Citizen Information, Election Outcomes and Good Governance

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Abstract:

Does a better informed citizenry lead to better government? Citizens cannot possibly attend to the infinite amount of information that politics produces, but can be more or less wise in deciding what cues to vote choice their trust. Where their information environment is rich in cues coming from dependable and knowledgeable sources, they can probably vote as if they were fully informed. But this ideal situation may be approximated to varying degrees as studies simulating the difference between fully informed outcomes and actual election results demonstrated (cf. e.g. Bartels 1996; Lau and Redlawsk 2001; Althaus 2003; Sturgis 2003; Sekhon 2004). This paper takes this gap as the measure of citizen knowledge and tests if wise voting behavior of citizens improves the quality of governance after the election. 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 remarkably strong correlation but also that the effect only materializes over multiple elections.

This paper intends to provide a new empirical test of the proposition that elections enhance collective welfare to the extent that citizens are equipped by political communication to vote as if they were fully informed. In other words, it asks if we can generalize 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: 180-1). To the detriment of our understanding of democratic government, the connection between citizen knowledge – read political communication – and collective welfare has rarely been subjected to empirical testing. The first section of this paper seeks a justification for the hypothesis in terms of theory and previous findings. Section two 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 three presents the empirical analysis, and section four concludes.

1. A plausible, important, but insufficiently tested proposition

The claim that elections enhance collective welfare to the extent that citizens vote as if they were fully informed is underlined by two of the probably most frequent assumptions in political science, namely that politics, by and large, is not a zero-sum game (Laver 1981), and 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. Incidentally, they also provide an influential argument about the supremacy of decisions made by representative assemblies over direct democracy (Berelson 1952; Schattschneider 1960; Schumpeter 1942). Thus, if the proposition that an informed citizenry is essential for collective welfare were to fail, other victims may follow suit.

As an example of how the proposition may work, lack of corruption among office holders can be considered a public good, and we can think of a knowledgeable electorate as its guardian. Adserà *et al.* (2003) offer a formal model establishing this claim. In this 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 favored policies even if those deviate from the preferences of the constituents. Through periodic elections, citizens can punish their representatives for reducing their welfare via extracting these private benefits. Politicians care about reelection and rather sacrifice some of these private benefits than risk losing the next election. The price that citizens pay for delegating power to someone else are the remaining inefficiencies in aggregating their preferences into decisions made on their behalf, such as the degree of corruption among officials. The model shows that the size of these inefficiencies is partly a function of the information asymmetry between principals and agents regarding the acts of the latter. Thus, citizen knowledge enhances collective welfare.

The model applies for any political outcome, not just corruption. For its ultimate implication to hold, one does not even need to assume that certain preferences are shared by all, or that it enhances collective welfare if the desire of numerical majorities prevails over minorities. Instead, the median voter theorem suggests that even if there are no commonly shared preferences at all, the outcome favored by the median citizen must maximize collective welfare (Mueller 1989: xxx). The reference point of this theorem is of course a spatial model where the alternative policies on each issue form a continuum, and all relevant actors who prefer a policy A located to the left of another policy B will also prefer A to any policy C that is to the right of B on the same continuum. If voters always support the party that, to their best knowledge, is closest to their most preferred point in the multidimensional space of relevant policies, then perfect knowledge of party positions may well be one of the conditions to elect a legislature in which outcomes are dominated by the preferences of the median voter.

Thus, as long as democratic rule lives up to its own premises, it should also provide for good government to the extent that citizens are adequately informed when it comes to choosing between political alternatives. This proposition would, of course, be inconsequential if citizens were always perfectly well-informed – or perfectly ignorant – in every election. Yet this seems highly unlikely. The scholarship of the last two decades extensively explored two reasons why collective outcomes may, at times, reflect 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" – in spite of the rational ignorance of individual citizens. The less important one for the present discussion is the Condorcet jury theorem, which asserts that errors of judgments committed by individual voters cancel out each other in the aggregate (see e.g. Miller 1986; Page and Shapiro 1992;

Austen-Smith and Banks 1996(Austen-Smith and Banks 1996)(Austen-Smith and Banks 1996)(Austen-Smith and Banks 1996)(Austen-Smith and Banks 1996)). The other is that a large amount of political information is dispersed among voters at virtually no direct cost to citizens themselves. This happens either as a by-product of non-political activities that they undertake anyway (Fiorina 1981, 1990), or because political entrepreneurs, interest groups and news media readily underwrite the costs of information gathering and dispersion in the form of accessible cues among citizens (Becker 1985; Lupia 1994; Popkin 1991; Wittman 1989). Both arguments solve the paradox that a rationally ignorant electorate is consistent with impressive levels of collective welfare, and both found some support in empirical studies of public opinion and voting behavior (see e.g. Page and Shapiro 1992; (Miller 1986)(Miller 1986)(Miller 1986)(Miller 1986)Lupia and McCubbins 1998).

However, a number of recent works suggest that even information shortcuts and aggregation mechanisms may, at times, fail in helping the most preferred collective outcome to emerge (Lau and Redlawsk 1997, 2001; Martinelli 2006). As a result of these occasional failures, election results and aggregate public opinion sometimes coincide, and sometimes significantly differ from what the same counts might show if all citizens were fully informed (Althaus 2003; Bartels 1996; Sekhon 2004). It would seem then that variability in the availability of efficient information shortcuts to voters, and thus in patterns of political communication, creates at least some space for variations in citizen knowledge that can make a difference in collective welfare.

