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Voting Experiments

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This module presents a variety of studies on voting. They tackle different subjects: participation, Condorcet cycles, strategic voting, electoral campaigns, the voter's behaviour and psychology. They use different methods that can be labelled 'experimental' and all of them are more or less direct tests of models and theories. The module is therefore an introduction, by examples, to various experimental methods in use in political science. The presentation in three sections goes by increasing complexity of the experiments themselves or of their analysis, starting with 'classroom' experiments that can be organized very simply and used for pedagogical purposes.

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Voting, participation, strategic voting, voting behaviors, voters psychology, experiments

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Voting Experiments

Antoinette Baujard, Herrade Igersheim and Jean-François Laslier¹²

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12.1 In the Classroom

The first section of this module describes simple experiments that can be easily conducted in the classroom. Participating in these experiments shows some of the main points at the heart of the study of voting rules.

- The act of voting is morally loaded and, for most people, its importance does not reduce solely to its possible consequences.
- Voting rules matter; in the same circumstances different voting rules may produce different outcomes.
- Voters think ahead; they do not simply vote mechanically but learn from the past.

12.1.1 Participation: The Price of a Right

The standard economic analysis assumes that people act rationally, and that citizens decide whether to vote or not based on a cost-benefit analysis. This process, foundational to the theory of rational choice, involves evaluating the possible consequences of alternative actions with their likelihood of occurrence. Voting always has a cost: the time (and perhaps money) spent travelling to the polling booth, the time waiting in line (for many hours sometimes), the time taken to register as required to be eligible to vote, and so on. Compared to the cost, the likely benefit from voting in a mass election is very small. An individual vote makes a difference only in the very specific case in which two (or possibly more)

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candidates are tied, or within one vote of a tie. In mass elections with thousands if not millions of voters, such an event has a probability so small that the expected benefit to a voter is negligible. This ‘rationality’ argument—in its modern form usually referred to as Downs’ Paradox of Voting—suggests that voting is irrational because the probability of being a *pivotal* voter who will determine the election outcome is almost zero. Note that this paradox, which is actually a ‘paradox of *not* voting’, must not be confused with the Condorcet paradox, also sometimes called the ‘paradox of voting’. Various scholars have developed this argument (see section 12.1.4) and have placed great emphasis on computing the probability of ties, also known as the pivot probabilities.

The same reasoning also applies when the voter is interested not only in the result of the election—the *instrumental motive* for participation—but also in the total vote for the various candidates, including those who are not elected. Such a concern makes sense because the overall distribution of votes in an official election signals the relative political strengths—popularity, electability and so on—of candidates (the *expressive* or *signaling motive* for participation). But even in that case, an individual’s vote can have no noticeable consequence in a mass election. The rational theory thus predicts that, as soon as voting is not costless and the number of voters is large, there is essentially no incentive to vote.

However, the rational choice theory’s prediction of voting behavior is not confirmed by observation. While many complain that turnout rates are declining in established democracies, the majority of citizens still do take the time to vote, at least in national elections. This seems to contradict rational choice, and the relevance of ‘pivot event’ theories is therefore not evident. The rational choice approach also contradicts other motives for voting, in particular the social norm according to which a democratic election provides an opportunity for citizens to voice their opinion. Laboratory and classroom experiments are useful tools with which to examine more closely what is going wrong with rational choice theory.

One of the first tests of this theory concerns participation itself. In order to estimate how much individuals value their right to vote, a simple way is to ask them for how much money they would sell their right to vote. Under the supervision of W. Guth and H. Weck-Hannemann, such an experiment was conducted five days before the election of the Bundestag (the German Parliament) in October 1994 (see subsection 12.1.4 for reference to this and other experiments and research covered in section 12.1). About 200 German university students beginning an introductory course in microeconomics were asked to take part in the experiment, and 142 agreed.

The problem the authors are dealing with in this experiment is quite common in the field of experimental economics. Imagine that we know that an individual values something, say one unit of a good, and we want to attach a price to it. Simply asking the person, “how much are you willing to pay?” will not always work because there is no compelling reason for the person to answer truthfully. Such a rough experimental protocol would give the respondent no incentive to tell the truth. A more convincing approach could be to propose a real transaction, for instance “I’ll pay you \$100 for that”. However, a response to such an offer will only reveal if the value attached to the good is equal to or higher than \$100 if the transaction is accepted, or lower than \$100 if it’s rejected. While these responses give *some* useful information, experimental economists employ various subtler mechanisms to derive more accurate information. Such methods were used effectively with the students in the pre-Bundestag Election experiment which we turn to now.

The participants in the experiment were given instruction sheets and questionnaires which asked them to indicate the price, from 0 to DM 200, they would want for giving up their vote in the upcoming election. (Note: DM are Deutsch Marks, the German currency used before the Euro was adopted for general use in 2002. The range between 0DM—200DM would value today between 0 and 120 USD, approximately 100 Euros).

The experimenters made it clear to all participants that a price of more than DM 200 would mean that the student did not want to sell. The offer price was then determined by a random draw between DM 0 and DM 200. Among the students whose price was less than or equal to the randomly chosen price (DM 112), four students were randomly selected to destroy their official voting card in front of a student monitor, and were paid the chosen price of DM112. By assigning a code number to each decision sheet students were guaranteed anonymity. This random selection of a small sets of students willing to destroy their official voting card against a price (and thus, who will have no time to ask for a new one before

the German official vote) guarantees the validity of the experiment.

Note that, when asking participants to state their price for not voting, the price had not been set yet. This meant that participants had no better response than to declare their ‘true’ value. Giving a higher value was not a good strategy because the participant would just risk missing a good opportunity. Declaring a lower value could result in having to make an unfavorable transaction. Such a mechanism is said to be ‘incentive-compatible’ and ‘truth-revealing’.

The results derived from the Bundestag Election experiment were quite in line with the turnout rates of the actual German national election. In the experiment, 90 participants (about 63%) demanded a price greater than DM 200, indicating that they would not give up their vote in the election; this is comparable with the turnout rates in German elections, which always exceed 77%. Only 7 participants agreed to sell their vote for DM 10 or less (about 4%), while the remaining 45 students (about 32%) agreed to sell for prices ranging between DM 10 and DM 200. The results clearly show that the rational choice theory as stated above is not experimentally validated.

Thanks also to a post-experiment questionnaire completed by the student participants in the Bundestag experiment, together with some additional information about their age, sex, and so on, it is possible to shed light on several different explanations regarding the positive value of voting in elections, and thus some alternative reasons for whether to vote or not than is predicted by rational choice. Tables 12.1.1 and 12.1.2 summarize the data derived from the students grouped as ‘sellers’ (those who are willing to sell their right to vote) and ‘non-sellers’ of their votes (those whose price is above DM 200), and about the respective reasons for their choice.

Table 12.1.1 Some reasons to vote

	Sellers (52)	Non-sellers (90)
Expressing my political views	35%	57%
Decisiveness of my vote	17%	24%
Serving my personal interests	29%	33%
Civic duty regarding democracy	40%	63%
Interesting/fun	25%	24%

Table 12.1.2 Some reasons not to vote

	Sellers (52)	Non-sellers (90)
It doesn’t change anything	2%	0%
I don’t know which candidate/party to vote for	2%	0%
I don’t know how to serve my personal views/interests	6%	0%
I don’t know the parties’ programs	2%	0%

It is worth stressing that only one student seller gave as a reason for not voting that ‘a single vote does not change anything’—the essence of the argument for the so-called rational theory. Moreover, no student mentioned that the act of voting is time-consuming, another main factor in the cost-benefit analysis. While it might be thought that a better explanation of the behavior of abstainers from voting might be gleaned from their calculation of the costs of voting in keeping with their political preferences and attitudes—‘I don’t know the parties’ programs’, ‘I don’t know which candidate to vote for’, and so on—that does not appear to be the case. On the contrary, both sellers and non-sellers invoke *decisiveness* and expressing their political preferences among their main reasons for voting. It is also noteworthy that the reasons for voting given by both sellers and non-sellers do not differ significantly; except for ‘expressing my political views’ and ‘civic duty’ which are more often chosen by the non-sellers.

When closely scrutinized, the answers to the post-experiment questionnaire provided two kinds of explanation regarding voting behavior. First, that the rational theory cannot be entirely rejected, for

some students seem to rely on it (wrongly) to justify their choice to vote, particularly in terms of decisiveness of their own vote. Some voters erroneously choose to do so because they vastly *overestimate* the probability that their individual vote will influence the ballot count. This observation is much in line with early theoretical work that more recent experimental results also corroborate. The explanations in terms of social norms and pure rational choice theory, as discussed above, are far from sufficient to explain the high turnout rates observed in many mass elections. The data collected for the Bundestag Election experiment offers another kind of explanation. We see that other non-instrumental factors—that voting is a civic duty, or that it keeps alive one’s political interests, or that it’s fun, interesting, and so on—help to explain high turnout. Recent research confirms the impact of such ‘expressive voting’ rationale at an experimental level. It also suggests that the will to express one’s ethical preferences has a strong impact on the results of mass elections. In fact, the lower the probability of being a *pivotal* elector, the stronger is the will to express one’s ethical preferences through voting. Thus, people who choose to vote have strong ethical motivations.

All these experiments clearly show that people do not vote for only rational or consequentialist reasons, just because they are interested in the issue of the ballot, but because they care about voting per se. By voting, many people feel they have performed their duty as citizens, or fulfilled their perceived moral obligation, expressed their preferences, and so on.

12.1.2 Committee Decision-Making: The Road and the School

In this voting experiment we describe a well-known classroom experiment that is easily set up with students and shows some of the conceptual difficulties in collective decision processes. The experiment works well with a group of about 20 students and lasts about 40 minutes.

The question is to decide, collectively, about two projects: repairing a road and/or repairing a school. To conduct this experiment and to elucidate some of the problems of collective decision-making, the preferences of the student voters regarding these two projects need to be induced. To define the voters’ preferences for the experiment, each student receives an envelope containing two cards: a Heart card means a favorable opinion to repairing the road, a Spade means a favorable opinion to repairing the school, Club and Diamond cards are neutral.

The distribution of cards prescribing the opinions of 7 student voters, is as set out in Table 12.1.3. For 14 or 21 students, the distribution should just be replicated twice or three times respectively. If the number of students is not a multiple of 7, some students can play in little groups of two students.

Table 12.1.3 The Road and the School Preferred Options

Voter 1	Heart	Spade	Road and School
Voter 2	Heart	Spade	Road and School
Voter 3	Heart	Club	Road only
Voter 4	Heart	Club	Road only
Voter 5	Club	Spade	School only
Voter 6	Club	Spade	School only
Voter 7	Club	Spade	School only

Both the Road and the School projects are costly, so that a player who is not favorable towards a project strictly prefers it not to be done. To make the preferences precise it is useful to refer to monetary values. Each project costs a participant 240 USD (200 Euros) through extra taxes. Repairing the road has a value of 360 USD (300 Euros) for each voter who is favorable to it, which means a net benefit of 120 USD (100 Euros) for each of them. Repairing the road has no value for the other voters, which means a net loss to them of USD 240 (200 Euros) if the project is implemented. The same cost, benefit and loss holds respectively for the School project.

