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A Banana Republic? Trust in Electoral Institutions in Western Democracies – Evidence from a Presidential Election in Austria

Niklas Potrafke
Felix Roesel

CESifo
Center for Economic Studies & Ifo Institute

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Abstract

We examine the extent to which political scandals influence trust in electoral institutions in established Western democracies. The second ballot of the 2016 Presidential election in Austria needed to be repeated because of inconsistencies in individual electoral districts (scandal districts). In particular, postal votes were counted carelessly. We use a difference-indifferences approach comparing the regular and the repeated round of the second ballot, and examine whether voter turnout, postal voting, invalid voting and the vote shares of the candidates changed in scandal districts. Postal voting declined, but the results do not show that districts with inconsistencies differ regarding voter turnout, postal voting, invalid voting, and vote shares of the candidates. Voters' trust in electoral institutions does not seem to depend on individual failing local administrations.

Niklas Potrafke
Ifo Institute – Leibniz Institute for
Economic Research
at the University of Munich
Poschingerstrasse 5
Germany – 81679 Munich
potrafke@ifo.de

Felix Roesel
Ifo Institute – Leibniz Institute for
Economic Research
Dresden Branch
Einsteinstrasse 3
Germany – 01069 Dresden
roesel@ifo.de

1. Introduction

Western democracies enjoy established political institutions. Elections are free, secret, and equal (one man, one vote). Elections are usually not manipulated. By contrast, the second ballot of the 2016 presidential election in Austria needed to be repeated because there were inconsistencies in individual electoral districts (scandal districts). The constitutional court did not confirm manipulations, but manipulations were possible. The public discourse portrayed that citizens' trust in political institutions declined. The Austrian media and even the Austrian Chancellor, Christian Kern, described Austria as seeming like a "banana republic"[2]. Regional differences in inconsistencies in the Austrian presidential election in 2016 are a natural experiment for investigating whether political scandals influence trust in electoral institutions in established Western democracies. Previous studies examine the extent to which voters punish parties and politicians involved in political scandals and the extent to which political scandals influence voter turnout (see, e.g., Ferraz and Finan 2008, Chang et al. 2010, Costas-Pérez et al. 2012, Hirano and Snyder 2012, Pattie and Johnston 2012, Vivyan et al. 2012, Eggers 2014, Kauder and Potrafke 2015, Ceron and Mainenti 2016, Fernández-Vázquez et al. 2016, Rudolph and Däubler 2016, Sulitzeanu-Kenan et al. 2016, Larcinese and Sircar 2017). Yet scholars did not investigate how inconsistencies in the counting of votes influence participation in subsequent elections.

We use a difference-in-differences approach to examine whether voter behaviour in scandal districts changed in the repeated ballot of the Austrian Presidential election. The hypothesis to be investigated is crystal clear: "If voters fear that polls are corrupt, they have less incentive to bother casting a vote; participating in a process in which they do not have confidence will be less attractive, and they may well perceive the outcome of the election to

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[2] See, e.g., the comment of Anneliese Rohrer in the newspaper Die Presse, 18.06.2016, or the interview with Chancellor Christian Kern in OE24, 11.06.2016, http://www.oe24.at/oesterreich/politik/Christian-Kern-Wir-haben-eine-Chance-vergeben/239273674.

be a foregone conclusion" (Birch 2010, p. 1603). We distinguish between scandal districts that were subject to the summons of the constitutional court, and scandal districts where inconsistencies were confirmed by the court. There were 117 electoral districts in the 2016 Austrian presidential election. The constitutional court issued a summons on representatives of 20 electoral districts, and confirmed inconsistencies in 14 electoral districts (see Figure 1). Voter turnout, and postal voting in particular, is likely to decrease in electoral districts with inconsistencies because voters may be gutted about politics.[3] Postal voting declined, but the results do not show that districts with inconsistencies differ regarding postal voting and voter turnout.

[Figure 1 about here]

We also examine whether inconsistencies in an individual electoral district may have influenced the votes for the individual candidates. It is conceivable, for example, that voters in electoral districts with confirmed inconsistencies that came to light because of the initiative launched by the populist rightwing party were more likely to vote in favour of the populist rightwing party's candidate Norbert Hofer in the by-election. Scandals may also increase the share of invalid votes if voters use invalid ballots to signal disenchantment with politics. We show, however, that the populist rightwing vote shares and the share of invalid votes were not influenced by inconsistencies in individual districts.

We conclude that voters' trust in electoral institutions and their resulting participation levels do not seem to depend on the failure of individual local administrations. National media coverage may have equalised the perception of inconsistencies across the entire country. Recounting votes in individual regions, like, for example, the recount of ballots in

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[3] On the effects of electoral institutions such as compulsory voting and extended opening hours of polling stations on voter turnout in Austria see Ferwerda (2014), Hoffman et al. (2016) and Potrafke and Roesel (2016). For surveys on determinants of voter turnout see Geys (2006) and Cancela and Geys (2016).

Wisconsin in the course of the US presidential election 2016, are likely to impact national, rather than specific regional voting behaviour.

2. Related Literature

2.1 Political trust and electoral outcomes

Some studies elaborate on the correlation between trust such as in international organisations and voter turnout. Cox (2003) uses cross-sectional data on trust and voter turnout in 13 European countries. Voter turnout is for national elections and elections to the European Parliament in the year 1999. Parliamentary trust is measured by 1999 Eurobarometer data in which participants were asked: "For each of the following European institutions, please tell me if you tend to trust it or tend to not trust it". In particular, the author also uses the percentage of respondents that expressed their trust in the European parliament. Interpersonal trust is measured by the 1990-1993 and 1995-1997 World Values Surveys (WVS) questions: "Generally speaking, would you say that most people can be trusted or that you can't be too careful in dealing with people?" The results show that interpersonal trust was not correlated with voter turnout in elections to the national and European parliament. By contrast, political institution trust, and especially trust in the European Parliament was significantly positively correlated with voter turnout in elections to the European Parliament.

In Latin America, distrust in elections was huge: Carrerras and İrepoğlu (2013) use survey data and report that about 33.2 % of Latin American respondents did not trust in (domestic) electoral processes compared to 5 % in North American and Western Europe. The authors also use data from the 2010 Americas Barometer, a survey "administered by the Latin American Public Opinion Project (LAPOP) at Vanderbuilt University" (p. 614). The survey includes respondents from 18 Latin American countries. The authors estimate a micro-

econometric model using a binary dependent variable measuring whether respondents voted in the last Presidential elections. The main explanatory variable is based on the answers to the question "To what extent to you trust elections?" This variable is categorical assuming values between 1 and 7. The results show that respondents with low trust in elections (1) were some 3.8 percentage points less likely to participate in elections than respondents with high trust in elections (7).

In US presidential elections over the period 1968-1996, declining political trust increased support for the non-incumbent party, when two candidates were running. When three candidates were running for office, however, distrustful voters were inclined to vote for the third-party candidate, at the expense of the Republican and Democratic candidates. Hetherington (1999) arrived at these results using micro data of the National Election Surveys (NES). Political trust is measured by the respondents' mean score on a four item trust index. Political trust in government – as measured by the NES-data – was hardly correlated with voter turnout in US presidential elections over the period 1964–1972 (Citrin 1974), and especially in the 1972 election (Knack 1992). By contrast, trust in people was positively correlated with voter turnout in the 1972 presidential elections (Knack 1992).

2.2 Scandals and electoral outcomes

Political scandals are expected to have electoral consequences. Voters may, for example, vote incumbents out of office if the incumbents are accused of having violated the law or ethical rules. Scholars examine the electoral consequences of individual political scandals such as the MP's 2009 expenses scandal in the United Kingdom. The research design often used is to estimate a difference-in-differences model exploiting being involved in a scandal as treatment. Studies compare party vote shares or voter turnout in the pre-scandal and post-scandal elections.

