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Item Type	Journal article
Authors	Tóka, Gábor
DOI	<a href="https://doi.org/10.1016/j.electstud.2007.11.006">10.1016/j.electstud.2007.11.006</a>
Publisher	Elsevier
Download date	2024-10-03 13:18:32
Link to Item	<a href="http://hdl.handle.net/20.500.14018/9481">http://hdl.handle.net/20.500.14018/9481</a>

## **Citizen Information, Election Outcomes and Good Governance**

Published in *Electoral Studies* 27 (1): 31-44.

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### **Abstract:**

This paper provides a new empirical test of the common sense proposition that a better informed electorate helps producing greater collective welfare. The innovation lies in an arguably more adequate measurement of both the independent and the dependent variable than those found in previous studies. The data come from the cross-national post-election surveys of the Comparative Study of Electoral Systems (CSES) project plus World Bank data on the quality of governance across the globe. The findings show some significant effects of citizens' ability to emulate fully informed choices on the quality of governance after the elections in question. However, the effect only materializes over multiple elections, and may not extend to all aspects of good governance.

*Keywords:* elections; political knowledge; information shortcuts; vote choice; governance; corruption

## **1. Introduction**

This paper intends to provide a new empirical test of the proposition that elections enhance collective welfare to the degree that citizens vote as if they were fully informed. In other words, it asks if we can apply to large human collectives the assumption that ‘a person’s interest or good is whatever that person would choose with fullest attainable understanding of the experiences resulting from that choice and its most relevant alternatives’ (Dahl, 1989, 180). The next section seeks a justification for the hypothesis in terms of theory and previous findings. Section three explains how the present empirical test offers a relevant improvement over previous attempts, and introduces new data and measures in the examination of the problem. Section four presents the empirical analysis, and section five concludes.

## **2. A plausible, important, but insufficiently tested proposition**

It is often claimed that elections only enhance collective welfare to the extent that citizens are knowledgeable about politics (cf. Gastil, 2000). This proposition seems to be underlined by two of the probably most frequent assumptions in political science. The first is that politics is not a zero-sum game (Laver, 1981), and the second that highly informed actors are usually more effective than know-nothings in obtaining the outcomes that best conform to their preferences (see e.g. Delli Carpini and Keeter, 1996, 56; Downs, 1957, 258; Hutchings, 2003). Indeed, the two assumptions together seem to form the most common justification for the existence of political science itself, namely that a better understanding of politics advances the common good. They also provide an influential

argument about the superiority of decisions made by representative assemblies over direct democracy (Berelson, 1952; Schumpeter, 1942). Thus, important bits of the conventional wisdom about democratic politics may be mistaken if an informed citizenry turned out to be unnecessary for collective welfare.

That it is necessary is not just an important but also a highly plausible proposition. Deininger and Mpuga (2005) provide evidence that a knowledgeable citizenry can and does act as effective guardians of lack of corruption among office holders. Yet, the relationship that they find can exist even in the absence of elections, as a result of other exchanges. What is of interest here is thus rather the model proposed by Adserà *et al.* (2003), who link citizen knowledge to collective welfare through an electoral connection. In their principal-agent frame citizens delegate considerable powers to politicians to make decisions on their behalf. Politicians, in their turn, earn their income partly by acting to satisfy their constituents, and partly by extracting private benefits from holding office, such as illegally enriching themselves or implementing their own favoured policies even if those deviate from the preferences of the constituents. Yet they also have a long time horizon, and rather sacrifice some of the mentioned private benefits than risk losing the next election. The remaining inefficiencies in the aggregation of citizens' preferences by the political process – like some residual corruption among officials or bureaucratic inefficiencies – are a stealth tax that people pay for delegating collective decisions to someone else. The Adserà *et al.* (2003) model shows that these inefficiencies are partly a function of the information asymmetry between representatives and the represented. Thus, greater citizen knowledge enhances collective welfare.

Under appropriate democratic institutions the model holds for any political outcome, not just corruption. Under (seemingly) perfect citizen knowledge, elections can

conceivably be decided by small performance differences between the competing politicians in reducing such inefficiencies in the aggregation of citizens' preferences by the political process as red tape, corruption, poor regulations, arbitrary violations of the rule of law, limitations on feedback from citizens, and so forth. Big performance differences may then bring about devastating losses at the polls. Hence, the Adserà *et al.* (2003) model implies that the electoral incentives for politicians to improve governance – and hence promote a diffuse feelgood factor among citizens in reflection of reduced costs of good government – increase as citizens' voting behaviour approximates their fully informed behaviour.

That good government is a function of citizen knowledge would, of course, be an inconsequential proposition if citizens behaved as if they were perfectly informed in every election. Indeed, rational ignorance among citizens can be consistent with collective outcomes reflecting what Downs (1957, 246) called the voter's 'true views' – i.e. 'the views he would have if he thought that his vote decided the outcome'. As in the Condorcet jury theorem, errors of judgments committed by individual voters may cancel out each other in the aggregate (see e.g. Miller, 1986; Page and Shapiro, 1992; Austen-Smith and Banks, 1996). Moreover, political entrepreneurs, interest groups and news media readily underwrite the costs of political information gathering and dispersion among citizens, and the cues that they provide may suffice to produce seemingly informed voting behaviour even among cognitive misers (Becker, 1985; Lupia, 1994; Popkin, 1991; Wittman, 1989). However, a number of recent works employing a wide range of methods and data suggest that the information shortcuts provided by election campaigns and the aggregation mechanisms of vote counting may still fail in helping to emerge the outcome that an

informed citizenry would produce (Althaus, 2003; Bartels, 1996; Lau and Redlawsk, 1997, 2001; Martinelli, 2006; Sekhon, 2004).

Hence the proposition examined in this paper is not that citizens' level of lexical knowledge makes a difference in collective welfare, but that their ability to emulate fully informed voting behaviour does so. This distinction is also important in assuring that the causal link between citizen knowledge and welfare that this article looks at really operates through elections, rather than some other forms of citizen pressure on officials. Moreover, this way of formulating the proposition allows us to make cross-nationally comparative statements about variance on the key independent variable. Were we to study the impact of lexical knowledge instead, it may well be impossible to make such statements given that the same knowledge (say the name of the finance minister or the size of China's military expenditure) may have different practical relevance in different contexts.

Before proceeding to develop a test of the proposition, it is worth highlighting what is new about it. The proposition certainly involves a rather long causal chain. Many elements of this chain have been examined before, but surprisingly few previous studies attempted to test the relationship between the two endpoints head on.

