpretation, I recode all of these as missing values. Additionally, I analyze the unweighted dataset, dropping all duplicates in data cleaning. Furthermore, it might be more likely that voters in pilot municipalities had absorbed the information on the new semi-open list system after the election than before. Hence, the full sample of pre- and post-election interviews might confound the true effect of preference voting in pilot municipalities in 1994 with individuals unaware of the introduction of preference voting before the election took place. Therefore, I will also separate the full sample into the pre-election and post-election sample, respectively, where possible[9]. As seen in Figure IV in appendix A.3, the share of respondents who are in the pre-election sample in control municipalities is overall comparable to the share of respondents who are in the pre-election sample in treated municipalities. The same goes for the share of respondents in the post-election sample. Importantly, one of the main conclusions of the governmental evaluation of the 1994 pilot (SOU 1996:66) was a general lack of information to the public about the upcoming pilot. If that conclusion holds up to scrutiny, there should only be discernible effects in the post-election sample.

4.1.1 Outcomes of Interest

Section 2.4 presented the outcomes of interest and their associated theoretical predictions. Here, I return to the outcomes of interest that may be extracted from the SNES data[10]. The remaining outcomes will be elicited from Statistics Sweden data, and are thus reviewed in 4.2.

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[8] For the full election evaluations, see Holmberg (1986); Holmberg and Giljam (1991a); Holmberg and Giljam (1991b); Holmberg and Giljam (1995); and Holmberg and Giljam (1997).

[9] There is one variable for respondents who were *intended* to be interviewed before or after the election, and another for respondents who were *actually* interviewed before or after the election, respectively. The divergence between the two stems from non-responses. I use the latter variable.

[10] Respondents were also asked to rank four objectives in their importance for society to fulfill, one being "Providing individuals greater opportunity to influence political decisions" (the being other "Maintaining law and order", "Protecting freedom of speech", and "Combating inflation"). It would have been interesting to examine whether the pilot caused a positive effect on the share of respondents ranking the importance of influencing political decisions as

Trust in politicians and the political system: One of the main aims of the reform was to increase both trust in politicians and trust in the political system. There are various questions on trust in the SNES. The two variables with values from all elections 1982-1994 are the following statements: "Those [politicians] in Parliament often disregard the opinion and thoughts of ordinary people" and "Political parties only care about people's votes, not their opinion" (my translations), meaning they can elicit respondents' perception of politicians and political parties, respectively. It is notoriously difficult to reliably measure trust in politicians and the political system (Möller, 1999; see also Glaeser et al., 2000 and Naef & Schupp, 2009 for experimental measurements of trust). A positive perception of politicians may not necessarily capture high trust, and a positive perception of parties may not necessarily capture neither high trust nor a positive perception of the political system. Indeed, (Möller, 1998) notes that although the share of respondents agreeing with these statements had increased continuously from their 1968 introduction until 1994, he believed they likely overestimated respondents' true trust. What is more, the SNES also provides a direct measure of trust. Respondents were asked to self-assess their trust in politicians in general in three of the elections included in my time span (1988, 1991, and 1994) – despite not being asked in all years, at least it was asked sequentially. Möller (1998) highlights that there was an increase from 1988 to 1994 also in the share of respondents stating that they do not trust politicians using this measure, and that this might be a more reliable measure of trust than evaluating said statements. Still, using also the statements as outcomes may prove a useful complement to the direct trust measure with only three periods.

Partisanship: One SNES question asks respondents whether they identified as partisan for any of the current parties of Parliament. Respondents were also asked whether they were members of a political party. This could have proxied for partisanship, because although not all partisan voters are members of a party, most party members are likely partisan. However, I limit my focus to self-assessed partisanship, since I aim to examine whether the bond between a partisan voter and their party is weakened if they are given the opportunity to preference vote. Party membership might confound this aspect of voting with predictors for, for example, wanting to become a politician oneself, of which there are no proxies in the SNES data to use as control variables (the included controls are presented in 4.4).

4.2 Data from Statistics Sweden

The SNES data will be complemented with aggregate-level data from Statistics Sweden for three of the outcomes of interest reviewed in 2.4, namely voter turnout, invalid votes, and parties' share of votes.

Voter turnout: It is particularly important to consider both individual- and aggregate-level data when studying voter turnout. On the one hand, survey-based measures tend to yield unreliable results, as unobserved respondent characteristics might correlate with the propensity to over- or understate one's political participation (Holbrook & Krosnick, 2010; Robinson, 2009; Silver et al., 1986). However, the SNES collects voter turnout for their respondents from the official electoral rolls, meaning there is little risk of the social desirability bias that might otherwise plague survey-based measures of voter turnout[11]. Still, they do not record voter turnout for Municipal Council elections in the entire country in each period, but only the voter turnout *among those surveyed*. For example, if all x respondents of Vänersborg voted in 1994, this would indicate a 100 percent voter turnout in Vänersborg in 1994, when in reality it was 85.3 percent in 1994, according to the data from Statistics Sweden. On the other hand, the Statistics Sweden aggregate-level data may lead to the ecological fallacy, since I cannot control for individual characteristics using this data. That

the most important or second most important objective out of four for society to fulfill. That is, did the enhanced opportunity to influence political decisions, via preference voting, positively affect how voters valued this opportunity? Unfortunately, this variable was coded differently in 1994 than in previous years, preventing solid analysis of this particular outcome.

[11] Social desirability bias means in this context that respondents might be prone to falsely claim that they did vote in the election, as that could be perceived as the socially desirable thing to do.

is, the aggregate-level voter turnout results may not apply to individuals within those aggregated groups, as opposed to survey data (see for example Matsusaka & Palda, 1993). All in all, the Statistics Sweden data will complement the SNES data on voter turnout in the Municipal Council elections, available in digital format from 1973 and onwards, amounting to 2,260 observations[12]. I review the voter turnout results using both sources of data in section 5.

Invalid votes: I will use the aggregate data from Statistics Sweden also when studying the share of invalid votes in Municipal Council elections, as the SNES imports no such data for their respondents from the official election statistics. The Statistics Sweden data on invalid votes was available in digital format from 1976 onwards, amounting to 45,168 observations[13]. Statistics Sweden did not distinguish in 1976-1994 whether the invalid votes were blank votes or invalid for some other reason. However, in much of the literature, all votes that do not count toward the final election result are usually merged (see for example Kouba & Lysek, 2019 and Uggla, 2008).

Parties' share of votes: Finally, the share of valid votes for each party in the Municipality Council elections will also be analyzed using aggregate data from Statistics Sweden. This data was available in digital format from 1973 onwards, amounting to 17,010 observations[14]. Notably, the Green Party was not founded until 1981, so it cannot be considered in any elections before 1982. Also note that there is a residual category called "Other parties" which includes, but is not limited to, the Sweden Democrats in 1988, 1991, and 1994, and New Democracy in 1991 and 1994. This category may thus include such local parties that, according to Johansson et al. (1996), were generally in favor of introducing preference voting, apart from the nationally established parties. Statistics Sweden does not allow digital sorting for such local parties, though, meaning it is not possible to study the conclusion of Johansson et al. (1996) any further[15].

4.3 Methods

The 1994 pilot allows for a difference-in-differences (DiD) methodology, using the seven pilot municipalities as treated units and the remaining 283 municipalities as controls. Common alternatives to the DiD method are different types of weighting exercises, such as the synthetic control method or propensity score matching. These are mainly advantageous for experiments where the control group needs to be adjusted. (Cunningham, 2021) However, since the municipalities in this experiment were distinctively assigned into control municipalities and treated municipalities – an assignment which appears to have occurred as good as randomly, considering that the decision to volunteer for the pilot was not unanimous in all municipalities (see 2 Background), hence alleviating selection effects – I argue that no such weighting exercise is needed.

I will estimate the causal effect of the 1994 pilot using both static and dynamic DiD models. In general, dynamic DiD models are preferred over static when there are lags in the timing of treatment, as static DiD models simply compare the average levels of outcomes between post- and pre-treatment. Hence, dynamic DiD models are especially useful for examining the long-term effects of a particular treatment. (Cunningham, 2021) This is arguably of limited importance for the 1994 pilot, since all municipalities were treated immediately in the 1995 EP election and the 1998 general election – and the effects of introducing preference voting tend to be slow-paced (see for example Berg et al., 2015). However, estimating the effects of the government experiment using a dynamic DiD could still provide an insightful complement to the static DiD. Pre-treatment trends can be more appropriately evaluated with a dynamic model, since it singles

[12] One observation per year and municipality, minus missing values.

[13] One observation per year, municipality, and constituency, minus missing values.

[14] One observation per year, municipality, and party, minus missing values.

[15] Since the SNES collects some data from the official electoral rolls, they do have an adjusted measure of parties' share of votes, but in 1991 the survey-based measure of party votes was not corrected with official data, meaning I cannot use this data for analysis, although that would have been preferred given the possibility to include controls.

out the effect of being in a treated municipality individually for all years preceding treatment. Hence, while the static DiD model can uncover significant treatment effects in *average* differences, the dynamic DiD model can uncover significant treatment effects in differences *for each year considered*. All in all, utilizing a dynamic DiD model to evaluate the assumption of parallel pre-trends (presented below) could strengthen the validity of my estimations.

Lastly, a note on the methodological context. As noted in 2.1, the mandate period was extended in 1994 from three to four years. That is, general elections were held in 1988, 1991, 1994 – and then in 1998, 2002, etcetera. This should not affect the applicability of the DiD method, as all municipalities are arguably affected in the same way.

4.4 Model Specifications

I will first estimate the following static DiD model:

$$y_{imt} = \beta \mathbf{1}[\text{Year}_t = 1994] \times \text{Semi-Open}_m + \boldsymbol{X}_{imt} + \gamma_m + \delta_t + \epsilon_{imt} \quad (2)$$

where y_{imt} is the outcome of interest in municipality m in year t for individual i. Semi-Open_m is a dummy for being among the seven pilot municipalities m. Year_t is a dummy for being in post-treatment in year t, meaning it is essentially a dummy for 1994. Hence, β is the regression coefficient of interest, displaying the added effect of a municipality having a semi-open list system on the outcome variable in the year 1994 specifically. γ_m represents municipality-fixed effects, meaning I will estimate my results using within-municipality variation. δ_t is year-fixed effects, meaning I control for year-specific shocks. Thus, any confounders that are fixed over time should be absorbed by the municipality-fixed effects, and the year-fixed effects should control for shocks that are common for all municipalities in a given year. ϵ_{imt} is the error term and I will cluster the standard errors at the municipality level. Finally, $\boldsymbol{X}_{imt}$ is a vector of controls, including age, education, gender, home ownership, marital status, occupation, and urbanization. Essentially, running the regressions without controlling for these factors might confound the true effect of the pilot[16].

These are characteristics that previous literature has identified as important aspects of political participation and preferences, both generally (see meta-studies by Kouba & Lysek, 2019; Smets & van Ham, 2013; Stockemer, 2017) and in Sweden specifically (see below) while also being available in the SNES data (see their summary statistics in appendix A.3). These factors were also discussed in the government reports evaluating the implementation of Sweden's semi-open list system (Holmberg & Möller, 1999; SOU 1993:21; SOU 1996:66; SOU 1999:136), particularly the occurrence of preference voting across socioeconomic groups and the socioeconomic spread among politicians receiving preference votes. For example, Holmberg (1999) concluded that education, occupation, urbanization, and immigration status[17] were particularly important socioeconomic predictors of the propensity to preference vote in local elections in 1998. Gender, too, was discussed thoroughly (see in particular Wängnerud, 1993 in SOU 1993:63 and Wängnerud, 1999 in Holberg & Möller, 1999). Apart from an explicit connection to preference voting, the variables of X_{imt} are also discussed in research on the specific outcomes of interest.

Möller (1998) summarized the current literature on *trust in politicians* in the 1990s and concluded that female, rural, and young-to-middle-age voters tended to trust politicians less. Hence, if living in a pilot municipality in 1994 correlates with being a female, rural, young-to-middle-age voter, then any

[16] Note that this vector of controls, as well as the i subscript, apply to the SNES regressions only, and not to the Statistics Sweden regressions.

[17] Although immigration status is a well-researched factor in explaining not least invalid votes (Herron & Sekhon, 2003; Lundell & Högström, 2021; Martinez i Coma & Werner, 2019; McAllister & Makkai, 1993; Petersson et al., 1999), it cannot be controlled for, since the SNES did not record respondents' immigration status in the 1980s or 1990s.

effects on trust in politicians may be confounded by there simply being more female, rural, and young-to-middle-age voters in pilot municipalities in 1994, unless the regressions control for these factors.

For explaining *voter turnout*, cheap access to accurate information seems to be key. Certain socioeconomic factors can explain economies of scale in information acquisition, including occupation, marital status, level of education, age, income, and residency duration (Matsusaka, 1995; see also Dehdari 2021 for the Swedish context). Home ownership seems a suitable proxy for residency duration (Matsusaka 1995; Dehdari 2021; Hall and Yoder 2022) as residency duration was not recorded in the SNES 1980s and 1990s surveys. Information costs have also been discussed as explanatory for *invalid votes* – urbanization has been suggested as particularly important for the tendency to both protest vote and acquire enough information about elections not to cast unintentionally erroneous votes (Kouba & Lysek, 2016; Power & Garand, 2007; Power & Roberts, 1995). Furthermore, level of education has particularly strong support in the literature for predicting voter turnout and invalid votes. For example, Power and Roberts (1995) suggest a higher sense of civic duty (a higher D parameter) discouraging higher educated from casting invalid votes (see also Gallego, 2009 for voter turnout and Lundell & Högström, 2021 for invalid votes in Sweden). In Figure VI of the appendix (A.3), there is evidently an increase in 1994 in the difference between the treated and controls in the share of respondents having three-year upper secondary school as their highest level of education. Running a t-test on this difference yields a p-value of 0.0002, so the null hypothesis of zero difference is rejected. In 1991, the p-value is 0.14, which is also quite low, but for the remaining years, the p-value is higher and ranges between 0.3 and 0.5. Hence, there is a risk that the increase in average educational attainment in pilot municipalities in 1994 will drive the results, if more educated people respond differently to the survey questions of interest. In fact, this is not unlikely given the previous research reviewed on, for example, the effects of preference voting on trust in politicians occurring exclusively among highly educated (see 3.1). I return to this issue in the discussion section.

In regard to *partisanship* and *parties' share of votes,* Petersson et al. (1998) analyzed the state of the Swedish 1990s democracy and found that party membership is more common in the countryside, and that "all aspects of civic engagement [...] clearly correlate with education, occupation and unemployment" (p. 88, my translation). Unemployment is not, however, recorded in comparable ways across the years of study. Möller (1999) also emphasizes the widening gender gap in party sympathies in 1990s Sweden, which would increase the importance of controlling for gender (see also Carreras, 2018 and Kostelka et al., 2019 for a discussion on gender and voter turnout and interest in politics). Finally, occupation is arguably more important to control for than income when examining partisanship and parties' share of votes in Sweden (neither income nor wealth was recorded in comparable ways in SNES across my years of interest). Möller (1999) namely raises class voting in Sweden following the parties' formation in the wake of industrialization, by highlighting the significance of the occupation variable in party affiliation: "the Center Party always mobilizes nearly half of all farmers; the Liberal Party still has a strong position among white collar workers and academics [...]; and nearly half of all business owners (44 percent) vote for the Moderate Party" (p. 267).

As a robustness check of the results yielded by estimating eq. (2), I will also run the following static DiD model:

$$y_{imt} = \beta \mathbf{1}[\text{Year}_t = 1994] \times \text{Semi-Open}_m + \boldsymbol{X}_{imt} + \gamma_m + \delta_t + \gamma_m \times \delta_t + \epsilon_{imt} \qquad (2a)$$

where all parameters are the same as in eq. (2), with the addition of $\gamma_m \times \delta_t$ which denotes municipality-specific time trends. While γ_m and δ_t control for observable time-invariant factors and common time-specific shocks, respectively, their interaction term accounts for unobserved factors that might affect each municipality differently over time. These could be changes in local economic conditions, policy changes at the municipal level, or other dynamic factors that are not captured by the non-interacted fixed effects.

As previously said, a dynamic DiD model could be an important complement to the static DiD models of eq. (2) and eq. (2a). Thus, I will also estimate the following dynamic DiD model:

$$y_{imt} = \sum_{t=1973}^{1994} \zeta(\text{Year}_t \times \text{Semi-Open}_m) + \boldsymbol{X}_{imt} + \eta_m + \theta_t + \iota_{imt} \quad (3)$$

where η_m is municipality-fixed effects, θ_t is year-fixed effects, and ι_{imt} is the error term. I will cluster the standard errors at the municipality level also in the dynamic DiD model. In eq. (3), $\text{Year}_t \times \text{Semi-Open}_m$ estimates the differential effect of the treatment of preference voting *over time and across different municipalities*. The coefficient of interest is thus ζ. With an interaction term for each year, I consider the possibility that the effect of being in a pilot municipality on, for example, trust in politicians differs each year. Note that t does not always go from 1973 to 1994, given the data availability described in 4.1.1 and 4.2. In all regressions, though, 1991 will be the reference group, to illustrate any potential causal effect of the pilot more clearly in the coefficient plots.

Using the model specifications of eq. (2), eq. (2a), and eq. (3), I will test the following hypotheses for each outcome reviewed in 2.4:

$$H_0: \beta, \zeta = 0$$
$$H_A: \beta, \zeta \neq 0$$

Two assumptions must hold to interpret such effects of the 1994 pilot causally. The first is the assumption of the fixed effects absorbing any confounders, as laid out previously. The second is the parallel pre-trends assumption that, had the pilot municipalities not introduced semi-open lists in 1994, then any given outcome of interest, such as trust in politicians, would have evolved in the same way over time in the pilot municipalities as in the control municipalities. While the assumption of parallel pre-trends is not directly testable, one could briefly assess the plotted time trends in the appendix (A.5). From a purely visual assessment, the respective pre-treatment trends of a majority of the outcomes of interest appear parallel indeed, which supports the applicability of a DiD framework[18]. In the next section, coefficient plots from the dynamic DiD model will also be presented, as more precise assessments of pre-treatment trends.

5 Results

I present below the results from running the DiD regressions of eq. (2) and (3), and discuss potential mechanisms of the findings. The average time trends are presented in the appendix (A.5). The robustness checks of eq. (2a) are covered briefly in the main results, while their associated regression tables are reviewed in appendix A.6. Finally, the coefficient plots of the dynamic DiD model are presented in this section, while their corresponding regression tables are presented in appendix A.7.

5.1 Trust in and Perception of Politicians and Parties

In section 2.4, I hypothesized that the pilot would increase the respondents' trust in politicians and the political system. In sum, there seems to be some support for preference voting increasing trust in politicians. There seems to be no support, however, for the pilot affecting the respondents' perception of political parties.

5.1.1 Trust in Politicians

In the 1988, 1991, and 1994 elections, SNES respondents were asked to self-assess their trust in politicians along a four-point scale ranging from very high trust to very low trust. Using a continuous scale as outcome does not allow for analyzing which groups of respondents are affected by the pilot. Instead, the outcome of

[18] That is, where there is sufficient pre-treatment data to distinguish pre-treatment trends. I will come back to this issue in both the results section and the discussion section.

interest can be recoded into an indicator variable in various ways, equaling one if respondents say they have (1) very high trust in politicians, or (2) very or pretty high in politicians, or (3) very high, pretty high, or pretty low trust in politicians – in other words, everyone not saying they do not have very low trust in politicians. The extreme values of the survey scale are arguably the most important for eliciting the respondent's views, indicated by the much lower number of responses on the extreme ends (Table III in appendix A.4). I present below all results for each possible coding for the sake of transparency.

Very High Trust in Politicians

Measuring trust in politicians as everyone stating that they have very high trust in politicians yields no statistically significant results, as presented in Table 1 and Figure 1 below (see also Table IX in appendix A.7 for the corresponding DiD regression table).

Table 1. The table reports the regression coefficients from the static DiD of eq. (2) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high trust in politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here.

Very high trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.0226	0.0362	0.00519
	(0.0168)	(0.0322)	(0.00685)
Observations	7,312	3,341	3,971
Adjusted R-squared	0.054	0.095	0.098
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0215	0.0187	0.0238

Standard errors in parentheses, clustered by municipality

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

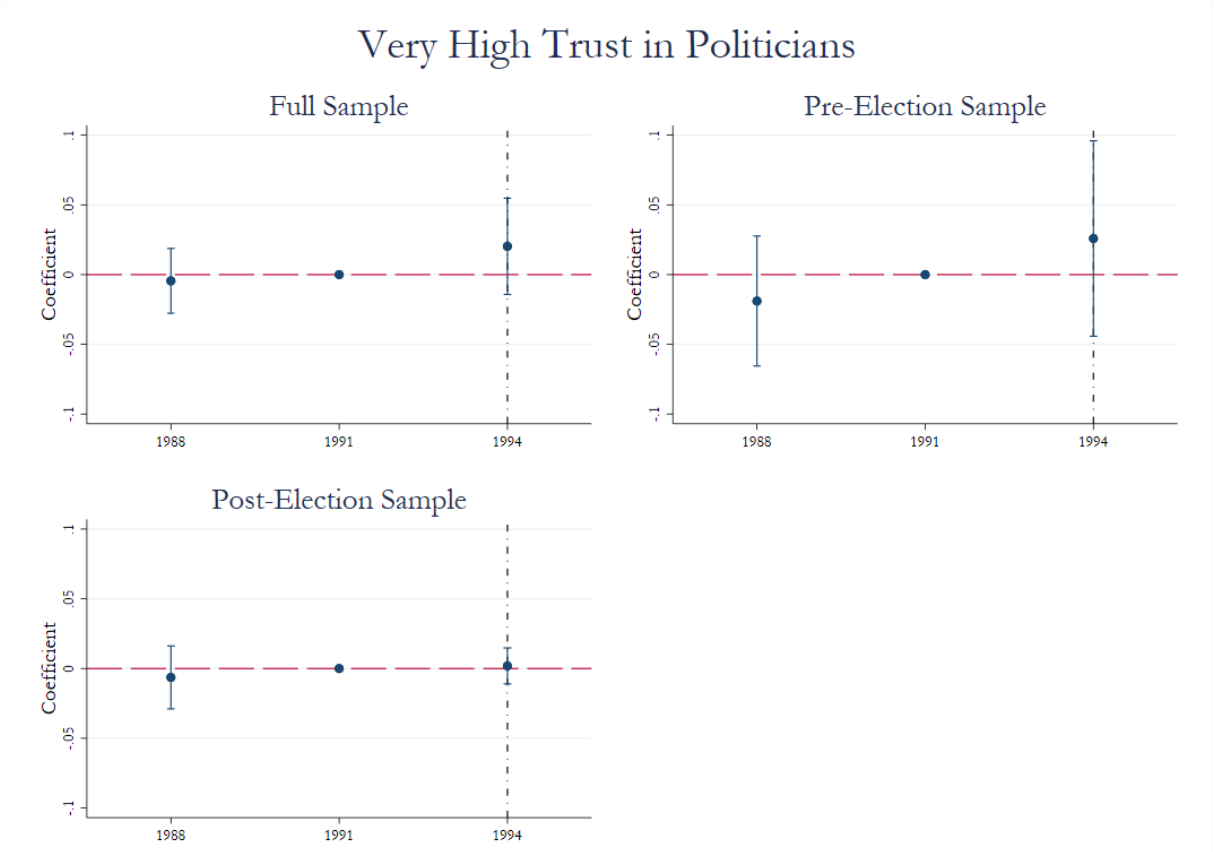

Figure 1. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome, separately for the full sample, pre-election sample, and post-election sample. The outcome is coded here as an indicator variable which equals one if respondents said they have very high trust in politicians. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table IX in appendix A.7.

Very or Pretty High Trust in Politicians

Measuring trust in politicians as everyone stating that they have very high or pretty high trust in politicians yields no statistically significant results either, as presented in Table 2 and Figure 2 below (see also Table X in appendix A.7 for the corresponding DiD regression table).