The proposition that this really happens involves long causal chains. Several elements of these have been examined before, but surprisingly few previous studies attempted to test the proposition 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 1996, 1998, 2001, 2003; Alvarez 1997; Andersen, Heath and Tilley 2005; Barabas 2004; Bartels 1996; Delli Carpini and Keeter 1996: 238ff; Fishkin 1997: 214-228; Lupia 1994; Luskin, Fishkin and Jowell 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. 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, Heath and Tilley 2005; Bartle 2004; Delli Carpini and Keeter 1996: 256-8; Gomez and Wilson 2001; Goren

1997; Hobolt 2004; Lau and Redlawsk 2001; Lupia 1994; Luskin 2003; Sniderman, Glaser and Griffin 1990; Sturgis and Tilley 2004; but cf. Zaller 2004). In addition, evidence from deliberative polls demonstrates that cycles in collective preferences become less frequent as citizens become more knowledgeable due to deliberation (see Farrar *et al.* 2006; List *et al.* 2006). All this provide indirect evidence that as citizens' political knowledge increases, vote choices become increasingly more accurate expressions of the policy preferences that people would hold if they were fully informed. The same point is borne out by formal models and experimental results (McKelvey and Ordeshook 1985, 1986; Lupia 1992; Lau and Redlawsk 2001).

Another part of the long causal chain, the translation of expressed preferences into public policy, is far less clear. Yet, there is a bit of evidence that the popular desires impacting election outcomes influence not only who gets elected but also the policies adopted (Bartels 1991; Erikson *et al.* 1993; Geer 1996). Of course, 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). But some empirical studies nonetheless suggest that democratic elections may, 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; MacDonald *et al.* 2004).

Of course, all this falls short of a comprehensive test of all components in the long causal chain that may link the position of the median citizen and the information level of the electorate to public policies. Yet given the difficulty of mapping citizen preferences – and especially the voter's "true views" – and public policies in the same multidimensional space, we may have to wait for quite some time before much more can be achieved in terms of a step-by-step analysis. Hence, it seems a timely task to provide a direct test of the link between citizen knowledge and collective welfare that disregards the multiple elements of the complex causal chains that may link them.

Yet, most studies that mention the possibility of this link fall far short of empirically demonstrating it. For instance, experimental studies stop at pointing out the role of specific information or general knowledge in allowing citizens to vote as if they were fully informed (McKelvey and Ordeshook 1985, 1986; Lupia 1992; Lau and Redlawsk 2001). Delli Carpini and Keeter (1996), in turn, use a wealth of cross-sectional survey data but stop with demonstrating that more knowledgeable citizens show greater attitude constraint and arguably less prejudiced opinions in selected attitude domains. Norris (2004) looks at macro-level data and shows bivariate 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 either common causes or the political role of media themselves. As Milner (2002) demonstrates, one difficulty with testing the link is the dearth of cross-nationally comparable data on citizens' political information level. Even the exploration of national trends over time is rather complicated and hardly yields results that could be readily compared to data about possible temporal changes in collective welfare (cf. Smith 1989).

Thus the one seminal study to date that claims to establish the link beyond reasonable doubt 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. The basic finding 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 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 arguably better indicators of the key variables.

To see the motivation for the proposed changes in measurement, note that newspaper readership may be a better proxy for the resources available to independent mass media than for level of political knowledge among citizens. The credible threat of widely publicized critical reports appearing in the press may in itself motivate good behavior among politicians and bureaucrats even where electoral sanctions of bad behavior are not possible or likely. Clearly, Adserà et al. (2003) would be more convincing if they relied on somewhat more direct measures of informed voting behavior than newspaper readership in the population.

This caveat becomes particularly relevant when we consider what measures of government quality provide support for their key claim. In the first data set analyzed 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 readership once the lagged value of the dependent variable is controlled for. These are "lack of corruption" and "bureaucratic quality". In contrast, the effect on the "rule of law" is positive but far from statistically significant, and the effect on "freedom from the risk of expropriation of property" is negative and highly significant. The second data set that they analyze 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 newspaper readership 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 behavior of variables related to corruption, and to a lesser extent on findings regarding bureaucratic quality. Adserà *et al.* (2003: 459) do add explanations why their theory may apply less for other legitimate indicators of government quality. First, the rule of law variable incorporates perceptions of compliance among citizen, 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." Hence, they imply, newspaper readership may well promote expropriation of property, a sign of poor governance.

Yet, these explanations appear less than fully convincing. If we accept Adserà *et al.*'s (2003) reasoning, then the negative impact of newspaper readership on freedom from the risk of expropriation of property suggests that a well-informed public is more likely to demand the kind of policies towards private property that the international agencies providing the raw data for this indicator clearly consider instrumental for high quality governance. This is not impossible, but the opposite expectation would seem at least equally plausible. With respect to their explanation regarding the rule of law, a troubling point is that a similar argument could just as well be advanced with respect to bureaucratic quality – surely governments can only influence some aspects of that variable only on the long run. 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' behavior play at most a marginal role in measuring the rule of law (cf. Kaufmann *et al.* 1999: Appendix A).

