

Voter Turnout and the Dynamics of Electoral Competition in Established Democracies since 1945

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with assistance from

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Introduction

Voter turnout regularly makes news. Seemingly, whenever an election is held, the question comes up: How many people voted? Sometimes the turnout is unexpectedly high. Commentators were amazed at how many people stood for hours in the hot African sun waiting to vote in South Africa's first truly free and universal election. But it is rare to see stories about higher than expected turnout. More often we see stories that express concern at the fact that turnout is lower than expected – so much more often, indeed, that one might be forgiven for supposing that low or declining turnout was ubiquitous in contemporary democratic elections. One prosaic reason for this is the newsworthiness of turnout decline. Stable turnout is not news. Moderately increased turnout is not news. Low or declining turnout is newsworthy. So commentators draw attention to the level of turnout mainly when it is down. How many people are aware that turnout was higher at the American presidential election of November 2000 than at the previous presidential election, in 1996? Of course, the level of turnout in the more recent of those elections was overshadowed by its other, more newsworthy, features – butterfly ballots and such. But the lack of press attention given to increased turnout, when it occurs, is one reason why we have this general perception that turnout everywhere is in decline. What is true is that, whenever turnout is down, the decline makes news.

The reason why declining turnout makes news seems to be because it allows commentators to pontificate about the dire state of democracy in the country concerned. Low electoral turnout is often considered

to be bad for democracy, whether inherently or because it calls legitimacy into question or by suggesting a lack of representation of certain groups and inegalitarian policies (Piven and Cloward 1988, 2000; Teixeira 1992; Patterson 2002; Wattenberg 2002). Above all, low turnout appears to be seen by commentators as calling into question the civic-mindedness of a country's citizens and their commitment to democratic norms and duties. Indeed, falling turnout is often seen as a mark of disengagement, if not of actual disaffection (Teixeira 1992; Dalton 1999; Norris 1999).

That turnout should be a mark of civic virtue is not self-evident. In the early days of empirical social enquiry, those who studied turnout (Merriam and Gosnell 1924; Gosnell 1927; Boechel 1928; Tingsten 1937) took it for granted that turnout would be higher when an election's outcome hung in the balance and when "issues of vital concern are presented" (Boechel 1928:517). Seen in this light, low voter turnout would be blamed on parties and politicians for failing to present issues of vital concern – or for failing to present such issues in an election where the outcome was seen as likely to determine the course of public policy. Thus low voter turnout would have been blamed on the character of the election, not on the characters of those who failed to vote.

It was the rational choice approach to explaining political behavior that changed our ideas about why people vote. Writing in 1968, Riker and Ordeshook, elaborating on the ideas of Downs (1957), pointed out that the chances of any one vote affecting the outcome of an election for nationwide public office were virtually zero – even in a close race. For this reason, they went on to argue, people (unless they had quite unreasonable expectations about the importance of their vote) could not be voting with the purpose of benefiting from the outcome (Riker and Ordeshook 1968:28). Whatever the benefits any individual might receive as a consequence of policies adopted or blocked by an election's outcome, those benefits would be enjoyed whether the individual voted or not. So the only rational reason for an individual to vote would be to gain nonmaterial benefits, such as the satisfaction of pulling one's weight and other aspects of civic virtue.

In the years that followed the publication of this argument, those who studied electoral participation seldom paid attention to benefits that might accrue to voters from the outcome of any specific electoral contest. Instead they focused on voting as a habit that people

learned during their formative years – a learning experience dominated by education and social status.¹ In their seminal work *Participation in America*, Verba and Nie (1972) built their explanation of electoral participation on what they called a “baseline model” consisting of income, occupation, and education. This baseline model (later renamed the “resource model”) dominated explanations of individual turnout decisions in the United States and elsewhere until the present time (Wolfinger and Rosenstone 1980; Parry, Moiser, and Day 1992; Rosenstone and Hansen 1993; Verba, Schlozman and Brady 1995, Wernli 2001). In recent research, the resource model has been joined by a “mobilization model” that takes into account the fact that people also vote because they are mobilized to do so by parties, interest groups, and candidates (Rosenstone and Hansen 1993; Verba, Schlozman, and Brady 1995). But in all of this work the focus for explaining why people vote is centered on the individual and things that happen to the individual rather than (as in earlier research) on the election and things about the election.² In the light of this focus, it is not surprising that commentators should take low or declining turnout to be a reflection on the capacity and motivation of individual citizens.