From Table 12.1.3 we see that all participants are favorable to at least one project, but note also that the 5 participants who favor one project *only* prefer no project to be implemented rather than both projects. For such a participant, the realization of both projects results in a loss: 360 USD (300 Euros) – 2 × 240 USD (200 Euros) = –120 USD (100 Euros) < 0. If we consult the table further, we see that 4 voters (1 to 4) out of 7 favor repairing the road; 5 voters out of 7 favor repairing the school. According to majority rule, it seems that both projects should be realized. But 5 voters out of 7 prefer the *status quo* (no project) to implementing both projects.

The preference profile defined by Table 12.1.3 entails the following Condorcet cycle as depicted in Table 12.1.4: ‘Road only’ > (beats, or is majority preferred to) ‘status quo’; ‘school only’ beats ‘road only’; ‘road and school’ beats school only; ‘status quo’ beats ‘road and school’.

Table 12.1.4 The Road and the School: A Condorcet Cycle

Road only	>	Status Quo
School only	>	Road only
Road and School	>	School only
Status Quo	>	Road and School

In the Road and School experiment the students vote under several pairwise comparisons between two options (Road vs. School, Road and School vs. Status quo, etc.) They soon realize that something strange is going on and that the way ballots are organized is a key factor in determining the outcome of the vote, and with it, the importance of a chairperson who organizes the votes. Some students also learn that it is possible to ‘manipulate’ the process by not stating their ‘true’ preference, as voting for a less preferred option on an early vote can lead to a more preferred final outcome.

The procedure needed to conduct this experiment properly is quite flexible. Here is an example. To begin with, each student receives an envelope with an instruction sheet that details the computation of values. The participants see only their own card. The chairperson (who can be the instructor or another student) calls out the votes by a show of hands and records all the results on the board. There is no need to forbid abstention for the experiment to work.

Table 12.1.5 illustrates a possible experimental set-up, as well as the result of a typical session recording four agendas with 14 students. The agendas followed in succession with a discussion between Agenda 3 and Agenda 3* (Agenda 3 is the same as Agenda 3*, but students are allowed by the instructor to discuss the results between both agendas).

Table 12.1.5 The Road and the School: Agendas

Agenda 1	vote 1	Road yes	(8/6)	Road no
	vote 2	School yes	(11/3)	School no
	vote 3	result of 1 and 2	(4/10)	Status Quo
Agenda 2	vote 1	Status Quo	(6/8)	Road only
	vote 2	result of 1	(5/8)	School only
	vote 3	result of 2	(6/8)	Road and School
Agenda 3	vote 1	School only	(10/4)	Status Quo
	vote 2	result of 1	(6/8)	Road and School
Agenda 3*	vote 1	School only	(6/7)	Status Quo
	vote 2	result of 1	(9/5)	Road and School

More generally, Table 12.1.5 can be read as follows. For instance, Agenda 1 comprises three votes:

1. First step: Vote on road construction
- The group must decide whether they want to build a road or not.

2. Second step: Vote on school construction
 - After deciding whether to build the road or not, the group must now decide if they want to build a school.
3. Third step: Vote on the action to take
 - Previous decisions (to build the road, build the school, or both) are taken into account.
 - The group must decide if they want to implement the decisions made in previous steps (either build the road, the school, or both) or do nothing at all.
4. Remuneration based on the final decision
 - Only the decision made in the third step determines whether the group will be remunerated or not.
 - If the group decides to build something (road, school, or both), they will receive the value of the construction minus the costs. Otherwise, if they decide to do nothing, they will not be remunerated.

Typically, following the preferences of the voters which have been induced by the cards, the first agenda leads to a rejection of both projects, and the next two lead to accepting them both. After the third agenda, some students who favor the School only come to think that they should vote strategically at the first vote in favor of the status quo. This maneuver may work and finally lead to rejecting both projects, as happens in the example reported in Table 12.1.5. But this is not always the case. Some students, even at the beginning, do not vote according to their cards. This may be due to isolated mistakes, a more general misunderstanding of the experiment, or any other reason—for instance, some refuse to take a hostile stand with respect to building a school. Many of these remarks or behaviors are good prompts to discuss the notions of preferences, interests and many other relevant concepts raised during the experiment, such as:

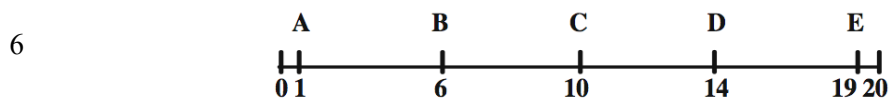
- Individual preferences: Having an opinion about a political platform is not only having an opinion about each item of the platform, even if the opinions about them can be asserted independently, as is the case in this experiment.
- Collective preferences: Transitivity of collective revealed choices, Condorcet cycles, potential inefficiency of the collective outcome
- Sequential voting and non-neutral choice procedures
- Strategic behavior and manipulation

12.1.3 Voting Rules Decide the Winner

The next experiment demonstrates two things. First, different voting rules may lead to different results in an election, even when citizens' opinions stay the same. Second, the voting rules which are most commonly used in elections throughout the world are subject to a phenomenon called the 'path dependence effect'—namely, that current decisions taken by people, voters in this instance, are conditioned by their past actions, knowledge and experience. Thus, the same voting rule, and the same electorate may yield different results. In other words, voters do not simply vote mechanically each time they do so but learn from the past.

We now describe an experimental set-up in which 21 subjects vote among five alternative candidates, labeled *A*, *B*, *C*, *D* and *E*, symmetrically located at five distinct points on a horizontal axis, as in Figure 12.1.1. There is an extreme left candidate (*A*, in position 1), a moderate left candidate (*B*, in position 6), a centrist candidate (*C*, in position 10), a moderate right candidate (*D*, in position 14), and an extreme right candidate (*E*, in position 19). There are 21 points on the horizontal axis (from 0 to 20) and each of the 21 subjects is assigned a unique position on this axis. The experiment is carried out by having 21 sealed envelopes available at the beginning of the session, one for each participant. The envelopes contain the required material for the experiment and assign each subject a position on the axis.

Figure 12.1.1 Candidate positions



The goal of the experimental subjects is to elect a candidate who is as close as possible to their position. The subjects are told that they will be paid 20 Euros, minus the distance between the elected candidate's position and their own position. For instance, a voter whose assigned position is 11 will receive 10 Euros if candidate *A* wins, 12 if *E* wins, 15 if *B*, 17 if *D*, and 19 if *C* wins. For the classroom, the experiment leader or teacher can adapt this instruction by asking the students to *imagine* that they will be paid or will receive some other suitable reward.

The set of options—the candidates in this case—and the payoff scheme are identical for all elections in the experiment under three different voting rules. Elections go in sequences of four rounds under each of the three rules. Four times voters vote, the votes are counted, and the scores of each candidate and the winner are announced. During a sequence of four elections, nothing changes except that voters are informed of the results. Each voter stays at the same position on the axis for the four rounds of voting under each voting rule. Voters change position after each sequence of four. Cooperation and communication among voters are not allowed.

The objective is to vary the electoral rules and observe the results under each. The following describes an experimental session: one sequence of four elections uses the one-round Plurality rule (1R), often called First-Past-the-Post; Two-Round majority rule (2R), often called Runoff voting, governs one sequence; and one sequence is done under Approval Voting (AV). It is explained at the beginning of each series that the four elections in each series are held with the same voting rule. For each series, each participant is assigned one of the 21 positions at random (Figure 12.1.1). There is a total of 21 positions, and each participant has a unique position. The participants are given the following information: their own positions, and that each possible position is filled exactly once but not who occupies each position, the voting rule (1R, 2R or AV) is also announced. Voting is anonymous. After each election, ballots are counted and the chairperson publicly announces the five candidates' scores and the winner of the election.

In what follows, the experiment uses the following three voting rules, but it can be done with any other.

- *One-Round Plurality Rule (1R)*. This is a very simple single-round voting rule. Each voter votes for one candidate. The candidate with the largest number of votes is elected. Ties are broken randomly, by a flip of a coin for example. Plurality is mainly used in countries with an Anglo-Saxon tradition such as the UK, the US, and India.
- *Two-Round majority voting (2R)* A first vote takes place as in Plurality with each voter voting for one candidate. A candidate who receives more than half of the votes is elected; otherwise, the two candidates with the largest number of votes qualify for a second round, or runoff. At the second round Plurality is again used but, with only two candidates (and in the absence of a tie), one is guaranteed to win a majority of votes. Most countries other than the U.S. use some variant of Two-Round majority voting for presidential elections.
- *Approval Voting (AV)* Each voter can vote for/‘approve of’ as many candidates on the ballot as desired. The candidate receiving the most approval votes is elected. This rule is used in some professional societies, such as the American Mathematical Society. Approval voting is currently used in Fargo, North Dakota and St. Louis, Missouri.

What follows comes from data collected in 23 sessions in Lille, France (4 sessions, with 63 subjects), Montreal, Canada (8 sessions, of which four featuring 63 subjects), and Paris, France (11 sessions, of which six sessions include a third series under AV), with a total of 734 participants. All subjects in the experiments are students. All sessions used both 1R and 2R, only six Paris sessions used also AV. Table 12.1.6 summarizes the aggregate electoral outcomes and shows the percent of elections won by the various candidates.

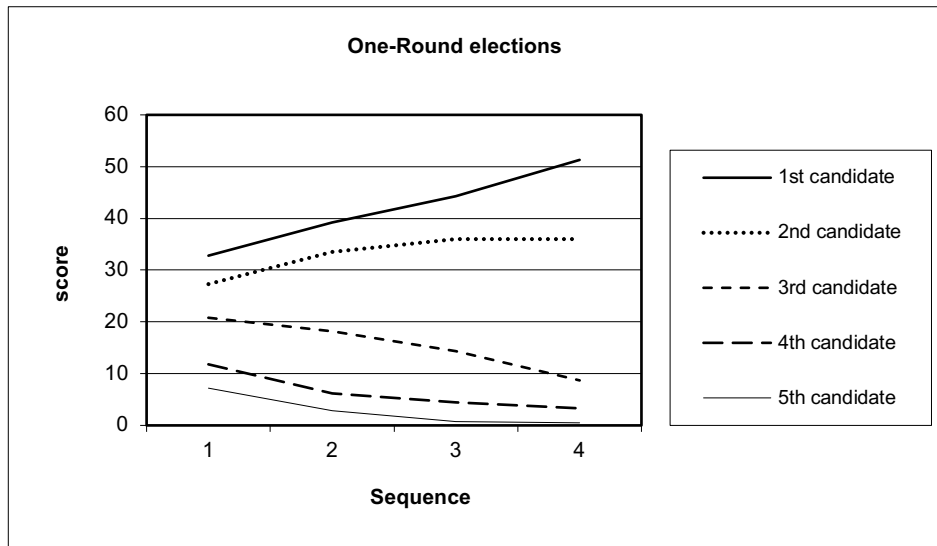
Table 12.1.6 Winning candidates (all)

	1R	2R	AV
C	49 %	54 %	79 %
B or D	51 %	45 %	21 %
A or E	0	0	0
Total	92	92	24

Whatever the voting rule, the extremist candidates (*A* and *E*) are never elected. 1R and 2R elections elect candidate *C* (the centrist candidate, a Condorcet winner in our case) in about *half* of the elections. Things are quite different under Approval Voting. In AV elections, *C* is elected in 79% of the elections.