A growing number of works, based on deliberative polls and other panel surveys as well simulations using cross-sectional data, suggest that political attitudes and vote intentions often change as people become more knowledgeable (Althaus, 1998, 2001, 2003; Alvarez, 1997; Andersen *et al.*, 2005; Barabas, 2004; Bartels, 1996; Delli Carpini and Keeter, 1996, 238ff; Fishkin, 1997, 214-228; Lupia, 1994; Luskin *et al.*, 2002; Sekhon, 2004; Sturgis, 2003). The same studies also make it clear that such changes often lead to sizeable shifts in the aggregate distribution of expressed preferences (but cf. Battaglini *et al.* 2007). Other studies add that better informed citizens are more likely than information

underdogs to anchor their vote choices in their own issue preferences, ideological orientation and performance evaluations (Andersen *et al.*, 2005; Bartle, 2005; Delli Carpini and Keeter, 1996, 256-8; Gomez and Wilson, 2001; Goren, 1997; Hobolt, 2004; Jacoby, 2006; Lau and Redlawsk, 2001; Lupia, 1994; Luskin, 2003; Sniderman *et al.*, 1990; Sturgis and Tilley, 2004; but cf. Zaller, 2004). Evidence from deliberative polls further demonstrates that cycles in collective preferences become less frequent as citizens become more knowledgeable (see Farrar *et al.*, 2006; List *et al.*, 2006). All this provide indirect evidence that as citizens' political knowledge increases, vote choices often – though probably not always – become increasingly more accurate expressions of the policy preferences that people would hold if they were fully informed, and that informed preferences may be more likely to reflect the 'true views' of citizens than their uninformed preferences. The same points are borne out by some formal models and experimental results (McKelvey and Ordeshook, 1985, 1986; Lupia, 1992; Lau and Redlawsk, 2001).

There is also considerable evidence that the popular desires impacting election outcomes influence not only who gets elected but also the policies adopted (Bartels, 1991; Canes-Wrone 2006; Cohen 1997, Erikson *et al.*, 1993; Erikson *et al.* 2002; Griffin and Newman 2005; Hersch *et al* 2004; Manza and Cook 2002; Monroe 1983, 1998; Stimson 1998; but cf. Brooks 1985, 1987, 1990; Jacobs and Shapiro 2000). Of course, nearly all empirical evidence come from the US, and social choice theory raises doubts about the extent to which collective outcomes can regularly match the position of the median voter along multiple dimensions (Riker, 1982). Some empirical studies nonetheless suggest that democratic elections may, for at least some of the time, do fairly well in reflecting the position of the median voter in the composition of the executive (Enelow and Hinich, 1984; Powell, 2000; McDonald *et al.*, 2004).

Yet all these studies combined still fail to provide a comprehensive test of the links between seemingly fully informed voting behaviour and collective welfare. Hence, it seems a timely task to test the link between citizen knowledge and collective welfare directly. The extant literature has not got very far in this. Experimental studies typically stop at pointing out the role of specific information or general knowledge in allowing citizens to vote as if they were fully informed (Lupia, 1992; Lau and Redlawsk, 2001). Delli Carpini and Keeter (1996) use a wealth of cross-sectional survey data, but also stop after demonstrating that more knowledgeable citizens show greater attitude constraint and arguably less prejudiced opinions in selected attitude domains. Norris (2004) shows significant country-level correlations between measures of good governance on the one hand, and press freedom and media penetration on the other – but clearly such a result is open to all sorts of alternative explanations in terms of common causes that would need to be controlled for in a causal analysis.

The study with the best claim to date to establish empirically the link between enlightened citizens and good government chose an indirect indicator of citizen knowledge. Adserà *et al.* (2003) demonstrate strong, robust and positive links between some indicators of government quality on the one hand, and newspaper readership on the other.<sup>1</sup> The basic finding of their seminal study holds in multiple data sets and in spite of extensive controls for alternative explanations and careful tests for the direction of causality. In conclusion, Adserà *et al.* (2003, 479) suggest that ‘the presence of a well-informed electorate in a

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<sup>1</sup> In their cross-national analyses they actually replaced newspaper circulation data with its interaction with level of democracy, but this has no apparent relevance for the present discussion and is thus ignored throughout this text.



democratic setting explains between one-half and two-thirds of the variance in the levels of government performance and corruption.’ The present study aims to examine this bold claim with better indicators of the key variables.

The first motivation for the proposed changes in measurement is that newspaper readership may be a better proxy for the resources available to independent mass media than for fully informed voting among citizens. Even if newspaper readership were related to citizen knowledge, the latter can impact corruption and the quality of government services without voting behaviour entering the causal chain as an intervening variable (Deininger and Paul 2005). Moreover, the credible threat of widely publicized critical reports appearing in the press may in itself motivate good behaviour among politicians and bureaucrats even where electoral sanctions of bad behaviour are unlikely. This caveat becomes particularly relevant when we consider what measures of government quality provide the key finding of Adserà *et al.* (2003). In the first data set analysed by them – a short time-series covering the 1980-1995 period for over 100 countries –, only two of the four measures of government quality show the expected significant positive relationship with newspaper circulation in democracies once the lagged value of the dependent variable is controlled for. These are ‘lack of corruption’ and ‘bureaucratic quality’. In contrast, the effect of newspaper circulation on the ‘rule of law’ is far from statistically significant, while the effect on ‘freedom from the risk of expropriation of property’ is negative and highly significant, thus raising doubts about the beneficial impact of the independent variable on governance.

The second data set analysed by Adserà *et al* covers, depending on the indicator, 155-173 countries at a single time-point around 1997-1998. With a few controls at place, the impact of free newspaper circulation on the ‘rule of law’ becomes insignificant, but the

positive effect on ‘lack of corruption’ remains highly, and on ‘bureaucratic quality’ marginally significant. Finally, in their third data set, the only dependent variable is the number of officials actually indicted for corruption in 48 US states over time, and this data again confirms a robust effect of newspaper readership in spite of extensive controls for alternative explanations, including lagged values of the dependent variable.

