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CAN VOTERS BE EQUAL? A CROSS-NATIONAL ANALYSIS* Part 2.

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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. The first part of the paper (published in the previous issue of this journal) explored the assumptions behind the proposition itself and the further assumptions that need to be made in order to test it empirically. The second part of the paper offers an empirical test. 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.

Keywords: electoral behavior, level of information, turnout effects

MODELING INFORMATION EFFECTS

Obviously, not all social groups are equal in their propensity to participate in elections and to be knowledgeable about relevant political facts. The first part of this paper showed what kind of assumptions and data are needed for an analysis of whether this simple fact makes the formal equality of citizens in the electoral arena illusionary. This section shows how one can model empirically the relationship between vote

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choices and political knowledge, so that in the next section we can proceed to an analysis of turnout- and information effects on election outcomes.

Political knowledge may affect vote choice in two ways: in addition to, or in interaction with the impact of other variables. For instance, knowledge of a financial wrongdoing by a party may make everyone who knows about it less likely to vote for the party responsible: this is an additive information effect. But it may also happen that extra information only influences the behavior of some groups but not others, or that additional information moves the vote choices of different people in opposite directions, depending on the dominant tendency of their political predispositions. To capture such interaction effects Bartels' (1996) simulation procedure modeled 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 first part of this paper, 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 political information level (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 on vote choice.

Voters who score .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 respondents are conceived as this kind of split personality in the estimation of constants too: one constant applies for the totally uninformed and one for the fully informed voters. It follows that for any given respondent we can then calculate an appropriate weighted sum of the two constants, yielding a constant specific to his or her information level. Thus, the probability of supporting each party *j* in any given socio-demographic group can be estimated for any given information level. Bartels estimated so-called fully informed votes by substituting INFO = 1 in the estimated vote function, but leaving the parameters and the other variables unchanged.

Note that only one of the two "constants" in Bartels analysis – i.e. the one that applies for respondents with INFO=0 – is a constant in the conventional sense of the word. What Bartels calls the constant for the fully informed voter is added to the first constant for every respondent *i* in proportion to his or her score on the INFO variable. That is to say, the second constant is nothing else but the additive effect of information on vote choice. Thus, while the socio-demographic variables only enter the equation through the interaction terms with INFO and (1-INFO), the INFO variable also enters the equation as a separate independent variable.

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 effect of FEMALE*(1-INFO) on the probability of supporting Party A is zero, while the impact of FEMALE*INFO is a positive number. 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.[1]

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 registered the biggest effects on political information level (cf. *Table 1* of Tóka 2002). 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 a particularly big – or particularly stable, or at least inevitable, etc. – 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). As long as this assumption holds, Bartels' model provides valid estimates of the impact of knowledge on aggregate vote distributions in socio-demographic groups, no matter whether the "sociological model of the vote" is valid or not.

In Bartels's (1996) analysis, the design of the interaction terms between INFO and the sociodemographic variables also 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. Yet it seems 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, while follow some totally different curves in the case of other parties. The exact shape of the relationship

1 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.

between information level and vote may vary widely depending on which party, which election and which demographic variable we are talking about.

Unfortunately, the number of nonlinear specifications that one can think of is simply too large to explore them all.[2] In a previous report on the current analysis, *Appendix D* of Tóka (2002) presented 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 were reassuringly similar across the six models, and therefore the present 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.[3] Like all other variables in the analysis, VOTE is described in the *Appendix.* The discriminant analyses were run separately for 18 samples extracted from the CSES data set. On case selection and weighting procedures, the reader is referred to part one (see *Appendix A* of Tóka 2003). 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. 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 the first part of this article (see *Table 1* of Tóka 2003 in the previous issue of this journal).

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 can be estimated for every respondent in the sample. 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.

2 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 (the logistic curve) that is just as rigid as in the linear formulation – it is just better suited for the analysis of dichotomous dependent variables. Taking the possibility of non-linear information effects seriously would require a far more radical specification, which could accommodate the infinite variety of nonmonotonous patterns alluded to above.

3 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.

