VOTER INEQUALITY, TURNOUT AND INFORMATION EFFECTS IN A CROSS-NATIONAL PERSPECTIVE

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Gábor Tóka (1962) is an Assistant Professor at the Political Science Department of the Central European University, Budapest, Hungary. He is co-author of *Post-Communist Party Systems: Competition, Representation, and Inter-Party Cooperation*, and author or editor of three English-language books and over two dozen articles on electoral behavior, public opinion, political parties and democratic consolidation in edited volumes and in British, Croat, Czech, Hungarian and Polish political science journals. His most recent major publications are: *Inventory of Political Attitude and Behaviour Surveys in East Central Europe and the former Soviet Union 1989–97* (Bergisch Gladbach: Edwin Ferger Verlag, 2001); "Issue Voting and Party Systems in Central and Eastern Europe," in *Bürger and Demokratie in Ost und West, Studien zur politischen Kultur und zum politischen Prozess—Citizens and Democracy in East and West. Studies in Political Culture and the Political Process*, edited by Dieter Fuchs, Edeltraud Roller, and Bernhard Wessels (Opladen: Westdeutscher Verlag, 2002, pp. 169–85) [with Marina Popescu] "Campaign Effects and Media Monopoly: The 1994 and 1998 Parliamentary Elections in Hungary." In *Do Political Campaigns Matter? Campaign Effects in Elections and Referendums*, edited by David M. Farrell and Rüdiger Schmitt-Beck (London: Routledge, March 2002).

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ABSTRACT

The paper empirically tests the proposition that because of the unequal social distribution of politically relevant resources, some groups of citizens may be less successful in expressing their specifically political preferences in the vote than others. Hence, the electoral arena may give different people different degrees of political influence even when the formal equality of all citizens before the law is rigorously upheld in the electoral process. Survey data on voting behavior in 18 democratic party systems from the Comparative Study of Electoral Systems and Larry Bartels's (1996) simulation procedure—now extended to the analysis of multiparty-systems, turnout effects and non-linear information effects on the vote—are utilized to explore the question. The results show that social differences in both turnout and political knowledge may lead to the hypothesized political inequalities but their size is remarkably modest.

RESUMEN

Este trabajo pone a prueba la proposición que debido a la distribución social desigual de los recursos políticamente relevantes, algunos grupos de ciudadanos pueden tener menos éxito que otros al expresar sus preferencias específicamente políticas en el voto. De este modo, la arena electoral puede dar diferentes grados de influencia política a diferente gente, aún cuando el proceso electoral respete rigurosamente la igualdad formal de todos los ciudadanos ante la ley. Para explorar la cuestión se utilizan datos de relevamientos sobre el comportamiento electoral en 18 sistemas democráticos de partidos tomados del Estudio Comparativo de Sistemas Electorales y del procedimiento de simulación de Larry Bartels (1996)—ahora extendido al análisis de sistemas multipartidarios, de los efectos de la asistencia electoral y de los efectos no lineales de la información sobre el voto. Los resultados muestran que las diferencias sociales en la asistencia electoral y el conocimiento político pueden llevar a las desigualdades políticas hipotetizadas, pero, notoriamente, el tamaño de estas desigualdades es modesto.

The starting point of this paper is simple and familiar. Because of the unequal social distribution of politically relevant resources, some groups of citizens may be less successful than others in expressing their specifically political preferences in the vote. Hence, the electoral arena may give different people different degrees of political influence even when the formal equality of all citizens before the law is rigorously upheld in the electoral process. The key question examined here is to what extent this proposition is correct.

I begin with the discussion of some conceptual issues related to the problem. Then, I try to operationalize the political inequality that may or may not result from the trivial fact that some voters are far more likely to vote and/or be more knowledgeable than others. Finally, I present some empirical analyses that aim to assess the potential electoral relevance of these inequalities. I rely on a method independently proposed by Bartels (1996) and Delli Carpini and Keeter (1996). Both studies tried to compare observed and "fully informed" preferences among citizens, but it is the first that is directly relevant for my purposes, since it considered information effects on votes, rather than survey responses to attitude questions. As I will argue, the method is not really suitable for some of the ways it was used in Bartels's pioneering study. However, it can be used to evaluate whether some groups of citizens are better represented than others through the electoral arena. I offer an extension of the original simulation procedure to multiparty systems, nonlinear information effects, and—following the suggestion of Bartels himself—electoral participation.

All empirical analyses reported in the paper are based on the pooled cross-national data from the June 2000 version of the Comparative Study of Electoral Systems (CSES) Integrated Micro Data Set.[1] Any errors of data handling and interpretation are

mine. Regarding the construction and coding of variables, the exclusion, inclusion and weighting of cases in the analysis, the reader is referred to the appendices.

The Concept of Voter Inequality

Citizens' equality is, of course, a central component of the notion of democracy. Ordinary citizens may often mistake simple majority rule for democracy—but majority rule itself derives its powerful normative appeal from the fact that it allows each voter to have an equal influence on the outcome.[2] Citizens are always unequal in their motivation and opportunities to engage in political activity as well as in the resources they can mobilize while pursuing political goals. Yet, apparently all contemporary definitions of democracy reserve the term for political systems where the equality of citizens is the norm in national elections.[3]

Below I examine the possibility that the de jure equality of the voters does not guarantee their de facto equality even in the most narrowly understood electoral arena. I try to quantify, as much as possible, the extent to which this may be the case. Of course, an empirical analysis of the question must be designed in such a way so as to allow the disconfirmation of the hypothesis, too, i.e., to demonstrate the unrivalled political neutrality of electoral procedures—provided that they really are neutral. Such a negative finding would put democratic theory on a much firmer ground than it is today when voter equality is too often assumed to automatically follow from general franchise.

Explicitly or by implication, the scholarly literature on voting points at many possible sources for political inequalities (see e.g., Althaus 1996; Bartels 1998; Delli Carpini and Keeter 1996: 6; Downs 1957: 91, 94, 235, 252–56, 263–66, 273; Hill and Leighley 1992; Lijphart 1997; Moore 1987; Norpoth and Buchanan 1992; Pacek and Radcliff 1994; Rosenstone and Hansen 1993; Verba and Nie 1972: 309–18). This paper

only considers two of them: those that may derive from the socially unequal distribution of two aspects of political involvement: electoral participation and political knowledge. For the sake of brevity, I shall call them *turnout-based* and *knowledge-based voter inequalities*, respectively. Although the first received far more attention in the empirical literature on political involvement, the second seems equally plausible for some theorists. Anthony Downs, for instance, asserted that "systematic variations [among voters] in amount of free information received and ability to assimilate may strongly influence the distribution of political power in a democracy".[4] Indeed, empirical studies suggest that mere uncertainty about the true traits of candidates may make citizens not vote at all or vote for other parties/candidates than the one probably closest to their ideal point in the space of relevant policy dimensions (cf. Alvarez 1997; Bartels 1986; Palfrey and Poole 1987). It seems logically to follow that the electoral behavior of the least informed conveys less information about their political preferences than that of the better informed.

Innocent as this last inference may sound, it carries some assumptions that are central for the present analysis. Taken together they amount to saying that knowledge, in some ways, does mean power. Like the standard conception of rationality, I presume that preferences precede choices, and that with identical preferences, information and cost of decision making, anyone would consistently arrive at the same transitive ordering of the alternatives in the same choice set. Differences in knowledge may cause differences in choices even if preferences remain the same.[5] The greater the voters' stock of previous information, the lesser the probability that any new information can change their vote (Alvarez 1997; Zaller 1992). This could be otherwise if some antidemocratic mechanisms—like censorship of the press—assured that specific pieces of information become widely available in an inverse proportion to their ability to affect electoral choices. But freedom of speech and vigorous competition for public office in a

democratic framework guarantee that many actors try to make those pieces of information which are expected to influence voting behavior most widely available and easily accessible. To some extent, these actors presumably get their messages through. Consequently, out of two groups that are both entirely homogeneous in terms of their preferences and general level of political information, the one with higher general level of knowledge is likely to be less susceptible to random variations in the exact composition of individual group members' stock of information. Thus, its electoral choices will speak more clearly about the underlying preferences in the group than the choices of the less informed group. Alas, the more informed group will be more likely to vote the way it would if it were fully informed. It is hardly implausible that poorly informed voters have difficulties in expressing their views clearly—just think of the arguments about how an odd paper ballot in the 2000 US presidential election had probably made some in Florida vote for someone other than whom they really wanted.

The offshoot of this argument is this: the rising level of information among voters usually increases the valid information that vote choices convey about the voters' underlying preferences to an observer lacking extensive information about the composition of each voter's stock of information. Neither politicians nor other observers can possibly discount the effect of all the misinformation and misunderstanding that can influence votes. The less such observers can read into votes, the less likely contenders for elected office respond to popular preferences in the order of their true incidence in—and salience for—the electorate.[6] In other words, we can assume that more knowledge facilitates a better use of the vote by citizens—the meaning of "better" being defined here by the democratic ideal that elected officials should be responsive and accountable to citizens' preferences. Similarly, voting for a particular party or candidate will normally carry more information about a voter's preferences than nonvoting, and thus give more

political influence to a given citizen. Hence, the possible conflict between the democratic ideal on the one hand, and social inequalities in the distribution of turnout and political knowledge on the other.

I would like to stress two characteristics by calling the phenomenon in question: voters' inequality. First, some preferences may have less than their fair weight in the election outcome under the 'one person-one vote' rule for reasons that concern the voters themselves, rather than the distribution of relevant resources between the candidates and parties, or some other characteristics of the latter.[7]

Secondly, I submit that the root of the phenomenon is not simply the unequal intensity of political preferences or commitment among citizens. Let me just briefly allude here to the notorious incentive problem commonly labeled as the paradox of voting. Democracy requires that, at least at some critical junctures, many can participate in political decisions. But if many participate, the impact of a single vote on the outcome is negligible. Hence, the specifically political benefit of voting becomes unable to motivate citizens' participation, since the cost of voting—albeit tiny—easily exceeds it. Therefore, electoral participation, at least partially, is driven by other factors than the intensity of preferences regarding election outcomes. The most likely motives seem to be a sense of citizen duty, and various pleasures that may stem from the act of voting itself.[8] Thus, whatever social mechanisms generate a sense of citizen duty or entertainment value from electoral participation, the groups that appreciate them best are bound to have an advantage over the others in the electoral arena. Unlike unequal influence generated by differences in the intensity of preferences or a low turnout among the supporters of a party caused by disillusionment and second thoughts among its supporters, this advantage is inevitably at odds with the notion of citizens' equality.

Obviously, the same argument applies for citizens' political information level. The minuscule impact of their own vote on the outcome cannot be the sole reason why rational citizens attend to any political information. Thus, political information level is likely to reflect other factors than the intensity of political preferences or the strength of commitment. If it is distributed unequally across social groups with diverging preferences, then it can be a further source of voter inequality—provided that it influences people's capacity to vote *as if they were fully informed*, i.e., getting as close as possible to how they would vote if they had perfect knowledge and their decision-making costs were zero.[9] Therefore, both turnout- and knowledge-based voter inequalities are rooted in the same incentive problem, and their existence may well be an ineradicable feature of democracy.

To be sure, there may be considerable cross-national and cross-time variation in knowledge gaps and turnout differences between different groups. Some of the former may even cancel out the political effect of the latter. For instance, as Table 1 demonstrates, rural residents in contemporary democracies tend to show above-average turnout, but below-average political information level.

Yet, the same table makes it clear that social groups with low turnout also tend to be less informed than the average. Furthermore, cross-national differences in, for instance, gender effects on political involvement (see Claibourn and Sapiro 2000) do not cancel out each other fully in the pooled data. Across a wide range of democracies, young and old, people whose income or education is low, women, racial minorities and some occupational groups tend to participate less in elections and know less about politics than other citizens. Sociodemographic traits may not go far in explaining voting behavior, but we certainly know about many ways that the electoral and policy preferences of the above groups may be special. Thus, in the abstract, one can easily imagine that election

outcomes may be systematically different if the less involved members of these groups voted in greater numbers or their political knowledge increased.

TABLE 1

Logistic Regression of Electoral Participation and OLS-regression of Political Information Level on Sociodemographic Variables in the Pooled Cross-national CSES Data

	Dependent Variable				
	Voting		**Info**		
	b	***s. e.***	***b***	***s. e.***	***beta***
Age/10	.287**	(.014)	.012**	(.001)	.132
ABS(Age-45)/10	–.207**	(.026)	–.009**	(.001)	–.054
Female	–.137*	(.046)	–.049**	(.002)	–.166
Education Low	–.355**	(.053)	–.040**	(.003)	–.136
Education High	.358**	(.079)	.034**	(.003)	.082
Rural Residence	.105*	(.056)	–.007*	(.003)	–.019
Farm Job	–.092	(.114)	–.025**	(.006)	–.035
Manual Work	–.131*	(.054)	–.014**	(.003)	–.042
Income	.159**	(.020)	.010**	(.001)	.083
Devout	.551**	(.060)	.006*	(.003)	.015
Race	–.246*	(.116)	–.050**	(.007)	–.055
Turnout	.074**	(.002)	—	—	—
Info-Mean	—		–.172	(.197)	–.006
Constant	–5.648**	(.223)	.561**	(.098)	
Nagelkerke R^2	.184		—		
Adjusted R^2	—		.100		

Notes: Table entries are regression coefficients (with standard errors in parentheses). On data source, weighting and variable coding, see the appendices. The data are weighted to correct for cross-system differences in sample size and various sampling problems in some of the data sets. The weighted *N* in the analysis is 16616, and the unweighted *N* is 27401.

**Two-tailed significance < .01
*Two-tailed significance < .10

There are, however, other, better reasons to doubt the seriousness of voter inequality. The institutional design of representative democracies aspires to make national elections relatively infrequent, and a channel for expressing citizens' preferences on an open-ended and potentially infinite variety of issues. These two factors assure, via

various mechanisms, that even the most active and best-informed citizens can convey only very little information about their preferences through the vote.