In a nutshell, the evidence supporting the above cited conclusion rests disproportionately on the behaviour of variables related to corruption, and to a lesser extent on findings regarding bureaucratic quality – which seem conceptually quite close to each other. Adserà *et al.* do explain why their theory may apply less for other legitimate indicators of government quality. First, the rule of law variable incorporates perceptions of compliance among citizens, which is not easily influenced by government. Second, ‘the quality of electoral and informational controls are even less relevant to determine the kind of policies governments may pursue towards redistribution and private property – the latter will depend on the preferences and demands made by the public or the governing elite’ (Adserà *et al.*, 2003, 459). Hence, they imply, high newspaper readership may well promote more frequent expropriation of private property by government.

These explanations are not fully convincing though. A troubling point regarding the first is that a similar argument could just as well be advanced with respect to bureaucratic quality – surely governments can influence some aspects of that variable on the long run only. Yet for bureaucratic quality the data supports Adserà *et al.*’s theory. In contrast, for the rule of law variable the hypothesis is rejected in their second analysis too, where judgments about ordinary citizens’ behaviour play at most a marginal role in measuring the rule of law (cf. Kaufmann *et al.*, 1999, Appendix A). Adserà *et al.* might be right that a well-informed public would demand the expropriation of private property, but the opposite

expectation would seem at least equally plausible given how economic freedom relates to economic growth in at least some empirical studies (cf. Bhila, 1997; Stroup 2007). If newspaper circulation is nonetheless linked to expropriation, then one could imagine as intervening variable the presence of a strong radical left promoting readership among militants, rather than fully informed voting behaviour. Thus, it would add further weight to the evidence regarding the electoral link between citizen knowledge and collective welfare if improved measures of an informed electorate and a wider set of indicators for government quality could be involved in the test. The next section explains how the present study seeks to achieve this.

### 3. Models and measures

The aim of the empirical analysis is to estimate two regression models, the first of which states that:

$$Governance_t = a + b_1 Governance_{t-1} + b_2 InformationEffect_{t-1} + \sum_{j=1}^k b_j C_j + \varepsilon \quad (1)$$

For simplicity, all equations in this paper omit indexing for contexts  $i$ , which will be 70 individual elections in estimating Equation (1), and 35 pairs of elections for Equation (2) described below.  $Governance_t$  and  $Governance_{t-1}$  are various indicators of government quality at the time of two successive elections in the same country.  $InformationEffect_{t-1}$  is the estimated difference between fully informed and observed election outcomes in the earliest of the two elections. Various control variables denoted as  $C_j$  enter Equation (1) to assure that the  $b_2$  estimate about the impact of  $InformationEffect_{t-1}$  on  $Governance_t$  is not biased. Coefficients  $b_1, b_2 \dots b_k$  and constant  $a$  are to be estimated with an ordinary least

squares equation, and  $\varepsilon$  is simply a residual error term with a zero mean that is assumed to have a normal distribution.

The reason for developing a second model is twofold. First, given the inertia of government machineries, it may be unrealistic to expect noticeable changes in governance quality within a single electoral cycle. Second, and quite independently of this, it may not have much lasting effect on politicians if a single election produced an unusually well-informed – or unusually poorly informed – result. It is always disputable – and indeed disputed – if and to what extent one election may have gone differently if citizens were better informed about something. Politicians cannot directly observe such information effects on the vote. What they may have instead is some indirect and largely intuitive knowledge of generally how far voting behaviour may be subject to information effects in their country. This expectation may make them more or less diligent in following the preferences of their constituents rather than seeking other routes of getting re-elected. But it is unlikely that politicians dramatically revise their intuitive knowledge of citizen behaviour and their responses to it merely on the basis of difficult-to-observe and easy-to-dispute information effects in a single election.

Hence, one may want to examine not only single electoral cycles, but also how multiplicative information effects over several successive elections influence the quality of government. Thus, the second part of my analysis will estimate equations that take the following general form:

$$\begin{aligned}
 \text{Governance}_t = & a + b_1 \text{Governance}_{t-n} + b_2 (3 + \text{InformationEffect}_{t-m}) \times \\
 & (3 + \text{InformationEffect}_{t-n}) + \sum_{j=1}^k b_j C_j + \varepsilon
 \end{aligned} \tag{2}$$

where  $m < n$ , and  $b_2(3 + InformationEffect_{t-m}) \times (3 + InformationEffect_{t-n})$  is the estimated effect of several elections on the quality of governance through multiplicative information effects. The addition of the constant 3 appears in this formula merely to avoid multiplying negative numbers.<sup>2</sup>

The control variables that enter the analysis are possible determinants of the size of information effects on election outcomes. Following Lau and Redlawsk (1997), they include the *Ideological Polarization of the Political Parties* and the *Effective Number of Parties* in the election, both calculated from the CSES data.<sup>3</sup> The first is expected to reduce, and the latter to increase information effects. The final control variable is a dichotomous variable called *New Democracy*, inspired by Sekhon's (2004) finding that established

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<sup>2</sup> *InformationEffect<sub>t-m</sub>* and *InformationEffect<sub>t-n</sub>* will be constructed as factor scores and thus their lowest values fall between zero and minus 3.

<sup>3</sup> The *Effective Number of Parties* was calculated from the distribution of recalled votes in the last election as reported by the respondents in the CSES surveys. Where several votes were reported – e.g. both for president and parliament –, the one that entered the calculation of information effects was used to calculate the effective number of parties too (see the Appendix). The *Ideological Polarization* of the parties was determined by calculating the mean left-right self-placement – in Japan, the mean placement on an equivalent progressive-conservative scale – of the parties by the total national sample, and calculating the standard deviation of these party positions separately for each of the 70 elections.

democracies probably benefit from smaller information effects.<sup>4</sup> Since these characteristics of the political system may create spurious correlations between information effects and governance indicators, their impact on the latter seemed worth to control for.

