

WHAT AFFECTS VOTER TURNOUT?

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■ Abstract Why is turnout higher in some countries and/or in some elections than in others? Why does it increase or decrease over time? To address these questions, I start with the pioneer studies of Powell and Jackman and then review more recent research. This essay seeks to establish which propositions about the causes of variations in turnout are consistently supported by empirical evidence and which ones remain ambiguous. I point out some enigmas and gaps in the field and suggest directions for future research. Most of the research pertains to established democracies, but analyses of nonestablished democracies are also included here.

INTRODUCTION

The dominant view in the literature is that the existing research on voter turnout has established some robust patterns, that we know relatively well why turnout is higher in some countries than in others, and that the main factors that affect variations in turnout are institutional variables. My verdict is different. Many of the findings in the comparative cross-national research are not robust, and when they are, we do not have a compelling microfoundation account of the relationship. And the impact of institutional variables may be overstated.

THE PIONEER STUDIES

The study of voter turnout started with Powell's (1982) award-winning book, *Contemporary Democracies*, which posited electoral participation as one of the three main indicators of democratic performance, and two *American Political Science Review* articles by Powell (1986) and Jackman (1987).

Powell's APSR article examined mean turnout in 17 countries in the 1970s. He found turnout to be higher in countries with “nationally competitive districts” and “strong party-group linkages.” Nationally competitive districts enhance turnout because “parties and voters have equal incentive to get voters to the polls in all parts of the country” (Powell 1986, p. 21), and vote choice is simpler when and where groups (e.g., unions, churches, professional associations) are clearly associated with specific parties (Powell 1986, p. 22). Powell's main conclusion is that

American turnout is inhibited by its institutional context, and the main emphasis is on party-group linkages, which is the most powerful variable in his model.

Jackman's (1987) article followed in the same spirit, with an even stronger emphasis on institutions. Jackman looks at mean turnout in 19 countries in the 1970s, and he comes out with much cleaner results, showing that five institutional variables affect turnout: nationally competitive districts, electoral disproportionality, multipartyism, unicameralism, and compulsory voting.

Jackman was inspired by Powell's work, but the specific set of variables he retained was different. Most importantly, Powell's main factor, party-group linkage, was left out, because it was found to have no systematic effect. It was Jackman's set of variables that defined the research agenda.

A number of comments can be made about Jackman's study. First, although the emphasis is on institutional factors, one of them—the number of parties—should be considered as the consequence of the institutional context. Second, two variables—national competitive districts and electoral disproportionality—are aspects of the electoral system. They are correlated with each other (larger districts produce more proportional outcomes), and it is not clear why the two should be incorporated into the same model. Third, Jackman's analysis does not include any socioeconomic variable. A fuller model should integrate the role of the socioeconomic environment.

Powell's 1982 book considers a greater array of countries, 29 in total, although the analysis of turnout is restricted to 23 cases. His model distinguishes three blocs of variables: the social and economic environment, the constitutional setting (institutions in the strict sense of the term), and party systems and election outcomes. In his final path analysis model (figure 6.1, p. 121), Powell (1982) identifies four significant variables: one socioeconomic (gross national product per capita), two constitutional (proportional representation and mobilizing voting laws), and one party system (party-group linkage).

Powell's sequential model, which identifies a distant set of variables (socioeconomic), an intermediate set (institutions), and more proximate factors (party systems and election outcomes), seems quite useful. I review the evidence on the effects of these three types of factors, beginning with the impact of institutions, which has been the focus of most research.

THE IMPACT OF INSTITUTIONS

Jackman identifies three institutions that appear to foster turnout: compulsory voting, the electoral system, and unicameralism. Other institutional variables have also been proposed.

Compulsory Voting

Jackman (1987) estimates that compulsory voting increases turnout by about 13 percentage points. This pattern has been confirmed by every study of turnout in western democracies, and the magnitude of the estimated impact is almost always

around 10 to 15 points (Blais & Carty 1990; Blais & Dobrzynska 1998; Franklin 1996, 2004; Blais & Aarts 2005). “Compulsory voting increases turnout” can be construed as a well-established proposition.