Table 2. The table reports the regression coefficients from the static DiD of eq. (2) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high or pretty high trust in politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here.

	Very or pretty high trust in politicians		
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.0154	-0.0409	0.0125
	(0.0415)	(0.0715)	(0.0584)
Observations	7,312	3,341	3,971
Adjusted R-squared	0.066	0.117	0.099
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.3998	0.3785	0.4167

Standard errors in parentheses, clustered by municipality

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

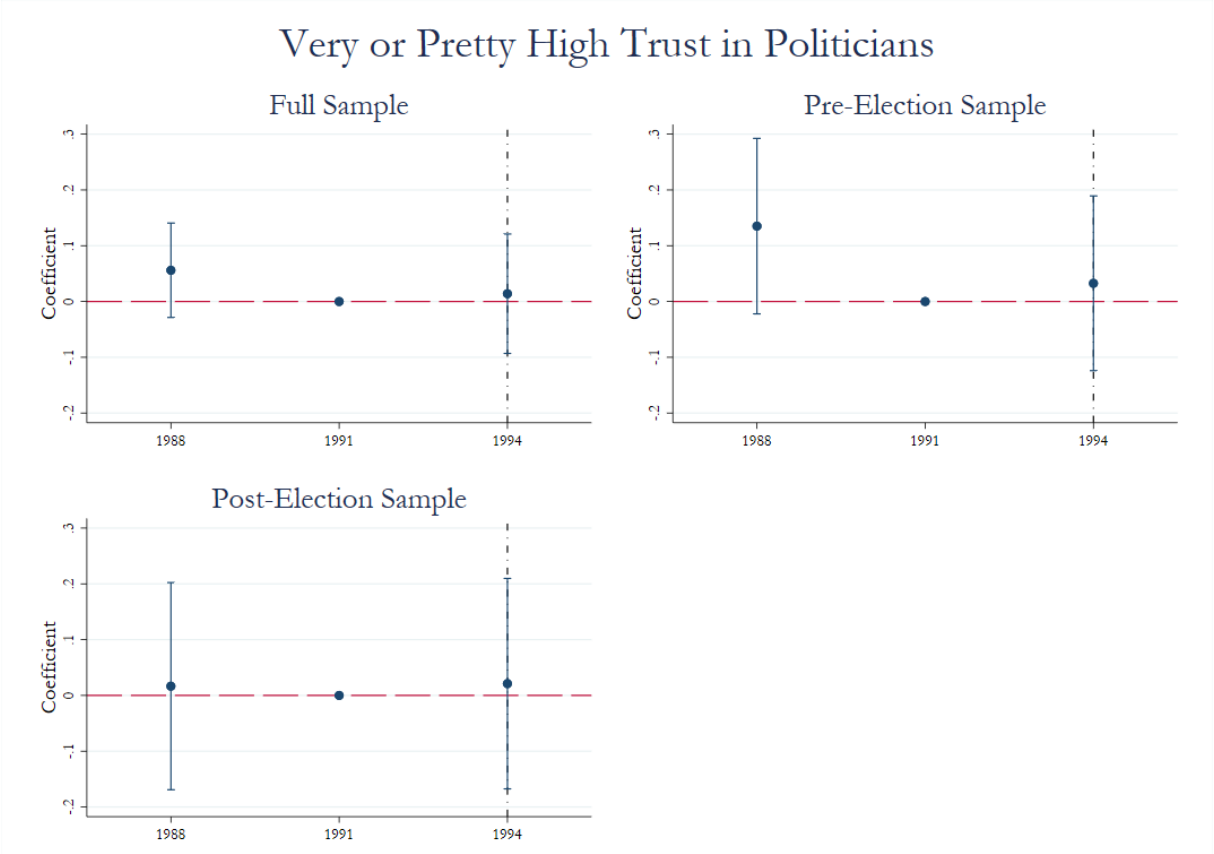

Figure 2. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome, separately for the full sample, pre-election sample, and post-election sample. The outcome is coded here as an indicator variable which equals one if respondents said they have very high or pretty high trust in politicians. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table X in appendix A.7.

Do Not Have Very Low Trust in Politicians

As seen in Table 3, measuring trust in politicians as everyone not saying that they do not have very low trust in politicians yields statistically significant results in 1994 at the one-percent level. The estimated coefficient of the interaction term corresponds to the added effect of a municipality having a semi-open list system in 1994 on trust in politicians. As seen in column (1), where the regression was run on the full sample, the fraction of people reporting high trust in politicians was on average 10.7 percentage points higher in pilot municipalities in 1994. That is, given the assumptions, introducing preference voting increased the fraction of respondents stating that they do not have very low trust in politicians by, on average, 10.7 percentage points. Considering the pre-election sample only, in column (2), the corresponding increase in the fraction of respondents reporting high trust in politicians before the election was on average 10.5 percentage points. The increase in the fraction of respondents reporting high trust in politicians after the election (column (3)) was on average 10.6 percentage points.

Table 3. The table reports the regression coefficients from the static DiD of eq. (2) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high, pretty high, or pretty low trust in politicians, meaning it includes everyone not saying that they did not have very low trust in politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here.

Do not have very low trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.107***	0.105***	0.106***
	(0.0262)	(0.0322)	(0.0274)
Observations	7,312	3,341	3,971
Adjusted R-squared	0.056	0.117	0.079
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.8823	0.8796	0.8838

Standard errors in parentheses, clustered by municipality

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

Given that the average population size of a pilot municipality in 1994 was about 59,000 (see Table I in appendix A.3), even the smallest of the significant increases presented above of 10.5 percentage points corresponds to a considerable number of people affected by the pilot, namely about 6,200 people. These increases are also economically meaningful – if the assumptions hold, the introduced possibility to vote for individual candidates made voters trust politicians more. Not only is this interesting to the field of political economy, but it was also one of the aims of the reform (see 2.2). Apart from the assumption that the fixed effects absorb any confounders, the assumption of parallel pre-trends is crucial for the causal interpretation of these results. However, as seen in Figure XIV in appendix A.5, the averages over time in treated and control municipalities do not seem to align. It is also difficult to evaluate pre-treatment trends with only two pre-treatment periods. I return to this issue in the discussion. More importantly, these results do not seem robust to interacted fixed effects, although the positive direction of the coefficients remains (Table IV, A.6).

In Figure 3 below (see also Table XI in A.7 for the corresponding regression table), the result of interest is the coefficient for $\text{Year}_{1994} \times \text{Semi-Open}_m$, which is the additional effect of the year being 1994 and the municipality having a semi-open list, *on top of* the average difference. In other words, it shows the causal effect of having a semi-open list in 1994 on trust in politicians. Here, only the regression run on the full sample proves statistically significant, at the one-percent level. The share of respondents in the full sample having high trust in politicians in pilot municipalities was on average 11.0 percentage points higher

than in control municipalities in 1994 (see Table XI in A.7). Again, referring to what was said on the magnitude and economic significance of the static DiD results, this is indeed an interesting result. The parallel pre-trends assumption might hold, given that the coefficient for 1988 is not statistically significant, but pre-treatment trends would, again, be best evaluated with a longer pre-treatment period.

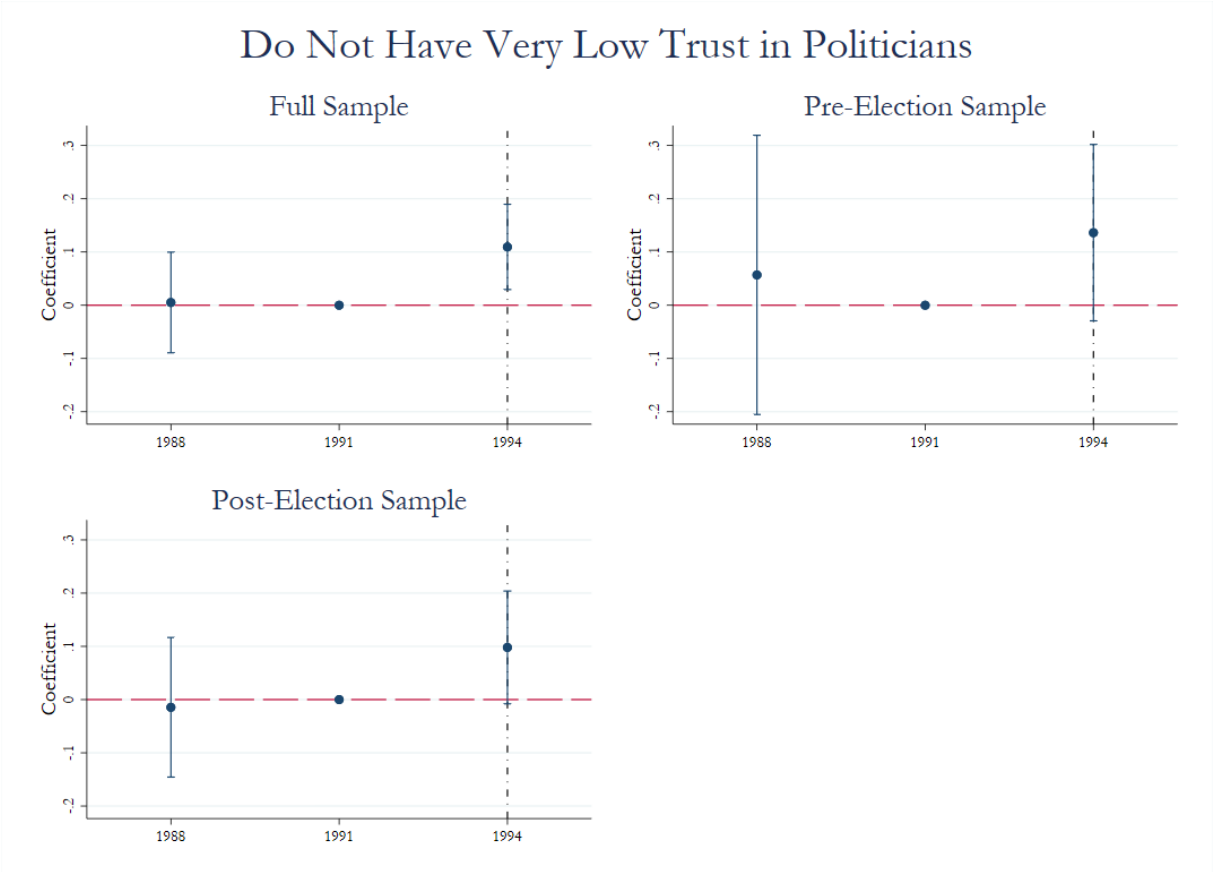

Figure 3. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome, separately for the full sample, pre-election sample, and post-election sample. The outcome is coded here as an indicator variable which equals one if respondents said they have very high, pretty high, or pretty low trust in politicians, meaning it includes everyone not saying that they did not have very low trust in politicians. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XI in appendix A.7.

The dynamic DiD yielding significant results only for the full-sample model is unexpected, as I hypothesized in 4.1 that the full sample might confound the true effect of preference voting in a pilot municipality in 1994 with individuals unaware of being part of a governmental experiment on preference voting before the election took place. There seems to be no support for this hypothesis when considering trust in politicians.

5.1.2 Perception of Politicians

Respondents were asked to rank their level of agreement with the statement "Those [politicians] in Parliament often disregard the opinion and thoughts of ordinary people" along a four-point scale. Again, this can be recoded into an indicator variable equaling one if respondents (1) completely disagree that politicians disregarding the opinion of ordinary people, or (2) completely or mostly disagree, or (3) completely agree, mostly agree, or mostly disagree, meaning everyone not saying they completely agree. Yet again, I argue that the recoding of these responses is ambiguous, where the most important for eliciting a positive perception of politicians is setting "Completely agree" to zero and "Completely disagree" to one

(see also the distribution of responses in Table III in A.4). Hence, I run the regressions on all three alternative codings.

Completely Disagree with Statement "Politicians in Parliament Often Disregard the Opinion of Ordinary People"

Measuring a positive perception of politicians as everyone saying that they completely disagree that politicians disregard the opinion of ordinary people yields no statistically significant results using the static DiD model, as presented in Table 4 below.

Table 4. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should a positive perception of politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Completely disagree with "Politicians in Parliament often disregard the opinion of ordinary people"			
	(1) Full sample	(2) Pre-election sample	(3) Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.0271	0.0370	0.0185
	(0.0242)	(0.0232)	(0.0234)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.030	0.055	0.051
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0362	0.0444	0.0478

Standard errors in parentheses, clustered by municipality

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

Using the dynamic DiD model, however, yields a statistically significant enhancement of the perception of politicians of 4.81 percentage points at the five-percent level for the pre-election sample, as presented in Figure 4 below (see also Table XII in A.7 for the corresponding regression table), but the statistical significance of also the 1982 coefficient prohibits the causal interpretation of this increase.

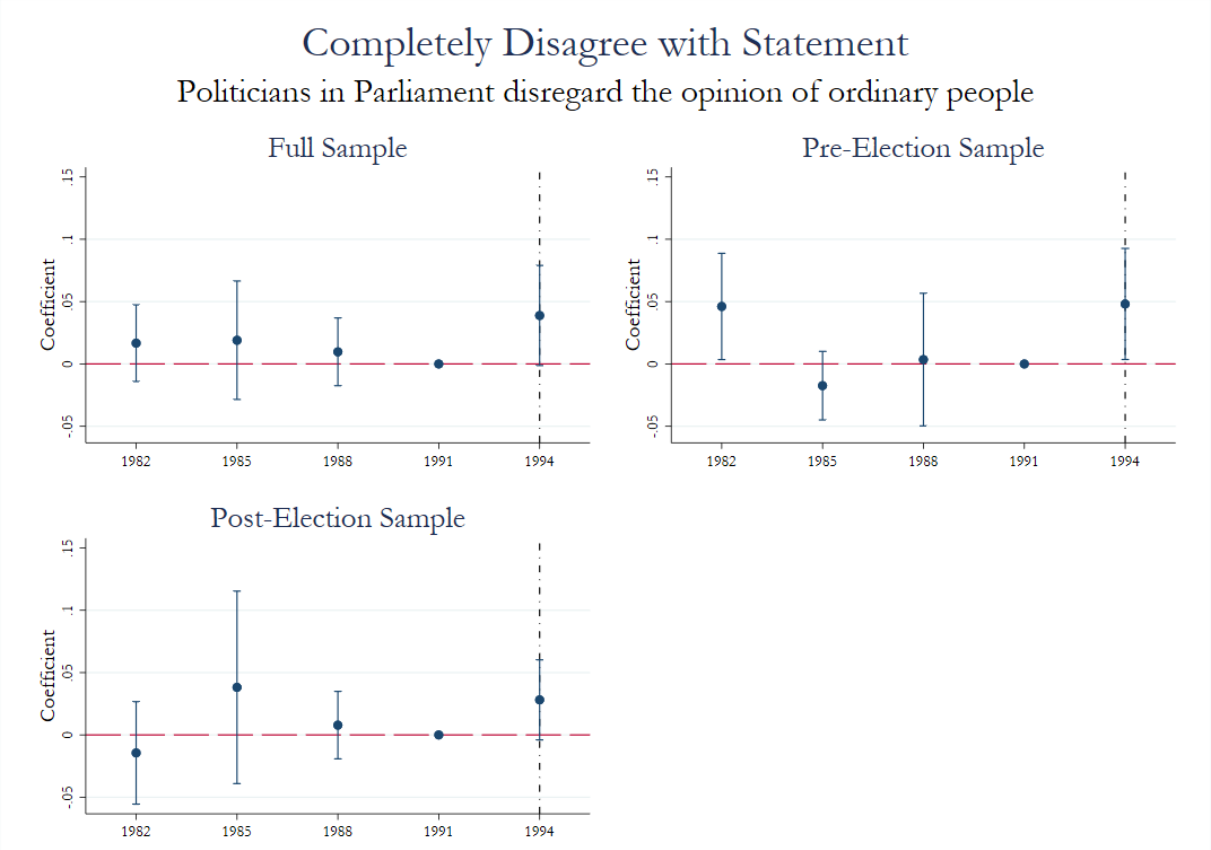

Figure 4. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should exhibit a positive perception of politicians. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XII in appendix A.7.

Completely or Mostly Disagree with Statement "Politicians in Parliament Often Disregard the Opinion of Ordinary People"

Measuring a positive perception of politicians as everyone saying that they completely or mostly disagree that politicians disregard the opinion of ordinary people yields no statistically significant results, as presented in Table 5 and Figure 5 below (see Table XIII in A.7 for the corresponding DiD regression table).

Table 5. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree or mostly disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should exhibit a positive perception of politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Completely or mostly disagree with "Politicians in Parliament disregard the opinion of ordinary people"			
	(1) Full sample	(2) Pre-election sample	(3) Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.00952	-0.0422	0.0152
	(0.0253)	(0.0383)	(0.0590)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.070	0.100	0.086
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.1940	0.2615	0.2393

Standard errors in parentheses, clustered by municipality

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

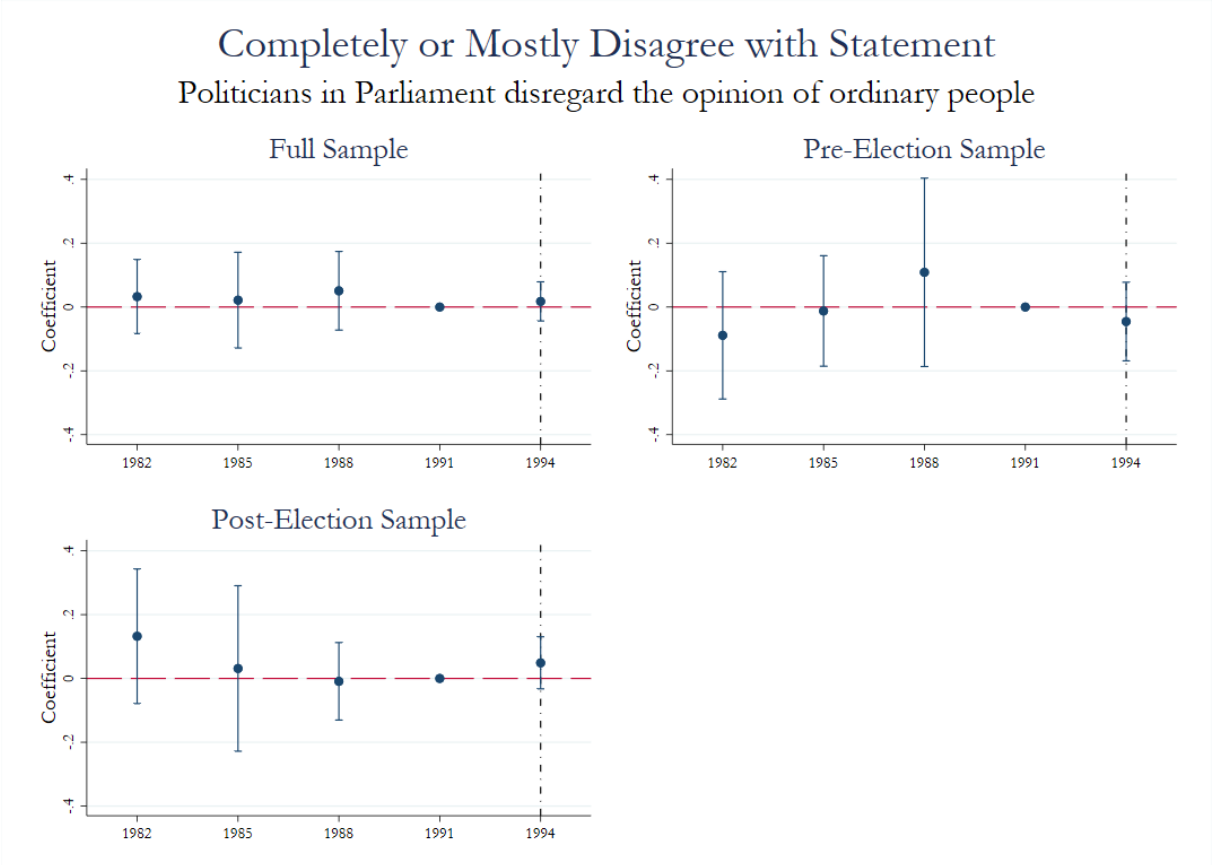

Figure 5. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely or mostly disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should exhibit a positive perception of politicians. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XIII in appendix A.7.

Do Not Completely Agree with Statement "Politicians in Parliament Often Disregard the Opinion of Ordinary People"

Measuring a positive perception of politicians as everyone not saying that they completely agree that politicians disregard the opinion of ordinary people yields statistically significant results at the five-percent level in the pre-election sample, as presented in Table 6 below. As seen in column (2), the fraction of people reporting a positive perception of politicians before the election was on average 9.9 percentage points higher in pilot municipalities in 1994. That is, given the assumptions, introducing the semi-open list increased the fraction of respondents indicating before the election that they have a positive perception of politicians by, on average, 9.9 percentage points.

Table 6. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Politicians in Parliament often disregard the opinion of ordinary people". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Do not completely agree with "Politicians in Parliament disregard the opinion of ordinary people"			
	(1) Full sample	(2) Pre-election sample	(3) Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.0648	0.0990**	0.0247
	(0.0657)	(0.0461)	(0.0944)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.079	0.116	0.091
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5088	0.7203	0.6018

Standard errors in parentheses, clustered by municipality

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

Given that the average population size of a pilot municipality in 1994 was about 59,000 (see Table 1 in appendix A.3), the increase of 9.9 percentage points corresponds to a considerable number of people affected by the pilot, namely about 5,800 people. This increase is also economically meaningful – if the assumptions hold, the introduced possibility to vote for individual candidates made voters' perception of politicians more positive. Not only is this interesting to the field of political economy, but it was also one of the aims of the reform (see 2.2). Furthermore, the significance of the coefficient for the pre-election sample might contradict the conclusion in SOU 1996:66 of lack of information on the pilot. However, it is then unexpected that the coefficient for the post-election sample is non-significant. Perhaps this can be explained by voters misconceiving that they would get more power than they actually did in circumventing the parties' ordering of candidates. That is, perhaps trust in politicians went up before the election in anticipation of the new semi-open list system but went down again after the election, when the election results had been announced, and respondents realized that their preference vote had not been sufficient to elect their preferred candidate.

Apart from the assumption that the fixed effects absorb any confounders, the assumption of parallel pre-trends is crucial for the causal interpretation of these results. Hence, I plot in Figure XVII in the appendix (A.5) the averages over time, separately for treated and control municipalities. The pre-trends appear somewhat parallel, from a purely visual assessment, although the time trend for treated municipalities in the pre-election sample looks more volatile than the corresponding time trend for controls. Moreover, the pre-election sample regression does not prove robust to interacted fixed effects, as the p-value increases to about ten percent, although the positive direction remains (see Table V, A.6). Finally, the dynamic DiD regression run with the same outcome, also separately for the full sample, pre-election sample and post-election sample, yields no statistically significant results, as presented in Figure 6 below (see also Table XIV in appendix A.7 for the corresponding DiD regression table).