The indicators for the *Governance* variables come from Kaufmann *et al.* (2007), and cover aspects of good governance that – unlike freedom from expropriation of property – are relatively neutral with respect to substantive policy content. Unlike the 1999 release of the same data analysed by Adserà and associates, the 2007 update provides six aggregate measures of governance for over 200 countries and territories of the globe for all but three individual years between 1996 and 2005. For 1997, 1999, and 2001, I replaced the missing data with the average of the previous and the subsequent year for the same country. The six measures tap *Voice and Accountability*, *Political Stability*, *Government Effectiveness*, *Regulatory Quality*, *Rule of Law*, and *Corruption Control*. Of course, it is not inconceivable that some people prefer more rather than less corruption, and desire the absence rather than the presence of accountability, rule of law, political stability, effective government and sensible regulations. But it is unlikely that an informed public in a democratic society (and

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<sup>4</sup> The following countries and territories were classified as new democracies or imperfect democracies and coded 1 on the *New Democracy* variable: Brazil, Bulgaria, Belarus, Taiwan, the Czech Republic, Hong Kong, Hungary, South Korea, Mexico, Peru, the Philippines, Poland, Romania, Russia, and Ukraine. The remaining countries included Australia, Belgium, Canada, Denmark, Finland, France, Germany, Iceland, Ireland, Israel, Japan, the Netherland, New Zealand, Norway, Portugal, Spain, Sweden, Switzerland, the United Kingdom and the United States, and they were all classified as consolidated democracies and coded zero.

this analysis will only look at polities with more or less competitive elections) would not appreciate the value of what the six indicators measure. They all refer to public goods that must facilitate the effective allocation of public assets towards the implementation of whatever public policies were decided by the respective authorities, thus minimizing the stealth tax paid by citizens – in varied forms from opportunity loss to unnecessary taxes, from bribes to unusual effort or risk that it takes to voice political opinions – for obtaining whatever is delivered by government. While these six dimensions of performance – with the obvious exception of corruption control – are rarely issues in elections, they all should contribute in their way to the creation of the same feelgood factor that peace, prosperity and other supposedly key factors of incumbent success promote. They are more clearly linked to acts of government than economic growth or unemployment, and their links to a diffuse feelgood factor among citizens may be as clear as the impact of the latter.

Note that Kaufmann *et al.* (2007) constructed these indicators with an unobserved component model from hundreds of variables provided in 33 data sources by 30 different organizations. The data are mostly perceptual but fare well in the light of extensive tests for reliability and correlated errors (see Kaufmann *et al.* 2007 and previous works cited therein). Thus, they indeed are a nearly exhaustive aggregation of the currently available information on governance quality around the globe. Since the six variables obtained are highly correlated with each other, both the individual indicators and the scores of the individual cases on the single common factor defined by them will be considered in the present analysis.

The dependent variables in the analysis (*Governance<sub>t</sub>* in the equation above) stand for the resulting level of government quality at the end of a particular electoral cycle, i.e. in

the last 12 months before a national election.<sup>5</sup> The starting level of government quality ( $Governance_{t-1}$  in the equation above) is measured as the average of the 12 months prior to the election opening the given cycle. It enters the analysis to control for the possibility that  $InformationEffect_{t-1}$  and the resulting level of governance at time  $t$  are only correlated because they are both dependent on the starting level of governance. Monthly values of the governance indicators are calculated by assuming that governance quality remains constant across the 12 months of each year. Both the starting and the resulting levels were calculated for each of the six governance indicators – *Voice and Accountability*, *Political Stability*, *Government Effectiveness*, *Regulatory Quality*, *Rule of Law*, and *Corruption Control* – as well as for a summary measure, which is the single common principal component formed by the six variables for the starting and the resulting level, respectively.

The key independent variable in the analysis is  $InformationEffect_{t-1}$  and refers to the relative difference between actual election results and those that would obtain if all citizens were fully informed. The smaller the information effect on election outcomes, the closer citizens' collective behaviour is to perfectly emulating fully informed voting behaviour.

Estimates of fully informed vote distributions can be derived with the simulation procedure developed by Bartels (1996) from any election survey that contains measures of vote choice, appropriate control variables for the shared determinants of vote choice and political knowledge, and good measures of political knowledge. The unit of analysis in the simulation procedure are individual respondents  $i$ . The data in the present analysis come

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<sup>5</sup> Some electoral cycles covered by the CSES data (will) only end after 2006, and for those cases the governance indicators for 2006 were taken as estimates of  $Governance_t$ .



from post-election surveys carried out after 70 elections in 37 countries on five continents, mostly in advanced post-industrial democracies, in the framework of the Comparative Study of Electoral Systems project between 1996 and 2006 (see CSES 2003, 2007).<sup>6</sup>

The simulation models estimate the multinomial dependent variable *Vote* (vote choice in the last election) as a function of a constant  $a$ , the *Knowledge* variable that runs from 0 to 1, and the interactions of the latter with various exogenous determinants of political preferences – i.e. sex, age, income and so forth, indicated as the matrix of  $X_j$  independent variables in Equation (3) below:

$$Vote = fn \left( \begin{array}{l} a + b_1 X_1 Knowledge + b_2 X_2 Knowledge + \dots + b_k X_k Knowledge + \\ + b_{k+1} X_1 (1 - Knowledge) + b_{k+2} X_2 (1 - Knowledge) + \dots + b_{2k} X_k (1 - Knowledge) + \\ + b_{2k+1} X_1 Knowledge^2 + b_{2k+2} X_2 Knowledge^2 + \dots + b_{3k} X_k Knowledge^2 + \\ + b_{3k+1} X_1 (1 - Knowledge^2) + b_{3k+2} X_2 (1 - Knowledge^2) + \dots + b_{4k} X_k (1 - Knowledge^2) + \\ + b_{4k+1} Knowledge + b_{4k+2} Knowledge^2 \end{array} \right) \quad (3)$$

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<sup>6</sup> Some of the elections covered by the CSES studies had to be excluded from the analysis because of missing variables. These included Belgium 2003; Chile 1999; Kyrgyzstan 2005; Lithuania 1997; Russia 2000; Slovenia 1996; Thailand 2001; United States 1996.

On the  $X$  variables and *Knowledge* see the Appendix.<sup>7</sup> Here the *fn* linking function is provided by discriminant analysis. The point of this model is that if the  $X$  variables included all shared determinants of political knowledge and vote choice, then the statistical models following Equation (3) can be used to derive estimates of the direction and size of the *net aggregate* change that would occur in the distribution of political preferences in the

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<sup>7</sup> The missing values on these  $X$  variables were mean-substituted because of the need to base the inferences on the entire voter sample and because multiple imputation would have been impractical given the very large number of equations in the analysis. The sample design or demographic weights provided with the CSES data sets were used. Non-voters were excluded from the entire analysis so as to conflate the impact of any turnout change with the direct impact of knowledge change on vote distributions. For concurrent elections of two different houses of parliament or legislature and president, the vote choice variable measured vote in whichever of these elections is more important for government formation in the given country: e.g. presidential vote choice in the US, but party list vote in the lower house elections in Romania. Parties and presidential candidates with less than 30 (unweighted) voters in the data set were collapsed into a single ‘other candidates’ category. If the frequency of this other category still remained below 30, then these respondents were excluded from the analysis. Countries that show great regional variations in electoral alignments and provided sufficiently big subsamples for specific regions of interest were split in two – i.e. Belgium into Flanders and Wallonia, with Brussels included in the latter; Canada into Quebec and the rest of the country; Germany into East and West; and The UK into England and Wales on the one hand, and Scotland on the other. The ultimate values of the *InformationEffect* variables for these countries are nonetheless national aggregates.