First, turnout in national elections can come close to 100 percent—some argue that compulsory voting alone may make it so high that virtually no social inequalities remain in rates of participation (Verba, Nie and Kim 1977; Lijphart 1997). Obviously, this is achieved partly by making elections infrequent. Second, open political competition should be able to guarantee that citizens live in an environment very nearly saturated with handy information shortcuts, mechanisms of delegation, and other ingenious devices that can empower even the least resourceful (cf. Lupia and McCubbins 1998). In contrast, the number of relevant party alternatives is usually limited, and hence, vastly simplifies the task of the voter. As a result, there may be a low ceiling beyond which extra knowledge may have diminishing or absolutely no effect on voting behavior (cf. Lupia 1994).

This property of the voting act was often noted by electoral researchers: "An individual facing a choice situation like voting, where the number of alternatives is limited, need only gather enough information to determine which alternative is preferable" (Popkin et al. 1976: 789). It is not only that, as Lau and Redlawsk's (1997) evidence seems to imply, most citizens may not be able to utilize any more information than what they already have. This would still leave the possibility open that differential cognitive capabilities transform into inequalities of politically relevant skills.[10] But if the choice-set is reasonably small, fairly small stocks of information may already suffice for a voter to emulate fully informed behavior—without all the risks inherent in an information overload:

> "[B]y making up their minds in a different way, voters who are *not* well informed about politics—as well as those who are—may make approximately rational electoral choices." "[T]he poorly informed voter

> may lack the information to make the kind of choice the well-informed voter can, that is, a choice that turns on comparison of the candidates, for instance, with respect to their policy commitments. All the same, the poorly informed voter may have the information needed provided he or she treats the choice as a choice for or against the incumbent; poorly informed or not, the voter is in a position to judge if the incumbent's performance is satisfactory. ...So, in these alternative ways, the choices of voters can be approximately rational not in spite of—but because of—shortfalls in information." (Sniderman, Glaser and Griffin 1990: 117, 135).

Indeed, the scholarly literature on voting discusses a wide range of devices assisting low information rationality (cf. Popkin 1991). To be sure, not all empirical studies are unambiguously reassuring about the efficiency of these tools (see, for example, Weisman 1994 on the role of opinion leaders, Huckfeldt and Sprague 1995 on interpersonal communication, and Luttbeg and Gant 1985 on ideological labels). But formal models and laboratory experiments suggest that, given enough time, either blind reliance on retrospective assessments of government performance or taking cues merely from public opinion polls may enable poorly informed voters to emulate fully informed behavior (McKelvey and Ordeshook 1986, 1990). At least, random errors in individual choices cancel out each other's effect. True, experiments with deliberative polling suggests that there are systematic patterns to the way citizens' attitudes change under the impact of reflection (Fishkin and Luskin 1999), and Bartels's (1996) findings imply that even in long-established democracies, election results may be different from what they might be without knowledge-based voter inequalities. But the political relevance of the remaining differences between observed and fully informed behavior may still be negligible.

Apart from a low ceiling to any one citizen's influence and low information rationality, there is a third mechanism that can also reduce voter inequalities. The pulling

of many political decisions across time and issues into a single vote assures the usually—though not always—small impact of any one social characteristic on the vote. Therefore, all contenders may end up with fairly similar proportions of likely nonvoters and poorly informed voters among those who, given their preferences on all matters political, would have presumably voted for them if they had voted at all and had been fully informed.

Probably the best way to appreciate the importance of this third mechanism is to consider a counter-example. Suppose that there is a party advocating permissive positions on a range of moral issues, and it appeals to young people in particular. Young people, as we just saw, vote less frequently and know less than their elders. One likely consequence is that the morally permissive party ends up with a lower percentage of the vote than it would have if turnout were 100 percent and all voters equally and fully informed. The wide-ranging political consequences of this percentage difference are the price that the potential electorate of this party—i.e., those who would vote for the party if turnout were 100 percent and all voters perfectly informed—pays for voter inequality. However, their loss may be negligible if all relevant parties appeal to socially diverse constituencies, and thus have fairly similar proportions of young citizens in their potential camp. Indeed, it is a familiar finding that the political attitudes of voters and nonvoters barely differ (Gant and Lyons 1993; Studlar and Welch 1986; Teixeira 1992: 100). Hence, even if political involvement depends, to some extent, on age, class and other things that influence vote choice, voter inequality can still remain low.

The above arguments about the likely insignificance of voter inequality also imply that its degree depends on the political context: e.g., how closely party alignments follow social cleavage lines, and how conducive the institutional design is to high turnout. The information shortcuts that less than fully informed voters can rely on may be

more or less relevant, reliable and abundant depending on the skills of the competing parties, communication patterns within and across particular groups, the institutions of civil society, the media system, the age of democracy and so forth. All in all, the problem calls for an empirical investigation.

Before proceeding to the development of a measurement procedure for voter inequalities, consider a possible objection that questions the significance of potential voter inequalities immediately, even without assessing their degree. One may want to argue that nonvoting (and possibly also political ignorance) results from individual choices for which the supposedly underrepresented individuals must remain responsible. My response is that talking about individual responsibility is misleading in this context. The rationality of ignorance and abstention does not protect citizens from the consequences of collective abstention and ignorance—quite the opposite, it only makes it more likely that those consequences will emerge. Since election results influence collective outcomes, the potential victims of the political inequality stemming from the socially unequal distribution of turnout and relevant knowledge are not the poorly informed voters and the nonvoters as such. Rather, they are all who, irrespective of their own political information level and participation, share their underlying political preferences with nonvoters rather than voters, and with uninformed, rather than knowledgeable voters. No matter how they personally vote, and whether they vote at all, their preferences may have a weaker expression in the election outcome than the numerical presence of these preferences in the electorate would justify under the ‘one person–one vote’ rule. It would be hard to argue that citizen X deserves a weaker voice in the electoral process than citizen Y simply because the people who tend to have similar policy preferences to X—because of their age, education, income, race and so forth—are usually less informed or less participatory than the likes of Y.

The last remarks offer a starting point to operationalize the voter inequalities in question. Since the potential victims are not so much the nonvoters and the poorly informed voters as such, we need not estimate how these individuals would vote if they voted and/or became fully informed. The task is simpler than that.

First, we have to identify those groups that, because of arguably *non-political* influences, show below average turnout and political knowledge, and at the same time *may* differ in the distribution of their vote choices from other groups. They are the only ones who can remain, in one way or another, underrepresented at the polls, specifically because of turnout- and knowledge-based inequalities.

Note that these inequalities do not emerge from just about any temporary differences in turnout and information-level between politically relevant groups. Many of the latter surely reflect just passing apathy in some circles—caused, for instance, by the appalling recent record of their favorite party, or unequal mobilization efforts by the different political camps. The first has nothing to do with political inequalities: this temporary drop in political involvement is caused by a weakened commitment by its potential voters to a party. The strength of commitment may well be a valid and effective expression of underlying political preferences. Unequal mobilization, in its turn, may have more to do with political inequalities, but not with those explored in this paper.

Voter inequalities, as such, stem from *persistent differences in political involvement between groups of citizens that are caused by entirely different factors than the fact that the underlying political preferences of these groups are not identical*. However, if the political preferences of these groups would not differ, than their unequal political involvement would not generate political inequalities.[11]

The Ideal Test: An Outline

Voter inequalities can only exist between groups defined in terms of such variables that may affect relevant political preferences and political involvement simultaneously, but for different reasons. The prime candidates are already identified by the independent variables of the equations shown in Table 1. Once relevant groups have been identified, we need to calculate the difference between actual election outcomes and those that would have been obtained in the absence of turnout and/or knowledge-gaps between them. Below, I explain the method in detail, but it may be useful to state it in the most general terms first, without any of the compromises that imperfect knowledge and data availability may have imposed on me in the present analysis. The simulation of the election results in question requires a model of vote choice that satisfies a rather different criterion than maximal explanatory power. Instead, the model must be able to isolate any residual association that remains between vote choice and political knowledge once those determinants of knowledge and turnout are held constant, which like the variables in Table 1, may influence vote choice, too. Once these factors are held constant, we can easily model the impact of turnout and information level on aggregate election outcomes.

As a final step in the analysis, the difference between observed and simulated election results needs to be scrutinized. The relevant question is not whether the gap between them is bigger than the one that made all the difference between winning and losing in particular elections. Had typical turnout been historically higher and voters more informed, all political actors would have behaved differently, and the nail-bitingly close elections would, presumably, have occurred in some other years, involving entirely different issues, parties and candidates. So, the critical question is not so much the size of the difference between what was and what could have been the result in particular elections. Rather, it is whether the simulated election results would have systematically

increased, and to what extent, the weight of exactly those preferences in election outcomes that are overrepresented in groups that show below-average political involvement for reasons other than their political preferences.

This test is rather straightforward in the case of knowledge-based voter inequalities. The distribution of votes within relevant groups has to be contrasted with how this vote distribution may look if particular changes occurred in the level and distribution of political knowledge in the electorate. How much change occurs in votes within particular groups depends not only on the extent of change in information level, but also on how extra information influences vote choice among people with particular preferences. This is what the vote function has to reveal. The gap between expected vote distributions at the observed information level and a suitable selected higher level should be computed for relevant groups. This gap shows how far a particular group is from its own fully informed behavior. The kind of voter inequality that this paper addresses exists if this gap tends to decrease with the information level expected in a group for reasons other than their political preferences.

The same comparison would be meaningless in the case of turnout-based political inequalities, since these operate through a simpler mechanism than complex effects on within-group distributions of the vote. If unequal turnout can, on its own, cause political inequalities, then it is only because a less unequal turnout would make previously low-turnout groups account for a larger percentage of the electorate than before. Turnout-based voter inequalities exist to the extent that equal turnout would make election results more similar to the actually observed distribution of the vote within those groups, whose turnout is negatively influenced by something unrelated to their political preferences.

Note that this result is not at all inevitable. Unequal turnout has no real electoral relevance if the variables correlated with turnout—let’s say age—are, by and large,

unrelated to vote choice. Moreover, if the nonvoters are, as is likely to be the case, generally less informed than other citizens with identical underlying preferences, then their simulated vote choices may differ dramatically. Thus, merely eliminating turnout-based inequalities may not make any difference in aggregate election results.

Briefly, the simulations attempt to capture the electoral impact of a hypothetical disappearance of some or all inequalities in political participation and political knowledge level. We saw that such inequalities systematically occur between sociodemographic groups (cf. Table 1). The question is whether they have any sizeable and systematic impact on election outcomes. If the answer is no, we cannot talk of voters' inequality in terms of electoral influence. Whatever other inequalities of electoral influence may exist, they do not derive from the voters' own characteristics.

Assumptions

Even social scientists have to work in a less than ideal world. That is why they make assumptions. I have already introduced some. The first was that the preferences that affect voters' choices are fixed, but their impact on vote choice is mediated by citizens' knowledge (see note 6). It was also argued that voting is more likely to accurately reflect these preferences than nonvoting, and the higher the general information level of the voters, the more accurately their votes express their preferences—recall, however, note 7 as well as the caveat about the role of noncensorship and vigorous electoral competition. The next assumption was that the gap between given individuals' expressively and instrumentally rational votes decreases as information level increases (see note 10).

This section introduces a few more assumptions that I deem both necessary and reasonable. These additional assumptions are required because it seems impossible to estimate with an acceptable precision how previous nonvoters or poorly informed voters voted if all voted and were fully informed. The impact of additional information on

political attitudes is often a polarization of the voters along some underlying preferences, e.g., to move generally liberal voters towards even more consistently liberal policy and candidate preferences, and generally conservative voters in the opposite direction.[12] Unless we can develop a miraculously accurate model of vote choice, we will incorrectly estimate how particular nonvoters might vote or how many individuals may alter their vote decision if they became fully informed. This wonderful model would include all possible determinants of vote choices and perfectly model their relative salience for individual voters as well as how they shape vote choice at different information levels.

Even if we had such a model, it might be worth less than it might seem. Of course, we could then estimate better how the electoral performance of individual parties may have been affected by turnout or the voters' information level in particular elections. But we would still not be able to model the likely reaction of candidates and parties to a major change in citizens' knowledge or turnout. So the whole counterfactual analysis would be a bit pointless, since all it would tell us is which parties or candidates would have done better if all were fully informed (and voted), but the parties failed to anticipate this. At most, we would learn something about the *kind of* appeals that may work better in a fully informed electorate.

Hence, Bartels (1996) was at least partially wrong in setting the targets for his analysis of information effects in election. At any rate, he inferred that incumbents and Democrats would do worse in US presidential elections if all citizens were fully informed, while my present analysis suggests that in the 1996 American election the Democratic incumbent would have done even better if the electorate were fully informed. The problem, as I will argue below, is that all these estimates about individual elections, including mine about 1996 and his about one election with a Democrat and four with

Republicans in the White House, may be hopelessly polluted by measurement error, and therefore one should not even look at them.

At the same time, I believe, as Bartels did, that the key to the analysis of systematic voter inequality is in modeling, *vote choice as a function of interactions between political information level and various sociodemographic traits of citizens*, and *then analyzing differences between observed vote choices and those simulated for a hypothetical information level in pooled data from several elections.*

First, I will explain how turnout effects on election outcomes will be explored here, and highlight the assumptions involved in the analysis afterwards. Bartels's model will be used to show how vote choice was influenced by factors *i* among the actual voters in a particular election. The parameter estimates obtained can be used to simulate how nonvoters might have voted if they had participated in the election and their vote choice was influenced the same way by factors *i* as those of the actual voters. The only difference between the simulated election results among the nonvoters and the observed result among voters will be due to the fact that some of factors *i* were directly correlated with electoral participation after controlling for all other variables in the vote function.