Since 1988 MP's in the United Kingdom have been allowed to make expenses claims to cover, for example, the costs of running their offices or of running a second household (one household in London and one in the home constituency). "Some seemed bizarrely out of touch (as with the senior MP who claimed against the cost of having someone replace faulty light bulbs)" (Pattie and Johnston 2012: 733). The scandal leaked out in May 2009 when the newspaper the *Daily Telegraph* began to report about the issue, one year before the parliamentary elections in 2010. Four studies investigate issues of electoral accountability in the course of the scandal: Eggers (2014), Larcinese and Sircar (2017), Pattie and Johnston (2012) and Vivyan et al. (2012).

The vote share of MPs who were implicated in the scandal was by about 2.6 percentage points lower than the vote share of MPs who were not involved, compared to the previous 2005 parliamentary election (Eggers 2014).[4] Partisanship in the electoral districts, however, had a major impact on the extent to which voters punished MPs. Eggers (2014) exploits variance across constituencies considering whether an individual constituency had a strong Labour-Conservative-battleground (high partisanship); or whether a Liberal Democrat was the main challenger to a labour or conservative politician (low partisanship) in an individual constituency. The results show that in constituencies with low partisanship, the vote share of MPs who were implicated in the scandal was by about 6 percentage points lower than the vote share of MPs who were not involved. Politicians in constituencies with low partisanship were hence relatively less punished, but were also more likely to be implicated in the scandal in the first place.

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[4] The authors measure "implication in the expenses scandal based on the proportion of news stories in the Google News archive mentioning an MP and her constituency during the period between the beginning of the scandal and the 2010 election that also mention the word "expensens"" (p. 449). The author computes a continuous variable for "sinners" and "saints," which he translates into a binary variable indicating whether an MP was implicated in the scandal.

The main explanatory variables of Larcinese and Sircar (2017) are media coverage and the amount of money abuse the report by Sir Thomas Legg (who was commissioned to audit the MPs' expenses) included. Media coverage is measured by the number of articles on individual MPs in the seven newspapers *Daily Telegraph, The Guardian, The Times, The Independent, The Sun, Daily Mail,* and the *Scotsman* over the period 8 May 2009 to 7 August 2009. The results show that the scandal-related news coverage reduced the probability that an individual MP left parliament, the amount of money abused did not turn out to be statistically significant. In a similar vein, the amount of money abused did not predict the difference in MPs' vote shares between the 2010 and 2005 parliamentary elections, but a one standard deviation increase in news coverage gave rise to a 0.9% decrease in votes in 2010 compared to 2005.

Pattie and Johnston (2012) use micro data for the 2010 British Election Survey (BES) and show that individuals who were angry about the 2009 expenses scandal were more likely to abstain in the 2010 parliamentary elections (the dependent variable was the pre-election likelihood). Similarly, anger over the expenses scandal decreased the chances of an individual voting for the incumbent labour party. Using micro data from the 2010 BES, the results of Vivyan et al. (2012) also show that a voter whose MP was implicated in the scandal was more likely to believe that her/his MP over-claimed on expenses than a voter whose MP was not implicated in the scandal.[5] Moreover, a voter who believed that his/her MP over-claimed on expenses was about 5 percentage points less likely to vote for him/her in the 2010 election than a voter who did not believe that his/her MP over-claimed on expenses. Larcinese and Sircar (2017) also use the BES and examine what predicts voters' views on whether MPs

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[5] Being implicated in the scandal is a binary variable assuming the value one when "an MP was coded as being implicated by either the Telegraph or the Legg report" (p. 753).

over-claimed on expenses. The results show that perceived involvement of an MP increased by about 0.07% when the money MPs were accused to have over-claimed increased by 1%.

In April 2013, it leaked out that members of the Bavarian parliament (the largest German state by area) hired relatives using tax payer's money. Hiring relatives was not illegal because there was an interim arrangement in the year 2000 that allowed MPs hiring relatives as assistants, but certainly smacked as of nepotism.[6] In particular, there were state and federal elections in September 2013, and politicians of the conservative incumbent party feared being punished by the voters. Many MPs who hired relatives using tax paper's money repaid that money. Kauder and Potrafke (2015) used data from the 2013 state elections and the previous 2008 state election and estimated a differences-in-differences model to examine whether voters punished MPs who hired relatives. The results do not show that being involved in the family scandal influenced the vote share of the incumbent conservative Christian Social Union (CSU) and voter turnout. Rudolph and Däubler (2016) also examine the electoral consequences of the very same family scandal and conclude that voters punished the incumbent CSU politicians, especially politicians who became elected under open-list proportional representation rather than under single-member district systems.[7]

Media coverage of scandals is often important to whether voters punish politicians for being implicated in a scandal.[8] When the media hardly report about a scandal and/or an individual politician is involved in the scandal, electoral accountability is far less pronounced than when the media do publish extensive reports. Costas-Pérez et al. (2012) show how

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[6] Candidates running in electoral districts with large majorities were more likely to use the interim arrangement than candidates running in electoral districts with small majorities (Kauder and Potrafke 2016).
[7] Kauder and Potrafke (2015) use those (73 of 91) districts that were not adjusted between the 2008 and the 2013 state elections (146 observations). Rudolph and Däubler (2016) use 180 observations. An important issue is which candidate hired relatives (or in which district a previously running candidate in the 2008 election did hire relatives). Kauder and Potrafke (2015) used the list of politicians published by the President of the Bavarian parliament on May 3 (16 scandal districts). Rudolph and Däubler (2016) "observe 23 implicated districts, of which 14 are with running "affair MPs"" (p. 755).
[8] See Puglisi and Snyder (2011) on newspaper coverage of political scandals.

important news coverage is. The authors use data from 5,144 news stories about corruption in Spanish local governments over the period 1996-2009 and data for the 2003 and 2007 municipal elections.[9] The results show that being involved in corruption scandals reduced the re-election probability of an implicated politician by up to 14% when the press extensively reported on the scandal. By contrast, voters did not punish politicians when cases were dismissed or when the judiciary did not intervene. Fernández-Vázquez et al. (2016) elaborate on Spanish local politicians being involved in scandals between the local elections in 2007 and 2011 to examine whether voters punished corrupt politicians. The vote shares of corrupt mayors were about 1.8 percentage points lower than those of non-corrupt mayors. Voters distinguished, however, between types of corruption. The vote share of mayors involved in "welfare decreasing corrupt practices" ("Would the local economy have benefited in the short run if the mayor had not engaged in the illegal practice in question?" p. 387) was about 4.2 percentage points lower than non-corrupt mayors. The vote share of mayors involved in other corruption issues did not differ from the vote share of non-corrupt mayors.

In Brazil, mayors described as being corrupt lost about 10 to 30% of their vote share compared to the previous election. The re-election chances of corrupt mayors decreased by about 7 percentage points (or 17%). Ferraz and Finan (2008) employ publicly released audit reports to measure the corruption of mayors and to estimate the causal effect of corruption on their re-election chances. The Brazilian federal government randomly selected municipalities to audit their expenditure. The overall sample includes 373 municipalities in which 205 municipalities were audited before municipal elections in 2004 (treatment group) and 168 municipalities were audited after the municipal elections in 2004. In particular, voters punished corrupt mayors to a large extent in municipalities in which local radio stations were present and reported on the findings of the audit report. In turn, when local radio stations were

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[9] On the effect of being involved in scandals on trust see Solé-Ollé and Sorribas-Navarro (2014).

present and reported on the findings of the audit report, the re-election prospects of non-corrupt incumbents increased.