Scenario 1 models the argument that given the sociodemographic differences between voters and nonvoters, election outcomes would differ if all citizens voted. The real question here is only how big the difference would be. The model takes notice that nonvoters tend to be less knowledgeable than voters with similar sociodemographic characteristics. Given this, 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. Therefore, Scenario 1 only assumes that turnout rises to 100 percent, but keeps the information level of the erstwhile nonvoters unchanged.

In contrast, Scenario 2 envisions that all vote and that they all become "fully" informed too, i.e. the true score on the INFO variable is one for everyone in the sample. 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, 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 distinguished in the present analysis.

The vote shares of each party under the different scenarios were estimated through the mean conditional probability of voting support in the sample for the given party under the given scenario. These probabilities were conditional on (1) the sociodemographic characteristics of the respondents; (2) the multiplicative relationships between vote choice, sociodemographic variables and information level, as estimated from the data with the given model; (3) the given scenario's assumptions about respondents' information level; and (4) each party'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 that 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, under a particular scenario, among all citizens *(*see *Table 1),* while the third (GROUP-SWING) shows the same for up to 90 sociodemographic groups within each country (on the definition of these groups see the *Appendix*). They were both calculated as half the sum of the absolute change in the mean conditional probability of voting support for each party *j* in the given population or group: i.e. the

absolute values of the PARTY-CHANGE variable were summed up across all the parties in the given country, and then the sum was divided by two. The difference between NATIONAL-SWING and GROUP-SWING is merely that for the first, this calculus was carried out at the level of individual countries, while for the second, the same calculus was done separately for each 90 groups in each country. Of course, there are as many pairs of NATIONAL-SWING and GROUP-SWING variables as many scenarios of change in election outcomes in the analysis.

Table 1. Estimated percentage change in election outcome under different hypothetical scenarios of change in turnout and voters' information level

Scenario	1	2	3
Presumed turnout	100%	100%	100%
Presumed information level	INFO remains unchanged	INFO = 1 for everyone	INFO rises to .65 or remains higher
Australia 1996	0.0	21.1	5.3
Czech Republic 1996	0.9	16.5	3.6
England & Wales 1997	1.1	16.8	2.5
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
Scotland 1997	1.1	9.2	2.0
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

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 the *Appendix*.

FINDINGS

As explained in part one of this paper, the estimates about individual elections and parties are polluted with some random measurement errors of unknown size. Thus, only the general trends are worth looking at. To begin with, *Table 1* 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. 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.[4] 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, but still closer to that than to the stunning swings in Scenario 2.

While a one, two or even three percent change in election outcomes is relatively modest, such changes could, of course alter the outcome of real-world elections quite frequently. Moreover, turnout effects may be underestimated in *Table 1* for the simple reason that far more respondents report to have voted than as many 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 is likely to 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 degree of this overlap 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 the first part of this paper (see *Table 1* of Tóka 2003) on the other. With $N = 18$, both variables registered a statistically significant impact 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). A visual inspection of partial plots suggested that both effects were linear. The parameter estimates implied that every 10-percentage-point change in electoral participation causes a 0.54 percentage point 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 1* 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.

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

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. The chance is slim, since 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.

The size of this effect 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 the *Appendix*). 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* of Tóka 2002). Compare this with the –.02 value of the agrarian-populist FKGP (see the same source) in the same election. These figures suggest that in this election, turnout was 7–(–2) = 9 percentage point 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.[5] In the first column of *Table 2,* we see that the bivariate 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 percentage point 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 a 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 larger 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

5 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 *Table 2*, 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.

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 2.*

Table 2. 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 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	–	–	–	–.070** (.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. But the explained variance is no reliable guide to model choice in this case. We know 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* of Tóka 2002). 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 percentage point 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. 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 votes in the sample. 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 times (–.10)*(.021)*(11.3) = .0016611 larger fraction, or 0.17 percentage point more of the vote if turnout had been 100 percent. As the reader will recall, the –.070 value is the relevant parameter estimate from the last column of *Table 2.*

Hence it seems 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, in particular elections, of people with similar sociodemographic make-up but different political preferences. 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, except 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 2* 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. The importance of these figures is not what they say about the 1996 election. Rather, the interesting implication is that in American presidential elections the median voter is considerably less left-wing than she would be if turnout were 100 percent. Since the parties would presumably adjust to such a change in the composition of the electorate, the result may not be less frequent Republican control of the presidency. However, most probably both Democratic and Republican presidents and legislators would need to be more left-wing to be electable under full turnout than they are now. However, the present results also suggest that in those countries where turnout is more like 80 than 50 percent, the direction of public policies are relatively little affected by less than full turnout.