I will assume that no political attitude, opinion and evaluation that directly influences the vote, across a large number of democratic countries and elections, is systematically correlated with vote choice once we control for the sociodemographic variables that are likely to influence turnout systematically, and vote choice occasionally. To the best of my knowledge, there is neither empirical evidence nor good theoretical argument to refute this assumption.[13] Hence, we can just as well omit the attitude variables from the vote function. Apart from sociodemographic variables, only political information level has to be included in the vote function. That is, because political information level influences both vote choice and electoral participation. For instance,

when we add political information level, i.e., the dependent variable of the right-hand side equation in Table 1, to the predictors of turnout in the left-hand side equation of the same table, the explained variance as measured by the Nagelkerke R^2 statistics increases from .184 to .201. Alas, the previous literature on voting offers compelling theoretical arguments and empirical evidence that political information level frequently and substantially influences both vote choice (see especially Alvarez 1997; Bartels 1996; Zaller 1992). Thus, it is unlikely that if the current nonvoters suddenly became voters, but their information level did not become similar to those of their sociodemographic look-a-likes among the current voters, they would distribute their votes the same way as the latter's support is divided between the contenders. Bartels' model enables us to simulate the vote distribution among the nonvoters that matches the distribution of votes among those voters who are exactly like them in terms of their sociodemographic characteristics and observed information level.

To explore the assumptions underlying the analysis, consider now the case of political information. As we saw above, knowledge-based voter inequality exists if two conditions are met. The first is that fully informed and observed election outcomes differ in the electorate as a whole. The second is that the difference between the fully informed and the observed vote decreases as we move from groups that—for reasons other than their political preferences—are expected to know less about politics to groups that—for equally nonpolitical reasons—are expected to know more. Consequently, any empirical analysis of knowledge-based voter inequality must start with exploring how vote choices and political information level are related within various groups. This is what Bartels's model does.

Vote choice and knowledge can be correlated for two sets of reasons. On the one hand, some variables that influence vote choice may also influence information level. On

the other hand, information level may influence vote choice. It is the latter effect that one would isolate, in an ideal world, in order to simulate fully informed election outcomes. In a less than ideal world, Bartels's model still allows us to control for many relevant influences on political information level. The remaining residual relationship between political information level and vote choice can be used to estimate fully informed election outcomes with some measurement error. Obviously, most individual level information effects will not be captured, but the point of interest is the net information effect on the vote for each party within groups that, for reasons unrelated to their political preferences, are expected to be unequally involved in politics. The measurement errors in the estimates stem partly from mere sampling errors in surveys. Sampling errors inevitably spill over to any simulation based on surveys, but in a large enough pool of independent surveys, they are randomly distributed with a mean of zero.

Another source of measurement error in the simulated changes in collective outcomes is our inability to identify all relevant effects on citizens' information level. For example, Bartels (1996) finds that other things being equal, Jimmy Carter won fewer votes among the more, than among the less informed in 1980. Therefore, the simulation shows Carter getting even fewer votes in the fully informed electorate than the president actually got. Indeed, it is quite possible that the predominantly bad news about Carter—just think of the drama of the Tehran hostage crisis unfolding before polling day—and the positives about Ronald Reagan were best counteracted by incumbency advantage and the slight Democratic edge in party identification exactly among those least exposed to the facts. If so, a more informed electorate would have voted even more heavily for Reagan. But, it is also possible that there was something peculiar about Carter supporters that made them seek knowledge less than their sociodemographic look-a-likes in the Reagan camp. For instance, dissatisfaction with the administration may have

weakened the partisanship of Democrats. Weaker partisanship leads to a lower information level (Luskin 1990). If so, a higher information level among Democrats would probably not have reduced Carter's vote.

I doubt that situational influences on information level can be nearly as large as situational information effects on the vote, but it is difficult to determine how much credit each of the two above interpretations deserves. Of course, Bartels (1996) could have included controls in the vote function for party identification, political interest and all sorts of other nondemographic determinants of political information level, too. But these attitudes themselves may well be subject to change as information level increases. Thus, had he included them in the vote function, he might have underestimated information effects on election outcomes. Having excluded them from the vote function, he probably overestimated information effects. The measurement error is inevitable.[14]

Three things can be done to assure that this second source of error in simulated changes in each party's vote is also randomly distributed with a mean of zero, both across elections and any grouping of voters used in the analysis:

a. *control* for all systematic influences on information level in the vote function generating fully informed votes;

b. when analyzing the difference between vote choice at the observed and a suitably selected hypothetical information level, *pool* estimates from a large enough random sample of democratic elections to assure that situational influences on the information level of particular groups are randomly distributed with a mean of zero; and

c. *choose* the research question so that the remaining measurement errors in the parameter estimates are likely to remain random with respect to the question of interest.

In the remainder of this section, I explain how the present analysis follows these guidelines and what assumptions need to be added to the earlier, more philosophical ones.

I define *situational* as those effects that, in the relevant universe of democratic elections, are randomly distributed with a mean of zero across any groups of voters that we may want to distinguish. Effects that are not randomly distributed or do not average zero will be identified as *systematic*. For instance, self-observations suggest that partisanship increases my appetite for domestic political news (and hence, probably my political knowledge, too) when things are going well for my party, but makes me a less enthusiastic newspaper-reader when the same party is in a dismal state. Hence, the direction of my partisanship is likely to have some situational effects on my political information level, but since party fortunes are changeable, probably no systematic effect at all. In contrast, my gender appears to have a systematic positive effect on my political information level (see Table 1).

The critical assumption that I make here is that the independent variables in Table 1 include all factors that may systematically influence political information level across the entire universe of democratic elections and, at the same time, systematically influence vote choice, too. Apart from having a large effect on political information level, age is likely to influence support for parties advocating permissive positions on moral issues throughout this universe. There may be a few odd contexts where the elderly are the most likely to vote for such parties, but the opposite should be the general tendency. In contrast, strength of partisanship or political interest may systematically influence political information level, but are very unlikely to systematically influence vote choice. Across a large number of democratic elections, their average net effect on voting support for any particular type of party in any particular group of citizens is likely to be zero.

Thus, we can omit these variables from the vote function as long as it is only meant to study whether the gap between fully informed and observed behavior is larger in some sociodemographic groups than others, or whether voting support for some broad type of parties is hurt by voter inequality. This is not a proposition that I can test, and neither is it a matter of definition: it is an assumption that can be contested.

The obvious challenge to these assumptions is that some preferences, which are not perfectly captured by sociodemographic variables, may influence vote choices and political involvement at the same time. However, there seems to be no cross-contextual evidence that would clearly identify any other shared determinant of vote choices on the one hand, and turnout and political knowledge on the other, than a relatively well-known and manageable set of sociodemographic variables—plus, in the case of turnout effects, political information level—that I will control for in my analyses. Hence, I can see no reason to believe that a particular political taste would, like a low level of education, consistently and repeatedly lead to below average political participation or information level, once the sociodemographic determinants of political taste are held constant.

Of course, anyone can invent neat theories about how a particular set of attitudes can systematically influence political involvement and, at least occasionally, vote choice, too. Suppose that the weakness of integration in the political community is an important determinant of vote choices, and, at the same time, a major cause of young people showing below average political knowledge. Then, even if the relatively ignorant young voters were to become more knowledgeable, they may not vote the same way as the currently more involved, young people do. They will still remain different from the latter with respect to an attitudinal determinant of vote choice. If so, the analysis of this paper is, to that extent, wrong. Similarly, if we were to look at democratic elections around 1920, we might worry that an uneducated worker's preference for a social democratic

party may have systematically increased the probability that he or she was heavily targeted by mobilizing organizations and networks, and thus likely to vote. Hence, a higher turnout would probably not have increased the social democrats' share of the vote among uneducated workers even if most of them, when they did vote, voted for the social democrats. Indeed, many politicians who then advocated the adoption of mandatory voting laws in as diverse places as Australia or Hungary expected that a higher turnout would show less support for socialism among citizens than some election results suggested.

I doubt that in a typical democracy in the 1990s social democrat supporters were systematically more strongly targeted by mobilizing efforts than their fellow citizens. However, the argument shows the validity of the warning that my analyses might have produced different results if I had controlled for more variables in the vote function. But the warning is no more valid in this context than in the case of any other empirical analysis. As long as there is no systematic evidence pointing to missing control variables that (1) can demonstrably influence vote choice across a large number of democracies; and (2) are systematically correlated with turnout or political knowledge; but (3) nevertheless resistant to changes in the examined aspect of political involvement, the epistemological objection boils down to the familiar warning that further research may prove me wrong. If more variables of this type are identified, they can easily be incorporated into the model proposed here, without requiring changes in any other feature of the analysis.

Consider now the second device of randomizing measurement errors in the estimates of interest. To tell the effects of voter inequalities apart from the situational effects of preferences on information level and of information on the vote, the analysis has to consider trends that persist across a large number of countries and elections. In

individual elections, the impact of systematic voter inequality may even become hidden if it runs against the current of situational influences. Only an analysis of a heterogeneous sample of polities and elections can say anything of relevance about how much impact voter inequality can have in any election.

Therefore, my analysis pools data across many elections and countries. First, I estimate election-specific models of how information level influenced vote choices. These models yield estimates of fully informed election outcomes, i.e., what percentage of the vote particular parties received in the electorate as a whole, or within particular subgroups. As discussed above, the estimates about particular kinds of parties and social groups will include some measurement errors that must be randomly distributed and average zero if a large random sample of democratic elections is considered.

The CSES data used in my analysis covers an exceptionally large and geographically heterogeneous sample of elections by the standards of electoral research. Eighteen cases do not amount to a "large" sample in statistical jargon, but sound quite enough to assume the random distribution of those measurement errors in the simulated election outcomes that are caused by sampling error in the original surveys. However, this assumption may not hold for the measurement errors caused by the insufficient control for situational effects on political involvement in the vote function. Here, the problem is that the sample is definitely not random. For instance, only elections held between 1996 and 1999 are covered. It is not inconceivable that there was a dominant direction of information effects throughout the democratic elections held in this period.

I try to alleviate the problem of sampling bias through choosing a research question for which the sample can be conceived as unbiased. I see no reason to expect that turnout- or information-based voter inequalities were any more or any less likely to occur in the cases covered in this analysis than in other democratic elections. Quite

independently from this, there is no reason to believe that the measurement errors polluting the simulated election results make the rejection of the null-hypotheses any more or any less likely than it should be. The risk of error is probably higher than one would like, but at least we cannot tell the direction of error in advance.

Moreover, situational effects on political information level—and hence the measurement errors that the lack of control for them causes—are probably not too large to begin with. Previous studies, as well as Converse's (2000) recent review, suggest that political information level is not only the simplest, but also as valid and reliable a measure of political sophistication as any more complex instrument would be.[15] This observation itself suggests that we are speaking of a relatively stable individual trait here, which is not easily affected by short-term fluctuations under the influence of situational factors. The most comprehensive studies of political information level to date have also demonstrated that the empirical referent of this concept is a single dimension: individual citizens tend to be just as informed (or uninformed) in one political domain as in any other.[16] This, again, suggests that political information level cannot be strongly related to the intensity or the content of political preferences. Luskin (1990) traced its roots to interest in politics, cognitive capabilities and opportunity to learn. These are all fairly stable traits of an individual. This leaves me with some optimism regarding the likely size of the measurement errors polluting my findings.

Modeling Information Effects

With the assumptions clarified, we can now move to the empirical analysis, which starts with modeling information effects on the vote. Political knowledge may affect vote choice directly, but above all, it can alter the impact of other variables on the vote. A special case of this is the polarization effect, when additional information moves the vote

choices of different people in opposite directions, depending on the dominant tendency of their political predispositions. This is why Bartels's (1996) simulation procedure models vote choice as a function of interactions between sociodemographic characteristics and political knowledge, measured on a scale running from 0 to 1 and henceforth abbreviated with the INFO variable name. Recall that the choice of independent variables in the vote function follows the methodological assumptions outlined in the previous section, and does in no way imply a belief in a "sociological model" of vote choice, i.e., in a particularly and universally strong impact of sociodemographic variables on vote choice.

The first set of the interaction terms in Bartels' model consisted of the pairwise products of INFO with politically relevant sociodemographic variables (income, gender and so forth). The second consisted of the pairwise products of the same socio-demographic variables with (1–INFO). Bartels's probit analysis yielded two constants and two sets of parameter estimates showing the impact of each interaction term.

A voter who scores .4 on INFO can be conceived of as the mix of a maximally informed and a maximally uninformed voter: as 40 percent of the former and 60 percent of the latter. Thus, gender's impact on vote choice at INFO = .4 can be obtained as .4 times the estimated effect of FEMALE*INFO, plus .6 times the estimated effect of FEMALE*(1-INFO) on vote choice. The constant for any individual is estimated the same way, thus the two constants capture the direct impact of political information level on the distribution of vote in the electorate as a whole.[17] Fully informed votes were obtained by substituting INFO = 1 in the estimated vote function but leaving all other variables unchanged.

To appreciate the meaning of these equations, consider a fictitious example. Suppose that at minimal information level, support for Party A is unrelated to variable FEMALE (coded 0 for men and 1 for women), but drops by 10 percent among men and

increases by 10 percent among women as INFO changes by one unit (i.e., from 0 to 1). Thus, the impact of FEMALE*INFO on the probability of supporting Party A is a positive number, while the effect of FEMALE*(1-INFO) is zero. Since women are more numerous and tend to be less knowledgeable than men, this also implies that a fully informed electorate would give more votes to Party A.[18]

From the parameter estimates, it is straightforward to estimate the distribution of votes in every sociodemographic group for any given mean and distribution of INFO. Here, and below, a sociodemographic group means a group of respondents who have identical values on a set of variables that entered the vote function in interaction with INFO. To retain a meaningful number of respondents per group, I will distinguish 90 groups only, defined in terms of the four variables that register the biggest effects on political information level in Table 1. The probability of voting for each party *j* by each respondent *i* at INFO = *k* is estimated. These probabilities run from 0 to 1, add up to 1 for each respondent and are identical for every member of a sociodemographic group if their information level is set equal. Since the estimates do not take into account differences in relevant preferences between members of a sociodemographic group, they cannot be interpreted as if they said, for instance, that for a given respondent *i* the predicted vote at INFO = *k* would be 45 percent Party A and 55 percent Party B. But the same numbers can be legitimately interpreted so that if every person in a given sociodemographic group had had an information level *k,* then 45 percent would have voted for Party A and 55 percent for Party B. As explained above, this inference does not presume that sociodemographic variables have either big or inevitable impact on the vote. It only assumes that the interaction terms in the vote function included every variable that simultaneously influences both vote choice and political involvement (i.e., turnout and knowledge).