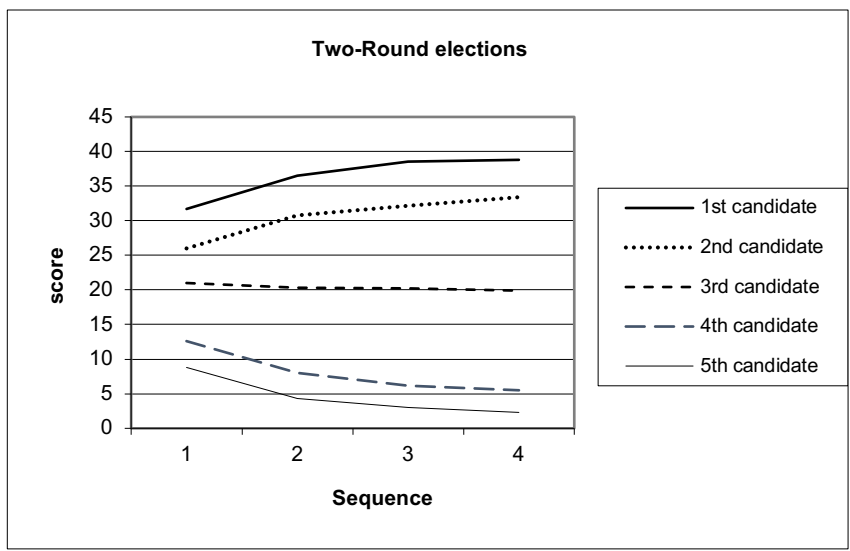
Figures 12.1.2, 12.1.3, and 12.1.4 show the percentage of votes, averaged over our 23 sessions (and 6 only for AV), obtained by the candidates ranked first, second, third, fourth and last over the course of the four elections held under the same voting rule (from first to last), for each electoral system.

Figure 12.1.2 Evolution of scores in one-round elections



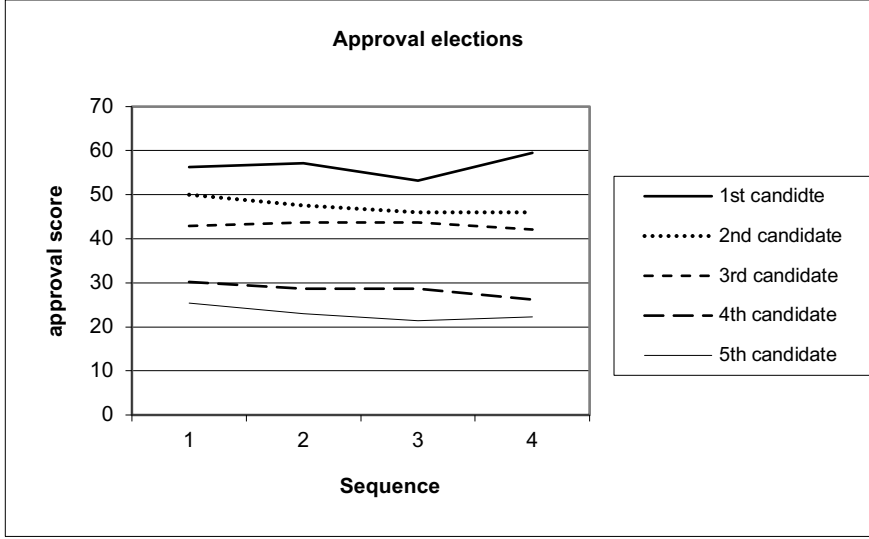
In the case of 2R elections, we consider only the first round.

Figure 12.1.3 Evolution of scores in two-round elections



For Approval Voting, the figures represent the percentage of voters who vote for the candidate; these percentages do not sum to 100.

Figure 12.1.4 Evolution of scores in Approval elections



Note that as time passes, votes gather around two candidates for 1R elections, or three candidates for 2R elections. The three viable or leading candidates are always the same for 2R elections—candidates *B*, *C*, *D*. For 1R elections, the pair of leading candidates is not the same in all elections—however, the leading candidate pairs are always from the set *B*, *C*, *D*. The picture for AV does not show any time-dependence effect.

These aggregate results confirm that such experimental set-ups can implement in the laboratory several of the theoretical issues about voting rules: namely, with the same preference profile, voting rules designate the Condorcet winner (Approval Voting), or designate a candidate which depends on history, that is, on observing past electoral development over time (1R and 2R).

The fact that the result of the popular vote under two of the three procedures depends on past elections in the sequence as it does in these experiments is known as a *path-dependency* effect. For elections, the path-dependency can be interpreted as the effect of public polling and voters' anticipations, but various voting rules are *more or less* prone to path-dependency. In the experiments described above, Approval Voting seems to be highly resistant to this pathology, unlike Plurality and Two-round Majority. These

findings are not absolute: note that, in the above experiment, Plurality as well as Two-round Majority would never elect the extreme candidates, thus restricting the uncertainty due to path-dependence. Many experiments of that kind have been done and show that Approval Voting, if not *totally* free of path-dependency, is more stable than Plurality and Two-round Majority.

12.1.4 Bibliographic Notes and Further Reading

Guth and Weck-Hannemann (1997) conducted an experiment described in section 12.1.1 on how people assess the value of voting rights in a democracy. The seminal work on the modern paradox of (not) voting is Downs (1957). Riker and Ordeshook (1968), Ledyard (1984) and Palfrey and Rosenthal (1985) provide further developments. For arguments supporting other motives for voting contradicted by the rational theory, see Fiorina (1976), Brennan and Hamlin (1998), Blais and Young (1999) and Blais (2000). In addition to Riker and Ordeshook's early observation that voters tend to overestimate their probability of decisiveness, see more recent experimental work by Duffy and Tavits (2008) and by Ditman et al (2014). See Feddersen, Gailmard and Sandroni (2009) for interesting experiments on the impact of non-instrumental, 'expressive' and ethical factors that prominently motivate voters. To explore the background to the Road and School experiment in section 12.1.2 as it relates to agenda setting and strategic voting see Holt and Anderson (1999). Van der Straeten et al. (2010) provides the background to section 12.1.3 on why voting rules can decide the winner in elections. Seminal experimental work by Forsythe et al. (1993) analyzes the impact of public polls, histories and path-dependence on election outcomes.

12.2 Real Political Elections

Field experiments experimentally manipulate (i.e., control) the *independent* variable—for example, specific policy devices, incentives, or the use of various voting rules; and observe the *dependent* variable—such as participation rate or voting outcome—in the real world. Compared to laboratory experiments where preferences are controlled by the design in a constricted environment, setting an experiment in the real world is meant to enhance the external validity of the experimental results—i.e., the conclusion of a scientific study remains valid outside the stringent context of this particular study, either because it can be generalizable, or transportable. Its generalizability across situations and across people notably depends on how *representative* the sample of subjects is. In particular, there may be a selection bias when samples are self-selected—for example, if only interested individuals wish to test alternative voting rules, observing their preferences for alternative voting rules after the experiment informs us about the characteristic of the sample, but tells little about how experiencing novel voting rules may impact preferences for rules. There are randomization methods, which we will introduce in this section, devised to reduce or avoid self-selection and improve the representativeness of the sample. Transportability requires that the result from a given population in a specific field is still valid when transported to another population; the more important features of the field should be well documented, so that we can identify when the conclusion can be transported. The design and analysis of such experiments hence require an understanding of their social and political context.

12.2.1 Participation in the USA: The Neighbors are Watching

As discussed earlier, turnout is more likely to be linked to a voter's sense of duty than to an actual cost-benefit comparison of the cost of voting in relation to the likelihood that a single vote may influence the outcome of the election. Note, that the link to 'duty' may well be indirect; for example, I know that my neighbor is likely to see whether I fulfill my civic duty as a good citizen or not. In this section, we will compare the relative importance of some of the different factors that may motivate people to vote, and explore what inducements are more or less efficient at getting people to vote and improving turnout.

Varied methods can be employed to persuade eligible citizens to vote: these include reinforcing the sense of duty, exerting peer pressure, threatening to shame registered voters who fail to vote, letting their neighbors know whether they voted or not, and so on. While in theory such methods may seem plausible, what induces citizens to vote must be determined empirically and perhaps experimentally. When evaluating which 'Get Out the Vote' policies or inducement methods are more efficient, mere observation is not enough. What makes people vote may be very complicated. If we observe that a district adopts a particular policy in one year, and turnout increases the next year, the increase may well be due to some cause other than the new policy. In more technical terms, to claim that a variable (policy/inducement method) is the cause of some fact (increased voter turnout), we must experimentally *control* that variable. In this case, we aim to claim that implementation of Policy A increases turnout, in all or most circumstances. Ideally, perfect control of a variable allows us to change this variable without affecting anything else. We can in this case observe things *ceteris paribus*, that is, 'everything else being equal', or 'held constant'. In this way, 'spurious correlations' produced by other variables than the manipulated variable can be eliminated. Allowing the researcher to control some of the variables under observation is the main reason why experiments are so useful.

The experiments which we will now describe provide clear examples from the methodological perspective. In this experimental context, a *policy* is a type of message sent to some citizens, the *participants*, about voting. The *treatment* refers to the precise design of these messages. Those who do not receive any message form the *control group*. One simply observes the turnout rates in the different groups. The object of the analysis in the experiment is to see how the turnout rate changes from one

treatment to another, and to determine what is the most likely to have caused the changes. Observations during the experiment may reveal a general trend which is unrelated to the policy. By referring to the control group for which no policy is being applied, we may explain and correct the general trend quite easily. If the trend is present in the control group too, then it cannot be attributed to the policy.

There may also be a *selection bias* in the experiment. This occurs when the observed result is not caused by the policy itself, but by the selection of the specific sample of subjects to whom we applied the policy. For instance, if we design a policy to increase turnout and apply it only to those who agreed to be the target of this policy, this target group is likely to include those who are most open-minded and receptive to political information and may be especially responsive to the policy. To avoid this problem, we need to focus on the results that can be attributable only to the policy, regardless of the subjects selected. Randomization, as typically used in medical research such drug trials, in biology, and more recently in economics and political science, is the tool used to circumvent selection bias.