In Israel, the vote share of corrupt mayors in large cities was about 10 percentage points lower than the vote share for non-corrupt mayors in the 2013 local elections (Sulitzeanu-Kenan et al. 2016). Cities that had corrupt mayors did not, however, experience lower voter turnout than cities with non-corrupt mayors. Experimental evidence from Mexico also shows that incumbent corruption increased the vote share of the challenger's party. In Mexico, by contrast, voter turnout also decreased (Chong et al. 2015).

In Italy, voters hardly punished politicians who were implicated in malfeasance: Chang et al. (2010) use data on all members of the Italian lower house of parliament over the period 1948–1994. The results do not show that (a) a charge of malfeasance discouraged individual candidates to run in the subsequent election and (b) a charge of malfeasance decreased the probability of getting re-elected, the exception being the period 1992–1994. Furthermore, voters only punished corrupt politicians when party leaders were solely responsible for selecting candidates in electoral districts (in contrast to when voters were able to participate in candidate selection): Ceron and Mainenti (2016) use panel data for seven general elections over the period 1983-2013 and disentangle the effects of corruption on vote shares by the candidates' selection process.

In the United States, US House incumbents involved in scandals were less likely to win primary and general elections compared to incumbents who were not involved in scandals (Hirano and Snyder 2012).[10] The authors use data over the period 1978–2008 and use seven dependent variables. The results show, for example, that in primary (general) elections

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[10]The authors use a dummy variable for scandals assuming the value one when "(1) the House Committee on Standards of Official Conduct investigated the incumbent; or (2) the Department of Justice investigated the incumbent, or (3) Congressional Quarterly Weekly Reports specifically discussed the scandal in at least one article" (p. 450).

incumbents involved in scandals received about 16 (11) percent fewer votes than incumbents who were not involved in scandals.

3. Institutional background

Austrian presidents have been elected in direct elections since 1951. The duration of their term-in-office is six years. The president is the federal head of the state of Austria and theoretically enjoys a great deal of power. In practice, however, the Austrian president administrates ceremonial events such as receptions and addresses of welcome. In contrast to the indirect presidential elections in the United States (system of electors), for example, the Austrian president is proportionally and directly elected. A candidate needs over 50% of the valid ballots casted nationwide to win. There are two rounds. If no candidate receives more than 50% in the first ballot, a second (run-off) ballot is held among the two candidates that received the most votes in the first round. Votes cast at the "regular" ballot box are collected at the local level (*Gemeinde*); postal votes, by contrast, are counted at the (electoral) district level (*Bezirk*), which is the upper-local administrative level. There is no information on postal voting at the local level.

The 2016 presidential election in Austria was unique in many ways. Austrian presidents since World War II were either members of the Social Democratic Party (SPÖ) or the Conservative Party (ÖVP), and one president, Rudolf Kirchschläger, was crossbench (in office over the period 1974–1986). In the first ballot of the 2016 presidential election on 24 April, however, neither a candidate of the SPÖ nor the ÖVP made it to the second ballot, but a candidate from the populist rightwing Freedom Party (FPÖ), Norbert Hofer, and of the Green Party (Greens), Alexander van der Bellen did. The Austrian party system has changed dramatically as in many other western democracies in recent years. In the second ballot on 22 May 2016, the Green candidate van der Bellen won the election against the populist rightwing

candidate Norbert Hofer with a marginal lead of 30,863 votes (van der Bellen received 50.35 % of the votes, Hofer received 49.65%). Postal votes turned the balance in favour of van der Bellen. Voter turnout was 72.7%.

The defeated FPÖ was concerned about inconsistencies, especially in dealing with postal votes. As a result, the chairman of the populist rightwing FPÖ, Hans-Christian Strache, went to the constitutional court to contest the second ballot. The constitutional court scrutinized the constitutional complaint for about a month. On 1 July 2016, the constitutional court acceded to the constitutional complaint because of inconsistencies. For example, postal votes must be counted on the Monday after the election (which takes place on a Sunday) at 9 a.m. In Bregenz, campaign workers started to count postal votes at 8 a.m. In Innsbruck-Land, campaign workers started to count the postal votes at 9 a.m., but already opened the envelopes of postal votes on Sunday.[11] The constitutional court made clear that there was no concern over manipulation, but the rules were broken. The inconsistencies affected 77,926 votes; an amount of votes which may well have changed the outcome of the election, because Alexander van der Bellen won by 30,863 votes against Norbert Hofer. The constitutional court called for a repetition of the second ballot.

The by-election of the second ballot was scheduled to take place on 2 October 2016. There were, however, inconsistencies with the papers for the postal votes (some of the envelopes that were sent out to the citizens who wanted to use postal voting could not be sealed). On 12 September 2016, the government therefore decided to postpone the by-election of the second ballot. The by-election of the second ballot thus took place on 4 December 2016. Alexander van der Bellen won the by-election of the second ballot with 53%. Voter turnout was 74.1%. The register of eligible voters was updated to account for mortality, naturalization, and population passing the voting age of 16 between February 2016 and

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[11] See Frankfurter Allgemeine Zeitung, 02.07.2016, "Wenigstens der österreichische Wein taugt noch was".

September 2016. The total electorate, however, increased only by 0.27% from 6,382,507 to 6,399,572. We do not believe that this update of the eligible voters influences our inferences. In any event, we will also control for changes in the district electorate.

4. Empirical analysis

4.1 Identification strategy

We use a difference-in-difference approach to identify the causal effect of being a scandal district on voter behaviour (postal voting, voter turnout, invalid voting, and candidate vote shares). Our key identifying assumption is that scandal and non-scandal districts follow a common trend, which would have applied in the absence of the scandal. We describe why we believe that the common trend assumption holds.

Firstly, Figure 2 suggests that there are parallel pre-scandal trends in postal voting, voter turnout, FPÖ vote shares and invalid vote shares in the scandal and non-scandal districts. Vertical lines describe the timing of the scandal. The upper panel compares districts that were subject to constitutional court summons to the remaining districts, the lower panel compares districts with confirmed inconsistencies to all other districts. Postal voting was introduced in 2007, data are therefore available since the national election of 2008. In both scandal and non-scandal districts, the share of voters using postal voting increased to around 15% in the first round of the 2016 presidential election. Voter turnout in nationwide elections decreased from around 80% in the early 1990s to around 70% in 2016. The vote share of the populist rightwing FPÖ was usually between 10% and 30% and has not differed significantly in scandal and non-scandal districts over the last ten years. The same holds true for invalid votes. The share of invalid votes was substantially larger in presidential elections (1998, 2004, 2010, and 2016), but did not differ among scandal and non-scandal districts. As a result, both

groups of districts seemed to have followed a common trend. Group mean differences are small and even diminish when the 23 electoral districts of Vienna are excluded (see Appendix, Figure A.1).

[Figure 2 about here]

Secondly, we test whether observable pre-scandal characteristics differ across scandal and non-scandal districts. Table 2 indicates that there were no significant differences between non-scandal districts and districts where inconsistencies were subject to court summons (column (4)), or where inconsistencies were confirmed (column (5)). Neither pre-scandal electorate, rainfall nor other socio-demographic variables, e.g., the population share of female, foreigners, elderly people, differ across district groups. The exceptions are the opening hours of polling stations, unemployment, and the share of foreigners that are somewhat larger in non-scandal districts. If we exclude the districts of Vienna (lower panel of Table 2), however, these differences also lack statistical significance.

[Table 2 about here]

Thirdly, there seems to be no spatial clustering of scandal districts. Figure 1 shows that many scandal districts are in the West (Tyrol, Vorarlberg), in the South (Carinthia, Styria), and in the North (Upper Austria, Lower Austria) of Austria. Inconsistencies were widespread; the FPÖ accused 97 out of 117 districts of possible manipulations.