In Bartels's (1996) analysis, the design of the interaction terms between INFO and the sociodemographic variables assumed that INFO has a linear effect—if any—on the way the sociodemographic variables influence VOTE, but that this effect can vary across sociodemographic variables and elections without any constraint. The assumption of linear information effects is parsimonious and consistent with the inherently probabilistic nature of vote choice and information-processing, but simplistic and unrealistic. There is an infinite number of alternatives: e.g., men's probability of supporting the Alliance of Free Democrats in Hungary may remain steadily low between INFO = 0 and INFO = .3, then sharply and linearly increase until INFO = .7, then decline exponentially beyond that point, but follow a totally different curve in the case of other parties. The exact shape of the relationship between information level and vote may vary widely depending on which party, which election and which demographic variable we are talking about.

To allow for nonlinear information effects, Althaus (1998) proposed to replace Bartels's (1996) probit and Delli Carpini and Keeter's (1996: 219–67) OLS-regressions with a logistic regression analysis, while leaving the rest of the procedure largely unaltered. But this is not even half a step ahead. Information effects are still assumed to be monotonous and follow a format that is just as rigid as in the linear formulation—it is just better suited for the analysis of dichotomous dependent variables. A far more radical specification seems to be called for, which could accommodate the infinite variety of nonmonotonous patterns alluded to above.

Unfortunately, the number of nonlinear specifications that one can think of is simply too large to explore them all. Appendix D presents the findings obtained with six different vote functions, all of which allow information effects to vary across elections, parties and sociodemographic variables, and four of which allow that these effects are

nonlinear, and not even monotonous. The results of interest here are reassuringly similar across the six models, and therefore the main text of the paper only discusses the results obtained with the baseline linear specification.

To adapt Bartels's model to the analysis of multiparty contexts, I employed discriminant analyses with VOTE as my dependent variable.[19] Like all other variables in the analysis, VOTE is described in Appendix B. The discriminant analyses were run separately for 18 samples extracted from the CSES data set. On reasons of exclusion and weighting procedures, the reader is referred to Appendix A. Because of their special electoral alignments and substantial oversampling in the German and British election studies, East Germany and Scotland were treated as if they were separate countries. The other 16 cases in the analysis were Australia, the Czech Republic, Western Germany, Hungary, Japan, Mexico, the Netherlands, New Zealand, Norway, Poland, Romania, Spain, Taiwan, Ukraine, the United States, and England and Wales combined. All surveys were carried out within three months after a national election held some time between 1996 and 1999.

As we saw above, the two constants of Bartels's model captured the direct impact of INFO on VOTE. In order to substitute them, information level was entered as a separate independent variable in the discriminant analyses. Note that the mean and standard deviation of INFO were held constant across samples, since I could not resolve their cross-nationally and longitudinally comparable measurement. The remaining predictor variables were the various interactions between information level and each of a set of sociodemographic variables appearing in Table 1.

Of course, the observations about the empirical relationships between VOTE and the interactions of sociodemographic characteristics with INFO are based on the actual voters only. However, the vote probabilities could be and were estimated for every

respondent included in the analysis. Thus, election outcomes can be readily estimated for a potentially infinite variety of hypothetical changes in turnout, as well as political knowledge in the electorate. The present paper considers three such scenarios.

Scenario 1 models the argument that given the sociodemographic differences between voters and nonvoters, election outcomes would differ if all citizens voted. Since this is almost trivially true, the real question is how big the difference would be. The model takes notice that nonvoters tend to be less knowledgeable than voters with similar sociodemographic characteristics. Therefore, the votes can differ between the two groups if the rise in turnout occurs without a corresponding rise in political information level among the former nonvoters. In other words, Scenario 1 assumes that turnout rises to 100 percent, but the information level of the erstwhile nonvoters remains unchanged.

To assess Scenario 2—envisioning that all voters become "fully" informed—the election outcome at INFO = 1 was estimated. Given the construction of the INFO variable, there is not a single respondent in the data set with such a high information level. However, this is only due to the relatively small number of political-knowledge questions in the study, which does not allow for much differentiation among the most informed respondents. Given the way INFO is constructed here, if the observed distribution of the variable perfectly approximated the normal distribution, the least informed member in the best informed one-half percent of each national sample would have ended up with INFO = 1. Since some may consider it unwise to extrapolate to an information level that we cannot even observe in our data, Scenario 3 models a situation where all citizens reach at least INFO = .65, i.e., roughly one standard deviation above the sample mean of INFO, but remains unchanged for those citizens whose observed value on INFO is already at or above .65. This value is exceeded by the mean political information level of only 5 percent of the groups analyzed in Table 4.

The vote shares of the various parties under the different scenarios were estimated through the mean conditional probability of voting support for each party *j* in the sample under the given scenario. These probabilities were relative to (1) the sociodemographic characteristics of the respondents; (2) the observed relationships between vote choice on the one hand, and the interactions of sociodemographic variables with information level on the other; (3) the given scenario's assumptions about respondents' information level; and (4) each party *j*'s share of recalled votes among the actual voters in the sample.

Three indicators of change were created for each scenario. The first is called PARTY-CHANGE and shows the change in the mean conditional probability of support for each of the 108 parties in the analysis among all citizens under the given scenario. For Scenario 1, these changes are relative to the mean probability of vote for the given parties among the actual voters, i.e., the change in a party's vote is caused by rising turnout only. For the remaining scenarios, the extent of change is evaluated relative to the simulated outcome under Scenario 1, where everyone votes but no one gets better informed.

The second indicator called NATIONAL-SWING shows the total change in election outcomes among all citizens (see Table 2), while the third (GROUP-SWING) shows the same for up to 90 sociodemographic groups within each country (for these groups see Appendix B). They were both calculated as half the sum of the absolute change, under a particular scenario, in the mean conditional probability of voting support in the given population or group, for each party *j* distinguished on the VOTE variable.

TABLE 2

Estimated Percentage Change in Election Outcome Under Different Hypothetical Scenarios of Change in Turnout and Voters' Information Level

Scenario	1	2	3
Presumed turnout Presumed information level	100% INFO remains unchanged	100% INFO = 1 for everyone	100% INFO rises to .65 or remains higher
Australia 1996	0.0	21.1	5.3
Czech Republic 1996	0.9	16.5	3.6
Germany (West) 1998	0.5	9.7	1.4
Germany (East) 1998	0.7	8.1	4.9
Hungary 1998	2.4	21.3	5.8
Japan 1996	1.5	39.8	12.1
Mexico 1997	0.9	14.3	1.1
The Netherlands 1998	1.0	15.7	5.1
New Zealand 1996	1.3	14.5	5.8
Norway 1997	1.0	14.2	3.3
Poland 1997	3.9	26.3	10.3
Romania 1996	0.8	18.3	4.9
Spain 1996	0.4	11.9	4.3
Taiwan 1996	0.8	20.8	7.7
USA 1996	2.9	9.4	3.1
Ukraine 1998	1.8	19.3	5.5
UK (England & Wales) 1997	1.1	16.8	2.5
UK (Scotland) 1997	1.1	9.2	2.0

Notes: Table entries are the values of the NATIONAL-SWING variable multiplied by 100. For the description of the different scenarios, see the main text; for a description of the NATIONAL-SWING variable, see Appendix B.

Findings

As explained above, the estimates about individual elections and parties are polluted with some random measurement errors of unknown size. Only the general trends are worth looking at. To begin with, Table 2 suggests that election outcomes would not be all that different if all voted, but might change dramatically if all voters suddenly

became far better informed, and the parties did not adapt to this change instantaneously. The modest 1 percent average change in election results under Scenario 1 pales in comparison with the stunning average of 16.9 percent change under Scenario 2.[20] The reason for the big difference seems to be that changes in turnout involve far fewer people than the admittedly earth-shattering move modeled by Scenario 2, where every voter becomes as knowledgeable as the least informed person in the best informed one-half percent of the electorate. This point is neatly supported by a comparison with the 4.9 percent average swing under Scenario 3. Here, the most knowledgeable citizens do not experience any change in their information level, but for someone with below average information there still is a sea change. Under this scenario, the information-induced electoral change is still much larger than under Scenario 1 of turnout change, but closer to that than to the stunning swings in Scenario 2.

An alternative explanation of the modest turnout effect could be that far more respondents report to have voted than actually did. This seems to be true in every country covered by the CSES data (data not shown). Though distorted recalls of electoral participation may inflate rather than deflate the correlation between social status and turnout (cf. Anderson and Silver 1986), their overall impact might lead to an underestimation of the potential for turnout effects on election outcomes.

To correct for this error, I regressed the simulated impact of Scenario 1 on sample means of reported turnout (i.e., the VOTING variable), while controlling for the overlap between the sociodemographic determinants of party choice and turnout in the given country. The latter was measured through the *eta* correlations between VOTE on the one hand, and the predicted score derived from the country-specific equivalent of the turnout function shown in Table 1 on the other. With $N = 18$, both variables registered a statistically significant impact of the expected direction on the change in election

outcome under Scenario 1, and explained about 70 percent of the simulated cross-election variance in *NATIONAL-SWING* (data not shown). Visual inspection of partial plots suggested that both effects were linear. The parameter estimates implied that every 10-percentage-point change in electoral participation may cause a 0.54 percent change in the election outcome (with a margin of error of ± .18 percentage point). Thus, for elections with turnout just around 50 percent—like those covered by the CSES study in Hungary, Poland and the United States—and an average degree of overlap between the sociodemographic determinants of turnout and vote choice, the scope for the total turnout effect on election outcomes may be anywhere between 2 and 3.5 percent. Incidentally, this estimate matches those in Table 2 regarding Hungary, Poland and the United States. Hence, as far as the critical questions of this study are concerned, I see little reason to be worried about the measurement errors introduced by biased recalls of electoral participation.

Let's now move to the evaluation of turnout-based voter inequality, i.e., the proposition that those parties would gain more votes if turnout increased to 100 percent, which would attract bigger vote gains in low-turnout than in high-turnout sociodemographic groups. Given the model set-up, the only chance that the proposition can be refuted is provided by the inclusion of INFO in the vote function. I think that this suggests not that there is a problem with the model, but that the truly interesting question concerns only the size, not the (indeed very predictable) direction of systematic turnout effects on each party's share of the vote.

This is assessed by regressing PARTY-CHANGE under Scenario 1 on a variable called RELATIVE-TURNOUT and its various interactions with both party size and how far the reported turnout was from 100 percent among the respondents. RELATIVE-TURNOUT is estimated for each party on the basis of the sociodemographic

characteristics of its voters (see Appendix B). The more favorable for a high turnout the sociodemographic set-up of a particular party's constituency was, the higher the score that the party obtained. In the sample, the highest value of RELATIVE-TURNOUT (.07) is registered by the 'other parties' in Hungary (see the value for all 108 parties in Table 5 in Appendix C). Compare this with the –.02 value of the agrarian-populist FKGP (under Hungary, Table 5, Appendix C) in the same election. These figures suggest that in this election, turnout was 7–(–2) = 9 percent higher in a group of Hungarians who had exactly the same sociodemographic composition as the voters of the 'other parties' than in another group of Hungarians whose sociodemographic composition matched those of the FKGP voters.

The impact of RELATIVE-TURNOUT on PARTY-CHANGE is in the expected (negative) direction, and highly significant despite the conservative bias of the test.[21] In the first column of Table 3, we see that the univariate effect is –.207. This implies that, for instance, the Hungarian FKGP would have won a (–.207)*(–.02) = .00414 larger fraction of the vote in the 1998 Hungarian election if turnout had been 100 percent. That is to say, they would have gotten 0.4 percent more of the total vote. Similarly, the vote share of the 'other parties' in the same election would have changed by a (–.207)*(.07) = (–.01449) fraction of the vote, i.e., they would have had about one and one-half percent less of the total vote if turnout had reached 100 percent.

These estimates can be improved by taking into account that larger parties are likely to experience both larger losses and gains simply because of their size. Similarly, the changes in the vote share of any party are likely to be larger where turnout jumps to 100 percent from a low, rather than a high initial base. These effects are controlled for by replacing in the equation RELATIVE-TURNOUT with its interactions with PARTY-SIZE—i.e., each party's fraction of recalled votes in the given election—and

TURNOUT-RISE, i.e., the difference between 100 percent and the proportion of respondents in the given country who reported to have voted in the given election. The results are shown in the last three columns of Table 3.

TABLE 3

OLS-regression of the Potential for Turnout-induced Swing Across Parties on the Relative Turnout of Party Supporters and its Interactions with Party Size and the Scope of the Simulated Rise in Turnout

Dependent variable	**PARTY-CHANGE (under Scenario 1)**			
	b *s.e.*	*b* *s.e.*	*b* *s.e.*	*b* *s.e.*
Relative-turnout	–.207** (.060)	–	–	–
Interaction of Relative-turnout and Party-size	–	–1.763** (.226)	–	–
Interaction of Relative-turnout and Turnout-rise	–	–	–.009** (.002)	–
Interaction of relative-turnout and turnout-rise and party-size	–	–	–	–.069** (.009)
Constant	–.000 (.001)	.000 (.001)	–.000 (.001)	.000 (.001)
Adjusted R-squared:	.394	.779	.411	.763

Notes: Table entries are unstandardized regression coefficients (with standard errors in parentheses) and the adjusted R-squared. For data source, weighting and variable coding, see the appendices. The data are weighted to correct for cross-system differences in the number of parties and the nonindependence of observations taken from the same party system. The unweighted *N* in the analysis is 108, and the weighted *N* is 18.