Yet the idea that declining turnout is due largely to “something about citizens” runs counter to some very obvious facts. In the first place, turnout varies from election to election both up and down; and while it is possible to imagine secular trends in civic virtue, it is hard to imagine what would cause it to fluctuate both up and down from election to election. Moreover, if civic virtue drives turnout, why does virtue have more effect in some elections (U.S. presidential elections, for example) than in others (U.S. midterm elections, for example)? The same citizens vote in a presidential election who fail to vote in the following midterm election. Presumably it is not something about those citizens that makes them more likely to vote in some elections than in others.

In the second place, turnout varies enormously between countries. There are countries (like Australia, Belgium, and Malta) where virtually

¹ The dominance of education and social status has not been found ubiquitously outside the United States (see Chapter 1).

² The mobilization model, of course, serves to some extent to bridge the gap between the two (see Chapter 1).

everyone votes. If high turnout is due to “something about citizens,” then how come the citizens of these three countries are so different from the citizens of the United States and Switzerland (two countries where turnout in national elections is particularly low)? It is true that in Australia and Belgium voting is compulsory, but the law that makes abstention illegal does not affect the character of those countries’ citizens. On the contrary, what it affects is the character of the elections in those countries; and, if compulsory voting can affect voter turnout, then perhaps other things about the character of elections can also affect voter turnout. Voting is not compulsory in Malta, so (unless we want to assume that Maltese citizens are uniquely civic-minded) it seems clear that there must be at least one other feature of elections’ character that can bring about universal turnout – or perhaps a combination of several features.

The purpose of this book is to take a closer look at these and other puzzles that bedevil the study of voter turnout. Its argument is that Riker and Ordeshook’s view of elections and electoral behavior was incomplete (cf. Whiteley 1995). When we allow ourselves to suppose that some citizens might be motivated to vote primarily by a self-interested desire to gain the political benefits that victory by one party or candidate promises to bestow on them (or to prevent those benefits from going to someone else), then both of the just-mentioned puzzles resolve themselves. Indeed, it is by demonstrating that these (and other) puzzles do resolve themselves that we prove (insofar as such things can be proven) that the approach to understanding voter turnout set out in this book improves on the conventional wisdom.

The importance of the book lies in its ability to establish how voter turnout serves as an indicator of the health of a democracy and to enumerate the conditions that can result in low voter turnout. Most of the commentators who decry declining turnout seem to assume that the development is consequential – either that falling turnout is bad in itself or that it is an indicator of bad things happening to the society. However, falling turnout might be incidental to deliberate changes made in a country’s electoral system or other political arrangements. Thus, the abolition of compulsory voting would result in lower turnout, but this would have been an anticipated consequence of a reform that was enacted nevertheless. Lower turnout might also be accidental, if an election occurred on a day of particularly bad weather (for example).

Accidental developments can be expected to reverse themselves in due course. The weather is a clear factor of this kind, but other developments may be hard to place in terms of whether they are accidental, incidental, or consequential. A major objective of this book is to distinguish accidental and incidental developments from consequential ones and to make suggestions for reforms that might alleviate the consequential problems that have led to low or declining turnout in certain countries.³

In Chapter 2 we will revisit the assumptions made by Riker and Ordeshook and show how they can be elaborated in the light of more recent political science research. This will enable us to build a model of voter turnout that rests on a more elaborate set of assumptions – a model whose assumptions and implications are the real subject of this book. But first, in Chapter 1, we describe the various puzzles in greater detail and set out our strategy for solving them.