population if the knowledge level of everyone increased to the maximum value on the *Knowledge* variable (Bartels 1996; Tóka 2003). That this conventional causal interpretation of regression models is valid in the given application has been confirmed by Gilens (2001) and Sturgis (2003), who found convergence between the result of Bartels-type simulations and the actual changes of political preferences that were observed in experiments where the subjects gained knowledge. The total information effect on an election result can thus be estimated as

$$InformationEffect = \frac{1}{2} \sum_{i=1}^m \hat{\Pr}(V_i | Knowledge) - \hat{\Pr}(V_i | Knowledge^*) \quad (4)$$

where  $\hat{\Pr}(V_i | Knowledge)$  is the expected probability of a vote for the  $i$ -th of  $m$  parties given the observed distribution of *Knowledge* – this probability is of course equal to Party $_i$ 's observed share of the recalled votes in the sample –, and  $\hat{\Pr}(V_i | Knowledge^*)$  is the same expected probability under full information in the sample.

To increase the robustness of the results, four slightly different vote functions were estimated in the present study. Two of the four models forced the simultaneous entry of all predictor variables in the estimated vote choice model, whether or not they had a significant effect; and the other two relied on a stepwise entry of predictors. One in both of these model pairs allowed only linear information effects – i.e. used a measure of political knowledge (see below), the socio-demographic variables listed below, and the simple interactions between knowledge and each socio-demographic variable – as predictors of vote choice. The remaining two models – one with stepwise and one with forced entry of all independent variables – allowed for non-linear information effects as Equation (3), while the other two only allowed for linear interactions between *Knowledge* and the  $X$

variables, i.e. set zero all parameters from  $b_{2k+1}$  to  $b_{4k}$ . The four models result in somewhat different but strongly correlated estimates of information effects for each of the 70 elections covered in the analysis, and yield a single common factor in a principal component analysis. The factor scores of the cases serve as the *InformationEffect* variable in the empirical analyses reported in the tables.

Tables 1 and 2 show descriptive statistics about the variables in the first and second part of the analysis, respectively. As it will be recalled from the discussion of Equations (1) and (2) above, the units of analysis are individual elections in the first part of the analysis but pairs of elections held in the same country in the second. There are total of 25 countries for which information effects can be estimated from the CSES data for more than one election in the 1996-2006 period, and they yield a total of 35 pairs for the second part of my analysis. For Spain, for instance, relevant estimates can be derived for the 1996, 2000 and 2004 parliamentary elections. The three Spanish elections allow forming three pairs of observations. Since these pairs are clearly not independent observations from each other, they are weighted so that Spain accounts for just one case in the weighted sample in the second analysis – just like all those countries for which only two elections are covered by the relevant data. For each pair of elections in this second analysis, the start and end dates of the electoral cycle contain between them at least two, and occasionally three electoral cycles, spanning 5 to 10 years. The starting and resulting levels of the governance indicators and the factor score summaries of the estimated information effects were calculated accordingly.

#### **4. Analysis**

The key results of regression analyses following the general format shown in Equation (1) are shown for each governance indicator in Table 3 for the 70 individual elections in the analysis. The control variables *Ideological Polarization*, *Effective Number of Parties* and *New Democracy* registered only one effect significant at the .05 level in any of the seven models estimated, which is just what we would expect to occur by chance during three times seven attempts. The fact that the only significant effect – a positive effect of the *Effective Number of Parties* on improvement in *Government Effectiveness* – seemed hard to interpret in substantive terms further underlined the possible chance nature of the effect, and therefore Table 3 displays results for models in which the lagged value of the dependent variable and *InformationEffect<sub>t-1</sub>* were the only independent variables.

As the standardized coefficients suggest, the starting level of the dependent variable invariably has a nearly deterministic impact on the resulting level of governance at the end of the given electoral cycle, and the R-squared values (not shown) all hover around or above .9, reaching the .96 value in three of the seven models. It comes as no surprise then that *InformationEffect<sub>t-1</sub>* has just a very limited chance to impact the outcome and is indistinguishable from zero in all equations.

The results do not change substantially if the ten elections that occurred in non-democratic or imperfectly democratic (i.e. ‘not free’ or ‘partially free’ polities in the Freedom House terminology) are excluded from the analysis, or if the control variables are added to the equations (results not shown).<sup>8</sup> The negative results stand firmly also when

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<sup>8</sup> The excluded imperfectly or non-democratic contexts were the 1997 Mexican election, and all elections in the data set from Belarus, Hong Kong, Peru, Russia, and Ukraine.

one drops from the analysis the ten cases for which less than two years passed between the election and the timepoint to which the “resulting level” of governance indicators refer to. Overall, with three of the relevant effects in Table 3 being insignificant and positive and four insignificant and negative, there is no good reason to believe that an informed electorate influences the quality of government on the short haul of a single electoral cycle.

Let’s proceed now to the second part of the analysis, where the number of cases in the analysis considerably drops, which should make it even harder to find statistically significant effects. Table 4 summarizes key results obtained with the likes of Equation (1) above. Once again, since the control variables *Ideological Polarization*, *Effective Number of Parties* and *New Democracy* had, with a minor exception, no statistically significant effect in any of the models estimated, the table displays results for models where only the lagged value of the dependent variable and the multiplicative (or cumulative) effects of information effects on election outcomes over a pair of elections enter as independent variables.<sup>9</sup>

The results are clearly more encouraging than before regarding the validity of the key hypothesis. The impact of multiplicative information effects is, as expected, negative on all indicators of governance. While only the effects for *Voice and Accountability* and *Corruption Control* pass the  $p=.05$  significance level in a two-tailed test, the factor score summary of all other governance indicators also records an effect significant at the  $p=.06$  level. These findings remain much the same if the imperfectly democratic contexts

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<sup>9</sup> The exception concerns the negative effect of the *New Democracy* dummy on the resulting level of the *Regulatory Quality*. Again, out of three times seven attempts one would really expect one such effect to occur by chance alone.

remaining in this analysis are ignored (data not shown). Overall, on the basis of Table 4 one should conclude that an informed electorate has a beneficial impact on the level of corruption control over multiple electoral cycles, and there is probably a chance that the benefits of an informed electorate also extend to other aspects of good government.