This raises more questions. Must compulsory voting legislation be accompanied by sanctions in order to be efficient? What kinds of sanctions are more prone to induce recalcitrant citizens to go to the polls? How “tough” must these sanctions be? How strictly must they be enforced? The literature provides precious little to answer these questions.

Perhaps there is more to be learned from the experience of nonestablished democracies. Norris (2002) finds that compulsory voting increases turnout only in “older” democracies, and she speculates that the law may be enforced less strictly elsewhere or that its impact is conditional on the presence of broader norms about the desirability of obeying the law. Fornos et al. (2004) develop a four-point compulsory voting scale, and they report a strong impact of compulsory voting on turnout in Latin America, the region with the highest frequency of compulsory voting laws. They do not sort out, however, the specific contribution of sanctions and their degree of enforcement. Finally, Blais et al. (2003) examine the effect of compulsory voting with and without sanctions in their sample of 61 countries, covering both established and new democracies. They find that compulsory voting makes a difference only when there are sanctions (they do not examine the effect of enforcement).

In summary, we know that compulsory voting increases turnout and that its impact depends on its enforcement. But we do not know how strict that enforcement must be in order to work. We know nothing about the public’s awareness and perceptions of the law and its implementation. And there are no comparative analyses of the determinants of turnout in countries with and without compulsory voting. This is an unfortunate state of affairs. If a sense of duty is a crucial motivation for voting (Blais 2000), most people should be predisposed to vote, and loosely enforced, light fines should be sufficient to produce a high turnout. And according to rational choice, the factors that shape the decision to vote or not to vote should be very different when there is a concrete financial cost associated with abstention. In short we know nothing about the microfoundations of compulsory voting (Achen 2002, but see Bilodeau & Blais 2005).

Electoral System

Jackman (1987) finds that turnout is higher in systems with nationally competitive districts, the reason being that in large districts parties have an incentive to mobilize everywhere while some single-member districts may be written off as hopeless.

Jackman’s four-category ordinal variable takes into account the electoral formula and the size of the districts. Further research has utilized either the same variable, or dummy variables that distinguish electoral formulas, or a summary disproportionality index (also included by Jackman). Studies that have been confined to advanced democracies (Blais & Carty 1990, Jackman & Miller 1995, Franklin 1996, Radcliff & Davis 2000) as well as one study of turnout in post-communist countries (Kostadinova 2003) have confirmed that turnout is higher in

proportional representation (PR) and/or larger districts, whereas research dealing with Latin America reports no association (Pérez-Liñán 2001, Fornos et al. 2004), and an analysis that incorporates both established and non-established democracies concludes that the electoral system has a weak effect (Blais & Dobrzynska 1998). See Blais & Aarts (2005) for a more detailed review of these studies.

There are two possible interpretations of the available evidence. The more optimistic view is that PR increases turnout except perhaps in Latin America, a region where there is some dose of proportionality in every country. The more pessimistic view is that once one moves outside Europe there is no generalized correlation between the electoral system and turnout. I lean toward the second, more skeptical position. On the one hand, the study by Fornos et al. (2004) that comes up with negative results in Latin America is at least as methodologically sophisticated as research on established democracies. On the other hand, as I indicate below when discussing the impact of the party system, those studies that come up with positive results have failed to specify how and why PR fosters turnout.

Unicameralism

Jackman's (1987) last key institutional variable is unicameralism. He shows that turnout is significantly higher in countries where power is concentrated in one legislature. The reason is that, when there are two chambers, power is usually shared between the two and "elections for the lower house play a less decisive role in the production of legislation where bicameralism is strong" (Jackman 1987, p. 408). The more powerful the body that is being elected, the stronger the incentive to vote. We would expect turnout to be particularly low when and where the legislature has little power. Jackman uses a scale [proposed by Lijphart (1984)] with the highest score for unicameral countries and the lowest score for countries in which the upper house is as powerful as the lower house.

Jackman focused on the division of power between the lower and upper houses, but the same rationale should apply to the division of power between the president and the legislature, between the central government and subnational (or supranational) governments, or between the government and the courts. The general proposition is that the more powerful the body that is being elected, the higher the turnout.