Control and randomization are important methods to increase the validity of the results of field experiments in the social sciences. Their use in political science for studying turnout stimulating techniques goes back to the U.S. Presidential election in 1924, when H. Gosnell experimentally examined the effect of non-partisan mailings to potential voters on voting turnout. His experiment, took place in Chicago with more than 6,000 citizens (see section 12.2.4).

Gosnell first ordered a complete survey of all adult citizens in twelve districts of Chicago representing the diversity of the city during the months preceding the 1924 presidential election, The survey was a necessary first step in providing a typical cross-section of the population. In a second step, Gosnell randomly assigned the citizens in each precinct: to either the treatment or control group. The control group did not receive any experimental mailings. Each member of the treatment group received several mail messages designed to stimulate turnout spread over the pre-election period. If turnout was larger in the treatment group than in the non-treatment group, Gosnell could confidently attribute this to the policy applied in the experiment.

The next step was the analysis of the list of citizens who were registered to vote or not, who voted or not, or who left the sample for other reasons. This data was collected by the cross-checking of observations by watchers at the polling stations on the day of elections against registration lists, and other demographic information. At the end of the study, Gosnell undertook a complete survey based on 6,000 personal interviews designed to identify the reasons for abstention.

Besides the policy variations between the targeted treatment group and the non-treatment group in Gosnell's experiment, all factors were kept constant from one group to the other. This enabled the evaluation of whether the policy intervention was or was not effective in the first place. Which sector of the population that the policy was more likely to influence could also be assessed, since the factors influencing results could be observed: sex, mobility, foreign-language habits, derivative citizenships.

The experimental device consisted in sending mailshots (postcards in those days) to stimulate interest in registration and voting. Gosnell tested different versions of stimulation on registration and turnout. The first card was a factual notice on the importance of registration; the second card was also a factual notice on registration; there had also been some cartoons circulated depicting the non-voter as a slacker.

The results were the following: the treatment group did turn out and vote at a measurably higher rate than the control group. While the first two mailshots had similar effects, the cartoon had more effect among the women. The cumulative effect of the notices and cartoons resulted in 75% registration among the experimental groups, compared with a 65% registration among the non-experimented group. From this experiment, it was learned that a large part of weak registration was due to illiteracy, or more generally, with difficulties in understanding the English language. Personal interviews were helpful in highlighting these problems, and also in displaying personal motivations.

Long after Gosnell, the new millennium marked the beginning of a new series of field experiments in political science. We now turn to a large-scale experiment which also focused on the stimulation of non-partisan voter turnout in the USA.

We saw earlier that turnout may not only depend on internal personal reasons such as the 'sense of duty', but also on external reasons, like the interest I get from others knowing about my sense of duty. Policy interventions to increase turnout may vary with the reasons for voting. To know what policy is

most effective with respect to various motivations for voting, a masterpiece field experiment was conducted prior to the August 2006 primary elections in the US state of Michigan.

The experimental sample numbered 180,002 households. The sample was a randomized subset of the households, defined to include everyone with the same name at the same address, who appear on the official state voter registration list. The first step of the sampling process consisted in eliminating apparent errors and ambiguous cases in official files. The second step assigned households randomly to distinct groups: 99,999 households in the control group, and 4 distinct treatment groups of 20,000 households each.

- While the control group received no stimuli, all experimental groups were sent the message by post mail: “Do your civic duty—Vote”! A later mailing targeted each of the four treatment groups with different messages.
- The first treatment provides the baseline for comparison: it just emphasizes *Civic Duty*: “Remember your right and responsibility as a citizen. Remember to vote”.
- The second treatment captures a mild form of social pressure. It informs the citizens that researchers are observing them: “You are being studied”. Consistent with what is known as the “Hawthorne effect” discovered in the context of an experimental study on productivity at the Hawthorne electric plant (where workers were more productive when observed by scientists, rather than depending on lightening), the purpose of this treatment is to test whether mere observation may influence the dependent variable.
- The third treatment, called *Self*, uses a strong form of social pressure by informing citizens that who votes is public information, and lists the recent voting record of each registered voter in the household. The recipients of this message can read the list of all voters in their household and see whether each of them has voted in the last elections. A follow-up mailing reminds this group that each member of their household will be told who voted or not in the forthcoming election.
- The fourth treatment, called *Neighbors*, employs the strongest form of social pressure by listing the voting records of those living nearby. The participants can read the list of all voters in all households in their neighborhood. They can tell whether each of them has voted in the last elections. They also know that they will receive an updated chart of who voted after the current elections.

The results of the study were impressively clear and robust. Turnout in the control group was significantly lower than in the *Civic Duty* group, which was less than in the second treatment group, which was lower than the *Self* group, which was less important than in the *Neighbors* group. What we can conclude is that social pressure has a decisive role for turnout.

12.2.2 Electoral Campaigns in Africa: What Are We Voting On?

Voters take part in choosing their representatives collectively through elections. But what are the reasons and motivations that explain how they cast their ballot? An important topic concerns what voters are *voting on*. An optimistic view associates the act of voting with what voters think is best for the society—that is, for policies that are likely to improve its social welfare. At the other extreme, elections are seen as totally dissociated from any benefit to social welfare and as a device for corruption through vote buying or bribery. Others may think voting is just a means to defend their own personal interests in the form of *clientelism*. Clientelism, which is distinct from corruption, is defined as a transaction whereby politicians offer voters material favors in return for political support at the polls. This constitutes a form of interest-group politics.

The extent to which electoral politics is clientelist attracted much attention by those who study comparative politics and political economy. It is an important subject linked to the efficiency of budget spending, and whether the choice of a policy is directed towards policy goals and the general interest. Comparative political studies reveal that clientelism is more common in societies with low productivity, high inequality and rigidly hierarchical social relations. Its importance depends on cultural and historical

factors, levels of economic development, and the size of the public sector economy. But these generalizations do not help us to identify the variables which explain the intensity of clientelist linkages within countries. A field experiment was set up by the Beninese economist and political scientist, Leonard Wantchekon, specifically for this purpose in the first round of the 2001 Presidential election in the West African Republic of Benin.

Benin was considered an appropriate choice for a case study on clientelism because it is one of the most successful instances of democratization in Africa and has a long tradition of political experimentation. It was also a place where a field experiment could be conducted without any risk of impacting the election outcomes, because of the voting rule and of the political forces. The voting rule for the presidential election is the simple majority with runoff; and the composition of the second round was, whatever the experimental manipulation, a near certainty.

The field experiment results confirmed the standard view that clientelism dominates electoral politics in Benin, but Wantchekon's study went much further. It measured the relative electoral gain or loss associated with campaign promises based on clientelism at the local level—given the ethnic affinities of voters—as opposed to promises pertaining to public policy and the distribution of national public goods. Knowledge and understanding of the local political situation are necessary both for designing a suitable experimental procedure and for analyzing the results correctly. In this case study, a researcher concerned with clientelism but unaware of the local situation might well *neglect* the importance of ethnic affinities or gender differences, two key issues in understanding the workings of clientelism in Benin. Alternatively, a researcher may *overweigh* the importance of ethnic affinities and confuse key features of the context with causal factors. The design of Wantchekon's experiment intended to address these two challenges all at once.

It considered three core political actors: two political parties and a set of voters. The two parties, an incumbent party, and an opposition party, also differed in their ethnic affiliation and their electoral platforms. Ethnic affiliation determined which party a voter is more likely to support. The party platforms may have pledged to provide public goods for the benefit of all voters or favor only one group. Moreover, the party platforms may have promised provisions for the public good directed toward the national interest or toward local interests only. Promises made in the national interest of all citizens were viewed as public policy while the local interest pledges were viewed as indications of clientelism. Wantchekon aimed to discover how voters responded to public policy inducements compared to clientelist.

Among 84 electoral districts in Benin, only five or six were *politically competitive*—that is, quite evenly divided between the two main parties with election results that vary over time. Most districts were non-competitive: they were the *strongholds* of a party, giving the same party at least 70% of the votes in the two past presidential elections. Strongholds were typically ethnically homogeneous.

The non-competitive districts of Benin provided a unique opportunity for Wantchekon's field experiment as these districts had never been exposed to professionally designed national political campaigns. In fact, both dominant parties generally ran unstructured and poorly organized campaigns in these districts. Wantchekon managed to enlist the support and active collaboration of the parties themselves to run an experimental campaign with experimentalist candidates. The parties also agreed to adapt the messages of their party platforms for the clarity of the scientific case study.

Two villages in each district were selected. In one of them, researchers designed the party campaigns to capture voter attitude towards nationwide public policy campaign pledges—for example, in education, health care, infrastructure development and the like. In the other one, they designed the campaigns to capture voter attitudes towards clientelist policy: such as, government patronage at the village level for local jobs, services and other local public goods and benefits. As a result, in each district the clientelist party could be easily disentangled from the public policy party. The other villages of each district served as the control group.

Eight non-competitive districts—four incumbent-dominated and four opposition-dominated—and two competitive districts were selected for the experiment. In each, villages were randomly partitioned into three subgroups: one clientelist treatment village with more than 6,600 registered voters, in which voters were exposed to a clientelist-type message by the so-called experimentalist party; one public

policy treatment village with over 6,700 registered voters, in which voters were exposed to a broad nationwide public policy message defended by the so-called experimentalist party; and one control group composed of the remaining villages of the district totaling more than 28,000 registered voters, in which both types of campaign messages were run. After the elections, researchers conducted a survey in all ten experimental districts and interviewed a sample of voters in each district.

The treatment effects were computed with the actual vote returns—that is, the average vote choice variable, given that the vote choice variable takes a value of 1 if the voter votes for the experimentalist candidate running a campaign in the district, 0 otherwise. On aggregate, the mean of voting returns (with standard deviation in parentheses) for candidates in the *clientelist* villages, *policy* villages and *control* villages were respectively 0.84 (0.05), 0.69 (0.30) and 0.74(0.25). These numbers supported a clear positive clientelist treatment effect on average and a negative public policy treatment effect on average.

Some exceptions drew attention, and describing them illuminated some salient characteristics of Beninese politics, regarding, for example, national versus regional, northern versus southern, and incumbent versus opposition divides. For instance, the north-south divide is important in understanding the politics and economy of Benin, with the southern part of the country being more exposed to global affairs and the north more isolated. Clientelism was observed to be a more effective strategy for the campaigns of northern candidates and in districts controlled by the incumbents. Public policy was more effective in the south and in districts controlled by the opposition.

Another important result of the study was the observation of a gender effect in the public policy treatment. Women reacted positively to public policy issues, showing their preference for public goods, especially in the north and in districts controlled by the opposition.

Younger and older voters exhibited similar voting behaviors and political preferences. In the end, clientelist oriented campaigns were shown to work well for all types of candidates, and especially well for regional and incumbent candidates. By comparison, a field experiment conducted in the West African island republic of Sao Tome and Principe just a few years later, revealed that vote-buying is more effective for challengers.