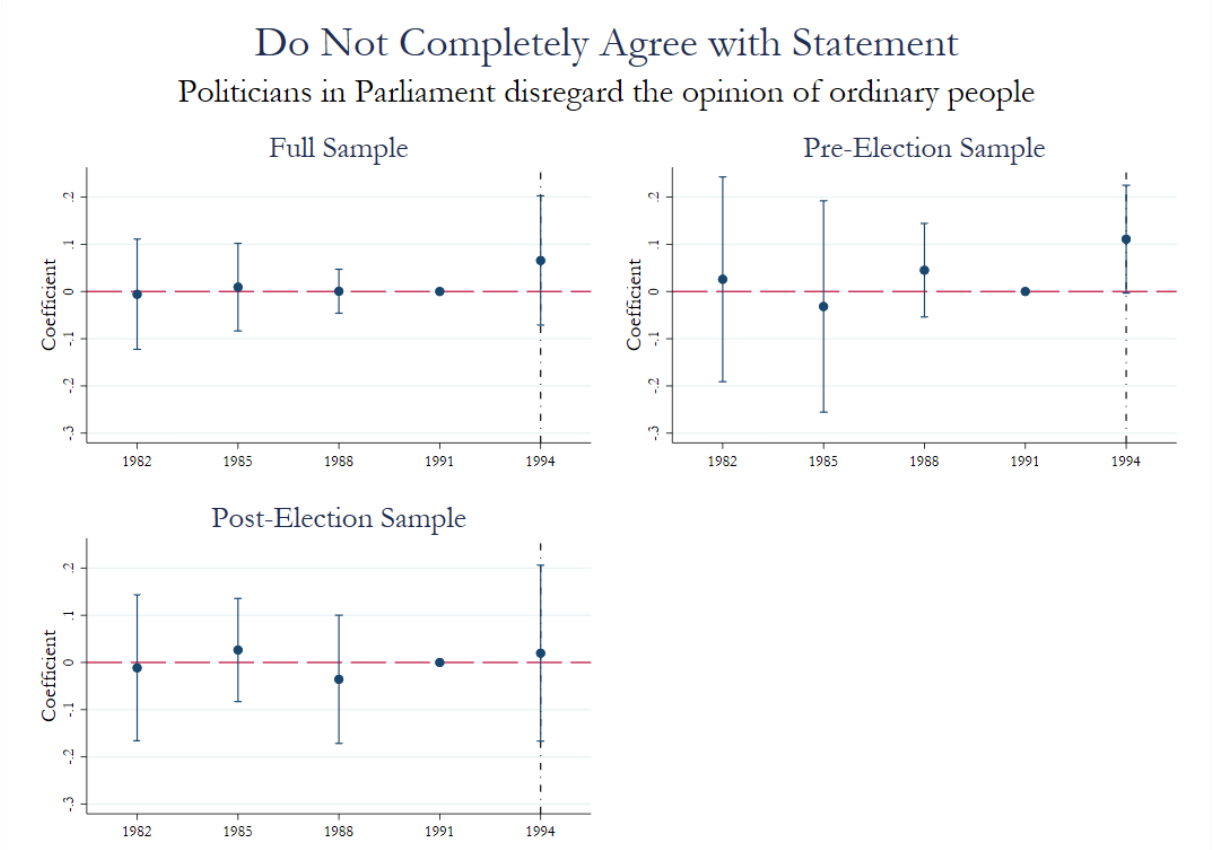

Figure 6. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Politicians in Parliament often disregard the opinion of ordinary people". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of politicians If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XIV in A.7.

5.1.3 Perception of Parties

Respondents were asked to rank their level of agreement with the statement "Parties only care about people's votes, not their opinion" along a four-point scale. Again, this can be recoded into an indicator variable equaling one if respondents (1) completely disagree that parties only caring about people's votes, or (2) completely or mostly disagree, or (3) completely agree, mostly agree, or mostly disagree, meaning everyone not saying they completely agree. As previously argued, the recoding of these responses is ambiguous, where the most important for eliciting a positive perception of parties is setting "Completely agree" to zero and "Completely disagree" to one (see also the distribution of responses in Table III in A.4). Hence, I run the regressions on all three alternative codings.

Completely Disagree with Statement "Parties Only Care About People's Votes, Not Their Opinion"

Measuring a positive perception of parties as everyone saying that they completely disagree that parties only care about people's votes yields no statistically significant results, as presented in Table 7 and Figure 7 below (see also Table XV in appendix A.7 for the corresponding DiD regression table).

Table 7. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should a positive perception of parties. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Completely disagree with "Parties only care about people's votes, not their opinion"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.000426	0.0135	-0.00963
	(0.0221)	(0.0358)	(0.0166)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.033	0.058	0.047
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0414	0.0526	0.0535

Standard errors in parentheses, clustered by municipality

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

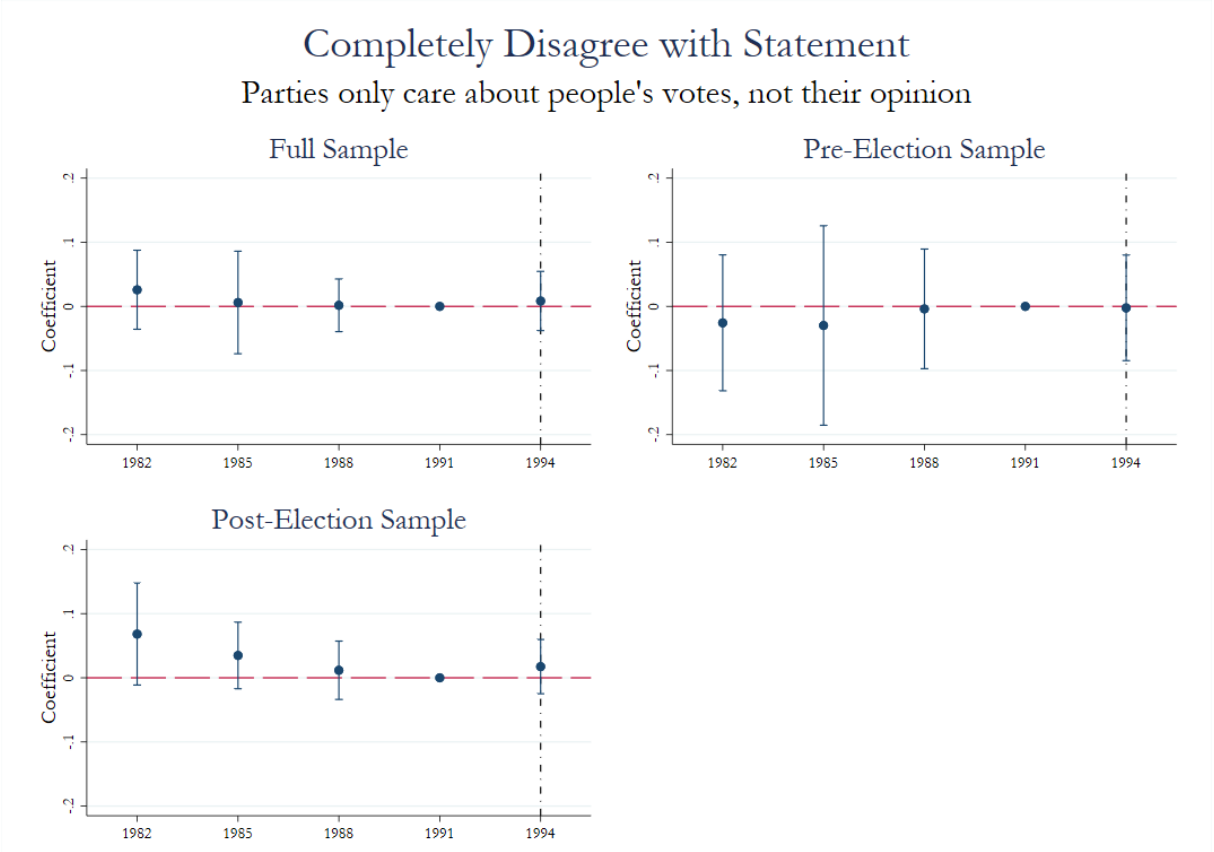

Figure 7. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should exhibit a positive perception of parties. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XV in appendix A.7.

Completely or Mostly Disagree with Statement "Parties Only Care About People's Votes, Not Their Opinion"

Measuring a positive perception of parties as everyone saying that they completely or mostly disagree that parties only care about people's votes yields no statistically significant results at the five- or one-percent level, as presented in Table 8 and Figure 8 below (see also Table XVI in appendix A.7 for the corresponding DiD regression table).

Table 8. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely or mostly disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should exhibit a positive perceptionof parties. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Completely or mostly disagree with "Parties only care about people's votes, not their opinion"			
	(1) Full sample	(2) Pre-election sample	(3) Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.00706	-0.0499	0.0366
	(0.0292)	(0.0469)	(0.0567)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.067	0.094	0.087
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.2096	0.2874	0.2549

Standard errors in parentheses, clustered by municipality

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

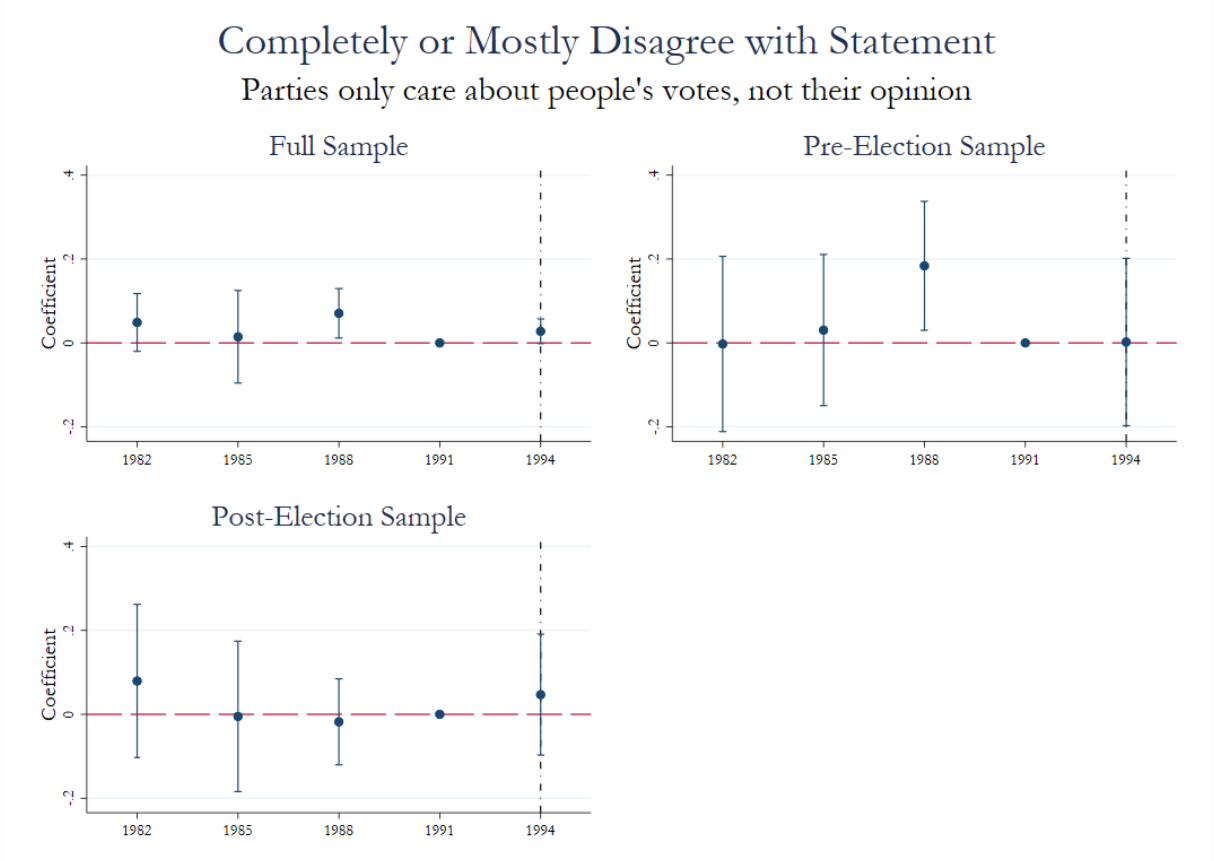

Figure 8. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely or mostly disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should exhibit a positive perception of parties. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the

coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XVI in appendix A.7.

Do Not Completely Agree with Statement "Parties Only Care About People's Votes, Not Their Opinion"

Finally, measuring a positive perception of parties as everyone saying that they do not completely agree that parties only care about people's votes yields no statistically significant results either, as presented in Table 9 and Figure 9 below (see also Table XVII in A.7 for the corresponding DiD regression table).

Table 9. The table reports the regression coefficients from the static DiD of eq. (2) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Parties only care about people's votes, not their opinion". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of parties. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Do not completely agree with "Parties only care about people's votes, not their opinion"			
	(1) Full sample	(2) Pre-election sample	(3) Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.00736	0.0312	0.00185
	(0.0742)	(0.0891)	(0.0922)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.093	0.128	0.111
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5168	0.7340	0.6097

Standard errors in parentheses, clustered by municipality

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

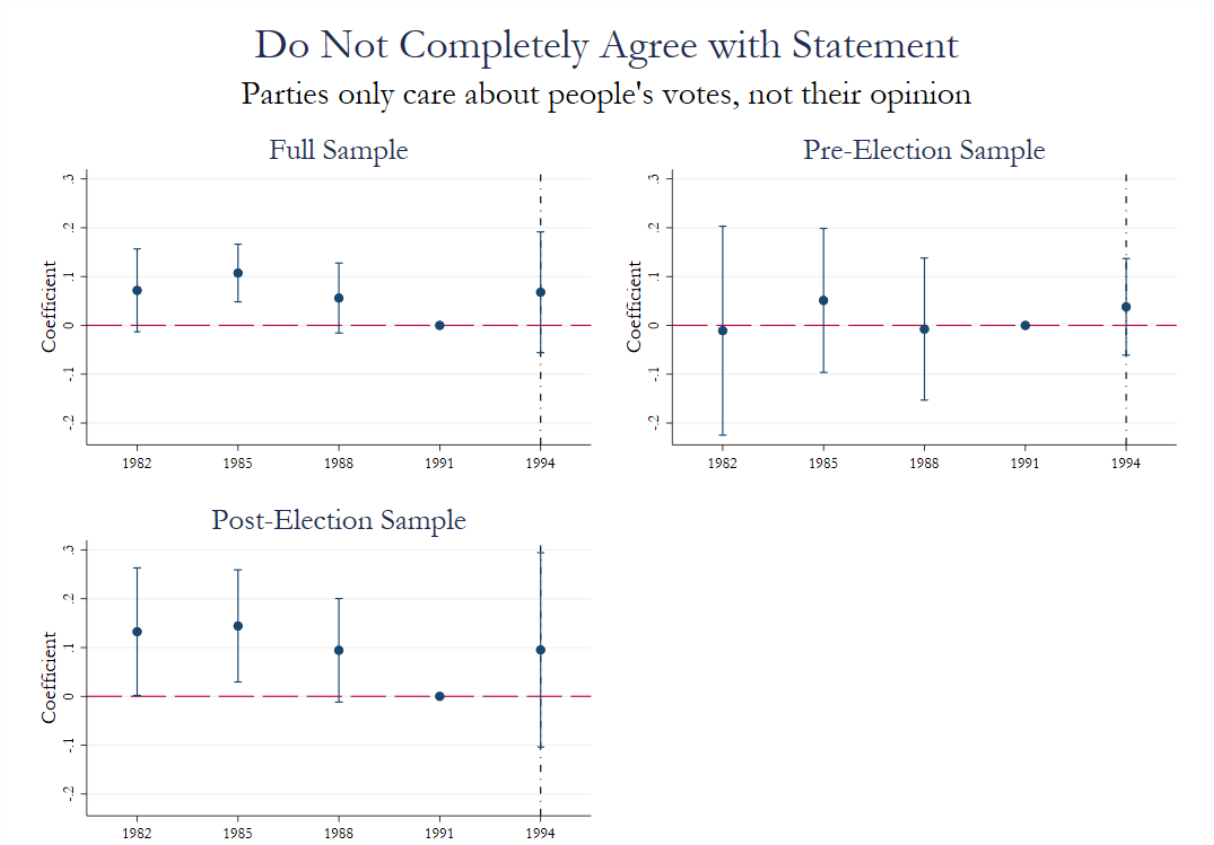

Figure 9. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with respondents' perception parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Parties only care about people's votes, not their opinion". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of parties. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XVII in appendix A.7.

5.2 Voter Turnout

As argued in 4.2, voter turnout is best examined using both the Statistics Sweden data and the SNES data. As seen in Table 10 and Figure 10 below, there seems to be no statistically significant effects of the 1994 pilot on voter turnout in the Municipal Council elections, using neither the aggregate-level Statistics Sweden data (1973-1994) without controls, nor the individual-level data from the SNES (1982-1994) with controls. Hence, there is no support for the hypothesis that the pilot caused an increase in voter turnout.

Table 10. The table reports the regression coefficients from the static DiD of eq. (2) with voter turnout to Municipal Council elections as outcome. Column (1) uses data from Statistics Sweden (1973-1994) without controls, and column (2) uses data from the SNES (1982-1994) with controls.

Voter Turnout (%)		
	(1) Statistics Sweden data	(2) SNES data
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.526	-1.575
	(0.375)	(2.469)
Observations	2,260	12,790
Adj. R-squared	0.926	0.064
Year & municipality FE	✓	✓
Controls	-	✓
Mean of dependent variable	84.702	88.944

Standard errors in parentheses, clustered by municipality

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

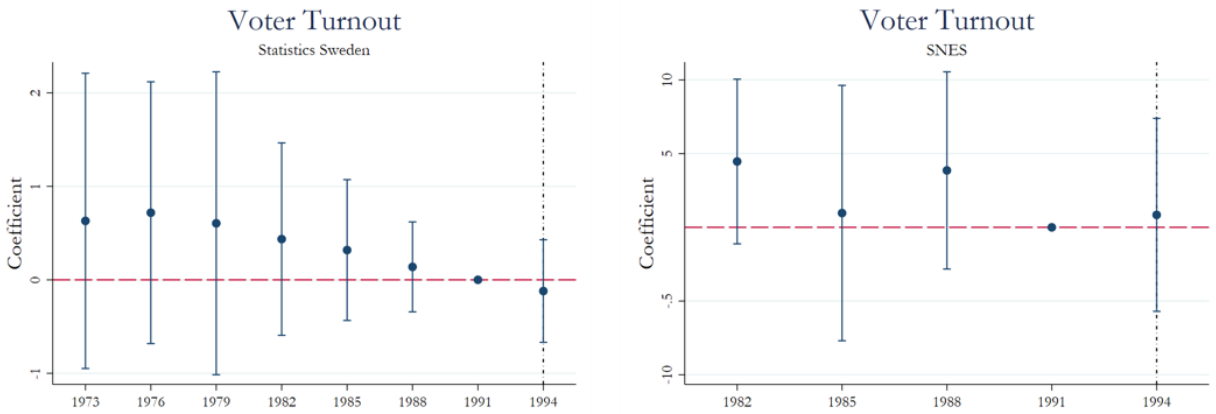

Figure 10. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with voter turnout to Municipal Council elections as outcome, using aggregate-level data without controls from Statistics Sweden (1973-1994) t.t.l. and individual-level data with controls from SNES (1982-1994) t.t.r. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XVIII in appendix A.7.

5.3 Partisanship

In section 2.4, I hypothesized that the pilot would decrease the respondents' partisanship. The SNES variable for partisanship was already coded into an indicator variable (see Table III in appendix A.4 for the distribution of responses). Using this indicator variable as outcome yields statistically significant results at the five-percent level when run on the post-election sample. As seen in column (3) of Table 11, the fraction of people considering themselves partisan after the election was on average 9.53 percentage points lower in pilot municipalities in 1994. That is, given the assumptions, introducing the semi-open list decreased the fraction of respondents stating after the election that they were partisan by, on average, 9.53 percentage points.

Table 11. The table displays the results of running the static DiD specification in eq. (2) with an indicator variable for being partisan as outcome, which equals one if respondents say "Yes" to being partisan. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only.

Partisanship, self-assessed			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.0711*	-0.0504	-0.0953**
	(0.0397)	(0.0513)	(0.0421)
Observations	12,381	5,530	6,851
Adjusted R-squared	0.102	0.143	0.111
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5425	0.5435	0.5434

Standard errors in parentheses, clustered by municipality

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

Given that the average population size of a pilot municipality in 1994 was about 59,000 (see Table 1 in appendix A.3), the decrease of 9.53 percentage points corresponds to a considerable number of people affected by the pilot, namely about 5,600 people. This decrease is also economically meaningful. If the assumptions hold, the introduced possibility to vote for individual candidates made voters less partisan after the election. Voters may have formed stronger ties with the individual candidates that they preference voted for than with their associated party. Not only is this interesting to the field of political economy, but strengthening the bond between voters and politicians was also one of the goals of the reform (see 2.2). Furthermore, the significance of the post-election sample only would be in line with the conclusion of SOU 1996:66 that there was a general lack of information on the new system among pilot municipalities.

Apart from the assumption that the fixed effects absorb any confounders, the assumption of parallel pre-trends is crucial for causal interpretation of these results. Hence, I plot in Figure XXII in the appendix (A.5) the averages over time, separately for treated and control municipalities. From a purely visual assessment, the time trends seem parallel at large, although the time trend for treated municipalities in the pre-election sample again looks more volatile than the corresponding time trend for controls. More importantly, as seen in Table VI of the appendix (A.6), this result does not prove robust to interacted fixed effects, although the direction of the coefficient remains negative. The dynamic DiD yields no statistically significant results either, as seen in Figure 11 below (see also Table XIX in appendix A.7 for the corresponding DiD regression table). Hence, there seems to be no robust support for the hypothesis that the pilot would cause a decrease in partisanship.

Figure 11. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with an indicator variable for being partisan as outcome, which equals one if respondents say "Yes" to being partisan. If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XIX in appendix A.7.

5.4 Invalid Votes

In section 2.4, I hypothesized that the share of invalid votes in Municipal Council elections would increase from the pilot. Running the DiD regressions of eq. (2) and (3), however, yields results indicating instead a decrease. As seen in column Table 12 below, the share of invalid votes in Municipal Council elections was on average 0.264 percentage points lower in pilot municipalities in 1994. That is, given the assumptions, introducing the semi-open list decreased the share of invalid votes by, on average, 0.264 percentage points. The corresponding decrease with interacted fixed effects (Table VII in appendix A.6) is 0.396 percentage points, which is statistically significant even at the one-percent level. With the dynamic model (see Figure 12 below and Table XX in A.7), the decrease was 0.409 percentage points, also statistically significant at the one-percent level.

Admittedly, given that the average population size of a pilot municipality in 1994 was about 59,000 (see Table 1 in appendix A.3), these decreases do not correspond to a considerable number of people. The smallest decrease of 0.264 percentage points, for example, corresponds to only about 160 people. However, considering that the mean of the dependent variable across the entire data set is merely about 1.1 percent, these results are still economically interesting. Apart from the assumption that the fixed effects absorb any confounders, the assumption of parallel pre-trends is crucial for the causal interpretation of these results. The pre-trends in Figure XXIII in the appendix (A.5) appear parallel indeed, from a purely visual assessment and the coefficients preceding 1994 in Figure 12 below are not statistically different from zero.

Table 12. The table displays the results from running the static DiD specification of eq. (2) with the share of invalid votes in Municipal Council elections as outcome, using Statistics Sweden data (1976-1994).

	Share of Invalid Votes (%)
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.264**
	(0.118)
Observations	45,168
Adjusted R-squared	0.317
Year & municipality FE	✓
Controls	-
Mean of dependent variable	1.132

Standard errors in parentheses, clustered by municipality
*** p<0.01, ** p<0.05, * p<0.1

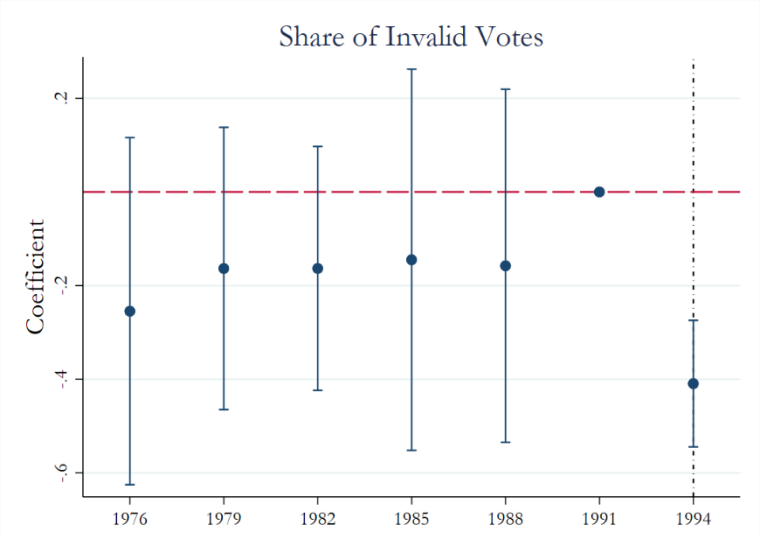

Figure 12. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with the share of invalid votes in Municipal Council elections as outcome, using Statistics Sweden data (1976-1994). If the confidence intervals of these coefficients include zero – that is, if they intersect with the dashed horizontal line – then the coefficient is not statistically significant from zero. The dotted vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. For the corresponding regression table, see Table XX in appendix A.7.

