



# Exit polls, turnout, and bandwagon voting: Evidence from a natural experiment



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## ABSTRACT

We exploit a voting reform in France to estimate the causal effect of exit poll information on turnout and bandwagon voting. Before the change in legislation, individuals in some French overseas territories voted after the election result had already been made public via exit poll information from mainland France. We estimate that knowing the exit poll information decreases voter turnout by about 11 percentage points. Our study is the first clean empirical design outside of the laboratory to demonstrate the effect of such knowledge on voter turnout. Furthermore, we find that exit poll information significantly increases bandwagon voting; that is, voters who choose to turn out are more likely to vote for the expected winner.

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## 1. Introduction

“A Californian plans to vote after work in what she believes to be a close presidential election. (She has little interest in the race for congressman for her district, although it is closer.) The day is rainy and as she approaches the polling place she sees a long line. On the radio she hears that one presidential candidate has a substantial lead in other states. She says why bother and turns her car around and drives home.” [Sudman \(1986, p. 332\)](#)

In August 2009, exit poll results for key regional elections in Germany were leaked on Twitter before voting ended. These polls showed that Chancellor Angela Merkel's conservative party had much less support than in previous elections. Wolfgang Bosbach, deputy parliamentary head of Merkel's Christian Union bloc, said that the leaked results “damaged democracy” and a spokesman for the pro-business Free Democrats, Merkel's preferred coalition partner, commented that the leaks were “unacceptable.” In addition, such reporting is against the German law with a fine of up to 50,000 euros, so German election officials immediately began to investigate whether the Twitter messages violated the law.<sup>1</sup> The German

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<sup>1</sup> See [Carter \(2009\)](#).

case is not unusual: a survey of 66 countries worldwide finds that of the 59 that permit exit polls during an election, 41 prohibit publication of the results until after all voting has concluded (see [Spangenberg, 2003](#)).

Yet in recent 21st century elections, incidents similar to the 2009 Twitter controversy in Germany abound. In 2007, the websites of several Swiss and Belgian newspapers crashed when French citizens attempted to access exit poll results during an election, and in the 2012 French presidential election, results were also available online while voting was still in progress.<sup>2</sup> Countries with multiple time zones such as France, the United States, Canada or Russia, face particular difficulties, as voting takes place at different times in different regions.<sup>3</sup> The notorious reporting on Floridian exit poll results in the 2000 United States presidential election occurred while voters in the western part of the state and the rest of the nation were still voting. In 2004 leaked US presidential exit poll results were commonly discussed among voters while east coast voters were still going to the ballot booths.<sup>4</sup> With the proliferation of Twitter, Facebook, and other social media worldwide, the ability of governments to control and limit leaked exit polls both inside and outside their countries is becoming increasingly difficult.

Beyond the increasing ability of later voters to find out results of early voting on election day, many nations are also experimenting with “convenience voting,” in which voters can easily cast their ballots days or even weeks in advance of the official election day. As [Gronke et al. \(2007\)](#) report, almost half of the democratic nations in the EPIC Project database allow individuals to vote prior to the designated election day, with over a third allowing all voters this privilege. Convenience voting is particularly popular in the United States where voting by mail or at the mall is commonplace; 25% of U.S. voters in the 2006 elections used convenience voting methods (see [Gronke et al., 2007](#)).

In this paper, we make use of a unique natural experiment to consider whether the behavior of later voters is affected by exit poll information about earlier voter choices. Specifically, we exploit a 2005 voting reform in France to estimate the causal effect of exit poll information on turnout and bandwagon (or underdog) voting.<sup>5</sup> Before the change in legislation, individuals in French western overseas territories voted after the mainland election results were already known via exit polls. Since 2005, the western overseas territories vote on the day before the mainland and hence those voters do not have any knowledge of the choices on the mainland or the eastern territories. This reform allows us to use a difference-in-difference strategy. It creates an exogenous variation in information for a well identified group of voters and provides therefore the setting for a natural experiment to study the effect of exit poll information on voters' behavior. Such an approach has two advantages. First, relative to existing studies on the West Coast effect, our natural experimental setting allows us to eliminate many of the possible caveats in the analysis of the causal effect of information by providing a counterfactual situation (same constituencies with and without exit poll information). Second, our study does not suffer from the possible concern about the ecological validity of the results, a criticism often raised about laboratory experiments.

Using this voting reform to study the effect of exit poll information, we find evidence that the public knowledge about such polls not only decreases turnout by about 11 percentage points, but also increases bandwagon voting. We therefore conclude that exit polls can indeed have consequential effects on voter behavior and that the advent of social media reporting on exit poll information may fundamentally change the democratic process in many countries where such information was previously unavailable.

Despite the growing tendency for voters to learn exit poll results while an election is ongoing, previous observational data-based research on the effects of such information on voting behavior is limited and inconclusive. One possible natural setting for exploring this issue is the so-called West Coast effect in the U.S.; that is, the release of early East Coast election returns before the polls close on the West Coast due to the fact that the presidential election takes place in three different continental time zones. The debate over this effect emerged in the early 1960s after the introduction of sophisticated computer models, improved survey techniques for predicting election results, and rapid access to media information. The first set of related studies, however, which explored the 1964 presidential election, showed barely any West Coast effect (see for example [McAllister and Studlar, 1991](#)).

A second set of major studies focused on the 1980 election,<sup>6</sup> in which pre-election polls indicated a close election between Reagan and Carter, but Carter conceded defeat even before the polls closed in the west.<sup>7</sup> However, although these researchers used a richer set of data (aggregated data on various elections, regional, data or congressional districts, or better

<sup>2</sup> See [Sayare \(2012\)](#).

<sup>3</sup> The United States does not prohibit the reporting of exit poll results, so elections are often publicly decided before voters in Alaska and Hawaii have voted, and sometimes while voting in California and other western states is still ongoing. In Canada polls close at the same time across the country to reduce the potential of leaked results. In India, although the country is not subject to the same time zone issues, voting is conducted at different times in different regions for security reasons and reporting early results is illegal.

<sup>4</sup> See [Best and Krueger \(2012\)](#) for a review.

<sup>5</sup> By bandwagon voting, we mean that voters are more likely to vote for the predicted winner in an election and by underdog voting we mean the opposite.

<sup>6</sup> It should be noted that other elections between 1964 and 1980 have also been explicitly analyzed. For example, [Tuchman and Coffin \(1971\)](#) explore the influence of election night television broadcasts on the close 1968 election but find no evidence that it affected voting.

<sup>7</sup> NBC was also strongly criticized for declaring Ronald Reagan the President of the United States at 8:15PM eastern standard time based on its own computer projections: “Articles, reports, and letters were written describing how voting lines outside polling places disappeared, how turnout decreased over prior years, and how voters stayed home after it became apparent that their favored candidates had already won or lost” ([Leonardo, 1983, p. 297](#)). Criticism also emerged that national voter turnout decreased to 53.95% (the lowest rate since 1948, see [Dubois, 1983](#)). All this criticism led to a congressional hearing, journalistic commentary, private studies, and a task force report, as well as the proposition of remedial bills to regulate either poll closing times or the timing of network election predictions, but no action was taken once interest began to wane ([Leonardo, 1983; Carpini, 1984](#)).

survey data), they produced mixed results. For example, [Jackson \(1983\)](#) finds some evidence that media coverage of exit polls in the 1980 U.S. presidential election did lead to a reduction in turnout. However, the survey data used in his and a number of similar studies of the 2000 election have been strongly criticized as unreliable (see the review in [Mason et al., 2001](#)).<sup>8</sup>

In an interesting recent study, [Meredith and Malhotra \(2011\)](#) examine the effects of convenience voting on vote shares in the 2008 California presidential primary. They exploit the fact that some small precincts are forced by the state to use vote-by-mail procedures for all ballots while in other precincts voters can choose when to participate. They find significant differences in vote shares of the candidates, suggesting information effects on voter choices in precincts where vote-by-mail was optional. In particular, they find that in precincts where all voters were forced to vote-by-mail the vote shares for candidates who had withdrawn from the election before election day were significantly higher than in precincts where convenience voting was optional. Moreover, the vote share for Obama (the front-runner at the time of the California election) was lower in the vote-by-mail precincts as well, suggestive of bandwagon voting.