In brief, this book seeks to revive and develop a long-neglected approach to the study of voter turnout that focuses on individual motivations.⁴ The strategy is to pull together a number of building blocks that come from different branches of political science – research into the rational underpinnings for the decision to vote, research into the socialization and immunization of new voters, research into voter turnout at the aggregate and individual levels, and research into the generational basis of political change – and use them to develop a theoretical edifice that is both more elaborate and more comprehensive than those previously constructed to examine voter turnout. This framework generates hypotheses at the level of the country, election, electoral cohort, and individual voter. These hypotheses in turn are tested by using, on the one hand, datasets constructed from survey data for all of the elections ever studied, back to the 1960s, in every country (all six of them) for which there is a continuous series of such studies for every

³ We do not investigate the deleterious effects that low voter turnout may bring by virtue of the unrepresentative nature of those who vote when turnout is low. These effects are in need of more careful research than they have previously received (see Chapter 8), but that is not the purpose of this book.

⁴ Research on aggregate-level turnout often assumes such motivations while evidently failing to test for their existence (such a test cannot be implemented at the aggregate level). The mismatch in findings between individual-level and aggregate-level studies gives rise to one of the puzzles that this book seeks to resolve (see Chapter 1).

national election held since then, and, on the other hand, a specially collected set of aggregate data spanning fifty-five years from the end of World War II, covering all of the national elections conducted during that period in all of the twenty-two countries that have an accessible and continuous statistical record of free and fair national elections held since within one electoral cycle of the end of that singular global convulsion – 356 elections in all.

The hypotheses that we derive from our theorizing fly in the face of a number of common presumptions about turnout and its decline. In particular, the findings of this book are not expected to support the notion that turnout declines (where it does) because of a shortfall in civic virtue and dutiful behavior or because of political alienation or disaffection. Electoral turnout, we will contend, is not (generally speaking) about how people approach elections; rather, it is mainly about how elections appear to people. The theoretical viewpoint developed here expects turnout to vary either when elections change their character or when demographic shifts change the sizes of groups that pay attention to the character that elections have. Features of the character of elections that affect turnout are hypothesized to include some of the rules under which elections are conducted (including the electoral system that is employed), features of the party system at the time of each election (particularly its fractionalization and cohesion), and features of particular elections (including how much time has elapsed since the previous election and whether its outcome was too close to call). Most of these features are encapsulated in the concept of electoral competition, and our model could be typified as the “electoral competition” model of voter turnout.

Notably absent from our hypotheses are expectations that voter turnout in established democracies will be affected by changes in the resources of individuals or of the social structures in which they are embedded (though the age structure of the electorate is expected to be important). These common presumptions are, however, investigated to see whether they add anything to the electoral competition model that is built to test this book’s hypotheses.

It is important to stress at the outset that our hypotheses are intended to apply to established democracies – expectations for transitional and consolidating democracies would be different (as explained in Chapter 1), but those are not studied in this book for reasons that

will become clear as our theoretical framework unfolds. Our objective is to arrive at general conclusions that would apply to any established democracy. Democracies that are not yet established should in due course display the same dynamics as are shown in the countries that we study. In Chapter 7 we will discuss the ways in which democracies that are not yet established can be expected to display different dynamics from those that we find in established democracies.

The book is organized as follows. Chapter 1, as already stated, describes the key puzzles that will be addressed in the remainder of the book. Chapters 2 and 3 set out the theoretical framework that will guide our investigations – and that will give rise to the hypotheses put forward in later chapters. Chapter 3 also investigates fundamental aspects of this framework at the level of electoral cohorts. Chapter 4 presents a number of case studies and statistical analyses that support the operationalization of variables to be employed in subsequent chapters. Chapters 5 and 6 use those variables to test hypotheses (derived from the theories developed in earlier chapters) at the level of the country, election, and individual. Chapters 7 and 8 elaborate on the findings, establishing to what extent they account for actual changes in turnout that have occurred in the twenty-two countries that this book studies and discussing the implications of those findings in terms of the likely future evolution of turnout and measures that could or should be taken to avoid, or even reverse, future turnout decline.