A last question that may arise is whether information effects on election outcomes are themselves endogenous to the quality of governance. One would, after all, expect the rule of law, efficient public bureaucracies, transparent systems of regulation and powerful corruption control to reduce the political information costs of citizens. If they do so, then the above findings about a reverse direction of causation may well be just spurious in spite of the control for the lagged value of the dependent variable in the regressions that yielded the results shown in Tables 3 and 4.

However, this seems quite unlikely when we regress first  $InformationEffect_{t-1}$ , and then  $(3 + InformationEffect_{t-m}) \times (3 + InformationEffect_{t-n})$  on the starting value of each of the seven governance indicators while controlling for *Ideological Polarization*, *Effective Number of Parties* and *New Democracy*. The estimates about the impact of the governance indicators – all considered one at a time – in the 14 regressions are shown in Tables 5 and 6 and are all except one statistically insignificant.<sup>10</sup> The effect is almost always negative,

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<sup>10</sup> To save space, Tables 5 and 6 do not show the effect of the three control variables that entered all 14 regression equations. Note, however, that *Effective Number of Parties* always had a statistically significant, and, as expected, positive effect on the size of information effects on election outcomes – i.e. party system fragmentation reduces the correspondence between observed and fully informed election outcomes. *Ideological*

which might indeed suggest some dependence of information effects on the quality of governance. However, the effects are always very weak and only reach statistical significance in the equation estimating the impact of *Regulatory Quality* on *InformationEffect<sub>t-1</sub>* (see the fourth row in Table 5). Moreover, the impact of *Regulatory Quality* on multiplicative information effects over pairs of elections is insignificant and actually positive (see the fourth row in Table 6), and in Tables 3 and 4 *Regulatory Quality* was the governance indicator showing the weakest evidence of being influenced by information effects on election outcomes. *Voice and Accountability* and *Corruption Control*, in their turn, showed the strongest evidence of being subject to negative information effects, and show really no sign at all of having a significant influence on them in Tables 5 and 6. Thus, we cannot refute the proposition that some aspects of good governance may slightly reduce information effects on election outcomes, but these effects can hardly account for the observed negative impact of *InformationEffect<sub>t-1</sub>* on *Corruption Control* and *Voice and Accountability*.

## 5. Discussion

My first conclusion is negative: information effects on the outcome of a single election probably do not influence the quality of government during the subsequent legislative cycle. The situation seems to be different though when effects over multiple elections are considered. It is in fact impressive how much sign of information effects on

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*Polarization* and *New Democracy* – against prior expectations – always had insignificant effect.



governance Table 4 actually identifies in spite of the low number of cases in the analysis and the near deterministic impact of the lagged values of the dependent variables. All seven tests show the effect in the expected direction, and almost half the time the effect is significant at or below the  $p=.06$  level. This can hardly occur by chance alone.

The clearer evidence for an impact of information effects on governance in the second analysis may be due partly to the longer time span between the starting and ending dates of the periods analysed here than in the first part of the analysis. This is also suggested by minor (and admittedly insignificant) improvements in model fit when the multiplicative term for interaction effects is replaced with its interaction with the number of months passed between the two dates (results not shown). The interaction between the length of the period and multiplicative information effects has the expected negative impact on all governance indicators, which is significant at the  $p=.02$  level for *Corruption Control*, at the  $p=.05$  level on the summary factor score, and at the  $p=.03$  level for *Voice and Accountability*.

A clear parallel between the present findings and those of Adserà *et al.* (2003) is that information effects are probably more in evidence on corruption control than on other aspects of governance. This finding seems very robust in the face of changes in the measurement of both the dependent and the independent variable, the range of controls employed, the time period or the sample of countries analysed. The reason maybe that information effect on corruption control work through more direct routes than other information effects on the quality of governance. Spectacular corruption scandals often are subject of election campaign discourse, and more or less obvious culprits often have to leave office as a result. Other aspects of governance are probably less easily influenced by citizens' practical political knowledge on the short term. The present analysis also provided

some hints at a possible – though weak – endogeneity of information effects to these other aspects of good governance, though not to corruption control.

The present findings are, thus, ambiguous about whether other aspects of governance than corruption control are influenced by information effects on election outcomes at all. The balance of the evidence is probably leaning towards a positive answer, but with question marks remaining about endogeneity. However, if these information effects are real, then they probably account for a non-negligible part of the usually very small changes that occur over time in the quality of governance in any given country. The pairwise correlations between change in the governance indicators on the one hand, and multiplicative information effects measured over two successive elections across the 25 countries are -.48 (p=.02), -.20 (p=.35), -.26 (p=.22), -.17 (p=.43), -.30 (p=.14), and -.50 (p=.01) for the six indicators, respectively, and -.42 (p=.04) for the summary measure.

These figures do not support the contention of Adserà *et al.* (2003) that between one-half to two-thirds of the change in governance over time may be explained by citizens' information level. Probably their interpretations overlooked some other mechanisms through which a resourceful free press can promote good government. However, the more modest present findings point far more directly to a probably very important role of information shortcuts available to citizens, as well as the soft infrastructure of electoral democracy that provides these, in increasing collective welfare.

### **Acknowledgement**

This paper was written while the author held a Marie Curie Intra-European Fellowship under the TMR program of the European Union, contract no. 025384. Comments by Pippa Norris, James Stimson and two anonymous reviewers helped to improve the paper.

**Appendix: Independent variables in Equation (3), i.e. the simulation function  
generating the *Information Effect* variables**

AGE: the age of the respondent in years;

AGESQ: age squared;

DEVOUT: a measure averaging the within-country standardized scores of the frequency of church attendance (from 1=never to 6=weekly) and subjective religiosity (from 1=has no religious beliefs to 4=very religious), with missing values on both input variables replaced by the sample mean.