Surprisingly perhaps, the findings about the impact of unicameralism on turnout are mixed. Positive results are reported by Jackman (1987), Jackman & Miller (1995), and Fornos et al. (2004). However, Blais & Carty (1990), Black (1991), Radcliff & Davis (2000), and Pérez-Liñán (2001) indicate no effect. Siaroff & Merer (2002) find support for the hypothesis that turnout is lower where there is a "relevant" directly elected president and where there are strong regional governments. Blais & Carty (1990) and Black (1991) indicate that turnout is not higher in federated countries. All in all, the studies that have looked at specific indicators of the relative power of lower chambers relative to other institutions have not systematically confirmed the conventional wisdom that turnout is higher where the lower chamber has greater leverage.

Perhaps what is needed is a summary measure of the “power” of national lower houses that takes these many dimensions into account. Blais & Dobrzynska (1998) created an “electoral decisiveness” scale that considers the presence or absence of subnational elections in federations, upper house direct elections in bicameral countries, and presidential direct elections. They find a strong positive correlation with turnout, but their results have not been replicated.

Franklin (2004) pays close attention to parliamentary responsibility in his account of turnout change in established democracies. Franklin’s main concern is to explain why turnout increases or decreases over time in different countries. His key variable is competitiveness, which I consider below. He also argues that turnout in legislative elections increases when parliamentary responsibility increases, and that it decreases when parliamentary responsibility is weakened. The former is exemplified by Malta gaining independence in the 1960s and decisions of the legislature no longer being subject to ratification by a British appointed governor. The creation of a government cartel in Switzerland after the 1960s, which made elections meaningless, is an illustration of the latter.

It is hard to believe that turnout is unaffected by the salience of an institution. Yet the empirical evidence on that question is ambiguous. The challenge is to come up with reliable scales that encompass the different dimensions of salience. The measures used in the extant research are not very satisfactory.

Other Institutional Variables

At least two other institutional factors have been shown to affect turnout: voting age and rules designed to facilitate voting. It is a well-established fact that the propensity to vote increases with age (Wolfinger & Rosenstone 1980, Blais 2000), and so we would expect turnout to be lower when the voting age is 18 instead of 21. Research that examines turnout in contemporary advanced democracies does not incorporate that variable for the simple reason that the voting age is now 18 almost everywhere (Massicotte et al. 2004), and there is thus no variation.

Blais & Dobrzynska (1998), whose sample of elections starts in the 1970s, do include a voting age variable and they find a relatively strong effect; their results suggest that lowering the voting age from 21 to 18 reduces turnout by five points. Voting age is also a key factor in Franklin’s (2004) study of turnout dynamics. He estimates that the lowering of the voting age in most democracies has produced a turnout decline of about three percentage points.

The evidence on the effect of vote-facilitating rules is more limited and ambiguous. Franklin’s (1996) initial analysis suggests that turnout is higher when voting takes place on Sunday, so that people presumably have more time to go to the polling station, and when postal (absentee) voting is available. But these same variables proved incapable of predicting changes in turnout over time (Franklin 2004). Norris (2002) examines the effect of specific rules (number of polling days, polling on rest day, postal voting, proxy voting, special polling booths, transfer voting, and advance voting), and she finds no significant effect. Blais et al. (2003) created a summary scale that reflects the presence or absence of postal, advance,

and proxy voting, and they find a rather strong positive association between the presence of such voting facilities and turnout.

It makes sense to assume that people are more prone to vote if it is easy. Gimpel & Schucknecht (2003), in particular, have shown that turnout is affected by the accessibility of the ballot box. Likewise, there is strong evidence that allowing voters to vote by mail increases turnout (Southwell 2004, Rallings & Thrasher 2006). The question is not whether voting facilities influence turnout but rather which ones matter most, and how great a difference they make. In order to correctly address these questions we need more accurate measures of these voting facilities over time and across countries, which means that we need to know not only whether such facilities exist but also how easy it is to use them. We also need to take into account the endogeneity of election laws; measures to facilitate the vote may be more likely to be adopted in countries where turnout is low or declining (Franklin 2004, p. 148). This is no easy task. For the time being, the verdict must be that we know little about how much difference these rules make.

Conclusions

The primary focus of cross-national studies of turnout has been on the impact of institutional variables. That impetus was shaped in good part by Jackman's influential article. The general perception in the field (and, I must confess, my own perception before I re-examined the evidence more closely in preparation for this article) is that cross-national differences in turnout can be relatively well explained by institutional variables. The perception is that we have come up with a number of well-established propositions about how institutions influence turnout.