A few laboratory studies suggest that information about early voting can have significant effects on voter behavior. These studies indicate that later voters' choices appear to be influenced by information about early voting. More specifically, later voters who learn the choices of earlier voters make different turnout decisions (see [Battaglini et al., 2007](#)) or vote for different candidates (see [Hung and Plott, 2001](#); [Morton and Williams, 1999, 2001](#)) than those made by voters with the same induced preferences but without that information.

Our results show that as the experimental evidence suggests, later voters are indeed influenced by early voting results. Therefore, the advent of increasing social media reports of election results through exit polls can lead to fundamental changes in the way voters behave. Such reporting may thus have important consequences for how democracy works in many countries. Most obviously, candidates and political parties may have incentives to manipulate the reported results in order to seek advantages. But even if the results are accurately reported, other serious effects might occur. For example, if later voters are less likely to participate or have a tendency to engage in bandwagon voting, then the candidates preferred by earlier voters may be more likely to win elections. To the extent that the timing of the voter participation decision is exogenous and depends on voter characteristics such as income, ethnicity or other factors that arguably affect voter preferences, then voters may be unequally represented even though their votes are theoretically equal. At the same time, to the extent that the timing of voting is endogenous, candidates and political parties will have an incentive to engage in strategic manipulation of the factors that influence whether individuals choose to participate, much like the strategic manipulation in the timing of presidential primaries in the United States.<sup>9</sup> Hence, given that the election process is fundamentally changing with social media reporting of exit poll results, the issue of whether later voters' choices are influenced is an important empirical question for many countries in which such information has historically not been available.<sup>10</sup>

In the next section, we review the relevant theoretical literature on information and voting behavior. [Section 3](#) then outlines our empirical research design and the natural experiment, [Section 4](#) presents our results regarding the effect on turnout, [Section 5](#) addresses robustness concerns and [Section 6](#) presents the results on the Bandwagon effect. We conclude and discuss the implications of our analysis in [Section 7](#).

## 2. The role of exit poll information in voting choices: theory and experimental evidence

When individuals know the results from earlier voting via leaked exit polls, voting becomes sequential in nature. In the case of complete information about the choices before them but incomplete information about other participants' preferences learning, the results of earlier decisions simply provide later participants with information about the likelihood that their vote may be pivotal. It is thus straightforward to show that if voting is costly (even if the costs are minimal), learning that one's own decision will not affect the outcome implies that a rational individual should abstain. If however, a voter learns instead that the election is extremely close and there is a high probability of their vote being pivotal, then later voters may actually participate at greater rates than they would if voting were simultaneous and they had less precise information about other voters' choices.

If voters have private information about the choices before them in an election, then early voting results not only reveal the extent to which individual choices may be pivotal but may also provide later voters with insights into the information held by early voters about the choices. As shown by [Battaglini \(2005\)](#), when voting is costly the set of equilibria in sequential private information voting games are disjoint from those in which voting is simultaneous. That is, later voters' choices will be influenced by the results of early voting. [Battaglini et al. \(2007\)](#) also find support for these qualitative theoretical predictions in laboratory elections using a three-voter game; in particular, they find significant evidence of strategic abstention by later voters. Other results, however, are at variance with theory – they find that early voters tend to participate more than theoretically predicted, whereas later voters abstain more, sometimes even when their votes could be pivotal. They conclude that, as predicted, although sequential voting tends to be more informationally and economically efficient

<sup>8</sup> Frankovic, Kathleen, "Part Three: Historical Perspective," in Linda Mason, Kathleen Frankovic, and Kathleen Hall Jamieson, CBS News Coverage of Election Night 2000: Investigation, Analysis, Recommendations, January 2001.

<sup>9</sup> See [Morton and Williams \(1999\)](#) for a discussion of these manipulations.

<sup>10</sup> [Thompson \(2004\)](#) presents additional normative and philosophical arguments against the revelation of such information to later voters.

than simultaneous voting, later voters benefit at the expense of early voters, so there is a cost in terms of equity. Nevertheless, they find no evidence of later voters ignoring their private information and engaging in bandwagon or underdog voting.

Callander (2007) considers the comparison of simultaneous and sequential voting under asymmetric information when voters receive utility from conforming to the majority (voting for the winner) independent of the utility they derive from whether the winner is their own best choice. Specifically, he derives an equilibrium under sequential voting in which voters engage in bandwagon voting (voting for the leading candidate) even though their private information may suggest that the leading candidate is not their own best choice. He finds that such bandwagon voting may occur even when later voters' choices are not pivotal and the outcome is already decided (because of the additional utility voters receive from the act of voting for the winning candidate). This argument is supported by earlier work by Hung and Plott (2001), which provides experimental evidence of conformity voting when subjects are rewarded for doing so. Presumably, if voters similarly receive utility from voting for an underdog candidate (or are rewarded for doing so in an experiment), then later voters may also engage in underdog voting even when they previously believed the leading candidate to be their own best choice.

Hence, both theory and the experimental evidence suggest that if the act of voting is costly, when later voters learn from exit poll information that their decision is unlikely to be pivotal, they are more likely to abstain. If, however, they receive utility from the act of voting for either the winner or the underdog (independent of whether the winner is their own best choice), then such exit poll information may lead them to engage in either bandwagon or underdog voting, respectively.

Our natural experiment allows us to evaluate the extent to which exit poll information affects the turnout of later voters and whether later voters are more likely to engage in either bandwagon or underdog voting.

### 3. The natural experiment: institutional background and empirical strategy

#### 3.1. The French electoral system

France has a semi-presidential system<sup>11</sup> through which French president is directly elected by the citizens via a two-round runoff system. In the first round a large number of candidates can participate.<sup>12</sup> If one candidate receives more than 50% of the votes in the first round he or she is declared the winner. Such an immediate victory, however, has only happened once since the beginning of the Fifth Republic, in 1958.<sup>13</sup> Usually, the two candidates that receive the most votes participate in a second round to determine the winner.

This two-round runoff system model is also used in most other elections in France with some variation. The French parliamentary elections differ slightly in the sense that the two-round runoff elections within each constituency allow more than two candidates to participate in the second round.<sup>14</sup> In practice, however, although a few second rounds are disputed by three candidates, most involve only two.

Balloting traditionally takes place on a Sunday. French electoral law prohibits exit poll publication until the close of voting in mainland France (Bale, 2002) and bans publication, broadcasting and commenting on opinion polls for the day before and the day of the election (Saturday and Sunday). The electoral law also stipulates that the official campaign has to stop for these last 2 days.<sup>15</sup>

When a French presidential election is held, it is always the only contest on the ballot, which stands in contrast to, for example, U.S. elections, in which ballots include local, congressional and senatorial posts, and even local propositions and initiatives. The French case thus allows us to measure turnout for presidential elections only, meaning that the turnout measured is not confounded by effects from other elections.

During the day of the election, the release of exit polls is therefore not allowed until after the closure of the last polling booth in mainland France, on Sunday at 8:00PM CET.<sup>16</sup> At exactly 8:00PM, TV channels release highly precise early estimations of the final results. These are based on large exit polls and on the first results from the majority of polling booths, which close at 6:00PM.<sup>17</sup>

<sup>11</sup> The French system has been called "semi-presidential" in political science since Duverger (1970). The motivation for such a label was that in situations where the President's party does not lead the majority coalition in the Parliament, the Prime Minister has de facto the leadership over all domestic policies.

<sup>12</sup> To be eligible to run, a candidate must gather 500 signatures from local politicians such as town councilors.

<sup>13</sup> This presidential election was the first one of the Fifth Republic. De Gaulle had overseen the design of the new constitution and was seen as having saved France from a potential military coup by the many army generals opposed to the process of Algerian independence. In these dramatic circumstances, De Gaulle won the election with more than 78% of the votes.

<sup>14</sup> In order to participate in the second round, candidates must gather a minimum proportion of registered voters in the constituency, currently 12.5%. In presidential elections, however, there are always exactly two candidates in the second round.