Fourthly, the education and skills of the civil servants in the districts may have influenced the selection into treatment. The less educated the civil servants are, the more likely inconsistencies may be. However, differences in education between scandal and non-scandal district administrations are unlikely. The education of civil servants is standardised in Austria. We also do not find any evidence that failing district officials resigned in the course of the scandals. Quite on the contrary, the district authority of Bregenz (state of Vorarlberg)

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complained that their failing civil servants were somehow subject to "witch hunting" which "they do not deserve".[12] Administrative abilities thus do not predict selection into treatment.

Fifthly, we examine whether the detection of inconsistencies may have been manipulated, and consequently, have given rise to a selection into treatment. We do not believe that there was such selection into treatment for two reasons. Members of the three main parties (the social-democratic SPÖ, the conservative ÖVP, and the populist rightwing FPÖ) were allowed to join the committees counting postal votes in all districts. Thus, FPÖ district branches had the same chance in all districts to detect inconsistencies. Moreover, the broad media coverage encouraged many advocates and party members of the FPÖ to report inconsistencies via Facebook. FPÖ officials collected, analysed and conjoined all reports. The complaint of the FPÖ explicitly referred to Facebook sources. Therefore, even a small number of voters per district (e.g., in districts in which citizens hardly vote for the FPÖ) were sufficient to enter the document for the investigation by the constitutional court.

Altogether, we believe in common trends and a selection mechanism that is orthogonal to observable, but also to unobservable characteristics. The nationwide repetition of an election is a unique event in Austrian history; in 1970 and 1995, national elections only had to be repeated in individual municipalities where minor inconsistencies occurred.

4.2 Data

We use data at the level of the 117 electoral districts of Austria for the original and the repeated second ballot of the Austrian presidential election in May and December 2016.[13] Electoral districts are administrative entities for structuring the counting of votes, and of postal votes in particular. Election data and data on the opening hours of polling stations are

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[12] See, Der Standard, 14.07.2016, http://derstandard.at/2000041085212/Disziplinarstellen-ermitteln-gegen-Beamte.

[13] Vienna accounts for 23 of the 117 electoral districts of Austria.

obtained from the Austrian Federal Ministry of the Interior. We compile data on rain from the weather website wetteronline.de. All other variables are collected from the publications of Statistics Austria. We code scandal districts accordingly to the twitter postings and press releases of the constitutional court.[14]

4.3 Econometric model

Our difference-in-difference Ordinary Least Squares (OLS) model takes the following form:

$$Y_{ijt} = \alpha + \beta_1 Incons_i + \beta_2 Repeat_t + \beta_3 (Incons_i \times Repeat_t) + X'_{it}\gamma + \varepsilon_{it}$$

$$\text{with } i = 1, \ldots, 117, j = 1, \ldots, 4, \text{ and } t = 1,2$$

where Y_{ijt} describes the $j = 1, \ldots, 4$ four dependent variables in district i for election t ($t = 1$: regular second ballot of presidential election 2016, $t = 2$: repeated second ballot of the presidential election 2016): the share of all voters in district i using postal voting, voter turnout, which is the overall share of voters, invalid votes measured as the share of invalid votes, and the FPÖ vote share, which is the vote share of the populist rightwing presidential candidate. $Incons_i$ is a dummy variable that equals one for districts with inconsistencies, and zero otherwise. We use two different measures for inconsistencies: summons to the constitutional court and confirmations of inconsistencies by the constitutional court. $Repeat_t$ is one for repeated second ballot in December 2016 and zero for the first second ballot in May 2016. ($Incons_i \times Repeat_t$) describes the interaction and is our variable of interest (treatment). X is a vector of control variables, including the number of the district electorate

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[14] Districts subject to summons: See the Twitter tweets of the speaker of the Austrian constitutional court on June 16, 2016: "Geladen sind mittlerweile an die 90 Mitglieder von Bezirkswahlbehörden. Am Montag soll mit folgenden Bezirken begonnen werden: Südoststeiermark, Innsbruck-Land, Villach-Stadt, Kitzbühel, Villach-Land und Schwaz. An den weiteren Tagen: Landeck, Wien-Umgebung, Hermagor, Hollabrunn, Wolfsberg, Freistadt, Liezen, Bregenz, Kufstein, Graz-Umgebung, Gänserndorf, Leibnitz, Völkermarkt sowie Reutte" (tweets aggregated by the authors). Districts with confirmed inconsistencies: "In the districts of Innsbruck-Land, Südoststeiermark, Stadt Villach, Villach-Land, Schwaz, Wien-Umgebung, Hermagor, Wolfsberg, Freistadt, Bregenz, Kufstein, Graz-Umgebung, Leibnitz and Reutte the rules governing the implementation of the postal voting system were not complied with." See press relase 07/01/16 of the Austrian constitutional court.

(log), the amount of rain in the district capital city at the election day, the district average in the opening hours of polling stations that differ substantially across Austria, and the unemployment rate. We estimate the difference-in-differences model with standard errors robust to heteroskedasticity (Huber-White sandwich standard errors – see Huber 1967, White 1980).

5. Results

5.1 Baseline

The baseline results do not show that voting behaviour changed across scandal and non-scandal districts in the repeated ballot. The upper panel of Table 3 shows the difference-in-differences results for a specification excluding control variables. Columns (1) to (4) refer to districts that were subject to summons to the constitutional court, columns (5) to (8) refer to confirmed inconsistencies by the constitutional court. We observe significant differences across scandal and non-scandal districts in terms of postal voting, voter turnout and FPÖ vote shares (*Inconsistencies*). We also find that all outcome variables change from the regular to the repeated ballot. For example, voter turnout increased by about 1.4 percentage points on average (*Repeat*), and FPÖ vote shares decreased by about 3.3 percentage points. The interaction effect of *Inconsistencies* and *Repeat* (treatment effect), however, does not turn out to be statistically significant in any specification.

We include several control variables (lower panel of Table 3). Voter turnout and invalid voting decrease in the size of the electorate, and in unemployment rates. Rainfall was associated with a higher FPÖ vote shares, but rain was fairly rare on the election day of the regular and on the repeated election. The longer opening hours of polling stations were associated with decreases in populist rightwing and invalid voting, as well as with increases in

voter turnout and, somewhat counterintuitively, increases in the share of postal votes. Including/excluding individual control variables does not change the inferences regarding the scandal district effects.

5.2 Postal votes and ballot box votes

We distinguish between different ways to cast the ballot: postal voting and "regular" voting at the ballot box. The center and lower panel of Table 4 show that effects are similar for postal votes and ballot box votes. None of the effects turns out to be significant. The sole exception is invalid postal voting: in scandal districts, the share of invalid postal votes is about 7.6 percentage points higher than in non-scandal districts. It is, however, unclear why postal voters in scandal districts should be more inclined towards invalid voting than in non-scandal districts. We believe that this effect is more likely to be a result of behavioural changes in district administrations. District administrations tainted by scandal may have payed even more attention to correct ballots in the repeated by-election to avoid future accusations. Thus, scandal district administrations qualified more questionable votes to be invalid than non-scandal district administrations. If this is true, more invalid votes may result from the more careful counting of votes, rather than of from a change in voters' behaviour.

5.3 Robustness tests

We test whether the results are robust in several ways. We exclude the 23 electoral districts of Vienna. The Austrian capital of Vienna differs from the more rural parts of Austria. For example, postal voting is more common in Vienna than in the rest of the country. Figure A.1 in the Appendix indicates that the assumption of common trends of scandal and non-scandal districts is even more likely to be fulfilled when we exclude Vienna. The upper panel in Table 5 shows the results. Again, none of the coefficients of the treatment effect turns out to be statistically significant.

We include the control variables shown in Table 2, which, however, we only observe in the cross-section (center panel of Table 5). Observable characteristics that may predict voter behaviour are the population shares of women, foreigners, and inhabitants over the age of 75 years at the beginning of 2016. We also include the population share of citizens with low-education, employees, the self-employed and the population shares associated with agriculture and industry (services is the reference category). The treatment effect does not turn out to be statistically significant, albeit the R² doubles or triples in some specifications when including the control variables.