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 used in the analysis than in previous studies. The next section explains how the present study seeks to achieve this.

2. Data and measures

The dependent variable of interest is the quality of government. The aim of the empirical analysis is to estimate simple OLS-regression models that take the following general form:

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

For simplicity, the equation above omits indexing for contexts i, which will be 61 elections in the first, and 23 countries in the second part of the analysis. $Governance_t$ and $Governance_{t-1}$ will be 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. C_j are various control variables that enter the equation to increase our confidence that the b_2 estimate about the impact of $InformationEffect_{t-1}$ on $Governance_t$ is neither spuriously low nor spuriously high. Coefficients b_1, $b_2 \ldots$ b_k and constant a are to be estimated with an ordinary least squares equation, and ε is simply a residual error term with a zero mean that is assumed to have a normal distribution.

The indicators for the $Governance$ variables come from Kaufmann *et al.* (2006), a further improved and enlarged version of the second data set used by Adserà *et al.* (2003). This is by far the most comprehensive data set currently available on those aspects of good governance that are relatively neutral with respect to substantive policy content but are conducive for the effective allocation of public assets towards the implementation of whatever public policies decided by the respective authorities. Unlike the 1999 release analyzed by Adserà and associates, the 2006 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 the missing data were replaced here with the average value of the given indicator for the given country one year before and one year later. The six measures tap *Voice and Accountability*, *Political Stability*, *Government Effectiveness*, *Regulatory Quality*, *Rule of Law*, and *Corruption Control*. These measures comprehensively cover those aspects of governance that can help minimizing the deviation of all other policy outputs from the preferences of the median voter, and thus are likely to be preferred by that voter unambiguously. Note that Kaufmann *et al.* (2006) constructed these indicators with an unobserved component model from hundreds of variables provided in 31 data sources by 25 different organizations. Thus they indeed are a very comprehensive 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 factor scores of individual cases on a single common factor defined by them will be considered in the present analysis.

The theory to be tested presumes that it is the electoral control of politicians by voters that mediates the relationship between citizens' political information level and collective welfare. Thus the dependent variables in the analysis ($Governance_t$ 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. 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 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 with 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 citizens' political information level. However, it is not exactly the overall amount of lexical political knowledge that our theory attributes this key role. Rather, it is the difference between actual election results and those that would obtain if all citizens were fully informed. Following

Althaus' (1998, 2001, 2003) terminology, this effect can be called the information effect on the election outcome. The total size of this effect is calculated with the Pedersen index of volatility, i.e. as half the sum of the absolute differences between each party's actual share of (recalled) votes and their share of the fully informed votes (on the latter see below). The smaller the total effect, the closer citizens' behavior is to perfectly emulating fully informed voting behavior – which is all what is required from citizens' stock of knowledge to facilitate maximal electoral control of elected officials.

Following the simulation procedure developed by Bartels (1996), fully informed vote distributions can be estimated in 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 data used here come from post-election surveys carried out after 61 elections in 32 countries on five continents, mostly in advanced postindustrial democracies, in the framework of the Comparative Study of Electoral Systems project between 1996 and 2005 (see CSES 2003, 2006).[1] The exact way these data were used to estimate information effects on the vote is described in the Appendix. At this point it is enough to note that the resulting macro-variable, *Information Effects on Election Outcome at Time t-1*, is a factor score summary of 16 separate but strongly correlated estimates of cross-election variation in the variable in question. Thus it has a mean of zero and unit variance across the cases in the analysis (see Tables 1 and 2).

Further control variables enter the analysis as 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.[2] The first is expected to reduce, and the latter to increase information effects. The final control variable is a dichotomous variable called *New*

[1] 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; Lithuania 1997; Russia 2000; Slovenia 1996; Thailand 2001; United States 1996.

[2] 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 fractionalization too (see Appendix A). 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 61 elections.

Democracy, which follows up on Sekhon's (1994) suggestion that established democracies experience the benefit of smaller information effects.[3] Since these characteristics of the political system may create spurious correlations between information effects and governance indicators, their impact on the latter will be controlled for.

A valuable feature of these data is that they allow separate analyses of (1) short-term effects of citizen knowledge on collective welfare, which occur over a single electoral cycle as a result of a more or less close correspondence between actual and fully informed outcome in the immediately preceding election; and (2) similar effects that materialize over a slightly longer time span because of a rational expectation of politicians that they can – or they cannot – easily get away with poor performance in office by relying on other electoral assets than closely matching the preferences of prospective supporters. This distinction is relevant for two reasons. First, given the inertia of complex government machineries it may be unrealistic to expect that large changes can occur along any of the six indicators of 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. Politicians cannot directly observe information effects. They may have some indirect and largely intuitive knowledge of generally how far voting behavior may be subject to information effects in their country, and this expectation may make them more or less diligent in following the preferences of their constituents or seeking other routes of getting reelected. But it is unlikely that they dramatically revise their intuitive knowledge of citizen behavior 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, separately from single election cycles, the multiplicative impact of information effects over several successive elections on how the quality of government changes as a result. In order to achieve this, the data set covering 61 elections was reorganized below into a smaller data set covering the 23 countries for which information effects