**two-tailed significance < .01
*two-tailed significance < .10

The best-fitting model, which explains a whopping 78 percent of the variance in PARTY-CHANGE, seems to be the one involving the interaction of PARTY-SIZE and RELATIVE-TURNOUT but omitting TURNOUT-RISE. Note, however, that the true variance in the rise in turnout implied by Scenario 1 is partly underestimated by distorted recalls of turnout among the respondents, and must be partly captured by the

RELATIVE-TURNOUT variable itself. It would be a contradiction in terms to accept the proposition that turnout influences election outcomes and to argue at the same time that the amount of change caused by 100 percent turnout is *not* dependent on observed turnout. Therefore, the single best parameter-estimate in the table must be the –.070 figure found in the last column.

To decipher the meaning of this estimate, consider the Dutch Labor Party (PVdA) first (see Table 5 in Appendix C). Its RELATIVE-TURNOUT is negative (–.02), it had 30 percent of the recalled votes among the Dutch respondents, and, if we were to believe recalls, the turnout was 9.5 percent short of 100 percent in the 1998 election in the Netherlands. Thus, had turnout been 100 percent in 1998, the PVdA's share of the vote would have changed by a positive (.00399) fraction of the vote. This figure is calculated by multiplying the respective parameter estimate (–.070) with the party's score on the interaction term, i.e., (–.02)*(.30)*(9.5). In other words, the PVdA would have had 0.4 percent more of the total vote.

To take another example, consider the party with the lowest RELATIVE-TURNOUT in my entire sample (–.10), New Zealand's Aotearoa Legalize Cannabis ALC. As one would guess from the name, the party attracted a youthful group of voters, and ended up with a tiny 2.1 percent of the recalled votes. About 88.7 percent of New Zealand respondents recalled to have voted in 1996. So I estimate that the ALC would have won a (–.070) multiplied by (–.10)*(.021)*(11.3) = .0016611 larger fraction, or 0.17 percent more of the vote if turnout had been 100 percent.

It seems then, that turnout-based voter inequality exists, and the preference schedules of some groups—like the apparently intense preference in some circles for the legalization of cannabis—remain underrepresented in election outcomes. The difference that this factor makes may decide a close election. Remember, however, that the

estimates about individual parties and elections are of no real interest here, since the parties would presumably adjust their behavior to a 100 percent turnout in ways that we cannot predict, and the estimates that I present are polluted by some measurement error. As I argued, our best guess is that these measurement errors are randomly distributed. In the case of turnout, these errors may occur because of the unequal mobilization of people of similar sociodemographic make-up, but who have different political preferences in particular elections. Therefore, what we need to focus on is the general trend that emerges from the estimates: a jump of turnout would only make for a small difference in the vote of most parties. As we saw in Table 2, these differences get significant only in countries with particularly low turnout. For the 1996 American elections, for instance, the estimates derived from the equation in the last column in Table 3 suggest that Clinton may have gotten 1.9 percent more, and Dole 3.1 percent less of the presidential vote if turnout had reached 100 percent.

Are knowledge-based voter inequalities any larger? Indeed, are they systematic at all? Table 2 suggests that contemporary electorates are probably a lot farther away from fully informed behavior than from a 100 percent turnout. However, information effects on election outcomes may be far more variable in their direction than are turnout effects. A move from observed to 100 percent turnout would change the sociodemographic composition of the electorate much the same way in any election. Whatever change occurs in election outcome under Scenario 1, it happens because of some previously underrepresented groups that account for a larger percentage of the voters. The direction of the effect on party fortunes is rather predictable. In contrast, a move from observed-to full-information level may have extremely varied effects on individual parties, depending on the myriad of situational effects active in any election.

To determine whether the unequal distribution of political information level may cause systematic political inequalities between sociodemographic groups, GROUP-SWING, i.e., the variable showing the net change of vote distribution in each of 90 demographic groups, is regressed on the average political information level in the groups. This latter variable is called GROUP-INFO, and the units of observation are the 90 sociodemographic groups defined by four variables described in Appendix B. Since the number of respondents within the groups would rapidly diminish if more detailed demographic breakdowns were employed, only the variables showing the strongest effects in Table 1 were taken into account: age, education, gender and income.

These regression analyses also control for NATIONAL-SWING, i.e., the estimated swing at the national level. Once again, the observations are pooled across party systems to filter out the effects of random measurement errors and situational effects. Within countries, the observations are weighted by the size of the respective group, and the weights of the 18 party systems are set equal in the pooled data set. The results are displayed in Table 4.

Under both Scenarios 2 and 3, the relevant coefficient is significant and negative: the scope of change tends to be higher for the initially least informed groups. The higher we set the threshold of full-information level, the less concentrated the behavioral changes are in the low-information groups. Indeed, the explained variance in Table 4 is markedly lower under Scenario 2 (when everyone's information level rises) than under Scenario 3.

The findings of Table 4 imply that for the less informed groups there is a bigger gap between observed and fully informed voting behavior than for the more informed groups. If fully informed choices are better than less informed choices, this finding means that unequal information turns into unequal political influence in the electoral arena. To

interpret the parameter of interest, remember that the standard deviation of the underlying normal variable of political information is set at 1/6 and that the metric of GROUP-SWING retains the metric of vote probabilities. Thus, the –.18 net effect of GROUP-INFO on GROUP-SWING under Scenario 2 tells us the following: In a typical national election, the difference between fully informed and actual vote distributions is approximately 18/6=3 percent bigger in a sociodemographic graph group that, on average, is one standard deviation (i.e., approximately .16) below the national mean on political information level than in a sociodemographic group approximating the national average on the latter variable. Under Scenario 3, the gap is about two and one-half times bigger yet.

TABLE 4

OLS-regression of the potential for information-induced swing across sociodemographic groups (GROUP-SWING) on system-level swing (NATIONAL-SWING) and the group mean of observed political information level (GROUP-INFO) under two scenarios of change in the citizens' information level

	Scenario 2	**Scenario 3**
	INFO = 1 for everyone	INFO reaches .65 or remains unchanged
	b *s.e.*	*b* *s.e.*
GROUP-INFO	–.178** (.052)	–.436** (.019)
NATIONAL-SWING	.831** (.044)	.757** (.042)
Constant	.211** (.027)	.264** (.010)
Adjusted R-squared	.209	.375

Notes: Table entries are unstandardized regression coefficients (with standard errors in parentheses) and the adjusted R-squared. On the variables see the main text and Appendix B. The cases are sociodemographic groups (up to 90 per country), and are weighted so that each country has equal weight in the total sample, each group has a weight proportional to its size within the original national sample in the CSES data set and the weighted *N* (1,390) is equal to the unweighted number of cases in the analysis.

**two-tailed significance < .01
*two-tailed significance < .10

A cursory reinvestigation of Table 1 reveals that deviations of this magnitude from the national average of political information level are rare but not inconceivable for groups that combine several sociodemographic characteristics associated with low political sophistication (i.e., young women with low education working in agriculture or belonging to a racial minority). Yet, there are few groups like this and not too many citizens belonging to them. Judged from this perspective, that 3 percent gap may even look fairly small: but the two and one-half times 3 percent (the respective figure under Scenario 2) cannot be dismissed hastily as irrelevant.

The final question is to what extent do the knowledge- and turnout-based voter inequalities cumulate. The question is not trivial, since, as we saw, the overlap between the determinants of TURNOUT and INFO is only partial, and the two-voter inequalities operate through different mechanisms. Probably the best way to answer the question is to look at whether changes in support for each party *j* under Scenario 1 correlate with those registered under Scenario 2 and 3. This way we can determine whether turnout- and knowledge-based inequalities strengthen or cancel out each other. With the 108 parties again weighted as in Table 3, the Pearson correlation between PARTY-CHANGE under Scenarios 2 and 3 is an impressive .82, significantly well below the .001 level. The correlation between the information- and the turnout-induced changes are, however, indistinguishable from zero. The pairwise correlation between PARTY-CHANGE under Scenario 1 and PARTY-CHANGE under Scenarios 2 and 3 is .01 and –.10, with $p = .959$ and .694, respectively.

Therefore, I conclude that the two types of inequalities neither strengthen nor cancel out each other: they live side by side. This finding also implies that there is no need to fear that if nonvoters started voting, or mandatory voting forced them to do so,

they would disproportionately support those parties that would lose support if the information level of the electorate increased. The present analysis should at least raise doubts about this popular belief.

Implications

In recent years, the problem of voter inequality attracted considerable attention among political theorists.[22] Some proposed radical measures to combat it,[23] while others pointed at a wide range of potential victims.[24] Yet, the scholarly literature on voting has rarely elaborated on voter inequality and for most of the time, probably deemed it an inevitable consequence either of democratic elections themselves or of the complex social environment in which they occur.[25] The more recent literature is dominated by a tide of ingenious works on how and why relatively uninformed citizens may be able to emulate the choices of political sophisticates,[26] or at least to make very good use of the little information they have.[27]

The present evidence suggests that the socially unequal distribution of turnout and political knowledge does introduce a systematic bias into the electoral arena. If turnout- and information-level among citizens were both higher and more equal, systematically different election results may obtain—presumably forcing political parties to adjust their offering to the behavior of a different electorate. But the magnitude of the political inequalities generated by unequal participation, and probably even those based on unequal knowledge, are such that elections may still be the most egalitarian decision-making mechanism ever invented—apart from lottery games, of course. There are three fundamental reasons for this.

First, turnout in national elections may seem low, but is still much closer to its theoretical maximum than to its minimum. Second, the determinants of vote choice and

political involvement overlap only weakly. It is in this context that the conspicuously weak effect of national turnout in Table 3—undeniably caused partly by the methodological artifacts discussed in the interpretation of the table—deserves attention. As we saw, the size of change in party fortunes that may occur if turnout increased to 100 percent is explained by RELATIVE-TURNOUT—a peculiar measure of the overlap between the demographic correlates of vote choice and participation—, and not by how far actual turnout is from 100 percent. Thus, turnout-based voter inequality is produced not so much by unequal turnout in itself, but by its interaction with voter alignments.

Third, information effects work very much like Russian Roulette. It is nearly random which groups and parties they put at a disadvantage in a given election. No doubt, the central tendency is that the difference between observed and fully informed behavior tends to decrease with actual political information level. However, even this relationship is weak, stochastic and may break down in some national elections. Replicating Table 4 for every party system separately, the relevant coefficient, showing the impact of group information level on the net effect of information on party switches under Scenario 2 in the group, was positive (though usually not significantly so) in 7 out of 18 times (data not shown). Though the same effect was statistically significant and in the expected negative direction in every party system under Scenario 3, the results regarding Scenario 2 raise the possibility that not only measurement errors, but true situational effects of information on election outcomes may also be involved in the reversal of the effect.

For example, imagine an election where political information level influences vote choices in just two ways. On the one hand, we see the old Marxist story staged in real life. Political awareness polarizes voters along social class lines, by making lower class voters more likely to vote for an antimarket party as their information level

increases, and pulling middle class voters towards a pro-market party in proportion to their political information level. Now, suppose that there is last-minute breaking news about a money scandal in the antimarket party, but it does not reach everyone. As a result, all better informed voters become more likely to vote for the pro-market party, since they are the most likely to learn about the scandal. But, if poorly informed middle class voters miss the news on the money scandal, they will be far less likely to accurately express their preferences regarding either money scandals or economic issues in the vote than the better informed upper class voters will. In contrast, poorly informed lower class voters may end up dividing their vote between the two parties exactly the same way as their better informed sociodemographic look-a-likes, provided that the pull of the two information effects is equally strong.

Clearly, the victims of less-than-perfect information in the electorate are, in this example, both the well-informed and the poorly informed middle class voters, in the first place, and—to the extent that the money scandal did not receive a fair enough punishment from the voters—the electorate as a whole, in the second place. On the individual level, there still is a positive relationship between political information level and the probability of casting a seemingly fully informed vote, although this correlation may have totally disappeared among lower class voters. But, on the level of social groups, the relationship between fully informed behavior and information level is simply reversed: the middle class is less likely to vote as if its members are fully informed.

Another subtype of these lottery-like information effects may occur when the breaking news can influence party choice only in a small fraction of the electorate, while it remains irrelevant for all other voters. If the information in question does not reach all members of the affected group in at least some indirect and mediated form, the whole

group will pay the political price for this—unless, of course, members of the group were prone to be pulled in conflicting directions by the news, anyway.

What distinguishes these situational effects from the kind of voter inequality that has been discussed in this paper is that they do not systematically discriminate between social groups. Rather, anyone can fall victim to the inequalities of the political influence temporarily induced by them. While these situational effects probably increase the absolute difference between fully informed and actual votes in the electorate, at the same time they weaken the systematic relationship between sociodemographic status and fully informed voters.

Briefly, the victims of information-induced inequalities change constantly, and they are certainly not always the low-information and low-turnout groups. Rather, it is the electorate as a whole that systematically shows a big gap between fully informed and observed behavior. The systematic variations across social groups are pale in comparison.

Overall, then, the electoral arena may be a nearly, though not perfectly, neutral arena for aggregating political preferences in an electorate characterized by unequal turnout and knowledge. This finding should be reassuring for normative democratic theory, but leaves us with some empirical puzzles. Consider the following.

It is often argued that left-wing parties, because of the social composition of their electorate, are disadvantaged by less than 100 percent turnout (cf. Pacek and Radcliff 1994; Lijphart 1997). We can test this proposition with the present data by regressing change in each party's share of vote on the interaction of observed turnout in the given population and the left-right position of the given party (data not shown). I define this interaction term as (100 – T) * I, where T is the proportion of respondents from the given party system who recalled to have voted, and I is a dichotomous measure of party ideology coded 1 for left-wing parties and –1 for right-wing parties.[28]

The impact of this interaction term on the percentage change of support for a party under Scenario 1 is 0.010 (with *N* set at 18, the standard error of the estimate is .009, and the effect statistically insignificant with $p = .307$). The parameter implies that for every one percent increase in turnout, a left-wing party can expect a change in its vote share anywhere between a one-hundredth of a percent *loss* and a three-hundredths of a percent *gain*.