Finally, we should also not expect any significant effect for pseudo treatments. We re-assigned the treatment status by the names of districts (lower panel of Table 5). The first 14 (20) districts in alphabetical order were "pseudo-treated" to be scandal districts, the other districts being non-scandal districts. As expected, we do not observe any significant effect.

6. Discussion

Why is it that the inconsistencies do not seem to have influenced *local* voter turnout, postal and invalid votes and the candidates' vote shares, indicating that trust in electoral institutions was not eroded?[15] We propose six explanations.

Firstly, voters did not consider the inconsistencies in the first ballot of the presidential elections as a severe shortcoming. Neuwirth and Schachenmayer (2016) estimate the probability that inconsistencies changed the final result of the second ballot to be 0.0000000132%. This finding notwithstanding, related studies examining electoral consequences of corrupt politicians also show that voters may be tolerant to some forms of inconsistencies, i.e., corruption. Citizens may well support corrupt politicians (being aware of

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[15] It is worth noting that we cannot address a *global* decline in trust.

the corruption) because they believe that other dimensions of the politicians' performance such as providing public goods are more important (the evidence, however, is mixed: e.g. Winters and Weitz-Shapiro 2013).[16] Experimental evidence from India shows that citizens discriminate between the motives for corruption: citizens were more likely to punish corrupt politicians who aim to enrich themselves than corrupt politicians who aim to buy votes (Weschle 2016). The performance of the economy has been shown to influence how voters evaluate corrupt politicians: in Moldova, a high-corruption country, voters responded negatively to corruption (only) when the economy was performing poorly. In Sweden, a low-corruption country, voters always responded negatively to corruption, the performance of the economy notwithstanding (Klašnja and Tucker 2013).

Secondly, voters did not disentangle scandal from non-scandal districts: either because they did not know about individual scandal districts or because they did not want to discriminate and thought about the inconsistencies as being a general issue across the districts. The media often reported inconsistencies in general and did not envisage individual districts. Newspapers and FPÖ officials accused the Federal Minister of the Interior of being responsible for the supervision of counting votes, and thus for inconsistencies. The minister, in turn, said that all levels of government were somewhat responsible.[17] In the end, voters were not able to identify the exact source of failure.

Thirdly, trust in political institutions has been shown to be pronounced in closely-knit jurisdictions: by using data on municipal mergers in Denmark, the results of Hansen (2013) suggest that political trust decreased in the course of merging municipalities. The comparably

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[16] Scholars examine why corruption has been prevalent in many countries for a long time. On "corruption traps" see Klašnja et al. (2016).
[17] See, for example, Die Presse, „Wahlanfechtung: ‚Vorwürfe zusammengebrochen'", 26.06.2016, http://diepresse.com/home/politik/innenpolitik/5035340/Wahlanfechtung_Vorwuerfe-zusammengebrochen.

small size of Austrian districts with an average population of 55,000 may have prevented distrust from materialising in election outcomes.

Fourthly, there may have been inconsistencies in other "non-scandal" districts that have not been discovered. The citizens living in individual districts may well have been aware of the inconsistencies. The populist rightwing FPÖ described observing inconsistencies in 97 districts (basically all districts except the districts in the capital Vienna). The constitutional court may not have selected all individual districts with inconsistencies.

Fifthly, the districts include many municipalities. It is possible that voters did not participate or punished individual candidates in the second ballot in individual municipalities that have been prone to inconsistencies, but our data measured at the district level do not shed light on these individual municipalities. There is, however, no data on postal voting available at the municipal level and hence we cannot examine effects on overall voter turnout, candidate vote shares and postal voting *within* districts and across heterogeneous municipalities.

Sixthly, Austrian voters may want to signal that Austria is certainly not a banana republic and enjoys stable political institutions. In the autumn of 2016, many observers were surprised by Donald Trump winning the US elections. There was concern about political stability in industrialized countries. Very similar levels of participation in the second ballot of the Austrian presidential elections in scandal and non-scandal districts and not voting for the populist rightwing candidate Norbert Hofer sent a signal of political stability in Austria. Conservative voters also did not want to elect a populist rightwing candidate and support by conservative voters for the Green candidate Alexander van der Bellen increased in the repeated ballot compared to the regular ballot.

7. Conclusion

Trust in electoral institutions is important to maintaining stable political institutions. For example, societies that place high levels of trust in politicians and the political system and display generalised trust are far less corrupt than societies with low trust (Seligson 2002, Anderson and Tverdova 2003, Chang and Chu 2006, Morris and Klesner 2010, Richey 2010).[18] Local inconsistencies in the first ballot of the Austrian presidential elections 2016 does not seem to have eroded political trust in scandal districts. We examined whether the inconsistencies in scandal districts gave rise to lower voter turnout, more invalid votes and postal votes, and more votes for the populist rightwing FPÖ candidate. We do not find any evidence of this and propose six explanations for why the inconsistencies did not seem to influence citizens' voting behaviour. Most importantly, the low probability of manipulations, mutual recriminations among all layers of government, and the small size of districts may have prevented distrust from coming into play.

We cannot address whether the *global* level of trust eroded. The share of voters using postal voting declined from 16.7% to 13.3%, but voter turnout increased slightly from 73.1% in the first ballot to 74.7% in the repeated second ballot. Some voters may have lost trust in postal voting (an individual electoral institution), but not in participating in elections and democratic institutions in general. One avenue for future research is to disentangle the effects of global and local scandals on voter participation.

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[18] Trust has also been shown to be correlated with, for example, income equality and education (Knack and Keefer 1997). On social trust – as measured by the degree to which people believe that strangers can be trusted – and governance see Bjørnskov (2010): social trust was positively associated with economic-judicial governance, but has not been shown to be associated with electoral institutions.

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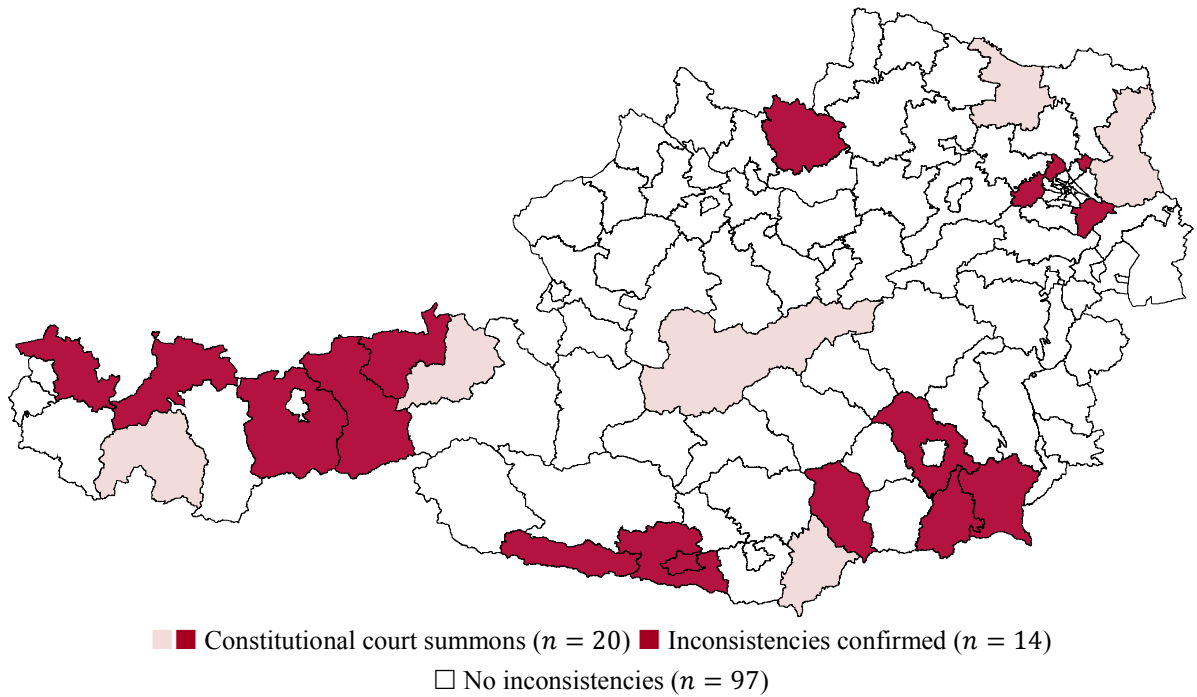
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FIGURE 1. SCANDAL DISTRICTS