[3] 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, the eastern states of the united Germany, Hong Kong, Hungary, South Korea, Mexico, Peru, the Philippines, Poland, Romania, Russia, and Ukraine. The remaining countries included Australia, Belgium, Canada, Denmark, Finland, France, the western states of the united 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.

can be estimated from the CSES data for more than one election in the 1996-2005 period. For Spain, for instance, relevant estimates can be derived for the 1996, 2000 and 2004 parliamentary elections. The three 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 this 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 analysis, the start and end dates of the electoral cycle contain between them at least two, and occasionally more than two electoral cycles. The starting and resulting levels of the governance indicators were calculated accordingly. Thus, the second part of the analysis estimates equations that take the following general form:

$$\begin{aligned} Governance_t = a + b_1 Governance_{t-n} + b_2(3 + InformationEffect_{t-m}) \times \\ (3 + InformationEffect_{t-n}) + \sum_{j=1}^{k} b_j C_j + \varepsilon \end{aligned} \tag{2}$$

where $m < n$, and $b_2(3 + InformationEffect_{t-m}) \times (3 + InformationEffect_{t-n})$ is the multiplicative effect of several elections on the quality of governance through information effects. The addition of a constant of 3 appears in this formula to avoid multiplying negative numbers – remember that $InformationEffect_{t-n}$ and $InformationEffect_{t-n}$ are constructed as factor scores from 16 different estimates –, and thus deriving a bigger product for two very small information effects than for two medium size information effects on subsequent election outcomes.

Tables 1 and 2 show descriptive statistics about the variables in the first and second part of the analysis, respectively. Note that the factor scores for the starting and resulting levels of governance as well as for the size of information effects at time *t-n* and time *t-m* always have a zero mean and unit variance across a particular data set because the factor analyses were run separately for the two versions of the data set and for each variable.

3. Analysis

Regression analyses following the general format shown in Equation (1.1) above are shown for each governance indicator in Tables 3-9 for the 61 individual elections in the analysis. Since the control variables *Ideological Polarization*, *Effective Number of Parties* and *New Democracy* had no statistically significant effect in any of the models estimated and their exclusion made no substantive change in other results, the tables display results for models where only the lagged value of the dependent variable and $InformationEffect_{t-1}$ enter as independent variables.

As the tables witness, all the dependent variables show high stability over time. The lagged value of the dependent variable invariably has a nearly deterministic impact on the resulting level at the end of the given electoral cycle, and the R-squared values (not shown) all hover around or above .9, reaching the .97 value in three of the seven models. It comes as no surprise then that any other independent variable has very limited chances to impact the outcome. Indeed, just as none of the control variables registered any statistically significant effect, the impact of $InformationEffect_{t-1}$ too is indistinguishable from zero in all but one of the seven equations. This exception concern *Control of Corruption*, for which the impact of information effects is, as theoretically expected, negative, and is significant at the p=.072 level (two-tailed test). However, the same effect fails to reach even the p=.10 significance level if panel corrected standard errors are used, and p becomes as high as .22 when each of the 34 countries in the analysis are given equal weight and the weighted number of cases is set to 34 (results not shown). Since either of these corrections seems reasonable in the light of the clustering of elections by countries, the conclusion must be that information effects on the outcome of a single election do not influence the quality of government during the subsequent legislative cycle. The results do not change substantially if the 9 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).[4]

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. Tables 10 to 16 summarize key results obtained with the likes of Equation (1.2) 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, and their exclusion made no substantive change in other results, the tables display results for models where only the lagged value of the dependent variable and the

[4] 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.

multiplicative (or cumulative) effects of information effects on election outcomes over a pair of elections enter as independent variables.[5]

The results are mixed, but more encouraging than before regarding the validity of the key hypothesis than before. The impact of multiplicative information effects is, as expected, now negative on all indicators of governance. While only the effect for *Corruption Control* passes the p=.05 significance level in a two-tailed test, some – most notably the ones for *Rule of Law* and the factor summary of all other governance indicators – pass some less demanding but still respectable significance levels. These findings are not altered if the three control variables are added to the equations, or the imperfectly democratic contexts are excluded from the analysis.

4. Discussion

The first conclusion of this analysis must be negative: information effects on the outcome of a single election do not influence the quality of government during the subsequent legislative cycle. The situation is clearly different when effects over multiple elections are considered. Given the conservative weighting of the cases that is employed in the second analysis to adjust for the three times larger number of election pairs for Hong Kong, Mexico and Spain than for all other cases, no further adjustment of the standard error estimates seems needed there. Thus, given the fairly low number of cases, the still relatively short time-span allowed for information effects, and the near deterministic impact of the lagged values of the dependent variables, it may seem quite impressive how much sign of information effects on governance that analysis actually identifies.

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 analyzed 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 significant at the p=.03 level in the equation for the summary factor score, and at the p=.06 level for *Voice and Accountability*.

[5] The exception concerns the marginally significant (p=.06) positive effect of the *New Democracy* dummy on rule of law over time, which fits common sense.

Another reason for the stronger information effects in this analysis may well be the shift to their multiplicative conceptualization. At least when the multiplicative term is replaced by its two component parts in the models reported in Tables 10 to 16, none of the information effects reaches the p=.10 significance level, most are very weak, and a few even positive. Thus, it would seem that only the cumulative experiences of a country with consistently below-average (or consistently above-average) information effects on election outcomes make a difference for the quality of governance.