The insignificance and small size of the coefficient may be caused by the fact that in many of the new democracies covered by CSES, the meaning of left and right, and consequently the composition of the left-wing electorate, is rather different from what it is in the older democracies. Hence, the analysis was replicated on a smaller sample of old democracies, including Australia, England and Wales, Japan, the Netherlands, New Zealand, Norway, Scotland, Spain, the United States and the Western part of Germany. In this more limited sample, with the weighted *N* set at 10, the interaction effect had the expected sign and was significant at the .018 level, suggesting that for every one percent increase in turnout, the average left-wing party can expect a 0.038 (± 0.025) percent of change in voting support. Thus, for an old democracy with a roughly 80 percent turnout in a national election, the average left-wing party would gain 20 times this much, i.e., between 0.26 and 1.26 percent of the vote if turnout had been 100 percent.[29] Since there can be more than one left-wing party in a system, and the losses of the right mirror the gains of the left, the total gain of the left can be bigger than this. Yet, it is certainly not astronomical. A clear implication is that nearly all of the strong link between left-party support and turnout observed across national election results by Crewe, (1981) as well as Pacek and Radcliff (1994), might be due to factors other than turnout effects on left-party vote.[30]

This leaves us with some puzzles. In the US, higher turnout was found to be associated with higher agreement between the elite and the masses on policies (Hansen 1975; Powell 1982; Verba and Nie 1972: 309–18), and with higher responsiveness of public policies to lower class interests (Hicks and Swank 1992; Hill and Leighley 1992; Hill, Leighley and Hinton-Andersson 1995). Assuming that these findings hold cross-nationally, the present results suggest that the reason for all this may not be a massive change in election outcomes under the impact of higher voter participation. But then, what is it?

Endnotes

[1] The data are made available through the website of the American National Election Study at <http://www.umich.edu/~nes/cses/>, Ann Arbor, MI: University of Michigan, Center for Political Studies [producer and distributor], 1995–1999. The data collection was supported by many different organizations around the world. The CSES Secretariat is supported by the National Science Foundation under Grant Nos.: SBR-9317631 and SES-9977967.

[2] Cf. Riker (1982). This is not to say that majority rule is always the best means of guaranteeing citizens' political equality. Majority rule, however, is impossible to justify in normative terms without a reference to the egalitarian ideal.

[3] Cf. Collier and Levitsky (1997). The major contemporary exception is also telling about the importance of citizens' equality in the electoral arena. When Przeworski (1991: 11–4) omits the equality requirement from his definition of democracy, he justifies this with the argument that free electoral competition for political power leads to the introduction of general franchise more or less automatically—an inference that he himself admits was contradicted by the long reign of the apartheid regime in South Africa.

[4] Downs (1957: 223). Similar remarks abound in the literature, e.g., "information and transaction costs [...] introduce class bias into the electoral system, so that those who are on top in terms of wealth and other resources also come out on top in terms of political influence" (Page 1978: 190). Cf. also Delli Carpini and Keeter (1996: 6); Donohue, Tichenor and Olien (1975), and Moore (1987).

[5] For all practical purposes, this paper treats preferences as given. Some may want to counter that some preferences may be subject to change under the impact of new information about their incompatibility with higher order preferences. However, in the present context, this phenomenon can be conveniently lumped together with the impact of information on the 'objective' congruence between preferences and choice. After all, vote choice can be seen as a derived preference that may be revised when new information reveals its conflict with some other preferences.

[6] Of course, beyond a certain point, further information to the voters may not significantly improve the information content of their vote. Increasing decision-making costs may counterbalance the beneficial impact of more information, and more information also means a greater probability of possessing at least some potentially misleading information. Yet, given the typical information level in mass electorates—succinctly characterized by Converse (1990, 2000) as a variable with a low mean and very high standard deviation—, more information must usually give voters greater influence on the representatives via the electoral arena.

[7] Note again that many political inequalities postulated in the previous literature fall outside of this definition. For instance, some argue that damage is done to the political influence of low-turnout groups already by the fact that politicians anticipate their low turnout and therefore neglect their supposed interests (Verba et al. 1993: 304). However, if their entry in the electorate would not alter the vote shares of the existing alternatives, then such a limitation of political influence would not be caused by the social inequality of participation per se. Rather, the reason is the prevalence of some beliefs among politicians about the electoral significance and likely reactions of these groups and the entry costs faced by new challengers. These beliefs may be right or wrong. The political consequences they generate simply do not belong specifically to the voter inequalities that this paper addresses. The latter are impossible to imagine without some change in election outcomes (provided the alternatives remain exactly the same) when the inequalities in question disappear.

[8] Regarding the counter-arguments and their inadequacy, see especially the reviews of Blais (2000: 5ff) and Mueller (1989: 348–69).

[9] A note is due here about the problem of expressively vs. instrumentally rational behavior. In the more conventional instrumental models of voting, commonly associated with the Downsian legacy, voters are interested in the political consequences of election outcomes: especially in

government policies and performance. In the expressive model, the benefit of voting for the individual citizen derives solely from the intrinsic reward of casting a vote for a particular party or candidate (see especially Brennan and Hamlin 1998; Schuessler 2000). The reference point for any concept of voter inequality must be citizens' influence on political outcomes, that is to say, the extent to which vote choices correspond to fully informed instrumentally rational behavior. Empirically, however, voting behavior may well be motivated by its intrinsic reward. Therefore, fully informed vote choices may have little to do with fully informed and instrumentally rational choices. For the sake of making the problem of voter inequality empirically tractable at all, I propose to assume here that the gap between instrumentally and expressively rational voting behavior decreases as one's information level increases. Indeed, it seems really implausible that voters' expressive benefits from a particular course of action would not be reduced by the knowledge that it may—to the infinitesimal extent that a single vote matters at all—go against the election outcome that they, given their preferences, would most like to see.

[10] Lau and Redlawsk (1997) tried to simulate "correct" (i.e., fully informed) votes through experiments. The results suggested that receiving abundant candidate information does not make any difference in how citizens actually vote. Yet, apart from the unavailability or high cost of information, the absence of the cognitive styles, the contextual knowledge stored in long-term memory, and the self-confidence that normally come with high information level may also prevent citizens to process relevant information and behave as if they were fully informed (Althaus 1998: 547). Hence, skepticism is warranted regarding experimental simulations of fully informed votes.

[11] In that case, we may even consider individual-level variation in political involvement a spontaneous social process of delegation and representation through self-selection, which is certainly close to how Berelson et al. (1954: 110–12) interpreted the role of opinion leaders.

[12] Cf. Zaller (1992); Sniderman, Brody and Kuklinski (1993).

[13] Some may counter that there are so many relevant political attitudes that some must violate my assumption. However, the same law of great numbers that this counterargument invokes also suggests that there will be several political attitudes violating my assumption, and their effect on my estimates will be randomly distributed and cancel out each other.

[14] Bartels's justification for the exclusion of nonsociodemographic traits from the vote function was precisely that unlike gender, race, education, age or income, such traits may change as a result of getting more politically active and/or better informed.

[15] Luskin (1987); Zaller (1992: 333ff). Smith (1989) finds that the number of considerations mentioned while responding to open-ended questions about what you like and dislike about individual political parties has similarly attractive properties as a measure of political sophistication, and figures that this measure really reflects knowledge rather than talkativeness or something else.

[16] Delli Carpini and Keeter (1996: 142–46 and Appendix 4). See also Neuman (1986: 68–73) and Price and Zaller (1990). Note that the unidimensionality of political information level does not mean that all voters are equally likely to be moved by the same considerations or issues.

[17] Obviously, if the constant for the fully informed voters is bigger than the constant for the fully uninformed voters, then increases in political information level are likely to move voters closer to the party in question, irrespective of their sociodemographic traits.

[18] I.e., provided that women are not much better informed than men. If they were, support for Party A might even decline when voters become fully informed, since this step would prompt a bigger drop of support among men than the simultaneous gain among women.

[19] This choice was motivated by the convenient generation of predicted vote probabilities by the respective module in the SPSS package. Purists may find this method objectionable and recommend multinomial logit/probit instead, but the only gain offered by the latter would be in the precise estimation of the standard errors of parameters and predicted scores—both of which are irrelevant for my present purposes.

[20] The standard deviation of these changes across the 18 samples was 1 and 7 percentage points, respectively.

[21] Obviously, the vote gains and losses of rival parties in a particular election mirror each other, and cannot be considered as independent observations. This prevents the precise estimation of statistical errors. In the regression analyses reported in Tables 3 and 4, the 108 parties were weighted by one divided by the number of parties distinguished by my analysis in the given party system. This yields a weighted number of cases of 18 (the number of party systems in the analysis), which is certainly a radical understatement of the true sample size, and must therefore inflate the estimated statistical errors of parameter estimates more than necessary. In other words, I set my tests extremely conservative.

[22] Cf. Offe (1997); Simpson (1997).

[23] In this context, Knight and Johnson (1997) discuss measures like granting special veto rights to disadvantaged groups, government support for civic associationalism and weighted representation.

[24] Breton and Breton (1997: 179–80) cite previous works discussing this problem with respect to the elderly, the homeless, the mentally disabled, native people and people of color, poor women, single mothers, children in state custody, immigrants, refugees, abused women, the physically disabled and disadvantaged youth.

[25] See Berelson et al. (1954: 59); Converse (1987: S20–S23, 1990: 387); Downs (1957: 94, 221, 223, 235, 252–56, 263–66, 273); Smith (1989: 6). Bartels's (1998) discussion of voter inequality follows an entirely different line of argument than the present paper and is therefore not considered here.

[26] Cf. especially Lau and Redlawsk (1997); Lupia (1994a); McKelvey and Ordeshook (1986, 1990).

[27] Cf. Popkin (1991); Sniderman, Glaser and Griffin (1990).

[28] The data on party ideology are based on the mean self-placement of a party's voters in the sample on a left-right scale. The self-placements were standardized to have a sample mean of 0. Each party with a negative mean among the supporters was classified as 'left', and all parties with a positive 'mean' were classified right-wing. In Japan and Taiwan, the left-right scale was substituted with the supplementary issue scale provided with the CSES data. The conservative endpoint of the Japanese liberal-conservative, and the "favors preserving the status quo" endpoint of the "change and reform" vs. "preserving the status quo" scale in Taiwan were treated as equivalents of the right-wing position on the standard left-right scale.

[29] These estimates assume, of course, that of two citizens with identical sociodemographic profile, and information level, the one with a left-wing preference is equally likely to vote as the right-winger.

[30] For instance, it may be that the presence of a strong socialist left leads to a higher mobilization of both pro- and anti-socialist citizens.

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APPENDIX A: Data Source

All empirical analyses reported in the paper are based on pooled cross-national data from the June 2000 version of the CSES (Comparative Study of Electoral Systems) Integrated Micro Data Set, made publicly available through the website of the project hosted by the American National Election Study. In each country covered by the study, national probability samples of the adult population were interviewed shortly after a national election. Because of their different party systems and very substantial oversampling in the CSES study in the respective CSES surveys, East Germany and Scotland were treated in the present analysis as if they were separate countries. Hence, the total number of countries/party systems in the analysis is 18. Errors found in the party codes were corrected with the prompt help of the principal investigator of the study in the Ukraine. For further information regarding study design, the reader is referred to the codebook of the study that can be downloaded together with the data set at <http:/www.umich.edu/~nes/cses/>.

Inclusion and Exclusion of Cases in the Analyses

Argentina, Israel and Lithuania were omitted from all analyses reported in this paper because the political knowledge variables for these countries are missing from the June 2000 version of the CSES data set. A further 1,442 respondents were excluded from the analysis who claimed to have voted in the last election, but did not give a valid answer to the question about vote choice. Another 486 respondents with missing values on both variable V115 (participation in the last election) and VOTE (vote choice) were excluded from all analyses. Five respondents with a valid response to the question about vote choice in the last election, but originally assigned a ‘did not vote’ answer at V115, were recoded on all variables pertaining to participation in that election, as if they had voted. Supporters of ‘other parties’ in Japan, the Netherlands and Scotland were excluded because they had been too few to be treated as a separate category on the VOTE variable (on the coding of this variable see below).

Weighting

Within countries/party systems, the data are weighted with the country-specific weighting variables if any were provided with the CSES data set. Out of the multiple choices available, GERWT1 was used to weight the German and NZWT2 to weight the New Zealand data file. The mean of the weight variable was adjusted to equal exactly 1 within each of the 18 countries/party systems. For all analyses that involved a pooling of data across the 18 systems, the weights were adjusted so as to give equal weight to each country/party system.

APPENDIX B: Variables and Coding

Variables in Table 1 and the vote function

AGE/10: age of respondent in years divided by ten. Missing values were recoded as 4.5.

ABS(AGE – 45)/10: absolute value of (AGE – 45) divided by ten.

DEVOUT: coded 1 for weekly church attendance and 0 otherwise.

EDUCATION LOW: coded 1 for primary education or less and 0 otherwise.

EDUCATION HIGH: coded 1 for university education or more and 0 otherwise.

FARM JOB: coded 1 for agricultural occupation and 0 otherwise.

FEMALE: coded 1 for women and 0 otherwise.

INCOME: personal income, divided into quintiles (from 1 = lowest to 5 = highest) by country. Missing values recoded as 3.