One possible explanation for the unexpected decrease could be that the potential decline in protest votes indeed outweighed the potential increase in invalid votes from the enhanced complexity of the electoral system. In other words, the introduction of preference voting may have increased voters' satisfaction with the political system (see 3.1), for example with the increased element of political competition (see 3.4), making them less prone to protest vote. This is, of course, assuming that a majority of the invalid votes in previous years were indeed protest votes. If this assumption is not reasonable, another possible mechanism would be the extensive information campaigns launched in some of the pilot municipalities to prepare the population for the new electoral system. Perhaps these outreaches picked up voters who would otherwise have made unintentional errors when filling out the ballot. That is, an information campaign on how to preference vote enhanced the population's understanding on how to cast valid votes overall. The information about the upcoming pilot might have alerted voters to, for example, pay extra attention to voter instructions than before. This is, however, assuming that information on the pilot was distributed in all pilot municipalities – while Johansson et al. (1996) rather point to the differences between pilot municipalities in terms of information distribution. Alternatively, in line with the findings of Johansson et al. (1996), less politically interested voters tend to see politics more in terms of people. Hence, assuming that the invalid

votes in previous years were unintentionally erroneous votes primarily cast by voters with little interest in politics, the introduction of preference voting made them more interested in and more capable of casting valid votes in general. All in all, while there is no support for the hypothesis that the pilot increased the share of invalid votes, there seems to be support for the pilot having decreased the share of invalid votes.

5.5 Parties' Share of Votes

Finally, I hypothesized in section 2.4 that the pilot would cause a decrease in the share of votes for the Social Democratic Party, the Left Party, and the Green Party, the anti-preference voting bloc, and an increase in the share of votes for the Moderate Party, the Center Party, the Liberal Party, and the Christian Democrats, the pro-preference voting bloc. Running the regressions with the voter share for each party in the Municipal Council election as outcome, yields a statistically significant decrease in the share of votes for the Left Party, and the pre-trends seem parallel (see Figure XXIIV in the appendix, A.5). As seen in column (5) of Table 13 below, the share of votes for the Left Party was on average 1.362 percentage points lower in pilot municipalities in 1994. That is, given the assumptions, introducing the semi-open list decreased the share of votes for the Left Party by, on average, 1.362 percentage points. This decrease also seems robust to interacted fixed effects (see Table VIII in the appendix, A.6), although the decrease then diminishes to 0.759 percentage points, but remains statistically significant at the five-percent level. Compared to the mean share of votes for the Left Party, these are not non-significant decreases.

Notably, there are no discernable effects on the share of votes for neither the Social Democratic Party, the biggest party in the anti-preference voting bloc, nor the Green Party. This might be because the Left Party was more vocal about their opposition to preference voting (see for example dissenting opinion of the Left-Party committee member Mats Einarsson in SOU 1999:136). However, no party of the pro-preference voting bloc seems to have been affected in a statistically significant way either. In fact, non-significant results on the share of votes by party might be explained by the debate on the preference voting pilot being limited in outreach at the time. According to SOU 1999:136 and Möller and Holmberg (1999), the debate on preference voting was substantiated in the mass media in 1998, when preference voting was introduced in all municipalities for the general election. Hence, the differences between the parties may not have been internalized among voters already in 1994 when the pilot commenced. Indeed, the dynamic DiD model yields no significant result for any party, as displayed in Figure 13 (see also Table XXI in appendix A.7 for the corresponding DiD regression table).

Table 13. The table displays the results from running the static DiD specification of eq. (2) with the share of votes in Municipal Council elections as outcome, using Statistics Sweden data (1973-1994). In column (1), this regards the share of votes for the Social Democratic Party; column (2) regards the Moderate Party, and so on. Note that the Green Party of column (6) was founded in 1981. Also note that Other Parties of column (8) include, but are not limited to, the Sweden Democrats in 1988, 1991, and 1994 and, New Democracy in 1991 and 1994.

Share of Votes by Party (%)								
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Democratic	Moderate Party	Center Party	Liberal Party	Left Party	Green Party	Christian Democrats	Other Parties
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	-0.212	-0.286	1.056	-0.175	-1.362**	-0.686	-0.164	2.059
	(0.779)	(1.178)	(1.318)	(0.988)	(0.627)	(0.459)	(0.230)	(2.227)
Observations	2,260	2,260	2,260	2,260	2,201	1,355	2,225	2,189
Adjusted R-squared	0.936	0.928	0.923	0.848	0.832	0.770	0.911	0.541
Year & municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	-	-	-	-	-	-	-
Mean of dependent variable	42.944	15.870	19.733	4.695	8.773	3.405	3.2542	2.957

Standard errors in parentheses, clustered by municipality

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

Figure 13. The figure plots the estimated regression coefficients, along with their 95 percent confidence intervals, of the dynamic DiD of eq. (3) with the share of votes by party in Municipal Council elections as outcome, using Statistics Sweden data (1973-1994). If the confidence intervals of these coefficients include zero – that is, if they intersect with the solid horizontal line – then the coefficient is not statistically significant from zero. The dashed vertical line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. The coefficient for 1991 is omitted, since 1991 is the reference category. Note that the Green Party was founded in 1981, limiting analysis to the election years 1982-1994. Also note that Other Parties include, but are not limited to, the Sweden Democrats in 1988, 1991, and 1994 and, New Democracy in 1991 and 1994. For the corresponding regression table, see Table XXI in appendix A.7.

6 Discussion

This section summarizes the results and evaluates whether the pilot's aims can be considered fulfilled (6.1), discusses internal (6.2) and external validity (6.3), and proposes avenues for future research (6.4).

6.1 Summary of the Results

Trust in and perception of politicians and parties: One increase in self-assessed trust in politicians proved statistically significant using the static DiD model, for all samples. However, it was not robust to interacted effects. The same measure proved statistically significant using the dynamic DiD model, but only for the full sample. The parallel pre-trends assumption may not hold in these instances though. I discuss this issue in 6.2. What is more, the measure that yielded statistically significant results was the indicator variable for trust which equaled one for any respondent stating that they did not have very low trust in politicians. The results thus indicate that the pilot affected those with very low trust in politicians. A reform reaching mainly the most distrusting voters is arguably the most effective in increasing trust in politicians among the population. Respondents' perception of politicians, in turn, measured as everyone completely disagreeing with a statement implying that politicians in Parliament were cunning, seemed to turn more positive, with statistical significance on the regression run on the pre-election sample using the dynamic DiD. However, it could not be excluded that there had not been a statistically significant effect also in 1982. When instead measuring the perception of politicians as everyone not completely agreeing with the statement, the static DiD model yielded an increase which was statistically significant for the pre-election sample. However, this increase did not prove robust to interacted fixed effects, although the pre-trends seemed parallel from a visual assessment. Again, this result points to the potential efficacy of the pilot in reaching the targeted group. Finally, respondents' perception of parties did not seem to have been changed by the pilot, irrespective of which way the indicator variable was coded.

Voter turnout: Neither the data from SNES nor Statistics Sweden yielded statistically significant results on voter turnout. *Partisanship* seemed to significantly decrease in the post-election sample using the static DiD model, and the average pre-trends seemed parallel visually, but this result did not prove robust to interacted fixed effects. *The share of invalid votes* decreased in pilot municipalities, using both the static and dynamic DiD models, and this result proved robust as well. Hence, while there was no support for the introduction of preference voting increasing the share of invalid votes, there seems to be support for a decrease. *Parties' share of votes*: There was a significant and robust decrease in the share of votes for the Left Party, using the static DiD, and the pre-trends appeared parallel from a purely visual assessment. However, no such effects were found on the share of votes for any other party. *Pre- versus post-election samples*: Finally, I hypothesized in 4.1 that the full sample might confound the true effect of preference voting in a pilot municipality in 1994 with individuals unaware of being able to preference vote before the election took place. I also claimed that robust results for the pre-election sample would contradict the SOU 1996:66 finding of inadequate information. All in all, my results are ambiguous in this regard, so no conclusions can be drawn on this aspect.

Formally, the results indicate that, for the 1994 pilot, H_0 of no effect can be rejected for trust in politicians; the share of invalid votes; and the share of votes for the Left Party. For the other outcomes, H_0 of no effect cannot be rejected. Hence, the pilot can be considered successfully implemented overall. The results indicate that one of the main aims of the pilot, to increase trust in politicians, can be considered fulfilled – especially if decreased partisanship is also interpreted as part of the aim to strengthen the bond between voters and individual politicians. Moreover, contrary to surging invalid votes, the pilot actually seemed to have caused a decrease in the share of invalid votes. As discussed in 5.4, perhaps this was due to fewer protest votes from enhanced satisfaction with the political system with preference voting being introduced, or perhaps the information on the new semi-open list system was either more expansive than regular election information, or more absorbable for certain voters, which might have caused a decrease in

the share of unintentionally erroneous votes in general. However, the goal of increased voter turnout does not seem to have been fulfilled. Also, the political fragmentation preceding the introduction of preference voting seems to have affected voter support for the naysayers of the reform. That is, the pilot seems to have caused a decrease in the share of votes for the Left Party, which may be considered a more vocal opponent than the other opposing parties.

6.2 Internal Validity

Some aspects of these results are potentially troublesome for their reliability. As said, there was data only from 1988, 1991, and 1994 for the one measure of trust in politicians that the pilot seemed to have affected in a statistically significant way. Hence, it is hard to argue that the parallel pre-trends assumption holds in this particular case. While the parallel pre-trends assumption can never be strictly proven, there simply is no pre-*trend* to consider here, but only one pre-treatment data point (1988), as 1991 is the baseline. This limitation weakens the robustness of the result that the 1994 pilot caused an increase in trust in politicians. It is also difficult to determine whether the respondents regarded their local politicians and parties or the politicians and parties in Parliament when surveyed. The SNES questions did not differentiate between national and local politicians. Indeed, asking respondents about their trust in local politicians specifically might have yielded effects of the pilot on trust that were both greater in magnitude and more precisely estimated, given the body of research suggesting amplified effects when moving to the local level (see for example Holmberg 1993 and Westerståhl 1993 on trust; Feddersen 2004 and Leighly 1996 on voter turnout; Johansson et al. 1996 on local name recognition and personal contacts being greater in local elections). Moreover, expressing a positive perception of politicians may as said not necessarily capture high trust, and a positive perception of parties may not necessarily capture neither high trust nor a positive perception of the political system. Trust being notoriously hard to measure aside, the support for introducing preference voting increasing trust in politicians cannot be considered robust.

It is also harder to argue for parallel pre-trends also when there are changes in the respondent characteristics over time. Not only the results on trust in politicians and parties may be illegitimized by such imbalances, but also the results on partisanship – despite having controlled for education. As laid out in 4.4, there is a risk that the results are driven by an increase in average educational attainment in pilot municipalities in 1994 (see Figure VI in appendix A.3), if more educated people respond differently to the survey questions of interest. This is not unlikely given previous research on, for example, the effects of preference voting on trust in politicians occurring exclusively among highly educated (see 3.1). As for partisanship, lower-educated voters have been suggested by previous research to use partisanship as a decision-making shortcut for voting, meaning an increase in education may make them less partisan (although there are findings that also higher educated voters may use partisanship as a cue (see 3.3)). Nonetheless, it is not unlikely that increased educational attainment in pilot municipalities in 1994 drove the decreased partisanship in pilot municipalities in 1994, instead of the introduction of preference voting.

The effects being limited to specific subgroups of the sample is also an issue for the results overall, since about a third of each sample for the other SNES outcomes were nonresponses (see Table III in A.4). Hence, the responses might not make up a random sample, but may be skewed from the probability of answering likely correlating with what one will answer. This so-called survivorship bias means that those who choose to answer are likely also those who already care strongly about the issue. Thus, the effects discerned might not be average effects at all, but rather effects on a, for instance, particularly politically sophisticated group of respondents.

Finally, the aggregate-level data from Statistics Sweden does not allow controlling for the socioeconomic factors identified by previous research (see 4.4) as predictors of political participation and preferences. One may thus fall for the ecological fallacy if assuming that these aggregate-level results apply to individuals within all municipalities examined. Consequently, the seemingly statistically significant and robust effects of the pilot on the share of invalid votes and the share of votes for the Left Party, are not so

robust after all. I cannot rule out that these results are in fact driven by, for example, young female students being more prone to both vote for the Left Party and live in one of the pilot municipalities in 1994.

Finding no robust effects of introducing preference voting in the pilot municipalities on any of the outcome variables is largely in line with the governmental evaluations of the reform. For example, SOU 1999:136 concluded on the 1998 election, more clearly than did SOU 1996:66 on the 1994 pilot, that introducing preference voting did not affect overall voter turnout. Interestingly, Möller and Holmberg (1999) concluded that any unfulfilled goals of the reform might be due to the reform potentially being "too modest" (p. 8, my translation) in the sense that the proposal was not to go full-on open-list, but rather to assign more power to the people relative to before, while the parties would remain in control of their candidates. This approach has been criticized by political scientists for being contradictory (for instance, Holmberg & Möller, 1999 and Petersson et al., 1999)[19]. Also, previous research points to introducing preference voting requiring rather long periods to reveal any effects (see Berg et al. 2015 for a summary; Holmberg & Möller 1999 also emphasize this after the 1998 full-scale implementation).

6.3 External Validity

The 2024 democracy Super Bowl does underscore the importance of electoral research. However, it is difficult to assert that the results of the 1994 pilot are generalizable to *other countries*, since the electoral systems in which preference voting would be introduced differ. However, it may be noted that the Swedish pilot design was based on Denmark's electoral system (therefore named "the modified Danish model" by the SOU 1993:21 committee), meaning it is not impossible that the positive effects of introducing preference voting on, for example, trust in politicians, may occur in contexts similar to Sweden using the same pilot design. The results of this study are likely not generalizable to Swedish elections to *other councils* (regional, national, EU) or *other periods*, mainly because of the many regulatory changes of the semi-open list system that were implemented after the 1994 pilot. Johansson et al. (1996) predicted after the 1994 pilot that "the [general] campaign activity would have to increase" (p. 223, my translation) to facilitate future preference voting in non-local elections. Ever since the semi-open list system was put into effect, preferential voter turnout has generally been the highest in elections to the Municipal Councils in Sweden, but with major differences between municipalities, depending on which characteristics the municipalities are sorted for (Järnbert et al., 2023; SOU 2007:68). For instance, in the latest election of 2022, 26 percent of voters placed a preference vote in the Municipal Council elections, while 20 percent did so in the election to Parliament (Järnbert et al., 2023). Again, several research findings have suggested amplified effects at the local level. While this thesis does not claim to say anything about preference voting in Sweden today, precisely 30 years after the pilot, it may be noted that the share of preference votes in Swedish elections has steadily declined since 1998, when preference voting was introduced to all Municipality Council elections for the first time (the only trend break being the 2010 election). It would thus be interesting to investigate further the long-term effects of the 1994 pilot, which I elaborate on below.

6.4 Further Research

In the government experiment of introducing preference voting, all municipalities were treated with preference voting immediately in the 1995 EP election and then in the 1998 general election. Hence, this thesis can say nothing about the long-term effects of the pilot. Potentially, however, there has been a learning effect for pilot municipalities to have tried preference voting in 1994 on their utilization of preference voting in the years following the pilot. In fact, SOU 1999:136 evaluated the full-scale implementation of 1998 and noted that the share of voters placing a preference vote had increased in pilot municipalities since 1994.

[19] Semi-open list systems have been subject to criticism in general in previous literature, being regarded by some "as little more than 'closed lists in disguise'" (Folke et al, 2016, p. 576). Karvonen (2004), on the other hand, concludes that moderation is key. He claims that the ideal system is a semi-open list system with an element of preference voting – which is precisely how SOU 1993:21 framed their proposal. Having the party as an intermediate has also been highlighted as an anti-corruption force in systems of preference voting (Chang et al., 2010; Chang & Golden, 1995).

They do not, however, identify whether these increases are causal learning effects. A future research question would thus be: *Were there any learning effects from the 1994 pilot?* For example, *Have the effects of introducing preference voting on trust in politicians persisted, or even been enhanced, over time?* Previous research namely points to the introduction of preference voting requiring rather long periods to reveal any effects (see Berg et al. 2015 for a summary).

7 Conclusion

Sweden is regarded to have had a semi-open list system since 1998, but in 1994, a governmental pilot commenced, in which preference voting was introduced in seven municipalities. This natural experiment allows for a difference-in-differences framework. My thesis aimed to answer, *Did the 1994 pilot causally affect the intended outcomes, including trust in politicians and voter turnout, or related outcomes of interest, including political partisanship, the share of invalid votes, and voter support for any political party?* To this aim, I analyzed individual-level survey data of Swedish voters from the Swedish National Election Studies Program (SNES, 1982-1994) and aggregate-level data of Swedish elections from Statistics Sweden (1973-1994) using both static and dynamic difference-in-differences models. In sum, one of the main aims of the pilot to strengthen the bond between voters and individual politicians can be considered fulfilled, since the pilot seemed to have increased trust in politicians and decreased partisanship. Surprisingly, the share of invalid votes decreased as a consequence of introducing preference voting, despite the enhanced complexity of the voting process. Perhaps this was due to fewer protest votes from enhanced satisfaction with the political system with preference voting being introduced, or perhaps the information on the new semi-open list system was either more expansive than regular election information, or more absorbable for certain voters, which might have caused a decrease in the share of unintentionally erroneous votes in general. Overall, the pilot can thus be considered successfully implemented, although the goal of increased voter turnout does not seem to have been achieved. Moreover, opposing the implementation may not have come without political sacrifice, as the share of votes for the Left Party, a vocal opponent of introducing preference voting, was on average lower in pilot municipalities in 1994 than in control municipalities.

Not all of these results proved robust to interacted fixed effects, though, and the parallel pre-trends assumption is difficult to assert in some cases. Furthermore, the results on trust and partisanship may have been driven by an increase in average educational attainment in pilot municipalities in 1994. More importantly, the results on invalid votes and voter support were generated by the aggregate-level data from Statistics Sweden which, unlike the SNES data, does not allow for controlling for respondent characteristics. Hence, the results of this thesis are only indicative and should be interpreted with caution. It is also worth noting that these results, despite using a dynamic difference-in-differences model, do not reflect long-term effects of the pilot, since all municipalities were treated immediately after the pilot in the 1995 European Parliament election and in the 1998 general election.

Despite these limitations, this thesis constitutes the most extensive evaluation of the 1994 pilot and its intended outcomes, to the best of my knowledge. Johansson et al. (1996) evaluated the pilot, but did not investigate causality. Neither did they fully examine the fulfillment of the main aim of the reform, to strengthen the bond between voters and politicians. Hence, the main contribution of this thesis is its supplementation of the parsimonious evaluation of a large-scale government experiment. In broader terms, it thus adds to the often non-causal governmental evaluations of public policy. Furthermore, this thesis contributes to a core research question of political economics, namely the impact of institutions on political behavior. The results on trust in politicians are particularly interesting in this regard. Trust in politicians was, according to Möller (1998), understudied in the 1990s Sweden, especially its causal determinants. More importantly, Algan et al. (2017) discuss what appears to be a general decline in trust in politicians and political institutions in today's Europe, and how this distrust reinforces political extremism. Policymakers refer to a European "trust crisis" (Murtin et al., 2018, p.7). Hence, my results of a 1994 electoral reform shaping trust in politicians are interesting also for policymaking in contemporary Europe. Lastly, it fills a research gap on the causal relationship between preference voting and partisanship in Sweden.

In conclusion, the rationale for introducing preference voting in Swedish municipalities in the 1990s was to assign more power to the people to affect the election outcome. The seemingly successful outcomes of the 1994 pilot indicate that allocating more power to the people can, indeed, strengthen the bond between voters and politicians.