A clear and somewhat puzzling parallel between the present findings and those of Adserà *et al.* (2003) underlines that information effects on corruption control are far bigger and more consistent 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 analyzed. One reason maybe that this effect works through more direct routes than other information effects on the quality of governance. Spectacular corruption scandals often are subjects of election campaign discourse, and more or less obvious culprits often have to leave office as a result. Probably these personal changes alone can cause a change in atmosphere and perceived corruption in a country. Other influences of citizen knowledge on governance may indeed take a longer period of time to mature.

The present findings are not unambiguous about whether other aspects of governance than corruption control are in fact influenced by information effects on election outcomes. But the balance of the evidence seems to lean towards a positive answer. Moreover, the results suggest that if these effects are real, then they probably account for an impressive 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 23 countries are -.31 (p=.15), -.25 (p=.26), -.26 (p=.23), -.12 (p=.60), -.39 (p=.07), and -.54 (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. But they do point 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.

Clearly, further studies are needed to explore these relationships in larger data sets, over longer periods of time, with a greater range of control variables, and more advanced statistical methods. Yet the present study probably did enough to demonstrate the feasibility and, maybe, great relevance of such undertakings for our better understanding of democratic processes and the origins of collective welfare.

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Appendix: The construction of the information effect variables

The key independent variable in the analyses reported here (i.e. *InformationEffect* in its various incarnations) is the difference between actual election results and those that would obtain if all citizens were fully informed. The total size of this 'information effect' is calculated with the Pedersen index of volatility, i.e. as half the sum of the absolute differences between each party's actual share of (recalled) votes and their share of the fully informed votes (on the latter see below). The smaller the total effect, the closer citizens' behavior is to perfectly emulating fully informed voting behavior – which is all what is required from citizens' stock of knowledge to facilitate maximal electoral control of elected officials.

Following the simulation procedure developed by Bartels (1996), fully informed vote distributions can be estimated in 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 simulation involves regression-type analyses of cross-sectional data about political preferences and their determinants. These equations must follow the general pattern shown in Equation 3 below and involve, among the independent variables, a constant a, various exogenous determinants of political preferences – i.e. sex, age, etc., indicated as the $X_1, X_2, \ldots X_n$ variables in Equation 3 below -, and interactions between all the X variables on the one hand, and political knowledge (the *Knowledge* variable) on the other.

$$\text{(3)} \qquad Preference = fn\begin{pmatrix} a + b_0 Knowledge + b_1 X_1 + b_2 X_2 + \ldots + b_i X_n + \\ +b_{n+1} X_1 Knowledge + b_{n+2} X_2 Knowledge + \ldots + b_{2n} X_i Knowledge \end{pmatrix}$$

Note that the *fn* linking function can be linear as in work by Delli Carpini and Keeter (1996), probit as in Bartels (1996), or assume any number of other functional forms that are appropriate given the distributional properties of the dependent variable and its expected relationship with the independent variables.[6] In any case, equations of the above type can readily generate estimates of how particular individuals may change their revealed political preferences – e.g. vote choices in elections or responses to attitude items in surveys – if their knowledge level increased. All one has to do is to:

1. substitute some fictitiously high value to the actual observations about the individual respondents' political knowledge level; then
2. use the information contained in the X variables and the empirically estimated b coefficients to estimate how given respondents would be expected to vote if their political knowledge increased to this fictitiously high value and the assumptions underlying the statistical model held; and then

[6] Note too that if the *Knowledge* variable is set to have values falling strictly within the 0 to 1 range, then Equation 1.1 can be rewritten without any further ado as in this equation (cf. Bartels 1996):

$$Preference = fn\begin{pmatrix} a + b_0 Knowledge + b_1 X_1 Knowledge + b_2 X_2 Knowledge + \ldots + \\ +b_i X_n Knowledge + \\ +b_{n+1} X_1 \left(1 - Knowledge\right) + b_{n+2} X_2 \left(1 - Knowledge\right) + \ldots + \\ +b_{2n} X_i \left(1 - Knowledge\right) \end{pmatrix}$$

3. compare these predicted preferences to those expected if the assumptions underlying the regression model held and the respondents' knowledge level remained as observed.

These estimates are not reliable regarding particular individuals when the initial regression analysis omits important determinants of vote choices either for lack of data, or because the given determinant is suspected to be endogenous to knowledge, or vote choice, or both.[7] However, the same estimates have a very useful property if the estimated vote function allowed all interactions between knowledge on the one hand, and all shared determinants of political knowledge and vote choice on the other. In that case, statistical models following Equation (3) can be used to derive approximately correct estimates of the direction and size of the *net aggregate* change that would occur in the distribution of political preferences within any group defined in terms of the regression equations' independent variables if the knowledge level of all group members changed to a given degree.

This merely requires calculating the difference between the two sets of predicted values mentioned above. Remember that one of these sets of predicted values refers to the vote probabilities predicted for the respondents under the given statistical model, given their observed information level and their specific constellation of characteristics on the *X* variables. For instance, if the dependent variable was voting preference among three parties, and we run a multinomial regression or a discriminant analysis to model the impact of political knowledge on these preferences, the predicted value will be the probability of support for each of the three parties, say .65, .25, and .10 for parties A, B and C, respectively.