That perception may not be well founded. We can safely assert that compulsory voting increases turnout, but we do not know whether a very light sanction suffices and whether that sanction needs to be enforced. Most of the literature supports the view that PR fosters turnout, but there is no compelling explanation of how and why, and the pattern is ambiguous when the analysis moves beyond well-established democracies. Many studies support the common-sense proposition that turnout increases with the saliency of the election, but many studies report no effect. I find it hard not to believe that turnout is higher when and where it is relatively easy to vote, yet the empirical evidence on the effect of voting facilities is inconsistent. All in all, our understanding of the impact of institutions on turnout is shaky.

THE SOCIOECONOMIC ENVIRONMENT

We know that at the individual level the propensity to vote is associated with a number of sociodemographic characteristics, particularly age and education (Wolfinger & Rosenstone 1980, Blais 2000). It would be natural to assume, in the same vein, that cross-national variations in turnout are associated with socioeconomic differences between countries. Powell (1982) considers the impact of the socioeconomic

environment and finds that turnout does tend to be higher in more economically developed countries. He also reports that turnout tends to be higher in smaller nations, but the relationship is not statistically significant.

The most influential analyses thereafter have neglected this line of inquiry (see especially Jackman 1987 and Franklin 1996, 2004), perhaps because they deal with a small number of established democracies among which there is little variance in the level of economic development. There is, however, relatively strong support for the hypothesis that turnout is higher in economically advanced countries (Blais & Dobrzynska 1998, Norris 2002, Fornos et al. 2004). The relationship is not linear, the main difference being between the poorest countries and all others (Blais & Dobrzynska 1998).

This raises the question of whether turnout increases or decreases with downturns in the economy. As Radcliff (1992) points out, both effects are possible; economic hardship may induce people to mobilize to redress grievances, but it may also lead them to withdraw entirely from the political process. Given these two contradictory possibilities, the most likely outcome is a nil overall effect, and this is precisely what most studies report (Arcelus & Meltzer 1975, Blais & Dobrzynska 1998, Blais 2000, Kostadinova 2003, Fornos et al. 2004; for an exception see Rosenstone 1982).

Radcliff (1992) argues that economic downturns increase turnout at high and low levels of welfare spending but depress it at intermediate levels. However, some of the findings are perplexing (Blais 2000, p. 34), and they have failed to be replicated (Jackman & Miller 1995, Appendix B, note 3). The conclusion must be that there is no clear relationship between the economic conjuncture and turnout.

In my own research, I have been struck by the fact that the highest levels of turnout are reported in small countries such as Malta (Blais & Carty 1990, Blais & Dobrzynska 1998). The real difference is between very small countries and all others, and the pattern is less clear at the subnational level (see Blais 2000, p. 59). The same pattern has been observed at the local level (Oliver 2000). I have speculated that this might result from stronger social networks in smaller communities, but that hypothesis is inconsistent with the absence of a correlation between turnout and urbanization [see Siaroff & Merer 2002, Fornos et al. 2004; Kostadinova (2003) reports a negative correlation but it is quite weak]. Another interpretation is that voters are more likely to feel that their vote could be decisive in a small country. Still another interpretation, and the one I find the most plausible (although it is contradicted by Rose 2004), is that smaller countries have fewer electors per elected member, which makes it easier for candidates and parties to mobilize the vote.

Not surprisingly, political scientists have paid closer attention to the impact of institutions than to the effect of the socioeconomic environment. Still, extant research shows that turnout is substantially lower in poor countries and exceptionally high in exceptionally small countries. Few other consistent patterns have been reported. Given the prominence of the resource model in the field of political participation (Brady et al. 1995), we would expect more systematic analyses of how poverty and/or illiteracy affect turnout.

PARTY SYSTEMS AND ELECTORAL OUTCOMES

Powell's (1982) initial analysis indicated that turnout was higher in countries with strong linkages between social groups and parties. That finding was not replicated by Jackman (1987), and subsequent studies have left out this variable. Jackman introduced a new variable, the number of parties, which is now incorporated in most research.