<sup>15</sup> The law was initially voted in 1977. At that time, the publication and broadcasting of opinion polls were banned for 1 week before each of the two rounds of voting. It was changed in 2002 to limit this interdiction to the last 2 days before the results.

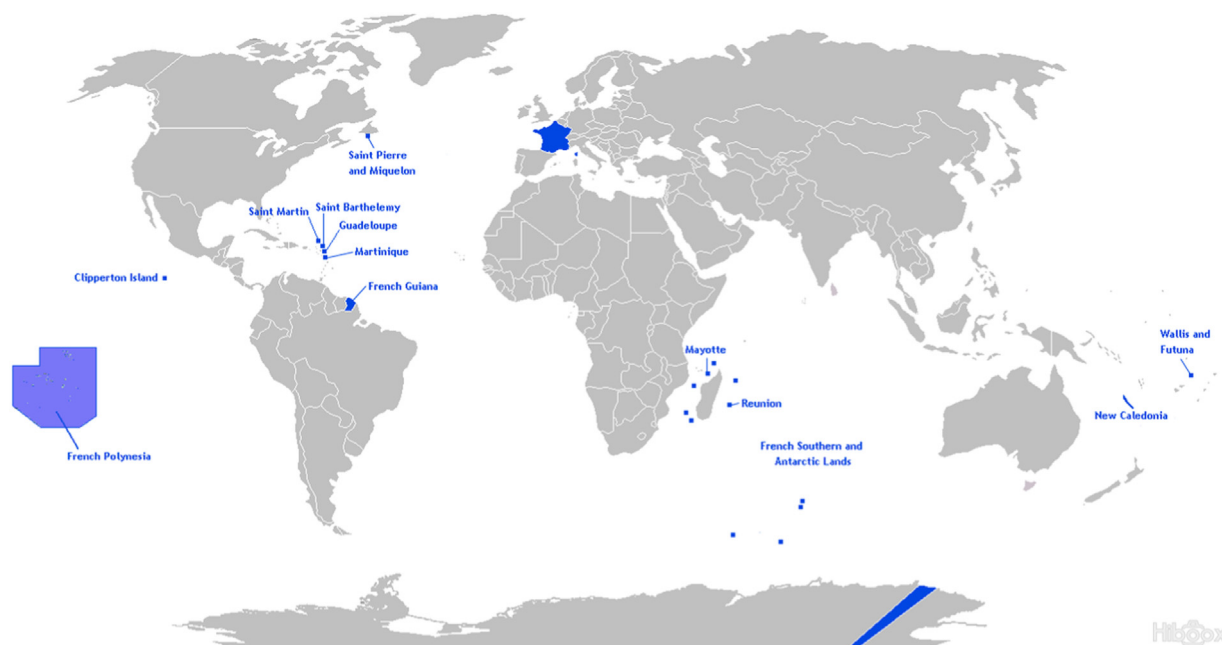
<sup>16</sup> Central European time (CET) is used in most parts of the European Union and is 1 h ahead of Coordinated Universal Time (UTC). Polling booths in main urban areas close at 8:00PM while most polling booths in the countryside close at 6:00PM.

<sup>17</sup> Given that all votes are aggregated at the national level, the law of large numbers helps these estimations to be precise. Unlike the case in the U.K. or U.S., the result does not depend on some swing constituencies/states from which the final result can take time to emerge. In fact, the final difference between early estimations and the final official results is typically less than 0.3 percentage points for any given candidate, see Table 1.

**Table 1**

Forecasts as presented by France's main public TV channel France 2 at 8:00PM. All numbers are in percentage points. "Pivotal candidates" refers to the first and second candidate in the second round and the second and the third candidate in the first round.

Year	Round	First candidate			Second candidate			Third candidate			Diff pivotal candidates		
		Forecast	Result	Diff	Forecast	Result	Diff	Forecast	Result	Diff	Forecast	Result	Diff
1981	1	28.3	28.32	−0.02	25.2	25.85	−0.65	17.9	18	−0.1	7.3	7.85	0.55
	2	51.7	51.76	−0.06	48.3	48.24	0.06	–	–	–	3.4	3.52	0.12
1988	1	34.4	34.1	0.30	19.5	19.94	−0.44	16.5	16.55	−0.05	3	3.39	0.39
	2	53.9	54.02	−0.12	46.1	45.98	0.12	–	–	–	7.8	8.04	0.24
1995	1	23.4	23.3	0.1	20	20.84	−0.84	18.5	18.58	−0.08	1.5	2.26	0.76
	2	52	52.64	−0.64	48	47.36	0.64	–	–	–	4	5.28	1.28
2002	1	20	19.88	0.121	17	16.86	0.14	16	16.18	−0.18	1	0.68	−0.32
	2	82.1	82.21	−0.11	17.9	17.79	0.11	–	–	–	64.2	64.42	0.22
2007	1	29.6	31.18	−1.58	25.1	25.87	−0.77	18.7	18.57	0.13	6.4	7.3	0.90
	2	53	53.06	−0.06	47	46.94	0.06	–	–	–	6	6.12	0.12
2012	1	28.4	28.63	−0.23	25.5	27.18	−1.68	20	17.9	2.1	5.5	9.28	3.78
	2	51.9	51.64	0.26	48.1	48.36	−0.26	–	–	–	3.80	3.28	−0.52



**Fig. 1.** France and its overseas territories (© Hoshie).

To demonstrate the quality of these early estimates, we collected the 8:00PM CET forecasts for each candidate from the main public channel (France 2) at every election in our sample. Table 1 lists these forecasts, together with the actual results and the differences between the two. In the second round of the 1981 election, for example, the difference between the final result and the 8:00PM predictions is the same for both the first candidate and the runner-up at 0.06 percentage points, with the forecasted difference between the two candidates at 3.4% and the actual difference at 3.52%. As Table 1 clearly shows, exit poll forecasts are extremely accurate, the final ranking is always the one predicted by the forecast and the proportion of votes is often similar down to a few decimal points.

### 3.2. The 2005 electoral reform

Fig. 1 provides a map of France and its overseas territories (hereafter OST), which are spread across the globe with time differences ranging from +12 h UTC<sup>18</sup> (Wallis and Futuna) to −10 h UTC (French Polynesia). People living in the OST are

<sup>18</sup> Coordinated Universal Time (UTC) is the international time standard. It closely resembles the "Greenwich Mean Time" with the latter being outdated.



fully fledged citizens of the French Republic and therefore participate in the French presidential election in the same way as French citizens on the mainland.<sup>19</sup>

Before 2005, French elections were held on Sundays, meaning that they began in the most eastern parts, New Caledonia, Wallis and Futuna, and then moved progressively across the more westerly territories as the opening time for polling booths arrived (typically between 8:00AM and 9:00AM). As a result, the territories located to the west of the French mainland (e.g. the Caribbean, Guiana) and in the Pacific (French Polynesia) voted partially or completely after mainland France.

Because the mainland accounts for approximately 96% of the total French population,<sup>20</sup> in national ballots like presidential elections, the result on the mainland almost certainly determines the overall election result. This setting is therefore different from that of the U.S., where the number of electoral votes determined by California can empirically decide the outcome of a close contest.

Table 1 shows that in most cases the result is fully determined once the mainland results are known. Voters in overseas territories west of mainland France only represent around 1.5% of the French electorate. The predicted difference between the two “pivotal candidates” (the first versus the runner-up in the second round and the second versus the third candidate in the first round) given by exit polls is almost always above 1.5%.

To illustrate this point further, Table 2 shows the number of votes by which the runner-up was ahead of the third candidate in mainland France in the first round and the vote edge of the first versus the second candidate in the second round, respectively. It also gives the number of registered voters in the western OSTs and the corresponding difference between both measures for each election in our sample. It is clear that there are only two elections in which it would have been mathematically possible for the western OST to make a difference: the first rounds of 1995 and 2002. In 1995, the difference is so large that changing the result would have required at least 95% of the registered voters in the west to vote for the third candidate and the remaining 5% not to vote for the second candidate. Such a scenario seems extremely unlikely. Moreover, in this election, both candidates were moderate conservatives, giving limited incentives for voters to change the outcome. The case is somewhat different, however, for the first round of 2002. Here if the difference between the second and third candidates in OST voting was approximately 33% of registered voters, then the outcome of the election would be affected by choices made in the OST. Although such a figure still seems quite unlikely, it is at least not completely impossible with evident consequences for voter turnout. In Section 5, therefore, we conduct robustness checks to show that our estimates remain unchanged by this event.