A second set of such predicted probabilities can refer to the preferences predicted for the respondents at any fictitious, arbitrarily high knowledge level, given the statistical model and their specific constellation of characteristics on the *X* variables. Given how they are derived, these predicted values are identical for every respondent with identical (fictitious or observed) knowledge level and values on the *X* variables. As long as the model controls for all variables that influence both the dependent variable and knowledge level, the mean of these predicted within any group defined in terms of the *X* variables gives an unbiased estimate of the fraction of vote that each party can expect in that group. The electorate as a whole can also be such a group.

Thus, the numeric example above can be interpreted as follows. In a certain group of respondents who share identical characteristics along the *X* variables, we expect 65, 25 and 10 percent voting for Party A, B and C, respectively, if everyone voted and their knowledge level remained unchanged. Suppose that the simulated distribution of the same votes is 68, 28 and 4 percent if the individual knowledge levels of these people increased to that fictitious level that our estimation process focused on. The sum of the absolute differences divided by two is, in our case, six percent. This is the total "information effect" on the vote.

To increase the robustness of the results, four different scenarios of information change were simulated with four slightly different vote functions for 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 lexical knowledge (see below), the socio-demographic variables listed below, and

[7] However, Sturgis (2003) provided evidence that these simulated estimates of possible knowledge-induced opinion change broadly correspond to the actual changes that occur in the political opinions of the respondents when they attend a deliberative poll after an initial survey.

the simple interactions between the knowledge variable 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, i.e. added to the above set of independent variables the squared value of the knowledge variable plus its interactions with each socio-demographic variable.

The four different scenarios of information change in the electorate assumed, respectively, that every citizen's knowledge level rises to .5, .67, .83 or 1 on the knowledge variable, or remains unchanged if it was observed to be higher than that. Thus a total of 16 different estimates of information effects were derived for each election. These 16 indicators were strongly correlated across the 61 elections in the analysis. Their first principal component served as the aggregate-level *InformationEffect* variable appearing in the analyses reported in the main text.

The *X* variables that entered the vote functions directly and/or in interactions with political knowledge and its squared value were as follows:

AGE: the age of the respondent in years;

AGESQ: age squared;

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 election;

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

MINORITY 1: coded 1 for Asians in Australia; Belorussian-speakers in Belarus; American Indians, Blacks, and Mulatto in Brazil; French-speakers in Flanders and Dutch-speakers in Wallony 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; non-Catholics in Chile; Moslems in France; Swedish-speakers in Finland; Catholics in either part of 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 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.

Note that missing values on these *X* variables were mean-substituted. The sample design or demographic weights provided with the CSES data sets were used. Nonvoters 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. For the 2002 Portugese election data were available in both the CSES1 and the CSES2 data sets, and showed slightly different information effects. The *InformationEffect* variables for that election show the average of the two estimates. Technically similarly, countries that show great regional variations in electoral alignments and provided sufficiently big subsamples for specific regions of interest were split during the estimation of information effects. Thus, estimates for the *InformationEffect* variables in Belgium, Canada, Germany and the UK are always the population-weighted averages for separately derived estimates for two parts of the respective country, i.e. Wallony and Flanders, Quebec and the rest of Canada, the Eastern and Western states of Germany, and Scotland versus England and Wales combined.

The individual-level knowledge measure used in the vote functions is based on how the respondents placed various political parties on eleven-point scales running from "left" to "right". In brief, it was first determined how much political knowledge different responses to these questions implied, and then the "truth-values" of all responses given by the respondents regarding all the parties they were asked about were summed up. The number of parties that the respondents placed on the left-right scale ranged from three in Britain, for instance,[8] to nine in the Dutch data in the CSES 2 data set. For Japan, a progressive-conservative scale was used instead of left-right.

I reckon that different respondents probably have different "anchor points" on the same scale. For instance, a left-wing respondent may place left-wing parties closer, and right-wing parties further away from the perceived mid-point of the left-right scale than a right-wing respondents does (see e.g. Kitschelt 1995). Similarly, two equally highly informed respondents may give more or less widely scattered responses about the position of different parties on the same scale depending on minor differences in how they interpret the endpoints of the issue scales, or whether they think that the parties in their country generally offer too little choice or ways too polarized positions on relevant issues. How far someone places a party on a scale from what seems to be the best response category may say something about how knowledgeable the respondent is, but also speaks about the political views of the person. There appears to be no way of telling apart the valid information about knowledge from the information about political views.

Given that the purpose of my analysis is an analysis of the direction of relationships between political knowledge and voting preferences, it seemed more important to minimize the systematic error variance on the knowledge variable than to minimize its random error variance. Thus, the absolute party placements on the two ten-point scales were replaced with relative placements involving pairs of parties, and all responses regarding each pairs 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". This simplification of the responses most probably involved the

[8] The placements of the Scottish Nationalist Party and Plaid Cymru were ignored because these were only available for small regional subsets of the UK sample.

loss of some valuable information about political knowledge, but almost certainly made the resulting knowledge variable less polluted with systematic biases towards a specific political perspective.