The intuition is that turnout should be higher the more parties there are, for at least two reasons. First, voters have more options to choose from. When there are six or seven parties instead of two or three, voters are more likely to find a party whose platform is reasonably close to their own views on the major issues of the election, and they should be less inclined to feel that none of the options is satisfactory. Second, the more parties there are, the greater the electoral mobilization.

As Jackman points out, party fractionalization may also have negative consequences on turnout. The more parties there are, the greater the likelihood that the government will be made of a coalition of parties. In systems that have coalition governments, electoral outcomes are less decisive, because the final composition of the government depends on the deals that parties are willing (or unwilling) to make. The presence of many parties may mean that voters have little say in the actual selection of the government (Downs 1957).

Because of these possible contradictory consequences, it is not clear whether we should expect the correlation of turnout with the number of parties to be positive, negative, or nonexistent. Moreover, it is not clear that it is the number of parties per se that counts. If it is the decisiveness of the outcome that matters, then we should look at the (anticipated) presence or absence of deals after the election and the most important distinction could be between elections producing single-party majority governments (which are decisive) and those producing minority or coalition governments.

Almost all empirical research has found a negative correlation between the number of parties and turnout (Jackman 1987, Blais & Carty 1990, Jackman & Miller 1995, Blais & Dobrzynska 1998, Radcliff & Davis 2000, Kostadinova 2003). The only exceptions are studies of turnout in Latin America, where there seems to be no relationship (Pérez-Liñán 2001, Fornos et al. 2004).

This is a perplexing finding. It seems to imply that people are not more inclined to vote when and where there are more options to choose from, and/or that party mobilization does not matter much (or that the arrival of new parties does not enhance overall mobilization). Furthermore, the usual interpretation that a higher number of parties reduces turnout because they produce coalition governments (and elections are therefore less decisive) is not empirically supported. Blais & Carty (1990) and Blais & Dobrzynska (1998) report that turnout is not higher in elections that produce single-party majority governments.

The bottom line is that we have a poor understanding of the relationship between the number of parties and turnout. We must reject the simple intuition that having more parties fosters turnout. This is an important nil finding. PR and/or larger

district magnitude increases the number of parties (Taagepera & Shugart 1989, Blais & Carty 1991, Lijphart 1994, Cox 1997). We can say that if PR fosters turnout, it is not because it produces more parties. Because we do not know exactly how and why PR may affect turnout—that is, we do not understand the microfoundations (Achen 2002)—the pessimistic reading that there is no generalized correlation between electoral system and turnout seems justified.

It is also time to question the standard interpretation that the often observed negative correlation between the number of parties and turnout reflects the impact of the decisiveness of elections. That interpretation must be directly tested, which means developing measures of election decisiveness. Such measures have been used in other streams of research (see, in particular, Powell & Whitten 1993); they might have to be amended or refined, but they should be incorporated in future studies. It could also be argued that what really matters is clarity of choice, that is, voters need to know with relative certainty the coalitions that might be formed. If that is the presumed process, then clarity-of-choice indicators must be constructed. As things stand, the fact that turnout appears to be lower when there are more parties is intuitively odd, and the supposition that this is so because more parties mean less decisive elections is only a supposition. (I do not find the interpretation that the number of parties increases information costs very plausible either. Voters do not have to inform themselves about each party. Information costs may increase, however, if and when the party system is in great flux.)

There is one final variable that was not included in the pioneer work of Powell and Jackman but has been incorporated in many subsequent studies: the closeness of the electoral outcome. This variable has produced the most consistent findings. My earlier summary of the evidence still holds: “the verdict is crystal clear with respect to closeness: closeness has been found to increase turnout in 27 of the 32 studies that have tested the relationship, in many different settings and diverse methodologies. There are strong reasons to believe that, as predicted by rational choice theory, more people vote when the election is close” (Blais 2000, p. 60). This is the most firmly established result in the literature. I cannot see how this finding could be wrong.

This does not mean that the issue is settled. It does not suffice to say that closeness fosters turnout; we need to specify the magnitude of the impact. I have been struck, in my own research, by its smallness. My cross-national analysis suggests that turnout is reduced by one or two points when the gap between the leading and the second parties increases by 10 points (Blais & Dobrzynska 1998). Very similar patterns emerge in cross-sectional analyses of constituency level turnout (Loewen & Blais, unpublished)¹ and time-series studies of national turnout in Canada (Nevitte et al. 2000).