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Appendix

A.1 Map of Pilot Municipalities

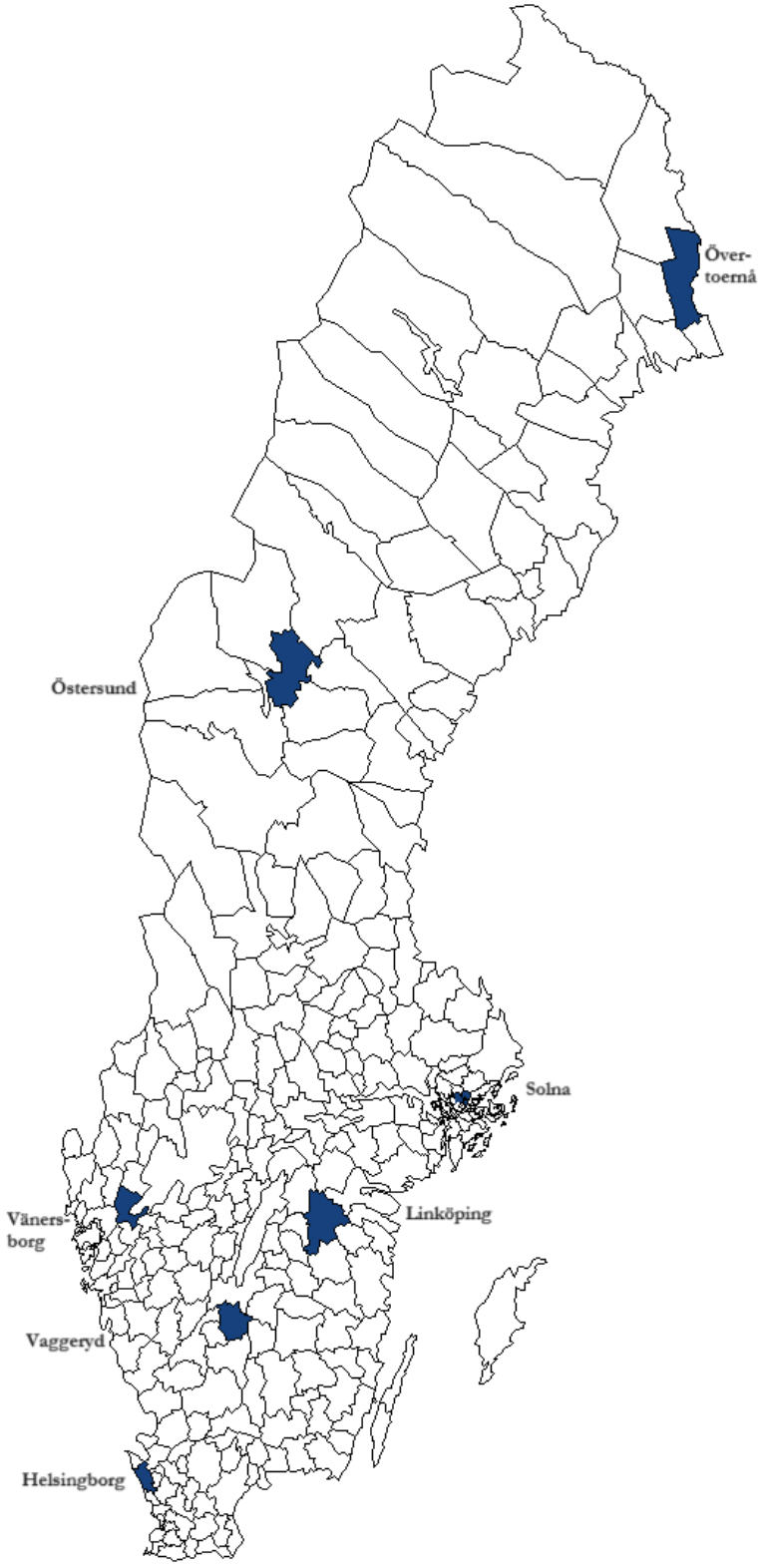
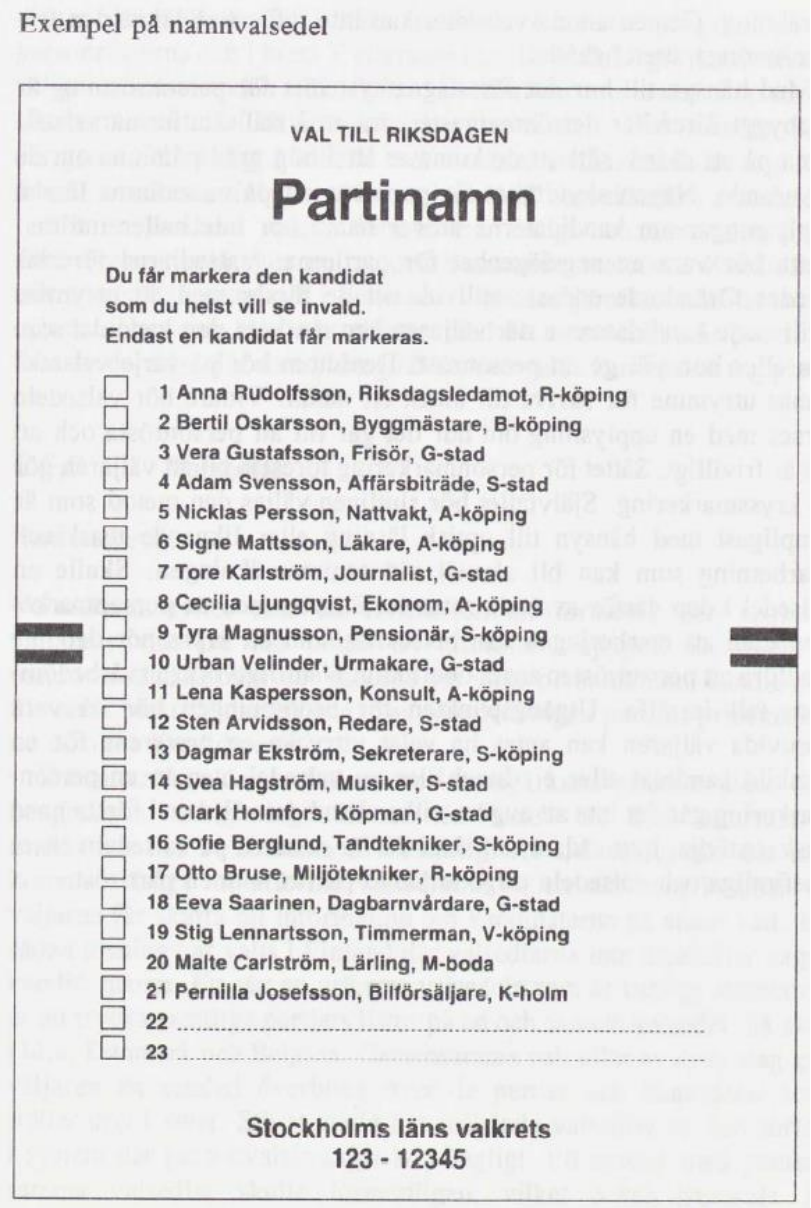


Figure I. The figure displays the 290 Swedish municipalities. The seven municipalities treated with the new semi-open list system in the 1994 election are marked up on the map – from north to south: Övertorneå, Östersund, Solna, Vänersborg, Linköping, Vaggeryd, Helsingborg. The municipality map is from Statistics Sweden (n.d.).

A.2 New Ballot Designs



Exempel på namnvalsedel

VAL TILL RIKSDAGEN

Partinamn

Du får markera den kandidat
som du helst vill se invald.
Endast en kandidat får markeras.

1 Anna Rudolfsson, Riksdagsledamot, R-köping
2 Bertil Oskarsson, Byggmästare, B-köping
3 Vera Gustafsson, Frisör, G-stad
4 Adam Svensson, Affärsbiträde, S-stad
5 Nicklas Persson, Nattvakt, A-köping
6 Signe Mattsson, Läkare, A-köping
7 Tore Karlström, Journalist, G-stad
8 Cecilia Ljungqvist, Ekonom, A-köping
9 Tyra Magnusson, Pensionär, S-köping
10 Urban Velinder, Urmakare, G-stad
11 Lena Kaspersson, Konsult, A-köping
12 Sten Arvidsson, Redare, S-stad
13 Dagmar Ekström, Sekreterare, S-köping
14 Svea Hagström, Musiker, S-stad
15 Clark Holmfors, Köpman, G-stad
16 Sofie Berglund, Tandtekniker, S-köping
17 Otto Bruse, Miljötekniker, R-köping
18 Eeva Saarinen, Dagbarnvårdare, G-stad
19 Stig Lennartsson, Timmerman, V-köping
20 Malte Carlström, Lärling, M-boda
21 Pernilla Josefsson, Bilförsäljare, K-holm
22 .....
23 .....

Stockholms läns valkrets
123 - 12345

Figure II. The SOU 1993:21 fictional example of the new candidate list ballot, which was ultimately used in the 1994 pilot. Rough translation of the instructions: "You may mark the candidate you prefer to be elected. You can select only one candidate".

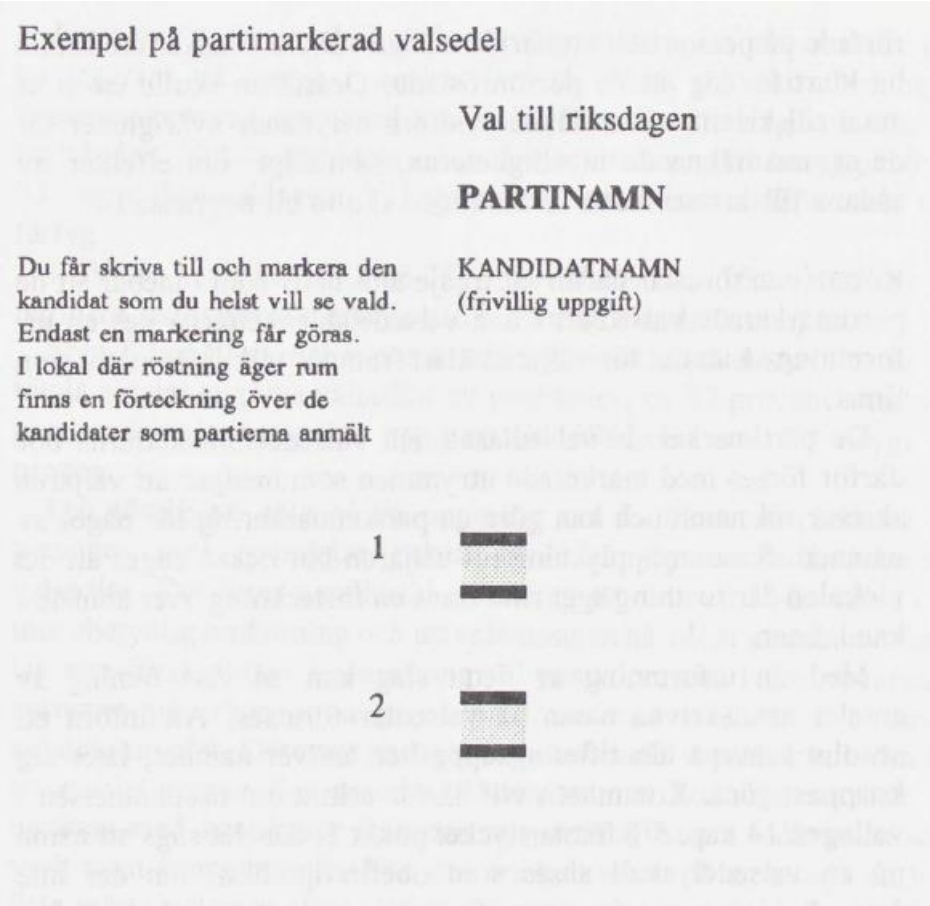


Figure III. The SOU 1993:21 fictional example of the new party ballot, which was ultimately used in the 1994 pilot. Rough translation of the instructions: "You may add and mark the candidate you prefer to be elected. You can select only one candidate. In the polling station, there is a list of candidates nominated by the parties.".

A.3 Background Characteristics

This section presents some relevant background characteristics of the data analyzed. First, as seen in Table I below, the average population size of a pilot municipality in 1994 was about 59,000. This is continuously referred to in the main text to contextualize the magnitude of the statistical results.

Table I. The table displays the population size of each pilot municipality in 1994, and the 1994 average population size for all pilot municipalities. Data from Statistics Sweden.

Pilot municipality	**Population size in 1994**
Helsingborg	113,411
Linköping	130,489
Solna	54,159
Vaggeryd	12,382
Vänersborg	36,691
Östersund	59,730
Övertorneå	6,159
Average	**59,003**

Overall, there is an even distribution of the background characteristics of the respondents in my sample. In Table II below I present three descriptive examples. In the sample as a whole, the female-to-male ratio is

50.2 percent. Similarly, the age groups are fairly evenly distributed. The lowest share (5.8 percent) of 18–21-year-olds is most likely due to the narrower age span of that group compared to the other age groups. Groups of occupation in the entire sample as a whole are, however, not as evenly distributed as the age groups, but are according to the SNES descriptives representative of the Swedish population as a whole.

Table II. The table displays the number of respondents of each gender, age group, and occupational group, and their corresponding share of the sample. The increase in missing values in occupation comes from occupation being asked directly from respondents, while age and gender were collected from the Swedish population registration.

Background characteristic	**No. of respondents**	**Share (%)**
Gender	17,894	100
Female	8,912	49.8
Male	8,981	50.2
Missing values	*1*	-
Age	17,894	100
18-21	1,044	5.8
21-30	3,234	18.1
31-40	3,420	18.1
41-50	3,148	17.6
51-60	2,646	14.8
61-70	2,520	14.1
71-80	1,881	10.5
Missing values	*1*	-
Occupation	17,894	100
Industrial worker	2,190	12.2
Other type of worker	3,081	17.2
Entry-level employee	1,281	7.2
Middle manager	2,631	14.7
Executive	1,810	10.1
Entrepreneur	962	5.4
Farmer	493	2.8
Student	720	4.0
Missing values	*4,691*	*26.2*

Now, for a more thorough evaluation of the distribution of the background characteristics of the respondents in my sample, I present below the distribution of respondents across years and treatment status for pre-election and post-election respondents, respectively (Figure IV), as well as for respondents' age, education, gender, home ownership, marital status, occupation, and urbanization (Figures V-XI), in other words the variables included in the vector of controls, $\boldsymbol{X}_{imt}$.

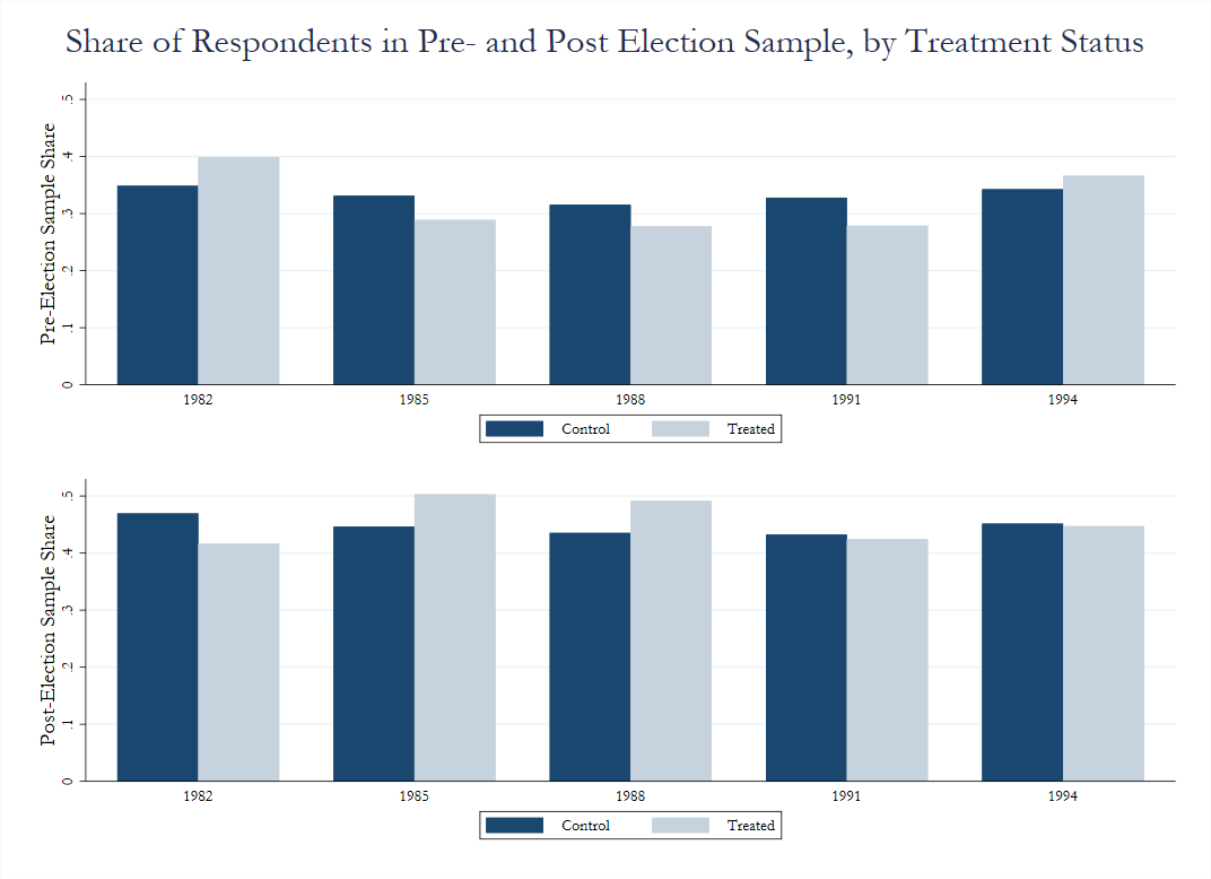


Figure IV. The figure displays the distribution of pre- and post-election respondents across years and treatment status.

Figure V. The figure displays the distribution of age across years and treatment status.

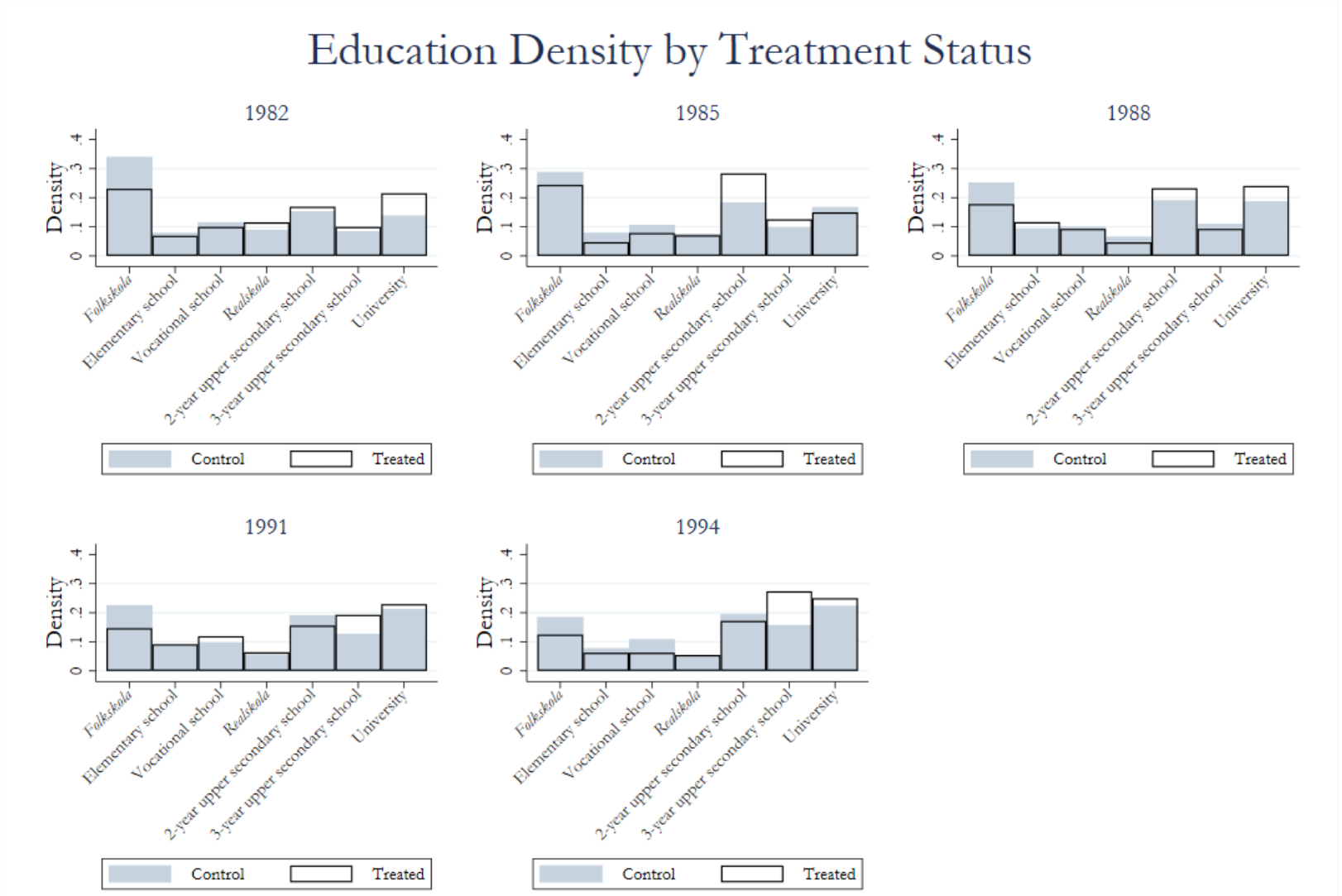


Figure VI. The figure displays the distribution of education across years and treatment status.

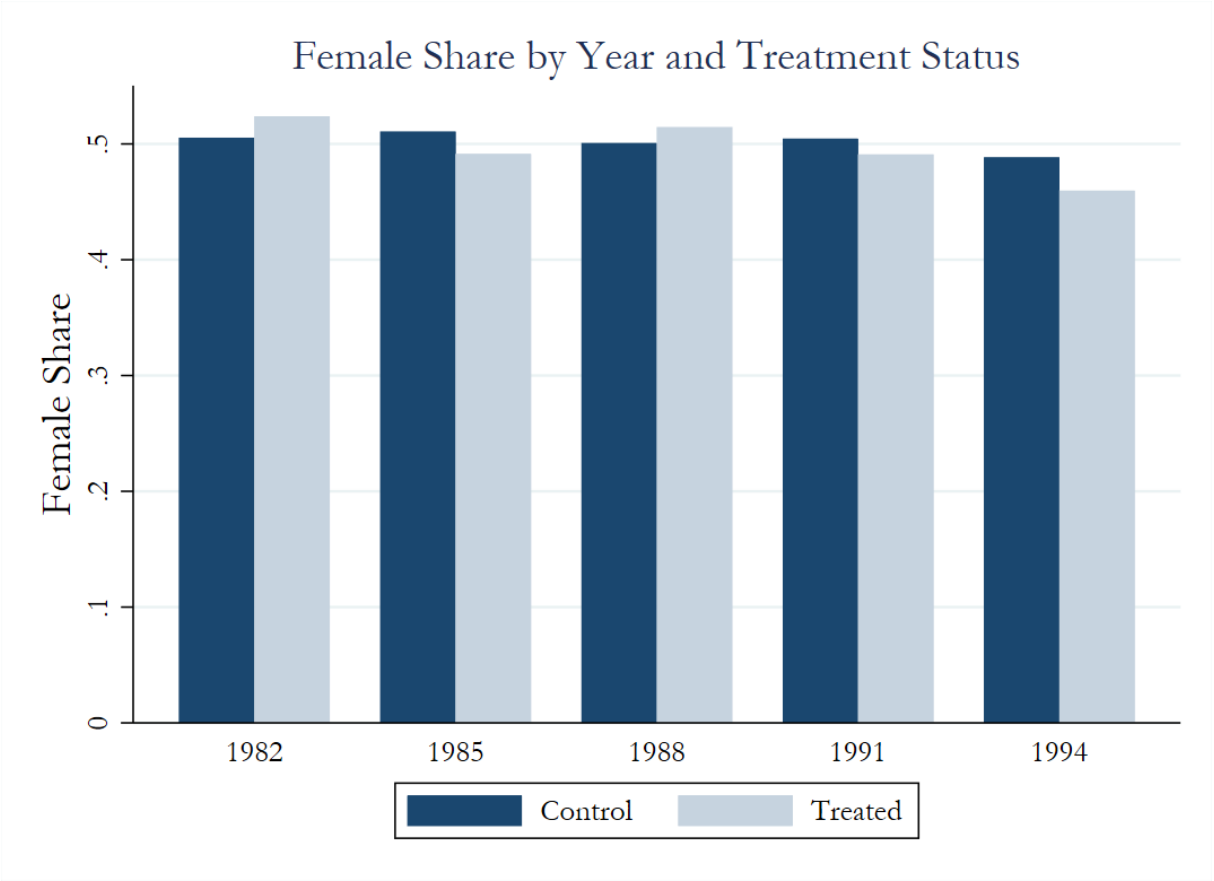


Figure VII. The figure displays the distribution of gender across years and treatment status.

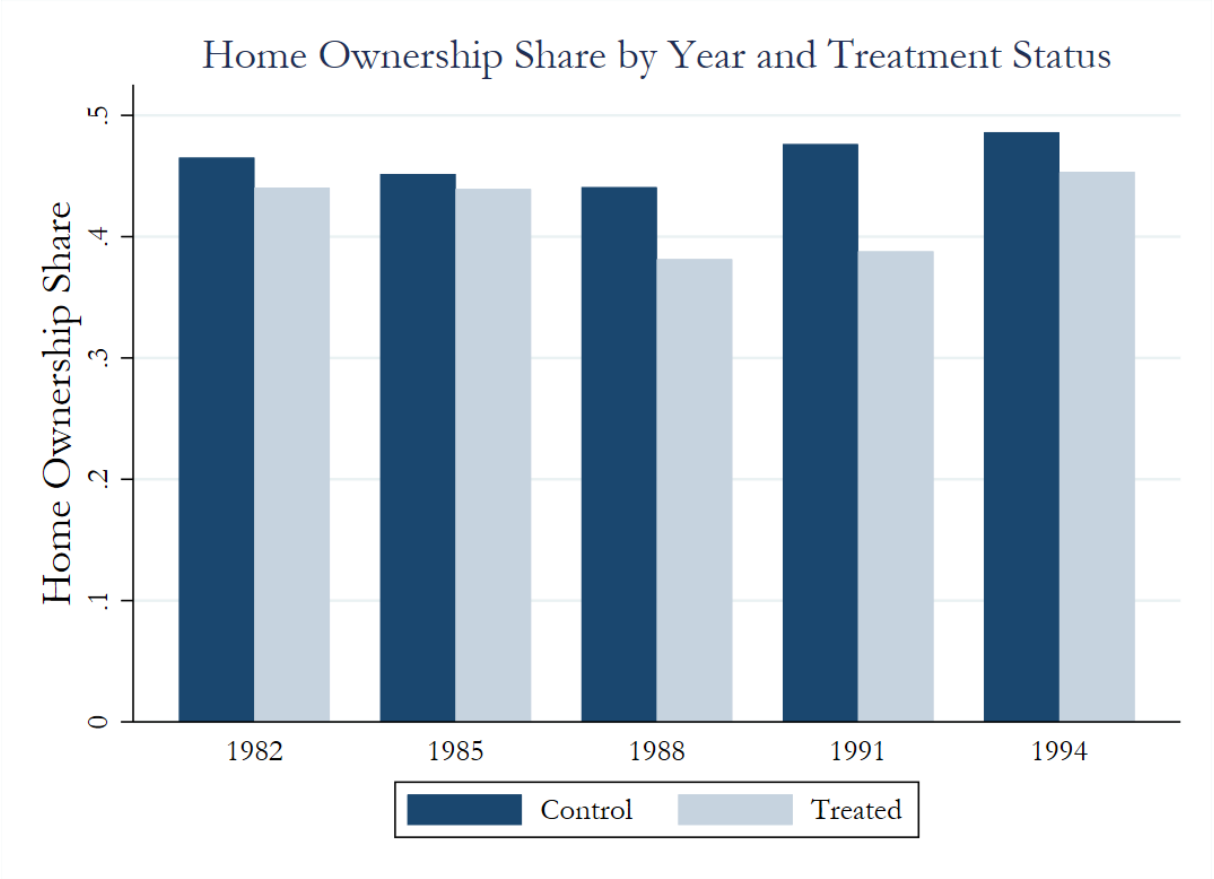


Figure VIII. The figure displays the distribution of home ownership across years and treatment status.