INFO: the respondents' general political information level. This summary measure is based on variables V110, V111 and V112 of the CSES study, which record responses to three neutral, factual and unequally demanding country-specific political knowledge questions. For instance, American respondents were asked to name the office held by William Rehnquist (correct response: Chief Justice of the Supreme Court), Al Gore (Vice President) and Newt Gingrich (Speaker of the House of Representatives); and 7, 85 and 54 percent of them gave correct answers, respectively. By way of comparison, the questions in the Czech Republic concerned the percentage threshold that parties have to pass to win any seat in lower house elections, the name of the Minister of Transportation at the time of the election, and the number of seats in the lower house, which were correctly identified by 72, 59 and 57 percent, respectively. To create variable INFO, the number of each respondent's incorrect responses was subtracted from the number of his or her correct responses. The resulting score was recoded using the Blom procedure so as to assign such values to the variable that—within each country—INFO's distribution approximated, as closely as possible, that of a continuous variable with a normal distribution, a mean of 0.5 and a standard deviation of 1/6. This was achieved by first computing the normal scores with

SPSS 10, then recoding all normal scores lower than minus 3 to minus 3 and all normal scores higher than 3 to plus 3. Finally, the normal score values were linearly transformed so that the theoretical minimum and maximum of variable values became 0 and 1, respectively. Thus, the country mean and standard deviation of political information level is essentially constant across samples.

MANUAL WORK: coded 1 for nonagricultural manual workers and 0 otherwise.

MINORITY 1: coded 1 for Asians in Australia, residents of Moravia in the Czech Republic, Catholics in either part of Germany and the Netherlands, Roma in Hungary, natives in Mexico, Maori people in New Zealand, ethnic Hungarians in Romania, Catalan-speakers in Spain, mainland Chinese in Taiwan, African-Americans in the US, ethnic Russians in the Ukraine, people of Asian or African origin in England and Wales, and 0 otherwise.

MINORITY 2: coded 1 for Catholics in Australia and New Zealand, Buddhists in Taiwan, Catholics and Jews in the US, residents of three Western regions in the Ukraine, and 0 otherwise.

RACE (used instead of MINORITY1 and MINORITY2 in the pooled cross-national analysis reported in table 1): coded 1 for Asians in Australia, Roma in Hungary, natives in Mexico, Maori people in New Zealand, African-Americans in the US, people of Asian or African origin in England and Wales, and 0 otherwise.

RURAL RESIDENCE: coded 1 for residents in rural areas and 0 otherwise.

TURNOUT: the sample mean of VOTING, in other words the fraction of respondents with nonmissing values on V115 of the CSES study who reported to have voted in the last election.

VOTE: recalled party or candidate choice in the last national election. For coding, see Table 5. Nonvoters are assigned a missing value.

VOTING: participation in last election. Coded 1 if the respondent recalled to have voted in the last election, and 0 if the respondent recalled to have abstained. See Appendix A on the handling of missing values and contradictory responses.

Variables used in constructing the sociodemographic groups that are the units of analysis in Table 3

AGE 5: coded 1 for 30 years old and younger; 2 for the 31–40 years old; 3 for the 41–50 years old; 4 for the 51–60 years old; 5 for 61 years and older.

EDUC 3: coded 0 for less than completed secondary education, 2 for a college degree or higher, and 1 otherwise.

GENDER: coded 1 for women and 0 otherwise.

INCOME 3: personal income, coded 0 for respondents in the bottom two quintiles, 2 for the top two income quintiles, and 1 otherwise.

Variables in Tables 2, 3 and 4

GROUP-INFO: the mean value of variable INFO in the 90 demographic groups defined with the help of the variables listed in Appendix D.

GROUP-VOTING: the mean value of variable VOTING in the 90 demographic groups defined with the help of the variables listed in Appendix D.

GROUP-SWING: the equivalent of NATIONAL-SWING on the level of the 90 demographic groups defined with the help of the variables listed in Appendix D. The computation is identical to that of NATIONAL-SWING except that the values are calculated for each group separately.

NATIONAL-SWING: system level estimate of change in election outcome under hypothetical scenarios. This variable is observed at the level of 18 party systems/countries. For scenarios 1 and 2, these changes are relative to vote shares defined by the mean conditional probability for each party among the actual voters under the given scenario. For the remaining two scenarios, the extent of change is evaluated relative to the election outcome under Scenario 1. Calculated by adding the absolute values of the PARTY-CHANGE variable for each country, and dividing the sum by two.

PARTY-CHANGE: the change in the mean conditional probability of support for each of the 108 parties in the analysis among all citizens under a given scenario. For Scenarios 1 and 2, these changes are relative to the mean conditional probability

of vote for the given parties among the actual voters under the given scenario. For the remaining two scenarios, the extent of change is evaluated relative to the election outcome under Scenario 1. For the remaining two scenarios, the extent of change is evaluated relative to the election outcome under Scenario 1.

PARTY-SIZE: a party's fraction of all recalled votes in the last legislative (in the US presidential) election in the CSES data.

RELATIVE-TURNOUT: the difference between the mean 'predicted turnout' of a party's voters and the mean 'predicted turnout' of all voters in the same sample. High values signal that the party's voters tended to come from sociodemographic groups that show above-average turnout. Values of predicted turnout were derived from a logistic regression equation with VOTING as the dependent, and all other variables appearing in Table 1 except INFO as the independent variables. The regressions were run separately for each of the 18 party systems distinguished in the analysis.

TURNOUT-RISE: the difference between 100 (percent) and the mean 'predicted turnout' of all voters in the same sample. Values of predicted turnout were derived from a logistic regression equation with VOTING as the dependent, and all other variables appearing in Table 1 except INFO as the independent variables. The regressions were run separately for each of the 18 party systems distinguished in the analysis.

APPENDIX C: The 108 Parties and Party Groups in the Analysis

Column 1 of the table below shows each party's percentage share of recalled votes in the last legislative (in the US presidential) election in the CSES data.

Column 2 shows the values of the RELATIVE-TURNOUT variable for each of the three scenarios discussed in the text.

Columns 3–5 show the values of the PARTY-CHANGE variable for each of the three scenarios discussed in the text.

TABLE 5

Parties	Vote Share (%)	Relative turnout	Change in the party's fraction of the vote under Scenario		
			1	2	3
Australia					
Liberal Party	46.2	0.001	0.000	–0.061	–0.004
Australian Labour Party	36.3	0.000	0.000	–0.140	–0.040
National (Country) Party	5.8	–0.001	0.000	0.043	0.018
Australian Democrats	6.6	–0.001	0.000	0.059	0.017
Greens	2.4	–0.003	0.000	–0.011	–0.008
other parties	2.6	0.000	0.000	0.110	0.018
Czech Republic					
CSSD	25.5	–0.006	0.003	–0.066	–0.018
KDU-CSL	7.3	0.039	–0.007	0.028	0.003
KSCM	9.8	0.007	–0.001	0.018	0.008
ODA	9.2	–0.004	0.000	0.065	0.013
ODS	33.6	–0.001	0.003	–0.090	–0.009
SPR-RSC	6.4	–0.024	0.003	–0.008	–0.01
other parties	8.3	0.002	0.000	0.055	0.012
Germany (West)					
CDU, CSU	37.2	0.007	–0.003	–0.047	0.005
SPD	42.2	–0.007	0.003	–0.05	0.000
FDP	6.9	0.014	–0.002	0.079	0.010
B' 90/Grünen	7.3	0.003	0.001	0.002	–0.004
other parties	6.4	–0.013	0.001	0.016	–0.01

Continued on next page

TABLE 5—*Continued*

Parties	Vote share (%)	Relative turnout	Change in the party's fraction of the vote under Scenario		
			1	2	3
Germany (East)					
CDU, CSU	27.3	0.011	–0.005	–0.026	–0.010
SPD	35.1	0.003	–0.002	0.026	0.025
FDP	3.3	–0.034	0.002	0.032	0.012
B' 90/Grünen	4.2	–0.002	0.000	–0.010	–0.007
PDS	21.6	–0.002	0.000	0.023	0.013
other parties	8.6	–0.032	0.004	–0.044	–0.033
Hungary					
Fidesz-MPP	31.3	–0.025	0.022	–0.124	–0.034
FKGP	12.8	–0.023	0.002	0.013	0.019
MIÉP	3.6	0.018	–0.002	0.066	0.011
MSZP	35.1	0.009	–0.006	–0.090	–0.024
MP (Munkáspárt)	3.5	0.002	–0.003	0.070	0.009
SZDSZ	8.6	0.034	–0.006	0.048	0.015
other parties	5.2	0.073	–0.008	0.016	0.004
Japan					
Liberal Democratic Party	40.8	0.018	–0.008	–0.242	–0.061
New Frontier party	26.3	–0.023	0.015	–0.156	–0.060
Democratic party of Japan	16.7	–0.006	–0.001	0.045	0.036
Social Democratic Party	6.9	0.008	–0.002	0.045	0.021
Japan Communist Party	9.3	–0.005	–0.003	0.308	0.064
Mexico					
PAN	25.3	0.013	–0.005	–0.067	–0.008
PRI	37.0	0.005	–0.002	–0.037	–0.003
PRD	31.3	–0.018	0.009	–0.038	0.001
PT	3.6	0.002	–0.001	0.078	0.006
other parties	2.8	0.022	–0.002	0.065	0.004
The Netherlands					
PVdA	29.9	–0.018	0.008	–0.086	–0.024
CDA	18.0	0.012	–0.005	0.043	0.014
VVD	22.3	0.003	0.000	–0.065	–0.017
D'66	11.9	0.016	–0.002	0.026	0.012
GroenLinks	8.8	–0.002	–0.001	0.085	0.025
SGP, GPV and RPF together	4.6	0.039	–0.002	–0.006	–0.001
SP	4.5	–0.020	0.001	0.002	–0.009

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TABLE 5—*Continued*

Parties	Vote share (%)	Relative turnout	Change in the party's fraction of the vote under Scenario		
			1	2	3
New Zealand					
Labour Party	28.5	0.001	–0.004	–0.007	0.018
National Party	32.1	0.009	–0.005	–0.032	0.011
New Zealand First	13.7	–0.012	0.003	–0.015	0.001
Alliance	11.4	–0.002	–0.002	0.035	0.014
ACT	5.7	0.021	–0.002	0.038	0.010
Christian Coalition	4.4	–0.013	0.003	–0.066	–0.029
United Party	0.6	0.032	–0.001	0.061	0.003
Aotearoa Legalize Cannabis	2.1	–0.104	0.006	–0.025	–0.026
other parties	1.4	–0.020	0.001	0.012	–0.003
Norway					
Socialist Left Party	6.9	–0.020	0.002	0.067	0.010
Labour Party	35.6	0.004	0.000	–0.093	–0.013
Liberal Party	4.9	–0.005	–0.001	0.047	0.009
Christian People's Party	15.4	0.008	–0.003	–0.01	–0.003
Centre Party	7.8	–0.012	0.001	0.001	0.002
Conservative Party	15.6	0.026	–0.007	0.026	0.012
Progress arty	11.2	–0.026	0.004	–0.013	–0.001
other party	2.6	–0.046	0.003	–0.025	–0.016
Poland					
UP	3.7	–0.022	0.001	0.192	0.026
UW	14.5	0.034	–0.009	–0.084	–0.028
AWS	39.3	0.003	–0.015	–0.161	–0.008
SLD	24.5	–0.006	–0.012	–0.002	0.040
PSL	6.7	–0.026	0.004	0.036	0.029
ROP	5.1	–0.026	0.031	–0.002	–0.050
KPEIR RP and KPEIR together	3.4	–0.046	0.003	–0.014	–0.017
Other parties	2.9	0.026	–0.003	0.035	0.008
Romania					
Social Democratic Union*	14.7	–0.009	0.001	0.030	0.004
Romanian Party of Social Democracy	26.2	–0.013	0.006	–0.108	–0.042
Democratic Union of Hungarians	4.5	0.037	–0.004	-0.032	0.000
Greater Romania Party	4.3	0.012	0.001	0.024	–0.007
Romanian Democratic Convention**	45.7	0.004	–0.003	–0.042	0.029
other parties	4/6	0/013	–0.001	0.129	0.017

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TABLE 5—*Continued*

Parties	Vote share (%)	Relative turnout	Change in the party's fraction of the vote under Scenario		
			1	2	3
Spain					
PP	29.0	–0.005	0.002	0.000	0.012
PSOE	37.3	0.004	0.000	–0.079	–0.023
IU	9.9	–0.013	0.001	0.119	0.031
CiU	6.2	0.016	–0.004	–0.021	–0.013
other parties	17.6	0.001	0.001	–0.019	–0.007
Taiwan					
Kuomintang	52.2	0.001	–0.003	0.208	–0.047
Democratic Progressive Party	30.2	0.003	–0.005	0.141	0.061
Chinese New Party	11.7	–0.010	0.006	0.039	0.016
other parties	5.9	–0.007	0.001	0.028	–0.030
United States					
Bill Clinton	53.1	–0.018	0.019	0.014	0.013
Robert Dole	37.6	0.043	–0.029	–0.094	–0.031
Other presidential candidates	9.3	–0.069	0.010	0.080	0.018
Ukraine					
All-Ukrainian Association Gromada	7.0	–0.008	0.002	0.067	0.005
Party of Greens of Ukraine	7.7	–0.059	0.010	–0.053	–0.019
Communist Party of Ukraine	34.3	0.005	–0.005	–0.079	0.006
People's Rukh of Ukraine	8.5	0.063	–0.012	0.079	0.03
People's Democratic Party	4.2	–0.007	0.000	0.047	0.007
Electoral block of the Socialist Party	5.1	0.026	–0.002	–0.014	–0.018
Social-Democratic Party of Ukraine	4.4	–0.043	0.005	–0.004	–0.008
Progressive Socialist Party	4.8	–0.018	0.001	–0.011	–0.010
other parties	24.1	0.000	0.001	–0.031	0.007
United Kingdom (England and Wales)					
Conservative Party	30.5	0.023	–0.009	–0.078	–0.012
Labour Party	47.6	–0.015	0.011	–0.09	–0.014
Liberal Democrats	18.2	–0.003	–0.001	0.064	0.01
other parties	3.7	0.016	–0.002	0.105	0.016
United Kingdom (Scotland)					
Conservative Party	13.4	0.034	–0.005	0.030	0.002
Labour Party	51.6	–0.014	0.010	–0.092	–0.017
Liberal Democrats	15.2	0.026	–0.006	0.001	–0.003
Scottish National Party	19.8	–0.007	0.001	0.061	0.018

*Also includes the separately coded Democratic Party and Romanian Social democratic Party, which run together in the election as Social Democratic Union.