When considering the reasons for these conclusions, Wantchekon shows that the credibility of clientelist appeals and accessibility of public goods strongly influence voting behavior. For instance, incumbent candidates have the means to make clientelist appeals more convincing by delivering a part of the promised goods before the election. Women are more likely than men to respond positively to public policies appeals, such as child-welfare policies and public goods, because most clientelist policy disproportionately benefit men over women.

These experimental results demonstrate that we cannot support that voting is entirely determined by ethnic determination in Benin, far from it. They also show that clientelism is not inevitable nor universally accepted, even among poor voters.

The results of such specific studies as Wantchekon's and others that followed later are important, especially in terms of policy implications. First, since incumbents have a distinct advantage in political competition with clientelist policies, term limits, clean campaign financing and an independent electoral commission should be introduced to limit or regulate their power. Second, as women are less sensitive to clientelist messages, initiatives to promote women's participation in the political process at all levels of government are likely to improve the provision of public goods. Third, while vote buying favors party candidates and improves turnout, it is also harmful for the long-term public good. We know that measures to protect the secrecy of the ballots to stop coercion, intimidation and bribery on observable voter behavior have been proven to reduce the prevalence of the phenomenon. Fourth, since messages such as 'voting in conscience' are quite efficient at reducing vote-buying, and vote-buying and clientelism afflicts less educated and poorer voters, any policy that improves voter education bundled together with local development is likely to reduce their impact.

12.2.3 Parliamentary Elections in Europe: The Voters Have Their Say

The type of information needed to understand how voters might behave under alternative electoral

systems is inaccessible in real-world election contexts. However, researchers have circumvented this problem by inviting real voters to cast experimental ballots for real candidates in real elections but under alternative electoral rules. Researchers intend to imagine what voters would do, should the voting rules be different. This is a relatively recent method in political research, and there are now several such experiments. Note that this method cannot be called a *survey* because the results are based on a thought experience of participants and observing their counterfactual behavior; it cannot be called a field experiment because individual behavior in this experiment does not have any impact on the actual outcome of the election. This is why the authors soberly talk about ‘online experiments’. We focus now on a Europe-wide experiment conducted online.

The *EuroVotePlus* experiment took place in several European countries during the three weeks before the 2014 elections for the members of the European Parliament. A consortium of researchers created the website *eurovoteplus.eu* where visitors could obtain information about the electoral rules used in different European member states for this election, vote with different electoral systems and even test them in a pan-European election.

The EU Parliament, back in 2014, was an assembly of 751 members (MEPs) from 28 countries. Each member state elects its MEPs by universal suffrage in their respective countries. Countries are allocated a certain number of seats following a principle of degressive proportionality: for example, the smallest state of Malta with a population about half a million, along with the slightly larger states of Luxembourg, Estonia, and Cyprus, have 6 seats each, while the largest is Germany with a population of about 85 million and has 96 seats. Each country is free to choose the system it uses for electing its members subject to the European requirements that elections be held under universal suffrage by secret ballot, using some system of *proportional representation* (PR). However, each country can use its own type of PR, so a large variety of ‘proportional’ electoral rules are in use for the election of the European Parliament. See Module 10 for a more detailed introduction to PR.

The proportional voting rules in use vary. Some countries are divided into smaller constituencies, and most countries use party-list systems but with different thresholds of representations. A principal issue is the degree of flexibility voters have in their choice of candidates. Under *closed-list* systems, voters can vote only for a party-list as a whole, and the party decides which candidates will occupy the seats allocated to it. Under *open-list* systems, voters can vote directly for candidates.

Most voters are not aware that a variety of voting procedures are available, because each country typically uses the same system in both their national and European elections. Thus, observing the reaction of voters to different systems requires experimentation.

The *EuroVotePlus* study was open to any individual from any country. National and local media throughout the EU advertised the website, which was accessible in 14 languages.

First, the website described three voting rules—based on the systems used in France, Latvia and Luxembourg for the European elections—and explained some of their advantages and disadvantages.

1. The French voting procedure for European elections is a Closed-List PR system. Citizens vote for one party’s list which ranks its selected candidates. Seats are allocated to parties in proportion to the votes they receive. The ranking of the candidates on a party’s list determines who on the list will be elected. Notice the districting and the voting procedure is in France only specific for the European elections.

2. The Latvian system is an Open-List PR system with *preferential voting*. Voters vote for one party list and seats are distributed to a party in proportion to the votes it receives. After choosing a list, a voter can reject candidates by crossing out their names, and select others by giving them a ‘+’. The score of a candidate equals the number of voters who chooses the list, minus the number of voters’ rejections, plus the number of voters’ ‘+’s received. Within a list, candidates are ranked and elected according to their scores.

3. The Luxembourg system is an Open-List PR system with *panachage* and *cumulative voting*. Panachage allows the voter to vote for candidates on any list. Cumulative voting gives each voter as many votes as there are candidates to be elected, which may be freely distributed among candidates (except that no more than two votes may be go to a single candidate). Votes are counted by lists, and candidates within each list are elected according to their total votes.

Second, the *EuroVotePlus* website not only presented these three distinct systems, but participants could also cast simulated ballots according to each of them. Each visitor could hence vote three times, in the same order—the French, the Latvian, then the Luxembourg system—and each voter would vote for the same list of candidates, all of whom were from the voter’s own district. The simulation of these national elections reveals the effect of these voting rules on the outcome of the election.

Third, in addition to these three PR voting rules, visitors were also invited to participate to a simulated pan-European election: in this experimental setting they could vote directly for the European delegates of the European parliament through pan-European party lists. In order to make the voting situation more realistic and the result more robust, the list of candidates of this simulated election belonged to the seven existing political groupings of the members of the European Parliament at the time of the election. For each of these seven lists, for each voter, ten candidates were randomly selected from the registered members of the political groups coming from across all Europe for each participating voter. Hence each voter would receive a different list of candidates. The participants would be invited to vote for these candidates, successively under the three voting rules tested above. This study was meant to compare the impact of national with trans-national election for the European election, to reveal whether voters rather tend to vote for the representatives of their country or of their party, and to inform about the impact on gender.

Fourth, after voting three times—or six times if they also agreed to vote in the pan-European elections—the visitors were asked to fill in a questionnaire with standard items pertaining to their social status, political leaning, their opinions on the three voting rules, and on the opportunity to vote for pan-European representatives rather than for national representatives.

The website received around 22,000 visitors between May 4 and May 26, 2014, of whom 3,673 took part in the voting experiment. While this sample of voters was not representative of the overall European electorate, data from the actual electoral results could be used to weight the survey responses to produce more representative data.

Many lessons can be derived from the data. They enable us to assess the effects of electoral systems on individual voting behavior and election outcomes in the countries in which these voting systems are used. For example, the Swedish data from 499 national respondents show that party scores are similar under closed and open lists. Furthermore, the same candidates were elected in both cases, except for just one candidate—the single-elected candidate from the centrist party was third on the list with the open list system. However, differences are substantial when panachage is introduced. The panachage rule appears less favorable to the main parties and more favorable to small parties. More markedly, while the impact of these rules on the number of seats won by each party is clear but small, the effect on which candidates are elected is significant: compared to its official results (where the elected candidates are the top-ranked candidates from the party lists), 15% of the Swedish delegation would be replaced using the Luxembourg rule, and 25% would be replaced using the Latvian rule.

In these experiments, participants are usually questioned afterwards about various aspects of the systems tested. Most voters tend to say that they prefer the systems that gave them more opportunities to express their preferences than the current ones. This observation should be qualified.

First, some research published in 2015 highlighted that ideology matters: more conservative voters are less attracted to such expressive systems. This characteristic may be linked to differing conceptions of representation and democracy associated with various political ideologies.

Secondly, the opinions expressed by the participants in these experiments are usually based on quite limited political experience, and they judge the varied electoral rules by considering the form of their ballots only, while many other aspects of a voting rule matter. When subjects are better informed things may change. Research in Italy has been particularly interesting since Italy entirely changed its voting system in 1993. It switched from an open-list to a closed-list PR system, as the Italian open list system was considered responsible for generating a patronage style of politics. Researchers who studied the Italian data of the *EuroVotePlus* experiment observed that older participants, who had experience of the open list system, were less supportive of it than younger Italian participants, who knew this system only from the experiment.

The European experiments also enabled researchers to test the reactions of European citizens to the

prospect of a Pan-European constituency and to examine how they might vote. The opportunity for panachage appealed to the subjects and clearly favored female candidates. Participating voters—and especially females—welcomed the ability to allocate their votes across the open party lists. With the panachage system, voters also tended to vote for candidates of their own country, and this effect was greater in the smaller EU member states. The pan-European simulations also revealed a puzzling divide between countries. Candidates from Sweden, Germany, Portugal and Finland attracted the votes of most European citizens, while candidates from Poland, Hungary, Italy and Romania received the least.

Compared to laboratory experiments, these kinds of studies have the advantage that the objects of decision are real candidates and parties rather than fictitious alternatives over which subjects have controlled preferences, as is usually the case in laboratory experiments. A difficulty with this method is that the sample is self-selected: open participation tends to overrepresent individuals who are more interested in answering on-line surveys. As we have seen, this can be compensated for by appropriate weighting of responses.

Field and online experiments, whether randomized or not, can provide researchers with a contextualized setting within which they can apply theoretical knowledge to specific cases. The experiments also augment studies in comparative politics, allowing deeper analysis in particular cases by identifying which parameter is the most important to focus on. Experiments have become a necessary companion to theoretical politics and standard political science, helping them to make sense of politics in practice in the real world.

12.2.4 Bibliographic Notes and Further Reading

Section 12.2.1 focuses on what experiments have taught us on turnout. Green and Gerber (2004) provide a survey of experimental methods in political science. Gerber et al. (2008) provide a general review of the impact of social pressure on voter turnout. Gosnell (1927) describes his seminal large-scale field experiment in Chicago and uses various techniques to ‘stimulate’ eligible citizens to vote. Following Gosnell, Gerber et al (2008) conducted large-scale experiments in Michigan employing modern analytical techniques to isolate the major stimuli on voter turnout. Section 12.2.2 focuses on the motives of vote casts. The ambitious field experiment in Benin by Wantchekon (2003) considers that votes may motivated by clientelism, public policy, given that ethnic affiliation may also be an important determinant. He provides the evidences that efforts on campaigning may affect clientelist or public policy parties differently, depending on age, gender, southern or northern regions in Benin. Vicente (2008) found *challenging* candidates gained most from vote-buying. Published results from an experiment jointly by Vicente and Wantchekon (2009) provided new insights and understanding that in turn had important government policy implications. Laslier et al. (2015)’s *EuroVotePlus* on-line experiment sets the background for section 12.2.3. This type of study parallels the practical with the theoretical approaches to voting behavior and introduces participants to alternative voting rules, most often for the first time. Blais et al. (2015) noted from further analysis of the *EuroVotePlus* data that personality traits as well as ideology impacted voters’ attitudes to alternative voting systems. Bettarelli et al. (2016) provide a study of the *EuroVotePlus* data in Italy on voter preferences and electoral systems.