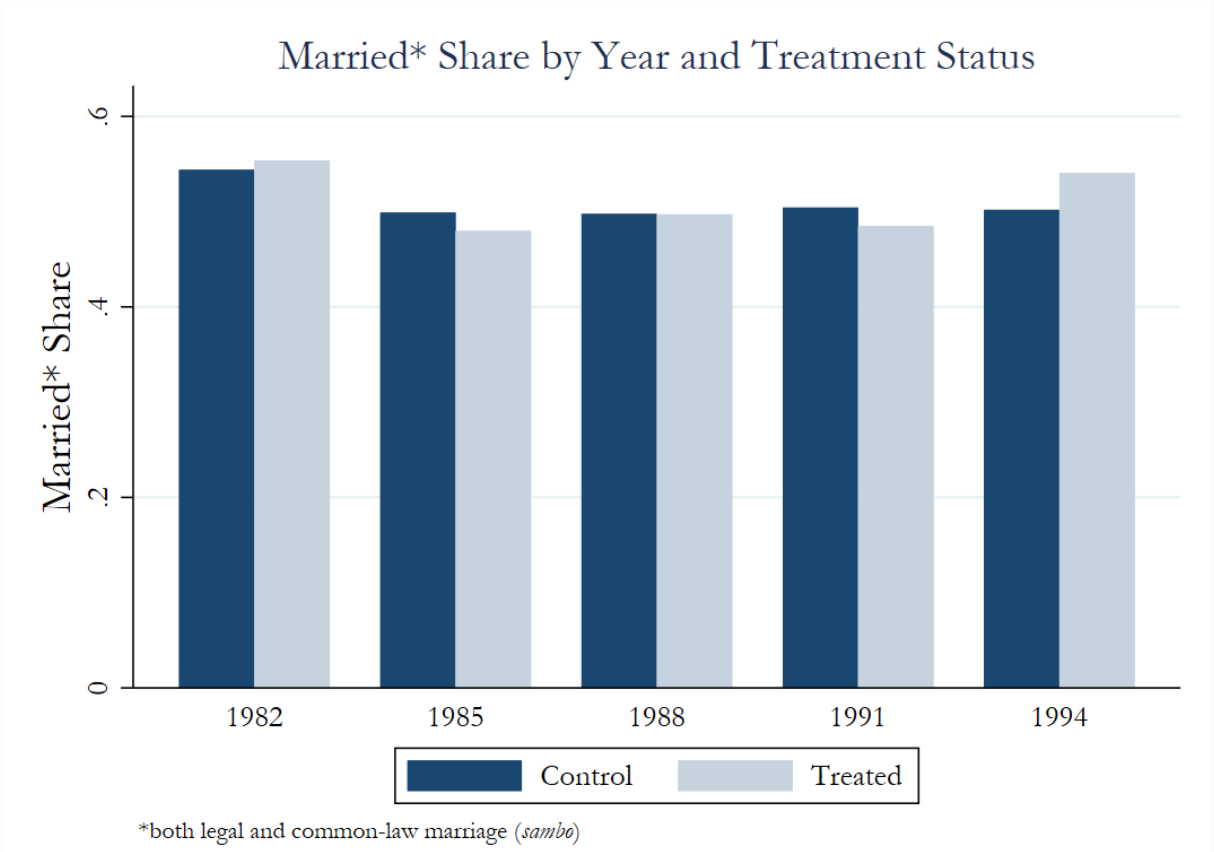


Figure IX. The figure displays the distribution of marital status across years and treatment status. It may be noted that in SNES, marital status is a categorial variable with categories (1) legally married or and common-law married (*sambo*) (2) divorced or unmarried (3) widow or widower. Here, however, marital status is an indicator variable which equals zero regardless of the individual being divorced, unmarried or widowed. Indeed, widowed respondents may have gained informational economies of scale during previous elections, but only their marital status in the current election is interesting.

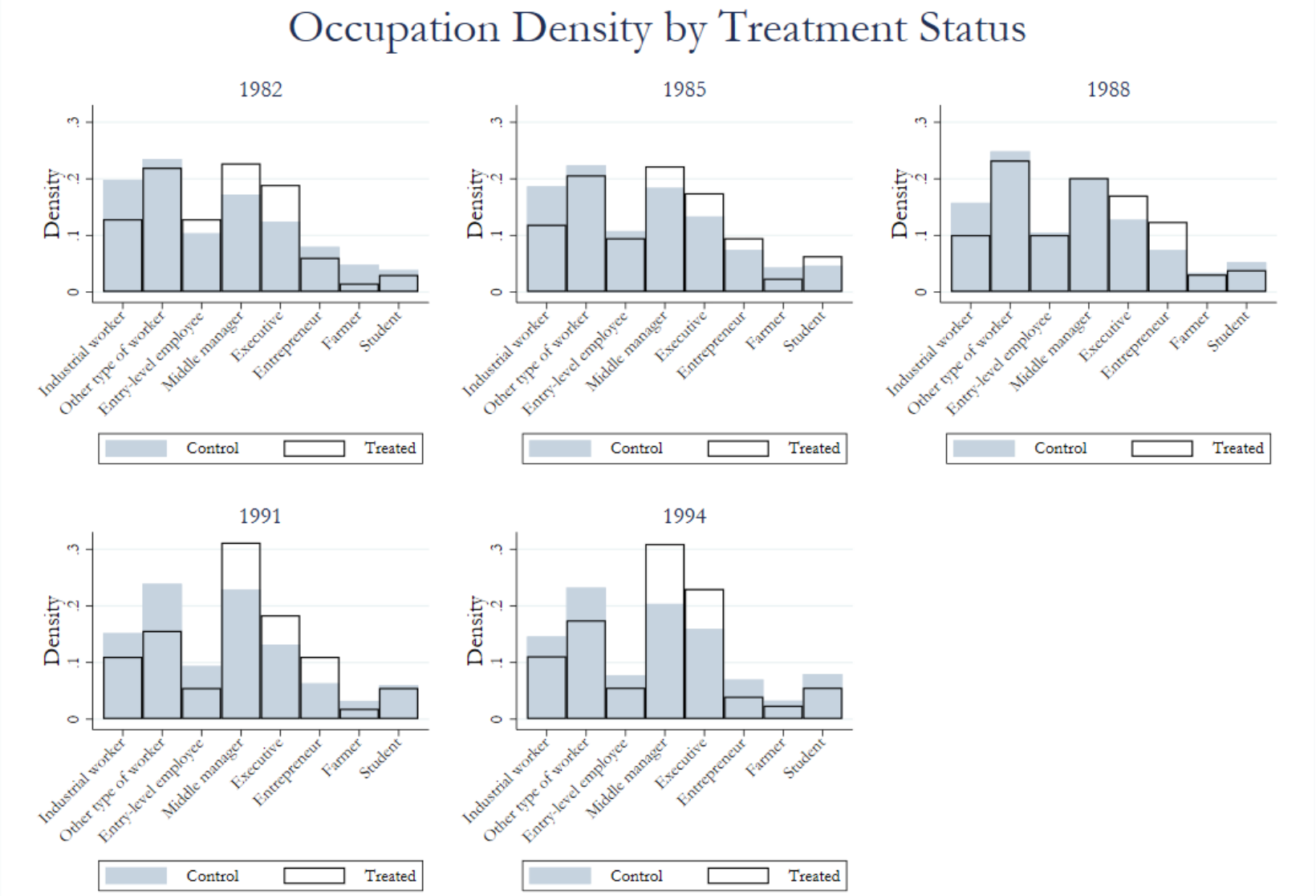


Figure X. The figure displays the distribution of occupation across years and treatment status.

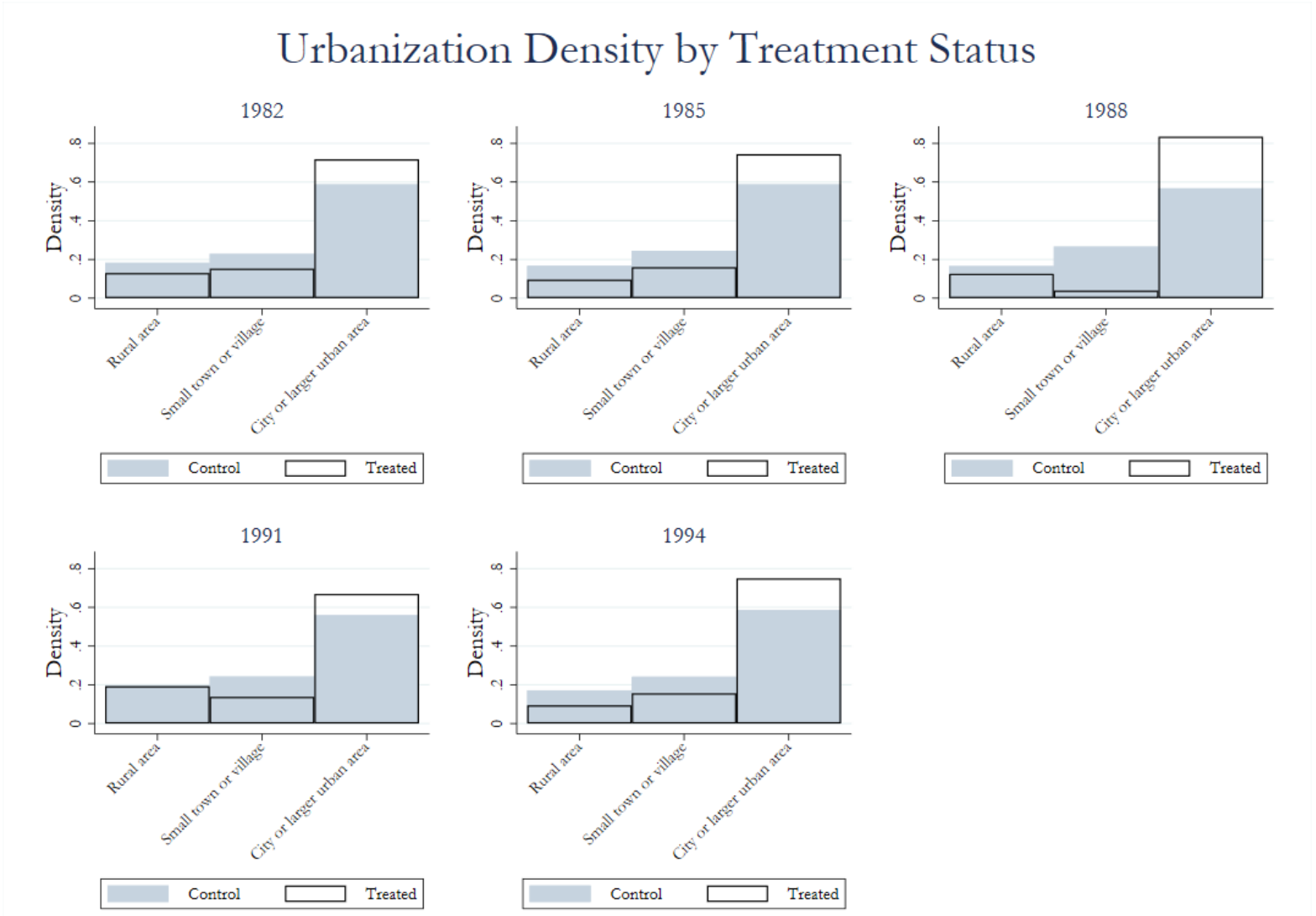


Figure XI. The figure displays the distribution of urbanization across years and treatment status.

A.4 Distribution of SNES Variables

Table III. The distribution of respondents' self-assessed trust in politicians (1988-1994); perception of politicians (1982-1994); perception of parties (1982-1994); and self-assessed partisanship (1982-1994).

SNES variable	**No. of respondents**	**Share of total sample**
Trust in politicians	10,593	100 %
Very high trust	159	2 %
Pretty high trust	2,830	27 %
Pretty low trust	3,633	34 %
Very low trust	887	8 %
Missing values	*3,084*	*29 %*
Stance on statement "Politicians disregard ordinary people"	17,894	100 %
Completely disagree	647	4 %
Mostly disagree	2,825	16 %
Mostly agree	5,632	31 %
Completely agree	2,821	16 %
Missing values	*5,969*	*33 %*
Stance on statement "Parties only care about votes"	17,894	100 %
Completely disagree	741	4 %
Mostly disagree	3,010	17 %
Mostly agree	5,497	31 %
Completely agree	2,752	15 %
Missing values	*5,894*	*33 %*
Partisanship	17,894	100 %
Yes, do consider self partisan	6,898	6,898
No, do not consider self partisan	5,798	5,798
Missing values	*5,198*	*5,198*

A.5 Plotted Time Trends

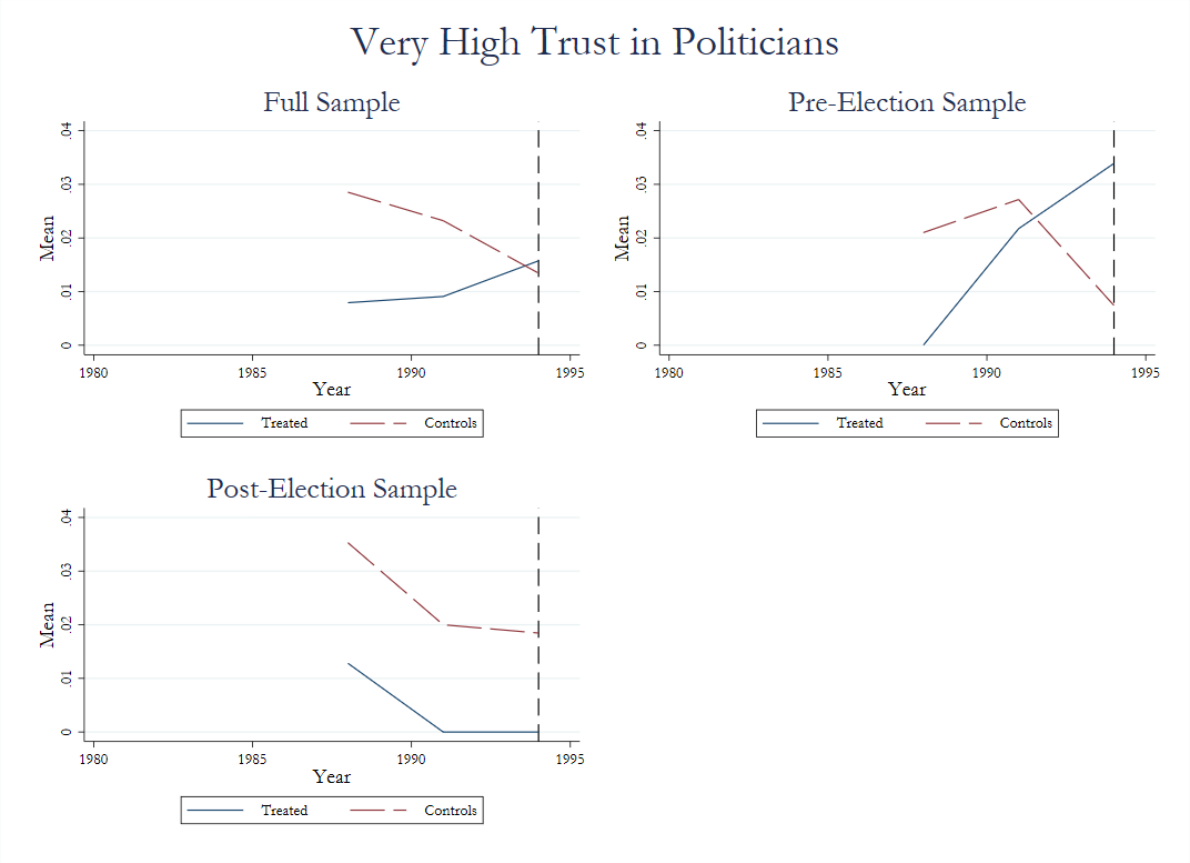


Figure XII. The figures plot the mean over time for an indicator variable for having very high trust in politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they have very high trust in politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that this question was introduced into the SNES in 1988.

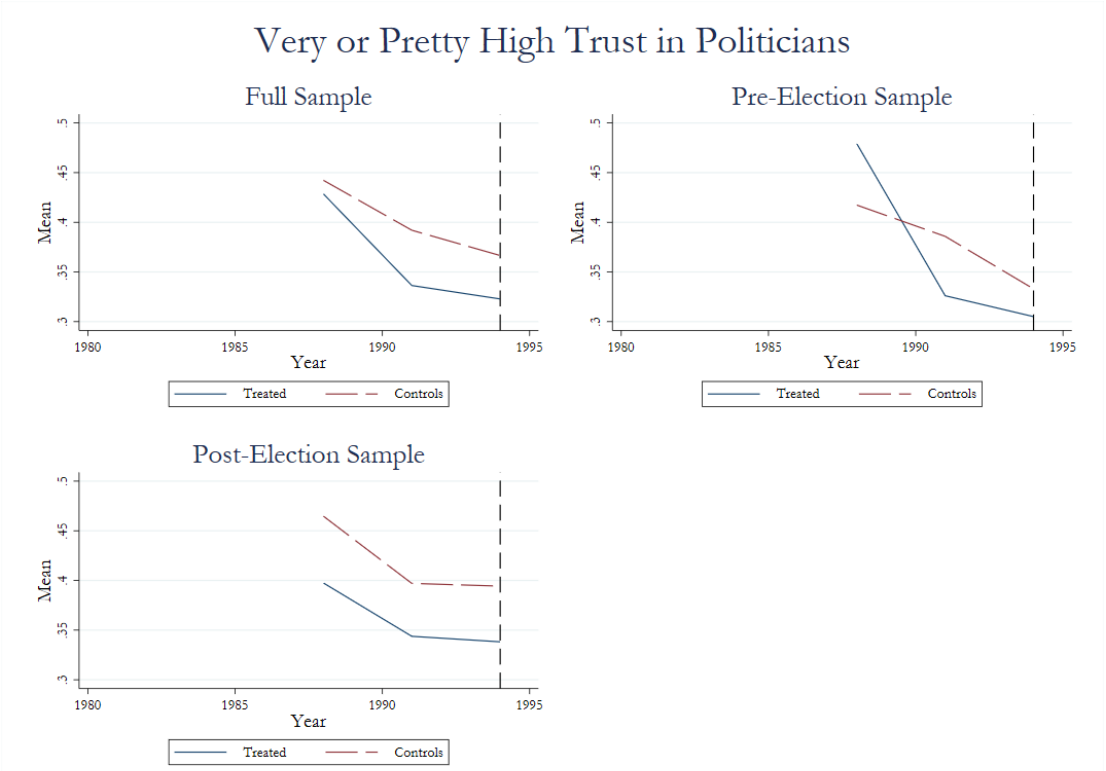


Figure XIII. The figures plot the mean over time for an indicator variable for having very or pretty high trust in politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they have very or pretty high trust in politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that this question was introduced into the SNES in 1988.

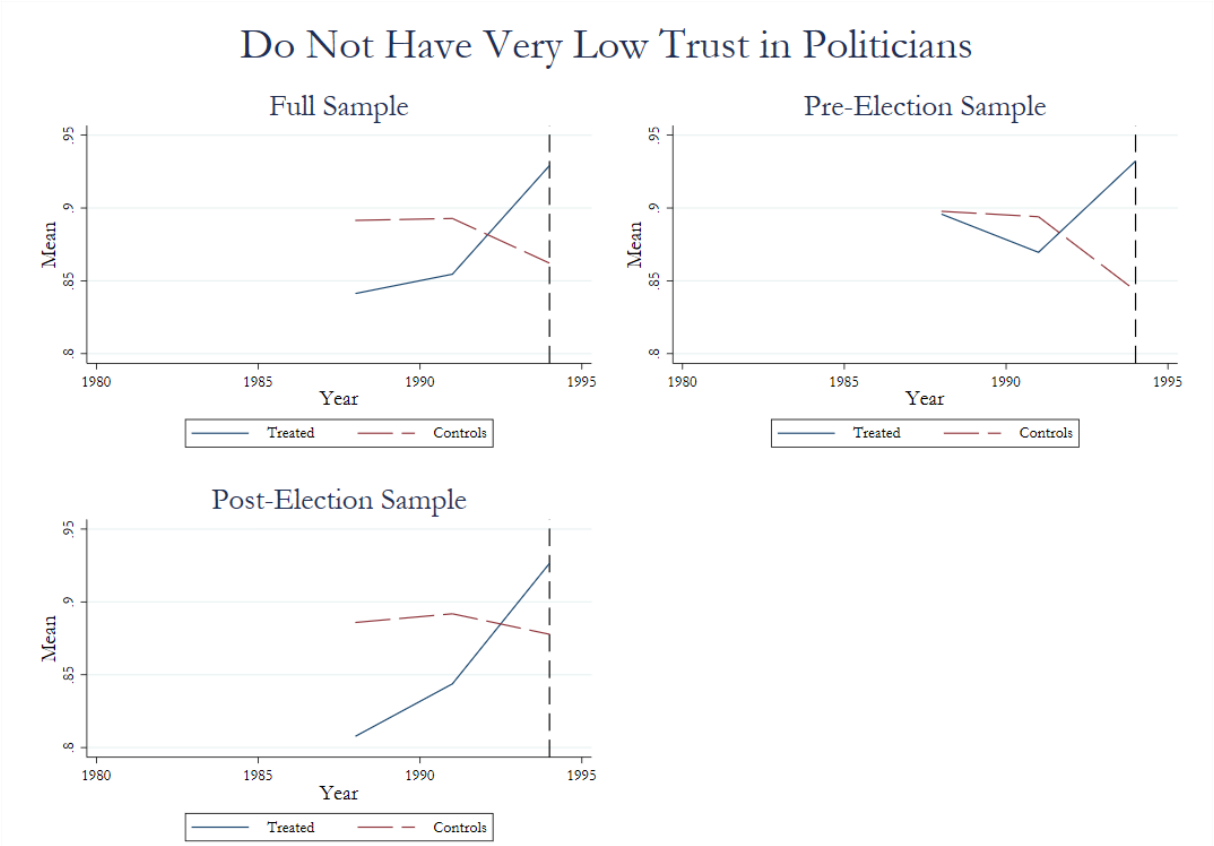


Figure XIV. The figures plot the mean over time for an indicator variable for having very high trust in politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they have very high, pretty high, or pretty low trust in politicians, meaning it includes everyone not saying that they did not have very low trust in politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that this question was introduced into the SNES in 1988.