The crux of the matter is defining what really is a knowledgeable answer regarding these relative party placements. Obviously, in everyday political discourse left-right placements are eminently disputable questions, so we should not believe that there is a single right answer to the respective questionnaire items and that all other responses are simply and equally wrong. Rather, the truth-value of each answer is a matter of degree. The solution adopted here allows for the possibility that "do not know" or missing answers to such questions may not always represent less knowledge than some other responses do (see Berinsky 2002; Mondak and Davis 2001; Mondak and Canache 2004). But more importantly, the "truth-value" of each relative party placement is determined by how much more likely a maximally informed respondent was to give that response than a maximally uninformed respondent was. This can be estimated by regressing relative party placements on other available indicators of political knowledge in the CSES surveys, which included country-specific questions about lexical political knowledge and - in the CSES 1 surveys – name recognition of candidates running for election in the respondent's electoral district.

Since the simultaneous dependence of both knowledge and party sympathies on socio-demographic background may create spurious correlations between these simple knowledge variables and certain patterns of relative party placements on the left-right, which really reflect just a particular political perspective shared by individuals who, because of their socio-demographic background, are likely to score high on lexical knowledge variables. To filter out these spurious correlations from the process of determining the "truth-value" of each relative party placement, the multinomial logit analyses that were carried out for each pairwise comparison of parties on the left-right scale included among the independent variables age, gender, income and education (for their coding see above).

The results of these regressions are of no substantive interest here and cannot be reported for sheer reasons of space, given the large number of national samples and pairwise comparisons between parties for which the regression analyses had to be carried out separately. The relevant yield of these analyses was the predicted probability 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 interest in, and exposure to the campaign. 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.

Suppose now, for instance, that the fictitious Superinvolved respondent had a predicted probability of .2, .2, .4 and .2 to place party A to the left of Party B, to the right of Party B, to the same place as Party B, or fail to place at least one of the two parties on the left-right scale, respectively, while the same probabilities for the fictitious Superuninvolved respondent were .0, .3, .4 and .3, respectively. The modal answer for both – with a probability of .4 - is that the two parties have the same position. Maybe in some objective sense – such as in expert judgments – this is the "correct" answer to this particular placement problem. However, since this answer is equally frequently given by both people who are likely to be highly informed and those who are mostly likely uninformed, we cannot guess from these answers whether the person who gave it is from among the first or the second group. Thus, the contribution to such an answer to a good knowledge scale is exactly zero.

In contrast, the Superinvolved respondent has a twenty, while the Superuninvolved a zero percent probability to place Party A to the left of Party B. Clearly, this is a minority opinion, but the view of a sophisticated minority. Maybe it reflects some relatively new information, or a very subtle reading of the leaves, possibly relying on different left-right semantics than what is most common in the electorate. Either way, if someone gives this answer, our best guess is that the person is probably rather knowledgeable. So, in constructing the knowledge scale, respondents should be given a plus .2 (.2 minus .0) score for this answer. Similarly, they should be given a negative -.1 score for either not placing both parties on the scale, or for placing Party A to the right of Party B, because these answers are ten percentage point more likely for a Superuninvolved than for a Superinvolved respondent.

This method of determining the relative truth-value of the responses has numerous advantages. It even allows for the possibility – however unlikely it is - that for some parties "do not know" may be the most informed response that any citizen can possibly give regarding their position on certain issues. In yet other instances there may be several equally good answers to the same party placement question, and if so, then this method is capable of discovering that. No matter how small a minority gives an answer, it can qualify as the best possible answer according to this method, provided that the probability difference between the Superinvolved and Superuninvolved respondents is higher for offering this response than for any other. The method gives a natural weighting of party pairs and scales for the building of the knowledge scale that can vary across countries as it seems appropriate, and which 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 is straightforward and yields a very nearly normal distribution of scores within most national samples in the CSES data set. To standardize the distribution across countries, the resulting knowledge variable was converted into normal scores constrained to fall in the 0 to 1 range, with a mean of approximately .5 and a standard deviation of approximately .16. This rescaling completed the construction of the individual level political knowledge that was then used in the simulation of aggregate-level information effects on election outcomes as described above.

Table 1: Descriptive statistics for variables about individual election cycles

	N	Minimum	Maximum	Mean	Std. Dev.
Resulting level of Voice and Accountability	61	-1.68	1.61	0.91	0.67
Resulting level of Political Stability	61	-1.16	1.58	0.59	0.71
Resulting level of Government Effectiveness	61	-1.19	2.28	1.18	0.88
Resulting level of Regulatory Quality	61	-1.53	1.89	1.05	0.69
Resulting level of Rule of Law	61	-1.04	2.10	1.05	0.93
Resulting level of Control of Corruption	61	-1.02	2.49	1.16	1.04
Resulting level of Governance (factor score)	61	-2.74	1.17	0.00	1.00
Starting level of Voice and Accountability	61	-1.36	1.68	0.91	0.68
Starting level of Political Stability	61	-1.54	1.57	0.61	0.72
Starting level of Government Effectiveness	61	-1.04	2.48	1.22	0.94
Starting level of Regulatory Quality	61	-2.36	1.92	0.97	0.75
Starting level of Rule of Law	61	-1.12	2.20	1.06	0.93
Starting level of Control of Corruption	61	-0.90	2.49	1.21	1.02
Starting level of Governance (factor score)	61	-2.73	1.22	0.00	1.00
Information Effect on Election Outcome at t-1	61	-1.53	3.40	0.00	1.00
Ideological Polarization	61	0.21	3.08	1.80	0.66
Effective Number of Parties	61	1.69	7.69	4.01	1.31
New Democracy	61	0.00	1.00	0.42	0.50