As a result of this geographical distribution, before 2005 voters in the territories to the west of mainland France had access to information about the presidential election results while voting booths were still open, while French Polynesia and the territories off the American continent had precise information on election results by 9:00AM and 2:00PM, respectively. Hence, most voters probably knew who would win the election before voting. In fact, in 2002, the defeated presidential candidate Lionel Jospin resigned from his office before people in the western OST had even voted.

In 2002 the Constitutional Council suggested changing the voting order for western OST citizens' so that the balloting would take place before the results in mainland France were made known. The Constitutional Council is the highest constitutional authority in France and its role is to ensure that the principles of the constitution are respected by the laws. In March 2003, the government enacted a constitutional law (2003-276) that changed the French Constitution by removing the word “Sunday” in the article 7 of the Constitution relative to elections. This change allowed the western territories to switch their election day to Saturday. Answering to a question in the Senate about the day of elections in western overseas territories, the Overseas Ministry indicated that this change in constitutional law intended to remedy the fact that OST voters had access to the mainland results and that this situation was likely to “lead to higher level of abstention and possibly influence the sincerity of the voting choices”.<sup>21</sup> Following this law in 2003, it was decided to change the voting day to Saturday in all territories to the west of the mainland: French Polynesia, St. Pierre and Miquelon, Guadeloupe, French Guiana and Martinique. This reform became effective in 2005.

### 3.3. Other reforms and relevant events

Any analysis of the effect of a policy change over a given period needs to ensure that the observed changes in the variable of interest cannot be explained by other policy changes or events happening during the same period. In our case, four potentially relevant events happened over the period studied that warrant discussion. First, in 2002, the duration of the presidential mandate was reduced from seven to five years, which may have affected the overall turnout at the national level. There seems to be no reason, however, why it should affect turnout differently in the OST relative to the mainland, and it is a priori even less likely that it would affect turnout differently in the OST to the west and east of mainland France.

<sup>19</sup> The OST are divided into two categories: overseas departments (DOM) whose laws are identical to mainland France, and overseas territories (TOM) which are granted sufficient autonomy for local laws to differ from mainland France. French OST status and its relationship with mainland France change from time to time. For example, New Caledonia is an overseas territory that is expected to vote on an independence referendum over the next few years. Conversely, Mayotte, an island in the Comoros archipelagos, recently voted by referendum to abandon TOM status and become a DOM, making it a new French department with the same laws and regulations as mainland France.

<sup>20</sup> 2.7 million citizens in the OST versus 63.1 million in mainland France.

<sup>21</sup> Réponse du Ministère de l'outre-mer à la Question écrite no. 04510 de M. Emmanuel Hamel, Journal Officiel Sénat, 24/04/2003, p. 1421.

**Table 2**

Vote differences between the two pivotal candidates (2nd versus 3rd in first round, 1st versus 2nd in second round) on the mainland and in the east compared to the number of registered voters in the western OST. Numbers are in 1000 votes.

Year	Round	Vote edge on mainland	Registered voters in western OST	Difference
1981	1	2293	470	1822
	2	1247	470	776
1988	1	995	556	439
	2	2405	556	1849
1995	1	644	675	– 30
	2	1486	675	810
2002	1	254	756	– 502
	2	19,605	754	18,851
2007	1	2520	826	1694
	2	2200	826	1374
2012	1	3232	869	2363
	2	1027	870	157

Second, 2002 saw the first candidate from an OST (Guiana) participating in the first round of the presidential election, which could have led to a higher turnout in Guiana in this specific election. We control for this concern by including a dummy variable indicating Guiana in the first round of 2002 in the estimations (see [Section 5.1](#)).

Third, 2002 was also the first year in which a candidate from the far-right reached the second round, an unexpected event that created a political shock in the country. As the majority of the population in the overseas territories is not ethnically white, they are likely to be averse to this party's political objectives. We address this concern more closely in [Section 5.2](#); we find no empirical evidence that this event affects our conclusions.

Finally, the last decade has seen the growth of the Internet, making access to information easier. Hence, in practice, early estimations of the election results are produced by polling companies as early as 5:00PM CET on the day of the election. In the 1980s, the laws preventing publication of early polls were easy to uphold because the only media able to report such early results would be punished severely for doing so. More recently, however, Belgian and Swiss newspapers have begun posting early estimations on their websites during election day but as they are based outside of France, even though French speaking, they are not bound to respect French electoral law. Unlike the early 2000s when access to the Internet was limited, by the 2007 and 2012 elections, the spread of online election result information after 5:00PM had grown substantially, leading to a debate about the usefulness of a law which could be barely enforced. We allow for different time trends in our estimations to control for such changes over time.

### 3.4. Data

Our primary data set comprises French presidential election results, especially turnout for first and second election rounds from 1981 onwards (1981, 1988, 1995, 2002, 2007, and 2012).<sup>22</sup> Although we organize the data at the departmental level to make group sizes as comparable as possible, the population sizes still vary from around 4000 in St. Pierre and Miquelon to about 1.8 million in the Nord department. We treat each OST as a department in the analysis.<sup>23</sup> There are four OSTs in the east and five in the west, which in addition to the 96 departments on the mainland comprise a total of 105 such departments in France. The departments we refer to as “treated” are French Polynesia, Saint Pierre and Miquelon, Guadeloupe, French Guiana, and Martinique.<sup>24</sup>

In order to conduct robustness checks, we use similar data from French parliamentary elections, taken directly from the French Ministry of Internal Affairs, which are also arranged at the departmental level and span a period from 1997 to 2012. In parliamentary elections – unlike presidential elections – voters elect representatives in their local constituencies, independent from the results of mainland voting. Although the overall outcome of the parliamentary elections is in most cases decided on mainland France, the uncertainty about the identity of the local MP has not yet been resolved when voters in the OST vote. Hence, if the 2005 reform has a causal effect on voting behavior in the OST to the west of mainland France, this effect should be primarily evident in presidential elections and less so in parliamentary elections. In fact we do not find any significant effect on turnout in parliamentary elections, which increases confidence that we actually identify the causal parameter of interest (see [Section 5](#)).

<sup>22</sup> The data were collected from the French ministry of Internal Affairs and from the [www.politiquemania.com](http://www.politiquemania.com) website, which provides easily accessible information about French elections at the local level. Given the large amount of information to be retrieved, it was impossible to make use of official French ministry data that are not downloadable. We did, however, compare random samples from the data we received with the official numbers and found that in every case they were exactly the same.

<sup>23</sup> For simplicity, we consider both TOMs and DOMs to be geographical units similar to French departments even if, formally, only DOMs are “departments”.

<sup>24</sup> Saint Martin and Saint Barthelemy split from Guadeloupe in 2007 to become separate OSTs. We nevertheless count their results and population in the 2012 elections jointly with Guadeloupe to be consistent over the whole sample. The population of these two territories combined represents 8.5% of the Guadeloupe population.

### 3.5. Empirical strategy

To assess the impact of knowing the election outcome on voter turnout, we estimate equations of the following form:

$$Y_{st} = \alpha + \eta t + \delta \mathbb{1}_{[t \geq 2005]} + \lambda \mathbb{1}_{[s \in TG]} + \beta \mathbb{1}_{[t \geq 2005]} \mathbb{1}_{[s \in TG]} + \mathbf{X}_{st} \boldsymbol{\gamma} + \varepsilon_{st}, \quad (1)$$

where  $TG$  indicates the treated OST (i.e. the OST to the west of mainland France) and  $Y_{st}$  the turnout by department  $s$  in year  $t$ .  $\mathbb{1}[\cdot]$  represents the indicator function and  $\mathbf{X}$  a vector of controls that contains time trends, second round election dummy and OST dummies,  $\varepsilon_{st}$  is the usual error term.  $\beta$ , the coefficient on the interaction of the treatment group and the time dummy, is the difference-in-difference estimator (DID) of the causal effect of interest.