Constitutional court summons ($n = 20$) ■ Inconsistencies confirmed ($n = 14$)
□ No inconsistencies ($n = 97$)

Notes: The figure shows districts which were subject to constitutional court summons (light red), and districts which were subject to constitutional court summons *and* inconsistencies were confirmed (dark red). White colored are non-scandal districts. Note that the scandal district of Wien-Umgebung (which is located in the North-East surrounding Vienna) has four regions.

FIGURE 2. TRENDS OF OUTCOME VARIABLES

Constitutional court summon
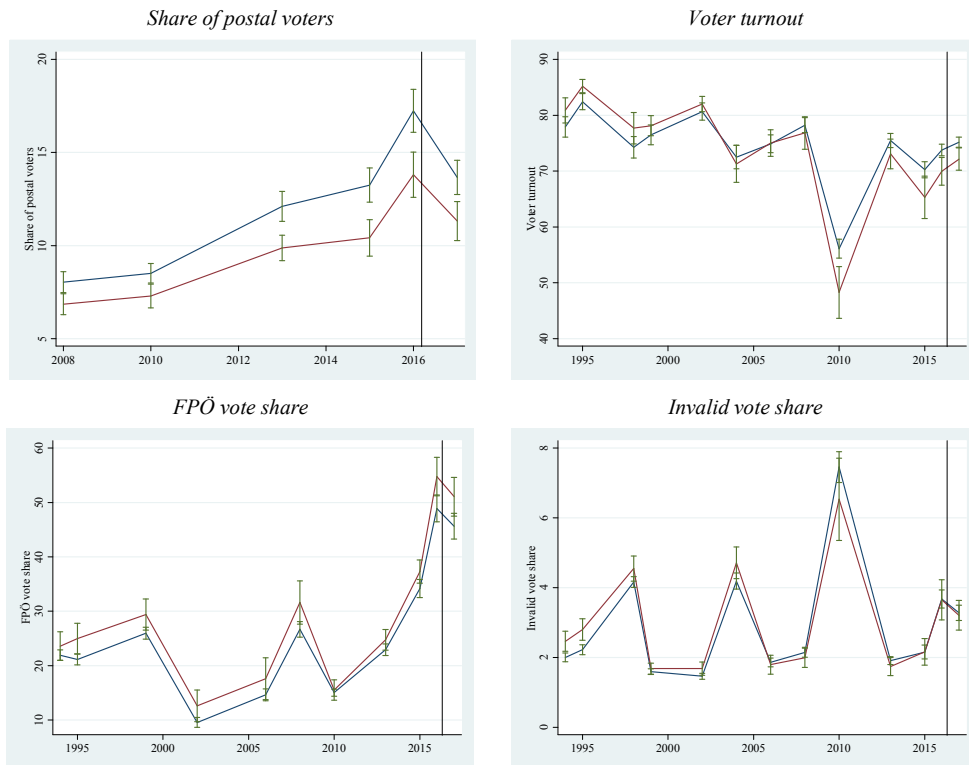
Share of postal voters

Voter turnout

FPÖ vote share

Invalid vote share

Inconsistencies confirmed

Share of postal voters

Voter turnout

FPÖ vote share

Invalid vote share
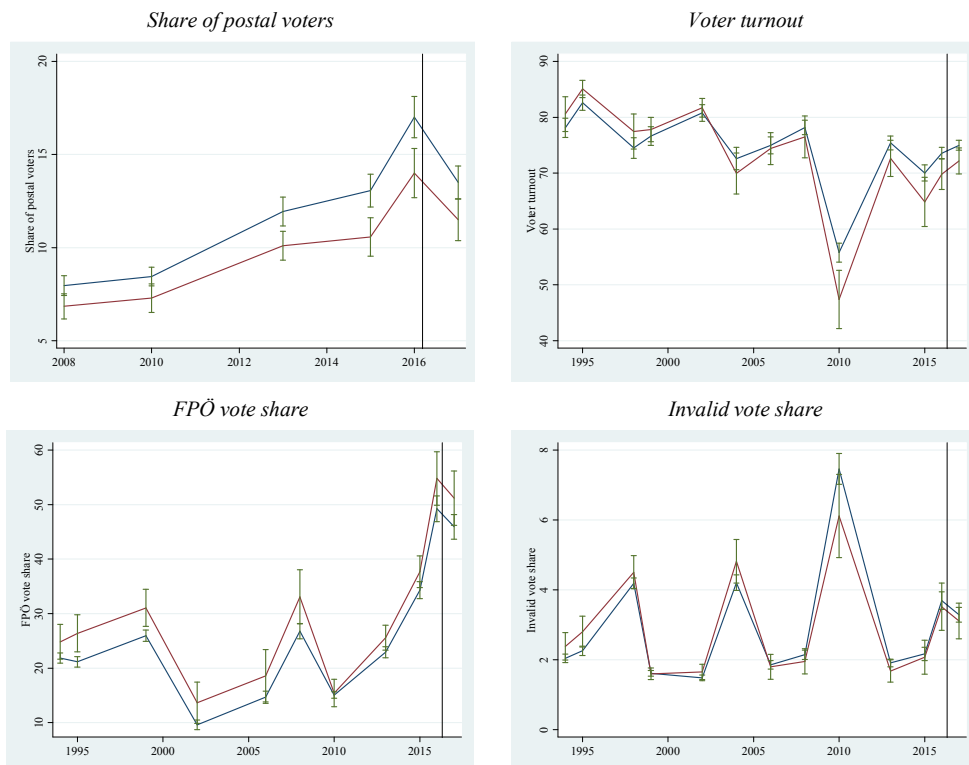
■ Non-scandal district (= 0) ■ Scandal district (= 1)

Notes: The figure shows election outcomes in districts which were subject to constitutional court summons (upper panel, $n = 20$), and districts where inconsistencies were confirmed (lower panel, $n = 14$). Vertical lines represent the timing of the scandal. Total number of districts: 117. 1998, 2004, 2010, 2016 (two rounds): Presidential elections. 1994, 1995, 1999, 2002, 2006, 2008, 2013: Parliamentary elections.