**Also includes the separately coded National Liberal Party, National Peasant and Christian Democratic Party, National Liberal Party—Democratic Convention, and Romanian Ecologist party, which run together in the election as Romanian Democratic Convention.

APPENDIX D: Replications of the Analysis with Different Specifications of the Vote Function

The main text of this paper and Tables 2–4 presented results that were obtained with a baseline linear model of vote choice. This model, labeled *Model 1* below, closely follows Bartels's (1996) specification, and includes the interaction of every single sociodemographic variable appearing in Table 1 with both INFO and (1-INFO) among the independent variables affecting vote choice. The choice of variables that interact with INFO and (1 -INFO) in the model was justified in the main text.

The design of the interaction terms in Model 1 assumes that INFO has a linear effect—if any—on the way sociodemographic variables influence VOTE, and that this effect can vary across variables, countries and parties without any constraint. The reason for this is that the assumption of linear information effects is parsimonious and consistent with the inherently probabilistic nature of vote choice and information-processing. Furthermore, it is no less plausible than any one alternative in the infinite pool of nonlinear specifications, and largely eliminates the problem that the results regarding voter inequality may depend on where the threshold of "full" information is drawn. Under the assumption of linearity the choice of this threshold still influences the estimated gap between simulated "fully informed" and observed election outcomes. However, as long as the threshold is set well above the observed information level of most respondents (at INFO = .7 or above, for example), its precise value cannot possibly influence the direction and relative magnitude of the estimated between-group differences in the gap between observed and simulated vote distributions.

However, the assumption of linear information effects is often criticized for its lack of realism. Its ideal replacement should accommodate the infinite variety of nonlinear and indeed, nonmonotonous information effects that one can conceive: for instance, that support for US Republicans among women increases as INFO rises from near zero to .4, and then declines until INFO = .6 and remains steady beyond that information level. Unfortunately, a flexible model that can allow for this will radically increase the number of interaction terms in the vote function, and hence, both the number

of parameter estimates and the multicolinearity between the various interactions of political information level and other variables. The predictable result is that the random measurement error component of the simulated vote distributions simply explodes.

Another problem with nonlinear models is that one can think of an infinite variety of them, and neither their fit to empirical data nor theoretical plausibility give a clear clue about which one should be adopted. For the purposes of the present analysis, I tried to pick the most salient options, which both allow for a very large, though not infinite, variety of nonmonotonous information effects on the vote. *Model 3* includes all the variables that entered Model 1, and adds to them $INFO^2$, as well as interactions between each sociodemographic variable and both $INFO^2$ and $(1\text{-}INFO^2)$. *Model 5* includes all the variables that entered Model 1, and adds to them $(.5\text{-}INFO)^2$, as well as interactions between each sociodemographic variable and both $(.5\text{-}INFO)^2$ and $(1–(.5\text{-}INFO)^2)$. Thus, Model 3 follows a convention in the statistical estimation of nonlinear models, while Model 5 mimics Zaller's (1992) influential reception acceptance model, which suggests that information effects are often larger at medium than at either very low or very high information level.

One could criticize Model 1, and especially Models 3 and 5 for allowing a large number of statistically insignificant effects to enter the vote function, and hence to contribute some noise to the simulated election outcomes. This certainly contributes to the error component of the estimated outcomes, but no obvious remedy is available. Some of the weaker interaction effects that did actually occur in a given election are unlikely to be statistically significant in the relatively small samples in the analysis, especially in the light of the multicolinearity between the numerous interactions allowed between information level on the one hand, and the sociodemographic variables on the other. Thus, excluding all statistically insignificant effects from simulation procedure would only repeat the same error with the opposite sign.

The most prudent way of dealing with this tradeoff is probably to commit both errors and see what happens. Thus, *Model 2* replicates Model 1 with a stepwise inclusion of predictor variables in the vote fun6tion. Hence, the pool of interaction terms that are allowed to enter the vote function is the same as in Model 1, but only those enter the

model in each of the 18 samples in the analysis that happen to register a statistically significant effect in the sample in question. Similarly, *Model 4* replicates Model 3, and *Model 6* replicates Model 5 employing the same stepwise procedure, rather than automatically including in each election-specific vote model all the independent variables allowed in the given vote function.

Table 7 shows the fit of the six models of vote choice to the empirical data on the 18 samples in the analysis. The closer the Wilks lambda statistic are to 0, the better VOTE (vote choice) is explained by all the estimated discriminant functions collectively. Since the statistics on model fit are not adjusted to the number of independent variables in the model, adding more independent variables to a discriminant analysis always reduces the Wilks lambda statistics. Thus, the fit of Models 1, 3 and 5 is by definition better or at least the same as that of their stepwise pairs, i.e., Models 2, 4 and 6, respectively. Similarly, since Models 3 and 5 include all the variables included in Model 1, their fit cannot be worse than that of the latter. The fit of Models 4 and 6 can, however, be worse than that of Model 2, since the outcome of stepwise analyses is always path-dependent: the fit of the final model can, occasionally, drop as the initial pool of the possible independent variables is enlarged. This is what we see in the Australian data, for example.

The fit of the linear models tends to be slightly worse than that of its nonlinear alternatives, but the differences are rather trivial (discriminant analysis does not offer a significance test of these differences). The biggest is observed in the Ukraine, but even there the improvement is a modest shift from a fit statistics of .561 in Model 1 to .480 under Model 5, and, among the stepwise solutions, from .713 in Model 2 to .692 under Model 6. The results also show that the explanatory power of the two rather different nonlinear models can barely be distinguished.

The modesty of the payoff on the move to nonlinear models is underlined by Table 8, as well. There, the cross-sample mean of the estimated net change in election outcomes is displayed for all six models and for all three scenarios. When, in Models 3 and 5, all the independent variables allowed in the given models enter the vote function, the estimated information effects—i.e., net change in election outcomes under Scenarios

2 and 3—reach implausibly high magnitudes. The results obtained with the stepwise nonlinear models (Models 4 and 6) predict dramatically less change in election outcomes under both Scenarios 2 and 3, not unlike those estimated with the linear Models 1 and 2. The vast changes predicted under Models 3 and 5 must, therefore, be due mainly to models allowing a large number of statistically insignificant effects to enter the vote function, and hence to pollute the simulated election outcomes with some random noise.

Tables 9 and 10 assess the robustness of the most important parameter estimates of this in the face of changes in model paper, i.e., those directly bearing on the issue of voter inequality in the face of changes in model specification. Table 9 shows that model choice makes absolutely no statistically significant difference in the estimated relationship between the dependence of voting support for a party on the interaction of relative turnout of party supporters with party size and the simulated rise in turnout.

Table 10 shows a somewhat different story about information effects on election outcomes. With stepwise models of vote choice, we find a stronger negative effect of GROUP-INFO on GROUP-CHANGE: i.e., the difference between low- and high-information groups in the space for information induced electoral swing is much bigger when we estimate it with the help of stepwise vote functions. In fact, information-induced voter inequality totally disappears in one of the four results estimated with the nonlinear Models 3 and 5, namely under Scenario 2 and Model 5, where the effect of interest is statistically insignificant, and, in fact, positive. Recall, however, that in Scenario 2 we extrapolate to a practically unobserved information level, and Model 3 yields the implausible estimate that the average difference between observed and fully informed election outcomes may be above 40 percent. In contrast, the results obtained with Model 3, stepwise vote functions and all the estimates obtained under Scenario 3 suggest that knowledge-based voter inequality exists: the effect of GROUP-INFO on GROUP-CHANGE appears to be usually around –.2, just slightly larger than the one discussed in the main text.

Overall, then, the findings regarding turnout- and knowledge-based inequalities seem fairly resistant to changes in the details of model specification: only in one (arguably the least reliable one) of 12 estimates about information effects do we obtain a

statistically insignificant relationship between the variables of interest. In all the rest of the tests, a systematic, though still not very big extent of voter inequality seems to be observable.

TABLE 7

Wilks Lambda Statistics for Six Different Models of Vote Choice to Post-election Data from 18 Countries/Party Systems

Model number	**1**	**2**	**3**	**4**	**5**	**6**
Independent variables included in the model						
INFO	yes	yes	yes	yes	yes	yes
Interactions with INFO and (1-INFO)	yes	yes	yes	yes	yes	yes
$INFO^2$	no	no	yes	yes	no	No
Interactions with $INFO^2$ and $(1-INFO^2)$	no	no	yes	yes	no	No
$(.5-INFO)^2$	no	no	no	no	yes	yes
Interactions with $(.5-INFO)^2$ and $(1-(.5-INFO)^2)$	no	no	no	no	yes	yes
Stepwise entry of variables	no	yes	no	yes	no	yes
Australia 1996	.825	.899	.801	.901	.801	.908
Czech Republic 1996	.566	.674	.537	.672	.533	.677
Germany (West) 1998	.701	.794	.652	.790	.652	.770
Germany (East) 1998	.690	.795	.639	.793	.639	.802
Hungary 1998	.713	.814	.671	.811	.671	.820
Japan 1996	.853	.894	.831	.888	.822	.893
Mexico 1997	.848	.906	.797	.917	.781	.893
The Netherlands 1998	.455	.492	.429	.491	.429	.482
New Zealand 1996	.601	.636	.582	.631	.582	.635
Norway 1997	.553	.595	.536	.595	.534	.593
Poland 1997	.614	.711	.575	.706	.557	.705
Romania 1996	.303	.343	.290	.346	.285	.343
Spain 1996	.746	.821	.711	.821	.703	.824
Taiwan 1996	.697	.796	.675	.784	.666	.784
USA 1996	.773	.819	.756	.802	.755	.806
Ukraine 1998	.561	.713	.489	.710	.480	.692
UK (England & Wales)	.852	.881	.837	.873	.837	.872
UK (Scotland) 1997	.745	.812	.710	.808	.696	.810
Mean value	.672	.744	.640	.741	.635	.739

TABLE 8

Average Value of the Estimated Percentage Change in 18 Election Outcomes Under Different Hypothetical Scenarios of Change in Turnout and Voters' Information Level Using Six Different Vote Functions

Scenario	1	2	3
Presumed turnout Presumed information level	100% INFO remains unchanged	100% INFO = 1 for everyone	100% INFO rises to .65 or remains higher
Model 1	1.3	17.1	4.9
Model 2	1.0	13.4	4.4
Model 3	1.4	39.1	5.4
Model 4	1.0	18.1	4.5
Model 5	1.4	40.5	5.4
Model 6	1.0	16.3	3.1

Notes: Table entries are the mean value of the NATIONAL-SWING variable estimated with six different models of vote choice under three different scenarios and multiplied by 100. For the description of the different scenarios, see the main text; for a description of the NATIONAL-SWING variable, see Appendix B; for a description of the six models, see Appendix D.

TABLE 9

Bivariate OLS-regression of Turnout-induced Swing Across Parties Under Different Models on the Interaction of Relative Turnout of Party Supporters with Party Size and the Simulated Rise in Turnout

Dependent Variable	PARTY-CHANGE (under Scenario 1)					
Model	**1**	**2**	**3**	**4**	**5**	**6**
b	–.069**	–0.58**	–.069**	–.061**	–.067**	–.056**
s.e.	(.009)	(.005	(.014)	(.006)	(.014)	(.005)
Adjusted R-squared	.763	.867	.577	.854	.560	.876

Notes: Table entries are unstandardized regression coefficients showing the total impact of the interaction of RELATIVE-TURNOUT with TURNOUT-RISE and PARTY-SIZE (with standard errors in parentheses) and the adjusted R-squared. Constants are not shown. The units of observations, their weighting and variable coding are the same as for Table 3.

**two-tailed significance <.01
*two-tailed significance <.10

TABLE 10

OLS-regression of the Potential for Information-induced Swing Across Socidemographic Groups (GROUP-SWING) on System-level Swing (NATIONAL-SWING) and the Group Mean of Observed Political Information Level (GOUP-INFO) Under Two Scenarios of Change in the Citizens' Information Level and Six Models of Vote Choice

	Scenario 2		**Scenario 3**	
	INFO = 1 for everyone		INFO reaches at least .65	
	b	***s.e.***	***b***	***s.e.***
Model 1				
GROUP-INFO	–.178**	(.052)	–.436**	(.019)
NATIONAL-SWING	.831**	(.044)	.757**	(.042)
Adjusted R-squared	.209		.375	
Model 2				
GROUP-INFO	–.315**	(.028)	–.308**	(.011)
NATIONAL-SWING	.850**	(.024)	.758**	(.032)
Adjusted R-squared	.500		.499	
Model 3				
GROUP-INFO	–.156*	(.075)	–.473**	(.022)
NATIONAL-SWING	.788**	(.031)	.761**	(.049)
Adjusted R-squared	.320		.335	
Model 4				
GROUP-INFO	–.252**	(.033)	–.286**	(.011)
NATIONAL-SWING	.917**	(.019)	–.817**	(.030)
Adjusted R-squared	.625		.504	
Model 5				
GROUP-INFO	.046	(.077)	–.474**	(.023)
NATIONAL-SWING	.704**	(.026)	.787	(.049)
Adjusted R-squared	.338		.330	
Model 6				
GROUP-INFO	–.219**	(.039)	–.211**	(.010)
NATIONAL-SWING	.984**	(.016)	.955**	(.023)
Adjusted R-squared	.726		.600	

Notes: Table entries are unstandardized regression coefficients (with standard errors in parentheses) and the adjusted R-squared. Constants are not shown. For the variables, see the main text and Appendix B. For the units of observation (*N* = 1,390) and their weighting, see Table 4.

**Two-tailed significance <.01
*Two-tailed significance <.10