The advantage of this empirical design is that all department-specific fixed-effects that might influence turnout are canceled out; for example, different political attitudes in the different territories, differences in political knowledge and costs of voting (distance to the next voting booth, climatic conditions etc.). We first present results from the most basic DID where we only use the pre- and post-treatment election and only the eastern OST as a control group.<sup>25</sup> In doing so we are able to more explicitly control for OST specific characteristics, such as specific political attitudes towards the political candidates and towards the mainland in general. In this most basic specification the treatment effect is estimated as a difference-in-difference of four sample averages. Hence no parametric functional form assumptions and extrapolations are needed. Nevertheless, later on we use all elections from 1981 to 2012 and also include the departments from the mainland as an additional control group. In this case we control for different time trends of turnout. In the most detailed model we allow every department to be on a different linear and quadratic time path, which is sometimes labeled as a “random trend model” (Wooldridge, 2007, p. 315), but is essentially a difference-in-difference-in-difference model where previous periods are used as pre-program tests. Empirically, this approach implies that we replace  $\eta t$  in Eq. (1) by  $\eta_s t$  and  $\eta_s t^2$ . Allowing the different departments to be on different time trends over the whole sample period is an effective tool for examining whether the treatment effect we estimate in fact only captures different trajectories of turnout for these different departments. As it turns out the results remain robust relative to the simple nonparametric DID-model.<sup>26</sup>

Although splitting up the French mainland into departments helps to mitigate concerns about the differing sizes of the underlying voting populations for each observation, these differences might still be a concern in our analysis. To tackle this concern, we apply weighted least squares to Eq. (1) using the number of registered voters in each department as weights. To assess the reliability of our standard error estimates, we compare OLS, clustered and bootstrapped standard error estimates.

## 4. Results

Fig. 2 illustrates the evolution of presidential election turnout averaged by year for all three territories – the mainland and the western and eastern OST.<sup>27</sup> Here, turnout trends seem to be reasonably parallel before the legislative change, and the increase in turnout is visibly larger in the treated OST between the 2002 and 2007 elections. In 2007, however, there is a distinct increase in turnout in all parts of France, after which, the time trends again seem to follow a parallel path. Fig. 3, which plots the same data for each western OST individually, supports these conclusions. This graph also shows that the results are not driven by some outliers, but confirms the consistent pattern for all western OSTs.

Table 3 displays the results from the most basic nonparametric difference-in-difference model where we compare only the two rounds of the last election before the law change with the two rounds of the first election after (2002 and 2007). We also exclude the departments from the mainland and use only the eastern OST as a control group. Naturally, this approach dramatically decreases the number of observations ( $N=36$ ).<sup>28</sup> We find a positive and significant point estimate of about 11 percentage points. This finding implies that knowing precise exit poll information decreases turnout by about 11 percentage points.<sup>29</sup> Weighting by the size of the population (columns (3)–(4)), as well as controlling for the round of the election does not affect the estimated treatment effect. It seems likely that there is some within-department correlation of the error term

<sup>25</sup> Naturally, in DID settings there is a concern over choosing the proper control group, see Sekhon and Titiunik (2012) for example. The eastern OST appear to be a natural control group for the western OST in our case.

<sup>26</sup> The fundamental assumption underlying a difference-in-difference design is that the outcome variable follows a parallel time trend in the absence of treatment in the treated and the control groups. In general this assumption might be violated if variables that are potentially related to the outcome are imbalanced between both groups, requiring large extrapolation, see Abadie (2005). This problem might arise, for example, if selection into treatment is influenced by pre-treatment territory specific shocks. However, in our setting we argue that the law change is an exogenous event which is not determined by covariates such as population characteristics. Hence, it was not triggered by specific shocks, but instead was a relatively long and complicated legal process. Also, in our setting it is not immediately clear what observable characteristics could influence turnout differently in the treated and the control groups.

<sup>27</sup> Hence turnout is also averaged over the two rounds by territory and year.

<sup>28</sup> In the appendix we also present results where we exclude the mainland but estimate over all six elections and restrict the sample to the 2002 and the 2007 election, using the eastern OST as well as the mainland as a control group. The point estimates remain remarkably stable and statistical significance at conventional levels is given in all cases. Moreover, Bertrand et al. (2004) also show that standard errors in DID frameworks that rely on long time series are susceptible to autocorrelation, which might lead to overconfidence in the precision of the point estimates. To this end, we collapse the sample into a pre- and post-period (see appendix).

<sup>29</sup> The raw turnout averages are 55% in 2002 and 64% in 2007 for the eastern OSTs and 46% in 2002 and 65% in 2007 for the western OSTs.



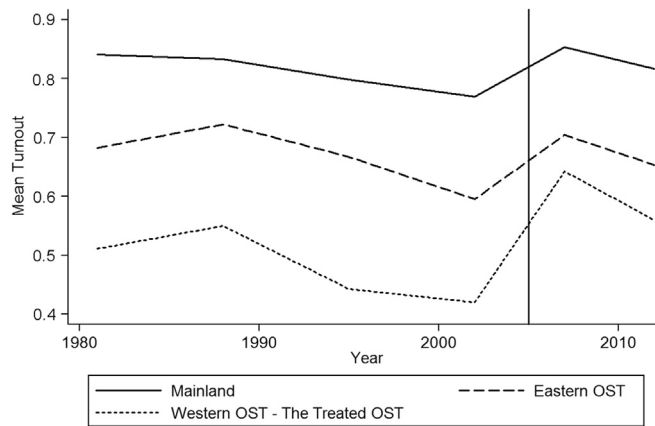


Fig. 2. Average turnout by geographical area and year. The vertical line indicates the year in which the law change occurred.

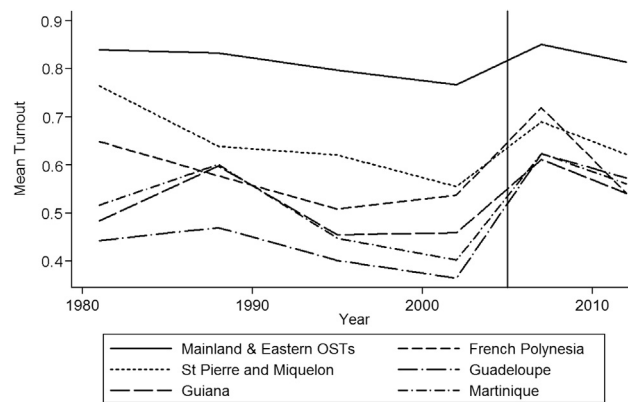


Fig. 3. Average turnout in each treated OST versus the turnout in the mainland plus the eastern OSTs. The vertical line indicates the year in which the law change occurred.

Table 3

Difference-in-difference estimates with turnout as dependent variable. Sample includes both rounds of the 2002 and 2007 elections and the eastern OST constitute the control group,  $N=36$ . The point estimates and significance levels are very similar when we additionally exclude the first round of 2002 because of the 'Le Pen' effect.

	Eastern OST as control group			
	Baseline		Weighted regression	
	(1)	(2)	(3)	(4)
Treatment effect	0.11*** (0.04)	0.11*** (0.04)	0.11* (0.06)	0.11** (0.05)
Round dummy	No	Yes	No	Yes
SEs	Block bootstrap	Block bootstrap	OLS	OLS

\* Significance at the 10% level.

\*\* Significance at the 5% level.

\*\*\* Significance at the 1% level.

and hence it is appropriate to allow for clustered errors. We do so whenever possible. However, when dropping the departments from the mainland we have nine clusters which is not enough to estimate clustered standard errors. In this case we block-bootstrap errors which has been shown to work well with a small number of clusters.

Table 4 presents the results using all six elections and all 105 departments, which makes a total of 1260 observations. We include OST and round dummies and control for different common time trends of turnout (columns (1)–(5)) and territory-specific

**Table 4**

Difference-in-difference estimates with turnout as the dependent variable. Standard errors are clustered on the department level and given in brackets below.  $N=1260$ . Specifications (6)–(7) correspond to the random trend model, in which the territory indicator is coded as 0=mainland, 1=eastern OST, 2=western OST.