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 election;

MANUAL WORK: coded 1 for non-agricultural manual workers and 0 otherwise;

MINORITY 1: coded 1 for Catholics in Albania; Asians in Australia; Belorussian-speakers in Belarus; American Indians, Blacks, and Mulatto in Brazil; French-speakers in Flanders and Dutch-speakers in Wallonia in Belgium; Moslems and Turkish or Pomak ethnicity in Bulgaria; English-speakers or English/Scottish/Welsh/Irish/British ethnicity in Quebec; French-speakers or French ethnicity in the rest of Canada; residents of Moravia in the Czech Republic; Moslems in France; Swedish-speakers in Finland; Catholics in Germany; Christians in Hong Kong; Roma in Hungary; Protestants in Ireland; in Israel for respondents

whose or themselves were born in North Africa, Ethiopia or Asia; Christians in South Korea; people of Polish ethnicity in Lithuania; natives in Mexico; Catholics in the Netherlands; Maori people in New Zealand; Tagalog in the Philippines; people of African or Asian racial origin in Portugal; ethnic Hungarians in Romania; anyone who is not a Russian-speakers or of Russian ethnicity in Russia; Croatian, Serb or ‘Moslem’ ethnicity in Slovenia; Catalan-speakers in Spain; Catholics in Switzerland; 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 Orthodox in Albania; Catholics in Australia; Polish-speakers, Polish ethnic origin, and Catholics in Belarus; Catholics in English-speaking provinces of Canada; Buddhists in Taiwan; people of Russian ethnicity in Lithuania; Catholics in New Zealand; Cebuano in the Philippines; Moslems in Russia and Thailand; Italian-speakers or ethnics in Switzerland; 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.

KNOWLEDGE: this variable sums up the ‘truth values’ of the respondents’ placement of major political parties on eleven-point left-right scales.<sup>11</sup> The estimation of truth

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<sup>11</sup> The placements of small regional parties that were only available for small subsets of the British and Spanish samples were ignored. The number of parties that the respondents placed on the left-right scale ranged from three in Taiwan, the UK and the US, to nine in the 2002 Dutch data set. To compensate for the relatively low number of parties for which left-right party placements are available, party placements on an alternative issue

values reckons that different respondents of equally high knowledge may place the same parties differently on the scale depending, for instance, on their own partisanship, or their idiosyncratic interpretation of the scale and its endpoints. Therefore, those aspects of the responses that may reveal more about idiosyncratic political views than knowledge were disregarded in two ways. First, the absolute placements of individual parties were replaced with relative placements involving pairs of parties. All responses regarding each pair were recoded into just four categories: (1) party A is to the left of party B; (2) party A is to the right of party B; (3) party A and party B have the same position; or (4) the respondent did not answer the question, or responded with a ‘do not know’.

Second, since left-right placements are eminently disputable questions in everyday political discourse, the truth-value of each answer was conceptualized here as a matter of degree, revealed by the extent to which a maximally informed respondent was more likely to give that response than a maximally uninformed respondent.

This difference can be estimated by regressing relative party placements on other available indicators of political knowledge in the CSES surveys, which included three country-specific questions and – in the CSES 1 surveys – name recognition of candidates running for election in the respondent’s electoral district, all recoded into

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scale were also included in the analysis alongside the left-right scale in the analysis of the 1996 Taiwanese and the 1997 UK data; and leader placements on the left-right scale were considered alongside party placements for the 2004 US and the 2005 UK data. In the analysis of the 1996 and 2004 Japanese data, a progressive-conservative scale was used instead of left-right because the latter was not available at all.

correct answers versus any other response. The multinomial logit analyses that were carried out for each pairwise comparison of parties on the left-right scale also included among the independent variables AGE, AGESQ, FEMALE, INCOME, EDUCATION LOW and EDUCATION HIGH (see above). These controls assure that the estimated truth values are not affected by the fact that the socio-demographic groups that are likely to score high on lexical knowledge variables may share a particular political taste that impacts the parties' perceived left-right stances.

The results of these multinomial regressions are of no substantive interest here. The relevant yield of these analyses were the predicted probabilities of each of the four response categories for two fictitious respondents: both exactly matching the national sample mean on the socio-demographic variables, but one showing the highest, and the other the lowest possible degree of knowledge. Then, the truth-value of each response category was determined as the difference between its predicted probability for the maximally involved and the maximally uninvolved respondent.

This method of determining the relative truth-value of the responses allows for the possibility that 'do not know' or missing answers may not always represent less knowledge than some other responses do (cf. Mondak and Davis, 2001; Mondak and Canache, 2004; but see Luskin and Bullock, 2005), and that sometimes there are several equally good answers to the same party placement question. The method also gives a natural weighting of party pairs and scales for the building of the knowledge scale, and uses the same metric across the whole universe of between-party comparisons and response categories. Summing up the respective 'truth-value'

of the individual responses across all pairwise comparisons available yields a very nearly normal distribution of scores across respondents within most national samples in the CSES data set (data not shown). To fully standardize the distribution across the voting populations in the 70 elections – which was necessary given that the sample mean and variance was dependent on the number of parties placed on the left-right scale in each survey –, these scores were converted into normal scores constrained to fall in the 0 to 1 range, with a within-sample mean of approximately .5 and standard deviation of approximately .16. This rescaling completed the construction of the individual level *Knowledge* variable that was then used in the simulation of aggregate-level information effects on election outcomes as described above.

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**Table 1: Descriptive statistics for variables about individual election cycles**

	<b>Min.</b>	<b>Max.</b>	<b>Mean</b>	<b>Std. Dev.</b>
<i>Resulting level of Voice and Accountability</i>	-1.69	1.72	0.87	0.70
<i>Resulting level of Political Stability</i>	-1.26	1.60	0.54	0.72
<i>Resulting level of Government Effectiveness</i>	-1.20	2.28	1.09	0.91
<i>Resulting level of Regulatory Quality</i>	-1.55	1.95	0.99	0.71
<i>Resulting level of Rule of Law</i>	-1.07	2.03	0.93	0.96
<i>Resulting level of Control of Corruption</i>	-1.02	2.57	1.04	1.06
<i>Resulting level of Governance (factor score)</i>	-2.56	1.24	0.00	1.00
<i>Starting level of Voice and Accountability</i>	-1.36	1.68	0.86	0.68
<i>Starting level of Political Stability</i>	-1.54	1.57	0.54	0.73
<i>Starting level of Government Effectiveness</i>	-1.04	2.48	1.12	0.96
<i>Starting level of Regulatory Quality</i>	-2.36	1.92	0.92	0.75
<i>Starting level of Rule of Law</i>	-1.12	2.20	0.95	0.96
<i>Starting level of Control of Corruption</i>	-0.90	2.49	1.08	1.05
<i>Starting level of Governance (factor score)</i>	-2.57	1.30	0.00	1.00
<i>Information Effect on Election Outcome at t-1</i>	-1.55	3.52	0.00	1.00
<i>Ideological Polarization</i>	0.21	3.08	1.77	0.68
<i>Effective Number of Parties</i>	1.43	10.00	4.03	1.50
<i>New Democracy</i>	0.00	1.00	0.46	0.50

N=70 election cycles.