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TABLE 1. DESCRIPTIVE STATISTICS

	Obs.	Mean	Std. Dev.	Min	Max	Definition
	(1)	(2)	(3)	(4)	(5)	(6)
Outcomes before scandal						
Share of postal votes	117	16.66	5.47	9.85	37.77	Postal votes per total votes
Voter turnout	117	73.14	5.30	62.12	82.00	Total votes per electorate
FPÖ vote share	117	49.92	11.84	19.01	69.56	FPÖ votes per valid votes
Invalid vote share	117	3.67	1.27	1.64	6.35	Invalid votes per total votes
Outcomes after scandal						
Share of postal votes	117	13.26	4.36	8.02	32.54	Postal votes per total votes
Voter turnout	117	74.66	4.56	65.20	82.21	Total votes per electorate
FPÖ vote share	117	46.55	11.31	17.63	66.68	FPÖ votes per valid votes
Invalid vote share	117	3.27	1.06	1.41	5.63	Invalid votes per total votes
Scandal dummies						
Constitutional court summon	234	0.17	0.38	0	1	District subject to court cummons
Inconsistencies confirmed	234	0.12	0.33	0	1	District with confirmed inconsistencies
Controls						
Electorate (log)	234	10.73	0.65	7.34	12.20	Electorate (log)
Unemployment rate	234	3.79	1.81	1.09	8.99	Unemployed share of population
Rainfall	234	0.02	0.10	0.00	1.00	Rainfall in mm per cm² on election day
Opening hours of polling stations	234	6.90	2.10	2.63	10.00	Opening hours of polling stations
Cross-section controls						
Population (log)	117	11.03	0.67	7.56	12.54	Total population (log)
Pop. share of female (2016)	117	50.85	0.80	49.26	53.69	Share of female population
Pop. share of foreigners (2016)	117	13.40	8.87	2.21	40.55	Share of non-Austrian population
Pop. share of pop. > 75 (2016)	117	9.25	1.74	5.64	14.02	Share of population > 75 years old
Pop. share of low educated (2013)	117	23.63	3.94	12.67	32.34	Share of low educated population
Pop. share of agriculture (2013)	117	2.11	1.72	0.07	7.67	Population share associated with agriculture
Pop. share of industry (2013)	117	11.96	4.23	2.36	20.08	Population share associated with industry
Pop. share of employees (2013)	117	45.23	2.16	33.96	48.54	Population share of employees
Pop. share of self-employed (2013)	117	6.30	1.74	2.86	14.42	Population share of self-employed

Notes: The table presents the descriptive statistics of the dataset.

TABLE 2. PRE-SCANDAL CHARACTERISTICS

	Full dataset				
	Mean			Mean difference to Non-scandal	
	Non-scandal	Constitutional court summon	Inconsistencies confirmed	Constitutional court summon	Inconsistencies confirmed
	(1)	(2)	(3)	(4)=(1)–(2)	(5)=(1)–(3)
Controls					
Electorate (log)	10.70	10.90	10.97	-0.20	-0.27
Unemployment rate	3.74	3.23	2.86	0.51	0.88*
Rainfall	0.03	0.05	0.07	-0.02	-0.04
Opening hours of polling stations	7.14	5.92	6.01	1.22**	1.13*
Cross-section controls					
Population (log)	11.00	11.17	11.23	-0.16	-0.23
Pop. share of female (2016)	50.89	50.64	50.62	0.25	0.27
Pop. share of foreigners (2016)	14.06	10.23	10.50	3.84*	3.56
Pop. share of pop. > 75 (2016)	9.22	9.36	9.19	-0.13	0.04
Pop. share of low educated (2013)	23.61	23.70	23.70	-0.09	-0.09
Pop. share of agriculture (2013)	2.00	2.60	2.52	-0.60	-0.52
Pop. share of industry (2013)	11.72	13.16	13.63	-1.45	-1.92
Pop. share of employees (2013)	45.22	45.26	45.61	-0.04	-0.39
Pop. share of self-employed (2013)	6.22	6.73	6.64	-0.51	-0.43
	Vienna excluded				
	Mean			Mean difference to Non-scandal	
	Non-scandal	Constitutional court summon	Inconsistencies confirmed	Constitutional court summon	Inconsistencies confirmed
	(1)	(2)	(3)	(4)=(1)–(2)	(5)=(1)–(3)
Controls					
Electorate (log)	10.71	10.90	10.97	-0.19	-0.25
Unemployment rate	2.97	3.23	2.86	-0.26	0.11
Rainfall	0.04	0.05	0.07	-0.01	-0.03
Opening hours of polling stations	6.25	5.92	6.01	0.33	0.24
Cross-section controls					
Population (log)	10.97	11.17	11.23	-0.20	-0.27
Pop. share of female (2016)	50.69	50.64	50.62	0.05	0.07
Pop. share of foreigners (2016)	9.73	10.23	10.50	-0.49	-0.76
Pop. share of pop. > 75 (2016)	9.73	9.36	9.19	0.37	0.55
Pop. share of low educated (2013)	24.28	23.70	23.70	0.58	0.58
Pop. share of agriculture (2013)	2.59	2.60	2.52	-0.02	0.06
Pop. share of industry (2013)	13.54	13.16	13.63	0.38	-0.09
Pop. share of employees (2013)	45.35	45.26	45.61	0.09	-0.26
Pop. share of self-employed (2013)	6.33	6.73	6.64	-0.40	-0.32

Notes: The table show mean t-tests on pre-scandal differences (columns (4) and (5)).

TABLE 3. BASELINE RESULTS

	No controls							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inconsistencies	-3.438***	-3.822***	5.841***	-0.026	-3.017***	-3.757***	5.562**	-0.178
	(0.817)	(1.271)	(2.082)	(0.301)	(0.818)	(1.343)	(2.506)	(0.330)
Repeat	-3.582***	1.390**	-3.297*	-0.399**	-3.517***	1.412**	-3.330**	-0.406**
	(0.747)	(0.690)	(1.735)	(0.172)	(0.719)	(0.685)	(1.653)	(0.166)
Repeat × Inconsistencies	1.091	0.791	-0.403	-0.042	1.013	0.949	-0.299	-0.002
	(1.062)	(1.646)	(2.930)	(0.378)	(1.061)	(1.767)	(3.546)	(0.417)
Constant	17.245***	73.789***	48.918***	3.677***	17.018***	73.585***	49.251***	3.693***
	(0.583)	(0.519)	(1.256)	(0.131)	(0.561)	(0.517)	(1.197)	(0.127)
Obs.	234	234	234	234	234	234	234	234
Adj. R²	0.152	0.091	0.054	0.030	0.132	0.070	0.044	0.032
	Controls included							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Inconsistencies	-1.722*	-3.762***	1.046	-0.322	-1.447	-4.379***	1.125	-0.537
	(0.948)	(1.271)	(2.131)	(0.283)	(1.062)	(1.314)	(2.716)	(0.328)
Repeat	-3.496***	1.781***	-3.073**	-0.358***	-3.411***	1.851***	-3.165***	-0.360***
	(0.605)	(0.586)	(1.184)	(0.120)	(0.587)	(0.585)	(1.136)	(0.117)
Repeat × Inconsistencies	1.128	1.372	-0.275	0.041	1.029	1.783	0.276	0.125
	(1.300)	(1.537)	(3.046)	(0.343)	(1.459)	(1.589)	(3.875)	(0.406)
Electorate (log)	-0.921	-1.316***	-0.302	-0.262***	-0.934*	-1.286***	-0.327	-0.250***
	(0.562)	(0.330)	(1.098)	(0.082)	(0.563)	(0.333)	(1.101)	(0.083)
Unemployment rate	0.055	-1.641***	-0.130	-0.274***	0.020	-1.746***	-0.102	-0.286***
	(0.227)	(0.162)	(0.401)	(0.035)	(0.229)	(0.163)	(0.396)	(0.034)
Rainfall	0.962	0.584	11.786***	-0.228	1.069	1.022	11.695***	-0.164
	(2.907)	(1.535)	(3.712)	(0.781)	(2.925)	(1.651)	(3.800)	(0.754)
Opening hours of poll. stat.	1.240***	0.529***	-3.751***	-0.173***	1.283***	0.623***	-3.772***	-0.166***
	(0.230)	(0.191)	(0.389)	(0.039)	(0.227)	(0.185)	(0.379)	(0.038)
Constant	18.006***	90.215***	79.034***	8.747***	17.858***	89.487***	79.382***	8.626***
	(5.647)	(3.677)	(11.450)	(0.884)	(5.678)	(3.684)	(11.535)	(0.894)
Obs.	234	234	234	234	234	234	234	234
Adj. R²	0.412	0.356	0.518	0.489	0.408	0.356	0.518	0.496

Notes: Significance levels (robust standard errors in brackets): *** 0.01, ** 0.05, * 0.10.