	Potential common time trend					Territory specific time trend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment effect	0.06*** (0.02)	0.06*** (0.02)	0.06** (0.02)	0.06** (0.02)	0.06** (0.02)	0.11*** (0.02)	0.14*** (0.02)
OST dummy	No	No	No	Yes	Yes	No	No
Common linear time trend	No	Yes	No	No	Yes	No	No
Common quadratic time trend	No	Yes	No	No	Yes	No	No
Round dummy	No	Yes	No	Yes	Yes	Yes	Yes
Weighting	No	No	No	No	No	No	Yes
Territory indicator	No	No	No	No	No	Yes	Yes
Territory-specific time trend	No	No	No	No	No	Yes	Yes
SEs	OLS	OLS	Clustered	Clustered	Clustered	Clustered	Clustered

\* Significance at the 10% level,

\*\* Significance at the 5% level,

\*\*\* Significance at the 1% level.

time trends (columns (6)–(7)).<sup>30</sup> In the basic specification the point estimates are around 6 percentage points and then increase to 11 and 14 respectively when we allow for territory specific time trends (column (6)) and weighting (column (7)). Controlling for the election round and whether a department is an OST does not affect the estimate. Columns (1) and (2) show that OLS errors are in a similar range to clustered errors.

In Table 5 we take the analysis one step further and estimate a different linear (columns (1)–(4)) and quadratic time (columns (2) and (4)) trend for all of the 105 departments. The coefficient of interest remains remarkably stable at around 11 percentage points in the baseline specification and again increases somewhat when we introduce weighting (columns (3) and (4)), but is now much more precisely estimated with standard errors around two percentage points.

In summary, in all specifications, the point estimate remains stable and quite precisely estimated. We report an increase in turnout from not knowing the election result of 11 percentage points. Moreover, concerns about different population sizes underlying each observation do not seem to matter.<sup>31</sup>

## 5. Robustness checks

### 5.1. Placebo-estimates and the candidate from French Guiana

In order to check for robustness, we first calculate placebo estimates using 1994 as the year in which reform was implemented (excluding post–2005 observations)<sup>32</sup> and secondly (falsely) using the *eastern* OST as a treatment group (on the full sample). We do not find any significant coefficient estimate in either case (the results are not reported here).

The first-time appearance in the first round of the 2002 election of a candidate from French Guiana might have led to an overly increased turnout in this department. As a consequence, our estimates might be biased since Guiana is one of the treated territories. As the 2002 election is before the 2005 reform, this bias would create a downward bias on the estimated effect of the reform on the change in turnout. Given that we observe an increase in turnout following the reform, the existence of a Guiana candidate in 2002 can only make our estimate conservative (meaning that the effect of exit poll information can only be higher than the one estimated and not lower). In any case, we control for a possible bias due to this event by including a dummy indicating Guiana in the first round of 2002. As expected, we find a positive and significant coefficient on this dummy. Nevertheless, all results from the tables presented previously remain unchanged (and are thus not reported here), indicating that this event has no impact on our estimates.

<sup>30</sup> We refer to the mainland, the eastern and western OST respectively as a 'territory'.

<sup>31</sup> We also implemented Papke and Wooldridge's (1996) quasi-maximum likelihood estimator in order to account for the fact that the dependent variable is naturally restricted to the unit interval (results are not reported here). In this approach the conditional mean of the dependent variable is specified as a logit function and the log likelihood as Bernoulli distributed. Since the Bernoulli likelihood is part of the linear exponential function family, a quasi-maximum likelihood estimator is consistent even when the Bernoulli distribution is misspecified. This result goes back to [Gourieroux et al. \(1984\)](#). We cluster the standard errors in all cases on the department level and find that the results are very much in line with earlier findings: not knowing the election results has a significant positive impact on turnout.

<sup>32</sup> We prefer not to conduct this test on a sample that overlaps with 2005 since in this case the treatment effect might potentially affect the pseudo-estimate. On the other hand, we only observe four elections at two time points after the law changed. Hence it seems reasonable to split the pre-treatment period into half and run this same test on that sample.

**Table 5**

Difference-in-difference estimates with turnout as dependent variable. Separate time trends for each of the 105 departments are included. Sample includes all presidential elections between 1981 and 2012,  $N=1260$ . The point estimates and significance levels are very similar when we additionally exclude the first round of 2002 because of the 'Le Pen' effect.

	Department specific time trends			
	Baseline		Weighted regression	
	(1)	(2)	(3)	(4)
Treatment effect	0.11*** (0.02)	0.09*** (0.02)	0.14*** (0.02)	0.18*** (0.01)
OST dummy	Yes	Yes	Yes	Yes
Round dummy	Yes	Yes	Yes	Yes
Department-specific linear time trend	Yes	Yes	Yes	Yes
Department-specific quadratic time trend	No	Yes	No	Yes
SEs	Clustered	Clustered	Clustered	Clustered

\* Significance at the 10% level,

\*\* Significance at the 5% level,

\*\*\* Significance at the 1% level.

### 5.2. "Le Pen elections"

In [Section 3.3](#), we expressed concern that the anomalous result in the first round of 2002 – in which right-wing candidate Jean-Marie Le Pen surprisingly made it into the second round – might have impacted our results. First, we argue that this event is likely to bias our estimates downward, meaning that we are presumably estimating lower bounds of the actual effect. This is because the closeness of the election is likely to increase turnout more in the west, which on average has better information when voting, than in the eastern parts of France. As a result, the DID estimates tend to be smaller than in the counterfactual case without this highly polarizing candidate. Second, we replicated the regressions from [Tables 3–5](#) excluding the first round of 2002. Reassuringly, in all cases the estimates are still statistically significant and, if anything, the point estimates increase slightly.

[Table 6](#) replicates [Table 3](#) without the Le Pen election. The results clearly withstand the exclusion of this election, the coefficients are however less precisely estimated which is most likely a consequence of the small sample size. Nevertheless, the conclusions also seem to be robust using larger samples and including the mainland as a control group. Further results can be found in [Table 12](#) in the appendix. Taken together, these findings strongly suggest that the results reported earlier are not biased by unusual events such as the 'Le Pen election'.

### 5.3. Testing for other shocks on turnout

A threat to the validity of our difference-in-difference estimates would be a different time path for turnout in the treated OST versus the control group in the absence of treatment. While it is reassuring to find that allowing for different linear and quadratic turnout trajectories in every department does not impact the results, it is possible to go one step further. We use turnout data on parliamentary elections in France to re-estimate the same difference-in-difference models as in [Section 3.5](#). Although these elections are national we would expect a smaller or even no treatment effect because in each department voter chooses between different candidates who are seeking the position as their *local* representative. Therefore, the result on the mainland does not influence who will represent an OST district in the national parliament.<sup>33</sup> In that sense estimating difference-in-difference models with data from parliamentary elections can provide an indication as to whether other unobserved shocks that could potentially confound our estimates of affected turnout in that period, because such shocks, if they exist, are likely to affect other elections as well.

[Table 7](#) replicates [Table 3](#) using the parliamentary data; that is, here only the eastern OST serve as a control group and the sample is restricted to the election right before and right after the law change. Given that no estimated coefficients are significantly different from zero at the 10% level and all are also much lower in absolute terms, OST-specific shocks do not seem to be a concern. This finding assures us that we are indeed identifying the causal effect of interest and not other time-variant shocks. We again also introduce weighting but find no significant point estimates. We present additional results in the appendix. [Fig. 4](#) summarizes the turnout trends at these parliamentary elections by territory.

<sup>33</sup> It might be the case that voters in the OST care whether their local representative is a member of a party of influence in the parliament and in that case results in the mainland may affect their voting behavior. We assume that these concerns, if they exist, are not considerable.

**Table 6**

Excluding Round 1 of 2002. Difference-in-difference estimates with turnout as dependent variable. Sample includes both rounds of the 2002 and 2007 elections and the eastern OST constitute the control group,  $N=27$ .

	Eastern OST as control group			
	Baseline		Weighted regression	
	(1)	(2)	(3)	(4)
Treatment effect	0.09* (0.05)	0.09* (0.05)	0.14* (0.07)	0.14* (0.07)
Round dummy SEs	No Block Bootstrap	Yes Block Bootstrap	No OLS	Yes OLS

\* Significance at the 10% level,

\*\* Significance at the 5% level,

\*\*\* Significance at the 1% level.