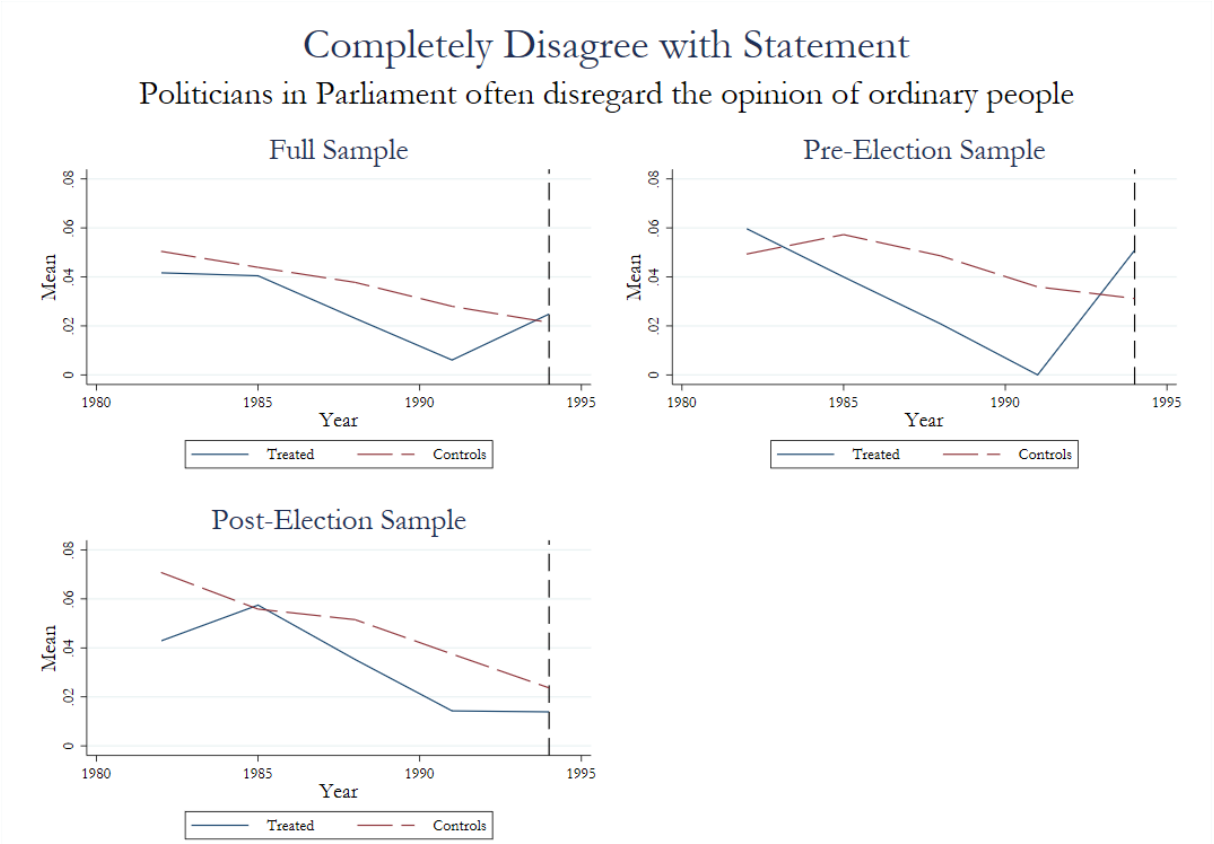


Figure XV. The figures plot the mean over time for an indicator variable for having a positive perception of politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should a positive perception of politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

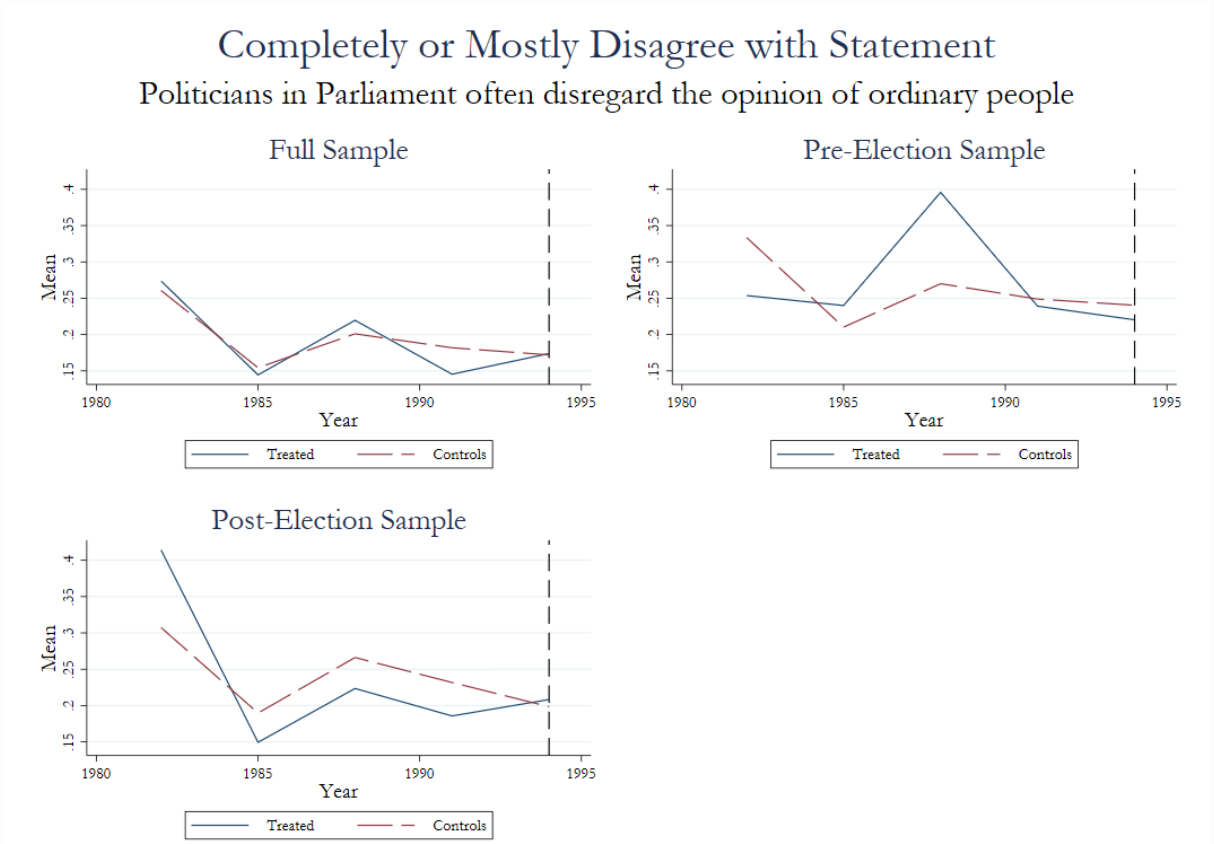


Figure XVI. The figures plot the mean over time for an indicator variable for having a positive perception of politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely or mostly disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should a positive perception of politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

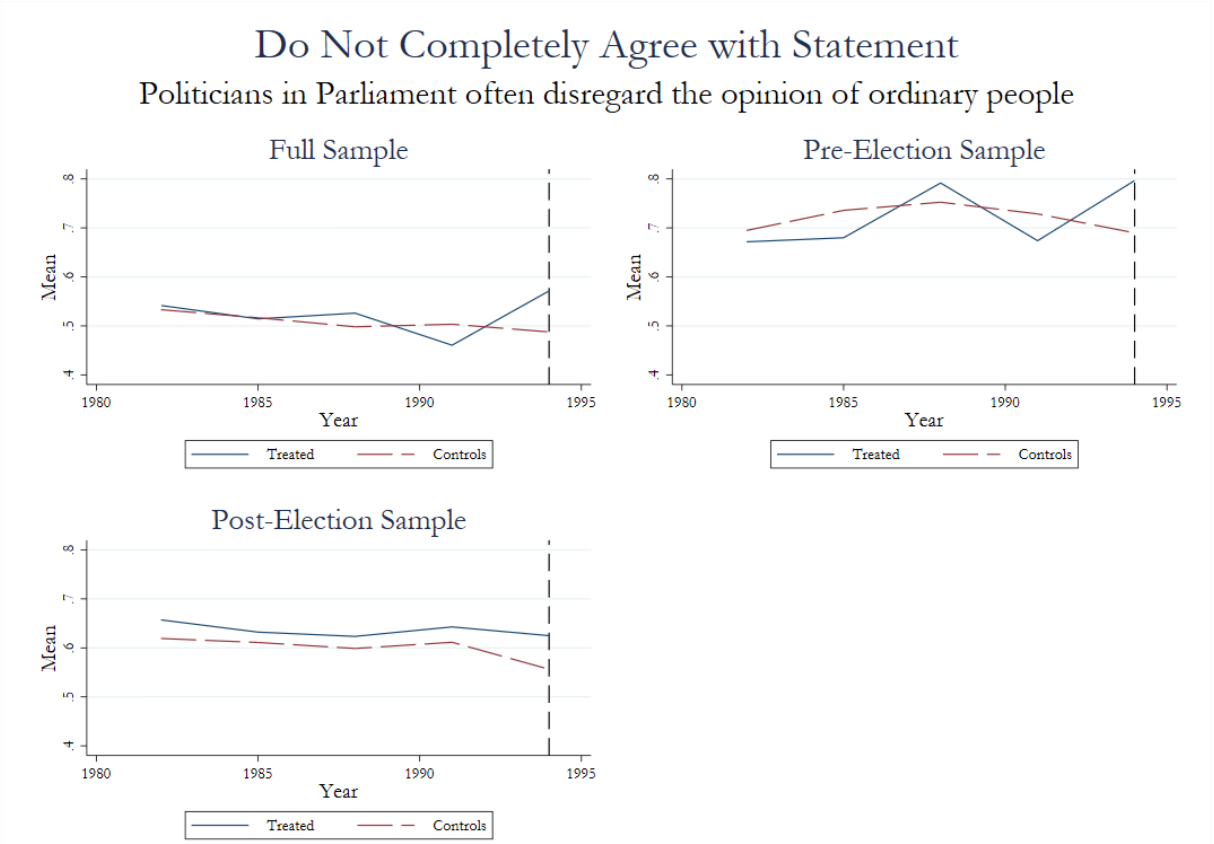


Figure XVII. The figures plot the mean over time for an indicator variable for having a positive perception of politicians, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Politicians in Parliament often disregard the opinion of ordinary people". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of politicians. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

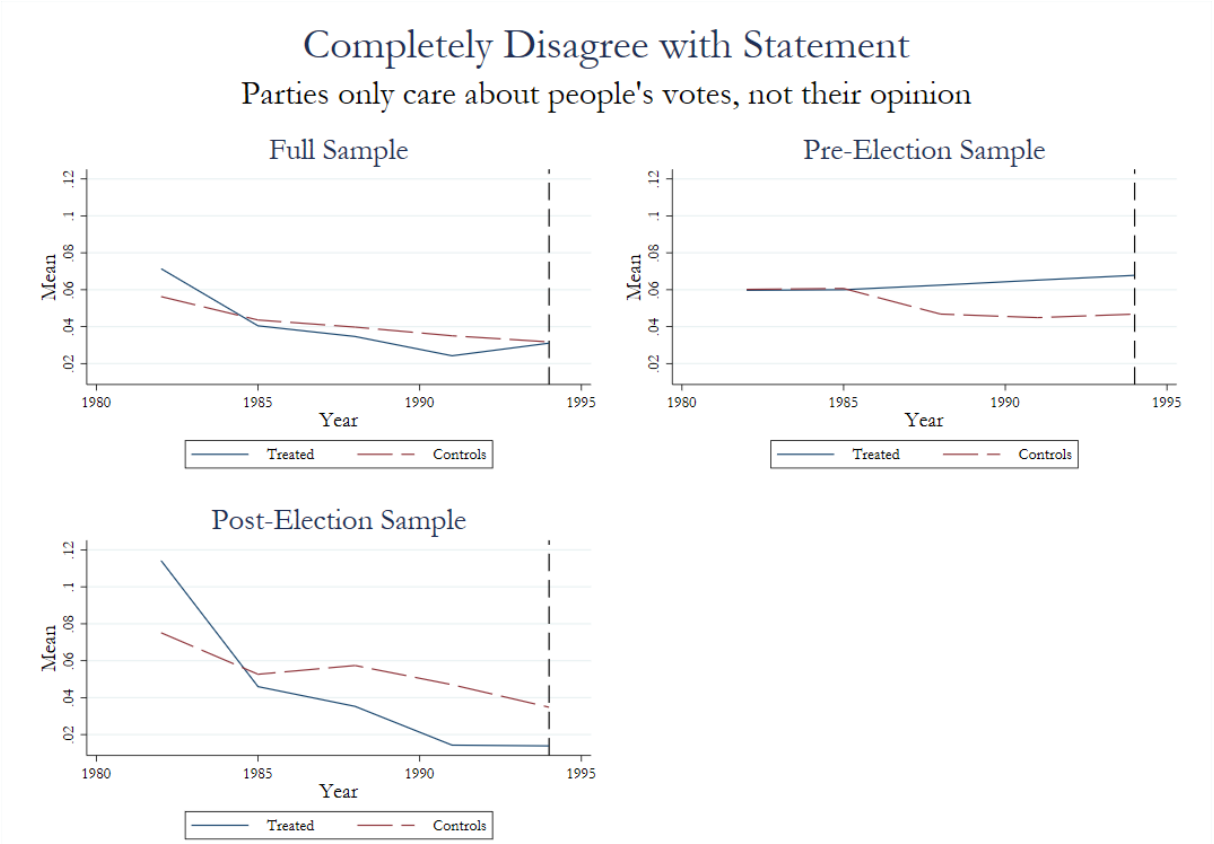


Figure XVIII. The figures plot the mean over time for an indicator variable for having a positive perception of parties, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely disagree with the statement "Parties only care about people's vote, not their opinion", meaning they should a positive perception of parties. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

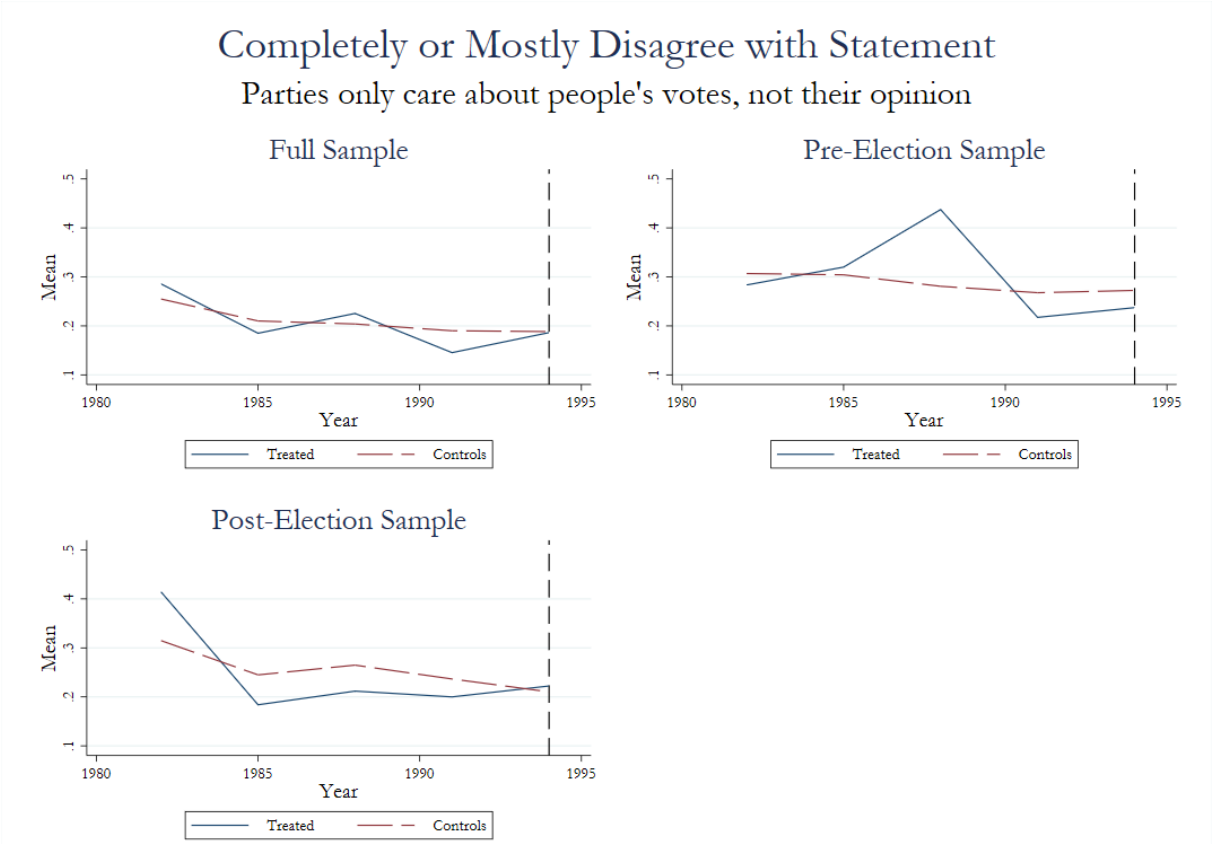


Figure XIX. The figures plot the mean over time for an indicator variable for having a positive perception of parties, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely or mostly disagree with the statement "Parties only care about people's vote, not their opinion", meaning they should a positive perception of parties. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

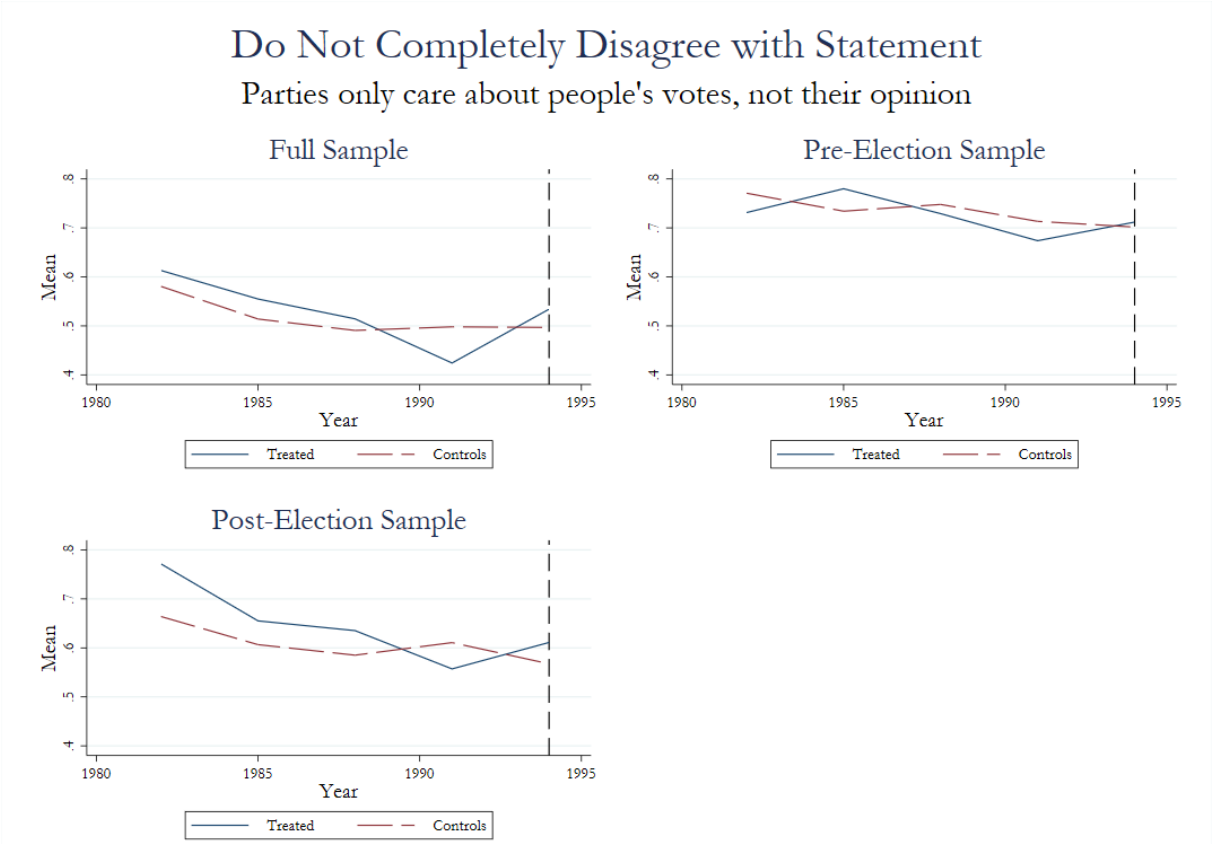


Figure XX. The figures plot the mean over time for an indicator variable for having a positive perception of parties, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The indicator variable is coded to equal one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Parties only care about people's vote, not their opinion". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of parties.The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

Figure XXI. The figures plot the mean over time for voter turnout to Municipal Council elections, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) using the Statistics Sweden data (1973-1994) and the SNES data (1982-1994), respectively. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

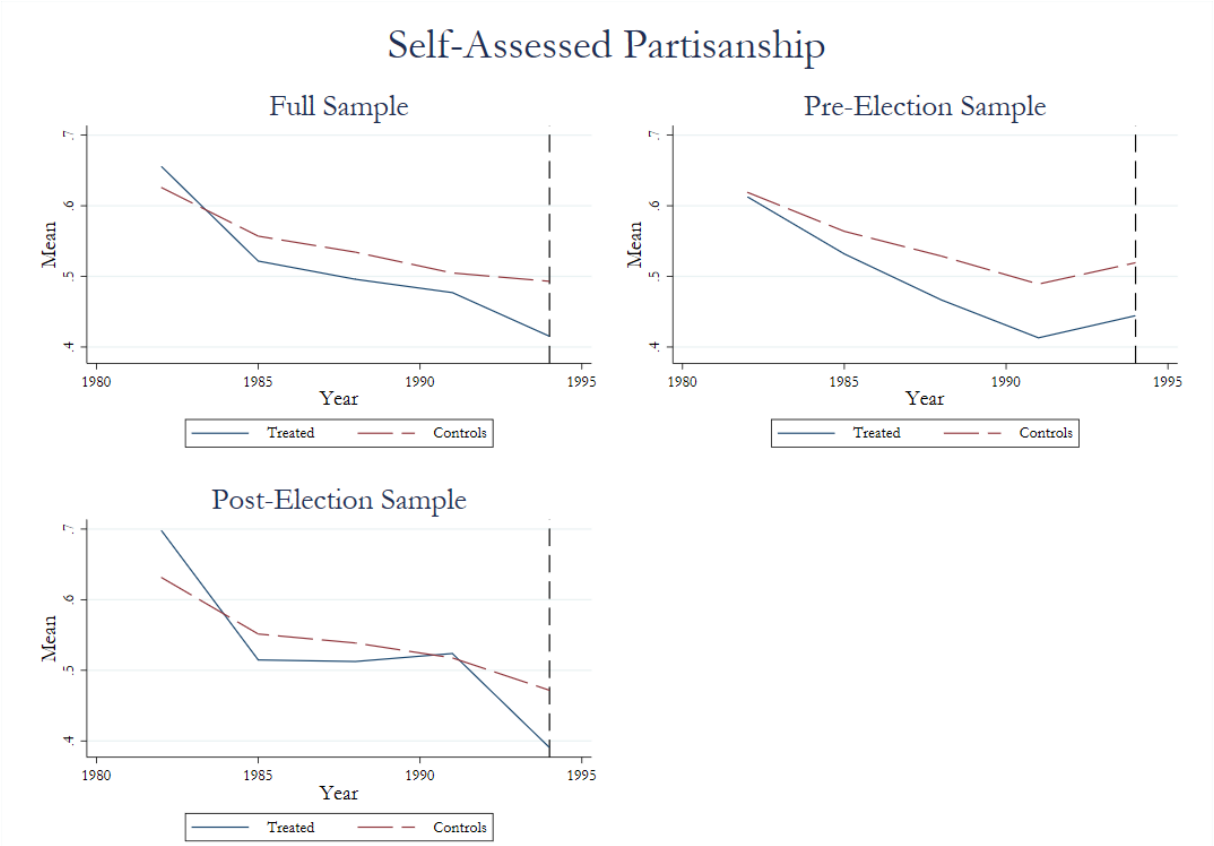


Figure XXII. The figures plot the mean over time for respondents' self-assessed partisanship, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) for the full sample, pre-election sample, and post-election sample, respectively. The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

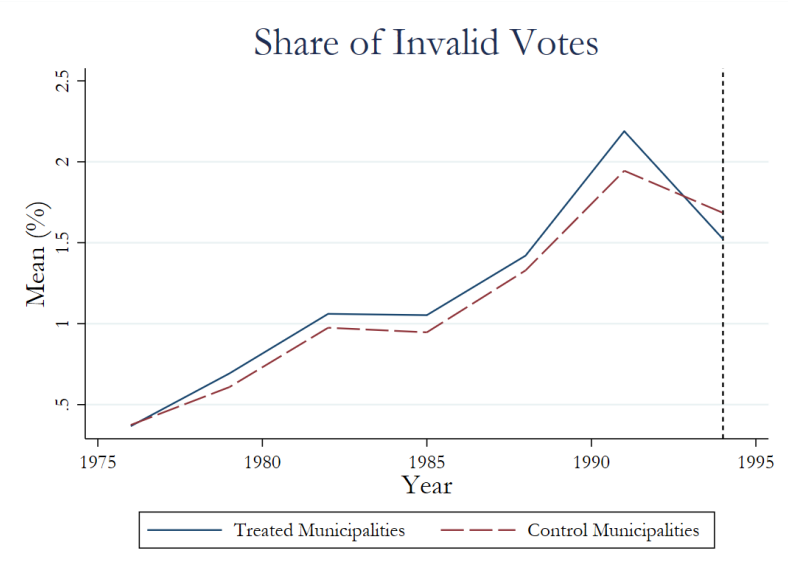


Figure XXIII. The figures plot the mean over time for the share of invalid votes in Municipal Council elections, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) using Statistics Sweden data (1976-1994). The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available.