TABLE 4. POSTAL VOTES AND BALLOT BOX VOTES

	Total voting							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	1.128	1.372	-0.275	0.041	1.029	1.783	0.276	0.125
	(1.300)	(1.537)	(3.046)	(0.343)	(1.459)	(1.589)	(3.875)	(0.406)
Obs.	234	234	234	234	234	234	234	234
Further controls	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R²	0.412	0.356	0.518	0.489	0.408	0.356	0.518	0.496

	Postal voting							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	–	1.019	-0.369	0.079**	–	1.011	0.265	0.088**
	–	(0.951)	(2.521)	(0.035)	–	(1.068)	(3.167)	(0.039)
Obs.	–	234	234	234	–	234	234	234
Further controls	–	YES	YES	YES	–	YES	YES	YES
Adj. R²	–	0.403	0.535	0.561	–	0.398	0.536	0.561

	Ballot box voting							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	–	0.353	-0.279	-0.037	–	0.772	0.273	0.037
	–	(1.705)	(3.059)	(0.329)	–	(1.819)	(3.880)	(0.392)
Obs.	–	234	234	234	–	234	234	234
Further controls	–	YES	YES	YES	–	YES	YES	YES
Adj. R²	–	0.384	0.518	0.476	–	0.387	0.518	0.483

Notes: Significance levels (robust standard errors in brackets): *** 0.01, ** 0.05, * 0.10.
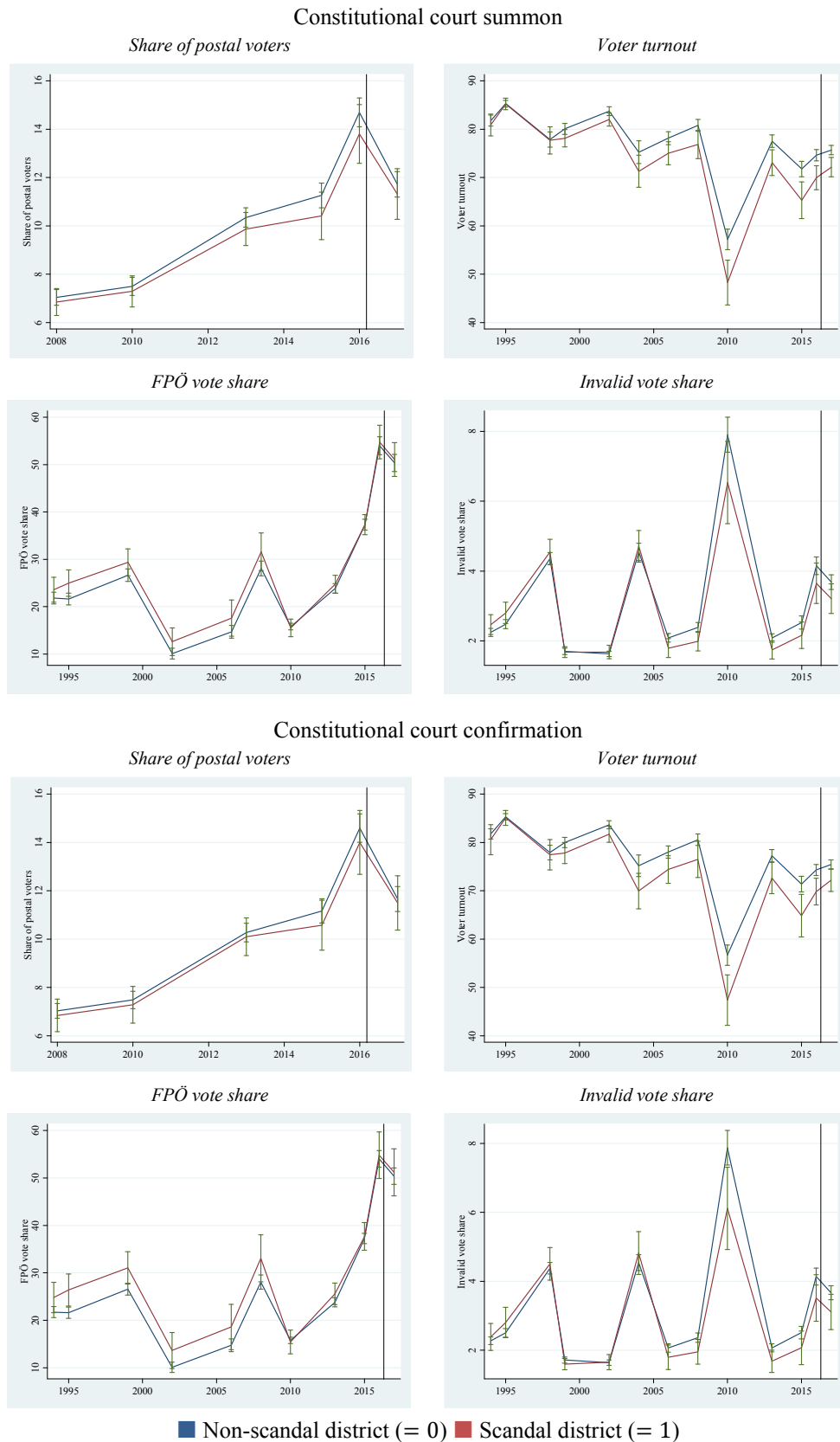
TABLE 5. ROBUSTNESS TESTS

	Vienna excluded							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	0.605	1.527	-0.077	0.113	0.702	1.927	0.311	0.192
	(0.803)	(1.590)	(2.775)	(0.335)	(0.837)	(1.663)	(3.496)	(0.374)
Obs.	188	188	188	188	188	188	188	188
Further controls	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R²	0.340	0.245	0.314	0.320	0.337	0.241	0.316	0.334
	Cross-section control variables included							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	1.124	1.161	-0.719	0.012	1.180	1.515	-0.641	0.064
	(0.991)	(0.997)	(1.980)	(0.226)	(1.202)	(0.972)	(2.198)	(0.259)
Obs.	234	234	234	234	234	234	234	234
Further controls	YES	YES	YES	YES	YES	YES	YES	YES
Cross-section controls	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R²	0.788	0.604	0.825	0.748	0.782	0.585	0.827	0.751
	Alphabetical pseudo treatment							
	Constitutional court summons				Inconsistencies confirmed			
	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share	Share of postal voters	Voter turnout	FPÖ vote share	Invalid vote share
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)
Repeat × Inconsistencies	0.840	-0.197	-0.475	0.012	0.679	-0.307	0.190	-0.035
	(1.625)	(2.050)	(3.840)	(0.439)	(1.299)	(1.677)	(2.977)	(0.374)
Obs.	234	234	234	234	234	234	234	234
Further controls	YES	YES	YES	YES	YES	YES	YES	YES
Adj. R²	0.406	0.314	0.527	0.516	0.410	0.304	0.533	0.496

Notes: Significance levels (robust standard errors in brackets): *** 0.01, ** 0.05, * 0.10.

Appendix

Constitutional court summon

Share of postal voters

Voter turnout

FPÖ vote share

Invalid vote share

Constitutional court confirmation

Share of postal voters

Voter turnout

FPÖ vote share

Invalid vote share

■ Non-scandal district (= 0) ■ Scandal district (= 1)

Notes: The figure shows election outcomes of districts (Vienna excluded) which were subject to constitutional court summons (upper panel, $n = 20$), and districts where inconsistencies were confirmed (lower panel, $n = 14$). Vertical lines represent the timing of the scandal. Total number of districts: 94. 1998, 2004, 2010, 2016 (two rounds): Presidential elections. 1994, 1995, 1999, 2002, 2006, 2008, 2013: Parliamentary elections.