**Table 2: Descriptive statistics for variables about pairs of elections**

	Min.	Max.	Mean	Std. Dev.
<i>Resulting level of Voice and Accountability</i>	-0.87	1.72	1.01	0.61
<i>Resulting level of Political Stability</i>	-1.16	1.60	0.58	0.73
<i>Resulting level of Government Effectiveness</i>	-0.55	2.20	1.25	0.82
<i>Resulting level of Regulatory Quality</i>	-0.45	1.95	1.15	0.59
<i>Resulting level of Rule of Law</i>	-0.91	2.03	1.09	0.91
<i>Resulting level of Control of Corruption</i>	-0.76	2.46	1.19	1.01
<i>Resulting level of Governance (factor score)</i>	-2.42	1.11	0.00	1.00
<i>Starting level of Voice and Accountability</i>	-0.49	1.68	0.95	0.65
<i>Starting level of Political Stability</i>	-0.71	1.52	0.67	0.67
<i>Starting level of Government Effectiveness</i>	-0.88	2.48	1.31	0.99
<i>Starting level of Regulatory Quality</i>	-1.02	1.70	0.91	0.62
<i>Starting level of Rule of Law</i>	-0.95	2.20	1.16	0.93
<i>Starting level of Control of Corruption</i>	-0.90	2.48	1.33	1.05
<i>Starting level of Governance (factor score)</i>	-2.44	1.10	0.00	1.00
<i>Information Effect on Election Outcome at t-n</i>	-1.64	3.37	0.00	1.00
<i>Information Effect on Election Outcome at t-m</i>	-1.35	2.19	0.00	1.00
<i>Multiplicative information effects over pairs of elections (Information Effect on Election Outcome at t-n multiplied with Information Effect on Election Outcome at t-m)</i>	3.44	27.63	9.36	5.79
<i>Ideological Polarization</i>	0.64	2.98	1.80	0.69
<i>Effective Number of Parties</i>	2.56	5.88	3.98	0.95
<i>New Democracy</i>	0.00	1.00	0.40	0.50

N=35 pairs of elections in 25 countries weighted to give each country a weight of one.

**Table 3: Selected results from OLS-regressions of seven governance indicators on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1***

<b>Dependent variable</b>	<b>B</b>	<b>Std. Error</b>	<b>Beta</b>	<b>sig.</b>
<i>Resulting level of Voice and Accountability</i>	-0.037	0.022	-0.054	0.098
<i>Resulting level of Political Stability</i>	-0.007	0.024	-0.010	0.774
<i>Resulting level of Government Effectiveness</i>	0.000	0.027	0.000	0.998
<i>Resulting level of Regulatory Quality</i>	0.028	0.029	0.040	0.337
<i>Resulting level of Rule of Law</i>	0.012	0.012	0.012	0.326
<i>Resulting level of Corruption Control</i>	-0.034	0.025	-0.032	0.178
<i>Resulting level of Governance (factor score)</i>	-0.007	0.018	-0.007	0.693

N=70 election cycles. The constants and the effects of the lagged dependent variable are not shown.

**Table 4: Selected results from OLS-regressions of seven governance indicators on the lagged value of the dependent variable and multiplicative information effects over pairs of elections**

<b>Dependent variable</b>	<b>B</b>	<b>Std. Error</b>	<b>Beta</b>	<b>sig.</b>
<i>Resulting level of Voice and Accountability</i>	-0.019	0.008	-0.177	0.039
<i>Resulting level of Political Stability</i>	-0.009	0.010	-0.073	0.352
<i>Resulting level of Government Effectiveness</i>	-0.008	0.010	-0.058	0.421
<i>Resulting level of Regulatory Quality</i>	-0.004	0.011	-0.043	0.701
<i>Resulting level of Rule of Law</i>	-0.006	0.005	-0.041	0.215
<i>Resulting level of Corruption Control</i>	-0.017	0.008	-0.095	0.043
<i>Resulting level of Governance (factor score)</i>	-0.016	0.008	-0.095	0.062

N=35 pairs of elections in 25 countries weighted to give each country a weight of one. The constants and the effects of the lagged dependent variable are not shown.

**Table 5: Selected results from OLS-regressions of *Information Effect on Election Outcome at t-1* on seven different governance indicators and an unchanging set of control variables**

<b>Governance indicator in the given model</b>	<b>B</b>	<b>Std. Error</b>	<b>Beta</b>	<b>sig.</b>
<i>Starting level of Voice and Accountability</i>	-0.183	0.265	-0.125	0.491
<i>Starting level of Political Stability</i>	-0.141	0.180	-0.104	0.436
<i>Starting level of Government Effectiveness</i>	-0.086	0.190	-0.083	0.652
<i>Starting level of Regulatory Quality</i>	-0.459	0.179	-0.343	0.012
<i>Starting level of Rule of Law</i>	-0.079	0.188	-0.076	0.677
<i>Starting level of Corruption Control</i>	0.087	0.180	0.091	0.632
<i>Starting level of Governance (factor score)</i>	-0.157	0.175	-0.157	0.372

N=70 elections. The constants and the effects of the control variables are not shown.



**Table 6: Selected results from OLS-regressions of multiplicative information effects over pairs of elections on seven different governance indicators and an unchanging set of control variables**

<b>Governance indicator in the given model</b>	<b>B</b>	<b>Std. Error</b>	<b>Beta</b>	<b>sig.</b>
<i>Starting level of Voice and Accountability</i>	1.204	2.604	0.136	0.649
<i>Starting level of Political Stability</i>	-2.565	1.721	-0.299	0.152
<i>Starting level of Government Effectiveness</i>	0.523	1.850	0.090	0.780
<i>Starting level of Regulatory Quality</i>	1.631	1.980	0.174	0.420
<i>Starting level of Rule of Law</i>	-0.741	1.932	-0.119	0.705
<i>Starting level of Corruption Control</i>	-0.260	1.873	-0.047	0.891
<i>Starting level of Governance (factor score)</i>	-0.174	1.703	-0.030	0.920

N=35 pairs of elections in 25 countries weighted to give each country an equal weight of one. The constants and the effects of the control variables are not shown.