**Table 7**

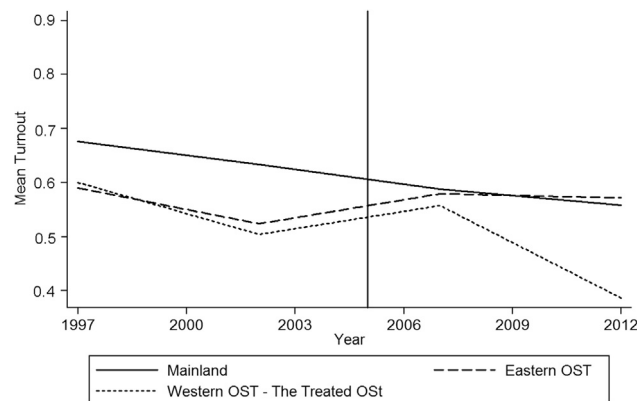
Placebo difference-in-difference estimates analogous to earlier tables with parliamentary election turnout as the dependent variable. Standard errors are given in brackets below. The sample includes parliamentary elections of 2002 and 2007 only and uses only the eastern OST as control group.  $N=35$ .

	Eastern OST as control group			
	Baseline		Weighted regression	
	(1)	(2)	(3)	(4)
Treatment effect	0.04 (0.05)	0.04 (0.05)	−0.00 (0.07)	−0.02 (0.07)
Round dummy SEs	No Block Bootstrap	Yes Block Bootstrap	No OLS	Yes OLS

\*Significance at the 10% level.

\*\*Significance at the 5% level.

\*\*\*Significance at the 1% level.



**Fig. 4.** Average turnout by territory and year for the parliamentary elections. The vertical line indicates the year in which the law changed.

## 6. Estimating potential bandwagon effects

In political science, the bandwagon effect refers to the phenomenon in which people might vote for a candidate just because he or she is likely to win the election. Bandwagon voting can be of two types: (1) individuals may receive utility from voting for the winner such that they disregard their private information or preferences and vote for the candidate who feel ex ante and has more support (bandwagon vote switching effect) or (2) individuals receive utility from voting for the winner such that they are willing to turnout to vote for their most preferred candidate when he or she is expected to win than when he or she is expected to lose (bandwagon turnout effect). In the first type of bandwagon voting, voters choose to vote for the winner contrary to their preferences or information and in the second type, voters always choose according to their preferences or information if they vote, but are more likely to turnout if their preferred candidate is the expected

winner.<sup>34</sup> Although this effect attracted a lot of attention among scholars, it was quite difficult to provide empirical evidence. An opposite effect, the “underdog” effect, where voters tend to favor the disadvantaged candidate, has also been discussed as a possibility.<sup>35</sup>

The French natural experiment, however, provides a unique setting for examining such effects in that prior to the 2005 reform, voters in the western OST knew the winner when they went to the polls. Our identification strategy thus relies on estimating the impact of the leading presidential candidate's vote edge, as compared to that of the runner-up on the mainland (and in the eastern OST), on the vote difference in the western OST. To determine this impact, we estimate the following equation:

$$\Delta_{st} = \alpha + \beta \Delta_{mainland,t} + \gamma \mathbb{1}_{[t > 2005]} + \delta \Delta_{mainland,t} * \mathbb{1}_{[t > 2005]} + \varepsilon_{st}, \quad (2)$$

where  $\Delta_{mainland,t} \in [0, 1]$  is the normalized difference of votes between the candidate with the most votes and the runner-up at time  $t$  on the mainland (and in the eastern OST) and  $\Delta_{s,t} \in [-1, 1]$  is the same difference in the western departments at time  $t$ . We use only the vote difference in the western OST as the left-hand variable in this estimation since the non-treated departments contribute to  $\Delta_{mainland,t}$ . Our approach minimizes endogeneity concerns because the regressors are pre-determined.  $\mathbb{1}_{[t]}$  once again denotes the indicator function. Essentially, Eq. (2) estimates a pre- and post-2005 slope, in which the parameter of interest,  $\delta$ , indicates the *difference* between both coefficients. This approach allows us to test for a bandwagon effect using simple  $t$ -tests on  $\delta$ . If such an effect exists, we would expect  $\delta$  to be significantly different from zero. A negative  $\delta$  would indicate that the results in west OSTs are less correlated with mainland results after the reform (without exit poll information) than before the reform (with exit poll information). Thus a negative parameter indicates a bandwagon effect, where voters in western OST tended to follow the announced mainland results before the reform. On the contrary, a positive  $\delta$  indicates an underdog effect with western OST voters voting less for the mainland favourite before the reform than after.

Our estimates are summarized in Table 8. We estimate Eq. (2) via OLS and find a pre-treatment slope coefficient of 1.17, which is statistically significant different from zero at the 1% level. We also find a large negative slope coefficient of  $-4.05$  for the post-2005 period. The difference  $\delta$  between both coefficients is therefore negative ( $-5.22$ ) and statistically significant at the 10% level using ordinary clustered, and bootstrapped standard error estimates (middle of Table 8) in the  $t$ -test. Both coefficients, when taken alone, are statistically different from zero, indicating that the positive relation when voters know who will win becomes negative after the law changes.

We also estimate the same equation excluding observations where the vote edge from the mainland was larger than 20 percentage points (first round of 1988 and second round of 2002). Visual inspection of the raw data suggests that these observations could be driving our estimates. Nevertheless, the last column of Table 8 shows that such outlier observations are not a problem. The point estimates and standard errors increase somewhat but all estimates are still significant in all cases. Fig. 5, which plots the fitted and raw  $\Delta_{treated,st}$  versus  $\Delta_{mainland,t}$  relation separately before and after 2005, confirms this conclusion graphically.

Overall, these results clearly suggest that election results in western OSTs were more positively correlated with the mainland results before the reform when western OST voters could find out the winner in mainland results before voting. In practice, the candidate ahead in mainland France was more likely to win in western OSTs before the reform (when voters had access to information on the identity of the leader on the mainland) than after the reform (when voters no longer had access to such information). As indicated above, there are two possibility explanations for the observed bandwagon voting; either the information has a bandwagon vote switching effect or a bandwagon turnout effect or both. Our data does not allow us to distinguish between the two possible explanations. Nevertheless, to our knowledge, our results provide the best available evidence demonstrating a bandwagon effect.

## 7. Concluding remarks

The advent and proliferation of new electronic information sources and social media has had profound impacts on many aspects of politics across the globe. Increasingly, individuals are able to use the web to coordinate protests against governments and other groups, acquire secret and classified information, and to monitor exit poll results while an election is still in process. In this paper we investigate the possible effects of the last development on political behavior. Previous

<sup>34</sup> For a discussion of the psychological aspects of a bandwagon effect or impersonal influence, see, for example Kenney and Rice (1994) or Mutz (1997). Information can also affect confidence in a voting decision (Matsusaka, 1995). Electoral momentum has also been found in early presidential primaries (candidates performing well in Iowa or New Hampshire receive a future primary voter boost, as discussed in Morton and Williams (1999) and Morton and Williams (2001)). As reviewed previously, Callander (2007) provides a formal model of bandwagon voting. See also the experimental evidence of bandwagon voting in Hung and Plott (2001). Schmitt-Beck (1996) also refers to a consensus heuristic. If a multitude of voters are behind one of the candidates, people take the choice of others as an indicator of political quality of the candidate.

<sup>35</sup> West (1991, p.153) discusses the case of Walter Mondale and Gary Hart in the 1984 US presidential nominating process to illustrate the underdog effect. Mondale had a lead over Hart. Hart then applied the strategy of “do not let the powerbrokers tell you the race is over.” This strategy helped Hart to attract a substantial amount of voter support, losing at the end but performing much better than anticipated.