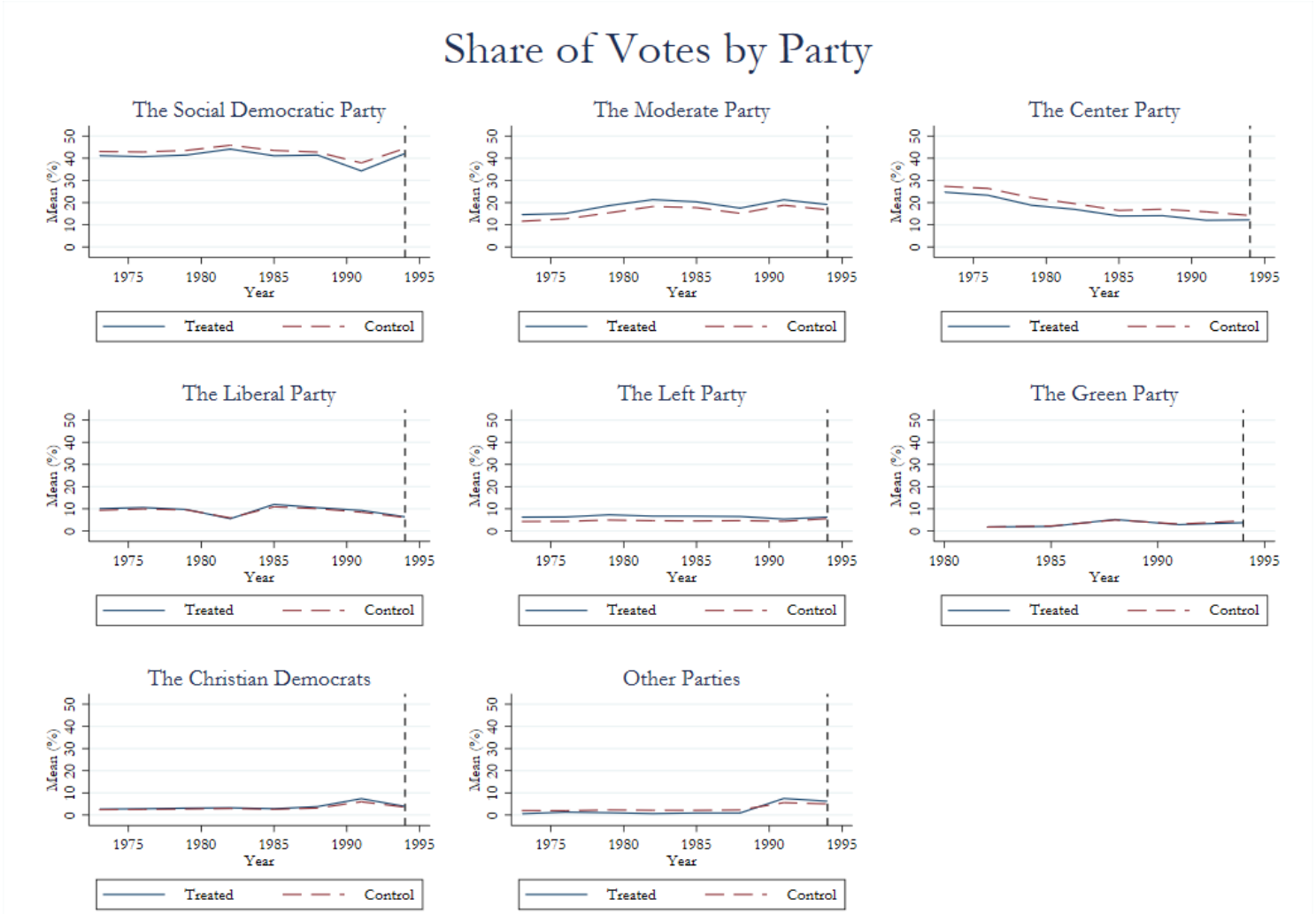


Figure XXIV. The figures plot the mean over time for the share of votes for each party in Municipal Council elections, separately for treated municipalities (solid trend line) and control municipalities (dashed trend line) using Statistics Sweden data (1973-1994). The vertical dashed line represents the beginning of treatment in 1994, which in this experiment is also the only post-treatment period available. Note that the Green Party was founded in 1981. Also note that Other Parties include, but are not limited to, the Sweden Democrats in 1988, 1991, and 1994 and, New Democracy in 1991 and 1994.

A.6 Robustness Checks

Trust in Politicians

Table IV. The table displays the regression coefficients from running the static DiD specification in eq. (2a) with the one direct measure of trust in politicians that proved statistically significant as outcome, namely an indicator variable which equals one if respondents said they have very high, pretty high, or pretty low trust in politicians, meaning it includes everyone not saying that they did not have very low trust in politicians. In column (1), the static DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here.

Do not have very low trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.118	0.193	0.0846
	(0.0850)	(0.214)	(0.121)
Observations	7,312	3,329	3,963
Adjusted R-squared	0.016	0.036	-0.002
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Interacted FE	✓	✓	✓
Mean of dependent variable	0.8823	0.8796	0.8838

Standard errors in parentheses, clustered by municipality

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

Perception of Politicians

Table V. The table displays the regression coefficients from running the static DiD specification in eq. (2a) with the one proxy for trust in politicians that proved statistically significant as outcome, namely an indicator variable run on the pre-election sample which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Politicians in Parliament often disregard the opinion of ordinary people". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit trust in politicians.

Do not completely agree with "Politicians in Parliament disregard the opinion of ordinary people"	
	Pre-election sample
$[\text{Year}_t = 1994] \times \text{Semi-Open}_m$	0.138*
	(0.0798)
Observations	5,800
Adjusted R-squared	0.075
Year & municipality FE	✓
Controls	✓
Interacted FE	✓
Mean of dependent variable	0.7203

Standard errors in parentheses, clustered by municipality

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

Partisanship

Table VI. The table displays the results of running the static DiD specification in eq. (2a) with the indicator variable for being partisan, which was statistically significant only when using the post-election sample.

Partisanship, self-assessed	
	Post-election sample
$[Year_t = 1994] \times Semi\text{-}Open_m$	-0.0364
	(0.0995)
Observations	6,847
Adjusted R-squared	0.064
Year & municipality FE	✓
Controls	✓
Interacted FE	✓
Mean of dependent variable	0.5434

Standard errors in parentheses, clustered by municipality
*** p<0.01, ** p<0.05, * p<0.1

Invalid Votes

Table VII. The table displays the results from running the static DiD specification of eq. (2a) with the share of invalid votes in Municipal Council elections as outcome, using Statistics Sweden data (1976-1994).

	Share of Invalid Votes (%)
$[Year_t = 1994] \times Semi\text{-}Open_m$	-0.396***
	(0.0862)
Observations	45,168
Adjusted R-squared	0.332
Year & municipality FE	✓
Controls	-
Interacted FE	✓
Mean of dependent variable	1.132

Standard errors in parentheses, clustered by municipality
*** p<0.01, ** p<0.05, * p<0.1

Parties' Share of Votes

Table VIII. The table displays the results from running the static DiD specification of eq. (2a) with the share of votes for the Left Party in Municipal Council elections as outcome, using Statistics Sweden data (1973-1994).

Share of Votes by Party (%)	
	Left Party
$[Year_t = 1994] \times Semi\text{-}Open_m$	-0.759**
	(0.371)
Observations	2,198
Adjusted R-squared	0.906
Year & municipality FE	✓
Controls	-
Interacted FE	✓
Mean of dependent variable	8.773

Standard errors in parentheses, clustered by municipality
*** p<0.01, ** p<0.05, * p<0.1

A.7 Tables of Regression Results from the Dynamic DiD Model

Trust in Politicians

Table IX. The table reports the regression coefficients from the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high trust in politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category.

Very high trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1988} \times Semi\text{-}Open_m$	-0.00448	-0.0190	-0.00637
	(0.0119)	(0.0238)	(0.0115)
$Year_{1994} \times Semi\text{-}Open_m$	0.0203	0.0259	0.00187
	(0.0176)	(0.0358)	(0.00659)
Observations	7,312	3,341	3,971
Adjusted R-squared	0.054	0.095	0.098
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0215	0.0187	0.0238

Standard errors in parentheses, clustered by municipality

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

Table X. The table reports the regression coefficients from the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high or pretty high trust in politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category.

Very or pretty high trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1988} \times Semi\text{-}Open_m$	0.0560	0.135*	0.0166
	(0.0432)	(0.0803)	(0.0947)
$Year_{1994} \times Semi\text{-}Open_m$	0.0139	0.0326	0.0211
	(0.0546)	(0.0799)	(0.0962)
Observations	7,312	3,341	3,971
Adjusted R-squared	0.067	0.118	0.099
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.3998	0.3785	0.4167

Standard errors in parentheses, clustered by municipality

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

Table XI. The table reports the regression coefficients from the dynamic DiD of eq. (3) with a direct measure of trust in politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they have very high, pretty high, or pretty low trust in politicians, meaning it includes everyone not saying that they did not have very low trust in politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. Note that respondents were asked to self-assess their trust in politicians in 1988, 1991 and 1994 only, meaning the 1982 and 1985 elections are excluded from analysis here. The coefficient for 1991 is omitted, since 1991 is the reference category.

Do not have very low trust in politicians			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1988}$ × $Semi\text{-}Open_m$	0.00514	0.0569	-0.0145
	(0.0482)	(0.134)	(0.0669)
$Year_{1994}$ × $Semi\text{-}Open_m$	0.110***	0.136	0.0980*
	(0.0408)	(0.0845)	(0.0541)
Observations	7,312	5,803	3,971
Adjusted R-squared	0.056	0.756	0.079
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.8823	0.8796	0.8838

Standard errors in parentheses, clustered by municipality

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

Perception of Politicians

Table XII. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should exhibit a positive perception of politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Completely disagree with "Politicians in Parliament disregard the opinion of ordinary people"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982}$ × $Semi\text{-}Open_m$	0.0167	0.0461**	-0.0144
	(0.0157)	(0.0217)	(0.0210)
$Year_{1985}$ × $Semi\text{-}Open_m$	0.0190	-0.0174	0.0382
	(0.0242)	(0.0140)	(0.0394)
$Year_{1988}$ × $Semi\text{-}Open_m$	0.00972	0.00349	0.00787
	(0.0139)	(0.0271)	(0.0138)
$Year_{1994}$ × $Semi\text{-}Open_m$	0.0389*	0.0481**	0.0281*
	(0.0205)	(0.0227)	(0.0164)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.030	0.055	0.052
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0362	0.0444	0.0478

Standard errors in parentheses, clustered by municipality

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

Table XIII. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree or mostly disagree with the statement "Politicians in Parliament often disregard the opinion of ordinary people", meaning they should exhibit a positive perception of politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Completely or mostly disagree with "Politicians in Parliament disregard the opinion of ordinary people"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	0.0327	-0.0888	-0.0782
	(0.0592)	(0.102)	(0.0990)
$Year_{1985} \times Semi\text{-}Open_m$	0.0216	-0.0123	-0.00389
	(0.0766)	(0.0883)	(0.0925)
$Year_{1988} \times Semi\text{-}Open_m$	0.0509	0.109	0.123
	(0.0629)	(0.151)	(0.147)
$Year_{1994} \times Semi\text{-}Open_m$	0.0177	-0.0455	-0.0285
	(0.0313)	(0.0628)	(0.0550)
Observations	12,930	5,803	5,900
Adjusted R-squared	0.070	0.101	0.089
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.1940	0.2615	0.2393

Standard errors in parentheses, clustered by municipality

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

Table XIV. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of politicians as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Politicians in Parliament often disregard the opinion of ordinary people". In other words, it is coded as everyone not completely agreeing with the statement, meaning they should exhibit a positive perception of politicians. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Do not completely agree with "Politicians in Parliament disregard the opinion of ordinary people"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	-0.00587	0.0258	-0.0113
	(0.0595)	(0.111)	(0.0788)
$Year_{1985} \times Semi\text{-}Open_m$	0.00918	-0.0317	0.0264
	(0.0473)	(0.114)	(0.0558)
$Year_{1988} \times Semi\text{-}Open_m$	0.000475	0.0450	-0.0357
	(0.0237)	(0.0505)	(0.0693)
$Year_{1994} \times Semi\text{-}Open_m$	0.0657	0.111*	0.0197
	(0.0698)	(0.0582)	(0.0950)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.079	0.116	0.092
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5088	0.7203	0.6018

Standard errors in parentheses, clustered by municipality

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

Perception of Parties

Table XV. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should exhibit a positive perception of parties. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Completely disagree with "Parties only care about people's votes, not their opinion"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	0.0259	-0.0257	0.0681*
	(0.0314)	(0.0541)	(0.0406)
$Year_{1985} \times Semi\text{-}Open_m$	0.00610	-0.0298	0.0348
	(0.0408)	(0.0793)	(0.0265)
$Year_{1988} \times Semi\text{-}Open_m$	0.00169	-0.00389	0.0117
	(0.0210)	(0.0476)	(0.0232)
$Year_{1994} \times Semi\text{-}Open_m$	0.00841	-0.00234	0.0174
	(0.0236)	(0.0420)	(0.0216)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.033	0.058	0.048
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.0414	0.0526	0.0535

Standard errors in parentheses, clustered by municipality

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

Table XVI. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of parties as outcome. It is coded here as an indicator variable which equals one if respondents said they completely or mostly disagree with the statement "Parties only care about people's votes, not their opinion", meaning they should exhibit a positive perception of parties. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Completely or mostly disagree with "Parties only care about people's votes, not their opinion"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	0.0487	-0.00257	0.0794
	(0.0351)	(0.107)	(0.0932)
$Year_{1985} \times Semi\text{-}Open_m$	0.0146	0.0306	-0.00492
	(0.0563)	(0.0920)	(0.0914)
$Year_{1988} \times Semi\text{-}Open_m$	0.0705**	0.184**	-0.0179
	(0.0301)	(0.0784)	(0.0523)
$Year_{1994} \times Semi\text{-}Open_m$	0.0276*	0.00223	0.0469
	(0.0150)	(0.102)	(0.0735)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.067	0.095	0.088
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.2096	0.2874	0.2549

Standard errors in parentheses, clustered by municipality

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

Table XVII. The table reports the regression coefficients from the dynamic DiD of eq. (3) with respondents' perception of parties as outcome, coded here as an indicator variable equaling one if respondents said they completely disagree, mostly disagree or mostly agree with the statement "Parties only care about people's votes, not their opinion", meaning everyone not completely agreeing, i.e. exhibiting a positive perception of parties. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The coefficient for 1991 is omitted, since 1991 is the reference category.

Do not completely agree with "Parties only care about people's votes, not their opinion"			
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	0.0717*	-0.0108	0.133**
	(0.0433)	(0.109)	(0.0666)
$Year_{1985} \times Semi\text{-}Open_m$	0.107***	0.0512	0.144**
	(0.0301)	(0.0752)	(0.0586)
$Year_{1988} \times Semi\text{-}Open_m$	0.0560	-0.00756	0.0944*
	(0.0366)	(0.0743)	(0.0541)
$Year_{1994} \times Semi\text{-}Open_m$	0.0679	0.0380	0.0951
	(0.0632)	(0.0505)	(0.101)
Observations	12,930	5,803	7,127
Adjusted R-squared	0.093	0.129	0.112
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5168	0.7340	0.6097

Standard errors in parentheses, clustered by municipality

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

Voter Turnout

Table XVIII. The table displays the results from running the dynamic DiD of eq. (3) with voter turnout to Municipal Council elections as outcome. Column (1) uses data from Statistics Sweden (1973-1994). Column (2) uses data from the SNES (1982-1994) with controls. The interaction term for 1991 is omitted, as 1991 is the reference group.

Voter Turnout (%)		
	(1)	(2)
	Statistics Sweden data	SNES data
$Year_{1973} \times Semi\text{-}Open_m$	0.631	-
	(0.806)	
$Year_{1976} \times Semi\text{-}Open_m$	0.718	-
	(0.714)	
$Year_{1979} \times Semi\text{-}Open_m$	0.605	-
	(0.827)	
$Year_{1982} \times Semi\text{-}Open_m$	0.436	4.466
	(0.525)	(2.850)
$Year_{1985} \times Semi\text{-}Open_m$	0.319	0.967
	(0.384)	(4.419)
$Year_{1988} \times Semi\text{-}Open_m$	0.138	3.860
	(0.245)	(3.413)
$Year_{1994} \times Semi\text{-}Open_m$	-0.120	0.845
	(0.0499)	(3.339)
Observations	2,260	12,790
Adj. R-squared	0.926	0.064
Year & municipality FE	✓	✓
Controls	-	✓
Mean of dependent variable	84.702	88.944

Standard errors in parentheses, clustered by municipality

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

Partisanship

Table XIX. The table displays the results of running the dynamic DiD specification in eq. (3) with an indicator variable for being partisan as outcome, which equals one if respondents say "Yes" to being partisan. In column (1), the dynamic DiD is run on the full sample of respondents. In column (2), it is run on the pre-election sample only, and in column (3) the post-election sample only. The interaction term for 1991 is omitted, as 1991 is the reference group.

	Partisanship, self-assessed		
	(1)	(2)	(3)
	Full sample	Pre-election sample	Post-election sample
$Year_{1982} \times Semi\text{-}Open_m$	0.0453	0.0934	0.0413
	(0.0673)	(0.0989)	(0.0718)
$Year_{1985} \times Semi\text{-}Open_m$	0.0181	0.110	-0.0340
	(0.0239)	(0.0968)	(0.0590)
$Year_{1988} \times Semi\text{-}Open_m$	0.00726	0.0760	-0.0237
	(0.0587)	(0.103)	(0.0423)
$Year_{1994} \times Semi\text{-}Open_m$	-0.0528	0.0221	-0.102
	(0.0565)	(0.0930)	(0.0712)
Observations	12,381	5,530	6,851
Adjusted R-squared	0.102	0.144	0.111
Year & municipality FE	✓	✓	✓
Controls	✓	✓	✓
Mean of dependent variable	0.5425	0.5435	0.5434

Standard errors in parentheses, clustered by municipality

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

Invalid Votes

Table XX. The table displays the results from running the dynamic DiD specification in eq. (3) with the share of invalid votes in Municipal Council elections as outcome, using Statistics Sweden data (1976-1994). The interaction term for base category 1991 is omitted.

	Share of Invalid Votes (%)
$Year_{1976} \times Semi\text{-}Open_m$	-0.255
	(0.189)
$Year_{1979} \times Semi\text{-}Open_m$	-0.163
	(0.154)
$Year_{1982} \times Semi\text{-}Open_m$	-0.163
	(0.133)
$Year_{1985} \times Semi\text{-}Open_m$	-0.145
	(0.208)
$Year_{1988} \times Semi\text{-}Open_m$	-0.158
	(0.192)
$Year_{1994} \times Semi\text{-}Open_m$	-0.409***
	(0.0689)
Observations	45,168
Adj. R-squared	0.317
Year & municipality FE	✓
Controls	-
Mean of dependent variable	1.132

Standard errors in parentheses, clustered by municipality

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

Parties' Share of Votes

Table XXI. The table displays the results from running the dynamic DiD specification of eq. (3) with the share of votes by party in Municipal Council elections as outcome, using Statistics Sweden data (1973-1994). In column (1), this regards the share of votes for the Social Democratic Party; column (2) regards the Moderate Party, and so on. Note that the Green Party of column (6) was founded in 1981. Also note that Other Parties of column (8) include, but are not limited to, the Sweden Democrats in 1988, 1991, and 1994 and, New Democracy in 1991 and 1994. The interaction term for base category 1991 is omitted.

	Share of Votes by Party (%)							
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
	Social Democrats	Moderate Party	Center Party	Liberal Party	Left Party	Green Party	Christian Democrats	Other Parties
$Year_{1973} \times Semi\text{-}Open_m$	1.793	0.442	1.225*	-0.180	0.983	-	-1.186	-3.331
	(1.871)	(1.358)	(0.685)	(1.073)	(1.171)		(0.956)	(2.082)
$Year_{1976} \times Semi\text{-}Open_m$	1.582	-0.139	0.740	-0.248	1.176	-	-0.967	-2.749
	(1.825)	(0.990)	(0.887)	(0.992)	(1.096)		(1.099)	(2.206)
$Year_{1979} \times Semi\text{-}Open_m$	1.653	0.658	0.409	-0.714	1.646***	-	-0.902	-3.216*
	(1.774)	(0.508)	(0.757)	(0.672)	(0.607)		(1.076)	(1.748)
$Year_{1982} \times Semi\text{-}Open_m$	1.877	0.613	1.253	-1.179	1.372**	-0.0233	-1.028	-3.366*
	(1.394)	(0.646)	(0.763)	(0.905)	(0.595)	(0.381)	(0.900)	(1.845)
$Year_{1985} \times Semi\text{-}Open_m$	1.206	0.117	1.301	0.264	1.190**	0.0438	-1.134	-3.066
	(0.824)	(0.609)	(1.105)	(0.868)	(0.509)	(0.301)	(0.972)	(1.863)
$Year_{1988} \times Semi\text{-}Open_m$	2.237**	-0.117	0.891	-0.385	0.906***	0.458	-0.720	-3.288*
	(0.882)	(0.645)	(0.990)	(0.443)	(0.300)	(0.581)	(0.671)	(1.816)
$Year_{1994} \times Semi\text{-}Open_m$	1.267	-0.0617	1.887*	-0.524	-0.324	-0.560	-1.012	-0.655
	(1.147)	(1.104)	(1.005)	(0.738)	(0.350)	(0.385)	(0.785)	(1.432)
Observations	2,260	2,260	2,260	2,260	2,201	1,355	2,225	2,189
Adjusted R-squared	0.936	0.928	0.923	0.848	0.832	0.770	0.911	0.542
Year & municipality FE	✓	✓	✓	✓	✓	✓	✓	✓
Controls	-	-	-	-	-	-	-	-
Mean of dependent variable	42.944	15.870	19.733	4.695	8.773	3.405	3.2542	2.957

Standard errors in parentheses, clustered by municipality

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