It is possible that the impact of closeness is underestimated because the variable is not adequately measured. The standard indicator is the vote gap between the

¹Loewen PJ, Blais A. 2005. *Did C-24 Affect Voter Turnout? Evidence from the 2000 and 2004 Elections*. Typescript.

leading and second parties. The indicator makes sense, although it is not clear whether it is the seat or the vote gap that matters. (I personally think it is the vote gap because voters receive much information about vote intentions from polls and because many have a poor understanding of how votes are translated into seats.) In systems with coalition governments, it may be the closeness of the race between the two major coalitions that matters. More complex measures need to be constructed. Furthermore, it is always assumed that the relationship between closeness and turnout is linear. It could be that what matters is that the outcome not be a foregone conclusion, and that the real difference is between elections where the winner wins by a very wide margin and all others. Or perhaps it is only very close elections that excite voters and boost turnout.

Cross-national research typically looks at the overall closeness of the national election. It could be that what matters is the closeness of the race at the district level. Franklin (2004) uses mean margin of victory at the district level as an indicator of closeness, and this is clearly an avenue worth exploring. We should not assume, however, that closeness must absolutely be measured at the district level. In an analysis of individuals' decision to vote or abstain in the 1996 British Columbia election, we found that the perceived closeness of the race at the provincial level had a greater impact than perceived closeness at the district level (Blais et al. 2000).

Finally, there is the question of whether closeness matters in PR systems. Franklin (2004) takes the radical view that the margin of victory matters only in plurality systems. He may be right, but that is an empirical proposition that should be directly tested. The most difficult question is whether closeness (or competitiveness) should be measured the same way in different electoral systems. Margin of victory is the logical indicator in plurality systems because the probability of casting a decisive vote is directly related to margin of victory. In a PR system, however, the outcome can sometimes be a foregone conclusion even if it is "close." In a five-seat district, for instance, it may be obvious to voters that party A and party B will each win two seats and party C one seat (and that could have been the outcome of the previous three elections), yet party A's lead over party B may be minuscule. Such an outcome would be coded as very close, yet the probability of casting a decisive vote in such a district would be as tiny as in single-member "noncompetitive" districts. The probability of casting a decisive vote is equally minuscule in PR and non-PR systems. We need to think hard about what closeness or competitiveness actually means in PR systems.

DIRECTIONS FOR FUTURE RESEARCH

The dominant view in the field is that cross-national variations in turnout can be explained mostly by institutional factors that make some elections more salient and competitive than others. That view is well expressed by Franklin (1996, p. 232): "A country with low salience elections and an electoral system that was not very proportional could easily show turnout levels 40% [sic; it should be "40 percentage

points”] below a country with high salience elections and a highly proportional system. Such differences arise purely from differences in the institutional context within which elections are conducted.”

I am not convinced. As I have indicated, the evidence that turnout is higher under PR (which is supposed to produce more competitive outcomes) and in more “important” elections is far from robust. The evidence on the impact of closeness is consistent, but that impact appears to be strikingly weak. Institutions matter less than we are prone to believe. Their impact is conditional on the presence of other factors.

In order to disentangle these more complex relationships, we need to reconsider our research designs and methodologies. The standard approach in the field has been a cross-sectional analysis of variations in turnout across countries. This approach is appropriate for sorting out the effect of variables that tend to be stable over time, such as the socioeconomic environment or the electoral system and compulsory voting. The challenge is to include more cases, as the number of democracies expands, so as to test the robustness of the findings observed among established democracies.

But many variables differ from one election to another, and for these variables the analysis should be explicitly dynamic. In his ground-shaking work *Voter Turnout and the Dynamics of Electoral Competition in Established Democracies Since 1945*, Franklin (2004) confronts the issue of moving variables and clearly points in the direction that future research should take (see also Franklin et al. 2004). Franklin makes two crucial points. First, the logical way to ascertain the impact of a variable on turnout is to examine whether turnout increases or decreases when that variable changes. In other words, the analysis should be dynamic. Second, the impact of any change should be felt mostly on the new cohorts, who have not yet developed a habit of voting (or abstaining).