12.3 Analyzing the Voter's Mind

This section describes three more experiments on turnout, voting rules, and the psychology of voters, now analyzed using more sophisticated tools.

12.3.1 Participation in the Lab

As stressed in section 12.1, the standard economic argument regarding rational participation in mass elections predicts that a voter will not take time to vote as the probability of being a pivotal voter is tiny. As we have seen, the evidence does not support this prediction. A second prediction is that turnout should be higher when an election is very close between two or more candidates. Research suggests this prediction has some validity (see section 12.3.5 for references to research and experiments in section 12.3).

At any rate, a simple cost-benefit analysis doesn't explain why masses of voters take part in real elections. However, that a portion of the population follows this logic and votes only in very close elections is also compatible with observation. Nevertheless, other motivations influence most of the population. In addition to observational data, a more rigorous test of rational participations predictions can be set up in a laboratory setting in which individuals are placed in situations in which political and social motivations—such as social pressure social norms—are removed, so that only the potential benefits expected from the outcome of elections remain. In such circumstances, voters may develop a purely instrumental and individualistic way of reasoning.

Such laboratory experiments can be set up quite readily. In the simplest versions, the experiment is organized as a game—the words 'election' and 'vote' are never used—and participants, split in two groups, do not 'vote' but 'buy tokens'. In the experiments that we discuss below, however, we do use the terminology appropriate for voting and elections. The decision to buy tokens or not is individual, but the group that buys the highest number of tokens wins. Many variations of this laboratory experiment have been carried out. All results are similar to what is observed in real elections; turnout rates are higher than what standard rational choice theory would suggest. We will now describe some experiments of this type, which clearly invalidate the standard rational choice predictions.

The first experiment we shall discuss was held in Brussels and Montreal in January and February 2011 with groups of 21 participants in each location randomly distributed on a 0 to 20 scale. The experiment had three parts—referred to as Experiment 1, 2 and 3 respectively both in the original study and in our discussion below. The participants, who were instructed not to communicate with one another, knew only their own position on the scale, and that positions would be randomly redistributed after each election.

Two series of ten elections were held: the first under single round Plurality Voting (1R), and a second one under Proportional Representation (PR). For each election, there are two candidates or parties located at positions 5 and 10 on the scale. Participants can either vote or abstain. Their monetary payoff is 16 points minus the distance between the position of the winning candidate and their own position. The cost of voting is one point. Results, which include the election outcome plus personal payoffs, are announced after each election. The winning position under 1R is the party (*A* or *B*) with the highest number of votes; the winning position under PR is the average of the votes received by both parties. For instance, if 50% of the votes go to *A* and 50% to *B*, the winning position is the position 10, thus modeling a kind of coalition between parties *A* and *B*.

The two main hypotheses to be tested here, H1 and H2, are based on a simple model of rational participation. The model predicts that participants vote if and only if the expected benefits of voting (rather than abstaining) are higher than the cost of voting; that is, only if they believe that their vote will have a decisive impact on the outcome of the election.

According to this basic model of rational participation, H1 pertains to aggregate turnout: it should be significantly higher under 1R than PR. This point, though not obvious, derives from the equilibrium

analysis of the game. Recall that a Nash equilibrium in game theory (see Box XX and Module 8) refers to the state reached in a game in which no player can earn more by changing strategy, that is, every player's strategy is a 'best response' given the choices of the other players. We will not go into how the participation rates are derived, but simply state the conclusions. If we suppose that an individual participant behaves rationally, and that all the others do the same, then it can be computed that most of the Nash equilibria under 1R are characterized by participation rates typically above 85%. On the other hand, on all types of equilibria considered under PR the theoretical participation rates remain below 30%.

Note that because H1 is based on the notion of equilibrium, it can be rejected even if most voters act rationally. In fact, the best response of a rational participant may depend on what other voters do. If even a small number of voters are not playing a best response, then it may well be rational for the other voters to play strategies which are not equilibrium strategies. It is logically possible that most (but not all) participants follow strategic rationality even if the aggregate outcome is far from equilibrium. Therefore, H1 is not a strong test of rational theory, and it can be rejected even if many voters are behaving rationally.

The aggregated results of the first Brussels/Montreal experiment are as follows: the average turnout under 1R is 72% and 69% under PR. Even if the participation rate under 1R is higher than under PR, this small (3%) difference does not support H1, which predicts respectively at more than 85% and less than 30%. The higher turnout under PR is particularly puzzling with respect to the theory that predicts a 30% ceiling. An explanation could be as follows: these average turnouts could hide a learning and coordinating process and thus participation rates could progressively increase under 1R up to 85% and more, and decrease under PR. But the observations do not match this idea. On the contrary, the participation rates are progressively decreasing for both voting rules in similar proportions: the turnout is 75% on average for the first three elections and 71% for the last three under 1R, compared correspondingly to 74% and 65% under PR. The authors of the experiment further note that the locations in Montreal or Brussels had no influence on the participation. Location might well have been expected to be a major influence since 1R corresponded to the official Canadian voting rule and PR to Belgium's.

The second hypothesis, H2, directly concerns individual decisions. According to H2, only potential voters whose expected payoff of voting is positive will actually vote. It should be stressed that H2 is the stronger test of the standard rational choice theory, for H1 can be rejected even if most voters act rationally as noted above.

For each participant, the differential between the number of points received for taking part in the election (positive or negative), and the number of points for abstaining, is computed. An individual who decides to participate in the election will always vote for the party closest to her position. If the individual's vote is not decisive--- that is, does not change the outcome ---, the differential is -1 because of the cost of voting (which is 1). In such cases, and according to the standard rational theory, the individual lacks any 'objective' support for any action and thus should not vote. Conversely, an individual whose vote is decisive and has an expected payoff larger than 1 has objective support and therefore should vote.

Table 12.3.1 shows that under 1R, and for Experiment 1 described above, masses of people take part in the election, whether or not they have 'objective' support for doing so. Therefore, H2 is not supported.

Table 12.3.1 Turnout under Plurality (1R) elections

	Experiment 1	Experiment 2	Experiment 3
Among subjects who objectively should vote	82%	82%	60%
Among subjects who objectively should not vote	71%	68%	56%
Among subjects who subjectively should vote	72%	79%	51%

Among subjects who subjectively should not vote	76%	67%	60%
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An explanation for the participation rates on a mass scale could be that subjects do not predict well enough others' behaviors; namely, if their decision to vote is not objectively justified, they might be consistent with their 'wrong' beliefs. To test this explanation, some groups of subjects were asked to predict the number of voters for *A*, for *B* and of abstainers. The same differential can be computed, but based now on the prospective voters' subjective predictions. The turnout rates for this second differential are given in the two last lines of Table 12.3.1. Again, one can see that, under 1R, most subjects vote whether they are subjectively justified in doing so or not.

Another explanation for these high rates of participation could be that subjects do not have the time to learn how best to respond to the situation, because their position changes on the 0 to 20 scale at every single election. Experiments 2 and 3 address this issue. In Experiment 2, a series of elections now consists of three blocks of three elections each. Participants keep the same position during three elections. In Experiment 3 the participants keep the same positions during a whole series of ten elections again, as in Experiment 1. Experiments 2 and 3 were held in Montreal and Paris in 2012. The objective and subjective differentials were computed with these two variants and the obtained turnout rates are presented in Table 12.3.1. Except from a strong decrease in participation for Experiment 3, in all the experiments, subjects do not seem to consider, either objectively or subjectively, what they should or should not do. There is, therefore, no evidence of a learning effect.

Under PR, the test of H2 offers quite similar results, leading to the disqualification of H2 for this voting rule too. For all three experiments, both the objective and subjective differentials are negative for almost all individuals under PR, meaning that subjects should always abstain. Yet, the average turnout is 69% for Experiment 1, 76% in Experiment 2 and 72% in Experiment 3.

Taken together, the results of these experiments demonstrate that the standard rational choice theory is largely unable to explain the observed high participation rates. Moreover, econometric analyses of the same data also show that under PR, and in contradiction with rational predictions, participants vote more when their preferred party is strong and turnout is high, a phenomenon commonly known as the 'bandwagon' effect.

12.3.2 Strategies in the Lab

Even if rational choice theory, based on the extremely low probability of being decisive, is not sufficient to explain the substantial level of turnout observed in mass elections, that does not imply that it cannot explain at least part of voters' behavior in the voting booth after they have incurred the 'cost' of voting. This section examines experiments in the laboratory that focused on *how* people vote, that is how they choose to fill in their voting ballot. From this perspective, the main question experimenters try to answer is related to 'strategic voting'.

The term *strategic voting* sometimes does not have the same meaning in the economic and political science literatures. In the tradition of economics, strategic voting is a synonym of rational voting, whereas in political science, strategic voting usually means non-sincere voting. In many situations, it is in the best interest of the voters to simply state their true preferences through their votes. For instance, under Plurality Voting, if a voter's preferred candidate is one of the two main contenders of the election, the voter has a good, rational, reason simply to vote for that candidate. This is a common case that an economist would traditionally count as a strategic vote, but not a political scientist. When the meaning is not clear, it is therefore preferable to use the more precise expression 'strategic and non-sincere' vote to refer to strategic voting in the political science sense.

While many theoretical voting models take for granted that people systematically vote in a strategic (but possibly sincere) way, the question remains widely open in practice.

As the collective choice literature has developed, two different branches can be distinguished. One

focuses on problems with only two options such as elections with only two candidates. In such cases, the strategic behavior seems intuitive and clear: voters vote for their preferred candidates. However, a game-theoretical literature has developed following a paradox devised by economists and political scientists, David Austen-Smith and Jeffrey S. Banks, and related to ‘pivot reasoning’ in uncertain situations. The paradox is derived via a return to the original work of Condorcet in the 18th century on collective binary decisions under uncertainty, the so-called Condorcet Jury theorem. The paradox theoretically showed that if the members of a jury vote following the information they have without any strategic considerations—that is, in accordance with Condorcet’s hypothesis—the resulting situation does not form a Nash equilibrium. This subtle problem had remained unnoticed in the history of political science from the 18th century until the developments of the rational theory of games. The observation gave birth to a new stream of research and has also been experimentally tested, but we shall not further follow this line of research about binary choice.

In this section the focus turns to research about collective choice with more than two options, or elections with more than two candidates. It will be seen that, in these cases, rational choice theory often provides useful insights. Rational choice predicts the ‘desertion’ by the voters of all but two candidates in single-round Plurality voting (1R). If it is clear to the voters who the two main competitors are, voting for any other candidate is to ‘waste’ one’s vote. In general, these two ‘main’ candidates could be any two, so that some form of coordination between voters is necessary. In practice, the coordination is achieved as a result of history, publicity, media exposure and polling, and leads to the marginalization of other candidates.