**Table 8**

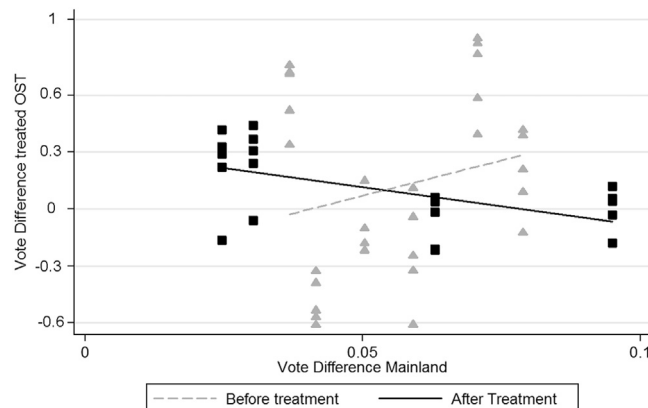
Estimating Eq. (2) with  $\Delta_{treated, st}$  as the dependent variable. Several standard error estimates are given for the  $t$ -test of  $H_0$ : the difference in pre- and post-2005 slopes is zero.

	Full sample	Excluding outliers
Pre-2005 slope estimate	1.18*** (0.29)	7.45 (4.71)
Post-2005 slope estimate	−4.04* (2.89)	−4.04 (3.07)
<b>Difference <math>\delta</math></b>	−5.22	−11.49
Standard errors:		
OLS	(2.91)*	(5.62)**
Clustered by department	(2.04)*	(3.78)**
Bootstrapped	(1.23)***	(5.76)**
Block bootstrapped by department	(1.73)***	(3.31)***
N	60	50

\* Significance for this  $t$ -test at the 10% level.

\*\* Significance for this  $t$ -test at the 5% level.

\*\*\* Significance for this  $t$ -test at the 1% level.



**Fig. 5.** Plot of  $\Delta_{mainland}$  versus  $\Delta_{treated}$ , separately before and after 2005 for the sample without outliers. The former exhibits a clear positive trend and the latter a distinct negative trend, indicating that, ceteris paribus, the leading candidate on the mainland loses votes when these voters have no exit poll information from the mainland.

research using naturally occurring elections has suffered from a number of methodological problems in identifying whether such information can have a causal effect on voting behavior.

We take advantage of a unique natural experiment from 2005, in which French citizens from overseas territories to the west of mainland France began for the first time to vote without knowing the choices made by mainland voters. We find that knowing the outcome of early voting decreases turnout by about 12 percentage points in our preferred specification. We also find empirical support for bandwagon voting in which later voters, if they participate, are more likely to vote for the expected winner.

Our results suggest that when voters can access exit poll results during an election voting behavior is significantly affected. These effects on voting behavior (lower participation and a bandwagon effect) provide advantages to candidates and political parties favored by early voters, which do not exist in the absence of the information. If later voters differ from early voters in terms of demographics and ideological preferences, then we would also expect such information to have an effect on the types of public policies chosen by elected officials as well. Candidates and political parties, moreover, have an incentive to manipulate the timing of voting and the type and accuracy of information revealed through exit polls. Concerns about the effects of exit polls on elections as expressed by many government officials, candidates, and party leaders and calls for restrictions on such information are thus strongly supported by our results.

Furthermore, our results suggest that the increasing use of convenience voting in which individuals cast ballots in advance of election day either online, at the mall, or through the mails, may have serious repercussions on voter, candidate,

and political party behavior. While many advocate such policies as a way of increasing participation and making elections more “friendly” to voters as discussed in [Gronke et al. \(2007\)](#), coupled with information leakages, increasing convenience voting can fundamentally change the nature of the election process in unanticipated ways.

## Acknowledgments

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## Appendix A. Additional results

See [Tables 9–13](#).

**Table 9**

Difference-in-difference estimates with turnout as dependent variable. Clustered standard errors are clustered on the department level and given in brackets below. Sample includes both rounds of the 2002 and 2007 elections,  $N=420$ . The point estimates and significance levels are very similar when we additionally exclude the first round of 2002 because of the ‘Le Pen’ effect.

	Mainland and Eastern OST as control group					
	Baseline				Weighted regression	
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.11*** (0.02)	0.11*** (0.02)	0.11*** (0.02)	0.11*** (0.02)	0.14*** (0.02)	0.14*** (0.02)
OST dummy	No	Yes	No	Yes	No	Yes
Round dummy	No	Yes	No	Yes	No	Yes
SEs	Clustered	Clustered	Block Bootstrap	Block Bootstrap	Clustered	Clustered

\* Significance at the 10% level.

\*\* Significance at the 5% level.

\*\*\* Significance at the 1% level.

**Table 10**

Difference-in-difference estimates with turnout as dependent variable. Sample includes all presidential elections between 1981 and 2012 but uses only the eastern OST as a control group,  $N=108$ . The point estimates and significance levels are very similar when we additionally exclude the first round of 2002 because of the ‘Le Pen’ effect.

	Eastern OST as control group			
	Baseline		Weighted regression	
	(1)	(2)	(3)	(4)
Treatment effect	0.10*** (0.03)	0.10*** (0.03)	0.11*** (0.03)	0.11*** (0.03)
Round dummy	No	Yes	No	Yes
Linear time trend	No	Yes	No	Yes
Quadratic time trend	No	No	No	Yes
SEs	Block bootstrap	Block bootstrap	OLS	OLS

\* Significance at the 10% level.

\*\* Significance at the 5% level.

\*\*\* Significance at the 1% level.

**Table 11**

Difference-in-difference estimates with turnout as the dependent variable where the sample is collapsed into a pre- and post-treatment. Bootstrapped standard errors are drawn with 999 repetitions and  $N=211$ .

	Two period sample			
	(1)	(2)	(3)	(4)
Treatment effect	0.07*** (0.02)	0.07*** (0.03)	0.10*** (0.03)	0.10*** (0.03)
Weighting SEs	No OLS	No Clustered	Yes OLS	Yes Clustered

\*Significance at the 10% level.

\*\*Significance at the 5% level.

\*\*\*Significance at the 1% level.

**Table 12**

Difference-in-difference estimates, excluding “Le Pen Elections”, with turnout at presidential elections as dependent variable.  $N=1155$ . Clustered standard errors are clustered on the department level and given in brackets below. Specifications (6)–(7) correspond to the random trend model, in which the territory indicator is coded as 0=mainland, 1=eastern OST, 2=western OST.

	Excluding round 1 of 2002						
	Potential common time trend					Territory specific time trend	
	(1)	(2)	(3)	(4)	(5)	(6)	(7)
Treatment effect	0.06*** (0.02)	0.06*** (0.02)	0.06** (0.03)	0.06** (0.03)	0.06** (0.03)	0.12*** (0.02)	0.15*** (0.01)
OST dummy	No	No	No	Yes	Yes	No	No
Common linear time trend	No	Yes	No	No	Yes	No	No
Common quadratic time trend	No	Yes	No	No	Yes	No	No
Round dummy	No	Yes	No	Yes	Yes	Yes	Yes
Weighting	No	No	No	No	No	No	Yes
Territory indicator	No	No	No	No	Yes	Yes	Yes
Territory-specific time trend	No	No	No	No	No	Yes	Yes
SEs	OLS	OLS	Clustered	Clustered	Clustered	Clustered	Clustered

\* Significance at the 10% level,

\*\* Significance at the 5% level,

\*\*\* Significance at the 1% level.

**Table 13**

Placebo difference-in-difference estimates with parliamentary election turnout as the dependent variable. Standard errors are given in brackets below. The sample includes parliamentary elections from 1997 to 2012,  $N=838$ .

	No time trends			Common linear and quadratic time trend		
	(1)	(2)	(3)	(4)	(5)	(6)
Treatment effect	0.02 (0.03)	0.02 (0.03)	0.03 (0.03)	0.02 (0.03)	0.02 (0.03)	0.01 (0.03)
OST dummy	No	Yes	Yes	No	Yes	Yes
Round dummy	No	Yes	Yes	No	Yes	Yes
Weighting	No	No	Yes	No	No	Yes
SEs	Clustered	Clustered	Clustered	Clustered	Clustered	Clustered

\*Significance at the 10% level.

\*\*Significance at the 5% level.

\*\*\*Significance at the 1% level.

## Appendix B. Supplementary data

Supplementary data associated with this article can be found in the online version at <http://dx.doi.org/10.1016/j.eurocorev.2015.03.012>.

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