Table 2: Descriptive statistics for variables about pairs of elections

	Weighted N	Min.	Max.	Mean	Std. Dev.
Resulting level of Voice and Accountability	23	0.04	1.59	1.09	0.43
Resulting level of Political Stability	23	-1.16	1.58	0.65	0.70
Resulting level of Government Effectiveness	23	-0.60	2.20	1.37	0.73
Resulting level of Regulatory Quality	23	0.10	1.89	1.27	0.46
Resulting level of Rule of Law	23	-0.77	2.10	1.24	0.79
Resulting level of Control of Corruption	23	-0.49	2.49	1.34	0.90
Resulting level of Governance (factor score)	23	-2.62	1.14	0.00	1.00
Starting level of Voice and Accountability	23	-0.49	1.68	1.04	0.59
Starting level of Political Stability	23	-0.70	1.52	0.75	0.63
Starting level of Government Effectiveness	23	-0.20	2.48	1.50	0.80
Starting level of Regulatory Quality	23	0.40	1.70	1.06	0.35
Starting level of Rule of Law	23	-0.61	2.20	1.32	0.78
Starting level of Control of Corruption	23	-0.48	2.48	1.50	0.92
Starting level of Governance (factor score)	23	-2.31	1.19	0.00	1.00
Information Effect on Election Outcome at t-n	23	-1.51	3.25	0.00	1.00
Information Effect on Election Outcome at t-m	23	-1.54	1.70	0.00	1.00
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	23	2.90	29.21	9.45	5.92
Ideological Polarization (first election)	23	0.66	2.98	1.84	0.69
Effective Number of Parties (first election)	23	2.56	5.88	3.87	0.99
New Democracy	23	0.00	1.00	0.36	0.49

Table 3: OLS regression of the *Resulting level of Voice and Accountability* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.943	0.036	0.958	26.350	0.000
Information Effect on Election Outcome at t-1	-0.030	0.024	-0.045	-1.236	0.222

N=61 election cycles. Constant not shown.

Table 4: OLS regression of the *Resulting level of Political Stability* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.947	0.037	0.957	25.256	0.000
Information Effect on Election Outcome at t-1	-0.009	0.027	-0.013	-0.348	0.729

N=61 election cycles. Constant not shown.

Table 5: OLS regression of the *Resulting level of Government Effectiveness* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.910	0.031	0.968	29.477	0.000
Information Effect on Election Outcome at t-1	-0.005	0.029	-0.005	-0.164	0.871

N=61 election cycles. Constant not shown.

Table 6: OLS regression of the *Resulting level of Regulatory Quality* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.871	0.042	0.948	20.887	0.000
Information Effect on Election Outcome at t-1	0.030	0.031	0.044	0.962	0.340

N=61 election cycles. Constant not shown.

Table 7: OLS regression of the *Resulting level of Rule of Law* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.995	0.014	0.995	73.024	0.000
Information Effect on Election Outcome at t-1	0.010	0.013	0.011	0.824	0.413

N=61 election cycles. Constant not shown.

Table 8: OLS regression of the *Resulting level of Corruption Control* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	1.005	0.023	0.986	42.894	0.000
Information Effect on Election Outcome at t-1	-0.044	0.024	-0.042	-1.835	0.072

N=61 election cycles. Constant not shown.

Table 9: OLS regression of the *Resulting level of Governance (factor score)* governance indicator on the lagged value of the dependent variable and *Information Effect on Election Outcome at t-1*

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.988	0.020	0.988	49.397	0.000
Information Effect on Election Outcome at t-1	-0.007	0.020	-0.007	-0.355	0.724

N=61 election cycles. Constant not shown.

Table 10: OLS regression of the *Resulting level of Voice and Accountability* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.702	0.056	0.955	12.466	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.007	0.006	-0.095	-1.235	0.231

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 11: OLS regression of the *Resulting level of Political Stability* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.995	0.101	0.896	9.808	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.012	0.011	-0.104	-1.141	0.267

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 12: OLS regression of the *Resulting level of Government Effectiveness* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.869	0.074	0.951	11.770	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.008	0.010	-0.065	-0.807	0.429

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 13: OLS regression of the *Resulting level of Regulatory Quality* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	1.068	0.195	0.802	5.471	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.007	0.011	-0.088	-0.598	0.556

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 14: OLS regression of the *Resulting level of Rule of Law* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	1.010	0.037	0.998	27.381	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.009	0.005	-0.069	-1.902	0.072

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 15: OLS regression of the *Resulting level of Corruption Control* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.977	0.053	1.000	18.321	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.022	0.008	-0.146	-2.678	0.014

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.

Table 16: OLS regression of the *Resulting level of Governance (factor score)* governance indicator on the lagged value of the dependent variable and multiplicative information effects over pairs of elections

	B	Std. Error	Beta	T-value	sig.
Starting level of the dependent variable at t-1	0.983	0.059	0.983	16.675	0.000
Information Effect on Election Outcome at t-n x Information Effect on Election Outcome at t-m	-0.020	0.010	-0.117	-1.980	0.062

N=29 pairs of elections in 23 countries weighted to give each country an equal weight of one. Constant not shown.