Laboratory experiments clearly confirm the reasoning process. An experiment conducted in 1993 at the University of Iowa in the United States consisted of two sessions with 28 participants per session. Each participant received a voter identification number and was first assigned to a subgroup with 14 players. A subgroup comprises three types of voters (O, G and B) with 4, 4 and 6 voters, respectively. The payoff of each participant depends on the winning candidate and on the participant’s own type. Table 12.3.2 thus gives the preferences for each voter’s type.

Table 12.3.2 Payoffs in dollars depending on the voter’s type and the elected candidate

	Candidate O	Candidate G	Candidate B	Nb per type
Type O	1.20	0.90	0.20	4
Type G	0.90	1.20	0.20	4
Type B	0.40	0.40	1.40	6

The two subgroups vote under single-round Plurality, and after the election, voters are randomly reassigned to new groups and new types. Each subject took part in 24 elections, totaling 48 elections in an experimental session. In one of the sessions, players had no information on the voting choice of the other voters. In a second session, non-binding polls were conducted prior to each election. In both sessions, aggregated results were announced after each election.

Within each group of 14 players, it is clear that candidate B loses two-way races against either of the other candidates (6 voters against 8). As the majority of voters are equally divided between candidates O and G, under Plurality voting, and without coordination between voters, the winning candidate is B, the Condorcet loser. However, when polling is introduced prior to the elections, asking for vote intentions, the results obtained are dramatic. In the session conducted without polling, the frequency of candidate B, the Condorcet loser, winning the election is 87.50%, while it drastically decreases to 33.33% with polls. This demonstrates the ability of voters to vote strategically and to coordinate their efforts based on the polls. The aggregate results clearly show that the outcome of the subsequent election varies according to the scores predicted by the polls—whether they favor candidate O or candidate G, or predict a tie between them. In short, the coordination of voters is relatively simple to obtain in the case of Plurality (1R), but it has been demonstrated that this is not the case for the two-round voting system (2R).

In the experiment described in section 12.1.3, the research team studied the progressive emergence

of manipulation of the subjects' own preferences for the different voting methods they tested—notably one-round Plurality (1R), Plurality with Runoff (2R) and Approval Voting (AV). The authors devised three distinct theoretical behaviors that might be followed by the players—pure strategic voting, sincere voting, and heuristic voting—and tested the respective theoretical predictions on laboratory data.

For most voting rules the notion of sincere voting is clear. When the voting rule requires the voter to select only one candidate from the ballot, a sincere voter simply votes for the candidate she most prefers. When preferences are known, as is the case in the laboratory setting, the 'sincere voting' theory generally predicts accurately. The prediction is even unique, except if the voter is indifferent between several candidates she prefers to the others. A remarkable feature of sincere voting is that it depends only on each voter's own preferences and is independent of the other voters' preferences or votes or of the history of past votes. However, for Approval Voting the notion of sincere voting is not clear because there is no obvious way to decide where to draw the line between approved and non-approved candidates given a voter's preference ordering.

Strategic voting in the decision-theoretic sense depends, not only on a voter's own preference but also on the voter's knowledge or beliefs about the other voters' likely choices. This is because strategic reasoning requires the decision-maker to consider the consequences of the actions available. In particular, a single vote makes a difference only in very specific circumstances. Thus, in the case of voting, the consequences depend on what the others do. To test the 'strategic voting theory', that is whether participants vote strategically or not, one has thus to make an hypothesis about these beliefs.

A specific hypothesis of central theoretical importance is the so-called 'rational expectation' hypothesis. The idea is that the belief of the decision-maker is not systematically wrong; either she knows exactly the world she is living in, including what the other players are doing, or, at least, the probabilities she assigns to the various possibilities show no systematic bias. The assumption that beliefs are correct is the one used in the definition of the Nash equilibrium and in most models in theoretical economics.

In between pure sincerity and perfect rationality, it is reasonable to introduce intermediate notions that would capture the idea that real actors might be rational *to some extent*. As a scientific principle, the concept of 'bounded rationality'—that rationality is limited and imperfect—does not allow for precise and testable theories in most context, but it may be helpful in the case of voting. Following an ad hoc decision rule that is not derived from first principles but may capture part of these principles, is called heuristic behavior. For example, in the first round of a two-round election, voting for the candidate a voter most prefers among the three candidates whom the voter believes are the going have the largest first round scores, is a well-defined if not always rational behavior. Moreover, it captures some degree of rationality. Call this heuristic the 'Top- Three' rule. This rule seems to be rational if two candidates are to be elected under plurality voting. The first round of a two-round election bears some resemblance with this situation; the difference is that instead of being elected, two candidates qualify for a second round.

Voting for the candidate a voter prefers among the two candidates whom the voter thinks are going have the largest scores is another heuristic behavior called the 'top-Two' rule. In a one-round election, this heuristic behavior is in practice equivalent to pure rational behavior. At the first round of a two-round election, it is not. It is certainly not irrational, and one can even say that it captures some degree of rationality.

In the laboratory, under 1R, the Top-Two strategic theory outperforms the sincere voting theory to explain voting behavior when the elections are repeated.

Under 2R, all these theories differ. The Top-Three rule, is the best one, doing better than both sincere and rational theories. This can be explained by the surprisingly high level of complexity the rational calculus requires at the first round of a two-round election. On the one hand, a voter must anticipate all the possible outcomes for the first round as well as their consequences for the second round. On the other hand, the voter must also reason in terms of *backward induction*. That is, from the choice made at the second round, the voter must deduce the result desired at the first round. Moreover, the whole reasoning process must take place within the context of very small 'pivot probabilities'.

Rational prescriptions, in these circumstances, can be quite counterintuitive; for example, advising

voters to vote for a candidate in order to later favor the election of another one. Voters rarely follow such recommendations.

Experiments in economics have shown for a long time that, in practice, backward induction reasoning is seldom used. This has also been proved to be true as well for 2R voting, even when the extent of the backward reasoning required is small—2 steps only in 2R voting. Overall, the comparison between the kind of behaviors used under 1R and 2R shows that when the reasoning to follow is not too complex, voters vote in a more strategic way; and in the other case, they rely on simpler heuristics.

Further laboratory experiments focusing on different voting methods—such as the Borda rule or single transferable vote—describe strategies voters use under those rules. Regarding strategic voting, the conclusions of these studies are in line with our previous statements. In the laboratory, voters tend to vote strategically if the reasoning is not too complex. When things become too sophisticated, they adopt other kinds of reasoning, using various heuristics, which are only partially rational.

12.3.3 Voting Rules In Situ

The experiments conducted in the laboratory naturally focus on outcomes and thus induce voters to play in a rational way to increase their monetary payoffs. To put it plainly: they are just payed to focus on outcomes. Because of this, laboratory experiments neglect important aspects of voting rules and how voters actually behave in real elections. For this reason, an alternative—yet complementary—stream of experiments dealing with the behavior of *real* voters in the context of *real* national elections has emerged in recent years.

The first large-scale experiment on alternative voting methods was conducted in France by a team of researchers in 2002 during the first round of the French presidential election. This seminal experiment generated a novel methodology, on the basis of which large-scale political experiment since conducted in France and elsewhere has proceeded. The term *In Situ* was the name given by the American political scientist, Bernard Grofman, to the methodology that will be discussed now.

A few weeks before the official list of candidates for the French presidential election was announced, the *In Situ* research team sent a letter to all registered voters of the official voting stations in which the experiment was meant to take place. This letter contained information needed regarding the experiment, including its date (the same day as the first round of the official election) and location (either in the same room or close to the room of the official election), its aims (academic research), objectives and the principles of the voting methods to be experimentally investigated. After casting their vote in their official voting station, participants were invited to take part in the experiment and to vote again using alternative voting methods. The experiment provided participants with ballots, envelopes, voting booths, and a ballot box. Research assistants managed the experimental voting stations.

All these *In Situ* experiments tested *plurinominal* voting rules which allow voters to give their opinion on all candidates on the ballot. The simplest example of such a rule is Approval Voting (AV), the single-round voting rule under which the voter gives a binary statement (approve or not) about each candidate. A typical experimental voting ballot included three steps: a brief statement on the experiment and on its scientific stakes, the experimented voting ballot(s) per se, and a short questionnaire to be completed afterwards, about the participant and her voting behavior.

We will now focus on three *In Situ* experiments that took place during the first round of the French presidential elections in April 2002, 2007 and 2012 and which gathered 2,587 participants from five voting stations, 2,813 participants from six voting stations, and 2,320 participants from five voting stations, respectively. During the 2012 French presidential election, one of the research teams in addition carried out an important internet-based experiment with more than 8,000 participants aimed at testing various voting methods.

One of the aims of these experiments was to examine how voters actually use the various voting methods. Here, we will see how they are able to give new insights on two common voting phenomena: candidates who remains ‘above the fray’ and candidates subject to a ‘center squeeze’.

Most democracies, whatever their electoral system, have more than two parties, Even in a Plurality election, the concentration of almost all votes on only two candidates, as is the case in the United States, is a rare phenomenon. Pushing this idea to an extreme point, a theoretical 'above the fray' situation describes the failure to elect the candidate (among three or more) seen as the best by most voters. Under Plurality voting one can imagine that most voters support one or other of two quite unpopular candidates (for example, because they have been nominated by the two major parties) because, as a matter of *individual* calculation, voters believe that it is useless to 'waste their votes' on a third minor party or independent candidate.

While the loss of a candidate preferred by most voters is plausible under Plurality voting, it is not with Approval Voting (AV), because this rule does not preclude the possibility of voting for two or more candidates.

The conclusions of the *In Situ* experiments, which aimed at assessing and comparing how voters vote under the official Two-Round Runoff system used in French Presidential elections, and AV corroborate the theory that AV is not subject to the 'above the fray' paradoxical phenomenon. In fact, research shows that many voters took advantage of the opportunity to support several candidates. The aggregated statistics of AV gathered in 2002, 2007 and 2012 clearly demonstrate this point.

The overall trends are similar for all of the three experiments. On average, each participant supported 3.15 candidates out of 16 in 2002, 2.23 out of 12 in 2007 and 2.63 out of 10 in 2012. The average number of approvals thus remain quite stable over the experiments. Table 12.3.3 shows the number of approvals per ballot in each year.