This approach leads Franklin to perform empirical analyses in which previous turnout is included as a control variable, thus making the analysis explicitly dynamic. Franklin also creates interactive variables between institutional factors and the proportion of the electorate that is new (facing one of its first three elections). In some cases, he also uses cohorts as the unit of analysis, which allows him to directly test the hypothesis that institutional variables have a stronger effect on new cohorts.

This is an impressive accomplishment. Franklin (2004) has challenged us to revisit how to test hypotheses about the influence of institutions or party systems on turnout. However, the study has three serious flaws. First, Franklin omits the main effects associated with new cohorts in his estimations because of the presence of multicollinearity. This is not a compelling justification. Brambor et al. (forthcoming) show that when the theoretical model entails interaction effects, all the constitutive terms must be included and that the problems associated with multicollinearity have been greatly overstated. Second, Franklin frequently refers to how generational replacement affects turnout, yet he confines his analysis to the consequences of new cohorts entering the electorate. He does not tackle the

crucial and difficult question of whether new cohorts vote less because they enter politics in a less competitive context (his argument) or because they belong to a new generation with a different set of values (Blais et al. 2004). Third, the kind of model that Franklin proposes calls for the use of multilevel analysis, in which characteristics of voters interact with characteristics of the electoral context.

Despite these shortcomings, Franklin has indicated the new direction that research in the field has to follow. We must pay closer attention to the dynamics of turnout, we must examine how changes in the party system and/or closeness of the election outcome affect electoral participation, and we should explicitly test whether these factors have a greater impact on new cohorts. In that sense, Franklin's book is as much a pioneer study as Powell's and Jackman's work 20 years ago.

Franklin's central argument, which corresponds to the dominant view in the field, is that the degree of electoral competition is the most crucial determinant of turnout. I remain skeptical. As indicated above, turnout is only weakly affected by the closeness of an election. Extremely close elections typically "boost" turnout by a few percentage points. Furthermore, I have seen no evidence that elections are becoming systematically less competitive over time, and so the recent decline in turnout can hardly be attributed to the lack of competition.

Franklin alerts us to the possibility that the impact of institutional characteristics may vary across types of voters. We must also examine the possibility that their effects vary across systems. For instance, turnout may be differentially related to the number of parties and/or the closeness of the election in PR and non-PR countries. Likewise, what increases or decreases turnout may be quite different in rich and poor countries. Because the number of democracies and democratic elections is greatly expanding, it is now possible to test interaction effects between the socioeconomic environment, institutional variables, and party systems and to separate general patterns that hold everywhere from conditional ones that apply only in some specific contexts.

CONCLUSION

Cross-national studies of turnout have produced a number of robust findings. We can confidently say that turnout is lower in poor countries and higher in small ones, that compulsory voting fosters turnout, and that turnout increases in closely contested elections. But I am more impressed by the gaps in our knowledge. We have a poor understanding of how compulsory voting enhances turnout, and we have a poor appreciation of how much or little competition matters and of how it plays out in PR systems. It makes sense to believe that turnout is lower in less salient elections but what makes an election more or less salient is still obscure.

We can do better. As the number of democracies and the number of democratic elections are greatly expanding, we can test our hypotheses with more cases and with greater variance in both the dependent and independent variables. This means

we must move beyond established democracies and check whether the patterns that we observe among them hold in new democracies. This is why the work of Fornos et al. (2004), which tests some of the standard hypotheses about the determinants of turnout in a new environment (Latin America), is so useful and important. If some factors, such as the electoral system or district magnitude, appear to have an impact only in some subset of countries, we should develop a more complex theory about when and where they matter more and less—or we should perform additional analyses to check whether the apparent relationship could be spurious.

With the advent of data sets such as the Comparative Study of Electoral Systems, it also becomes possible to examine the conditional impact of institutions on different types of voters (see, e.g., Long & Shively 2005). This opens up a fascinating avenue of research. Franklin (2004), Gerber et al. (2003), and Plutzer (2002) have all argued that there is an important habit component in voting. If they are right, we should expect contextual factors to have a much greater effect on new cohorts. That logically calls for a multilevel analysis linking institutional variables with individual voter characteristics.

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