Are knowledge-based voter inequalities larger? Indeed, are they systematic at all? *Table 1* 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 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, was 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 the variables described in the *Appendix*. 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 on information level were taken into account: i.e. 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 3*.

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 3* is markedly lower under Scenario 2 (when everyone's information level rises) than under Scenario 3.

The findings of *Table 3* 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 more faithful to the underlying preferences 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 percentage point 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 a half times bigger yet.

A cursory reinvestigation of the data presented in the first part of this article (see *Table 1* in Tóka 2003) 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 belong to them. Judged from this perspective, that 3 percent gap may even

look fairly small: but the two and a half times 3 percent gap – i.e. the respective figure under Scenario 2 – cannot be so easily dismissed as irrelevant. At any rate, it seems that the revealed preferences of the median voter, and hence probably also the direction of public policies would be appreciably different if voters were fully informed. Whatever information shortcuts assist the citizens of contemporary democracies in making sensible electoral choices without turning omniscient, they are apparently not perfect.

Table 3. 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 INFO = 1 for everyone *b* *s.e.*	**Scenario 3** INFO reaches .65 or remains unchanged *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 the *Appendix.* 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

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 types of 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 2,* the Pearson correlation between PARTY-CHANGE under Scenarios 2 and 3 is an impressive .82, significant well below the .001 level. The correlation between the information- and the turnout-induced changes is, however, indistinguishable from zero. The pairwise correlations between PARTY-CHANGE under Scenario 1 and PARTY-CHANGE under Scenarios 2 and 3 are .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. In other words, nonvoters would not use their vote in a less reasonable way than voters do. The present analysis should at least raise doubts about the popular belief to the opposite.

IMPLICATIONS

In recent years, the problem of voter inequality attracted considerable attention among political theorists.[6] Some proposed radical measures to combat it,[7] while others pointed at a wide range of potential victims.[8] 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.[9] 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,[10] or at least to make very good use of the little information they have.[11]

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. On the other hand, 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 in most elections is still much closer to its possible maximum (100 percent) than to its possible minimum (zero percent). 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 2* – 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.

6 Cf. Offe (1997); Simpson (1997).

7 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.

8 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.

9 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.

10 Cf. especially Lau and Redlawsk (1997); Lupia (1994); McKelvey and Ordeshook (1986, 1990).

11 Cf. Popkin (1991); Sniderman, Glaser and Griffin (1990).

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. The frequency of such breakdowns can be assessed by replicating *Table 3* 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). In other words, in more than a third of the contexts covered the generally better informed groups were a bit more likely to vote in an uninformed way than the generally less knowledgeable groups. 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 situational effects of information on election outcomes may often reverse the usual relationship between group information level and the probability of voting as if one were fully informed.

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 political influence temporarily induced by situational effects. While these 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 votes. 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 does not seem to be a perfectly neutral arena for aggregating political preferences in an electorate characterized by unequal turnout and knowledge. Picking our legislators and governments by a lottery would certainly eliminate this bias over the long term. However, it would probably produce results far from the preference of the median citizen much more often than elections do. In fact, it is hard to imagine a device of aggregating preferences that would produce a lesser bias than the present findings identified in the case of elections: 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.[12]

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 95 percent confidence interval of 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.[13] 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.[14]

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?

12 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.

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

14 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: VARIABLES AND CODING

Variables in 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.

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

Variables used in constructing the socio-demographic groups that are the units of analysis in Table 2

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 1, 2 and 3

GROUP-INFO: the mean value of variable INFO in the 90 demographic groups, which were defined with the help of the variables listed above.

GROUP-VOTING: the mean value of variable VOTING in the 90 demographic groups.

GROUP-SWING: the equivalent of NATIONAL-SWING on the level of the 90 demographic groups. 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 socio-demographic 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.