Table 12.3.3 AV statistics of the number of approvals per ballot in 2002, 2007 and 2012

		0	1	2	3	4	5	6	7	8	9	10	10+	Total
2002	ballots	36	287	569	783	492	258	94	40	16	6	1	5	2,587
	%	1.4	11.1	22.0	30.3	19.0	10.0	3.6	1.5	0.6	0.2	0.0	0.2	100.0
2007	ballots	120	736	905	673	264	75	23	13	1	1	1	1	2,813
	%	4.3	26.2	32.2	23.9	9.4	2.7	0.8	0.5	0.0	0.0	0.0	0.0	100.0
2012	ballots	73	465	619	616	320	154	38	18	6	3	8	0	2,320
	%	3.1	20.0	26.7	26.6	13.8	6.6	1.6	0.8	0.3	0.1	0.3	0.0	100.0

To cite an example, the table can be read as follows: in 2002, 569 participants approved of two candidates in their ballot, which is 22% of the 2,587 participants. Note also, that the peak values of the distributions of approvals are typically two or three candidates, only a few voters chose to support six or more candidates, and generally fewer than one quarter of participants give one approval only. All these indicators confirm that the participants in the *In Situ* experiments took advantage of the opportunity offered by AV to express their electoral preferences more fully. They did not lock themselves in the rational of the single name ballot. Consequently, under AV, candidates would not suffer under non-viability beliefs. The behavior of voters as observed in these experiments show that the 'above the fray' paradox could not exist under plurinominal voting methods.

The second, and more common, non-desirable voting phenomenon is the 'squeezing of the center' phenomenon which can occur under 2R. In the first round, voters tend to desert candidates from the extreme left and right, producing more support for center-left and center-right candidates, thereby dramatically decreasing the possibility that a centrist candidate can go to the runoff.

This situation is troublesome for a centre party as their candidate often appears to be the Condorcet winner that would win a two-way race against either other candidates, in particular against the two main candidates from left and right, and so would win a second-round vote if he could get into it. . This paradoxical situation is a typical feature of 2R voting and has major consequences for real politics outside the laboratory. Its effect is a bipolarization of the political spectrum along the line defined by the two moderately left and right parties. Notably, it has shaped the French political landscape for a long time.

However, the ‘squeezing of the center’ that afflicts 2R does not occur under approval voting and other similar plurinomial voting rules. The results of the 2002, 2007 and 2012 *In Situ* experiments confirm this assertion. During these years, the French centrist candidate was François Bayrou, founder of the Democratic Movement party. He was never able to reach the official runoff, despite the substantial of votes he received on first rounds (6.84% in 2002, 18.57% in 2007 and 9.13% in 2012). Under AV rules in the experiments, his political strength is revealed: his percentages of support are comparable to those of the leaders of the main parties (the moderate right and left parties). 27.1% of the participants approved him in 2002 (against 36.7% and 32.9% for the right and left leaders, respectively) and 39.2% in 2012 (against 40.47% and 49.44%). In 2007, 42.8% of the participants would have approved him, and with such a score, he would have won the 2007 French presidential election.

In short, all these studies of the impact on plurinomial voting rules and comparing them with official voting methods, offer a contrasting picture. Voters know how to act rationally, as the official results show, but at the same time they want to express their electoral preferences more accurately. When alternative voting rules provide the chance to do so, voters do not hesitate to take advantage of this opportunity.

12.3.4 The Voter’s Psychology

It has been stressed in earlier sections of this module that even when voters could easily adopt a behavior close to the standard rational theory in some circumstances—under simple voting methods such as one-round Plurality Voting, for example—they rarely do. Although their individual votes will very rarely be decisive in an election, masses of voters turn out seemingly for two kinds of reasons. First, when the voting method is more complex, under 2R for instance, they prefer to follow heuristics such that their behavior is neither purely rational nor totally naïve. Second, most voters also seem to have a strong desire to express their electoral preferences, even though their ballots will essentially never be decisive. Such a motivation could help explain the paradox of participation, as well as the results obtained in the *In Situ* experiments discussed earlier.

Perhaps personality of voters is a key factor in explaining whether they adopt strategic behavior. The literature on personality traits is abundant in political behavior and political psychology. It addresses for instance the link between personality and ideological orientation or political participation (see section 12.3.5 for references). In this final section, we describe an experiment related to the potential influence of personality traits on strategic voting. It aims to explain strategic voting with the help of five personality traits, the so-called *Big Five*: Openness to Experience, Conscientiousness, Extraversion, Agreeableness, and Emotional Stability.

Strategic voting is not an easy or natural way of voting. First, the voters have to order all the candidates from the most to the least liked, which is not easy when there are a large number of candidates. Second, voters need to assess the viability of the contending candidates. To do this properly requires collecting information from various sources—traditional and social media, newspapers, polls, and so on. Thirdly, voters need to understand the details of the voting rule being used to process information regarding viability effectively and to maximize their payoff. These three specific aspects of strategic voting imply that the ability and propensity to vote strategically might depend significantly on the personality of voters. The research team for the experiment postulated that, each of the Big Five personality traits might have a distinct influence on strategic voting, as follows.

Openness to Experience The research and literature on political behavior suggests that the openness to experience trait, which is associated with creativity and imagination, is associated with liberal ideologies and voting for left parties. Moreover, it is believed to lead as well to riskier behaviors such as drinking or smoking. This would mean that such people would be more willing to think strategically, to take risks, and to adjust their voting behavior accordingly. Thus, a positive correlation between openness to experience and strategic voting is expected.

Conscientiousness Political psychologists link the conscientiousness trait with the ability to be organized and hard-working. Regarding political attitudes, conscientious people tend to support

conservative parties and to stick to their views in most circumstances. Therefore, this personality trait might be more in line with sincere voting.

Extraversion People with this personality trait tend to interact socially, participate in politics, and engage political discussions. Such outgoing people are more willing to take risks and thus to vote strategically.

Agreeableness Associated with kindness, honesty and altruism, it is difficult to associate agreeableness with any specific political attitude. However, agreeable people are less likely to be competitive and calculating, and are expected to vote sincerely.

Emotional Stability Emotionally stable people are less likely to suffer from stress and anxiety. While anxious people would be less prone to engage in strategic calculations, the authors support the idea that higher emotional stability increases strategic behaviors.

Turkish college students participated in an experiment that aimed to assess the role of personality traits on strategic voting. Each session included 18 participants and six sessions were conducted. Before starting the experiment, and once settled in the laboratory, students completed a questionnaire that included measures of their personality traits and political attitudes. They had to assess the way they thought of themselves in terms of the Big Five personality traits using a five-point scale ranging from strongly disagree to strongly agree. After reading the instructions and taking part in a trial session, the students started the voting experiment using proportional representation with a threshold (the official Turkish voting rule) that consisted of six series of three elections.

Each participant was assigned a *first* position on a horizontal axis numbered from 1 to 19, except the middle position 10 remained empty. Eight parties were represented on the axis, and form two coalitions. A left coalition consists of positions 2, 4, 6 and 8, and a right coalition at positions 12, 14, 16 and 18. For each election, participants had to vote for a party, and their positions changed at each new series. To win a seat, a party had to obtain at least three votes during the first two sessions, and four votes in the last one: the *threshold of representation* was three or four. After each election, results were publicly announced, including the number of votes cast for each party, the number of seats for each party, the winning coalition (left or right), and the leading party with the most seats within the coalition. The payoffs were computed as follows: participants belonging to the winning coalition received five points, plus a bonus of five points less the distance between the leading party in the winning coalition and their position. No bonus was awarded if the distance was greater than five.

The experiment was designed so that voters have an incentive a priori to vote for the party closest to their position. But since participants are aware of the uniform distribution of the others along the scale, they cannot expect that every party will reach the threshold to win a seat if everyone votes sincerely. When the threshold increases from three to four votes, a coalition may not win any seats. Due to the threshold of representation, participants need to coordinate their vote to avoid wasting them.

To analyze the data, the researchers defined a variable to measure strategic voting among participants whose preferred party did not reach the threshold in the previous election. The strategic voting variable was 0 if such participants continued to vote for their preferred party, and 1 if they changed their voter to a party that was above the threshold in the previous election. Thus, strategic voting is not measured in the first elections of each series. Table 12.3.4 reports the main findings of the experiment.

Table 12.3.4 The influence of the Big Five traits on strategic voting

	Expectations	Finding
Openness to Experience	+	+
Conscientiousness	-	not significant
Extraversion	+	not significant
Agreeableness	-	-
Emotional Stability	+	not significant

Openness to experience and agreeableness appear to have positive and negative effects on strategic

voting respectively. However, the other three personality traits do not appear to be related to strategic voting. The analyses shed light on two other features of strategic voting. First, strategic voting increased as the experiment progresses. Second, the increase in the electoral threshold from three to four votes increases the rate of strategic voting, thus showing the ability of voters to adapt. These two factors indicate the presence of a learning effect as the experiment progresses.

This experiment suggests that many voters in mass elections may feel a dilemma between two kinds of considerations. On the one hand, voters probably want to express their true preferences (a desire that likely led them to vote in the first place). On the other hand, in certain circumstances they may decide to cast more votes that are more effective but contrary to their true preferences. A major advantage of ‘pluri-nominal’ rules such as Approval Voting is that they can reconcile both kinds of considerations and thereby minimize the voter’s dilemma. From this perspective, the laboratory and the *In Situ* experiments are complementary. Among many interesting issues pointed out by the research in these relatively new fields, the comparison between strategic practices in and outside the laboratory appears to be very promising in helping us to understand voting behavior.

12.3.5 Bibliographic Notes and Further Reading

For simple lab experiments on participation see Schram and Sonnemans (1996); Tyran (2004); Levine and Palfrey (2007) and Sauger et al. (2012). Blais et al (2014) is the main reference for the lab experiment to assess the influence of the rational theory on voting under Plurality and PR. See also Blais (2000) for further research on rational theory predictions, and on turnout in close elections.. Forsythe et al. (1993) is the background lab experiment to section 12.3.2 focusing on the importance of polling and history on voting behavior. See Austen-Smith and Banks (1996) for their game-theoretical paradox on ‘pivot reasoning’ derived via the Condorcet Jury Theorem and tested subsequently by Guarnaschelli et al. (2000). Cox (1997) explains how rational choice predicts the ‘desertion’ by the voters of all but two candidates in single-round Plurality Voting. Van der Straiten et al’s (2010) experiment observes and categorizes the manipulation of voters’ own preferences under different voting rules. Experiments in McKelvey and Palfrey (1992) showed backward induction reasoning to be used only rarely, later proved by Van der Straiten et al (2010). Igersheim, Baujard and Laslier (2016) studied voters’ strategies under Borda rule and STV in their *In Situ* experiments. For the origins of the *In Situ* method of experiment started by French researchers in 2002, see Laslier and Van der Straiten (2008). Baujard et al (2014) is the main reference to the Voting Rules *In Situ*, section 12.3.3. A more comprehensive study of the *In Situ* experiments and its methodology must include those that experiments that paralleled the French presidential elections in 2007 and 2012 covered respectively in Baujard, Igersheim and Senné (2011), and Baujard et al (2014). For an analysis of the failure to elect most favored candidate in the theoretical ‘above the fray’ situation see (Myerson 2002). Blais and Erisen (2016) is the central reference to the final section on voter psychology. Carney et al (2008), Blais and Labbé St-Vincent (2011) and Gerber et al. (2013) are a select few of the studies on the role of personality traits in political behavior.

12.4 References

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